

ON ARCHAEOLOGY AND ALTERITY

by

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Abstract

Responding effectively to alternative ideas about humanity's past is a growing concern for many archaeologists, as popular television programs, the Internet, and best-selling books increasingly promote theories which dramatically oppose accepted academic archaeological interpretations. However, this problem has traditionally been undertheorised, or considered primarily within scientific formulations which simplistically contrast good, logical, 'orthodox' archaeology with bad, illogical, 'lunatic fringe' archaeology. As an analysis of some current ideas about the archaeological past demonstrates, this can be a false dichotomy that impedes constructive thinking. Neither dowsing nor psychic archaeology, for example, are clearly illogical or marginalised practices; designating them as such, however, reveals much about *a priori* convictions. Earth Mysteries in the U.K. has features in common with British academic archaeology. Apocalyptic threads running through some alternative archaeology books are ancient in their structure, not strange and new, and can also emerge in academic archaeological writings. This does not mean that there are no grounds for distinguishing between different accounts of the past, but it does mean that some standard intolerances within archaeology, based upon notions of demarcation which do not work, must be questioned. Outside the discipline but exerting their influence within it, the Skeptics' movement and the 'anti-anti-Science' contingent of the Science Wars are increasing polarisation around issues of rationality, and responding to divergent ideas inside and outside academia with open hostility. However, there are well-established philosophies that permit the avoidance of such divisive and all-encompassing conflict. A hermeneutic rather than foundationalist approach to the dilemmas of alterity provides more robust and responsible possibilities.

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The sublime and the ridiculous are often so nearly related that it is difficult to class them separately. One step above the sublime, makes the ridiculous; and one step above the ridiculous, makes the sublime again.

- Thomas Paine, *Age of Reason*

Common sense is the most widely shared commodity in the world, for every man is convinced that he is well supplied with it.

- Descartes, *Le Discours de la Méthode*

Every time a child says 'I don't believe in fairies' there is a little fairy somewhere that falls down dead.

- J.M. Barrie, *Peter Pan*

Who can refute a sneer?

-William Paley, *Moral Philosophy*

Behind epistemological choices there are social forces. We all know that violence hides behind the most noble and pure statements. But with a more developed consciousness of the fact that the taking of epistemological positions always involves the position of the scientific field of those who take them, and the type of capital which it commands, we see that scientific strategies which are presented as absolute and universal choices are often little more than rationalizations of their own limits.... among the most rigorous censors, there are the internalized censors, the categories of thought which determine that there is only black and white, and that grey areas do not exist. So, defining the limits of thought is not at all an exercise in pure speculation. There is nothing more practical.

- Pierre Bourdieu, "Thinking About Limits." (1992:48).

Happy Birthday, Mark

Preface

A Beginning in Four Parts

Dear Reader,

All dissertations are documents of a journey; often, however, the travels are suppressed or hidden within the text, and the focus is on what was actually the end point. In the case of a dissertation like this, which is about learning how to approach a certain problem, that would be both disingenuous and counterproductive. So, in the absence of a conviction that I have reached a solid understanding, in the absence of a conviction that there is a solid understanding to be reached, and in the absence of appreciation for conventions of writing which pretend that either of these convictions are present when they are not, I have tried to make the journey transparent. It seemed to me to be the only option with integrity, and that would make sense.

This is inevitably a personal dissertation, likely more so than many in archaeology. In part this is because it is a chronicle of how one person began to think through a problem. And in part this is because it has been intensely social – my concern has been more with people who are living than those who are dead, and my engagement with the subject has been primarily future-oriented rather than historical. Thus, the experiences I have had in the course of the work have had much to do with who I am. There has been a good deal of writing lately in anthropology on positioning one's subjectivity; it is possible to heavily theorise the matter and to discuss the role of the author at considerable length. I would rather simply take it as a given that I am very much present in this work, and that there is no point pretending even for a moment that these words came into being of their own accord; thus, I use the first person regularly in some parts of this dissertation. I also think it only right to introduce myself to you, to ease the burden of interpretation. I don't think, dear reader, that telling you about myself will prevent misunderstanding. I am certain that we will understand and misunderstand each other just the same, for that is the nature of our relationship. But I am equally certain that not telling you any of what follows wouldn't help.

An experienced reader of academic texts in archaeology will (for better or worse) be able to infer much of my background from my use of words and ideas, just as a specialist in accents would be able to tell that I am a child of Welsh parents who emigrated to Canada, born and raised near the American border in Southwestern Ontario, who then spent several years in England, primarily in the North. Or just as a perceptive psychologist would suspect that I am a child of teachers, taught to love both sciences and humanities, but brought up in a small industrial city which one had to leave to pursue university education. However, there are things that probably should not be left to the reader to infer, things that if made explicit, might make this dissertation more intelligible.

This work has taken me from age 24 to 29, completing the formal schooling in matters anthropological and archaeological which has filled all of my adult years. McMaster University in Ontario, Canada was my academic home for my BA and MA, each in a combination of physical anthropology and archaeology. I then began my PhD at the University of Sheffield, lived in Cambridge for my first year of study, then in Sheffield for two years, and moved home to Canada to complete the dissertation, first in London Ontario and then in Toronto. In sum, this work has been written on two continents, in two nations, five cities, eight homes, and on five computers. Smaller but no less important parts have been written in libraries of several universities in the UK and Canada, and in the strangely inspiring nowheres of train stations and airports, and on planes crossing the Atlantic.

This motion, this constant change in perspective, is the key to this dissertation. It is, all at once, the history, the outline of the script, the stage upon which the action is played, and the set which gives it some of its meaning.

Some Limits

Of course, like everyone, I am more than my surroundings, and am capable of thinking at least a little beyond my indoctrination and my intellectual environment. But their influence is not to be underestimated. I write on an IBM using Windows 95 and Word 7. Even these constrain my thought and limit my possibilities, just as the ergonomic chair in which I sit does not permit me to slouch as I type, or at least forces me to find creative ways to do so. My broader physical and cultural environment constrains and limits in the same way.

There are things I can conceive of and understand in Britain which I cannot fathom in Canada. The words can be the same, but the meanings slide into distant unintelligibility. And so this thesis could not have been written just anywhere, and it could not have been written in just one anywhere.

Growing up in my corner of Canada, Southwestern Ontario, the past was elsewhere to me. It was invisible. I didn't know it – one doesn't understand that something isn't there until a contrast is experienced, which makes the absence evident. My knowledge of this grew during my initial years in the U.K., but crystallized abruptly as I sat in a café across from King's College in Cambridge, idly watching a pigeon defile a statue of Henry the Eighth which was built into the college's facade. I had Pierre Nora's *Realms of Memory* open in front of me, and the light went on. It was, hilariously enough, one of those moments of clarity that everyone experiences and treasures. Right then, I felt I really understood – not just in my head, not just in words, but in my eyes, in my knees – what a number of European scholars have said about where the past really is.

I finished my coffee – no sense in trying to prolong the moment of epiphany, for that rarely works – and left, knowing, even as I photocopied far more of Nora's book than was technically permissible, that I could take the words with me, but that they are themselves concrete signifiers to which one relates, as to a megalith, and that where one stands is at least half of the installation, half of the resulting comprehension. My understanding of those words is situated in time and place, and without that context they can be but little to me.

The next day, I flew back to Ontario, where the terms of engagement with history are so different, and, as I have learned to expect, over several days my internal gyroscope shifted, the colours changed, and I saw with different eyes, and I had to fight to revisit the way I had thought less than a week before. In a way, this brought a new understanding of Nora's *lieux de memoire* – what to do when the *lieux* are gone? – but it primarily brought a feeling of loss. It always does.

And so it has taken me some time to understand that the cornfields surrounding Avebury, which I visited two summers past, and the cornfields at which I stared from the window of the tall apartment building in Ontario where I wrote last summer, are not the same, not

even close. They both signify relationships between people and land, and between past and present, but they mean entirely different things to those who live near them. The archaeologist and the farmer both know that beneath the Ontario fields lie the remains of the villages of people who lived here before Europeans came, and for a short while thereafter, until they were forcibly dispersed and eventually 'resettled'. The archaeologist and farmer know that the field walker will be rewarded with flint scatters and pottery, and the excavator will have middens to poke through, and hundreds of post moulds outlining longhouses and palisades. But to those who drive past those cornfields every day, there is no visible hint, and no general knowledge, of the complex societies who inhabited that space only a few hundred years ago. In contrast, Avebury physically intrudes into the present in a way that is visceral, immediate, and impossible to ignore.

Differences such as these have meant that my comprehension of the British engagements with history – everyday, avocational, and professional – is coloured by habits of seeing and not seeing. In one sense, this has been useful, for the landscape and people's reaction to it have been easier to watch from an alienated position than a familiar one. But it does mean that there are some things about people in the British landscape that I can frame intellectually, but not truly understand.

The Trajectory of This Work

Explaining the direction from which I initially approached questions of alterity, or 'otherness', in archaeology may help to explain the fundamental tension that underlies this dissertation, between an explicitly scientific approach to archaeology and a more humanistic approach.

I began with an intense interest in pre-contact and contact-period health in the Americas. My MA thesis considered some of this in detail; on one level, it was about using archaeological settlement data as a basis for inferences about community health in Cahokia and the Mississippian communities of the American Bottom. However, there was a second theme that was equally important to me – the problem of how archaeological evidence was used to come to conclusions about prehistoric health. Specifically, there were two things that concerned me. First, in bioarchaeology, there has often been an underlying conception of skeletal remains as 'direct evidence', and of osteological data as epistemologically superior

to archaeological data, despite the many interpretive filters involved in translating a bone into a theory about past population health. Second, there has been an emphasis on the formulation and rigorous testing of hypotheses as the only appropriate methodology for investigating past health.

These two ideas are interrelated, in that the archaeological data were usually seen as only fit for *generating* hypotheses about past health, which would then have to be *tested* using osteological data. However, given that skeletal remains are not 'direct evidence' about past population health – the record they provide has been transformed by the selective response of bone to stress, by cultural and taphonomic processes, and by multiple layers of interpretation, some of which are by no means secure – I had to ask if they were appropriate as final arbiters of hypotheses about past health which were based on archaeological evidence. I concluded that they were not necessarily, and that testing a hypothesis with osteological data did not always provide a solid basis for evaluating its veracity, not only because of the problems with the osteological data themselves, but also because of problems with embedded auxiliary hypotheses. I further argued that in some cases, theories based on archaeological settlement evidence taken in conjunction with modern epidemiological evidence could be just as useful on their own as they were in a context where they were being 'verified' by skeletal evidence.

Perhaps more important to me now is the fact that, although most osteoarchaeologists certainly recognize the existence of interpretive filters, and reject the assumption that bones are 'direct evidence', it is not difficult to find cases where research programs have been designed around that very assumption, and where researchers limit themselves to rigid hypothesis-testing forms of investigation, despite the strong argument within the philosophy of science that testability or 'verification' is not necessarily meaningful.

For example, Rothschild wrote that "If a testable hypothesis cannot be generated, perhaps the problem should be placed on hold, or an interdisciplinary council formed for determining new technologies appropriate to its assessment" (1992:131). Similarly, Buikstra (1991:188) wrote that bioarchaeology should aspire to being a discipline where "sophisticated models are developed, expectations are formally derived, and tests are statistically rigorous"; yet, in the very next sentence, she notes that "the major limiting factor would seem to be sample size and the quality of the contextual data." These are limiting factors indeed, and would seem to indicate that there may sometimes be a fundamental incompatibility between the data available and the method advocated.

This is just one example of an epistemological or methodological barrier obstructing the realization of the full potential of the data available to researchers. But more than this, there is a mismatch between the *representation of the process* of bioarchaeological research and the *actual process*. In particular, it is perfectly clear that in reality, not all research into prehistoric health aspires to the hypothetico-deductive format. Quite often, untestable speculations are unconcernedly made about past health conditions. So, theory does not match up to practice, and the only recommendations being made for the improvement of practice are incompatible with reality. Examination of the *real* situation, and building upon that foundation to provide recommendations, I believed, would surely be more helpful than an abstracted model that is disconnected from research practice. This concern was the source of my commitment to the philosophy of archaeology.

So, the particular areas which concerned me when I finished my M.A. were problems in archaeological approaches to the assessment of the quality of evidence, the definition of appropriate methodology, and adjudication between incommensurable theories. Hard on the heels of finishing that degree, I got on the plane to England, and so that point of completion was also effectively my point of entry into the current work.

The current project was defined after I arrived in the UK and learned of ley hunting, which I initially understood as a discourse about the past that was separate but parallel to archaeology. This seemed an excellent case study through which to look at the practical relationships of incommensurable theories, and a good way of getting at archaeology's intradisciplinary representations of the ideal research process. And so I began by examining the current and historical relationship between orthodox and heterodox archaeological research communities in the UK, noting with some surprise the elastic and permeable nature of the boundaries between these groups. 'Alternative' archaeology and 'orthodox' academic archaeology are usually represented in public and scholarly debate as mutually exclusive in terms of members, methods, and theory; however, the two are actually intertwined on many levels. For example, in Britain, although dowsing for buried features is almost never mentioned in site reports, and those who proclaim its utility are frequently mocked by some academics, it is often practiced by 'orthodox' archaeologists, and is sometimes even taught to archaeology students at major universities. Similarly, many of the key tenets of archaeoastronomy in Britain were, in the end, abruptly accepted by orthodox archaeologists who had vilified its proponents for years, while conversely, diffusionism, long an instance of

orthodoxy, is now the hallmark of lost continent theorists. Thus, it is fairly straightforward to demonstrate that theories and methods (as well as people) routinely move back and forth across the orthodox-heterodox divide.

Thus, it became clear that framing the entire dissertation around 'the relationship between the orthodox and the alternative' would mean that my direction was essentially controlled by an idea that I had rejected. But escaping this dichotomy wasn't easy, because it meant that I needed a new place to stand. My search began for a theoretical framework which permitted me to stand far enough outside archaeology to see it as something that wasn't the centre of the universe, yet which would allow me to make a contribution to it. It is not surprising, in retrospect, that this search is not over.

But this problem was circumvented to a degree by the second main phase of this doctoral work, which evolved out of close contacts with people in 'alternative' archaeology in Britain. I came to realize that useful understandings of these people and their ideas, like useful understandings of people involved in 'orthodox' archaeology in the U.K., meant seeing them within their historical and social contexts. There was no sense in drawing up abstract typologies of characteristics of their thought, no sense in trying to derive general statements about the essential nature of 'alternative' archaeological ideas. (Cf. previous archaeologists writing on 'the fringe', in Chapter 2.) I began to learn of the role of archaeological monuments in shaping understandings of past and present, and consolidating personal identities, in modern Britain. Whatever the ancient monuments of the British Isles may have meant to people in prehistory, and whatever their use in the establishment of modern regional and national histories, their meanings for individuals and small interest groups today are continually renegotiated and created through personal study and social activity. From engaging in investigations in Earth Mysteries and geomancy to pagan religious activities – from dowsing stone circles to sleeping inside dolmens in search of meaningful dreams, to worshipping within long barrows – some people in Britain today interact with the stones in ways that go beyond simple sight-seeing, and beyond the comparatively detached archaeological gaze.

This led me in turn to a third place, quite disengaged from the needs of archaeology itself. This consisted of contemplation of the role that ideas about humanity's distant past play in modern society, and of archaeology's function as a generator of historical narratives which have far-reaching influence. It wasn't long before I noticed some surprising similarities

between the kinds of stories told in very popular alternative archaeology books and far older stories of Judaeo-Christian origin.

It would have been simpler, and tidier, to end the conceptual evolution of this work there. (It would have been easier to develop the apocalyptic theme into three chapters than into the one included here.) And indeed, perhaps that is where it would have ended had I completed this work in three years, while still living in Britain. However, the additional time plus the shift in perspective back to the New World, with its different engagements and immediate challenges (both as an inhabitant and as an archaeologist), conspired to convince me that I needed to come full circle, back to the original business of addressing shortcomings in archaeological theory. This was not so much because of the major problems in the theory which underlies the notion of a simple ‘orthodox’ vs. ‘alternative’ dichotomy, but because of the real-world ethical implications. Of course, I believe in the validity and importance of the idea that archaeology, viewed from a distance, is but one expression of a human need to create histories, and not necessarily superior. But just reasserting or showing this does precious little to counter the notion, still painfully prevalent within some sectors of archaeology today, that archaeologists really are purveyors of truth, and that others’ views are merely pale, pernicious imitations.

Therefore, toward the end of this dissertation, I have made a preliminary effort at recasting the problem from the philosophical roots up, as well as explaining why it is important to do so. Of course, this is not the end of the matter, for even if – as is unlikely – all archaeologists began to see the problem as partly one of coping with diversity and alterity, rather than just right interpretations and wrong interpretations, archaeologists and others would still remain, as groups, alienated from one another. This may, however, be the best that we can do.

So, although this work began with philosophical questions of the epistemological kind, I eventually arrived at questions of ethics (better late than never). But had I never left the library, it is doubtful that I would have made this leap.

Things People Say

This research involved talking to many people outside academia with diverse ideas about the past. According to my academic colleagues, doctoral students in archaeology might not

ordinarily meet these people, or might not realize that they have met them. But, of course, they are everywhere, not just in organized ‘alternative archaeology’ groups (real-life or electronic), but in bookstores, coffeeshops, pubs, taxis, airplanes, at parties, and around the supper table in the homes of friends and family. (Indeed, one does not have to look far or hard to find archaeological ‘unorthodoxy’ – orthodoxy is much scarcer – but one does sometimes need to encourage people to talk about their beliefs.) This research has also involved learning more about others whom I met within academic circles; I discovered that when I make it clear that I will not shut down conversation about ‘unusual’ topics, people have an urge to confess and discuss all their ‘heresies’. What kinds of people and what kinds of ‘heresies’? Here are just a few of many who spontaneously spring to mind as I write.¹

José is a psychologist who was born in Mesopotamia 3800 years ago, despite his South American accent, and is the 58-year old father of the Chosen One who will lead humanity past the apocalyptic destruction of 2010. (Thoughtfully, he has invited me to help.) Ruth holds an Oxbridge PhD in archaeology, and believes passionately in magic and astrology. Stephen holds a Master’s degree in folklore, has discovered earth zodiacs as well as archaeological sites, and has been actively involved in the Earth Mysteries scene for two decades. Jenny, an administrative assistant, wanted to know all about lost continents because her mum wouldn’t stop talking about civilisations in Antarctica at teatime. Chris is a taxi driver in Chicago who is saving to go to school to be a pilot. He wonders whether aliens built the pyramids of Egypt, and was so happy to talk to an archaeologist about it that he gave me a free tour of the city. Thomas holds a British PhD in the history of popular music, and is a prominent author on ufology and the paranormal. Sarah, a nurse, is quite certain that benevolent beings from the Pleiades are guiding human development on earth; she knows because they talk to her. Paul is doing postgrad research in anthropology while also formally studying druidry. Carys is an undergraduate student in archaeology and president of her university’s pagan society. Jeffrey, a librarian, is sure that human beings evolved in an aquatic environment. Sian, a PhD in geography, once wrote to say that a stone circle in Wales needed to talk to me. (To this day I wonder what it wanted to say.) Peter is a museum curator, Oxbridge-trained in history, a Buddhist, and a prolific writer on Earth Mysteries topics from fairies to geomancy. John holds an Oxbridge doctorate in

¹ All names have been changed, primarily to protect the academics who spoke in confidence.

physics, and believes that crop circles are linked to ancient sites and the religions once practiced there.

Tina is an academic archaeologist who regularly employs psychics and dowsers on her excavations, but has to keep it secret from her department chair. Allen, Oxbridge-educated in history, sleeps on sacred hills in Wales to court dreams of the Goddess. Thor is a traveller and alien abductee who visits British archaeological sites because of their location on leys, which correspond to the flight paths of UFOs. Melanie is an archaeology student at an American university who doesn't understand why she can't even speak Marija Gimbutas' name in class without being mocked. Cecil is a retired British army officer whose renowned prowess with dowsing rods has helped him to locate invisible concentric rings of energy around several stone circle sites. Andrew was dumbfounded by the sudden success of his 'alternative history' book and the phenomenal rates at which he is paid to lecture in Japan. Kathy, a member of a local Earth Mysteries group and a graphic artist, uses her pendulum at archaeological sites to learn 'yes/no' answers to questions about the lives of people who lived there. Matthew, who holds a doctorate in psychology, frequently speaks on his psychic discoveries about King Arthur.

Conversations with all of these people have been instructive, to put it mildly. Since I knew virtually nothing of alternative views about the past before I started this research, my learning curve has been steep. Given my conventional background in archaeology, it was initially not easy to refrain from rapid judgement when encountering other ideas about the past. (It is still a challenge at times.) However, four and a half years later, it is almost as hard to contain my disbelief when I hear an archaeologist categorically state that they have proven something about the belief systems of prehistoric people, or that their own understanding of the past is based on uncontaminated reason alone while all others are clearly irrational.

Chapter 1

Introduction

This is not a dissertation about an archaeological problem. It is, rather, about an archaeologist's problem – that is, a problem which archaeologists face, partly because they are archaeologists and partly because they are human beings. The problem is this: what should one do when another disagrees with one's interpretation of the past, in a profound and irreconcilable way?

Many archaeologists are coming to understand this as a central issue of contemporary archaeological practice. Difference comes in many forms and from many places. Within the academy, anticolonial and feminist views have emerged, and are beginning to change some internal dynamics in archaeological discourse (Schmidt and Patterson 1995; Wylie 1997). Outside the academy, some archaeologists struggle to find meaningful engagements with the public (McDavid in press). Meanwhile, other archaeologists inveigh against the proliferation of 'alternative' or 'fringe' archaeologies in bookstores, on television, and on the Internet, and fret about "the misuse of archaeology and the non-scientific application of the data from this field" (Feder 1996:34).

This dissertation is primarily concerned with the latter dimension of this problem of alterity. It seeks to evaluate aspects of the current situation, to situate them in historical, social, and philosophical context, and to make steps toward useful contemporary orientations for the archaeologist. In one sense, I hope this dissertation will be utterly superseded and made irrelevant within a few years, through the commitment of archaeologists to reevaluate disciplinary ideals and their location in a rapidly changing world among a multitude of other voices.

This may, however, be overly optimistic. Such reevaluation has been a major preoccupation for some archaeologists of late, but not for others. Hodder (1998) reminds us that some archaeologists today do believe in the separation of theory and data, and refuse to acknowledge that archaeology is a socially embedded activity, never mind that this reality has consequences for our practice in the world. He warns:

it is dangerous to trust in the separation of ethics and epistemology. To do so is to lose sight of the manipulation of epistemology and the construction of ethics for social purposes. Both are embroiled in the daily practices of archaeology. We have to remain alert as to how epistemologies are used within the discipline and to what the effects are on society. (Hodder 1998: 216)

Ultimately, such vigilance may be compulsory rather than optional. In many locales, archaeology is under considerable pressure, external as well as internal, to answer for its activities in the world. Wylie identifies two important presuppositions which underlie most archaeological practice in the West:

1. that archaeological practice can be clearly distinguished from nonscientific and, increasingly, nonprofessional, uses of the record;
2. that the scientific goals central to archaeological inquiry can be presumed to yield an understanding of the cultural past that is a common good, that serves humanity or society as a whole.

(Wylie 1996:166)

She goes on to observe that lately, “it is precisely these assumptions, and the priorities they establish among disciplinary goals, that are being challenged by critics both within and outside the discipline” (1996:166). Wylie describes the increasing difficulty of keeping professional and commercial interests separate, and the challenges of nonarchaeological interest groups (primarily First Nations), and concludes that archaeology’s disciplinary identity is being strained to the limit by such pressures.

Another source of pressure on disciplinary identity comes, of course, from within. It may be, as Saitta (1999) observes, that there has been a general retreat to realism in archaeological philosophy, and that the hard relativist program supposed by some to lie at the heart of postprocessualism has not been followed through. However, questions about what archaeologists know, how they know it, and how they should represent it, have had a substantial impact upon disciplinary discourse. Despite accommodations (some reluctant) from both sides of the processual/postprocessual debate, there are very real divergences evident within archaeology. Some contingents simply do not converse, but pursue their own distinctive research programs, governed by incommensurable ideals. Further, efforts to define away these splits (e.g. Van Pool and Van Pool 1999) by identifying postprocessualism as truly scientific after all, are in one sense futile, for they replicate the terms of the original debate, terms which are simply now being transferred to new ground

with the same divisive results. Wylie notes that the old confrontation between advocates and critics of “science” in archaeology is being “regenerated now by the advocates of evolutionary/Darwinian archaeology”, not just through the challenges of nonarchaeologists (1999:1).

It will be difficult, for as long as some archaeologists aspire to be scientific, to escape this rupture – it is reinforced by the world around us. This brings us to the matter of the Science Wars.

The Science Wars

As Wylie (1999:1) puts it, “We are well and truly in the era of the Science Wars and archaeology is one place where the protracted battle for the heart and soul – and crucially, the authority – of ‘science’ is clearly evident.” Indeed, she observes that “a great many ongoing theoretical and methodological debates in archaeology are deeply structured by this larger cultural, political debate about the credibility of science” (1999:2).

The Science Wars have been messy and pervasive, dredging up old sources of strife between scientists and society, and mixing in modern economic concerns (Editors of *Scientific American* 1997). Erupting with the publication of Gross and Levitt’s *Higher Superstition: The Academic Left and its Quarrels with Science* (1994), the conflict escalated with physicist Alan Sokal’s now-famous hoax in the science studies journal *Social Text*, subsequently revealed in *Lingua Franca* (Sokal 1996a; 1996b). Essentially, the Science Wars has been a no-holds-barred match, pitting natural scientists against those in science studies who observe their practices, and the many other scholars who critique either scientific practices in society or the philosophies that underlie them. It has widened the ‘Two Cultures’ schism by calling into serious question the processes of the authorisation of knowledge, and whether different camps will ever be able to ~~even~~ agree on what, if anything, constitutes rationality or truth. The stakes are perceived to be high – not just the future of particular scientific research programs or positions in sociology departments, not just the future of the academy, but the future of civil society itself – and so the fight has been dirty, often personal, and extensive, with scholars from a multitude of

disciplines and countries wading into the fray.¹ It is not surprising that, given the central positions of natural scientists and anthropologists in this conflict, and given archaeology's divided allegiances to these fields, archaeologists can find themselves situated most uncomfortably within it.

The Science Wars are relevant here not only because of their profound influence on the structuring of some current debate in archaeology, as articulated by Wylie (1999), but also because they have not stayed contained within academic borders. Fujimura (1998) notes the battle's repercussions throughout society. Of particular interest here is its substantial extension into the realms of 'the public understanding of science', and what might be considered its activist wing, the international and growing Skeptics' movement, devoted to the eradication of pseudoscience and irrationalism. Examples of the influence of the Science Wars can easily be found in *The Skeptical Inquirer*, published by the Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP), an organisation closely linked to archaeologists' vigorous debunking of various 'alternative' ideas (as discussed in Chapters 2, 4, and 8). There was, for example, more than a little gloating therein over the Sokal hoax (see *SI* issues in 1996 and 1997), and self-described "moderate" philosophers such as Susan Haack publish articles in its pages which sometimes caricature, en masse, the reservations of "radical feminists, multiculturalists, sociologists of science, literary theorists" in order to better dismiss them (1999:48). And so not just directly, but also through the secondary, reinforcing channels of the Skeptics' movement, the 'anti-anti-Science' side of the Science Wars is influencing some archaeologists' conceptions of how to deal with diverse archaeological ideas in the public sphere. That is, some archaeologists are receiving encouragement from CSICOP and similar organisations as they take an inflexibly negative stance on 'alternative' archaeology, citing clichéd descriptions of scientific rationality as their reason for doing so. This intellectual and social context for the current topic should be kept in mind, and will be returned to at this dissertation's close.

There are ways to opt out of the quagmire – and it is a quagmire, for the present terms of debate simply do not permit a resolution. Wylie (1999:11) recommends for archaeologists

¹ Useful entry points into the extensive literature of the Science Wars may be found at <http://www.physics.nyu.edu/faculty/sokal/index.html> and at <http://members.tripod.com/ScienceWars/>; many relevant articles, comments, and book excerpts are reproduced electronically at these sites or at linked locations.

a strategic recasting of the discussion, and the abandonment of “ideological debates about ‘science’ and the ‘scientific’ status of their discipline.” Until that blessed day, however, the best hope for archaeological progress in dealing with different ideas about the past might come from new engagements between archaeologists and the public.

Archaeology and the Public

Parker Pearson (1998:680) writes of archaeology that “It is the best of times and it is the worst of times.” This captures the turmoil in which archaeology finds itself, and the confusion which can dog any attempt to elucidate trends in archaeological thought. Perhaps because of the simple fact that there are so many of us, working in vastly different milieux, it would seem that almost every possible view of reality, our place within it, and the job we are meant to do, is held by some archaeologist, somewhere. And indeed, in one sense, it is only appropriate that the discipline be diverse and even fragmented, for we deal with localised challenges. It is interesting to see how these local exigencies and general imperatives relate in terms of archaeologists’ relationship to the public.

Calls for a more effective engagement with the public are plentiful on both sides of the Atlantic ocean (e.g., Parker Pearson 1998; Fagan 1999; Vitelli 1999). Sessions on ‘Archaeology and the Public’ appear to be a staple at most major archaeology conferences; however, while it has been easy to find discussion reiterating the importance of talking to the public, it has been rather more difficult to find discussion about *why* this is important, and/or what the public’s needs actually are.

At conferences such as *Archaeology into the New Millennium: Public or Perish* (Calgary, 1995) and in Public Archaeology sessions at the Society for American Archaeology meetings (e.g. Chicago 1999), or at the Theoretical Archaeology Group conferences, the archaeologists actually addressing matters of the public are usually involved in local (often museum-based) outreach programs, and their talks are often limited to practical topics such as underfunding or program cancellation, logistical problems of having members of the public work on excavations, or the effectiveness of different kinds of displays. When motivations for better public outreach are mentioned, they often fall into one of two categories: the mercenary and the missionary. By mercenary, I mean the notion that if archaeology fails to interest a wider public, its future funding may be jeopardised. Thus, in

those cases, the driving reason for better contact with the public is ultimately disciplinary self-interest. By missionary, I mean the idea that archaeologists need to spread their truth about the past, either because the public is simply ignorant or uncaring on the subject (and this is self-evidently bad), or because they hold ideas which are misguided (also self-evidently bad). Encounters with the latter idea recur frequently in this dissertation.

There can be a fundamental disrespect inherent in the 'missionary' attitude. There is, in particular, a too-frequent suggestion from archaeologists that their ideas are not reaching the public because their works are written too intelligently. (I heard it recommended at an Institute of Field Archaeologists meeting in 1997 that archaeologists should write to an average reading age of a seven-year-old, lest our ideas go straight over readers' heads.) This stands in contradiction to what I know first-hand of the informed appetite for archaeological knowledge outside of the academy. Surely flexible presentations which can cater to variable audiences are needed instead. But, this issue of how best to communicate aside, I think it can be fairly said that archaeologists' steady interest in having the public listen to them has been primarily partnered by an equally steady disinterest in what the public might themselves have ^{to} say about the past.

However, partly through the acceptance of some of postprocessualism's social implications, and partly through grassroots commitments and understandings, some archaeologists are now taking another approach to relationships with others. Top-down theory and local ground-up imperatives are converging to produce remarkable results, which are breaking down the stark 'archaeologist vs. public', 'expert vs. audience' dichotomy. Examples include: equal time being given to nonprofessional interpretations in presentations; involvement of local communities in the development of research agendas; explicit attention to multiple voices in archaeological publications; and the deliberate study of the public reception of archaeological sites (Blake in press; Duke and Saitta 1998; Hodder 1998; Holtorf 1998; McDavid in press; Piccini 1997; Ronayne 1997; Thoden van Velzen 1996). The latter has begun to receive particular notice because of a remembered realisation that people have always made their own histories, and that understanding how monuments are received today can help us understand something of their place in the social and cognitive worlds of the past – and how 'right' or 'wrong' those public interpretations are can be quite irrelevant (Holtorf 1998; Blake in press).

These hopeful developments notwithstanding, by and large, 'alternative' archaeology is still a source of distress, not interest, to most archaeologists – that is, the ones who are not diligently ignoring it. My key contention in this dissertation is that it is here to stay, and we need to find new ways of engaging with it, rather than simplistically hostile attempts to contain and control it. There are a host of theoretical reasons for this position, but also one very practical reason, which is difficult to contest no matter what one's convictions might be. That is, it is less possible than ever before to control the proliferation of alternative ideas about the past, due to a new development of the 1990s: an increasing public presence in the generally unregulated, wildly democratic territory of cyberspace.

The Internet

The Internet is causing a radical transformation in the way that people around the globe share information. This has only become truly pronounced in the years since I began this dissertation research. In fact, in 1996, I had the privilege, with other Sheffield archaeology postgraduates, of founding one of the first online archaeological journals in the world, which indicates something of the newness of the enterprise of electronic publishing within archaeology. Its potential for archaeology is profound, and the implications are extensive; abundant comments on these themes may be found in the pages of *Assemblage* (<http://www.shef.ac.uk/~assem/>) and *Internet Archaeology* (<http://intarch.ac.uk>). Here, I should like merely to point out that the flexibility of electronic media in general, and the accessibility of the World Wide Web in particular, combine to allow striking possibilities both for sharing archaeological information with a wide audience, and for inviting that audience to participate in dialogue (Denning 1997a; Hodder 1998; McDavid in press; Winters *et al.* 1997).

Other types of electronic communications, such as email discussion groups, have been prevalent for a little longer, but are still developing in form and extent. These may prove to be the most powerful forces of all in the globalisation of archaeology, for they enable informal discussion about archaeological problems and practice with people whom one might never meet in person. They ease long-distance collaboration, and also act as an equaliser at times, for on an email discussion list, everyone from senior professors to undergraduate students to those with an avocational interest can participate. This returns

me to the point alluded to above, that professional scholars and students are not the only ones making use of the Internet.

Of course, not everyone has a computer, or access to the Internet, and not everyone wants it. But more people have this than have borrowing privileges at research libraries, and its increasing importance in education at all levels, in commerce, and in social life means that the Internet is becoming a more and more powerful presence in many people's lives. Of particular interest here is the fact that the Internet has become a tool for groups who traditionally have had restricted access to means of distributing information. Websites are now a favoured means of text dissemination for many whose views would rarely or never make it into mainstream media, who only five years ago were restricted to producing books for companies without extensive marketing apparatus, or to producing paper journals with limited circulation. The Web has also become a locus of political resistance for marginalised groups worldwide, and a means of transmitting information from inside war zones. Because websites are still largely unregulated in *most* countries, and because any crackdowns on content focus on major violations of law (e.g. child pornography), there is little central or 'expert' control over content. Certainly, there is nothing preventing people from posting any archaeological theory they like on websites – in as authoritative a style as they please – and thus there are abundant 'alternative' archaeology sites to be viewed. But equally, there is nothing preventing 'orthodox' archaeologists from putting up their own websites vigorously contradicting them, and indeed some do. The 'orthodox'- 'alternative' conflict has thus transited smoothly into the ether, and seems likely to continue there unchanged in principle, though more extensive in exposure.

So, often, academic archaeologists complain that there is too much 'crap' on the Web. What this frequently means is that websites contain uncensored, non-peer-reviewed, unverified, raw, personal takes on different subjects, including archaeology. Given the often apparently random functioning of Web search engines at the initial point of inquiry (e.g. without a focused use of Boolean operators etc.), when a seeker types in a term such as "Maya", or "pyramids", one is presently more likely to turn up 'alternative' opinions than 'orthodox' information on the subject. It is not surprising that this distresses some academic archaeologists, but for those of us who want to know more of the breadth of popular ideas about human history, it is actually a boon. In practice, this unregulated flow of information means that academics have some direct access to 'what people really think', where previously this was simply unavailable or mediated either through personal contacts,

or other academics. This applies equally to finding out 'what archaeologists really think', for tacit taboos against formal publication of unorthodox opinions are sometimes disregarded in cyberspace; this proved a significant advantage in this research, as described in Chapter 4. I believe that future study of the state of archaeology as a discipline could be greatly enhanced by using this tool more extensively.

The scope of this research, and a note on terminology

As may already be evident, this research departs from some usual engagements with 'alternative' archaeology, because it was undertaken not as a debunking mission, and not for the purpose of developing criteria for demarcating between valid and invalid interpretations of the past. Rather, I have been more concerned with finding a new way to think about diverse approaches to, and interpretations of, archaeological subjects. This process has entailed consideration of philosophical problems, surveys of 'orthodox' and 'alternative' archaeologists on a range of subjects, and close examination of three case studies, centred respectively upon a method, a community, and a literary theme. The method of dowsing is a hot topic in both the U.K. and North America, both within and outside archaeology, and forms one case study. I also investigated the community interactions and literature within Earth Mysteries in Britain. My other focus here is on the apocalyptic theme which runs through many 'alternative' archaeology books which are best-sellers in the U.K., North America, and beyond.

A comment on terminology is crucial before I begin the discussion of the topics above. The way one frames, labels, and describes a problem often constructs that problem; in cases like this, therefore, language is particularly important. Often those who hold views other than those espoused by the academic establishment, in any discipline, are labeled as part of the 'lunatic fringe', or more politely, 'fringe'. In the case of archaeology, other favourite terms have included 'cult archaeology', 'pseudoarchaeology', and 'alternative' archaeology. None of them are particularly satisfactory, any more than the label 'orthodox' is. Often, they carry connotations that are inappropriate, but they are also problematic because they all define other groups as explicitly in opposition to mainstream archaeology. To begin with, this sets up and maintains a false dichotomy, for the reality is more aptly seen as a spectrum than as black and white. Further, it implicitly simplifies the diversity on each side of that posited boundary; in reality, both groups are composed of many smaller

groups, who may overlap but do not necessarily share orientations, methods, or beliefs. (There are, for example, radical streams within academic archaeology that can hardly be considered majority views.) Finally, there is a temporal dimension that must not be ignored: what is on the fringe today *may* not always be, and what is orthodox today *will* not always be. In short, the terms used for archaeological ideas, and their proponents, coming from outside the academy often reveal much more about the intent of the speaker than they do about any observable situation.

The only way to circumvent this problem of terminology, and leave the attendant conceptual baggage behind, is to consistently avoid generalisation, which I try to do in this dissertation by focusing on specific case studies. However, there are times when labels are essential either for general descriptive purposes or for the sake of confronting existing ideas, and so I do use the terms 'alternative' and 'orthodox'. I generally enclose them within inverted commas, although this can be tiresome to the reader, because I consider it essential to indicate that they are problematic.

Shouldn't this stuff be X-rated?

The question of censorship is always an intriguing one, in terms of both what should be kept unseen, and why. I have on occasion been asked why I study things about archaeology which some would prefer not to be known, lest the profession fall into disrepute – such as the simple fact that quite a few archaeologists drowse. I generally take the opportunity to ask the questioner why this should be kept secret, and whether or not there might be other aspects of archaeologists' behaviour which are rather more shameful than waving coat hangers about or even consorting with psychics on occasion. And there are more shameful things. I do not take particular pleasure in examining unfortunate episodes of individual archaeologists' bad temper or ethical lapses, or in picking apart systematic biases in professional archaeology which have harmed people – all told, I would rather be fieldwalking or reading delicious descriptions of Neolithic sites in Orkney. However, if we do not examine what has gone before, we cannot learn from it, and learn we must. And so, I think that the material covered herein should not be X-rated on either count.

Thus, this dissertation ventures into delicate territory, exploring human inconsistency, carefully maintained illusions, and sometimes even hypocrisy. I have attempted to be even-handed, for my intention overall has not been to attack or accuse, but to learn about ways in which archaeology might move forward in its engagement with the outside world. In making the choice to pay such attention to archaeology's liminal or problem zones, areas which can seriously undermine the image of the objective archaeologist, I drew inspiration from Stephen Brush who, twenty-five years ago, wrote an article called "Should the History of Science be Rated X?", which I think is worth quoting at length. Brush begins with an editorial from the Washington Post, in the wake of the last American election in which Nixon ran, suggesting that perhaps public reporting of the campaign should be censored. That editorial read:

It is time to consider whether this campaign ought not to be rated X for children, on the grounds that young and inexperienced minds might form the impression that our national politics is mainly composed of hypocrisy and cynicism. Adults know that to be wrong, of course, but there is not much in the current campaign by which to prove it.

(Brush 1974:1164)

Brush goes on to wonder whether:

young and impressionable students at the start of a scientific career should be shielded from the writings of contemporary science historians for reasons similar to the one mentioned above – namely, that these writings do violence to the professional ideal and public image of scientists as rational, open-minded investigators, proceeding methodically, grounded incontrovertibly in the outcome of controlled experiments, and seeking objectively for the truth, let the chips fall where they may.

(Brush 1974: 1164)

Brush concludes that, despite quite vigorous arguments to the contrary, censorship of disciplinary history to preserve this traditional image is not the answer, writing that

If the new approach to the history of science really does give a more realistic picture of the behavior of scientists, perhaps it has a 'redeeming social significance'. Then, rather than limiting the conception of science to the strict pattern allowed by traditional local standards, one might try to change those standards in such a way as to reflect the freedom that the boldest natural philosophers have always exercised.

(Brush 1974: 1171)

Brush's formulation was powerful, but it seems not to have convinced everyone outside the history of science, judging by some of the invective in the Science Wars. I cannot suppose that it will wield much more force within the archaeological community. However, it has been suggested by archaeologists more than once in the last decade that we needn't fear

letting go of our traditional ideals of rationality. And it has been suggested more than once that archaeologists should talk openly about how we form inferences, why we believe what we believe, and how we know what we know. And so, through the examination of areas which have traditionally been 'off-limits', and in observing how archaeology actually functions at these boundaries, I have hoped to begin building up knowledge with the power to question and inform current archaeological theory and practice.

A map of the route ahead

In the pages that follow, I discuss a series of issues in approximately the order in which I first confronted them. My first two tasks, undertaken concurrently, were to learn more about what was termed archaeology's 'fringe', and to learn more about how archaeologists have previously interacted (or avoided interacting) with it. The latter topic forms the substance of Chapter 2 as well as Appendix A. Having concluded that there were some significant unsuitabilities with existing systematic approaches to alternative archaeology, I set about looking for a new way to study the problem. Chapter 3 recounts some of the problems I encountered in that search. Questions of methodology and theoretical framework give way to more substantive matters in Chapter 4, which covers some of the complexities in current opinions about dowsing, both within 'orthodox' archaeology and 'alternative' archaeology; I conclude that the dichotomy so starkly represented by some archaeologists truly fails here, even on related matters of psychic archaeology. The situation revealed by the literature, informal communication, and survey results is, in fact, much more interesting than a simple rational vs. irrational split – that is, if one refuses to begin with the assumption of some archaeologists and devoted debunkers, that dowsing is a practice which should be eradicated. Chapter 5 covers some of the salient aspects of Earth Mysteries, both independently and in relation to academic archaeology in Britain. Once again, if one begins an engagement with that literature and its authors and readers without negative judgements prepared in advance – in contrast to some archaeologists whose opinions are related in Appendix A – it is possible to gain an appreciation for the diversity of ideas about the past, and for what they hold in common underneath the surface.

This theme is developed further in Chapter 6, which considers the apocalyptic formula running through both some popular alternative archaeology books, and some mainstream archaeology. I argue that this ancient formula, of situating readers in relation to the

beginning of time *and* the end of time, is what gives force to many works on archaeological topics, and that this is another intriguing intersection of different approaches to the past. Chapter 7 returns to theoretical concerns, in an attempt to suggest philosophical ground for a new way of approaching diverse opinions about the archaeological past. It includes brief discussions of relevant ideas from Fuller, Feyerabend, and Rorty – three thinkers who have each worked out some social implications of problems in epistemology. Finally, Chapter 8 concludes with some thoughts on why it is critical now, in 1999, for archaeologists to avoid the easy adoption of debunking agendas espoused by some science popularisers and skeptics' groups, and instead develop their own effective and mature approach to alterity.

Chapter 2

Archaeologists Looking At 'The Fringe'

It seemed logical to commence this research by examining previous engagements of archaeologists with 'the fringe'. Initially, I was looking for guidance, but I then came to look upon these as approaches which were not necessarily appropriate to the question, and from which I would prefer to diverge.

Some histories of archaeology address alternative archaeological thought in passing – for example, Trigger (1989) and Malina and Vašicek (1990) both make a few mentions of 'fringe' movements from the past – but it seems usually to be outside the scope of such books, probably because it is produced, by definition, by amateurs. Likely for the same reason, most archaeologists simply ignore alternative theories about the past, and their proponents, even if they disagree with them; it would seem that they perceive it as nothing to do with them, or perceive that there is no benefit to responding, or they are unwilling to put in the time required to issue a careful challenge to the ideas. As Ellwood (1971) noted of the plethora of books concerning spacemen and history, such refutations can be an Augean task, and can require considerable expertise. However, some archaeologists do attempt to systematically challenge – or, as is often said, debunk – 'fringe' theories about the past. Sometimes this takes the form of a short letter to the editor of a journal, sometimes an article, sometimes a book. Often these are single skirmishes, but sometimes they become wars that last decades. A good example of the latter, which continued in Britain for much of this century, was the debate between archaeologists and ley hunters, especially visible in the 1970s in *The Ley Hunter* and sometimes in *Antiquity*. Occasionally these responses are best described as rants, however; Raikes (1984), in a review of the book *Dowsing and Archaeology* (Graves ed. 1980), provides a good example of such intemperate tones. Sometimes there are detailed challenges – for example, McKusick (1982, 1984) and Feder (1980) took on psychic archaeology in journal articles, Feder (1996) addresses a host of 'fringe' topics, and Williamson and Bellamy (1983) provided the first full-length critique of the ley hypothesis.

There is, however, a second-order level of response from some archaeologists, consisting of reactions which concern 'the fringe' at its most general level – its place, what archaeologists should do about it, and where it belongs in relation to archaeology's history.

Cole (1980), for example, offers some analysis of the fringe as a social phenomenon, as opposed to simply commenting on the faults of individual fringe theories. He defines 'cult archaeology', describes its characteristics (with reference to other forms of 'pseudoscience'), and suggests that "These movements can still be better understood as a cargo cult than as unorthodox science or amateur hobby associations" (1980:19). His primary mandate, however, is not the sociological description of these movements, but the suggestion of constructive ways for mainstream archaeologists to deal with cult archaeology, which he contends they must do "if they are to maintain or broaden their support rather than cede it to cult movements by default" (Cole 1980:27). His suggestions include: responding more frequently to cult archaeology's claims through the popular news media; giving higher education courses on cult archaeology, using it to teach critical analysis skills and as a way to promote scientific archaeology; and challenging cult archaeologists to scientific debate in an open forum to expose poor arguments.

Feder (1980) is also highly critical, not only about his subject, psychic archaeology, but also towards those archaeologists who have not taken a stand against it. Like Cole, Feder (1980, 1983, 1984, 1987, 1996) feels that fringe archaeology in general should be addressed aggressively by orthodox archaeologists, and casts himself in the role of debunker. So does McKusick (1982:44), who expresses concerns that the public needs protection from fringe archaeology; he complains that professional archaeologists are not responding to these fallacies and are thus allowing them to proliferate, perhaps because of ever-growing factionalisation within the discipline which prevents the formation of a unified front for defence. He portrays the situation in black and white, us vs. them, orthodox vs. lunatic fringe, and offers no suggestions like Cole's about what orthodox archaeologists should do in response to the fringe, but clearly implies – through his own emphasis on destroying the credibility of the authors he is reviewing – that the only right thing to do is relentlessly debunk. The reasoning and internal dynamics of fringe archaeological movements do not interest him in and of themselves.

Kerby, however, responds to McKusick (1982) by saying that such debunking efforts are futile, and offers an illustration of the common 'ignore it, maybe it will go away' response

mentioned by Cole (1980): “while we seem to be drowning in a sea of creationists, clairvoyants, creeps and cretins, let’s just do our jobs, research honestly, publish quickly and depend on the rising generations of the few who have always supported the search for truth. We cannot educate the mass – it is ineducable” (Kerby 1982:399).

These, then, are some suggestions made by archaeologists regarding what to do about the fringe in general; these examples can be read as part of a manifesto for the protection of archaeology’s interests and borders, and of rationalism in general. It goes one further in the compilation *Cult Archaeology and Creationism: Understanding Pseudoscientific Beliefs about the Past* (Harrold and Eve, ed. 1987, expanded and reprinted 1995), one of the very few analyses of the social context of belief in fringe archaeological theories. As discussed at greater length in Appendix A, the contributors to this book (including Feder and Cole, discussed above) examine fringe belief not merely out of curiosity, but with the express intent of learning how best to combat such ‘deviancy’. To these authors, fringe archaeology is representative of a serious threat, and the only thing to be done is attempt to save as many of the heretics’ souls as possible.

These fears are amplified by associations with the Committee for the Scientific Investigation of Claims of the Paranormal (known as CSICOP) and its journal, *The Skeptical Inquirer*, whose mission is to fight irrationalism in society at large. Many archaeological ‘debunking’ articles have appeared in *SI*, and those which appear elsewhere often cite CSICOP as a valuable resource. The implications of this will be discussed a little more in Chapter 8.

One might suppose that the approaches discussed – from Cole, Feder, McKusick, and the contributors to Harrold and Eve’s book – above are distinctively American, and do not need to be considered outside of that context. Or one might expect that their wave of influence has passed, for after all, some of the works cited above are now fifteen years old. However, space and time are no boundaries here. CSICOP is alive and well, and these works have been influential in setting the tone for discussion of alternative archaeology by authors on both sides of the ocean (e.g. citations of Feder in van Leusen 1999), and terms like ‘cult archaeology’ are often used by archaeologists in informal contexts¹. In addition,

¹ For one use of the term “cult archaeology”, see Doug Weller’s Archaeology web page with many links to critiques, at www.ramtops.demon.co.uk.

there has been a major retrenchment of naïve rationalism in some sectors of academia as a result of the challenges of the Science Wars, and this is showing, too, in academic archaeology. In his much-used text, *Frauds, Myths, and Mysteries: Science and Pseudoscience in Archaeology*, Feder (1996: 20) summarily dismisses deconstructionists' critiques of science by suggesting that they test their view of reality by standing in front of a moving train. In the last chapter to the new edition of *Cult Archaeology and Creationism*, Harrold et al. (1995:168) see postmodernist thought in anthropology as simply 'anti-anti-pseudoscience', a troubling enemy which unfortunately gives credibility to what they seek to combat. Other responses to the Science Wars reflected within archaeology have included both attempts to make the definition of 'science' more inclusive (Van Pool and Van Pool 1999), and the resurgence of simplistic, outdated 'unity of science' theses, described by Wylie (1999). This is not limited to the American sphere, nor is the foundationalist approach to knowledge that underlies it.

It would have been possible to take the approach used by Feder, Cole, McKusick, Harrold & Eve, etc., and adopt it wholesale for this research, squeezing the available information into their frameworks. When one is reading them, the works can seem logical and plausible and correct in their assessments of alternative archaeologies, especially if one has a previous allegiance to the idea of a scientific, epistemologically privileged archaeology. However, there are two sets of problems with such an adoption: one consists of the embedded suppositions about the particulars of orthodox and 'fringe' archaeology, and one lies in the general theme of scientific archaeology as benevolent protector.

Some Suppositions Made about the Particulars of Orthodox and 'Fringe' Archaeology

There are exceptions to each of the generalisations I am about to make, but I believe that they are generally representative features of the writings of the 'anti-pseudoarchaeology' contingent of authors introduced above. (Others writing on this topic, such as Williams (1991) and Hiscock (1996), diverge significantly on some points but I think could still be broadly identified with this school of analysis.) I include my own objections after each point.

♦The situation is often represented as a simple dichotomy of ‘orthodox’ vs. ‘fringe’, these being value-based distinctions meaning worthwhile and worthless, rather than simply sociological observations.

(In contrast, I believe that this dichotomy isn’t as real as supposed – ideas and people move back and forth across the putative boundaries, both synchronically and diachronically.)

♦It is often supposed that it is simple to distinguish between the two discrete camps on logical grounds.

(In contrast, I believe that the nature of archaeological inference is such that this is often, though not always, impossible. Certainly, sometimes there are blatant fallacies or leaps of logic, but this applies to both sides. Also, two theories may be incommensurable, but this does not require that one is not cogent. Further, the ‘demarcation problem’ is notorious among philosophers for its insolubility.)

♦It is often suggested that the ‘orthodox’ belongs exclusively to the realm of science, and the ‘fringe’ exclusively to the realm of the spiritual, the esoteric, and/or the political.

(In my reading, I have seen ‘fringe’ theories frequently involve strictly scientific matters, and the ‘orthodox’ have been known to venture beyond the strictly objective.)

♦It is sometimes implied that individuals are themselves either ‘orthodox’ or ‘fringe’ because of some characteristic inherent in their thinking.

(In contrast, I think many counterexamples exist of specific individuals who participate in multiple discourses, either simultaneously or sequentially.)

♦It is often argued that ‘fringe’ theories are simply poor substitutes for ‘the real thing’, and if only people received proper education in science and archaeology, they wouldn’t resort to such ideas.

(In my opinion, this could apply in some circumstances, but certainly not in all.)

♦ The critiques often seem to assume exactly what needs to be established, i.e., the nature and existence of the differences between ‘orthodox’ and ‘fringe’ archaeology. The views given are based in part on wishful thinking about how archaeology and science *ought* to work, or ideally work, rather than observation of how it does work and has worked.

(I think that a more realistic observation of how archaeology generally works, coupled with assessments of how it overlaps with what is usually considered 'fringe' archaeology, may lead to a much more powerful understanding.)

- ♦ There is often an emphasis on the creation of 'taxonomies of deviance' or typologies of 'fringe' archaeologists, which can preclude local understandings.

(I consider that these can predispose the reader towards slotting others into these typologies, and thus can encourage a stark polarisation where hostility rather than conversation is the rule.)

- ♦ Professional/academic archaeology is often represented as either an entity that *is* unified in opinion, or an entity that *should* be unified.

(It seems obvious to me that it isn't unified, and I cannot imagine a persuasive argument that it should be.)

- ♦ There is often a reluctance to consider the social context of the production of archaeological knowledge, and the concomitant assumption that those who do have lost all faith in science, archaeology, the possibility of knowing anything.

(I see this as a useless and counterproductive response to a problem that should concern us all.)

- ♦ It is assumed that the main reason for understanding alternative archaeologies is to better effect their eradication.

(I believe that, just as some orthodox theories need to be combated, so do some alternative theories. However, I contend that this is untenable as a blanket policy.)

I do not mean to suggest that their work of Harrold & Eve, Feder, Cole, McKusick, CSICOP, etc., is entirely invalid, for I think some of it performs a valuable function. I do mean to suggest that if we peel back some preconceptions and start a little further back, there are ways of looking at the situation which are likely to be more productive for the current research. Such a step back might also help prevent the accidental importation of the biases mentioned below.

The Theme of Scientific Archaeology as Benevolent Protector

In a statement fairly characteristic of archaeological debunkers, Feder wrote:

Archaeology and prehistory have been major targets of the purveyors of pseudoscience and superstition. In fact, as many of us are painfully aware, our field has always attracted a tremendous amount of completely unsupportable speculation by what must be explicitly labeled a pseudoscientific fringe. Members of this fringe seem bent on proving, through the misuse of archaeological evidence, all sorts of untenable racist theories, particular religious ideologies, and various esoteric views of reality. Some, of course, are for the most part content simply to make money. (Feder 1984:526)

There is much to comment upon here. First, Feder doesn't seem to allow that there might sometimes be genuine and non-pernicious difference of opinion about the past; he doesn't seem to consider the possibility that creating histories is a normal human activity, and that this is partly why alternative theories are so prevalent. Second, he appeals as much to other theories' social undesirability as their scientific inadequacy as reason why they should be combated, seemingly conflating the two issues as part and parcel of the same problem. The latter is worth additional discussion here.

The topics chosen by archaeological debunkers are often heavily emotionally loaded – modern-day Moundbuilder myths, Barry Fell's theories, von Däniken's apparent supposition that prehistoric people everywhere except Europe were essentially stupid, etc. (e.g. Feder 1996). This is as it should be, for important topics like these need addressing. However, these debunking critiques rarely restrict themselves to the topic at hand, but usually have an additional point to make, regarding the power of science (e.g. Feder 1996; McKusick 1982). And so, close examination reveals that some of the rhetorical force of these critiques goes to support their implication that scientific archaeology will protect people from social injustice. I see this as a dangerous idea to perpetuate, for it comfortably ignores what we know about how science works. It is neither relentlessly rational nor essentially kind. Thus, it would be folly to suppose that the imperatives of scientific archaeology would always put its practitioners on the right side of moral conflicts. These cases are but a convenient and possibly even coincidental convergence of archaeological and social interests. For when we ask, has archaeology always or even usually assumed this role of champion of social good? we have no choice but to conclude that it very obviously has not. Historically, for example, the wishes of Native North Americans have barely made a dent against scientific archaeological imperatives. It is true that this is

changing through legislation like the recent Native American Graves Protection and Repatriation Act². However, the mere fact that the legislation was necessary to prevent some North American archaeologists and museums treating human remains from other cultures as nothing but objects of scientific curiosity speaks volumes (Hammil and Cruz 1989; Zimmerman 1989). Many other examples of the systematic disrespect of archaeologists towards living Native Americans belie the idea that archaeology has always acted as a force for their protection (Trigger 1988). Indeed, the agenda of entire sectors of American archaeology has been specifically alienated from such concerns; as Trigger (1984) pointedly comments, one of the distinguishing features of the American processualist program has been the use of data from Native American sites as fodder from which to derive generalised principles about all human behaviour, rather than as information from which local, particular prehistories could be reconstructed.³

My intention here is not to point an accusing finger at the particular archaeologists who have made the comments discussed above, for it is quite possible that they are, as individuals, devoted advocates of aboriginal causes. I do not know.⁴ It is, rather, to say that their argument diverts attention away from the complexity of the issues – and that a close look at their claims reveals them to be mired in political rhetoric, very far from the cold, hard logic which is implied. I belabour this point because I perceive a crucial confusion here, the suggestion that method can replace conscience – a truly frightening idea. Further, there is the implication that it can do so transcontextually.

This transcontextuality is also an important feature. It generally goes unspecified in their texts that the descriptions these authors provide of the problems of ‘cult archaeology’ are meant to elucidate the American situation; so either it doesn’t need saying, or the authors believe that their descriptions and solutions are applicable internationally. At least some of the time, the latter would seem the case. This is not improbable, given that the context of

² This Act was implemented in 1990 and has been undergoing refinements and additions since. An archive of documents may be found at <http://www.cast.uark.edu/other/nps/nagpra/nagpra.html>

³ Malina and Vašicek (1990:121) quote Kent Flannery, writing in 1967: “we must search for ‘that system behind both Indian and artifacts’ and not for ‘the Indian behind the artifacts’.”

⁴ However, archaeologist Larry Zimmerman (1989:61) describes an incident in the 1970s, in which Marshall McKusick, in his role as State Archaeologist of Iowa, fought a Native American request for reburial of Native skeletal remains so adamantly that it took a court order for him to relinquish custody of the bones.

origin of these critiques is American ‘scientific’ archaeology, which is supposed to delineate ways of thinking which are not context-dependent. As mentioned, Trigger (1984:366) pointed out that the New Archaeology, American-style, was explicitly devoted to the exportation and standardisation of an agenda of alienation from local or national circumstance, i.e. the establishment of universal generalisations, useful in the modern day, about human behaviour – rather than the understanding of prehistory. This is, as Trigger put it, an example of “archaeology with a world mission” (1984:365), “imperialist” in its ambition. It is not hard to suppose that this attempted transcendence might apply to this topic too, especially given Bourdieu and Wacquant’s (1999:41) comment on the tendency of American social science towards “neutralization of historical context”, and observation that it is often exported, read, and adopted wholesale in ways which make little sense.

So, take this universalist ambition – whether explicit or implicit – and add it to the purported context-independence of the insights of scientific archaeology, and whether or not Feder, Cole, McKusick, etc., intended it, one ends up with arguments which can be read as applicable far outside the circumstances of their origination. This is problematic. I have yet to discover whether or not many British archaeologists rely on these books, or use them to teach, as North American archaeologists do, but certainly some of their formulations are echoed in comments I have encountered informally in the U.K.. And, as will be discussed in Chapter 8, if British archaeologists aren’t using such texts already, some may very well begin to, as the sense of “moral panic” there regarding the public understanding of science, which Steve Fuller (1997:3) described, takes hold.

This prospect is a cause for concern not only for the reasons outlined above, but also because of more undercurrents which can be inferred from these works, whether their authors intended them or not. If readers, including students, take at face value the generalisations which these archaeologists put forth, they may be left with the idea that archaeology is scientific in the often-understood sense of having privileged access to truth, and that archaeologists are therefore always right in conflicts with others. The corollary of this can be arrogance on the part of the archaeologist, the assumption that they are entitled to prejudge an ‘alternative’ topic without even engaging the material. This attitude is easily found even among professional scholars, sadly. Further, some of these generalisations can also be inferred to mean that the purveyors of alternative ideas, and those who believe them, are all wilfully being racist or otherwise evil. Some undoubtedly are. But in other cases, the error of alternative authors – if and when they are in error – can lie in incomplete

information, literary license, and in the cultural imperialist habits of thought which are still common in many western nations, reflecting an underlying idea that not just other times, but other countries and other cultures, offer but a canvas upon which they can paint their own histories. And these errors are *not* so very different from the errors that some archaeologists have made and continue to make. Further, some who write 'alternative' archaeology are mortified when they realise that this is what they are doing.

I will digress into anecdote for a moment here because I consider this latter point to be an important one, worth illustrating, and it is not addressed at any length elsewhere in this dissertation. One of the most vivid and instructive encounters of this research, for me, came by way of a Psychic Questing conference in London in November 1996. A British author, popular within that movement, spoke at the conference about his wildly successful adventures and publications concerning King Arthur, and outlined the next stage of his research, involving Arthur and Madoc in America. Afterwards, I waited for the chance to speak with him (while a woman promised him that when he finally came around to Barry Fell's way of thinking, he would feel much better). I professed my curiosity as an archaeologist, and so he gave me a copy of his book proposal. I read it later that evening with a sinking feeling, as his plan was to link Arthur to Madoc's exploration of the New World, and the old references he cited as evidence of ancient European settlement in the U.S. interior had already been liberally criticised (e.g. by Silverberg 1968) as being part of the Moundbuilder mythos, i.e. racist and often deliberately inaccurate. Although this author's intent was not to demean native North Americans, but rather to write an entertaining exploration of some interesting historical possibilities, I felt the proposal warranted some cautionary feedback.

So I wrote to him, explaining a little of the prehistory of the central U.S.A., the persistence of European colonists' disinformation about prehistoric sites, the disquieting political corollaries, etc. – topics with which I was familiar because of my Master's research on Cahokia. And I wrote about the fact that the Madoc-in-America legends were not relayed by disinterested, objective observers, but by people whose agendas and preconceptions have been obscured by time, whose accounts cannot simply be taken at face value. I concluded by asking that he consider the potential consequences of this work for living native people, because the battles that were fought at Wounded Knee and elsewhere are not over yet, land claims are still being contested, lives are being lost, and popular opinion is affected by ideas that Europeans were among this continent's earliest inhabitants. In

essence, I wrote to him to highlight the unwanted biases which he could easily import into his book if he wasn't extremely careful, to ask that he be cautious to avoid the portrayal of highly questionable evidence as straightforward historical fact, and to note that ideas such as these can have serious consequences which he might not desire. He telephoned a couple of days later, honestly chagrined at what he had almost wandered into, and genuinely grateful for the input. He said he'd never looked at it that way, never had input from an archaeologist before, never considered that this proposed book could possibly have any real-world repercussions among the living... and said that he had decided to abandon the project in favour of another one without those political implications. Thus, we were both reminded of something – he was reminded about the persistent importance of North American prehistory in the present, and I was reminded that caricatures of 'alternative' authors as close-minded, inflexibly committed to their 'heresies', and intentionally blind to the achievements of non-European cultures, are not necessarily accurate.

In sum, then, after due reflection upon the general stance of Feder, Cole, McKusick, etc., and its implications, I came to believe that adopting it as a basis for this research would be problematic for a host of reasons. Not least of these reasons was doubt about how well it reflected the situation in the U.K., for although I haven't restricted myself to British subject matter, it has been a crucial component of this research.

What do archaeologists in the U.K. really think about 'alternative' archaeology?

One of the problems with relying on articles explicitly on the subject of alternative ideas about the past, is that generally, the whole reason the authors wrote about the matter is that they find it troublesome. There is, therefore, a strongly negative bias inherent in almost any paper or book that addresses the topic specifically at any length.⁵ Further, these publications framed around the issue of 'fringe' archaeology have tended, as mentioned above, to be American in origin. It was necessary, therefore, for me to be wary of assuming that some or even any British archaeologists thought in the same way. Of course, there were many specific examples of a British archaeologist attacking a specific

⁵ There are, however, a good number of archaeologists thinking constructively about difference in other respects – such as gender and multiculturalism – some of which overlap at times with the present topic. The exploration of these ideas in relation to the kinds of alterity dealt with in this dissertation will likely be a focus of my future work.

alternative claim (e.g., Atkinson 1966; Atkinson et al. 1981; Raikes 1984; Burl and Michell 1983; Daniel 1975; Williamson and Bellamy 1983), but I could find no sustained treatments of the overall issue of 'alternative' archaeology, from the British 'orthodox' side. ^(But see Hutton 1991) The possible reasons for this lie, no doubt, in a host of factors to do with the discipline's development in the U.K., and are very interesting in themselves to contemplate. However, my point here is that this was a stumbling block for this research. My concern in this dissertation has been to begin a consideration of contested archaeological knowledge in a way that might be useful to the archaeological community, rather than as an abstract theoretical analysis (though no doubt the latter would be tidier). But for the work to be useful in that sense, I needed to know what, if anything, British archaeologists perceived the problem to be.

This led me to try a small survey project to learn more about 'orthodox' British archaeologists' opinions about 'alternative' archaeology (and vice versa). Specifically, I distributed questionnaires at an 'orthodox' archaeological conference, the Theoretical Archaeology Group meeting in Reading, December 1995, and at an 'alternative' archaeological conference, the Ley Hunter Core Moot in London in November 1995. Appendix A contains more details, including some transcribed comments from respondents.

The bias inherent in literature on 'fringe' archaeology is not completely eradicated by using a survey, for it is quite possible that many of the TAG attendees who answered the questionnaires did so because they are concerned about the topic. Nonetheless, this exercise was most instructive and helpful. It served to introduce me to the Earth Mysteries 'scene', and began invaluable discussions with its members, and started similarly productive discussions with other academic archaeologists. But further, the survey results tended to confirm several key understandings, already suggested to me through perusal of the literature and by informal conversations with British archaeologists.

First, the results indicated that beliefs and knowledge on a range of relevant topics are generally not divided up neatly into 'orthodox' and 'alternative' sectors. Generally, the variation within samples exceeded the variation between them, and distinct constellations of beliefs, reading habits, etc., were not the rule. (Appendix A and Appendix B offer some detailed commentary on this point, as do later chapters.)

However, the survey results also indicated that while some in each sample questioned the reality or the utility of the 'orthodox' vs. 'alternative' division, some British archaeologists firmly believe that it exists, and that those on the 'alternative' side are pernicious nuisances, either ill-educated or mad. That is, some echoed the opinions of some American commentators, discussed above. Further, the questionnaires returned to me by the TAG sample helped to confirm that intolerance for alternative archaeology truly exists among some of the British archaeological establishment; the 'alternative-hater' is not a rhetorical device of my own construction, a 'straw archaeologist'. Enough negative sentiment was expressed by some members of the TAG sample to convince me that the archaeologists who have actually written against alternative archaeology are not merely isolated protesters, but representatives of a larger discontentment. This was subsequently confirmed through informal discussions both with British archaeologists and with Earth Mysteries contacts. Some British archaeologists seem to believe that better proselytising is the answer to the problem, while others consider that the best solution is a simple division of territory, with 'alternative' and 'orthodox' keeping to themselves. However, their rationales for these options seem rarely securely grounded in logic. Thus, a certain incoherence – like that outlined in the previous section – resides in some of these assertions. Moreover, some display a refusal to face realities of the outside world; people outside of archaeology departments *will* unquestionably persist in reinterpreting the past in ways which clash with some archaeologists' sensibilities, and the negotiation of boundaries is necessarily an ongoing and complex process. Neither aggressively attempting to 're-educate' those others nor doggedly ignoring them is likely to produce a satisfactory result for long.

Other respondents in the TAG sample expressed regret over the historically troubled relationship of archaeology with others in the U.K. who are interested in the past, and indicated a desire for improved communication, unimpeded by disciplinary prejudices. Given a reciprocal hope on the part of many of the Moot respondents, this also seemed to be further cause for careful examination of the problems of alterity.

In short, the territory here is ripe for exploration, and this issue is real. And, as discussed elsewhere in this dissertation, its importance is not restricted to localised matters of, for example, ensuring civil conversation and constructive debate between university archaeologists and Earth Mysteries readers and writers in Britain. This work is not intended to be an admonition to all concerned to play nicely together. Rather, it is

intended to begin to illustrate, from one perspective, something of the past's importance in the present to many people, the creation and justification of archaeological knowledge, and the difficulties and challenges of accommodating alterity of all kinds within a professionalised archaeology. These issues are at the heart of some of the most significant disciplinary issues archaeology is facing today: the consequences of some postprocessual stances on archaeological interpretation; the implications of various postmodern critiques as they begin to affect intellectual endeavour on the most general of social levels throughout the West; the negotiation of multiple (multicultural, gendered, indigenous) often highly politicised understandings of the past; and making archaeology relevant to a public which is rapidly changing.

Archaeologist Mark N. Cohen once wrote that an academic discipline is “a group of scholars who have agreed not to ask certain embarrassing questions about key assumptions” (1989:viii). Such agreements are almost never actually articulated, and it appears that the participants often forget what the embarrassing questions and key assumptions even are. Part of the challenge, then, is to rediscover these assumptions, and part of the process of growth, surely, is to ask the embarrassing questions at least once in a while. This would seem essential in this case.

But how is this best approached?

Chapter 3

Where to Stand, How to See?

Doris Lessing's unpleasant novel, *Briefing for a Descent Into Hell*, deals with the madness of a man who finds himself unable to cope with the unfortunate reality that, although it is his professional and personal goal to know the truth about what happened in the past, he cannot. This, of course, is the conundrum that has faced post-positivist archaeology. How archaeologists who have taken this problem seriously (and many don't) have dealt with it is a subject of considerable interest. At first glance, there appear to be eight major responses of archaeologists to this problem of the impossibility of fulfilling archaeology's usual stated purpose, of truly knowing the past.

1. First, of course, there are those blessed with confidence that against considerable odds – perhaps by some mystical means, never actually discussed – they *do* know. Falsificationism and verification etc. are irrelevant to these lucky individuals, because they are on a special plane which permits them to count as knowledge that which most are forced to dismiss as speculation. It's not that they don't take the problem seriously, it's just that somehow, they know it doesn't apply to *them*. Everyone else smells of brimstone but they don't.

2. Second, there are those who are convinced that the history/ prehistory they write is important politically, and so they write with the intention of challenging prevailing notions about power, nationalism, ethnicity, gender politics, etc.. They put forward interpretations of their archaeological material which reflect the kind of political critique that they like to see in the present. It is the sense that they are forcing people to reconsider the nature of human society, and perhaps fostering positive change, that helps them to sleep at night. It's still pretty hot though.

3. Some go mad. Some simply stay drunk for a couple of decades. This can be comforting, although hangovers in Hell are vicious.

4. Some leave. Whether they too end up in Hell, just a different section, we don't know.

5. A few turn to esoteric forms of knowledge and of knowing, which can provide a certainty which empiricism can't. Others, however, often consider them damned for such occult manoeuvres – so effectively, they're still in Hell.

6. Some restrict themselves to what we *can* know with some degree of verifiability. Generally this involves measuring things and leaving the interpretation to others. Or it can involve a retreat to CRM, where much of the time, one has no time for synthesis or theorising, only for saving sites from oncoming earth-moving equipment and cataloguing what's been recovered. Hell in this case lies in the paperwork.

7. Some try to get beyond epistemology and issues of evidential adequacy etc. by saying that archaeology is merely one way of knowing the past. (There are a number of variants on this, from a generalised not-very-theoretical relativism to a quite-rarefied radical constructivism.) In short, what this does is remove the yardstick against which archaeology was found to be wanting, and replace it with new criteria for theory evaluation (internal coherence, etc.), according to which, archaeology looks pretty good. This permits them to continue doing exactly the kind of archaeology that they were doing before, because they have just as much right to their own way of knowing the past as anyone else does. So, they're in Hell, but they figure that's OK, because everyone else is too, so it's as though nobody's in Hell. Hell is relative.

8. Some retreat, via the route of number 7, to the study of archaeology's disciplinary dynamics, history, or place in modern society amongst other ways of knowing the past. This works for some because it allows them to continue their institutional affiliation with archaeology, and to work in general contact with it – perhaps even sustaining the belief that they are doing it some good – while not actually dealing with the central problem. This is a little bit like being in a special air-conditioned side chamber in Hell, halfway between the room for archaeologists and the room reserved for middle managers.

But, as air-conditioning is wont to do, it breaks down some days, and even when it is at peak performance, the heat has a way of seeping in under the door. Further,

interdisciplinary linkages are one of the built-in features of Room #8, and can act like conduits which periodically channel sulphurous blasts of their own into it.

And so, where many dissertations in archaeology have a methodology chapter designed to lower the thermometer reading a few degrees by providing some measure of certainty and control over interpretation, this one does not. This one has an essentially anti-methodology chapter explaining how some different methods of studying archaeology's disciplinary dynamics, history, and place in modern society do not necessarily provide straightforward assistance in the search for both a cool place to stand, and a view unencumbered by smoke, fumes, or tricks of the light. Such a chapter is, like much of this dissertation, necessarily quite personal in orientation, but I include it here as a record of some preliminary efforts to work across disciplinary lines.

The actual research activities I have undertaken during this project include: archival research; survey research; participant observation in meetings, field trips, and fieldwork; interviews; and textual analysis. But because I have been chiefly interested in a 'real-world' problem rather than an intellectual problem, and because each time I tried to navigate within a set of disciplinary constraints, there was an unacceptable compromise, this dissertation has perhaps ended up being more anti-disciplinary than inter-disciplinary. Below, I address some of the reasons why, beginning with a quick look at the past range of approaches to 'fringe' science, moving to a slightly more detailed assessment of the philosophy of science and science studies as they might be applied to archaeology, and closing with some observations on previous work devoted to the scrutiny of archaeology.¹

¹ A note on some abbreviations used henceforth in this dissertation:

Like most, I use 'HPS' as an abbreviation for 'History and Philosophy of Science'. I follow David Hess (1997) and others in using 'STS' as an abbreviation for 'Science and Technology Studies', which generally may be read as encompassing sociology, psychology, anthropology, etc., of science, as well as some elements from HPS. And in this context, I use 'science' more in the general sense of 'systematic inquiry' than in any particular sense involving hypothesis-testing research programmes or the like – that is, in a general sense which is broad enough to include archaeology in all its forms, without diminishing its interpretive dimensions.

Others' approaches to the fringes of science

At the start of this research, it seemed that an obvious place to look was to previous treatments of heterodox science. Much has been written on such subjects; my aim here is only to give examples, at the coarsest level, of different kinds of analysis.

Philosophers of science have written often on the problem of demarcating science from pseudoscience (*e.g.*, Grim 1982), though most ultimate conclusions on this matter have been equivocal (Van Pool and Van Pool 1999). Scientists with an interest in philosophy and history of science also confront this issue, but with somewhat less sophistication and somewhat more 'common sense' (*e.g.*, Wolpert 1992).

Others devote themselves to the debunking of claims made by the fringe, much as have the archaeologists mentioned in Chapter 2. CSICOP Fellow Martin Gardner (1983, 1988), for example, has spent a lifetime writing about topics involving pseudoscience, the fringe, and the New Age movement. He gives historical analyses of his subjects, and distinguishes between different flavours of the fringe, (from cranks to idiots to religious fanatics to lunatics, charlatans to maverick scientists, to those who hold only one eccentric conviction). Notably, Gardner does not stop short of picking on some areas of mainstream science that he finds questionable, such as some work on black holes and superstring theory, or ape communication skills. James Randi (*e.g.*, 1991) is a more dogmatic and less sensitive CSICOP crusader, while Carl Sagan also did his tour of debunking duty, for example on the Velikovsky issue (*e.g.*, see Sagan 1979), using an approach much like those of archaeologists attacking von Däniken – that is, simply taking apart his claims one by one.

Others, working within STS (see below), approach the fringe of science from a purely historical perspective, relating what happened in a given controversy, but not passing judgement. Collins and Pinch (1993) exemplify this approach, although they consider instances of controversial orthodox science, rather than fringe science (*cf.* Collins and Pinch 1982), such as the chemical transfer of memory (in planarian worms) and cold fusion, as well as other older examples including relativity. They describe the sequence of events, the work done, the articles published, and the disputes, but withhold normative comment.

Some others also working ostensibly within sociology or anthropology are not so even-handed; Vogt and Hyman, for example, begin and conclude their work, *Water Witching USA*

(1959) with the assumption that dowsing does not work and that those who believe it does are either superstitious or stupid. They seem concerned primarily with the question of why apparently ordinary people could be this way.

Finally, no description of approaches to the fringe would be complete without a mention of Richard De Mille (1976), whose gently good-humoured demonstration that Carlos Castaneda was not what he seemed to be, manages to expose without condemnation, and reveal some facts without completely devaluing any insights Castaneda may have imparted to his readers.

There is an abundance of models to choose from; yet, none seemed quite right for the topic at hand. I have already discussed the shortcomings I perceived in a 'debunking' approach, but some observations follow on the particular approaches of the philosophy of science, and science studies.

Philosophy of Science

Archaeologists have often looked to the history and philosophy of science in their quest to better understand the nature of archaeological reasoning, to formulate ways of making archaeological interpretation more robust, and sometimes, for legitimation. It is certainly arguable that the results of these engagements have been mixed at best, as they have too frequently led to prolonged, confused and needless conflict within the discipline, even though constructive debate and improvement of practice was the original intent. For example, in her discussion of "Philosophy and the new archaeology", Alison Wylie makes it clear that new archaeologists failed to realise that positivism – to which they appealed as a basis for better archaeological research – is in fact a variety of the empiricism which they were attempting to discard, and that the resulting inconsistent framework left a "legacy of incoherence" (1989a:20). Any Western archaeologist of the late twentieth century will know something of what this has meant for the structure of archaeological discussions, both within and between the processual and post-processual 'camps'.

One problem in past uses of philosophy of science within archaeology is that they have involved 'top-down' abstraction, i.e. lists of categorical, context-oblivious imperatives about what Philosophy X says we must do in order to be rational. Too often in the past, this approach has ignored quite thoroughly many of the relevant aspects of what

archaeologists actually do, and the constraints they actually work with, and worse, has introduced new unnecessary problems while failing to resolve old, real problems (Denning 1994).

However, philosophy of science can also permit us to analyse from 'the ground up', i.e. describe how people actually do think, or have thought. One can dissect assumptions, document how specific arguments move from premise to conclusion, describe leaps of inference, etc. The catch is that this is not necessarily useful as a comparative method; or rather, it is only as useful as the cases selected. In the case of this research, I did consider engaging in highly detailed, point-by-point analyses of texts in 'orthodox' and 'alternative' archaeology, to examine the reasoning processes at work. This would have had the practical advantage of being text-intensive research, rather than people-intensive research, and could have been tightly controlled and neat around the edges. However, two things at least convinced me that this wasn't a good idea. First, the differences in the context of production, traditions of rhetoric, and the context of reception of such texts can make comparisons unfair. Second, I could not see how this would be anything other than an argument to desired conclusions, because detailed analysis would require that only a few cases be examined, and the cases which I chose would totally determine the outcome. I could have successfully argued, through the judicious selection of examples, that Earth Mysteries research is far more rational than academic archaeological research, or vice versa, or that they're the same. It might have been a quite satisfying rhetorical exercise... but in the end, I was not convinced that detailed philosophical analysis was the most appropriate approach to the matter at hand.

Science and Technology Studies

Upon commencing this research, I predictably became interested not just in how to analyse archaeological knowledge claims, but in the functioning of archaeology as a discipline and its situation within society. I was introduced to the classic science studies works of Latour and Woolgar, and then found Collins, Shapin, Knorr-Cetina, Traweek, and others, and read voraciously. (Hess 1997 provides an overview of these and other authors.) The material was tremendously thought-provoking, and fed my longstanding curiosity about both the history and the present workings of science in society. I enjoyed reading case studies on everything from quarks to parapsychology, and have since found that the science studies

side of the 'Science Wars' is often absorbing. [See the many responses to the initial 'anti-anti-science' provocations of Gross and Levitt (1994) and Alan Sokal (1996a) and (1996b); Fujimura (1998) is just one example of hundreds.] Indeed, as mentioned earlier, I now consider these disputes about how to see science and knowledge to be a crucial backdrop to the present work.

But after months of reading and consideration in the early stages of this research, I had to conclude that these were not appropriate models for my own inquiry, for at least two reasons. First, as should perhaps have been more immediately obvious, both my location and my experience were simply incommensurate with the task of disengaged observation of archaeologists in the U.K. Second, as a Ph.D. student in a department of archaeology, funded to carry out archaeological research, I considered myself obliged to make my work directly relevant to archaeology itself. And these works in STS were, more often than not, primarily contributions to discussions in STS, rather than to the science studied, say biology or physics. They are externalist not only in perspective *but also in intent*. This is not to say that they are useless to the discipline under scrutiny, because of course it is always useful for an individual practitioner to read analyses of their own discipline's inner workings. But it is to say that the provision of information with which to improve practice in the discipline observed has not often been the agenda of research in science studies. For instance, Traweek's (1988) ethnography of high energy physicists, *Beamtimes and Lifetimes*, is just that: an ethnography. It is not intended as critique or as an exercise in critical historiography or in *applied* anthropology, to help optimise or smooth practices within the physics research communities that surround particle accelerators. To take one example, Traweek's descriptions of the friction between experimentalists and theorists within high energy physics are broadly accurate, according to a particle physicist I know, but not of any particular help to the people described, as they continue to go about their business of practicing physics. Similar is an article in the *International Journal of Comparative Sociology* (Stebbins 1980) describing "the amateur routine in archaeology", which, although perceptive, and evidently interesting enough to a sociologist, is of quite astonishing irrelevance to any practicing archaeologist, amateur or otherwise. We know already that rocky soil is hard to sift, that discovering a projectile point "may even evoke a whoop from the finder", and that sometimes "lumps of dirt can look significant" (1980:42-43). This is not necessarily a flaw in Stebbins' or Traweek's work, but a facet of its orientation.

And so, STS seemed not to provide what I sought – that is, a model for inquiry which allowed the critical investigation of my own discipline, but which also fed back into it in a positive way.² The orientation didn't seem right, the observational distance required was too great, and my location as a student researcher was too complex.

But what, then, about using the approach of STS to look at Earth Mysteries communities? Some of the same issues were problems. First, there were the practical problems of inadequate preparation for such a task, insufficient time to develop background knowledge of those communities and make practical arrangements to study them, and all the ethical questions attendant to such research involving other communities. Second, part of what makes the approach of STS sensible at all is that its focus has been on loci of considerable power and public expenditure in our society; thus, its critiques are to a certain extent inherently justified, and the same justifications could not possibly apply to a small and somewhat marginalised group of people who consume few if any public resources. The third objection is broader, and counters not just an STS orientation, but *any* anthropological approach to studying 'alternative' archaeology communities; that problem is the certain alienation inherent to such a project. The observer must look at the observed as Other, and this was the very stance that I was struggling to avoid having as the grounding principle of this research.

Bernard McGrane is devastating on this point:

Anthropology has been an extremely subtle and spiritual kind of cognitive imperialism, a power-based monologue, a monologue *about* alien cultures rather than, and in active avoidance of, a dialogue with them... Anthropology is interested in the Other and at the same time remains altogether alien to the Other; in the best of cases anthropology speaks well *of* the Other, but with very few exceptions anthropology does not speak *to* the Other and it is as Todorov says only by speaking *to* the Other – not giving him orders but engaging in dialogue – that I can acknowledge him as subject, comparable to what I am myself. ... Anthropological 'scientific method' is the decay of dialogue, the sustained, cultivated, and epistemologically enforced atrophy of dialogue... (McGrane 1989:127)

² This has started to change since I began this research; as mentioned, the eruption of the Science Wars has altered the terrain a great deal, pushing scientists and their observers into a new kind of dialogue, often hostile, but sometimes with a view to new shared agendas. Chapter 7 discusses Steve Fuller's work, which can be seen as a positive development in this context.

He continues:

Anthropology never *listened* to the voices of 'alien cultures', it never *learned* from them, rather it studied them; in fact studying them, making sense out of them, making a 'science' about them, has been the modern method of *not* listening, of avoiding listening, to them. The Other's empirical presence as the field and subject matter of anthropological discourse is grounded upon his theoretical absence as interlocutor, as dialogic colleague, as audience.
(McGrane 1989:127-8)

I do not believe that McGrane's assertion holds for all anthropology, but I do believe that he has very precisely identified the bias that its historical trajectory has preserved. And this bias, this structure of inquiry, did not – and does not – feel right for this research, even though the explicit use of anthropological concepts (e.g. of the creation of social knowledge) could have provided an intellectually elegant account of the world of Earth Mysteries.

Previous work scrutinising archaeology: some risks

Meltzer has commented that “for the majority of archaeologists, the history of archaeology is what one does in those brief sections of dissertations, reports, or monographs labelled, as Flannery put it, “Previous (Bad) Work in the Region” (1989:10). However, from a time when the chief writings about archaeology were histories of excavations and individuals, the scrutiny of archaeology has expanded to include histories of archaeological ideas and movements, philosophy of archaeology, analyses of the socio-political contexts (past and present) of archaeological practise, and the literary analysis of archaeological texts, as well as the anthropology and sociology of archaeology. (For an overview of sorts see Wylie and Pinsky 1989).

These analyses of archaeology can be divided into “internal” and “external” varieties. Meltzer, drawing from Kuhn, describes this distinction as seen in the history of science:

internal history is predominantly concerned with the development of scientific ideas by practitioners; understanding how participants made sense of their science; determining the catalysts for change... and understanding how those changes were integrated into the discipline. This, of course, contrasts with external history, which concentrates on the interface between the sciences as a whole and the society in which they were practiced as well as on the social, economic, industrial or governmental forces [which affected the discipline]... The dichotomy between internal and external history, while primarily methodological, frequently divides

along disciplinary lines. Internal accounts are generally written by practicing or trained scientists; external accounts by historians or sociologists. (Meltzer 1989:17)

In part, the recent flourishing of internal studies of archaeology has taken place because the New Archaeology provided so much fodder for critique and analysis, and because concerns about sociopolitical context, from gender issues to aboriginal rights, have become so timely. But it seems possible that there is more to it, and that this proliferation of ways of looking at archaeology is the beginning of something new. Not only is increasing sophistication evident in the scrutiny of archaeology, but external approaches seem to be more frequent. These external analyses can be said to come in two flavours: first, those which adopt sociological/historical methods and perspectives, and second, those with external rather than internal analytical goals.

One example of the use of sociological/historical method is provided by Zubrow's 1980 article, "International Trends in Theoretical Archaeology", which uses citation analysis to chart academic movements within archaeology. However, Zubrow was not a disinterested sociologist concerned only with creating models of paradigm change *etc.*; rather, he was a missionary for the New Archaeology, checking up on the progress of the flock. In contrast, there are many who offer critique (whether explicit or implicit) of the status quo within archaeology by using elements of sociological or literary analytical perspectives and techniques, *e.g.*, Tilley (1993) and Flannery (1982). These papers by Zubrow, Flannery, and Tilley are all cases of self-reflexive work by archaeologists, using elements of external analysis, but still with the goal of contributing to archaeology in some way.

But now, it seems, a much more external kind of study of archaeology is emerging, which has a different character, born of different analytical goals. Sometimes written by archaeologists, but often by those with dual allegiances to archaeology and another discipline, or by philosophers or sociologists, this kind of work, while about archaeology (the discipline), does not necessarily contribute to archaeology (the subject), or claim to, except rather tangentially. In particular, these works tend to focus on the social context of archaeological practice rather than the practice itself – and thus are much more in keeping with external history than internal history, as described by Meltzer above.

Examples include Kenneth Hudson's *A Social History of Archaeology: The British Experience* (1981); though it is *about* archaeology, it is probably exquisitely irrelevant to any

archaeologist without a fetish for trivia about Victorian experts on monumental brasses. Embree's (1989) research into the structure of American theoretical archaeology is also not explicitly concerned with contributing to archaeology, though archaeology is its subject. Similarly, Chapman (1989:151), in an article on the institutional history of British archaeology, made his position crystal clear by stating: "I am less interested in what we might consider the growth and development of the field – the permanent contributions to knowledge – than I am in the preoccupations of British archaeologists in the 1860s." At TAG '95, there was an entire session on *The Organisation of Archaeology*, including papers by Sue Thomas on the expression of disciplinary organisation through archaeological writings, Stephanie Moser on the disciplinary culture of Australian archaeology, and Koji Mizoguchi on the constitution and reproduction of Japanese archaeological discourse.

This proliferation of essentially externalist analyses of archaeology prompts a question: just as in the last few decades there has developed 'science studies', are we seeing the birth of 'archaeology studies'? (*i.e.*, work related to the discipline of archaeology which, while having other merits, does not, and perhaps even cannot, impact upon the practise of archaeology?)

In turn, this leads to another question: why do people scrutinise archaeology? As the purposes of different sorts of scrutiny of archaeology vary, so do their justifications. It would seem wise to periodically reevaluate these justifications, ask what different kinds of analyses of archaeology contribute to the field, and consider whether we should pay more, or less, attention to them.

There are standard justifications for internal critiques of archaeology. Moser sums them up:

First, the authors call for the development of a research tradition which is based on the premise that it is necessary to critically examine all aspects of archaeology's past and current identity. Second, they argue that such a tradition be part of, and integral to, mainstream archaeological research. The other feature characteristic of this research, and perhaps the most significant, is that the researchers emphasise the need for sustained work on the social context of archaeology. (Moser 1995:3-4)

But Moser goes on to comment that little has actually been done in the area of archaeology's social context, and that we still have little idea what it really means. As well as wondering about this, we might also wonder if the many studies of this kind have actually affected archaeological practise much. Similarly, we might ask if studies of the philosophy of

archaeology have affected archaeological practise; does knowing that the hypothetico-deductive method is in fact ultimately inductive actually affect the way one thinks? Arguably not, although most archaeologists would probably say that they're happy that issues of politics and subjectivity, *etc.*, have been brought explicitly into archaeological discourse, and that they've benefited from heightened awareness of these issues.

So, despite the statements made about the importance of internal, critical, reflective works on the history, philosophy, and social context of archaeology, the exact reasons for this importance remain unclear. Likewise, much of the time, archaeologists' justifications for studying the history of their discipline (*i.e.*, as a part internal, part external enterprise) are correspondingly vague. For example, the session abstract for *The Organisation of Archaeology* (TAG '95), by organiser John Carman, reads:

This is not a session about how to set up a field unit, a local authority archaeology department or the internal workings of English Heritage. It will, however, be of interest to those who do these things and who work in such organisations. It is a session about the nature of contemporary archaeology, archaeology as a contemporary practice and archaeology as something created by but also instrumental in the creation of contemporary society. It is therefore of interest to all archaeologists. (Carman 1995)

Why, exactly? Carman seems to think that these assertions are obvious or self-explanatory, but they are not. Pinsky is a bit more precise:

...there is an important place – in fact a pressing need – for a critical historiography of the discipline that articulates the past directly with both contemporary and future philosophical and theoretical interests. In so far as these interests are to gain a deeper understanding of the nature of the archaeological enterprise as a vehicle for its continued growth and reformulation, the historiography of archaeology should be viewed as an integral and vital component of... 'internal analysis' of the discipline. (in Wylie and Pinsky 1989:88)

Christenson goes further still. In *Tracing Archaeology's Past* (1989), he estimates that the number of books published on the history of archaeology has doubled every ten years since the beginning of the century, and justifies this output by referring to Schiffer's (1987) statement that "the behaviour of the archaeologist is the greatest source of variability in the archaeological record." Christenson further argues that "many deeply held beliefs about the archaeological record and about the past are based upon shaky historical foundations ... careful examination of how such ideas were passed from generation to generation may help archaeologists be more critical of such accepted interpretations" (1989:3).

Meltzer concurs with the general orientation of Pinsky and Christenson but also asks: “must the history of archaeology be relevant at all?” His answer is, “Not necessarily. Archaeologists study the history of archaeology for the same reason they study archaeology: because it is there. Equally true, studying the history of archaeology is really nothing more than studying the anthropology of archaeology, and as such, it should require no further justification for a group who are, at least nominally, anthropologists” (Meltzer 1989:10).

If we leave aside the question of justifying anthropology itself, this leads to the question of what to do about the heavily external analyses that might constitute a field of ‘archaeology studies’. If this really is a nascent discipline, what should archaeologists be hoping for from it? To return to the example of the high-energy physicists described in Traweek’s *Beamtimes and Lifetimes* (1988), it is hard to imagine these physicists learning much that they consider important from her work. They are no doubt already well aware that most crucial scientific information is exchanged along informal networks in the cafeteria rather than through publications, that the way one dresses affects how one is regarded, that the discipline is male-dominated, and that there is a long-standing schism between experimentalists and theoreticians. (If they hadn’t realised these things, they couldn’t succeed as part of the research community.) In contrast, they may not be aware of some of the narratives implicit in their work, or that their behaviour mirrors that seen in different cultures (*e.g.*, theorists and experimentalists stereotyping each other and practicing ritual avoidance) – but would becoming cognisant of these anthropological insights help them to do physics? Might such cognisance even impede them? We might consider again the opinions of Brush (1974), who questions the wisdom of exposing science students to works on the history of science which are ‘subversive’ or undermine science’s traditional image.

So, such studies of archaeology could be worse than irrelevant. If, once again, we look to what has happened in science studies, greater concerns emerge. Some scientists feel that recent critiques proffered by science studies are not only irrelevant to the practise of science, but are contributing to an atmosphere that is seriously obstructing scientific progress. Gross and Levitt (1994), began by offering a lengthy indictment of members of the ‘academic left’, including respected scholars in science studies such as Bruno Latour, who they feel are actively misrepresenting scientific activity for reasons which are simply born of political ideology. Might this extreme antagonism between the observers (science studies) and the observed (scientists) – perhaps already heralded by some archaeologists’ (*e.g.*, Trigger 1995)

denunciation of certain post-processualist internal critiques – be what archaeology has to look forward to, as it continues to fragment? The prognosis may not be good.

So, there are risks inherent in any close scrutiny of archaeology's workings, and some might deem the possible compromise of disciplinary integrity worrisome enough that clear justifications for such close scrutiny should be demanded. Perhaps history for its own sake is not enough, and perhaps the standard, often fuzzy reasons for analysing disciplinary organisation are also not enough. You, dear reader, know by now what my justification for the present work is, and must judge for yourself whether or not it is enough.

Still pretty warm in here...

In a sense, as I have said before in this dissertation, the search for a place to stand is itself the project, but it is a project which must always be ongoing, for the ground is never stable and the air-conditioning is rather variable. I confess to a lingering disappointment, for – like Feyerabend's mortified rationalist, and Rorty's foundationalist (see Chapter 7), and like the archaeological debunkers mentioned in Chapter 2 – I had hoped there would be something transcontextual, something secure, something invariant, something non-negotiable, something unquestionably sound, and there is not. However, I see this negative awareness as positive knowledge, hard-won but very useful, and the process of investigation to be quite like others one encounters in the study of archaeology and related subjects: painstaking excavation of completely sterile units to establish a site's perimeter; a careful search through historical archives for information that in the end, simply is not there; consultation with senior colleagues leading to the realisation that sometimes an old dental X-ray just does not have enough resolution for accurate age estimation; the discovery that a just-developed method of isotope analysis could have avoided the destruction, last week, of rare samples through an older method; or repeated experimentation leading to the conclusion that replication in ancient DNA studies is still so hard that all results must be seen as provisional. Etc. This kind of knowledge is, in short, part of the research process, and my commitment to the authentic representation of inquiry has required that it be reported upon with transparency here.

So, I began by looking at ways of studying disciplinary dynamics, research communities, etc., hoping for a concrete model to follow, a structure to impose, some rigidity to hold the

project together. But the only answer, in the end, was to let the material – the literature, the surveys, the conferences, the informal discussions, the observations of other people – take the lead and to feel my way along without much theoretical guidance beyond the desire to be as honest and fair in my interactions and assessments as possible. The results can be seen in Chapters 4, 5, and 6. Each combines elements of all the approaches discussed above, but has no allegiance to any in particular. I do not think there is a deliberate methodology that binds them together, although there are some lines of continuity, chief among them a refusal to take previous assumptions about ‘orthodox’ and ‘alternative’ archaeology as givens. In turn, Chapter 7 deals with escaping the need for straightforward foundations to knowledge – a need which, after all, is the common source both for the kinds of assumptions I have sought to avoid, and for the methodological difficulties recounted above.

Before turning away from matters of seeing to slightly more concrete topics, however, I must add one more personal observation, and a plea.

I know by now to expect some puzzlement on social occasions when someone asks about my profession. If I say I am an archaeologist, the question is invariably “So where do you dig?” This is not a problem, as it often leads to a good conversation about all the different ways in which the past matters to people, and I can learn about what my questioner thinks about human history. What *is* a problem, I believe, is the odd occasion when an archaeologist says to me, of my work, “But that’s not archaeology.”

The power of names is one thing that I began to understand through this research. Names are not merely labels of convenience, inconsequential beyond their facilitation of conversation. This much is obvious when dealing with the spectrum of ideas about humanity’s past – i.e., the choice to call something ‘cult’ archaeology is very different from the choice to call it ‘lunatic fringe’ or ‘alternative’, and the choice to label it as a category at all strongly shapes subsequent discussion. But this power of names applies, too, to research such as this, about the way that archaeology works. I now believe that we should resist the temptation to push these kinds of inquiry about archaeology into other fields and departments, or to subdivide the discipline, into ‘history of archaeology’ or ‘sociology of archaeology’, or indeed, ‘metaarchaeology’. Maggie Ronayne (1997) convinced me of this when she argued that naming something can be a very powerful way of setting it aside and diminishing its influence. For example, ‘gender archaeology’ has

become at least partly normalised within archaeology as a discrete subcategory of the discipline, neutralising somewhat the power and politics of a feminist approach. Of her own work, which in part involved engaging people at Irish archaeological sites in discussions about their perceptions of the past, Ronayne says, “it could be and will be named in many ways but for me any possible use it might have is probably best helped by rather perversely calling it archaeology!”

I agree. Archaeology is big enough to include all this, and more.

Chapter 4

On Unseen Forces and Imaginary Boundaries

In January 1996, I (and all the other members of the Arch-L email discussion list) received a message from a lecturer in archaeology at an American university, forwarding a strongly anti-dowsing article from James Randi. The message read:

I offer this FYI because I have found that many of the amateur archaeologists I deal with believe in the power of dowsing. Part of this belief is reinforced by the numerous articles and advertisements found in various treasure hunting and 'artifact collector' magazines. (PS – in case you don't know, James Randi is a world renowned magician and debunker of psychic and paranormal activity and consumer fraud. He has published numerous books on the subject.)¹

The Randi article being forwarded, entitled "A Travesty of Science", was a response to a pro-dowsing article which had just been published in the Smithsonian's magazine. Randi's article reads in part:

The author of this trash writes, "The relationship between dowsing and established science has always been distant, mutually suspicious." Nonsense! There is NO RELATIONSHIP! Science is logic, rationality, careful investigation, and experimentation that works: dowsing is wishful thinking, superstition and mythology that doesn't work. Was there any mention in this Smithsonian magazine article of the numerous carefully-controlled tests of dowsing that have been done, tests that showed it was totally without merit? No, in the pages of this prestigious SCIENCE magazine, only anecdotal experiences are given, and blatantly unproven 'theories' and 'facts' from wide-eyed dummies who don't know logic from lingerie.²

The reader will gather not only that Randi disbelieves the claims of dowzers, but that he feels very strongly on the matter. The article continues with an exhortation for readers to express their outrage to the editor of the Smithsonian magazine, and a specific reiteration of Randi's longstanding offer of over \$500 000 to any one who can prove dowsing's

¹ Email posting from Stephen Nawrocki, to the ARCH-L list <ARCH-L@TAMVM1.TAMU.EDU>, on Thursday January 18, 1996.

² As forwarded in email posting from Stephen Nawrocki, to the ARCH-L list <ARCH-L@TAMVM1.TAMU.EDU>, on Thursday January 18, 1996.

efficacy in Randi's controlled test.³ So, what we have here is a vigorous and vocal anti-dowsing contingent outside archaeology, whose views were being relayed within an archaeological discussion, on the basis that archaeologists should know of this anti-dowsing position, because many amateur archaeologists believe in dowsing.

There are several different threads in the comment above, which need some examination: the conflict between those who claim that dowsing works and those who claim it doesn't; the infiltration into archaeology of heated disputes with their origin in the defense of science and the public interest against pseudoscience and psychic claims; the idea that belief in dowsing is prevalent in amateur archaeologists and the implied corollary, that it's not prevalent in professionals. Inquiry into these and related issues is the purpose of this chapter.

Dowsing is, I think, a good place to begin with the dissection of the purported 'orthodox vs. alternative' divide discussed previously in this dissertation. Dowsing is useful as a case study because it is a method, not a theory, and thus has the content and significance which people ascribe to it, rather than carrying an intended meaning within it. It is also compelling because despite the comparatively innocuous nature of the practice – *in itself*, dowsing is noninvasive and thus not damaging to archaeological sites, and costs very little – the rhetoric which surrounds it can be vehement, which as usual suggests that more than the proper use of metal coat hangers is perceived to be at stake. Further, it illustrates that archaeologists are certainly subject to cognitive biases, sometimes knowingly and happily so, in their methods of understanding archaeological sites, which can firmly place their activities outside the realm of the stereotypically 'objective'. Dowsing is also a good case study because its coverage in print with respect to archaeology is increasing, probably due ultimately to Randi and cohort as well as to the start of systematic testing in archaeological contexts, and so the debate is likely to get more, rather than less, interesting in the near future. And finally, the case of dowsing demonstrates the necessity of using informal channels – surveys, conversation, and the Web – to study prevalent opinions among archaeologists; relying upon formal publications alone is, in this case, bound to result in a

³ Randi is one of the most vocal members of CSICOP, the Committee for the Scientific Investigation of Claims of the Paranormal, about whom more shall be said in Chapter 8. Debunking dowsing has been one of the organization's favourite projects, to judge from the many articles on it which appear in the Committee's widely read journal, *The Skeptical Inquirer*.

very skewed perception of what archaeologists really think, and can thus provide erroneous support for the idea that dowsing is a ‘fringe’ practice, espoused by only a few.

Below I will address, in turn, the background to the current dowsing debate, conflicting attitudes towards dowsing in archaeological practice and in archaeological publications, dowsing in ‘alternative’ archaeology, and the continuum from dowsing into psychic practices used in archaeology by professional archaeologists and by others. Hopefully this discussion will make clear that dowsing in archaeology is not *actually* a marginalised or ‘fringe’ practice, despite some prevalent assumptions to the contrary, and that it is a live question as to whether or not it *should* be, despite the straightforwardly negative opinions of some archaeologists (*e.g.*, Feder, Van Leusen, Aston, Mouer, Jeske, and Gaffney *et al.*, below). I also hope the reader will discern that there are subtexts old and new to the debate, which seem to have precious little to do with the realities of archaeological practice, but everything to do with *a priori* convictions about the world and how it can and should be known. As for psychic archaeology, this too may be best understood not as an aberration, but as an outgrowth of a genuine difference of opinion – longstanding within the Western tradition – over what constitutes knowledge.

Dowsing: A Long-Running Debate⁴

Dowsing as we know it goes back at least to the sixteenth century, for Georgius Agricola’s 1540 *De re Metallica* provides a description of the dowsing rod. It may, however, be a good deal older (Hansen 1982). Instruments are diverse: the plastic Y-rod, forked twig, paired L-rods, and the pendulum are currently most common (though one occasionally hears of exotic instruments like German sausages). So is the terminology: ‘water witching’, ‘divining’, ‘rhabdomancy’, and ‘radiesthesia’ are, broadly, synonyms for

⁴ The reader is bound to be curious, and so I should specify my own position here. I do not have a personal belief about dowsing’s efficacy, but am firmly agnostic on this point. I am commenting here *only* on the structure of the debate as it pertains to archaeology. I have observed people dowsing on archaeological sites, but not in a test situation. I have tried it myself briefly on archaeological sites on two different occasions, with a nylon V-rod and with metal L-rods, to get a sense of what the instruments feel like in the hands. I have not, however, been involved in any kind of dowsing trial. Nor have I evaluated all of the abundant statistical and related arguments concerning the proper testing of dowsing’s efficacy. These are simply outside the remit of this study.

dowsing. The uses of dowsing include: locating water, metal ores, archaeological features, underground pipes, lost objects; healing; and answering questions about the past and future. My initial comments here pertain to conventional dowsing with a twig, a Y-rod or L-rods, in search of physical things such as water, metal, or archaeological features.

Three general types of explanations have been forwarded by those who believe that dowsing works: first, that it is the result of actual physical forces acting on the instrument; second, that it derives from the human response to subtle environmental clues or variations in electromagnetic fields; third, that it is a psychic phenomenon. The latter theory detracts from dowsing's general credibility among many. Also compounding the debate is the fact that dowsing is associated with many 'alternative' areas, from parapsychology to Earth Mysteries to cerealogy (the study of crop circles). These associations tend to cloud discussion, and also to obstruct orthodox researchers who wish to investigate the physical aspect of the subject impartially.

But it is certainly not the case, as Randi claimed above, that "There IS NO RELATIONSHIP!" between established science and dowsing, however much he might wish this to be true. His stance is at one extreme of the 'dowsing discourse', probably paralleled only by Vogt and Hyman (1979) in its negativity. Many physical scientists, especially in Europe, have investigated the subject and found it worthy of attention, and continue to do so. Recently, emeritus professor of astronomy Vincent Reddish (1995) and emeritus professor of physical electronics and radio astronomy Roger Jennison (1995) have both reported their experiments in dowsing with various detectors, and concluded that there is a real physical phenomenon at work. Of the longer trajectory of research into dowsing, Williamson (1993) provides a good overview, as does Reddish (1993), and Hansen (1982). These works generally indicate that for every set of "carefully controlled tests of dowsing that have been done, tests that showed it was totally without merit", in Randi's words, there seems to be a set of tests which offer some support for the phenomenon's veracity. Sometimes the same set of results lends itself to quite different conclusions. Contending, for example, that in one case, Randi's statistical analysis was "completely illegitimate" and "illustrates dramatically the need for a trained scientist to design and oversee such experiments as opposed to flamboyant, ill-informed conjurers", Charles Osborne recalculated experimental trial statistics given by Randi as proof that dowsing for water doesn't work, and concluded that in fact, Randi's experiment proved that dowsing *does* work (Osborne 1982:38). Osborne continues: "In view of James Randi's

apparent inability to recognize significant results we are forced to wonder how many previous experiments yielded positive results without Randi recognizing them as such" (1982:39). Randi, in turn, offered a spirited rebuttal (1982), and so the debate continued, as it does to the present day.

Anyone who reads enough of these reports will discern that there has been considerable miscommunication between camps about what precisely is being tested, what precisely the claims being made are, and considerable disagreement about appropriate testing procedures and methods of statistical analysis. It is initially rather surprising that people of honest intent have been absolutely unable to agree on a resolution to what would appear a simple problem. But then it also becomes evident that this is no minor dispute over methodology, but is imbued with and animated by ideological differences over much larger issues, concerning the nature of the world around us, how we can know it, the appropriate limits of scientific investigation, what should be done when those limits are exceeded, etc.⁵ (This is a theme to which I shall return, for it is at the heart of not just the dowsing discourse, but others central to this dissertation.) Certainly, it is these differences which give the dowsing debates their force and emotional content; why else could it possibly matter so much that some people like to engage in the benign practice of waving twigs or metal rods around?⁶

And indeed, these sorts of disputes go back a long, long way. Kloosterman (1985) relates that the U.S. Geological Survey began engaging in printed debates about the efficacy of dowsing in 1917, and considers it an irony that their original stance – that it should be avoided – was the same as that held by the Christian Church. One might be excused for supposing that this is no mere coincidence, but regardless, the history of Church opposition to dowsing is long indeed. Martin Luther, for example, claimed dowsing to be the work of the devil (Hansen 1982), and at least one famous seventeenth-century water dowser died in prison because Cardinal Richelieu did not appreciate her activities (Williamson 1993:33). Eighteenth century artwork sometimes depicted dowsers as horned, tailed, cloven-hoofed

⁵ I suspect that in the background, there may also be disputes relating to distinct scientific traditions in different countries, *i.e.* Germany vs. France vs. the U.K. vs. the U.S.A., but haven't surveyed the extensive continental literature enough to confirm this.

⁶ The CSICOP anti-dowsers (e.g. Randi, Feder) would contend that it is important for reasons of consumer protection against fraud, but I read this as a practical justification for their essentially ideological stance against what they see as irrationality in society.

demons with human masks, being revealed by gowned priests. This is apparently not yet a dead issue; for example, in the *Christian Research Journal*, Weldon (1992) argues vociferously that dowsing works through occult forces and should therefore not be practiced – even citing James Randi. They are unlikely allies, perhaps, and ultimately Weldon’s argument is highly idiosyncratic... but nonetheless, it is clear that some Christian writers are still quite perturbed about dowsing’s popularity. In contrast, a fascinating article in a Jesuit journal by Herbert Thurston, S.J. (1934), covered some of the history of the Catholic Church’s negative stance on dowsing, but disavowed concerns about the Devil himself animating the rods, for the Devil surely has better (or rather, worse) things to be doing: “Diabolic intervention... is hardly to be thought of here... the business of locating a spring or a lode of metal ore seems too commonplace to call for such an explanation” (1934:445). In the end, the author took a noncommittal position on the powers of dowsing:

Their nature, their cause, their range of application, indeed their very existence, are all matters in dispute. We have already seen that the use of the divining rod has generally, in past years, been regarded by enlightened theologians and by men of science as a mere superstition. But now, after experimental trials have lasted for more than four centuries without adequate result, this simple instrument, though its virtue is still contested, has come again to be looked upon with favour, even among men who are critical of all other abnormal phenomena.... The point upon which I would wish to lay stress is the recognition that we are still confronted by many psychic mysteries which we are not in a position to solve.... The only remedy is – patience. (Thurston 1934:444-5)

Sixty-five years later, we are actually no closer to consensus. Dowsing is still hotly disputed in its every aspect, as easily revealed by a Web search (<http://www.phact.org/e/dowsing.htm> was a useful jumping point as of April 1999), and as easily seen by bringing the matter up in a room full of archaeologists.

The area of dowsing for less tangible things is more complicated, as we shall see, but in print and in conversation, I have encountered each of the following positions regarding the basic questions of dowsing for physical things – minerals, water, or archaeological features:

It works

- it works, but through unacceptable (occult) means and should be avoided
- it works, through an amplification of human intuition, and one might as well use it
- it works, through a real physical force which we need to develop instruments to better understand
- it works, through a human physiological reaction to magnetic variance
- it works, for some things but not others, and for some people but not others
- it works, but not in the presence of skeptics⁷

We don't know if it works

- we don't know if it works so we might as well try it
- we don't know if it works so we shouldn't bother with it
- even if it works, we don't know how, and so it should be avoided

It doesn't work

- it doesn't work so we shouldn't bother with it
- it doesn't work so we must actively crusade against it

On Testing

- controlled tests have shown unequivocally that it does not work
 - counter – those tests were not fair
- controlled tests have shown unequivocally that it does work
 - counter – those tests were not fair
- it is impossible to devise controlled tests in realistic field conditions to determine whether or not it works
- anecdotal evidence counts
- anecdotal evidence doesn't count

⁷ This may seem a silly suggestion at first glance – how could the mere presence of a skeptic prevent something from working? – but it is prevalent within serious parapsychological literature, and is occasionally even persuasively argued. Collins and Pinch (1982:170) mention that the “‘sheep-goat’ effect... is claimed to be one of the most well-established parapsychological generalisations.”

With these variants in mind, let us move ahead to the specifics of what archaeologists say about dowsing.

Dowsing in Archaeology

My research has satisfied me that it is clearly not the case, as obliquely implied by Nawrocki in the posting at the start of this chapter, that belief in dowsing is limited to archaeological amateurs. Belief in dowsing is prevalent among professional archaeologists as well, as surveys, informal conversations and email postings indicate. (Indeed, in some British departments of archaeology, dowsing is routinely taught alongside more ‘hi-tech’ methods of remote sensing in field methods courses.) It has been comparatively rare until recently to see an archaeologist formally advocate dowsing in print, but the debate is now coming into the published realm, and is proving interesting to watch.

Dowsing in Archaeology... In Practice

As part of my efforts to learn about archaeologists’ opinions of dowsing, I conducted a small survey within Sheffield’s Department of Archaeology, and also asked a dowsing question in the survey administered to both TAG attendees and Moot attendees. Neither survey was intended to sustain statistical analysis, and samples were small, but I think some worthwhile observations can be drawn from the results. (The results are presented in more detail in Appendix B.)

Clearly, academic and professional archaeologists at Sheffield and TAG have a wide range of opinions about dowsing. Few indicated that they believed it didn’t work at all. More have tried it and found it useful, and some would recommend its use on site as at least part of their survey strategy. Many were willing to take confirmation through excavation as proof of dowsing’s efficacy. Few were willing to accept dowsing as a valid source of information without confirmation through excavation; similarly, few indicated belief in the psychic aspects of dowsing, e.g. answering questions. There did appear, in the TAG

survey, to be a slight bias against the acceptance of dowsing's efficacy for finding archaeological features, as compared to accepting its efficacy for finding water or mineral deposits; however, ¼ of respondents did indicate that they believed dowsing worked for finding features.

These results certainly suggest that dowsing is used and accepted often enough by academic and professional archaeologists that it cannot be described accurately as a 'fringe' practice – regardless of whether some people think it *should* be marginalised. This suggestion is further supported by other informal evidence.

After my first public mention (in a 1995 conference paper) of dowsing as an example in which many archaeologists' conduct is not, shall we say, archetypically 'scientific', I was surprised by the response. There were no questions on that subject during the question period, but afterwards, no less than three archaeologists separately sidled up to me and quietly confessed that they were dedicated dowsers but didn't usually talk about it. One of them said that if he was going to dowse a site, he made sure he arrived very early, so that the rest of the crew didn't witness it. Others had similar tales to relate, as well as observations about the extremely negative reception that the practice sometimes receives, despite its apparent harmlessness. Since that incident, I have made a point of mentioning dowsing, briefly and neutrally, in conference talks and guest lectures, in the hope of obtaining feedback afterwards. Each time, I have been gratified by the informal conversation generated, ranging from guilty-countenanced confessions of closet dowsing, to discussions of the prevalence of dowsing in other kinds of fieldwork (e.g., construction, engineering, geology, military etc.), to revelations of mystically-named groups of dowsing academic archaeologists in California who meet in secret at dawn. Sometimes other more dramatic heresies are confessed too, particularly in the pub after the speaker has confirmed through preliminary conversation that I am not going to issue a knee-jerk condemnation or 'out' them, but will listen impartially and not repeat what I've heard.

Documenting this in a scholarly fashion is, of course, a problem, since the conversations are usually rather anonymous, and even if not, the speaker generally would not wish to be tape-recorded, would certainly cease to talk if I whipped out a pen and notepad to 'take a statement', and would not want to be identified in print. (And a shame it is, too, since it means that the most interesting discussions I've had during the course of this research cannot be included here.) However, one of the defining modes of communication in the

late 1990s is electronic mail, which has two advantages in matters of quasi-academic discussion: first, people tend to treat it more as a conversation, more open in tone and more amenable to short statements of personal opinion than conventional publication; and second, email discussion lists (as opposed to personal email messages) are publications (distributed widely in the first instance and subsequently archived on the Web), which means that they can be cited without confidentiality being violated, or permission being sought. So, for more information on informal opinions regarding dowsing in archaeology, I looked to email discussion lists.

The comments on dowsing have been very much like the ones which come up in person, though a little less extreme (after all, they are not anonymous, and no-one likes getting abusive email in return, from opposing camps). Following are a number of reasonably representative postings made by professional and academic archaeologists to email groups devoted to the discussion of general archaeological issues. I reproduce the postings here at some length in order to preserve their individuality and nuances.

1. Twelve years ago I fired an archaeologist for dowsing on a project he was paid to be surveying. Last semester I downgraded a senior's undergrad thesis for dowsing on a site she was surveying. Now, don't get me wrong. You can call the psychic hotline if you think it will get you answers in the field. Just don't do it with my crews and my client's money.
Alright, that's a hard line. We all know that experienced field archaeologists can develop strong intuitions about where sites and features of various sorts lie buried. These intuitions may be fed by unconscious observations of microtopography, barely discernable soil or crop colours, whatever. Maybe dowsing and Ouija boards and scapulamancy help some folks tune this intuition. But you still have to dig the holes.... As for the coat hangers, which don't even have a vaguely reasonable theoretical defense, keep them off my sites, please.⁸
2. Dowsing has a proper place among remote sensing methods. These methods include resistivity, metal detection, soil compaction measurement, infrared photography, oblique photography, and so on and so on. Not a single one of these methods is "replicable" or totally effective. Remote sensing simply is neither consistent nor reliable, in an absolute sense....
I don't want to impoverish my clients, so I will use any clue and every method that might reduce the amount of expensive dirt-moving. In the case presented at the onset of this thread [of discussion], the problem was to find a cemetery that was "known" to exist in a tract of about an acre. My approach would begin with the local locational model, coupled with historical research.
Then I would visit the site to grok⁹ its fullness.

⁸ Dan Mouer, in a post to the HistArch mailing list (Historical Archaeology HISTARCH@ASUVM.INRE.ASU.EDU), Tuesday 27th May 1997.

⁹ I am assuming that Heite is using the term "grok" as did Robert Heinlein, in a sense explained nicely by Ralph Abraham:

"We grok something (an archaeological find, artifact, artwork, text, poem, letter, natural

Then I would walk the site with a dowsing rig to see if my suspicions were correct.

Then I would hire a Gradall and begin stripping in the area where my hunches, my dowsing, and the models predicted the cemetery should be.

My client is paying for the use of my brain, my experience, and my intuition. Whatever gimmickry I choose to employ is immaterial if I find the cemetery.¹⁰

3. I know several archaeologists, practicing professionals all, who swear that dowsing works for them. I wonder if the esteemed James Randi would like to put up the money to test dowsing in an archaeological context?¹¹
4. I would think that an experienced archaeologist using a dowsing rod would be unconsciously drawing upon their experience to 'detect' areas where there would likely be artifacts or structures. To test that theory, one would have to conduct trials with a complete neophyte and someone with a lot of experience in archaeology. I would be willing to bet (although not \$502 000!) that the neophyte would have a very poor success rate while the experienced archaeologist would be phenomenally successful... hey, it if works... But, I wouldn't want to try to write it up in the Methods section.¹²
5. I found the initial post [Nawrocki/Randi's, from the beginning of this chapter] on dowsing interesting, and overstated. There is a connection between dowsing and archaeology for some folks. I have not tried dowsing but I have seen people do it on sites under the direction of very educated, talented, and scientifically-disposed archaeologists. Moreover, on occasion (I have no numbers to say if it is 1 in 1000 or 1 in 10) the blasted dowsing seems to work.
... Finally, since such abilities as dowsing are not predicated upon specialised education and training, they are more accessible to the public than theory and sampling strategies. I think we should look upon an interest in dowsing as a benefit to archaeologists, and a reflection of popular interest in the past. Rather than come across as draconian overlords of specialized training and frightful control over excavation territories...we should channel our energies into reaching out to more people.¹³
6. This dowsing stuff is an interesting thread what with post-modern debates and the death of positivism and whatnot, and every single publishing house putting

process, and so on) by a cycle of observing, thinking, poking, and once again observing. This is not the same as explaining it, representing it, or translating it.

Robert Heinlein introduced the word 'grok' in his science fiction classic of 1961, *Stranger in a Strange Land*. It's a translation into English of the technical term *Verstehen*, which was introduced by Wilhelm Dilthey into the literature of hermeneutics. *Verstehen*... refers... to a special form of sympathetic, experiential, and intuitive understanding." (Abraham 1994:13)

¹⁰ Ned Heite, in a post to the HistArch mailing list (Historical Archaeology HISTARCH@ASUVM.INRE.ASU.EDU), Thursday 29th May 1997.

¹¹ Posting by Chris Andersen to the ARCH-L list <ARCH-L@TAMVM1.TAMU.EDU>, on Thursday January 18, 1996.

¹² Posting by Jim Barnes to the ARCH-L list <ARCH-L@TAMVM1.TAMU.EDU>, on Thursday January 18, 1996.

¹³ Posting by John Staeck to the ARCH-L list <ARCH-L@TAMVM1.TAMU.EDU>, on Thursday January 18, 1996.

out New Age stuff and sometimes straight hard science in the same mailing... I've been dowsing for 15 years, and frequently it finds me something worth digging... Maybe there's been enough past activity on the historic farmsteads I look at, that any place one looks one finds such 'anomalies'. I never have enough money to strip the entire site, so dowsing I go. I really don't care how dowsing works, though of course I want to refine the method. It is a lot cheaper than magnetometer survey, and I've seen those suckers find 'anomalies' that we couldn't recognize with a trowel. I always enjoy the expressions of 'nonbelievers' when I hand them the rods and they see them cross or separate at the boundary of an anomaly, and then we discover that there really is something like a feature at that anomaly. Of course I've also seen some major failures wherein the dowsing said something was present and we didn't 'see' anything. Maybe some 'observational science' problems?¹⁴

7. [A senior Texan archaeologist after a surprise success with dowsing on site]: Naturally, you can bet your butt I've done some very extensive reading and some further research into dowsing (of course, this was done VERY discreetly, as a scientist sure can't be getting' hisself associated with foolishness like this). As an outgrowth of this inquiry, I had some correspondence with J. Scott Elliott, a retired Brigadier General of Her Majesty's Royal British Army. He wrote a very interesting little book called "Dowsing: One Man's Way".... Some archaeologists (those secure enough in their reputations that they aren't frightened of being labeled kooks or charlatans) may find his methods of interest.... they don't work for me, but I have little doubt that they worked for him.¹⁵
8. The modern Western tradition has often been too quick to abandon many such 'focusing' exercises.... As anthropologists, we should acknowledge that such exercises are part of all cultural traditions. Shamans, for example, were experts in effecting 'attitude adjustments'. The physical structure and layout of archaeological sites, especially ones that served as nodes for religious/shamanistic activities, is also likely to have served this purpose. To deny the possible efficacy of dowsing and other similar 'paranormal' activities because there are no acceptable scientific explanations is to throw the baby out with the bath water. We need a better understanding of how the material world structures, facilitates, and otherwise affects human cognition. This should be a fruitful avenue for archaeologists, as illustrated by the recent Cambridge Press book on 'Cognitive Archaeology'.¹⁶
9. In my opinion, anyone who believes in dowsing is welcome to dowse away as long as they're not wasting my time or money – or the taxpayers'. It's always nice to know that there's *someone* you can feel superior to.¹⁷

¹⁴ Posting by Skip Stewart-Abernathy to the ARCH-L list <ARCH-L@TAMVM1.TAMU.EDU>, on Monday January 22, 1996.

¹⁵ Posting by Bob Skiles to the ARCH-L list <ARCH-L@TAMVM1.TAMU.EDU>, on Saturday January 20, 1996.

¹⁶ Posting by John Hoopes to the ARCH-L list <ARCH-L@TAMVM1.TAMU.EDU>, on Friday January 19, 1996.

¹⁷ Posting by Bob Jeske to the ARCH-L list <ARCH-L@TAMVM1.TAMU.EDU>, on Friday January 19, 1996.

10. In fact, they [coat hangers] can be used to locate buried 17th century French iron trade axe heads on former Indian village sites in agricultural fields. My only 'explanation' (and one that I believe is current – not to pun – in some dowsing circles) is that the buried objects must be creating an electromagnetic field that is expressed or indicated by the crossing of the hangers. This, of course, cannot be of relevance to 'water witching' using forked sticks. At least, I don't think so. And then there were the psychics...¹⁸

In these postings, then, can be found a range of beliefs about dowsing's efficacy and a range of positions on the use of archaeology in dowsing. Only two postings were strongly negative: # 9 indicated that the author has no patience for it, but didn't say specifically why. The author of # 1 accepts that it might help with focusing intuition, but objects so strongly to the use of something with no good theoretical explanation that he absolutely will not tolerate coat hangers on his sites, not even as student experimentation or a supplementary survey method. One author (#8) was noncommittal but clearly didn't think dowsing should be dismissed out of hand because there is no acceptable explanation. Others (#5, #3) didn't dowse themselves but have seen it work at least some of the time, or are willing to trust their colleagues' statements that it works for them. Others have had personal success with dowsing, though their beliefs as to why it works are different: author #4 believes dowsing works well but only as a method of honing intuition; author #10 believes it works for metal, probably through an electromagnetic field. The authors of postings #2 and #6 are clearly far more concerned with results than with theoretical explanations or the lack thereof, and have had enough success that they regularly use dowsing as part of their battery of survey methods. Additional points of particular interest here were the comment in #5 regarding the accessibility of dowsing to the public, and the mentions in #4 and #7 of the issue of needing to watch one's reputation if involved in dowsing.

Judging from informal discussions, survey results, and narratives such as those related above, it seems to me that most archaeologists' belief in dowsing is structured and mediated by personal experience, rather than through statistical evidence or controlled tests à la Randi. There is, at times, a sense of allegiance to rural tradition, and also to archaeology as 'craft' rather than as 'science'; certainly there is a sense of pride in the special intuitive knowledge that a field archaeologist has, and an unwillingness to

¹⁸ Posting by Bill Fox to the ARCH-L list <ARCH-L@TAMVM1.TAMU.EDU>, on Thursday January 18, 1996.

relinquish this in exchange for the appearance of being scientific. Personal, idiosyncratic encounters with dowsing – actually using the rods oneself – seem to be a big factor. If an archaeologist tries dowsing, and obtains a response, this is a surprising sensation which confirms that something worth noting is going on. And if, in turn, he or she excavates at the indicated location, and finds a significant archaeological feature, this is a powerful experience, unlikely to outweighed by theoretical articles against dowsing. (Even just watching this happen, rather than participating directly, can also be powerfully convincing.) Under such circumstances, why wouldn't the archaeologist try it again? If dowsing was more expensive, resource-intensive, damaging or even very time-consuming, there might be greater reservations about its use. But it is not, and so, as we have seen, many archaeologists do it. And some even believe in it enough to go 'on the record' in a formal publication saying so, either on the basis of personal experience or on the basis of experimentation, as we shall see below.

Dowsing in Archaeology ... in Print.

In the first generally cited instance of dowsing being tested in an archaeological context, Aitken (1959) supervised a showdown between a dowser and a magnetometer for detecting a kiln. In his blind test, the dowser failed to locate the kiln, although he had some responses elsewhere on the site. Aitken concluded:

Whether or not any feature, archaeological or geological, lies under the areas of dowsing response is unknown at present.... On the other hand, because the kiln produced no dowsing response and because the regions where dowsing response was obtained were devoid of magnetic disturbance, the question of correlation between dowsing response and magnetic disturbance has been answered unambiguously in the negative. (1959: 59)

On the other hand, Ivor Noël Hume gave dowsing a favourable mention as part of his section on surveying in his text on historical archaeology, saying that coat hanger angle rods are "included in every Williamsburg archaeologist's box of tricks" (1969:38). He placed a good deal of confidence in dowsing for metal on historic sites, on the basis of successful tests in field conditions, but noted that it doesn't work for everyone, and that there is no good theoretical explanation. He observed that "one feels a little idiotic walking across a field intently watching two pieces of coat hanger", but considered them to be a

very useful and sensitive method, especially given that other metal-detecting equipment is so expensive (1969:37-8).

The biggest landmark in the study of dowsing in archaeological contexts is undoubtedly the 1988 publication by Bailey, Cambridge and Briggs of *Dowsing and Church Archaeology*. (See also the related publications, Bailey 1983, Briggs et al. 1983, Briggs et al. 1985.) The authors express no surprise that although field archaeologists they know use it, “none have admitted as much in print”, and that mention of dowsing has been edited out of at least one final excavation report of which they are aware (1988:30). Indeed, Bailey seems to think it appropriate that archaeologists have distanced themselves, given the “near lunacy” of some archaeological applications of dowsing, citing the British Society of Dowsers anthology (Graves, ed. 1980, more below) as an example. Nonetheless, partly because conventional remote-sensing devices are awkward to use inside old English churches, and partly because they felt it was time for dowsing to be rigorously tested in an archaeological context, the team undertook a detailed study, even though, in Bailey’s words, he was “bitterly attacked” by colleagues and advised not to write the book. Some critics apparently suggested that *any* coverage given to dowsing as a remote sensing technique on archaeological sites would confer “academic respectability on the whole world of ley lines and space gods, zodiac patterns and pyramidology”, and indeed, Bailey notes that the correspondence which followed his 1983 article was “often vituperative”. So in the introduction to *Dowsing and Church Archaeology*, Bailey et al. very clearly specified that “we have no wish to lend any credence whatsoever to such lunacies. On the contrary, our results suggest that dowsing does not belong in this strange company.” (1988:xii).

In their tests of Briggs’ dowsing abilities in a number of circumstances, they found that despite mixed results overall, there were situations in which the dowsing appeared to be effective. Particularly notable is their claim that “the technique *is* capable of recovering details about phases of construction which would otherwise remain inaccessible” (Briggs et al. 1985:143), *i.e.*, dowsable imprints of features which had been removed earlier. Ultimately, the conclusion drawn by Bailey was that “archaeologists should now seriously consider adding dowsing to their battery of remote sensing devices, on the simple grounds that it appears to work” (1988:109).

Responses to Bailey et al. (1988) have been varied. Tom Williamson's *Dowsing: New Light on an Ancient Art* is an interesting example because Williamson, a dedicated dowser, is the academic landscape historian/archaeologist who also cowrote *Ley Lines in Question* (1983), the highly critical work on ley hunting. Williamson's overview of the use of dowsing to locate buried anomalies covers everything from U.S. Marines dowsing for Viet Cong tunnels in Vietnam to some Russian and French case studies, before coming to rest on Bailey et al.'s work. Williamson reiterates their findings and mentions support they have received from other archaeologists, but seems to cautiously avoid adding his own vote (1993:67-69), or relating any personal archaeological dowsing experiences, although he is forthcoming about his belief in dowsing in other circumstances.

On the other hand, academic church archaeologist Warwick Rodwell was quite effusive regarding Bailey *et al.*'s work. Noting that conventional geophysical methods – resistivity and magnetometer survey – are awkward on church sites, he lauds dowsing:

It would appear that a means of circumventing these seemingly insurmountable problems has been found in the art of 'dowsing'. There is no doubt whatever that dowsing works – it is used daily by farmers, water engineers, and others, to discover the whereabouts of buried pipes, drains, etc. – but its scientific basis remains largely unexplained. Moreover, since dowsing has long been a favourite pastime of the 'lunatic fringe', it has been shunned in professional archaeological circles. But now the injustice has been corrected [through the work of Bailey et al]....A stern word of warning is required, however: the impeccable Northumbrian-based research into dowsing must not be confused with the wholly ludicrous claims of some other church dowsers. (Rodwell 1989: 112-3)

Martin Locock is another archaeologist who has tested the technique of dowsing in an archaeological context. Following on from Bailey *et al.*'s work, Locock invited a dowser to map subsurface features in the historic garden at Castle Bromwich. The results suggested to him that "the technique performed better than random selection for metal objects, but not for old soil disturbance or some masonry features" (1995:17). Subsequently, Locock distributed questionnaires to colleagues in an effort to find out about other instances where dowsing has been tested by excavation. He had "very few cases of sufficient detail to compare: the results of the dowsing were usually so vague or improbable that they were dismissed out of hand by the excavators" (pers. comm.). Nevertheless, his final conclusion mirrored that of Bailey et al. (1988:109), that dowsing does merit further careful investigation.

I am aware of only two more publications outlining controlled tests of dowsing in archaeology: recent amateur investigations include the North East Hants Archaeological Society's project using dowsing to locate a moat filled in around 1700. The experiment was structured to include multiple dowzers and to provide for controls, and results were being compared against resistivity surveys (Whaley 1993, 1994).

Overall, one can see that archaeologists publishing statements in favour of dowsing in archaeology have tended to be very cautious. And no wonder, for those against archaeological dowsing can be as vehement 'on the record' as in the informal comments seen in the previous section. For example, in an episode of *Time Team* which was devoted to the investigation of a fogou in Cornwall¹⁹, archaeologist Mick Aston expressed extreme skepticism over the offer from Hamish Miller (one of the U.K.'s most famous dowzers, about whom more below) to dowse the site. Aston agreed to check Miller's predictions by putting in "our first New Age Trench", but seemed barely able to contain his glee when it turned up nothing. During a subsequent lecture to Sheffield's Department of Archaeology (April 24/96), Aston made it clear that he thought this single instance was a fair test, and that no more attention need be paid to dowsing. (Many experimentalists would probably disagree.)

A more moderate statement in Gaffney *et al.*'s overview of geophysical techniques in archaeology recalls some informal comments in the previous section, taking the position that dowsing should not be used because its mechanism is unknown: "Unfortunately the scientific principles, if there are any, are not understood, and as such the technique should not be used for evaluation purposes." (cited in Van Leusen 1999:35).

Archaeologist Kenneth Feder, in his *Frauds, Myths, and Mysteries* (1996) takes the CSICOP hard line, though he doesn't discuss physical (non-psychic) dowsing in much detail. He cites Randi's tests of water dowzers as showing that the dowzers "failed utterly", and adds that

There has even been a test of the ability of dowzers to locate buried archaeological objects (Aitken 1959). ... Aitken concluded from this that dowsing for archaeological remains simply did not work. Though practitioners of dowsing hold quite strongly to their beliefs, they seem to be unable to produce

¹⁹ Viewed on U.K. Channel 4, January 7, 1996.

under scientifically controlled conditions. Until they do, dowsing cannot reasonably become a part of the archaeologist's repertoire. (Feder 1996:206-7)

Feder's position and phrasing are problematic, to begin with because (a) Aitken tested a single dowser, not dowsers, and Aitken's observation, as cited earlier in this section, specifically concerned magnetic disturbance²⁰ and (b) there have been very few tests of archaeological dowsing under anything resembling controlled conditions, and so generalisations seem unfair, and (c) dowsing is *already* part of the archaeologist's repertoire. I think it can be assumed from Feder's inclusion of dowsing in this book, that he considers dowsing to be 'fringe' in nature (he also includes dowsing in his 1995 list of dismaying archaeological ideas, p.35). However, I do not consider this to be *descriptively* accurate, given the number of archaeologists who do practice dowsing; and Feder simply doesn't carefully argue a *normative* case for why archaeological dowsing *should* be discouraged. Why must success in scientifically controlled conditions be the arbiter? This is simply a given to Feder, but its validity is not actually obvious.

For now, the latest word on dowsing in archaeology comes from archaeologist Martijn Van Leusen, who picks up where Feder left off, publishing a major article in *The Skeptical Inquirer* in 1999, continuing that journal's long history of 'dowsing-debunking'. He begins with the observation that "in informal contacts with fellow archaeologists both in the U.K. and in the Netherlands, I found many archaeologists who believe there is something worthwhile in dowsing, and few that have categorically denied its value; however, neither belief appears to be based on any serious study of the evidence" (1999:34). This Van Leusen considers a cause for worry, and he sets about rectifying the situation.²¹

Since *Dowsing and Church Archaeology* is the only major work describing tests of dowsing in archaeological contexts, it is the main subject of his article's detailed analysis. He questions the authors' assessment of statistical significance, though he admits that the

²⁰ It is hard to resist the speculation that if Aitken's conclusions had been positive, Feder or someone of similar anti-dowsing inclination would have found grounds, such as statistical insignificance, for disregarding this experiment.

²¹ Where Van Leusen is coming from is clear. As Feder includes dowsing in *Frauds, Myths, and Mysteries*, Van Leusen includes dowsing on his WWW page on "Fringe Archaeology: Hare-brained ideas about material remains from our past" <http://odur.let.rug.nl/arge/Themes/fringe.html>. It would appear that he has yet to fully absorb the fact that, as he himself has indicated by noting its popularity among archaeologists, dowsing is not a 'fringe' practice except by arbitrary definition.

intractable nature of real archaeological conditions “makes the design of good tests almost impossible” (1999:40). Interestingly, rather than seeing flexible strategies (e.g. dowsing) as an effective contingent mechanism through which archaeologists cope with the challenges posed by those conditions, he considers them a failure because of their statistical inadequacy and inability to conform to the expectations of experimental science. This is a curious stance, given archaeology’s nature, and belies the ideology underneath Van Leusen’s position.

Van Leusen has made some interesting observations, but I believe erred on at least one count. He contends that the rise in New Age thinking in the ’60s and ’70s is the cause for the positive mentions of dowsing in some archaeological texts since that time:

A review of attitudes toward dowsing both in the general and the archaeological literature shows that, as 1960s and 1970s New Age thinking made its mark in academia generally, a more favorable view of dowsing slowly made its way into influential introductory books on archaeology. (1999:35)

Van Leusen’s argument is clearly for a creeping irrationalism in society at large manifesting itself within archaeology’s borders. This is in line with the general philosophy of *The Skeptical Inquirer*, but surely difficult to sustain, especially given that these mentions are very infrequent overall, and that no change over time in archaeological mentions of dowsing has actually been demonstrated. Examples Van Leusen gives include Philip Rahtz’s *Invitation to Archaeology* (1985:127) which does include a very brief positive mention of dowsing, but from Rahtz’s next sentence, a dubious comment on paranormalism and stern caution about “pure fantasy”, and Rahtz’s rather cool allusions to ‘alternative’ culture at Glastonbury (1993:10, 132), it is difficult to suppose that he is particularly ‘trippy’ in inclination. Moreover, it seems just as possible – and to me, more probable, based on archaeologists’ personal accounts of discovering dowsing – that dowsing within archaeology has primarily been a profoundly *conservative* practice, stemming from its traditional use in the field in occupations of all kinds. For example, Denis Briggs (the dowser in the Bailey, Cambridge, and Briggs trio), was hardly a crystal-bedecked maven of the British New Age, but a retired engineer – one quite standard profile for an avocational dowser in the U.K.. Furthermore, Bailey, Cambridge, and Briggs very specifically disavow all such “lunacies” (1988:xii). In short, Van Leusen doesn’t have to like dowsing, but he will have to provide more evidence to soundly argue that belief in dowsing in archaeology is new, rising, or correlated to – never mind actually *caused by* –

proliferations of ‘pseudoscience’ in the media. (This is all a little ironic, given that his entire argument in this piece is about archaeologists’ misinterpretation of apparent correlations, due to their “basic lack of understanding... due to their lack of education in these areas, about the nature of proof and probability” (1999:40).)

So, then, if one approaches the issue of dowsing in archaeology with the belief that dowsing does not work, and an agenda to search for and destroy ‘fringe’ beliefs, one will find ample evidence of ‘irrationalism’ – so much so that the discipline seems riddled with it, and many archaeologists seem gravely confused. If, on the other hand, one approaches it with a simple desire to find out what archaeologists believe about it and why, the results are much more interesting. It becomes evident, for example, that there is a spectrum of belief, based on different criteria from practicality in the field to theoretical satisfaction, and that this is entirely in keeping with the range of opinions about dowsing within the larger scientific and trades communities. It also becomes evident that it is a very touchy issue precisely because of the very old ‘search and destroy’ Inquisitorial attitude towards dowsing that has recently been revived in a new guise. Some archaeologists (Bailey *et al.*, Rodwell, Rahtz) who have supported dowsing in formal publications in the last fifteen years or so have professed a great concern not to be taken as lunatics, and take great care to set themselves apart from other dowzers; it is as though the disavowal of ‘flaky’ tendencies is a requirement for the privilege of remaining within the archaeological fraternity.²² One wonders how exactly this insistence has come to be necessary, for it has not always been so. Hume (1969), for example, treated archaeological dowsing quite matter-of-factly, as a continuation of the traditions of plumbers and electricians, without disclaimers. One then has to ask – is the rise of New Age dowsing to blame for this polarised and agitated state of affairs, or is the rise of anti-New Age sentiment within archaeology to blame? Or both?

At any rate, an examination of current opinions about dowsing within ‘alternative’ archaeology in the U.K. reveals a spectrum which, perhaps contrary to expectation,

²² One can very nearly hear echoes of a sworn oath like: “I am not now, and have never been, a member of the Communist party...”. Perhaps it is not as serious overall as McCarthyism, but perhaps it is for some. As was proudly related by an anti-dowser earlier in this chapter, people lose their jobs over this issue.

overlaps substantially with opinions about dowsing within academic and professional archaeology.²³

Dowsing, Archaeology, and Earth Mysteries²⁴

Much more will be said about ley hunting and Earth Mysteries in the next chapter. Here, the subject is the importance of dowsing within this discourse and the interesting fact that it has caused as deep a rift there as within academic and professional archaeology. The lines of dispute are slightly different, but do boil down to precisely the same questions, *i.e.*, what constitutes admissible evidence, what constitutes verification, and what should be done with methods and theories that don't conform to one's own answers?

Let us begin with one of the few engagements, in print, of an academic archaeologist with 'alternative' archaeological dowsing. Tom Graves is one of the best-known dowsers in the U.K. Earth Mysteries and dowsing scenes, but among archaeologists, Graves is probably only recognised as the editor of a rather infamous anthology of works from the Journal of the British Society of Dowsers, called *Dowsing and Archaeology* (1980). The anthology includes works by Reginald A. Smith and Guy Underwood, amongst other lesser-known archaeological dowsers. Reviewing this book in *Antiquity*, Robert Raikes had this to say:

This kind of nonsense would normally hardly merit a review in any respectable journal. However, it seems that the book is selling quite briskly to the lunatic fringe, of which the interest and support, in its saner moments, unfortunately has to be reckoned with. It seems worthwhile therefore to take it far more seriously than it seems to deserve...

²³ An interesting, related issue is that of the relationship of orthodox archaeology and metal-detecting in the U.K. Abundant commentary can be found in British archaeological publications of the 1980s, especially. In 1984, Tony Gregory and Andrew Rogerson made a provocative argument in favour of using metal-detectors on archaeological sites.

²⁴ Because it suggests something about perceived domains of knowledge, I cannot resist relating the Cambridge University Library's ordering of books. Mermet's 1935 *Principles and Practice of Radiesthesia*, Mager's 1931 *Water Diviners and their Methods*, Tromp's 1949 *Psychical Physics*, and Vogt and Hyman's 1959 *Water Witching USA* – all books on dowsing – were shelved together with such titles as *Satan, The Story of the Poltergeist Down the Centuries, Psychic Surgeon, Ghosts Vivisected, News from the Next World, Hex, Phrenology*, and innumerable treatises on astrology, witchcraft, ghosts, ESP and general parapsychology. On the other hand, *The Second Book of the Cosmic Forces of Mu* and various other Atlantean and lost continent books, are shelved with ley lines, Earth Magic, modern orthodox archaeology and *The Brasses of England* and *English Church Furniture*.

To accept that some people apparently experience a reaction, often translated into physical movement of a rod or pendulum or wire or whatever, is one thing. To claim that this reaction, to the expert dowser, can be quantified in terms of how much water (or foundations, or metal, etc.), at what depth, appears to be completely unjustified. To go on and link the object, particularly if it is water, so found, with religious ritual is lunacy; to claim that what one of the contributors calls 'mental' dowsing can, by a process of preselecting a period of history or prehistory, predict the age of the object, is stark, staring madness.

(Raikes 1984:
231)

It is pretty clear where Raikes draws his line, and that he feels strongly enough about it that he is unable to deal with the possibility that there might be a genuine and valid difference of opinion about how dowsing can be used and what real knowledge is, instead vigorously doubting the authors' sanity. He does discuss some of the problems in the anthology with the depictions of the behaviour of underground water, and with the assertions about the behaviour of prehistoric people, but stops there because he doubts it is worth his while:

I doubt if logical criticism is likely to influence a collection of people that is conspicuous for dispensing with logic. ... One of the fundamental freedoms is to write what you like, including nonsense: another, which I am exercising, is to denounce it. I think it is legitimate to ask the editor and authors for a solemn assurance that the whole volume is not just a gigantic leg-pull.... While I for one keep an open mind about the possibility of there being some hitherto unexplained phenomenon to which certain people are subjectively more responsive than others, I do feel very strongly that archaeology is already lumbered with far too much lunatic fringe – mostly born out of ignorance for the natural sciences; and that this sort of pernicious nonsense can serve no purpose other than to increase confusion. (Raikes 1984:232)

Raikes reserves particular contempt for the writings of Guy Underwood, whose work is an interesting episode in the history of dowsing and archaeology because it began not with New Age sensibility or psychic divination or anything of that kind, but with an archaeologist well entrenched within the British establishment. Underwood's interest in matters archaeological had been sparked by Reginald Allender Smith, archaeologist and former Keeper of the British and Roman Antiquities department at the British Museum. Smith's activities during his brief retirement before his death in 1940 included an escape from "the sterner stuff" of archaeology (Smith 1980 [1939]:42), and giving a talk to the British Society of Dowsers which stated that underneath major prehistoric sites such as Avebury, Stonehenge, and Stanton Drew, there was always a confluence of underground springs, detectable by dowsing. Smith closed his talk by saying "Any new light on the ancient Britons should be welcome to students, not only in this country but abroad,

wherever dowsing is becoming a subject of serious study in spite of 'scientific' opposition" (p.50).

Underwood took Smith's talk to heart, and commenced his own research. The result, *The Pattern of the Past* (1968) is one of the books on dowsing archaeological sites which was most influential within alternative circles, and very different in character from some recent trends in New Age dowsing. It is a fascinating and earnest book, including exhaustive detail, careful referencing, precise diagrams and maps, perceptive commentary, and discussions of underground water patterns Underwood discovered through dowsing megalithic sites which are rather astonishing to the minds of most conventionally trained archaeologists (as Raikes indicated). The book was published posthumously, possibly because Underwood had no illusions about how his work would be received by those outside the British Society of Dowzers: "Archaeology is an art rather than a science – in that its 'facts' are not always capable of proof. Yet new ideas can be as unwelcomed by the profession as they are in other arts." (1968:11)

Very interesting is a comment from Underwood on an encounter with an archaeologist, for it illustrates what the archaeologist felt to be sufficient disconfirmation of dowsing:

When I first became convinced of the truth of Reginald Smith's theory, I thought it wise to look round for an expert archaeologist who was also an expert physicist. Such a man should be capable of reliable judgement on both subjects. At last I found such an individual, and wrote a tactful letter. He replied in a most friendly way, but said that he could not accept the alleged facts of dowsing. His reasons were that some years previously a well-known dowser visited his excavations, and, among other things, claimed that by the use of coloured rods he could indicate the presence of buried metal objects. The dowser said that violet-coloured rods would indicate bronze. He proceeds: "To put his claim to the test, I excavated in his presence several large and deep holes... where he claimed bronze existed. No evidence was forthcoming, and he finally agreed that his claims could not be substantiated." A valuable recruit was thus lost to dowsing, and a weighty opponent created. (Underwood 1980:59-60)

This would seem fairly typical both of the type of test which some archaeologists have tried with dowzers, and typical of the generalisation from a single failed instance to a rejection of all dowsing. It also shows the kind of opposition that Underwood keenly felt as he proceeded with his work, and goes some way to explain why his further interaction with archaeologists was limited.

Underwood's influence was pervasive, with many dowsers studying and citing his work (e.g. Dineen 1977, Taylor 1984), and is still evident within 'alternative' publications on dowsing and archaeology. But it was far from the last word, for in the 60s and 70s there arose a keen interest in energy dowsing, which fused with a revived and transformed interest in leys to create a new area of research and writing into dowsable energy lines, as well as into the possibilities that earth energies were known and deliberately manipulated by ancient peoples (Hitching 1978).

John Michell (1991) notes the role of archaeologists in the genesis of this new discourse. He describes the 1935 finding of two French archaeologists, Merle and Diot, that dowsing revealed that every megalithic monument they checked was located over top of underground streams, a finding then echoed in Britain by Smith and Underwood. He continues:

Also influential were the books of T.C. Lethbridge, a respected archaeologist who made use of the diviner's pendulum for discovering and dating antiquities. This led him deep into the world of mysticism, and he came to perceive that stone circles had been charged by ancient priests with magical powers which still lingered. At the Merry Maidens circle in Cornwall, he and his wife were shocked by a discharge of energy, and Lethbridge formed the idea that stone circles were beacon stations for space travellers. His views were widely disseminated by the polymathic Colin Wilson. (Michell 1991:7)

Nigel Pennick, famous within EM as an authority on geomancy, noted in 1979 that dowsers can detect both water and energy anomalies, and believed in their ability to help discover anomalies which can shed light on the physical situation of churches and megalithic sites:

What is certain is that they reveal to the dowser a discernible special pattern which was there before the edifice was constructed, the patterns having been previously identified by some agency – a geomancer, a priest, a member of the arcane brotherhood, a 'wise' man or woman, or even an animal... The attributes of these special places were thus captured, enhanced and guided into the service of mankind by the geomantic construction, the creation of special physical, psychological and spiritual states attainable only with difficulty elsewhere. (1979:37-8).

This captures reasonably well the mood of some dowsers within EM during that period, a period which also saw the birth of a collective initiative called The Dragon Project, whose aim was to objectively study the possibility, suggested by folklore, personal experiences, and anecdotal evidence, that some ancient sites in the U.K., such as the Rollright stone circle, actually exhibit anomalous energy (see Devereux 1990; Robins 1985, most post-

1977 issues of *The Ley Hunter*). The Dragon Project is remarkable for many reasons, but in this context it is particularly interesting because it was run within the Earth Mysteries community rather than out of a university, but included the direction and collaboration of Don Robins, an archaeological scientist known for his work on electron spin resonance, who has held various academic posts in the U.K.. The Dragon Project included dowsing as one of its battery of methods to study the possibility of anomalous energies existing at prehistoric sites. Members have also used ultrasonic monitoring, infrared photography, conventional soil resistivity, Geiger counters, and psychics doing direct readings from the stones (Robins 1985). In recent years, the Dragon Project has been continued through experiments with dreaming at selected archaeological sites. This move away from the attempted direct study of earth energies may well related to a growing schism within the Earth Mysteries community; that is, a strong bias in one sector away from ‘unverifiable’ forms of dowsing.

Michell describes the lack of concern among some “New Age” energy dowsers regarding replication or verification of their results; to many, it is quite irrelevant whether or not another dowser uncovers the same pattern of energy lines (1991:9). This, plus the perceived ‘hijacking’ of the term ‘ley’ to include energy lines, caused a rift within the Earth Mysteries community, still evident today. Danny Sullivan, longtime EM writer and editor of *The Ley Hunter* journal since 1996, is very clear that although one of the journal’s founders, Jim Goddard, and others still embrace energy dowsing and dowsable leys, Sullivan and by extension *TLH* “no longer entertains the fiction” that these forms of dowsing work (Sullivan 1997). Certainly Paul Devereux, another major Earth Mysteries figure, though maintaining faith in the use of dowsing as an experiential tool through which to understand one’s surroundings, has long distanced himself from claims for energy dowsing, “as ridiculous as they are unverifiable” (1996:190).

The results I obtained in my survey of TLH Moot attendees (see Appendix B, part 2) also reflect this opinion, which is noticeably similar to that of most academic archaeologists. The energy dowsers are still out there, however; the *Journal of the British Society of Dowsers* and many New Age publications certainly reflect this, not only on the subject of dowsing and healing, but also on dowsing sacred space.

Sig Lonegren is a prominent dowser with experience in dowsing sacred sites in the UK and the Americas (1986). He explains his understanding of the reasons why people are interested in doing this:

There seem to be at least two quite different motivational factors that cause people to investigate sacred space. First, there are those who research these places to gain scientific knowledge. They utilise the scientific method where repeatability is a key criterion. If others cannot duplicate your findings, you have not complied with the rules of science, and therefore you cannot claim to have new knowledge. You do not have 'truth'. Dowsers who help archaeologists locate buried artifacts fall within this first category – either there are bones/pottery/bronze daggers etc. where they say “dig” or there aren't.

A second group of people visit sacred sites looking for something quite different from what scientific researchers seek. These are pilgrims on the spiritual path, and they seek to utilise these sacred spaces for spiritual enlightenment.... it is not expected that their experience of the numinous will be consistent with others' experience. (Lonegren 1991:11).

Notably, Lonegren (1986:60-2) actively encourages dowsers of the second group to think twice before assisting archaeologists on excavations at sacred sites because of the destruction caused, unless it's rescue excavation. Giving further detail on the actual processes of dowsing, Lonegren distinguishes between “tangible targets” – water, a lost ring, archaeological evidence – and “intangible targets” – earth energies, auras, and thought forms (1991:13). With the latter, he says, empirical verification is impossible, and so is repeatability, but this needn't be cause for concern, merely for wonder at the range of human spiritual experience. He explicitly disavows the requirements of a scientific model:

Scientists also love the fact that dowsing isn't always accurate or repeatable because then they get to discredit the whole process: to throw the baby out with the bath water. Because different dowsers don't come up with the same things at any given site, they decide dowsing is an invalid tool. Maybe, maybe not. We're just relearning how to use these tools.... it could also be that, like the proverbial three blind men, each of us is 'seeing' a slightly different part of the elephant.... We will find different things. And that's OK.
(1986:62)

Hamish Miller, the dowser tested by Mick Aston on “Time Team”, is prominent for his book *The Sun and The Serpent*, written with Paul Broadhurst about the giant dowsable ley they call the St. Michael Line. Miller seems a little less blithe about the problem articulated by Lonegren, and hopeful for something more concrete in the future:

There is an indefinable lament to the art of dowsing which at this stage almost precludes the establishment of absolute data, particularly for those whose narrow disciplines require results written in concrete before they are

acceptable. I believe it is important to submit all data as it is honestly found, rather than selected items geared to shore up a pet theory.

Dowsing is not yet an exact science... However, if each dowser presents data truthfully and is acutely aware of the ever-present danger of indulging the required results, some consistent meaningful stuff will emerge.

(Miller 1991:17-24)

Tom Graves, noted above as one of the best-known dowsers appearing in the Earth Mysteries literature, was ultimately far more damning: “Energy dowsing, as far as the Earth Mysteries scene is concerned, is a mess. I’ll have to admit that some of this is my fault: I may have been one of the first to publish detailed descriptions, well over a decade ago now, but I failed to include enough warnings about mistakes that can so easily be made” (1991:1). His critical assessment of contemporary energy dowsing focuses on those who do not pay attention to properly learning dowsing’s methods, who have relinquished all adherence to the idea of an external reality against which their results can be checked, and who rush their findings into print before careful, time-consuming confirmation, yet claiming that their work is ‘science’. Graves writes to other dowsers that “We *cannot* hide in a metaphysical mist on unaccountability” (1991:1), for to do so brings not only dowsing but all of Earth Mysteries into disrepute, and compromises his own mission of understanding the relationship of people to sacred landscapes.

Long-time Earth Mysteries writers Paul Screeton (1994) and Bob Trubshaw (1993) also register their serious reservations about the current state of energy dowsing and their hopes that people will move on.

The reader is referred to Jeremy Harte’s *Research in Geomancy 1990-1994* for more references to recent dowsing work within EM, but it should be clear from the examples given that while there has been a spectrum of opinion in ‘alternative’ circles about what can and cannot be dowsed, some of the objections echo those articulated by professional and academic archaeologists in the previous sections of this chapter. As in the case of conventional archaeological dowsing, issues of proof in archaeological contexts, how we know what we know, etc. simply have not permitted general consensus, despite hardline statements from some that views other than their own are quite impossible to hold.

And so there is still a great deal of recent discussion of dowsing in ‘alternative’ publications, which most archaeologists will find profoundly alien. In 1990,

Gloucestershire Earth Mysteries published notes on Richard Anderson, a dowser “able to dowse the intrinsic patterns of crop circles up to two years before they appear” (Willocks 1990: 2). Similarly, the *Journal of the British Society of Dowsers* continues to report on physical dowsing in archaeology – e.g., Major Haswell’s (1992) anecdotal reports of his success in locating subsurface archaeological features on several sites in Devon, to the impressed surprise of archaeologists – and also on some rather more psychic dowsing which might be a trifle startling. I will give only one example.

John Harvey’s (1994a, b, and c) discussion of energy lines at British archaeological sites makes for surprising reading; his belief is that the energy lines he charted were used as “pathways for telepathic communication”, information given him by question-and-answer sessions with his L-rods. He considers his findings to be easily confirmed: “all you have to do is take your dowsing instruments to the nearest ‘Energy Lines’ and ask the same questions that I asked” (1994a:88). Harvey’s more in-depth studies have revealed to him many invisible Iron Age peasant burials in Exmoor, which he has dated to 1910 years ago by asking the rods their age. For Harvey, the most interesting features of these burials include the configuration of dowsable energy circles around deep rock fissures in a way that precisely mimics cup-and-ring marks. The fact that nothing tangible was left in these invisible burials might have been worrisome if not for his discovery, through excavation of these sites, of “Life Stones” within their boundaries. These small stones – of which Harvey has analysed over 300 – were given to each person by the village druid, and mercifully recorded details of Iron Age life, from rites of passage to the specific details of the occupations, ages, weapons owned, and cause of death of the person commemorated. For example, Harvey reports that dowsing the life stone of a horse herder from 2600 ybp reveals that he drowned by falling in a local pond at night-time after eating magic mushrooms (1994c:174). Many others, however, fell victim to Roman violence. . .

The archaeologist may have to shake their head vigorously at this point, and might even breathe a sigh of relief that psychic methods of gaining information about the past have nothing to do with academic and professional archaeology.... but once again, the situation is rather more interesting than that.

Psychic Archaeology²⁵

Psychic archaeology is an incredibly varied subject, and hardly new. As with the case of dowsing, an investigation of belief regarding the topic is hampered by its 'lunatic fringe' image; indeed, the influence of such categorisation is probably much stronger here. Archaeologists with merely casual or wavering belief in psychic methods would, I think, be *very* unlikely to say so in print. Although I have no formal data, in the course of this research I have heard many a rumour about academic and professional archaeologists occasionally using psychics on site, and a carefully documented examination of the structure of belief on this issue would indeed be an interesting future research project. For the time being, I shall merely point out that psychic archaeology is not simply the province of the avocational or the 'alternative'; there are a good number of examples of the influence of psychic theories or methods on archaeologists whose training and placement would normally be considered 'orthodox' (i.e., they are longstanding members of the professional archaeology community and/or have done post-graduate training in anthropology and archaeology). I strongly suspect that people have occasionally attempted to use psychic methods in archaeological investigation since such investigation began, but the first generally cited example is that provided by Frederick Bligh Bond.

Frederick Bligh Bond was a prominent ecclesiastical archaeologist and architect chiefly known for his work on Glastonbury Abbey in the first decades of this century. His early work on the site has been held up as quite solid in recent years (McKusick 1984); in particular, Rahtz (1993) comments in his volume on Glastonbury that Bond's methods and recording were of a high standard. As Director of excavations at the Abbey, the story goes, Bond was something of a hero because he successfully located the long-lost Edgar Chapel, of which there had been no record since Elizabethan times. All was well until 1918, when he published *The Gate of Remembrance*, which, as he put it, told the true story of the Edgar Chapel excavations. Bond was a member of the Society for Psychical Research (a Cambridge-based organisation which included many intellectual luminaries of the day), and his interest in psychic matters had led him to try to connect them with archaeology. According to Bond (1978[1918]), he and a friend made contact with long-dead monks and received maps of the chapel via automatic writing. Despite his hope that the world was

²⁵ Time for another personal note to assuage the reader's probable curiosity: I have no particular beliefs either way about psychic or paranormal phenomena or ESP etc.. Once more, my interest here is simply in the structure of the discussion about psychic matters, as they pertain to archaeology.

finally ready to deal with such ideas, Bond was attacked on multiple fronts, both critiqued by those who disbelieved his claim to psychic knowledge (Wilkins 1922), and summarily dismissed by his employer, the Church of England, for dallying with the spirit world. Barred from the site and blacklisted from practicing archaeology, he moved to the U.S., wrote several more books about his communications from the "Company of Avalon" and other esoterica such as the Cabala, and died alone and ruined in 1945 (Rahtz 1993; Bligh Bond 1977[1917]). However, Bond's influence has continued, not only through a later generation of archaeologists experimenting with psychic methods, but also through the Earth Mysteries scene (Larkman and Heselton 1985:17) and through groups such as the Research Into Lost Knowledge Organization (R.I.L.K.O.), which has republished several of his works within the last twenty years.

Similarly, one cannot bring up psychic archaeology without mentioning T.C. Lethbridge, once a respected archaeologist, for the dramatic story of his falling-out with the Cambridge archaeological establishment in the late 1950s after a long and conventional career is legendary. His contention that he had found a new chalk hillfigure at Wandlebury, which he named Gogmagog, was the stuff of scandal, but it was just the beginning of a new career in researching and writing about diverse paranormal topics, such as ghosts, dowsing (particularly the more psychic aspects), and extraterrestrial intervention in human history (Lethbridge 1980). It can be safely said that mainstream archaeological interest in Lethbridge effectively ceased from any perspective but the voyeuristic, but his new ideas were adopted by others. As Clark puts it, regarding Gogmagog,

The Archaeologists may have abandoned Tom, (so serious was the split that Tom left the area), but the group which Archaeologist Glyn Daniel was to later call the 'lunatic fringe of archaeology' took him to their hearts. Even today, nearly forty years on, dowsers, ley-liners, occultists etc., keep his 'Goddess' alive. Some even look upon it as a fertility object and bring various offerings to lay reverently in her breast. (1997:9)

Indeed, Lethbridge was often cited as an influence in Earth Mysteries publications in the 1970s and 80s, especially with respect to his psychic theories and dowsing work (e.g., Larkman and Heselton 1985). (However, all of Lethbridge's conclusions are by no means accepted in all 'alternative' archaeology publications in the U.K.; for example, 3rd Stone

recently published an article once more dismissing Lethbridge's claim regarding the Gogmagog hillfigure (Clark 1997).)²⁶

On the other side of the ocean, another mainstream archaeologist who apparently had a stunning conversion in later life was Canadian J. Norman Emerson, a professor at the University of Toronto who trained many of the current generation of senior Canadian archaeologists. Emerson used psychics in his archaeological work in the 1970s, and reported on the results in a series of conferences and publications (Goodman 1977, Emerson ¹⁹⁷³ 1974, Jones 1979). Certainly other Canadian archaeologists experimented as well – I personally know of at least two who swore by map dowsing – but Williams (1991) suggests that it was Emerson's influence that resulted in the publication, shortly thereafter, of three full-length books on psychic archaeology by American archaeologists. Judging from the books, Bond and Edgar Cayce (of whom more in a moment) likely share the credit, as must the general rise in commercial psychic work, and the renaissance in parapsychological research in major universities which was then occurring, for example at Stanford, partly as a result of the Cold War (Berger 1988; Collins and Pinch 1982). Regardless, the appearance of these three works – Jeffrey Goodman's *Psychic Archaeology: Time Machine to the Past* (1977), Stephan Schwartz's *The Secret Vaults of Time: Psychic Archaeology and the Quest for Man's Beginnings* (1978), and David E. Jones's *Visions of Time: Experiments in Psychic Archaeology* (1979) – has been taken by critics as something of a watershed.

These authors show variations in their use of psychic information, but all three did place emphasis on it as an intuitive tool, the value of which could then be verified through conventional means. The extent to which this verification meets disciplinary standards is usually the point on which commentators object (e.g. Williams 1991), that is, when they can manage a semblance of dignified criticism. Schwartz and Jones took pains to situate themselves carefully in relation to the history and philosophy of science – Schwartz supposing hopefully that he was at the forefront of a Kuhnian paradigm shift. Jones, an anthropology professor who became convinced of the existence of psychic ability through

²⁶ There has been a fair bit of informal psychoanalysis of Lethbridge within the U.K. archaeological community, along these lines: Did he just finally lose patience with the straight-laced Cambridge contingent? (Clark 1997) Did he actually go insane? Did he succumb to the influence of famous archaeologist and witch, Margaret Murray? (!) I certainly don't know the reason for Lethbridge's apparent change of heart about the best way to approach the past, but do find it hard to suppose that he didn't mean a word of the several paranormal books he wrote in the years after the Gogmagog incident. See Daniel (1986) for another account of Lethbridge's departure from Cambridge.

a personal reading, wrote explicitly of his difficulty in reconciling this new research project with his background, only finding comfort in the idea that it was practical, applied research, and that *how* it might work was outside his remit (1979:4). Both Schwartz and Jones focused quite tightly on protocols for testing the results of psychic work, although their implementation has been thoroughly criticised (Feder 1996). The reception for these and subsequent works wasn't exactly warm – indeed, the backlash was vigorous in the reviews – and one who was at least open-minded about it eventually changed his stance, perhaps in response to this pressure. David Hurst Thomas originally included a short, noncommittal overview on psychic archaeology in his introductory archaeology text book (1979), even quoting Carl Hempel to suggest that the origination of a hypothesis is irrelevant given that testing is what counts, but dropped it from the second edition. Criticism notwithstanding, Schwartz and Goodman both continued their work, Schwartz with the Mobius Group and work on Alexandria, and Goodman as an author promoting the view of an extremely early occupation of the Americas (Williams 1991).²⁷

Other works in psychic archaeology with academic institutional links which are periodically cited include Karen Hunt's above-ground dowsing for impressions of historic buildings (Feder 1996), Maxine Asher's 1974 PhD dissertation on intuitive perception in archaeology, and the 1930s work of Polish clairvoyant Stefan Ossowiecki with academic colleagues, on psychometric readings of Palaeolithic artifacts (Williams 1991). In these cases, and those discussed above, there is a wide range of attitudes towards verification of psychic information, which defies easy categorisation. And to complicate matters, seeking verification of psychic impressions can blend into matters of fulfilling prophecy, and archaeology is by no means exempt from this either.

Edgar Cayce (1877-1945), also known as The Sleeping Prophet, is now one of the best-known psychics ever, renowned for his readings on topics ranging from faith healing to the past and future of mankind on Earth, especially our spiritual destiny. The Cayce-founded Association for Research and Enlightenment has over 40 000 members world-wide

²⁷ Goodman's later works have been none-too-popular either; Christy Turner, for example, suggested that *American Genesis*, with its claim that Native Americans have occupied North America for over 500 000 years, provides "an embarrassing example of the muddled and naïve results that can happen when mythology and evolutionary science are commingled", and relates it to the creationist attack on archaeology's borders (1982:72).

(Hancock and Bauval 1996:89)²⁸. His readings, published in hundreds of books, have influenced generations of mystics, as well as perpetuating some older traditions from Ignatius Donnelly, Theosophy, and Anthroposophy.²⁹ His readings on Atlantis and ancient Egypt are particularly of interest here; it was on the basis of a Cayce reading that English professor David Zink (1978) sought – and, he thought, found – Atlantis at Bimini, off the Florida coast. The archaeologist John Steele was involved in this project, and also, interestingly, in the work of the Dragon Project in the U.K. (Robins 1985).

Archaeologist Marshall McKusick wrote in 1984 in dismay of the adoption of Cayce's views by Zink and Jeffrey Goodman, suggesting that Cayce's influence should have run its course by that point. Williams (1991) would seem to concur. Both would probably be dismayed to know that Cayce's links to archaeology don't end there. As Graham Hancock and Robert Bauval, themselves now famous in this realm, put it, Cayce's "pronouncements concerning a supposed Atlantean 'Hall of Records' at Giza have quietly spawned a multimillion-dollar New Age industry that has embroiled itself deeply within mainstream Egyptological research into the Pyramids and the Sphinx" (1996: 86). This might be a little overstated, but the ARE has certainly been involved in funding archaeological research projects at Giza, and indeed, academic archaeologists have yet again been involved (Lehner 1996; Hancock and Bauval 1996). Mark Lehner, then a student supported by the Edgar Cayce Foundation, published a book called *The Egyptian Heritage: Based on the*

²⁸ From the ARE website (<http://www.are-cayce.com/whatis.htm>):

"The Association for Research and Enlightenment (A.R.E.), Inc., is the international headquarters of the work of Edgar Cayce, considered the most documented psychic of all time. Founded in 1931 to preserve, research, and make available insights from Cayce's information, the A.R.E. is a non-profit organization which sponsors activities, services, and outreach throughout the world. Many people contact us for information or research on a specific topic, only to discover that the Cayce readings explore more than 10,000 subjects."

²⁹ Theosophy was founded in 1875 by Madame Blavatsky, a renowned medium. It can be seen as an offshoot of Spiritualism, although Theosophy denies one fundamental tenet of Spiritualism, that one can contact individual spirits of identifiable people. Blavatsky published her manifesto, *Isis Unveiled*, in 1877, and followed it up with *The Secret Doctrine* in 1888. The latter was presented as an ancient text describing the creation of the universe, the development of the earth, and the evolution of humanity, and included tales of lost races, from Hyperboreans to Lemurians and Atlanteans. Blavatsky's work was followed up in the same vein by Scott-Elliot, who wrote in 1896 *of the Story of Atlantis* and in 1904 *of The Lost Lemuria*. Scott-Elliot detailed an alternative outline of human evolution beginning with nonmaterial spirit beings and proceeding through some quite monstrous forms. Like Blavatsky, he said his information was received clairvoyantly. Anthroposophy is the offshoot of Theosophy that was started by Rudolf Steiner. Steiner has many followers today, and the Theosophists are also alive and well; their Publishing House has been a notable producer of 'occult' literature.

Edgar Cayce Readings in 1974, seeking to demonstrate correspondences between Cayce's readings and empirical evidence at Giza. Lehner subsequently became a respected Egyptologist – Assistant Professor at the University of Chicago, no less – having gradually revised his opinions on the Cayce story, and has been a vigorous opponent of Hancock and Bauval's own claim that the Great Sphinx substantially antedates the Fourth Dynasty, and is a relic of a much older civilisation (Lehner 1996; Hancock and Bauval 1996). Lehner himself comments (in response to Hancock and Bauval's effort to distinguish between his 'conventional' and 'Caycean' periods) that beliefs on subjects like the Cayce readings can drift and transform over a lifetime, that lines between "orthodox" and other ideas are not easily drawn, and that the value of doing so is doubtful (1996:292).

Of course, there are other types of psychic archaeology, easily found through wandering the New Age section of any large bookstore. Mediums channeling information from benign Pleiadeans who have guided human evolution on earth are not exactly a rarity nowadays. The Psychic Questing movement in the U.K. is taking off, with annual conferences in London, and new information about King Arthur; not to mention group seances and psychometrizing which seem to lead inevitably to poking about at historic sites in the middle of the night, and the revelation of startlingly evil worldwide conspiracies traceable through historical esoterica (Collins 1991). (There is also a more sedate side to it, concerning past lives and tuning in to the landscape, and this has absorbed some of the Earth Mysteries contingent who are unhappy that the "mysteries" side of things has been disappearing from view.) There is also the older variety of popular psychic history and archaeology represented by *Window to the Past: Exploring History through ESP*, happily recycling its way through the occult sections in second-hand bookshops. This is a rather riveting work by best-selling ESP authority Hans Holzer – also known for his great 1978 work, *Elvis Presley Speaks* – who, working with medium Sybil Leek's impressions of various historic sites, believed he had uncovered new information about historical events ranging from the Lincoln assassination to the presence of Leif Ericsson in Cape Cod to the real location of Camelot. Verification, though welcomed when it incidentally occurred, was not considered a concern by Holzer, for he took as given that the impressions left on objects and places by intense emotional experiences could be accurately psychometrized by gifted psychics.

There may be much more of the latter kind of psychic archaeology around, but one cannot dismiss psychic archaeology overall as merely a 'pop' or amateur phenomenon on that

basis. From the examples given earlier in this section, it must be clear that there is not just a continuum but a certain consistency with the case of dowsing. However much some might wish otherwise, the encounters of archaeologists with matters psychic have most certainly not been restricted to debunking. And if one bears in mind that for every published statement from a professional or academic archaeologist in favour of psychic methods, there are probably a good few more archaeologists out there with psychic sympathies who aren't willing to admit it in print (academic suicide, anyone?).... it then becomes difficult to suppose that it is a nonissue. It seems distinctly possible that the use of psychic methods in archaeology is not *only* a means of arguing to desired and bizarre conclusions using invented authority, as some critics would have it (e.g. McKusick 1984). Nor is it clear that all psychic archaeology is attributable to influences of key individuals like Cayce and Bond. Nor is possible to ascribe the advocacy of psychic methods *only* to midlife crises or idiosyncrasy in archaeologists under great mental or personal strain (Williams (1991) makes the latter excuse for Emerson; the same has been suggested for Lethbridge).³⁰ No doubt these factors can come into play, but they are not simple explanations which set psychic archaeology apart from the ordinary, for teleological argument, strong devotion to the work of unsung heroes, crises in faith, and excessive credulity are not exactly unknown elsewhere in archaeology. When all is said and done, use of psychic methods in archaeology may also be a natural outgrowth of the drive to explore the past using any means possible, of an affirmation that human intuition and experience are appropriate mediators for historical knowledge, and of entirely logical

³⁰ A detailed analysis of explanations offered regarding archaeologists who depart so substantially from the party line is beyond the scope of this chapter, but I would like to make two preliminary observations, based on informal conversations and reading. First, they are sometimes seen as being *converted* by the influence of flaky, irrational, yet persuasive personal contacts – usually women. This could be called the ‘Respected Scientist Becomes Infatuated with Trashy Medium and Dumps Good and Faithful Lady Reason’ model, seen also in the Victorian era history of paranormal investigation. Colleagues seem to feel a need to apologize for them that is quite disproportionate, often stronger for example than responses to fellow academics’ flagrant violations of ethical codes, if of a standard or accepted nature (involving, say, money or sex or politics). Thus, once again, we can surmise that a great deal is perceived to be at stake, including disciplinary authority as well as global security. Second, claims regarding another’s sanity during ‘border disputes’ of this kind can take a curious but powerful form at times, i.e. Only crazy people believe Proposition P; Mr. X believes P; Therefore, Mr. X is crazy. The bit of the equation which is suppressed and assumed to be beyond contention is that Proposition P is indeed a crazy thing to believe. The equation can be amplified by the presence of another suppressed premise – that only crazy people would question that only crazy people believe Proposition P. This is a nifty circularity which can effectively rule out debate.

questions about where lines should be drawn in the complex relationship of evidence to story.³¹

Like dowsing, psychic archaeology can be defined away, and its professional supporters only mentioned in special articles or books on aspects of 'fringe' archaeology (e.g., McKusick 1984; Williams 1991; Feder 1996). One can take psychic archaeology's highly varied proponents and theories out of their individual contexts, lump them together on the basis of their 'deviance' from the norm, tar the moderate and genuine by association with the extreme or the fraudulent, and thus effectively write them all out of the discipline. And so one might, if one has a specific, previously held ideological reason for doing so. But this carries a price, the price of alienated understanding of how some archaeologists really think. Not only does it ensure that current believers will stay even further 'underground' than dowsing supporters, but it is revisionist in its decontextualism, and thus inhibits our understanding of how archaeology works as a social and personal enterprise, its significance in the world, and the hopes which drive people to devote their lives to it. And finally, this sectioning off of archaeologists with curiosity about, or allegiance to, psychic methods denies their place within intellectual history by ignoring the reality that the dispute about extrasensory perception or paranormal knowledge is an old one, not yet resolved, in which many scientists have been, and continue to be, involved (Collins and Pinch 1982; Oppenheim 1985).

Conclusion

In conclusion, this chapter has argued that while there are some constellations of belief regarding dowsing and psychic methods – subjects some deem to be 'fringe' or 'pseudoscientific' – these do not lend themselves to a simplistic dichotomy of orthodox vs. alternative. In these cases, this division has little social or logical reality. One can prescribe such a division or make normative claims about what should or should not be acceptable, as most commentators on 'fringe' archaeology do, but these prescriptions ultimately seem

³¹ With respect to the latter – there may be little dispute that artifacts exist, but what one makes of them is another question, not only in the matter of their immediate interpretation but also in the problem of creating a meaningful synthesis, a history. And, indeed, there has been long and vigorous debate over what constitutes a historical fact, and if history exists outside of the mind of the historian in any real sense; these are perennial questions of the philosophy of history, after all (Jenkins 1995).

ideological in nature, often rhetorically ill-justified, and potentially harmful to our understanding of the spectrum of beliefs about the past. Taking a step back can permit us to see something of the force of history, the fluidity of boundaries, the difference between what some archaeologists say and others do, and something about the criteria used in theory and in practice to distinguish the legitimate from the illegitimate.

Chapter 5

Conflicts, Commonalities, Communities:

Earth Mysteries and Archaeology

Early dissertation outlines can be truly hilarious, and mine are no exception. I have been writing, and discarding, versions of this chapter for several years now. Since this dissertation has ultimately turned out to be about learning how to approach ‘alternative’ archaeology constructively, the present version deals in part with the failings of earlier versions, in the hope that those failures are themselves illustrative of some of the subject’s challenges.

As mentioned in the Preface, this project began with the problem of choosing between archaeological interpretations, and conceptions of evidence and inference. When I first heard of ley hunting, and read *Ley Lines in Question* (Williamson and Bellamy 1983), it seemed that this would be an excellent case study for tackling the problem head-on, for it appeared that there were two parallel discourses about the past, often conflicting, rarely intersecting. In time, however, it became clear that: (a) I was about as qualified to assess the actual arguments’ content as I would be to properly assess Graham Hancock’s version of Egyptology – i.e., not qualified, and not situated to become so within three years; and (b) a simple analysis of the arguments’ *forms* in comparison to those of their competitors in ‘orthodox’ archaeology would have little purpose, given that the social context and informal components of the debate were crucial to its understanding. Thus, the nature of the research shifted to an examination of the relationship of the research communities themselves. This, it may be appreciated, is a very different matter from textual analysis, and brought its own challenges and limitations, some of which will be discussed below.

From ley hunting to Earth Mysteries

Ley hunting as a group activity began in the 1920s with The Old Straight Track Club, who quickly got off to a bad start with the archaeological establishment. Ley hunting lost

popularity in the 1940s as most British people were preoccupied with more pressing matters, but had a major resurgence in the 60s and onwards, as part of the more diversified movement of Earth Mysteries (Heselton 1995; Pennick and Devereux 1989; Hayman 1997).

In the words of one Earth Mysteries publication,

Earth Mysteries is an holistic approach to the study of ancient sacred sites, a left and right brain synthesis, a systems based view which draws together into a coherent whole picture the seemingly disparate strands of archaeology, geographical and astronomical alignments of ancient sites, landscape lines, little-understood energy effects to do with both human consciousness and the earth, UFOs, dowsing, astrology and ancient astronomies, ancient history and folklore, the geometry of megalithic and sacred structures, virtual and symbolic landscapes, anomalous and paranormal phenomena and the art and science of 'being and seeing' at sacred sites.

- masthead from *Gloucestershire Earth Mysteries* magazine, 1993

Earth Mysteries (or EM) has recently been flourishing in Britain, as researchers and enthusiasts of every imaginable kind, from local historians to folklorists to astronomers to ufologists to psychics, contribute to EM publications and participate in EM conferences, etc. EM has links to different forms of neopaganism, but the two realms do not coincide perfectly, and agnostics, Christians, Buddhists, and those of many other religious persuasions also participate.¹ Although EM has often focused on sites and aspects of

¹ Wright (1999) relates the following results of a survey of *Northern Earth* readers: 2/3 of respondents were male; 2/3 were 30 – 50 years old; there were Christians, pagans, and agnostics each in substantial fractions; there were academics, manual workers, technical and arts workers; there were equal numbers of degreed and non-degreed readers. As Wright puts it, "Trying to pin a typical [reader] down is a bit like trying to photograph a ghost" (1999:23).

It has occasionally been wondered where the women are. Hilary Byers (1984) suggested that women tend not to be involved in Earth Mysteries field research because when they were young, they didn't have the same freedom to ramble and explore the countryside as their brothers did. Jill Smith and Monica Sjöo (1994) expressed concerns that it might be altogether more political; however, Helen Woodley countered that although she appreciated the general problem of disempowerment, she did not see the ley system as a patriarchal construct. Jo-Anne Wilder (1994) argued vigorously that although "women are conspicuous by their absence in this subject", she has experienced no discrimination and personally would rather avoid the insecure anti-Patriarchal crystal-wearing Goddess worshippers. There have, however, been outbursts in the Readers Forum pages of *The Ley Hunter* (e.g. issue 124) from women in the larger EM community who were concerned, to put it mildly, about what they felt to be male biases both in archaeology and in the pages of *TLH*.

My own experience in EM involved contact with many more men than women, but this surely reflects my entry point into the literature and the community – had I started with *Meyn Mamvro*, long edited by Cheryl Straffon, self-described as a "Goddess-celebrating pagan/e.m. writer and feminist" (1995:27), or one of its events, rather than *The Ley Hunter*, this might have been different. For a detailed analysis of the Goddess and her long and varied career in archaeology, see Hutton (1997).

landscape which are assumed to be sacred in some way, the ideas put forth are of both secular and religious natures. Often, research is personal and local; veteran EM researcher Philip Heselton characterises worthwhile activities for beginners as including “exploring their local area, rediscovering ancient sites, unearthing legends from library shelves and talking to local people who still have much knowledge and wisdom to give” (1995:103).

The outline of the juicy history of archaeology’s relationship with EM can be sketched something like this: *Antiquity* editor O.G.S. Crawford refuses to print an advertisement for Alfred Watkins’ *The Old Straight Track* (1970[1925]); *Antiquity* editor Glyn Daniel refuses to publish an advertisement for *The Ley Hunter* (then edited by Paul Screeton) and starts labelling ley hunters as ‘the lunatic fringe’; archaeologist R.J.C. Atkinson (1981) addresses the subject directly in measured but critical tones in *The Ley Hunter*; Tom Williamson and Liz Bellamy deliver *Ley Lines in Question* in 1983 to the applause of archaeologists and criticisms of ley hunters; but minor rapprochement on alignments is achieved in 1989 with the publication of a paper by Paul Devereux (editor of *The Ley Hunter*) in *Antiquity* by the new editor Christopher Chippindale. (See Hayman 1997 for more detail.)

This is far too easy, however, and makes the mistake of relying only on the published comments of a few individuals.

A Canadian archaeology student encounters Earth Mysteries

This was a festival of possibility! I excitedly began with immersion in the varied Earth Mysteries literature and related topics.² I felt I had read enough background and started to

² Earth Mysteries literature is abundant but not always easily available, at least not until one knows where to look. Most university libraries don’t have much of it. Cambridge University Library, being a copyright library, does have a substantial number of journals – though often incomplete sets – and of course most of the books. Key journals (not all extant) include: *The Ley Hunter*, *3rd Stone*, *Gloucestershire Earth Mysteries*, *Northern Earth*, *At the Edge*, *Meyn Mamvro*, *Mercian Mysteries*, *Markstone*, *Touchstone*, *Caerdroia*, *Journal of the British Society of Dowsers*, *Fortean Times*, *R.I.L.K.O. Journal*. Other sources of literature include local EM publishers – whom one can trace through the journals – the book tables at EM conferences, esoteric bookshops, and EM journal editors themselves. I am particularly grateful for the assistance of editors Bob Trubshaw and John Billingsley, who shared extra back issues of a variety of publications with me. For earlier ley hunting history, there are also the Old Straight Track Club archives in Hereford, but I have not used them.

actually meet Earth Mysteries participants in the autumn of 1995, and continued contact with some after leaving the U.K., up until the time of writing. My research included conference attendance and participation, the survey in Appendix A, further reading, and informal interviews, correspondence, and field trips with EM writers, researchers, and enthusiasts.³

In 1994, I was starting from a position of near-complete ignorance. I had studied some British archaeology in Canada, but was not well-acquainted with the bodies of archaeological scholarship, past or present, most relevant to this topic.⁴ I knew little of the landscape either from a technical perspective or from an experiential perspective. The only megalithic site I had personally seen was Stonehenge, and I couldn't have told a long barrow from a lump. I didn't know what the different heritage-related institutions were or how they worked (what SMR, IFA, PPG16, or an archaeological 'unit' were), the first things about field systems, or medieval churches. I was also unaware of many other factors that were most relevant to understanding how living British people interacted with each other, as well as with the past. I didn't know what different accents typically signify to people, what 'New Labour' was, didn't fully appreciate the real and perceived differences between different kinds of educational institutions (from 'public' vs. 'private' schools to Oxbridge vs. redbrick universities vs. polytechnical schools), didn't know what a 'traveller' was, had never heard of 'The Battle of the Beanfield'. I was unaware of the long and venerable history of amateur antiquarianism and the popularity of archaeology as a hobby in the U.K., and didn't know about traditions of rambling in the countryside. I knew but dimly of dowsing, and had scarcely heard of crop circles, let alone earth zodiacs or Morris dancing. The concept of sacred landscape was entirely new to me. And finally, I knew absolutely nothing of paganism, ancient or modern.

³ In general, I took pains to ensure that people knew why I was there at EM events, because it would have been most unpleasant for them to later realise that they were being, in some sense, observed. However, I was quickly and congenially accepted by most – only two ever took me to task for the failures of the orthodoxy which I represented – and people spoke freely, easily, and generously with me. It may have helped that I was comparatively young, female, Canadian, genuinely interested, and obviously non-combative in intent, despite being an archaeologist. In the end, I apparently 'blended in' well enough, for at the 1997 Avebury Moot, I was approached in the crowd by a BBC radio reporter who wanted me to talk about my beliefs regarding leys.

⁴ I might add here that the understandings I did have from the literature were quickly challenged by the realities on the ground; in much the same way, the representation of British postprocessualism by American doctoral students VanPool and VanPool (1999) may be literally accurate, but is nonetheless now scarcely recognisable to me.

I might as well have been from the moon. This essentially outsider status may have helped – for example, it meant that I could approach the EM literature simply with curiosity, rather than the immediate visceral reactions which some archaeologists apparently experience. Similarly, it meant that I didn't begin with the reverence for some key figures in British archaeology which I would have had, if I had previously studied their work or been their student, and it meant I experienced some of U.K. academic culture from an participant observer's perspective. So in some senses, the baggage which I brought to the project was light, and this helped. But in other senses, I was simply ill-equipped.

My reason for including this background here is that it reveals *precisely* the kind of mistake which is easy for a starting researcher to make, when working across disciplinary and cultural lines on 'fringe' topics; that is, the assumption that something which initially appears straightforward and easily characterised actually *is* reasonably straightforward. This assumption usually derives from secondary sources in one's own field. A good many Earth Mysteries writers have been studying the British landscape for over twenty years now, and the supposition that I might be able to parachute in and begin to evaluate their ideas with only a quite rudimentary knowledge of British prehistory was classic academic arrogance, and didn't last long once I actually read some of what they had written. (And this was strictly in terms of the archaeology, never mind the folklore, the mythology, the history, the astronomy, and the literature in geomancy and the occult.) Would I have entertained, for even a moment, the notion of looking at Earth Mysteries in relation to archaeology for a Ph.D., if my first encounter with the field had been through an EM work like Pennick and Devereux's (1989) *Lines on the Landscape*, rather than through the debunking archaeologist's lens of Williamson and Bellamy's (1983) *Ley Lines in Question*? Probably not – I might instead have realised that the topic was too complex, better suited to someone with previous years of experience in the actual subject matter of British archaeology *and* its social context. There are a host of other 'might-have-beens' which also illustrate potential pitfalls for academic researchers approaching the topic of alternative research communities. If I had gone to Earth Mysteries looking for irrationality, or characteristics which fit a model of 'cult archaeology' (e.g. as outlined by Cole 1980), I probably would have found just that. If I had approached EM assuming that it was *all* best understood through a literal, empirical filter, much of it *would* have seemed 'lunatic'. If I had only thought about EM for six months instead of fifty-six months, as

surely some commentators on ‘fringe’ matters do, I would likely have found it difficult to shed preconceived notions and arrive at a new understanding.

And so, there is a social history to be written, but it is not for me to write. Boxes of notes notwithstanding, all I can do here is provide some distilled observations, gleaned from the literature, encounters with archaeologists, attendance at Earth Mysteries conferences and from discussion with Earth Mysteries researchers and enthusiasts. They will not do justice to the topic of EM as related to archaeology, but they are a beginning, which hopefully demonstrates that the situation is complex and evolving, and that caricatures designed to support *a priori* theories about orthodox-alternative relationships are to be avoided.

A Social History?

Of his own invective, Glyn Daniel noted in his memoirs that “from time to time [my] gentle ridicule has moved to sarcasm and scorn” (1986:406); but context is everything. This scorn he mentioned in reference to another archaeologist’s refusal to rethink Elliot Smith’s hyperdiffusionism, and to archaeoastronomer Professor Alexander Thom of Oxford, of whom Daniel regretted that after some amiable association, he “made an enemy.” He wrote, too, of his sadness at having fallen out with Tom Lethbridge, but was comfortable assessing their split as the result of Lethbridge exemplifying the phenomenon of “the scholar who goes berserk” (1986:405). In all, Daniel’s memoirs reveal someone who took archaeology seriously enough to lose friends over, and yet felt that he was engaged in a game of sorts, with rules of conduct that could be bent: for example, he wrote of Sir Mortimer Wheeler that the man was “admittedly, an egoist and satyromaniac, and often behaved very badly – who doesn’t?” (1986:408). Did Daniel behave badly in his interactions with ley hunters or, as he often called them, “the lunatic fringe”? Probably, at least some of the time. Certainly a more even-handed tone in his editorials would have helped avoid some animosity which developed in publications from each side, and lasted a long time. But without detailed interviews with a whole host of relevant people on both sides of the fence, the all-important informal interactions will remain out of the picture. And whenever I asked directly, it seemed that there were regrets and old resentments all round, but that people on both sides would prefer to let what is now a bygone conflict stay bygone.

The same is true of what is known to many as ‘*The Ley Lines in Question* fiasco’. Archaeologists applauded the book – the first full-length archaeological commentary on leys – but then, of course, they would generally not have seen either the works Williamson and Bellamy commented upon, or the reviews of the book in EM publications. In EM circles, the memory of what they felt to be unfair commentary did not fade quickly or entirely. For example, Tom Graves (1988:2) complained in passing that his “descriptive analogies” were taken literally in “Williamson and Bellamy’s childish games”; Trubshaw (1996, note 2) alluded to Williamson and Bellamy’s misquoting of a statistical argument concerning leys.

It would be easy enough to trace the paths of barbed comments in publications, or to muckrake by taking statements from participants on each side about what they think now of these past episodes of conflict, but the point of doing so is doubtful. And a worthwhile social history of the relationship of ley hunting with academic archaeology involves historical contexts which are complex, alien to me, and not quickly understood, and thus a sensitive discussion would be a far greater undertaking than can be accomplished here.

Communities

It is commonly supposed by those not personally involved in British prehistoric archaeology that ley hunters and archaeologists have nothing to do with one another. This is not the case. More than one professional archaeologist got their start in ley hunting. And when one actually attends Earth Mysteries conferences, it is not uncommon for academic archaeologists to be there. The two groups *do* communicate. This illustrates another one of the potential pitfalls of looking at ‘alternative’ archaeology through literature alone, or on a superficial level without a good deal of discussion with those involved. The literature is, overall, biased to give the impression of conflict and estrangement, and without less formal information regarding interactions, that bias can remain uncorrected. Similarly, it takes a good deal of familiarity with both ‘scenes’ to understand the significance of the occurrence of a name in a conference lineup.

So the communities do touch. But they also have some interesting similarities in their relationship with the material world.

Edmonds (1996) works through in detail the premise that stone tools had social lives, and played a role in identities between the point of manufacture and final deposition. He writes of pasts and flint alike as things worked and reworked (1996:189). The same is true of prehistoric monuments, for the meanings which grow up around them have lives, and this is no less true in the present than it presumably was in the past (Holtorf 1998).

A large part of the identity of EM enthusiasts seems to develop as does the identity of the archaeologist; that is, in explicit relation to archaeological sites. True, conferences and literature and abstract belief systems are also structuring principles – as for the archaeologist – but for many, what consolidates them within EM is the institution of the group field trip.⁵ These experiences are social as well as intellectual and sometimes spiritual, and thus bonds are forged in ways similar to the camaraderies between archaeologists that are born of time spent together excavating, surveying, or fieldwalking. This is clear both from events organised by regional groups, and from the long-running collective research efforts of the Dragon Project, which brought many people together in its investigations of anomalous energies at prehistoric sites (Robins 1985). The cementation of EM identities around the chief site studied in the Project's early stages, the Rollright Stones in Oxfordshire, was sufficient that when Rollright came up for sale, the Dragon Project mounted an extensive fundraising campaign, to which members past and present contributed (*TLH* 128). (Others with links to the EM community were also attached enough to the site to counter the bid.) Rollright and the Dragon Project are not unique in this. Favourite sites have often served as longterm foci for the attention of EM groups. And in this, there is a certain symmetry, for whether or not the electromagnetic forces the Dragon Project sought actually exist at those sites, the stones themselves continue to be sources of energy in their own way.

⁵ This is something which may be rather taken-for-granted by the British archaeologist and EM researcher alike, in the same way that they breathe air, eat, *etc.* However, it is not the only way of forming an identity in relation to the archaeological past. For example, I initially came to archaeology through words and books, more than things or places. Southwestern Ontario has some remarkable archaeology, but very little in the way of monuments that physically and visibly intrude from the past into the present, in a fixed location. The interested public and schoolchildren tend instead to encounter the material of prehistory through pots and points and carvings in museums. Thus the past is explicitly dislocated; they do not travel to it, but rather, it travels to them. Similarly, avocational enthusiasts in SW Ontario primarily derive their identities from their collections (e.g. of points) rather than from the sites where those artefacts were found.

The dedicated 'amateur'

The majority of EM researchers, authors, and publishers do other things for a living. Very few have had much material gain from their writing. Those who produce the journals have traditionally done so as a labour of love, not infrequently losing money in the process. Many live far from research libraries but make it a priority to find new reading material whenever they can. They bring a wealth of backgrounds, from folklore to local history to religion to engineering to just about anything else, to bear upon the question of how people in the past lived and thought. They develop their ideas over years, do original research, and push themselves to learn more. EM enthusiasts spend weekends and holidays visiting archaeological sites, enjoy some popular archaeology magazines and television programs, and look forward to events where they can hear people talking about archaeology in accessible terms. A few make a point of attending academic conferences, and keeping up on the latest archaeological literature, even though this is costly and time-consuming and brings no reward other than sated curiosity. Many carefully monitor the condition of local monuments, watch for vandalism, and pick up garbage left by others; indeed, there is a code of great respect for archaeological sites, and it is a point of honour to do no damage. A substantial proportion lean towards belief systems which are centred upon egalitarian principles and a deep respect for the environment. Some discover new sites, diligently reporting them to the local archaeological authorities. As well as a national network, there are longstanding EM regional groups who have for decades traded ideas about the past and present, shared the experience of going on field trips, and welcomed new members. Generally, in my experience, such groups are nonexclusive, do invite archaeologists to participate by lecturing at meetings, and provide respectful if sometimes questioning audiences.

And yet, some British archaeologists currently find them worthy only of derision or silence (see Appendix A). Where have they gone wrong – in having ideas of their own? In sometimes framing those ideas within 'strange' paradigms? In expressing those ideas in imperfect academes? In admitting anecdotal evidence and personal experience into their theories? In seeing the landscape subjectively? In taking offense quickly when their ideas are challenged?

I think these have all been contributing factors, but of course there are two sides to each of them. It is hard to suppose that the EM community alone bears the blame for the often poor relationship.

Damnation

As Bob Skinner (1990) observed of Fortean phenomena, there are a host of effective strategies through which a body of research/literature can be dismissed without really engaging with it, some more dignified than others. Each tends to become amplified through repetition, and this is certainly the case with EM in archaeology. True, it is not necessarily easy to find EM publications to read for oneself, if one does not already know where to look – and academics quite often lack a good sense of where to find literature that isn't in university libraries, not to mention the incentive – but nonetheless, the situation seems to be one where strong opinions about EM are held by at least some archaeologists who have never read any (see Appendix A). Of course, some probably do have reasoned objections, but that is a separate matter; I am not in a position here to canvass opinions to evaluate the validity of every theory ever put forward in EM, but merely want to comment on some strategies used for its dismissal.

I do not think it worthwhile to dwell further here on the problem of derogatory labels; if the reader does not yet suspect that the term “lunatic fringe” is profoundly troubling, then this dissertation is doomed. I would rather briefly discuss three other strategies that archaeologists use to avoid worrying about EM. (Bob Trubshaw (1996) offers some reflections on a similar theme.)

(a) It's not really about the archaeological past and thus has nothing to do with us.

This takes a host of forms, but usually relies on generalisations about the groups under discussion, which suppose that it's all about religion, or all about political counterculture, etc.

Are Earth Mysteries researchers and enthusiasts all New Agers? Hardly. On the contrary, some vocally distinguish themselves from what they perceive as New Age nonsense (e.g.

Graves 1988). Are all EM participants pagans? No. Is all EM just about spirituality? No – there is unquestionably a strong spiritual side to it (e.g. Heseltun 1995; Michell 1989; Dames 1992; Meaden 1991), but this is not easily separated from the direct references to the archaeological record, and there has also been a good deal of attention to simply empirical, historical, and literary matters. ⁶

In another variation on this theme, Michael Morris attempts a deft sidestep, by suggesting that alternative archaeology is politically driven by a Golden Age mythos, and that this in itself grounds for ignoring and dismissing it: “Thus for example we can see the power of alternative archaeology, yet at the same time reject its legitimacy as a view of the past” (1988:82). Curiously, he acknowledges that some mainstream views of Aegean prehistory have been politically coloured as well, and even generalises that “models and perceptions about the past are embedded in contemporary ideology” (1988:82), but his initial statement seems to imply that somewhere there is a privileged, apolitical, neutral past to be had. Of course alternative archaeology reflects contemporary preoccupations – indeed, Graham Hancock’s latest, *The Mars Mystery*, is as surely evidence of that as John Michell’s *The Flying Saucer Vision* – but this is hardly a characteristic which orthodox archaeology does not share (Bintliff 1988). ⁷

⁶ Yes, there are works out there which bear more hallmarks of modern religion than of archaeological scholarship, and some of these must be judged as being of low academic quality, if such judgements are undertaken. On this matter, however, the most sensible comment I have read from an ‘orthodox’ commentator is this statement on the many UFO books which appeared in the 1960s:

These books may be pitiful stumbling efforts in the morasses of technical and historical scholarship, and rather too much *tours de force* for pet ideas. But as religion they are worthy of respect as picture-language wrestlings with the deep matters all persons face – or evade – in the stillness of the heart. On this level their scientific and historical failings may not matter so much. In what pertains to the ultimate beyond the circles of science and history, all language is picture-language only shadowing what is beyond words. It is, in the old Zen phrase, the finger pointing at the moon. (Ellwood 1971:125)

⁷ If one wished, one could also dismiss EM by association with scary ideas, but I’m guessing that archaeologists don’t often know enough about the topic to try this one. But certainly, there are negative associations to ideas that appear in Earth Mysteries contexts; for example, geomancy had Nazi aficionados. Similarly, Golden Age themes have been linked to polar myths, which in turn are tied to Nazi ideology (Godwin 1996). But Godwin also shows that theories of Arctic and Aryan races developed in mainstream scholarship, and if in general we are going to engage in tarring people by association, then anthropology and archaeology are also highly vulnerable. (How many degrees of separation ever keep atrocities and ideas apart?) Recently, Thomas (1996) has argued that it is too easy to simply dismiss Heidegger’s philosophy on the basis of his Nazi associations; archaeologists, he says, are wiser to look at it, but with careful attention to possible effects.

(b) It's nothing new, we were there first.

Ley hunters were obsessing about the intervisibility and interrelationship of sites in the 1920s, as well as people's experience of the landscape, decades before orthodox archaeologists in Britain concerned themselves much with such things. As Hayman (1997:220) puts it, "Watkins, Michell and their followers are studying not ruins in the landscape but a landscape in ruins." Landscape archaeology in Britain only emerged in the latter half of this century (Thomas 1993). An academic emphasis on ritual, social, and symbolic landscapes in Britain came later still (Barrett 1994; Tilley 1994).

All of this belies archaeologist Peter Fowler's acerbic evaluation of Pennick and Devereux (1989):

'Earth mysteries' is not opening up new fields here as it seems to think it is; rather, it is somewhat naively blundering into the well-founded discipline of landscape archaeology. Nevertheless, there is much common ground. The authors indeed adumbrate 'an approach to the past that encompasses many disciplines: a "general systems" or "holistic" approach to the study of ancient sites and landscapes....'. In view of their praiseworthy scientific aspirations, they might note that there is already a scientific discipline relevant to their needs in terms of research and respectability. It is called 'archaeology'... They know what the rules of the archaeological game are, however, and the decision must be theirs. It is a straight choice. (Fowler 1990:194-5)

Ignoring the fact that ley hunting arguably precedes landscape archaeology, dismissing the possibility of the use of alternative paradigms, and offering little substantive comment beyond these assertions of disciplinary borders, Fowler – making it clear that he feels that it is generously communicative of him to review the book at all – refers the authors to Williamson and Bellamy (1983), as though they hadn't read it.

(c) It's all been criticised before, and rightfully dismissed, and doesn't need further attention. The proponents of these ideas cling to them out of cranky dogmatism and lack of imagination and the absence of a will to improve themselves through study.

This is the kind of attitude towards EM that is difficult to document in publications, of course, because those who hold it tend not to write about it. However, it certainly surfaces in informal discussion with archaeologists about EM, and allusions to the idea surface

occasionally in academic archaeological writing; e.g. “Some consider that any admission that we do not know the truth will give the upper hand to the creationists, UFOlogists, ley-liners and other groups who believe unshakably that they have privileged access to the truth” (Parker Pearson 1998:684). So, do EM researchers spout the same ideas year in year out, never testing them, never doubting their veracity?

Not in general. Of course there are some individual enthusiasts like that, and there is tolerance for them within EM. After all, there is no formal peer review apparatus, not everyone involved actively conducts research, and some are far more concerned with experiencing the landscape and enjoying the company of other EM enthusiasts than with reading at the library. But equally within EM, there are lively and critical debates, active attempts to test hypotheses and gather new information, and a will towards the improvement of understanding. Harte (1997^b) provides a comprehensive bibliography of some relevant work since 1990.

But going back a bit further, the Dragon Project again provides a good example. Its specific purpose was to investigate the properties of stone circles and megaliths in Britain, because it was widely felt among Earth Mystics, both on the basis of folklore and on personal experiences at such sites, that there was something special not only about the locations, but about the stones themselves. Indeed, there is a veritable cornucopia of folktales in Britain about stones that walk, drink water, defy counting, resist removal or relocation, dance, or have magical effects upon human visitors, and these taken in conjunction with anecdotal evidence from modern individuals’ own experiences, seemed enough to warrant closer examination. This was duly undertaken, using a wide variety of methods, long-range planning, and a good deal of volunteer labour (Robins 1985; Devereux 1990).

Similarly, the ley hypothesis has transformed significantly since its original formulation. Its evolution – and in some cases, near-abandonment – is easily witnessed in the pages of *The Ley Hunter* over the last twenty-odd years, in *At the Edge*, or by cross-reference between books like Watkins (1970[1925]), Hitching (1976), Devereux (1994), and Stone (1998). Some within EM no longer accept traditional ideas of leys in any literal sense. For others, there is still a wide range of allegiances on the matter; as Billingsley (1999:25) reports of Northern Earth readers, “Only 15% believed leys are spirit/shamanic paths, as against 42% energy lines, 31% trackways – oh dear.” However, it is clear that views on

leys within EM are not monolithic and static. Ultimately, some of the most critical voices on the different hypotheses certainly come from within EM itself (e.g., Stone 1998), and very astute assessments of the general history and phenomenon of ley hunting are also to be found in EM publications (e.g. Harte 1997^a; Pennick 1997). It is obvious, however, that the matter of leys is still touchy for both orthodox and alternative archaeologists (Hutton 1996).

It is hard to know how long a belief in some form of ley will last, given the radical transformations in the theory of late, and the harsh criticisms from many who themselves once took pencil and ruler to maps. The term 'ley' certainly may fall out of use; 'alignment' is now preferred by most. Further, the run of *The Ley Hunter* magazine, it would appear, is at an end. Started in April 1965 by Philip Heselton and Jimmy Goddard, then edited in turn by Paul Screeton, for twenty years by Paul Devereux, and most recently by Danny Sullivan, its last issue is going to press as I write. There are no plans to hand it over to a new editor (Danny Sullivan, pers. comm.). The new dominant force in archaeology-based EM magazine publishing will, it appears, be the professional, attractive, and substantial 3rd *Stone*, which last year combined forces with *At the Edge* with the mission of providing articles from alternative and academic contributors alike.

Monumental Developments

Other things have changed over the years. Stonehenge provides an excellent example, as it has long been a locus of contestation of imperatives in English society (Bender 1993). Christopher Chippindale, former *Antiquity* editor, Cambridge archaeologist, and eminent Stonehenge authority, takes an active interest in popular culture issues surrounding Stonehenge, and openly wrote of respecting modern Druids' wish for access, supporting them for being "at least authentic in knowing Stonehenge to be a sacred place of mysterious and superhuman power" (1997:10). Further, though admitting that he finds New Age ideas about Stonehenge to be strange, he points out that they are no stranger a way of seeing than the heritage manager's financial calculations of the site's worth. This is a far cry from the statement of Glyn Daniel, *Antiquity* editor and Cambridge archaeologist, nineteen years previously, about "bogus Druids" and "modern Druidic follies", strongly advocating that the "notably spunkless" Ancient Monuments Board and Department of the Environment "should very properly regret that permission is not forthcoming" (1978:177).

Subjective Gains?

Embracing subjectivity in archaeology has not exactly exploded in popularity, perhaps because one of the greatest fears of some archaeologists, and thus one of the most vociferous criticisms directed at post-processualism, is that we thereby lose the ability to differentiate between justified archaeological interpretation and outright fabrication. For example, Renfrew wrote that Shanks and Tilley's "proposed rejection of the methods of scientific enquiry... [means that] there is nothing to distinguish the research which they would produce from the most fantastical assertions of the lunatic fringe about flying saucers, earth magic and corn circles" (1994:9). Of course, that this upsets Renfrew so much is of interest in light of the broader intentions of this dissertation; however, my point here is that (while I reject Renfrew's judgement here about the implications of relativism) epistemology in archaeology has indeed been pushed so far that the lines *are* being seen as increasingly blurred. This should not be cause for alarm, however, but for satisfaction that we are coming to understand the process of interpretation and may soon be in a position to value the archaeologist's imagination as a research tool, rather than reviling it as a contaminating influence.

In trying to reconfigure archaeology, and exploring other modes of thought with less demarcation between subject and object, it seems probable that archaeologists could benefit from the wisdom accrued by other traditions concerned with understanding the past. Those affiliated – however loosely – with the 'New Age' have often been considered by archaeologists to be pursuing knowledge of a different kind, knowledge belonging to a domain which has no business being mixed up with the knowledge produced by archaeology (Meskell 1995). It has been suggested that those involved in alternative archaeologies are looking for a different kind of truth, that they are on a quest for higher meanings and guiding forces in the cosmos rather than information about the past.⁸ This categorisation is easily

⁸ This has often been assumed by the 'orthodox' and 'alternative' alike. Answers to the questionnaire I distributed at the Ley Hunter Moot and at TAG often showed this characterisation. For example, one university archaeologist (male, 30-39), when asked whether the theories and methods of 'orthodox' and 'alternative' archaeology are compatible, answered:

"Exactly parallel to biologists vs. creationists. These are different belief systems. One explains the world well and has good predictive value; the other is effectively a religious belief and can have spiritual value to observers. But it's not a useful tool to explain the world." (Needless to say, this respondent felt communication with researchers in alternative archaeology was a "waste of time.")

Obviously, few 'alternative' respondents judged themselves in the same way, but some did indicate, like one Moot-goer (female, 50-59, teacher) that "Alternative styles have more space for intuition and [a] spiritual dimension."

bolstered by reference to the links between some alternative archaeologies and religions both new and old. But are 'orthodox' archaeologists so very different? Have they not often sought guiding principles, order, uniformities, anything to escape contingency? Are they not also, in some sense, searching for design?

'Orthodox' archaeologists are not necessarily concerned with different goals than Earth Mysteries researchers (though this is not to say that the domains of their concern coincide perfectly), the kinds of knowledge each seek are not mutually exclusive, and thus it is quite possible that they have something to learn from each other. In particular, it is possible that archaeologists may gain from the explicit admission of subjective, experiential emphases which is seen in Earth Mysteries. For example, although a 'feel', or intuition, has always had a major role to play in the practice of archaeology, and many archaeologists are not shy about discussing this in the field or in informal contexts (e.g. Ned Heite's 'grokking' in Ch. 4), it will doubtless be some time before it is routinely acceptable in a mainstream archaeology publication to explain that one was prompted to survey an area because its "atmosphere" or "psychic resonance" suggested the presence of sites nearby (cf Billingsley and Bennett's (1996) account, in an Earth Mysteries magazine, of their discovery of new Bronze Age sites on Midgely Moor). In the same way, although many an archaeologist has sat quietly at an archaeological site and regarded their surroundings for a few hours, this is not usually reported upon with the freedom that it is in EM circles.⁹ In short, it is one thing to explore subjective aspects of human experience, but quite another to make such explorations explicitly acceptable as research tools. Archaeology may stand to gain from the latter, however. For example, Barrett (1997) reflects that not only archaeological understandings of Stonehenge, but also its management, have been sadly deficient as a result of ignoring the actual experience of the site.

A corollary to this orientation in some alternative work is the idea of trying to deal with subjective experience in ways other than through language, especially written text. EM has always placed great emphasis on actually going to sites and spending time there. Some archaeologists do the same; some don't. But the point here is really one of goals. For the archaeologist, it has been necessary for a research endeavour to end with some kind of written interpretation. For many in Earth Mysteries, the meaning lies instead in being there. Given that there is much that cannot be expressed in words, and that technological

⁹ Although Bender *et al.* (1997) made strides in this direction with their work at Leskernick.

developments already permit passable visual reproductions of reality, might a whole new mode of communication and discourse be possible in archaeology? The potential for a revolution in our understanding of past peoples seems profound and enormous.

Chapter 6

Archaeology and Apocalypse

In many recent best-selling 'alternative' books related to archaeology, such as *The Mayan Prophecies* (Gilbert and Cotterell 1995) and *Keeper of Genesis* (Bauval and Hancock 1996), there is a common theme – the idea that in the remains of 'extinct' civilizations, there are not only clues to the fate which will befall our own society, but sources of ancient wisdom which, if interpreted correctly, may help us to avoid cataclysm. These works and others like them are provocative because they resonate with traditional Judaeo-Christian apocalyptic literature, both in form and in effect, using other-worldly revelations about the past and future to guide understanding and action in the present. Apocalyptic narratives can act as powerful historical forces – stories to live and die by – particularly at significant junctures in time, such as the upcoming millennium. Thus, it is of interest that apocalyptic/revelatory threads also run through more 'orthodox' writings in archaeology. The idea that peoples of the past can tell us something crucially important about our own future has always been in the background of much archaeological research, but in this time of environmental degradation, instability, and rapid social change, such themes have begun to appear in more specific forms, and in sharper relief, provoking questions about the use of the past in the present, and about archaeologists as tellers of stories.

If we recall that our history's end has not yet been written, and that we ourselves are its coauthors, then perhaps we will come to see that no ending is inevitable, and that the saving bliss of catastrophe is a luxury we can ill afford.
(O'Leary 1994:228)

During the course of this research on prehistory's importance to people in Britain, and the many forms which its interpretation takes, multiple lines of connection between archaeology and folklore quickly became evident to me. There are, for example, frequent links between archaeological sites and local traditions or legends in Britain, as well as the strong tendency for avocational researchers there to be interested in both, rather than either

subject in isolation. This is intriguing territory indeed. But if one adopts a broader definition of folklore, moving outwards from 'the traditional beliefs and stories of a people' (Concise Oxford Dictionary) to include all stories, including religious narratives, which play an important role in societies today, there emerge many more lines of connection with archaeology. One example is that of Western apocalyptic writings, both secular and religious, for these versions of history can easily be seen in parallel to modern archaeologically based narratives. Followed far enough, these lines intertwine in surprising ways, and put fairly ordinary observations about the popularity of archaeology, or the importance of history, into larger contexts – contexts which archaeologists may find worthwhile to consider as they read and write upon their subject, for the archaeologist's role in modern society can be more substantial than is sometimes assumed.

Archaeology is important socio-politically, and not only in explosive situations like India's Ayodhya (Ascherson 1998; Kitchen 1998), but also in the way that archaeologically based narratives are woven, just as folklore is, into the very fabric of our lives. This has increasingly been the subject of study for scholars, especially those concerned with national and ethnic identities and their representation and construction through archaeological material (e.g. Ronayne 1997; Piccini 1997; Kohl and Fawcett 1995; Blake in press). But there is another dimension in which archaeology is socially consequential, which is becoming ever more obvious now, in a time when idle conversation often turns to the fast-approaching year 2000 AD. Archaeology is also important because of the role it plays in generating narratives about the way our world will end. These narratives in turn are important socially (not just to academics and ideologues) because human beings understand and describe their world through stories, and more than that, because stories help us to situate ourselves, as individuals and as groups, in time. More significantly still, stories – whether born of history, religion, science, or myth – tell us how to live. And sometimes, they tell us how to die.

The Millennium Cometh¹

A former professor from Taiwan named Hon-Ming Chen is now the leader of God's Salvation Church, and has been, at least until recently, patiently waiting in a Texan suburb with 150 devoted followers for God to return to Earth. Specifically, they were waiting at 3513 Ridgedale Drive in Garland, with a 'spacecraft' made of radial tires, plywood, and lamp posts, where they spent each day reading Chen's books and praying in preparation for God to come in a spaceship, to save all humanity from nuclear war. But, despite Professor Chen's assurances that on Wednesday 25 March 1998, 'the Almighty will make a preliminary appearance on Channel 18 of every TV set in the world' – interestingly enough, the Home Shopping Network in Texas – God has so far been a no-show (Perkins and Jackson 1998:24).

This is, by now, a familiar sort of story, for there is an increasing amount of coverage in the media about activities relating to the end of this millennium. Interest is not climbing only in isolated religious groups, however; this is a secular phenomenon too, and has been growing steadily for decades (Russell 1978:23; Zamora 1982). The word 'apocalypse', allusions to the Book of Revelation, and even photographs of the Four Horsemen, are showing up with amusing regularity in tabloid newspaper headlines, while elements of the storyline from Revelation are often used in movies, in fiction, and vividly in comic books. One particularly lurid tale of extraterrestrial invasion even opens with a line from Revelation: 'And I saw another sign in heaven, great and marvelous, seven angels having the seven last plagues' (Wheeler 1996). This is not surprising, for it can be argued that the obsession with extraterrestrials now gripping much of the West is a result of this end-of-the-millennium, end-of-the-world-as-we-know-it mindset. Certainly, the themes of extraterrestrial visitation and the end of the current world order were blended in some of the most popular Hollywood films of the late 1990s, from *Independence Day* to *Men in Black*, *Contact*, *Sphere*, and *Starship Troopers*. Themes of mass destruction by inexorable

¹ I finished the penultimate draft of this chapter at precisely 514 days and 13 hours before the new millennium. (At least, this is true according to the school that arbitrarily designates 1 January 2000, as the first day of the new era. See Gould (1997) for alternative notions.) I have included references to current events, and presently popular books and films, although this will undoubtedly make this chapter seem dated quickly. But this chapter *is* dated, and a product of its time; I see no reason to camouflage this fact. It is my hope, however, that the larger point will stand the test of time and remain thought-provoking for later readers.

natural forces are played out in *Deep Impact*, *Armageddon*, and in different ways in *Titanic*, *Volcano*, *Twister*, and the remake of *Godzilla*. Cities or even countries being laid waste are apparently almost prerequisite to engaging the American movie-going public's attention; and as Ralph Melcher commented, 'at the end of the Millennium the highest achievement of popular culture is the construction of the perfect disaster' (1998:2).

Of course, academics are not exempt from end-of-the-world fervour, and so there has also been a proliferation of scholarly interest in social behaviour as the millennium approaches. There seems to be a new sociological, historical, or literary book out every week analyzing apocalyptic or millenarian movements, or offering general comment on what has come to be known as 'pre-millennial tension'.

There are also many very popular books being published which are specifically preoccupied with dramatic ends to ancient civilizations, and with looking to sources of ancient wisdom which may help us to avoid a dramatic end of our own. Given that 'alternative prehistory' over the last century can illuminate the influences operating on academic archaeologists, and the complex relationships between accepted and marginalized discussions of the past, I think this would be well worth examining in considerable detail. As that is a book in itself, however, I will here only sketch, at the most general level, some areas of connection between such popular books, apocalypses and their social role, and archaeological writing.

Apocalypse

It is probably wise to begin with what 'apocalypse' means. For most of us, in general usage, the word 'apocalypse' means disaster, or the end of the world. Bible readers may even have some more specific images in mind, like the four Horsemen bringing unspeakable cataclysm, the blowing of trumpets, fire in the sky, and the sea turning to blood, or plagues of exceptionally nasty locusts, and stars falling into the sea, poisoning the water. But actually, the word 'apocalypse' properly means 'revelation'.

The essence of apocalypse is captured by Revelation 6:1-8, where the four Beings, in sequence, lead St. John by the hand to show him the Horsemen, crying 'Come and See'! Similarly, all the events we associate with 'the apocalypse' – again, the Horsemen, various

scenes of destruction and heavenly intervention – are integral to the story, but it is St. John's experience, his revelation, that is apocalyptic. The four Horsemen are not the *bringers* of the apocalypse, they are characters in the story which is *itself* the substance of the apocalypse.

Thus, Biblical scholars speak of 'apocalypses', including the apocalypse of Daniel, of St. John as written in Revelation, the apocalypses of Zachariah, Isaiah, Joel, Enoch, Baruch, Ezra, Abraham, and the apocalypses of Greco-Roman, Gnostic, and Persian origin (Hellholm 1983). Apocalypse or 'apocalyptic' is in fact a literary genre, including narratives dealing with divine revelation through dream, vision, or supernatural intermediary. These narratives usually pertain to eschatology, or 'the study of the End Times', but not invariably. Sometimes they emphasize the bad things that are going to happen to everyone on the face of the earth, and sometimes they emphasize the subsequent happiness for the righteous in the glorious kingdom of God.

Apocalyptic is complex, and there is a great deal written about it, not only about interpretation of the texts themselves, but also about their purposes. Most pertinent here is the observation made by Christopher Rowland, in his book *The Open Heaven*, that Judaeo-Christian apocalyptic writings did not deal only with what was to come – they covered past, present, and future, explaining history and contemporary events in terms of prophecies for what lay ahead. This is critical to their interpretation. In short, they sought not only to relate divine wisdom about the future, but to provide an understanding of all history.

Bernard McGinn, in his book *Visions of the End*, wrote that

The structure and meaning of time, the meeting place of this age and eternity, are consistent concerns... The desire to understand history – its unity, its structure, its goal, the future hope which it promises – is not a passing interest or momentary whim, but a perennial human concern. A sense of belonging in time, as well as the need to understand the special significance of the present, is the anthropological root of apocalyptic systems of thought.
(McGinn 1979:30)

One could say that it is also the root of the disciplines of history and archaeology. These different studies and writings are all about our place in time, our place in history, and our relationship to the beginning and the end.

But more than this, in our modern era, archaeology provides some of the raw material of revelation. Voices from the past replace divine messages. And accordingly, popular archaeology books have at least partly replaced traditional religious wisdom in guiding our thought about who we are, where we've been, where we're going, and how we should live.

Popular Archaeology, Revelation, and Eschaton

The popular 'alternative' archaeology books that will be discussed here have the elements that can be considered the chief hallmarks of apocalyptic. They tell elaborate stories based on epiphanies (revealed, in this case, by the archaeological record), make eschatological statements, and also make comments on the state of society today, and recommendations for our future action. There are many books with similar themes, so only two of the most recent bestsellers will be discussed here. These are also, happily, two of the better ones, which are at least somewhat grounded in the interpretation of material archaeological reality.

The Mayan Prophecies, published by Adrian Gilbert and Maurice Cotterell in 1995, is an international bestseller which revolves around the notions of the end of the world, and of revealed wisdom from another sphere. Their claim is, essentially, that in some Mayan carvings, particularly the Lid of Palenque, there are secret messages that can be decoded and understood. Further, the authors argue that the Mayan obsession with calendars concerned cycles of sunspots. They then relate sunspots to dramatic drops in human fertility, and thus explain the hitherto-apparently-enigmatic Mayan collapse². It is all entwined with the Popol Vuh, the sacred Mayan book which can itself be considered an apocalypse of sorts, concerned as it is with prophecy, the past, and the future. Based on the Mayan calendar, Cotterell and Gilbert make specific predictions for the year 2012, of the greatest catastrophe that mankind has ever known. We are to expect a reversal of the magnetic field, pole changes, giant floods, submerged landmasses, a drop in temperature.... the works. We are entreated to sit up and take note while we still can.

² Of course, archaeological explanations of this transformation abound. But Bintliff (1988:9-11) reiterates Wilk's interesting correlation of changing interpretations of the Mayan collapse in American archaeology with events in American life in the '60s and '70s, e.g. the invasions of Vietnam and Cambodia, and the passing of legislation on air and water quality. One is compelled to wonder about the interpretations of the Mayan decline which might appear in the wake of the troubled Clinton administration.

The second popular 'alternative' archaeology book may become even more widely read. My copy of the best-selling *Fingerprints of the Gods* bears the notable cover blurb, 'a Quest for the Beginning and the End'. But the author, Graham Hancock, was just warming up with his proposal that Antarctica is the place to look for the high civilization that spawned all others. Its sequel, *Keeper of Genesis*, by Hancock and Robert Bauval (of *The Orion Mystery* fame), says that the monuments of the Giza acropolis, together with the ancient texts and rituals which are linked to them, were specifically designed to transmit a message to us, across time. They say that this specially encoded message pertains to the predecessors of the ancient Egyptian civilization, and to their quest for immortality, and the transcendence of their physical being. Once again, the authors plead with us to take heed, for if we can decode and understand this message, it could mean a wonderful renaissance for humanity, and the solution to many of our problems.

This theme is played out, too, in another very popular book of a somewhat different kind. Unlike *The Mayan Prophecies* and *Keeper of Genesis*, however, this one is written as fiction, but fiction of that particular New Age sort which is implied to be deeply truthful at its core. *The Celestine Prophecy* has spent years on best seller lists, and millions of copies are in print (*The Economist*, 7 December 1996, p.6). The cover reads:

In the Rain Forests of Peru, an ancient manuscript has been discovered. Within its pages are 9 key insights into life itself – insights each human being is predicted to grasp sequentially, one insight then another, as we move towards a completely spiritual life on earth.

(Here we have both the elements of revealed wisdom from a distant archaeological source, and in this case, a happy ending – a jolly eschatology.) Although this book is presented as fiction, the author James Redfield, a sociologist, also puts out a special newsletter chronicling his experiences with the spiritual renaissance now occurring on our planet. There is also a sequel – *The Tenth Insight*, presumably based on a new Mayan manuscript inexplicably found in Peru – and a series of spin-offs, including *Celestine Prophecy Pocket Guides*, *Experiential Guides*, and audio tapes. Indeed, there is a full-blown *Celestine Prophecy* industry, and apparently, the books are being used by spiritual study groups the world over. The specifically archaeological slant seems more a rhetorical device used for legitimation than anything else, but it is difficult to suppose that all the book's earnest

readers reject the central premise, that the Maya were the source of these prophecies and they disappeared because by raising their energy vibrations through meditation, they actually crossed over into a higher plane, transcending all materiality and even death.³

Typically, most archaeologists respond to such books with derision, or respond not at all. The books are seen as either bad archaeology, or nothing to do with archaeology. However, this is underestimating the complexity of these narratives and of their position within society, not to mention taking a rather narrow view of archaeology. There are more productive ways to look at the situation.

These books are hugely popular around the world. People's lives are changed by them, and their views of the past and future transformed. Why should that be so? Archaeologists often suspect that books like *The Mayan Prophecies*, *Keeper of Genesis*, and *The Celestine Prophecy* are so well-received as historical or cultural reading by the general public simply because of an appetite for the supernatural and the sensational in place of the human and the ordinary. This may be part of it, but it is just as likely that their popularity is due to the way they are written. This is not to say that they are presented in an accessible style, because some of the time, such books are overly long, every bit as dense as an archaeological site report, and twice as boring. Rather, they may well be popular because they use the age-old formula of the apocalypse, which tells us about the beginning, the end, and where we are in relation to them, and what we can do about it. This formula continues to have great resonance for people today, as it always has (Russell 1978); the implications of this, in the context of the creation and relation of history, are worth considering.

³ There is one more book which I must mention – although it is tangential to this argument – simply because it is so fascinating! The author is Andrew Collins, a prominent figure in the 'psychic questing' movement in the UK, which has many connections to alternative history and archaeology. Collins' book, *From the Ashes of Angels* (1996) draws from the Book of Enoch. Collins contends that the descriptions therein, of the 'sons of God' marrying the 'daughters of men', are literal truth, and that these sons of God, or fallen angels, or 'Watchers', were physical, historical entities. Similarly, he takes the descriptions of the Judgement and Last Days out of context of Judaic eschatology. Probably drawing on traditions in Theosophy, *From the Ashes of Angels* takes the Book of Enoch literally, as historiography, rather than as a revelatory vision. This literalism has led some readers to surprising conclusions – on a group tour around the British Museum with Collins, surely one of the most unusual I've ever had, the suggestion was made that the great stela in the Assyrian gallery, showing anthropic figures with the heads of falcons, were not merely representations of gods or similar entities, or of humans with hats, but actual portraits of chimeric beings – the Watchers – that existed in fact.

Apocalypses Within Social History

All old stories – whether folklore, religion, or history – must be seen in the social and political contexts of their origination and perpetuation, and the early Judaeo-Christian apocalypses are no exception (Hanson 1979). They were written in times of great oppression. For example, the book of Daniel was written when Antiochus Epiphanes was attempting to annihilate Judaism, and St. John wrote Revelation from prison, convinced, correctly, that tribulations lay ahead for the churches of Asia Minor (Russell 1978:16).

And so, Biblical scholars consider that these writings were in part about revenge and a reversal of power (Hanson 1979). Indeed, in the Book of Enoch, it says that ‘the evil doers [will] be consumed, and the power of the guilty be annihilated...’ and that ‘This Son of man ... shall hurl kings from their thrones and their dominions... Darkness shall be their habitation, and worms shall be their bed’ (Laurence, trans. 1995:178, 51). In Revelation 19 and 20, John describes the triumph of God and the angels over the False Prophet and the Great Beast – who are best understood as representing political figures – as well as the subduing of the eternal characters of Satan and the Dragon. But more than revenge, the apocalypses, especially the Revelation, were letters of consolation to the oppressed, for there is a happy end for the righteous, after all the carnage is finished. These writings did not intend to incite rebellion or active resistance against the oppressors of the day, because the final war between good and evil would take place in heaven, not on earth, and God would deliver the protagonists from their enemies (Rowland 1982).

But the historical context of the perpetuation of apocalypses is not necessarily the same as that of their origination. In reference to some American apocalyptic movements, Hanson points out that today, ‘no longer are war and desolation prerequisites for the apocalyptic response. A vague feeling of dissatisfaction with modern life seems sufficient basis for laments over “the late great planet earth”’ (1979: 427).⁴ However, what remains –

⁴ Similarly, O’Leary remarks that ‘The early Christians who responded favorably to the book of Revelation were, by most historical accounts, subject to intense persecution that included execution and public torture. If the largely middle-class group of fundamentalist Christians in the United States who today form the core of [apocalyptic prophet] Hal Lindsey’s [very extensive] readership believes itself to be similarly persecuted, this is surely a rhetorically induced perception; for there is an obvious difference between being torn apart by lions in front of cheering crowds and being forced to endure media onslaughts of sex, violence, and secular humanism. As one critic puts it, “the crucial element is not so much whether one is actually oppressed as whether one feels oppressed”; and this is always a subject for persuasion.’ (O’Leary 1994:11)

regardless of the reality of the oppression – in apocalyptic narratives is what Rowland called ‘an insistence on the working out of God’s will through the processes of history’ (1982:159). History is seen as a trajectory which people cannot change. But in practice, that does not mean that people don’t choose how to act, for they do. They can make the choice to be passive and let history take its course, or they may become very proactive, because although the trajectory cannot be changed, the end can be hastened.

An example of the latter behaviour may be found in the 1997 millenarian mass suicide of the Heaven’s Gate group in California – 37 people killed themselves in the belief that a spaceship following in the wake of Comet Hale-Bopp would take their spirits to a higher plane. It was generally considered to be a bizarre anomaly, but this is not the only way to see it. If one actually peruses the group’s web site or official literature, and reads beyond the sensationalist news coverage, it becomes apparent that this was not merely an isolated group of lunatic UFO enthusiasts, but that the eschatology of the Heaven’s Gate group was based on traditional tenets held dear by many western Christians; in short, their doctrine was the syncretic end result of combining ‘apocalyptic Christian bits and pieces with folk myths of our own contemporary culture’ (Gould 1997:53). Baudrillard made sense of acts like this when he wrote:

Whole communities have gone to the point of putting their lives on the line to hasten the advent of the Kingdom. And since this has been promised to them at the end of time, all one had to do is put an end to time, immediately (and personally).
(Baudrillard 1997:4).

And so they did, just as many others have before them, and others will again.

Just as the contexts of perpetuation of apocalyptic are highly varied, O’Leary relates that the roles played by apocalyptic beliefs in recent centuries, and the political consequences of those beliefs, are diverse – apocalyptic cannot be seen as only radical, only conservative, only encouraging passivity, or only a call to arms (1994:12). But what remains constant is the high human cost. Gould makes it clear that the price has been exacted many times, in many places. He writes that ‘the fusion of Christian millennialism with traditional beliefs of conquered (and despairing) people has often led to particularly incendiary, and tragic, results’, citing as examples the nineteenth century defeat of the

Xhosa of South Africa, and the Ghost Dance movement of Native Americans in the late 1800s, which led to the massacre at Wounded Knee (1997:52).

It is critically important that despite the striking drama of these human tragedies, these were not anomalies or deviations from 'normal history', although this is how they are usually represented. Millennial history specialist Richard Landes asserts that traditionally, historians of apocalypticism, messianism, and chiliasm have had difficulty transforming their observations into 'productive historical analysis' – that although they 'have identified a number of times and places where eschatological beliefs played a central role in a culture's imagination (e.g., first century Palestine, fifth century Mediterranean, thirteenth century Europe, seventeenth century England, eighteenth century America, nineteenth century China)', such phenomena have been considered as contained events (1996:165). Landes argues that there has been a significant and systematic underestimation of apocalyptic beliefs as "normal" historical forces, and that careful study of their influence in history is warranted, given that 'in favorable circumstances, apocalyptic beliefs can launch mass movements capable of overthrowing (and forming) imperial dynasties and creating new religions' (1996:165). Landes submits that apocalyptic rhetoric – as analyzed in exhaustive detail by O'Leary (1994) – is given its persuasive force by 'apocalyptic time', which he defines as 'that perception of time in which the End of the World (variously imagined) is so close that its anticipation changes the behavior of the believer' (1996:165). Clearly, this is an important phenomenon of history; however, although 'such perceptions of time operate on several levels of cognition, of individual, group, and mass psychology', and have often been studied by social scientists such as anthropologists and sociologists, Landes contends that historians have not often dealt with apocalyptic time.

The historian, however, has been largely removed from the subject because his documents almost always reflect the perspectives or the editorial blade of post-apocalyptic, normal time, with its retrospective knowledge that the end did not come.

(Landes1996:165)

And so Landes argues that historians must think again, for an awareness of this perception of time, and its effects on the believer's behaviour, is essential to understanding the historical influence of eschatology on society. Most importantly, Landes suggests that this understanding is in turn essential to a real comprehension of our modern age, steeped as it is in the resurgence of religious fundamentalism worldwide (1996:166).

Apocalyptic movements, then, emerge and spread in both unpredictable and predictable places, have the force of inexorability behind them, act in themselves as strong and underestimated historical forces, and seem to be particularly relevant on the world stage at the moment. They provide narratives that can and do guide people's actions in a very real way, for stories specifically about the end of the world, or of a world, are exceptionally powerful in their effect on people. Archaeologists contribute to those narratives – these stories to live and die by – both directly and by implication, as the popular works discussed in the previous section remind us, and as professional academic archaeologists have long been aware.

Archaeology and Apocalypse: Adapting the Formula and Changing the Ending

It is, by now, a commonplace observation that narratives created by archaeologists to explain the past are coloured by what we see happening around us in the present. What is less considered, but surely equally important, is the fact that in turn, these stories about the past shape people's view of our future. Some have written about this, and some have acted upon it.

In his *History of Archaeological Thought*, Bruce Trigger acknowledges the power that archaeology has to contribute to the narratives by which people today understand their current situation and choose how to live. For example, he wrote that 'cataclysmic' evolutionary archaeology encourages a world view which

attributes the shortcomings of a world economy to largely immutable evolutionary forces rather than to specific and alterable political and economic conditions that have evolved under American hegemony. This explanation has attracted a willing audience among the insecure middle classes of Western nations, who are anxious to believe that they are not responsible for the fate they fear is overtaking them. (Trigger 1989: 323)

Trigger retains the hope, however, that archaeology can also have the 'ability to act as a positive force in human history' (1995:279). This is a hope that other archaeologists share. The best example, worth discussing at some length, comes from Paul Bahn and John Flenley's 1992 book, *Easter Island, Earth Island*. The line on the cover, 'A message from our past for the future of our planet', is a noteworthy parallel to the theme central to *The Mayan Prophecies* and *Keeper of Genesis*.

Bahn and Flenley, rather than fixating on the traditional questions of how Easter Island's inhabitants got there, and how they made those amazing statues, concern themselves with the evidence surrounding the culture's violent decline, after centuries of peace and stability. They ask: 'What cataclysm could have had such a devastating impact on the island's culture?' (1992:9), and introduce their case without mincing words:

the answer to this question carries a message that is of fundamental importance to every person alive today and even more so to our descendants. Given the decline of the island's culture, we should consider the parallels between the behaviour of the Easter Islanders in relation to their limited resources and our cavalier disregard for our own fragile natural environment: the earth itself. This is more, therefore, than an account of the rise and fall of an extraordinary prehistoric culture... it is, indeed, a cautionary tale relevant for the future of all humankind.
(Bahn and Flenley 1992:9)

The ecological catastrophe that befell Easter Island centered on the disappearance of the once-abundant palm trees. Without trees, there could be no canoes to go deep-sea fishing, erosion became a problem, and fresh water supplies dried up. As the stresses increased, there was war, a drastic population decline, and a rapid degeneration of the society. Finally, the great statues were toppled over and mutilated, providing a powerful metaphor for the end of a "golden age". European visitors later on reported that to the Easter Islanders, 'even driftwood was looked on as a treasure of inestimable value, and a dying father frequently promised to send his children a tree from the kingdom of shades' (Bahn and Flenley 1992:172).

It's a gripping tale, but what is especially relevant to this discussion is the way that Bahn and Flenley chose to tell it. There were three main factors that contributed to the complete deforestation of the island: drought, tree cutting, and rats eating the trees' seeds. Where others have argued for climate being the most important variable, Bahn and Flenley choose to emphasize the factor which was under human control. Their message was this:

Easter Island is small, and its ecosystem relatively simple. Whatever one did to alter that ecosystem, the results were reasonably predictable. One could stand on the summit and see almost every point on the island. The person who felled the last tree could see that it was the last tree. But he (or she) still felled it.
(Bahn and Flenley 1992:214)

A more poignant historical parable is hard to imagine.⁵ Yet, surprisingly, some might read such a powerful true story about humanity's relationship with the natural world and remain unmoved. Stephen Jay Gould, in making the point that it is not only the socially downtrodden or oppressed who hold millenarian beliefs, relates this intersection of apocalyptic thought and ecology:

James Watt, Ronald Reagan's unlamented secretary of the interior, a deeply conservative thinker and prominent member of the Pentecostal Assembly of God, stated that we need not worry unduly about environmental deterioration (and should therefore not invest too much governmental time, money, or legislation in such questions) because the world will surely end before any deep damage can be done. (Gould 1997:50)

It is at this point that the real importance of the intersection of apocalypse and archaeology becomes clear. It is here that the spheres of political philosophy, eschatology, and archaeology come together, and – at the risk of sounding prophetic – it is here that the line must be drawn. And indeed, although Bahn and Flenley use many elements of traditional apocalypses in their writing, including the ideas of revelation of wisdom from another dimension, and the situating of the reader within a timeline, there is a crucial departure from the formula which makes all the difference: Bahn and Flenley reject inevitability, they reject the notion that we are on a historical trajectory which cannot be changed, and in so doing, they put the responsibility for our future back, squarely, on our human shoulders. They write that the Easter Islanders

carried out for us the experiment of permitting unrestricted population growth and profligate use of resources, destruction of the environment, and boundless confidence in their religion to take care of the future. The result was an ecological disaster leading to a population crash. A crash on a similar scale (60 per cent reduction) for the planet Earth would lead to the deaths of about 1.8 billion people.... Do we have to repeat the experiment on this grand scale? ... Would it not be more sensible to learn from the lesson of Easter Island history, and apply it to the Earth Island on which we live?

(Bahn and Flenley 1992:213)

⁵ I cannot resist quoting from Dr. Seuss' *The Lorax*, a similarly moving cautionary tale:

"And at that very moment, we heard a loud whack!
From outside in the fields came a sickening smack
of an axe on a tree. Then we heard the tree fall.
The very last Truffula Tree of them all!"
(Geisel and Geisel 1971:50)

Bahn and Flenley have written this book in the knowledge – almost instinctive to historians, archaeologists, and prophets alike – that stories about the past and the future are the axes around which revolve the worlds of listeners in the present. This is the point made by Landes and others, above, that apocalypses are historical forces in themselves. But Bahn and Flenley depart from the traditional formula because they place the making of future history into human hands. In a work which is both honest and courageous, they deliberately use the revelatory might of archaeology, with their ecological agenda made transparent, and their eschatology explicit.

Trigger implies that archaeological narratives about the course of history can neatly excise a population's political conscience. Bahn and Flenley hope that archaeological narratives can help restore ecological responsibility and save us from ourselves. Perhaps it is also true that the manner in which archaeologists represent past societies can affect the future in ways which are less direct but no less important.

In journals of political science and international relations, one can often see reviews of books like *Anticipating the Future: Twenty Millennia of Human Progress* (Buzan and Segal 1998). In this case, the book starts at the beginning, with prehistory, charts what the authors consider to be the (progressive) trajectory of human development, and makes predictions for the future on that basis. This is significant, not just because books like this exist, but because of who reads them and why. High-profile popular futurists Toffler and Toffler (1998) reviewed *Anticipating the Future for Foreign Affairs*, for a readership that is not just intellectually curious about what the coming years will bring, but is actively involved in designing corporate and political strategies for survival in those years. The extent to which prophecies like those of Buzan and Segal can actually shape the future through this influence is inevitably difficult to ascertain, but the potential cannot be neglected. Books forcefully combining humanity's history and future have undeniable power; those who doubt need only see Marx and Engels (1988 [1848]) and a textbook of twentieth century European history for an effective reminder.

Toffler and Toffler, committed to a view of history as discontinuous, punctuated by massive revolutions or waves, disagree with Buzan and Segal's continuist stance, but moreover, claim that such a stance 'blinds them to some of the most important changes that lie ahead' (1998:136). More specifically, Toffler and Toffler argue that although both ways of interpreting history have merit, their own model of 'wave conflict' – strife

between societies on opposite sides of historical discontinuities, 'rural vs. urban, agrarian vs. industrial' – might enable one to 'do a better job of anticipating hot spots before they blast their way into the headlines' (1998: 136). Toffler and Toffler suggest that a better academic understanding of the processes of world history, from its very beginning, could help the international community prevent the human tragedies of Bosnia, or to better interpret the actions of the Taliban in Kabul. Perhaps, then, millennium or no millennium, now is the time for archaeologists to take stock of their role in the production of social knowledge for the future.

Conclusion

Our world is growing smaller and the communities in which we live are growing larger. Traditional stories are told not only from one to another in villages, but replicate themselves in news reports on the Internet. They change, die, and are reborn in different forms, and never cease to exert an influence on the minds of those they touch. Indeed, they are living forces in themselves.

So if we take the word 'folklore' writ large, at its broadest and most forward-looking; if we conceive of 'archaeology' at its most extensive, meaning the study and the creation of the past in the present; if we understand popular books with strange theories about the ancient Egyptians and the Maya not as somehow deviant, but as indicators of what matters to people, and how they read history; if we see 'apocalypses' as revelatory stories situating ourselves in relation to the beginning and the end; if we recognize the role of the archaeologist in creating narratives which can be understood as apocalypses; and if we are concerned that these narratives be forces for good... then perhaps the intersection of archaeology and folklore, history and story, is a stranger, more important, and more remarkable place than is usually suspected.

There is a suggestion embedded in popular alternative archaeology books like *Keeper of Genesis* and *The Mayan Prophecies*, and echoed by critics like Trigger, that part of archaeology's task is to help us recover ancient knowledge which we desperately need today to avert disaster. This may or may not be so, but one thing is clear. Archaeology has never been only about that which has gone before. It is also about that which is yet to come.

Chapter 7

Three Escapes from Epistemology

In Chapters 4 and 5, I attempted to counter the popular notion that there is a straightforward dichotomy of 'orthodox' and 'alternative' archaeology, and suggested that instead, at times, the situation is much more fluid and interesting. In Chapter 6, I pushed this idea further, and avoided the problem discussed earlier in this dissertation – that is, how best the 'orthodox' archaeologist might understand, and negotiate relationships with, competing discourses about the past – by standing aside, and viewing both 'orthodox' archaeology and 'alternative' archaeology from the same distanced vantage point. I believe this kind of perspective is useful; there could be considerable benefit to simply dropping the old framework completely, redefining the terms and resolving the problem by seeing it as a nonproblem. (There is, for example, some lovely work to be done on lost civilization and hyperdiffusionist narratives as they are manifesting themselves in popular culture today, or on the resurgence of archaeological catastrophism both inside and outside of academe.) But, however elegant and appealing this option might be, I feel compelled to return to the initial formulation because it is not merely a theoretical difficulty of my own invention, which can be manoeuvred out of existence, but a problem of perception which exists through a degree of intradisciplinary consensus, and thus cannot be wished away. Whether or not an essentially dichotomous framework is the *most productive* way of looking at the matter, it is how many archaeologists *do* look at it, and many are unlikely to be persuaded to simply drop that way of thinking any time soon; there is too much invested in it. Thus, the most effective proposed solutions, practically speaking, must at least engage with the issues generally deemed problematic. Let us assume, then, for this chapter, that there are big differences between the views of 'orthodox' archaeologists and others. The challenge then becomes how to deal more effectively with those different views and their proponents.

Postprocessualism has, of course, involved a great deal of concern for the implications, for archaeological interpretation, of jettisoning specific notions of proof, truth, etcetera.¹

¹ Unfortunately, many of the arguments grounding this programme, although now rarely disputed in philosophy, are still not accepted widely in archaeology; many archaeologists continue to speak blithely of proof, testing hypotheses, objectivity, value-neutrality, etc., without recognizing that

However, overall, much less ink has been used in considering the implications of those implications, *for archaeologists' interactions with other people*. Reasons for this are varied. In part, it may be because these interactions are viewed as a personal and political matter, not a disciplinary one; thus, they are discussed more in pubs after work, or in messages on email discussion groups, than in formal publications. It also may be that some archaeologists are simply coming from a different place, and have always considered interactions with others to be paramount in importance, not requiring philosophical objections to logical positivism or anything else to support them in this belief. This is probably particularly true of those who have long worked in 'public archaeology'; often, in my experience, 'theoretical' concerns are quite secondary in importance to them. Regardless, I submit that working through the social implications of the postprocessual stance on truth and method is an important issue, and so, as a starting point for coping with diversity, this long chapter is devoted to the treatments of related problems by three philosophers of different stripes. There are no firm answers here, but there are some relevant questions, and certainly some contradictions to the recommendations of archaeologists mentioned in Chapter 2, regarding archaeology's engagement with alterity.

Many thinkers concern themselves with relationships between human beings, and so, I could have chosen to situate the problem within discourses of power, formal ethics, multiculturalism, gender, rights, or many others. However, I decided to limit my

their formulations are problematic. This is not the place to reiterate the arguments, but I take it as given that the discussion is not over and that they do need reiteration. In this case, a simple division of archaeology into different camps who have agreed to disagree may not be good enough. Arthur Koestler made a similar point beautifully in his discussion of behaviourist psychology:

If one attacks the dominant school in psychology.... one is up against two opposite types of criticism. The first is the natural reaction of the defenders of orthodoxy, who believe that they are in the right and that you are in the wrong – which is only fair and to be expected. The second category of critics belongs to the opposite camp. They argue that, since the pillars of the citadel are already cracked and revealing themselves as hollow, one ought to ignore them... Or, to put it more bluntly, why flog a dead horse? (1989[1967]:4)

Koestler's answer comes in his priceless description of the secret "Society for the Prevention of Cruelty to Dead Horses":

English insularism, class distinctions, social snobbery, trial-by-accent, are all declared to be dead horses, and the inane neighings that fill the air must be emanating from ghosts. The same applies to American dollar-worship, materialism, conformism. You can continue the list as a parlour game. In the Sciences, the S.P.C.D.H. is particularly active. We are constantly assured that the crudely mechanistic nineteenth-century conceptions in biology, medicine, psychology are dead, and yet one constantly comes up against them in the columns of textbooks, technical journals, and in lecture rooms." (1989[1967]:349).

In the case of archaeology – as with behaviourist psychology – the reason for continuing to reiterate the point is that people other than archaeologists are affected by how we think, and sometimes profoundly so.

explorations to three approaches, which each begin in the vicinity of some key contentions that were catapulted into the general archaeological consciousness a dozen years ago: that objectivity and proof are luxuries we do not, and can never, have; and that an insistence upon formal methods of inference, based on the reverse assumption, is erroneous. These contentions are but one facet of the larger questioning of the security and nature of the epistemological enterprise, a questioning in which Steve Fuller, Paul Feyerabend, and Richard Rorty have all been deeply involved.

I made this choice of emphasis deliberately – rather than opting for, say, a framework taken from French literary theory – because these thinkers’ beginning points and terms of engagement with the problems are more likely to be *familiar* to most Anglo-American archaeologists, if not in the points and terms themselves then at least in the views against which those are defined. Thus, the reorientations, the ‘escapes from epistemology’ which these authors attempt (with varying degrees of success), might offer some accessible alternatives for archaeologists to consider in their approaches to other people’s ideas about the past – perhaps especially to those with which they disagree.

I begin with Steve Fuller, whose potential contribution to the discussion may not be immediately obvious. I include Fuller here because his “Social Epistemology”, developed within HPS/STS, may be of help to the increasing number of scholars who specialize in looking at archaeological disciplinary practice rather than at archaeological remains. His concern with moving forward from knowledge about how disciplines work and are received, to decisions about where they should go next – that is, putting the normative and the social back into epistemology – is a departure from tradition in HPS/STS, and I think, relevant to developing critical assessments about archaeological disciplinary practice. Fuller points out that these questions of policy are appropriate activities for scholars and should not be restricted to matters decided at a government level. He also has a very interesting argument pertaining to the dangers of the philosophy of science, which I shall mention briefly in relation to archaeology.

Paul Feyerabend’s iconoclastic views within the philosophy of science have been profoundly influential, and further, he was not afraid of looking beyond insular academic discourse to consider the social implications of his reading of scientific knowledge. Although his “democratic relativism” deliberately only took a roughly defined form, he asked many questions which probably resonate with the experience of the academic

archaeologist wondering how to use institutional structures for pluralist ends, rather than the simple replication of existing ways of seeing. Feyerabend is also one of the comparatively few philosophers of science who actually addressed the question of 'fringe' ideas – even archaeological ones – at any length, and his observations are thought-provoking.

In one sense, Richard Rorty travels a path parallel to Feyerabend's. His abandonment of the epistemological enterprise is complete; he has taken a position that truth lies in language alone, and that our practical response must therefore be conversation with one another, in the hope of achieving human solidarity in difference, rather than uniformity of opinion.

FULLER'S SOCIAL EPISTEMOLOGY: APPLIED PHILOSOPHY FOR ARCHAEOLOGISTS?

Domains once unified in Western philosophy, then separated into philosophy of science, history, ethics, metaphysics, etc., are becoming conflated once more in the newest generation of science studies research. As scholars absorb the implications of the realization that matters of scientific method and development are inseparable from matters of history and humanity, some are moving to explicitly structure their work to address such concerns. Steve Fuller's program of Social Epistemology is a case in point. Departing from conventional history, philosophy, and sociology of science, Fuller and like-minded scholars follow the study of the social organization of science, and the tenets of social constructivism, through to their logical conclusions. Fuller characterizes Social Epistemology as "the normative study of knowledge systems", with the agenda of using the information gained by studying the history, philosophy, and sociology of academic disciplines to help those disciplines work better for all concerned. This short overview briefly addresses the reconfiguring of science studies to address normative concerns, and offers some thoughts about how work done in this vein might be of interest to archaeologists, in terms of developing coherently critical and productive approaches to our discipline's future development.

* * * * *

In Chapter 3, I mentioned some aspects of the programme of Science Studies that I consider to be shortcomings – at least, with respect to reflexive analysis of archaeology – chief among them being the disconnection of the analysis from practice. However, some current STS researchers are covering ground that may be particularly relevant to those who are interested in integrating ethnographic and sociological approaches into the study of archaeology as a discipline. I believe this, despite my reservations, because some of the same reservations are being addressed within STS itself. There is, for example, a struggle underway in some sectors to transform STS into a more "applied" discipline. One of the proposed destinations is Social Epistemology: as its chief proponent, Steve Fuller, defines it, this is the systematic attempt to address the questions, regarding science, of "Who should be doing what, in what setting, to what end(s), for whose benefit and at what cost to whom?"(1997d: 3) Instead of the knee-jerk political answers which are too common,

Fuller advocates using the approach of STS – that is, drawing on HPS, sociology, ethnography, psychology, etc. – to answer these questions.

There is potential for this kind of discussion to help archaeologists with their own reflections. Those concerned with archaeological theory, disciplinary practice, public archaeology education, and heritage management could potentially explore common issues through this kind of framework. At the very least, exploring fresh STS territory might suggest to philosophically inclined archaeologists some new questions to ask.

Social Epistemology and How It Might Be Useful to Archaeologists

Steve Fuller began to publish on Social Epistemology in the late 1980s, and has continued to write steadily for the last decade on related subjects.² His main books include *Social Epistemology* (1988), *Philosophy of Science and Its Discontents* (1989), *Science* (1997d), and *Philosophy, Rhetoric, and the End of Knowledge: The Coming of Science and Technology Studies* (1993). As a philosopher of science with a deep interest in science studies, from the psychology to the sociology of science, he has provoked a good deal of debate within STS and cognate fields, because of his challenging and forthright ideas about what STS could and should be.

Fuller's work is extensive, often steeped in the past of philosophy of science, and sometimes dense with references to works which I am only beginning to really know. I am not even quite sure what I think about how right he is. But he is discussing issues about which I am concerned. As William Keith wrote, in review of Fuller's *Philosophy, Rhetoric, and the End of Knowledge*: "It is the sort of innovative, exciting, exasperating, infuriating, and provocative book that's good even when it's bad, because it sets everyone to talking and arguing about all kinds of things." (1995:488). And in that spirit, I think Fuller's work might be interesting for the archaeological community to consider, for at least these two reasons (if not more):

- The normative questions at the heart of Social Epistemology – "Who should be doing what, in what setting, to what end(s), for whose benefit and at what cost to whom?"

² His list of publications is online at the University of Durham's Department of Sociology web page, <http://www.dur.ac.uk/Sociology/staff/steve.htm>.

(1997d:3) – are surely relevant to developing a coherent basis for thinking about archaeological research, education both within and outside the university, and heritage management. Further, they are relevant to forming policy on all of these subjects.

•Better awareness of the nature and context of the work in HPS and STS from which archaeologists have drawn and are likely to draw, might help to prevent some needless debate within our discipline in the future.

I shall discuss each of these briefly below.

A Return to Normativity

Fuller on the current state of HPS and STS

Fuller discusses at length, in various works, the problem of normativity in HPS and STS. To be more precise, he discusses how it came to be effectively absent, why this should change, and how it could be changed in principle and hence in practice.

Beginning with the current state of HPS, Fuller observes that

Although HPS has been animated, if not outright motivated, by normative considerations, the prescriptions issued have been more in the spirit of a schoolmaster giving marks than of the policymaker trying to improve the conduct of inquiry. Thus as philosophers of science we know that it was good to choose Copernicus over Ptolemy by Galileo's day and that it would have been better to have made the choice sooner, but we have precious little to say about what line of research we ought to pursue next. (1991:169)

He continues, suggesting that many philosophers of science might well be dismayed,

if they realized just how close their current schoolmasterly mode places them to literary criticism and art connoisseurship, two disciplines whose practices have become increasingly alienated from their putative objects of evaluation. Contrary to nineteenth-century hopes, the judgements of critics typically do not feed back into the creation of better art or even better publics for the reception of art. What is produced, instead, is a self-sustaining body of scholarly literature. The positive impact of critics on the course of art in this century has been fortuitous at most, much like the impact of philosophy on the course of science. (1991:169)

In STS, the avoidance of normative issues has been much more comprehensive, including not just policy questions, but epistemological ones too. Indeed, a pronounced emphasis on

non-normative issues had achieved ascendance by the 1980s. Fuller described the situation in STS in 1988 in his book *Social Epistemology*:

The continuing failure of sociologists of knowledge to address normative issues may be seen in the discussion pages of the leading journal in the field, *Social Studies of Science*, edited at the University of Edinburgh. Originally designed as a general forum for both empirical and normative questions, over the years the empirical has outweighed the normative to the point that normative issues are now deliberately eschewed by the journal. In fact, this trend has probably been the most publicized and abrasive feature of the New Wave of the sociology of knowledge... (Fuller 1988: 267-8)

Why the avoidance of normative policy issues?

But how has this come to be the case? This is a rather interesting question in itself, and indeed, there are many fascinating twists and turns in the history of the development of STS and HPS, which Fuller covers in his analysis of how things came to be this way. First, tracing the history of epistemology, Fuller suggests that old biases have been responsible for the current situation in HPS. He argues that epistemologists have hitherto been working with a strangely restricted definition of 'normative', which somehow includes "the decisions that individual scientists ought to make for regulating their own research practices in idealized settings" but almost completely excludes "the decisions that policymakers ought to make for regulating the research practices of the scientific community as a whole in more realistic settings" (1988:275).

Second, he observes, there has been a schism somewhere along the line, which effectively ruled out even those 'individual' normative questions as subjects for inquiry in STS:

The disciplinary boundary separating the sociology of knowledge from epistemology has been suspiciously silent for quite some time. An implicit agreement seems to have been made to let the sociologists concern themselves only with what actually passes as knowledge in particular cases, while the epistemologists take care of what ought to pass as knowledge in general. (1988:263)

How to get back to normativity?

And so it is that HPS doesn't concern itself with 'practical' issues, and that the avoidance of all normative issues became naturalized within STS. And so it is that "the ideal regulation of real knowledge systems, to which the sociologist's expertise would likely prove relevant... has been consigned to the realm of "mere" policy-making and technical

applications” (Fuller 1988:263). However, Fuller is just warming up with these analyses, for he is concerned not only with how the sociology of knowledge “los[t] the right to call itself a normative discipline” but with “how might it regain that right”(1988:263). Being a philosopher of science, he is not content with merely suggesting that changes in practice be made. Rather, Fuller proposes a philosophical basis for this shift, tracking backwards again to the original source of the problem in HPS. Tracing the narrow concept of the normative to “the idealist bias in classical epistemology”, he recommends its undoing through the admission of a materialist perspective (1988:275). That is, Fuller suggests that the problem of knowledge include the economics of its production.

This is a valuable beginning, a move towards establishing a coherent basis for a body of applied STS research. But Fuller goes further still, not satisfied with merely outlining a way of recasting the philosophical roots of STS in order to permit a more normative engagement with the groups studied. He envisions practical programs devoted to training students specifically in knowledge policy studies (1988: Appendix C), from which they will go forth to actually work in the field on policy issues.

So, as De Laet has put it, Fuller’s goal is

the redefinition of, and the setting of a new agenda for, science and technology studies. STS has emphasized the social, political, and mundane character of knowledge practices. It has begun to analyze knowledge as an unmistakably practical and intensely political *agent*, rather than a cognitively defined *state of mind*. According to Fuller, that awareness about knowledge has not yet permeated the practices of STS itself. STS has yet to acknowledge and use the knowledge that *it* produces as an agent; an agent that actively dissolves boundaries between science and society, insides and outsides, and disciplines – with the reform of the academy as its goal. (De Laet 1995:491)

Fine, so what about archaeology?

The reader may at this point be thinking ‘Yes, well this is very nice that some within STS may at last be moving towards applying its knowledge – which it acquires through empirical study of the way that science works – to help make normative decisions regarding science policy. But what does this have to do with archaeology?’ And a perfectly legitimate question this would be; after all, Fuller has not really been writing for the archaeologist or even for the ‘generic scientist’, but to those in HPS and STS.

My answer is threefold:

First, archaeologists have historically had a habit of drawing upon the history and philosophy of science whether or not it particularly applies to their situation, and so they may as well have a look at some which seems suited to their needs. Certainly it would seem wise to keep abreast of the critiques of earlier HPS which Fuller and other scholars currently publishing sometimes provide (see “An Aside on Trojan Horses”, below).

Second, if Fuller’s vision achieves fruition, and Social Epistemology does become a prominent program within STS and model for knowledge policy-making, then perhaps we will start to see those from outside the discipline studying the workings of archaeology and offering suggestions about how disciplinary practice, heritage management, and research programs might be improved. This would be interesting indeed and archaeologists might do well to think about how they would greet such a development.

Third, those who – like me – locate themselves within archaeology and hope that their research will help the discipline in some way, but find themselves needing externalist methods to observe and understand the contexts and processes of the production of archaeological knowledge, might find it useful in the future to keep an eye on debates concerning Social Epistemology, and research done which follows the broad strokes which Fuller has outlined. Perhaps some philosophical foundations or methodological inspiration may be found there.

Why actively worry about policy issues relating to archaeology, both within and beyond the academy?

This is a question that does come up, for those who are not involved in archaeology policy issues are very often content to stay that way. But perhaps they shouldn’t be. First of all, there is the fact that nonarchaeologists are very much involved in those decisions, sometimes even controlling them. And certainly they *should* be involved, for much research is ultimately supported by society at large, and it is too much to ask that there be no external regulation of research endeavours. (Fuller, like Latour, has made this point vehemently; see Fuller 1991:170-171.) But archaeologists might do well to take a more active role in some of these discussions. For example, Wiseman notes that archaeologists are often comparatively uninvolved in matters of heritage management in the USA, and that this is less than optimal for all concerned:

The problem is that preservation and management require a commitment to long-term care, a concern of great interest to the public, but one to which archaeology as a discipline has not responded well... Many preservationists, with some justification, have come to view archaeologists with some suspicion, as people who put monuments at risk by uncovering them, and then walk away, leaving to preservationists (or to no one) the critical decisions on what should be preserved, in what way, how, and for how long. These ethical issues should be concern of all archaeologists. And heritage management, as a field, would benefit from a greater involvement of archaeologists... (Wiseman 1998:3-4)

A second reason why some archaeologists may wish to actively conduct research on issues that will help in policy formation is that the default – the lack of such proactivity – is not really a default, but itself a choice. Fuller has articulated this clearly, in response to a critic who suggested that a Social Epistemology approach might be overly controlling:

as tends to befall the analysis of other forms of social action, “science policy” is treated as something that occurs only when traces of intervention are left (e.g., added funding or regulation), but not where such traces are lacking (e.g., allowing science to continue as is). Consequently, it is easy to forget that a refusal to steer the course of sciences is itself a very potent form of science policy. (Fuller 1991:170-171)

The good news, of course, is that many archaeologists have already been rising to the challenges of positively shaping archaeology’s development as a discipline and professional practice. Wiseman addresses this, too. First, he identifies some shortcomings in American archaeology training, such as the fact that university archaeology programs rarely provide the practical training needed by those going into CRM archaeology, and that courses on “archaeological administration, ethics, and the law, so much needed by professional archaeologists, are still rare” (1998:5). Thankfully, Wiseman notes, these failings are now being acknowledged. A collective workshop which included representatives of many of the professional archaeology groups in North America was convened in 1998, to address those questions of curriculum, and quickly recognized that “while the social, political and employment contexts of practicing archaeology have changed enormously over the last 20 years, curricular structure and content have been left relatively unaltered” (quoted by Wiseman 1998:9). Now, there are moves afoot to redress these discrepancies.

Perhaps others will follow suit; after all, now is surely a good time for archaeologists to consider in detail, and debate in depth, the questions identified by Fuller (1997d:3) as the central issues of Social Epistemology – that is, “who should be doing what, in what setting,

to what end(s), for whose benefit and at what cost to whom?" As Fuller hopes, use of the methods of STS may well provide the information and perspectives needed to help us answer these normative questions.

An Aside on Trojan Horses

I have suggested above that some sectors within STS and HPS are producing work which archaeologists could find useful. However, some reflection on cross-disciplinary borrowing is also needed here.

One of the difficulties I encountered in my initial explorations of HPS and STS was the problem which many archaeologists have when attempting to work across disciplinary lines; that is, the mission of intellectual cartography, or locating the contours of another discipline's discourse, finding the work which is likely to be most useful, and appreciating its context. [How I wish that there had then been an introductory book providing an overview of STS. Luckily, there is one now, a nice accessible text by David Hess (1997).] I have found Fuller's writings to be quite useful in this regard. For examples one might see Fuller 1996, and 1990; the latter gives a very nice account of some of the tensions and intertwinings of key issues in philosophy and sociology of science, and very clear and provocative analysis of current work and recent history in these fields (e.g., the effects of Quine's underdetermination thesis on the discourse in sociology of science).

What I am finding most interesting at the moment on this problem of background, however, is Fuller's critique of Thomas Kuhn. I am interested because Kuhn's name is cited by archaeologists very frequently in comparison to other figures in HPS; his *Structure of Scientific Revolutions* has been highly influential not just in the intellectual community at large, but within archaeology in particular.³ Valerie Pinsky summed up the extent to which Kuhn's version of disciplinary history shaped not just the understanding of archaeology's history, but its actual development, especially in North America during the 1970s:

Early optimism over the potential of the new archaeology to promote the development of a scientific anthropological discipline led a number of its

³ This is despite Kuhn's avowed ignorance of social science. Apparently, when asked to attend a conference on *Structure* and the social sciences, Kuhn wrote back: "I know a great deal less than I should, and in any case virtually nothing at all, about social science." (cited in Fuller 1992:246)

proponents to claim that the new archaeology represented a revolutionary break with traditional archaeology. This claim was given authority by polemical appeal to Kuhn's popular but highly controversial model of scientific development in the physical sciences through a discontinuous process of incommensurable paradigm replacement Despite considerable disagreement over the precise degree of fit of Kuhn's model with actual archaeological developments ... appeal to this model has dictated and to a considerable extent constrained the nature of discourse about these developments. (Pinsky 1989a:13)

This reality takes on a particularly intriguing and quite dismaying aspect in light of Fuller's critique (1992, 1997a, 1997b, 1997c), which characterizes Kuhn as a "Trojan Horse." There is, of course, an abundant literature on Kuhn from a host of perspectives – with much disagreement – but Fuller's analysis interests me here because it highlights the context in which Kuhn produced *Structure*, and the ideas which, through it, became entrenched in Anglo-American social science. It is, I think, worth quoting at length here.

The key to understanding the impact of Kuhn's work is the dedication of that classic work to Harvard President James Bryant Conant, an administrator of the US atomic bomb project, staunch Cold Warrior and Kuhn's early academic mentor. It was Conant who conceived of the courses "General Education in Science", in which Kuhn developed his famous conception of science as iterated cycles of paradigms and revolutions in the decade following World War II. The constituency for Conant's courses was returning soldiers whose education was funded by the U.S. government. They were expected to become managers who would be increasingly asked to decide on projects containing a strong scientific component. From Conant's standpoint, it was important that they remained friendly to science, despite public calls for greater regulation of scientific research in the wake of the US atomic bombing of Japan (which Conant strongly encouraged). Conant's pedagogical strategy was to show that the scientific mindset has remained constant from Galileo and Boyle to Einstein and Heisenberg, changes in the material conditions of research notwithstanding. Kuhn was among a number of Harvard graduates who had become disillusioned with their career prospects as scientists while serving in WW II. Nevertheless, they sympathised with Conant's attempt to perpetuate what was quickly becoming a nostalgic "little science" image of enquiry. Conant's courses – and Kuhn's book – downplayed the role of economics and technology in the conduct of enquiry, let alone the political pressures exerted on science from the larger society. Unwittingly, this selective vision of science obscured traditional differences between the study of the natural and social worlds, which emboldened social scientists in the 1970s to claim that they too were in hot pursuit of paradigmatic enquiry. Ultimately, this strategy enabled them to purchase academic respectability in return for muting their critical sensibility.... (Fuller 1997c:1)

At most, this can make one feel rather forlorn about at all the ink spilled, and good intentions misdirected, in writings about where archaeology should go next to achieve the goal of becoming a fully paradigmatic science. At the very least, this effectively reminds us of one of the central points of STS – that scholarly work is affected by the conditions of its production. And of course, this applies reflexively: work in HPS and STS is affected by

these variables as much as anyone else's is. As always, interdisciplinary borrowing brings with it risks of the accidental adoption of unrecognized agendas.

In short, *caveat emptor*.

Some final thoughts on SE

I chose "Social Epistemology" as one of the Three Escapes for this chapter for several reasons: I hadn't seen anything written on it yet in archaeological circles, even though it seems promising; Fuller's mission is clearly relevant to some of my own research problems, in its focus on both the "ought" and the "is" of knowledge production (Fuller 1988:xi); and Fuller's writings can offer some good overviews and useful direction for the new arrival in HPS and STS territory. I must, however, add two additional observations.

First of all, of course, Fuller is not the only one in HPS and STS who is changing direction (e.g. see Stump 1996 for a brief synthesis), any more than he is the only one to provide useful background on STS, or social/political critique of Thomas Kuhn. There is much provocative work out there in these areas, which for the archaeologist may be as well worth exploring as traditional philosophical questions of how to see the world, time, space, other people, the nature of historiography, ethics, etc. I hope to read more about other archaeologists' explorations in STS as time goes on.

Second, there is an increasing quantity of excellent work which addresses the particularities of the archaeological enterprise from an HPS/STS perspective, and it is probably wise for archaeologists to start there before backtracking into works in HPS/STS which are devoted to scrutinizing subjects other than archaeology. Alison Wylie's work (e.g., Wylie 1989a, 1989b, 1985, 1992, and 1997) is irreplaceable in this regard, not only for her discussions of archaeological reasoning and disciplinary structure, but also for her analyses of archaeology's previous entanglements with philosophy.

ON FEYERABEND'S 'DEMOCRATIC RELATIVISM' AND ARCHAEOLOGY⁴

Feyerabend's influential challenge to the Popperian view of scientific progress is summed up in the preface to his book *Against Method*, originally published in 1975:

'Anything goes' is not a 'principle' I hold – I do not think that 'principles' can be used and fruitfully discussed outside the concrete research situation they are supposed to affect – but the terrified exclamation of a rationalist who takes a close look at history.
(Feyerabend 1987a:vi)

Feyerabend has been widely misunderstood. Parts of his work in the history and philosophy of science are extremely technical, while others are phrased in polemic which lends itself to selective quoting out of context, and hence misapprehension. In one sense, though, the entirety of the book which caused most of the difficulty was out of context, for *Against Method* was intended to have been published in tandem with a work arguing 'For Method' by Imre Lakatos, a plan thwarted by Lakatos' death. Would its reception have been different in this case? Probably⁵. *Against Method*'s nature as polemic, a collage of ideas, a letter to a friend (Feyerabend's own descriptions) – *not a systematic treatise or platform* – might have been more clear, and the recent history of Western philosophy might read quite differently. But as it was, the book was published alone, and Feyerabend's notoriety stemmed in part from not always saying precisely what he meant, as he cheerfully admitted to in his autobiography, *Killing Time*. Ultimately, this cost him a great deal, for in addition to the healthy debate that *Against Method* spawned, there was also much unhealthy debate. Feyerabend later wrote, "I often wished I had never written that fucking book" (1995:147). In retrospect, he begrudged the time and effort he spent responding to the harsh criticism and clarifying his position: "Writing and rewriting tedious chapters about tedious things wasted precious time that I could have spent lying in the sun, watching television, going to the movies, or possibly even producing a few plays" (1995:148).

⁴ Here, I am greatly indebted to Cornelius Holtorf, whose discussions of Feyerabend, both in print (e.g., Holtorf forthcoming) and in personal conversations, prompted me to consider Feyerabend's work more carefully.

⁵ This is not the only death of a philosopher which changed the course of Feyerabend's career; he originally had meant to do postdoctoral study with Wittgenstein, but switched to Popper after the former's death (Feyerabend 1995:86). The mind boggles...

Reading those works today is an intriguing exercise, especially given the benefit of their author's hindsight. For example, Feyerabend's *Science in a Free Society* is a fascinating book, half essay and half spirited response to the critics of *Against Method*. The author mercilessly mocked those who ~~have~~ misunderstood him, and seemed profoundly amused by the fact that some people read *Against Method* as a consistently serious treatise and totally missed his ironic 'in-jokes' with Lakatos (e.g. 1978:185). He aimed hilarious jibes at other philosophers of science, mocked them as "a small gang of autistic intellectuals", and pitied them, for they

take their intellectual games seriously. So, poor things, they have the worst of all possible worlds. They are neither 'relevant' nor do they have any fun. Small wonder they are upset at somebody who does. (1978:209)

This kind of snickering commentary makes *Science in a Free Society* a gaspingly funny read; it is easy to laugh along with Feyerabend the iconoclastic wise-cracking philosopher-hero. But one ought not to forget, as Feyerabend himself passionately argued on better days (e.g. 1987a:3), that ideas are serious in their transformative potential. It is for this very reason that Feyerabend is worth the attention of any scholar, including archaeologists as they work through the consequences of a world without comfortable and easy rules. Feyerabend sums up this absence of boundaries in two statements which may look to be common sense on the surface, but are in fact radical beginnings:

(A) the way in which scientific problems are attacked and solved depends on the circumstances in which they arise, the (formal, experimental, ideological) means available at the time and the wishes of those dealing with them. There are no lasting boundary conditions of scientific research.

(B) the way in which problems of society and the interaction of cultures are attacked and solved also depends on the circumstances in which they arise, the means available at the time and the wishes of those dealing with them. There are no lasting boundary conditions of human action.

(Feyerabend 1987b:304)

Feyerabend drew out many implications of these propositions; I shall only highlight a few here.

Fairness in Debate: the Underdog in Absentia, and Irrationality in Academia

Feyerabend had much to say about the peculiarities of intellectual discourse that can serve as a healthy reminder to anyone engaged in academic pursuits. He was often aggravated by

the tone used by philosophers of science, and sometimes by scientists too. And so, one of his favourite activities seems to have been picking something against which some of them had argued vociferously, and using his considerable polemic talents to instead argue for it. His point? That all views deserve a fair hearing, an audience listening in good faith and humility, then responding reasonably and without prejudice. A famous example comes from his Second Dialogue (Feyerabend's voice can be heard in the words of B):

A: Nobody takes astrology seriously.

B: I am sorry to contradict you – a lot of people take it seriously.

A: Nobody who has an inkling of science takes it seriously.

B: Of course not – science is now our favourite religion.

A: Do you seriously want to defend astrology?

B: Why not if the attacks are incompetent?

A: Are there not more important things?

B: Nothing is more important than to prevent people from being intimidated by ignorant bullies. Also astrology is an excellent example of the way in which the ignorant – i.e. scientists – joining hands with ignoramuses – philosophers of science, for example – have succeeded in deceiving everybody.

(Feyerabend

1991[1976]:60)

Feyerabend later commented that he had no particular fondness for astrology: “much that is written in this area bores me to tears” (1991[1976]:74). However, others' responses to it could turn his boredom with astrology to serious antipathy for its opponents. Using as his example the anti-astrology statement signed by 186 scientists, including Nobel Prize winners, and published in *The Humanist* in 1975⁶, he wrote:

You see, I would not at all object if the opponents of astrology were to say: we do not like astrology, we despise it, we shall never read books about it and we certainly shall not support it. This is quite legitimate... But our scientists, our rational and objective scientists, do not just express their likes and dislikes, they act as if they had arguments and they use their considerable authority to give their dislikes force. But the arguments they actually use only show their pitiful illiteracy... (1991[1976]:66)

And so Feyerabend considered that “astrology is an excellent example of the way scientists deal with phenomena outside their area of competence. They don't study them, they simply curse them, insinuating that their curses are based on strong and straightforward arguments” (1991[1976]:74).

⁶ Some scientists felt discomfort too. Carl Sagan, to his credit, refused to sign this statement – not because he believed in astrology, but because he felt its tone was too authoritarian (Sagan 1996:285).

For me, this recalls some attitudes towards ‘alternative’ archaeology mentioned in Chapter 2. It also resonates with personal experiences, particularly in informal contexts, with archaeologists who believe that in discussions of the ideas of people outside the academic arena – especially if those people are not present – ordinary conventions of good argumentation may simply be dispensed with. If the point is then made, even gently, that their argument (or diatribe) is sloppy or unfair, one is often assumed to support the opposing view, *i.e.* to believe in extraterrestrial interventions on earth, invisible lines of energy on the landscape, etc. The point, that a certain level of debate should always be upheld, is not always understood as such.

Feyerabend, interestingly, also makes an explicit link with archaeology in the Second Dialogue, recalling the early resistance to archaeoastronomy within the British academy as an example of the irrational workings of disciplinary discourse in the sciences (1991[1976]:68-70)⁷. Feyerabend is a little unfair, quoting from R.J.C. Atkinson’s ‘*Mea culpa d’un sceptique*’ in the *Journal of the History of Astronomy* (so-called by Daniel 1975:82), to apparently extract and then gloat over the fact that irrational reasons of conservatism and unfamiliarity had affected archaeoastronomy’s standing. This was, in fact, what Atkinson was admitting to; indeed, in reading papers of that time, one can sometimes imagine it as a golden age when archaeologists’ reasons for believing what they believed were deliberately made quite transparent. However – especially given the intervening period in archaeology of the rhetoric of scientific substantiation – Feyerabend’s larger point here, that scientists are never justified in simply relying on their status as experts to win an argument, stands:

B: ... Take scientists in a certain area of research. They have basic assumptions which they hardly ever question, they have ways of viewing the evidence which they regard as the only natural procedures, and research consists in using the basic assumptions and methods, not in examining them. It is true the assumptions were once introduced to solve problems, or to remove difficulties, and that one knew then how to see them in perspective. But this time has long gone by. Now one is not even aware of the assumptions made one defines research in their terms and regards research that proceeds in a different way as improper, unscientific and absurd. You say that scientists often make fools of themselves when pontificating outside the area of their competence but must be listened to when speaking about things they have studied in detail. Well, they never studied assumptions of the kind I just

⁷ Some lively contemporary commentary can easily be found in most issues of *Antiquity* in the late 1960s and early 1970s.

described and yet their research could not start without them. This means *that every part of science lies at its periphery and that expertise is never an argument.* (1991[1976]:70-1)

So what does this mean in practice, in the academy and society?

Relativism and Space for Dissent

One of the most influential philosophers of the twentieth century wrote of striving towards

an open society which rejects the absolute authority of the merely established and merely traditional while trying to preserve, to develop, and to establish traditions, old or new, that measure up to its standards of freedom, of humaneness, and of rational criticism.

Drop the last three words, and it might well be Paul Feyerabend speaking. However, it was actually his one-time mentor and later nemesis, Karl Popper, in the preface to his book *The Open Society and Its Enemies* (1950: viii) – which, notably, Feyerabend later translated.

I mention this to highlight three points, which only seem fair to mention: that Feyerabend is best understood in the context of dialogue with his contemporaries; that the problems of knowledge and society are perennial questions in philosophy; and that a concern with freedom and antiauthoritarianism is not solely the domain of the methodological anarchist. Feyerabend formed his main opinions, according to his autobiography (1995), fifty years ago. He began *Against Method*, and published some of its arguments, before I was born, finally retired when I was an undergraduate, and to the world's loss, died in 1994. I cannot imagine calling a philosopher's work irrelevant or obsolete – but it seems inevitable that I see the world differently than Feyerabend did. So I believe Feyerabend's work cannot be the place to look for *comprehensive* understandings of the role of science or archaeology in our society at the turn of the millennium, or guidance in creating actual programs for educational or social reform – but, that said, he asked questions and made observations of lasting import.

Feyerabend as a relativist social thinker

John Preston wrote of Feyerabend's political philosophy that “it has received little attention from within the academic philosophical community, probably because of a

perceived failure to live up to the relevant standards of argument. Philosophical commentators tend to agree that his political theory was not well thought out” (Preston 1997: 207). He also enumerates the generally accepted difficulties with Feyerabend’s democratic relativism (Preston 1997:207-9). But considerable uncertainty and possibly the most radical criticism of Feyerabend’s relativist stance came, in the end, from Feyerabend himself.

Science in a Free Society is, in fact, extremely cagey on what good relativism would look like in practice, apparently because Feyerabend already realized that he was swinging a double-edged sword. To begin with, he decided in 1964 that in the case of the long-awaited admission of minorities into Berkeley, he and other professors there were still engaged in a suppression of sorts:

Equality meant that the members of different races and cultures now had the wonderful chance to participate in the white man’s manias, they had the chances to participate in his science, his technology, his medicine, his politics.... [My] task – this now became clear to me – was that of a very refined, very sophisticated slave-driver. (1978:119)

But years later, he wrote “I now realise that these considerations are just another example of intellectual conceit and folly. It is conceited to assume that one has solutions for people whose lives one does not share and whose problems one does not know”. Feyerabend then asked of himself, “So what remains?” and answered thus: “Two things remain. I could start participating in some tradition and try to reform it from the inside... another possibility is to start a career as an entertainer.” (1978:121-122). This view closes his arguments about knowledge and education in *Science in a Free Society*, and is essentially recapitulated without development in the 1987 version of *Against Method*. However, Feyerabend’s *Farewell to Reason* was much more comprehensive on relativism, effectively contradicting Bell’s dismissive assertion, in “Anarchy and Archaeology” (1991),⁷⁶ that “contemporary relativism is much the same no matter where one finds it.”

Feyerabend made it clear that he hadn’t worked out the details – and indeed, that there was no way to do it. His commitment to relativism was not straightforward, for example when he wrote that “none of my critics seem to have noticed that I introduce ‘anarchism’ as a medicine, not a final philosophy and that I envisage periods in which rationalism is preferable” (1978:186 fn). He wavered more in 1989’s *Third Dialogue*: “I was a relativist, at least in one of the many meanings of this term, but I now regard relativism as a very

useful and, above all, humane approximation to a better view... [which] I haven't found ... yet" (1991: 156-7). Finally, Feyerabend went much further in his autobiography and last work, *Killing Time*, concluding that "objectivism and relativism not only are untenable as philosophies, they are bad guides for fruitful cultural collaboration" (1995:152, emphasis mine). Preston terms this a "general recantation of relativism" (1997:209), but this may be too absolute a reading of Feyerabend's intent. I think the relevant passage in the autobiography hints at a new understanding, but also at an unwillingness to spend time writing about it.

Accommodating alterity in practice

So, then, how might archaeologists best achieve a positive coexistence with diverse perspectives about the past? Feyerabend unquestionably had some relevant ideas:

the necessary institutions *already exist*: almost all traditions are part of societies with a firmly entrenched protective machinery. The question is therefore not how to *construct* such a machinery, the question is how to *loosen it up* and to detach it from the traditions that are now using it exclusively for their purposes; for example, how to separate state and science. The answer to *this* question is that the methods employed cannot be discussed independently of the tradition that wants to achieve equality and the situation it finds itself in. The democratic relativism I have discussed will not be imposed '*from above*', by a gang of radical intellectuals, it will be realized *from within*, by those who *want* to become independent, and in the manner *they* find most suitable.... What counts are not intellectual schemes, but the wishes of those who want change. (1981:33)

So far so good. Indeed, what people actually want is rather pertinent. For example, some groups have no desire to be part of institutions whose legitimacy they reject. (It is possible to overestimate the importance of formal education!) In my experience, a significant number of Earth Mystics would positively gag at the prospect of incorporation within a structured academic institution. Similarly, including a host of traditions within an essentially authoritarian structure might be a useful interim measure, but will not ultimately obviate the problem of authority – there will simply be more experts giving their views, and positions which were originally those of critique will develop orthodoxies of their own.

But as should be expected, Feyerabend's views are not quite as applicable to archaeology in 1999 as they were to the American 'big science' of 1960s and 1970s, the context of his

critique's origination. Neither archaeology's influence nor its resource consumption are of the order of the scientific programs (space exploration, conventional medicine) which seemed to most concern Feyerabend when he wrote of the need to give alternative "ideologies, practices, theories, traditions... a fair chance to compete" (1978:103) – a sentiment also evoked by Holtorf (forthcoming). Compete where? In the university? In the 'outside world'? On school boards? In the marketplace? For government financial support? In the case of archaeology, by most criteria, it must be observed that many alternative traditions are already doing very well indeed. Surely Graham Hancock doesn't need a university chair in archaeology to influence a wide audience. Erich von Däniken probably doesn't need a research grant to subsidize his travel. Earth Mysteries journals and websites have circulations that surely trump those of most professional archaeology journals. The treasure-hunters of the world outnumber the archaeologists, and the dowsers outnumber the users of ground-penetrating radar. There are more neopagans in the U.K. than professional archaeologists, and much of the time, their access to archaeological sites is not much more restricted. Creationists successfully make themselves heard on public school boards in the U.S.A. (And even university archaeology departments harbor, as they always have, pockets of unorthodoxy; as discussed earlier in this dissertation, a dichotomy of 'archaeologists vs. others' doesn't hold, for some professional archaeologists *are* pagans, dowsers, psychics, etc..)

In short, academic archaeologists are a minority, usually are not so very powerful, and our orthodoxies are limited in their reach. Yes, at times, archaeologists have privileged access to sites, to artifacts, to funds, to the power of legitimacy, to political influence, and to students' minds; however, other voices speak and are heard, other beliefs are held, and other practices are enjoyed. In part this is due to the past labours of forward-thinking people to ensure socially responsible state policies regarding freedom of speech, to individual commitments from those in 'orthodox' positions, and in part it is due to popular will and the force of the market.

That said, of course there are problems, and *of course* it is past time for our discipline to officially recognize the legitimacy of many other traditions which are, after all, here to stay.

Disengaging from a doctrine of absolute scientific truths, as Feyerabend requires of us, means reengaging with the ethical. The power of Feyerabend's critique of science is not

only in the freedom from an obligation to choose which theory is the best, it is also in the freedom to make judgements on grounds other than the epistemological. Importantly, this applies in and out of the academy. Obviously, not all traditions (orthodox or alternative) are benign and mutually compatible. Some are distinctly hostile to democratic or pluralist ideals. Further, democracy is not necessarily stable; a democracy does not sustain itself simply by being democratic. This means ethical dilemmas sometimes, and we cannot avoid discussing them. Feyerabend made important observations on this process, however, criticizing two views:

(C) that science and humanity must conform to conditions that can be determined independently of personal wishes and cultural circumstances...

(D) that it is possible to solve problems from afar, without participating in the activities of the people concerned.

He continued:

(C) and (D) are the core of what one might call the intellectualistic approach to (science and) social problems. They are a matter of course for academic Marxists, liberals, social scientists, businessmen, politicians eager to help 'underdeveloped nations' and prophets of 'new ages'. Every writer who wants to improve knowledge and save humanity and who is dissatisfied with existing ideas (reductionism, for example) thinks that salvation can only come from a new *theory* and that all that is needed to develop such a theory are the right books and a few clever ideas. (Feyerabend 1987b:304-5)

And so, enough generalities. Good recommendations about how to move forward in accommodating alterity are much more easily and effectively made in specific circumstances, when we know who is there, what they want, what the contested issues are, what is ultimately at stake, and how the communities involved work.

" an abstract discussion of the lives of people I do not know and with whose situation I am not familiar is not only a waste of time, it is also inhumane and impertinent."

(Feyerabend 1987b:304, emphasis original)

RICHARD RORTY'S PRAGMATISM AND THE ARCHAEOLOGIST IN SOCIETY

Richard Rorty is both one of the most prominent intellectuals in the U.S.A. today, and “probably the most influential critic of the epistemological enterprise in contemporary English-speaking philosophy” (Haack 1995:126). For this reason alone, he would be an interesting thinker to consider here, but in addition, some archaeologists have recently begun to work with his ideas (see below). The corpus of his writings is extensive (see Bibliography in Saatkamp ed. 1995), and there has been a minor industry in ‘Responding to Rorty’ over the last few years, so raw material is in abundant supply. This supply is enhanced by a century of philosophical history, for Rorty and similar thinkers have drawn on a long tradition of American pragmatism, from William James through F.C.S. Schiller, and John Dewey (Flew 1979)⁸. It is important to note for the record that Rorty’s pragmatism is regarded by many other pragmatists as “highly idiosyncratic” (Bernstein 1995:62), but this is of minor import here; it is a place to start.

In their book on varieties of relativism, Harré and Krausz (1996:189-206) profile three ‘strong’ relativists or ‘philosophical anarchists’, i.e. thinkers who reject universalism, objectivism, *and* foundationalism.⁹ K.J. Gergen is the first, Paul Feyerabend is the second, and Richard Rorty is the third. In the end, Harré and Krausz conclude that “The project of

⁸ The claim is frequently made that the formal logician C.S. Peirce was the father of pragmatism; however, Bernstein describes how “pragmatism is the movement that began with James’s misunderstanding of Peirce” (1995:57).

⁹ Harré and Krausz categorise forms of relativism in two ways: first, by topic (semantic relativism, ontological relativism, moral relativism, and aesthetic relativism); and second, by what the relativist negates.

“(a) *By denying the thesis of universalism*: All people at all times and in all cultures could be brought to agree on the assessment of meaningfulness, existence, goodness (moral worth) and beauty (aesthetic value) of the relevant entities. Relativist denial: No such agreement is possible.
(b) *By denying the thesis of objectivism*: assessment of meaningfulness, existence, goodness and beauty and the foundations on which they rest are capable of being presented from a point of view that is independent of the point of view of any human being in particular and of human kind in general. Relativist denial: No such point of view could exist.
(c) *By denying the thesis of foundationalism*: there is a given and permanent and only one given and permanent foundation for all assessments of meaningfulness, existence, goodness and beauty. Relativist denial: No such foundation could exist.” (1996:23-4)

Many relativisms deny only the thesis of universalism; others combine denials of two of the theses with the acceptance of the third. ‘Strong’ relativists, or those who deny all three theses, are rather less common. (1996:25) See Feyerabend’s *Farewell to Reason* for another system of categorising relativisms.

defining and defending a relativism so radical that it rejects all three of the root ideas that define strong absolutism, at least as it is realized in the writings of our exemplars, Feyerabend, Gergen and Rorty must be adjudged a failure” (1996:206).

Rorty probably doesn't mind this too much. The point of Rorty's program, after all, is in fact to be “post-Philosophical” (1982). On a pragmatist theory of truth, he has this to say:

truth is not the sort of thing that one should expect to have a philosophically interesting theory about. ... Pragmatists think that the history of attempts to isolate the True or the Good, or to define the words “true” or “good” supports their suspicion that there is no interesting work to be done in this area.... The history of attempts to do so, and of criticisms of such attempts, is roughly coextensive with the history of that literary genre we call ‘philosophy’ – a genre founded by Plato. So pragmatists see the Platonic tradition as having outlived its usefulness. This does not mean that they have a new, non-Platonic set of answers to Platonic questions to offer, but rather that they do not think we should ask those questions anymore. ... They would simply like to change the subject. (Rorty 1982:xiii-xiv).

This is not to say that Rorty does not believe in, or value truth. As Jenkins (1995:125) specifies, Rorty does give in to the need to define the concept, and this is his formulation: “truth is ‘the name of whatever proves itself to be good in the way of belief, and good, too, for definite, assignable reasons’.” So where is Rorty's great challenge, then? I take it to be in his reiteration of James' antiessentialism, in his abandonment of the epistemological enterprise, in his belief in conversation as the basis for human solidarity, and his belief that fostering such conversation is the responsibility of both the philosopher and the citizen. I shall give Rorty the floor on each of these, in turn.

First, antiessentialism, which Rorty characterizes as one feature of pragmatism:

truth is not the sort of thing which has an essence.... those who want truth to have an essence want knowledge, or rationality, or inquiry, or the relation between thought and object to have an essence. Further, they want to be able to use their knowledge of such essences to criticise views they take to be false, and to point the direction of progress toward the discovery of more truths. James thinks these hopes are vain. There are no essences anywhere in the area. (1982:162).

And so there is no point in looking for them in the tradition of epistemology. Rorty contends. And no point in filling the void left by the abandoning of the search; rather, we should move on, past epistemology and past its absence, to what he terms hermeneutics.¹⁰

the desire for a theory of knowledge is a desire for constraint – a desire to find “foundations” to which one might cling, frameworks beyond which one must not stray, objects which impose themselves, representations which cannot be gainsaid.... the demise of foundational epistemology, however, is often felt to leave a vacuum which needs to be filled.... In the interpretation I shall be offering, “hermeneutics” is not the name for a discipline, nor for a method of achieving the sorts of results which epistemology failed to achieve, nor a program for research. On the contrary, hermeneutics is an expression of hope that the cultural space left by the demise of epistemology will not be filled – that our culture should become one in which the demand for constraint and confrontation is not longer felt.... epistemology proceeds on the assumption that all contributions to a given discourse are commensurable. Hermeneutics is largely a struggle against this assumption. (1979: 315-6)

Rorty then explains what this different starting point means for one’s orientation towards others:

Hermeneutics sees the relations between various discourses as those of strands in a possible conversation, a conversation which presupposes no disciplinary matrix which unites the speakers, but where the hope of agreement is never lost so long as the conversation lasts.... Epistemology views the participants as united in what Oakeshott calls an *universitas* – a group united by mutual interests in achieving a common end. Hermeneutics see them as united in what he calls a *societas* – persons whose paths through life have fallen together, united by civility rather than by a common goal, much less by a common ground. (1979:318).

This position has sometimes been considered to implicate Rorty as a hyperrelativist (see Jenkins 1995), one who has relinquished the ability to discriminate between good ideas and bad ideas. But this is quite thoroughly missing his point. He doesn’t suggest that there are no grounds left for judgement between views, merely that those grounds for judgement cannot and should not be reduced to formulae:

Except for the occasional cooperative freshman, one cannot find anybody who says that two incompatible opinions on an important topic are equally good. The philosophers who get called ‘relativists’ are those who say that the grounds for choosing between such opinions are less algorithmic than had been thought... the real issue is not between people who think one view as good as another and people who do not. It is between those who think our culture, or

¹⁰ N.B. Rorty’s use of the word “hermeneutic”, while not unrelated to other uses in philosophy (and in archaeology), is a little unusual here, and so those definitions should not necessarily be read into his statement.

purpose, or institutions cannot be supported except conversationally, and people who still hope for other sorts of support. (1982: 166-7)

So, then, what might a society based on such conversation look like? In short, its humane principles would supersede the need for hard-and-fast rules about truth. In the Introduction to *Consequences of Pragmatism*, Rorty proceeds to outline a post-Philosophical culture. He returns to this theme hopefully, in *Contingency, Irony and Solidarity*, as he sketches his utopia's ideal citizen, a character he calls the "liberal ironist":

I borrow my definition of 'liberal' from Judith Shklar, who says that liberals are the people who think that cruelty is the worst thing we do. I use 'ironist' to name the sort of person who faces up to the contingency of his or her most central beliefs and desires – someone sufficiently historicist and nominalist to have abandoned the idea that those central beliefs and desires refer back to something beyond the reach of time and chance. Liberal ironists are people who include among these ungroundable desires their own hope that suffering will be diminished, that the humiliation of human beings by other human beings may cease.

(Rorty 1989:xv)

This, then, is Rorty's dream for the future. He sees human solidarity as something which can be achieved "not by inquiry but by imagination, the imaginative ability to see strange people as fellow sufferers" and emphasizes that "Solidarity is not discovered by reflection but created" (1989:xvi). As to the role of philosophers in this endeavour, Rorty firmly believes it should be one of fostering reconciliation, not revolution, of "serving as honest brokers between generations, between areas of cultural activity, and between traditions" (1995:203).

And so, while Rorty's beginning point – that of jettisoning of essentialist and correspondence theories of truth – is an important place in its own right for archaeologists, his larger message of abandoning hopes for transcendence and instead living in the here and now, and maintaining conversations between people and between traditions, is one with which many archaeologists today can well identify. In a time when archaeologists have recognized and have begun to responsibly confront problems of contested heritage and histories, and are searching for humane accommodations and inclusions among the jumbled fragments of past, present, and future, they can use the encouragement.

Archaeology and Pragmatism

No doubt some archaeologists have always had pragmatic proclivities, in terms of the way they make decisions about which research to pursue, and how to involve others in their discussions. However, they haven't usually explicitly expounded upon pragmatism – it probably hasn't been necessary. In contrast, in the last three or four years, several archaeologists have begun to work explicitly with the concepts of pragmatism (Rortyan and others), suggesting that they offer an escape from the debates about the nature of archaeological knowledge which have preoccupied archaeological theorists for quite some time, and that they can help us to find a new conversation. Dean Saitta (1999) argues that although post-processualism's brief flirtation with radical relativism is essentially over, and although effectively in archaeology, "we are all realists now", we should not merely drop the discussion about knowledge and how we make it.¹¹ Rather, Saitta says, it should begin anew: we should follow Dewey's 1917 exhortation to turn our attention from the problems of philosophy, or those concerned with how we know, to the problems of men – that is, to how we should live.¹²

Such a position resonates at least in part with some articulated by prominent British postprocessualists. For example, Parker Pearson (1998) addresses the same general points, discussing the consequences of a Rortyan stance for archaeological practice; his conclusions recommend a focus on "what archaeology can do for society". Hodder (1998:216) contends that it is "dangerous to trust in the separation of ethics and epistemology", and argues that "the need to listen to other voices has taken on a particular urgency at the end of the 20th century" (214). Barrett concurs that archaeology "informs us

¹¹ Actually, even if archaeologists *are* all realists of some sort, I am not convinced that dropping even this part of the discussion is wise, given the vocality of the 'hard science' contingent still so evident in processualism, apparently comfortable ignoring most of the gains in understanding about the nature of archaeological reasoning and the dangers of misrepresenting scientific inquiry, gains hard-won by scholars like Alison Wylie (e.g. 1989a, 1989b, 1999).

¹² Although I like the sentiment being expressed, I cannot say I am fond of Dewey's wording, "the problems of men". I appreciate its original context, and its profoundly egalitarian intended meaning when adopted by Saitta (1999), but it is not phrasing that I should particularly like to see perpetuated, for try as I might in the spirit of Philosophy, I just cannot manage to feel included in the plural word "men", though I can with "mankind". Conversely, I might note that as a reader, I was pleasantly surprised with Rorty's (1989) use of feminine pronouns in his discussion of the liberal ironist. I don't mean to harp on linguistic conventions, merely to point out to other authors that as a female reader, I had surprisingly specific feelings of exclusion and inclusion, respectively, in encountering these texts.

of humanity in a way which is essential in our coping with a world polarised by wealth and poverty and divided by ideology. It will not solve the problems of the modern world, but it should allow us to think about how we live with humanity as difference" (1995:3).

Thus the general climate within at least some archaeological arenas would appear favourable to the growth of an explicitly pragmatic discourse. And certainly, the Rortyan distinction between foundationalist epistemology and hermeneutics is relevant to the specific questions of this dissertation (Denning 1997b). But what would a pragmatic approach to archaeological practice 'in the world' look like?

Emma Blake's discussion of local perceptions of Sardinia's Bronze Age monuments (in press) is an excellent illustration. Blake points out that seeing a Bronze Age tower, today incorporated within a farm, as a structure which "*is a shed, and a dog house, and a lookout point, and a source of curiosity*" is much more useful than treating its modern significances as merely "background noise", as archaeologists are often wont to do. She describes pragmatism's power to "supervene the problem of cultural relativism without sacrificing openmindedness and tolerance", a power which then permits an actual engagement with local traditions rather than a simple disengaged, uninformed respect for them. This is a different approach to pluralism, and the rewards of such an engagement are potentially many. Local approaches can include an emphasis on the individuality of a structure rather than categories of monuments: as Blake puts it, "While we must make a conscious effort to talk of 'context', and introduce a phenomenological element into descriptions of a site, [those who live nearby] manage to blend individual sites and their landscapes seamlessly." Modern local understandings can also underline the fact that objects, as well as sites, are usually experienced differently from the way that an archaeologist insists on seeing them. Origins can be obscured, meanings are not always cumulative, and at times, "Collective forgetting may be as significant as collective memory." This in turn can remind the archaeologist that receptions of monuments in prehistory could have been as haphazard and creative as they are today.

Carol McDavid's work also provides a rich example of pragmatism in practice, touching on matters of contested histories, political sensitivity, and the use of new media in the conversation of archaeology (1997, 1998, in press). At the Levi Jordan Plantation in Texas,

where excavation of nineteenth-century slave and tenant quarters has been ongoing for fifteen years, McDavid has been coordinating a public interpretation project, using the Web to

provide a way for contemporary actors to conduct critical dialogues with archaeologists, with each other, and with 'the past'. This work attempts to see the borderlands between archaeologists and 'publics' as flexible and permeable – to see archaeologists as audiences, and 'publics' as mediators and interpreters of archaeological data. It strives to deny the dualisms between these groups, in order to create a new discursive space from which a more democratic, socially engaged archaeology may emerge. (1998:1)

The project is remarkable archaeologically in part because McDavid is working directly with descendants of the people who actually owned or worked as slaves on the plantation – the inhabitants' names are known, there is abundant oral and documented history to draw upon, and the descendants are known to each other and embroiled in an ongoing negotiation of social and political roles. As McDavid puts it,

I frequently find myself sitting around someone's dining room table with people whose ancestors owned, or were owned by, each other. The notion that truth-finding is a result of our continuing experience with each other is something that, on one level, is taken for granted in interactions between these individuals. Indeed, they are very matter-of-fact about the notion ... that 'what happened' for a slave was obviously very different from 'what happened' for an enslaver.

(in press: 5)

McDavid (in press) relates that for her, the value of the pragmatic approach has been threefold. First, it enabled her to interact with members of the community as one participant in a conversation, rather than an authority, with the result that actually, she has *gained*, rather than lost, credibility within the community. Consequently, people have come forward with "more family stories, more documents, more pictures" than they had previously. Second, rewriting archaeological texts on the Jordan site in a deliberately "conversational voice" not only enhanced their accessibility, but actually gave her, as an archaeologist, a fresh, more reflexive understanding of the data. Third, a pragmatic approach has helped McDavid to see that there are no easy answers "when we must open the conversation to people with whom we disagree" – for example, the potential for conflict with white supremacists certainly exists – but that there is in such cases an opportunity for debate, and opportunity to make relevant and clear her own stance.

My personal experiences as a researcher, though very different from McDavid's, have led me to concur with her position. I agree wholeheartedly with the project of subverting the simple archaeologist-public dichotomy (Winters, McElearney and Denning 1997), with the

potential of hypertext in allowing a new kind of conversation (Denning 1996, 1997a), and with McDavid's experience – described on the Levi Jordan Plantation project page at <http://www.webarchaeology.com/html/about.htm> – that at the beginning, one hardly knows what the issues even are, and that an opening up of the conversation has to take place carefully and gently if mutual cooperation is to result.

As a final example, Duke and Saitta's paper, "An Emancipatory Archaeology for the Working Class" (1998:4), proposes "an epistemological reorientation around the pragmatist programme" in order to permit a new engagement between archaeology and contemporary society. The exemplar here is their community-based historical archaeology project at the Ludlow Memorial in Colorado, the site of a National Guard massacre of striking coal miners 85 years ago, and their mission is to collaboratively produce a history relevant to miners and others in the area today. Their position is this:

We believe that an emancipatory archaeology for the working class should be (1) less concerned with accumulating knowledge than with expanding social/organisational possibilities; (2) less concerned with 'getting things right' than with 'making things new'; and (3) less concerned with achieving hegemony for a particular theoretical position (or some new, eclectic synthesis of 'the best' of existing paradigms) than with 'keeping the conversation going' among the diverse sets of clients and constituencies served by archaeology. All of these interests are at home in a pragmatist epistemology. To the extent that what matters in pragmatism is our loyalty to other human beings struggling to cope rather than to the scientific hope of getting things right, it is an epistemology that better serves our activist agenda.

(1998:4)

In response, Frazer (1998:6) notes that some "archaeological adoptions of pragmatism seem more like justifications for faux populism, in terms of public outreach.... for some so-called archaeological 'pragmatists', it's business as usual, as far as archaeology's relationship to non-archaeologists goes, and it's business as usual as far as entrenched views of positivist, processual archaeological practice go."

Frazer has a good point; archaeologists have a habit of diluting radical discourses as they combine them with the rigors of practicing archaeology and relating to the outside world. Similarly, Saitta (1999:6) points to Reid and Whittlesey's (1999) version of pragmatism in archaeology as a curious one, essentially a strand of empiricism with its primary emphasis on challenging processualism, which is, emphatically, alive and kicking, especially in the U.S.A.. But equally, not everyone will concur with Duke and Saitta's interpretation of

pragmatism, or their dedication to an archaeological discourse of class (perhaps not even Rorty, who seems to have no love for Marxism¹³).

Where, then, is all this going? I think we should expect to see, and welcome, a steady increase in discussion about pragmatism in archaeology. Unfortunately, a cynical reading of the history of archaeological theory would suggest that ‘Archaeology and Pragmatism’ might take the familiar and predictable path of becoming an archaeological discourse in its own right – wherein archaeologists learn of pragmatism primarily through other archaeologists, occasionally mine the abundant primary literature on the topic for applicable quotations, probably ignore the basic philosophical issues at the core of the programme, and spend a good deal of time in insular debate arguing with each other about what pragmatism ‘really is’, not necessarily productively. (And thus profoundly missing the point.) Those who wish to argue against pragmatism’s aims will probably do so from the positions articulated by Rorty’s critics (e.g. in Saatkamp ed. 1995), that is, either the technical critiques, or the political critiques based upon the fact that he is a liberal American, and writes like one. Perhaps the discussions will at times degenerate into muddy replays of arguments about knowledge which essentially date back to the ancient Greeks. Such seems to be the fate of most interesting propositions in archaeological theory today. Perhaps this is as it should be; I don’t know.

I hope, however, and am inclined to believe, that there will be many bright spots as well. So far in the pragmatist agenda we have some promising beginnings and rallying points for a shift in archaeologists’ relations with the world; those committed to conversation about the past with others will in all likelihood seize the space afforded by pragmatism to begin some wonderful and constructive projects. I only hope that their endeavours do not end up marginalized within the ‘archaeology and public education’ ghetto – too often viewed as somehow secondary in importance to ‘real archaeology’ – but remain in full view within the theoretical debates which can influence the discipline’s course and goals as a whole.

¹³ E.g. Rorty commented, “there seems no particular reason why, after dumping Marx, we have to keep on repeating all the nasty things about bourgeois liberalism which he taught us to say.... Bourgeois liberalism seems to me the best example of [human] solidarity we have yet achieved, and Deweyan pragmatism the best articulation of it.” (1982:207).

Chapter 8

Conclusions

In the absence of tremendous confidence that there is a single truth, and a single way to it, and that one is in sole possession of both, the idea that archaeologists' responsibility to the public is to rehabilitate the deviants, educate the idiots, and blacklist the heretics, is untenable. I believe we can do better.

We might begin with remembering that Plato's portrait of Atlantis is two millennia our senior, that dowsers have wielded twigs for over five centuries, that people have dreamed of aliens for as long as humanity has looked to the stars, and that Western society has still somehow survived long enough to produce archaeology departments. We might continue by refusing to think or speak in easy dichotomies based upon wishful thinking about archaeology's privileged access to knowledge of the past. We might maintain standards of scholarly conduct, resisting casual generalisation, unfair labels, or flippant dismissal of others' ideas based on hearsay and dogma alone. We might appreciate that archaeological practice is socially situated and that whether or not we like it, we are in the world, and the world is in us.

There is no tidy resolution here; that is the point. Archaeologists' negotiations with contesting views are necessarily ongoing, context-specific, and difficult, and the ground will continue to shift underneath our feet. Sometimes we will handle this well – as some archaeologists always have – and sometimes we won't. But I think the best approach is to keep discussion alive, not to shut it down. On this, I have more to say.

Against silence

I am concerned that this dissertation can be taken to mean that I do not believe we can today know anything of what happened in the past, or that all interpretations are equivalent in their merit.

This is absolutely not my intent.

My point is that I do not believe the converse to be true. I do not believe we can know everything of what happened in the past, and I do not believe that it is simple and straightforward or necessarily desirable to rank interpretations according to their merit. I believe that certain knowledge is elusive, and that archaeologists do not work with methods or data which can provide it. And I believe that they should not act or write as though they do, for this has consequences outside of insular academic discourse.

I do not believe that it is unimportant what, for example, the dynastic Egyptians or the classic Maya or the prehistoric inhabitants of Wessex thought, or how they lived. I do not believe that it is *ideal* for their lives to merely be a playground upon which people writing from a cultural and temporal distance project their desires. But I am quite certain that people cannot be prevented from engaging in such projections and reinterpretations, and I believe the activities of archaeologists in this regard are not inherently separate from those of anyone else and should not be regarded as such.

My quarrel, then, is with unthinking privileging of academic discourse, confidence in final truths, and the idea that the professional generation of historical narratives somehow guarantees their validity and virtue. It is with notions that any other discourse is automatically inferior and should be ignored or challenged merely because of its source or the means used in its generation. This is of enormous importance to me because I believe that through the histories which some people create, and the choices others make to accept, tolerate, or reject those histories, our present and future are changed.

Let me explain. I described, in the initial pages of this dissertation, the usual near-invisibility of the past in southwestern Ontario, where I grew up and from where I now write. This invisibility is pervasive and culturally significant. As must be the norm in most places with historically high immigration rates, it is common to be a first-generation Canadian, and thus common for children to grow up hearing little about the histories of their parents' homelands or about Canada's past. People have often moved to Canada looking for a future, trying to escape the weight of their own pasts as individuals and groups. Perhaps this is as it should be, but it has resulted in a gap in our society, a tendency to look forward and rarely back.

It is not surprising that the general education system has rarely done much to compensate for this. The history I was taught before university consisted of caricatures of the European

explorers who mapped out the landscape, fur traders and voyageurs, pioneers, sketches of the fight for supremacy between the English and the French, and lectures on Confederation and early Canadian government which few students had the political consciousness to decode. The other picture of Canada's emergence as a nation, a tableau of violence and bloodshed, has not only been neglected, but deliberately painted over, and the noble watercolour version internalised and made real. Genocide, slavery, apartheid, forced resettlement, prison camps, martial law, and political executions are not included.

Last summer I read *Stolen Continents* by Ronald Wright (1992) – a book recommended to anyone with a desire to know about the bloody birth of the nations of the Americas, and what was lost in the process – in the reference room of the library in Thunder Bay, Ontario. After I read the passage in Chapter 15 about Canada's Indian Act, which for many years stated that "The term 'person' means an individual other than an Indian", I had to stop. This matter of words was a small thing compared to the many atrocities Wright described, but the literal enshrinement *in law*, in my home country, of this stance had a magnitude all its own, and I was ashamed that I had not known of it. Then through my tears I saw that a few feet away on the wall was a large shiny poster advertising *The Dictionary of Canadian Biography*. At the top, in large print, it read "Who is Canada?" The answer, wrapped around the picture of a clever-looking European man, was this text:

Canada is the early traders, the explorers, the army officers, politicians, clergy, businessmen, artists, builders and teachers. Their precise and informative biographies, set against the background of the period in which each lived, form the Dictionary of Canadian Biography, published by the University of Toronto Press and Les Presses de l'Université Laval with support from the Social Sciences and Humanities Research Council of Canada.
DICTIONARY OF CANADIAN BIOGRAPHY: A NATION IS THE SUM OF ITS PEOPLE.

This colonial view of history, of who counts in a nation's equation, of who belongs in Canada, has meant that people are still dying over First Nations land disputes. And until history teaches us to see differently, it is hard to believe that this will change.

This dissertation has not been about issues of contested histories or aboriginal people in Canada. Indeed, it has been primarily, ostensibly, about other people, ideas, and pasts, some of them thousands of miles away. But in their convolutions, these subjects touch, and these intersections must surely compel us to be humble in what we claim to know about the past, and what we think that knowledge is worth.

This is why, as an archaeologist, I cling to the hope of knowing what really happened, but accept that understanding is ever imperfect, insist that there are different views depending on where one stands, and reassert that archaeology and history are everything but neutral in this imperfect world. And this is why, no matter the context, I abhor exhortations to the systematic silencing of voices expressing dissent with generally accepted pasts. It is never good enough to assume without listening that there is nothing worthwhile to be heard. And it is never good enough to define other people in ways that normalise such refusals to hear.

On being Skeptical

Well you know
We all want to change the world...

(John Lennon, Revolution 1)

Earlier, I mentioned the influence of the Science Wars and the Skeptics' movement upon archaeologists seeking a model for coping with different views of the past. This is worth returning to because I believe, with Wylie (1999), that archaeologists might learn from the consequences of battles of ideology and rhetoric over science.

The Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP) was founded in 1976, is based in New York, and is now "the world media's leading source for critical and scientific information on the paranormal and the pseudoscientific".¹ CSICOP includes in its list of Fellows archaeologists John R. Cole and Kenneth Feder, physical anthropologist Eugenie Scott, anthropologist Thomas Sebeok, and some of the giants of modern science writing, including Richard Dawkins and Stephen Jay Gould. Others include Francis Crick, L. Sprague de Camp, Martin Gardner, Murray Gell-Mann, Susan Haack, Douglas Hofstadter, Ray Hyman, W.V. Quine, Stephen Toulmin, and Steven Weinberg. All in all, the list includes several Nobel laureates in science, science writers, a good number of philosophers of science, and several famous magicians. The Scientific

¹ CSICOP 1998 annual summary report, April 23/99, from list CSICOP-ANNOUNCE@listserv.aol.com

and Technical consultants listed include several more anthropologists, including Robert Funk, Laurie Godfrey, George Agogino, and Carla Selby. Most but not all are based in the U.S.A. There is, however, a substantial and growing network of affiliated organisations internationally, including the recently formed Association for Skeptical Enquiry (ASKE) in the U.K.

CSICOP's official organ, *Skeptical Inquirer: The Magazine for Science and Reason*, appears bimonthly on newsstands and is in its twenty-third volume, with an international distribution of 40 000 copies.² Their website (<http://www.csicop.org>) is professional and streamlined, with many links for buying books, joining mailing lists, or becoming a paid member, and their electronic mailing list is very active. The Jan/Feb 1999 *SI* includes information on a CSICOP Ph.D. research scholarship, solicitation of charitable bequests, a donation campaign for funding their Center for Inquiry's educational program and library, and details on its Ten-Year Plan for expansion. Email updates on the CSICOP mailing list indicate a determined campaign to gain influence over the mass media through the acquisition of shares in major media conglomerates, including News Corp, Disney and CBS. In short, CSICOP's Skeptics movement is a major and growing force.

Figureheads within the movement include Richard Dawkins, evolutionist extraordinaire and, since 1995, the first Professor of the Public Understanding of Science at Oxford, a chair endowed by Charles Simonyi of Microsoft. Some fellow scientists display an almost worshipful solidarity with Dawkins. Dust jackets of his books are covered with comments from scientific luminaries such as Francis Crick, who wrote of *The Blind Watchmaker*: "I

² It is important to note here that the CSICOP use of the word "skeptic" is idiosyncratic, reflecting a simplistic distinction between 'people inclined to believe in things we have designated 'paranormal' and 'people not inclined to believe in those things', "Skeptics", of course, being the latter. Judging from the overall tenor of CSICOP publications, such Skeptics seem to believe that definitive knowledge of the world is possible, and that the most appropriate means to achieving it is 'the scientific method' (by which is meant the traditional caricature of the hypothetico-deductive model). This is in dramatic contrast to the usual use of the word "skeptic" within philosophy, for the skeptical tradition which began with the preSocratics and continued through Hume into the present day has as its central contention the idea that "sure knowledge of how things really are may be sought, but cannot be found" (Flew 1979:314; for much more detail, see also the remarkable Williams 1996). Hence, when I use "Skeptic" as a capitalised term, I intend the CSICOP-related movement rather than the more usual philosophical meaning. This is important, because I am quite solidly in favour of 'skepticism' – which was after all originally the opposite of 'dogmatism' – but have reservations about CSICOP-type 'Skepticism'.

urge you, to save your soul, to read Dawkins's book."³ This is a strange endorsement, considering Dawkins' atheism, but it is revealing.

Unsurprisingly, I agree with Dawkins that science education should be better, and I agree with him that science is full of wonder. These are ideas I too hold dear. But I contest his presumption that those who do not agree are clearly deluded. Dawkins's position on matters of the public and science can be inferred from a few choice selections from his most recent book, *Unweaving the Rainbow: Science, Delusion, and the Appetite for Wonder* (1998): astrology is "meaningless pap" and we need an antidote to the "paranormal disease" (115); his views on our place in the world and on the gradual evolution of complex adaptations are self-evident to any "sane" person: (ix, 199). Dawkins is gravely affronted by those who disagree with, challenge, or ignore science. He genuinely cannot understand why someone might actually like their non-scientific beliefs, and is astonished at one who is not "grateful for having the wool pulled off her eyes" (xi). He takes someone's question of the utility of science education to be "so idiosyncratic I shall pass on it" (37). His "ideal intelligent layman" is a British Lord (xiv). His idea of "trying to gently amuse a six-year-old child at Christmas time" involves calculations proving the impossibility of Father Christmas delivering presents around the world in one night (141).

Dawkins's position on dissenting colleagues in the Science Wars is similarly untroubled by ambiguity. Gross and Levitt were completely right in their "splendid" 1994 book, *Higher Superstition*, and those who still disagree with them are but "vocal fifth-columnists" who "waste the time of the rest of us" (Dawkins 1998:20). He comes down solidly on the Freeman side in the Margaret Mead - Derek Freeman controversy (1998:211), unconcerned with damning critiques of Freeman's work since.⁴

³ This blurb was on the back cover of Dawkins (1998).

⁴ This is an interesting episode in Skeptical history. Margaret Mead has long been a favoured target of the movement, ever since her successful lobbying at the American Association for the Advancement of Science for the inclusion of the Parapsychological Association as a member organisation in 1969. Derek Freeman's critique of Mead's Samoan research fed nicely into Skeptical biases, and has received regular mention within that literature. However, amusingly, there has recently been a reversal of opinion in the pages of the *Skeptical Inquirer*, due to emerging critiques of Freeman. (See the Letters to the Editor page (63) in the March/April 1999 issue.) See also Holmes (1987).

Konner's (1999) review gives a succinct response:

Lighten up. People believed in God, ghosts, imps and fairy tales before you arrived and will do so after you're gone... As for people who believe things for which there is no evidence, they too adapt, survive and reproduce. If a spider's web is beautiful, why not a cathedral? If a butterfly's wing, why not faith?

I am inclined to agree. Certainly, I find Dawkins to be a strange choice for the only chair of the Public Understanding of Science in Britain; his role seems to be more that of the Public Proselytiser of Science, only concerned with the public's understanding of the world insofar as it does not conform to his own. This theme, of treating divergent views as *deviant* inconveniences to be eliminated, should be a familiar one by now in this dissertation.

To take another key figure in the Skeptical movement: The founder and Chairman of CSICOP, Paul Kurtz (Professor Emeritus in Philosophy at SUNY Buffalo), is also founder of the Council for Secular Humanism and publisher of Prometheus Books, one of the primary sources of Skeptical literature today. Kurtz, like Dawkins, is explicitly anti-religion.⁵ The home pages of the Council for Secular Humanism firmly place the blame for the world's most significant problems upon religion and other paranormal belief, and link them to totalitarian and fascist episodes in twentieth century history.⁶ Hope for the future of humankind, these pages claim, is to be found in reason, democracy, free inquiry, the right to private property, an unfettered free market, a naturalistic concept of the world, belief in evolution, and objective science.

So, then, Kurtz, Dawkins, and some CSICOP and CSH members would have it that the very future of the world depends on the overthrow of the irrational. There is little doubt that their mission is motivated by a desire to see the world as less troubled. This seems to

⁵ Secular Humanist Humor:

“ As the storm raged, the captain realized his ship was sinking fast. He called out, ‘Anyone here know how to pray?’

One man stepped forward. ‘Aye, Captain, I know how to pray.’

‘Good,’ said the captain, ‘you pray while the rest of us put on our life jackets - we're one short.’ ”

http://www.secularhumanism.org/library/www/sinking_10_98.html, April 1999.

⁶ (See especially Kurtz's *A Secular Humanist Declaration*, with its prominent signatories, at <http://www.secularhumanism.org/intro/declaration.html>.)

be the source of their carefully orchestrated, aggressive, and well-supported campaign to obtain converts and spread their words.

All of this is of interest here, of course, because of the link with anthropology and archaeology. Articles on archaeological matters have appeared quite regularly in the *Skeptical Inquirer*, for example (most recently Van Leusen 1999). And as I mentioned in Chapter 2 and Appendix A, only a few archaeologists have specifically addressed the issue of 'alternative' views of the past in any sustained fashion, prominent among them CSICOP members Kenneth Feder and John R. Cole. In particular, Feder's *Frauds, Myths, and Mysteries*, just released in a third edition, is a dominant resource for archaeologists and archaeology students wanting to know about 'alternative' archaeology.

It is not my purpose here to condemn CSICOP or those with associations to it, for no doubt the opinions of CSICOP fellows, and *Skeptical Inquirer* readers, etc., are diverse.⁷ Furthermore, I have often found good reading within the pages of *SI*. Rather, I wish to point out that the CSICOP organisation has arisen in a very specific historical and social context to fill quite particular needs, and that its mode of discourse and mission may not be appropriate for exportation to other topics or other places. That context is the late twentieth century United States: one of the needs has been to deal with mass media which are of truly extraordinary dimensions and influence; another need has been to combat creationist lobbying within the educational system.

And the latter is specific indeed. Stephen Jay Gould writes that "The saga of attempts by creationists to ban the teaching of evolution, or to force their own fundamentalist version of life's history into science curricula of public schools, represents one of the most interesting, distinctive, and persistent episodes in the cultural history of twentieth-century America" (1999: 126). He recounts the puzzlement of French and Italian clergy over 'scientific creationism', but also notes that most American religious leaders do not support the creationist agenda.

⁷ I would expect, for example, that at least some *Skeptical* archaeologists disagree with Dawkins, who feels comfortable passing judgement on such delicate issues as the appropriate treatment of 'Kennewick Man', the 9 000 year-old skeleton found in the U.S.A. in 1996. Clearly, in his opinion, there should be no barriers to archaeological study of the skeleton, and certainly no question regarding appropriate custody. Indeed, he finds this attempt to mediate between different cultural groups and scientists to be "laughable" (1998:18).

The legal battle began with the passing of statutes that forbade teaching evolution in some southern states, and progressed through the Scopes trial of 1925, to the eventual ruling of the statutes' unconstitutionality in 1968. Then came the creationist "equal time" laws in the late 1970s, reversed with a U.S. Supreme Court decision in 1987, which ruled by a seven-to-two majority that the creationist "equal time" law was unconstitutional. But although the legal chapter is over, the fight has certainly not ended, for committed creationists have a great deal of political clout, and continue to vigorously pressure textbook publishers and school boards (Gould 1999: 133-147). Thus, American scientists generally perceive a strong need to maintain their bulwark against creationism, especially given its associated political positions in the present day.⁸

And so, at least some of the archaeology-related fellows of CSICOP have been part of an anti-anti-evolutionism (or simply 'anti-creationism') attack force. For example, Eugenie Scott and John Cole are key personnel at the National Centre for Science Education, which in its own words is "a nonprofit, tax-exempt membership organization working to defend the teaching of evolution against sectarian attack. We are a nationally-recognized clearinghouse for information and advice to keep evolution in the science classroom and 'scientific creationism' out."⁹ The NCSE's list of supporters reads like a 'who's who' of evolution research in North America.

The aggressive stance does seem called for in that context. However, it is unreasonable to expect that it would stay contained to creationism, and indeed, some publications such as Harrold and Eve (1987) display a distressing conflation of U.S. creationism and other alternative pasts. I strongly question the necessity of such an intensive mobilisation against all other 'fringe' beliefs about archaeology – and this is a risk that the generalisation of an intolerant Skeptical agenda carries with it. And it may not respect national borders, either. Bourdieu and Wacquant reflect that the successful exportation of American thought is reaching an all-time high, and note that "the neutralization of the historical context resulting from the international circulation of texts and from the correlative forgetting of

⁸ In fairness, I should mention Gould's caveat, that William Jennings Bryan, the prosecutor on behalf of creationism at the Scopes trial, acted in the belief that Neo-Darwinism influenced German military aggression in WWI, and had correctly identified a very serious problem with evolutionary doctrine at that time, one which definitely needed challenging: that is, a strong bias towards eugenics (Gould 1999: 155, 166).

⁹ From the NCSE homepage at <http://www.natcensied.org/>.

their originating historical conditions produces an apparent universalization” (1999:41). This can have the unexpected effect, Bourdieu and Wacquant argue, of essentially replicating distinctively American problems elsewhere. This is why I think it necessary for British archaeologists to be alert as new Skeptics’ groups begin to form in Britain.

Of up-and-coming interest is the Association for Skeptical Enquiry (ASKE), one of CSICOP’s international network members, and the first national Skeptics organisation in the UK with open membership. This fledgling Skeptics group was founded in June 1997 with the desire “to promote the critical scrutiny of paranormal and pseudoscientific claims with the United Kingdom”, and as of April 1999, has held two conferences, the first on “Paranormal and Superstitious Beliefs” and the second, notably, on “Cult Archaeology: Sensational but Questionable Claims About the Past.” The latter was held in the University of Sheffield’s Psychology Department in March, and was co-sponsored by the British Association for the Advancement of Science as part of Science and Technology Week 1999.¹⁰ To my regret, I missed the meeting and cannot report fully; however, there are features of interest which I could observe from a distance electronically. First, there is the replication of originally American terms of discussion regarding alternative archaeology – that is, the conference’s title using Cole’s (1980) and Harrold and Eve’s (1987) term “cult archaeology”. Second, there is the mirroring of the terms of engagement – that is, seeing the situation as essentially dichotomous, with good ‘real’ and bad ‘pseudo’ archaeology. Third, the impetus behind the conference was very much the standard Skeptical rationale for action, the desire to protect the public from fake archaeology. And fourth, the conference was not initiated by professional archaeologists – ASKE has no professional archaeologists in its membership, only amateurs – but did feature their participation.¹¹ These are characteristics which ASKE’s treatment of archaeology so far has in common with the American Skeptics’ approach.

One hopes that British archaeology is not headed for another period of unproductive conflict with and about ‘alternative’ sectors, but multiple lines of division and alliance make this prospect thinkable. These include: rifts within archaeology itself; the easy adaptation of persuasive high-level theory from the Science Wars to bolster both pro- and

¹⁰ See the ASKE homepage, <http://linus.mcc.ac.uk/~moleary/ASKE/> for details.

¹¹ I am indebted to Wayne Spencer, founder of ASKE, for sharing this information with me in a personal communication, May 2/99.

anti- Skeptical positions; the traditional enmity of Fortean and Skeptical groups; the historic solidarity of Earth Mysteries and Forteanism; and the links between explicitly nonscientific, Goddess-oriented prehistories, new feminisms, and neopaganism. This is a powerful mix, and if the debate heats up, initial good intentions may be of little import. A little caution now in these early days of archaeology's partnership with new British Skepticism might prevent a quagmire of personal and ideological battles later.

This, then, is the current influence of Skepticism, apparently growing steadily in its extent. It is not my purpose here to argue that Dawkins, Kurtz, CSICOP or the CSH are wrong on every count; assessing their premises is another dissertation. Nor would I suppose that archaeologists like Cole or Feder or Van Leusen, for example, agree with everything contained in the publications of these groups. Nor can it be suggested that ASKE mirrors CSICOP exactly. However, it does seem that archaeology, both in North America and in Britain, is in steady and increasing contact with influences which promote the Skeptical agenda. Certainly, given the tendency of some archaeologists to engage in the kind of ideological debate about science which characterises the Science Wars, and given the linkages of the Science Wars to Skepticism, there would seem ample opportunity for the Skeptical agenda to slip into disciplinary thinking, just as a certain view of scientific progress slipped into the AngloAmerican intellectual mainstream via Thomas Kuhn's *Structure of Scientific Revolutions*, and as soldiers glided through the gates of Troy within their wooden horse (see Fuller on Kuhn, Ch. 7).

Is this bad? Is the Skeptical agenda clearly wrong? That depends upon the archaeologist and the context. Personally, I find *parts* of this agenda, as articulated by key figures like Dawkins and Kurtz, to be arrogant, philosophically problematic, socially unrealistic, and of limited applicability, but I do not think anyone should take my word for it. Rather, I think that archaeologists need to be aware of the constellation of beliefs within which the most prominent assessments of 'fringe' archaeology are located, and should make a habit of asking themselves if they share those beliefs, and share the larger agenda, before accepting the assessments at face value. I say this because it is endlessly surprising how generally clear-thinking, tolerant, moderately relativist, and even religious scholars can jump without caution onto a Skeptical bandwagon whenever the security of their own discipline's borders seems compromised. The polarising consequences of such moves can be unfortunate.

Irenicon

Stephen Jay Gould, another prominent advocate of science and reason, though no ally of Dawkins', has taken a different approach to clashes of worldviews in his latest work, and I have been captivated by his resurrection of a lovely and usually-forgotten word. We often hear the term 'polemic' – originally a theological term, but now a more general word meaning 'the art of controversial discussion', derived ultimately from the Greek *polemos*, or 'war'. But Gould (1999: 208-222) reminds us of its counterpart. The Greek *eirene*, or 'peace', has given us 'irenic', meaning 'aiming or aimed at peace' and 'irenicon', or 'a proposal made as a means of achieving peace' (*OED*).

Gould's proposal for irenics is specifically about religion and science but, I think, is applicable to some of the general challenges of archaeology and alterity as well. He rejects two means of peacemaking: first, the attempt at false unifications of theories from different realms; and second, avoidance of conflict between groups through rigid separation and avoidance of all communication. Rather, Gould encourages a coexistence, based on contact, dialogue, mutual respect for different forms of logic and their proponents, and openness to reciprocal enlightenment (1999:211).

It's not a bad idea.

On Candles

Have I ever heard a sceptic wax superior and contemptuous? Certainly.... the chief deficiency I see in the sceptical movement is in its polarization: Us v. Them – the sense that we have a monopoly on the truth; that those other people who believe in all these stupid doctrines are morons; that if you're sensible, you'll listen to us; and if not, you're beyond redemption. This is unconstructive.
(Sagan 1996:280-282)

I shall close this work at the general level on which it began, through reference to one more icon of popular science writing, the late Carl Sagan – a scientist beloved by Skeptics and the reading public alike – for I think reflection on Sagan's words might benefit those who worry about the proliferation of alternative archaeologies. His final book, *The Demon-*

Haunted World: Science as a Candle in the Dark, begins with Plato's ancient grumblings about scientific illiteracy, and makes an argument for better science education and more responsible media in the U.S.A. and beyond. Though he used the science vs. pseudoscience distinction, he generally did so with care, and addressed alternative beliefs in historical context, realising that many are "not doctrines for nitwits" (1996:23).

He took his title from *A Candle in the Dark*, a seventeenth-century work by Thomas Ady, an attack upon the irrational barbarities of witch-hunting. Like Ady, Sagan feared for the consequences of pervasive irrationalism for our society:

The candle flame gutters. Its little pool of light trembles. Darkness gathers.
The demons begin to stir.

(1996:29)

I respect Sagan's intentions but am concerned for the interpretation of his oft-quoted words. CSICOP has just inaugurated "Candle in the Dark" and "Snuffed Candle" awards for services for and against science, respectively. If Sagan's words are understood as benevolent, humane, and hopeful, then perhaps all will be well. But if his words become coopted by an aggressive fundamentalist science lobby – who sometimes identify candles, light, darkness, and demons with human institutions and individuals rather than transcendent principles – we may all be the poorer and in the end, more lost. It would be a bitter irony indeed if Sagan's plea for reason was used as a rallying call in a new witch hunt, with Science, rather than the Church, as prosecutor. And it would be a grave error to forget that irrationalism alone did not denouncers and burners of witches make; intolerance, prescribed by ideology, rewarded by material incentive, and encouraged under law, was the other key ingredient (Crowley 1989:38).

My fears are these: that the science-pseudoscience distinction will continue to be made on harsh and questionable bases which actually betray rational principles; and that those who lionise science, ignore its human context, proclaim its unerring path to truth, assume its guarantee of social progress, and deem all other views inferior and in need of eradication, may forget that candle flames can also burn, and may never know that some of their 'demons' are other people, their visages twisted in the shadows of ignorance and prejudice.

Appendix A – TAG and Moot Surveys

A strictly literature-based approach to the relationship of ‘alternative’ and ‘orthodox’ archaeology is inherently limiting in many respects – partly because of the selective nature of what gets published about the ideas, interactions and behaviour of these groups, and partly because detailed information about the individuals involved is rarely revealed – and so it can be beneficial to supplement literature searches with other means of obtaining information about people’s beliefs and interactions. One means of doing this is of course through direct participation in the communities in question, and personal interactions; see main text, especially Chapter 5, for discussion of my experiences in this regard. Another means of supplementing the literature is through surveys. Accordingly, this research did include some survey work (see also Appendix B). The main survey was designed from the outset to be strictly supplementary, not to be analysed with any degree of statistical sophistication, but to enhance the opportunity for conversation, as well as to permit some anonymous written commentary. I consider the survey work a success from these subjective perspectives, although the limited sample sizes compromise the objective usefulness of the results. Those results will be discussed at the end of this Appendix. There is, however, other relevant ground to cover first.

This is not entirely virgin territory, for some researchers have previously conducted surveys on related topics. Their subjects, orientations, goals, and interpretative frameworks have not necessarily been consistent with my own – in general, they have focused on students with a view to explaining, and learning to combat, ‘deviant’ beliefs – but a review of their work seems appropriate nonetheless, for two reasons. In part, these previous surveys are relevant as parallels to my current work because of their methodology, and in part, they are relevant as subjects because the comments of their authors provide vivid examples of orthodox opinions about the fringe.

Review: Previous surveys on related subjects

Questionnaires have been administered to anthropologists often enough in the past, for example, Wienker and Bennett (1992) surveyed the members of the American Association of

Physical Anthropologists to determine the areas which they considered to be most important for training. No doubt many other similar reports on general professional opinions exist. Citation analyses have also been done to elucidate opinions and trends in anthropology and archaeology (e.g., Sterud 1978; Zubrow 1980), and these disciplines have, of course, come under scrutiny from within in numerous other ways.

However, I am aware of only one survey of professional archaeologists' opinions on the subject of alternative archaeology (see discussion of Feder 1984 below). Cole (1980:16-7) created a typology of (American) professional reactions to what he terms "cult archaeology," but this was derived from the perusal of the few publications on the subject, and personal observation, conversations and correspondence, rather than responses to an actual questionnaire. Typically, orthodox archaeologists' writings about fringe subjects focus on debunking or criticising the claims made (e.g., Aitken 1959; Atkinson 1966; Atkinson *et al.* 1981; Burl and Michell 1983; Feder 1983; Fowler 1990; Greenwell 1980; McKusick and Shinn 1980; Pringle 1995; Raikes 1984; Ridpath 1978). On occasion, such works have advocated more effective public responses from archaeologists to fringe claims (Cole 1978; Daniel 1992; Feder 1980; Harrold and Eve, eds. 1987; McKusick 1982, 1984; Story 1977; Williamson and Bellamy 1983). Only rarely do they examine the existing responses of archaeologists to the fringe in any detail (Cole 1980, Feder 1984). However, surveys have been conducted on a fairly large scale in order to study the response of American college students to alternative archaeology, as well as to other fringe topics.¹

Bainbridge's Survey of Students

Bainbridge (1978), a sociologist, conducted the first major survey of American college students involving fringe archaeology, with the aim of ascertaining the correlates and causes of belief in von Däniken's theory of ancient astronauts. In particular, he was examining the applicability of

¹ Similar surveys have included that done by Morris (1980), who tested the effect of an ESP 'demonstration' and subsequent 'dehoaxing' on the ESP belief of American university students, and Greenwell (1981), who compared rates of U.S. college student belief in paranormal phenomena with rates reported for the general population.

selected theories used in sociology to explain “deviant behaviour” (1978:34). Two hundred and thirty-five students taking an introductory sociology course at the University of Washington were surveyed.

Bainbridge observed that general unhappiness and social strain did not explain acceptance of von Däniken’s theory (1978:36), and that, contrary to his expectations, “enthusiasm for conventional science and a positive attitude toward technological progress” did not prevent students from believing von Däniken’s theory (1978:37). Interestingly, students’ program level and subject major also did not affect their belief; *i.e.*, “[s]tudents who were seniors and about to graduate from college were *not* more likely than freshman students to reject the theory. It did not matter whether students had taken courses in astronomy, anthropology, ancient history, social science, or physical science” (1978:37). On the other hand, a correlation did exist between belief in von Däniken’s theory and a low grade-point average (1978:44).

On the basis of these results, Bainbridge charged the American educational system with responsibility for students’ “gullibility,” for “[a]pparently our university does not give students the knowledge to protect them from intellectual fraud” (1978:39). In particular, Bainbridge asserted that “it is rare for a college professor to mention von Däniken” or refute his claims, implying that this is a serious failing (1978:39). However, some of the blame was placed firmly on (or in) the students’ heads, for Bainbridge also concluded that a low GPA indicated lower intellectual ability, and suggested that “[l]ess intelligent students are more likely to believe in ancient astronauts” (1978:44).

Bainbridge maintained that there are additional extra-curricular factors in play – specifically, he argued that belief in von Däniken’s theory “is part of a generalized occult and pseudoscientific subculture” (1978:41) because of associations with belief in ESP, UFOs as products of intelligent life on other planets, astrology, Eastern religious practices (Zen, Yoga, TM) and biorhythm theory. He also found correlations with endorsement of premarital sex, and fondness of science fiction, although the results made it clear that “neither the culture of modern science nor the culture of traditional religion” (1978:40), nor “left-wing politics, drug use, severe criticism of the government, and opposition to police surveillance of dissidents” (1978:42) affected acceptance of von Däniken’s theory.

These conclusions are certainly thought-provoking, and my questionnaire was designed to address some of the same issues, such as correlations with belief in nonarchaeological fringe theories. Further, I would agree with some of Bainbridge's interpretations – for example, it seems self-evident that proceeding through the university system does not guarantee a student's ability to discern fraudulent from sound claims. Yet, some of Bainbridge's assertions are obviously questionable. For example, further exploration to clarify the meaning of a low Grade Point Average is surely warranted; lower intelligence – whatever that means – was certainly not the only possible cause.

More to the point, there is a marked ideological bent implicit in Bainbridge's treatment of belief in these areas, for he defines such belief from the beginning as “deviant” and this shapes his explanatory framework. (The surveys discussed below follow this orientation.) In addition, a certain conservatism is revealed by his classification of yoga as an “exotic spiritual practice” (1978:41), and his suggestion that approval of premarital sex is characteristic of membership in a “counterculture” (1978:43). This is also an effective reminder of the difficulty of taking such survey results, and their analyses, out of their cultural and temporal context.

Feder's Survey of Archaeologists

Kenneth Feder is an archaeologist who has often written very critically on fringe archaeological claims (*e.g.*, 1980, 1983, 1996). His 1984 contribution to *American Antiquity* reported on a prototype for the Harrold *et al.* student survey discussed below, and also described a survey of professional teaching archaeologists in the U.S.A. Picking up the gauntlet thrown down by Bainbridge, Feder wrote a questionnaire asking the lecturers whether in their introductory archaeology/prehistory courses, they covered various fringe topics – specifically, ancient astronauts, creationism, Barry Fell's theory of 'Bronze Age America,' Atlantis, Bigfoot, Noah's Ark, King Tut's curse, New World genesis of humans, psychic archaeology, dowsing, and the Loch Ness monster – and asking whether these topics were taught in a positive or negative light. Six hundred and ten questionnaires were sent out, and three hundred and forty-nine were returned (1984:532). Feder's goal was to determine what archaeologists were teaching their students, and how effective they were as educators.

As it turned out, about 60% of the respondents did give coverage in their classes to subjects such as creationism, hyperdiffusionism, and von Däniken's ancient astronauts. Atlantis, New World genesis, and Bigfoot received attention from about 30 - 40% of the responding lecturers, while the other subjects were covered in class by 10 - 25% of the respondents. Thus, Feder wrote that "we can conclude from these results that teaching archaeologists, as a general rule, do in fact take up many of the extreme claims of an archaeological nature presented in the popular media" (1984:534).² The results also indicated that overwhelmingly, fringe topics are treated in a negative manner, although creationism, Bigfoot, Noah's Ark, psychic archaeology, dowsing, and the Loch Ness Monster were dealt with neutrally by around 10 - 20% of respondents, and dowsing was presented in a positive light by a significantly high 13.5% of the lecturers.

Feder's agenda is exposed when he writes in support of the negative treatment of fringe subjects, obviously pleased that, by most lecturers, "untestable 'theories', unsupportable claims, invalidated hypotheses, unverified statements, and outright lies about the past and its study are presented as such" (1984:533). Without delving too deeply into concerns about the simplistic scientism that such phrasing implies, I would argue that this is at the very least elevating orthodox archaeology to an epistemologically privileged position, which is inappropriate. Feder seems primarily concerned with exonerating archaeologists for what he perceives as the appalling proliferation of nonsense among their students. Thus, he writes that "[m]ost [teaching archaeologists] live up to what is here seen as a crucial responsibility: providing students with the necessary intellectual tools, both in terms of scientific method and specific archaeological data, to assess archaeological claims intelligently" (1984:534). This may, of course, be true; however, the general data collected from Feder's survey can hardly have provided him with the information necessary to make this judgement. Rather, it seems that he is simply eager to contradict Bainbridge's statement, above, that university lecturers are failing their students in this regard (Feder 1984:526,533).

² One is inclined to wonder, however, whether the other 261 lecturers who did not return their questionnaires taught their classes about these subjects. If they did not – which might in itself be a reason for not returning the form – then Feder's statement may be rather optimistic.

Gray's Survey of Students

Thomas Gray (1987), a specialist in social psychology, further addressed the issue of paranormal belief and education. Starting from the assumption “that people, at least in part, believe in what we know to be scientifically unsubstantiated claims because they are ignorant of what constitutes good versus bad evidence” (1987:22), Gray designed a survey to test the effect of higher education on belief in paranormal phenomena, including ESP, UFOs, astrology, reincarnation, and von Däniken’s theory. Four hundred and nineteen students from a variety of courses at Concordia University in Montréal were given questionnaires. The results were “disconcerting to those who, like myself, expected to see dramatic differences in the level of belief in the paranormal as a function of university experience” (1987:22). In general, reported belief in these theories was quite high: around 80% believed in ESP, 60% in UFOs, 60% in reincarnation, 50% in astrology, and 40% in von Däniken’s ancient astronauts (1987:25). There were some minor gender differences (females were more likely than males to believe in ESP, astrology, and reincarnation, as likely to believe in ancient astronauts, and less likely to believe in UFOs), but far more striking was the fact that, as in Bainbridge’s survey, the differences between belief levels of junior and senior students were not generally statistically significant. More worrisome still was the fact that for psychology students, belief in von Däniken’s theory climbed from around 30% to around 50% between the introductory and advanced level (1987:26). To top it off, 1 in 5 advanced psychology students indicated that they believed in a theory which did not even exist, but was invented by Gray as a control variable (1987:28).³

Deciding to pursue the clue that university education was not helping these students to be critical thinkers, Gray took his research one step further. He handed out a questionnaire asking about students’ beliefs at the beginning of a critical course called “The Science and

³ Of course, this raises a whole host of new questions about what exactly the figures produced by these surveys mean. The students are clearly responding, at least in part, to something other than the actual theory specified. There is therefore a difference between saying that “40% of students believe in von Däniken’s theory of ancient astronauts” and saying that “40% of students checked ‘yes’ when asked if they believe in von Däniken’s theory of ancient astronauts,” but this distinction is rarely made in these survey reports.

Pseudoscience of Paranormal Phenomena,” gave them the same form at the end of the course, and also did a follow-up check on their answers to the same questions one year later. Gray found that:

[t]here were statistically significant decreases in [belief] at the end of the semester, but clearly the immediate gains⁴ dissipated (except in the case of UFOs) over the course of the year the students spent back in the generally proparanormal environment... The finding that a course specifically dealing with evidence for the paranormal has only modest and not very durable effects on beliefs makes it less surprising that a more general university education has virtually no effect. It does not, however, make me more comfortable with what appears to be a failing in our attempts to improve students’ critical abilities. (1987:32)

And so, it could be said, the plot thickens. Gray’s results suggest that in the long run, it does not matter much what students are taught – rendering Bainbridge’s (1978) accusation of inadequate teaching, and Feder’s (1984) gainsaying, irrelevant. It is Gray’s opinion that neither training in how to assess evidence, nor specific debunking of particular claims, produces lasting decreases in belief in paranormal phenomena, and that the reason for this is the weight carried in people’s minds by single instances of confirmation of paranormal belief (1987:33)⁵. This “cognitive bias” is implicated again below as a cause of belief in the paranormal.

Harrold, Eve, Hudson, and Feder’s Survey of Students

Finally, more extensive surveys among American college students have been reported in Harrold and Eve’s edited volume, *Cult Archaeology and Creationism* (1987), in papers by Feder (1987), Harrold and Eve (1987b, 1987c), and Hudson (1987). This volume’s stated purpose is to contribute to the understanding of “pseudoscientific” beliefs about human history, and propose ways of dealing with them.⁶ The editors ask:

⁴ Here, Gray means “gains” in rationality, *i.e.*, decreases in belief in the paranormal.

⁵ For example, if a person’s horoscope is incredibly accurate one day, but very inaccurate the next, they may be more inclined to remember the instance of accuracy, and consider that it confirms astrology’s worth.

⁶ Harrold and Eve comment on their terms; they say “pseudoscientific” means “beliefs which are unjustifiably claimed by their advocates to have solid scientific support” and leave it at that

Just how popular are such pseudoscientific beliefs, especially among college students; that is, those who will eventually be among the most influential members of our society? In a relatively well-educated, scientifically sophisticated nation, why are these beliefs so popular? What do they say about our educational system and cultural values? Finally, what can and should be done in the areas of science education, mass communications, and future research to deal with such claims? (Harrold and Eve 1987a:x)

The authors' orientation and agenda are in keeping with those of the other surveys outlined above. That the theories in question are worthless and harmful is accepted as given, belief in them is assumed to be deviant, and it is considered that the only valid reason for studying them is to learn how best to effect their eradication. (For the record, I accept none of these premises.)

With the above questions in mind, a project was carried out by three archaeologists and a sociologist, in which nearly one thousand students from universities in Texas, California, and Connecticut were surveyed on their beliefs relating to creationism, evolutionism, "pseudoscience," and "pseudoarchaeology" (Harrold and Eve 1987a). The most interesting results pertaining to the latter were as follows:

- Contrary to expectations, there was little regional variation in the prevalence of "cult beliefs" in UFOs, Bigfoot, the Bermuda Triangle, Atlantis, ghosts, astrology, the Shroud of Turin, pre-Viking Europeans in the Americas, and similar topics (Hudson 1987:54)
- About 10% indicated acceptance of the theory that "aliens from other worlds are responsible for ancient monuments like the pyramids, which primitive people could not have built"; about 25% accepted that "aliens from other worlds visited Earth in the past"; and around 30% agreed that "the lost continent of Atlantis was the home of a great civilization" (Hudson 1987:55-57).
- Creationist and "cult" beliefs tended to be "somewhat mutually exclusive" (Hudson 1987:56).
- Exposure to sensational media and exposure to scientific media **both** correlated weakly with "cult" beliefs (Hudson 1987:64).
- Unlike belief in creationism, belief in "cult" theories did not correlate significantly with

(1987a:x), while "cult archaeology" or "pseudoarchaeology" are a subdivision of this category, including theories like those about Atlantis or Phoenician visitors to the New World.

sociocultural factors like age, sex, or religion (Hudson 1987:64).

These last two points in particular challenge Harrold and Eve, because of the explanatory framework they adopted for interpreting the survey's results. Drawing from Singer and Benassi's (1981) typology of factors which are possible contributors to the acceptance of pseudoscientific theories, Harrold and Eve considered four possible explanations for the beliefs seen in their survey:

1. "Cognitive biases, or natural errors in human reasoning. These include the perception of patterns in random data or the failure to consider alternative hypotheses to explain an occurrence when an emotionally attractive one is already available."
2. "Heavy and uncritical media coverage of pseudoscientific claims which lends a spurious respectability to them."
3. "Inadequate science education, which leaves many people unable to evaluate the validity of such notions as scientific creationism, or ignorant of possible natural explanations for UFOs."
4. "Sociocultural factors affecting the availability and attractiveness of such beliefs. Some people hold pseudoscientific beliefs because they find them more satisfying than alternatives."

(Harrold and Eve 1987b:68-9)

Since the available data (see above) did not indicate that #2 and #4 were responsible for beliefs in "cult archaeology" theories, we are returned once more to the theory that the prevalence of such beliefs is due either to the failure of higher education, or to habits of thought which are difficult to correct through such education. Gray's results, above, suggest that the latter is most likely the culprit.

So, then, what about the Harrold and Eve book's main purpose, recommendations for action? Despite the combined evidence of the Harrold *et al.* survey and the Gray survey, which together suggest that cognitive biases are the ultimate cause of belief in pseudoscience, other contributors to the Harrold and Eve (1987) volume persist in simply recommending aggressive anti-pseudoscience education (Engler 1987; Williams 1987).

So: what use are these past surveys?

The fact that these surveys are part of the corpus of orthodox work on the subject of fringe archaeology is sufficient reason for their examination as background to this dissertation. More than this, however, they are especially relevant because they are rare attempts by orthodox scholars to study the patterning of beliefs in fringe archaeology, instead of efforts to show that those beliefs are wrong. Thus, these surveys are important to contemplate for two reasons: first, because they reveal attitudes of orthodox scholars towards fringe material; and second, because they offered possible models for emulation in my own survey research into patterning and prevalence of 'fringe' beliefs in the U.K.

With respect to the latter, I should note that I modified aspects of the Harrold *et al.* (1987) survey for incorporation into my own questionnaire. In particular, the answer scale for Part 3 of my questionnaire, giving the respondent the options of "Undecided/evidence is inconclusive" and "Never heard of it" in addition to agreeing and disagreeing, is an innovation of the Harrold *et al.* survey which I adopted. I also learned from some other aspects of their methodology, for example periodically switching the phrasing for statements in Part 3 from positive to negative, to ensure that the reader is forced to think through each statement a little more carefully.

However, methodology aside, it seems clear that many of these previous surveys' results cannot contribute much to a specific understanding of fringe archaeology in Britain. To begin with, the topics covered are rather different; understandably, the American questionnaires devoted more space to creationism and hyperdiffusionist theories than would be appropriate in a British survey, and conversely, include nothing about primarily British concerns such as leys, Neolithic astronomy, druids, or earth zodiacs. Moreover, the populations being surveyed are different; most of the North American surveys dealt exclusively with college students, whereas my focus here is upon professional and avocational specialists in both 'orthodox' and 'fringe' fields. Feder. it is true, surveyed archaeological specialists, but was very specifically concerned with their teaching practices, rather than their own beliefs.

However, some of the characteristics which made the approaches of the previous authors

inappropriate for use in my survey make them all the more interesting from the perspective of examining orthodox reactions to the fringe. For example, these authors' overwhelming orientation towards indiscriminately stamping out fringe beliefs, while thoroughly incompatible with my own approach, is very revealing. In one way, it is typical of orthodox scholarly attitudes towards the fringe – indeed, it is mirrored by the attacks of some British archaeologists on ley hunters – but on the other hand, it is indicative of some powerful underlying fears which are fairly specific to North America. Fringe archaeology in North America is associated with creationism (hence the dual focus of Harrold and Eve, eds. (1987)), the prevalence of creationism is seen as an index of the strength of Christian fundamentalism, and the political correlates of Christian fundamentalism are deeply worrisome to the American academic establishment. Thus, fringe archaeology is representative, in these academics' minds, of something far more insidious and frightening than troubles in the education system, or pockets of quirkiness in the population. And not only is it associated with the fundamentalist threat, but it is associated with a general assault on a reasonable way of life. Thus, Hudson (1987:65) writes, “[a]s long as a psychic can sue a doctor for taking away her psychic powers and win the suit, then we must always be combative,” while Stiebing fears that if orthodox archaeologists don't fight pseudoarchaeology, they may “risk having supporters of one or more of these views become politically potent enough to use the state to promote their cause... a situation which we must resist at all costs” (1987:8-9). This perception of fringe archaeology as part of a distinctly menacing wave goes some way towards explaining the attitude of these orthodox North American scholars towards the fringe; where British archaeologists are primarily sarcastic and drolly denigrating to fringe camps, the North Americans are academic evangelists and crusaders. (This may, however, be shifting; new developments in the intellectual scene in the West, i.e. the Science Wars, could well cause an increase in aggressive Skepticism in the U.K. – see Chapter 8.)

A correlate (or cause?) of this crusading mentality is a world view untroubled by ambiguity. The scholars who conducted the surveys above are, by and large, convinced that they know the difference between science and pseudoscience – without examining the issue closely – and are even more convinced that they are on the right side of this distinction. Harrold and Eve, for example, are clearly not burdened by any angst over this demarcation problem, which has often troubled philosophers. Nor do they think it worthwhile to spend time fussing over distinctions

between different sorts of unconventional theories (1987a:x). In the same volume, Stiebing

(1987) is similarly blessed with a clear notion of the simply scientific nature of archaeology and its relentless march towards truth.

It will be clear by now that this attitude troubles me. The reasons for this are several, but in part I'm bothered by it because this kind of moral certainty encourages discourse with the aim of winning rather than learning; in short, it encourages sophistry. Resultant examples of unfair characterisations and critiques of the fringe abound. Furthermore, this attitude does not encourage introspection, or even exploration of other academics' work; hence, Harrold and Eve, for example, seem unaware of research in the sociology of science (1987c:150).

These issues will be returned to elsewhere in the dissertation; here, suffice it to say that there is much to be learned from these surveys, one way or another. Now, on to my own survey work.

This survey's purpose

This survey was not intended to be the basis for a major part of my analysis, but simply to provide me with a better sense of people's awareness and sensibilities. I designed the questionnaire with the intent that the responses would not be exhaustively quantitatively analysed. (I do not believe that fretting over statistical significance and quantitative methods would add anything meaningful to this endeavour, given the inherent fuzziness of people's responses to questions about their beliefs. Furthermore, I did not want to end up in the trap of the novice survey-analyst, making egregious generalisations like some of those I encountered in the reports discussed above.) In general, I was as interested in looking at the profiles revealed by each individual questionnaire as in quantitatively comparing answers across questionnaires.

As will probably be evident from the questions, I sought information about the following:

- degrees of belief in, and knowledge of, mainstream archaeology and science, and 'fringe' claims regarding the past and other areas. In general, I hoped this would help me to assess how credulous and well-informed the respondents are.

- the relationship of age, sex, level and type of education, occupation, religion, and political affiliation to the above patterns of belief
- how much each group knows about the other, in terms of methods, theories, and familiarity with each other's literature
- how people's interest in the past was first generated, the degree and type of their current involvement in research about the past, and their opinions about heritage issues
- how people classify themselves, and their opinion of the relationship of orthodox and heterodox archaeology

I was especially interested to see if the respondents fell into clear 'camps', and to compare the knowledge and beliefs of the different groups sampled.

I had originally considered sending out surveys to quite large samples, for example *Antiquity* subscribers, Fortean UnConvention attendees, etc., but revised this plan after obtaining results from two samples, the attendees at the Theoretical Archaeology Group conference in Reading (December 1995) and attendees at the Ley Hunter Core Moot in London (November 1995). Plans for more extensive surveys were abandoned for two reasons. First, I had an excellent set of responses from the Moot sample, wonderfully helpful especially in their written commentary, and in invitations for further discussion – this, plus my attendance at the Moot (my first Earth Mysteries meeting) led me to conclude that discussion and correspondence with individuals in Earth Mysteries, and participation in more EM meetings and events, could help me towards a much more sophisticated understanding of their beliefs and reasons for interest in the past, than a crude tool such as a survey could. (The survey had, however, served its purpose of allowing me to introduce myself to the community and begin dialogue.) The second reason was the very low response rate from the TAG sample, which suggested to me that further efforts in this direction might be wasted. There may indeed be useful information to be gleaned from more surveys of a larger scale, but this is a project for another day.

Following are the questionnaires distributed at the TAG conference and at the Moot. They are identical in the questions asked, different only in the introductory paragraph at the top. I spoke briefly at the Moot and distributed the questionnaires in person during the session; they were either returned to me later that evening in the pub where attendees had gathered, or mailed in

envelopes I had provided. In contrast, the TAG questionnaires were distributed within the conference packages given to registered participants, and returned to a box at the conference reception desk.

Discussion of the Survey Design

Part 1: Personal Background

The questions regarding age, sex, education, religion, occupation, and political affiliation were intended to allow for observation of rough correlations between these factors and constellations of beliefs. The question regarding self-classification was designed to help me learn about people's self-identifications.

Part 2: Reading Habits

The journal titles were chosen after a year of library research which indicated the journals most active in Earth Mysteries, 'orthodox' archaeology journals covering topics of interest to both EM researchers and academic archaeologists, as well as the most popular general science journals. I wanted to see if people in each sample were aware of or read 'orthodox' archaeological publications, Earth Mysteries publications, popular science journals, popular archaeology journals, popular Fortean publications (historically strongly linked to EM), and American journals as well as British and European ones.

Part 3: What Do You Believe?

Agree – Disagree questions were chosen to examine degrees of awareness and belief regarding:

- common 'fringe' archaeological ideas
- mainstream archaeological discoveries then recent
- mainstream scientific knowledge
- common 'alternative' ideas in science
- common 'paranormal' subjects
- common Fortean subjects then receiving coverage in journals
- the trustworthiness of science and higher education
- modernity in general

Multiple choice questions were chosen to more carefully examine knowledge and beliefs about specific topics.

Part 4: Familiarity with various authors

This list of authors was, like the journal selection, based upon a year of library research into authors commonly cited within British academic archaeology and within EM (some current as of 1995, and some more prominent in the previous decades), as well as a few North American archaeologists and commentators on alternative belief.

Part 5 and Part 6 were designed to let people comment in more detail upon issues of interest to them, and to permit discussion bound somewhat less by my design than the structured questions.

Responses

There were a total of 41 respondents from TAG, and 27 from the Moot, representing return rates of approximately 16% and 60%, respectively, of questionnaires distributed at each event. Given the small numbers and survey design, and the differences in the samples and contexts of their completion – e.g., TAG questionnaires were filled in actually during the conference, some quite probably in the pub, and returned there, whereas most Moot questionnaires were primarily filled in at home at greater leisure and with more care, and returned by post – it seemed most unwise to undertake any exhaustive statistical analysis. I do not wish to contribute to the mass of statistics discussed above without better survey design and control over the process. (These preliminary results might help in the design of later questionnaires, however. The actual completed questionnaires are available from the author, should anyone wish to see them for future research purposes.)

I include here, therefore, only some brief general observations, the figures for a few key questions, and some of the written commentary produced by respondents.

SURVEY YOUR ASSISTANCE IS REQUESTED!

I am engaged in Ph.D. research on the relationship, past and present, between 'orthodox' archaeologists and alternative researchers also concerned with human history. I'm seeking information, through this questionnaire, about some of the beliefs, opinions, and reading habits of T.A.G. participants. Comparing these results with those obtained from attendees at 'alternative archaeology' conferences will help me to understand the groups' similarities and differences, and learn how much they know about each other's work. If you can assist me by filling out this questionnaire, it would be *greatly* appreciated. This form may be returned to the T.A.G. registration desk, or mailed to me at the address below. I intend to report upon the results at next year's T.A.G. conference.

Thank you!

Kathryn Denning, Research School of Archaeology and Archaeological Sciences, University of Sheffield, 2 Mappin St., Sheffield S1 4DT. Email: <K.E.Denning@Sheffield.ac.uk>

PART 1: PERSONAL BACKGROUND (To be used ONLY for interpreting survey results)

1. Age group (Please circle): < 21 22-25 26-29 30-39 40-49 50-59 60+

2. Sex (Please circle): Male Female

3. Please summarize your education (e.g., O-levels, university), especially noting the education you've had about human history (e.g., self-taught, field school, university (where?), local archaeology societies).

4. How would you classify yourself with respect to your interest in the past? (e.g., ley hunter, professional academic/ field archaeologist, Earth Mysteries researcher, archaeology student, archaeological dowser, amateur archaeologist)

5. Religion (if practising): _____ 6. Political affiliation: _____

7. Occupation: _____

PART 2: READING HABITS

1. Which newspapers do you read? _____

2. How often do you read the following journals? Please check the appropriate column.

	frequently	occasionally	read it once	aware of it but haven't seen it	never heard of it
The American Dowser					
Archaeoastronomy					
Current Anthropology					
Proc. Prehistoric Society					
Norwegian Arch. Review					
Scientific American					
Man/ J. Royal Anthro. Inst.					
National Geographic					
J. British Soc. of Dowsers					
Mercian Mysteries					
American Antiquity					
J. Archaeological Science					

	frequently	occasionally	read it once	aware of it but haven't seen it	never heard of it
Caerdroia					
Archaeology					
Meyn Mamvro					
Fortean Times					
The Ley Hunter					
INFO Journal					
Antiquity					
3rd Stone					
New Scientist					
Markstone					
RILKO Journal					
Archaeometry					

PART 3: WHAT DO YOU BELIEVE?

*Please check the appropriate column next to each statement.
If none of the given answers fits, please add a note.*

	agree strongly	agree somewhat	not sure	disagree somewhat	disagree strongly	the evidence is inconclusive	never heard of it
There are likely other planets in the universe with intelligent life.							
New and Old World archaeological remains are similar (e.g., pyramids) because they come from the same ancestral culture.							
The Beast of Bodmin is merely a large domestic cat.							
Weathering of the Great Sphinx proves it is several thousand years older than modern Egyptologists have stated it to be.							
A Viking settlement site has been excavated in Newfoundland, Canada.							
The structure of some prehistoric mounds (alternating organic and inorganic layers) proves they functioned as orgone accumulators.							
Astrology is useful in predicting a person's future.							
The dangers of nuclear energy outweigh its current benefits.							
The two Viking landers set down on Mars by NASA in 1976 found clear evidence that very primitive life once existed there.							
The legend of Atlantis is just a story that someone invented.							
All Yeti/Bigfoot/Sasquatch sightings are either mistakes or hoaxes.							
A tooth recently found at the Boxgrove quarry site proves that physically modern people lived in Britain over a million years ago.							
A true perpetual motion machine has never been built.							
Extraterrestrials visited the Earth long ago, influencing many ancient cultures.							
The Loch Ness Monster is a real animal, probably like an aquatic dinosaur.							
Science has done more good than harm for the world.							
Homeopathic remedies are very effective in treating many illnesses.							
The Neolithic societies of continental Europe were generally matriarchal.							
Cases of spontaneous human combustion have been scientifically verified.							
The dimensions of Egyptian pyramids represent cosmological numbers.							
Analysis of DNA from a crime scene (e.g., from blood) can reliably prove a suspect's guilt or innocence.							

	agree strongly	agree somewhat	not sure	disagree somewhat	disagree strongly	the evidence is inconclusive	never heard of it
Anomalous patterns of radioactivity and magnetism have been detected at British stone circles.							
Uri Geller and some other people can bend metal by psychokinesis.							
The greenhouse effect has already caused significant global warming.							
Modern druids can trace their history directly back in an unbroken sequence to the Roman era.							
Monuments such as Avebury or Silbury Hill could not have been built without military coercion and bureaucracy.							
Secret organizations like the Illuminati, Rosicrucians, and Knights Templar have often conspired to change the course of history.							
Biblical accounts of historical events are accurate.							
Our society is becoming too dependent upon computers.							
Average people can, just by thinking, influence the output of an electronic random-number generator.							
Modern people are physically healthier than prehistoric people were.							
Folk tales are always based on truth.							
Nostradamus made many highly specific predictions which have come true.							
ESP and telepathy have been scientifically proven to exist.							
Worship of a Mother Goddess was widespread in prehistory.							
University education is often a waste of time.							
250 million years ago, all the earth's landmasses were joined into a supercontinent which geologists call Pangaea.							
Europeans settled in America long before the Vikings or Columbus arrived.							
People can live many different lives through reincarnation.							
Ghosts do not exist.							
The Shroud of Turin has been conclusively shown to be a forgery.							
Everything we think now is likely to be proven wrong in the future.							
Extinct animals such as dinosaurs can currently be recreated from ancient DNA.							
The Dogon people of Africa had extremely detailed knowledge of the star Sirius long before Europeans had studied it through the telescope.							

PART 3, continued: WHAT DO YOU BELIEVE?

Please answer the following questions by circling one or more appropriate letters.
Please add a note if none of the answers given represents your opinion accurately.

1. Which of the following are used as archaeological dating techniques?

- (a) C14 (radiocarbon)
- (b) thermoluminescence
- (c) Electron Spin Resonance
- (d) enamel hypoplasia
- (e) dendrochronology
- (f) nitrogen isotope ratios
- (g) Magnetic Resonance Imaging
- (h) amino acid racemization

2. Leys are probably: (circle all that you believe apply)

- (a) related to lines of energy
- (b) remnants of ancient trackways
- (c) related to UFO activity
- (d) related to shamanism and astral travel
- (e) purely coincidental alignments of completely unrelated sites
- (f) remnants of coffin paths/corpse ways/death roads, or ghost paths

3. Studying material remains from prehistory can provide us with reliable knowledge of:

- (a) what people built and ate, and the tools they used
- (b) people's spiritual life – rituals and beliefs
- (c) how people were organized politically
- (d) how people perceived their surroundings
- (e) the social aspects of domestic life, e.g., gender relations

4. Dowsing can be effectively used:

- (a) to find underground water
- (b) to locate energy lines
- (c) to find underground mineral deposits
- (d) to find lost objects and people (i.e., map dowsing)
- (e) to answer questions asked by the dowser
- (f) to locate and map buried archaeological features
- (g) in no circumstances; it does not work

5. Crop circles are probably:

- (a) man made hoaxes
- (b) created by natural atmospheric vortices
- (c) the product of unknown intelligences
- (d) made by landing alien space-craft
- (e) don't know

6. Which of these visions of life in prehistoric Britain comes closest to your own?

- (a) difficult and short: people were usually unhealthy and died young because of the harsh conditions
- (b) peaceful and idyllic: people lived in harmony with each other and with nature
- (c) highly civilized: there was much scientific and spiritual knowledge which has since been lost
- (d) primitive: people were intellectually inferior, often violent, and amoral

7. The slightly flattened shape of many stone circles is probably:

- (a) a byproduct of deliberate efforts to create complex astronomical alignments
- (b) an accident; people were trying to lay out a true circle by eye
- (c) because people made them by marking the edges of crop circles created by natural phenomena
- (d) due to post-depositional drift of the stones

PART 4: FAMILIARITY WITH VARIOUS AUTHORS

The following are names of authors who have written on archaeological topics and/or some topics of related interest (e.g., evolution, psychic research). Please place one checkmark next to the name if you recognize it, and two checkmarks next to the name if you are familiar with the person's work.

R.J.C. Atkinson	Richard Bailey	John C. Barrett	Barbara Bender
H.P. Blavatsky	Frederick Bligh Bond	Aubrey Burl	Edgar Cayce
Christopher Chippindale	David Clarke	Andrew Collins	Glyn Daniel
Erich von Däniken	Paul Devereux	J. Norman Emerson	Kenneth Feder
Barry Fell	Andrew Fleming	Charles Fort	Martin Gardner
Marija Gimbutas	Jimmy Goddard	Jeffrey Goodman	Tom Graves
Leslie Grinsell	Graham Hancock	Gerald S. Hawkins	Ian Hodder
T.C. Lethbridge	Sig Lonegren	James Lovelock	Euan MacKie
Katherine Maltwood	Marshall McKusick	G. Terence Meaden	John Michell
Nigel Pennick	James Randi	Jenny Randles	Maria Reiche
Colin Renfrew	Bob Rickard	G.V. (Don) Robins	Stephan Schwartz
Paul Screeton	Ivan van Sertima	Reginald Allender Smith	Alexander Thom
Guy Underwood	Alfred Watkins	John Anthony West	Mortimer Wheeler
Alasdair Whittle	Tom Williamson	Alison Wylie	David Zink

PART 5: WHAT IS IMPORTANT TO YOU ABOUT THE PAST?

1. How did you first become interested in the study of the past? (e.g., school, family, friends, TV, books)

2. How often in the last year have you been to an archaeological site? (e.g., once or less, once a month, once a week, daily during some periods) What did you do there?

3. Which aspect of the administration of public heritage concerns you most? (e.g., access to sacred sites like Stonehenge for ritual/religious purposes, access to fields for walking, permission/funding to conduct research on sites, education, conservation, or other issues?)

4. Are you currently engaged in research about the past? If so, please briefly describe your subject.

PART 6: WHAT DO YOU THINK ABOUT THE RELATIONSHIP OF
'ORTHODOX' ARCHAEOLOGY AND 'ALTERNATIVE' ARCHAEOLOGY?

1. What do you think is the best way of referring to research concerned with human history, but operating outside of orthodox archaeology? (e.g., "Alternative Archaeology", "Earth Mysteries", "heterodox archaeology") Do you think such labels are important? Why or why not?

2. Have you ever had a 'paranormal' experience? (e.g., ESP, seeing a ghost or UFO) If so, please describe it. Did it affect your beliefs about the past?

3. Please rank the following subjects from 1 to 8 in order of your interest in them. (1 representing the most interesting subject, and 8 the least interesting subject)

___ prehistory ___ history ___ folklore ___ landscape studies ___ classics
___ human physical evolution ___ consciousness studies ___ cultural anthropology

4. Have you ever participated in orthodox archaeological research? If so, please describe briefly. If not, would you like to? Why or why not?

5. Have you ever participated in alternative archaeological research? If so, please describe briefly. If not, would you like to? Why or why not?

6. Do you think the theories and methods of alternative and orthodox archaeology are compatible? Why or why not?

7. Do you think that alternative archaeologists and orthodox archaeologists communicate with each other enough? Why or why not?

THANK YOU FOR YOUR ASSISTANCE!!

SURVEY

I am a research student in archaeology at the University of Sheffield. My interest is in the relationship, past and present, between 'orthodox' archaeologists and 'alternative' researchers also concerned with human history. Your input will help me to understand the similarities and differences between these groups, and how much they know about each other's work. If you wish to elaborate, please feel free to add notes. Thank you! Kathryn Denning

PART 1: PERSONAL BACKGROUND (To be used ONLY for interpreting survey results.)

1. Age group (Please circle): < 21 22-25 26-29 30-39 40-49 50-59 60+

2. Sex (Please circle): Male Female

3. Please summarize your education (e.g., O-levels, university), especially noting the education you've had about human history (e.g., self-taught, field school, university (where?), local archaeology societies).

4. How would you classify yourself with respect to your interest in the past? (e.g., ley hunter, professional academic/ field archaeologist, Earth Mysteries researcher, archaeology student, archaeological dowser, amateur archaeologist)

5. Religion (if practising): _____ 6. Political affiliation: _____

7. Occupation: _____

PART 2: READING HABITS

1. Which newspapers do you read? _____

2. How often do you read the following journals? Please check the appropriate column.

	frequently	occasionally	read it once	aware of it but haven't seen it	never heard of it
The American Dowser					
Archaeoastronomy					
Current Anthropology					
Proc. Prehistoric Society					
Norwegian Arch. Review					
Scientific American					
Man/ J. Royal Anthro. Inst.					
National Geographic					
J. British Soc. of Dowsers					
Mercian Mysteries					
American Antiquity					
J. Archaeological Science					

	frequently	occasionally	read it once	aware of it but haven't seen it	never heard of it
Caerdroia					
Archaeology					
Meyn Mamvro					
Fortean Times					
The Ley Hunter					
INFO Journal					
Antiquity					
3rd Stone					
New Scientist					
Markstone					
RILKO Journal					
Archaeometry					

	agree strongly	agree somewhat	not sure	disagree somewhat	disagree strongly	the evidence is inconclusive	never heard of it
Anomalous patterns of radioactivity and magnetism have been detected at British stone circles.							
Uri Geller and some other people can bend metal by psychokinesis.							
The greenhouse effect has already caused significant global warming.							
Modern druids can trace their history directly back in an unbroken sequence to the Roman era.							
Monuments such as Avebury or Silbury Hill could not have been built without military coercion and bureaucracy.							
Secret organizations like the Illuminati, Rosicrucians, and Knights Templar have often conspired to change the course of history.							
Biblical accounts of historical events are accurate.							
Our society is becoming too dependent upon computers.							
Average people can, just by thinking, influence the output of an electronic random-number generator.							
Modern people are physically healthier than prehistoric people were.							
Folk tales are always based on truth.							
Nostradamus made many highly specific predictions which have come true.							
ESP and telepathy have been scientifically proven to exist.							
Worship of a Mother Goddess was widespread in prehistory.							
University education is often a waste of time.							
250 million years ago, all the earth's landmasses were joined into a supercontinent which geologists call Pangaea.							
Europeans settled in America long before the Vikings or Columbus arrived.							
People can live many different lives through reincarnation.							
Ghosts do not exist.							
The Shroud of Turin has been conclusively shown to be a forgery.							
Everything we think now is likely to be proven wrong in the future.							
Extinct animals such as dinosaurs can currently be recreated from ancient DNA.							
The Dogon people of Africa had extremely detailed knowledge of the star Sirius long before Europeans had studied it through the telescope.							

PART 3, continued: WHAT DO YOU BELIEVE?

Please answer the following questions by circling one or more appropriate letters.
Please add a note if none of the answers given represents your opinion accurately.

1. Which of the following are used as archaeological dating techniques?

- (a) C14 (radiocarbon)
- (b) thermoluminescence
- (c) Electron Spin Resonance
- (d) enamel hypoplasia
- (e) dendrochronology
- (f) nitrogen isotope ratios
- (g) Magnetic Resonance Imaging
- (h) amino acid racemization

2. Leys are probably: (circle all that you believe apply)

- (a) related to lines of energy
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Colin Renfrew	Bob Rickard	G.V. (Don) Robins	Stephan Schwartz
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3. Please rank the following subjects from 1 to 8 in order of your interest in them. (1 representing the most interesting subject, and 8 the least interesting subject)

___prehistory ___history ___folklore ___landscape studies ___classics
___human physical evolution ___consciousness studies ___cultural anthropology

4. Have you ever participated in orthodox archaeological research? If so, please describe briefly. If not, would you like to? Why or why not?

5. Have you ever participated in alternative archaeological research? If so, please describe briefly. If not, would you like to? Why or why not?

6. Do you think the theories and methods of alternative and orthodox archaeology are compatible? Why or why not?

7. Do you think that alternative archaeologists and orthodox archaeologists communicate with each other enough? Why or why not?

THANK YOU FOR PARTICIPATING!

Please return this questionnaire to: *Kathryn Denning, Research School of Archaeology and Archaeological Sciences, University of Sheffield, 2 Mappin Street, Sheffield S1 4DT*. If you would like to share your ideas about these or related subjects in more detail, please contact me at the above address, or via email at <K.E.Denning@Sheffield.ac.uk>.

Responses to the “Fill in the Blank” Portion of the Survey

In general, I found that in the “check the box” section, there were few distinct clusters of opinions, that the 2 samples could be equally shaky on some relevant facts, and that certain archaeologists were a bit quicker to give a negative opinion than they were to admit they didn't know something, while certain of the Moot attendees were a little quicker to agree with highly controversial statements. But despite all of this, the variation *within* samples far exceeded that *between* samples.

Configurations of answers were diverse and individual enough that careful selection from the sample could support almost any hypothesis put forth about ‘alternative’ or ‘orthodox’ archaeologists. I have tried, however, to be fair in my selections here of representative questions.

QUESTION ON READERSHIP

Q: How often do you read the following journals? Please check the appropriate column. (listed 24 journals)

TAG sample (n=37) on how often they read *The Ley Hunter* :

frequently	occasionally	read it once	aware of it but haven't seen it	never heard of it
-	-	1	12	24

Moot sample (n=22) on how often they read *Antiquity* :

frequently	occasionally	read it once	aware of it but haven't seen it	never heard of it
4	4	5	7	2

Taking *Antiquity* and *TLH* as leading publications in each research community, this is a rather interesting result. It is more interesting still, however, taken in conjunction with the comments provided later in the survey. For example, if 9 of 22 Moot respondents had never even seen a copy of *Antiquity*, how are they sure it's boring? Equally, how can certain TAG respondents be so certain that it isn't worth communicating with Earth Mysteries researchers if they have never read any of their work?

QUESTIONS OF FACT

A Viking settlement has been excavated in Newfoundland, Canada.

	agree strongly	agree somewhat	not sure	disagree somewhat	disagree strongly	evidence is inconclusive	never heard of it
TAG (n=35)	7	11	8	1	2	1	5
MOOT (n=24)	6	4	5	–	–	–	9

This statement is quite unequivocally true. The site of L'Anse Aux Meadows is well-known, especially among researchers interested in pre-Columbian transatlantic travel. Interesting that some of the TAG sample leaned towards disagreement.

The two Viking landers set down on Mars by NASA in 1976 found clear evidence that very primitive life once existed there.

	agree strongly	agree somewhat	not sure	disagree somewhat	disagree strongly	evidence is inconclusive	never heard of it
TAG (n=35)	–	2	11	2	11	3	6
MOOT (n=24)	2	–	3	3	5	5	6

This is a statement which, barring creative conspiracy theories, is quite false. Unfortunately, there was no evidence whatsoever reported back from these landers for life of any kind on Mars. It's interesting, then, to get any agreements at all, or any suggestions that the evidence was inconclusive. In general, though, both groups were appropriately skeptical.

AWARENESS OF 'OTHERS' RESEARCH

A tooth found recently at the Boxgrove quarry site proves that physically modern people lived in Britain over a million years ago.

	agree strongly	agree somewhat	not sure	disagree somewhat	disagree strongly	evidence is inconclusive	never heard of it
TAG (n=33)	7	8	4	2	9	2	1
MOOT (n=21)	1	6	3	1	1	2	7

I deliberately included two errors in this statement (thus, more correct answers would be in the “disagree” or “inconclusive” zone). First, the hominid tooth was not known to be from an anatomically modern human, and second, the date was guessed to be less than 500 000 years before present. These are details which should matter to an orthodox archaeologist, so I was interested that some erred here. But also of interest is the fact that 33% of Moot respondents hadn't heard about this, arguably the most publicized find in the UK in the two years previous to the survey.

Anomalous patterns of radioactivity and magnetism have been detected at British stone circles.

	agree strongly	agree somewhat	not sure	disagree somewhat	disagree strongly	evidence is inconclusive	never heard of it
TAG (n=36)	–	3	12	3	6	5	7
MOOT (n=20)	12	4	3	–	–	1	–

This question was designed to examine the level of awareness of, and belief in, the results obtained by the Dragon Project's research (mentioned in Chapter 4). It would seem that these answers fall almost along the 'party lines' that would be expected.

OPINIONS

Extraterrestrials visited Earth long ago, influencing many ancient cultures.

	agree strongly	agree somewhat	not sure	disagree somewhat	disagree strongly	evidence is inconclusive	never heard of it
TAG (n=32)	-	-	3	3	25	1	-
MOOT (n=22)	1	1	-	2	14	3	1

Some of the 'orthodox' contingent have been known to make the blithe assumption that anyone with even slightly alternative opinions about prehistory is a raving Erich von Däniken supporter. Obviously, this is not the case. In both samples, a strong majority disagreed with this statement. I am not unduly surprised by the 1 strong Moot agreement, but must confess to being interested by the TAG respondents who were not sure.

The Dogon people of Africa had extremely detailed knowledge of the star Sirius long before Europeans studied it through the telescope.

	agree strongly	agree somewhat	not sure	disagree somewhat	disagree strongly	evidence is inconclusive	never heard of it
TAG (n=35)	3	6	7	3	3	-	13
MOOT (n=22)	3	8	4	1	-	2	4

This statement, I thought, was an interesting test to see how much attention people pay to widespread, high-impact theories which are extensively criticized after the fact (see Ridpath 1981). Although one might expect the TAG respondents to be very conservative on this theory, the ones that had heard of it didn't generally object. Possibly they were unaware of the reference to Robert Temple's 1976 'fringe' work, *The Sirius Mystery*. Once again, this is a situation where the spectrum of responses from each sample is wide. The variation within each is greater than the variation between them.

Written Responses

Following is a selection of responses to questions in Part 6, a selection which I believe fairly represents the spectrum seen in the two samples. (N.B. Not all respondents provided written answers of any detail.) Once more, it would seem unwise to draw hard and fast conclusions from this information – in one sense, the individual responses are best left to speak for themselves, taken one by one by the reader – but I shall make some general comments here. (Additional discussion may be found elsewhere in the dissertation, especially Chapter 2 and Chapter 8.)

There is unquestionably a layering of meanings at work here. I wrote Part 6 of the questionnaire using the terms ‘alternative’ and ‘orthodox’ because those are the generally used categories (or rather, the polite versions thereof), but I was hoping that some would question the nature and validity of the distinction. Indeed, some respondents in each sample did.⁷ Similarly, I was hoping that respondents would take advantage of the change to write their own opinions, and shape my understanding of the matters at hand, rather than having to respond in ways which simply fed into my own predetermined framework. One problem with using these general terms and questions, however, is a loss of precision; I don’t know precisely what, for example, the very negative TAG respondents were thinking of when they read the words “alternative archaeology”, or what their previous experiences or exposures to it were. And so some of their answers might be read as a reaction to the more extreme examples of ‘alternative’ genres, such as von Däniken. Or these answers could be a knee-jerk reaction to a perceived category, rather than an informed response with specific theories or authors in mind. It is impossible to ascertain which is the case from the comments alone, but they are useful nonetheless as an indicator of the difficulties to be overcome when talking to ‘orthodox’ archaeologists about their relationships with others.

⁷ I received several very thoughtful letters from members of the Moot sample when they returned their questionnaires, in some cases beginning long and fruitful correspondences. One wrote in lieu of the questionnaire, explaining that he couldn’t answer the questions sensibly because he had long ago rejected the orthodox-alternative division as meaningless.

There was open hostility in some of the TAG responses regarding 'alternative' archaeology, in addition to disparaging remarks about other people's sanity. Some were indifferent to it, considering that it had nothing to do with them. Some perceived that more communication was needed, but only for the purpose of remedial education for the 'alternative' sector, since 'alternative' archaeology is clearly only a poor and pale substitute for 'real' archaeology, which would evaporate if its adherents were better educated and rational. A few TAG respondents professed curiosity to learn more about 'alternative' ideas, and a very few noted that there were good ideas and enthusiasm to be found in 'alternative' groups, and that the division was unfortunate.

Answers given by some TAG respondents reflected that they believed in a clear and comfortable distinction between 'real' science/archaeology and 'pseudo' science/archaeology. Indeed, some insisted that labels were important for this very reason, to keep others apart from themselves. Conversely, some noted that movements in post-processualism were blurring the boundaries through a subversion of the 'objective' research emphasis.

Some respondents from each sample made perceptive comments regarding the sociological dimensions of disciplinary behaviour, but the Moot sample seemed more aware of these factors affecting the acceptance of different theories and methods. Overall, although there were reservations, the Moot sample was rather more congenial on the question of whether or not they would like to participate in 'orthodox' research, than the TAG sample were on whether or not they'd like to participate in 'alternative' research

The Moot sample was also far more hopeful overall that there was some convergence between camps finally occurring. Yet, they also displayed an antipathy for some behaviour seen in 'orthodox' archaeologists, and sometimes also for the processes of orthodox archaeological research (e.g. boring, destructive). At the same time, some also displayed an antipathy for New Age elements within Earth Mysteries, which were perceived as unwelcome influences, and a desire to see more rigour within EM.

Finally, many respondents in both samples appeared to respond readily to the typology

implicit in the questions; that is, they knew what 'orthodox' and 'alternative' archaeology each were, and quite precisely what they thought of them. This type of consensus on matters which – by some indications – the respondents hadn't necessarily examined with any care, is certainly an interesting point with regard to the larger aims of this research.

TAG sample

Female 30-39, professional academic/field archaeologist

Question 6: Do you think the theories and methods of alternative and orthodox archaeology are compatible? Why or why not?

"To some extent the theories are, especially with the post-processualist movement in mainstream archaeology which is quite close, especially in 'experiential' work, to some alternative strands. Methodology is more divisive; process versus intuition (although not exclusively so)."

Question 7: Do you think that alternative archaeologists and orthodox archaeologists communicate with each other enough? Why or why not?

"No – alternative archaeologists have an interest and enthusiasm which isn't sufficiently utilized; e.g. Meyn Mamvro's recording of damage to standing stones in Cornwall, and interesting ideas on uses of fogous, which are no less valid than the mainstream theories. However, many alternative archaeologists are suspicious of an orthodoxy which demands academic methodologies and training drawn from 'Establishment' sources, and like many other people in this country, are undereducated in Science and thus fearful/resentful of it (some post-processualists could also be described this way!) Mainstream archaeologists are fearful of losing credibility, and being associated with the "loony fringe", if they step outside acceptable theories and vocabulary."

Male 30-39, University archaeologist

Question 1: What do you think is the best way of referring to research concerned with human history, but operating outside of orthodox archaeology? (e.g., "Alternative Archaeology", "Earth Mysteries", "heterodox archaeology"). Do you think such labels are important? Why or why not?

"Pseudoarchaeology". While claiming to study the human past, pseudoarchaeologists think and argue under a fundamentally different paradigm of knowledge to science. This should be labelled and separated."

Question 5: Have you ever participated in alternative archaeological research? If so, please describe briefly. If not, would you like to? Why or why not?

"It's only possible to sustain the beliefs of alternative archaeologists by distorting and hiding evidence. This is opposite to the academic ideal of full disclosure of evidence and open argument. So there can be no dialogue."

Question 6: Do you think the theories and methods of alternative and orthodox archaeology are compatible? Why or why not?

"See above! Exactly parallel to biologists vs. creationists. These are different belief systems. One explains the world well and has good predictive value; the other is effectively a religious belief and can have spiritual value to believers. But it's not a useful tool to explain the world."

Question 7: Do you think that alternative archaeologists and orthodox archaeologists communicate with

each other enough? Why or why not?

"See above! But there is still a lack of communication with the general public as a whole. Most excavators still think of the public as a nuisance! But it's a waste of time talking to the lunatic fringe."

Female 30-39, Field archaeologist

Question 6: Do you think the theories and methods of alternative and orthodox archaeology are compatible? Why or why not?

"No. I can't really take it seriously."

Question 7: Do you think that alternative archaeologists and orthodox archaeologists communicate with each other enough? Why or why not?

"I don't really want to communicate with alternative archaeologists."

Male 22-25, Research student.

Question 5: Have you ever participated in alternative archaeological research? If so, please describe briefly. If not, would you like to? Why or why not?

"No. Yes. I'd like to see if any scientific method could be introduced to the research to make its results more acceptable in mainstream discourse."

Question 6: Do you think the theories and methods of alternative and orthodox archaeology are compatible? Why or why not?

"The heavily romantic end of archaeology, such as Gimbutas, share similar methods. People with interest in earth energies ought to be compatible with mainstream archaeology, but only with a tempering of rhetoric."

Question 7: Do you think that alternative archaeologists and orthodox archaeologists communicate with each other enough? Why or why not?

"No, because both sides view the other as enemies of the truth."

Male 40-49, academic/field archaeologist

Question 1: What do you think is the best way of referring to research concerned with human history, but operating outside of orthodox archaeology? (e.g., "Alternative Archaeology", "Earth Mysteries", "heterodox archaeology"). Do you think such labels are important? Why or why not?

"(Lunatic) Fringe."

Question 7: Do you think that alternative archaeologists and orthodox archaeologists communicate with each other enough? Why or why not?

"Communication would be pointless, futile."

NB I cannot help but note that this individual checked boxes indicating he had never heard of *The Ley Hunter*. strongly disagreed that there is a Viking site in Newfoundland (which is true), strongly agreed on

the Boxgrove statement (which isn't quite true), strongly agreed on the Dogon statement (which most 'orthodox' researchers assume isn't true), and disagreed strongly on the Pangaea statement (which is generally conceded to be true).

Female 22-25, student

Question 5: Have you ever participated in alternative archaeological research? If so, please describe briefly. If not, would you like to? Why or why not?

"I have not, as of yet, no. I wouldn't mind having a go to make up my own mind about how rigged or 'objective' the methods are."

Question 6: Do you think the theories and methods of alternative and orthodox archaeology are compatible? Why or why not?

"They are not really compatible, as much alternative archaeology is based on ignorance of what has been/can be achieved by archaeology. I have read several 'alternative' books, which have made me think "If they had studied archaeology at university (i.e. properly), they would realise that what they write is naive and ignorant." "

Question 7: Do you think that alternative archaeologists and orthodox archaeologists communicate with each other enough? Why or why not?

"See above. The 'real' archaeologists just laugh at the alternative ones, for whatever reasons, often with cause. Why not address 'alternative' issues with 'proper' methods?"

Female 30-39, academic/field archaeologist

Question 1: What do you think is the best way of referring to research concerned with human history, but operating outside of orthodox archaeology? (e.g., "Alternative Archaeology", "Earth Mysteries", "heterodox archaeology"). Do you think such labels are important? Why or why not?

"I think such labels are divisive and destructive. Serious research concerned with human history cannot be seen as being outside of orthodox archaeology – only 'crackpot' research can."

Male 30-39, field archaeologist

Question 1: What do you think is the best way of referring to research concerned with human history, but operating outside of orthodox archaeology? (e.g., "Alternative Archaeology", "Earth Mysteries", "heterodox archaeology"). Do you think such labels are important? Why or why not?

"Research into human history outside of archaeology is called history, the study of past written documents. There are artefactual subsets of valuable study e.g. art history. People studying the supernatural are cranks."

Question 5: Have you ever participated in alternative archaeological research? If so, please describe briefly. If not, would you like to? Why or why not?

"No, don't want to, I believe it has zero benefit and is a bad thing, trivialising the past, strengthening views that people in the past were stupid and incapable and methodologically unsound."

Question 6: Do you think the theories and methods of alternative and orthodox archaeology are compatible? Why or why not?

"No. Orthodox archaeology has a methodology and a theoretical basis. Alternative archaeology as I understand it has not. It's for people who have no faith in their own or human history."

Question 7: Do you think that alternative archaeologists and orthodox archaeologists communicate with each other enough? Why or why not?

"No, which is a good thing. I would not want my work devalued by new age loonies. I strongly do not wish to be bracketed with this false discipline and strongly resist any attempt to do so."

Male 22-25 archaeology student

Question 1: What do you think is the best way of referring to research concerned with human history, but operating outside of orthodox archaeology? (e.g., "Alternative Archaeology", "Earth Mysteries", "heterodox archaeology"). Do you think such labels are important? Why or why not?

" 'Fringe archaeologies'; it's important, not to conflate it with 'serious' arch."

Question 7: Do you think that alternative archaeologists and orthodox archaeologists communicate with each other enough? Why or why not?

"There has to be no communication, just education; a need to understand why 'alternative' archaeologies exist."

Female 30-39 Field archaeologist

Question 7: Do you think that alternative archaeologists and orthodox archaeologists communicate with each other enough? Why or why not?

"No - the proliferation of Alternative Archaeology/Earth Mysteries highlights the failure of orthodox archaeology to reach, inform and interest a wider audience."

Female 20-25, conservation cleaning supervisor

Question 7: Do you think that alternative archaeologists and orthodox archaeologists communicate with each other enough? Why or why not?

" No - all alternative archaeologists are viewed as cranks by most orthodox archaeologists. Some are (in my view) but those with valid points to make are ignored as people don't like to risk their reputations (or economic standing) through it."

Male 30-39, field archaeologist, doing PhD

Question 5: Have you ever participated in alternative archaeological research? If so, please describe briefly. If not, would you like to? Why or why not?

"No. There is little reason to do so, as its aims do not match my own."

Question 6: Do you think the theories and methods of alternative and orthodox archaeology are compatible? Why or why not?

"No. 'Alternative' archaeologists tend to be 'believers' who merely collect evidence to fit their theories, which are frequently monocausal, while 'orthodox' archaeology proceeds by debate and uses the methods of social science."

Question 7: Do you think that alternative archaeologists and orthodox archaeologists communicate with each other enough? Why or why not?

"There is little point in communication. The communication failure is that of orthodoxy, which has failed to reach the general public and demonstrate how untenable many of the ideas of 'alternative' archaeologists really are."

Male 26-29, research student

Question 5: Have you ever participated in alternative archaeological research? If so, please describe briefly. If not, would you like to? Why or why not?

"No, but I have met such researchers. I would like to learn more about it because I feel it is important but not taught in academia."

Question 6: Do you think the theories and methods of alternative and orthodox archaeology are compatible? Why or why not?

"Yes. Different interpretations of the same material evidence."

Question 7: Do you think that alternative archaeologists and orthodox archaeologists communicate with each other enough? Why or why not?

"No. Plenty of prejudices against alternative arch. abound in academia."

Male 26-29, field archaeologist

Question 5: Have you ever participated in alternative archaeological research? If so, please describe briefly. If not, would you like to? Why or why not?

"No. Because it is surrounded by unhelpful mystic/religious connotations that are unnecessary and unappealing."

Question 6: Do you think the theories and methods of alternative and orthodox archaeology are compatible? Why or why not?

"Possibly, but the agenda of 'alternative' archaeology seems geared to some forms, of mystical/spiritual that appear more important than the history itself."

Question 7: Do you think that alternative archaeologists and orthodox archaeologists communicate with each other enough? Why or why not?

"No, because the above-mentioned agenda puts 'orthodox' archaeology off, quite rightly too!"

Male 30-39, field archaeologist

Question 1: What do you think is the best way of referring to research concerned with human history, but operating outside of orthodox archaeology? (e.g., "Alternative Archaeology", "Earth Mysteries", "heterodox archaeology"). Do you think such labels are important? Why or why not?

"Spooof nonsense. Yes, because they are nutters. Harmless, so I have no problem with them, but they are nutters."

Question 5: Have you ever participated in alternative archaeological research? If so, please describe briefly. If not, would you like to? Why or why not?

"No, because it is all cobblers. Spooof nonsense."

Question 6: Do you think the theories and methods of alternative and orthodox archaeology are compatible? Why or why not?

"No, because it is all cobblers. Spooof nonsense."

Question 7: Do you think that alternative archaeologists and orthodox archaeologists communicate with each other enough? Why or why not?

"As much as necessary. I will tell them anything they wish to know and they can do what they like with the information. I am not interested in their spooof nonsense."

Moot sample

Female 30-39, teacher and PhD student in English.

Question 4: Have you ever participated in orthodox archaeological research? If so, please describe briefly. If not, would you like to? Why or why not?

yes, volunteer dig.

Question 6: Do you think the theories and methods of alternative and orthodox archaeology are compatible? Why or why not?

"Partially. Each can inform/stimulate the other and should encourage rigour in thought and argument. Maybe that's an optimist's view."

Male 40-49 , library assistant, long-term researcher in EM, folk art, etc.

Question 4: Have you ever participated in orthodox archaeological research? If so, please describe briefly. If not, would you like to? Why or why not?

" No and in certain circumstances, on certain sites. I would enjoy archaeological work; would also enjoy

mapping work. But probably not scientific analysis!"

Question 6: Do you think the theories and methods of alternative and orthodox archaeology are compatible? Why or why not?

"Yes, increasingly compatible, as world-views move closer together – EM towards accountability, arch. towards cognitive issues."

Question 7: Do you think that alternative archaeologists and orthodox archaeologists communicate with each other enough? Why or why not?

"No, but things are changing. Still much suspicion on each side."

Male 50-59, retired, professional surveyor

Question 4: Have you ever participated in orthodox archaeological research? If so, please describe briefly. If not, would you like to? Why or why not?

"Have observed. Would like to but have found participants arrogant, unfriendly, rather boring and most unwilling to discuss possible theories which differ from their own – a bit like religious fundamentalists."

Question 6: Do you think the theories and methods of alternative and orthodox archaeology are compatible? Why or why not?

"Yes, very much so, but unlikely with present structures. "Us vs. them" – "paid & amateur" – "experts and morons" – [establishment officers, entrepreneurial authors and voluntary sector amateurs, don't seem to gel.]"

Question 7: Do you think that alternative archaeologists and orthodox archaeologists communicate with each other enough? Why or why not?

"See under (6) but probably the real reason is the 'heretics' phenomenon. Academics seem to fear for their careers and follow or refuse to follow like sheep. A number of recent TV documentaries on people like Sheldrake, Hahn, Eysenk etc. and classic Velikovsky case over Worlds in Collision is the best example. A refusal to examine all evidence is NOT SCIENTIFIC and this is the irony."

Male 40-49, teacher

Question 4: Have you ever participated in orthodox archaeological research? If so, please describe briefly. If not, would you like to? Why or why not?

"Very little but would like to do more."

Male 50-59, former mayor, personnel manager

Question 4: Have you ever participated in orthodox archaeological research? If so, please describe briefly. If not, would you like to? Why or why not?

yes, some orthodox involvement

Question 7: Do you think that alternative archaeologists and orthodox archaeologists communicate with each other enough? Why or why not?

"... Of course communication is a good thing and there's too little, partly because university lecturers teach out of date theories and mould students into acceptance rather than encouraging new and challenging ideas."

Male 40-49, teacher

Question 6: Do you think the theories and methods of alternative and orthodox archaeology are compatible? Why or why not?

"No reason why they shouldn't be. There's more than one way to skin a cat, as they say."

Question 7: Do you think that alternative archaeologists and orthodox archaeologists communicate with each other enough? Why or why not?

"No. Narrow-mindedness on all sides. Scientists and researchers in all subjects have always been the same. Perhaps it's necessary to be this way in order to give 100% to your line of enquiry and not be distracted by doubt."

Female 26-29, editor of antiquarian magazine

Question 6: Do you think the theories and methods of alternative and orthodox archaeology are compatible? Why or why not?

"The two sides – at the moment – have trouble mixing. The orthodox side appears a closed world not willing to embrace what the unorthodox side have to say. But the unorthodox side is sometimes punctuated by wishy-washy new-age thinking which is a big hindrance."

Male 30-39, laboratory technician

Question 1: What do you think is the best way of referring to research concerned with human history, but operating outside of orthodox archaeology? (e.g., "Alternative Archaeology", "Earth Mysteries", "heterodox archaeology"). Do you think such labels are important? Why or why not?

"'Alternative archaeology'. Though this is far from ideal. A label is needed to differentiate what is now termed Earth mysteries from its 'new age' connotations."

Question 4: Have you ever participated in orthodox archaeological research? If so, please describe briefly. If not, would you like to? Why or why not?

"NO. I can think of few things more boring than to assist at an archaeological dig."

Question 6: Do you think the theories and methods of alternative and orthodox archaeology are compatible? Why or why not?

"No. Alternative archaeology crosses subject boundaries more easily. Researchers are unfettered by career/peer pressures to conform. Orthodox archaeologists tend to work through reductionist filter."

Question 7: Do you think that alternative archaeologists and orthodox archaeologists communicate with

each other enough? Why or why not?

"No. Though communications have noticeably improved over the years. Most archaeologists still view alternative archaeology as "lunatic fringe". There also seems to be an ivory tower mentality that "our approach is the only one valid". "

Female 40-49, tour guide, zoo animal nurse

Question 6: Do you think the theories and methods of alternative and orthodox archaeology are compatible? Why or why not?

"Can be, but tends to rest on personalities."

Female, 40-49, student

Question 6: Do you think the theories and methods of alternative and orthodox archaeology are compatible? Why or why not?

"Largely compatible now after a decade of convergence."

Question 7: Do you think that alternative archaeologists and orthodox archaeologists communicate with each other enough? Why or why not?

"No!! Sites are still being destroyed and archaeologists are not aware, or have to cut off from, other people's depth of feeling about this. They are still participant in the destruction of sites that should not be touched by invasive means, and they distance themselves from the ethics of removing human remains out of graves. They must debate outside of themselves about such concerns."

Male, 40-49, "lapsed landscape architect" (ex- professional earth manipulator)

Question 4: Have you ever participated in orthodox archaeological research? If so, please describe briefly. If not, would you like to? Why or why not?

"No -- too rigid, systemised, indoctrinated, academic, b...o...r...i...n...g, reductionist, destr..."

Question 7: Do you think that alternative archaeologists and orthodox archaeologists communicate with each other enough? Why or why not?

"No. Power, status/credibility attack, peer group opinion, elitism, professional snobbery, sexual insecurity.... y'know, all the usual neuroses and psychoses."

Female, 50-59, teacher

Question 6: Do you think the theories and methods of alternative and orthodox archaeology are compatible? Why or why not?

"Yes, to some extent. Alternative styles have more space for intuition and spiritual dimension."

??, 40-49, architect

Question 6: Do you think the theories and methods of alternative and orthodox archaeology are compatible? Why or why not?

"They should be. Some 'alternative' methods work outside of a scientific methodology but require testing in order to be of any use."

Question 7: Do you think that alternative archaeologists and orthodox archaeologists communicate with each other enough? Why or why not?

"No. But this is largely due to a lack of reading on the part of both parties of each others literature."

Male 40-49, publisher, graphic designer, trainer, consultant

Question 1: What do you think is the best way of referring to research concerned with human history, but operating outside of orthodox archaeology? (e.g., "Alternative Archaeology", "Earth Mysteries", "heterodox archaeology"). Do you think such labels are important? Why or why not?

" 'Earth Mysteries' tends to encompass my interests – it's also quite a good bookshop category from a marketing point of view. But I can see that it's not very suitable for gaining credibility with academics!"

Question 4: Have you ever participated in orthodox archaeological research? If so, please describe briefly. If not, would you like to? Why or why not?

"No. I don't think that I would like to – it all feels a bit dry and dusty on a symbolic as well as a reality level."

Question 6: Do you think the theories and methods of alternative and orthodox archaeology are compatible? Why or why not?

"I think that the methods are complementary. The trouble is that the practitioners need to have respect for all the theories -- not just their own -- for there to be a true spirit of cooperation."

Question 7: Do you think that alternative archaeologists and orthodox archaeologists communicate with each other enough? Why or why not?

"Yes, in a generation. It needs some of the old wood to die off first and a greater penetration into mass consciousness of wyrd ideas. It will happen – "The X-Files" and "Strange but True" are just the beginning!"

Male 26-29

Question 6: Do you think the theories and methods of alternative and orthodox archaeology are compatible? Why or why not?

"Yes - the problems lie in the fundamentalist nuts-and-bolts freaks on both sides who have irrational fears of 'giving in' to perceived 'loonies' or 'deadheads'. The roots to the problem lie in the childish strand of counter-culture in alternative practitioners and the previously prevalent public school tight-arsed nature of conventional practitioners! Rebellion versus conformity. It is time for both sides to mature!"

Question 7: Do you think that alternative archaeologists and orthodox archaeologists communicate with each other enough? Why or why not?

"We're getting there but we must all be brave and sacrifice a few sacred cows that should have been sent to the knacker's yard long ago."

Male 60+, food importer

Question 7: Do you think that alternative archaeologists and orthodox archaeologists communicate with each other enough? Why or why not?

"No, although it is quite possible to do so. If people will be tolerant and listen to what the others are saying there is undoubtedly much common ground. I have seen this happening over the last two or three years."

Male 40-49, clerical worker

Question 6: Do you think the theories and methods of alternative and orthodox archaeology are compatible? Why or why not?

"Occasionally -- especially now that both sides are borrowing heavily from cultural anthropologists and cognitive psychologists. However, some of the orthodox remain too hidebound -- and a good number of the alternatives are mad as hatters!"

Question 7: Do you think that alternative archaeologists and orthodox archaeologists communicate with each other enough? Why or why not?

"No, they don't. Because one lot think they are better-equipped to operate (through education, training, experience) and tend to be wary of compromising professional status by mixing with fringe elements. The alternatives do not trust the orthodox, and some believe in a conspiracy to suppress anomalous or heterodox findings."

Female, 40-49, mature student in information technology

Question 6: Do you think the theories and methods of alternative and orthodox archaeology are compatible? Why or why not?

"I think the theories and methods are compatible and could be complementary -- but I don't think the people are -- there are currently too many stereotypes."

Question 7: Do you think that alternative archaeologists and orthodox archaeologists communicate with each other enough? Why or why not?

"No. I get the impression that the extremists in each group are always trying to 'disprove' the extremists in the other group. It's as if they need to remain in opposition. I think and hope that this situation is changing."

Appendix B

Dowsing Surveys

1. Informal Dowsing Survey, University of Sheffield Dept. of Archaeology

This survey was conducted in January 1995, as a simple exercise for the author to learn about the range of beliefs regarding dowsing within the Sheffield Archaeology department, and to stimulate later informal conversation on the subject.

24 respondents – faculty and graduate students – filled in a one-page survey with the following questions. There were no boxes to check; respondents were simply invited to fill in the blank space with their answer. Typically, answers given were brief and unambiguous.

The survey was not designed to sustain statistical analysis; however, the distribution of responses is interesting enough to include here as a basis for some limited commentary. Thus, I have summarised the results below.

1. Does dowsing work?

Yes 9
No 3
Maybe 7
Huh? 1

2. Have you ever tried to dowse? If so, please describe your experience.

Yes 12 Of that 12: Experience was Helpful – 9 Experience was Unhelpful – 1
No 12

3. Do you know of any cases where dowsing has been used by archaeologists to locate underground features? If so, what was the outcome?

Yes 8 Of that 8: Experience was Helpful – 7
No 16

4. Would you dowse, or ask someone to dowse, your site to look for underground archaeological features?

Yes 5
Yes as PART of strategy 7
Maybe 6
No 6

5. Would you accept a dowser's map as representative of reality with no further investigation (as you might accept a resistivity or magnetometer survey)?

Both need checking 11
No 9
Investigate further 4

6. If you excavated and found a feature where the dowser indicated, would that constitute 'proof' or validation of the dowser's skill?

After continual success, yes 13
Yes 2
Maybe 6
No 3

7. Would you believe that if a dowser located a feature and excavation revealed nothing, that the dowser might have detected an 'imprint' of a feature which had once been there but was removed?

Yes 1
No 13
Noncommittal 11

8. Do you believe the claim, made by some dowsers, that all major megalithic sites are located over a confluence of underground streams?

No 15
Noncommittal 9

9. Would you believe someone who said they could date a site, and provide other details, (e.g., location of burials, age and sex of those buried) by dowsing?

No 19
Noncommittal 5

10. Would you believe someone if they told you they could dowse over a map of a site (as opposed to actually over the ground) and thereby locate underground features?

No 19
Noncommittal 5

Comments on the results (above) of the informal Sheffield survey

The most interesting aspect of these results for me was simply the incongruity between this fairly high level of belief in dowsing's helpfulness to the archaeologist, and its very infrequent mention in archaeological publications. Beyond this, however, there are some further comments to be made.

The questions were arranged, essentially, according to a gradient of what I guessed would be increasing reluctance to accept the given proposition. Respondents did seem to confirm these suspicions, 'drawing the line' more strongly towards the end of the survey. It would be interesting to reconfigure the questionnaire sometime to see if the pattern holds when the questions are taken out of order.

Only 3 respondents were willing to say unequivocally that dowsing does not work. Thus, many of those who had not tried it themselves (12) were willing to reserve judgement on the matter.

Most of those who had tried to dowse, and most of those who knew of cases of archaeological dowsing, had found the experience to be positive/helpful.

Fully half of the respondents indicated that they would use dowsing on site as a remote sensing method, while only $\frac{1}{4}$ said firmly that they would not.

13 of the 24 respondents indicated that repeated discovery of underground features where a dowser had said they would be, would constitute 'proof' or validation of the dowser's skill. This would seem to suggest a cognitive bias in assessing dowsing's validity, i.e. correlation might be taken to be significant even in the absence of statistical evidence on the probability of that correlation existing.

Respondents were negative and noncommittal in similar numbers on the subject of dowsing detecting 'imprints' of archaeological features which had been removed. This is of interest because this is something very difficult to test in 'natural' field conditions (rather than controlled experiments).

Respondents were primarily negative on the proposition, made popular by Guy Underwood and some dowsers and Earth Mystics since, that megalithic sites are located above confluences of underground streams. At this point, most respondents (63%) 'drew the line' and answered with an unequivocal 'no'; the rest were noncommittal.

Three-fourths of respondents were firmly negative, and the rest noncommittal, on the last two questions, regarding propositions about dowsing which were essentially 'psychic' rather than physical in nature. At this point, belief in dowsing's ability had decreased from the initial 67% positive/maybe, in the first question, to zero.

In summary: belief in dowsing was diverse, but key patterns of interest emerged. First, respondents were not generally willing to accept dowsing's acceptability as a source of information which can be taken as valid without testing (through excavation), but were willing to take a positive result (i.e. confirmation through excavation) as grounds for belief in dowsing's efficacy. Taken with the high proportion of those who tried dowsing and found it helpful, this *may* indicate a cognitive bias in favour of hypothesis-testing procedures, but a possible ignorance of the sound construction of such procedures. In turn, this could account for belief in dowsing's effectiveness on site just as easily as actual effectiveness could. Second, there was a clear disbelief in essentially psychic claims with respect to archaeological dowsing.

Further examination of these patterns with a large-scale survey might prove interesting indeed.

2. 1995 TAG/Moot Survey: Dowsing Question

As part of a long survey questionnaire distributed at TAG '95 and at the Ley Hunter Moot 1995 (see Appendix A for details), the following question was asked:

Dowsing can be effectively used: (circle one or more appropriate letters)

- a) to find underground water
- b) to locate energy lines
- c) to find underground mineral deposits
- d) to find lost objects and people (*i.e.*, map dowsing)
- e) to answer questions asked by the dowser
- f) to locate and map buried archaeological features
- g) in no circumstances; it does not work

In all, there were 41 respondents in the TAG sample, and 27 in the Moot sample. Of those, most did answer this question, though some left it blank.

These are the collated responses:

TABLE A

	find water	find energy lines	find mineral deposit	find lost objects	answer questions	find arch. features	does not work	don't know
TAG (n=36)	25	3	8	1	4	9	4	3
Moot (n=26)	26	10	20	16	13	18	–	–

Interesting patterns visible in these collated responses include these:

- the high overall apparent level of belief in both samples in dowsing's efficacy at finding water (unanimous in Moot sample, but also quite high in the TAG sample)
- members of the TAG sample were less likely to indicate belief in dowsing's ability to find archaeological features than were the Moot members. But nonetheless, fully 25% of TAG respondents to the question did indicate belief in dowsing finding archaeological features.
- although the members of the Moot sample were unanimous in indicating their belief in dowsing's ability to find water, only 28% indicated belief in dowsing's ability to locate energy lines
- the members of the Moot sample were much more likely to indicate belief in the 'psychic' aspect of dowsing, i.e. its ability to answer questions, locate lost objects

If we take the raw responses, in tables B and C below, more subtle patterning is evident.

Table B: TAG Sample's responses to dowsing question

Dowsing can be effectively used: (circle one or more appropriate letters)

- a) to find underground water
- b) to locate energy lines
- c) to find underground mineral deposits
- d) to find lost objects and people (*i.e.*, map dowsing)
- e) to answer questions asked by the dowser
- f) to locate and map buried archaeological features
- g) in no circumstances; it does not work

Respondent	a – can find water	b – can find energy lines	c – can find mineral deposit	d – can find lost objects	e – can answer questions	f – can find arch. features	g – does not work	??? penciled in 'don't know'
1	✓					✓		
2	✓							
3	✓		✓			✓		
4							✓	
5							✓	
6	✓		✓					
7	✓	✓	✓					
8	✓							
9	✓							
10	✓							
11	✓							
12	✓				✓	✓		
13	✓							
14	✓							
15						✓		
16						✓		
17	✓		✓					
18	✓							
19	✓		✓					
20	✓	✓						
21	✓							
22	✓							
23	✓				✓	✓		
24	✓		✓					
25	✓	✓	✓	✓	✓	✓		
26	✓							
27	✓		✓					
28	✓							
29							✓	
30					✓			
31							✓	

32						✓		
33	✓					✓		
34								✓
35								✓
36								✓
Total	25	3	8	1	4	9	4	3

Table C: MOOT Sample's responses to dowsing question

Dowsing can be effectively used: (circle one or more appropriate letters)

- a) to find underground water
- b) to locate energy lines
- c) to find underground mineral deposits
- d) to find lost objects and people (*i.e.*, map dowsing)
- e) to answer questions asked by the dowser
- f) to locate and map buried archaeological features
- g) in no circumstances; it does not work

Respond-ent	a – can find water	b – can find energy lines	c – can find mineral deposit	d – can find lost objects	e – can answer questions	f – can find archaeological features	g – does not work	??? penciled in 'don't know'
1	✓		✓	✓	✓	✓		
2	✓							
3	✓	✓	✓	✓	✓	✓		
4	✓	✓	✓					
5	✓			✓				
6	✓							
7	✓		✓					
8	✓		✓	✓	✓	✓		
9	✓	✓	✓	✓	✓	✓		
10	✓	✓	✓	✓	✓	✓		
11	✓		✓	✓	✓	✓		
12	✓	✓	✓	✓		✓		
13	✓							
14	✓	✓	✓	✓	✓	✓		
15	✓		✓					
16	✓							
17	✓		✓	✓	✓	✓		
18	✓							
19	✓		✓	✓		✓		
20	✓	✓	✓	✓	✓	✓		
21	✓	✓	✓	✓	✓	✓		
22	✓		✓			✓		
23	✓	✓	✓	✓	✓	✓		
24	✓		✓	✓	✓	✓		
25	✓	✓	✓	✓	✓	✓		
26	✓		✓			✓		
Total	26	10	20	16	13	18	0	0

The results shown in Tables B and C permit the observance of a rather interesting pattern. Due to the small sample and the nature of the survey, this cannot be taken as statistically significant, but is thought-provoking nonetheless, and possibly worthy of further exploration.

- in the TAG sample, 7/36 respondents (19%) indicated belief in dowsing's efficacy at finding water, AND in its ability to find energy lines and/or mineral deposits – but NOT archaeological features. In contrast, there were 3/26 members (12%) of the Moot sample who indicated belief in dowsing's ability to find water AND energy lines and/or mineral deposits but NOT archaeological features. Thus there was a bias in both groups against accepting dowsing's efficacy for finding archaeological features compared to other things, but this bias was somewhat greater among archaeologists.

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