"Interdisciplinary Practice"

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Alison Wylie, Professor of Philosophy at Washington University, is Archaeology's philosopher of science. Over many intensive examinations of archaeological thinking and practice Alison has used both philosophy of science and a feminist platform to develop insights into the nature of archaeological science.

Conversation Précis

June 7, 2006. In commenting on the state of affairs in contemporary archaeology, Wylie outlines an agenda for archaeology as an interdisciplinary science rooted in ethical practices of stewardship. In so doing she lays the foundations for an informed and philosophically relevant 'meta-archaeology'. (Editors' note: We invited Timothy Webmoor to join our conversation given his interests in philosophy, pragmatism and science studies.)

Michael Shanks: *Alison, what is your take on the state of affairs at the moment in archaeology? After almost thirty years of engaging with philosophical questions related to archaeological 'evidence', what is your view of what is occurring now? What direction do you feel it should go? Do you have an agenda that you would like to pursue in this regard?*

Alison Wylie: I have two thoughts about that. One falls under the heading of the relevance of interdisciplinary science studies to archaeology and the other under stewardship issues. Both have to do with the on-going debate in archaeology about disciplinary goals and how features of context and values shape epistemic questions.

Interdisciplinary science studies in/of archaeology

AW: Perhaps it's a generational thing, but I've always been fascinated by the question of why archaeologists should so actively debate questions about disciplinary purpose and identity. I think of David Clarke's "Loss of Innocence" (1973) as a pivotal discussion of this question, but as often as you see the catchy title, the real significance of this article lies in an historical thesis that is not often cited. Clarke argues that, after the Second World War, the technical resources for doing archaeology had expanded dramatically in ways that meant you couldn't just do 'business as usual' and know what that meant. You had many more choices to make about what kinds of data to collect, how to record them, what kinds of analysis to use, how to interpret the results, because there were vastly more kinds of data that you could be collecting, analyzing, and interpreting. Clarke mentions aerial photography, as a recording practice that made it possible to see medieval field lines in the landscape that were not visible on the ground during standard walk-over surveys. Archaeological horizons have expanded even more since Clarke reflected on their implications for 'innocent practice'. Think of all the forms of archaeological science that make it possible now to do microscopic edge wear and residue analysis, to source raw materials, to trace genetic affiliation, to reconstruct dietary profiles and paleoenvironments; once available, these open up a whole range of possibilities that require you to reflect on what questions can or should be asked. The social and political dimensions of practice add a whole other level of complexity, but even just technical development, taken on its own, means that you cannot assume a particular set of questions or form of practice as a given, as a settled tradition that can be pursued 'innocently', in Clarke's terms.

Clarke's response in 1973 was to make the case for an internal philosophy of science; he argued against imposing models that had been developed for the natural sciences. I want to broaden that mandate in ways that have now been argued for by a great many others. What one needs to make informed decisions in archaeological contexts is a rich social history of archaeology as well as the kind of conceptual analysis – a matter of disembedding underlying assumptions and teasing out their implications – that is a legacy of philosophical training. This 'second order' study of archaeology, 'meta-archaeology', is not a luxury (to paraphrase Lorde 1970).

MS: *It is not some kind of secondary reflection upon what really is happening in archaeology.*

AW: Right; it is reflective analysis that is constitutive of archaeological practice . . .

Chris Witmore: *To that end, I am often struck when I read Binford or Leone or Schiffer when they were writing in the 1970's and early 80's how they say something that was restated in a different way at a later time under the guise of a later theoretical camp. To forget the past through a rhetorical split from that which came before, whether we call it 'postprocessualism,' 'processualism,' 'culture history,' or indeed work even earlier in the discipline is to run the risk of repetition.*

AW: Yes, and often not happy repetition.

CW: *To be truly inventive, to do something which has not been done before requires us to remember. Forgetting the engagements that occurred and the questions that were asked under what has been blackboxed as 'processualism', what has been blackboxed as 'culture history', creates an environment where something of that effort may be, and often is, rephrased and repeated. Of course, this repetition occurs under a banner which is claimed (rhetorically) to be different. Tracking these iterative patterns has been a critical component for your work and this connects to your point concerning the relevance of science studies.*

AW: Yes. Although I would say that the account I've developed of this dynamic of debate is pretty thin history. It is intellectual history based on professional publications; I tracked citations back from debate about the New Archaeology in the 1960s and 1970s, initially to assessments of how archaeology had changed in the first couple of decades following the Second World War, then to the controversy generated by Walter Taylor's *Study of Archaeology* (1948), the 'typology debates' of the1930s and, finally back to arguments for a 'new archaeology' that had appeared in the First World War era. But this is the tip of the iceberg. There's a great deal of contextual understanding that readers at the time would have taken for granted and that we're likely to miss (or to mis-take), especially if we work only with published material. To get a sense of what the published record presupposes requires serious archival research and oral histories, where they're feasible. Even so, I was struck by the degree to which earlier rounds of debate had just disappeared, even when they were readily accessible – a matter of prominent archaeologists publishing in mainstream journals on issues described in essentially the same terms as in current debates.

One example of this amnesia that I discuss in the first section of *Thinking from Things* – "How New is the New Archaeology" – is the argument for a 'real, new Archaeology' published by Clark Wissler in 1917, and anticipated by Roland Dixon a few years earlier (1913). They described 'The New Archaeology' of Wissler's title as a scientific form of practice. Archaeologists who signed on to this program were no longer focused on artifact collecting for its own sake; the value of archaeological material lay, for them, in the use they could make of it in building and testing hypotheses about "the whens, the whys, and the hows" of the cultural past, as Dixon put it in 1913 (565). I see resonances here with a 19th century discussion of multiple working hypotheses published in *Science* by a geologist (Chamberlin 1890); the more 'reasoned' approach urged by Wissler and Dixon (among others) was a matter of systematically forming problem-specific questions and seeking evidence that would allow you to address those questions.

Whenever I describe *this* 'New Archaeology' to archaeologists steeped in the New Archaeology of the 1960s and 1970s there is a shock of recognition and surprise that they've never heard of it before. There is, of course, much that's distinctive in the way the most recent New Archaeologists elaborated the goals of explanatory understanding and the recommendations for instituting a problem-oriented, hypothesis-mode of practice; I don't mean to reduce everything to a simple common denominator. But the broad outlines of the problematic with which the most recent New Archaeologists wrestled were clearly articulated much earlier.

CW: *So, under this rubric of 'meta-archaeology' there is a need for richer histories of the discipline . . .*

AW: Yes; what's needed in the areas that have been of particular interest to me are the kinds of rich contextual histories that can explain why it was that, in the 20th century in North America, crisis debates took shape in archaeology roughly every thirty years, coinciding with the First World War, the Second World War, and the Cold War era. Why do these breaks emerge at these junctures? Is it a recurrent inter-generational dynamic? A general unsettling of the status quo? What else was going on? Who were Dixon and Wissler responding to? What were the unarticulated lines of opposition, and what were the views of those who just got on with what they took to be normal ('innocent') practice and never joined the debate in print? I don't explore these further questions, but even as thin a history as I tell brings into view a dynamic of debate of that helps put current debate in perspective. At the very least it counters the claims of discontinuity, the exceptionalism, so often associated with demands for 'the new.' But in addition, some structural features of these debates become evident that are not so obvious when you consider each of them independently.

What generates oppositional debate is, I argue, a persistent epistemic anxiety: that if you embrace ambitious anthropological, explanatory goals you must inevitably indulge in speculation, you must go beyond the data in ways that are unscientific. In its starkest form this presupposes a view of inference – ampliative inference – on which any claims that are not literally entailed by observations are all equally insecure. And in that case the only alternative to irresponsible speculation seems to be a form of narrow empiricism (as Clyde Kluckhohn called it) that significantly limits the scope of inquiry, undercutting the commitment to go beyond essentially descriptive 'space-time systematics'. But these options are not, strictly speaking, dilemmic. That is, they are neither exclusive nor exhaustive of the methodological and epistemic options open to archaeologists. There are all kinds of possibilities for making nuanced use of archaeological evidence that go beyond empirical description but do not necessarily collapse into arbitrary speculation – whatever you care to dream up – and are well within the range of inferential practice typical of the historical, natural and social sciences. What I have in mind are controlled forms of abductive inference that do not leave you in a state of conceptual paralysis, unable to make any reasoned judgment about the relative credibility of the claims on offer, even though they rarely deliver you a single, uncontrovertibly true conclusion. This is a brief for a robust but non-skeptical pluralism of the kind outlined by contributors to *Scientific Pluralism* (Kellert, Longino, and Waters 2006)

So the lesson I drew from tracking back several generations of crisis debate in archaeology is that these debates are structured by a false dilemma. They take the form of a contest between polarized positions that obscures a whole range of possibilities which, in fact, archaeologists have explored very successfully in practice. Another kind of history is needed to understand why those extreme positions keep being regenerated and why they are set in the strict 'either-or' choice structure of a dilemma.

CW: *And this dynamic runs really deep . . .*

AW: . . . It seems to run very deep. But to understand why it does requires an analysis of the social and institutional contexts in which dominant scientific ideals have been articulated, and of the political and economic interests that drive contestation over disciplinary status.[1]

Bill Rathje: *Alison, as a philosopher of science, why archaeology?*

AW: Before I answer this I'd like to pick up on a point I mentioned at the outset – about why we need to find ways to support, train, and nurture science studies practitioners who have deep roots in a range of disciplines (history and philosophy of science, sociology and anthropology of science) and who are just as deeply rooted in the sciences they study. My own experience in getting this cross-disciplinary grounding

[1] (AW): There is excellent work along these lines, although not on exactly the issues that have concerned me; I think of the kinds of socio-cultural analysis – an 'anthropology of archaeology' – called for by Kelley and Hanen (1988: 99-104), and by Gibbon (1989: 173-180) in the 1980s, and the socio-political histories developed by Tom Patterson (1995), Bruce Trigger (1989), Stephanie Moser (1992, 1998), to name a few.

underlines just how hard it is to sustain the vision of an engaged science studies given the way academia is set up today. I'll come back to the autobiographical details, but the point I want to make is that changing the institutional, disciplinary structures to support broad cross-field literacy – scientific literacy for humanists and an appreciation of the philosophy and social/history of science for scientists – is consequential, not only for science studies as a discipline but for the sciences themselves.

An example of the kind of socially and historically sophisticated 'internal/ analysis (to use Clarke's term) that I think needs to be done is illustrated by a set of projects I learned about when I served as commentator on a session on 'Eminent Mounds' that Sissel Schroeder organized for the 2003 SAA in Milwaukee.[2] For this session Schroeder brought together archaeologists working on Hopewell and Mississippian mound sites who were making innovative use of existing collections and excavation records; these included Fort Ancient, Poverty Point, Cahokia, Aztalan, Jonathan Creek, Marksville, Etowah, among others. The point of connection among them was that, in order to address current questions, they'd all had to undertake substantial histories of the work that had been done on these sites, often over 100-150 years: histories of the excavation techniques used, the curation (or, just as often, the dispersal) of records and collections, evolving traditions of interpretive thinking about these sites, their legal status and their public reception. These are fine-grained histories, much more richly contextualized than the kind of intellectual history I've done. The contributors to this session were all digging into the archival details, trying to figure out why archaeologists working in the 1880s, or in the early 20th century, or in the WPA era in the 1930s, had collected and recorded what they did, how their work had been taken up (or not) by later archaeologists, what hypotheses and presuppositions framed their fieldwork and publication practices.

What especially struck me was the innovative, critically discerning ways in which the Eminent Mounds contributors put surviving collections and records to work. In some cases these internal histories were the basis for reconstructing what had not been collected. In others they made it possible to integrate into a single GIS database information gleaned from surveys, site maps, feature logs, and day books spanning as much as 150 years. They made it possible to see spatial, temporal relationships more clearly. Sometimes they made it clear the need to substantially rethink long established claims about building sequences and occupational histories, about how sites had been laid out (their sight lines, and what purposes barriers could have served) and how they related to one another on the prehistoric landscape. In still other cases, these histories raised quite specific questions about context and provenience that could be resolved by strategic, low-impact fieldwork such as resurveying, or reexcavating stratigraphic profiles. At Cahokia, for example, this was designed to securely locate features that had been described in isolated terms and never tied in to other features or the stratigraphy of the site as a whole. Often enough what started out as a reassessment of narrowly empirical claims ended up destabilizing assumptions that had been the foundation for models of regional interaction and theories

[2] I've developed these comments into a paper that will appear in a collection of essays on "Agnatology" edited by Proctor and Schiebinger (Wylie 2008).

about the trajectory of cultural development; the effects ramify through the conceptual framework of Mississippian and Hopewell research.

This throws into relief the uncertainty of even our best-established archaeological interpretations; it counters any impulse to assume that we have in hand, or can expect to establish, conclusions about the cultural past that are stopping points in inquiry. But it also shows that this loss of innocence need not devolve into an arbitrary battle of preferences. The evidence from both the archaeological record and the historical record of archaeological research can pretty decisively eliminate whole families of interpretive options that had seemed plausible; it narrows the field at the same time as it opens up promising new possibilities.

To put this in the frame of Clarke's call for critical self-consciousness, the most obvious advantage conferred by this kind of rich contextual history is a broad awareness of the legacies (intellectual and technical, political and social) that shape contemporary practice. But as I've argued, it can also be put to work in more specific ways; it can be undertaken with the aim of reassessing and 'repositioning' the evidence in relation to particular questions or traditions of practice.[3] It is precisely the resource you need to make responsible use of old records and collections. But more than that, it can allow you to learn things that those who did the original collecting and recording did not necessarily intend to learn, or did not think they could learn from the material they assembled. It allows you to take stock of the partial, situated nature of the archaeological 'records' produced by previous generations and it is this that makes it possible the reuse, the further or extended use, of these resources. So there is a whole spectrum of ways in which social, critical history can be constitutive of research practice when it serves as the basis for framing a standpoint on knowledge production.

CW: *This is a very positive example of reiterative research: i.e. revisiting previous contexts, reworking previous materials in order to enrich, not only what was done before, but also what we are doing now.*

AW: Right.

MS: *Following on Bill's question, what is your agenda in these issues? Why would you want to raise such issues with archaeologists?*

AW: …because it seemed to me crucial for archaeological practice, not an ancillary interest, as history of archaeology is often treated.

MS: *It is absolutely necessary to do so because you need to be critical of your sources. If you are using maps, plans, datasets, you need to know under what conditions they were generated and this is what I am calling source criticism. It is the history of the development of your material.*

[3] This is language I draw from Rolph Trouillot's *Silencing the Past* (1995); a discerning history of archaeological practice can put you in a position to read the received record against the grain (including the literal records, archives and traditions, as well as the archaeological record).

But you are also saying something else, which is connected but I think distinct. You are saying, as Clarke was saying, that critical self-consciousness of your discipline is absolutely necessary because a radical split between doing something and then locating it, situating it, is not possible. All knowledge construction is located and situated and this is historical; it is social; it is cultural. That to me perhaps provides a way of approaching what Bill was asking and what I would like to know – motivation. What is your motivation, personal motivation, yes, but what about political location, or the fact that you can do better archaeology which is located?

Two formative questions: of archaeology and philosophy

AW: So there are (at least) two questions here: why philosophy of ***archaeology***, and why ***philosophy*** of archaeology. The short answer to both is that when I got actively involved in archaeological field work in the 1970s, debate about the latest New Archaeology was raging and it had an explicitly philosophical component; there was a sense that philosophy mattered to practice. At the same time, I was starting to study philosophy seriously as an undergraduate and I discovered that philosophers of science were also struggling with questions of relevance; the ones who most influenced me rejected the arid formalism of 'rational reconstruction', which had increasingly become a matter of puzzle solving for its own sake, and insisted that philosophical analysis must be grounded in a solid understanding of the history of science and of actual scientific practice. So I got interested in crisis debates in archaeology because I found myself in the middle of one, and I got interested in the intellectual, the epistemic dimension of these debates (as opposed to their historical, social, political dimensions) because it was amenable to the kind of conceptual analysis I was tooling up to do as a fledgling analytic philosopher.

That said, the questions that interest me, and the kinds of tools required to address them, really push the envelop of conventional conceptual analysis. In most general terms, what animates pretty much all my work are some quite traditional philosophical questions about evidence: what counts as evidence or, to put it in more active terms, what makes for credible evidential reasoning in a discipline that aims at understanding a past subject and, specifically, a social, cultural, human past. But the reason I was drawn to these questions, as opposed to others that were on the philosophical agenda at the time, is that they seemed relevant; if philosophy was going to make a difference to archaeological practice, it seemed to me that these were the questions to address. They're a persistent source of creative, critical tension in archaeology; they figure, not just when disciplinary crises break (when they become explicit), but in all kinds of more local, methodologically grounded debate. Reframed in field-specific terms, then, what I want to understand is how archaeologists adjudicate evidential claims in practice; how you use resources drawn from a wide range of fields to make nuanced, comparative judgments of credibility; and how, in the process, you negotiate the space that lies between, or beyond, the dilemmic options that so often structure internal epistemic debate. The Eminent Mounds papers make it clear that evidential reasoning is about more than facts and theories. By implication, they throw into relief the limitations of a strictly philosophical approach; the tools of conceptual analysis are not adequate, on their own, to the task of answering these questions.

A longer answer to your question about my motivation for focusing on archaeology, as a philosopher, is pretty much a matter of quirky personal history; a conspiracy of circumstances. My point of entry to philosophical analysis of archaeology was sustained involvement in archaeological fieldwork alongside an immersion in philosophy. I spent several seasons on prehistoric sites along the St. Lawrence River as a kid and then, while I studied philosophy of science in college and graduate school, I worked on historic sites for Parks Canada, spent a season at the Grasshopper field school, and worked several partial seasons in central Mexico and on an extant architecture survey in upstate New York.

Tim Webmoor: *You sketched the outlines of this in the preface to* Thinking from Things *(2002). Your experience on-the-ground at archaeological excavations fed into philosophical questions.*

AW: Yes. I am so often asked how, or why, I came to work at this intersection between archaeology and philosophy that I wanted to include an account of the trajectory that led to *Thinking from Things* (2002). One referee strongly recommended against integrating any autobiographical discussion into the chapters that make up the main body of the book, so Bill Woodcock (the editor who was responsible for *Thinking from Things*) suggested that I draft what he called a 'personal preface'. But here are some details that aren't included in that preface.

My first field experience came when my family spent several summers working on sites that Jim Pendergast was surveying and excavating along the St. Lawrence River valley in the 1960s. My father was an army colleague and good friend of Jim's, who is now something of a legend in Canadian archaeology. Jim had come up through the ranks in the Canadian military and, along the way, had conceived a passion for Iroquoian archaeology.[4] When Jim took early retirement from the Canadian army he became an associate director of the National Museum (in 1972), by which time he was well established in Canadian archaeology; he was known for what Wright describes as "his almost single-handed resurrection of the St. Lawrence Iroquois from undeserved obscurity" (2004: 2). But before he joined the Museum he would recruit family and friends to work with him on sites that interested him, or that the Museum wanted tested. He describes how he involved everyone he could in these projects in an interview with Ian Dyck, but notes with some regret that most of the kids (his own and those of various family friends) were "keen on the history side but not on the archaeology side" (2004: 28-29); evidently I was the only one who went on in archaeology.

[4] (AW): Jim Pendergast was essentially self-educated, but he developed a sophisticated field practice, published prolifically, and ultimately worked with a great many professional archaeologists, including Bruce Trigger at McGill and Scotty McNeish while he was in residence at the National Museum in Ottawa. Jim was recently honored by a feschtrift, *A Passion for the Past: Papers in Honour of James F. Pendergast* (Wright and Pilon 2004), that features an eminent list of contributors and an initial essay with the subtitle, "Blurring the Amateur-Professional Dichotomy" (Wright 2004: 1-5); he certainly did that!

BR: *Your experience on the ground has gained you a great deal of respect among archaeologists . . . Would you say that you had a passion for archaeology?*

AW: I definitely did not have a passion for it as a kid! I spent a lot of time holding a stadia rod and wishing I was at summer camp with my friends. But when I looked for a summer job after my first year of college, the experience I had had working with Jim Pendergast and his crew made all the difference. I applied to Parks Canada for a position as a field assistant and, to my surprise, I got it. They were developing a number of historic sites across Canada; I had hoped to go to L'Anse-aux-Meadows, a Viking site in Newfoundland that was getting a lot of attention at the time. But in the end I was assigned to Fort Walsh, a Northwest Mounted Police site in southwest Saskatchewan that dated to the late 19th century. This was the summer (1973) after my first year as an undergraduate at Mount Allison University, a small liberal arts college in New Brunswick. At that point no archaeology or even anthropology courses were being offered at Mt. A, so I didn't have any formal training for the job, just Jim Pendergast's endorsement – and that seemed to be enough.

The field director at Fort Walsh was Jim Sciscenti, who had done his graduate work in archaeology at the University of Arizona, at the time a hotbed of New Archaeology. My first contact with him was a long reading list he sent everyone who had been recruited to work at Fort Walsh that season. Before we arrived in the field we were all expected to have worked through, not just basic histories of the Northwest Mounted Police and reports on previous archaeological field work in area, but also a number of articles by New Archaeologists that were fast becoming classics and some of the philosophy of science they were citing.[5]

It turned out that all this reading was not just an intellectual exercise. In the field Jim Sciscenti insisted that we approach even the most mundane field work with clearly formulated hypotheses in mind, and he urged us to keep two sets of notes. One was a running record of what we actually did in a format required by Parks Canada, while the other was a sort of meta-narrative, an ongoing dialogue about what we suspected and what we inferred, a catalog of working hypotheses, hunches, commentaries on the process of doing fieldwork and especially on what surprised us, and how our thinking was changing as we worked. My impression, from low down in the hierarchy, was that Parks Canada really just wanted us to verify the historic maps and photographs of the Fort, and generate an assemblage of authentic NWMP

[5] I remember being puzzled but fascinated by the energy and intensity of articles by Fritz and Plog (1970), Deetz and Dethlefsen (1967), early Binford (1968, 1972), and Flannery's commentary (1967); I may also have read Longacre (1964) and some of the essays in Binford and Binford (1968) at that time, as well as a couple of chapters from Watson, LeBlanc and Redman (1971). And I remember struggling through Hempel on the function of general laws in history, and some Kuhn (1970 [1962]); I was intrigued even though I was not at all clear what connections I was supposed to be making to the archaeology. But even without the background to appreciate what was new and what was conventional, the sense of excitement was palpable!

artifacts that could be used in exhibits. In retrospect, I think Jim must have been at logger heads with his higher-ups from the time he joined Parks Canada.[6]

We found impressively rich evidence of what had been a large and extremely diverse community, with a number of much more substantial structures at the core of the townsite than we'd expected – trading establishments and residences with basements and rich assemblages of domestic artifacts – grading into more ephemeral structures on the periphery. Jim arranged for me to do a walk-over survey of the park property as a whole in the last summer I worked at Fort Walsh (1978), further expanding the time depth and breadth of the cultural history in which the brief NWMP presence in the Cypress Hills was embedded. I found evidence, well away from the Fort, of historic trails and what we described as Metis cabin sites – the foundations of chimneys, storage and refuge pits – that most likely dated to the early contact period when the Hills were a key locus of fine fur trade. We also identified sites that likely had components predating the historic period. But what really impressed me was the extent and the density of evidence of Native American presence in the area of the Fort, including a couple of sites that were rapidly being destroyed by construction of a visitors' center and a maintenance compound. I found lithic scatters and teepee rings on virtually every promontory from which you could sight the Fort – evidence, we assumed, of the tribal groups who took refuge in the Hills, displaced there by U.S. military action to the south and by encroaching Canadian-European settlement from the east. Fort Walsh was probably never militarily viable, but it was a key source of food relief during those winters, at a point when the buffalo herds had been hunted out, and it hosted a number of the treaty negotiations in which large groups of Sioux who sought asylum in Canada were convinced to return to the U.S., and local tribal groups signed agreements that would confine them to reservations.

So that's how I got into archaeology. I found myself in the field every summer, and I did develop a passion for archaeology largely because of the way Jim Sciscenti approached it. In lots of contexts at the time field assistants were not much involved in the thinking end of the operation; whether fair or not (where other projects were concerned), we certainly had the sense that we were doing business differently at Fort Walsh.[7]

BR: *What tipped you into philosophy as an undergraduate?*

AW: I had a philosophy of science professor, Paul Bogaard, who made the field come alive. I took an 'Introduction to Philosophy' class from him in my first year at Mt. Allison University and loved it. By the

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[6] Not only did Jim Sciscenti insist on hypothesis-driven investigation of the Fort itself, raising touchy questions about whether the palisade was ever defensible, why the NWMP barracks were sited in such a way that they risked regular flooding and washouts, how such a volume of whisky and beer bottles got into the privies and footing trenches, and why beads and other evidence of non-regulation clothing should so consistently appear under floor boards, embedded in dirt floors, and in dumps. He also quickly determined that some of the most interesting archaeological features on the park property lay outside the Fort; one season he surveyed the civilian townsite and the next he instituted a program of testing surface features, carefully structured as a stratified random sample of pits in different size categories.

[7] In addition to the way he approached fieldwork, Jim Sciscenti made a point of involving all of us in writing up the site reports (e.g., Sciscenti 1976), and in developing presentations for the professional meetings like the Society for Historical Archaeology (SHA).

end of that year I realized that no matter what course I was in – an introduction to Classics, or English literature, or History – I somehow always ended up doing philosophy. If I had to write a paper on literary criticism what really interested me were the philosophical issues: the moral puzzles, the questions about truth and personal identity and the nature of consciousness, that seem to come into particularly sharp focus in the work of great poets and novelists and playwrights. If I was writing a history paper I gravitated to intellectual history or to questions about what had happened in science and technology, and I was intrigued by questions about how historians come to know what they claimed to know. When I returned to Mt. A after my first season at Fort Walsh I knew I wanted to take more philosophy and discovered that Paul Bogaard was teaching a year-long course in History and Philosophy of Science. He was a philosopher of science who had a background in chemistry; he worked on questions about whether 'chemical' knowledge could or should ultimately reduce to physics and did some historical work on an influential French scientist/philosopher, Pierre Duhem. So I read Hempel and Kuhn over again, this time with more philosophical scaffolding, and began to understand the dynamic of debate within philosophy of science that had generated the models of confirmation and explanation I had first encountered in the 'fighting articles' by New Archaeologists that Jim Sciscenti had assigned the previous summer. Best of all, Paul was in touch with Merrilee Salmon through his philosophy of science networks and had a sense that there were philosophical issues in archaeology that would be worth exploring. He urged me to draw on my archaeological experience when thinking through philosophical questions; I ended up writing a paper on the question of whether archaeology had had, or was in the throes of having, a Kuhnian revolution.[8]

This interplay of philosophy and archaeology was intellectually very rich and my interest in each conditioned the other. Because of Paul Bogaard it just seemed obvious, right from the start, that it should be possible to seriously engage archaeology as a philosopher, in the same way he did chemistry and others were doing with a range of so-called 'special' sciences.

By the time I was finishing my undergraduate degree (in 1975-76), I knew I wanted to pursue a joint track – a philosophy of science PhD with a focus on archaeology – and I had the good fortune of learning, through Bogaard, about a graduate program that would support just this kind of training: the Program in History and Philosophy of the Social and Behavioral Sciences at SUNY-Binghamton. Unfortunately the HPSBS program no longer exists, but it was exactly what I needed; it required graduate students to come in with, or to get, at least a Masters level training in the social sciences they intended to study. In addition to the HPSBS program in Philosophy, archaeology at SUNY-Binghamton was enormously exciting at that time.

The fall I arrived (1976), John Fritz and Meg Conkey had just joined the Anthropology department. Chuck Redman had arrived the previous year and although Fred Plog had just left his influence was palpable. So I found myself in another hotbed of New Archaeology. I ended up doing a lot

[8] (AW): I concluded that it depended on what you thought Kuhn meant by 'revolution,' and on whether logical positivist philosophy of science could be said to constitute a paradigm—whether 'deductivism' in theory had any impact on practice.

of coursework in Anthropology: the 'Core Concepts' courses that all incoming anthropology students had to take in their first year, and then a full suite of courses in archaeology in the following two years. It was a tough program; I did the archaeology and anthropology courses as an overload, on top of what was required in Philosophy. But I got to work back and forth between robust graduate training in archaeology and a full slate of philosophy courses tailored to the interests of students who wanted to do serious philosophical work on the social sciences.

CW: *How big a blip on the radar screen of philosophy was archaeology at that time?*

AW: Not much of a blip at all! Merrilee Salmon was working seriously on archaeology; she and her husband Wes Salmon had been drawn into debate about the New Archaeology while they were at the University of Arizona in the 1970s. I met Merrilee when I was at the Grasshopper field school in the summer of 1977, and had the opportunity to work with her for one-semester as a visiting graduate student at the University of Arizona in the spring of 1978. The chance to talk through the philosophical aspects of the New Archaeology with someone who knew the details from the inside, but as a philosopher – that was absolutely crucial to the formation of my dissertation topic (*Positivism and the New Archaeology)*, and to my whole sense of what the issues were and how to approach them. She published *Philosophy and Archaeology* in 1982, the first monograph on philosophy of archaeology and still, I would say, the single most elegant and comprehensive philosophical analysis of the questions about explanation and confirmation that had been raised in particularly stark terms by the New Archaeologists.

Marsha Hanen and Jane Kelly, at the University of Calgary, were well into their philosophical-archaeological collaboration by the time I finished my dissertation and moved to Calgary as a postdoctoral fellow (1981).[9] Jane had approached Marsha because she was curious about the philosophical rhetoric of the New Archaeology, and together they wrote a series of articles and ultimately a book on *Archaeology and the Methodology of Science* which came out in 1988. I was based at the University of Calgary as a postdoctoral fellow from 1981-83 and then for another year in 1984-85, so I heard a lot about their work on this book and was much influenced by their particular brand of constructivism and their thinking about 'inference to the best explanation' in archaeology; they didn't convince me to abandon my retrograde scientific realism, but I did learn a lot from them about how philosophical analysis could be made accountable to the realities of archaeological practice.

I also met Red Watson and Patty Jo Watson in this period, and spent a year with them at Washington University as a postdoc in 1983-84. So I had a chance to argue through with Red all the reasons why I was skeptical of his resolute commitment to a kind of enlightenment-inspired positivist vision (e.g., Watson 1972, 1976), and to follow the development of Patty Jo's thinking as she turned *Explanation in Archaeology* (1971) into *Archaeological Explanation* (1984). I learned a lot from them, not

[9] (AW): Marsha Hanen had been trained as a philosopher of science by Nelson Goodman, but had an interest in legal reasoning and the use of evidence in legal contexts which she extended in interesting ways to archaeology.

just talking philosophy, but spending time in the field with Patty Jo on the shell mounds sites she'd been working on in Kentucky, and doing some cave archaeology (see Chapter 6).

There were also several philosophically trained archaeologists whose work was appearing by the late 1980s. I think especially of Guy Gibbon who I believe spent a year at LSE where he immersed himself in realist philosophy of science. He published *Explanation in Archaeology* in 1989 in which he argued that scientific realism made much better sense of the conceptually plausible and practicable aspects of the New Archaeology than the logical positivism they had appropriated from Hempel. My thesis supervisor, Rom Harré at Oxford, was a key player in the development of the realist philosophy of science that Guy put to work in his analysis, and had been the philosophical framework for my dissertation (e.g., Harré 1970, Harré and Secord 1972); I was much encouraged that Guy was pursuing this line of analysis, although I didn't meet him until much later. And, of course, there was the whole nest of philosophically engaged archaeologists – students of Ian Hodder's at Cambridge – who I met as a graduate student when I spent the Winter and Spring of 1979 working with Rom Harré at Oxford. I went across to Cambridge to attend a couple of their seminars that spring, and got to know them through the Symbolic and Structuralist Conference in which I participated the following year (April 1980; Hodder 1982). As emerging post-processualists, they were immersing themselves in a number of philosophical traditions that I knew only in passing, given my training in analytic philosophy of science; it was exciting to see the main lines of their critique of the New Archaeology take shape first hand.

BR: *So there was a quite lot of cross-field traffic in the late 1970s and early 1980s . . .*

AW: Yes. Even though philosophy of archaeology didn't have much visibility in philosophy – it wasn't a recognized sub-field – I didn't feel I was working in a void. Most important, the impulse to seriously engage archaeology was by no means an isolated development within philosophy of science. This was the period in which philosophy of science began to speciate into science-specific areas of study, spinning off autonomous societies like the International Society for the History, Philosophy, and Social Studies of Biology and journals like the *History & Philosophy of the Life Sciences.* Over the years I've found that quite a few philosophy colleagues are fascinated by the details of archaeological practice and intrigued by the philosophical puzzles it poses. So I get drawn into thematic conferences and symposia on topics like 'models in science' or 'explanation' or 'uses of new evidence' . . .

TW: *Or feminism and philosophy . . .*

AW: . . . or feminism and philosophy, which developed as an independent track for me – something I came to through quite a different kind of engagement with practice, in this case activist practice.

MS: *How idiosyncratic your trajectory was!*

AW: I was just lucky that the HSPBS Program at SUNY Binghamton existed for the short time it did, just when I was looking for hybrid graduate training in philosophy of social science. The founder and director of the program, Ted Mischel (a philosopher of psychology), died the first year I was in Binghamton and was never replaced. Very few of us finished the program.[10]

CW: *Archaeology is peculiar, and this is something that I would like to flush out with you, in that it stretches over the divide that separates the sciences and the humanities. While this position has contributed a wide range of situated practices, it has also donated to a peculiar set of anxieties along with fragmentation in general. Would you care to comment on this relation to science studies, the position of the discipline, and the issue of fragmentation?*

AW: I think of the fragmentation you describe as a legacy of processes of disciplinary formation that typically require archaeology, and philosophy, to be defined, in opposition to some set of cognate fields. Training, funding, peer-review processes, professional recognition, all are structured in obvious ways by this canalization of disciplinary streams.

For at least a decade now philosophers of science have been clear that to do responsible analysis of science, not only must we be grounded in the sciences we study (as I've mentioned), but we must also have a working knowledge of the sociology and history of these disciplines. For a philosopher it is demanding, to say the least, to keep all those balls in the air. Worse, you risk being penalized within philosophy if you are seen to be doing work that looks too much like history or sociology. This is not just a matter of research priorities that focus attention on different aspects of science, although that's part of the problem. For many of my colleagues what counts as really elegant mainstream philosophy is a form of conceptual analysis that disembeds the nub of a philosophical problem from all the messy contextual factors, systematically stripping out just the kind of details that interest historians and sociologists and, increasingly, philosophers of science. I can't tell you the number of times I've been asked whether my work is really philosophy. If you are too grounded in the discipline you study then you are not properly 'meta'; you are engaging problems internal to the science rather than problems that originate in philosophy. And if you take historical or sociological factors into account, you are assumed to be doing some kind of (merely) empirical rather than philosophical inquiry. So, ironically, what I consider to be embarrassingly thin intellectual history of archaeology is altogether too rich for many philosophical audiences. Which is just to say that the disciplinary divides among science studies still make it difficult to bring together the resources we need to do the kind of integrated analysis that post-positivist philosophers of science have been calling for.

But what I think you had in mind is fragmentation within archaeology, a field that is characterized by a kind of hybridity that has an impact on its relationship(s) with various fields of science studies. It is, famously, constituted by several quite different disciplinary traditions – Old World, New World,

[10] There was nothing like it at the time, and to this day nothing quite like it has taken shape elsewhere. Despite a clear appreciation, since the early 1970s, that we need to train what Bunge referred to as 'amphibious' philosophers of science – philosophers who are as at home in philosophy as they are in the sciences they study (1973: 16-18) – there is very little support for this kind of integrated training. Philosophy of science students are now invariably expected to have strong grounding in whatever science they study, as the HPSBS program required, but for the most part they have to get it on their own, typically as a prior graduate degree.

anthropological, art historical, classical – each with its own distinctive research agenda, each defined by a distinctive affiliation with historical, humanistic, or scientific forms of inquiry, and each with its own tradition of engagement with (or studied disengagement from) its own history, and the philosophical challenges posed by its distinctive mode of practice. There have been many different streams running as it were.

TW: *These streams are not holding their course; they are overflowing their canalized banks and cutting new and divergent channels* . . .

AW: . . . the question is, how do we make this diversity-cum-hybridity productive rather than divisive? I think you see here the same kind of disciplining as I experience in science studies. If you are an archaeologist in a predominantly sociocultural department, you're likely to get uptake to the extent that you can submerge the archaeology-specific elements of your research and treat archaeology as a case study or a tool kit, useful for addressing problems that are in vogue among symbolic anthropologists or subaltern studies scholars or evolutionary theorists. On the one hand, it is important to keep these connections in play, to explore the potential for archaeological inquiry framed in terms of these very different research programs. On the other hand, I feel that something is lost when the specifics of archaeological practice become interesting only insofar as they can be decontextualized and resituated in some other disciplinary context, brought to bear on problems defined in terms of some other research tradition.

CW: *Let's go with that for a second, what is specific about archaeological practice? What is peculiar to archaeology as an array of fields, as a range of practices?*

AW: Now there is a conceptual trap! I'm not a fan of demarcation criteria – to use a philosophical term of art – but when I think about archaeology, what usually strikes me as distinctive is a sophisticated focus on material culture. Of course, what this means and how it will be transacted varies across different contexts of archaeological practice, and it typically depends on techniques and forms of practice that are not the exclusive property of archaeology. Strategies for studying material culture are clearly relevant, and are increasingly recognized to be relevant, to the research questions asked in a wide range of historical subfields and, indeed, in any social science that makes use of 'unobtrusive measures', to invoke Webb's famous title (1966). So I don't see that you could ever expect to draw a line around archaeology and feel confident that that anything inside is archaeological and whatever lies outside is not.

What is distinctive about archaeology, what it has to offer the various disciplines now exploring 'materiality', is a much more sophisticated take on what you can do with material culture.

MS: *We are circling around the issue of what exactly archaeologists do and the issue of discipline, boundaries and the difficulties of crossing over. You have just described the almost accident that let to you making the connection. It was in spite of the world that you went into that you made the connections. It was contingent on the actual structure of the discipline. And I think that what we have now perhaps is* *an agenda of being interdisciplinary, cross-disciplinary. But the difficulties are still there and perhaps even stronger than they were. It comes out in situations where practitioners in other disciplines, be they philosophers of science, historians, anthropologists or science studies scholars, want to use the products of archaeology.*

AW: Often without engaging the discipline.

MS: *There is always the idea that archaeologists produce knowledge of the past. You are right in saying that is not just what archaeology is about! It is located, it is situated, it is people engaging materials and making stuff in very nuanced ways. There are difficulties related to practicing what you preach, but there are also those filters that get applied when one moves out to other fields. We raised this issue with Alain Schnapp (Chapter 13), the filtering systems present in the movement from disciplinary field to another and the selection process that occurs – one chooses certain components of another discipline that is of use to them. Most of our colleagues do this. And many see archaeology as discovering the past and that is it. You dig stuff up. Yes you can be source critical, certain people dug this up and certain people dug that up, but in the end, there is a fundamental notion that you are dealing with the past. The past is primary and of secondary importance is the history of engagement with the past. What we are saying is that the history of engagement is not secondary, but it is actually primary.*

TW: And that then questions the whole notion that you are discovering the past and gaining knowledge of the past – there is so much more to archaeology than this.

BR: *We should underline that the past/present relationship was not in your definition of archaeology.*

AW: The provocation to define boundaries – to identify what's distinctive about archaeology – that's the conceptual trap. I doubt that the appeal to a past/present relationship would do much better than the loose methodological criteria I've suggested. In fact, I don't think any very precisely framed definition could capture all the things that archaeologists do or that archaeology is said to be; and any definition catholic enough to include everything practitioners count as archaeology will no doubt admit any number of other kinds of inquiry, expertise, forms of knowledge that you would not want to call archaeology. I suspect that the best one can do is to identify features that link various kinds of archaeological practice, in the nature of a Wittgensteinian 'family resemblance'. A 'sophisticated focus on material culture,' and aligned strategies of inquiry – a suite of research techniques tuned to the investigation of the material dimensions of cultural life – is one such point of overlap between many, if not all, the members of this loose knit disciplinary family of practices.

MS: *Because you are back to drawing boundaries around things and you are pointing out that it is something else.*

TW: *Can we return to what you were saying in the beginning related to the role of meta-analysis in archaeology? There is more to your agenda here . . .*

Between 'meta-archaeology' and the philosophy of archaeology

AW: . . . Yes. For archaeology in its various forms to function well, to develop genuinely sophisticated, critically self-conscious forms of practice, it must incorporate the kinds of analyses for which historical/philosophical science studies scholars like Lorraine Daston and Ian Hacking are famous: analyses of how the subjects of inquiry are constituted by the tools of inquiry that researchers bring to bear in their investigation. Archaeologists like the contributors to the Eminent Mounds session – who are mainly concerned to solve archaeological problems, not to further the cause of science studies – are doing sophisticated, richly localized histories of research practice that show in some detail how particular techniques and conventions of research practice have played a role in stabilizing the constructs known as Hopewell and Mississippian cultures. Their work puts them in a position to read the received 'archaeological record' against the grain, to question the wisdom of pursuing questions about patterns of regional interaction and trajectories of development that are predicated on the reality of these constructs. But note that, even when they challenge fundamental framework assumptions, they do not trivialize archaeological practice; the point is not to expose archaeology as series of power plays sustained by self-delusion. It is to do better archaeology, and make archaeology more accountable. These social histories of research practice offer a constructive basis for moving forward; they represent what might be described as a mode of 'transformative criticism' rooted in the discipline itself (Longino 1990: 73-74).

CW: *Still, it is often the case that trivialization goes the other way whereby one encounters eclectic and quite superficial use of philosophy within archaeology . . .*

AW: There have certainly been a number of different modes of engagement with philosophy, and one has been rhetorical. The appropriation of 'scientific philosophy', post-war logical empiricism, clearly served a rhetorical purpose for the New Archaeology in the 1960s and 1970s. Then there was a backlash partly fostered by philosophers like Chuck Morgan, a particularly pugilistic analytic philosopher based at the University of Victoria who wrote a review of Watson, LeBlanc and Redman (1971) in which he took them to task for failing to understand not only the nuances of Hempelian positivism, but the most basic principles of philosophical practice (1973). But as Watson, LeBlanc and Redman said in their defense (1974), although Morgan had condemned them for appropriating philosophical models without fully understanding their presuppositions or the purposes for which they had been developed, he himself hadn't learned enough about archaeology to know why these models had been attractive to them. They challenged him to role up his sleeves and do some honest philosophical work, rather than trash archaeologists because they aren't philosophers. Why not put philosophical expertise to work for constructive rather than narrowly critical purposes?

This exchange really crystallized for me what was at stake in finding a way to sustain a truly amphibious philosophy of science. I had lots of problems with the uses New Archaeologists had made of positivist, Hempelian models, which were often inconsistent, rhetorical, superficial. But I thought Morgan's response could hardly have been more counterproductive; it was a negative object lesson, a model of how not to engage philosophical problems that arise within a science.

The response of a good many archaeologists at that point was to withdraw from philosophical debate altogether. But despite this no-nonsense, 'just get on with business' stance, even the most anti-philosophical archaeologists deal with profound, intriguing and complicated philosophical problems on a regular basis. And as the internecine battles between processual and post-processual archaeologists fade into the annals of disciplinary history, I've been struck by how willing archaeologists are to bring philosophical resources to bear on these issues, or at least to consider what philosophical analysis might have to offer. No doubt some kinds of professional divisions of labor are unavoidable. With all the things that you need learn to do a good job as an archaeologist, it just isn't going to be possible to add substantial training in analytic or continental philosophy (or in all the cognate fields that make up science studies, for that matter). At the same time, however, a growing number of archaeologists do read deeply in a range of philosophical traditions, and there is a growing numer of philosophers who have taken a serious interest in archaeology.

TW: *You regard meta-archaeology as a good interface between philosophy and archaeology. Merrilee Salmon (1982) and Lester Embree also advocated a meta-archaeology (1992). Do you feel that meta-archaeology is something that would benefit both fields for the future? Is this an agenda of yours that you would want to push?*

AW: I do, although I am not altogether comfortable with the rubric of 'meta-archaeology'. As Lester Embree uses the term it seems like it should be a good framework for characterizing interdisciplinary trade that goes in many directions. He did understand meta-archaeology to be multi-disciplinary, to include historical and sociological or anthropological as well as philosophical studies of archaeology. By contrast, Merrilee advocated, not meta-archaeology, but analytic philosophy of archaeology, which she understood to include a number of subfields of analytic philosophy (ethics, metaphysics, epistemology and philosophy of science), but not the multi-disciplinary range of fields Lester had in mind (1993: 324). Even though Lester's meta-archaeology is more inclusive than this, he insisted that, at the end of the day, the problems that interest meta-archaeologists are fundamentally different from those that interest archaeologists, even when they seem to be about the same things – like 'explanation' or 'confirmation'. On his view the questions philosophers ask arise within the context of a distinctively philosophical tradition and should not be expected to bear on, or be accountable to, archaeological interests. So, as Lester conceived it, meta-archaeology reentrenches the conventional disciplinary boundaries that divide science studies. It was, for him, a collection of sub-fields defined in relation to existing science studies disciplines as branches of philosophy of science, history of science, sociology or anthropology of science, rather than something truly inter- or trans-disciplinary. In this respect, it doesn't capture what I find most exciting about the work emerging at the interface between archaeology and the cognate fields of philosophy, history, and sociology of science. It doesn't take into account the ways in which the philosophical questions we start with are reconfigured by sustained engagement with the sciences we study.

BR: *Here is an agenda . . .*

AW: I do frequently write pieces that address different audiences, and I find it enormously productive to play philosophical and archaeological questions off against one another; often I end up seeing possibilities for addressing each that are not at all obvious when viewed exclusively within their contexts of origin. If I have an agenda it is to make possible the kind of inter-field traffic between archaeology and philosophy (and between archaeology and Science and Technology Studies more generally) that can be transformative for all the disciplines involved. Certainly the way I think about philosophical, epistemic questions is very different as a consequence of working closely with archaeological cases, even when I write for a philosophical audience.

So to return to the point with which I started, there is an institutional dimension to my intellectual agenda. Philosophical and historical science studies of the kind I've been describing will only thrive if we can establish ways of valuing and supporting it as a form of active engagement in and with the sciences – not just as a study of particular sciences that may or may not be intriguing given a slate of problems generated by historians or philosophers. It is not that I think philosophy should just wither away. I draw heavily on the analytic skills and substantive knowledge of intellectual traditions that are a legacy of my philosophical training; these are exactly right for some types of conceptual, analytic work. Similarly, a well-trained historian will be able to do very different things with the history of archaeology than I have done. We need all the different disciplinary specializations represented within science studies. So my agenda is to find ways of better integrating science studies disciplines into the ongoing practices of the sciences.

Ethics and stewardship

MS: *Just to round this off with the issue of stewardship. This is an interesting tack, angle, trajectory we have within archaeology, the angle of reflexivity and also ethnography as a practice to get at what we do on the ground. Would you care to comment on anthropology's role in relation to philosophy's role as a field that helps us understand our practices?*

AW: Let me come at this first from a philosophical angle. Philosophical ethics is going through a transition that parallels in some respects what I've been describing for philosophy of science. I remember that when I studied ethics as an undergraduate and graduate student the convention was that you should first sort out the moral principles, the ethical theory, governing moral decision making at a high level of abstraction – that was the core of philosophical ethics – and then (somehow) figure out how to bring these foundational principles to bear on particular ethical problems. Consider how in everyday practice you depend on rules of thumb, aphoristic guidelines, that help you decide what to do – like 'what goes around comes around' or, 'treat others as you would like to be treated'. But the real philosophical work of justification is all done a much higher level, in terms of principles like the Kantian categorical imperative – never treat others as a means only; you have a duty to treat others as an end in themselves. This presupposes a theory about the nature of justification (that you should endorse only principles you can hold as universals), and a view of human nature (as distinctively rational).

A standard alternative, if you work within a utilitarian framework is to develop a calculus for weighing the harm that will be done by given action, or type of action, against its benefits. Here, the philosophical action is in the meta-ethical arguments that establish how to individuate harms and benefits, why consequences for moral agents should count rather than their inherent worth, and how you can counter the standard objections that consequentialism provides a justification for grossly unfair social arrangements that benefit many but at the cost of a few (e.g., on the standard examples, a small subset who are enslaved or penalized). On these top-down approaches, ethics proper is quite sharply distinguished from applied ethics; it is the business of applied ethicists to figure out how to bring the high level principles down to the level of actual cases in which moral decisions have to be made.

I expect that the toes of any ethicist reading this will be curling! But without going into a complicated intellectual history, the shift in ethics has been toward a more bottom-up approach that emphasizes the role of situated practical reasoning, where moral deliberation is more informed by precedent than by constitutions. The parallels with philosophy of science are obvious. In both areas the problems you take seriously as a philosopher get much more complex, and the tools you need to address them quickly outstrip the resources of philosophy. Often medical sociologists and anthropologists are better equipped to think systematically about the nuances of contextual, practical reasoning than we are. That said, the weakness of narrative and anti-theory approaches, for example, is that they don't deal all that well with structural constraints and power dynamics. But it isn't clear to me that the principled, top-down, approaches do any better; at least there's space in these more practice-oriented models of ethical reasoning for considerations of power relations and systemic inequity.

Philosophical analysis will only be useful if it is contextualized. It is one resource, and an important one, for narratively constituting the problems that archaeologists deal with in terms that will allow for creative and open-ended negotiation of a resolution. But this process will only be as good as your understanding of the interests that are stake in any given conflict, and that requires rich ethnographic, sociological, historically situated understanding of how and why the conflicts arise. So the strategies of philosophical engagement that I've been exploring in connection with epistemic, meta-methodological issues have a direct parallel where ethics issues are concerned.

BR: *This brings us to the issue of stewardship which you began with.*

AW: Yes, let's talk about stewardship. The language of stewardship has been around in various forms for a long time, but it enters the ethics lexicon in archaeology most visibly with the Principles of Archaeological Ethics adopted by the Society for American Archaeology (SAA) in 1996. I was drawn into the process that resulted in those principles when I was a 'visiting scholar' at UC-Berkeley in 1990-92. Through Meg Conkey, who was on the executive board of the SAA at that time, I was invited to write up a brief on the ethics issues that arise when professional archaeologists make use of looted and commercially traded materials. I wrote a short analysis and made a presentation to the executive board in the spring of 1992 (Wylie 1995). I didn't know this then, but the executive board was deliberating on an editorial policy for *Latin American Antiquity* that they adopted at that meeting. It specifies that *LAA* will not publish any article that reports data "recovered in such a manner as to cause the unscientific destruction of sites or monuments", nor anything derived from illegally exported antiquities (SAA 1993). I didn't see the proposed language for this policy until it was published. Although I think I understand what the motivations were, I have always been concerned that this policy is essentially unenforceable. With no cut-off date specified, for example, prohibiting the publication of material that has been illegally exported since the UNESCO Convention on Cultural Property was adopted, and with no further specification of what counts as 'unscientific destruction', it effectively rules out any publication of most major museum collections. This at a time when there is considerable pressure for archaeologists to make more systematic use of existing museum collections.

CW: *In fact the rubric and language of destruction itself is quite problematic* (cf. Lucas 2001).

AW: Unless the details are worked out, I think it is.

So this was the context in which I started to think systematically about research ethics in archaeology. In the process of writing the brief on looted data I read up on debates that were current at the time that the SAA executive board was deliberating on these issues. One of the most high profile cases was Donnan's decision to publish looted material from the royal tombs of Sipán in the *National Geographic* (1988, 1990). A freelance writer for *Science* characterized this as a dispute between archaeologists who took the 'high road' and refused to publish looted data, and those who, by implication, traveled the 'low road', who were willing to collaborate with commercial dealers and collectors in order to document these data (Alexander 1990). Donnan was outraged and published a letter to the editor of *Science* in which he described a set of guidelines governing the publication of looted data that he had worked out with the *National Geographic* (1991). These prohibit the publication of antiquities that had been exported or were held in violation of the laws of national patrimony in country where they originated. It turns out that, although the artifacts Donnan published had been looted and sold to a prominent collector, they had never left Peru, and this collector had registered them with a national antiquities office, as required by Peruvian law. So on these guidelines his publication was perfectly legitimate.

TW: *This creates a very grey area because he is documenting it and using that for information about objects others might not necessarily see because it was a private collection that had been looted.*

AW: Donnan's argument was, in essence, why cut off our nose to spite our face? Archaeologists are not going to stop the antiquities trade by refusing to publish looted data, and if these data have any evidential value, they should be prepared to work with them.[11]

TW: *This is the middle road approach . . .*

AW: Although I thought Donnan's arguments were problematic in a number of respects, it struck me that there just wasn't going to be any hard and fast rule that could tell you what you could and couldn't do in unambiguous terms. It depended. It depended, for example, on whether the data have any integrity. Donnan evidently had close enough ties with the looters, dealers, and collectors who handled the material he published in *National Geographic* to be confident that he knew exactly where it had come out of the ground. This stands in stark contrast, for example, with the assemblage of Cycladic figurines much documented and analyzed by art historians, that Gill and Chippendale show to be fatally corrupted by the antiquities trade (1993). Donnan also had the good fortune to identify a few remaining undisturbed graves and on the basis of excavations of these he was able to develop a rich analysis of burial practice and social, political relations, interpreting the imagery he recorded from the looted ceramics in light of the disposition of material in the excavated graves. So the circumstances that made the Sipán data useable were unusual in a number of key respects.

A number of the archaeologists I spoke to who said they would never publish looted data also noted that there was no point; material looted in the areas where they work rarely has any secure provenience and it's the kind of material that has next to no value as evidence without a detailed understanding of its depositional context. So whether you even confront a moral dilemma about whether to publish or not depends heavily on the nature of the material you're dealing with.

That said, I think Donnan was just wrong when he claimed that archaeological publication has no bearing on the market for antiquities. There are plenty of well-documented cases showing that archaeological publication has enormous impact on the market, determining the trade value of antiquities and creating markets for them in all kinds of direct and indirect ways. But again, the question of consequences is complex. If archaeologists did stop publishing all looted and commercially traded data, as the *LAA* editorial policy seems to require, how much difference would that make? It might have a huge impact on some antiquities markets, and none at all on others. Does it make a difference to build into high profile exhibitions searing images of looted sites and the destruction of antiquities? Donnan developed this kind of documentation for a traveling exhibition of the material from Sipán. These are empirical questions and we have very little data to go on in answering them. One thing is clear: the answers are complex and highly contextual.

So, that was my first engagement with ethics issues in archaeology. One recommendation I made to the executive board of the SAA, when I presented my brief, was that they should not adopt any policy

[11] For a detailed analysis of this argument see Wylie (1996), updated and reprinted as the final chapter of *Thinking from Things* (2002).

on looted data until they had undertaken a thorough review of all the ethics policies and guidelines that were already on the books. I discovered that the SAA had ethics guidelines embedded in their bylaws and in publications like the "Four Statements for Archaeology", that appeared in *American Antiquity* in 1961.[12] Before adding to these, I thought it would make sense to do a systematic inventory and assessment. Evidently the executive board agreed with that recommendation. They asked Mark Lynott, of the National Park Service, to chair an ad hoc committee on ethics, and told us to set our own mandate – which we construed as directive to review these statements and bring them up to date.

We found we couldn't assemble all the members of this committee at the annual SAA meetings so Mark Lynott and I wrote an NSF grant proposal asking for support from the Program on Ethics and Values in Science for a working conference. We got funding for a conference that was held in Reno, hosted by Don Fowler and Cultural Resource Management Policy Institute at the University of Nevada-Reno, and finally convened all the members of the committee as well as number of consultants and advisors in November 1993.[13] These included representatives from the AIA, the SHA, and practitioners working in a range of different field contexts, as well as Native American advisors – Leigh Jenkins (now Leigh Kuwanwisiwma) played an especially key role in this conference – and, at the urging of the NSF, we invited an underwater archaeologist who had worked with commercial salvors (Chris Hamilton). We set this meeting up as an exploration of the issues starting with questions about the relationship between professional archaeologists and commercial interests in the record on the first day, and moving, on the second day, to questions of accountability to descendant communities and to others effected by archaeological practice.

It was early in the first day that Leigh described a case in which the Hopi had successfully repatriated a sacred artifact that had been sold to a collector on grounds that the member of the tribe who sold it had held it in trust, on behalf of a secret society, and didn't have the right to sell it. I've told this story in a paper on stewardship (2005) so I won't repeat the details here – but his intervention fundamentally reframed the discussion.

TW: *How did the concept of stewardship come into the discussion?*

AW: It came in when Chris Chippindale and others involved in museum practice reflected on how they value the collections they curate and exhibit – collections they hold in trust on behalf of a wider community. But more generally, without ever saying what the point was or what he thought we should do, Leigh Kuwanwisiwma's story had the effect of redirecting our attention; it just seemed clear that we should be thinking about the ethics responsibilities of archaeological practitioners in much broader terms, in terms of accountability to a wide range of publics, and to cultural heritage broadly construed.

[12] (AW): This document was drafted by a committee of the SAA that had been charged with developing standards of practice and a professional code of conduct (Champ et al. 1961).
[13] A description of this process is included in the introduction to Lynott and Wylie (1995).

Mark and I certainly didn't imagine that this was how the conference would go. Our aim was to do some ground-clearing; the most we thought we'd achieve was to figure out what the agenda should be for the new SAA ad hoc committee on ethics and chart a course forward . . .

TW: *So this first meeting went further than you anticipated it would?*

AW: Yes, by lunchtime that first day a couple of participants said "why don't we get a working group together and draft a statement on archaeological stewardship?" They came back with an outline that afternoon, and several breakout groups went to work drafting the other components of what ultimately became the core of the SAA's Principles of Archaeological Ethics (SAA 1996). And that's how the Principles took shape. Because they don't presume that scientific interests trump all other interests, they mark a significant break from what was on the books. In a sense they build on the conservation ethic that Bill Lipe had articulated in the mid-1970s, but where Bill kept archaeological interests at the center – conservation was a matter of ensuring that archaeological resources would be available for future archaeological research (1974) – these principles make archaeologists accountable to a wide range of interest groups.

In those formative discussions, as I remember them, the focus was on joint stewardship; stewardship was conceived as a collaborative undertaking. Although concerns arose almost immediately about the slipperiness and potential conservatism of stewardship ideals – that they could easily be used to assert privilege and control access – I think it fair to say that the motivation was not to entrench new language that could be used to re-assert privilege and justify appropriation, but to frame accountability in much broader social terms.

TW: *And stewardship seemed to open things up.*

AW: Yes, given the context.

In the process of review, before the Principles were voted on and accepted by the SAA, we set up a number of sessions at national and regional meetings where we made a point of inviting people to comment on the Principles who had not been involved in the Reno meeting. We sought people who we knew would bring a fresh critical eye to the process.[14]

CW: *Does the notion of stewardship presuppose a particular type of relationship to the past? If you say that I am going to be steward over this or watch over that, I am going to conserve this, then are you potentially and, by the same token, arbitrarily foreclosing on other potential pasts?*

AW: That's one way stewardship can be used, but think, too, of the way environmental activists and Native Americans use the language of stewardship . . .

[14] (AW): Among them were Rick Elia, who made the case that the principles dealing with commercial interests were not nearly strong enough, and several others – especially Larry Zimmerman, and Anne Pyburn and Rick Wilk – who really nailed the point you raise, that the language of stewardship is potentially very dangerous, whatever the intentions of those who drafted the Principles.

CW: *. . . by empowering themselves to a particular end within a system that utilizes that language. But is stewardship wrapped up with a modernist set of relationships to the past, where the past is out of date, outmoded, and stripped of its action? By treating the past as something to be looked after does it not presuppose a distance, an* a priori *separation?*

AW: And a passivity. That said, I don't see that stewardship is necessarily a passive moral stance, a matter of dissociation rather than active engagement. Even within deeply conservative, paternalistic traditions there is a legacy of active stewardship. Consider religious Christian notions of stewardship: that as 'stewards of creation' we were put on earth with a mandate not just to protect and conserve, but to leave the world a better place than we found it. Stewardship in this tradition is anything but passive! It's a matter of ensuring that creation flourishes, of actively striving for self-improvement, anchored in a culturally specific, theological vision of what counts as flourishing.

CW: *Yes, but I did not say stewardship was passive.*

AW: Nonetheless, stewardship can be understood in all kinds of ways. It comes with a lot of baggage and can even serve elitist and exclusionary interests. And yet, for better or worse, First Nations, Native American, Indigenous peoples take the risk that the language of stewardship will betray them and use it creatively to open space for claiming recognition as stewards and asserting their responsibilities to living traditions, traditions that are constitutive of their identities and necessary to the flourishing of their communities. When stewardship is conceived in terms of joint or collaborative practice in an archaeological context, it seems to me to be potentially very fruitful.[15] So the question is, how will principles of stewardship be articulated? What will they mean in practice?

MS: *You pointed out, quite rightly, the good thing about stewardship is that it has opened up a particular set of relationships that weren't there to begin with for the SAA. So it has done some good work.*

AW: Yes. But, it was work within a particular context.

One thing that Mark Lynott and I felt quite strongly about was that the business of articulating a set of principles of archaeological ethics should not be conceived of as a matter of formalizing absolutes, setting them in stone. We thought the process was at least as important and the product, and that any set of statements or guidelines or code should be treated as a catalyst, a jumping off point, for ongoing deliberation.

One part of that process, I thought, would be the development of more closely specified guidelines for particular types of practice, or practice in particular contexts. In fact, a number of these

[15] Certainly, as I've said, it marks a significant break with a professional ethos that makes scientific knowledge of the past the primary goal of the SAA, and one that takes precedence over all else. We faced a lot of vitriolic criticism on that score. At the same time, the fact that a broad cross-section of the SAA membership was willing to endorse principles of stewardship may just reinforce the point about their slipperiness. No doubt stewardship was attractive, or at least acceptable, to quite a few archaeologists because it could be understood in precisely the terms that concern you: "as an archaeologist I am properly the steward of these cultural resources . . . and I know what's best . . . ".

already existed when we set to work – like the codes of conduct that had been developed by the Society of Professional Archaeologists (SOPA), and its counterpart in the U.K., the Institute of Field Archaeologists (IFA); or the guidelines for working with indigenous peoples that were developed by the World Archaeology Congress (WAC) and, independently, by the Canadian Archaeological Association.

TW*: In the CRM world, membership in SOPA is taken quite seriously as well. With little peer review of the vast 'gray literature' produced by the majority of practicing archaeologists, there seemed to be a dire need for some oversight.*

AW: Indeed it is. One good outcome of the process of proposing and ratifying the SAA Principles was that a core group of SOPA members reopened negotiations with the SAA and, ultimately, the SHA and AIA, to establish a jointly supported, jointly endorsed register of professional archaeologists – what became the Register of Professional Archaeologists (RPA).

Given the success establishing the RPA, it surprises me that the SAA hasn't moved to develop similarly detailed codes of conduct in other areas. The SAA Principles identify a whole range of communities, stakeholders, affected groups, interested parties to whom archaeologists are accountable, but accountability can mean very different things; exactly what it requires, and how archaeologists should negotiate conflicting interests isn't specified – couldn't be specified – by the Principles themselves. Perhaps the most pressing need for specification is in just the areas addressed by the WAC code; it would make all kinds of sense for the SAA to develop a code of conduct that specifies the particular responsibilities you have when you work with indigenous, aboriginal, First Nations peoples for whom archaeological sites and materials are not just a record of something past but part of a living cultural heritage.

BR: *So ethics was an agenda item of yours?*

AW: Work on ethics issues wasn't something I sought out; it wasn't a research agenda I had set for myself. In retrospect, however, I can't think of anything that has had a more profound impact on how I think about archaeology – including the epistemic issues that I started with. Recognizing the situated, normative dimension of the epistemic commitments that define archaeology as a science, as a research field, deeply challenges the framework assumptions of the analytic philosophy of science in which I was trained. This is a case in which philosophical questions get substantially reframed when you take actual practice, seriously; I realized I couldn't extract a conventional philosophical puzzle about evidence from the nexus of ethical and political interests that determine why it matters.

I should say that this line of thinking about 'science and values' (as it is sometimes described) didn't take shape in a vacuum. There are now several streams of thought in philosophy of science that reconnect epistemic and normative issues and that have certainly influenced me. These include, for example, contextualist and constructivist thinking about how social values and pragmatic considerations condition research practice, entrenching 'styles of reasoning' (Hacking 1985) that determine what forms of argument or evidence will be seen as compelling, what counts as a well formed question and a salient answer, which assumptions will seem plausible and which require justification, whether simplicity or formalism of a particular kind will be preferred. Feminist philosophies of science, socially naturalized philosophy of science, social epistemology more generally, the turn to pragmatism – these all reinforce the point that the conventional divide between 'contextual' and 'constitutive' values cannot be maintained.[16] But paying close attention to the complicated relationships archaeologists find themselves negotiating with commercial interests (collectors, salvors, dealers), with descendent communities, with a range of other affected publics, and with all the institutions of professional CRM, has made it crystal clear how deeply entangled normative and epistemic issues are with one another.

I'm still working out the implications of all this. Feminist standpoint theory is one place where I've been thinking through a conceptual framework for analysis that takes seriously the role, in science, in systematic empirical inquiry, of social interests and values that are assumed, on traditional accounts, to play an exclusively negative role, as sources of bias that inevitably compromise objectivity. The point is that the integrity, the credibility of empirical inquiry, cannot be a function of its freedom from the influence of what used to be described as 'intrusive' contextual values. Feminist critiques of science, race critical analyses, and a whole range of other cultural and social historical studies of science show that contextual values are ineliminable; there is no 'view from nowhere', to use Nagel's catchy phrase (1986). But what feminists particularly emphasize is that this is not the end of the story. Supposedly biasing, context and standpoint-specific values also play a productive role in inquiry: they are often instrumental in drawing attention to systematic error; they provide the impetus for rethinking entrenched assumptions, for raising the bar methodologically and empirically; and they open up fruitful new lines of inquiry.

The challenge that many feminist theorists have taken up is to give a more realistic account of how supposedly compromising, biasing contextual interests can improve research, reframing ideals of objectivity and epistemic credibility in terms that don't depend on an implausible requirement of strict value neutrality or value freedom. There are clear parallels with the crisis debates in archaeology we talked about at the outset. What's needed here is a nuanced account of what counts as better and worse research practice that refuses the pressure to reduce all epistemic judgment to implausible extremes: absolute truth and disembodied objectivity on one hand, or arbitrary speculation that bottoms out in power dynamics on the other. To get at this through an archaeological lens I have in view a long-term project that would draw all of this work together – a study of evidence stabilizing technologies in archaeology, to use language from science studies.

[16] This is a distinction I draw from early work of Helen Longino's and discuss in a contribution to *Value-Free Science?* (Wylie and Nelson 2007).