

From the Ground Up: Philosophy and Archaeology

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I. Why Philosophy of Archaeology

I'm often asked why, as a philosopher of science, I study archaeology. Philosophy is so abstract and intellectual, and archaeology is such an earth-bound, data-driven enterprise, what could the connection possibly be? This puzzlement takes a number of different forms. In one memorable exchange in the late 1970s when I was visiting Oxford as a graduate student an elderly don, having inquired politely about my research interests, tartly observed that archaeology isn't a science, so I couldn't possibly be writing a dissertation in philosophy of science on archaeology. At job interviews a few years later a standard opening gambit was to ask: "so what's philosophical about the work you do on archaeology" or, more querulously, "what could possibly be of philosophical interest in archaeology"? And from archaeologists, weary of the "theory wars" that raged through 1980s and 1990s, the standard question was, "What's the point?! Better to step away from the wrangling about philosophical 'isms' and get on with the empirical work that needs doing."

More sympathetic questioners typically ask "how possibly" questions: how did I come to work at this odd intersection between fields? This is the question I will address here. Being a Dewey Lecture I take this as an opportunity to reflect, as well, on a major sea change that has taken place in philosophy of science in the forty years since I began thinking about how philosophy and archaeology bear on one another. I see this as an eminently Deweyan shift toward grounding philosophical analysis in a robust, richly contextualized understanding of scientific inquiry. Of course, since at least the early 1970s philosophers of science have been intent on naturalizing our field with the aim of ensuring that our analyses bear some relation to actual scientific practice; we've worked through several iterations of affiliation with history of science and some of us now see sociological science studies as a crucial resource rather than a threat to all things epistemic. But what is distinctive, and distinctively Deweyan, at this juncture is a commitment to take up questions arising *in* and *from* practice, questions that matter to researchers and also policy-makers, funders and, indeed, all those affected by the practice and results of scientific inquiry.

In response to the querulous I would argue that these questions-from-practice are every bit as philosophical, and at least as intellectually challenging, as the highly abstract questions that are still canonical in more rarified philosophical traditions of thinking about scientific knowledge. In my case, this turn to practice has chiefly been a matter of articulating an account of "evidence" in non-foundational terms, an undertaking that has been both provoked and informed by the challenges of archaeological practice. I begin with an account of my own trajectory in answer to the "how possibly" question, and then turn to broader reflection on "why": in what ways can philosophy and archaeology enrich one another? My aim is to bring into focus a reorientation that I think of as "philosophy engaged," evident in different forms and to differing degrees in all our varied fields of philosophical inquiry.

II. Archaeological and Philosophical Roots

I came by my interest in archaeology early on: doing fieldwork was a family affair. My parents were keen avocational archaeologists who, over several summers in the mid-1960s, arranged to join a family friend and colleague of my father's on a series of projects sponsored by the Canadian National Museum,

surveying and excavating contact period Huron and Iroquois sites along the St. Lawrence River. The family friend, Jim Pendergast, was later honored by the Society for American Archaeology with the 1991 Crabtree Award for lifetime achievement as an avocational archaeologist. He is described in the dedication of a festschrift as having “done more than anyone else” to build an understanding of the enigmatic Iroquoian-speaking communities encountered by Jacques Cartier in 1535-36 at Hochelaga and Satadacona (settlements in the vicinity of present day Montreal and Quebec City).¹ They were evidently a force to reckon with, decisively repelling the French after a contentious winter, but when Samuel de Champlain returned some seventy years later they had disappeared.

Debate about what had happened to the St. Lawrence Iroquois continues to the present day. Certainly they would have been hard hit by European diseases and no doubt targeted by their rivals in the fur trade wars given their strategic position on the St. Lawrence. But what was the significance of the distinctive St. Lawrence Iroquoian material culture that archaeologists found on Huron sites of the period? Did they take refuge with their Huron neighbors or were they captives? What exactly was the relationship between these communities? The complexity of the archaeological record defied interpretation in what had become conventional, culturally reifying terms. When he retired from the Canadian military Pendergast went on to serve as an assistant director of the National Museum and worked closely with Bruce Trigger, an eminent archaeologist at McGill. Together they are credited with having reframed the “mystery of the St. Lawrence Iroquois” in terms that recognized a much more dynamic pre-contact history, and much greater social and political complexity than suggested by the records of European intruders. A self-avowed Marxist and thoughtful historian of archaeology, Trigger was also an early and uncompromising critic of the racialized stereotypes that had informed the history and archaeology of contact period interactions between Indigenous people and Europeans. As a graduate student I reviewed a richly philosophical book that he published in 1978, *Time and Traditions: Essays in Archaeological Interpretation*, and I was later deeply influenced by his essay, “Archaeology and the Image of the American Indian.”²

I didn’t discover Philosophy until I took my first college-level classes at Mt. Allison University in New Brunswick in the early 1970s, but I was hooked by the time I finished the first semester of a year-long “Introduction to Philosophy.” As luck would have it, this was taught by Paul Bogaard, a philosopher who had started out as a chemist and was one of the early pioneers in philosophy of chemistry. In that context I realized that, although I’d never been formally taught “philosophy,” I had been drawn to philosophical questions – specifically epistemic questions – in just about every subject I’d studied, helped along by a particularly philosophical mathematics teacher in middle school and some highly intellectual Jesuits in high school.

The first time I made the connection between philosophy and archaeology was not in a philosophy classroom, however; it was when I took a summer job on an archaeological field crew that the Canadian National Historic Sites division (NHS) was dispatching to Fort Walsh, Saskatchewan. This was a Northwest Mounted Police fort, established in 1875 in the heart of the Cypress Hills, an uplifted plateau roughly 20 square miles in area that overlaps what became the provincial border between Saskatchewan and Alberta in 1905, just above Montana which had been incorporated as a U.S. territory in 1864. Crucially, the Cypress Hills were within comparatively easy striking distance of Fort Benton, the highest point on the Missouri River that was navigable for steamboats. By the early 1870s Fort Benton was well established as the nexus of an enormously wide-reaching trade network. It was a major center for the transshipment of furs, especially buffalo hides, from the Canadian Northwest Territories to the tanning factories and trading houses of St. Louis. And in return, goods as diverse as marmalade from Great Britain and Florida, bitters of various kinds from New York, Toronto and Philadelphia, cognac from France, lager beer from St. Louis and Milwaukee, and port from California made their way to Fort Walsh through Fort Benton, and from there into the Northwest Territories by way of the infamous “Whoop-up Trail.”³

The Cypress Hills are stunningly beautiful, described by the Palliser expedition as a “veritable oasis in the tract of arid country” they had travelled through in 1859.⁴ Deeply cut stream beds are home to striking stands of wolf willow and lodgepole pine, and would have been teeming with game in the 19th century: beaver and bear; fox, wolves and lynx; elk and deer. Exposed benches offer a panoramic view of the

prairie, a perfect vantage point from which to track the movements of the great northern buffalo herds until they were hunted to the point of extinction in service of the voracious fur trade by the early 1880s.

The official mandate of the NWMP was to “bring law and order to the Canadian West”; they were to staunch the “invasion of the Canadian prairies by a horde of American whisky traders who callously peddled their rotgut product to the local Indians.”⁵ In the process they were to settle the “Indian situation” on the outer edge of what the newly founded Canadian federation saw as its western frontier, a crucial step toward opening the region to European settlement. The North-West Territories Act, passed in 1875, made it illegal to “import, sell, trade or produce intoxicants of any kind except under permit,” a system designed to staunch the use of alcohol in the trade for buffalo hides. The Cypress Hills was an obvious location for a NWMP fort with a mandate to implement the Act given the number of independent trading posts operating in the Hills in that period. It was also the place where an infamous massacre had taken place in 1873 when wolfers and whiskey traders attacked an Assiniboine (Nakoda) party camped near two of these posts. As emblematic of the lawlessness of “Whoop-up Country,” the Cypress Hills Massacre drove home the need for the fledgling Canadian government to make good on its commitment to maintain a “peaceful frontier under British law” and, more pointedly, to “avoid a repetition of the conflict witnessed between Indians and settlers in the American west.”⁶

As the buffalo herds met their demise, destroying the Indigenous subsistence base and the frontier trade economy of the region, and as Native Americans were driven from their homelands by military action to the south – Sitting Bull (Tatanka Iyotanka) and some 5000 Lakota Sioux decamped to the Cypress Hills after their confrontation with Custer at Little Big Horn – the NWMP force stationed at Fort Walsh was increasingly at the center of settler state territorial negotiations. A number of treaties were negotiated at Fort Walsh with First Nations communities located in the Canadian territories, and the Sioux were persuaded to return to the U.S. In the meantime, rations were distributed, at times to thousands of starving Native Americans camped in the Hills around Fort Walsh.⁷ The NWMP were tasked with keeping the peace between them, the Métis, and the growing population of ranchers and settlers moving into the region from eastern Canada and the U.S. By 1883 the Canadian Pacific Railway line had reached Maple Creek, thirty miles north of the Cypress Hills, and the Fort and the townsite that had grown up next to it were abandoned.

Those who made a go of it in the region established large scale ranching operations. The Fort and massacre site were initially on the property of the Wood and Anderson ranch, established shortly after the Fort was abandoned. It was later owned by the Nuttall family from whom the RCMP purchased what became Fort Walsh National Historic Park in 1942. They built a rough replica of the fort based on historic photos and ran a horse breeding “remount” station on the site until the late 1960s when it was designated a National Historic Site. By the time I first visited the site in 1973, the Solmon and Farwell trading posts adjacent to the Cypress Hills Massacre site had also been reconstructed, and the whole park complex was open to a modest stream of visitors.

The mandate given the archaeological crews that worked at Fort Walsh between 1973 and 1978 was to “uncover the precise configuration of the original Fort buildings,” to determine exactly what materials had been used in their construction, and “recover as many artifacts as possible.”⁸ This was “historicalist” archaeology of a conventional kind that was pointedly not aimed at an appraisal of received archival history and popular legend where the Fort and the NWMP were concerned, much less at understanding the broader historical and cultural context of the Fort. We faced stiff resistance from head office in Ottawa when we undertook a survey and sampling project on the civilian townsite next to the Fort in the first year of the project, and there was strikingly little interest in documenting any of the Native American or Métis sites that surrounded the Fort. In fact, there proved to be a lot to be learned archaeologically that has since gotten uptake from Parks Canada.

On the matter of enforcing the NW Territories Act, we found an enormous wealth of alcohol bottles, including a great many high-proof patent medicine bottles, squirreled away in footing trenches and under the sub-floors of the enlisted men’s and officer’s barracks. The impression of widespread consumption of “intoxicants” gleaned initially from the excavation of structural features was reinforced when I later spent a summer excavating latrines. It soon became clear that drinking preferences were rank-differentiated: we

found an impressive number of beer, whiskey and medicine bottles in the latrines associated with the lower-ranked barracks, while the bottles in the officers' and Commissioner's latrines were primarily wine and champagne.⁹

Although our work on the civilian townsite was cursory, we turned up fascinating evidence of substantial houses and business establishments, in several cases with assemblages indicative of family households. Later salvage projects expanded this inventory and a recent M.A. thesis provides a close analysis of the range and distribution of this material which included children's toys; sherds of patterned china, fragments of a carafe, flatware, and a coffee mill; window pane glass, ceramic door knobs, a lock and key; painted buttons and amber beads; a perfume bottle stopper; bits of musical instruments and a book binding.¹⁰ As an assemblage this suggests a much different picture of life at a key transit point on the notorious "Whoop-up Trail" than we had expected.

Finally, in my second year of graduate school, I proposed an archaeological survey of the Park as a whole that I directed in 1978, at a point when the fort excavations were largely completed. One striking take-home from that project was the sheer density of the lithic scatters and clusters of tepee rings we found concentrated along the edges of every exposed bench terrace and at the head of every coulee that sighted onto the Fort. Exposed and often at some distance from a water source, these were not ideal campsites except that they were located exactly where displaced Native Americans could keep an eye on the NWMP. We also documented half a dozen clusters of pit features along historic trails that were likely the remains of Métis settlements, some no doubt pre-dating the Fort by several decades. And we reported some tantalizing evidence of a much deeper Native American history in the Cypress Hills, for example, an anomalous set of double tepee rings located on a protected hillside well away from the Fort and in a tight circular formation that fit accounts of prehistoric and protohistoric wintering sites in the Cypress Hills. We recommended much more systematic attention to these features in future Park research and management.

Unfortunately, by the time I was building this inventory of cultural heritage sites and features a visitor reception center had been constructed in the middle of one of the densest Native American encampment sites above the fort, and several historic period trails and Metis campsites had been transected by utility roads. In recent years there has been a shift away from this inattention to anything but a (selective) history of the NWMP. A 2013 Parks Canada report puts Native American and Métis history at the center of the "Management Plan" for Fort Walsh, emphasizing a commitment to "work cooperatively" with "representatives from the Carry the Kettle First Nation....descendants of those who suffered the Massacre."¹¹

lii. A Philosophy-Archaeology Trading Zone

As I soon learned in the first summer I worked at Fort Walsh (1973), North American anthropological archaeology was in the throes of a revolution, as described by advocates of "processual" New Archaeology. Self-identified as pro-science positivists, the New Archaeologists insisted that archaeology must move beyond merely collecting and systematizing data, and also beyond the "culture history" reconstruction of particular events and conditions of life. To be properly scientific the goal must be to explain these particulars in terms of long-term, large-scale cultural processes. As described in an influential review of early programmatic statements, the "process theorist is not ultimately concerned with the 'Indian behind the artifact' but rather with the system behind both the Indian and the artefact."¹²

The hallmark of the New Archaeology was an insistence that these ambitious goals required archaeologists to make a sharp break with the inductive, speculative modes of practice they attributed, somewhat unfairly, to "traditional" archaeology. Rather than first gathering archaeological data and then generating a "just so" story about what it might mean after the fact, New Archaeologists argued that reconstructive and explanatory hypotheses must be the starting point for inquiry; they advocated a rigorously problem-oriented, hypothesis-testing mode of practice designed, ultimately, to establish laws of cultural process. In this spirit they also insisted that archaeologists must abandon the Quixotic quest for a humanistic understanding of the cultural lifeworld of past cultures; they should embrace an "eco-

materialist” conception of culture as “Man’s extra-somatic means of adaptation” and focus their attention on human:environment interactions at the level of cultural systems.¹³

In fleshing out these recommendations, the New Archaeologists drew heavily on “received view” logical positivist and logical empiricist philosophy of science, specifically Hempel’s account of covering-law models of explanation and his hypothetico-deductive account of confirmation.¹⁴ Ironically, because their aim was to foment revolution, they also invoked Kuhn’s *Structure of Scientific Revolutions*; instituting positivist-inspired modes of inquiry meant decisively rejecting the entrenched “paradigmatic thinking” of traditional archaeology.

I learned about this intellectual ferment when I was sent a list of readings to slog through in preparation for the 1973 Fort Walsh field season. It included not only NWMP history and regional archaeology, but what I later came to appreciate were classic statements of the New Archaeology agenda by Lewis Binford, its dominant advocate, and proponents of processual approaches in historical archaeology; the review I cited earlier, and several explicitly philosophical analyses of the nature of archaeological explanation.¹⁵ It also included a few representative articles by “received view” philosophers of science, chiefly Hempel.

The director of the Fort Walsh archaeological project was James V. Sciscenti, an ardent New Archaeologist who strained mightily against the constraints of the NHS bureaucracy. He had been trained at the University of Arizona, a stronghold of the New Archaeology at the time, and his approach to fieldwork reflected this. Here’s how I described it in the preface to *Thinking from Things*:

What made archaeology worth doing, in [Sciscenti’s] view, was not just the intrinsic interest of the enterprise – the wholly absorbing process of recovering tangible evidence of past human actions, aspirations and accomplishments – but what it could teach us about the conditions of life, the reasons for cultural change and persistence, affinity and diversity, that manifested themselves in the gritty particulars of the archaeological record. For him, as for many others at the time, “archaeology was anthropology or it was nothing.”¹⁶

Sciscenti insisted that one should never put trowel or spade to ground without clearly formulating a research question and a strategy for addressing it. We debated not just what questions to ask but what counts as a research question, and the fine points of how best to implement a systematic program of testing the working hypotheses we generated as answers to these questions. We worried about the enigmatic nature of the archaeological record. Even when we could put a name and date to the whisky, wine and patent medicine bottles we were digging up we were acutely aware that we didn’t necessarily understand their significance as evidence: How had they come into the hands of the NWMP? Were they contraband, or were some, at least, legally acquired by permit? If legal, as seemed likely in the case of the officers’ and commandant’s wine and champagne, why were they dumped in latrines or buried in footing trenches and under floor-boards? Crucially, we wrestled with the implications of finding, disconcertingly often, that an unforgiving and unobliging archaeological record could not be counted on to conform to our best hypotheses, even (indeed, especially) those based on seemingly incontrovertible documentary history.

We also wrestled with ethical, political issues. It was frustration with the Euro- and military-centrism of the NHS archaeological program that motivated me to propose a walkover survey of the park property. And it was the experience of listening to running debate about what we should do if our excavations exposed human remains that brought home to me the ways in which we were complicit in the continuing legacy of displacement, appropriation, and denial enacted, in the form of colonial diplomacy, at Fort Walsh in the 1870s and 1880s. Current wisdom at the time was, hope that any human remains turn up with clear evidence of Native American identity, in which case they could be disinterred and sent to headquarters as a “find.” Otherwise you would be tied up in interminable “white tape” negotiating with the county coroner and chasing down possible descendants to consult about their disposition. These stark inequities in treatment were soon to become a prominent focus of the Native American activism that resulted in such game-changing legislation as NAGPRA, the Native American Grave Protection and Repatriation Act that was passed into law in the U.S. in 1990.

When I returned to Mt. Allison University for my sophomore year I took Paul Bogaard's "Introduction to History and Philosophy of Science," and read Hempel and Kuhn again, this time in the company of N. R. Hanson, Israel Scheffler, and Karl Popper, and among others who had challenged the orthodoxy of "received view" logical positivism.¹⁷ Bogaard encouraged us to write term papers on philosophical issues raised by actual scientific practice, historical or contemporary, so I wrote on the question, "Was archaeology undergoing a Kuhnian revolution?" (My answer was: it depends on what you count as a revolution, and whether you think the assumptions and practices of "traditional" archaeology constitute a paradigm.) This raised exactly the questions about Kuhn's account that were drawing critical attention from historians and philosophers of science at the time, and it proved to be an enormously fruitful point of engagement with debates about the New Archaeology that took shape in the next decade.¹⁸

In the process I discovered that several philosophers of science were already actively engaging the philosophical issues raised by the New Archaeology. A few were scathing critics of what they saw as ill-conceived and uninformed appropriations of the philosophical literature. Chief among these was Chuck Morgan, at the University of Victoria, who wrote a trashing review of the use New Archaeologists had made of "received view" accounts of explanation to define their research agenda, decrying their failure to grasp the niceties of philosophical debate about covering-law models.¹⁹ While he was right about many specifics of this failed attempt at "cross-field trade" as he described it, I had considerable sympathy for the archaeologists he eviscerated in classic analytic style when they objected that, for his part, Morgan had not bothered to learn enough about the problems that had led them to the philosophical literature to be any help in addressing them constructively. I took this exchange to be an instructive negative object lesson in how to alienate philosophically reflective practitioners and reinforce disciplinary boundaries, shutting professional philosophy off in its own little possible world.

By contrast, others had rolled up their sleeves and gone to work in ways that illustrated what might be learned by all parties if trading relations were predicated on a serious effort to understand what one another's terms of art mean in context and what the stakes are in using them. The earliest and most influential was Merrilee Salmon who had published several short articles in *American Antiquity* in the 1970s disambiguating key philosophical terms at issue in archaeological debates about explanation and hypothesis testing.²⁰ In 1982 she published *Philosophy and Archaeology*,²¹ the culmination of an ongoing exchange with the tight-knit group of New Archaeologists at the University of Arizona who were experimenting with processual approaches in the U.S. Southwest, the very ones who had had trained Sciscenti. Later, as a post-doctoral fellow at the University of Calgary, I worked with Marsha Hanen, a Harvard-trained philosopher of science and law, and Jane Kelley, a meso-Americanist known most recently for her fieldwork in Chihuahua, Mexico. Together they published *Archaeology and the Methodology of Science*, a sustained argument for understanding archaeological theorizing as a form of inference to the best explanation.²² A Mellon fellowship at Washington University in St. Louis gave me an opportunity to work with Patty-Jo and Red Watson; Patty-Jo, the lead author of *Explanation in Archaeology*, the primary target of Morgan's critique, is famous in archaeology circles for her work on the origins of agriculture both in the Near East (Turkey) and in Eastern North America, and Red is a Descartes scholar who also wrote extensively on philosophical issues in archaeology.²³ Although still very much a marginal subfield in philosophy of science I was by no means alone in finding philosophical interest in archaeology.

Returning to the "how-possibly" question – specifically, how did I manage to parlay these parallel interests in archaeological fieldwork and philosophy of science into a career in philosophy – I had the good fortune to learn about an interdisciplinary program in the History and Philosophy of the Social and Behavioral Sciences (HPSBS) that had been established by Ted Mischel at SUNY-Binghamton just when I was ready to apply to graduate schools. It was tailor-made for my purposes. It required at least a Master's level of training in a social science and supported those of us who came directly from undergraduate programs in getting this training at Binghamton, alongside our Philosophy coursework. As a one-of-a-kind experiment it attracted an extraordinary roster of visiting scholars: D. W. Hamlyn and J. J. Gibson gave lectures on perception; Dick Boyd came across from Cornell to offer a seminar in scientific realism; and Rom Harré, who was then developing his critique of positivism in social psychology and later supervised my dissertation, visited regularly from Oxford. It also gave me the opportunity to work with a cluster of

energetic “second generation” New Archaeologists, including Chuck Redman and Meg Conkey, who also served on my dissertation committee. Tragically, Mischel died in December 1976, the year I entered the program, and HPSBS soon lost its funding. I did get support to complete my degree at Binghamton, and to spend two semesters out of residence: one at the University of Arizona where I took courses with Merrillie and Wes Salmon, and a second at Oxford doing tutorials with Rom Harré as I transitioned to dissertation research.

In a dissertation on “positivism and the new archaeology” I explored what I saw as a fundamental disconnect between the positivist rhetoric of the New Archaeology and what seemed most innovative about this program in practice. The oft-stated goal of ardent processualists was to move beyond description of the “observables” of the archaeological record; it was to understand long-term, large-scale cultural processes, not only, or primarily, to “save the phenomena.” I argued that, in practice, their strategies of inquiry were better characterized as sophisticated practices of model-building; there were no laws in sight, at least none that met the description of the high-level nomological laws of cultural process that were to do the explanatory work the New Archaeologists had set their sights on as the goal of a properly scientific archaeology.

By the time I defended my dissertation in 1981 this internal “fault line,” as I described it, was clearly evident. Sympathetic internal critics had raised virtually all the central points of objection that had brought down “received view” logical positivism in philosophical contexts. In particular, they recognized that the “deductivism” of their H-D-inspired testing program was untenable, citing what amounted to concerns about under-determination and the theory-ladenness of archaeological data as evidence. A few years later a much more sharply adversarial critique had taken shape, led by a philosophically savvy young cohort of archaeologists at Cambridge who I’d met when studying with Harré. These advocates of “post-processualism” were deeply skeptical about expansive cultural process generalizations and were committed to reclaiming the humanistic goals of archaeology. Not surprisingly, they rejected outright the brashly confident scientism of the New Archaeology. Drawing on post-Kuhn contextualist philosophy of science, Strong Programme science studies, and a range of Continental sources, some (briefly) pushed to the limit an uncompromising anti-foundationalism, arguing that there is no escape from vicious circularity. The data that comprise the archaeological record are meaningless in themselves, requiring such substantial interpretive scaffolding to stand as evidence that archaeologists’ expectations necessarily determine what they will recognize as evidence relevant to adjudicating their claims about the past. Some explicitly, if briefly, embraced a frank ethnocentrism, as Rorty might describe it: archaeologists have no choice but to candidly acknowledge that they cannot avoid projecting a politically freighted, presentist image of the past onto enigmatic archaeological data.²⁴

These, then, are the theory wars I mentioned at the outset: a sharply oppositional debate that ran hot through the 1990s, and has since gone underground. As one commentator describes it, the “angry claims and counter-claims about relativism and its dangers ha[ve] largely subsided,” but not because the underlying issues have been resolved.²⁵ An unreflective ‘live and let live’ pluralism exempts a great many untenable assumptions from reasoned examination. I note that this dynamic of debate is by no means unique to archaeology; it is evident across the social sciences wherever positivist research programs have flourished, and in the high profile “Science Wars” debates of the 1990s that have been revived by the current rhetoric of “alternate facts.”

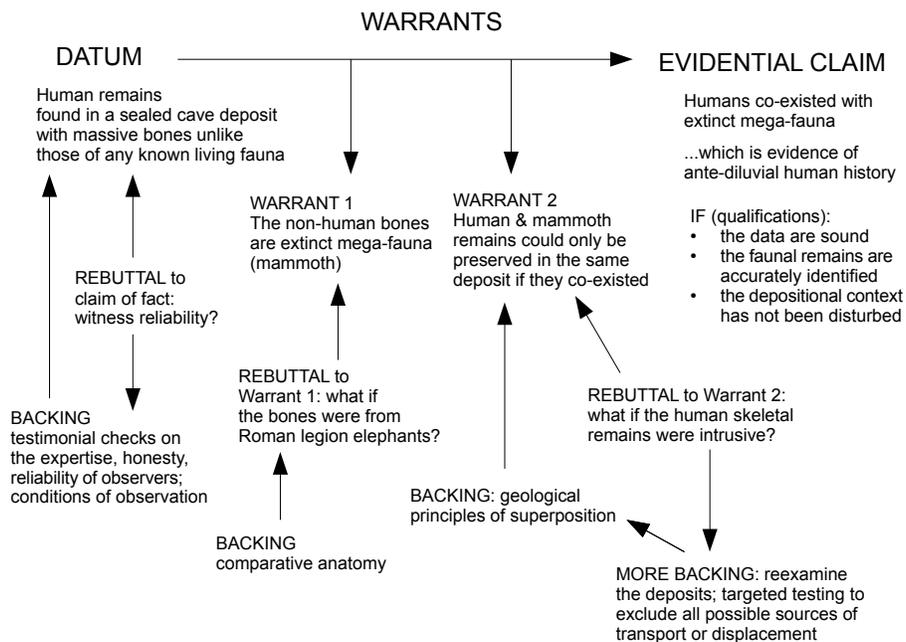
IV. Theorizing Evidential Reasoning

Although I was sympathetic to the critiques leveled against the New Archaeology and its embrace of “received view” philosophy of science by post-processualists, I was not at all compelled by their insistence that, if the positivist ambitions are untenable, wholesale skepticism and relativism is inescapable. I published a series of essays in philosophical as well as archaeological contexts that were designed to show that such conclusions do not, in fact, follow from arguments for recognizing that evidence is inevitably an interpretive construct.²⁶ When evidential reasoning-in-practice is examined at a lower altitude than generic theses of theory-ladenness it is clear that, however heavily selected and interpreted archaeological data are as evidence, they do have – as I learned first hand at Fort Walsh –

considerable capacity to “resist theoretical appropriation.”²⁷ This capacity arises from a complex network of conceptual and empirical constraints that is all too often obscured by the simplifying abstractions of philosophical models. The philosophical challenge, as I see it, is to tease out operating norms and strategies of evidential reasoning that are rarely made explicit, except under duress, and articulate their epistemic rationale. Here is a sketch of the model of evidential reasoning I’ve developed over the years, published in its most complete form last fall in a book I co-authored with archaeologist Bob Chapman, the outcome of a Leverhulme Visiting Professorship I held at Reading University in 2010.²⁸

The point of departure for this account is a recognition that in archaeology, as in many other fields, most of the inferential action in evidential reasoning is off-stage. The key to building evidential claims is to establish a robust body of background knowledge that links surviving material traces to antecedent conditions or events that produced it, the putative “phenomena” of which it is evidence, and to the hypotheses or models on which it bears as evidence. When you attend to the details it becomes clear that much of the laden theory on which archaeologists rely is better understood, not as arbitrary “interpretive conventions” of the kind vilified by ardent post-processualists, but as material postulates in John Norton’s sense: they comprise a body of substantive, domain-specific knowledge which functions as warrants in a Toulmin-like schema of practical reasoning.²⁹ Crucially, this background knowledge is itself subject to conceptual and empirical appraisal and, if you think of evidential reasoning in archaeology as a form of practical argument, in the sense proposed by Toulmin in the 1950s, it is clear that a great many different kinds of warrants are required to traverse the inferential gap between what he refers to as the “data” and the claims they support, in this case, evidential claims.

To illustrate, consider a Toulmin-style schematic representation of a subset of the arguments, warrants, rebuttals and backing that played a role in a famous historical example of debate about the standing of archaeological evidence: the “Red Lady of Paviland.” The anchoring “datum” in this case was a well preserved set of human remains discovered in 1823 by William Buckland in association with mammoth bones in a sealed cave deposit in southern Wales. They were taken to be pivotal evidence of an ante-deluvial human past, a claim that was sharply contested by defenders of a biblical conception of human antiquity, including Buckland himself. A meticulous geologist and paleontologist, Buckland scrutinized every aspect of the arguments supporting these evidential claims.³⁰



A Toulmin schema representation of arguments for and against claims about human antiquity.³¹

I use “rebuttal” in a broader sense than Toulmin does, to refer to challenges to any element of the argument, including the integrity of the data cited and the inference-mediating warrants which, together, establish the plausibility and determine the scope of the evidential claims they support. Even on this simplified schema it is clear that arguments establishing evidence of human antiquity depend on a great many moving parts, each of which can be and were contested. Nonetheless, over the course of a century, rigorous scrutiny of the anchoring data and the warrants for these arguments, much of it made possible by pivotal developments in a diverse range of other fields, provided the empirical and conceptual resources to answer the rebuttals brought by Buckland and his successors. As this case suggests, archaeological data are “laden” by many different kinds of “theory” in the process of being selected and interpreted as evidence; these inference-mediating theories can be assessed for the degree of security they carry in their own right, they are by no means all and equally insecure; and they do not necessarily collude in supporting the presuppositions that inform inquiry. When, as warrants, they are underwritten by substantially different kinds of background knowledge there is no reason to assume they are necessarily, as a matter of principle, bound into a hyper-coherent web of belief that could ensure that the evidential claims they support will conform to expectation. In short, pernicious circularity is not inevitable or pervasive.

I argue more specifically that these conditions of practice put archaeologists in a position to exploit epistemic independence on two dimensions.³² The first is vertical independence. This is bootstrapping in a generic sense where the warrants you rely on to establish evidential claims are not constituents of the hypotheses or theories you use these claims to support or to adjudicate. For example, the claims about human antiquity that the evidence from Paviland ultimately supported do not presuppose or entail the geological principles of superposition that establish the association of human with mammoth remains. The second is horizontal independence. This is the capacity of multiple lines of evidence to constrain one another when mediated by warrants that come from different domains and are backed by conceptually and methodologically distinct research programs. For example, the geological principles that establish the contemporaneity of the human and non-human remains are causally and conceptually independent from the faunal analysis that warrants an identification of the animal bones as extinct mammoth. This is robustness reasoning of a work-a-day variety.

Taken together, these strategies of evidential reasoning in archaeology exemplify what Hasok Chang describes as a process of “epistemic iteration” by which scientists in a wide range of fields “build scientific knowledge in the absence of infallible foundations.”³³ On this way of thinking evidential claims are stabilized, not by appeal to an idealized empirical bedrock, but by dense tangles of empirical and conceptual argument that are mutually reinforcing and mutually constraining.³⁴ What we need philosophically, I argue, is an account of how to distinguish in a principled way between virtuous and vicious tangles.

IV. Contextualism Extended

While my work on evidential reasoning is deeply informed by my field experience at Fort Walsh and on half a dozen other projects on which I worked subsequently, it is through a different kind of engagement with archaeology that my practice-learnings have taken a more robustly contextualist turn. In the late 1980s I participated a workshop and then a public conference that were pivotal in establishing what is now a thriving program of gender research in archaeology.³⁵ At the time I was also involved in feminist activism – the “chilly climate” initiative at the University of Western Ontario – and I was collaborating with Kathleen Okruhlik on a SSHRCC-funded project cataloguing the diverse array of feminist critiques of science that were appearing across the social and life sciences, and in the cognate fields of science studies.³⁶ A few years later I was enlisted by the executive board of the Society for American Archaeology (SAA) to co-convene a task force that ultimately drafted the “Principles for Archaeological Ethics,” adopted by the SAA in 1996.³⁷ It was in this connection that I learned first hand about the work of a growing constituency of archaeologists whose response to demands for repatriation had been to build collaborative partnerships with Indigenous descent communities around the world, on the model of well established traditions of community-based participatory research (CBPR) in a range of other fields.³⁸

In both cases I was shocked that, even as these initiatives got traction, proving themselves not only viable but in many cases highly productive, those advocating them faced profoundly hostile critiques from their colleagues. They were charged with unprofessional disregard for the goals and standards of archaeology; their work was dismissed as “just political” and condemned as a threat to the integrity and credibility of archaeology as a discipline. By contrast to these assessments I saw some excellent archaeology being done measured by any standards, some of it highly innovative. Feminist and CBPR practitioners alike focused attention on norms of “best practice” and framework assumptions that had not been questioned, offering well-grounded critiques that opened up an exciting range of questions not previously explored. In both cases it struck me that what had made it possible to disembled these taken-for-granted – to hold them accountable and think outside the box – was precisely the situated, in many cases explicitly political angle of vision that gender researchers brought to bear as women and feminists, and that Indigenous partners brought to bear as critically engaged stakeholders.

Thinking as a philosopher, this was another case in which the dynamics of actual practice raised pointed questions that mainstream philosophical models of science are ill-equipped to answer. How were these contributions realized in what seemed direct contravention of “received view” philosophical wisdom that requires properly scientific inquiry to be “value free,” rigorously insulated against the influence of situated interests? Most poignant in these cases is the role played by these very philosophical models when claimed as a source of legitimation by those who categorically reject the insights of dissident insider-outsiders. By no means do feminist and CBPR research programs, in archaeology and elsewhere, exemplify or support a corrosive anything-goes relativism; if anything, they often raise the bar epistemically in the fields where they have taken shape. In the last decade my focus has been on articulating an account of “situated objectivity” that has the resources to make sense of the insights and innovations arising from feminist and collaborative research programs across the social and life sciences.

V. To Conclude

In answer to the questions with which I opened, I would argue that archaeology, and indeed any serious program of empirical inquiry, is always already philosophical. Practitioners grapple with epistemic issues in their everyday practice, and the way they navigate these issues suggests a very different conception of the “scientific image” than has, until recently, informed mainstream philosophy of science. Applying feminist standpoint theory reflexively, I suggest that philosophy is much enriched by ongoing, intentional engagement with practitioners and with research practice. We often do our best work when we operate as a trading zone, expanding our horizons rather than policing ever-narrowing boundaries that divide us from the fields we analyze. This requires amphibious training and practice of a kind described, in the late 1970s by Mario Bunge when he insisted that philosophy of science must be informed by the kind of understanding that comes from “work in – not just the study of – some science,” alongside “parallel training in philosophy.”³⁹ This was a vision of philosophy of science that I found enormously inspiring when I came upon it as a graduate student. I wasn’t surprised to learn, years later, that it had made an impression on Bruce Trigger as well; he and Bunge were long-time colleagues and sparring partners at McGill.

Notes

¹ James V. Wright, “James F. Pendergast: Blurring the Amateur-Professional Dichotomy,” in *A Passion for the Past: Papers in Honour of James F. Pendergast*, ed. by James V. Wright and Jean-Luc Pilon (Gatineau, Quebec: Canadian Museum of Civilization, 2004), 1-5.

² Bruce G. Trigger, *Time and Traditions: Essays in Archaeological Interpretation* (New York: Columbia University Press, 1978); “Archaeology and the Image of the American Indian,” *American Antiquity* 45 (1980): 662-76.

³ A map of supply routes is included in a Parks Canada report, “Archaeology: The Mounties as Pioneers,” (Ottawa: Minister of the Environment, Minister of Supply and Services Canada, 1981), 2-3. See also Margaret A. Kennedy, *The Whiskey Trade of the Northwestern Plains* (New York: Peter Lang, 1997).

- ⁴ As quoted in Kimberly Aaron Wutzke, "Fort Walsh Townsite (1875-1883): Early Settlement in the Cypress Hills" (M.A. thesis, University of Saskatchewan, 2009), 17.
- ⁵ Karlis Karklins, "The Fort Walsh Townsite (8N): Feature Synthesis and Artifact Inventory" (Ottawa: National Historic Parks and Sites, 1987), 1-2.
- ⁶ Wutzke *op. cit.*, 24.
- ⁷ James V. Sciscenti, A. Campbell, B. Hromadiuk, S. MacLeod, J. S. Murray, and A. Wylie, "Archaeological Investigations at a Late Nineteenth-Century Northwest Mounted Police Post, Fort Walsh, Saskatchewan, 1973-74 Field Seasons" (Ottawa: National Historic Parks and Sites, 1976), 2.
- ⁸ *Regina Leader-Post*, July 1974.
- ⁹ Kevin Lunn, "A Study of Glass Bottles from a Late Nineteenth Century North-West Mounted Police Post" (Ottawa: Parks Canada, 1979). "Bottles Tell the Story" in Parks Canada, *op. cit.*, 1-4.
- ¹⁰ Wutzke *op. cit.*, 125-189. Olga Klimko, et. al, "Fort Walsh Townsite: 1992 Salvage and Assessment" (Saskatoon: Western Heritage Services, 1992).
- ¹¹ Parks Canada, "Fort Walsh and Cypress Hills Massacre National Historic Sites of Canada: Management Plan" (Ottawa, 2013), iv.
- ¹² Kent V. Flannery, "Cultural History Versus Cultural Process: A Debate in American Archaeology." *Scientific American* 217.2 (1967), 120.
- ¹³ Lewis R. Binford, "Archeology as Anthropology," *American Antiquity*, 28 (1962), 218.
- ¹⁴ I use "received view" as introduced by Frederick Suppe in his introduction to *The Structure of Scientific Theories* (Urbana IL: University of Illinois Press, 1977). Carl G. Hempel, "The Function of General Laws in History," *Journal of Philosophy* 39 (1942): 35-48; *Aspects of Scientific Explanation* (New York: Free Press, 1965).
- ¹⁵ Binford, *op. cit.* James F. Deetz, and Edwin S. Dethlefsen, "Death's Head, Cherub, Urn and Willow," *Natural History* 76 (1967): 29-37. John M. Fritz, and Fred T. Plog, "The Nature of Archaeological Explanation," *American Antiquity*, 35 (1970): 405-12. Patty Jo Watson, Steven A. LeBlanc, and Charles L. Redman, *Explanation in Archaeology: An Explicitly Scientific Approach* (New York: Columbia University Press, 1971).
- ¹⁶ Alison Wylie, *Thinking from Things* (Berkeley, California: University of California Press, 2002), ix.
- ¹⁷ N. R. Hanson, *Patterns of Discovery* (Cambridge: Cambridge University Press, 1958). Karl R. Popper, *Conjectures and Refutations*: (London: Routledge, 1963). Israel Scheffler, *The Anatomy of Inquiry* (New York: Bobbs-Merrill, 1963).
- ¹⁸ Wylie, *op. cit.*, "How New is the New Archaeology?," *Thinking From Things*, 25-41.
- ¹⁹ Charles G. Morgan, "Archaeology and Explanation," *World Archaeology* 5 (1973): 259-76. Watson, LeBlanc, Redman, "The Covering Law Model in Archaeology: Practical Uses and Formal Interpretations," *World Archaeology* 6 (1974): 125-32.
- ²⁰ Merrilee H. Salmon, "Confirmation and Explanation in Archaeology" *American Antiquity* 40 (1975): 459-64; "'Deductive' vs 'Inductive' Archaeology" *American Antiquity* 41 (1976): 376-80.
- ²¹ Merrilee H. Salmon, *Philosophy and Archaeology* (New York: Academic Press, 1982).
- ²² Jane Kelley and Marsha P. Hanen, *Archaeology and the Methodology of Science* (Albuquerque, New Mexico: University of New Mexico Press, 1988).
- ²³ Richard A. Watson, "The "New Archeology" of the 1960's," *Antiquity*, 46 (1972): 210-15. "Ozymandias, King of Kings: Postprocessual Radical Archaeology as Critique," *American Antiquity* 41 (1990): 210-15.
- ²⁴ Ian Hodder, "Archaeology, Ideology and Contemporary Society," *Royal Anthropological Institute News* 56 (1983): 6-7.
- ²⁵ Matthew Johnson, *Archaeological Theory* (Oxford: Wiley-Blackwell, 2010), 223.
- ²⁶ These essays appear in Wylie, *op. cit.*, Part Four, "On Being Empirical but not 'Narrowly Empiricist'."
- ²⁷ Michael Shanks, and Christopher Tilley, "Archaeology into the 1990s: Questions Rather Than Answers," *Norwegian Archaeological Review*, 22 (1989): 44.
- ²⁸ Robert Chapman, and Alison Wylie, *Evidential Reasoning in Archaeology* (Bloomsbury, 2016).
- ²⁹ John Norton, "A Material Theory of Induction," *Philosophy of Science* 70 (2003): 647-70. Stephen E. Toulmin, *The Uses of Argument* (Cambridge: Cambridge University Press, 1958).
- ³⁰ Marianne Sommer, *Bones and Ochre: The Curious Afterlife of the Red Lady of Paviland* (Cambridge, MA: Harvard University Press, 2007).
- ³¹ Chapman and Wylie, *op. cit.*, 35.

³² Alison Wylie, "Critical Distance: Stabilising Evidential Claims in Archaeology," in *Evidence, Inference and Enquiry*, eds. Philip Dawid, William Twining and Mimi Vasiliaki (London: Oxford University Press, 2011): pp. 371-94.

³³ Hasok Chang, *Inventing Temperature* (Oxford: Oxford University Press, 2004), 231.

³⁴ Nancy Cartwright, "The Philosophy of Social Technology: Get on Board," *Proceedings and Addresses of the American Philosophical Association* 89 (2015), 101-3. John D. Norton, "A Material Dissolution of the Problem of Induction" (Center for Philosophy of Science, University of Pittsburgh: 2013) 17; accessed July 2017, http://www.pitt.edu/~jdnorton/papers/Induction_problem.pdf

³⁵ Alison Wylie, "What Knowers Know Well: Standpoint Theory and the Formation of Archaeology," *Scientiae Studia* 15 (2017): 13-38.

³⁶ Chilly Collective, *Breaking Anonymity: The Chilly Climate for Women Faculty* (Waterloo: Wilfrid Laurier University Press, 1995). Alison Wylie, Kathleen Okruhlik, Leslie Thielen-Wilson, and Sandra Morton, "Feminist Critiques of Science: The Epistemological and Methodological Literature," *Women's Studies International Forum* 12 (1989): 379-88.

³⁷ Mark J. Lynott, and Alison Wylie, eds., *Ethics in American Archaeology: Challenges for the 1990s*, SAA *Special Report Series* (Washington DC: Society for American Archaeology, 1995).

³⁸ See, for example, the community initiatives supported by IPinCH, a collaborative research program on Intellectual Property Issues in Cultural Heritage; accessed July 2017, <https://www.sfu.ca/ipinch/project-components/community-based-initiatives/>

³⁹ Mario Bunge, *Method, Model, and Matter* (Boston: D. Reidel, 1973), 19-20.