

WORD OF WISDOM

Philosophy from the Ground Up: An Interview with Alison Wylie

Introduction

Alison Wylie is one of the few full-time academic philosophers of the social and historical sciences on the planet today. And fortunately for us, she happens to specialise in archaeology! After emerging onto the archaeological theory scene in the mid-1980s with her work on analogy, she has continued to work on philosophical questions raised by archaeological practice. In particular, she explores the status of evidence and ideals of objectivity in contemporary archaeology: how do we think we know about the past? Her other key interests include feminist initiatives in Anglo-American archaeology, and ethical conflicts in current archaeological practice. [Kathryn Denning](#) recently asked about her adventures in archaeology and academia and her thoughts on archaeology's past, present, and future.

kd: Most Assemblage interviews are done in the pub, but since you're in St. Louis and I'm in Toronto...

AW: It limits the options.

kd: Yeah, I'm just going to have to promise you a few pints the next time we meet. I'd fax you a beer if I could

AW: But wet doesn't do well by fax

kd: No it doesn't! Oh well -- let's start at the beginning. Tell us about your first archaeological experiences.

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Getting Started in Canadian Archaeology: From Holding the Stadia Rod to Testing Hypotheses

AW: Well, my father was a colleague in the military with Jim Pendergast – I bet Assemblage readers would not know who he is?

kd: Oh, you never know!

AW: But certainly any Canadian archaeologist would. He was a very committed avocational archaeologist, and used to get grants from the National Museum, then called the Museum of Man, to test or excavate sites that they were interested in.

**"The Legendary Jim
Pendergast, Alison's first site
supervisor."**

AW: My parents both had a longstanding interest in archaeology – they were involved when the Ottawa branch of the Canadian Archaeology Society was founded, and they lived in England for several years and were involved as volunteers in public archaeology over there. So back in Canada, for a couple of seasons, my family would take summer holidays for the same period that Jim and his family were going to be out digging, and join him and work as volunteers

on these projects. So I remember being on St. Lawrence Iroquois sites, in the 60s. I was a kid, 8 or 9 years old, maybe younger on the first sites we were on; everyone else got to go to summer camp, and I had to go and do archaeology with all of these military engineers, surveying these sites. I remember holding a stadia rod and being really pissed! All my friends got to learn canoeing and swimming and I had to hold the damned stadia rod! These guys were really serious about their surveying

kd: And you were scarred for life?

AW: Yeah, really. And they would put all the kids in a pit to dig, give us a little excavation square to dig on what they thought would be the edge of the site – you know, to test for sterile – and of course inevitably we came up with great and wonderful stuff and got bounced off of our own pit. I don't know if this is family mythology, but part of the lore is that my brother and I (he's two years younger than me) used to scrabble around in the back dirt looking for stuff that people had missed.

kd: So did you ever find anything in Pendergast's back dirt? (laughter)

AW: Well I know we found stuff, but I don't remember whose back dirt it was.
(laughter)

kd: So you started young.

AW: Yeah. My parents had a lot of site reports, and regional overviews of archaeology in the UK from when they lived there, and *Gods, Graves and Scholars*, and various other kinds of things that I remember reading as a kid, and I was sort-of interested. But I certainly wasn't one of those kids that thought, 'this is the one and only thing I want to do with my life', and in fact I was introduced to the brutal reality of just how *tedious* archaeology can be at a young age! It wasn't exactly glamorous from my point of view! But then in college, after my first year of undergraduate school at Mount Allison University in New Brunswick, I needed a summer job, and there happened to be posted an advertisement for a job – [Parks Canada](#) was starting a big program of excavation on a half dozen or more historic sites that were in their care. So I sent in an application to that, and Jim Pendergast wrote a reference – I didn't have any others, besides my teachers in my first year – and miraculously I got a job, for Parks Canada. I'd actually asked to go to [L'Anse Aux Meadows](#)

kd: Who wouldn't?

AW: I was really intrigued by that, but I ended up getting sent to [Fort Walsh](#) in Southwest Saskatchewan, and that was wonderful.

AW and the crew on a break at Fort Walsh

AW: It's just the best possible place I could have gone, because of Jim Sciscenti, who was running the site and was the director of that project for Parks Canada. He'd been trained at the University of Arizona, and was a hardcore New Archaeologist, and sent us all a reading list before the season started, and it had, you know, Fritz and Plog, and Binford, and Deetz, and

Detlefson, and Deetz.

kd: And more Deetz

AW: And some philosophy of science, some Hempel and some Kuhn

kd: So what year would that have been?

AW: 73. It was pretty exciting because Scisenti was really committed to trying to do more with it than just figure out where the foundations really were. I mean, everyone was committed to doing careful archaeology, but the dominant mode in which archaeology was conceived by Parks Canada, if not by the people actually running the sites, was very much the kind of Ivor Noel Hume, get what you need to do the restoration and fill in the historical details. But Scisenti thought you could do an awful lot more with the site than that, and was inspired by the New Archaeology of the time. And it wasn't just rhetoric. It did figure in the field all the time – I mean we were asked to keep two sets of notebooks, one a running descriptive log with what we were actually doing, but another, a kind of idea book. His approach was one where we were really encouraged, even when just doing Parks Canada- mandated excavation of the officers' quarters, to try to ask questions about what we'd expect might be there, given the historical accounts, and in whatever way possible to design the work to answer those questions.

kd: That's amazing that you had that kind of experience, on essentially your first time out digging as a student. If it had been rescue excavation, as so many do have as their first encounter, your approach might have ended up being quite different.

AW: Yeah. The place where he was able to push most strongly a kind of hypothesis-testing approach, loosely described, was in surveying and testing the town site next to the fort. We did a lot of documentation of surface features and tested a number of pits, of cellar holes and middens, things that were evident on the surface. I remember Scisenti was interested in testing a hypothesis that you would have the most stable permanent population at the core of the town down by the creek, and then you'd have kind of concentric circles of other parts of the population, and functionally different kinds of buildings. And that, as I remember, was pretty much exactly what we found. But down at the core, we found some much more permanent structures than we had expected, with basements and masonry foundations and some pretty fancy artefacts – this was a town that was only occupied for like seven years.

kd: That short, and yet structures of that permanence?

AW: Yeah, and two trading establishments out of Fort Benton had sort of 'downtown stores'. But in addition there were some domestic structures that were definitely not little log shacks, and in subsequent salvage excavations by Olga Klimko of a structure that was eroding in the creek bank, she found a door lock with a little key, and windowpane glass, and a tea service, and we found children's toys, and jewellery.

kd: So would this be one of the first times that you realised that you could be surprised by archaeological evidence?

AW: Certainly the most conscious. I was out there three seasons in a row, then took a break; spent a summer at the [Fortress of Louisbourg](#), and one at Grasshopper graduate field school at the University of Arizona, and then went back and ran a walkover survey of the Fort Walsh park property. Louisbourg was run in a very different way, very good systematic archaeology but nothing like this kind of engagement with problems. Much bigger enterprise, the Fortress of Louisbourg, and much more pressure to get things excavated so the building crews could come in and reconstruct. And it's probably not fair to attribute to the project as a whole, but certainly some individuals were really fixated on particular categories of artefact, and really couldn't tell you much about why they were interested in glass bottles, or buttons, or whatever it was (laughter) ... so you know, that made it clear to me that my experience might have been very different if I'd gone to a different place, even a different Parks Canada site. Anyway, after that first season at Fort Walsh, I went back and did my second year of undergraduate courses. And my philosophy teacher from the first year -- who I liked a lot, I liked the philosophy course most of all -- turned out to be a philosopher of science.

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The Hand of Fate: The Plunge into Philosophy of Science

AW: He was Paul Bogaard who works on the philosophy of chemistry. I took his history and philosophy of science course, and we read Hansen and Kuhn and Hempel. From the very beginning, Mount Allison University being a small college, he knew all the students -- like the 15 or 20 of us in that second-year seminar -- well enough to know what we were interested in and what we'd been doing, and he suggested that I do something on archaeology. It was that year that I found the first of Merrilee Salmon's two articles in *American Antiquity* --

he had some kind of connection with Merrilee or with someone who knew Merrilee, so he had a couple of her articles that weren't yet published, and he passed them on to me. So I wrote a paper that second year on whether archaeology was having a Kuhnian revolution or not (laughter).

kd: And did you think it was?

AW: Well you know I found that paper recently and – I couldn't bear to read all of it, but the line I took was "well, it depends on what you consider to be a revolution."

kd: Aha.

AW: By that time, the spring of 74, there was a lot of critical literature out on Kuhn. I think Margaret Masterman's paper was already out -- you know, the 37 varieties of paradigms -- and other critics were asking, when is science ever normal and if it is normal would we like it... it wouldn't be critical, it wouldn't be questing after new ideas and insights and things. So I think the line I took in my sophomore way, was that this might be a test case where you could rethink what should count as a revolution. Whether or not archaeology had had a revolution, you'd have to get real clear what counts as one. The other thing that became clear to me through that year, certainly by the time I went back to Fort Walsh for a second season, was that there was a real problem in the archaeology literature. There were all these people arguing that they were having a Kuhnian revolution, and that the outcome was going to be a Hempelian positivist normal science. Even as a sophomore, it was crystal clear that this was a profound category mistake

(both laugh)

AW: that *Structure of Scientific Revolutions* was the watershed at which the fortunes of positivism had decisively turned, that Kuhn was issuing an historically-based challenge which crystallised a lot of debate among logical positivists, effectively demonstrating that many fundamental aspects of that whole program were bankrupt, and that if a broadly empiricist view of science was to be salvaged, it could not be in the terms set out by Vienna Circle logical positivists in the interwar period. Hempel himself had been publishing articles since the 50s in which he made all this clear. "The Theoretician's Dilemma" is the one that really hit home to me, when I finally read it in grad school: all this hand-wringing and concern about what sense a good positivist like Hempel could possibly make of the role of theoretical propositions in physics -- these "detours through the realm of unobservables". You know, if they're respectable

and have cognitive content, they ought to be eliminable in favour of, or be replaced by, compendious descriptions of observations – of systematisation's of observations – but if they aren't then they're strictly speaking nonsense. It looked pretty clear that they weren't eliminable, they seemed to be doing some work, but in his own terms he couldn't make much sense of how to characterise theoretical propositions of that kind. And that was particularly telling for the archaeological case. This is something I didn't work out until I was in grad school about five years later: what I came to see was that archaeologists wanted to develop more robust and detailed and ambitious theories about the unobservable cultural past -- they wanted to take those detours -- but at the same time they invoked positivism as the vehicle for doing that, and that was just fundamentally untenable! They were building Hempel's dilemma right into the heart of their 'positive' program for archaeology.

kd: And that was clear to you even then!?

AW: Yeah, it started to be clear to me as a sophomore, and my dissertation project as a grad student was essentially working out the dimensions of this central contradiction. It seemed to me that the New Archaeologists had some really interesting and compelling ideas about what might be possible, and why one ought to be trying to do more than what they said traditional archaeology had done, but it was just really clear that the vehicle of positivism was not appropriate to their ambitions.

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More Philosophy and Archaeology: Positivism and Polarisation

kd: Years later, you wrote that "One of the central insights to emerge in and through critiques of 'processual' archaeology is that it is a serious mistake to let methodological commitments and disciplinary ambitions determine how you conceptualise your subject." (1992:51) I always liked that sentence, and I'm just wondering now just how well you think archaeologists have learned that lesson?

AW: Well, funny you ask. I was out at [Cahokia](#) a couple of weeks ago, and Jim Brown was there, and Jim Stoltzman, and we were having a rollicking conversation about things archaeological. It was a lot of fun. Jim Stoltzman was describing some example of archaeology gone wrong and he said something

like, "well that's a prime example of scaling down your problem to fit your tools" (laughter). Only it was more devastating than that, it was more like *trivialising* your problem to fit inadequate tools – or redefining the subject matter so it would be tractable. Certainly I think this is true of evolutionary archaeology; in some of its formulations it depends on a redefinition of the cultural subject of archaeology in terms that make it tractable for a certain pretty *narrow* conception of what counts as respectable archaeological science. And, so on the one hand it seems to me there's been enormous and exciting development in all kinds of areas, in which postprocessual and antiprocessual challenges have really opened up the question of what questions should be asked, and whether you can just avoid dealing with those aspects of the cultural past that processualist archaeologists (the more ecodeterminist ones), thought were intractable and should just be set aside. But then on the other hand, there is this resurgence, in the form of American evolutionary archaeology in particular, which in some forms is an extraordinarily, even more radically reductive conception of the cultural subject, which seems at some level motivated by very similar methodological ideals as the narrowest processual archaeology. I just find it fascinating – I mean I think it would be a really interesting project., which I don't want to take up (laughter), to do a conjointly historical and sociological, and philosophical analysis of this whole phenomenon, of this resurgent hard-core science vision of the discipline.

kd: So do you think it's spawning a new round of divisive rhetoric?

AW: Oh yeah. I mean the sessions I've sat in on, and some of the stuff I've read over the years of the evolutionary archaeology is *really* – well, some aspects of this stuff reads like fundamentalist religion.

kd: Do you think that's any coincidence?

AW: Well, no (laughter). It's interesting... and I don't know quite what to make of it. I mean, I am just astonished. From people who are claiming the importance of being scientific, it is quite remarkable to see such heavy-handed dogmatism about what counts and what doesn't, as questions, as research methods. And then the social and political dynamics around who's in, and who's out... it's extraordinary. And the hostility to any line of questioning that doesn't accept the premises of the whole program is remarkable. There was certainly a bombastic dimension to the New Archaeology, but (as a younger observer, and peripheral to its development) I remember there being an incredible positive energy, an incredible optimism. As often as not, it seemed to me the New Archaeologists invoked the term "positivist" because they just meant "positive" – you know, let's get a positive attitude about what we can do.

And some aspects of what they knew about positivist models of science could be used for legitimating purposes, rhetorical purposes, but a lot of times, it was just sort-of an upbeat, "let's try to do something really exciting here!" And there doesn't seem to be that kind of energy associated with evolutionary archaeology. It's a very diverse family of programs, and some I find more compelling than others, and some I just find plain repugnant, and the repugnant part of it is the defensive, hostile, 'there's only one true right way' rhetoric. It seems so overdetermined, there must be an interesting socio-political story to tell about why it's arising now, in the face of a protracted debate about precisely the same limitations as arose with the New Archaeology.

It really strikes me that with regard to the polarisation generated by processual arguments and then postprocessual critiques in the early 80s, that within 12 to 15 years, most postprocessualists backed off the most hardcore line that they were taking. Certainly they didn't give up an oppositional stance, but started to engage in a serious way questions like: now we've broadened the range of questions we think you have to ask, given what we know about the complexity of cultural phenomena, given ethnoarchaeological and just ethnohistoric background knowledge about cultural lives... now, given those arguments (I'm thinking about ones that come out of Ian Hodder's ethnoarchaeological work), how exactly are we going to make use of archaeological data as evidence to get at those highly dynamic, plastic aspects of the cultural past?

So whether it was effective and successful or not, there's been serious thinking through of some of the questions about interpretation. And a search for sources and resources that could help archaeologists figure out how to do this much more ambitious job – where you *don't* define your subject matter in terms that fit your tools. And on the other side, there was this recent collection of essays on behavioural archaeology (Skibo, Walker, Nielsen eds. 1995) that grew out of a session at the SAAs seven years ago, in which self-identified behavioural archaeologists, heirs to the core of the New Archaeology, were arguing that behavioural archaeologists had to come to grips with religious dimensions of cultural life, spiritual dimensions, symbolic dimensions, of cultural life. They were expanding the questions and trying to figure out how to use their tools to address them. So by the early 90s, it seemed to me there were really exciting things happening. The hard lines didn't seem to be drawn quite so hard. But unless I have the chronology wrong -- of course Dunnell was writing evolutionary archaeology much earlier -- the real tidal wave of evolutionary archaeology started to rise right at about that time. I think it *is* deeply divisive. And I think that's regrettable. There are definitely exciting insights to be drawn from behavioural ecology, and some aspects of sophisticated evolutionary

theory. But from what I've seen, most of the evolutionary theory that's being invoked is very problematic, from even a biological point of view, let alone in applications to archaeological problems that seem to me usually metaphoric and analogical, at best.

kd: But often not acknowledged as such.

AW: No. But problematic even in its source context, let alone in metaphoric application to cultural phenomena as complex as those that archaeologists study. I think the spirit of archaeology at its best has to be that you'll explore and exploit every resource you can possibly get your hands on. This is one hell of a hard job archaeologists have to do, and you cannot decide in advance what's going to be useful and what isn't, and there are certainly useful things in the biologically derived traditions of research. But in their "one true faith" mode, evolutionary archaeologists take a stance that is inimical to the spirit of the very tradition of science – critical, Enlightenment-derived science – that they are promoting.

kd: Part of your understanding of the dynamics of archaeological debates derives from larger debates in the philosophy of science, and the Science Wars – from watching how other disciplines have played out their battles . You're looking at the understandings developed from that, and applying them to the archaeological scenario, is that fair to say?

AW: Yes, that's certainly in the background for me. In particular the Science Wars. It seems to me no accident that you have an evolutionary archaeology backlash emerging in exactly the period when *Higher Superstition* has come out (Gross and Levitt 1994), and Sokal's hoax has been transacted (Sokal 1996a and b), and there's this kind of yearning for the hard-rock foundation of science, and the kind of knowledge that's supposed to give us.

kd: So what we're seeing is a backlash, then?

AW: (sigh) Well, you know, it's so complicated (laughter). There are certainly people who would not regard debates about science and the return to true science as a backlash, but I think it's pretty reactionary on most fronts. And it certainly is a dominant force in lots of public debate about the sciences, and then in lots of debate within particular disciplines about what a legitimate research program is, and how funding should be distributed and so on. Yes, I think there's a larger crisis of confidence in what science has to offer, and a defensive reaction to that, and I don't know what exactly the lines of influence

are, what direction it goes in, but I think there is something parallel happening in archaeology now.

kd: Most of your discussions about archaeology and philosophy are located within the explicitly scientific model, and of course, that makes sense within the North American context. But having spent time in different contexts, you know that archaeologists in different academic cultures tend to pull from varying traditions -- for example in some of the British archaeological subcultures, Hempel's the last thing that they would ever have thought of reading. .

AW: Yes

kd: So the philosophy chosen influences their understanding of archaeological practice differently, and in turn it complicates the interrelationships between these different groups of archaeologists. I'm just wondering if you've got any observations on how these different subcultures are intersecting right now.

AW: Interesting question. My training is almost exclusively in analytic philosophy of science, and that's a very specific kind of training. Not uniquely North American because it's a powerful force also in English-language philosophy around the world; Anglo-American analytic philosophy is what it's called. But that split between Anglo-American and Continental really originated in the inter-war period and was entrenched at the time of the Second World War, when many of the Vienna Circle logical positivists fled Europe, fled the Nazis and went to the UK and to North America, where they have been a real influence. In North America, that tradition had quite a positive reception, in part because of the tradition of American pragmatism which differs from it a lot in some respects, but in other respects has more in common with it than, say, with existentialist, phenomenological, Continental, hermeneutic types of traditions. So that split is of fairly recent origin, and it was very strongly marked at the time I was being trained in mainstream Anglo-American analytic philosophy of science. But the divisions have been very much eroded in the last twenty years. By the time I was writing my dissertation, one of the most influential overviews of philosophy of science that I relied upon was by a Scandinavian philosopher, Gerard Radnitzky, called *Contemporary Schools of Metascience* (1968a&b). He was specifically making the point that many of the issues which Anglo-American philosophers of science took to be core to their philosophy of science tradition, had long been a matter of concern for Continentals and vice versa. Being Scandinavian he had training in both traditions and he could put these side by side. And so I was always really intrigued by 'the other side' of the philosophical divide, though I didn't get any

training in it. The other thing is that I was looking for literatures that were relevant to thinking about archaeology. One of the main literatures to which I was introduced as a grad student in philosophy at Binghamton, by Leon Goldstein, was philosophy of history, which put me in touch with people like Collingwood and some aspects of the hermeneutic tradition and so on. But it wasn't a big part of my training, and I've always felt that that was a real lack. Somebody getting trained now, or you know, more recently than I was trained, would certainly have the opportunity to get the background I didn't.

Now very soon after I finished my dissertation, people like Linda Patrik pitched up, and Lester Embree, both of whom are philosophers trained in phenomenology and continental philosophy. So they were writing, in Linda's case one article, in the case of Lester Embree a more sustained research program, inspired specifically by continental philosophy. And then within archaeology of course, with the whole tradition growing out of postprocessualism, one place that people turned to find resources for criticising the positivism of the New Archaeology was precisely to the philosophical tradition that had always been opposed to positivism. That's been a very productive influence. In some cases, I think the work is not very carefully done, by phenomenological or hermeneutic standards -- I think the practice of appropriating little bits of philosophy and using it for rhetorical purposes goes on in that area as much as it did with the use of Hempel and the positivists -- but the prospect of archaeology being enriched by drawing on a wide range of philosophical traditions is there, and it's been in place for a long time. I mean, somebody like Ray Thompson, writing in the fifties, on the subjective dimensions of archaeological interpretation, was drawing on Dewey. And take people like Kluckhohn, and others who were writing in the 40s, criticising the narrow empiricism of archaeology. Kluckhohn actually published a paper in the journal *Philosophy of Science*, I think it was in 1939. And he was clearly aware of the debates that were happening in philosophy of science, and the limitations of the strict Vienna-Circle derived logical positivism, and he clearly understood the distinction between positivism and empiricism, and that narrow empiricism was the problem.

kd: And somehow that got lost?

AW: That was never recognised. Spaulding certainly recognised that distinction, he insisted on a liberal empiricism, but beyond that, in rhetorical appropriations of Hempel and so on, the distinction was lost. So anyway, the point is just there are a lot of resources there. And archaeology is *acompllicated, difficult* enterprise and you need all the resources you can get, and there are lots of different kinds of philosophical resources that are relevant

for different purposes. So with the questions I've been interested in, I've mainly drawn on analytic

kd: but it's not a deliberate exclusion

AW: It's an accident of how I was trained, and mainly what questions interest me. But the other thing that has to be said is that within philosophy there's been a real blurring and undermining and crossing of those old boundaries between continental and Anglo-American philosophy. It's no longer the case that Anglo-American analytic is narrowly in the mould of positivism. And so you see influences that can only be described as phenomenological or hermeneutic in that area; it's not as narrow as it once was.

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Metaphysical Anxiety

kd: In answering the last question, you were citing references going back forty years, fifty years, and certainly in your writings you've cited some that go back a good deal further than that. You've observed on more than one occasion that "archaeologists have debated a remarkably consistent core of issues" for a hundred years now, and those are primarily the ones concerning, as you put it, "the status and security of archaeological claims about the cultural past" (1989:1). And at the same time, you also noted that archaeologists have a special propensity for getting really anxious, in a metaphysical sense, about the nature of what it is that we're trying to do. That observation you made ten years ago, so I'd be interested in an update – how do you think we're doing now?

AW: I guess I think that's still going on, but at a theoretical level and at a philosophical level within the field -- with evolutionary archaeology for example, there's a regeneration of a certain kind of polarisation. And it does seem to me that at the root of those debates is this concern – how do you make use of archaeological data as evidence in a responsible, well-grounded way? If you set the bar too high on the scientific credibility scale, you can do very little more beyond describing the contents of the record and the most technical aspects of its production. And then the question is, if you really want to talk about the cultural dimensions of the past, in all the richness and complexity of that subject matter, how do you do that without lapsing into just-so stories and speculations? It strikes me that the pattern that keeps regenerating itself here is

predicated on the assumption that it's all or nothing: either you've got an absolutely rock-solid, empirical, scientifically respectable foundation for making the kind of claims you want to make, or you've got nothing but arm-waving and projection of fantasy or hope, or whatever, onto the past... the despised, the hoped for, you know. And it seems to me that that's the fundamental problem, a problem that is actually at the root of the Science Wars debate. There seems -- and this isn't unique to archaeology at all -- to be a really deep, not exactly animosity, but something between animosity and anxiety (laughter), a failure to recognise, that even if you can't grasp the brass ring and get God's truth, or Enlightenment Science Truth

kd: Really Real and Truly True, as you've said (laughter)

AW: Really Real and Truly True, yeah... then you've got nothing at all! And oftentimes when that standard is held up it gets illustrated by examples from areas where you *can* say with a *fair* degree of empirical security, how exactly a particular tool got produced, what technical resources and materials had to be available, even maybe where they came from. Or, or take a slightly more equivocal but still compelling kind of example, where you can reconstruct what somebody's lifetime dietary intake had to have been to have the bone isotope profiles that they do. And if that's taken to be the standard, the presumption is that you've got nothing at all like that when you get to the level of talking about -- this is Hawkes and Piggott's famous ladder of inference -- talking about social organisation beyond the most mechanical and pragmatic dimensions of it, or much less, what people actually believed or thought, what their symbolic visual culture meant to them, and so on. And it strikes me that it's just a mistake to think that you can draw a line somewhere on that continuum and say "below this, you've got nothing at all -- and anything that doesn't measure up to what's above the line, is completely irresponsible, laughable". And it strikes me that in fact, the line is a lot more vague than people tend to recognise. The complexity of the inference that goes into establishing radiocarbon dating, even, much less something like isotope analysis and so on, is *very* elaborate. There's an enormous range of background assumptions upon which you rely -- witness the long, agonising struggle to calibrate (laughter)

kd: So often forgotten

AW: Yeah, but that is actually a *good* example showing that the degree of security now realised in many cases -- with radiocarbon dating, for example -- is a function of really hard work on the background assumptions, and really careful assessment of what kinds of contaminants might be a risk in certain kinds of settings, and so on. And that suggests, first of all, on the negative side,

that even the most technically and empirically grounded inferences are always defensible, and are complicated, and even though they may be more secure and more credible, they're *not* structurally that different from the kinds of inferences that are at play in other areas that seem to be much more obviously speculative and uncertain. But on the positive side, that kind of example illustrates exactly the kinds of things that you can do to figure out what the ranges of reliability of those kinds of inferences are, to determine what degree of credibility they have, under what circumstances they're likely to be uncertain, what kinds of claims they can support and what kinds of claims they can't. And that represents a very, uh, positive (laughter) accomplishment. To be able to delimit, even with some uncertainty, what you don't know or what you can't know or what the limits of reliability are, is a major accomplishment. And it seems to me that archaeologists do that *all the time*, with respect not just to calibration of radiocarbon dating, where it seems sort-of obvious how you do it, and it's underwritten by 'real science', but also in a range of other areas where the inferences seem vastly more uncertain and have to do with much more dynamic and much more open-ended, and much more human and cultural, dimensions of the subject matter.

So I would make a plea for recognising that archaeological inference lies along a long continuum of different degrees of security, and first of all, you can't assume that a particular kind of inference, like all inferences to do with social dimensions of the past, are going to be by definition more insecure than all inferences to do with, let's say, technologies of production. And second of all, by extension of that, it's important to recognise that archaeologists do this all the time, and it's more than possible to make nuanced and well-grounded judgements about the degree to which an uncertain inference is uncertain or credible. So there aren't going to be hard-and fast lines, boundaries to be drawn between the thinkable and respectable and the unthinkable...

kd: The questions don't get any easier but we can find more constructive ways of approaching them?

AW: Yeah, and I think constructive ways of approaching them mean recognising the insecurity that's attendant on *all* archaeological inference, recognising that these judgements about degrees of security are just the stock-in-trade of doing archaeology, as well as most other disciplines. There's a compulsion to find what some philosophers call 'inference-stoppers' (laughter) -- Peter Kosso, I think, uses this term 'inference-stopper'. For foundationalists of all stripes -- whether the foundations are meant to be inspiration from God, or necessary analytic truth, or empirical givens -- the compulsion there is to find ground that you can appeal to, where there are no further questions to be

raised. I mean ‘inference-stopper’ in that sense, and there just aren’t such things to be had in most interesting fields of inquiry!

kd: It’s too bad really isn’t it?

(laughter)

AW: But on the other hand...

kd: Yeah, *is* it too bad? It probably *isn’t*.

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On Feminism and Academia

kd: In one sense this leads us nicely into philosophy and feminism, which has of course been another major interest of yours, that has intersected with your archaeological work as well. Could you tell us a little bit about how you got interested in feminism & philosophy in general?

AW: Well I guess my interest came in the philosophy, and it’s quite closely tied to an activist interest in the status of women. I’d always been interested in feminist literature and the second-wave women’s movement from the time I was an undergraduate, but the business of doing a graduate degree meant focusing (laughter) and I didn’t pursue those questions very much except at a more personal level. I wasn’t politically active and I wasn’t reading much feminist stuff. But autobiographically, I see that the point at which I started to get really interested in feminist academic scholarly literature and also feminist activism, was precisely the point when I was on the job market, and began to see up close and in ugly detail, just how the deck is stacked in various ways. And not especially against me -- I mean, I can certainly think of instances that were really politicising for me personally – but you know, you look around and see.

I remember when a sexual harassment officer at Western, my old university, had been invited to the department to give a presentation on what the university’s policies are, and what counts as sexual harassment. (This is a standard thing that this advisor was doing as part of the university program.) There were two advisors, a man and a woman, and the woman handled most of

the cases because they mainly came from women, there were complaints, but the man did most of the education and public outreach. He was a very effective presenter, *very* compelling, and he said to the assembled senior male faculty, "I remember when I was first started working on sexual harassment stuff, and I just thought I'd never witnessed sexual harassment, I didn't think it existed. And I got appointed to a committee, and I got involved, and I started hearing these accounts, and working on these cases, and it was just overwhelming." He was describing his own awakening to this issue. And he said if that's how you feel about it, which is perfectly plausible, the best thing you can do is ask one of your colleagues, not aggressively, not in a tone of denial, but just ask, 'Have *you* experienced this – what have your experiences been?', and give them the space to tell you, personally. He said, "I can guarantee you, that nine out of ten will have stories to tell you that will just curl your hair". And I went out of that room thinking Oh God, I hope none of them ask me, because I haven't had any personal experiences of sexual harassment! (laughter)

I had no illusions about its existence. But it was really interesting, because in the space of about two days, these memories started coming back. Not heavy-duty repression, no soul-destroying physical assault or anything, but I remembered an incident in grad school where I'd had to stay late to finish an exam that had been set, and there was this faculty member, who I had never directly worked with – in no way, he wasn't on my committee, I never took a course from him, nothing -- but he was proctoring this exam, because as it happened he was the last faculty member in that day, and he'd agreed to receive it when I was done. So I took it along and gave it to him, and he immediately started pestering, you know, to go out for a drink, and could he give me a ride home, it was late, and so on. So I accepted the ride home because otherwise, it was a long wait for the bus, and here's this guy grabbing at my knee as I leap out of the car at an intersection (laughter). I mean it was trivial in a way, but

kd: But nonetheless interesting that you'd forgotten about it?

AW: Yeah, I'd just forgotten. I mean, it didn't have any impact on my graduate career, and it was kind of a joke, really – I told a couple of other people and they laughed – but then I started thinking. There were two or three other women in the graduate program I was in, and then a woman postdoc who came to the department while I was finishing up. Two of them were *really* sexually harassed – in the case of the grad student, by her supervisor, and in the case of the postdoc, by the faculty member who was closest in her area. I mean, as in 'you want a letter of reference, you want grades, you want a dissertation, you want approval, here's what you've gotta do'.

It hadn't happened to me, and in the two cases I'm thinking of there were all kinds of other complications in these women's lives; they eventually left grad school or left the profession. But in retrospect, I thought, this is real interesting -- like, none of the guys dealt with anything like this, and they were certainly not more able as graduate students or postdocs than the women. Whatever problems they encountered -- and certainly there was a standard amount of mean-spiritedness and manipulation and political backstabbing and so on - - *this* was not part of what they had to deal with. And it really was *that* moment when I realised, yeah, I've been really lucky in this -- I have not been in a situation where my career or my sense of self has been compromised by sexual harassment -- but it's *all around me*, and part of the way I had dealt with it was just by not remembering it. And seeing a lot of other stuff, just sitting on search committees, sitting on P & T committees, sitting on graduate committees, seeing the discourse about women vs. men... really brought them home. In one case, I remember sitting on an appointments committee and noticing that the same credentials would be projected very differently. For a woman who worked closely with a senior scholar in the field, published in the journal he edited, co-authored with him, there were immediately questions raised about whether she was capable of an independent research program. For a man who worked with the very same guy, with the same publishing pattern and so on, obviously great things were to be expected because he clearly had won the confidence of this senior colleague, and was doing important work.

It got to the point where I just *couldn't miss* those things, and there were other experiences I had that made them real for me -- job searches in which I was a candidate and just saw close up how things unravelled. And so my interest in feminism really arose out of a commitment to political activism, initially in philosophy. And in retrospect this is sort-of *embarrassing*; I had the luxury of being white, and middle-class, and having education and background and upbringing that made me confident that this was a meritocracy, that if I worked hard, and was smart, and made the grade, then me and all my sisters would be rewarded, we would have the same crack at the same opportunities as the men. That I could have held on to those convictions until the point when I was a postdoc and on the job market -- in my late 20s -- and only *then* had them really shaken, is pretty stunning testimony to how naïve I was, and how privileged I was. So I'm not *proud* of how I got involved, but that's how it arose. I've since got involved with chilly climate activism for women faculty, staff, and students in universities and colleges, and -- playing more of a facilitating role than anything else -- with projects within archaeology on the status of women. [See The Chilly Collective 1995, and Wylie, Nelson, and Nelson 1994]. But more and more, in terms of political activism, what I'd really like to get involved

with are issues that bring together inequities that arise at the intersections *at least*, just to take the triumvirate, between gender, race and class.

As it happened, when I was at the University of Calgary in 84-85 as a postdoc, and had first started thinking about these issues and being really aware of them, I was asked if I would teach a Philosophy and Feminism course. It was going to be struck from the books if it wasn't taught within the next year. Under the circumstances I felt like I couldn't say no -- I mean I was interested in doing it, and it needed to be done -- so I started reading a lot, a LOT of feminist philosophy (laughter). It was a great class for me; I learned what the depths of my ignorance were. (laughter)

So that's how I got involved with teaching feminist philosophy. Then at Western I got involved in the development of Women's Studies -- drafted the curriculum proposal for a Women's Studies program, and developed some of the core courses like the senior seminar -- and I read more and more widely, and was teaching more and more widely in feminist theory. That was through the 80s, but it wasn't until I was invited to the Wedge conference that Joan Gero and Meg Conkey organised in the spring of '88 [where the papers in Gero and Conkey eds. 1991 originated -- ed.], that there opened up the possibility of making any connection between my growing interests in feminist scholarship, and archaeology. So I went to that conference, and went to Chacmool [1989, where papers in Walde and Willows eds. 1991 originated -- ed.], and so on, and that's how the convergence really happened.

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An Archaeology of Gender?

kd: So when you asked "Why is There No Archaeology of Gender?" in 1991, what sorts of responses did you encounter?

AW: One response was an unconditional "So why the hell should there be?"

(laughter)

kd: Uh-huh

AW: I was really aware that I was cashing in chips, you know, to the extent that these unsympathetic people would *listen* to me, it was because they thought I'd done some sort-of interesting work in the past

kd: People are still asking that question, if it makes you feel any better...

(laughter)

AW: Well yeah I know! So maybe they'd sit still for maybe a paragraph on why there should be an archaeology of gender, but my credibility as an objective hard-nosed serious scholar....

kd: Was on the line?

AW: It was certainly in question. But there was another response. I remember I gave a paper along those lines at Southampton -- maybe an earlier version of that Wedge paper that later got published (Wylie 1991) -- and I remember Peter Ucko scowling, and saying, 'Well there are people around here who would beg to differ with your title, there already *is* an archaeology of gender and there's always been one'. And there's a sense in which that's true. Peter had long been doing interesting and important work on the problematic ways in which Venus figurines had been interpreted, for some time. So on the positive side, there were already people raising questions, as he did, that subsequently have been a mainstay of some of the explicitly feminist work in the area, the work by Ruth Tringham and Meg Conkey (on 'Archaeology and the Goddess', 1995), for example. But on the other hand, it's true in another sense, which I think he didn't intend, and that is precisely the sense that Meg Conkey and Janet Spector and Mary Whelan identified in their early articles pushing for work in this area: *all kinds* of assumptions were being made about gender, *all kinds* of claims were being made about gender, that nobody ever thought they had to substantiate, they just seemed so obviously true. Women would be staying at a home base, and men would be ranging widely, and *supplying* the little woman, and the babes at home. (laughter) I mean, that's a caricature but

kd: Oh no it's not

(laughter)

AW: And many assumptions were just transparent, people didn't recognise them as even assumptions. And so in that sense, there had long been

kd & AW simultaneously: An archaeology of gender! (laughter)

AW: Robust and alive and well. When I gave a paper along these lines at Harvard, Steve Williams showed up and was *furious* and fuming and raised a *huge* hue and cry about how if archaeologists had to give up all the kinds of assumptions I was describing, they wouldn't be able to do anything at all. It was *really* interesting, he was just *raving* about how this was such a negative challenge I was posing, and wasn't the implication of my argument that -- I can't even remember now what the range of topics were that he thought were central to archaeology -- one would have to just *excise* most of archaeology and this was unthinkable. I said No, that's not the implication, here are all the kinds of positive research programs that open up when you take these problems seriously, but even if it *is* the implication in some areas, isn't that an important form of progress? I mean, this is after all the man who wrote about fads and fictions in archaeology

kd: *Fantastic Archaeology*, yeah

AW: So isn't it progress to identify areas where you've been making claims and making assumptions that you really ought to be scrutinising? He just kept restating this question and pushing it and insisting in the most bombastic and hostile manner and *finally*, this elder statesman rose up and said 'That is *not* what Wylie is saying, Wylie is arguing thus-and-such, and *I have a question*'. (laughter) I forget what the question even was, but I was subsequently introduced to this person who saved me from Williams, and discovered that it was George Cowgill (laughter), who I knew walked on water anyway, from what I'd read, but it was confirmed when I met him. So that was another reaction to the question, "Why is there no Archaeology of Gender?": because if we took it seriously, it would undermine the necessary foundations of all our favourite theories. That seemed to be the Williams view of the matter.

(laughter)

AW: So there are a couple of responses. But now that gender research is getting more and more mainstream, more often the question I get is why call it feminist, and what's the feminist component of this; isn't this just good archaeology?

kd: Yes, I was just going to ask how you'd distinguish between gender-informed archaeology and feminist archaeology -- or do you consider that distinction to be important?

AW: Well, I think that's a tough one. I've just been trying to write about it, and I find it *really* hard. There's a paper I just finished a couple of months ago for a

collection of essays that Londa Schiebinger and Angela Creager and Elizabeth Lunbeck, feminist historians of science, are publishing, called *Science, Technology, Medicine: The Difference Feminism Has Made*. This is supposed to be a paper on what feminism has contributed to archaeology. And I found myself having to step back and say at the beginning, well, here's what I think feminism is, and it's important to note that there is this growing, robust, vigorous literature on questions about women and gender in archaeology, but much of it is quite carefully *not* identified as *feminist*. There are feminists working in the area, for sure, self-identified feminists, but If you follow one strategy for drawing the line that has been used by feminist sociologists -- which is to say that you should consider as feminist whatever kinds of research are done by those who identify as feminists -- that's a problem in archaeology, because you lose most of this work, right (laughter)

kd: Yes indeed

AW: And on the other hand, it seems to me that there's a perfectly standard, good sense in which the research on gender is -- maybe at some remove -- motivated, inspired, informed by the women's movement and by feminist scholarship as developed in other areas, even though in some cases, these connections are not very clear. So I would want to say that there is something definitely feminist about this growing body of work on gender in archaeology, just even at the level of awareness that gender is a factor that matters, where it hadn't been taken seriously before.

kd: Yes. I'd like to pick up on that one, because in your 1997 paper, you shifted from your original question with respect to archaeology, gender and feminism-- you shifted from the question of 'why not before?' to 'why now?' And you argued that there was "nothing in the theoretical content, intellectual history, methodological refinement, or evidential resources of contemporary archaeology [that could] explain why an interest in questions about women and gender should have arisen (only) in the late 1980s". And you suggested instead that the determining factors would be the "socio-political features of the research community and its practice" (1997:93). Can you talk about those features a little?

AW: Yeah, that was a very important realisation. It now seems to me sort-of straightforward and obvious, in terms of general science studies -- *any* interesting development, in a research program or a new theoretical initiative, orientation, a paradigm if that's even a useable term anymore (laughter), anything of substantial, creative import and impact on a discipline,

is *not* going to be explicable in terms of, you know, ‘the evidence made me do it’ (laughter) or that ‘the conceptual analysis made me do it’ (laughter)

kd: Yeah, it doesn’t really work that way, does it?

AW: No, that’s clear, and the rapprochement -- a little uneasy to be sure -- between philosophy and history of science and increasingly sociological and anthropological studies of science, arises precisely from the recognition, even on the philosophers’ side, that most of the interesting developments in the history of science, most interesting aspects of science, aren’t going to be explicable in purely conceptual terms ... though that may just sound so obvious as to wonder how philosophy ever stumbled along (laughter)

A great many sociologists of science have long argued that you have to take account of socio-political dimensions -- which means institutional contexts in which researchers practice, the history, time-dependent aspects of their training, interaction with one another within a field, socialisation processes, and interaction between fields. Just to get at the intellectual history, you have to consider all those aspects of the socio-political contexts in which scientists work. And to understand things even as seemingly clearly epistemic as what counts as evidence or not, you really have to consider a whole complex range of factors that put some communities or individuals in a position to recognise some things as evidence and take them seriously, and make use of them in interesting ways. With something as complex as the formation of a pretty amorphous family of research programs that could broadly be described as sharing a concern with gender, and women, in archaeology... already, just to define what it is you want to explain is complicated enough. To think that it could be accounted for in terms of any one set of factors, seems to me patently ridiculous. But the fact is among people advocating for feminist or gender research approaches in archaeology -- one standard line was ‘well, Anglo-American archaeology or at least North American archaeology has been dominated by an ecosystem model which rules out of consideration aspects of the lifeworld, and gender is one of those, and so maybe that’s why nobody talked about gender.’

But that quote you were just reading -- I worked on that analysis a little bit. When I went to the Wedge conference, that was essentially what my paper was about, but the more I thought about it... Well, Binford in the late sixties, early seventies, was talking about sociotechnic and ideotechnic artefacts – that if the system is integrated you ought to be able to get at the ideological and social dimensions of it -- and there had been very interesting work done by New Archaeologists on social stratification and changes in socio-political structure

over time, and now the behavioural archaeologists who want to talk about spiritual and religious dimensions. So why would they never have considered gender? Which seems to be -- however plastic and variable it is in form -- a dimension of cultural systems, and sociocultural life, that structures people's lives in virtually all contexts we know. And then there were the postprocessualists arguing that there needed to be attention to a whole range of aspects of the cultural past that seemed to be ruled out of consideration by hardcore processualism. They routinely listed gender dimensions, and some people did do work on gender dimensions of cultural symbolism and so on, but there was *very* little work done overall by postprocessualists. And at the same time there are famously anecdotal – and now-and-then cropping up in the published literature – examples which are not just non-feminist or not gender-sensitive, but pretty straightforwardly misogynist treatments of archaeological material and interpretation.

kd: Is there a particular example that you have in mind?

AW: Well, the one that gets cited all the time, for example in Erika Engelstadt's and Roberta Gilchrist's reviews, is this one, where Shanks talks about archaeology being like

kd & AW simultaneously: A striptease! (laughter)

AW: Yeah, and you know, for much other archaeological literature you have to kind-of *infer* what the subject's position is intended to be (laughter)

kd: But it was sort-of in plain view in that case wasn't it?

AW: Yeah, and unless you imagine that Shanks identifies with dykes and S&M leather joints, you know (laughter)... that's just the most blatant one that gets cited all the time, but there's lots of other work where these kinds of assumptions about who really does archaeology are maybe less sexualised but just below the surface. And that's *not* surprising, I mean you find the same dynamic on the left. There are certainly some scholars on the left in various fields who immediately took on the challenge from feminists, that Marxist theories are gender-blind -- they were using this ablest language routinely in the 70s and 80s – but there were a lot who were very hostile to feminist interventions precisely because they saw them as undermining the integrity of their analyses, and they were confident that the factors that feminists wanted to look at had to be trivial and dismissable, beside the point of serious political-economic class-based analysis. So it's not that this is a new or unique dynamic around feminism, that those who you expect to be your allies may

be *precisely* those who are going to be most touchy about having to take on yet *another* dimension of oppression, yet *another* factor.

kd: So overall how do you see feminism and postprocessualism in archaeology relating?

AW: Uneasily. In fact, I look at the gender archaeology literature and a lot of those writing on gender are at *pains* to demonstrate that they are just good processual archaeologists who are taking on this new topic. And in many cases they are able to do quite interesting things, but the fact that they feel they can only take on one battle at once, or only do one innovative thing at once, is interesting. And in fact that survey I did of people who participated in Chacmool in 1989 – first of all, just barely over half would identify as feminist, with lots of caveats and qualifications

kd: Pretty startling, considering it was explicitly a gender conference

AW: Yes, and when I asked them, why do *you* think gender research is emerging now -- in particular there were questions, how does it relate to postprocessualism and so on -- from many of them, there were *very* equivocal responses about postprocessualism. Many were prepared to say that postprocessualism had opened the space for thinking about dimensions of the cultural past like gender, at a kind of *general* theoretical level, but they were adamant that it had no influence on their own archaeological thinking. So I thought that was *really* interesting. I don't know what the truth of it is, they clearly knew enough about it to be able to say that it made a difference in the discipline as a whole, but they were denying that it was a direct influence on them; they were *disaffiliating* from postprocessualism.

I guess the starting point of this discussion was the question, so why is the archaeology of gender emerging now? and I'm adding to it the question, why is taking the form that it has taken? I think that the form that it has taken is very much dictated by the conditions to which feminists and 'cryptofeminists', are responding in the field. But it's an extremely complicated question. And for me, primarily trained as a philosopher, thinking about gender research in archaeology, one of the most important lessons I learned from thinking about that question -- why has it arisen now, why not before -- was how many dimensions of the enterprise you have to be considering to give any kind of answer. I don't think any one person could have the training or background necessary to do this well. You really need a team of people with sociological and anthropological science studies training, history of archaeology and the social sciences as well as philosophical interests, to be able to use all the

different kinds of research tools you'd need, to track even just the intellectual tradition -- to identify what aspects of the intellectual, and methodological, theoretical and other kinds of debates, and research programs of archaeology since, let's say the 60s, would foreclose an interest in gender or feminist work until much later than in other areas in anthropology and in history, and what changes in the late 80s would make it possible.

The argument I made is that there is a very striking fact that in archaeology you only get cohorts with a large representation of women emerging in the late 70s.

kd: Yeah, and it does tend to be a fairly *collective* discipline as well – maybe more so than, literature, history, and some areas of anthropology – you tend to work in large teams, so maybe that's one of the factors as well

AW: Yeah, I think there's a lot to be done on that, and interesting comparisons to make between how women have fared in lab-bench based disciplines, and also in team fieldwork disciplines, not individual fieldwork but team fieldwork disciplines.

I was struck, though -- this was the point of departure -- when I did the survey of the participants in the 89 Chacmool conference, that the bulk of the women were in an age bracket, and had a number of years away from finishing their highest degree, that put them just at the cusp of beginning to establish themselves as professionals. They would have been members of the first cohorts in archaeology that had a 25 or higher percent of women in them, that were going through graduate training and in the late 70s, and so by the late 80s, whether their terminal degree was an MA, and they were in a museum position or contract archaeology, or whether their terminal degree was a PhD and they had an academic position or some other kind of position, they were at a point when they'd been out of grad school about a decade and were getting established. It struck me that they would have been coming into archaeology as part of a cohort in which their very *presence* was disrupting the demography of the field as a whole, and in which women were increasingly present on the SAA executive board, and in more powerful and visible positions than they'd ever been. Not that they hadn't been there before, they *had* been,

kd: It was just in sufficient numbers

AW: Yeah, numbers and visibility I think did change in that period. And so somebody who was an adamant *anti-feminist* maybe was aware of the gender changes and wanted to *disassociate* – ‘ I'm not just here because I'm a woman, I'm here because I'm a good archaeologist’ – and even for those who had a

neutral attitude and didn't care about it and didn't notice much, and hadn't ever gotten politicised and gotten active, it would have been just part of their living-in-the-world professional and personal consciousness, that they are in a field where the demography is changing, that they have opportunities that not that many women in the past had had, that they're negotiating working and personal relationships on different terms than have been typical for women of previous generations. Well, that's just the women's movement. When people say, has feminism died, well, maybe as a particular kind of organised movement, but the impact that it's had on people's lives, the trickle-down effects, are *profound*.

What people take for granted as the level of education a woman needs, what she can think about doing, and the terms on which – although this is less susceptible to change, it turns out – a woman can negotiate relationships with their family, and their partners, and with the world around them, are just different. And so I think there's an *awareness* that gender has been a contested category, and an awareness that women in a field like archaeology, coming in the late 70s, just by *being* there are contesting the gender conventions that had got entrenched in the field. So at that level, I would want to say, yeah, feminism had *something* to do with it! (laughter) even if many individuals doing gender research weren't directly influenced by feminist scholarship, don't identify as feminists, didn't do it because they felt they had a political commitment.

kd: Are you seeing similar changes on the horizon for other groups that have been traditionally marginalised within archaeology and within academia? Do you foresee similar waves of change?

AW: Well for First Nations and what the WAC describes as indigenous peoples, those whose cultural heritage archaeologists study, I would certainly hope that they would have a stronger presence. But I see more difference between areas than I do over time. (Though I have a fairly shallow time frame here.) I don't know that we have very good data on representation, but I don't see anything happening real fast on that front. Tom Patterson makes a really interesting argument about how the class structure of the field was profoundly changed with the G.I. Bill in the U.S., so a lot of women coming in the late 70s is not the first time that we've seen the demography of the field change. And with it, other kinds of intellectual, and methodological, changes follow, and patterns of employment change. So... but although there certainly are many more people of colour doing archaeology (in North America), and indigenous people doing archaeology, than I ever knew about before, I don't see the kind of change where there's like, 20 to 30 % representation. Which seemed to be the point at which women really began to have an impact. Not that African-

American archaeologists, or Native archaeologists aren't having an impact -- they are -- but I think it's going to be different in kind when it's a handful, as opposed to all these women, most of whom didn't identify as feminists, and didn't think much about the fact that they were women, who were suddenly part of a cohort that had 30 plus percent women in it.

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Archaeology and Ethics

kd: With regard to other areas of the social context of archaeological practice, another area that you've become increasingly involved in has been ethics committees, for professional archaeology organisations, like the [SAA](#) and the [CAA](#) (e.g. Wylie and Lynott 1995). And you've made some really interesting observations about the tensions and the conflicts of interest that are implicit in what we do, in, as you put it, "adopting a stance as protectors of a scarce and valuable resource *and* being a primary user of this resource" at the same time (Wylie 1996:166). You've pointed out that there are a lot of problems emerging about very blurry boundaries between archaeological/scientific and non-scientific and non-professional uses of the archaeological record, and also the fact that all of these different critiques, such as feminist and postcolonial critiques, have undermined the idea that archaeological inquiry can "be assumed to produce understandings which serve a common good". And those problems are major, obviously, as you pointed out in 1996. I guess my question is -- how do you think we're coping right now? And what do you foresee in terms of the changes with the way that we cope with those problems over, say, the next twenty or thirty years? Do you see archaeology still existing as a coherent discipline?

AW: (laughs) That assumes that it's a coherent discipline now!

**Aliso
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Wylie
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y.**

kd: True!

AW: I think what is really going to transform archaeology in the future is the political economy of the discipline. It seems to me to have really transformed archaeology as a discipline, not just since the 70s, when the major cultural heritage legislation came in in the U.S. for example, but since the 30s, again thinking of the U.S. context, where there were all these federal WPA projects. Professional archaeologists were being hired to *do* archaeology, not to teach it, not to develop museum presentations, but to *excavate* for the government. You know the debates about what counts as a professional archaeologist, and whether there should be a register with minimum standards of education and practice in force, those issues have been actively debated since the 30s. Tom Patterson (1995) has some interesting discussion of this in his chapter on interwar period, Second World War period archaeology, after the founding of the SAA, after '35 -- it's not like these issues arose in the 80s. But with the CRM industry, they arose with a vengeance. So where the employment is, where the money is -- large-scale forces of political economy -- how that changes in the future is the single most important factor in determining what shape archaeology will take as a discipline, who's doing it, how much of it's being done, who has a stake in it, who has control over it.

kd: So you figure that the economics are going to continue to define our ethical commitments?

AW: Or at least set the terms of debate. If there isn't major development happening, if states don't have money to build major pipelines and roadways, and you don't have every developer who can put the money together building yet another subdivision, shopping mall, and office building, there's going to be less pressure on archaeological resources. On the other hand, there's going to be a lot more interest on the part of the vastly many more impoverished people, people living at or below the poverty line, in exploiting whatever resources they can get their hands on, which will include archaeological resources.

kd: And certainly does include that now – you were recently helping to clean up after pot-hunters in Kentucky?

AW: Yeah, along the Green River, a shell mound. Yeah, that continues as part of the local economy in rural Kentucky

kd: And it would be naïve of archaeologists to suppose that we've ever controlled that

AW: Never, we never have. I think one thing that is really regrettable, and I lay this at the door of archaeologists, is the history of how archaeologists have related to indigenous peoples and especially Native Americans, because we *ought* to be allies. Certainly Native American interests, for example, preclude some uses of archaeological resources, and I think that should be respected, but on the other hand, I think archaeologists as a group, have an interest in the archaeological record that is closer to Native Americans' interests than those of developers, or *looters*.

kd: So it's a terrible irony, all the conflict over [NAGPRA \(see also here\)](#).

AW: Yeah it is. I think NAGPRA was a disaster waiting to happen. I recently reread an interesting article by Eldon Johnson in *American Antiquity*, dating to 1973, called "Professional Responsibilities and the American Indian". He basically lays it out. He says, we're walking into a disaster here; Native American activists, (well he wasn't using that language then, "Indian" activists) have some *serious* objections, and we should be taking them seriously, we're all trained as anthropologists

kd: And it took some archaeologists twenty-five years to listen.

AW: Yeah! and in that little two-page article, he explicitly called on the [SAA](#) to be proactive, said that the SAA ought to put in place guidelines for governing archaeologists' dealings with Native Americans, ought to be respectful, ought to take seriously these claims. He was certainly a minority in the field, but it isn't as if there weren't any archaeologists who were in situations, in his case in Minnesota, where these issues were already coming to the boiling point nearly 25 years ago. So I think something like NAGPRA was just about inevitable, given that archaeologists really had not been proactive on those issues. In some local areas, they had, and there were good working relationships with Native Americans, but it doesn't take more than a few examples of arrogant, high-handed archaeologists to provide grist for the political mill.

And of course on the Native American side, even those who aren't *particularly* interested in their archaeological heritage could see that this was a winnable battle; archaeologists set themselves up to be pilloried. The tragedy is that archaeologists' primary alliance could and should be with descendant communities, if they are not themselves members of the descendant communities.

kd: Do you think that the new codes of ethics that are emerging within professional archaeology organisations and international congresses hold any hope for really starting to reconfigure these relationships, or do you think that they're ultimately going to be lacking in teeth?

AW: Well, even if they had really powerful teeth... [SOPA](#), now becoming the [Register of Professional Archaeologists](#), *does* have teeth, there is a formal grievance procedure. The SAA has never had any form of grievance procedure. There is a provision in the bylaws to excommunicate -- if somebody violates the objectives and principles of the Society the executive can decide that they can no longer be a member -- but so far as I've found there's never been any set of guidelines for how you would enact that provision, how you would discontinue someone's membership. My own view is that even if you were to put in place a very specific code of conduct and really tough review procedures and penalties of various kinds, that's not what's going to change practice. What I think is going to change practice, and *is* changing practice, is a level of awareness, education, and not just in school. I wrote a paper a year and a half ago for a special session on ethics issues in anthropology and archaeology at the [AAAS](#), that Merrilee Salmon organised, and used as a framework for it the report of a AAAS committee from 1980 on the status of provisions for ethics and guidelines and practices of all of its affiliates. The SAA was the only archaeology society listed as one of its affiliates at that time. I used this report as a baseline for talking and writing about how the SAA's history of developing and negotiating various kinds of ethics issues had unfolded. On one hand the SAA wasn't any worse than most other professional science and engineering societies, but on the other hand, it's taken until now, with the implementation of the Register, for the SAA to sign on to and support the kinds of provisions that the AAAS was calling for in the late 70s. My feeling in retrospect is that even if the SAA had done all the things, or it had in place all the things that the AAAS was recommending in 1980, that wouldn't have obviated NAGPRA.

So I wrote that paper and circulated it to a number of archaeologists (Wylie 1999). At one point I argued that my impression is that many members of the SAA, maybe a majority of people in the SAA, are increasingly concerned about

these issues. As a chair of the SAA ethics committee we mainly heard, on the one hand, from people who thought that the principles that made stewardship central just sold the whole Society down the river, that we were compromising the central commitment of the SAA to the science of archaeology and that no other interests should ever be taken to compromise that commitment. And, on the other hand, we heard from a number of people who were equally adamant in the opposite direction, that the language of stewardship was just an excuse to reassert professional control over other people's heritage. In the AAAS paper I describe those opposing, polarised stances. My sympathies actually lie with the latter, but I wanted to say that I thought there *had been* movement more generally, that most archaeologists -- even those who aren't really active on these issues, aren't taking high-profile stands, or you know, responding to the Ethics Committee -- are concerned about these issues. But when I circulated this paper I got *very* strong responses from a couple of colleagues who have been really active. They described battles they're engaged in right this minute and argued that I was being overly optimistic and was naïve about the extent to which archaeologists had taken on board the fact that there's been a sea change.

I think there *is* a change happening. But they're right; there are still people who are opposed to compromising in any way the integrity of scientific archaeology and its goals, and a lot of people who grudgingly acknowledge that they have to negotiate and make compromises but they don't like it. But there are also a number of others who started off in the grudging category but are now prepared to say that they've learned a great deal -- that new questions are being raised and that the kind of collaborative relationships they're developing, in particular with Native Americans, are really changing how they do archaeology, in positive ways. Perhaps more important, I haven't met a single grad student currently or in the last five or six years who thinks that these are issues they don't have to deal with, and that's where the change is going to happen. They're not hankering for the 'glory days' when you could just go dig wherever you wanted, or you didn't have to ask people, apart from landowners who might sue you for trespassing (laughter), when they really could presume that their interests should take priority over other interests in the record. That has never been a reality for most younger members of the field and for grad students who are growing up with this sea change, who see it just as a fact of life; as they take over the field I think you'll start to see the development of really creative strategies for doing archaeology in different ways.

kd: That's a very hopeful way of looking at it.

AW: Yeah, I can only be hopeful about it, because I think archaeology *does* have a lot to offer, even to those who regard it as their worst

nightmare. It's true, in the ways in which archaeology has been practised by some people in some contexts, maybe by *many* archaeologists in *many* contexts, it *is* the worst nightmare, it's yet another kind of imperialist imposition that's not sensitive, not respectful, it's appropriative, demeaning, generating a body of knowledge that if it has any impact at all, serves to trivialise and marginalise, and I think those are things archaeologists have to come to terms with. But that's not all archaeology could be, and it's not all it's done.

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Last Words: Some Good Reading and a Thought for Graduate Students

kd: Now for the obligatory desert island question.... any book recommendations?

AW: Well the first book that came to mind was Derrick Bell, *Faces at the Bottom of the Well*. He's an African-American legal scholar, the one who was at Harvard and said that he wouldn't return to active teaching and faculty responsibilities at Harvard if they didn't do some minority hiring, specifically of women, in the law school. Harvard refused to do it, so he left. And his books, one is called *And We Are Not Saved*, and *Faces at the Bottom of the Well*, give a very compelling analysis of racism specifically in the U.S., but also I think in North America more generally. But what's compelling about it and would make it good desert island reading, is that it's experimental writing of the most intriguing kind. He came and spoke here at the law school last year, and it was one of the most compelling public lectures I've ever heard.

kd: And what reading do you have on your bedside table right now?

AW: Well I happen to have *The Cahokia Atlas*

kd: It's a good one isn't it! The red one

AW: Yeah, the Fowler book. And a special issue of the *Wisconsin Archaeologist* which is a collection of essays that were given at a conference at Cahokia. It was a conference held at the time of an eclipse, and it's a whole series of papers on how to understand Cahokia in symbolic and astronomical terms. And a book called *Savage to Negro*, which is a history of the role

anthropology played in the construction of racism, by Lee Baker. So those are my current ones.

kd: That's a big bedside table. OK, last question – if you could tell archaeology graduate students today just one thing, what would it be?

AW: You know, when I was at Western, one of the questions I used to find hardest to answer from grad students was when -- this was philosophy students, not archaeology -- they'd ask 'so what do you think, I've got these three or four different topics, dissertation topics that I'm considering, what should I work on? What's going to get me a job?' And I remember just being dumbfounded. Here again I confronted the fact of my own privilege, that I went through as an undergraduate and graduate student in the seventies and early eighties, and I didn't ever think in terms of a *career path*, I was just doing what interested me, and it worked out. But for most of my cohort it didn't, and it might well not have worked out for me... but I just have a real hard time when somebody's getting *strategic* about what topic is going to get you a job. It seems to me that you *do* have to be strategic to a certain extent, you have to be smart about getting your work out, learning what you need to do to be a viable professional in the field. But you should only be doing any of it *because you love it*. Dissertations are hard enough to write as they are, they're a tough enough rite of passage whatever you write on. If it isn't something that really engages you at a whole lot of levels, and deeply, it's not worth doing.

kd: Alison, on behalf of Assemblage, thanks very much for all your thoughts!

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Kathryn Denning <mailto:kdenning@wwdc.com> has almost recovered from Assemblage's startup in 1996, and from finishing her Ph.D. at Sheffield in 1999, though she still feels a little sleepy some mornings. She is pretty damned delighted to be contributing to Assemblage's fifth issue, and thanks the editors for their dedication. She will begin the new millennium as a lecturer in Anthropology at the [University of Northern British Columbia](#), where she will hopefully finally learn to ski. But she misses Sheffield pubs, chip butties, and the Yorkshire sunshine.

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2- Interview with B. Ottaway

What are the contents of your pockets right now?

I don't believe this. Nothing in one, a pound note in another and, (my favourite), a miniature pen - knife which is very useful.

Have you ever had a nickname?

Would I tell you? No I wouldn't tell you.

So you have?

I have but I wouldn't tell you.

When did you first realise you were an archaeologist?

When I started to bore my friends by talking about Hittites.

What age were you?

It was thirty years ago.

What's your vision of the perfect archaeological project?

For me? A perfect copper age workshop with everything intact, absolutely everything. It would be wonderful.

Boots or sandals? Jumpers or cardigans? Tweed and corduroy or leather and denim?

I don't know. Boots and sandals I should think.

If you could pursue another career, what would it be?

I don't know - the same but different aspects of it.

What's the most outrageous or embarrassing thing you've ever done during fieldwork?

I have been trying to think about this. I'll tell you that it involved bails of hay, but I leave the rest to your imagination.

Who makes you laugh?

[Mike Parker Pearson](#), [Colin Merrony](#) but also politicians.

Wine and chocolate, or beer and chips?

Wine.

What was the first record you bought?

Something by Bach.

If you could be reincarnated, how would you like to return?

Definitely as a cat.

Why a cat?

They have a wonderful life, being stroked. They are lovely, I have a beautiful black cat, and they have a very good life.

What do students think your funniest or most annoying habits?

Misquoting proverbs, I always get them wrong.

What book do you wish you had written?

Something by Bruce Chatwin, the short stories.

What is your field walking fantasy?

Coming across the perfect workshop again. Definitely, with all the tools there, everything so that you could just read it like a book. It would be fantastic.

Which word or phrase do you most overuse?

Probable, possible, perhaps.

Name three items that you would like to be interred with?

None. I am going to be burned, cremation for me. I am not going to leave anything for archaeologists; I am going to be mean. My ashes are going to be scattered over the Galgenberg.

Which cartoon character do you most identify with?

Do you know the lost consonants in the Saturday Guardian? There are some wonderful cartoons, I really like those. I think it's me always forgetting it all.

What luxury item would you take on your desert island dig?

Soap and music, I can't think of anything else. You're only allowed one are you?

Yes.

Music then.

You would have to take the equipment as well and you would have to choose a specific record.

Oh just one, oh dear, I would get pretty bored. I think something classical and I could listen to that once a day.

What wouldn't you do for money?

Write another book, ten years excavation, never again.

What do you wish you had paid more attention to at school?

English, that's why I came here in the first place because I failed my English.

Barbara Ottaway, MA, PhD is a reader in Archaeology and is head of the [Research School](#) at the University of Sheffield. Her research interests include early copper metallurgy and the prehistory of central and south - eastern Europe. Her field projects include excavations and surveys in southern Germany and Austria. She has recently published 'A Changing Place: The Galgenberg in Lower Bavaria from the fifth to the first millennium BC', BAR International Series 752.

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