

Archaeology and Critical Feminism of Science: Interview with Alison Wylie

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A discussion of questions about philosophy and archaeology; contextual ideals of objectivity and the role of non-cognitive values in science; what's feminist about feminist research; feminist standpoint theory and the relevance of feminist analysis to science.

Interviewer 1: First, we would like to know about your trajectory and your studies in the university and how you started your interest in archeology and in philosophy of archeology.

Alison Wylie: I'm Canadian originally and because my father was military we moved every few years, mainly within Ontario and Quebec. One constant, wherever we lived, was that my parents had a passionate interest in archeology. When based in Ontario my father and one of his colleagues would get funding from the Canadian National Museum to explore Iroquois and Huron sites along the Saint Lawrence River. So most summers as a kid I spent several weeks on archeology sites. That experience didn't exactly foster a love of archaeology. The work was hot and dirty, and we children were typically sent off to dig in areas on the edge of the site that everyone expected would be sterile; the adults didn't like to let the children excavate anything that might be important (sensibly enough) and this was a way of testing the extent of the site. Whenever we found something interesting, that would be the end of us excavating; our revenge was to rummage through the back dirt to see what the grown-ups had missed in the course of their excavation. As much fun as that was, it left me with a pretty realistic impression of how tedious field work can be. That was my introduction to archeology.

It wasn't until much later that I realized the significance of the work that my father's colleague, Jim Pendergast, had been doing. From the mid-1960s he worked with an eminent Canadian archeologist at McGill, Bruce Trigger, on questions about contact period interactions between tribal groups in the St. Lawrence Valley and incoming Europeans, reconceptualizing them in quite profound ways (Wright and Pilon 2004). Trigger later published an article that was an indictment of the racism of archeological work in Canada and the U.S. which presumed that Native Americans were static, culturally conservative groups that were just reacting to the incoming Europeans: "Archaeology and the Image of the American Indian" (Trigger 1980). Without diminishing the violence of displacement and appropriation, Trigger was intent on documenting the ways in which tribal groups were active agents, savvy and self-determining negotiators in their dealings with Europeans. The collaboration between Trigger and Pendergast resulted in a number of joint publications, including a 1972 book, *Cartier's Hochelaga and the Dawson Site* (see Trigger 2006).

As an undergraduate, I went to a small college in New Brunswick, Mount Allison University. At the time, they offered no archeology but when I started coursework I wasn't particularly interested in studying archeology; instead, I discovered a passion for philosophy. It seemed that whatever class I took I gravitated to philosophical questions without knowing quite what those were: in English classes I was intrigued by moral dilemmas and intellectual crises, and in History and Classics I was fascinated by the history of ideas. I took a wonderful introductory course in Philosophy that first year (1972-1973) taught by Paul Bogaard who was a philosopher of chemistry, and in my second year I enrolled in his History and Philosophy of Science course.

The summer after my first year in college I needed a job, and was lucky enough to get one working for the archaeology division of Parks Canada. The only relevant experience I had was summer excavations directed by Jim Pendergast who, by that time, had retired from the army and joined the National Museum; I'm sure I have him to thank for convincing Parks Canada that I was worth the risk despite having no formal training in archaeology. I was assigned to Fort Walsh, a late nineteenth century North West Mounted Police (NWMP) site in southwest Saskatchewan, close to the Montana border on the south and to Alberta on the west. It was in that context that I learned about the "New Archaeology" from Jim Sciscenti, the Fort Walsh project director. He had been immersed in a hot-bed of New Archaeology activism at the University of Arizona where he'd done his graduate work, and required anyone who

worked at Fort Walsh to prepare for the field season not only by reading up on NWMP history and local archaeology, but by working through a good long list of contemporary philosophy of science. The following Fall, when I returned to college and took my first course in history and philosophy of science I read many of the same classics over again: contemporary logical empiricists and their antecedents (Mach, Poincaré, Carnap, Hempel), and soon-to-be classics like Kuhn's *Structure* and Hanson's *Patterns of Discovery*. Thanks to Bogaard I began to articulate a set of questions about how philosophy of science bears on archeological practice that have been central to my interests ever since.

So those are the accidents of personal history that brought me to this unlikely intersection of interests. I soon discovered that there philosophers already working on issues raised by archaeology: first and foremost, Merrilee Salmon. When I was ready for graduate school I had the good fortune of finding an interdisciplinary program in the History and Philosophy of Social and Behavioral Sciences, at the State University of New York at Binghamton. The great virtue of that program was that it made it possible for me finally to do coursework in Archeology alongside a Ph.D. in Philosophy.

As unusual as this trajectory is, the combination of philosophy of science and archaeology has proven to be really generative. When I work on a particular issue sometimes I start with a paper that's an intervention in an archaeological debate where philosophical questions have been raised and then bring what I learn from the archaeology back to a philosophy audience. Early on I did work on analogical reasoning, initially as a response to some hard-line positivists in archaeology (the New Archeologists of the early 1970s) who had rejected all use of analogy as unscientific and speculative, fit only for generating hypotheses. I argued that well crafted analogical argument could carry evidential weight, and that even its most uncompromising archaeological critics relied on it in contexts of justification as much as of discovery (Wylie 1985). I then went on to develop a paper that was a response to philosophical debate about the role of analogy in science of science that had been opened up initially by Mary Hesse's *Models and Analogies in Science* (Hesse 1970; Wylie 1988). In other cases I start with a philosophical question and find it productive to address it through analysis of an archaeological case, sometimes bringing the results back to archaeology. So from the outset the work I've done in this interfield typically arises from a process of moving back and forth between these two fields; that dynamic continues to shape my work. How's that for trajectory?

Interviewer 1: I'd like to know just two more things about your trajectory. One: you spoke briefly about the New Archeology when you were in Canada, in Parks Canada. We saw that during your studies at the university, the archeological field was undergoing some transformations. Can you describe very briefly what these changes were? And another question is: Did this context encourage you to be interested in gender issues and, if it didn't, what motivates you to put gender archeology and feminist critique of science as a central piece of your work?

Alison Wylie: From the 1960s through the 1970s an aggressively pro-science movement took shape, at least in anthropological archaeology,¹ that came to be known as the New Archaeology. The acknowledged leader of the New Archeology was Lewis Binford, an enormously charismatic figure and a pugnacious critic of what he described as "traditional archeology." His "fighting articles," as he later called them, crystallized the frustrations of archaeologists, especially younger cohorts just entering the field at the time, who felt that the field was mired in the business of collecting data as an end in itself, at most teasing out spatial, temporal, and formal patterning in the record, but rarely making effective use of these data as evidence to answer questions about the cultural past (Binford 1962, 1972). This was not an altogether fair appraisal of the archeology that went before. For one thing, when I traced back the history of debate I found a recurrent pattern of complaint along exactly these lines, beginning in the late nineteenth century when, as archeologists professionalized, they distanced themselves from antiquarians, from collecting for the sake of collecting. The central question, restated again and again through the twentieth century, is: how do we use the wealth of material data recovered from the archaeological record as historical, anthropological evidence? How do we do more than just describe what we find in the ground? By the mid-twentieth century the critique was turned inward; antequarians

¹ As opposed to archaeology taught and practiced in the tradition of Classics or Art History, for example.

weren't the problem so much as professional archaeologists who were preoccupied with "space-time systematics." Archaeological claims that seemed ostensibly to be about past cultures were often just re-descriptions of empirical patterns observed in the record. Low-level generalizations about the distribution and change over time of distinctive artifact assemblages – for example, the European Neolithic pottery referred to as "bell beaker" ware – might be described as the evolution and movement and interactions of culture-bearing community – the "Beaker people." Binford was renewing these earlier calls for a type of archaeological practice that could contribute substantive understanding of the cultural past but, to crystallize what was new about the New Archaeology, he invoked logical positivist, logical empiricist accounts of science associated with Carl Hempel. The New Archaeology was to be a self-consciously scientific research program that took explanation as its primary goal and made hypothesis-testing its hallmark mode of practice. These explanatory goals were characterized in terms of Hempel's deductive-nomological covering-law model, and hypothesis testing in terms of his hypothetico-deductive model of confirmation.

A number of attempts were made to give Binford's deductivist vision clearer articulation by philosophically-minded archaeologists like Patty Jo Watson; she co-authored an early primer for the New Archaeology, *Explanation in Archaeology: An Explicitly Scientific Approach* (Watson, LeBlanc and Redman 1971). But ultimately, it was not clear how exactly this was supposed to work in practice. A pattern typical of many New Archaeologists was that they'd start their articles with a programmatic statement about what it means for archaeology to be a real (positivist) science, but what they actually did bore little resemblance to the D-N and H-D models they invoked. Even Binford, when he got down to business, more often used the language of causal process rather than laws to describe the goals of an explanation-oriented archaeology; he insisted that the goal of a scientific archaeology should be to get at large-scale, long-term cultural processes. (The New Archaeology is also referred to as "processual archaeology.") In Binford's hands this processualism took the form of a quite reductive functionalist and eco-materialist orientation. He argued that we should understand culture as "man's extra-somatic adaptation" to material environments, and ridiculed any preoccupation with "paleo-psychology" – with the intentions and beliefs of past actors or with past cultural lifeworlds – as hopeless and unscientific. Archaeologists should focus, instead, on how various kinds of system-level dynamics – manifest in distinctive assemblages of material culture, inferred subsistence practices, large-scale patterns of social organization – ensure the adaptive viability of cultures, and how interactions with the material environment shaped the trajectory, the evolution of different cultural systems. Those were some of the dominant themes in the New Archaeology literature that I read in the 1970s.

In pushing this program, Binford claimed that what archaeology needed was a Kuhnian revolution to become a properly positivist science. Even as a sophomore taking my first course in History and Philosophy of Science, the irony of this was inescapable; Kuhn's account, I learned, was meant to displace exactly the positivist models Binford saw as defining what it was to be a real science. So, by the end of my second year in college I began to grapple with what became the question central to my dissertation: "what can the New Archaeologists possibly mean by insisting that they were positivists?". I ultimately wrote a thesis entitled, *Positivism in the New Archeology* (1982); it was an analysis of how positivist rhetoric – especially its foundationalism – mischaracterized the innovative epistemic insights that were emerging in the problem-oriented practice of the New Archaeology. There was a conceptual fault-line running through the New Archaeology, I argued, and by the early 1980s it was clearly visible. Just as I was finishing my Ph.D., a contingent of "post-processual" archaeologists, mainly British, began to publish a series of trenchant critiques of the New Archeology, targeting both its epistemic positivism and its eco-materialist conception of culture. Some of them pushed the critique of positivism to its limit, taking a strongly relativist, social constructivist line. If archaeological evidence is never a given, if it is always, necessarily interpreted in light of laden theory, then it cannot be expected to provide a test of interpretive or explanatory hypotheses; testing is inevitably circular. I thought that this critique, as developed in the mid- to late 1980s by Ian Hodder, and his students Michael Shanks and Chris Tilley, for example, didn't capture the potential or the limitations of archaeology any better than Binford's positivism (Hodder 1984, Shanks and Tilley 1987). In the event, the critics of the New Archaeology didn't hold this position for long.

As *social* constructivists, many post-processuals were intent on challenging certain kinds of archeological claims; they were politically reflective and wanted to show that mainstream archeology routinely reproduced ethnocentric – sometimes explicitly nationalist, classist, elitist – presuppositions about what the past had to be like that were just wrong. In the most telling of these critiques they made savvy use of empirical evidence to expose the weakness of hypotheses that had enjoyed widespread acceptance because they conformed to expectation. Clearly, in practice these post-processuals didn't buy corrosive relativism any more than Binford did the stringent foundationalism and deductivism that he sometimes endorsed when trashing "traditional" archaeology. In fact, Binford put enormous emphasis on the need to build and test the "middle range theory" – the auxiliaries or background knowledge – on which archaeologists rely to interpret their data as evidence, so he didn't assume that archaeological evidence is in any way self-warranting. He also rejected seemingly naïve "inductivist" approaches: the idea that archaeologists should explore the archaeological record without any explicit agenda. In advocating what he called a "problem-oriented" approach Binford recognized that archaeological inquiry is always informed by some conceptual framework or other and, although he rarely cited them, he renewed explicitly anti-empiricist arguments that had been made a generation earlier for taking responsibility for these presuppositions (Kluckhohn 1939, 1940). He insisted that archaeological research should be deliberately designed to test not only archaeological hypotheses about the past, and also the background assumptions on which archaeologists rely to interpret data as test evidence. I thought that this was an especially important core insight of the New Archeology, one that was not at all well captured by hypothetical-deductive testing models and that was also a central motivation for post-processualists, despite all their other differences. I've worked on issues raised by the fast-moving debate between processual and post-processual archaeologists as they've taken shape, so the conflict I encountered in archeology when I started out has very largely defined the later trajectory of my work.

The feminist work I've done arose mainly from activism, through working on equity issues, workplace environment issues (sometimes called "chilly climate" issues) for women in male-dominated fields like most areas of academia, and on issues of violence against women. When I began teaching at the University of Western Ontario in the mid-1980s I got involved with what was then the Battered Women's Advocacy Clinic (BWAC). I served on the BWAC research committee, working to understand better who was being battered and using our findings to challenge prevalent stereotypes that tended to isolate and blame those who were victims of violence. BWAC was a resource for women who wanted to address the violence in their lives but, unlike most such agencies at the time, it wasn't a shelter; it provided short-term counseling, legal referrals, support for finding housing and jobs. We found that the demographic profile of the women who came to BWAC was exactly that of the average Canadian woman, by educational background, employment, income level, number of children, marital status, and so on; most of them had never made use of shelter services or seen police intervention so they didn't show up in the databases on which claims about battered women were typically based. We were able to show that the standard rhetoric of the time – like that only poor women, or immigrant women, women who were otherwise marginal were victims of domestic violence – didn't stand up to scrutiny. The results from BWAC were one basis for the arguments that brought about changes in the response to domestic violence in Ontario in the late 1980s.

A few years earlier, when I was a post-doctoral fellow at the University of Calgary, I had begun to read feminist philosophy. I'd been asked to teach a broad-spectrum course on "Women and Philosophy" that had just been approved; a senior philosophy of science colleague, Marsha Hanen, had got it on the books but then became Dean and couldn't teach it. I protested that I had no training in feminist philosophy and didn't know the field, but she insisted: "you can do this; you have to do this." It was one of the most productive, transformative teaching experiences I've ever had. What was in print at the time (Winter 1985) was predominantly work in feminist ethics and political theory so I read a lot of that, some of it on pornography and sexual violence that was directly relevant to the work I did with BWAC; I later published a few pieces on feminist methodology anchored in the BWAC research (Greaves and Wylie 1995). But, in addition, the Harding and Hintikka collection, *Discovering Reality* (1983) had just come out and included a number of papers by Evelyn Keller, Nancy Hartsock, and Sandra Harding that have since become classics of feminist philosophy of science. In the course of teaching that class I also discovered Helen Longino and Ruth Doell's early paper, "Body, Bias and Behavior" (1983), a feminist response to just the kinds of questions about the stability of evidential claims that I was wrestling with in archaeology. That's

when I began to think about what it would mean to bring a feminist lens to bear on the issues in philosophy of science that concerned me.

It wasn't until 1989, the Chacmool conference on *The Archaeology of Gender* (University of Calgary), that my feminist and archaeological interests converged in a direct way. I'd been puzzled that, although I knew a number of archeologists who were self-identified feminists – like Meg Conkey, who I'd studied with as a graduate student – there had been no very visible formation of a feminist or gender research program in archaeology, like those that had taken shape in sociocultural anthropology and in history in the 1960s. This was especially striking in North America where archaeologists are typically trained in departments of anthropology so archeologists who were in graduate school in the U.S. and Canada in the 1970s and 1980s would have taken courses in sociocultural anthropology at a time when feminist perspectives were having a real impact on the field. And yet somehow none of this got traction in archaeology until the late 1980s. One catalyst was the 1989 Chacmool conference, a large public meeting that drew participants from around the world, and another was a smaller working conference, "Engendering Archaeology," that preceded it in 1988, organized by Joan Gero and Meg Conkey. These resulted in publications in the early 1990s, like *Engendering Archaeology* (Gero and Conkey 1991), that laid the foundations for what came to be known as "gender archaeology." I was enormously lucky to have had the opportunity to participate in these meetings; I got to see first hand the formation of an exciting research program in archaeology – one in which archaeologists were taking up exactly the issues I'd been thinking about in the context of teaching feminist philosophy and doing activist work on feminist issues.

Interviewer 2: In your discussions on standpoint theory, you propose a new form of objectivity that must be contextualized and also be critically reflexive, confuting an orthodox or positivist account of impartiality and also the relativist view. Is the objectivity that you propose universal? If it's not, what is, in your opinion, the difference between a contextualized and a relativist form of objectivity? And then there's another related question. From your standpoint, is the objectivity of archeological theories a consequence of social and political interactions among researchers or is it constituted by those interactions? What we are trying to grasp here is the way that objectivity can be modified to include feminist standpoints.

Alison Wylie: There's been ongoing debate among feminist and critical race theorists about whether we shouldn't just ditch the language of objectivity altogether because it carries so much baggage – in the form of the kind of orthodox universalizing ideal you refer to – or whether we should, instead, keep talking about "objectivity" and reconceptualize it in terms that do what you describe: that capture a robust ideal of epistemic success but have the resources to make sense of the contributions of situated knowers, including politically motivated and ethically engaged epistemic agents. The sort of generic conception of objectivity that's typically invoked in archaeology, for example, trades on an assumption that cognitive and non-cognitive, social values can be sharply distinguished in something like the terms Hugh Lacey defends, but it doesn't disentangle impartiality from neutrality and autonomy in the sophisticated way Lacey does. Objectivity is conceptualized in terms that read out of account any choice of "strategy," as Lacey refers to it; there's no room to recognize the role of background assumptions, goals or contextual factors in setting the framework within which a research program unfolds. It's assumed that there is must be some self-warranting foundation of evidence and some universal set of norms that define what counts as rationality – as good reasoning from empirical evidence – regardless of research goals or strategy, otherwise all is lost. Even though no one has been able to nail down exactly what these come to, the fear is that if we abandon faith that our knowledge is grounded in unimpeachable empirical foundations or universal norms of rationality, we have no choice but to accept self-undermining relativism. There must be some set of standards such that, when they're met, knowledge claims can be accepted as "objectively true" full stop: they hold trans-contextually, trans-historically; they approximate to the proverbial "view from nowhere," a view that is not inflected by any local interests or situated values, any of the choices that set the terms of a research strategy. This set of ideals typically presupposes a highly abstract and individualistic conception of epistemic agency. Objective knowledge will only be realized if epistemic agents can transcend the push and pull of contextual factors. The social relations that make it possible for them, as individuals, to know anything, that constitute them as epistemic agents, are systematically disappeared because any contextual, non-cognitive factors are assumed to contaminate epistemic autonomy and impartiality. What a properly objective agent knows is just what any rational agent would know, given the evidence and proper exercise of reason.

This radically decontextualizing ideal of objectivity has done important work as a basis for contesting various forms of epistemic dogmatism and authoritarianism. But as Lorraine Daston and Peter Galison show in *Objectivity* (2007), it is of late and highly specific origin. It is a creature of the late nineteenth century, arising in scientific contexts where mechanical technologies of recording were instituted that displaced the skilled craftwork of expert observers and experimenters, producing data that seem impervious to human bias because they appear to be generated without human intervention. Daston and Galison argue that this formulation actually inverts earlier ideals of objectivity which were originally associated with ethical and aesthetic judgment; transposed to a scientific context the really objective knower was the highly skilled scientific observer who could hand-draw botanical specimens, for example, capturing their essence as representative of their species. It was a discerning knower, not a disengaged knower who could guarantee objectivity. In short, objectivity as a concept has been understood in a great many different ways.

One way of thinking about objectivity as a norm and a concept that takes this historical perspective seriously is to see it as an honorific: when you say that a knower or an item of knowledge is “objective” you are recognizing epistemic success. But what counts as epistemic success is defined, not in terms of some context-transcendent set of standards, but in opposition to whatever form of epistemic failure is most salient, most feared in a given context. The epistemic virtues that are prized, and valorized as marks of the objective, are those that are seen to be proof against a particular set of epistemic failings. As this line of argument is developed by Jill Fellows, in a recent Ph.D thesis (2011), if the dominant worry is that knowers will distort or misrepresent the data they report in ways that reflect their interests and preconceptions – whether this is conscious or unconscious – then a decontextualizing conception of objectivity that emphasizes mechanisms for counteracting this kind of bias will be especially compelling. If the kind of epistemic failure that matters is an inability to grasp an objective reality that lies beyond what ordinary, common sense makes available to us – the reproduction of ignorance – then the virtues of skilled, elite observers is likely to be emphasized in dominant conceptions of objectivity.

The point is, then, that ideals of objectivity are themselves context-specific and continuously evolving. Nobody has succeeded in identifying skyhooks (in the form of universal norms of rationality) or epistemic foundations (in the form of empirical data, sensory inputs) that are sufficiently robust to stabilize a universal ideal of objectivity. So rather than continuing the quixotic search for a viable articulation of the traditional (universal) conception of objectivity, I recommend that we take this historical lesson on board and ask what particular epistemic virtues are most salient in the contexts in which we are actually producing, ratifying, and relying upon knowledge claims. I reject the assumption that, if we don’t persist in the quest for universal epistemic ideals, the only alternative is to accept a hyper-relativism that undercuts any principled epistemic adjudication of knowledge claims. What I’m urging is that we get serious about what makes for better or worse knowledge in particular contexts of inquiry and for particular purposes. In fact, we have excellent resources on which we can draw to calibrate the epistemic virtues we invoke and to assess the effectiveness of specific procedures for realizing them. It’s a matter of making use of all the strategies we rely on when we want to figure out whether a knowledge claim is going to be a good basis for action and applying them not only to first-order objects of inquiry, but to scientific practice itself. This means treating the sciences as a form of situated practice, being explicit about their goals and how, in particular contexts, the epistemic virtues that inform judgments of epistemic success will be interpreted and applied.

So what I’m recommending is that we frame ideals of objectivity in rigorously contextualized terms: that we make explicit the virtues that have been entrenched as proxies for objectivity in various research programs, subject them to critical scrutiny, and hold them accountable to specific purposes and uses. This is what I have in mind in the last section of the paper I gave yesterday on gender research in archaeology: that its advocates should be prepared to challenge the traditional ideals of objectivity invoked by their critics. There are alternatives to untenable epistemic foundationalism and universalism that don’t involve embracing self-undermining relativism.

Interviewer 2: Yeah. From your standpoint, is the objectivity of archeological theories a consequence of social and political interactions among researchers or is it constituted by those interactions?

Alison Wylie: Is there any way to distinguish these possibilities? Consider Longino's procedural account of objectivity: that we ratify as objective those knowledge claims that arise from the right kind of collective process of critical scrutiny. On one reading this is an argument for being explicit about the fact that, in the end, we are only ever in a position to assert that, "under these (current) best possible processes of deliberation, these are the knowledge claims that we ratify as trustworthy, action-supporting"; there is nothing more we can add when we valorize a knowledge claim as "objective." I would add to this a further contextualizing claim: that we only ever ratify knowledge claims as useful or trustworthy for particular purposes, not as "true" or "objective" or reliable, full-stop. So objectivity is both constituted by and a consequence of the procedures we've developed to build, test, assess, and ratify or reject knowledge claims.

But I take it that what's behind your question is a concern that, on a procedural account, there's no basis for ever making the case that a claim ratified as objective by a community – following its own best practices – is not, in fact, objective. I don't see that this follows. In fact, characterizing objectivity in proceduralist terms signals a recognition of the fallibility of even our best current knowledge. It puts us in a position to make the retrospective judgment that a community process of deliberation has gone badly wrong. I see at least three possible grounds for making the judgment that knowledge claims that have been ratified as objective at one point have later proven to be limited or distorted or unreliable. One is that the community did not follow its own best practices; the process of ratification was flawed, often because of the play of non-cognitive values – social and political factors – that insulated favored views from criticism. Another is that the community ratified knowledge claims that were fit for then-current purposes, but these purposes changed; risks of error that seemed acceptable at one point are later found to be unacceptable, either on empirical grounds or for social, pragmatic reasons. And a third is that the community standards themselves are found wanting; critical scrutiny may reveal that they're systematically biased in some previously unrecognized way, or methodological refinements in a research tradition may raise the bar epistemically.

Feminist critiques of science are a rich source of negative object lessons of all three kinds. Lisa Lloyd's book on *Bias in the Science of Evolution* (2005) is a sobering catalogue of missteps by which a research community accepted on faith assumptions that fit with their colloquial wisdom about women's sexual response and their selectionist presuppositions, failed to apply their own standards of empirical adequacy to these assumptions, and vigorously defended the results against all criticism. But often the most worrisome type of error is more subtle, more inadvertent. A well-intentioned research community might do an excellent job of rigorously testing a set of hypotheses that all presuppose a sexist or racist or classist conception of their subject matter; they provide good reasons for ratifying one as the best on offer, but they don't consider any alternatives that lie outside this limited conceptual framework. Crucially, they might not even be aware that they're making these presuppositions, they're so much taken for granted, with the result that the claims they ratify have tremendous staying power. As on Lacey's account of research strategies, a shared understanding of the goals of inquiry comes with substantive assumptions – for example, assumptions about the nature of the subject domain, and along with these, about what counts as appropriate evidence and salient critique. The result is that the research community very often doesn't pursue lines of inquiry that could produce counter-evidence, and often doesn't recognize them as legitimate when pursued by others, so its members aren't responsive to criticism they should take seriously. If admission to the research community is conditional on internalizing these shared norms, there may be no one with standing in the community who has the critical resources to recognize what's being assumed, and what's being left out, distorted, misrecognized as a consequence.² In these cases, it may only be when social conditions shift and outsider perspectives are brought to bear that the limitations of entrenched norms become visible. Perhaps political action puts pressure on a research community to rethink its framework, as in the case of the women's health movement and AIDS/HIV activism, or the range of perspectives represented within the community shifts with the influx of women and underrepresented minorities. I discuss a range of such examples in my APA Presidential address, arguing that they have been a key catalyst for the development of feminist standpoint theory (Wylie 2012).

² This is a point made with particular clarity by Douglas in *Science, Policy and the Value-free Ideal* (2009: 172-173).

So I do want to defend a conception of objectivity that doesn't insulate whatever comes out the other end of a deliberative process from critical appraisal. But the key to understanding how it is that we can take distance from procedural norms and recognize error in claims that have been ratified as objective is not to invoke transcendent, universal standards and foundations. It is, rather, to subject these procedures and the norms themselves to the kind of critical scrutiny that can ensure not only that they are, as I said earlier, fit to purpose, but also that they are continuously updated in light of what's learned from experience about their reliability. In short, I would argue that you can expose error and bias that arises from socio-political interactions without appealing to an idealized conception of objectivity. But this requires you to recognize that non-cognitive factors are not just a source of compromising bias; they are also enabling. Indeed, often they are the key factor that makes transformative criticism possible. What's needed are community practices that make standpoint-specific interests explicit – that put them on the table for debate – rather than disappearing them behind a screen of presumed neutrality. What I argue for is an account of “strong objectivity,” as Sandra Harding refers to it (1991), articulated in proceduralist terms along lines suggested by Longino (2002).³ Spelling out how this works will require enormously detail-intensive case studies of actual practice and a great deal of second order research on what makes for reliable deliberative processes – ones that build in consideration of context and are subject to continuous updating. That's a challenge you all will need to address; it's down to you.

Interviewer 2: Another question is how does this notion of objectivity interact with similar ideas developed in the 1960s in Philosophy and the Social Sciences and especially in those studies about the effects of domination structures of class and race on the production of knowledge? What are the specificities of a feminist critique to science?

And then I would also like to ask you if you can tell us about some cases or evidences of violence and / or inequalities / inequities among genders in Archeology.

Alison Wylie: As a subject of Archeology...

Interviewer 2: Yeah...

Alison Wylie: OK, so the first question: connections with earlier conceptions of objectivity that have already been articulated by philosophers and social scientists. There certainly is a strong resonance between the contextual account of objectivity that I'm exploring – especially as formulated by feminist standpoint theorists – and what critical theorists of various kinds have had to say about the production of knowledge, especially those working within historical materialist, Marxist traditions of thinking about knowledge. The point of connection is a shared recognition that what knowers are likely to be attuned to, what they have access to observationally and what use they can make of evidence is deeply conditioned by the social, material conditions of their lives. I've described this as a structural “situated knowledge” thesis (2003, 2012). In a Marxist tradition claims about the situatedness of knowers were cashed out in terms of a substantive theory of class structure, which also gives you a very strong “inversion thesis”: the claim that, by virtue of their class position, the proletariat are most likely to understand exactly how profit is generated, to see relations of production for what they are and to recognize their exploitative effects – realities they know from their lived experience that are likely to be inscrutable to those who benefit from an exploitative and hierarchical system. The challenge for feminists was to make sense of how gender could constitute a standpoint that sustains something like these situated epistemic advantages; I'm thinking of Hartsock's 1983 paper, “Developing the Ground for a Specifically Feminist Historical Materialism.” Hartsock, and feminist standpoint theorists of the 1970s and 1980s generally, were immediately accused of being essentialists: taking gender identity as a given and as foundational to all other structural difference. I don't think Hartsock does any such thing.⁴ As an historical materialist she emphasizes the contingency of sex/gender systems and their epistemic effects. And although the

³ This may seem counter-intuitive, inasmuch as Harding originally described feminist standpoint theory in contrast to feminist empiricism. See Intemann (2010) for an account of how these positions complement one another.

⁴ I make this argument in my Pacific APA Presidential Address (Wylie 2012).

language of intersectionality wasn't much in use until the 1990s, she is clearly thinking in intersectional terms; she's asking "how do class and gender and race and ethnicity co-constitute one another?" – contingently, in specific historical contexts – and what impact does this have on epistemic capacities? This gives you a much more complicated picture of standpoint theory than is generally credited. It means that you can't appeal to a particular location that automatically generates privileged insight. But you can argue that, if these social divisions make a difference to our material conditions of life, who we interact with, how we interact, what work we do, what training we get – how could they not make a difference to our capacities as knowers? Even if these social structures are contingent and intersectional, they nonetheless have very real epistemic effects – effects that are not just idiosyncratic. The burden of proof should be on those who reject standpoint theory to show that the structural conditions that shape our lives have no systematic impact on our capacities and standing as knowers.

These days we can draw on a rich body of empirical psychology that documents how we internalize cognitive schemas that track social inequalities, and how these shape our expectations – and our observational and inferential capacities (Valian 1999). Yesterday I mentioned an annual review article on "Gender in Psychology," co-authored by Abby Stewart (Stewart and McDermott 2004). She makes the point that what we're internalizing with gender schemas is a set of heuristics for navigating power relations so, not surprisingly, those who are in subdominant positions in a workplace know a lot more about their superiors than they do about them. They have to, if they're going to successfully navigate a hierarchically structured social landscape where they're at a disadvantage. Patricia Hill Collins uses the example of what housekeepers know to illustrate this kind of insider-outsider knowledge (1991). Black women domestics – the focus of Collins' discussion – are disadvantaged in social contexts that are elite by race as well as class, and they're further discounted as knowers because of their gender, but they have to understand the psychological profiles, the motivations and power dynamics of those who are insiders to this elite world, often much more acutely than do those they work for. I made use of a particularly vivid example of this that figures in a murder mystery by Barbara Neely called *Blanche on the Lam* (1992). Neely has her character Blanche tell you exactly what she knows about the white community and how she knows it: that she has to be wise and alert, and also that she has all kinds of opportunities to hear and observe, not only because she takes out the garbage and makes the beds, but because she's invisible to her employers; they carry on their lives in her presence, making her privy to things they would never do or say in public, or in the presence of others in their social world. I gather that mystery enthusiasts find the *Blanche* mysteries pretty pedantic but for just this reason they were invaluable for me (Wylie 2003).

The same kind of point is made by Uma Narayan in terms of the example of line workers in a factory in the Maquiadora district: what they know about how profit is extracted from their labor that managers and owners, and certainly consumers, typically don't know. But Narayan also contrasts the epistemic advantage evident in these kinds of cases with the disadvantages that are also imposed by oppressive social conditions (1988). These are, then, the kinds of examples of differential access to knowledge, of contingent epistemic advantage, that feminist standpoint theorists draw on and that resonate with the sociological and philosophical literature that I think you're referring to.

Interviewer 2: Yes, but I would like you to talk about violence and inequities as a subject to archaeology.

Alison Wylie: In Archeology... this takes us back to the question you raised earlier about how a gender archeology can illuminate inequalities in the past and how this can be put to work addressing inequalities in the contemporary world.

One of the things that's wonderful about archeology is that, hard as it is to work with archeological data – and it's really hard – you can sometimes tease out lines of evidence that make it impossible to maintain the comfortable mythologies about the past that underpin our sense of self, our place in the world, our cultural and national identities, and our social relations, including gender relations. So I'm answering the second part of your question first: archeology can be a powerful resource for challenging what we take for granted. It can show us that key features of the contemporary world that we treat as inevitable haven't always been this way, that they're contingent and could have been otherwise, that they have histories and consequences that are sometimes starkly at odds with the values we think they embody, and that there's

a much wider range of possibilities for living humanly rich, productive lives than we'd imagined. These insights may not tell us how to effectively intervene in the present, but they can quite powerfully destabilize assumptions about the past – how things have always been, why they have to be as they are – that legitimate contemporary structures of inequality.

One example of this is the archaeology that ultimately got done on the New York city African Burying Ground in, a 17th-18th century site with over 400 burials that was discovered in 1991 when the U.S. General Services Administration (GSA) began building a new federal office building in lower Manhattan. The GSA had got approval to proceed; the necessary background studies evidently showed that there weren't like to be any archeological remains on the site that would need to be protected. Even though there was lots of historical documentation that a large non-consecrated burying ground and public space had existed in the area, there had been so much construction that it seemed unlikely that any original cultural deposits survived. So it was a real shock when excavation for the foundations began to expose human skeletal material, and not just a few isolated burials: over 400 burials, when all was said and done. Somehow the GSA had missed the fact that there was a back alley in the area where the historic African Burying Ground had existed that had never been built on. This turned out to be the oldest Colonial era cemetery of its scale that had been found, and it was unique in that it served African American slaves and Native Americans who were excluded from consecrated church cemeteries; it is an extraordinary archeological resource. The federal government sent in a forensic crew to recover the remains as they were exposed; they didn't want to stop the construction project. This generated a huge public outcry, and what was at issue was not just their failure to protect the site, but the fact that the forensic team was just going to do the type of race, gender, age profiling of the human remains that they use for crime victims. This was a strikingly limited research program and one that was inherently flawed, inasmuch as they didn't have the skeletal reference collections they'd need to do even this with any accuracy. The GSA was forced to stop excavation, and ultimately appointed Michael Blakey, one of very few African American physical anthropologists who was then at Howard University (a traditionally black college outside D.C.), to develop a research plan and direct a project worthy of the site.

A vast amount of data has been recovered and analyzed and published at this point,⁵ and the research Blakey oversaw included a whole battery of sophisticated studies designed to get at the social and material dimensions of the lives of those buried in this cemetery: macro-skeletal analysis for markers of disease, dietary stress, activity patterns; isotope and trace element analysis that makes it possible to reconstruct lifetime dietary profiles; analyses of the artifacts buried with human remains, and of the layout and structure of the cemetery. From the dietary profiles they could identify individuals who were born in Africa and had spent time in the Caribbean before they were transported to New York. Some also had evidence of teeth filing typical of African origins; Blakey and his team drew on Caribbean and African traditional knowledge to interpret these features and also the symbolic significance of beads and other artifacts as elements of, for example, Yorùbà traditions that had survived the Middle Passage. So that's evidence of the Atlantic slave trade in full swing, forcibly removing people from Africa and shifting them from Caribbean and southern plantations to industrial labor contexts in the colonial northeast. But it's the bone analysis where you see graphic evidence of violence: gunshot and stabbing wounds in some cases, and also striking evidence of the everyday violence of industrial slavery. So, for example, massively developed muscle and ligament attachments bear witness to the repetitive, heavy physical labor that went into building virtually all of lower Manhattan in the area that's now the famous Wall Street financial district. And it wasn't just adults. I remember Blakey describing a cervical fracture in the skeleton of a 12-year-old boy, most likely a consequence, he argued, of carrying heavy loads of building material on his head as a porter or laborer.

So archaeological evidence of violence and radical inequality – slavery – is quite literally written in the bones to those whose remains were excavated from the African Burying Ground. This was really significant, not just archeologically but also as an intervention in the political discourse about racism in New York. It disrupts any comfortable dissociation of the U.S. northeast from the realities of colonial era slavery; it challenges the assumption that, because the North fought for abolition in the civil war, it wasn't

⁵ For an overview, see Blakey (2011). The full report is available online at: <http://www.gsa.gov/portal/content/249941>

implicated in the slave trade, or that slavery was just a regrettable aberration in the history of the U.S., confined to the agricultural South and its plantation system. I'm reporting what I hear, as a Canadian, from friends and colleagues who describe what they learned in grade school; here was shocking, tangible evidence that the core of New York city was built by slave labor in the 17th and 18th century. Not that this was unknown to historians, of course, but it definitely got public uptake in the 1990s when the African Burying ground was in the news.

Gender archaeology also has lots of potential to challenge norms that sustain oppressive inequalities, in this case sex/gender systems. An early example, from the first Australian conference on *Women in Archaeology*, that made use of some of the same types of analysis as the African Burying Ground an argument Denise Donlon made for restudying Australian skeletal remains (1993). The puzzle here was that the sex ratio was radically skewed in the direction of males. After canvassing various explanations for this, Donlon suggested the possibility that at least some female skeletons were being mistakenly classified as male because of the robustness of their muscle attachments; attributions of biological sex depended on conventional models of sex dimorphism that assumed Eurocentric gendered divisions of labor, ignoring the fact that these were much less marked in Aboriginal communities (1993). So one running theme in the archaeology of gender is captured by the title of the early Norwegian collection of essays, *Were They All Men?*. I remember Liz Brufiel saying that what we need is a usable history for women and girls today – and a history that renders them literally invisible is certainly not that.

Another running theme is that when women and gender do figure in archaeological accounts of the past, all too often it's in terms that reproduce the conviction rejected by feminists at least since Beauvoir: that, for women, biology is destiny; they are born, not made, and they are born to subordinate roles defined by their reproductive capacities no matter what the social context. So women had been disappeared from any active role in major cultural transitions, like the Neolithic revolution or the building of state; I'm referring here to the Watson and Kennedy, and the Hastorf examples from yesterday. They're sequestered in one or another type of a domestic sphere, presumed to be limited in mobility and dependent upon dominant men. In an early book-length overview of results from gender archaeology that came out in 1997, Sara Nelson catalogued a whole range of contexts where there is evidence that women played powerful, public and economic roles typically assumed to be the preserve of men: women rulers, traders, healers, and warriors. Her central point is not just that the scope of women's activities in the past don't necessarily conform to current expectations, but that we should reassess the dichotomous gender categories in terms of which we think about gender. Conventional models of gendered divisions of labor and of spheres – gathering vs hunting, domestic vs public, for example – don't hold up ethnographically, and aren't any more adequate to archaeological subjects. Once that point is taken it becomes possible to recognize evidence of quite complex, sometimes fluid gender relations in archaeologically-studied cultures. So, for example, Rosemary Joyce offers a reanalysis of well-studied gender imagery in Mayan monuments that brings into focus a duality of gender identities; she argues that it's the relations between male and female rather than discrete, essentialist identities that are represented, and that Mayan gender imagery is inflected by a number of other dimensions of social difference and power (1997). These themes figured prominently in a 2004 conference, *Que(e)rying Archaeology* (2009), and here the keynote speakers –Dowson, Marshall, McCafferty, and Voss – all stressed the political implications, the political import of refusing the “innocence” of a discipline that had long participated in the process of naturalizing oppressive gender norms.

So archaeology is producing some quite disruptive counter-narratives about gender – what's natural, what's possible, and why we shouldn't take contemporary sex/gender roles and relations as a given. At the same time, however, several of the *Que(e)rying* keynote speakers were quite pessimistic about the extent to which this potential was being realized. McCafferty found that archaeology textbooks mostly hadn't taken on board what was being learned about gender in archaeological terms – the same binaries, the same lop-sided roles, the same conspicuous silences persist – and public discourse follows suit. Cave man diets are all the rage these days, but whatever their nutritional credibility, they reproduce exactly the flawed fictions about “man the hunter” that mobilized feminist critics thirty years ago. As Gero argued in the 1993 analysis that I mentioned in my lecture, the conceptual “rails” on which Paleoindian research had run for decades were built on manifestly sexist assumptions about the centrality of big-game hunting, and on a systematic erasure of the work done predominantly by women on expedient tools

that bear witness to a much more diverse Paleoindian diet than the mammoth/bison construct allows. Violence and inequities – gendered and otherwise – certainly are a subject of archaeological inquiry, but what impact this has more broadly varies a lot by area.

Interviewer 1: I'd like to come back a little bit to our discussion about objectivity. The question is: is the reformulation of objectivity that you propose - I don't know if reformulation is the better word, but we're going to stick with that - incorporates non-cognitive values and what is the role of social values in this kind of stronger objectivity? What kind of non-cognitive values are we talking about? And the last question, maybe the most important: [can] cognitive and social values be separated?

Alison Wylie: Let me see... I certainly understand the motivation for drawing a sharp boundary that separates social, non-cognitive values from cognitive values of the kind Hugh Lacey defends – I understand that he's done some new work on this. But, for now, the problem I see with such a distinction is that if what counts as cognitive values are generic epistemic virtues like empirical adequacy – always at the top of the list – there is no gold standard defining what this comes to, not even within a given field. What counts as empirical adequacy evolves over time, partly as a function of what technical resources you have for gathering and analyzing data, but also as a function of what it is you already know and what you want to know, and of what the stakes are – what you anticipate using that knowledge for and what the consequences are of being wrong (or right). So where you set the bar, even on something as seemingly non-controversial as empirical adequacy, is very much specific to what Lacey refers to as a research strategy, and the choice of research strategy is always going to be informed by social non-cognitive values. This means that when you're specifying how to interpret empirical adequacy in a particular context of application, even if self-serving interests aren't immediately in play – for example, in cases where practitioners deliberately set a standard that favors their preferred theory – broader social, non-cognitive values nonetheless play a role in determining what kinds of empirical adequacy you're going to require and in fine-tuning what level of adequacy will count as grounds for accepting or rejecting or choosing between rival hypotheses.

Where kinds (rather than degrees) of empirical adequacy are concerned you can distinguish between empirical fidelity, which puts a premium on empirical richness and precision with respect to a particular object of inquiry, and empirical breadth which requires adequacy in extension to other subjects. This contrast may align with Lacey's distinction between decontextualizing and contextualizing methodologies. A decontextualizing methodology will put the emphasis on the empirical adequacy of a set of claims with respect to a very broad, ideally universal, domain of application. A non-decontextualizing methodology will make it a priority to produce a really accurate empirical account of a particular component of that massive domain. On this latter standard, an account won't necessarily be judged false or inadequate if it doesn't apply across the domain, but on the former it may well be found wanting. And where degree of adequacy is concerned, what counts as an empirically robust or decisive result very clearly depends on the history of research in a field, the state of available technologies of observation and measurement, and what's at stake. To take an example that Heather Douglas considers (2000: 565-569), a standard 0.05 level of statistical significance may be adequate for some purposes but not for others depending on the field of study: how noisy the data are, what kinds of causal or other relations scientists are testing, what the consequences are of false negatives or false positives. But more often than not the justification for adopting one rather than another cutoff – as fit for purpose or as a proxy for substantive (causal) significance of a finding – is lost from view; it is a matter of convention that varies from one field to another.

Judgment calls about how to interpret epistemic virtues – the classic cognitive values central to Lacey's account of impartial judgment – are made all the time, often below the threshold of explicitly epistemic or ethical/political debate. In fact, I would argue that if these abstract values have enough specificity to guide research it is because they have been interpreted in light of a particular set of research goals, in the context of a particular research program or strategy. And as Lacy argues, the choices made when a research community takes up a specific research program (or strategy) are deeply inflected by non-cognitive, contextual values and interests. So the application of cognitive values like empirical adequacy within research program may be impartial, but they are themselves purpose- and value-specific creatures of context. Any number of different kinds of non-cognitive values and interests can play a role here. They

may be quite specific, like the commercial interests that put a premium on designing drug-test protocols so they'll show positive effects. Or they may be enormously broad cultural presuppositions, like the norms defining "able-bodied" functioning that make it more compelling (for funders, policy makers, researchers) to develop elaborate whole-body prostheses that enable walking, rather than improving wheelchair design and access, low-tech solutions that would make a difference for a much wider range of those with mobility issues.

Kristen Intemann and Inma de Melo-Martin work through several telling examples of medical research to show just how complex the entanglement of cognitive and non-cognitive values can be; they consider not just commercial drug testing where the interests at work seem obvious and problematic, but research motivated by a commitment to promote global health in the case of HPV vaccine development (2011, 2009). Another example is an analysis developed by Gwen Ottinger of long-term negotiations over what will count as evidence of environmental hazard for fence-line communities affected by petrochemical plant pollution (she considers the case of a community just north of New Orleans). Here again what's at issue is often not deliberate manipulation of standards and results to serve powerful commercial interests, but the taken-for-granted conventions of research practice which determine that toxicity is measured for individual chemical elements; the standards of adequacy entrenched in the expert community don't include testing for effects of a combination of elements and yet that's what matters for those living near these plants (2013). Non-cognitive values enter, then, not just as a factor that biases individual judgment, but in the articulation of the cognitive norms that constitute the authority of a research community and its epistemic practices.

I understand why it's important strategically and conceptually to maintain the distinction between cognitive and non-cognitive values; epistemic credibility is almost always understood, in public debate and within the sciences, in terms of an absence of vested interest. But it seems to me that, as soon as you recognize that non-cognitive values set your research agenda, they are also already at work defining the repertoire of hypotheses you will consider and, from there, they constitute your categories of description, your choice of methods and how you interpret standards of adequacy in the context of actually doing the science. I've focused here on empirical adequacy but this is just one of a several high-level epistemic virtues that are routinely cited by Kuhn, Longino, Dupré, McMullin for example. Others are the internal coherence of a theory or hypothesis, its consistency with well established other bodies of knowledge, sometimes also simplicity and related heuristic virtues. These seem plausible enough at first glance, but one key type of intervention for feminist critics has been to show how a requirement of consistency, for example, serves to reinforce the reproduction of sexist common sense across fields. Lloyd's (2005) analysis of theories about human female orgasm, mentioned earlier, illustrates this point in powerful terms, as do feminist critiques of theories that attribute gender difference in cognitive performance to sex differences in brain structure. In Lloyd's case, the best going theories drew credibility from their affinity with selectionism, and in the case of gendered theories of cognition, from the biological essentialist assumptions associated with evolutionary psychology and some strands of neuroscience. If you have reason to think that selectionism or biological determinism is flawed, then consistency with those families of theory doesn't look like such a compelling epistemic virtue. Where sex difference theories of gendered cognition are concerned, the implicit bias research represents a crucial shift in the theoretical and empirical landscape. Cordelia Fine argues that the reported gender differences in cognitive performance can be just as well explained in terms of internalized social norms as by appeal to brain difference (2010). In fact, the brain remodels in response to what use we make of it, so even an appeal to consistency with neuroscience has to be selective about what aspects of neuroscience are invoked. Which is to say, consistency with well established bodies of knowledge is another cognitive value that seems clearly distinct from non-cognitive, social values but, on closer analysis, requires contextually-specific interpretation to get traction in contexts of practice. Does this answer your question? Tell me the end of your question again.

Interviewer 1: Can we separate cognitive values from social values?

Alison Wylie: So, a more perspicuous answer to that question would be: conceptually, yes, we can make that distinction and it's a distinction that's often useful. But like many distinctions, I don't believe it sustains any hard and fast sorting of cognitive from non-cognitive values. Crucially, the blurriness here is not just a

matter of there being borderline cases. The argument I'm making is that, even if you maintain this distinction, non-cognitive values are in play whenever you make use of cognitive values in practice, perhaps they operate far in the background but they are no less consequential for being out of sight and out of mind.

Interviewer 3: We already asked you about that, but I want to emphasize it because we really wanted to listen to you about that. Apparently, even if an object is not directly gender-related, it can always be investigated in a way to stress this theme, but is there a limit to choose an object that may satisfy this finality? Are there specific objects to a feminist science? Is it about the issues chosen to be investigated? Is it the way of investigating them? Is it the effort to clarify gender issues and not to allow silence about them? Or is the development of the policies to reduce inequities? In this sense, doing science as a feminist always involves political engagement or a proposal of interventions in policies? Thinking specifically about Archeology, are there special or privileged objects for gender archeology? Is it possible to stabilize gender relations with any object?

Alison Wylie: Yeah, I remember that question – “What does it mean to do research as a feminist?” – now with an emphasis on whether it's the objects of inquiry that make the difference.

There are deep differences among practitioners in a range of fields about whether doing research as a feminist requires that you focus, as you say, on a particular kind of question or object, and whether your research must in some way be aligned with or support feminist activism. This generated some heated debate among participants in a week-long workshop on “Doing Archaeology as a Feminist” that I co-convened with Meg Conkey at the School of American Research in Santa Fe in April 1998 (Conkey and Wylie 2007). We didn't come to any resolution and I am, myself, torn about how to think about this. So rather than an answer, here's my response to some of the options for characterizing research as “feminist.”

First, the methodological option. Given growing evidence in the 1970s that even our best, most credible sciences were not self-correcting – well respected scientific methods had often just reproduced sexist and androcentric bias – some feminist science critics argued that standard scientific methods were themselves a key source of the problem. Maria Mies, for example, categorically rejected the objectifying stance that she associated with “positivist” methodologies (1983), and there was sharp contention among feminist social scientists about whether quantitative methods are inevitably a tool of the oppressor – characteristic of oppressive forms of social science that function as a form of “ruling practice.”⁶ One response was to insist that feminists needed their own science, their own methods: perhaps the qualitative methods that had given women a voice were uniquely appropriate to feminist goals? I was much compelled by a pair of papers that Sandra Harding and Helen Longino published in the late 1980s⁷ where they took on this question of whether there is a distinctive feminist methodology (Harding 1987), or “feminist science” (Longino 1987). In their different ways they both made the case that the quest for a distinctively feminist method effectively re-inscribes in feminist practice the kind of methodolatry that feminists themselves had so effectively critiqued; there isn't any methodological silver bullet that will protect against sexist or androcentric bias, that will guarantee the feminist credibility of a research program.

To put this in more concrete terms, pursuing a research program that's richly qualitative – where the focus is on getting at the texture and complexity of a particular lifeworld – doesn't guarantee that patterns of sex/gender inequality will be recognized. There's no question that there's a lot to learn from this kind of research – that it can give voice to those who have been silenced and re-center research on the realities of life on various kinds of social margin – but at the same time there's always a risk that you may not see the forest for the trees, so to speak. To bring a feminist perspective to bear may require you to step back and undertake some quantitative analysis to get at the configuration of the forest, to press the metaphor:

⁶ This is Dorothy Smith's term – sociology as a ruling practice – but she didn't argue that any particular tools of inquiry inevitably, necessarily served these purposes. For an analysis of this debate see Wylie (1992).

⁷ These appeared in one of the *Hypatia* special issues on *Feminism and Science* edited by Nancy Tuana in 1987.

the sex/gender dimensions of larger scale outcomes or structural conditions that may not be immediately visible in the dynamics of everyday interaction and insiders' self-understanding. In fact, it's a recurrent theme in the feminist methodology literature that, while you should never dismiss the wisdom of insiders to a particular lifeworld, often you need to go beyond insider knowledge, to set the local in a broader context, if you're going to address feminist research questions and be effective as an activist (for the details of this argument, see Wylie 1992). And to do this, it seems clear that feminists need to use a whole range of different research tools: whatever tools best serve the purpose at hand. Within a decade of the publication of Harding's and Longino's response to the "feminist method"/"feminist science" debate there was an outpouring of handbooks of feminist methodology and overviews of feminist research which demonstrated that feminists were, in fact, using all kinds of research tools, no holds barred.

So, a second possibility: if there's no distinctively feminist way of doing science perhaps there is, nonetheless, a suite of distinctive problems that feminists address, using whatever tools are appropriate. That seems like a plausible way of characterizing feminist research, but then the issue arises of how to characterize these problems; what makes a feminist problem feminist? Is it a matter of asking neglected questions about women and gender? That answer ties the definition of feminist research to a particular subject matter – the possibility you raised. But the gender archaeology case I described yesterday suggests that this can't be all that's involved. In a review of gender archaeology that Joan Gero and Meg Conkey published a decade after the initial conferences I was describing yesterday (1997), they object that quite a lot of well-intentioned "archaeology in the gender genre" still showed no serious engagement with feminist perspectives or analysis. In fact, some vocal champions of "gender archaeology" were quite explicit that they wanted to add questions about women and gender to the mainstream research agenda without bringing into play any overtly critical, feminist theory or analysis. They didn't want to ask whether conventional ways of thinking about the cultural past, or norms of disciplinary practice are, themselves, sexist and androcentric. In short, just because a researcher addresses questions about women and gender doesn't mean their work is feminist; indeed, it may be anti-feminist.

To take the complement of this problem, what happens if, in the course of conscientiously pursuing a feminist research project on, say, a question about the impact of welfare policy on women or the gender politics of a workplace, you discover that really what makes the difference for women is not so much their gender, but their race or age, perhaps their sexuality, or legal status as immigrants or refugees? At the end of the day you might end up with results that aren't so much about sex/gender relations as about the effects of inequality that operate along other lines of social differentiation – or, more typically, a number of them at once, cross-cutting gender in complicated ways. Is this still a feminist project? I don't want to say, no. In fact, I think that arguably the best kind of feminist project is one that makes it possible to find out that your own presuppositions – including feminist presuppositions – are wrong, in that way expanding the scope of feminist inquiry beyond a narrow focus on women and gender. That outcome seems to me to mark the success, not the failure of a feminist-inspired project. So I don't think there's much promise in defining properly feminist research in terms of questions about a particular subject matter: women and gender. In fact, given the insights from intersectional analysis long since developed by Crenshaw (1989), we have good theoretical reasons to conclude that it's never going to be possible to draw a boundary around a distinctively, uniquely "feminist" subject domain or set of problems.

So, working through your list, it seems that what makes a feminist project feminist can't be its methodology, or a domain-defined set of questions. A third possibility suggested by Helen Longino in the article I mentioned earlier is that feminist research is chiefly a matter of "doing science as a feminist" which, in turn, is characterized by what she describes as a "bottom-line commitment" to not "disappear" gender. So, whatever project you take up as feminist, you make sure that gender is not ignored even though you cannot presume that gender is relevant or, for that matter, that any other of the intersectional dimensions of gender roles and relations are irrelevant. To return to the gender archaeologists I just mentioned, does this mean that they're practicing as feminists even if they don't identify as feminist? There is certainly a sense in which they are counteracting a pervasive practice of disappearing gender in archaeology by taking up questions about women and gender. If they're right in claiming that they're not feminist – and if Gero and Conkey are right in objecting that their work is impoverished by its lack of feminist engagement – is it because they don't push these questions far enough? If a line is to be drawn here, it would seem to depend on a particular interpretation of what counts as "gender" that shouldn't be

disappeared. The advocates of gender archaeology who reject any feminist associations do certainly disappear the gender politics of their own research program and of the mainstream conventions of practice that they defend, even as they bring women and gender into focus as a subject of inquiry. Perhaps taking a feminist standpoint on research practice itself – a commitment to not disappear gender at a meta-level – is what's required to count as “doing archaeology as a feminist”?

A standard move at this point – a fourth option – is to insist that you're only practicing as a feminist if the research questions you take up are animated by activist commitments. You choose questions to work on that are relevant to the lives of women and gender minorities, questions about systems of social inequality that, if you can answer them, will provide the resources necessary to effectively address social injustice. So, to shift gears a bit, within philosophy this might take the form of a resolve to reject conceptions of philosophy that equate real (serious, deep) philosophical analysis with purely abstract inquiry – a presumption, familiar in many contexts, that philosophy is all and only what doesn't touch the world, aligned with methodological norms that systematically privilege decontextualizing approaches. Charles Mills has made the case for what he calls non-ideal theory (2005): philosophical inquiry that roots questions of ethics and of political philosophy in the messy realities of a world structured by a profoundly unjust racial contract. I find Mills' argument inspirational. He captures the animating spirit of the feminist philosophy that I most respect: a commitment to use the tools of philosophical analysis to address questions that actually matter in some domain of life, in face of colleagues who insist that, just by virtue of engaging issues of practical import, you can't be doing “real” philosophy. But, as my reference to Mills suggests, this is not uniquely a feminist commitment, and to build it into a definition of “feminist” philosophy returns us to the problems I raised earlier. What makes a commitment to practical, activist engagement specifically feminist? Does this anchor a definition of feminist research any more clearly than the appeal to a subject domain or a set of questions?

In the end I think the whole boundary-drawing enterprise is fundamentally problematic; its impulse is exclusive, and in this it is inimical to the inclusive spirit of the feminist scholarship and activism that I most value. If you must draw lines around what counts as feminist research and what doesn't, the most serviceable account will likely be some form of a family resemblance definition that combines a commitment to not disappear gender with a requirement of some form of activist commitment. But, again, that just defers the difficult questions I've been raising. Perhaps there is no more to be said than what Linda Alcoff and Libby Potter offer in response to the question, “why retain the adjective “feminist” in the introduction to their 1993 collection, *Feminist Epistemologies*: that identifying a research program as feminist is matter of recognizing its historical trajectory – what its sources of inspiration are, where it comes from – rather than delimiting what it is now, or where it's going (1993: 4).

Let me turn now to the other part of your question which I take it – from a comment you made earlier when you said: “what about atoms?” – has to do with the limits of feminist analysis: are there subject domains where feminist questions, a feminist commitment to not disappear gender, feminist perspectives more generally, are just irrelevant?

There is, indeed, longstanding controversy about whether a feminist research program could ever be expected to take root in fields that deal with non-gendered or projectively gendered, as opposed to intrinsically gendered, subject matters. Primate studies is an example of field concerned with a projectively gendered subject domain: we project gender relations and identities on to primates; their social lives are not constituted by gender norms and categories in the ways that our lives are. Rocks, atoms, quarks are standardly cited as examples of subject domains that are in no way gendered, although some make the case that gender symbolism inflects our understanding of these phenomena as well. So the question reframed is: can a case be made for the relevance of feminist analysis to the whole spectrum of sciences including, at the extreme, those that seem to have no obviously gendered content like geology, astronomy, molecular biology, high energy physics?

One way to make such a case is to argue that science as an enterprise is gendered, so feminist analysis is relevant across the board, no matter what any particular science is about. This is the approach that Evelyn Fox Keller took in “Gender and Science” (1978): that ideals of objectivity embody an inherently masculine stance toward the world. Drawing on psychoanalytic theory – object relations theory – she

argued that the canonically scientific norm of objectivity arises from a psychological orientation that men develop in the process of infantile socialization as they dissociate from their female caregivers. The result, in maturity, is a distinctive kind of spectator stance and an interest in control and manipulation of objects of knowledge. By contrast, a more "feminine" orientation reflects continued identification with female caregivers, so those socialized as women have less anxiety about identity boundaries and a greater capacity for identification with others and with objects of knowledge. Famously, Keller expanded on this account in her book on Barbara McClintock, *A Feeling for the Organism* (1983), arguing that McClintock's Nobel prize-winning research was characterized by this distinctive (gendered) ability to immerse in the micro-world of maize genetics that she studied. Although the details of her account of McClintock are fascinating, I don't find her psychoanalytic interpretation compelling, and I expect that I'm not doing it justice. But the point is that Keller used object relations theory to give a feminist account of gendered epistemic agency that could be applied across the sciences, whatever the nature of their subject domain.

Keller was immediately attacked and her account became something of a lightning rod. It was taken to be emblematic of everything that's wrong with feminist science studies, especially its presumed essentialism. I've worked through the details of this reaction in my Pacific APA lecture (Wylie 2012), but one critique that's salient here is a review of *A Feeling for the Organism* published by Stephen Jay Gould, who objected that Keller couldn't be right about there being distinctively feminine and masculine orientations evident in practice; he knew plenty of male scientists who identify with their objects of inquiry just as much as McClintock. Another, due to Sandra Harding (1986), was that the traits associated with the orientation attributed to women on this account bear a disturbing resemblance to those attributed to a stereotypically African or Afrocentric worldview: immersion in relations with the other and with nature, holistic thinking, a lack of clearly defined identity boundaries, and so on. Harding argued that, rather than picking up on an inherently gendered mode of engaging the world, these contrasts reflect a standard pattern of characterizing those in subdominant positions in oppositional terms: what it is to be a woman, a colonial subject, a member of a subordinate class is just to be not-male, not-white, not a member of a colonial elite. It is to be dependent rather than autonomous, emotional rather than rational, incapable of self-direction or discipline; it is to lack whatever is characteristic of those whose privilege includes the confident presumption that they are the norm against which all others are to be judged. This is, then, a sustained attack on the very idea that there are distinct, gendered modes of cognition. The upshot: the one-size-fits-all feminist analysis of science as a masculinist enterprise fails because the object relations theory Keller invokes it is untenable; it cannot sustain the strong claims about the gendered nature of cognition that she needs to establish that ideals of objectivity are inherently masculine. Moreover, I don't see any more plausible alternatives on the horizon, and there are good reasons not to take the risk of essentialism, strategic or otherwise.

So, that's one way of claiming that there is no limit to the range of fields, or subject domains, to which a feminist analysis could apply – one that has been pretty decisively rejected by feminists and their fellow travellers as much as by their critics. These objections establish, I believe, that it's folly to expect that any one type of feminist analysis will be relevant to all types of science. You have a certain kind of leverage, certain possibilities for feminist critique and intervention when dealing with a subject domain that is intrinsically gendered that you don't have with fields that study non-gendered subjects.

The classic model of feminist practice in intrinsically gendered fields was described in the 1970s as **research on women, by women, for women**. This vision of feminist research – influential at the time when the feminist method debate was active – does seem to limit feminist engagement, not just to the fields that study intrinsically gendered (human, social) subjects, but to ones where these subjects can be directly engaged and stand to benefit from the research. The ideal here is a form of activist self-study directed to issues like employment equity, violence against women – to take two areas of feminist interest for me – that stand to provide knowledge relevant to making a difference for women. And it is grounded in the situated, gendered experience of the researchers themselves and those they study; research questions are framed and evidence mobilized that will bring conditions of oppression – the "disappeared" and ignored dimensions of a subject domain – into sharp focus. A great deal has been learned from such research – research that "starts from women's lives" as Sandra Harding describes it. That said, as I argued earlier, feminist research programs have flourished in ways that push well beyond this model. Sometimes the most important subjects to study for activist purposes are not women themselves but their

oppressors, and since the mid-1980s feminists who do study women have argued that simplistic assumptions about the foundational nature of insider, experiential knowledge are problematic. They also routinely study subjects whose experience and perspectives are not directly accessible, for any number of reasons, and these domains of study often generate the most striking challenges to our own taken-for-granted assumptions sex/gender systems.

So, for example, a feminist perspective can clearly be brought to bear in archeology where the subjects of inquiry are long dead. You can't ask the subjects directly what the world looks like from their perspective, as a gendered beings, and you can't assume that their sex/gender systems were anything like those with which we're familiar. But what a feminist perspective brings is an awareness of just this contingency – a caution against assuming that patently ethnocentric and presentist sex/gender norms can be projected onto the past. As I described earlier, the point of departure for explicitly feminist work in archaeology is a critique of bio-essentialist assumptions about the fixed, binary nature of gender categories. This awareness of the contingent, constructed nature of sex/gender systems was what catalyzed the research programs that have generated archaeological evidence of gender roles and relations that defy interpretation in familiar terms: cases in which gender may not have been very significant, or where it seems disconnected from biological sex, inflected by age or status of other kinds, and where gender representation is ambiguous. There are lots of examples along these lines that illustrate how feminist questions can be productively raised about gendered subjects who not directly accessible.

At another remove there is primatology, a field in which, famously, non-human subjects have long been projectively gendered: “baboons with briefcases, langurs with lipstick” (Sperling 1991), or earlier iterations that cast female primates in roles fixed by sociobiology – passive coquettes playing up to aggressive male strategists in a relentless competition for reproductive success. In the 1970s and 1980s one after another study documented the central role of females in primate groups and, as attention shifted from a preoccupation with the dominant males, our understanding of primate social dynamics was completely reconfigured (Strum and Fedigan 2000). There has been active debate ever since about whether it was the influx of women into primatology that made the difference. Did their situated experience as women make them suspicious of the patently sexist assumptions that underpinned these descriptions of primate behavior, and lead them to collect data on patterns of interaction that had been ignored? Or was it non-demographic factors: the state of play in debate about evolutionary theory, and critical reactions against sociobiology? Or perhaps a move to import more rigorous research methodologies as primatology matured that made for more systematic research practice in the field: sampling and recording the behavior of all the animals in a troupe rather than just the most dominant and visible males? Whatever the answer, and a quite dizzying array of perspectives on this are to be found in *Primate Encounters* (Strum and Fedigan 2000), this is a case in which early work had been profoundly distorted by the projection of gender taken-for-granted, and these were only called into when a growing number of women in the field asked pointed questions and instituted field practices that generated the data necessary to challenge them. This type of intervention, focused on gendered metaphors rather than gendered subjects per se, is by no means an isolated case. Think of Emily Martin's classic feminist critique of how reproductive biology had been powerfully shaped by what she called “A Romance Based on Stereotypical Male-Female Roles” (1991). Wherever conventional sex-gender norms are embedded in the theoretical imaginary of a field there's scope for feminist analysis.

So what's to be said about the type of case that seems hardest – the STEM fields that investigate atoms, quasars, molecules and such? A standard argument in these fields has been that feminist critiques only bear on issues of injustice to the women who are marginalized or excluded, and the costs of squandering of their talent. As a prominent feminist activist in astrophysics likes to put it, “photons have no gender”; no matter how lamentably sexist the field may be, this has no impact on the integrity of the science (Urry 2008). I think this is profoundly wrong-headed. It doesn't follow from the fact the subject of inquiry is non-gendered that gender politics have no impact on the research agendas of these field or on the content of the knowledge they produce. That said, I do agree a quite different order of analysis is called for than in fields that deal with intrinsically or projectively gendered subjects. Yesterday I illustrated how this can work in terms of Gero's analysis of Paleoindian research (Wylie 2012: 66-67); here's a précis. The key considerations have to do with the way a research field is structured, and how information and resources flow within it. Imagine a field where there is workplace segregation along gender lines, so that there are

some problem areas or subfields or specialities where women are especially well represented – the phenomenon of ‘pink collar ghettos’, as it used to be called, is a common pattern in many sciences. And imagine that this field is characterized by gendered citation patterns, so that the work women do in the subfields where they cluster doesn’t get as much uptake, as much citational recognition, as the work by men in male-dominated areas. This means that work by women won’t have as much impact in the field as work by men which, in turn, has implications for funding that further reinforces these patterns of segregation, of results as well as of practitioners. In the archaeology example I mentioned, Gero found that women’s publications get less uptake even when they work in male-dominated areas, an effect that’s amplified when they work in subspecialties that are dominated by women. There are plenty of examples in fields that study nongendered objects where work done by women has been ignored or dismissed, only to be recognized much later as absolutely crucial; Keller’s biography of McClintock provides the details of such a case in biological science, and Kristen Intemann describes one in astronomy, Vera Rubin (2001: 516) to take just a couple of examples.

The point I’m making is that if these patterns are systemic – if it is whole subfields or problem areas that are ignored because they’re chiefly populated by women and work by women isn’t getting uptake or support – then, contra Urry, it’s hard to imagine how gender inequities **wouldn’t** have an impact on the research agenda and substantive results of a field – on its content. Even when research in these areas produces evidence that’s relevant to topics that are taken seriously in the mainstream, it won’t get the kind of traction that evidence from male dominated subfields will; exactly the type of case Gero documented with respect to Paleoindian research. And when women pursue innovative lines of inquiry in their subspecialties that don’t fit the mould of the mainstream research agenda, they won’t be recognized or supported. Even more worrisome, this whole system is powerfully self-reproducing, self-insulating. You could wait a long time before the research agenda is broadened enough to take these marginalized insights into account, and maybe they’ll never be taken on board. It’s the lucky few – McClintock, Rubin – whose contributions are recognized after the fact; who knows how much has been lost altogether, in the way of creative insights and empirical results generated not just by individual women scientists but by whole areas of research that have predominantly been pursued by women. So, contra Urry, gender discrimination – in the myriad, often subtle forms it now takes – isn’t just a matter of injustice to women in fields that study atoms and photons. As Longino argues, these social inequities compromise the integrity of the research enterprise itself. They are an epistemic injustice that has a direct impact on content, whether or not the subject domain is gendered.

So that’s how I would develop the argument for recognizing that different kinds of feminist analysis can productively be developed for different subject domains. In any field structural inequities can be epistemically compromising, but in fields that study intrinsically or projectively gendered subjects the impact of sexism or androcentrism on the research agenda and content of the field may be more direct.

Interviewer 3: The next question is related to what you were just talking [about]. It’s about agency. Despite the fact that women are marginalized in institutional research and the existence of gender inequity, what’s the field of action for women to engage in archeology research? Instead of victimizing or stressing the oppression suffered by women, how can we consider agency in this context? Is it possible to look at agencies of resistance and negotiations in asymmetrical power relations and see women’s agency? What can this agency illuminate? Would it be a possible to overcome the emphasis on the oppression suffered by women to take this strategy seriously and also be considered a way to destabilize gender relations?

Alison Wylie: Well, again, I think there are two parts of this question. As you put this question, you emphasize the constraints that women face exercising their agency within the communities and institutions in which they operate as archaeologists. But it’s important, as well, to consider the politics of attributions of agency to the subjects of archaeological inquiry.

I mentioned at the outset Bruce Trigger’s objections to what he saw as patently racist assumptions of archaeological accounts that effectively denied agency to Native Americans. Post-processual critics mounted a broader challenge to the dominant tradition of eco-functionalism associated with the New Archaeology on grounds that it was untenably reductive. Rather than assuming that individuals and

communities just respond to the pushes and pulls of ecological conditions, they argued that it was crucial to recognize the extent to which even very early humans manipulated their environments – successfully or not, sustainably or not – changing carrying capacity and pursuing forms of life that weren't always bio-energetically optimal. Much of human life, many of the cultural forms that we know from the archaeological record just can't be accounted in ecological terms. So arguments for attending to agency were getting traction in archaeology by the time gender archaeology took shape, and they became a key focus for feminist critics. Like Trigger, however, feminist critics argued not just for more attention to agency in general, but for rethinking gendered attributions of agency that were often not even recognized as such: differential assumptions about who was exercising agency in archaeological accounts of everything from foraging strategies to state formation.

But now turning to the research community, I agree that it's important to recognize that women can and do exercise many different kinds of agency as archaeologists even when they find themselves marginalized and discounted professionally. So although I find Marie Louise Stig Sørensen's caricature of feminism and of the history of gender research in archaeology profoundly misguided, my sympathies align with her's inasmuch as I want to see gender research taken seriously. And I recognize that she took the anti-feminist stance I described yesterday in a context where gender research of any kind faced stiff opposition. It was suspect from the outset as "just political," a partisan specialty interest that must be biased and could be dismissed without serious consideration. The strategy she adopted was to insist that gender archaeologists are just as scientific as their critics, in their terms. They're just doing objective, straight-up archeology – objective in the "value free" universalist sense – nothing feminist or political about it. I believe this strategy is fundamentally self-limiting; by refusing engage any of the broader epistemic issues that need to be addressed to open up space for non-mainstream archaeology, it undercuts the potential for challenging the disciplinary status quo in any but the most superficial ways. But I see the rationale for adopting such a strategy if, from Stig Sørensen's perspective, it seemed like gender research wouldn't get a foothold otherwise.

A related strategy is not uncommon among equity activists. I remember early meetings of a women's caucus at the Society for American Archeology that included a number of senior women who would never have identified as feminists but were doing an enormous amount to change the status of women in the field. From what they were prepared to say to one another, off the record, as it were, they had a crystal-clear feminist analysis of the gender politics of the discipline and they were acting on this analysis in really effective ways. They traded stories and advice about how to get women students through hostile graduate programs and into good positions, how to change the gender balance of key decision making bodies in the field, how to foster ways of doing archeological research that would create more egalitarian and inclusive spaces in which women could operate. I think of them as enacting a kind of stealth-feminism; the agenda seemed to me to be clearly feminist, but they didn't name it as such. The difference between them and Sørensen is that they didn't advance the cause of women by denouncing feminism.

So I would argue that there is room for exercising agency as women and as feminists in many different ways. Sometimes it's wise to proceed by stealth, but it's also crucial that there be some who are prepared to stand up and explicitly identify as feminists, and do the work that needs doing in those terms. The key thing is not to allow the power dynamics of inequality along gender and other lines to pit us against one another.

Interviewer 2: So this final question we prepared is... in your work, you argue that a feminist standpoint stresses the importance of neglected events and considers marginalized or neglected subjects as having an epistemic privilege. If we keep this perspective, do you believe that there are relations between a feminist science and a science committed to neoliberal market values?

Certainly feminist practitioners, and feminist research programs, are vulnerable to being appropriated by dominant reactionary interests; this is by no means a risk limited to feminists! Neoliberalism is incredibly sophisticated at appropriating whatever resources it can from its opposition and turning them to its own ends. One worry I have about stealth feminism is that it's particularly prone to co-optation. But I also worry about a tendency to denounce practices that have been appropriated as inherently reactionary – to give up on research strategies or programs because of this. Here's an example that's not feminist but

illustrates this point. There was an intense debate a decade ago about community-based collaborative research (CBPR) in development contexts. In a collection of intensely critical essays, *Participation: The New Tyranny?* (2001), Cooke and Kothari objected that CBPR practice effectively downloaded the burden of development research onto communities that had the least resources to take it up; it was an oppressive practice that exploited the very people it was meant to empower. And in a rejoinder to this collection, *Participation: From Tyranny to Transformation* (2004), Hickey and Mohan argued that CBPR practices certainly could be oppressive – they were when appropriated and scaled up by the World Bank – but they didn't have to be. They detailed the history of several different types of CBPR practice and made the case that participatory research has a great deal to offer in development contexts; it can be transformative and the failures are instructive. The fact that the World Bank can ruin a good thing shouldn't be a surprise, and shouldn't be a reason to reject CBPR in all its forms. Does this answer your question?

Interviewer 2: Yes, because I was trying to make a link between the fact that feminists usually get interested in marginalized subjects and usually neoliberals don't.

Alison Wylie: I see.

Interviewer 2: So that was an attempt to see a contradiction, but you see if there is a solution for this contradiction.

Alison Wylie: I want to go back to a point about standpoint theory: that from a standpoint theory perspective, those who are discounted as knowers because of social indicators that mark them as outsiders to a dominant tradition very often have significant epistemic resources that go unrecognized. Their situated experience of oppressive conditions puts them in a position to know a great deal that comparatively privileged subjects don't know or, as I said earlier, are actively invested in not knowing. But to develop a critical standpoint on knowledge production requires something more; there is labor involved in taking distance from a dominant world view, and collective labor involved in building the conceptual resources of an alternative that incorporates the kind of reflexive understanding necessary for action-guiding critique. A gendered, feminist standpoint is especially complex because it depends on situated knowledge that is often itself deeply conflicted. As Sheila Rowbotham put it years ago, women typically live with the class enemy (1973). They have intimate relations and family ties, all kinds of bonds of loyalty and affection and commitment to the men in their lives, which can make it hard to recognize the operation of gender politics and even harder to effectively act against them. In many contexts women are systematically pitted against one another, competing for male-controlled resources and actively discouraged from developing the kinds of relationships with other women that could be a basis for recognizing any community of interests or experience. So feminist activism has always been about building connections among women. It's not that these don't exist. Certainly black feminists emphasize the strong core relations among women that have always been the basis for their activism. But building a critical analysis of gender relations and systemic gender inequality on the basis of this conflicted experience, these split loyalties and intersectional identities, is a real challenge. It carries with it, always, the risk of privileging one set of subjects as marginal at the expense of others, of reverting to the kind of neoliberal stance that sees only individuals and disappears the effects of structural oppression.

So there is a contradiction here – and not just an intellectual contradiction. There are inherent tensions in any formulation of a feminist or, indeed, any identity-based critical standpoint; we all struggle to grasp structural conditions that operate as a network of social divisions – each refracted through the prism of others, as Rowbotham put it. No one angle of vision, no one type of situated experience, can bear witness to all the dimensions of oppression that affect us, in either a positive or a negative sense. Situated knowledge is situated – it is inevitably partial, useful or credible in a strictly contingent sense. Building a useable “standpoint on” knowledge production is, crucially, a matter of always being alert to these limitations as an effect of structural inequalities. So I would say that it isn't just an interest in marginal subjects that distinguishes a feminists from neoliberals; it's an interest in the structural conditions that constitute marginality.

Thank you! It's been an immense honor to have this much attention paid to my work – the whole diverse range of issues I've worked on over the years. These are magnificent questions – ones I'll be thinking about for some time to come.

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