

**Coming to Terms with the Value(s) of Science:  
Insights from Feminist Science Scholarship**

Lynn Hankinson Nelson (University of Missouri-St. Louis) and  
Alison Wylie (Washington University)

*Workshop on Science and Values*  
Center for Philosophy of Science  
University of Pittsburgh  
October 9-11, 1998

*I. Introduction: Nozick's not-so-neglected third option*

In the Presidential address delivered at the last Eastern Division meeting of the American Philosophical Association (December 1997), Robert Nozick took up the question of how we might best reconceptualize objectivity given challenges from various quarters that call into question the value-neutrality of the sciences, the disciplines whose practice and products we take to exemplify objectivist ideals. He argued that philosophical thinking about science has been dominated by two broad families of response (Nozick 1997). On one hand there are those who accept the central findings of critics who purport to establish the irreducible role of values in science (contextual as well as epistemic), and take this not just to mark a decisive break with objectivist and foundationalist theories of science, but to entail that science cannot be considered objective in any meaningful sense.<sup>1</sup> The advocates of such “radical” responses (as Nozick referred to them) treat this as a welcome challenge to the epistemic authority of science. On the other hand, Nozick observes that a great many philosophers of science strenuously resist such conclusions; if they are prepared to concede that contextual values play any role in science they insist, defensively, that science is objective *despite* the influence of these values. Nozick then makes the case for a third option which, on his account, has been neglected by philosophers of science: that science is objective *because* of the values with which it is infused.

As a description of the polarized debates that arise whenever the subject of values in science is broached, Nozick's account is unexceptional; Richard Bernstein described this pattern of deepening critique and defensive counter-response in considerable detail in *Beyond Objectivism and Relativism* (1983), but it had been clearly identified by commentators working within the tradition of analytic, broadly neo-empiricist philosophy of science much earlier (e.g., Scheffler 1963; Suppe 1973). As an account of the current state of play on these issues, however, Nozick's summary obscures more than it illuminates. The neglected third option that he hopes to reclaim is, in fact, a strategy of response to contextualizing arguments that has been actively explored by

---

<sup>1</sup> In this discussion, we presuppose the standard distinction between contextual values and epistemic or constitutive values: *i.e.*, the distinction between factors or considerations that are conventionally treated as appropriately ‘external’ to science—‘non-cognitive’ or non-epistemic’ interests and values—and those that are considered properly ‘cognitive’, ‘epistemic’, and ‘internal’ to science. Although we mean to problematize this distinction (as many others have in recent years), it provides a useful framework for our discussion precisely because it remains so widely influential.

a great many science studies scholars. Bernstein's central objective in 1983 was to delineate a range of mediating options that seemed immanent in the trajectories of three parallel philosophical traditions: philosophy of science, philosophy of social science, and philosophical hermeneutics. Joe Rouse adopts a similar strategy, although with a narrower focus on philosophy of science, in *Knowledge and Power* (1987). If the broadly continental, Foucauldian affinities of these accounts are not to taste, consider such well established arguments for a "third way" as have been developed by advocates of the highly productive research programs that are now flourishing under the rubric of "naturalized philosophy of science, particularly those that make the case for grounding philosophical analysis not only in, for example, empirical psychology and cognitive science but also in wide range of social and historical sciences. The scope of these recent naturalizing projects is described by Maffie (1990), and is represented by such collections as *Socializing Epistemology* (Schmitt 1994), *Rethinking Objectivity* (Alan Megill 1994), *Taking the Naturalistic Turn or, How Real Philosophy of Science Is Done* (Callebaut 1993), and *The Disunity of Science* (Galison and Stump 1996), to name just a few.

But among those most consistently committed to explore Nozick's third option are feminist philosophers of science.<sup>2</sup> Indeed, it is striking feature of feminist science studies that its practitioners—philosophers, historians, and scientists alike—have consistently refused the sharply polarized options that have dominated debate about "values and science," whether these options arise in the context of second order studies of science (philosophical, historical, sociological or anthropological), in the sciences themselves, or in public debate of the kind associated with the "science wars." There is good reason for this. Feminists have long been interested in science not only as critics who have played a prominent role in documenting the myriad ways in which the sciences embody the values of their makers, but also as practitioners and activists committed to the advancement of science and to the creative use of scientific knowledge to improve our collective lives. And in this capacity their work in and on the sciences reflects an acute sense of the importance of determining "how things are" with as much empirical accuracy and explanatory power as they can muster; this is crucial not only in achieving the internally defined goals of empirical research, but in choosing effective courses of action under conditions that are frequently hostile to women and to feminists.<sup>3</sup> In short, feminist

---

<sup>2</sup> We do not assume hard and fast distinctions here: many feminist philosophers of science are just as firmly committed to naturalized and socialized programs of research in philosophy of science as they are to feminist modes of inquiry in philosophy.

<sup>3</sup> To anticipate what follows, this is to say that feminists stand to gain a great deal from a realistic appraisal of the limits and fallibility of the knowledge claims based on specific kinds of inquiry; but, at the same time, there is little epistemic warrant for, or pragmatic advantage in, generalizing the most corrosive of anti- or post-empiricist critique. Indeed, *contra* critics who impugn feminist research and science studies as 'just political', the epistemic stakes are high for feminists precisely because of our political engagement. If anything, our standpoint as feminists predisposes us, not to wishful thinking and systematic distortion--to "dogmatic" as opposed to "critical" inquiry, to use Popper's distinction (1989[1963]: 50)--but to especially probing critique of the 'taken-for-granted' that frame not only established programs of inquiry but also our own research projects.

standpoints are by no means aligned with wholesale rejection of the constitutive values—the epistemic standards and ideals—typically identified with scientific practice, however skeptical feminists have been about specific claims to objectivity and/or to value neutrality.

What feminists bring to their practice, *as feminists*, is a loosely articulated set of empirical, ethical, and political commitments. It is perilous, indeed, to attempt to characterize feminism in terms of any very closely defined core of beliefs or values, but it does seem plausible to claim that, as Helen Longino suggests, feminist scholars share a broad commitment to a “bottom line maxim”: the commitment not to “disappear gender” (Longino 1994; Wylie 1995). More specifically, feminists typically assume that gender differentiation is an important and often hierarchical structuring principle in social, cultural contexts (although, not necessarily the only or the primary such principle) and, where conventional gender roles and relations prove to systematically disadvantage women, feminists usually embrace some form of normative commitment to do what they can to change these inequalities (Wylie 1997a; Nelson 1990, 29-37). These are contextual values that are formulated in widely divergent terms, but whatever the specifics, they are precisely the kind of values that are at issue in the debates described by Nozick.

In a great many contexts these values have put feminists in a position to make substantial contributions to the disciplines they engage, both critical and constructive. It is because of their explicitly partisan concern with the difference “sex/gender” systems can make to our lives that feminists have focused attention on a range of unexamined assumptions underlying research in the social and life sciences; in this connection feminists have exposed systematic patterns of error that compromise sometimes even the best, most rigorous scientific research in the social and life sciences. Often sharply focused and sustained critique of the limitations of existing research programs has served, in turn, as the catalyst for new initiatives; feminists have developed constructive programs that correct, extend, and refine existing bodies of knowledge, sometimes opening up highly new productive new lines of inquiry.<sup>4</sup> In short, the “angle of vision” feminists bring—the contextual values that constitute their feminism—has contributed a great deal to

---

<sup>4</sup> The fruits of these critical/constructive labors have been described in considerable detail by a number of feminist practitioners and science studies scholars. Summaries are included in work we have published (e.g., Nelson 1990; Wylie 1997a, 1997b), as well as in contributions to a recent special issue of *Osiris* (*Women, Gender, Science*, Kohlstedt and Longino 1997) and, prospectively, to an upcoming conference on *Science, Technology and Medicine in the Twentieth Century: What Difference has Feminism Made?* (Princeton, October 2-3, 1998).

disciplines across the length and breadth of the social and natural sciences. This very success poses a problem for the would-be “radicals” Nozick refers to: it suggests that contextual values are not only or always compromising of scientific practice but can have a positive impact as well. That is, feminist practice in a wide range of sciences illustrates Nozick’s point in concrete terms: often feminists improve the objectivity of the sciences they engage precisely because of the values they bring to bear as feminists.

From the outset feminist philosophers of science have been alert to this apparent philosophical conundrum; neither the radical nor the defensive response characterized by Nozick has been an option for those who take seriously the critical and constructive aspects of feminist engagement with the sciences. Despite wide and contentious differences among us, virtually all currently active feminist philosophers of science take as their point of departure an appreciation that reductive relativism is as untenable as naive objectivism. To the extent that we share a collective project it is to understand, in conceptually nuanced and empirically specific terms, the difference contextual values make to science, both positive and negative.

Given this characterization of the project of feminist philosophy of science—as an enterprise that has been predominantly concerned to explore Nozick’s third option—it is not surprising that one of its first priorities has been to focus attention on the contextual values that inform inquiry. Indeed, rather than suppress these values in an effort to sustain the illusion of a disembodied neutrality, feminist analyses suggest that they should be made as central and direct a subject for (internal) scientific scrutiny as the first order knowledge claims it is the business of the sciences to produce. In this case two key questions take on central importance both for philosophers and for practitioners:

- 1<sup>st</sup>: how are we to reconceptualize objectivity so as to undercut polarizing, dichotomous patterns of argument which take it to be inimical to any view of science as value-infused?
- 2<sup>nd</sup>: how exactly do ‘values’ of various kinds make a difference to objectivity, in one or another specific sense?

We focus here on the second question, considering two case studies in which contextual values make different kinds of constructive difference to scientific practice. In our conclusion we draw out the implications of these examples for concepts of objectivity.

To anticipate, we are primarily concerned with two of (at least) three quite distinct senses in which the term ‘objectivity’ is used, drawing on an analysis developed by Lisa Lloyd (1996). Lloyd is centrally concerned with objectivity as an attribute of the objects of knowledge: objectivity in a quasi-metaphysical sense whereby objective knowledge is specifically knowledge of entities and events that exist independently of any observer (entities and events that are “really real,” that have “objective reality”). We focus on tensions that arise between two further senses of objectivity discussed by Lloyd: objectivity as an attribute of knowers, and objectivity as an attribute of the knowledge they produce. In the first of these, objectivity is equated with a requirement of neutrality or disengagement in epistemic agents, and in the second, it is a requirement that knowledge claims should be reliable (empirically adequate, robust,

explanatorily powerful) under extension to some specific class of cases or domains. It is this latter sense that concerns Nozick when he characterizes objectivity as a matter of invariance under various kinds of transformation; more prosaically, objectivity in this epistemic sense requires that knowledge claims should have a capacity to “travel” beyond the contexts of their production (to use Donna Haraway’s evocative term).

Several things become clear when the objectivity of knowledge claims is distinguished from the objectivity epistemic agents. First, the objectivity (*qua* neutrality) of an epistemic agent by no means guarantees the objectivity (*qua* robustness and extendability) of knowledge claims. In the first case we consider (from archaeology), the value commitments of knowers--their non-objectivity--is instrumental contingently in improving objectivity in the second sense; this case illustrates the central points made above about the ways feminist practitioners have contributed to the sciences. Second, when objectivity is considered an attribute of knowledge claims, it is associated with (or is assessed with respect to) a wide range of more specific epistemic values that are not only heterogenous and (usually) incapable of simultaneous satisfaction, but are also contingent and pragmatic (which is to say conventional). Kuhn took this to be one of the important implications of the historical cases analyzed in *The Structure of Scientific Revolutions*;<sup>5</sup> there are inevitable trade-offs to be made between the various epistemic attributes of a knowledge claim associated with its ability “to travel.” Feminist philosophers of science extend this insight, arguing that we must recognize the role that contextual values play in shaping the choices scientists make between epistemic values and their judgements about what counts as adherence to these values (Longino 1995 and 1996). Our second case study (from biology) illustrates how contextual factors shape the local interpretation and the relative weight accorded to various of the epistemic values that are typically at issue in these deliberations.

## *II. How Contextual Values can Promote Epistemic Values*

---

<sup>5</sup> In “Objectivity, Value Judgment, and Theory Choice”, Kuhn argues that “five characteristics—accuracy, consistency, scope, simplicity, and fruitfulness—are all standard criteria for evaluating the adequacy of a theory. ... Nevertheless, two sorts of difficulties are regularly encountered by men [sic] who must use these criteria in choosing [between theories]. ... Individually, the criteria are imprecise: Individuals may legitimately differ about their application to concrete cases. In addition, when deployed together, they repeatedly prove to conflict with one another; accuracy may, for example, dictate the choice of one theory, scope the choice of its competitor” (Kuhn 1977).

*First case study: how feminist standpoints have enriched archaeological practice.* There are any number of cases in which the intrusion of contextual values figure not only as a compromising factor, but also as instrumental in improving the quality of scientific research, judged by quite conventional epistemic standards.<sup>6</sup> Recent developments in American archaeology offer a number of examples along these lines. Tom Patterson documents the difference the G.I. Bill made to the class structure of the discipline, and the ways this has, in turn, changed the questions asked, the hypotheses considered, the skills valued, and the relative influence of distinct regional traditions of archaeological practice in the U.S. (Patterson 1995). This legacy is by no means an unmixed blessing, but on Patterson's account it has played a crucial role in disrupting elitist assumptions and has served to significantly broaden the scope of North America American archaeology.

Another major demographic change in the recent history of North American archaeology has been the influx of women into the field in the 1970s and 1980s. In the space of a decade the representation of women in the field nearly doubled,<sup>7</sup> and by the end of the 1980s, when the first of these cohorts came of professional age, the first substantial work on questions about women and gender began to appear. These developments come much later to archaeology than to

---

<sup>6</sup> For example, the critics who most sharply questioned Burt's widely influential IQ studies seem to have been motivated to scrutinize the "procedural" aspects of his research because it was simply not plausible to them that working class children should consistently prove to be less intelligent than their upper class peers (see, e.g., Kamin, and Layzer, in Block and Dworkin 1976). In a similar vein, Patricia Hill Collins (1991) has recently described the (constructive, critical) difference it makes to be an "insider outsider" to sociology: the misrepresentations of black family structure and employment patterns inherent in post-Moynihan sociology are patently obvious to her as one who brought to her professional training as a sociologist a grounding in the culture, history, and experience of the communities in question.

<sup>7</sup> Before 1973 the representation of women was never more than 13% in the Society for American Archaeology; in that year it jumped to 18%, reaching 30% by 1976 and stabilizing at 36% after 1988 (see Patterson 1995: 81-83; Wylie 1994: 65-66).

neighboring fields (like socio-cultural anthropology and history) where vigorous traditions of feminist research have thrived since the turn of the 1970s. The first paper explicitly advocating feminist approaches to archaeology appeared in 1984 (Conkey and Spector), but it wasn't until 1988-89 that the first conferences on "the archaeology of gender" were organized, the proceedings of which began to appear in 1991. Since that time both critical and constructive projects on "gender" have been taken up by archaeologists working in virtually all major cultural, temporal, and geographical areas of interest, and on the full range of research problems typical of the field.

Recent assessments make it clear, however, that this work is relatively conservative politically and methodologically. From the outset commentators note a reticence among archaeologists working on gender to characterize themselves or their research as feminists, and a tendency to insulate themselves from the well established traditions of feminist research traditions that have developed elsewhere (Hanan and Kelley 1992). Indeed, Conkey and Gero (1997) have recently argued that this has compromised the degree to which "gender research" in archaeology has been able to deliver genuinely new insights. I have made the case that, even though explicitly feminist influences are muted, the growing body of research on gender in archaeology does embody what Longino describes as a "bottom line" maxim for feminist research: a commitment not to disappear gender (Longino 1994). The content of the work that has appeared, as well as the timing and circumstances that gave rise to it, suggests that a standpoint of gender sensitivity--a grass-roots feminist sensibility--played a crucial role in shaping these developments (Wylie 1997b, Hanan and Kelley 1992). Feminist commitments did directly influence most of those who organized the early conferences and publications but, for a growing number of others, the interest in questions about women and gender seems to reflect a latent awareness of contested and contestable nature of gender roles, considered both as a feature of daily life and as a possible topic for investigation in archaeology. This is no doubt a gendered standpoint and one that reflects the indirect influence of the second wave women's movement. Those drawn to "the archaeology of gender" are overwhelmingly women who entered archaeology at a time when their rapidly increasing representation disrupted the gender status quo of the field, and they are often quite explicit in rejecting gender-stereotypes that restrict their professional roles and recognition. Indeed, the early conferences seem to have been attractive because they provided an opportunity to engage broader questions about gender relations at arm's length, on the relatively safe (or at least familiar) terrain of archaeological inquiry. And as diffuse as it is, this standpoint of gender sensitivity has given rise to a number of critical and constructive insights that must be counted as improving our archaeological understanding of the cultural past in at least two respects.

First, the commitment "not to disappear gender" has generated a number of sharply focused and incisive critiques of implicit sexism or androcentrism in established bodies of archaeological knowledge that had gone unnoticed. These critiques typically depend on a combination of conceptual analysis and constructive empirical inquiry; they are not only or primarily destructive critiques but they do provide a systematic (critical) assessment of otherwise unrecognized limitations, errors, and uncertainties in archaeological reconstructions and explanations of the cultural past. Consider, for example, two critical (re)analyses of explanatory models of the Inka

and Aztec states which make it clear that our understanding of state formation processes are seriously flawed if they are identified exclusively with structural dynamics operating in the public, political sphere associated predominantly with male elites, while the domestic sphere associated with women and reproductive labor is treated as a stable (quasi-natural) social substrate. Chris Hastorf (1991) provides compelling evidence (based on analyses of skeletal material, paleobotanical remains, and a sequence of house floors in the Montaro Valley) that households and gender roles were substantially reshaped by the extension of Inka influence into the highland Andes; the household forms encountered at the time of Spanish conquest cannot be projected back into prehistory as if their form was a given, a social foundation unaffected by the rising and falling fortunes of larger state systems. In a similar vein, Liz Brumfiel (1991) argues not just that the Aztec system of economic and political control changed domestic relations, but that it depended fundamentally on the intensified and restructured exploitation of female (domestic) labor, given its basis in extracting tribute in the form of locally produced cloth. She bases this critical analysis, in part, on a reanalysis of well known assemblages of artifacts associated with food and cloth production from urban and hinterland sites in central Mexico Valley.

At a more prosaic level, gender-critical analyses often focus not so much on large-scale explanatory theories and the implications, for them, of leaving women and female-identified domains of activity out of account, but on the consequences of interpreting specific elements of the archaeological record in terms of stereotypic assumptions about gender roles, capabilities, activities and relations that are often profoundly ethnocentric. One straightforward example comes from a reanalysis of skeletal material recovered from archaeological contexts in Australia. Denise Donlon (1993) notes that, as currently catalogued, existing skeletal collections show a vast and implausible preponderance of male specimens. She canvasses various possible explanations for this (e.g., demographic anomalies, differential preservation, curatorial practices) and concludes that the skewed sex ratios reported for Australian Aboriginal skeletal remains probably reflect systematic errors in sex identification due, specifically, to pervasive reliance on measures of “robustness.” Given what is known historically and ethnographically about the activities typical of Aboriginal women, she argues, it is more than likely that they would show much the same levels of skeletal robustness as their male counterparts; it is a mistake to assume that sex difference stereotypes about physical dimorphism which reflect our own much more highly gender segregated activities can be extended to the skeletal record of Australian Aboriginal foraging communities. A range of other such critiques call into question models of subsistence practice which assume, for example, that it is the men in foraging societies who range widely to hunt or gather resources while the women (and children) must be bound to home bases (see, e.g., Sarah Nelson’s account of such critiques, 1997: 88-91), a conception of gender roles that flies in the face ethnographic studies of foraging group practices that have been in print since the early 1970s. In some cases it seems plausible to attribute to women a much more central role socially, and in subsistence practices, than has been typical. For example, Sassaman objects that the transition from mobile to sedentary prehistoric societies in North America should not be attributed automatically to changes in the organization of hunting practices associated with men: “considering...that the change coincides with the adoption of pottery, technology usually attributed to women, an alternative explanation must be considered” (1992: 249). In other cases

re-analysis of the stereotypes that inform interpretation suggests that it may be a mistake to assume any sharp gender-segregation of activities along lines familiar from contemporary contexts. Kelly McGuire and Bill Hildebrandt argue, in this connection, that the evidence suggests “a consistent pattern of relatively undifferentiated gender roles” through the period in which prehistoric cultural groups in California made the transition from a millingstone/handstone technology to bedrock mortars; the milling of major plant resources (e.g., acorns), and transformations in this practice, cannot be ascribed to exclusively to women (1994:51). They conclude that, “if we have learned anything [from] the emerging feminist critique of modern archaeology, it is perhaps the danger in viewing gender relationships as static, or at least limited in range...” (1994: 52).

Often critical analyses of these kinds the catalyst for reframing questions that open up new lines of inquiry; this represents a second way in which the “archaeology of gender” has contributed to archaeology as a whole. In many areas this takes the form of a newly insistent interest in documenting women’s activities that have been assumed inaccessible archaeologically because they are associated with ephemeral and perishable materials. There has been something of a rebirth of interest in netting and basketry, sometimes involving closer examination of surviving fragments of these materials (e.g., Bernick 1998), but often requiring indirect analysis, for example, of the bone awls that would have been used to make nets (Dobres 1995), breakage patterns in bones attributable to food processing rather than primary butchering (Gifford-Gonzalez 1993), and renewed attention to edgewear patterns in utilitarian categories stone tools that had largely been ignored (as described by Gero 1994). In a similar vein, a number of new avenues of inquiry focused on skeletal remains are being explored that promise (and in some cases have delivered) significant insights about the nature and degree to which routine activities, dietary status, and vulnerability to violence and disease, differ along sex lines. Hastorf’s argument for reconsidering standard models of state formation (or, the extension of the Inka power base into distant valleys in the Andean highlands) depends on an analysis that compares reconstructions of lifetime dietary intake based on isotope values with the sexing of skeletons. She finds that, although both types of analysis had been done in the past, it had not been noticed (until she looked) that male and female skeletons show a striking divergence in dietary intake (specifically, their levels of maize consumption) through the period when the Inka first appeared in the Montaro Valley (Hastorf 1991). Questions about “gender” have similarly inspired and/or refocused skeletal analyses that concern, for example, the differential development of muscle attachments and evidence of stress that reflects specific repetitive activities through which it is possible to determine whether, in fact, certain tasks were predominantly performed by women (see the summary provided by Cohen and Bennett 1993, and also Bentley 1996). Finally, a gender-sensitive standpoint has animated a number of research programs in experimental and ethno-archaeology that are designed to develop the background knowledge--the “auxiliary hypotheses”--necessary for recognizing and interpreting evidence of gender dynamics that may be present in the archaeological record but has not been effectively exploited. For example, although ethnographic studies of the “gathering” activities of women in foraging societies published in the 1960s and 1970s provided strong grounds for challenging “man the hunter” models of subsistence practice among prehistoric foraging societies (see, e.g., the summary given by Slocum 1975), they leave unanswered a number of archaeologically crucial questions about

the material correlates of specific gender roles and divisions of labor. Ethnoarchaeological studies that address these questions have been undertaken to good effect in a number of contexts.

In one such study Hetty Jo Brumbach and Bob Jarvenpa (1997) find that hunting practices among the subarctic Dene are much more complex than had been assumed and routinely involve women. They argue that this has important implications for how we understand the historic shift of emphasis from “bush” to “village” centered subsistence activities, and it suggests that standard ascriptions of function to sites and artifacts (as domestic vs. hunting, male vs. female associated) must be reassessed.

At the level of reframing, or proposing substantially new explanatory frameworks for understanding the form and dynamics of cultural systems, “the archaeology of gender” shows less substantial development; as Conkey and Gero (1997) observe, it has been largely an enterprise of “adding women” which has not yet taken the full measure of the implications that follow from its critical and constructive interventions. Some contributors to this literature have pursued the implications of more localized critiques and have been actively exploring more complex (multi-dimensional and multi-factor) explanatory models than has been typical in their areas of interest (e.g., Brumfiel 1992; Tringham 1995). But, in general, it remains an open question what new explanatory and theoretical insights are likely to result from a commitment to ensure that “gender” is on the research agenda of the discipline. Nevertheless, as the “archaeology of gender” grows not only in scope and volume but also in sophistication, it is inescapable that a standpoint of gender sensitivity--in some cases an explicitly feminist standpoint--has put some practitioners (overwhelming women from recent professional cohorts) in a position to identify critical limitations and exploit investigative possibilities that had simply not been recognized by colleagues for whom androcentric and gender stereotypic assumptions have been unrecognized presuppositions of the research enterprise. In these respects archaeological practice has been improved, measured against quite standard epistemic values (e.g., empirical adequacy, explanatory power, internal coherence, external consistency), by a certain incredulity about gender taken-for-granted that reflects a confluence of contextual factors: for example, a demographic shift in the discipline, the direct and indirect influence of feminist scholarship and activism, which together disrupt conventional assumptions about gender roles and relations. In this the late but vigorous development of research on women and gender in archaeology illustrates the kinds of substantive contributions feminist research programs have made to a wide range of social and life sciences in the last thirty years. Whatever their intent--however conservative they may be methodologically and epistemically--these interventions reinforce a growing number of critical voices that have been calling for more systematic and sustained analysis of the conditions (conceptual and socio-political) that shape archaeological practice; a recurrent theme in the “archaeology of gender” literature is the argument that reflexive critique, backed by substantive historical and socio-political analyses of archaeological practice, must be treated as an essential component of the research enterprise. And in this the practice and programmatic recommendations of these practitioners embody Nozick’s third option; they make it clear that the role of values in scientific inquiry is inescapable and that these values can be both constructive and compromising epistemically.

*Second case study: trade-offs among epistemic values and the role of contextual values in the weighing of epistemic values*

In our second case, we consider how the identification and critique of quite fundamental assumptions guiding research in developmental biology by feminist biologists transform the assessment of what was, in 1985, the textbook account of human embryonic sexual differentiation. In a number of respects, this standard account conformed to a broad body of research into fetal development in a variety of mammalian species. But, as we view this case, explicitly feminist perspectives threw into relief some widely-held and deeply submerged contextual values that shaped and organized much of this research, and contributed to the local interpretation and relative weight accorded to various epistemic values.

According to the standard account (see the illustration from Tuchmann-Duplessis *et al*'s *Illustrated Human Embryology* on p. 10A), sex—more precisely, genetic sex—is determined at conception: eggs fertilized by X-chromosome-bearing sperm develop into females and eggs fertilized by Y-chromosome-bearing sperm develop into males.<sup>8</sup> In the six weeks following conception, XX and XY embryos are anatomically identical. Both develop an “indifferent” gonad that includes a set of female (Müllerian) ducts (which in the female will later develop into the uterus, cervix, oviducts, and upper vagina) and a set of male (Wolffian) ducts (which in the male will develop into vas deferens, epididymis, and ejaculatory ducts).

---

<sup>8</sup> We should add “in what is taken to be ‘normal’ fetal development”. It is estimated that 4% of embryos develop into what the medical profession calls “intersexed” individuals (Fausto-Sterling 1993). This assumption of sexual dimorphism is discussed below.

Anatomical differentiation, *i.e.*, the development of gonadic sex, begins in the sixth week. The Y chromosome—more precisely, genetic information present on it—promotes the synthesis of the H-Y antigen in XY embryos; this protein in turn plays a role in the organization of the indifferent gonad into an embryonic testis (the details of how are not yet fully understood<sup>9</sup>). In addition to containing sperm-producing tubules, the embryonic testis is able to synthesize hormones. Two of these, testosterone and MIS (Müllerian Inhibiting Substance), function to promote additional development in a male direction: fetally-synthesized testosterone promotes growth and development of the male duct system; MIS causes the female duct system to “regress” or “degenerate”.

Through the eighth week, the external genitalia of XX and XY embryos are anatomically identical. Sexual differentiation occurs in the ninth through twelfth weeks, when the bi-potential genital tubercle develops into a penis or a clitoris, and tissues called “labioscrotal swellings” develop into a scrotum or large lips of the vagina. Fetal hormones have been found to be instrumental in the sexual differentiation of the bipotential genital tubercle. Specifically, the fetal testis secretes dihydrotestosterone and this influences development of the genital tubercle in a male direction. By twelve weeks, the structures of male external genitalia are evident in XY embryos; by then, the structures of female external genitalia are evident in XX embryos.

As we noted at the outset, this account of human sexual differentiation drew on and was consistent with a broad body of research in biology: e.g., the definitions of sex<sup>10</sup> and commitment to sexual dimorphism are baseline commitments in evolutionary biology, developmental biology, and endocrinology, and the account built on what was known about the anatomical stages of fetal development and sexual differentiation. The emphasis on a causal role for androgens was consistent with investigations in neuro and reproductive endocrinology into the roles of pre-natal androgens in brain organization in various species. And, in general terms, the assumption of gender dimorphism—and, more specifically, the assumption that various factors coded “male”

---

<sup>9</sup> This is an area of current research. See, e.g., Fausto-Sterling 1985; Muller and Urban (1981); and Muller *et al* (1982).

<sup>10</sup> *I.e.*, that males are organisms that produce sperm and females are organisms that produce eggs.

“actively” intervene in what would otherwise be a “normal” and “female” line of development—was consistent with hypotheses in developmental biology, endocrinology, and empirical psychology more broadly. In brief, external consistency contributed to the credibility of this account of human sexual differentiation.

In addition, the various stages of development in human sexual differentiation had parallels in other mammalian species, and the explanation of these stages was in agreement with a broad range of observational and experimental data. In this sense, the account can be said to enjoy generality of scope and, in several ways we have noted, to display accuracy under extension to a range of cases and domains (Nozick) or to have the capacity to “travel” (Haraway). Its generality of scope (in the senses noted) was in turn a function of its simplicity. The hypothesis that it is, in the end, the presence or absence of the Y chromosome that determines the trajectory and details of sexual differentiation and development<sup>11</sup>, streamlines a number of complex processes and variables.

For example, this hypothesis posits two uni-directional courses of development determined by the presence or absence of relatively few variables (Y-chromosome-bearing sperm, the H-Y antigen, the embryonic testis, and dihydrotestosterone). In addition, sexual dimorphism in the classification of embryos and humans, bifurcates a continuum of physiological, chromosomal, and hormonal traits using the simpler and more manageable categories of male and female. This in turn permits the classification of hermaphrodites and two types of pseudo-hermaphrodites as “intersexed” (rather than as distinct sexes) and focuses investigations of the development of such individuals on “abnormalities” (e.g., Fausto-Sterling 1993). Further, of the hormones synthesized by both XX and XY embryos—including androgens, estrogens, and progesterones—this explanation only invokes the first group; according to it, a male course of development occurs when these are present and a female course when they are absent. In addition, causal relationships posited between hormones and stages of development are assumed to be linear or uni-directional (cf. Longino and Doell 1983; Longino 1990). As a final example, potential complicating factors in the maternal environment, e.g., the hormones derived from the placenta and maternal circulation, are assumed to play no causal role in sexual differentiation (although there has been concern about why XY embryos are not “feminized” by the abundance of estrogen and progesterone in their environment).

---

<sup>11</sup> This is shorthand, of course, for “the Y chromosome promotes the synthesis of the H-Y antigen, which in turn promotes the development of the embryonic testis, which in turn is able to synthesize testosterone and MIS, and the former promotes further developments of male ducts and external genitalia while the latter promotes degeneration of female ducts”.

For all of these reasons, the standard account was widely influential and tenacious, and it led to and guided fruitful research (*i.e.*, research yielding predictions and reproducible results): for example, investigations of how the H-Y antigen functions to promote the development of the embryological testis; of which genes on the Y chromosome promote the synthesis of the H-Y antigen and how; and of how dihydrotestosterone promotes the growth of male external genitalia.

By the early 1980's, however, feminists in biology had begun to criticize this account as well as the general model of fetal development that shared features of it. In doing this, they drew attention to features of evaluative epistemic judgment important to Kuhn: specifically, that the epistemic values associated with the capacity of a knowledge claim “to travel” cannot all be maximized together and that trade-offs will be inevitable. We take feminist critiques of the standard account to make two arguments. One argument is that the value researchers attributed to simplicity and generality of scope *in one sense*, brought too high a cost in terms of generality of scope *in another sense*, explanatory power, and empirical adequacy. The second argument is that contextual values, albeit unrecognized by the scientists involved, were a factor in these choices. We will argue, further, that it was the contextual values that feminist biologists brought to bear that *revealed* these trade-offs among epistemic values and also revealed the role of (submerged) contextual values. We address each point in turn.

The trade-offs we are positing between simplicity and generality of scope in one sense, on the one hand, and generality of scope in another sense, explanatory power, and empirical adequacy, on the other, are perhaps most obvious when we consider the argument made by feminist biologists that the textbook account of sexual differentiation is an account of *male* sexual differentiation, not *embryonic* sexual differentiation. Fifteen years ago, feminists in biology began to analyze the role and consequences of what John Money referred to as “the Adam principle”: a commitment he attributed to research into fetal development and described as the principle that something is *added* to an embryo to make it a male (Money 1970, 5). As feminist biologists have recognized, the corollary of this hypothesis is that the female and female fetal development are, respectively, the default state and default trajectory of fetal development (Fausto-Sterling 1985, Gilbert et al 1988).

Neither formulation rules out the investigation of female development. But as feminist biologists recognized, the “presence” and “absence” metaphors informing the textbook account are aptly ascribed to the account itself. For one thing, largely absent from most versions of this account—and, until recently, largely absent in the bulk of research into fetal development—are discussions of or investigation into the role of entities classified as “female”: the hormones, estrogens and progesterone; the fetal ovary; the maternal environment; and to some extent, the X chromosome.<sup>12</sup> For example, until recently, when “a positive role for estrogen began to creep

---

<sup>12</sup> So, for example, feminist biologists have criticized the emphasis on the role of androgens in research into fetal brain development, including the hypothesis that testosterone “organizes” the developing male fetal brain and serves as the basis for sexually-differentiated brains and behavior in a variety of species. The roots of organizer hypotheses lie in research in reproductive endocrinology; as early as the 1960's, some in the field had posited “male brains” and “female brains” in rats and the hypothesis has since been

into parts of the literature”, what research there had been into the role of estrogen was devoted to the question, earlier noted, of why the developing male embryo was not feminized by them (Fausto-Sterling 1987, 67). And a review article published in the 1980's, while unusual because it mentioned a possible role of hormones derived from fetal ovary, also illuminated the lack of information about such a role: “Embryogenesis normally takes place in a sea of hormones... derived from the placenta, the maternal circulation, the fetal adrenal glands, the fetal testis, and *possibly* from the fetal ovary itself...” (emphasis added).<sup>13</sup>

---

extended to investigations of other species (see, e.g., Bleier 1984 and 1988; Longino 1990; and Nelson 1996).

<sup>13</sup> Wilson, George, and Griffin “The Hormonal Control of Sexual Development”, quoted in Fausto-Sterling 1985, 81.

More to the point, feminists in biology point out that the textbook account does not constitute an *explanation* of female sexual differentiation and development. The account maintains that “in the *absence* of male factors” of various sorts, the indifferent gonad of the XX embryo “develops into the internal female reproductive system” and the male ducts “degenerate”, and it notes that the female structures of external genitalia “become *evident* by twelve weeks”.<sup>14</sup> But, as Fausto-Sterling notes, *how* these things occur had received relatively little attention through the 1980's, and what resulted “in a supposedly general account of the development of the sexes ... [was] in actuality only an account of male development” (Fausto Sterling 1987, 64).<sup>15</sup>

The view that females develop from mammalian embryos deficient in male hormone seems, oddly enough, to have satisfied the scientific curiosity of embryologists for some time, for it is the only account one finds in even the most up-to-date texts (page). ... How does [female development] happen? What are the mechanisms?” (Fausto-Sterling 1987, 66).

The review article earlier mentioned does note that at about the same time that the XY gonad begins to make testosterone, the XX gonad “appears to begin synthesizing large quantities of estrogen”. Reflecting the lack of interest in and investigations into the role of the fetal ovary, Fausto-Sterling asked, “Just what does all that estrogen do?” (Fausto-Sterling 1985, 81).

The critiques so far summarized question the empirical adequacy, explanatory power, and generality of scope of the textbook account of sexual differentiation on the grounds that it does not explain female sexual differentiation. But feminist biologists have also argued that lack of information about the mechanisms of female sexual differentiation, about the role of hormones coded ‘female’, and about the role of the maternal environment, raises questions about the empirical adequacy of the textbook account of *male* sexual differentiation.

Consider, for example, the role of the X chromosome in embryonic sexual differentiation. The Y chromosome promotes the synthesis of the H-Y antigen which in turn promotes the organization of embryonic testes. The testis in turn are able to synthesize testosterone and MIS... and so forth. But it is not the presence of the Y chromosome that determines the ability to synthesize testosterone and MIS. It is genetic information on the X chromosome and on one or more of the twenty-three pairs of non-sex chromosomes that code for androgens and estrogens. So, while it seemed clear that the Y chromosome is somehow involved in the selective translation of some of this information, its role—rather than the mechanisms that determine “identical road maps for

---

<sup>14</sup> E.g., Bruce M. Carlson’s *Patten’s Foundations of Embryology*, a widely used undergraduate embryology text, states “In the absence of the H-Y antigen the gonad later becomes transformed into an ovary”, and that “the external genitalia ... develop... in the female direction if the influence of testosterone is lacking” (pp. 459-61).

<sup>15</sup> With the exception, noted above, of questions as to why XY embryos are not feminized by female hormones.

sexual development” in XX and XY embryos—received the bulk of the attention (Fausto-Sterling 1985, 79-85).

Feminist biologists also argue that conclusions can not be drawn from research apparently establishing the effects of pre-natal androgens until a similar amount of research is done on the organizing effects of pre-natal estrogens. The fetal environment is rich in both, males and females synthesize both (it is the amounts that differ), and there are continuous conversions among the three families of sex hormones (e.g., Bleier 1984 and 1988; Fausto-Sterling 1985). The difficulties in isolating the effects of these hormones are attested to by the recent reversal of claims that an “organizing effect” on fetal rat brains that researchers had attributed to androgens and linked to behavior they characterized as “masculine”, is now attributed to estrogen converted from testosterone by brain cells (Fausto-Sterling 1987; Nelson 1996). And, as the review article earlier mentioned puts the point, the “sea of hormones” in which embryogenesis occurs includes hormones derived from the placenta and the maternal circulation; but, relatively little attention has been paid to the possible role of these hormones in either male or female development.<sup>16</sup>

Finally, feminist biologists have criticized the uni-directional model of male sexual differentiation, citing experimental results that indicate complex and often non-linear interactions between cells, and between cells and the maternal and external environments, during every stage of fetal development (Bleier 1984 and 1988, Fausto-Sterling 1985, and Hubbard 1982).

It is on the basis of these several lines of argument that feminist biologists call into question hypotheses isolating so-called “male” hormones and citing these as the discrete cause of discrete effects and events in fetal development, and the empirical adequacy and explanatory power of the textbook account of sexual differentiation (Bleier 1984, Fausto-Sterling 1985 and 1987, Longino and Doell date). In the process, they illuminate a number of underlying evaluative judgements. One is the privileging of simplicity at the cost of generality of scope, empirical adequacy, and explanatory power. This issue is implicit in their critiques of the emphasis on so-called male factors; of the assumption that one line of fetal sexual development is the “default” direction; of

---

<sup>16</sup> Indeed, Fausto-Sterling attributes some of the recent interest in investigating a positive role for estrogen to the discovery that, in some cells of the body, testosterone may be converted into estrogen — not in interest in estrogen on its own terms (Fausto-Sterling 1987, 67).

the lack of attention to the role of estrogens, fetal ovary, and maternal environment in fetal development, and of the lack of explanation of female development.

Perhaps less obvious, because it is so deeply embedded in the biological sciences, and in social practices and institutions, is the simplifying role of the assumption of sexual dimorphism. On this principle, organisms that lie along a continuum in terms of physiological, chromosomal, and hormonal traits are organized into two categories, obscuring complexities and processes that might be visible with different sorting. As Fausto-Sterling notes, “Biologically speaking, there are many gradations running from female to male; and depending on how one calls the shots, one can argue that along that spectrum lie at least five sexes—and perhaps even more” (Fausto-Sterling 1993, 21; cf. Bleier 1984). On the one hand, the commitment to sexual dimorphism is consistent with the definition of the sexes as “egg producing” and “sperm producing” organisms. The commitment has also led to and guided fruitful research in a number of areas. For example, given its assumption, males and females provide a “natural” base line for investigating the relationships between hormones, neural events, and behavior with which reproductive and neuro endocrinology are concerned (cf. Longino 1990 and Nelson 1996). On the other hand, even if the commitment does not reflect an “over” simplification, feminist biologists argue that it has led to over simplifications in research into fetal development and in the textbook account of sexual differentiation. As we have seen, it organizes the classification of fetally-synthesized hormones and the investigation of their effects. Given that males and females produce, and develop within environments containing, three families of sex hormones, among which there are continuous conversions, feminist biologists maintain that explanations based on such classifications are over simplified.

We have so far emphasized ways in which feminist critiques reveal underlying epistemic judgments to privilege simplicity and generality of scope (in the sense of applicability to a variety of species) over empirical adequacy, explanatory power, and generality of scope in another sense. But these critiques also illuminate how contextual values can be a factor in epistemic evaluations.

One of the contextual values that feminist biologists have identified as guiding research into sexual differentiation is androcentrism, a value whose role in scientific theorizing was largely unrecognized prior to feminist science scholarship.<sup>17</sup> Examples of androcentrism in the present

---

<sup>17</sup> We focus here on androcentrism, but feminist biologists have identified the role of other contextual values in this and other areas of research in the life sciences. For example, works in the bibliography by Ruth Bleier, Scott Gilbert, Ruth Hubbard, and Evelyn Fox Keller identify relationships between socio-political contexts and commitments to linear, hierarchical models of biological processes. Gilbert analyzes the role and consequences of the “dominant” and “subordinate”, “husband” and “wife”, and “male” and “female”, metaphors attributed, respectively, to the nucleus and cytoplasm in debates over the respective roles of these in developmental biology earlier in this century. And Bleier, Hubbard, and Keller analyze the commitment to “dominant” and “subordinate” entities in the “executive DNA” model. In addition, Fausto-Sterling 1993 argues that the commitment to sexual dimorphism is as much a function of contextual values as it is of biological theory.

case include the emphasis on male development and relative lack of interest in the mechanisms of female development; the association of males (and entities associated with them, e.g., the Y chromosome and testosterone) with activity and females (and entities associated with them, e.g., the fetal ovary and estrogens) with passivity; and the association of males with “presence” and of females with “lack”. These associations are reflected in the assumption that there is “an intrinsic tendency [of the fetus] to develop according to a female pattern of body structure and behavior” that male factors “must actively counteract” (Goy and McEwen 1980),<sup>18</sup> and in the assumption that the fetal ovary, estrogens, and maternal environment have no causal role in sexual differentiation and development. These associations are also reflected in the names given to the sex hormones: ‘androgen’, from the Greek *andros* and Latin *generare*, means “to make a male”; ‘estrogen’, from the Latin *oestrus*, means “gadfly” or “frenzy”. As Fausto-Sterling notes, “... *gynogen* would be etymologically and biologically correct as a counterpart to *androgen* ... [but it] cannot be found in biological accounts of sexual development (or, for that matter, in any dictionary)” (Fausto-Sterling 1987, 66). The issue Fausto-Sterling raises is not, of course, about language. The name given to hormones coded females reflects the lack of causal role attributed to them in either female or male fetal development, including sexual differentiation.

In our introduction to this case, we noted that we take one of the general arguments made by feminist biologists to be that contextual values, albeit unrecognized by the scientists involved, were a factor in the choices made between simplicity, generality of scope *in one sense*, and interconnectedness, on the one hand, and generality of scope *in another sense*, explanatory power, and empirical adequacy, on the other. We can now be somewhat more specific. In this case, the privileging of simplicity and generality of scope in one sense, over explanatory power, empirical adequacy, and generality of scope in another sense was made possible by androcentrism: by interest in the stages and mechanisms of male sexual differentiation, and by the organization and classification of lines of development, entities, and processes along the lines of gender as well as sexual dimorphism, inclusive of associations of males with “activity” and “presence” and of females with “passivity” and “lack”.

### *Implications for objectivity*

On the one hand, then, the cases we have considered (and we take them to be representative) are ones in which feminist, or gender-sensitive, scientists have identified ways in which contextual values have unduly limited or shaped the questions pursued, the observations made, the interpretations of results, and the hypotheses generated. Typically these effects arise and persist because scientists are unaware of the values informing the research traditions in which they are educated and work, particularly when these involve a gendered dimension; for example, our presuppositions about gender roles and relations, attributes and identities, are so deeply entrenched and so pervasive that they are among the contextual values scientists are most

---

<sup>18</sup> An undergraduate embryology text by Bruce Carlson also discusses “the natural tendency of the body to develop along female lines in the absence of other modifying influences” (Carlson 1981, 459-61).

unlikely to recognize. One important implication of the critiques we have summarized and others similar to them is that, while feminists show how various well established programs of scientific research may be improved upon, these programs cannot be written off as categorically “bad science”;<sup>19</sup> the issues of evaluative judgement involved are much more complex.

On the other hand, and by extension of this point, the cases we have considered suggest that the role played by values in scientific research must be understood to be various, contingent, and complex. While feminists have documented myriad ways in which contextual values have limited or unduly shaped research in a number of sciences, they also show (by example) that contextual values can lead to advances; indeed, sometimes crucial epistemic advances depend fundamentally on the disruptive intrusion of such values. This generates the infamous “paradox of bias.” The problem we face—those of us who are committed to understanding and improving scientific practice—is no longer plausibly that of “cleansing” science of intrusive values but, rather, that of determining what kinds of contextual factors, under what circumstances, are likely to advance the cause of science in specific ways, where the goals and standards of science are themselves evolving, open to negotiation.

At the outset, we identified two questions of central importance to feminist philosophy of science:

- how are we to reconceptualize objectivity so as to undercut dichotomous/polarizing patterns of argument which take objectivity to be inimical to the view of science as value-infused?
  
- how exactly do ‘values’ of various kinds make a difference to objectivity, in one or another specific sense?

---

<sup>19</sup> As we earlier noted, the textbook account has led to fruitful research and reproducible results, is well connected to a broad range of research hypotheses and theories in the biological sciences, and enjoys generality of scope in terms of applicability to a variety of species. In these several ways, this account is similar to other theories feminist scientists have criticized.

The case studies we have considered suggest a strategy for addressing the question of “how exactly?”. They make it clear (we submit) that this question cannot be pursued from philosophical armchairs; adequate answers to it will require systematic, naturalized programs of research in which the roles of epistemic and contextual values in specific research programs and disciplines are investigated. The methodological assumptions guiding such investigations will have to be sufficiently open-ended to allow for the identification of local and context-specific assumptions and judgments concerning adherence to and balancing of epistemic values, for the identification of the pragmatic considerations that function in the trade-offs between epistemic values, and for the identification of the possible role of contextual values (submerged or explicit) that might be positive or negative.

This naturalizing proposal shapes, in turn, the insights emerging from feminist science and philosophy of science about how the first question may be answered, how objectivity might best be re-conceptualized. Insofar as we construe this as a question about the epistemic (or other) stance appropriate to scientists—as individuals or communities—the complex relationships between science and values revealed by feminist practice suggest the need to value reflexivity rather than “value neutrality”: the prospects for enhancing the objectivity of scientific knowledge are improved, not by suppressing contextual values, but by subjecting them to systematic scrutiny and by making such critical self-reflection a key component of good scientific practice (Bleier 1984, 203-205; Keller 1982; Longino 1990; Nelson 1990, Chapter 7). The rationale for this recommendation should be obvious. Objectivity *qua* neutrality (in epistemic agents) does not ensure that knowledge claims will exemplify some set of objectivity-related virtues; there are numerous cases from the history of science (in addition to the feminist examples we have described) which suggest that the integration of contextual values is sometimes a necessary condition for realizing important scientific advances. In short, it is a *contingent matter* what contextual conditions—collective or individual values, forms of community organization—are optimal for realizing objectivity, of specific kinds, in the products of inquiry.

When we consider the question of how best to reconceptualize objectivity as an attribute of knowledge, we take Nozick’s proposal—that objective knowledge is that which is invariant under various kind of transformation—to capture important intuitions: objective knowledge is that which maximizes some specific set of epistemic values having to do with the reliability of knowledge claims, including their accuracy under extension to some range of cases or domains, and their capacity for extension to new cases and domains. The second case we considered illustrates the point that the epistemic values associated with this capacity “to travel” cannot be maximized together. Trade-offs are inevitable and judgements about which values to privilege (and/or how to assess adherence to these values) are radically under-determined by evidence and good reasons intrinsic to the science itself; they are necessarily pragmatic and, in this, reflect some range of values, whether submerged or explicit, that inform the practice of the community (cf. Kuhn 1977; Longino, 1994, 1995, and 1996; Wylie 1995). These cases also indicate that the limitations associated with some contextual values (e.g., androcentrism) may be identifiable only when some practitioners bring a new angle of vision to bear on the problems and assumptions of a research tradition; at crucial junctures, contextual values that cut across the grain can generate significant advances in understanding. The challenge for a naturalized (and socialized)

philosophy of science committed to exploring Nozick's third option is, then, to delineate and assess the effects that particular institutional structures, community dynamics, contextual interests and values have on the forms of practice they (partially) constitute.

## Bibliography

- Bernstein, Richard, *Beyond Objectivism and Relativism*, University of Pennsylvania Press, Philadelphia, 1983.
- Bleier, Ruth (ed.), *Feminist Approaches to Science*, Pergamon Press, New York, 1988.
- Bleier, Ruth, *Science and Gender: A Critique of Biology and Its Theories on Women*, Pergamon Press, New York, 1984.
- Block, N. J., and Gerald Dworkin, *The IQ Controversy*, Random House, New York, 1976
- Brumfiel, L. M. 1991. Weaving and cooking: Women's production in Aztec Mexico. In *Engendering Archaeology*, ed. J. M. Gero and M. W. Conkey. Oxford: Blackwell's Press, pp. 224-253.
- Brumbach, H. J., and R. Jarvenpa. 1997. Ethnoarchaeology of subsistence space and gender: A subarctic Dene case. *American Antiquity* 62.3: 414-436.
- Callebaut, Werner (organizer and moderator), *Taking the Naturalistic Turn, or How Real Philosophy of Science is Done*, University of Chicago Press, Chicago, 1993.
- Carlson, Bruce M., *Patten's Foundations of Embryology*, McGraw Hill, New York, 1981.
- Conkey, M. W., and J. M. Gero, "Gender and Feminism in Archaeology," *Annual Review of Anthropology* 26(1997): 411-37.
- Conkey, M. W., and J. D. Spector, "Archaeology and the Study of Gender," in *Advances in Archaeological Method and Theory*, volume 7, ed. M. B. Schiffer, Academic Press, New York, 1984, pp. 1-38.
- Collins, Patricia Hill, "Learning from the Outsider Within," in *Beyond Methodology: Feminist Scholarship as Lived Research*, edited by M. M. Fonow and J. A. Cook, Indiana University Press, Bloomington, 1991, pp. 35-59.
- Dobres, M-A. 1995. Gender and prehistoric technology. *World Archaeology* 27.1: 25-49.
- Donlon, D. 1994. Imbalance in the sex ratio in collections of Australian Aboriginal skeletal remains. In *Women in archaeology: A feminist critique*, ed. H. duCros and L. Smith. Canberra: Australian National University Occasional Papers, pp. 98-103.
- Fausto-Sterling, Anne, "The Five Sexes: Why Male and Female Are Not Enough", *The Sciences*, March/April 1993.
- Fausto-Sterling, Anne, "Society Writes Biology/Biology Constructs Gender", *Daedalus* 116, 61-76, 1987.
- Fausto-Sterling, Anne, *Myths of Gender: Biological Theories about Women and Men*, Basic Books, New York, 1985.
- Galison, Peter and David Stump (eds.), *The Disunity of Science*, Stanford University Press, Stanford, 1996.
- Gilbert, Scott, "Cellular Politics", in *The American Development of Biology*, ADD TO

- Gilbert, Scott et al, "The Importance of Feminist Critique for Cell Biology", in Nancy Tuana (ed.), *Feminism and Science*, Indiana University Press, 1988.
- Goy, Robert W. and Bruce S. McEwen (eds.), *Sexual Differentiation of the Brain*, MIT Press, Cambridge MA, 1984.
- Haack, Susan, "Epistemological Reflections of an Old Feminist", *Reason Papers* 18, 1993.
- Hanen, M. P. and J. Kelley, "Gender and Archaeological Knowledge," in *Metaarchaeology: Reflections by archaeologists and philosophers*, ed. L. Embree, Reidel, Boston, 1992, pp. 195-227.
- Hastorf, C. A. 1991. Gender, Space, and Food in Prehistory. In *Engendering Archaeology*, ed. J. M. Gero and M. W. Conkey. Oxford: Blackwell's Press, pp. 132-159.
- Hubbard, Ruth, *The Politics of Women's Biology*, Rutgers University Press, New Brunswick NJ, 1990.
- Hubbard, Ruth, "Have Only Men Evolved?," in R. Hubbard, M. Henifin, and B. Fried (eds.), *Biological Woman—The Convenient Myth*, Schenkman, Cambridge MA, 1982.
- Keller, Evelyn Fox, *Reflections on Gender and Science*, Yale University Press, New Haven, 1985.
- Keller, Evelyn Fox, *A Feeling for the Organism: the Life and Work of Barbara McClintock*, W.H. Freeman, New York, 1983.
- Keller, Evelyn Fox, "Feminism and Science", *Signs* 7(3), 1982.
- Kohlstedt, Sally Gregory and Helen Longino (eds.), *Women, Gender, and Science: New Directions*, *Osiris* 12 (1997).
- Kornblith, Hilary (ed.), *Naturalizing Epistemology*, 2<sup>nd</sup> edition, MIT Press, Cambridge, 1993.
- Kuhn, Thomas, "Objectivity, Value Judgment, and Theory Choice", in Janet Kournay (ed.), *Scientific Knowledge: Basic Issues in the Philosophy of Science*, Wadsworth Publishing Co., Belmont CA 1987. Reprinted from Thomas Kuhn, *The Essential Tension: Selected Studies in Scientific Tradition and Change*, University of Chicago Press, Chicago, 1997: 320-339.
- Lloyd, Elisabeth, "Objectivity and the Double Standard for Feminist Epistemologies", *Synthese* 104(3), 1995: 351-381.
- Longino, Helen E., "Cognitive and Non-Cognitive Values in Science: Rethinking the Dichotomy", in Nelson and Nelson (eds.), 1996.
- Longino, Helen E., "Gender, Politics, and the Theoretical Virtues", *Synthese* 104(3), 1995: 383-397
- Longino, Helen E., "In Search of Feminist Epistemology" in *Monist: Feminist Epistemology—For and Against*", *Monist* 77.4 (1994): 472-485.
- Longino, Helen E. and Ruth Doell, "Body, Bias, and Behavior: A Comparative Analysis of Reasoning in Two Areas of Biological Science", *Signs* 9, 1983.
- Maffie, James, "Recent Work on Naturalized Epistemology", *American Philosophical Quarterly* 27.4 (1990): 281-293.

- Mcguire, K. R., W. R. Hildebrandt. 1994. The possibilities of women and men: Gender and the California millingstone horizon. *Journal of California and Great Basin Archaeology* 16.1: 41-59.
- Megill, Alan (ed.), *Rethinking Objectivity*, Duke University Press, Durham NC, 1994.
- Money, John, *Love and Lovesickness*, Johns Hopkins Press, Baltimore MD, 1970.
- Muller, U. and E. Urban, "Reaggregation of Rat Gonadal Cells *in vitro*: Experiments on the Function of H-Y Antigen", *Cytogenetics and Cellular Genetics* 31, 1981: 104-107.
- Muller, U. et al, "Ovarian Cells Participate in the Formation of Tubular Structures in Mouse/Rat Heterosexual Gonadal Co-Cultures", *Differentiation* 22 (1982): 136-138.
- Nelson, Lynn Hankinson, "Empiricism without Dogmas" in Nelson and Nelson (eds.), 1996.
- Nelson, Lynn Hankinson, "Feminist Naturalized Philosophy of Science", *Synthese* 104(3).
- Nelson, Lynn Hankinson (ed.), *Synthese* 104(3): Feminism and Science (1995).
- Nelson, Lynn Hankinson, *Who Knows: From Quine to a Feminist Empiricism*, Temple University Press, Philadelphia, 1990.
- Lynn Hankinson Nelson and Jack Nelson (eds.), *Feminism, Science, and the Philosophy of Science*, Kluwer Academic Press, Dordrecht.
- Nelson, Lynn Hankinson and Jack Nelson, "Feminist Values and Cognitive Virtues", *PSA II* (1994).
- Nelson, Sarah M. *Gender in Archaeology: Analyzing Power and Prestige*, AltaMira Press, Walnut Creek CA, 1997.
- Nozick, Robert, "Invariance and Objectivity", Presidential Address to the American Philosophical Association, Eastern Division Annual Meeting, December 1997.
- Patterson, Thomas C., *Toward a Social History of Archaeology in the United States*, Harcourt Brace & Co., 1995.
- Progress in Brain Research* 61, 1984.
- Rouse, Joseph, *Knowledge and Power: Toward a Political Philosophy of Science*, Cornell University Press, Ithaca NY, 1987.
- Sassaman, K. 1992. Lithic technology and the hunter-gatherer sexual division of labor. *North American Archaeologist* 13: 249-262.
- Scheffler, Israel, *The Anatomy of Inquiry*, Bobbs-Merrill, New York, 1973.
- Schmidt, Frederick F. (ed.), *Socializing Epistemology*, Rowman and Littlefield, Lanham MD, 1994.
- Science* 211: Special Issue on Sex Differences, 198.1
- Short, R.V., "Sex Determination and Differentiation", in C.R. Austin and R.V. Short (eds.), *Embryonic and Fetal Development*, Cambridge University Press, London, 1972.
- Slocum, S., "Woman the Gatherer: Male Bias in Anthropology," in *Toward an Anthropology of Women*, ed. R. Reiter, Monthly Review Press, New York, 1975, pp. 36-50.

Suppe, Frederick (ed.), *The Structure of Scientific Theories*, Second Edition, University of Illinois Press, Chicago, 1983.

Tringham, Ruth E., "Archaeological Houses, Households, Housework and The Home," in *The Home: Words, Interpretations, Meanings, And Environments*, ed. D. Benjamin and D. Stea, Avebury Press, . Aldershot, 1995, pp. 76-107.

Watson, P. J., and M. C. Kennedy. The development of horticulture in the Eastern Woodlands of North America: Women's role. In *Engendering Archaeology: Women and Prehistory*, ed. J. M. Gero and M. W. Conkey. Oxford: Basil Blackwell Press, pp. 255-275.

Wylie, Alison, "The Feminism Question in Science: What Does It Mean to 'Do Social Science as a Feminist'?", delivered at "Doing Archeology as a Feminist", School of American Research Seminar, April 1998.

Wylie, Alison, "Good Science, Bad Science, or Science as Usual? Feminist Critiques of Science" in *Women in Human Evolution*, edited by Lori D. Hager, Routledge, New York, 1997a, pp. 29-55.

Wylie, Alison, "The Engendering of Archaeology: Refiguring Feminist Science Studies," *Osiris* 12(1997b): 80-99.

Wylie, Alison, "The Trouble With Numbers: Workplace Climate Issues in Archaeology," in *Equity Issues for Women in Archaeology*, edited by M. C. Nelson, S. M. Nelson and A. Wylie, Archaeological Papers of the American Anthropological Association, Number 5, 1995, pp. 65-71.

Wylie, Alison, "Doing Philosophy as a Feminist: Longino on the Search for a Feminist Epistemology", *Philosophical Topics* 23.2 (1995): 345-358.