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# The Engendering of Archaeology Refiguring Feminist Science Studies

*By Alison Wylie\**

## INTERNAL CRITIQUES: THE SOCIOPOLITICS OF ARCHAEOLOGY

In the last fifteen years archaeologists have been drawn into heated debates about the objectivity of their enterprise. These are frequently provoked by critical analyses that demonstrate (with hindsight) how pervasively some of the best, most empirically sophisticated archaeological practice has reproduced nationalist, racist, classist, and, according to the most recent analyses, sexist and androcentric understandings of the cultural past. Some archaeologists conclude on this basis that however influential the rhetoric of objectivity may be among practitioners, the practice and products of archaeology must inevitably reflect the situated interests of its makers. A great many others regard such claims with suspicion, if not outright hostility. They maintain the conviction—a central and defining tenet of North American archaeology since its founding as a profession early in this century—that archaeology is, first and foremost, a science and that, therefore, the social and political contexts of inquiry are properly external to the process of inquiry and to its products.<sup>1</sup>

The feminist critiques of archaeology on which I focus here are relative newcomers to this growing tradition of internal “sociopolitical” critique. Not surprisingly, they have drawn sharply critical reactions that throw into relief the polarized positions that dominate thinking about the status and aims of archaeology. And yet, I will argue, these feminist interventions do not readily fit any of the epistemic options defined in this debate; they exemplify a critical engagement of claims to objectivity

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<sup>1</sup> As in many social sciences, archaeologists have set enormous store in establishing the scientific credibility and authority of their discipline and its products in the last thirty years. In North America this took the form of widespread commitment to the proscience, explicitly positivist goals of the New Archaeology, which embody objectivist ideals in an especially stringent form. Reconstructive hypotheses were to be treated as the starting point, not the end point, of research, and any investigation of the archaeological record was to be designed (on a hypothetico-deductive model of confirmation) as an empirical test of these hypotheses; whatever their sources, they were to be confronted with evidence from the surviving record of the pasts they purport to describe and accepted or rejected on this basis. The expectation was that a rigorously scientific methodology would preserve archaeologists from the pernicious influence of standpoint-specific interests and power relations as they either operate within the field or impinge on it from outside; they would ensure that archaeology is “self-cleansing” of intrusive bias and therefore produces genuine (i.e., objective) knowledge of the cultural past. These developments are discussed in more detail in Alison Wylie, “The Constitution of Archaeological Evidence: Gender, Politics, and Science,” in *Disunity and Contextualism: New Directions in the Philosophy of Science Studies*, ed. Peter Galison and David Stump (Palo Alto, Calif.: Stanford Univ. Press, 1996), pp. 311–343.

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that refuses reductive constructivism as firmly as it rejects unreflective objectivism. This is a strategic ambivalence that holds enormous promise and is typical of much feminist thinking in and about scientific practice. In this essay I first characterize what I will identify provisionally as the feminist initiatives that have emerged in archaeology since the late 1980s (qualifications of this designation come later) and then consider their larger implications. My immediate concern is how, within the rubric of feminist science studies, we are to understand the late and rapid emergence of an archaeological interest in questions about women and gender. This leads, in turn, to a set of reflexive questions about how to do feminist science studies.

### *Feminist Critiques in Archaeology*

Critiques of sexism and androcentrism in archaeology fall into two broad categories that parallel analyses of other dimensions of archaeological practice (e.g., its nationalism, classism, and racism): “content” and “equity” critiques. In addition—and in this feminist critiques are distinctive—there is emerging a move toward “integrative” analyses that combine content and equity critiques.

*Content Critiques.* Two types of content critique can usefully be distinguished. The first draws attention to erasure, to ways in which the choice of research problem or the determination of significant sites or periods or cultural complexes leaves out of account women and gender even when they are a crucial part of the story to be told.<sup>2</sup> For example, Anne Yentsch delineates previously unacknowledged patterns of change in the ceramic ware of domestic assemblages that testify to the gradual transfer of women’s productive activities (specifically, domestic dairy production) from the home to commercial enterprises whenever these became capable of industrialization; she argues that this largely unexamined process of appropriation of “women’s work” is crucial for understanding the transformation of the rural economy in the northeastern United States through the eighteenth and nineteenth centuries. Similarly, Donna Seifert describes the difference it makes to our understanding of the archaeology of urban centers if we take seriously the presence of prostitutes, for example, in “Within Sight of the White House.” And Cheryl Claassen draws attention to the rich insights that follow from a focus on the shellfishing activities associated primarily with women and children in the Shell Mound Archaic. To take a prehistoric example that I will discuss in more detail later in this essay, Pat Watson and Mary Kennedy argue that dominant explanations of the emergence of horticulture in the Eastern Woodlands share a common flaw: although women are presumed to have been primarily responsible for collecting plants under earlier gatherer-hunter/foraging subsistence regimes and for cultivating them when gourds and maize were domesticated, they play no role at all in accounts of how this profoundly

<sup>2</sup> These critiques closely parallel those that draw attention to the archaeological record of, for example, colonial and neocolonial domination in areas where archaeology has focused on the “eclipsed civilizations” or hominid origins of much earlier periods (e.g., in Latin America and Africa), that of slaves on plantation sites where the “great houses” and lives of landholding planters had been the primary focus of archaeological attention, and that of First Nations communities in areas long occupied by Euro-Americans that were not recognized because their patterns of settlement did not conform to the European model of nucleated villages. These examples are discussed in more detail in Alison Wylie, “Evidential Constraints: Pragmatic Empiricism in Archaeology,” in *Readings in the Philosophy of Social Science*, ed. Lee McIntyre and Michael Martin (Cambridge, Mass.: MIT Press, 1994), pp. 747–766.

culture-transforming shift in subsistence practice was realized.<sup>3</sup> Watson and Kennedy say they are “leery” of explanations that remove women from the one domain granted them as soon as an exercise of initiative is envisioned.

Often, however, straightforward erasure is not the problem; and so a second sort of critique is required, one that focuses on how women and gender are represented when they are taken into account. From the outset feminist critics have emphasized that, although questions about women and gender have never been on the archaeological research agenda, archaeological research problems and interpretations are routinely framed in gendered terms.<sup>4</sup> The functions ascribed to artifacts and sites are often gender specific, and models of such diverse cultural phenomena as subsistence practices among foragers, social organization in agrarian societies, and the dynamics of state formation often turn on the projection onto prehistory of a common body of presentist, ethnocentric, and overtly androcentric assumptions about sexual divisions of labor and the status and roles of women. Women in prehistoric foraging societies are presumed to be tied to “home bases” while their male counterparts quite literally “bring home the bacon,” despite extensive ethnohistoric evidence that women in such contexts are highly mobile and that their foraging activities are often responsible for most of the dietary intake of their families and communities.

More subtle but equally problematic are interpretations of large-scale cultural transformations that treat gender roles and domestic relations as a stable (natural) substrate of social organization that is unchanged by the rise and fall of states and is, therefore, explanatorily irrelevant. In another case that I will consider further, Christine Hastorf argues that the domestic units encountered in the highland Andes at the time of the Spanish conquest cannot be projected back into prehistory as if their form was a given. She offers compelling archaeological evidence that households and gender roles were substantially reshaped by the extension of Inka influence into these territories. In a parallel analysis, Elizabeth Brumfiel argues not just that the Aztec system of economic and political control changed domestic relations but that, given its basis in exacting tribute in the form of locally produced cloth, it depended fundamentally on the intensified and restructured exploitation of female (domestic) labor.<sup>5</sup> In these cases, critical (re)analysis reveals ways in which understanding has been limited not by ignoring women and gender altogether, but by conceptualizing them in normatively middle-class, white, North American terms.

*Equity Critiques.* Alongside these forms of content critique, there has grown up a

<sup>3</sup> Anne Yentsch, “Engendering Visible and Invisible Ceramic Artifacts, Especially Dairy Vessels,” in *Gender in Historical Archaeology*, ed. Donna Seifert, special issue of *Historical Archaeology*, 1991, 25(4):132–155; Seifert, “Within Sight of the White House: The Archaeology of Working Women,” *ibid.*, pp. 82–108; Cheryl Claassen, “Gender, Shellfishing, and the Shell Mound Archaic,” in *Engendering Archaeology: Women and Prehistory*, ed. Joan M. Gero and Margaret W. Conkey (Oxford: Blackwell, 1991), pp. 276–300; and Patty Jo Watson and Mary C. Kennedy, “The Development of Horticulture in the Eastern Woodlands of North America: Women’s Role,” *ibid.*, pp. 255–275.

<sup>4</sup> See, e.g., Margaret W. Conkey and Janet D. Spector, “Archaeology and the Study of Gender,” in *Advances in Archaeological Method and Theory*, Vol. 7, ed. Michael B. Schiffer (New York: Academic, 1984), pp. 1–38; and Spector and Mary K. Whelan, “Incorporating Gender into Archaeology Courses,” in *Gender and Anthropology: Critical Reviews for Research and Teaching*, ed. Sandra Morgen (Washington, D.C.: American Anthropological Association, 1989), pp. 65–94.

<sup>5</sup> Christine A. Hastorf, “Gender, Space, and Food in Prehistory,” in *Engendering Archaeology*, ed. Gero and Conkey (cit. n. 3), pp. 132–159; and Elizabeth M. Brumfiel, “Weaving and Cooking: Women’s Production in Aztec Mexico,” *ibid.*, pp. 224–253.

substantial and largely independent body of literature concerning the demography, institutional structures, funding sources, training, and employment patterns that shape archaeology. Feminist analyses of the status of women constitute some of the most fine-grained and empirically rich work of this sort.<sup>6</sup> These “equity critiques” document not only persistent patterns of differential support, training, and advancement for women in archaeology, but also entrenched patterns of gender segregation in the areas in which women typically work.

While such studies provide fascinating detail on ways in which women are marginalized within archaeology, rarely are they used as a basis for understanding how the content of archaeological knowledge is shaped. And although content critics provide compelling evidence that the silences and distortions they identify are systematically gendered, rarely do they make any connection between these and the gender imbalances in the training, employment, and reward structures of the discipline documented by equity critics. In general, sociopolitical critics in archaeology have tended to sidestep explanatory questions about how the silences and stereotypes they delineate are produced or why they persist.<sup>7</sup>

<sup>6</sup> Much of this “equity” literature appears in society or institution newsletters, in publications produced by in-house report series, or is circulated as informal reports and internal documents. Some of the more accessible and widely known of these studies and reports include Carol Kramer and Miriam Stark, “The Status of Women in Archaeology,” *Anthropology Newsletter*, 1988, 29(9):1, 11–12; Joan M. Gero, “Gender Bias in Archaeology: A Cross-Cultural Perspective,” in *The Socio-Politics of Archaeology*, ed. Gero, David M. Lacy, and Michael L. Blakey (Research Reports, 23) (Amherst: Dept. Anthropology, Univ. Massachusetts, 1983); and Gero, “Socio-Politics and the Woman-at-Home Ideology,” *American Antiquity*, 1985, 50:342–350. A number of related studies are collected in Dale Walde and Noreen D. Willows, eds., *The Archaeology of Gender: Proceedings of the 22nd Annual Chacmool Conference* (Calgary: Archaeological Association, Univ. Calgary, 1991); Hilary du Cros and Laurajane Smith, eds., *Women in Archaeology: A Feminist Critique* (Canberra: Australian National Univ. Occasional Papers, 1993); Margaret C. Nelson, Sarah M. Nelson, and Alison Wylie, eds., *Equity Issues for Women in Archaeology* (Archaeological Papers, 5) (Washington, D.C.: American Anthropological Association, 1994); and Cheryl Claassen, ed., *Women in Archaeology* (Philadelphia: Univ. Pennsylvania Press, 1994). The collection edited by Nelson *et al.* includes reprints of a number of earlier and otherwise inaccessible reports, along with newer studies and overviews of work in a number of different national contexts and subfields of archaeology.

In discussing this literature it is important to note that women are perhaps the only traditionally excluded group (with the possible exception of men from working-class backgrounds) to gain sufficient levels of representation within archaeology to develop such critiques on their own behalf. Nevertheless, studies of the sociopolitics of archaeology document many other dimensions on which the demographic homogeneity of the discipline has been maintained. See, e.g., the discussion of recruiting and training practices in Jane H. Kelley and Marsha P. Hanen, *Archaeology and the Methodology of Science* (Albuquerque: Univ. New Mexico Press, 1988), Ch. 4. Thomas C. Patterson considers ways in which the interests of intranational elites have shaped archaeology in Patterson, “The Last Sixty Years: Toward a Social History of Americanist Archaeology in the United States,” *American Anthropologist*, 1986, 88:7–22; Patterson, “Some Postwar Theoretical Trends in U.S. Archaeology,” *Culture*, 1986, 11:43–54; and in Patterson, *Toward a Social History of Archaeology in the United States* (Orlando, Fla.: Harcourt Brace, 1995), he offers an analysis of the impact of the GI bill’s educational support on the class structure of the discipline. Bruce G. Trigger explores the alignment of archaeology with nationalist agendas of various sorts in *A History of Archaeological Thought* (Cambridge: Cambridge Univ. Press, 1989).

<sup>7</sup> Typically these studies identify correlations, at a general level, between sociopolitical features of the discipline and of its products, offering an implicitly functional explanation for androcentric, nationalist, racist, or classist gaps and biases in content, but rarely do they supply an account of mediating mechanisms. Some important exceptions are reported in the landmark collection of essays, *The Socio-Politics of Archaeology*, ed. Gero *et al.*, that appeared in 1983. Working at a local, infra-structural scale, Martin H. Wobst and Arthur S. Keene argued that the fascination archaeologists

*Integrative Critiques.* There is one study, undertaken from an explicitly feminist perspective, that illustrates the potential fruitfulness of “integrative analyses”: analyses that explore the link between workplace inequities and androcentric bias in the content of research. It is an analysis of Paleo-Indian research undertaken by Joan Gero. She begins by documenting a strong pattern of gender segregation: the predominantly male community of Paleo-Indian researchers focuses almost exclusively on stereotypically male activities—specifically, on large-scale mammoth- and bison-hunting practices, the associated kill sites and technologically sophisticated hunting tool assemblages, and the replication of these tools and of the hunting and butchering practices they are thought to have facilitated. Gero finds that the women in this field have been largely displaced from these core research areas; they work on expedient blades and flake tools and focus on edge-wear analysis. Moreover, in the field of lithics analysis generally, women are cited much less frequently than their male colleagues even when they do mainstream research, except when they coauthor with men. Not surprisingly, Gero argues, their work on expedient blades and patterns of edge wear is almost completely ignored, despite the fact that these analyses provide evidence that Paleo-Indians exploited a wide range of plant materials, presumably foraged as a complement to the diet of Pleistocene mammals. Gero’s thesis is that these “social *relations* of paleo research practice” derail the Paleo-Indian research program as a whole: “women’s exclusion from pleistocene lithic and faunal analysis . . . is intrinsic to, and necessary for, the bison-mammoth knowledge construct.”<sup>8</sup> The puzzles that dominate Paleo-Indian research are quite literally created by the preoccupation with male-associated (hunting) activities. They turn on questions about what happened to the mammoth hunters when the mammoths went extinct: Did they disappear, to be replaced by small game and plant foraging groups, or did they effect a miraculous transformation as the subsistence base changed? These questions can only arise, Gero argues, if researchers ignore the evidence from female-associated tools that Paleo-Indians depended on a much more diversified set of subsistence strategies than acknowledged by standard “man the (mammoth/bison) hunter” models. This is precisely the sort of evidence produced mainly by women working on microblades and edge-wear patterns; it is reported in publications that remain largely outside the citation circles that define the dominant focus of inquiry in this area.

#### ARCHAEOLOGY AS POLITICS BY OTHER MEANS

When critiques of androcentrism and sexism appeared in archaeology in the late 1980s, debate about the implications of sociopolitical critiques was already sharply

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have with “origins” research should be understood as, at least in part, a consequence of structural features of disciplinary practice: Wobst and Keene, “Archaeological Explanation as Political Economy,” *ibid.*, pp. 79–90. Researchers who control the understanding of originary events or cultural formations must be acknowledged, in various ways, by all who work on later, linked periods and developments; they establish themselves as the “eye of the needle” through which all else must pass. This line of argument has recently been extended and reframed in feminist terms by Margaret Conkey, in collaboration with Sarah H. Williams, “Original Narratives: The Political Economy of Gender in Archaeology,” in *Gender, Culture, and Political Economy: Feminist Anthropology in the Post-Modern Era*, ed. Micaela di Leonardo (Berkeley: Univ. California Press, 1991), pp. 102–139.

<sup>8</sup> Joan M. Gero, “The Social World of Prehistoric Facts: Gender and Power in Prehistoric Research,” in *Women in Archaeology*, ed. du Cros and Smith (cit. n. 6), pp. 31–40, on p. 37.

polarized. Some of the most uncompromising critics of the explicitly positivist “New Archaeology” of the 1960s and 1970s parlayed local analyses of the play of interests in archaeology into a general rejection of all concepts or ideals of objectivity. Through the early 1980s they insisted, on the basis of arguments familiar in philosophical contexts (underdetermination of theory by evidence, theory-ladenness, and various forms of holism), that archaeologists simply “create facts,” that evidential claims depend on “an edifice of auxiliary theories and assumptions” that archaeologists accept on purely conventional grounds, and that there is, therefore, no escape from the conclusion that any use of archaeological data to test reconstructive hypotheses about the past can “only result in tautology.”<sup>9</sup> The choice between tautologies, then, must necessarily be determined by standpoint-specific interests and the sociopolitics that shape them; archaeology is quite literally politics by other means.

With these arguments, some critics within archaeology broach what Bruce Trigger has described as a nihilistic “hyperrelativism” now familiar in many of the social sciences. Given critical analyses that “shatter” pretensions to objectivity, demonstrating that there is no “view from nowhere,” no immaculately conceived foundation of fact, no transcontextual or transhistorical standard of rationality, it is assumed that epistemic considerations play no significant role at all.<sup>10</sup> What counts as sound argument and evidence (as “good reasons” for accepting a knowledge claim) is entirely reducible to the sociopolitical realities that constitute the standpoint of practitioners, or communities of practitioners, and the conventions of their practice. For a great many archaeologists, these conclusions were grounds for summarily dismissing postprocessualism and any aligned analysis that purports to bring into view the play of politics in archaeology. A dominant counter-response has been to call for a return to basics, to the real (empirical) business of archaeology. Not surprisingly, the feminist critiques that appeared in the late 1980s met with considerable skepticism.

What distinguishes the interventions of feminist critics in these debates is their refusal, for the most part, to embrace any of the polarized responses generated by this growing crisis of confidence in objectivist ideals. In most cases feminist critics in archaeology depend on painstakingly careful empirical analysis to establish their claims about gaps or bias in content, about inequities in the role and status of women in the field, and about the links between equity and content critiques. But however pervasive the androcentrism or sexism they delineate, and however sharply they criticize pretensions to neutrality and objectivity, they are deeply reticent to embrace any position approaching the hyperrelativism described by Trigger. They are clear about the social, political nature of the archaeological enterprise, and yet they do

<sup>9</sup> Ian Hodder, “Archaeology, Ideology, and Contemporary Society,” *Royal Anthropological Institute News*, 1983, 56:6; Hodder, “Archaeology in 1984,” *Antiquity*, 1984, 58:26; and Michael Shanks and Christopher Tilley, *Re-constructing Archaeology* (Cambridge: Cambridge Univ. Press, 1987), p. 111.

<sup>10</sup> Bruce G. Trigger, “Hyperrelativism, Responsibility, and the Social Sciences,” *Canadian Review of Sociology and Anthropology*, 1989, 26:776–797; Thomas Nagel, *The View from Nowhere* (Oxford: Oxford Univ. Press, 1986); and Richard Bernstein, *Beyond Objectivism and Relativism: Science, Hermeneutics, and Praxis* (Philadelphia: Univ. Pennsylvania Press, 1983). See also Alison Wylie, “On ‘Heavily Decomposing Red Herrings’: Scientific Method in Archaeology and the Laddening of Evidence with Theory,” in *Metaarchaeology*, ed. Lester Embree (Boston: Reidel, 1992), pp. 269–288.

not consider the outcomes of inquiry or the criteria of adequacy governing practice to be reducible to the sociopolitics of practice.

Two lines of argument support this stance. For one thing, it is evident that, as a matter of contingent empirical fact, “reasons”—appeals to evidence and considerations of explanatory power, as well as of internal and cross-theory consistency—do frequently play a critical role in determining the content of archaeological interpretations and the presuppositions that frame them, including those embraced or advocated by feminists. That is to say, reasons can be causes; they shape belief and the outcomes of archaeological inquiry, although their form and authority are never transparent and never innocent of the power relations that constitute the social contexts of their production. For another, close scrutiny of archaeological practice makes it clear that, as Roy Bhaskar argued years ago, one crucial and much-neglected feature of science “is that it is *work*; and hard work at that. . . . [It] consists . . . in the transformation of given products.” Most important, these “products” are built from materials that archaeologists do not construct out of whole cloth, whose properties they can be (disastrously) wrong about, and whose capacities to act or be acted upon can be exploited to powerful effect by those intent on “intervening” in the world(s) they study when these worlds are accurately understood.<sup>11</sup> Sociologically reductive accounts cannot make sense of these features of archaeological practice, including the practice of feminists and other critics in and of archaeology. Perhaps feminists have been more alert to these considerations because here, as in other contexts, they are painfully aware that the world is not (just) what we make it, and the cost of systematic error or self-delusion can be very high; effective activism requires an accurate understanding of the forces we oppose, conceptually, politically, and materially.

Most recently, the critics within archaeology who raised the specter of hyperrelativism have backed away from their strongest (and most untenable) claims.<sup>12</sup> They seem to have recognized that, insofar as they mean to expose systematic error and explain it (e.g., by appeal to the conditions that shape knowledge production), their own practice poses a dilemma: they bring social contingencies into view by exploiting precisely the evidential constraints and other epistemic considerations they mean to destabilize. They make good use of the fact that, as enigmatic and richly constructed as archaeological evidence may be, it does routinely resist appropriation in any of the terms compatible with dominant views about the past. This capacity of the world we investigate to subvert our best expectations can force us to reassess not only specific claims about the past but also background assumptions we may not have known we held, assumptions that constitute our standpoint in the present. As critics within archaeology have moved beyond reaction against the New Archaeology and have undertaken to build their own alternative research programs, they

<sup>11</sup> David Henderson, “The Principle of Charity and the Problem of Irrationality,” *Synthese*, 1987, 73:225–252 (reasons as causes); Roy Bhaskar, *A Realist Theory of Science*, 2nd ed. (Brighton, Sussex: Harvester, 1978), p. 57; and Ian Hacking, *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science* (Cambridge: Cambridge Univ. Press, 1983).

<sup>12</sup> See, e.g., Ian Hodder, “Interpretive Archaeology and Its Role,” *Amer. Antiquity*, 1991, 56:7–18; and Christopher Tilley, “Archaeology as Socio-Political Action in the Present,” in *Critical Traditions in Archaeology: Essays in the Philosophy, History, and Socio-Politics of Archaeology* (Cambridge: Cambridge Univ. Press, 1989), pp. 117–135.



tend to embrace epistemic positions that have much in common with those occupied by feminist critics and practitioners.

### *Parallels with Science Studies*

A similar polarizing dynamic has long structured relations between the constituent fields of science studies. After decades of rancorous debate between philosophers and sociologists it is now unavoidable, although still far from being universally accepted, that sociological challenges (themselves much modified in recent years) cannot simply be set aside by philosophers as misconceived or irrelevant; the traditional philosophical enterprise of “rational reconstruction” must be substantially broadened and not only naturalized but “psychologized” and “socialized.” The hallmark of postpositivist philosophy of science is a commitment to ground philosophical analysis in a detailed understanding of scientific practice (historical or contemporary), an exercise that has forced attention to the diversity and multidimensionality of the sciences. This, in turn, makes it increasingly difficult to sustain the faith that there is any distinctive, unifying rationality to be “reconstructed” across the historical and cultural particularity of the disciplines we identify as scientific.<sup>13</sup> At the same time, many sociologists of science now emphasize that science is work made hard, in part, by engagement with the “materiel” of its technology and subject domain; practice is conditioned by the sorts of considerations that have been central to epistemological analyses of science.<sup>14</sup>

One implication of these developments is that none of the existing science studies disciplines has the resources to make sense of the sciences on its own, in strictly philosophical, sociological, or historical terms. As Andrew Pickering puts the point, “Scientific practice . . . is situated and evolves right on the boundary, at the point of intersection, of the material, social, conceptual (and so on) worlds”; it “cuts very deeply across disciplinary boundaries.”<sup>15</sup> The crucial challenge, now taken up on many fronts, is to develop genuinely interdisciplinary strategies of inquiry, and for this we need problems and concepts, categories of analysis, that escape the dichotomous thinking that has structured disciplinary studies of science to date, setting “epistemic”/“internal” (constitutive) considerations in opposition to “social”/“external” (contextual) factors. This is, fundamentally, the challenge of building an integrative program of analysis capable of explaining how the thoroughly constructed materials of science—for example, whatever counts as evidence in a given context—*can*, in fact, “resist” appropriation, sometimes quite unexpectedly and

<sup>13</sup> See, e.g., contributions to the symposium “Discourse, Practice, Context: From HPS to Interdisciplinary Science Studies,” presented at the 1994 biennial meeting of the Philosophy of Science Association: Joseph Rouse, “Engaging Science through Cultural Studies,” in *PSA 1994*, ed. David Hull, Micky Forbes, and Richard Burian (East Lansing, Mich.: Philosophy of Science Association, 1994), pp. 396–401; Vassiliki Betty Smocovitis, “Contextualizing Science: From Science Studies to Cultural Studies,” *ibid.*, pp. 402–412; Andy Pickering, “After Representation: Science Studies in the Performative Idiom,” *ibid.*, pp. 413–419; and Brian S. Baigrie, “HPS and the Classic Normative Mission,” *ibid.*, pp. 420–430.

<sup>14</sup> See, e.g., Andrew Pickering, “From Science as Knowledge to Science as Practice,” in *Science as Practice and Culture*, ed. Pickering (Chicago: Univ. Chicago Press, 1992), pp. 1–28; and Pickering, “Knowledge, Practice, and Mere Construction,” *Social Studies of Science*, 1990, 20:682–729.

<sup>15</sup> Pickering, “Knowledge, Practice, and Mere Construction,” p. 710.

decisively, and sometimes with the effect of transforming the values and interests that frame our research programs.

These questions have been central to feminist analyses of science from the outset. Given political and conceptual commitments that make corrosive hyperrelativism as uncongenial as unreflective objectivism, feminists have been exploring positions between, or “beyond,” these polarized alternatives<sup>16</sup> throughout the period in which discipline-specific debates about the implications of hyperrelativism have run their course. Consider, for example, the efforts to articulate a viable standpoint theory made by Nancy Hartsock, Sandra Harding, and Donna Haraway; the multidimensional analyses of Evelyn Fox Keller; Helen Longino’s treatment of the interplay between constitutive and contextual values in science; and innumerable critical and constructive programs of feminist analysis in the social and life sciences (e.g., Anne Fausto-Sterling’s work on biological theories of sex difference and the range of feminist research in the social sciences analyzed by Shulamit Reinharz and anthologized by Mary Margaret Fonow and Judith Cook, Harding, and Joyce Nielsen).<sup>17</sup> It is a great loss to mainstream science studies that its practitioners have considered feminist work in these areas almost not at all, even when their own debates propel them in directions already well explored by feminist philosophers, historians, and sociologists of science.

I submit that the questions constitutive of these traditions of feminist research are worth pursuing not just because they are important for science studies and for archaeology, but because we badly need more nuanced critical appraisals of the fruits and authority of science if, as feminists, we are to exploit the emancipatory capacity that it may (yet) have. Despite their sometimes apocalyptic conclusions, the practice of archaeological critics, especially the feminists among them, demonstrates just how powerful systematic empirical inquiry can be as a tool for contesting the taken-for-granted that underwrite oppressive forms of life.

#### GENDER RESEARCH IN ARCHAEOLOGY

Feminist initiatives appeared much later in archaeology than in such cognate fields as sociocultural anthropology and history. It was not until 1984, just over a decade

<sup>16</sup> I use Bernstein’s language; see Bernstein, *Beyond Objectivism and Relativism* (cit. n. 10).

<sup>17</sup> Nancy Hartsock, “The Feminist Standpoint: Developing the Ground for a Specifically Feminist Historical Materialism,” in *Discovering Reality: Feminist Perspectives on Epistemology, Metaphysics, Methodology, and Philosophy of Science*, ed. Sandra Harding and Merrill B. Hintikka (Dordrecht/Boston: Reidel, 1983), pp. 238–310; Harding, “Why Has the Sex/Gender System Become Visible Only Now?” *ibid.*, pp. 311–324; Harding, *Whose Science? Whose Knowledge? Thinking from Women’s Lives* (Ithaca, N.Y.: Cornell Univ. Press, 1991); Donna Haraway, “Situated Knowledges: The Science Question in Feminism and the Privilege of Partial Perspective,” in *Simians, Cyborgs, and Women: The Reinvention of Nature* (New York: Routledge, 1991); Evelyn Fox Keller, *Secrets of Life, Secrets of Death: Essays on Language, Gender, and Science* (New York: Routledge, 1992); Keller, “Gender and Science,” *Psychoanalysis and Contemporary Thought*, 1978, 1:409–433; Keller, “A World of Difference,” in *Reflections on Gender and Science* (New Haven, Conn.: Yale Univ. Press, 1985), pp. 158–179; Helen Longino, *Science as Social Knowledge: Values and Objectivity in Scientific Inquiry* (Princeton, N.J.: Princeton Univ. Press, 1990); Anne Fausto-Sterling, “Introduction,” in *Myths of Gender: Biological Theories about Women and Men* (New York: Basic, 1985), pp. 9–12; Shulamit Reinharz, *Feminist Methods in Social Research* (Oxford: Oxford Univ. Press, 1992); Mary Margaret Fonow and Judith A. Cook, eds., *Beyond Methodology: Feminist Scholarship as Lived Research* (Bloomington: Indiana Univ. Press, 1991); Harding, ed., *Feminism and Methodology* (Bloomington: Indiana Univ. Press, 1987); and Joyce McCarl Nielsen, ed., *Feminist Research Methods: Exemplary Readings in the Social Sciences* (Boulder, Colo.: Westview, 1990).



**Figure 1.** *Woman carrying antelope in burden basket.* (William Endner Collection, G 495, Museum of Western Colorado. Al Ligrani Photo.) A drawing of this Mimbres bowl appears on the cover of Joan M. Gero and Margaret W. Conkey, eds., *Engendering Archaeology: Women and Prehistory* (Oxford: Blackwell, 1991).

ago, that the first paper appeared in Anglo-American archaeology that argued explicitly for the relevance of feminist insights and approaches to the study of gender. And it was another seven years before a book presented a substantial body of original work in the area. This took the form of a collection edited by Joan Gero and Margaret Conkey, *Engendering Archaeology: Women and Prehistory*,<sup>18</sup> which was the outcome of a small working conference convened by the editors in 1988 specifically for the purpose of mobilizing interest in the questions about women and gender posed by Conkey and Janet Spector in 1984 (see Figures 1 and 2). Most participants had never considered these questions and had no special interest in feminist initiatives.

The following year, the graduate student organizers of an annual thematic conference at the University of Calgary chose “The Archaeology of Gender” as their topic for the fall 1989 “Chacmool” conference. To everyone’s surprise, the open call for papers advertising this meeting drew over a hundred contributions on a wide range of topics, a substantially larger response than had been realized for any previous

<sup>18</sup> Conkey and Spector, “Archaeology and the Study of Gender” (cit. n. 4); and Gero and Conkey, eds., *Engendering Archaeology* (cit. n. 3).



**Figure 2.** Participants in “Women and Production in Prehistory,” the small working conference organized by Joan Gero and Meg Conkey that gave rise to Engendering Archaeology. The conference was held at the Wedge Plantation in South Carolina, 5–9 April 1988.

Russell G. Handsman	Alison Wylie	T. Douglas Price			
Janet D. Spector		Prudence M. Rice			
Cheryl P. Claassen	Patty Jo Watson	Ruth E. Tringham	Henrietta Moore	Thomas L. Jackson	Peter White
Christine P. Hastorf		Joan M. Gero	Margaret W. Conkey	Elizabeth M. Brumfiel	Susan Pollock
					Irene Silverblatt

(Conference photograph generously provided by the organizers.)

Chacmool conference.<sup>19</sup> The only previous meetings on gender had been annual colloquia at the meetings of the Society for Historical Archaeology (beginning in

<sup>19</sup> Marsha P. Hanen and Jane Kelley undertook an analysis of the abstracts for papers presented at this conference with the aim of determining how wide ranging they were in topic and orientation. The results are published in Hanen and Kelley, “Gender and Archaeological Knowledge,” in *Meta-archaeology*, ed. Embree (cit. n. 10), pp. 195–227. Chacmool conferences have been held at the University of Calgary every fall since 1966. They are sponsored by the archaeology undergraduate society of the Department of Archaeology, but graduate students and faculty are centrally involved in their organization. They have developed a strong reputation in North America and, increasingly, abroad as well-focused, congenial working conferences that have steadily increased in size and scope since their inception. The 1989 meeting represents something of a threshold, in which the number of submissions grew substantially, from the forty to sixty typical of previous years to more than a hundred, a pattern of growth that has been sustained by subsequent Chacmool conferences.

1988) and several Norwegian and British conferences and conference sessions.<sup>20</sup> The 1989 Chacmool proceedings were published two years later, and in the meantime at least five other widely advertised public conferences, and a number of smaller-scale workshops and conference symposia, were organized in Australia, North America, and the United Kingdom; several of these have produced published proceedings or edited volumes.<sup>21</sup> In an annotated bibliography of papers on archaeology and gender that were presented at conferences from 1964 through 1992, the editor/compiler, Cheryl Claassen, indicates that only twenty-four of a total of 284 entries were presented before 1988 and that only two of these appeared in print; more than half the entries are papers presented between 1988 and 1990, and fully 40 percent of those presented after 1988 have been published. So, despite the fact that little more than Conkey and Spector's 1984 paper was in print by the late 1980s, when various groups of enterprising organizers set about arranging archaeological conferences on gender, there seems to have been considerable interest in the topic that was, in a sense, just waiting for an outlet, an interest that has since taken hold across the field as a whole.<sup>22</sup>

The questions raised by these developments are conventional enough; they have to do with theory change, with why these initiatives should have appeared in the form they did and when they did, and with their implications for the presuppositions of entrenched traditions of research.<sup>23</sup> I have found these to be resolutely intractable questions, however, because the conditions shaping the emergence of gender research in archaeology are so multidimensional: a great many factors are at work, none of them separable from the others, and they operate on different scales, some

<sup>20</sup> These include a thematic conference held in Norway in 1979, the proceedings of which appeared eight years later: Reidar Bertelsen, Arnvid Lillehammer, and Jenny-Rita Naess, eds., *Were They All Men? An Examination of Sex Roles in Prehistoric Society* (AmS-Varia, 17) (Stavanger: Arkeologisk Museum I Stavanger, 1987). Also, several sessions on women and gender were organized for the annual meetings of the Theoretical Archaeology Group in the United Kingdom (in 1982, 1985, and 1987); see Karen Arnold, Roberta Gilchrist, Pam Graves, and Sarah Taylor, "Women in Archaeology," *Archaeological Reviews from Cambridge* (special issue), 1988, 7:2-8.

<sup>21</sup> Walde and Willows, eds., *Archaeology of Gender* (cit. n. 6) (Chacmool proceedings); Cheryl Claassen, ed., *Exploring Gender through Archaeology* (Monographs in World Archaeology, 11) (Madison, Wis.: Prehistory Press, 1992); Claassen, ed., *Women in Archaeology* (cit. n. 6); du Cros and Smith, eds., *Women in Archaeology* (cit. n. 6); and Seifert, ed., *Gender in Historical Archaeology* (cit. n. 3).

<sup>22</sup> Cheryl Claassen, "Bibliography of Archaeology and Gender: Papers Delivered at Archaeology Conferences, 1964-1992," *Annotated Bibliographies for Anthropologists*, 1992, 1(2). See also the annotated bibliography compiled by Elisabeth A. Bacus et al., eds., *A Gendered Past: A Critical Bibliography of Gender in Archaeology* (Technical Reports, 25) (Ann Arbor: Univ. Michigan Museum of Anthropology, 1993). Some earlier archaeological publications on women and gender include Anne Barstow, "The Uses of Archeology for Women's History: James Mellaart's Work on the Neolithic Goddess at Catal Huyuk," *Feminist Studies*, 1978, 4(3):7-17; Alice Kehoe, "The Shackles of Tradition," in *The Hidden Half: Studies of Plains Indian Women*, ed. Patricia Albers and Beatrice Medicine (Washington, D.C.: Univ. Press America, 1983), pp. 53-73; several important contributions by Alice Kehoe, Sarah Nelson, Patricia O'Brien, Pamela Bumstead, et al., to *Powers of Observation: Alternative Views in Archeology*, ed. Nelson and Kehoe (Archaeological Papers, 2) (Washington, D.C.: American Anthropological Association, 1990); Rayna Rapp, "Gender and Class: An Archaeology of Knowledge Concerning the Origin of the State," *Dialectical Anthropology*, 1977, 2:309-316; and Janet D. Spector, "Male/Female Task Differentiation among the Hidatsa: Toward the Development of an Archaeological Approach to the Study of Gender," in *Hidden Half*, ed. Albers and Medicine, pp. 77-99.

<sup>23</sup> The formulation of these questions is discussed in more detail in Alison Wylie, "Feminist Critiques and Archaeological Challenges," in *Archaeology of Gender*, ed. Walde and Willows (cit. n. 6), pp. 17-23.

highly local while others are quite general. Indeed, nothing brings home more forcefully the need for an *integrative* program of feminist science studies than grappling with the complexities of these recent developments in archaeology. What follows is a provisional and, most important, a *syncretic* account of the conditions responsible for the “engendering” of archaeology; fully integrated categories of analysis remain to be formulated. My aim is to illustrate why none of the familiar strategies for explaining science is adequate taken on its own. This will inevitably raise more questions than I can answer but will allow me to specify, in the conclusion, some of the tasks at hand in reframing science studies.

### *Theoretical and Methodological Considerations*

When Conkey and Spector argued the case for an archaeology of gender in 1984, they suggested that the dearth of such work was to be explained by the dominance of an especially narrow, ecologically reductive conception of culture that was associated with the New Archaeology. In an effort to make archaeology scientific, attention had been diverted from all types of “internal,” “ethnographic” variables; gender dynamics were just one casualty of a general preoccupation with interactions between cultural systems and their external environments, associated with the conviction that “internal” variables are both inaccessible and explanatorily irrelevant. Initially this explanation seemed persuasive; I have argued for it myself.<sup>24</sup> The difficulty, however, is that a great many of those who subscribed to the scientific ideals of the New Archaeology never did give up an interest in the social structures and internal dynamics of the cultural “systems” they studied; they showed great initiative in devising strategies for documenting, in archaeological terms, such inscrutables as interaction networks, kinds and degrees of social stratification, and modes of community and household organization (and changes in all of these over time). Given this, the real question is, Why did these more expansive New Archaeologists not turn their attention to gendered divisions of labor and organizational structures?

This lacuna is especially puzzling when we recognize that the New Archaeology and its most stringently ecologicistic models were decisively challenged at the turn of the 1980s, initiating a decade of wide-ranging exploration in which archaeologists reopened a great many questions that had been set aside by more orthodox New Archaeologists.<sup>25</sup> According to the “theoretical and methodological constraint” model, research on gender and critiques of androcentrism should have appeared with these other initiatives at the beginning of the 1980s, rather than a decade later. In fact, some early critics of the New Archaeology did explicitly advocate “feminist” initiatives as an example of just the sort of politically self-conscious archaeology they endorsed. Few pursued these suggestions, however, and several of these critics have since been sharply criticized by Norwegian and British feminists who argue that their own practice was often not just androcentric but quite explicitly sexist.<sup>26</sup>

<sup>24</sup> Alison Wylie, “Gender Theory and the Archaeological Record: Why Is There No Archaeology of Gender?” in *Engendering Archaeology*, ed. Gero and Conkey (cit. n. 3), pp. 31–54.

<sup>25</sup> See, e.g., Colin Renfrew’s baleful account of the proliferation of “isms” in Renfrew, “Explanation Revisited,” in *Theory and Explanation in Archaeology*, ed. Renfrew, M. J. Rowlands, and B. A. Segraves (New York: Academic, 1982), pp. 5–23.

<sup>26</sup> Examples of early interest in feminist approaches include contributions to *Ideology, Power, and Prehistory*, ed. Daniel Miller and Christopher Tilley (Cambridge: Cambridge Univ. Press, 1984):

In retrospect, it seems that the new generation of archaeologists shared with their predecessors a number of (largely implicit) presuppositions; despite other differences, they all tended to treat gender as a stable, unchanging (biological) given in the sociocultural environment. If the social roles that biological males and females occupy can be assumed to be the same across time and cultural context—to be “naturally” theirs—gender is not a variable that can be relevant in explaining cultural change. The question, Why not before? then becomes, Why now? Why would this particular set of taken-for-granted come to be seen as problematic now? Here the complexity of the explanandum outstrips the explanatory resources afforded by standard categories of philosophical analysis.

### *Sociopolitical Factors*

My thesis is that nothing in the theoretical content, intellectual history, methodological refinement, or evidential resources of contemporary archaeology can explain why an interest in questions about women and gender should have arisen (only) in the late 1980s. Sociopolitical features of the research community and its practice play a central role in determining the timing, the form, and the impact of the feminist critiques and research programs on gender that have begun to challenge the entrenched androcentrism of archaeology. In order better to understand these factors, I undertook a survey of everyone who participated in the 1989 Chacmool conference and did interviews with a number of those I identified as “catalysts”: those who had been instrumental in organizing this and related conferences and in producing the publications that drew attention to the need for and promise of feminist initiatives in archaeology. My immediate aim was to determine what factors had converged in creating the substantial constituency of archaeologists who were ready and willing to attend a conference on gender despite the lack of visible work in the area.

At the outset I assumed that the emergence of feminist initiatives in archaeology had followed roughly the same course as in other closely affiliated disciplines (e.g., sociocultural anthropology, history, paleontology): they appeared when a critical mass of women entered the field who had been politicized in the women’s movement and were therefore inclined to notice, and to be skeptical of, the taken-for-granted about gender that had hitherto structured archaeological interpretation and the research agenda of the field. In archaeology a significant increase in the representation of women was not realized until after the mid 1970s. I expected, then, that participants in the 1989 Chacmool conference would prove to be predominantly women drawn from the first professional cohorts in which women were strongly represented and that they would have been attracted to the topic of the conference because of prior involvement in feminist activism and scholarship. This account would suggest

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Mary Braithwaite, “Ritual and Prestige in the Prehistory of Wessex c. 2200–1400 B.C.: A New Dimension to the Archaeological Evidence,” pp. 93–110; and Ian Hodder, “Burials, Houses, Women, and Men in the European Neolithic,” pp. 51–68. For criticisms of this work see Ericka Engelstad, “Images of Power and Contradiction: Feminist Theory and Postprocessual Archaeology,” *Antiquity*, 1991, 65:502–514; and Roberta Gilchrist, “Review of *Experiencing Archaeology* by Michael Shanks,” *Archaeol. Rev. Cambridge*, 1992, 11:188–191. In the latter discussion Gilchrist draws attention to a notorious passage in which Shanks likens archaeology, specifically excavation, to strip-tease—each “discovery is a little release of gratification”—pretty clearly reaffirming dominant assumptions that the subject position of the archaeologist is normatively gendered male.

that the 1989 Chacmool conference afforded participants an opportunity to integrate preexisting feminist commitments with professional interests in archaeology.

In the event, 72 percent of the 1989 Chacmool participants responded to the survey, providing me with enormously detailed answers to a lengthy list of open-ended questions about their background training and research interests, their reasons for attending the conference, their involvement with feminist scholarship and activism, and their views about why gender research should be emerging in archaeology in the late 1980s. Preliminary analysis suggests that my initial hypothesis captures the experience and motivations of most of the “catalysts” but not of conference participants. The survey results do bear out my hypothesis about the demographic profile of contributors to the Chacmool program but confound my assumptions about their backgrounds and why they attended this first public conference on “The Archaeology of Gender.”

Those who attended the 1989 Chacmool conference were disproportionately women, and these women, more than the men, were drawn from cohorts that entered the field in the late 1970s and early 1980s, when the representation of women in North American archaeology doubled. Altogether 80 percent of submissions to the conference were made by women; this more than inverts the ratio of women to men in North American archaeology as a whole, where women make up roughly 36 percent of practitioners.<sup>27</sup> And while the average age of men and women at the time of the conference was very similar (forty-three as compared to forty years), the men were more widely distributed across age grades; altogether 60 percent of the women (twice the proportion of men) were clustered in the twenty-six to forty age range. Combined with information about their education and employment status, this suggests that, as I had expected, the majority of those who attended the conference were middle-ranked professional women who would have completed their graduate training and achieved some measure of job security by the mid to late 1980s, just when the first stirrings of public interest in questions about women and gender began to appear in archaeology. Moreover, most of these women made it clear that the call for papers tapped an existing interest in questions about gender; only a fifth reported ever having attended a Chacmool conference in the past (over half of the men reported being regular or previous attendees), and virtually all said the main reason they attended the 1989 Chacmool conference was the topic.

The survey responses also make it clear, however, that an avowed interest in questions about gender does not necessarily reflect a feminist standpoint. Nearly half of the women (and more of the men) said explicitly that they do not identify themselves as feminists, and many of those who embraced the label recorded reservations about what it means. Although three-quarters of respondents (both men and women) said they had a prior interest in research on gender, altogether two-thirds described the Chacmool conference as opening up a new area of interest for them, and less than half reported any previous involvement in women’s studies or familiarity with femi-

<sup>27</sup> Counts of the membership lists for the Society for American Archaeology and the Archaeological Institute of America show that, before 1973, women never made up more than 13 percent of the society’s members; in 1973 their representation jumped to 18 percent and by 1976 to 30 percent. Altogether 36 percent of SAA members were women in the fall of 1988, when the Chacmool call for papers was distributed, a level of representation that has been stable in the field since then. See, e.g., Kramer and Stark, “Status of Women in Archaeology” (cit. n. 6); and Patterson, *Toward a Social History of Archaeology* (cit. n. 6), pp. 81–82.



nist research in other fields. These results are consistent with Marsha Hanen and Jane Kelley's analysis of the conference abstracts, which reveals what they describe as a "dearth" of references to feminist literature, authors, influences, or ideas.<sup>28</sup> Most striking, just half of the women and a quarter of the men who responded to the survey indicated any involvement in women's groups, in action on women's issues, or in "feminist activism," and most described their involvement as limited to "being on a mailing list" or "sending money," usually to women's shelters and reproductive rights groups. Very few had been involved in any direct action or frontline work with the agencies and groups they supported. No doubt this level of involvement in the women's movement (broadly construed) is substantially higher than is typical for North American archaeologists. Even so, it does not support the hypothesis that the majority of participants in the Chacmool conference on "The Archaeology of Gender" had been independently politicized as feminists and had welcomed this conference as a first public opportunity to integrate their feminist and archaeological commitments.

The results of this preliminary analysis suggest, then, that the expanded cohort of women entering the field at the turn of the 1980s brought to their work in archaeology a standpoint of sensitivity to gender issues—no doubt in some sense a gendered standpoint—but not an explicitly feminist standpoint. Hanen and Kelley describe this orientation as a largely untheorized and apolitical "grass roots" interest in questions about gender relations and categories.<sup>29</sup> It would seem that the 1989 Chacmool call for papers resonated with a latent awareness of the contested and contestable nature of gender roles, considered both as a feature of daily life and as a possible topic for investigation in archaeology. Indeed, for many the conference seems to have been attractive because it provided an opportunity to engage these questions at arm's length, on the relatively safe (or at least familiar) terrain of archaeological inquiry. And for some this scholarly interest proved to be politicizing: a number of respondents noted, in their survey returns and in subsequent correspondence, that their work on the "archaeology of gender" has put them in touch with feminist scholarship in other fields and has led to an involvement in women's groups active on issues such as workplace equity, sexual harassment, reproductive rights, and violence against women. By contrast, almost all who played a role as catalysts had already been politicized as feminists and then brought this explicitly feminist angle of vision to bear on the programs of research in which they were engaged as archaeologists.

In these two rather different senses, then, the appearance of gender research in archaeology in the last few years seems to reflect a growing awareness, among relatively young professionals in the field, of the gendered dimensions of their experience, perhaps provoked by the fact that the gender composition of their own cohort disrupts the status quo. And perhaps, as amorphous and ill-defined a standpoint as this is, it was sufficient to incline (some) members of this cohort, especially the women, to greater awareness of and skepticism about the androcentrism inherent in extant research programs. Much remains to be done to determine what constitutes this "grass roots" standpoint of gender sensitivity, how it is articulated in archaeological contexts, how it relates to the gender politics of the larger society, and how it

<sup>28</sup> Hanen and Kelley, "Gender and Archaeological Knowledge" (cit. n. 19).

<sup>29</sup> *Ibid.*

shapes archaeological practice; evaluation and refinement of this hypothesis will depend, in part, on further analysis of the survey responses and, in part, on comparative analysis (within archaeology and across fields). But if this line of argument is plausible, it was a distinctive self-consciousness about gender relations that put these new participants in a position to think differently about their discipline and their subject matter, to identify gaps in analysis, to question taken-for-granted assumptions about women and gender, and to envision a range of alternatives for inquiry and interpretation that simply had not occurred to their older, largely male colleagues—colleagues whose gender privilege (as men working in a highly masculinized disciplinary culture) includes an unquestioning fit between their gendered experience and the androcentrism that partially frames the research traditions in which they work.

### *Content Analysis and the Role of Evidence*

If it is accepted that sociopolitical factors are centrally responsible for the emergent (archaeological) interest in women and gender as a research subject, broader questions about epistemic implications immediately arise. My thesis is that although such examples make it clear that the standpoint of practitioners affects every aspect of inquiry—the formation of questions, the (re)definition of categories of analysis, the kinds of material treated as (potential) evidence, the bodies of background knowledge engaged in interpreting archaeological data as evidence, the range of explanatory and reconstructive hypotheses considered plausible, and the array of presuppositions held open to systematic examination—they also demonstrate that standpoint does not, in any strict sense, determine the outcomes of inquiry. The results produced by those working from a gender-sensitive standpoint are not explicable, in their details, in terms of the angle of vision or social location that constitutes this standpoint. Consider, briefly, two examples that illustrate this point.<sup>30</sup>

In their critique of theories about the emergence of horticulture in the Eastern Woodlands, mentioned earlier, Pat Watson and Mary Kennedy begin with a conceptual analysis that draws attention to the conspicuous absence of women in accounts of how this transition was realized, even though they are accorded a central role in plant collection (before) and cultivation (after).<sup>31</sup> One explanatory model identifies male shamans as the catalysts for this transition; their interest in manipulating plant stocks resulted in the development of cultigens. In another, the plants effectively domesticate themselves by an “automatic” process of adaptation to conditions disrupted by human activities (in “domestilocalities”). As Watson and Kennedy point out, women passively follow plants around when they are wild and passively tend them once domesticated but play no role in the transition from one state to the other. The authors then deploy collateral evidence to show that this failure to recognize women as potential catalysts of change carries substantial costs in explanatory elegance and plausibility. The automatic domestication thesis must counter ethno- and paleobotanical evidence that the key domesticates appeared very early in environments that were by no means optimal for them, evidence that suggests that some human intervention must have been involved in the process of domestication. Like-

<sup>30</sup> The analysis of these examples has been developed in more detail in Wylie, “Evidential Constraints” (cit. n. 2).

<sup>31</sup> Watson and Kennedy, “Development of Horticulture” (cit. n. 3).

wise, the shaman hypothesis must ignore the implications of presuming that women were involved full time in the exploitation of the plants that later became domesticates, as well as ethnohistorical evidence that shamanism is by no means a male preserve and that women in foraging societies often hold the primary expertise about plant and animal resources that informs group movement and other subsistence-related decision making.

Whatever standpoint-specific factors might have put Watson and Kennedy in a position to notice the common incongruity in these explanatory models (the disappearing women), what makes their analysis compelling is their identification of internal contradictions in the logic of these models and their use of collateral evidence to call into question the assumptions that underlie these contradictions. They grant their opponents the assumptions they make about sexual divisions of labor in foraging and horticultural societies but point out that the archaeological record does not, in fact, deliver any evidence that, interpreted subject to these assumptions, would indicate that men mediated the transition to horticulture. And they make use of independent paleobotanical evidence to call into question the plausibility of “automatic domestication” accounts that deny human agency any substantial role in the transition. In neither case are the presuppositions of Watson and Kennedy’s (re)interpretation of the evidence dependent on the assumptions about women’s capacities that they criticize or embrace.

In a project that moves beyond critique, also mentioned earlier, Christine Hastorf relies on a similar strategy, exploiting independent background knowledge and sources of evidence to reassess the way in which women’s labor and domestic units are conceptualized in state-formation theories for the highland Andes.<sup>32</sup> She compares the sex ratios and lifetime dietary profiles of skeletal material recovered from burials in the Montaro Valley through the period when the Inka first made their imperial presence felt in this region. She found that the dietary intake of men and women was undifferentiated until the advent of Inka influence but then diverged sharply on the isotope value associated with the consumption of maize. Comparing these results with patterns of change over time in other aspects of the sites, she found independent evidence of intensified production and increasingly segregated work areas related to the processing of maize. To interpret these findings she relied on ethnohistoric sources that establish the association of women with maize processing and beer production and suggest that Inka rule involved negotiating with local men as the heads of households and communities and extracting them from their households to serve as conscript labor on Inka construction projects. These multiple lines of evidence suggest that the gendered organization of domestic units was significantly altered by Inka rule; the hierarchical, gender-differentiated divisions of labor and consumption patterns now familiar in the region were established when local communities were incorporated into a state system. This means that gender relations and household organization cannot be treated as a stable substrate that predates, and persists as a given through, the rising and falling fortunes of states; state formation in the Andes depended on a fundamental restructuring of household-based social relations of production.

As in Watson and Kennedy’s case, nothing in the social and political factors that may have informed Hastorf’s interest in questions about gender determines that she

<sup>32</sup> Hastorf, “Gender, Space, and Food in Prehistory” (cit. n. 5).

should have found such striking divergence in the dietary profiles of men and women or such congruence in patterns of change over time in several materially and inferentially independent aspects of the archaeological record. The presuppositions that Hastorf uses to construct data as evidence are not the same as those that define the questions she asks or the range of hypotheses she entertains; they may be radically theory-laden, but they are not laden by the same theories that are tested against this evidence or frame her research program. To put the point more generally, feminist practitioners exploit the fact that androcentric assumptions about gender roles generally lack the resources to ensure that archaeological data will conform to androcentric, presentist expectations; these assumptions do not deliver, along with reconstructive hypotheses, the linking principles necessary to establish evidential claims about the antecedent conditions that produced the contents of a specific archaeological record.

There is nothing unique to feminist or gender-sensitive research in this respect. The limited independence of facts of the record from the background assumptions that establish their import as evidence is the primary methodological resource that archaeologists use to assess the credibility of claims about the past in most contexts of inquiry.<sup>33</sup> It is in this that the capacity of evidence to resist our expectations resides. We can exploit this potential in any number of standpoint-specific ways; critics of racism, sexism, nationalism, and classism in archaeology use it to bring into view taken-for-granted that we should be questioning. That they should do so is no doubt overdetermined by a range of social, theoretical, political, and empirical factors, but exactly how they proceed and what they find out—where or, indeed, whether they locate incongruities that open a space for critical engagement—is also very much a function of the evidence they engage when they assess their own interpretive hypotheses and the background assumptions, the auxiliaries, on which they rely. It is this capacity of even quite remote subjects of inquiry to act back on us that we must keep in view when negotiating polarized debates about the implications of recognizing the limitations and partiality of our sciences.

### CONCLUSIONS

I draw four forward-looking conclusions concerning the tasks that face the proponents of a genuinely interdisciplinary, integrative program of feminist science studies.

1. Critiques that bring into view the pervasive ways in which social and political factors shape inquiry, including those foregrounded by feminist critics of science, should not be the *end* of discussion about such epistemological questions as what constitutes evidence and “good reasons” in a given context of scientific practice. Rather, they should be the beginning of a new kind of discussion, which feminists are especially well situated to carry forward.

2. To set discussion on a new footing, we must break the grip of the presupposition (held by objectivists and relativists alike) that objectivity is an all-or-nothing affair, that it is something we have only if the process and products of inquiry are (implausibly) free of any social or political entanglements and something we lose irrevocably

<sup>33</sup> This analysis is developed in more detail in Wylie, “Evidential Constraints” (cit. n. 2); and Wylie, “Constitution of Archaeological Evidence” (cit. n. 1).

if there is any evidence that science reflects the standpoint of its makers. We must also give up the view that “neutral” investigators are best fitted to maximize the cluster of (often sharply divergent) virtues we associate with “objectivity.”<sup>34</sup> We need accounts of knowledge production, authorization, credibility, and use that recognize that the contextual features of social location (standpoint) can make a *constructive* difference in maximizing these epistemic virtues, including quite pragmatic virtues such as reliability under specific ranges of application (a capacity to “travel”<sup>35</sup>) and intersubjective stability. The contributions of feminist researchers (both critical and constructive) make it clear that diverse standpoints can greatly enhance the likelihood of realizing specific sorts of empirical accuracy or explanatory breadth and may ensure that a rigorously critical perspective will be brought to bear (or, indeed, that detachment will be preserved) in the evaluation of claims or assumptions that members of a homogeneous community might never think to question.

3. These proposals have concrete implications for research practice. According to current objectivist wisdom, we can best safeguard the authority of science and of specific scientific claims by eliminating from the processes by which we evaluate knowledge claims any hint of contamination from social or political context. But from the foregoing, it follows that a commitment to objectivity may require direct consideration of the sociopolitical standpoint of inquirers in the adjudication of knowledge claims as an integral part of scientific inquiry.<sup>36</sup>

4. Finally, these observations have implications for feminist science studies. They suggest a need for analyses of science that are at once empirically grounded (historically, sociologically, and in the sciences themselves) and epistemically sophisticated, that work “right at the boundary” (as Pickering has put it) between the existing science studies disciplines, and that bring together equity and content critiques. They suggest, further, that feminist science studies must incorporate a normative as well as a descriptive component and that science studies practitioners, most especially feminists, should deliberately position themselves as “insider/outsidiers” with respect to the sciences they study. The work of a great many feminist theorists of science already exemplifies this hybrid stance.<sup>37</sup> Indeed, this active engagement with the sciences may be one reason why feminist practitioners and analysts of science have resisted the categories imposed by debates between philosophers and sociologists, constructivists and objectivists in their various fields of (metascientific) practice.

<sup>34</sup> See, e.g., Elisabeth Lloyd, “Objectivity and the Double Standard for Feminist Epistemologies,” *Synthese*, 1996, 104:351–381.

<sup>35</sup> Haraway, *Simians, Cyborgs, and Women* (cit. n. 17).

<sup>36</sup> Feminist researchers move in this direction when they insist on the need for rigorous reflexivity, in the sense explicated by Fonow and Cook and by Mies: Mary Margaret Fonow and Judith A. Cook, “Back to the Future: A Look at the Second Wave of Feminist Epistemology and Methodology,” in *Beyond Methodology*, ed. Fonow and Cook (cit. n. 17), pp. 1–15; and Maria Mies, “Towards a Methodology for Feminist Research,” in *Theories of Women’s Studies*, ed. Gloria Bowles and Renate Duelli Klein (New York: Routledge & Kegan Paul, 1983), pp. 117–139.

<sup>37</sup> See, e.g., the works of Keller, Fausto-Sterling, and Haraway cited in note 17, above, and the collaborative work of Helen Longino and Ruth Doell: “Body, Bias, and Behavior: A Comparative Analysis of Reasoning in Two Areas of Biological Science,” *Signs: Journal of Women in Culture and Society*, 1983, 9:206–227.