

# Questioning Theory: Is There a Gender of Theory in Archaeology?

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**Abstract** This paper raises questions about the practice of theory in anthropological archaeology. Particular attention is given to questions surrounding the gender of theory: what genders are more heralded in the theoretical spotlights and how the subject position of doing theory is gendered. An analysis of the contents of four Readers of Archaeological Theory shows the problematic selection and thus representation of women's theoretical contributions, including their effective ghettoization in gender and feminist archaeology. Insights from how feminists have been grappling with theory are considered, and archaeologists are urged to confront the ways in which "doing theory" is/is not valued and how it is differentially valued, and to discuss the place and uses of theory more explicitly and critically.

**Keywords** Theory · Gender · Feminism · Archaeological practice

## Part I: Feeling Ambivalent about Doing Theory: Overheard in a Professor's Office

Date: The year 2007

Place: Office of a senior male professor at a major research university

*Professor:* As the advisor assigned to you, I have called you three students here today to talk about your strategies for success. After all, I will soon retire and it's important there be a legacy, a reproduction of ideas and approaches that should be predominant in the field. You should each choose a niche for yourself in the field, a way to make your identity, a way to claim authority and thus to get grants and to get the better jobs and the better sites. You should first challenge some particular school of thought or approach to archaeology, and then offer something that at least sounds like "grand theory," a conceptual way out of the weaknesses of the approach you are

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attacking. After all, as they say, it is easy to critique but those who really “make it” are those who offer something “new,” something different and something that will encompass many research questions under a single rubric. How else can you call your own press conference and get your research into high profile media?

*Already socialized advanced student:* Yes, you’re right. I have been tracking the publications of several prominent archaeologists, who they cite and who they don’t cite, and their uses of language to generate what appear to be compelling and unassailable arguments about their data, methods, and theoretical importance. But it seems as if some of our discipline’s leaders have staked out certain intellectual ideas even if they have not been able to write very clearly about them and sometimes they have even used other people’s terms without citing them or used problematic data and analyses to try to support their points. While I certainly want to be a famous archaeologist and get access to some of the best sites for research, I am not sure which path to take—should I specialize in theory or should I try to run a really “big dig” and amass data suitable for *National Geographic* as well as *Science*? Maybe from my big dig I can then develop some equally big theory about the human past?

*Already socially conscious second year student:* I appreciate your concern for us, Professor Status, but I’m bothered by some of the implications of your message. First, is it really a wise choice to turn the production of knowledge into a game of strategy, and to advocate that offering a new theory is the way to advance a career? Theory building is an important part of archaeology, to be sure, but isn’t that something that should come after considerable reading, discussion and thought? Don’t we want to do better archaeology? And is theory building or “doing theory” the primary way to do better archaeology? How can you separate theory from practice?

Perhaps it would be better if practitioners were more explicit about their theoretical assumptions, and would bring them out into the open for scrutiny, and engage with the implications of one theoretical perspective or another. I would like to see more debate of theory, not just the promotion of theory by a few. “Theory” is often dismissed as being either “not real archaeology” (which is, it seems, mostly digging), or “too abstract” for the real on-the-ground work of archaeology. Others dismiss it as sometimes being political (such as in feminist or Marxist theory), purportedly blurring the objectivity of archaeological inquiry. But what you say suggests that maybe doing any theory is political, part of a political economy of archaeology. Are you suggesting that we present some new ideas in the guise of theory, as if it were theory, or should we perhaps find some apparently theoretical ideas in ancillary literature and import them into archaeology as something “new”?

*Thoughtful first year student:* While I happen to enjoy thinking more abstractly about some aspects of archaeology and about the human past, I’m uncomfortable with this characterization of theory as something to be used to not just guide but almost regulate and delimit the kinds of things we ask about in our archaeologies, and maybe even “railroad” what the answers or results will be. I’m not comfortable with the “bandwagon” approach to doing archaeology, either, where you have to align yourself with one theoretical approach or another, and perhaps close yourself off from alternatives.

I’ve never felt comfortable with that part of science that advocates Occam’s razor, whereby the most parsimonious explanation should be selected. Things are much

messier than that, aren't they? I am actually ambivalent about doing theory. On the one hand, while I recognize the importance of having some sort of conceptual framework, the readings are rarely easy. Everyone else appears to understand the theory and the theoreticians and I often feel that they know something that I don't know or maybe I can't even grasp it. Have you seen the way that some people at a lecture often quibble with a speaker about what some theorist said or didn't say in one text or another? I really feel marginalized in such situations.

On the other hand, it seems to me that in the archaeology of the 21st century, where we must increasingly engage openly with the politics of heritage and globalization, with the communities where we do archaeology, and with the impact of development on archaeological resources, what's really needed is having a flexible, open ended theoretical approach, one always subject to reflection and revision. If we are opening up the past to many voices, to having the archaeologist's expertise be only one of several voices, shouldn't we also be talking about theory in more accessible language and with more question marks than imperatives?

*Senior male professor:* Well, I hardly expected such responses, and I can see that you younger students have come in with some pretty unusual ideas that will give you trouble down the line. However, it is important to stress that we do value theory in archaeology—sometimes. And some of today's senior scholars have consistently shown their own ambivalence about the value of theory, often dismissing it through comic dialogues or harsh critiques, while simultaneously pursuing and promoting their own theoretical paths. After all, even I will admit that one must have some sort of theory, at least loosely speaking, some sort of assumptions and guiding principles to structure one's research. But you will not get to be a key figure in archaeology if you don't have a certain "stand" and a specialty. Some of you babble about such things as "politics of the past" or the "contextual values" of the researcher, but let me tell you, what you need to get that job is solid fieldwork, empirical data, and some sort of clear, crisp research problem that appears as if it is an original theory for archaeology, or part of a somewhat new theoretical approach. Yes, you're right to be "ambivalent" about theory and that one should not get too involved with it; after all, have you ever seen a job advertisement for an archaeologist who is primarily or only a theorist? According to the summaries of the papers presented at the annual archaeology meetings, relatively few authors of the papers identify themselves as presenting a paper about theory!

*Advanced student:* Well, I'm finishing my dissertation and will soon go on the job market. I know that I got my grant funded because I even chose a descriptive title (Test Excavations at Really Sorry Cave) for the proposal instead of a more theoretical one (Social Use of Space at Really Sorry Cave). And I put in only the theory that could be linked specifically to the data I thought I would be able to collect. I want to "close down" on the problem, to have something quite definitive to say in my job talks. What would happen if I said there was more than one best answer? Furthermore, while I accept your advice, Professor, in completing my time here in the program I can see that none of the faculty really mentored us in much theory, as theory.

*Second year student, turning to the advanced student:* I guess we have to all choose our own path. While it is good for you that you are finishing, with a grant in hand, I have some concerns about how I will move through the program. Now I am

not against your having what some might call a reductivist approach *per se*, but I am concerned about the uses to which some reduction is put, the political ends, such as naturalizing a bipolar sexual division of labor, or that only men made important technological breakthroughs in prehistory, or that social inequalities are inevitable and not mutable. I am convinced that archaeology is a political practice, which is not a bad thing, but it is a reality, and one that needs to be acknowledged so that we can evaluate how the knowledge we produce might have been affected by the politics. But it is often hard to work out the distinction between what is political and what is merely “politically correct”.

Nonetheless, if one holds certain theoretical assumptions that flow from the recognition of “the political”, then we have a responsibility, in fact, to be pro-active, not re-active, about those assumptions. If, for example, we are interested in scrutinizing the phenomena of social inequality, then, when we put forth our archaeological results, we want to be sure to include what the implications are for present day situations, and to consider how our findings might contribute to a scrutiny of everyday life. Don’t we have to use our knowledge to open up, not close down?

*First year student:* I guess I should have gotten an MA before coming in to this PhD program and maybe I would feel more comfortable about all this discussion about theory and careerism. From my perspective as a first year student, I feel uncomfortable about all the insistence on taking *a* theoretical stand; is that like taking the stand in a courtroom, where one must defend themselves? What if one has several versions of their story? Can’t some versions even undercut the authority that we think we have about certain things and ideas and issues? Perhaps I will not succeed in this program because I more often have to admit that I don’t understand, rather than that I do understand—the theory, for example. Even after just a semester here, I think there are structural resistances to teaching theory, to using it, and to making it more open, and open to revision. Can’t we use theory to challenge what we thought we knew, as much as, if not more than, to accommodate the data and information we are concerned with? Maybe we’ll always be ambivalent about theory, especially if we don’t talk about it more, at least in the sense of what it means to “do” theory, how we should learn it, how we should use it, and how it has been used and abused in the discipline. Funny thing,—theory—often heralded as important, if done by certain people, and often used negatively against other people.

## Part II: Theory Can Matter

Theory does matter in archaeology and doing it (or not) is just as much a part of our disciplinary culture as fieldwork. When we are interested in probing the dominant organizations of knowledge production in archaeology, theory must be one of our subjects. Most of us have accepted the point that the very under-determined nature of what we work with as archaeologists, especially if taken in the context of having anthropological goals, requires an explicit engagement with theory. This then is a paper about questioning theory; my point is, in part, that we have not often raised questions about the practice of theory.

Furthermore, as Catherine Lutz (1995a) has shown for the practice of ethnography and socio-cultural anthropology, “doing theory” can be gendered; she shows precisely how and why in socio-cultural anthropology it is gendered male. Lutz’s account of a predominantly masculinized theory in socio-cultural anthropology does not transfer easily to archaeological archaeology. For one thing, archaeologists appear to be somewhat less slavishly addicted to theory. When I suggested that there were those in archaeology who thought of theory as a luxury or as a waste of time, Lutz opined that this would be hard to imagine for socio-cultural anthropology. She asks, has “the cowboy masculinity of American archaeology had a class-gender dynamic that creates this contrast with the ironically more female cultural anthropologist...?” (Lutz 1995b). But this does raise the question: does archaeology have its own version of the gender of theory? What would it mean to say that theory is gendered, not just in its practice but also in its content? How might this “work”?

As Wylie (1981, 2002) suggests, the history of archaeology has been largely a history of vacillation between two options. On the one hand, there has long been the empiricist option that prefers to avoid [what the empiricists consider to be] inference beyond empirically given “fact”. And, on the other hand, there has been the idea that archaeologists must—if we are to treat archaeological data as cultural and historical material that cannot be separated from the “whole circuitry of interaction of its former contexts”—accept and embrace speculative modes of inquiry and engage with imaginative theoretical constructs (after Wylie 1981). Indeed, in much of archaeology’s past, theory was equated with speculation, and speculation—and by extension, theory—is often taken as more pejorative than positive. Even today, and especially in the political and economic realities of contract archaeology (Cultural Resource Management), the engagement with theory—as in the days of WPA archaeology—is often considered to be a luxury or, more negatively, not necessary or possible (given the constraints on research), or, by some, a waste of time (for more consideration, see Moss 2005:584–585).

But, as Wylie has also argued, the feminist practice of archaeology has not succumbed to lining up with either of these two polar—and still predominant—epistemic positions (Wylie 1997). Feminist interventions have shown a “strategic ambivalence”, one that “refuses reductive constructivism as firmly as it rejects unreflective objectivism” (Wylie 1997: 81). But has this mattered? Has the practice, content, and valuing of theory been altered?

Anthropological archaeology is embedded within the wider discourses of Anthropology and other social and human sciences, and it sits at the interface between science and the humanities, where it has often preferred to identify with science. Thus, it is not surprising that many of the general understandings that we have about the gendered—and gendered masculine—nature of theory more widely (*c.f.* Keller 1985, and many others’ writings about the gender of science) might hold for theory in archaeology. In Lutz’s piece, she succinctly discusses a characterization of what ‘theory’ is taken to be, and focuses on how it is that some work is designated as theory, and how theory is masculinized, especially in sociocultural anthropology. She shows how the “importance and source of the masculinization of theory can be rooted in its ideological control function in society at large” (1995a: 261).

What, then, can be said about theory in archaeology? In this brief foray into a topic of enormous scope, I will first raise some initial questions for us to ask and then turn to an analysis of some Readers in archaeological theory. From this, it is relevant to ask a more basic question, “what is theory, anyway?” and how do we think about what work it should do for us? From the initial questions, some specific archaeological examples, and a specific framing of “theory”, we can draw some implications for anthropological archaeology.

### Some Questions, to Start...

Here I broach several first round questions in an exploration of the gender of theory in archaeology. I start by trying to resist conflating the gender of the researcher or theorist with the gendering of the work—but not so fast. Given that recognized (and hence, valued) female theorists in archaeology are rare, especially if feminists are excluded, a first question asks “Who does theory, and who gets (positively) recognized for it?”. While there are many possibilities as to what theory is or might be—is it speculation or creative invention, is it simply a great imagination or the solid building of an intellectual scaffolding?—the answer to the question—what is theory?—all too often comes down to who is doing it.

It is more precise then to say that we must resist assuming that there exist distinctively and essentialized masculine or feminine ways of doing, thinking, theorizing. Yet there are, however, attributes of archaeological theory and theoretical practice that have gendered implications and/or that implicate specific historical and cultural gendered identities, strategies, styles, or “gender regimes”. If one looks at some recent Readers in Archaeological Theory and what works of which archaeologists are selected for a Reader, it is apparent that unless women archaeologists have written about gender or feminist issues, their theoretical works are not likely to appear in a Theory Reader. Specific analyses of some Readers will be presented shortly. But first, our general questions might include:

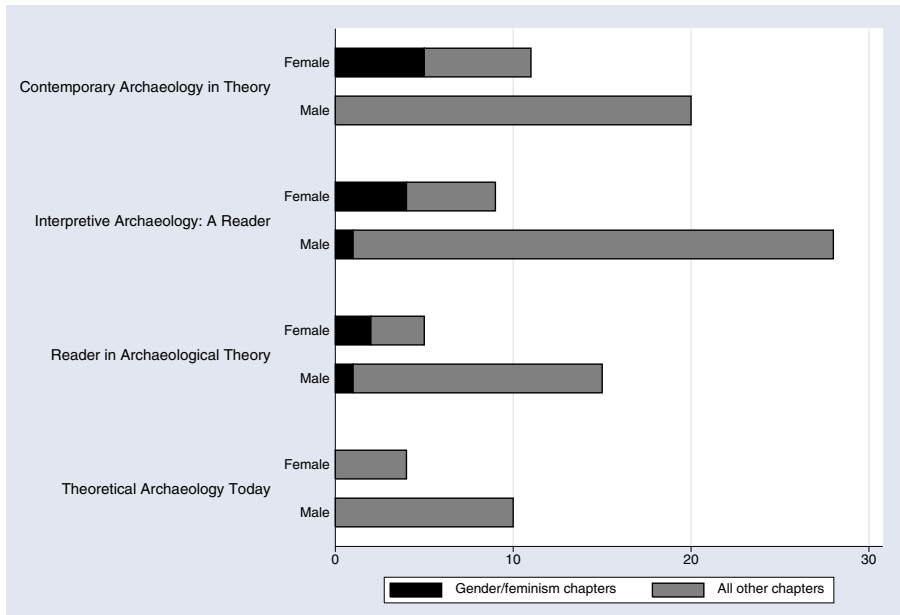
1. What counts as theory? What kinds of theory “count”? And what kinds of theory are promoted and which kinds are ignored or dismissed? Linked directly to these questions are others that relate to the contextual values of theory and how theory is (differentially) valued by different groups. Put simply, a feminist would ask: How does a piece of archaeological writing get established as more or less valuable and what does gender have to do with it?
2. How does the academic reproduction of theory “work”? For some (but by no means all), theory/theorizing are viable forms of symbolic capital in the discipline. Who advises their students to specialize in or “do” theory? Whose work gets praised for being theoretical and whose gets dismissed as being “too theoretical” or not being theoretical enough? And what kinds of theory are promoted and which kinds are ignored or dismissed? How do theoretical “dynasties” develop and get promoted and perpetuated? “Who shows up and who disappears—who gets disappeared—in our prevailing discourses?” (Suchman 2007). What are the mechanisms by which power can be concentrated in theory building, and in using theory to corner power?

3. Exactly how is theory involved in the gendering dimensions of becoming an archaeologist, given that the very process of becoming an archaeologist itself is a gendering process? As one comes in to the discipline, one “sees” and engages with, and one is enculturated into gendered practices; one’s own gendered self is sustained and/or re-formed. And, of course, the gendering process is not a unitary one, but is tangled up with classing, racializing, sexing and “ultimately politicizing” (Lutz 1995b). One dimension to explore is not merely the gender of the theorist that matters, but the gendered nature of the subject position of an archaeologist. Despite the (deeply problematic) appearance of Lara Croft, “Tomb Raider”, on the scene of popular culture’s depiction of archaeologists, the Indiana Jones style subject position can be shown to persist; that is, archaeology is primarily practiced—including theory—from the male subject position.
4. Is that which is being theorized about “marked” in gendered ways? Is (what is often considered to be) core theory—on big scale processes and the so-called “big” questions of archaeology, on technology, settlement, or politics—more “suitable” for male archaeologists? Are there certain domains of theory that are more acceptable/”suitable” for female archaeologists, such as style, households, gender, and feminism? That is, is there “pink collar theory”? Are the language and metaphors of the theoretical frameworks gendered somehow?

### The Gender of Theory: Archaeological Theory Readers

A review of four fairly recent and prominent Readers in Archaeological Theory is perhaps a useful way to consider how the discipline considers theory and significant theoretical works (Figs. 1, 2, and 3). The first reader is the co-edited volume, *Contemporary Archaeology in Theory* (Preucel and Hodder 1996, both males). Out of 26 chapters, 15 are single-authored by men and six are single-authored by women. Of those six, four are in the “Feminist and Gender Archaeologies” section (and these four constitute the entirety of this section); one other is about sex-related attributes of ceramics (in the “Meaning and Practice” section), and the sixth one is in the “Past as Power” section. There are two chapters co-authored by men, and two co-authored by women, and one co-authored chapter by a male/female (married couple). Considering that this is a volume that is not a history of theory in archaeology (*contra* Bawden 2003), it is not necessarily constrained by the absence of practicing women archaeologists that one might claim for most of the twentieth century (half of the papers are from the mid-late 1980s; the rest from the 1990s).

The second reader (edited by Whitley 1998, male), *Reader in Archaeological Theory*, has 16 chapters: 11 are single authored by males; one is single authored by a female, but it is not about gender or related subjects. The one chapter that is co-authored by women is, however, in the “Gendering the Past” section (one of two chapters here). There are two co-authored male chapters, and one by a male/female (married couple). In the Preucel and Hodder volume, with thirty-one “different” authors, 20 are men, and 11 are women (or 35%). In the Whitley volume, there are 19 “different” authors, only four are women (or 21%). If the writings of women are



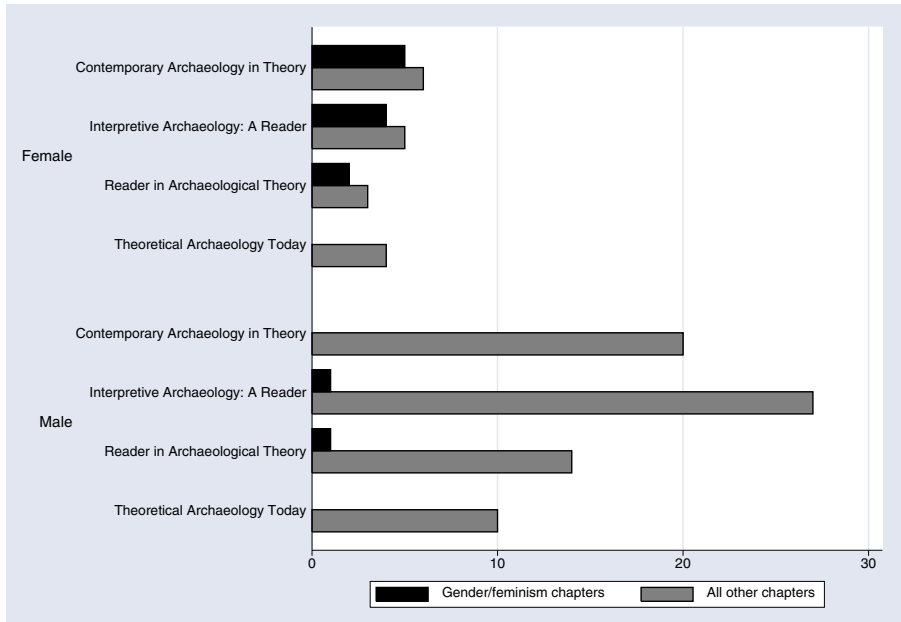
**Fig. 1** Four Theory Readers showing gender of authors for gender chapters and for non-gender chapters.

selected for their being about something other than gender, they appear by only two authors in the Whitley (or 10%) and as only six authors (or 19%) in the Preucel and Hodder volume. These percentages are significantly below the percentage of active publishing women archaeologists of the last decades (Hutson 2002).

A third reader (edited by Thomas 2000, male), *Interpretive Archaeology. A Reader*, also draws only on the literature of the 1980s and 1990s. It has chapters by thirty-four different authors, of whom only eight (or 23%) are female. Half of these eight female authors have chapters that are in the section entitled “Feminism, Queer Theory and the Body”; of the other four, two of them are, disciplinarily-speaking, philosophers more than practicing archaeologists. Thus, there are only two female authors with chapters in a section that is not explicitly about feminism, and both of them address what are considered ancillary or even core aspects of feminist approaches—identity and inequality.

The last Reader is the most recent of these four (edited by Hodder 2001, a male), and it is relatively short, with 12 chapters by 14 different authors; four of these authors (or 28%) are female, with one chapter co-authored by two women archaeologists. There are no separate sections, and there is no specific article on gender or feminist theory or “the body”; one chapter, “Archaeologies of Identity”, incorporates explicit attention to gender and sexuality. Paradoxically, some feminist archaeologists have expressed concern that gender is “disappeared” (after Longino 1994), or at least diffused, as a key concept when it is incorporated into a wider consideration of such a broad concern as identity. However, many of the other





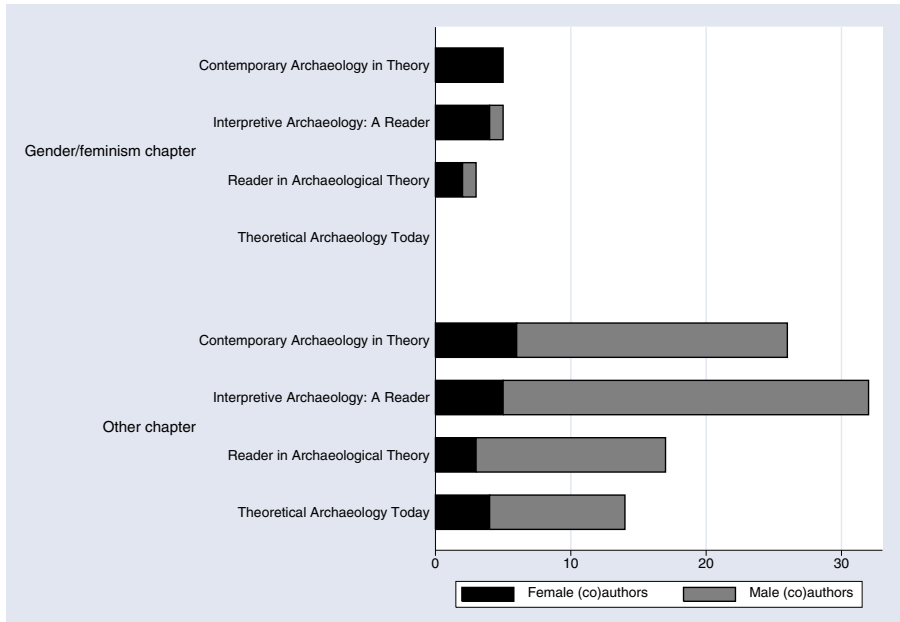
**Fig. 2** Gender of authors contributing to Theory Readers for both gender chapters and for other chapters.

chapters in this specific reader on other topics do include citations to gender or feminism; for some, this can be taken as mainstreaming or properly incorporating gender into all sorts of other topics and analyses.

There are 102 authors who contribute to these four Readers, but only 28 are women (or 27%). Eleven of these female authors have chapters on gender, feminism or sex, and most of these are in special sections of the Reader on such topics. There are only two male authors of such topics; the females thus comprise 85% of the authors in the Gender Sections. The other 17 female authors comprise only 19% of the theory-writers whose selected works are not specifically about gender.

In addition to these four Readers, one can find similar patterns in a 2003 Reader published by the SAA Press, (compiled by Garth Bawden, male), which is a selection of theoretical articles that have appeared (between 1962 and 2002) in the flagship journal of the Society for American Archaeology, *American Antiquity*, where the works of only two female authors have been selected, and one is a co-authored article with a male. Interestingly, most of the authors in all five Readers whose works have been selected as contributions to archaeological theory but who are not practicing archaeologists, are either women—e.g., Merrilee Salmon, Alison Wylie, Linda Patrik (philosophers), Shirley Strum (primatologist) or male native scholars—Gerald Vizenor, Jack Anawak, Carlos Mamani Condori, Gary White Deer.

These data confirm that if you are a female archaeologist and want to be represented in a Theory Reader, you had better write something about gender or closely related topics—such as inequality or identity. This also means that to read the



**Fig. 3** Types of chapters in Theory Readers, showing the male and female authors for gender chapters and for other chapters.

theoretical contributions of women archaeologists, one looks elsewhere than in Theory Readers. If you do write about gender, however, your work is likely to be placed in a special Section of the Reader that corrals the feminist and gender topics, and most of your co-authors in this section will be other women authors, though not always just archaeologists. This is not something to which that I myself would object. But perhaps this a part of the mobilization of “the complicity of women”, which Enloe (2004) reminds us is, after all, a successful strategy of patriarchy. Is this one way to “manage” feminism and women practitioners? Is this an archaeological version of a “room of our own”? Do we become “internal exiles” (after Baca, cited in Anzaldúa 1990: xxv)?

This containment makes it easy to *not* assign an entire section of the Reader, should one want to avoid most of the implications of feminist theory or a gendered archaeology. No matter how one crunches the data, and even if the edited volume is drawing from archaeological theory of the late 1980s and since, and even if (as is the case in the four analyzed Readers at hand) the editors are all explicitly or sympathetic post-processualists, the gender of the contributing authors to these Readers is not representative of the gender representation in the field. Women as theory authors are patently under-represented, especially if you do not “count” the gender/feminist contributions (see Figs. 1, 2, and 3). This is one of those “good news/bad news” kinds of phenomena: the good news is that there even *is* a “gender/feminism/the body” section in such Readers; 20 years ago this would have been

unheard of. The “bad news” part is that this is where most female authors are placed, although one could perhaps rightfully say that that is because they have done the best work in this field. But this is not the only domain of theory in which women have long been doing fine work, and much of the gender and feminist work is not separate from but integral to most other archaeological topics, from subsistence to symbolism.

Following the example of the Guerilla Girls’ feminist critiques of the art world (1995), we might ask: if 85% of female authors in theory readers are in the feminist/gender sections, and if only 27% of all authors are female in post-processually inclined theory readers that draw on quite recent literature, what might be happening in the rest of the discipline, with regard to the visibility, recognition, presentation, and circulation of female theory writing? Hutson (2002) has found, quite simply, that the citation of women authors is less than they publish. For example, there is still so little in our primary journals (save for *The Journal of Social Archaeology*) that takes up issues of gender or are mobilized by feminist theories, such that the work from the “ghetto” of these women theorists is not often visible in our journals. And, ironically, while the Readers may give visibility to some of this work, we nonetheless must be deeply concerned about the under-representation of women authors more generally, and perhaps especially if their work does not focus on gender or feminism. Levine (1994) has pointed out how earlier women archaeologists were able to “do” archaeology, often by taking up more marginal topics. Today, one might suggest that becoming established and recognized through theoretical work is still less of a viable option for women archaeologists (who are still grossly under-represented among the faculty of our major PhD institutions, taken together). Focusing on empirical work that is of undeniable and durable value and less easily dismissed is often a recognized coping strategy.

The Readers highlight, then, that women theorists are under-represented, and women theorists who write on gender, broadly speaking, are more likely to be included in such Readers, perhaps as “tokens”; despite the high caliber of this “selected” work, where are the other significant theoretical contributions? Where are pieces by Patty Jo Watson on ethnoarchaeology or reasoning in archaeological interpretations; by Barbara Bender on landscapes; by Rosemary Joyce and Susan Gillespie on house societies; by Marcia-Anne Dobres on agency, practice theory, and the social theory of technologies; by Polly Weissner on style; by Sian Jones on ethnicity and materiality; by Annette Laming-Emperaire on structuralist approaches to Paleolithic art, to name just a few?

But even if one is recognized as having made theoretical contributions, there is a danger for some do-ers to be seen as doing “only” theory; one is not necessarily considered to be a “real” archaeologist until one has done the requisite empirical work, *i.e.*, fieldwork, preferably excavation and a “big” excavation, at that. For example, in one (supportive) evaluation of my own National Science Foundation proposal for fieldwork (albeit not excavation but for survey, which Moser (1996) has suggested may be more “female”), the reviewer writes:

Conkey has been criticized from time to time for being ‘too theoretical’ (*i.e.*, not well enough grounded empirically). Here she’s trying to get funding for a field project that will produce hard data that she hopes to be able to incorporate

into her well-published views on style and social geography. I think she should have the chance to do this.

Despite the somewhat patronizing tone of this comment and the questions it raises about public perceptions of one's scholarship and about the relationship (or, as is implied, the differentiation) between doing theory and doing empirical work, I believe this reviewer (and the Excellent rating assigned by them to the proposal) helped me obtain the grant. Did the reviewer, in this case, actually or even consciously use the idea of being "too theoretical" strategically?

### **But What Is Theory?**

There are many different ideas as to what "theory" is; to some, anything that has reflexive content is theoretical. Others think of theory as being abstract and metaphysical, often at a high epistemological level, while others would locate it much closer to the ground, as in methods, as I suggest below for archaeology. Archaeologists are perhaps more ambivalent about theory than in other disciplines. We may more strongly identify as field researchers who are much more likely to privilege the data claims, yet, it has been part of the discipline for decades (Kluckhohn 1939–40) that there is a lot more need for theory than most recognize (see also Wylie 2002). As recently as the 2006 Society for American Archaeology meetings, a symposium asks "Is there an archaeological theory?"

One can think of theory in at least two different ways. On the one hand, the more traditional view is that theory is somehow foundational:

...a mental viewing, an idea or mental plan of the way to do something, and a formulation of apparent relationships or underlying principles of certain observed phenomena which had been verified to some degree. To have a theory meant to hold considerable evidence in support of a formulated general principle explaining the operation of certain phenomena" (Anzaldúa 1990: xxv, citing the 1959 Webster's New World Dictionary of American Language, College Edition).

Archaeology has attempted—and often learned a lot from—foundational theories. Many theories of this foundational sort are often promulgated, however, with programmatic statements (*e.g.*, Binford 2001; O'Connell 1995; O'Brien *et al.* 2003) that can be characterized as "masculinist project(s) of containment" (Lutz 1995a). This fosters the compartmentalization of knowledge-making, which often leads to certain "schools" of thought where one is "in" or "out". For example, Trigger (2003: 5) suggested that Darwinian archaeologists "are intent on establishing a hegemonic position for their own approach" (for a feminist objection to the compartmentalization of knowledge-production, and for alternatives, see Longino 1994; Wylie 1995).

On the other hand, there is a very different way to think of theory, a view much more compatible with feminist perspectives and much more suited, in fact, to the nature of most archaeological inquiry. This is a view of theory as revelatory, as opening up new spaces, as challenging assumptions so as "to conceive of our own

thinking... in new ways” (Culler 1994:13, citing Culler 1992:203). The nature of theory, by this account, is “to undo, through a contesting of postulates and premises, what you thought you knew” (Culler 1994: 15). In this view, the ambivalences and anxieties that our students (see above) may feel in the discussions of theory are, in fact, the point! In this view, the phrase, “the borrowing of theory” is redundant; the borrowing or the exogamous use of theory is inherent in its very definition.

Archaeology is always apologetic about the way it “borrows” theory all the time, but perhaps this is not so bad after all, unless it is done uncritically and/or slavishly. The very success and robust impact of the 1960s “New Archaeology” is recognized to be due to the application of combined external theoretical developments of cultural ecology, systems theory, and neo-evolutionary theory (e.g., Leone 1972). The very emergence and elaboration of post-processual archaeologies is due to the infusion of such things as Giddens-esque social action theory, critical theory, feminist theory, and the interpretive turn more generally in the social sciences. As Preucel noted (1995) in his review of the “post-processual condition”, the debates and varieties of archaeological theory that are awkwardly, if not uncomfortably, subsumed under the rubric of post-processual archaeology are not *internal* to archaeology but represent instead the particular and varied engagements of archaeologists with pluri-disciplinary theoretical issues and developments in wider scholarly and intellectual communities. And although there is often the use of a compartmentalizing label for “behavioral archaeology” (e.g., as in the Hodder 2001 Reader), recent re-framings consider this approach to be a “performance-based life-history” one (Skibo:pers.comm.) that draws on a variety of theoretical influences and examples external to archaeology. Indeed, one can make the case that some recent “theory in archaeology” *has* been “theory” in Culler’s sense of the word, with its challenges to what we thought we knew (e.g., as in Brumfiel 1992). The history of feminist theorizing conforms more closely to this view; theory as de-stabilizing.

Theory is not then a set of stable guidelines that one can just “apply”. “Theory produces effects that change people and the way they perceive the world” (Anzaldúa 1990:xxv). Theory, then, should be thought of in terms of its effects—what changes people’s views, makes them conceive of their objects of study/objects of knowledge *and* their activity of studying differently; and this will be “different for people differentially situated” (Culler 1994:16). Theory is inevitably practice, or as the late Black feminist Barbara Christian incisively remarked: “My fear is that when Theory is not rooted in practice, it becomes prescriptive, exclusive, elitist” (Christian 1990:340).

Of course, there are implications of this idea that theory is a practice in archaeology. Minimally, it reminds us that usually it is “who” does theory that seems to matter, and that these “who’s “ are differentially situated in society, history, politics, and the discipline. When we look at the “uses” of theory or who become “the” theorists (even if everyone ought to be a theorist on some level), how they do this, and why, it is not surprising to see, as is the case in other disciplines, that theory can often be used to create a niche for the practitioner, for the archaeologist. In the process of advocating theory, the theorist often *becomes* the archaeology: there is Binfordian archaeology, Hodderian archaeology, or even Dunnell’s archaeology, all of which are what Carroll (1990) notes as eponymous practices. So far, there are no eponymous archaeologies named for women, even if we are not at all sure that this would be a good thing. Sometimes the very identification of one’s kind of archaeology and one’s

theoretical positioning is in terms of an individual author, not “by the movement or social context that spawned them or the subject on which they focus” (Lutz 1995a).

Much production of knowledge is, however, marked by the contexts of production. For example, Brumfiel (1993) points to the direct connection between the absence of women at a conference and the limited perspectives on the topic that resulted. And one does not need any insider information to recognize the crucial role that certain university departments have had in the establishment of “new” theoretical trajectories, such as the University of Chicago for New Archaeology, or the Cambridge University flowering of early post-processual archaeology.

In a further twist, if so much of the recognized theory from and by women in archaeology is that having to do with gender or feminism or associated topics, it is interesting to note that even among feminists, there has long been a fundamental suspicion of theory. This is ironic in several ways. First, for feminists to be suspicious of theory is ironic, given that most feminists would admit that it is, in fact, theory that may allow for the imaginations of the world that would otherwise be ideologically invisible (after Christian 1990). How, for example, without theoretical musings, might we envision a world other than a patriarchal one, or one governed by capitalist-driven gendered power relations? In wider academic circles, feminist theory is often “put down” (Moss 1999) and seen as “unforgiveably middlebrow” (Lutz 1995a): it is a theory associated with women and presumed only (sic) addressing the practical concerns of political engagement.

While there has been avoidance of feminist theory by many archaeologists, even ones engaged with gender (e.g., Sørensen 2000), ironically, it is a genre of theory that itself tends to be effaced from that which is granted (positive) status *as* theory (c.f. Engelstad 1991). That which is accepted as theory usually must “meet criteria of coherence, value neutrality and abstraction that themselves may embody the false universalism that feminists criticize. Yet feminist approaches must resemble the objects of their attack” (Minow 1988: 55), despite that fact that feminist theorizing has been ever-changing, destabilized (Barrett and Phillips 1992), in conflict (e.g., Hirsch and Keller 1990), and even openly mocked (Lugones and Spelman 1983), and cited as exclusionary (Lâm 1994).

Perhaps, then, the fact that there is as much feminist inspired work as there is in our Archaeological Theory Readers, for example, embodies complex Catch-22’s. We are asking here how the academic reproduction of theory has worked to maintain and sustain “the discipline”. This question requires us to look more deeply: how do the “constitutive conventions” and “ruling relations” (Smith 1999:45–70) of archaeological discourse and archaeological knowledge deflect (repel or co-opt) the interventions that feminist inquiry is about. Has that “strategic ambivalence” of feminists in archaeology cited by Wylie (1997: 81) made a difference, had an impact? Has the content, practice and valuing of theory been altered? In order to even assess this, we need to engage further with what appears to be the playing field of archaeological theory.

### The Conflation of Method and Theory

To assess the state of theory today, especially in most North American anthropological archaeology, is a complex task. It still appears safe to say (see also the conclusions of

Chamblee and Mills 2001), that, despite Hegmon's (2003) assessment of "processual-plus" and the emergence of numerous exciting new approaches (e.g., Dobres 2000; Dobres and Robb 2000; Joyce and Gillespie 2000; Pauketat 2001; Stahl 2002) Americanist archaeology remains quite firmly processual in practice. There is little engagement with theory, not only in the sense of Culler, whereby thought and practice are being challenged and re-oriented by theory, but even in the old-fashioned sense of theory as "providing foundations and methods for further study" (Culler 1994: 13, 15). For example, O'Connell (1995) makes the plea that ethnoarchaeology needs a general theory of behavior. As an aside—and certainly not noted by O'Connell—ethnoarchaeology has long been a standard component of archaeology, but it is rarely noted that among its pioneers are Maxine Kleindienst and Patty Jo Watson's (1956) "action archaeology", and among its more recent practitioners who have set out important theoretical pathways are Barbara Bowser, Carol Kramer, Ann Stahl, Miriam Stark, Kathryn Weedman and other female scholars. O'Connell asserts not only that ethnoarchaeological research is being done without reference to much in the way of explicit theory, but also that there is an extant body of theory—namely, behavioral ecology—that, to him, can provide "a single theoretical framework" that can be operational(izing it) by means of formal predictive models" (1995:236, emphasis added). This re-affirms the monogamous sense of the place of theory in archaeology. Here there is both a claim for a lack of foundational or guiding theory—not for a theory that might destabilize and thus yield something "new"—and a simultaneous plea for the adoption of a single all-inclusive (all-occasion/all-purpose) theory.

An editorial (by editor, Lynn Goldstein) in the January 1998 issue of *American Antiquity* reinforces the idea that some Americanist archaeologists have been somewhat resistant to entertaining alternative accounts for their data and, furthermore, have come to see this as a theoretical issue. They claim, according to Goldstein, that the editorial critiques of submitted articles that ask for more elaboration of other possible explanations or accounts are motivated by a post-processual relativistic editorial policy. Goldstein (1998: 6) points out that the editorial concern is not a theoretical stance, but that, in requesting that authors "consider alternative explanations...is simply a call to logically outline and defend the view you hold so dear".

One might make the case that what seems to have happened in Americanist archaeology since the late 1970s has been the conflation of theory with method; here Mary Daly's (1978) coined term—"method-idolatry"—might be an appropriate concept. Of course, it is crucial that archaeologists understand, use, improve, and consciously attend to our methods, given, if for no other reason, the non-renewable nature of archaeological sites. Few of us will refuse the use of increasingly revelatory methods—phytolith analyses, new dating techniques, archaeopetrography, DNA analyses, for example—to know more about the past and about archaeological materials and contexts. But there continues to be some sharp disagreement (e.g., Binford 2001 versus Odell 2001) not only about the appropriate focus of archaeological research but about from where our research problems derive. Binford, for example, reminds us that it is the "archaeological record" that we must research, and that we should derive our theory from archaeological datasets; if we import theory from elsewhere, this is (somewhat pejoratively) labeled as "humanistic"

archaeology. If this were, in fact, the way that all archaeology was done, the conflation of theory with method would not be surprising. This argument, and others like it, perpetuate a retreat from the study of culture and people; that which is to be explained is the *archaeological record* and we are “to solve the puzzle of what the archaeological record means” (Nieman 1995). Certainly any theoretical infusions are abandoned in favor of methodological concerns, theory is conflated with method, or theory is masked in that methods (and their testing) are passed off and allowed to be taken as theory. But, as legions of feminist critiques of science have shown, the methods of scientific methodology are “non-objective” enterprises (Collins 1999); rather, they often masquerade as objective and are used to reinforce the status and authority of the method-maker (see also Lloyd 1995).

What is disturbing here and has gendered implications is that without active theory (especially in the Culler sense of theory as being a challenge that unsettles or un-does), the retreat to methods inhibits the possibilities of generating “new understandings of cultural forms and activities by [or that comes from] *widening the range of disciplinary references*” (Culler 1994: 16; italics added). Berggren and Hodder (2003) have recently argued, in fact, that some of the “disaffection in field contract archaeology today” results, in part, from “the separation of excavation from interpretation”.

A masculinized project of containment, as described by Lutz (1995a), is often made manifest in programmatic statements by Binford (2001) and others (e.g., O’Connell 1995; O’Brien *et al.* 2003). This containment—that, among other effects, encourages the compartmentalization of knowledge making—is precisely opposite to what the feminist practice of theory would call for. As Longino has so eloquently argued, theory should be about the democratization of knowledge-production, theory should be encouraged to be a pluralistic enterprise—one that would serve divergent goals, engage dissent seriously, and foster views from all sorts of vantage points (Longino 1994; Wylie 1995).

Particularly in an archaeology that has not engaged much in theory *as theory*, and that has conflated theory with methods, the excavation and field work practices themselves assume greater actual as well as metaphorical power (as discussed by Joyce with Preucel 2002). To be sure, in a field that was once described as being practiced by either of two sorts of archaeologists—the hairy-chested or the hairy-chinned (Ascher 1960; see also Gupta and Ferguson [1998:11 ff] on the archetypes of ethnographic fieldwork)—this kind of male-gendered metaphor for its core practice of fieldwork should not be surprising. As Gupta and Ferguson ask, in reference to their critique of “the field” in socio-cultural anthropology: “if a herorized journey into Otherness [for archaeology read, into The Past] is indeed a rite of passage, what sort of subject might we expect to be formed by such a rite?” (1998:16).

### Theory from/by the Masculinized Subject

It may not be so much if one is or is not engaged with theory that challenges and thus re-works what we do, but it may be *how* that theory is done: what are the narratives and images of theory, its production and its practice? Several observations can be made about the gendered (usually masculinist) subject position of archaeology.



First, there exists the assumption, acceptance, and enforcement that there is a privileged production of knowledge, and that this is done by means of totalizing, abstract, and decontextualized language (see Joyce 2002: especially 133–144). No one could disagree with the fact that the jargon and language, especially of the early New Archaeology was defiantly about defining what we would today call a completely new discourse. Consistent with this was the introduction of new terms (e.g., “sociotechnic”) and classifications (“the forager-collector continuum”). In post-processual theories as advocated by Hodder and others, there is also the use of language to define the fields of discourse, although this may be as much an influence of post-structuralist “styles” in general than anything inherent in theorizing just within archaeology. Many a culture-history archaeologist has been amazed at the appropriation and re-framing of archaeology as “contextual” (Hodder 1987), as they have long thought “context” was what all archaeology is about, even if Hodder would no doubt point to epistemic and even ontological differences.

To borrow from Rosemary Joyce’s use of the terms, “readerly” and “writerly”, some theory can be more “writerly”—requiring the user to not just “do the reading” but to have to work at it, sort things out, make choices and decisions. Culler would likely opt for this kind of theory. Yet there is a difference between this kind of “good” writerly theory and the well-known “separatist” kind of writerly theory, such as that of the “grand” theorists, which Lutz critiques for being exploitative in the ways they simultaneously make the reader labor—“requiring more of a reader’s imagination” due to the [intentionally?] abstract nature of the writing—while fetishizing the theorist” (Lutz 1995a:254). Of course, feminists also have to be careful here to not create and use an equally obscure discourse!

The known instances where some theorists in archaeology have not acknowledged their sources—such as Binford’s uncredited use of Merton’s term, “middle-range theory” (Binford 1981, see Raab and Goodyear 1984) or Hodder’s uncredited use of Le Roy Ladurie’s term (1975) “domus” (in Hodder 1991)—would be examples of where the expected standard of being an “original thinker” appears to have precluded an honest acknowledgement of one’s inspirations and sources. As Lutz notes (1995a), the less citation, the better, if one is aspiring to status as a theorist, *i.e.*, an “original” thinker (for an excellent and provocative analysis of the “politics of ‘originality’” and of the usefully vague concept of “originality” and its differential deployment regarding the evaluation of the scholarly work of men *versus* women, see Carroll 1990).

It is well-documented (Engelstad 1991) how some of the mainstream and leading voices of post-processualist archaeology have—as is the case with the “writing culture” episode in ethnography (Behar 1995)—not taken feminist writing and feminist archaeology seriously, allowed any space for it in their programmatic treatises, nor acknowledged its role in the rise and acceptance of post-positivist theory in archaeology, among other things. Hartsock has noted (1995: personal communication) how, in political science, “it is particularly feminist theory that’s relegated to the margins”. At the 2006 Society for American Archaeology annual meetings, in a session dedicated to evaluating the state of theory today (“Is there an archaeological theory?”), it was shocking to see that out of some nine participants seated at the front of the room, only one was a woman (Anick Coudart), who, as a French archaeologist, is known for articulating a French perspective (e.g. Coudart

1999) that finds little use for the attention and debate surrounding “theory” that characterizes Anglo-American archaeology. When Joan Gero asked from the floor where was feminist theory (especially when there was not just one, but two participants debating versions of phenomenology) or women archaeology theorists, the shocking answer was “Well, we invited Alison Wylie, but she could not be here”. This is not at all shocking regarding Alison Wylie, who, as a feminist theorist and philosopher of science is widely and deeply respected for her insights into archaeological reasoning and practices, but shocking because there are many other potential women theorists in archaeology, in a discipline that is now easily 50% female. Can it be so hard to locate another thoughtful, theoretically articulate female archaeologist to join in the discussion who would have had considerable potential to diversify the discourse? Have there been any changes since Engelstad’s (1991) critique of 15 years ago?

There exist certain aesthetic strategies and styles of producing, circulating, and defending theory. As discussed in some detail by Moulton (1980, 1983) for philosophy, the “duel”/“dueling” and adversarial nature of theory can be well-substantiated in archaeology. There are many examples of the general strategies that aim to replace one view with another rather than entertaining and explicitly engaging with multiple approaches. Often, there is work that goes beyond not tolerating or ignoring alternatives, but takes up specific combative postures and slash-and-burn discourses, often quite characteristic of many regional archaeologies. “Writing theory” in archaeology has traditionally been taken up as more confrontational than collaborative, more first person singular than first person plural (*e.g.*, Hegmon (2005) pleading to Moss (2005), “no more theory wars”).

In a critique of the academy from the feminist perspective, Evans (1995) invokes the characteristic “heroic imperative” within academic discourse, which stems, she argues, from the high value placed on individuals, individualism, and on having “original” ideas. She discusses the ease with which males are more likely to succeed “as hero” and that women tend to face the “double bind of enforced originality (the ‘hero’ of the academic world) and the expectations of appropriate womanhood”. Here she cites Spurling’s 1990 study that shows that, for women, “advancing novel ideas is far more strange and far more antithetical to the entire process of (female) socialization” (Evans 1995: 80). What Evans suggests then is that “feminists have to challenge the nature of [what counts as] originality and innovation” (see Carroll 1990 for more insights on this).

Furthermore, there will be an inevitable tension between the emphasis on individualism that is favored (and instantiated by our institutional and cultural reward systems) and the de-construction of the hero model. There are still resistances to team-teaching; there is the scrutiny of collaborative publishing, especially in the social sciences and humanities where “theory” and “originality” are both more at stake. What, we might ask, are the results of a process whereby we create heroes out of theoretical innovators but “assign serf-like status to those who do not ‘innovate’ in the same way?” (Evans 1995:81) or to the analysts who generate the data used by the “big man” Principal Investigators (Gero 1985). Carroll shows how there exists a class system of the intellect based upon claims of property in ideas. In such a system most men and almost all women, she writes, “are assigned to positions in the lower classes”, and the system “preserves for a small group of self-recruiting males both

hegemony over received knowledge and control of a variety of rewards and privileges” (Carroll 1990: 136), what Znaniecki (1968) calls the “men of knowledge”. Znaniecki recognizes that there are privileges that accrue to these “men of knowledge”, which enable them to use their positions, privileges and influence to instantiate disciplinary practices that are taken as givens. But it is Carroll who alerts us to the other side of the coin; that is, to the many techniques of depreciation and dismissal of the work of women, especially by denying its “originality”.

One might argue that the masculine subject position is enacted and reinforced through the use of imagery, whether with the actual illustrations accompanying the texts in the presentation of knowledge or in the ways of representing the practice of archaeology [as in Shanks’ (1992) now infamous strip tease metaphor]. The very dependence upon visuals in archaeology, as Moser (1996, 1998) and others (e.g. Joyce 2002: 136) have demonstrated, invites the use of visual imagery for reifying the masculinist position. For example, a photograph of Lewis Binford in the popular science journal, *Discover*, perhaps exemplifies several of these above points (although this could easily be seen in a photo of many contemporary archaeologists, as well). The image is one of just his head and shoulders; he is holding up a stone tool in his hand. One is impressed with the appearance of a personal control over the subject matter, over the object. The text of the caption is authorial, totalizing, and without equivocation: “Lewis Binford thinks Neanderthal men used stone tools like this one, but women used different, simpler implements”. Amazingly, in claiming that women made simpler “implements”—not even tools?—Binford is authorial on a subject—the sex/gender of artifact manufacture (and from some 30–80,000 years ago)—that he himself claims as one of those “ethnographic details” that lies *outside* of archaeological purview (c.f. Wylie 1991).

Furthermore, this photo and caption appears in *Discover (The World of Science)* (February 1992), one of those magazines at the interface between the academy and the lay public. In a provocative analysis by O’Rand (1989, cited by Lutz 1995a), she refers to a model for how scientific facts are diffused, which is what is happening here with the Binford example—by means of both the photo and the accompanying text of the article. O’Rand specifically discusses how it is not just “facts” (e.g., about the past) that interdigitate with popular culture, but how theoretical presuppositions (such as those embodied in Binford’s model for male/female relations among neanderthals in this same *Discover* article) are first drawn from wider cultural ideologies (in the Binford article, this is phrased in terms of “the visiting firemen model of sex”) that are then reconfirmed through science and funnelled back with new authority (see also Moore 1994). “Theory is then clearly about the politics of *all* culture, not just academic culture” (Lutz 1995a: 261).

Theory has been used in archaeology as a tactic for disciplinary control even when it is not actively engaged in: the very abandonment of theory to methods-centered practices and concerns disallows for the requisite challenges for a healthy discipline, and inhibits pluralistic possibilities. It also limits the discourse to the methods and to those who have access to the kinds of field sites and databases for taking up the preferred and currently promoted/touted methods (e.g., Geographic Information Systems, stable isotope analyses, etc). The identification and naming of what are to be the research problems is often where theory can be hidden, if not actually disguised.

In being taken up, deployed (note the military metaphor), circulated, and defended from the position of the male subject, archaeological theory is gendered male. There is little in the way of “fluid ego boundaries”, and the narratives and imagery of theory—even in the absence of the strip-tease—evoke not *inherently* male attributes, but those attributes that have been more closely identified with the masculine subject, in our times and in our specific academic and (limited) socioeconomic and cultural contexts—keeping in mind that Anglo-American archaeology is still nearly a completely white and middle-class enterprise. As Lutz succinctly noted, “when theory is gendered, it is simultaneously raced and classed” (Lutz 1995a).

### Re-Gendering Theory?

Is an ungendering/re-gendering of theory possible in archaeology; is it desirable or necessary? By a “re-gendering” of theory, I do not mean that it should be re-gendered to the female subject position, for to replace one cultural essentialism with another solves little. By re-gendering theory, I mean it in the sense of having theory that is, in fact, explicitly sensitive to the *contextual* factors, that is, responsive to context. We need to better understand, for example, the contextual values of theory: how is it valued differently by different groups? Furthermore, as Hartsock (1995: personal communication) has pointed out for political science, “some work in theory is highly valued, but it is not theory that is concerned with values”.

I am not advocating here the notion that we want to just “add women” or add a gender-sensitive component to doing theory in archaeology. Rather, a feminist perspective on doing theory includes several other components, such as the recognition that any discussion that highlights or centers on a singular, usually heroic (male) figure (read, theorist) obscures the *distributed* nature of knowledge production. This knowledge production is dependent upon socio-materialities—and differentially distributed ones at that; that is, there are both social and material support systems, networks, grants, access, structures, labors, values, and reward systems that underlie the necessarily collaborative work of archaeology, even that of theory. Despite many attempts to argue otherwise, especially given the modern capitalist contexts of knowledge production that promote the value (as if it were inherent) of individualistic creativity, originality and proprietary claims in ideas (after Carroll 1990:143), many historians of science have long recognized the “essentially *combinatorial* nature of ‘originality’” (Carroll 1990: 144, emphasis added), or what, especially in archaeology, is the *co-production* of knowledge. As Suchman (2007) suggests, it is still all too rare to expose the underlying constitutive socio-material practices and their distributions that enable who and what is “in”.

We need to re-historicize and re-present the production of theory in and for archaeology, and in such a way that we explicitly articulate and connect the intellectual and practical labors (such as manuscript reviews) that produced and continue to produce innovation and theory. For example, the loci or sites of innovation and theory need to be “de-centered” from singular persons, places, ideas and concepts—the Binford, the Hodder, the Cambridge School, the British Neolithic, the megaliths, etc.—so as to bring into view the “multiple acts of everyday activity” (Suchman 2007). This would include the analysis of precisely how it is that only

certain actors and their associated “achievements” come into public and disciplinary view. We need to develop a strategy for this de-centering so that we can try to *distribute* practices previously associated/identified (exclusively?) with certain “locations” (persons, schools, preferred archaeological phenomena) across our disciplinary landscape. Only occasionally do these “multiple labors” get exposed—how the work of the sociologist Merton, for example, lay behind Binford’s “middle-range theory” (Raab and Goodyear 1984), or Engelstad’s (1991) exposé of how certain post-processualists ignored some of the feminist work that underpinned their own “innovations”.

Another component to a feminist perspective on “doing theory” would agree with Christian’s (1990) concern over what she called “the race for theory” and all that seems to go with it, even within feminist communities. Rather than succumbing to the idea that we must join this race for theory, get on the theory bandwagon to be accepted, and make sure we have a visible and valued place on the stage, perhaps our reaction should be less about carving out a theory niche in the manner of extant and long standing theorist positions and posturings, and more about reconfiguring archaeology. That is, perhaps our efforts should go toward reconfiguring archaeology, not through the selection and promotion of new and other “inventor heroes”, but through what Suchman would call the “mundane, and innovative, practices of collective sociomaterial infrastructure building” (Suchman 2007). One example of this might be the emergence in academia more widely of “public scholarship” (Cantor and Lavine 2006), where scholars are “leaving their campuses to collaborate with communities”, even if it is not yet clear, as these authors note (Cantor and Lavine 2006), that such scholars will be rewarded for such public scholarship.

Thus, this is not a call for more, for “new” or for our own (whatever-groups-we-may-be) theory, but for questioning theory; that is, to continue to pursue the on-going and still much-needed critiques that feminist work of all sorts has initiated (even regarding its own theories). As well, perhaps archaeology is at a point where there is need for more of what might be called “practical justice”—not just more theory, especially for the sake of theory. As Anzaldúa has articulated, we need to “de-academize the theory and connect the community to the academy” (1990:xxvi).

### **Some Closing Thoughts: More Questionings of Theory**

The theoretical landscape of archaeology is, of course, more complex and diverse than is usually supposed. The simplistic mantras of theoretical periods (culture-history/processual/post-processual) are themselves ideological representations of disciplinary culture and its history, with not-so-hidden implications for disciplinary control and differential valuation of the production of archaeological knowledge. The tendency towards the monolithic, the all-encompassing (almost monotheistic?) is one of the mechanisms that contributes to the ideologies of dominance that feminists would seek to dismantle. “Inevitably”, says Barbara Christian (1990:341), “monolithism becomes a metasytem, in which there is a controlling ideal, especially in relation to pleasure. Language as one form of pleasure is immediately restricted, and becomes heavy, abstract, prescriptive, monotonous” (see Joyce 2002). When the

world (of archaeological theory) is seen as being not so simple, there is not likely to be a rush to create or adopt abstract theories. A major objection to the “race for theory” (after Christian 1990) must be the question: for whom are we doing what we are doing when we do archaeology?

One need not be a Marxist to see how “theory has become a commodity” (Christian 1990: 335), nor a feminist to see the old pattern, once again—“the ideas of a few [“big men”] are used repeatedly at the expense of the not-so-elite” (Mihesuah 2003: 28). The question now is how has this mattered, has archaeology been compromised, and how do we re-write or at least debate the “internal scripts” that we all have about the practice of archaeology; internal scripts for the criteria for theories, for what counts as evidence and for “how to know” (after Minow 1988:52)?

Archaeology as a field is inherently suitable to feminist practices because it is so full of ambiguities; it is already so necessarily collaborative, requiring in most instances a “diffusion of power” at least in the practice, if not in the public presentation of results. Archaeology is so often all about the “stuff” of everyday life as it was enacted by men, women, children and all sorts of social personae—the very nature of which feminist research does not want to have “disappeared”. This is why theory, including, but not only, feminist theory, matters for archaeology and not just for archaeologies of and about gender. The seeming rejection and avoidance of theory—by many, with many different strategies (“too theoretical”, “not empirical enough”, conflation with methods, etc.) is troubling because of the ways in which theory has worked to disappear the real issues of politics and power, to disappear the sources of one’s ideas and assumptions (since there is even a theoretical stance to being a-theoretical, to claiming that one is not doing theory). To discount the linkages between knowledge and power “encourages the reintroduction of the kinds of hierarchies that feminist analyses have repeatedly questioned” (Collins 1999). All too often, there is the valuing of scholarship of those doing (certain kinds of) theory, in certain ways, and about certain subjects or topics over those doing other “work”, in theory or in other domains, such as outreach, teaching, contract archaeology, or administration.

What is probably most threatening, but not openly discussed, about feminist theorizing and the implications for archaeological practice is *not* the idea that social inequities of gender, race, class have been perpetuated and underwritten by archaeology, that gender biases have (and still) exist in our archaeological accounts as well as in our practices of recruiting, hiring, promoting, and valuing. Rather, what is most threatening may be the feminist challenges to the “god’s eye” view and to the unmitigated objectivism that persists; that is, the admission that there are always “situated knowledges” (after Haraway 1988) in archaeology. To cast the debate as being one over if there is a “real past” or not is to deflect the real issue at hand, which is, namely: that there can and should be multiple perspectives on the past, and that these are not just due to the equi-finality or weaknesses of our archaeological data sets, but to the multiple subject positions from which “the past” is and can be understood.

What might a feminist approach to theory look like in archaeology? At its core, it would be about knowledge and power, difference and identity, social life and the social production of belief and praxis. While holding “gender” as ontologically

complex and varied phenomena, it would not disappear gender, if only because so much of archaeological terminology, core questions, categories, language and images are already deeply gendered and must continue to be scrutinized (e.g., Pyburn 2003). Theory in archaeology tends to be practiced from the masculinist subject position; the favored content of theory has gendered implications that all-too-often conform to the problematically gendered worlds we live in. If we accept even the possibility of gendered theory, along with the gender of theory-as-practice, what have the effects of this been? What, as well, are the effects of the simultaneous racializing and classing (e.g., Patterson 1995) of (gendered) theory? The fact that we can value theory or not means that it can be used and manipulated in many ways.

I have tried here to ask some of the questions that might yield insights into the practices of archaeology and how theory is used, valued, taught, circulated, reproduced, cited (or not). I have suggested that it is not just the doing of theory (or not) that matters, but it is who does it, and about which subjects we have chosen for our inquiry. There is much more work to be done to more fully understand the specific mechanisms whereby power—and thus the more visible and prevailing representations of the human past—can be gained and even concentrated through the production of theory. There are citational practices to better understand (Hutson 2002; Lutz 1990) and writing styles to analyze (Joyce 2002). There are genealogies of theory to chart, and more political economy of the discipline to probe (Wobst and Keene 1983). And there are different kinds of Theory Readers to produce.

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