

Shaken, Not Stirred: The Revolution in Archaeology

K. Anne Pyburn
Indiana University

ABSTRACT

A generation of archaeologists has shown that gender stereotyping has impeded our ability to understand the political economy, social organization, and cultural changes that are reflected in archaeological data. But feminist archaeology has also shown that to understand the past we could not simply “add women and stir”; that researching gender would require interrogating a great deal of received wisdom and might ultimately challenge some of our most cherished anthropological and ethnographic categories. By studying gender in the past we were not trying to place women within the standard anthropological framework of understanding, we were trying to shake the framework. The important contributions to this volume come from some of the first young scholars to be born into that shaken framework; each providing new perspectives, new interpretations, and new insights into the lives of ancient people that expand not only what we perceive about the past, but also what we can understand about ourselves.

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With this volume comes concrete evidence that gender stereotyping has impeded our ability to understand the political economy, social organization, and cultural changes that are reflected in the archaeological record. It was clear from the early feminist work by Gero (1983) that we knew almost nothing about the lives of ancient women and that archaeology as practiced was not going to improve the situation. But it was also apparent that to understand the past archaeologists could not simply “add women and stir”; that researching gender would require interrogating a great deal of received wisdom and might ultimately challenge some of our most cherished anthropological and ethnographic categories. By studying gender in the past we were not trying to place women within the standard anthropological framework of understanding, we were trying to shake the framework.

The empirically inclined feminism of the processual movement dealt a death blow to ahistorical generalizations about “mankind” by showing that essentialism is not benign and political awareness is a requirement rather than an im-

pediment to scholarship. Study after study has shown that stereotypical assumptions about women in the past are not just questionable, they are wrong. Men did not make all the stone tools, women did not make all the pots for cooking until markets arose, men do not control all economic specialization, and production of anything including food is cooperative in most places past and present. Rejecting familiar frameworks and vocabularies has proven daunting, and scholars often find themselves in uncharted territory where a certain amount of hubris is required to go forward. But forward we have come, and the new generation of archaeologists represented by the voices in this volume launch their investigations from firmer ground.

The chapters in this volume all reorient views on the past to see possibilities that were invisible to their predecessors. The division of labor, the origin of power, the reality of political maneuvering were all areas of investigation that were buried by assumptions about gender in the past. This is the first generation of scholars who are being intellectually born into our “shaken framework.” This is not to say that

literature on gender has not exploded in archaeology or that significant contributions to our understanding have not been accomplished but only that today's scholars have less to fight against, and they have more confidence in their footing.

Possibly the most important contribution of this volume lies in its theoretical allegiance to what has been called the "militant middle ground" (Wilk 2002). While steering past the Scylla of processual positivism and the Charybdis of postprocessual relativism, many archaeologists find themselves intellectually challenged by partisans from both camps. *Ungendering Civilization* (Pyburn 2004), a book also bringing young voices into the conversation, has been tarred as "antiscience" because it rejects cultural evolutionism and "positivist" because it rejects "other ways of knowing" for assertions based on verifiable data. It is a pleasure, and a relief, to find the archaeologists and anthropologists in this volume happily counting shells and measuring pot sherds in the service of sophisticated questions framed through confidently deployed *modern* social theory.

As we better understand the history of human beings we develop new intellectual weapons against bad practice. Now we know that sexist models of cultural development inaccurately portray the lives of women, as well as the lives of men and even more importantly the strategies that people used to create ancient households, communities, states, and civilizations. We begin to have a wider view of the possibilities for humankind. Collaborating across generations and developing an engaged community of scholars who think about the past in terms of an authentic present are part of a strategic framework poised for a renaissance in the discipline of archaeology. The importance of this development cannot be overstated; the future of archaeology is at stake in a world where frivolous ethnocentrism and "unintentional" sexism are too costly. However we approach the task of making a more responsive and more responsible academy, we can all agree that what we are talking about is not *just* women's work.

The Important Contributions of This Book

Houses and Households

Teotihuacan is a particularly interesting place to look at the relationship between gender roles and political economy, since Teotihuacan has long been the yardstick for state structures in the New World. For a couple of generations, Teotihuacan was regarded as the ultimate manifestation of a primate urban center in Classic period Mesoamerica and was used to show what Maya communities lacked on a cultural evolutionary scale of complexity. Here were the

nucleated populations, urban settlement densities, and hierarchical architectural arrangements just as expected by the New Archaeologists. Dwellings almost sort themselves into class-bounded groups by the relative wealth of their accoutrements, neighborhoods can be identified by the commonality of imported material culture implying ethnic enclaves, and workshops for specialized producers seem to glow from the archaeological record of Teotihuacan to illuminate the cultural evolutionary models so inspirational to the previous generation of researchers. If ever there was a place to look for the subordination of women, this must be it—a bastion of precapitalist capitalism where the control of the means of production, beginning with the control of women, should be as visible as goggles on a Chac rain god mask.

But De Lucia found little evidence for the expected patterns of male domination. Not only are gender hierarchies not obvious in iconography or burial furnishings at Teotihuacan, but other aspects of identity seem to take precedence. In particular, age and residential affiliation appear to have been more crucial than gender or sexuality in determining the social order.

De Lucia's chapter provides one of those satisfying surprises that come from the reanalysis of data that have been uncritically gendered. To put it simply, the expected differences in the mortuary treatment of men and women only appear if assumptions are made a priori about the relative value of different types of artifacts. Why does the association of a particular type of artifact with males indicate elite status? Why is it not just an indication that the person was male?

De Lucia is grappling with the paradox that worried the authors of *Ungendering Civilization*. In order to critique the gendered assumptions that were usually implicit (but sometimes explicit) in archaeological constructions of the rise of the state we had to work within a framework that made us uncomfortable. We expected that the investigation of gender would undermine traditional assumptions about "early states" and recast human history as a less teleological process than the "rise of civilization," but we had to start somewhere. Although it is tempting to simply turn away from simplifying generalizations and start fresh with new ideas as several scholars have effectively done, I think it is crucial that we rethink our categories from the ground up. If we are going to move forward toward new understandings we have to show conclusively why the current interpretations fail, not only logically or politically but in terms of the empirical claims made by the proponents of the original models. Despite the rising critique, and it was strong 25 years ago, the model of cultures that rise in complexity, overshoot, collapse, and disappear is still with us in our professional literature and prominently in the public domain

(Diamond 2005). The political repercussions of this sort of model for living people are often negative, but saying that will not make the ideas go away.

In fact, De Lucia echoes a number of points that were important in *Ungendering Civilization*, including that ambiguous iconography has been ignored or infused with gendered assumptions (Lowe 2004) and that a gender hierarchy has been the default assumption in the absence (and sometimes even in the presence) of data to the contrary (Alcalde 2004; Luedke 2004). I would add that preservation may be an issue as well, since some important status and economic items such as foods and textiles would not always be preserved. The pots themselves may have had negligible significance compared to what was in them.

Archaeologists have been extremely reluctant to consider the possibility that idiosyncrasy and emotion may influence burial assemblages and the treatment of individuals in death. For some reason assuming that ancient people behaved emotionally has come to be considered more ethnocentric than assuming that they did not. But there is no reason to argue that ancient people never placed artifacts with the dead that expressed personal grief and that all mortuary treatments necessarily and consistently index the general social order (Cocom and Pyburn 2006).

Furthermore, a spindle whorl may be buried with a woman because she used it, because she did not use it and was a bad mother, because she was a master weaver who controlled a workshop, because she liked it and wore it as an ornament, or because one of her children thought she might use it in her afterlife. There are innumerable possible competing explanations that can be imagined to have obtained within a single culture all at the same time, which is part of the reason that patterns are so easy to unintentionally fabricate (Brumfiel 2006): a limited number of objects can express an unlimited number of human ideas. Even when burial goods do index the status of the deceased, it is likely that these indications are a by-product of the quality and quantity of what was available rather than an intentionally constructed marker.

De Lucia uses the concept of a “house society” to explain the patterns of continuity and complementarity she identifies at Teotihuacan. According to Gillespie (2000), in a house society it is the structure of relationships among members that is culturally mandated rather than the role assigned to a particular sex or a certain age grade or a certain marital status. Gillespie takes from Lévi-Strauss (1963) the idea that structural transformations allow residential groups to function regardless of their gender composition, since what is structurally important is that there is someone to grind corn and care for children and plant fields, rather than that males and females adhere to a rigid division of labor.

The fact that weaving is considered women’s work does not mean that only women can do it or that a house must biologically produce a person of the correct age and sex in order to produce woven cloth. In Gillespie’s corporate house, regardless of its composition certain structural relationships must be upheld and people will be allowed, encouraged, paid, imported, adopted, or co-opted to perform the necessary tasks determined by the structure of the house. Corporate groups by definition have communal ownership or access to certain resources. Figuring out what these were for the urban enclaves at Teotihuacan will be an important topic for future research.

In contrast, Coleman Goldstein provides an elegant means of identifying noncorporate households in her analysis of Andean data. Households as described by Netting (1993) and Wilk (1983, 1997) are long-lived economic units in which strategies of production and consumption are certainly influenced by the relationships among members as is the case in house societies. But no particular role is so essential to the perpetuation of a household that it must be filled for the economic unit to persist. While the death of a mother may result in her replacement either by a new wife or by another member of the household who may or may not be female, it is also possible that the role of mother may be eliminated. Rather than import a new mother or reassign another household member to the task of raising children, a household may choose to export its young resident children to be raised by another household, or even leave them to do the best they can with no mother. The out-marriage of a specialist producer may result in emphasis on a different specialty already present among remaining members, or the household may simply do without disposable income.

By showing that the number of hearths is not the same as the number of grinding stones, although both are part of a domestic tool kit, Coleman Goldstein demonstrates that economic units are not contained within (and therefore not synonymous with) dwelling units or reproductive units and that basic human needs may not be met by a nuclear family but by a more flexible association of interdependent actors. While identification with a “house” or residence area may play a central role in social relations, household affiliations may vary seasonally or over the course of a lifetime, and the structure of any given household may or may not closely resemble that of its neighbors, as also noted by Morehart and Helmke (see below).

Cooking is an area of social relations that has been dramatically understudied in the context of ancient New World cultures. Probably this is because the preparation of food is assumed to be a female task more akin to drudgery than to political economy. But who prepares what for whom

and how they do it is an immediate index of status in extant cultures—why not in the past? While dietary studies have begun to unpack some of these assumptions, there is still a poorly examined tendency to associate a “healthy” diet with elite status and political domination in the past. This perspective contains implicit assumptions that all cultures value protein equally and recognize “health” in the same way, neither of which is correct in the present.

The preparation system described by Coleman Goldstein, which amounts to specialized equipment used by a group of technical experts for mass production beyond the household level, would be labeled “specialist production” were it associated with manufacture of anything besides food. Cooking is one example of a subject that has been doubly stereotyped in New World archaeology: not only is it assumed to be an uninteresting female task but also food preparation has been discussed exclusively in terms of nutrition and diet or the production of ritual foods associated with the need to propitiate deities or reify the social status of elites. But it is well known that ancient people in the Classical world had food preferences and went to great lengths to acquire particular delicacies that were specially prepared (Davidson 1997). New World people undoubtedly had special foods in addition to ordinary sustenance, just as they had specialist-produced pottery and worked stone in addition to ordinary vessels and tools. Not only the ingredients, which may have been more finely differentiated by consumers than they are by archaeologists, but also the manner of preparation is an important means of creating social cohesion as well as group boundaries. Variation and change in food technology and production are of great archaeological interest, but it may ultimately be recipes, serving styles, and contexts of consumption that hold the answer to our questions about household organization and political economy.

Undivided Labor

Preston-Werner grapples with the assumptions about gender inherent in most analyses that contrast domestic production with artistic production. Traditionally in archaeology the domestic production of food is contrasted not with food preparation as an art but food preparation as a ritual. Elaborate metates from Costa Rica, in contrast with more mundane grinding stones, are considered art, but art itself is interpreted as “ritual,” which is assumed to be male territory, despite the fact that these are perhaps the most commonly gendered “female” artifacts. The task of grinding food is never, as far as I know, attributed to men. Interestingly, the category of “art” automatically triggers the analytical binaries of a structuralist perspective. Preston-Werner develops

a much more nuanced argument avoiding the binaries “that essentialize gender and homogenize historical variability.”

Some of these binaries are oversimplifications not only of gender roles and relations but also of the preindustrial production process. Elizabeth Graham (2002) has pointed out that making Classic Maya pottery required

body clays, slip clays, paints, brushes, holders, resins, cleaners, paper for designs, mineral pigments, stands, wooden rollers, tempers, kilns, firewood, and sponges, not to mention help in preparing surfaces, preparing ingredients, stoking fires, regulating airflow, getting lunch on time, settling clays, toting water, ordering supplies, keeping track of transactions, training and feeding apprentices, and cleaning up the mess at the end of the production day. [Graham 2002:414]

Like grinding food, pottery-making is often considered a womanly task that changes to a male-organized process when the product becomes “art” with a ritual function. But Graham’s “to do list” makes it instantly clear how unlikely it would be for the entire process to be controlled by one gender, much less a single person. Insisting that some portions of the process are higher status or arbitrarily gendered seems unnecessarily ethnocentric.

The assumption that art must have a ritual function in ancient societies has been little challenged in New World archaeology. No one argues that the residents of Herculaneum ritually decorated their homes with murals or that the Greeks were not interested in aesthetics—they invented the word. While it may be ethnocentric to assume that ancient Costa Ricans took pleasure in the beauty of their metates, it is certainly ethnocentric to assume that they did not. But if things are valued for aesthetic reasons and those things are associated with female tasks, there can be a suggestion of artistry associated with that task. In fact, the existence of special elaborate metates alongside much more mundane grinding equipment might suggest a use *without* ritual significance. This is not to say that women were not involved in the production and use of ritual paraphernalia but that archaeologists need to take another look at the role of aesthetics outside the ancient Classical world.

Elaborate metates appearing alongside more ordinary metates in the archaeological context of a culture with only marginal evidence of maize production provide a conundrum for those who would uncritically gender artifacts. If the hearth-home-food preparation stereotype that identifies metates exclusively with female labor is correct, what do we do with metates that seem to indicate ritual practice, artistic virtuosity, social status, and political authority? The predictable response has been that they were not for the use of women and they were not *really* metates. But the fact

remains that something that appears to be a version of a metate has been made for some public, symbolic, and aesthetic purpose. If only men have significant political power and ritual control, why show this with an elaborate metate? Why not a giant atlatl or a stone effigy of a drum? If the elaborate metates were actually seats for important personages (and I find this possibility reasonable) I would certainly expect to see women seated on them. Why else associate authority with a gendered task? Alternatively, if either gender could grind food, we are liberated from an even more tedious stereotype.

The possibility that the ambiguous sexuality (at least from the archaeologists' perspective) of many Costa Rican figurines could be intentional is an unusual observation, though this is the case in the art of many cultures (Lowe 2004). That figurines show women, men, and unsexualized individuals on metate-like seats certainly opens the possibility that authority was not sexed. While it is easy to counter sexist interpretations of the sort leveled at Costa Rican art, Preston-Werner brings multiple lines of evidence to her analysis to suggest alternative readings, a much more difficult task, which she has done admirably.

Morehart and Helmke also consider the significance of heterogeneity in household production. By tracing the variation in botanical remains, they point to the variation in economic strategies of Maya households according to a given sociopolitical context and show how homogenized gender categories and static binary structures fail to capture the truly dynamic nature of domestic life. Morehart and Helmke discuss the ahistorical tendencies of iconographers and show just how misleading the gender stereotypes (supplied by a poorly realized application of the direct historical approach) can be. In their analysis of wood procurement they show clearly that evidence of micro-social variation is missed when archaeologists fail to consider how ancient domestic activities must have played out "on the ground." The trick is to consider experiential data enough to prevent us from attributing impossible lives to people in the past without going too far with phenomenological enthusiasm (cf. Barber 1994). While recognizing the validity of critiques that identify Maya archaeology as a discourse of power (Pyburn 1998a), the authors see this as cautionary rather than prohibitive, arguing that attention to historical context alleviates the worst excesses of stereotyping generalizations. Morehart and Helmke have given an excellent corrective to stereotypical gender categories by demonstrating how archaeological data simply rule them out.

The variation in the distribution of wood and in types of wood suggests several interesting lines of further inquiry. Because gathering firewood and water, making pottery, tending crops, weaving clothes, and grinding tortillas are not part

of the daily routine of archaeologists, we tend to think of each as a discrete task resulting in a single category of products. But such tasks may be broken down into constituent elements for more efficient production. So people returning from their fields may bring back wood that they pass on their way, potters searching for clay may stop to collect cochineal, and a trip to grind masa at a shared location may pass close to a spring offering an opportunity for water collection.

Gendered tasks may engender secondary tasks for the sake of convenience. Whether men or women (or children) collect or produce a particular product may depend on how tasks can be most efficiently combined and on what other tasks are on the day's agenda. If the family expert on growing beans is headed for the fields, she may be the person who usually collects water. If the task of collecting wood is left to children, as is sometimes the case in Maya villages today, larger children may be charged with the task when logs are needed, while smaller siblings can collect kindling. And it may be that the intended use of the clay, cloth, water, or wood determines who is eligible—or likely—to collect or produce it. Women who have caught a small peccary and decided to roast it underground may pick up one sort of wood on the way home while men who plan to do some face-painting may keep an eye out for wood types that make good smudging charcoal.

Again, it would be interesting to look for specific patterns of food preparation and the choice of particular ingredients as possible indications of "taste" or "palate" rather than exclusively as a reaction to local nutritional needs and variables of availability. Variable wood and plant use may imply not only variation in production but also culinary choices. For example, different types of wood burn at different rates and impart different flavors to foods. Morehart and Helmke certainly leave open a door to the possibility of culinary choices when they counter the simplistic application of the direct historical approach, which treats similarities between the Maya past and the Maya present as a result of the inherent "Maya-ness" of the actors (Pyburn 1998a). More interestingly, and probably more correctly, Morehart and Helmke suggest that types and degrees of "Maya-ness" can be deployed or not as appropriate to context, and the study of similarities between past and present Maya must take into account similar contexts rather than assuming rigid domestic routines and an improbably unchanging ethnicity.

Miller takes on the essentialist notions of pastoralist economies that have long constituted received anthropological wisdom. Her point that during transitional periods labor requirements would militate against rigid gender roles is certainly well taken and offers a satisfying counterpoint to the familiar assertion that women are culturally conservative and resistant to change (Billson 1995). Miller describes

a situation in which the need to respond to new economic pressures and opportunities results in greater flexibility in defined gender roles, which ultimately means not only that the role of women is not rigid but also that the ability to either remain constant or become flexible gives women important unrecognized avenues of social control. Women who refused to give up gathering or who insisted on herding could easily have determined history since men alone could not have succeeded at either.

Miller's analysis of the transition of hunter-gatherers in Egypt to the earliest herders brings up a number of interesting points about the use of ethnographic analogy. While ethnography must be our major source of hypotheses about the past to help us resist the ethnocentric influence of our own experience, it is important to remember that not all pasts have current analogs. The earliest pastoralists must have experienced conditions that cannot be replicable today but that need to be considered if we want to understand the relationship between cultural change and gender roles. Miller also discusses the inclination of iconographers to interpret rock art as representing everyday life and directly amenable to analogical interpretation. Such representations may have had a more formal significance, indicating preferred gender roles rather than average experience—or a less formal significance depicting a single incident or a particular family.

Using data collected from the Egyptian Saharan Site E-75-6 at Nabta Playa, evidencing an early shift from hunting and gathering to agro-pastoralism, Miller shows that more than one type of subsistence economy was probably practiced by people living in the same community and in some cases even in the same house. The two strategies, hunting and gathering and herding, have different gender implications, so at the very least people would have been aware that not all families constructed men's work and women's work in the same way. In fact, as new tactics were employed the decision about who did what probably had little to do with gender as no gendered tasks had been established.

This is similar to a point made by Mortensen (2004), who observes that societies in which men are frequently occupied or preoccupied and away from home give women a great deal of control by default. Although archaeologists have frequently suggested warfare privileges men by offering them a means of advancement not available to women, they ignore the reality of what happens to household finances and political decision-making when no men are available to control them. In the case of the pastoralists Miller describes, men are not absent, but in order to take up a new lifeway, they must have been willing to put results above convention.

Pastoralism, which places so much dependence on livestock, may never be as truly sex segregated as it appears (is presented?) to outsiders. Certainly the pastoral nomads

of Central Asia gender tasks and behaviors rather generally, training boys and girls together in shooting and riding and expecting women to fight under certain circumstances (Davis-Kimball 2003). What appears to outsiders to be a patriarchal organization may mean little on a daily basis when everyone rides, shoots, feeds and milks the animals, and moves camp.

Pankonien focuses on a closely related issue in her discussion of how shells implicate women's roles in ancient Oaxaca. The wives of fishermen have typically been depicted as passive and powerless, waiting for men to return from the sea to make all the decisions. She counters this construction with the important theme of collaborative production strategies that runs through many chapters in this book. Moving back and forth between the past and the present, she gives ethnographic weight to her considerations of archaeological data without devolving into direct historicism or simplistic functionalism.

She poses an interesting question about why people might make dye from shells in a technique that is so much more labor intensive than using the cochineal that was readily available. I think the answer to this lies in understanding ancient consumer culture. If, as I surmise, people acquired goods that they desired, and not just goods fulfilling needs, then we must begin to consider that different types of dye would have different market values in different places. Even if the resulting colors appear to archaeologists to be comparable, the two dyes may well have been considered completely different or at least have had very different uses and significance in the past.

In her discussion of gender and the division of labor in fishing communities, Pankonien echoes Miller when she makes the valid points that assuming that men were always at sea and women kept to hearth and home is an oversimplification, a stereotype, and that collaborative production and even role reversals are probably quite normal for any small-scale community. On the other hand, despite the fact that archaeologists continue to insist that warfare, trading, and hunting privilege males in social hierarchies, we still must come to terms with the reality that life does not stop in a home, a neighborhood, a community, or a state while the men are away. In fact, societies with chronically absent or preoccupied men rely on the political and economic acumen of the people left behind—the women—for sustained existence. Logically such conditions set the stage for power-sharing as much as they foreground patriarchy. It is time to turn the tables on the old argument that women are always dominated because they are preoccupied with children.

Pankonien's chapter also echoes that of Morehart and Helmke, who see it as necessary to challenge the hegemony of the direct historical approach, which traps interpretation

in a stereotyping loop. But as an ethnographer, Pankonien also takes the next step, showing how a stereotypical past has been used to imagine the present as something more, and less, than a living community: “[g]ender relations in Huatulco continue to shift, as they always have, shaped by industries that vary and economies that evolve.”

Consuming Politics

Tejeda considers how production changes would have been required by the changing political landscape of Middle Formative La Blanca in Guatemala. Like Miller, she argues that in transitional periods, rigid gender roles cannot be sustained and suggests that under outside pressure to produce for a burgeoning polity, households would have emphasized cooperation over role regulation. She asks the important question, How did the rise of the La Blanca polity change the social and economic strategies of households? Ultimately, she considers what impact these changes would have on gender roles, concluding that an increase in workload for everyone is most likely.

Tejeda’s analysis is excellent, but it is possible that she may have the causal arrow reversed. If, as it appears to me, what she is seeing through the archaeological record is the rise of a smallholder economy, then the land investments that increase productivity and promote (or allow) an attachment to place and create hereditary connections to the land also result in exactly the changes she identifies: intensification of agriculture, implementation of long-distance trade networks, and increases in craft specialization. But these changes do not occur in response to elite control or domination. In fact, control of production is difficult, inefficient, and potentially dangerous to smallholders whose subsistence strategies are microenvironmentally specific and not easily altered to suit arbitrary requests for increases (Pyburn in press).

Netting (1993) identified smallholders in Switzerland and Nigeria and in so doing he showed that smallholding can succeed in almost any sociopolitical economy that affords householders land tenure and land that can be improved through investment. Netting was not describing an evolutionary stage; he did not expect all farmers in a given environment to be smallholders (Nigerian Kofyar smallholders live side by side with Ibo who are not smallholders), and he argued that smallholders were crucially different from Chayanovian peasants because they can improve their land (Chayanov’s peasants lived where the land is very fertile but the growing season is only two months long). But smallholders do have three particularly important characteristics that relate to the origin of polities like La Blanca. Foremost is the

fact that they *love* surpluses. Farming is never a secure enterprise and any sort of cushion is desirable, so smallholders voluntarily produce as much as they can. Of course, many factors affect how much this will be, including the health and composition of any household, unpredictable weather, and the choice of crops, which may vary from year to year, but as long as smallholders control their own productivity, and benefit from it, they will produce as much as they can. Such farmers produce much more, in fact, than farmers working for a community or for an absentee landlord can be forced to work, since a surplus in these conditions offers limited benefit and can set a taxation standard impossible to sustain in all years.

There are other ways to create an economic cushion, and smallholders also *love* to diversify: any household with sufficient labor will add crafts to food production or acquire specialized skills to accumulate resources and stimulate reciprocal obligations among producers. Availability of extra hands is the only requirement for specialization. Weaving, pottery-making, tool-making, midwifery, necromancy, and so on are all strategies of accumulation of either real wealth or social indebtedness that mitigate the effects of bad weather, family tragedy, political upheaval, and crop failure that plague all agricultural communities. No coercion is involved, except perhaps between family members, in order for craft production to emerge. But obviously there is a limit to how much diversification can cushion a household among households all subject to the same pressures; if everybody’s crop succumbs to drought, the fact that one neighbor owes another a bushel of corn makes little difference and nobody will be interested in ready-made pots. So smallholders *love* markets. They go great distances to reach them and sometimes even create them (Netting 1993). Elites do not need to control or force surplus production of foods *or* crafts: there are more efficient ways to profit.

What elites do to gain and hold status and power is promote consumption and control distribution; the evidence of this strategy is seen in the presence of quantities of consumer goods in household middens. At Chau Hiix, the Maya site in Belize where I have worked for the past 20 years, household middens always include imported obsidian, chert, and even pottery, though chert and the ingredients for pottery are locally available. I have developed this anti-Polanyi (1971) argument at length elsewhere (Pyburn 1998b, in press), but at least superficially it appears to fit Tejeda’s data set without discounting the initiative of ordinary people or putting all the agency in the hands of elites. One important advantage of inverting the familiar elite-centered false-consciousness model of emerging states is that it leaves the role of women undefined and therefore more amenable to discovery (Robin 2006).

Tejada's chapter is a sophisticated restatement of archaeology's traditional model of the rise of social complexity coming about as a result of elites gaining control of the means of production. Patterns in the distribution of highly valued artifacts and shifts in the intensity of production and in dietary patterns have been documented all over the world as concomitant with material markers of centralizing and increasingly structured political authority. Without discounting the value of this model it is possible to add more dimensions to our understanding by weakening the assumptions underlying the archaeologist's typical definition of social complexity, which has echoes of a defunct theory of unilinear evolution toward "civilization."

Land tenure and access are probably the most important factors affecting household organization. Tejada touches this crucial issue when she notes that the development of house mounds would have made neighbors more visible to each other, possibly indicating increased interaction and cooperation among households. But Mesoamerican mounds often result from the buildup of reconstruction episodes, representing the reoccupation of a single locus over several generations. The development of household mounds may be an indication of changes in land tenure and in the value of certain types of land for intensive production. In this case, the visibility of the neighbors might signify increased surveillance of property and boundaries.

Engendering the Future

When I came up with the title of this chapter I did not know it had been used before but in retrospect it makes perfect sense that the idea of feminist research on the past as a tool for shaking up the world should have come early from historical archaeology. Margaret Purser titled an important group of papers presented at the 1991 Society for Historic Archaeology gender session "Shaken, Not Stirred: Current Gender Issues in Historical Archaeology." As long as archaeologists have uncritically regarded the past as peopled by anonymous "others" we have been comfortable with stereotypes. But when the other is us, when we are not thinking in terms of a contrast between simple and complex, between north and south, between men and women, between "us" and "them" we can see much more easily the inadequacies and the gaps in our explanations. The past generation of archaeologists was not personally offended by thinking about ancient women as bound to unchanging domestic routines, but historians and historical archaeologists have been thinking about women in much more detail, and sometimes they are thinking about their own heritage. Stereotypes are much harder to apply to particular indi-

viduals than they were to processualist-style archaeological cultures, and it is harder to notice we are patronizing other people than to recognize when we are being patronized ourselves.

In thinking about the future of feminist research in archaeology I have already alluded to several areas that should prove interesting. The contributions in this volume have reinforced my conviction that tracing consumer patterns may explain more about the rise of political elites than control of production and that economies based on pastoralism or fishing or warfare should be further examined for evidence of female influence on their political economies. Reading these stimulating essays has also led me to suggest that ancient New World cooking might be investigated as a specialist enterprise, that aesthetic appreciation may explain some ancient creations that have been overemphasized as ritual, and that household flexibility and efficiency explain more about ancient life than gender roles and elite control.

But all these avenues of investigation have an edge that is still unfamiliar to most scholars of my generation. The effort to undermine stereotypes began as a wedge into the intellectual stronghold of male-oriented interpretations of the past, and the early phalanx of feminist archaeologists was acutely aware that they were ultimately trying to undermine the male power hierarchy of archaeology as a discipline. Collected volumes on the archaeology of women and gender usually included a chapter on the status of women as archaeologists, implicitly (and sometimes explicitly) identifying the narrowness of interpretation with the disciplinary dominance of heterosexual white men.

The result of this campaign has been an influx of women and people of color into the field, though this battle is not yet won (Conkey 2005). Nevertheless, the frontiers of scholarship have widened considerably and the questioning of received wisdom is more commonplace. As I said at the beginning of this commentary, I see these chapters as establishing a new beachhead for archaeology: it is time to take our shaken categories back to their origins. It is time for academic research on the past of women, of power, of political economy, and of our discipline to be reflected back to the ethnographers and the cultural anthropologists of the present to ask them to acknowledge our findings and further shake the assumptions they originally gave us. And as archaeologists have become more theoretically aware and more conscious of the political origins and repercussions of our reconstructions of gender, we have joined historians in seeing the implications of our reconstructions for ourselves and for other living women and men.

This transparency to the political present, which I have argued is more obvious to women and other groups who have had to pursue their scholarship outside the shelter of

the ivory tower (Pyburn 1999), is the future of archaeology. Groups on the receiving end of unflattering and restricting stereotypes have valuable insights that can alter the course of scholarship, but for the next generation the rare privilege to do archaeological research will entail a new level of political engagement and public responsibility. It is not difficult to see that the minds at work in this book are questioning the past and questioning received wisdom, but they are also questioning themselves. Archaeologists of my generation could write about a sexist past and not see a sexist present, we could write about the “rise of civilization” oblivious to the context of colonialism, and we could identify an ancient “other” without regard for a political “us.” We have lost the security of political irrelevance, but archaeologists have a new sort of authority within our grasp.

No End in Sight

The topic of gender in archaeology has turned out to be more protean than anyone could have imagined; at this point so much ink has been spilled over gender issues by archaeologists that it is difficult to think of something yet to be said. The importance of these chapters is that each has gone beyond rhetoric and theoretical course correction to reanalyze data that have been overlooked or underassessed. What my students and I found when we researched *Ungendering Civilization* (Pyburn 2004) was just how much was not known about gender roles, and we were amazed at the layers of assumption that we unearthed. These chapters take the next step of advancing what we know by asking questions that have not been asked before and trying out analyses that are based on different assumptions. I come away from this volume convinced that we have actually begun to know something about ancient people.

According to Barbara Tuchman (1978), when the rural peasants of 14th-century Europe murdered their lord for his profligate behavior, their goal was not to create a new world order. They wanted change but not fundamental alteration in the system they knew—they just wanted a better lord. The same process was described a generation ago by Anthony Wallace (1956), who showed how millennial movements are often begun by scruffy outsiders with charisma and a passionate desire for change in the status quo, but who invariably themselves end up as the next leaders of the establishment with or without substantial changes to the structure they have come to rule.

Processual archaeology and then postprocessual archaeology both deepened our understanding of the past and improved our approaches to it, but both upheld a traditional disciplinary hierarchy with somewhat narrow goals. A fem-

inist perspective, developed in the context of a feminist practice, is ushering in a new sort of disciplinary heterarchy in which a variety of different goals and methods can contribute to archaeology. Key to the rise of this revolution is an emphasis on the very type of strategy detailed in many of the studies published here as common to early households; that is, an emphasis on collaboration rather than competition and an emphasis on inclusiveness rather than the exclusionary practices common to an earlier age. What we see in this book is the beginning of a fundamental change in what it means to do archaeology and a significant advance in the knowledge these changes have already started to bring.

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