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Queer Archaeology, Mathematical Modeling, and the Peopling of the Americas

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Issues of chronology, technology, and subsistence have long dominated discussions of the peopling of the Americas, to the near exclusion of more anthropological topics. For example, little attention has been given to the social implications of an unpeopled landscape for understanding and indigenous sex roles and gendered relationships of the first Native Americans. There has been some recent discussion of the sexual division of labor among Paleo-Indians—and even women’s fertility (MacDonald 1998; Surovell 2000; Waguespack 2005). However, many of these approaches are fraught with biological and environmental determinism as well as gender stereotypes. Taking a page from queer theory, in this paper I seek to (1) explore that which does not “make sense” from my 21st Century, feminist perspective, in terms of modeling Paleo-Indian colonization, and (2) move away from heteronormative and sociobiological assumptions in considering paleodemography—e.g., the assumption that the only unit of analysis that matters for modeling demography is the heterosexual, monogamous couple. Instead, I seek alternative, less “comfortable” and less “logical” behavioral and biological parameters from which to build more complex and less ethnocentric mathematical models, which can then be tested against the archaeological record. What I outline here is a research prospectus—a plan for a plan of action, rather than new data or a corrective interpretation. I begin with a retrospective.

In Search of Paleo-Women

I began to consider issues of gender in understanding Paleo-Indian lifeways in 1989, when I was working on a paper entitled, “In Search of Paleo-Women: Gender Implications of Remains from Paleo-Indian Sites in the Northeast,” a paper that was eventually published in the *Bulletin of the Massachusetts Archaeological Society* (Chilton 1994). In this paper I reviewed the extant literature on Paleo-Indian sites in the Northeast. I concluded that an over-emphasis on hunting—and an implicit (sometimes explicit) over-emphasis on the activities of men—had severely limited a more anthropological understanding of what was certainly a complex and variable set of adaptations. My 1994 article was “womanist” in the sense that it was “concerned with the actions, status, [and]...presence of women” in the past (Joyce and Claassen 1997:1). In retrospect, this paper was flawed in that it focused only on “finding women” in the archaeological record, rather than providing an in-depth discussion of what can be gained simply by *looking* for women and by considering gender or sexuality as general structuring principles (Joyce and Claassen 1997:2). It has since bothered me that, while I thought the critique itself was valid, I had failed to offer any alternative models, interpretations, or ways of truly “seeing differently” (Dobres 1999). As a result, the article was read and used primarily by other feminist archaeologists and by those teaching courses on the subject. I am not convinced that it had any larger impact on how Paleo-Indian sites were interpreted, in the Northeast or beyond.

Beyond “Big”

In 2000, Michael Barton and Geoff Clark at ASU contacted me and invited me to contribute to an edited volume on the peopling of the Americas. Michael Barton told me in his

invitation that as he was pulling the volume together he noticed that most of the selections dealing with human behavior still had a “big men, with big spears, hunting big animals” approach. He had heard from a colleague at ASU—a feminist archaeologist—that I might be able to help. Here was my chance to make up for what I had seen as the shortcomings of my 1994 paper. The chapter that I wrote for that volume was entitled “Beyond “Big”: Gender, Age, and Subsistence Diversity in Paleo-Indian Societies” (Chilton 2004).

In the 2004 chapter my goal was to move beyond “finding women” to “seeing differently” (Dobres 1999). I continued my critique of the over-emphasis on hunting by scholars of the peopling of the Americas, and I argued strongly that by either inadvertently or intentionally focusing on the activities of adult men, that we had excluded the lives of women, children, and elders. However, I was still focusing on data as the driving force behind new interpretations. For example, I lay my critique of the hunting bias at the feet of preservation problems and environmental reconstructions. I suggested that we needed to look for the “little things”—things like turtles, grapes, and babies in the archaeological record, and that we needed more sophisticated techniques for doing so. I called for a model of Paleo-Indians in which there might not be a strict division of labor, given both the small group size and the instability and unpredictability of the social and ecological landscape. However, even though I argued that women do hunt in many societies and in fact can and do make tools, I was still essentially supporting what some call a “traditional” division of labor: men hunt big things; women and children hunt little things (see Brightman 1996; Lancaster 2003).

In the last part of the chapter, I began to explore the implications of a feminist critique on demographic modeling for the peopling of the Americas. Todd Surovell (2000) had at that time recently published an article in which he considered issues of women’s fertility in modeling

Paleo-Indian mobility in the context of colonization. In my 2004 chapter I applauded—and still applaud—Surovell’s efforts to consider women’s health and activities in the context of Paleo-Indians. However, I found his results lacking in several ways. Surovell’s (2000) proximate goal was to evaluate whether high residential mobility is compatible with high fertility. Ultimately, though, his main objective is to determine whether it is *possible* that Paleo-Indians populated North America within a 700-year period as, he believes, the archaeological record indicates (ca. 11,500-10,800 radiocarbon years B. P.). Surovell (2000) necessarily makes a series of assumptions about the Paleo-Indian population before testing his mathematical model. First, he assumes that in terms of reproduction, the goal of Paleo-Indians to maximize reproductive potential. Second, he assumes a “classic division of labor...men hunt, and women gather” (Surovell 2000:497). As a corollary to this second assumption he posits that “males never bring young children on hunting forays, but females must always carry young children the roundtrip distance on foraging trips” (Surovell 2000:497). Third, the model predicts that for any “homogenous environment” people can maximize their reproductive output by moving residential camps frequently, thus minimizing child-related transport costs. I will spare you the full critique here, since it is outlined in detail in the 2004 chapter, but the most important parts of my critique for this paper are:

1. I do not think that we should assume that the goal of any particular group or individual is “to maximize its reproductive potential.” I know this is heresy for some sociobiologists, but in any given historical and cultural context, we need to leave the door open to other motivations in human relationships.
2. I do not assume that men hunting and women gathering is in any way “classic,” nor would I assume a priori that women do most of the carrying of children. The

ethnographic cases we have for a sexual division of labor among hunter-gatherers are not at all analogous to the environmental and social context of Paleo-Indians, and the sheer variety of caregiving and subsistence economies in human societies should make us very careful about how we build these into models of the past (see discussions in Brightman 1996 and Lancaster 2003).

3. Surovell assumes that it is women's fertility that limits population growth rather than fecundity (carrying a child to full term) or infant mortality. While a certain level of fertility is important for the survival of a population (especially since it indicates general health status), infant survival is actually far more important than female fertility for determining population growth and, thus, reproductive success (Jones 2000).

Despite my critique, it was this paper by Surovell, as well as subsequent conversations and consideration of the work of James Holland Jones (2000) and Brian D. Jones (2000, 2008) that prompted me to consider "how can we model the peopling of the Americas differently?" That is what I will explore in the rest of this paper.

Queer Archaeology

As some of you know, most of the papers in this session grew out of a graduate seminar that I directed this past semester at UMass Amherst, entitled "An Archaeology of Gender." This was a truly four-field anthropology seminar in terms of the participants, though the readings centered on archaeology—that is the material products and precedents of gender and sexualities. All semester, throughout our weekly readings and discussions, I kept asking myself "how do I

move beyond Surovell? How would I construct a different mathematical model? All models require assumptions, so what assumptions am I willing to make?"

I started to work my way out of my circular critiques when one week we focused on the application of queer theory to archaeology (e.g., Dowson 2000). In particular, I came to realize how much the past has been interpreted by archaeologists in a "strictly heterosexual manner" (Dowson 2000:162). Further, I realized that my continued frustration with my own work was based on the fact that I was trying to reconstruct the past in a "better" or more complete way. According to Dowson (2000:163) queer archaeology "does not mean learning to construct the past better, but learning new, different ways of approaching the past altogether." It "actively and explicitly challenges the heteronormativity of scientific practice" (Dowson 2000:163). In thinking about how this critique can be applied to Paleo-Indian demography, it is clear that in all the demographic models that I have seen for the peopling of the Americas, the assumption is that the unit of analysis is the Western concept of the family: man, woman, and their children. As Dowson (2000:164) puts it:

Archaeology presents this Western idealized notion of the family as being as ancient as humanity. In so doing, the consumption of these constructions justifies and legitimizes phobias and prejudices in our society today. While archaeology consistently underpins a heterosexual artifice of human prehistory - archaeologists need to be aware of their complicity in Western society's institutionalized homophobia.

I started to ask myself, "what if I don't try to find a model or interpretation that *makes sense* to me from my 21st century, feminist, heterosexual, and white, middle-class perspective, but instead challenge myself to test several competing models?"

Mathematical Modeling: Estimating the Parameters

A first step in the creation of any mathematical model is to tease apart the dependent and independent variables—what are the pertinent variables and what are their relationships? For example, a list of all the possible variables to consider in modeling the peopling of the Americas include (and this list is not exhaustive):

- Fertility of men and women of reproductive age
- Fecundity
- Infant mortality
- Age at weaning
- Childhood diet and contribution of children to subsistence
- Number and type of sexual relationships (and how these relate to the possibility of pregnancy)
- “Family” size and composition
- Residential group size and composition
- Total breeding population size and composition
- Energy costs of subsistence and mobility choices
- Carrying capacity of natural environment
- Division of labor (sex/gender/age/other)
- Social/reproductive group size
- Residential group size
- Colonization rate (i.e., the number of new immigrants into the system)

Obviously many of these variables are dependent on one another, and some could be considered constants in certain circumstances. My goal is to work towards a goal of constructive two or three models that can then be tested against the extant archaeological data. Of course the data we currently have are not randomly selected samples. Nevertheless, since that is the case for most archaeological data sets, it is at least a good “next step” in the attempt to construct and test models and interpretations in ways that let us know what we are “up to” and “up against.”

Since I have not yet begun to outline out these models for testing, I did not want to leave you completely hanging in terms of how I envision these models playing out. So I will end this

paper by simply stating my questions as I move forward in trying to construct and test new demographic models:

1. What happens if we do not assume that all adult members of the population do not wish to become parents?
2. What happens if we hold the fertility and fecundity variables constant, and instead vary infant mortality?
3. What factors would have the greatest impact on infant mortality in this context and how can we model those variables?
4. What if we do not assume heterosexual pair bonds with dependent children, but instead model more fluid and variable sexual and social relationships (e.g., there is a strong case to be made for the antiquity of transgender homosexual behavior in Native American societies [Hollimon 2001:124])?
5. What if we do not assume a “classic” or “traditional” or even a static sexual division of foraging labor? To me this last question offers the most possibility. Holding all other variables constant, one could easily model population growth with a strict sexual division of labor on one hand, and a fluid division of labor on the other. This alone would tell us a lot about whether assumptions about this division really make much of a difference in “maximizing reproductive fitness.”

Conclusions

I want to thank all of the participants in this session (and in the seminar) for helping me to think different about how to model Paleo-Indian demography. I want to offer a special thanks to Brian Jones and Angela Labrador for brainstorming, email-storming sessions, and a healthy bibliography on how to move beyond critique to new models (although I take full responsibility for any faulty logic outlined above). While I am only at the very beginning of this process, queer archaeology has certainly led the way out of simply adding women to the hunt or adding more data to the pile. Instead, for me it is no longer about creating more “realistic” reconstructions, but about challenging what we consider “real” and about striving to find ways to carve out new realities for Paleo-Indians and ourselves.

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