LDB response paper 5 February

(Wendorf and Schild “Nabta Playa and its Role in Northeastern African Prehistory” 1998)

In general I have found this week’s readings characterized by two problematic themes that I want to address in class. One is a tendency to sweeping statements. I know this is in part because the data are still quite thin from Neolithic North Africa, but the level of jumping I find often breathtaking. I felt this particularly true in the Wendorf/Schild article, with which I am principally dealing. I will address this further below. The second overarching concern I found was a tendency to set up contrasted polarities as a way of thinking about/expressing difference. I found this was often maintained even when the author expressly claimed not to be doing so. Interestingly, I realized that I had found this a theme while reading the MacGaffey article. It was the last article I read, and I expected it to be a joke because of its date (1966 – on race, no less). I found it not without problems, but in fact a very enjoyable and thought-provoking read and spot-on in much of its thinking. It was MacGaffey’s attack of the idealistic polarity of Negro/Brown (or whatever permutation you want) that made me realize how much polarizing had been done elsewhere. I will deal with this briefly below.

The Wendorf/Schild article begins with a summary of the types of sites, types of finds and dates from excavations at Nabta Playa in the western desert. This was a fantastic way for me to aquaint myself with something I should know better than I do. Information I found particularly helpful or interesting was the discussion of el Nabta phase house sites with their arrangement that seems to follow the landscape, and their yearly reoccupation in the same place despite flooding. Storage spaces and the potentially different treatment of sorghum in this phase might give us points to discuss with regard to the porridge/bread article. The authors now seem convinced, and in this case I see little reason to doubt them, that evidence is sufficient to show management of wild cattle stocks (though principally not for butchery) from the earliest periods of Nabta’s use.

I became slightly more skeptical when we moved on to evidence for ritual behaviors. I do think that the circles of stones, the cattle burials (particularly the fully articulated young cow), and the accumulation of significant trash in an area with no evidence of settlement, show convincingly that there was ritual behavior in this area. As the authors mention, there is no solid evidence to suggest that Nabta was the gathering place for diverse regional groups for regularly scheduled communitiy rituals. Nonetheless, they suggest it. I have to say I find the buried mushroom-shaped “table rocks” confusing, too. Buried sculpture? What is the evidence for working the stones? I grant that they are weird, and their placement beneath built surface features is suggestive, but something about them makes me hesitate in accepting their conclusions. Did others find this problematic?

I became positively alarmed when we began to discuss the potential influence of Nabta cattle cults on Old Kingdom Egypt. In my mind, there are at least three fundamental things that would have to be demonstrated in order to accept the notion that Nabtans moving permanently to the Nile were responsible for strains of cattle worship in the Old Kingdom. First, you have to demonstrate the cattle worship. Yes, cows are important and do show up in relgious contexts fairly early in Egypt. But I would not at all say that early Egyptian religion has a cattle focus. If I were to give it a single focus at all I would say it was a king focus. And while the king can sometimes be equated with cattle on some level, this is far from the most prevalent representation of the king. Second you would have to have some evidence, other than vague religious continuity over thousands of years, that the Nabtans ended back in the Nile valley. Here I want to know what others have said about pottery. In this article the authors say that Badarian pottery shows closer affinities to Ancient Near Eastern pottery than to Saharan, but this was not what I thought going in. I want to poke around more on this, but I think a general if circumstantial case could be made. At least we know those people didn’t stay in the desert when things started getting more arid. Third, I would want to come up with a model in which it made sense for a single religious point – cows – to survive a major move to a different landscape and different economic role. I am not totally against the second and third things being possible, but given what I see as the dubious nature of the first point I’m not sure it’s worth trying. Also to consider, is there evidence that cattle herding itself entered the Nile valley only when the desert became too arid to support cattle-herding populations? I think if cattle are present in the valley before the final aridification then I am going to be uncomfortable saying that cattle worship comes from outside. If cattle themselves came from outside, I mgith buy it. Wendorf and Schild do not make this argument either way (though I suspect if there were strong evidence that cattle only came to the Nile with the end of population-supporting climates in the desert they would have hung to that like barnacles to a ship).

In terms of polarizing, some of the issues I found reprsented in terms of perhaps simplistic dualities were Africa/Ancient Near East (is it not insane to regard either as a single place?); bread/porridge (man that article kind of irritated me – so much of the actual point boils down to something she didn’t mention until her conclusion – that African grains by and large have no gluten while ANE grains do. I also found it problematic to concentrate on grain based foods when in fact animal based foods were so much more central to changing economic/settlement patterns in Neolithic north Africa.); domesticator/hunter gatherer; ceramic culture/aceramic culture.

The questions that rise out of this for me center around trying to determine, if we can, where significant changes lie. Does it matter when a culture first uses ceramics, or when ceramics are prevalent enough to indicate a real change in the way food is gathered, stored, processed or eaten? Or do they both matter? Why? How can we trace this? Does it matter when domestication takes place (which most authors seem to agree is when it can be genetically recognized, though many also admit some nuance in)? Or does it matter when significant changes are made with relation to the way food sources are managed – ie perhaps the scheduling of kills and so potential herding of wild animals? (Oh and a problem I had with that article, which I generallyl found very thought provoking and interesting, is that the model for scheduled consumption affecting patterns of approach to food sources was based on ethnographic observations of plant collecting. But the model was used to discuss animal herding. Is this appropriate? And also the assumption of numbers in a herd was based on an analogy to hunting, but herded animals might be used for very different reasons than hunted animals. Hunted are just meat and skins. Herded appear (ethnog) to be mostly milk and blood.) Do we actualy learn anythign useful from a comparison of the Neolithic in the Near East and Africa?