What good is archaeology?
Archaeological and ethnographic scales

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The Collaborative Research Center that facilitated the meeting on which this volume is based was titled "Culture-Environment Interaction and Human Mobility in the Late Quaternary." One project goal was to use ethnographic and ethnological data, as well as agent-based modelling to devise a model, a First African Frontier model, that accounts for how modern humans, some 50-75,000 years ago (or thereabouts), migrated out of Africa into Europe and, in fact, to the rest of the world. The original idea for this model was not archaeological but ultimately it must be tested against archaeological data.

This matters because the period in question, the late Pleistocene, during which modern humans expanded out of Africa, was a unique time in world history. The hunter-gatherers that we know today or from the recent past are firmly embedded in the landscape. They know their territories in minute detail. They have strong emotional ties to their lands places of stories, where the lives of ancestors are written into the landscape. Modern hunter-gatherers are also people who cannot move freely into new territories – because they are hemmed in by other groups, some hunter-gatherers, but most not. In contrast, the Pleistocene migration out of Africa entailed moving into some land occupied by other humans, notably Neanderthals and Denisovans, who were most likely living at very low population densities. And they also moved into land *not* already occupied by our genus: far eastern Russia, Australia, and, of primary concern here, the entire western hemisphere.

The movement from Africa into Europe, across Asia, and into the western hemisphere was, in geologic time, very fast, and entailed a level of migration, of territorial shift, quite unlike anything known among ethnographically known hunter-gatherers. In what ways do we expect these ancient hunter-gatherers to behave like those we know from ethnographic accounts and in what ways might we expect them to be different?

The problem is difficult, because archaeological and ethnographic data sources are not the same, and so analogies from ethnography are not easily carried over to the study of prehistory. I am speaking, of course, of the obvious fact that archaeologists cannot talk with the dead and cannot directly observe their practices and so we must test ideas with analyses of material culture, and yet much of ancient material culture has been lost to decay. Although our methods improve every year, today we cannot in most cases know with certainty the language people spoke, the particulars of their religion and cosmological beliefs, details of their kinship and social organization (although strontium analysis has allowed us to infer post-marital residence in cases), whether cross-cousins or parallel-cousins (or someone else) were preferred marriage partners, whether people thought of trees and stones and animals as "like persons," or all the other elements of human culture that helped structure what people did.

But I am also speaking of a great difference in scale that is the focal point of this volume. A long-term ethnographic study might be 50 years, and it might cover a country. But some archaeologists study human societies over enormous spans of time and over enormous spans of geography (Kelly 2016). Archaeology is good at seeking, analysing, and interpreting patterns in material remains over long spans of time and wide expanses of space. It is less good at consistently and systematically obtaining the minute detail that makes humanity interesting. Any effort to bring ethnological data and the enterprise of archaeology together must bear these two facts in mind and focus on archaeology's strength.

So, what do we do with the fact that archaeology cannot infer many of the elements of past human cultures that ethnographic research shows us matter, and that it operates with a different temporal scale? Answering this question requires us to think about two things: the scale of archaeological data and what investigative strategy best suits that scale.

The scale of archaeological data

Archaeological data have two essential elements: age and location. Archaeologists are compulsive about location; we try to record an object's provenience to the most precise level possible, using instruments such as an EDM (electronic distance measure) to record an artifact's location to ± 3 mm relative to a 3-dimensional grid system. But the artifacts in many sites have been moved,

vertically and horizontally, through many processes making it difficult to associate even carefully plotted individual items with one another.

Age is also problematic. AMS radiocarbon dates come with standard errors in the 15 to 30-year range. That's excellent, but it means that the age's 95% confidence interval is 60-120 years – compare that to the standard length of a long-term ethnographic study. Worse, much of the time period of interest here in the Old World lies beyond the range of radiocarbon dating (~50,000 years). Sites more than 50,000 years old are dated by other, less precise means (e.g., optically-stimulated luminescence) that might provide a confidence interval of hundreds if not thousands of years. And this means that what archaeologists might consider a tightly-dated archaeological assemblage is a potential aggregate of artifacts left behind by many individuals – men, women, children, the elderly, etc. – during possibly many different uses of a location.

The temporal scale of archaeological data, even under the best of circumstances, is obviously quite different from that of ethnographic data. We must consider this when asking, what can archaeology tell us? What about human society and culture can we infer from those artifacts that survived what Francis Bacon called "the shipwreck of time" and that come from a record that is a palimpsest of the evidence of many activities?

We can draw an analogy between archaeological data and a radio. Sometimes the radio signal comes in clear, but sometimes it is poor, and full of static. At those times, one might be able to discern that the voice is male, and speaking English, but the precise words are impossible to hear. This does not mean the words are unimportant, only that we cannot hear them. If we cannot hear the "words" of prehistory, then we can either abandon archaeology or decide to use what it can consistently provide, its "strong signal".

A research strategy

Let me be blunt: the temporal and spatial patterns uncovered by archaeology, especially of the time period of concern here, most likely reflect, at archaeology's temporal scale, the broad ecological, subsistence, and demographic conditions of life. It is these factors that provide the "strong signal" of archaeology and thus its first-order interpretations. And let me be clear: Archaeology does sometimes allow glimpses at finer scales (as with Ötzi, the Neolithic man found frozen in the Italian Alps), but the most assured things we can *systematically* infer, and that provide us with an important comparative base,

are the ones that reflect the elements of life that deal with food, security, and reproduction.

I say this because numerous cross-cultural ethnological studies show that an environment's ecology exerts a strong influence on hunter-gatherers (Kelly 2013; Binford 2001). And anyone who accepts global warming as a reality, and something we must adapt to, must also accept that people before us have had to contend with climate change.

Humans also have daily caloric needs – varying with age, gender, size, and workload – and if people cannot satisfy those needs then little else can follow because those people will be dead. Finding food is basic. (We tend to forget this since we live in a world where the fortunate among us do not have to worry where their next meal is coming from.) The environment sets potentials and limits to ways of satisfying that daily need. We could begin with the banal fact that foragers will not eat much plant food in the arctic and then move to the less banal fact that the abundance and distribution of game and plants, combined with their costs of acquisition and caloric value, will condition which foods are used (claims that can be verified or not using the plant and animal remains recovered in sites). Likewise, the abundance and distribution of sites of a given time period are first and foremost telling us something about the abundance and distribution of people.

Given where I think the first-order interpretations of archaeological data lie, I suggest that the often-disparaged optimal foraging models offer a useful research strategy to approach interpreting the archaeological record. Optimal foraging models were brought into anthropology from the field of evolutionary ecology, where they were intended to unify ecological approaches with an evolutionary perspective. These models were brought to anthropology by Bruce Winterhalder and Eric Smith, both of whom studied hunting and gathering cultures. It is probably this historical accident, rather than the reprehensible assumption that hunter-gatherers are "closer to nature", that is responsible for their common use in the study of hunter-gatherers. All humans are equally "close to nature" and equally not.

In anthropology, the approach of evolutionary ecology takes the name *human behavioral ecology* (HBE) and modifies the models to account for the particulars of humans (e.g., division of labour, central place foraging, environmental knowledge, symbolic labelling of food and activities). These models privilege material conditions, especially food and reproduction, and focus on "maximizing" behaviours (e.g., how does an organism maximize reproductive advantage under such-and-such conditions?). Many anthropologists re-

ject these models, claiming they are nothing more than sociobiology (which they link to racist views), or capitalism written into the natural world. And yet, the models have been tested against ethnographic case studies and proven to be useful in predicting human behavior (Kelly 2013).

However, there is a scale problem here as well. Foraging models were developed to model individual decision-making, moment-by-moment. Given conditions A, B, C … what food resources might we expect the individual forager on a daily foraging trip to collect from an environment? (Or it could be what resources do we expect to be shared, or what size group do we expect an individual to opt to live in, and so on.) But archaeology, as I pointed out, deals with palimpsests that include the material evidence of human behaviour over a long time span but almost never that of a single individual's daily decisions. If a diet is broadening and contracting over some interval of time, we will not see that in an aggregated dataset – all the animal and plant remains that provide evidence of diet might be combined into an assemblage that cannot be disaggregated. This only means, however, that we must evaluate the data with the recognition in mind that, in this example, we are looking at the maximal diet breadth. And it means that a significant change in diet breadth between, say, two time periods indeed reflects a significant change in human behaviour. And the longer the time period entailed in formation of archaeological assemblages, the greater the likelihood that the "strong signal" in those data will reflect inescapable realities of foraging lifeways, and the lower the likelihood that other cultural variables produce significant patterning in the data *over the reaches of time that archaeologists normally must confront*. Again, I do not mean that cultural ideas have no effect. But cultural knowledge can change rapidly, and probably did change during any archaeologically-defined Palaeolithic period (e.g., the Aurignacian). And this suggests that not all changes in, say, a people's definition of relatives or cosmological beliefs, will lead to a large-scale change in, e.g., subsistence, especially if that change correlates with something that could affect subsistence choice, such as a change in climate or population density.

I admit this approach could lead us astray. But the utility of HBE foraging models is that they provide a way to know when an idea is wrong. Take a simple example: what if some foods are tabooed, or some taken for non-food reasons (e.g., certain plants for medicinal needs)? An optimal foraging model might simply say: if foragers are behaving according to a certain set of principles grounded in ecological and evolutionary theory, then in a particular environment we expect them to take, as food, resources A through G. If

the archaeological record shows something different (resource E is not taken, and instead resource H shows up), then we have good grounds on which to argue that some factor is at work other than those incorporated into the model. HBE's foraging models provide a useful strategy because they can recognize information that qualifies or even negates the original assumption.

A useful strategy, one that can help tell us when we are wrong, is important because it is, of course, absolutely true that humans live in a culturally constructed world. We deem some foods to be edible and others to be inedible; some people are proscribed as mates, and others are prescribed. We treat the environment one way if we think that trees, animals, rivers, and rocks are ancestral spirits and another way if we think God has given us a mandate to dominate the earth. HBE's models are not ready-made answers, but they provide a research strategy that is suited to the large temporal and geographical scales of palaeolithic archaeology.

Now let me turn to the last part of the first African Frontier, the colonization of the western hemisphere, to hypothesize what the nature of that frontier might have been like.

Colonization of the New World

The western hemisphere and Australia, as well as portions of far northern Asia, were lands first occupied by modern humans. They are particularly interesting cases since they were, as far as humans were concerned, *terra incognita* in the late Pleistocene.

I will focus on the western hemisphere as that is the case I know best. The timing, route, and adaptation of this region's first inhabitants are highly contentious topics. I can only give a quick summary here since my point is to discuss scale issues as they relate to hunter-gatherer migration. I currently think the best evidence points to an entry between 14,500 and 16,000 years ago to the continental US. There are a few very early (>20,000 cal BP) sites (e.g., the Cerutti Mastodon site in California, Chiquihuite and Coxcatlan Caves in central Mexico, and the White Sands footprints in New Mexico (Ardelean et al. 2020; Bennett et al. 2021; Holen et al. 2017; Somerville et al. 2021), but these have convinced few archaeologists (Braje et al. 2017; Chatters et al. 2021; Potter et al. 2018; 2021). Better evidence comes from the ~16,000-year-old Gault/Friedkin (Texas) and Coopers Ferry (Idaho) sites (Davis et al. 2019; Waters et al. 2011; Williams et al. 2020), and the 14,500-year-old sites of Page-Ladson (Florida) and Paisley Cave (Oregon; Halligan et al. 2016; Shillito et al. 2020).

The route from Asia could have been along the western, largely ice-bound coast or through the ice-free corridor (Potter et al. 2018); I lean toward the coastal route, where the earliest sites are located (McLaren et al. 2018). Boats might have been employed, though they were likely modest forms, and not ocean-traversing vessels. Slightly later populations might have come through the ice-free corridor once plant and, especially, animal food became available there.

But let me turn to the nature of adaptation at the time of colonization. The earliest culture that we know of in North America is the *Clovis* complex. Its primary material hallmark is a large, lanceolate projectile point with basal "flutes" created by one or more flakes removed from the base on each side, accompanied by grinding of the base's edges. These appear in all 48 states of the continental US, and a few occur in Canada, Alaska, and Mexico. The tradition might continue into South America in the form of (sometimes) fluted "fish-tail" projectile points.

Clovis currently dates to 13,050 to 12,750 cal BP (Waters, Stafford, and Carlson 2020), but this range is based on fewer than a dozen dated sites, which are clustered in the Plains and the northeast. Statistical studies show that the first appearance of Clovis is earlier than it appears, perhaps as early as 14,500 years ago (Prasciunas and Surovell 2014). It's likely that Clovis appeared first in the far west (assuming the coastal migration route is correct), where it has defied efforts to date it. Thus, our dated sample is both small and geographically biased. Regardless of whether someone first set foot on the continent south of the ice sheets 14, 15, or 16,000 years ago, it appears that virtually all of the western hemisphere was occupied in a short period of time. Why?

In 1988, Lawrence Todd and I proposed one model (Kelly and Todd 1988). We pointed out that the first entrants to the New World would have been hunters, since they were coming from the arctic (I think this would have been true even if they used a coastal adaptation since arctic coastal peoples are also terrestrial mammal hunters). This means they were comfortable with game, but perhaps less so with plants. Since mammal anatomy is basically the same – mammoths are just scaled-up rabbits – the knowledge of preparing meat can be transferred across environments; this is less true of plants. While hunting benefits from local knowledge of animal behaviour and terrain, it is possible to survive by hunting in unfamiliar land: 17th through 19th century European fur trappers lived primarily off game as they moved across the North

American continent (Hudson's Bay Company policy required trappers to live off the land or eat whatever they could get in trade with Indigenous peoples).

Animals are available year-round (although they are in better condition at some times than at others), while plants are not. Plants, in addition, can have more time-consuming processing needs and some, such as acorns, are quite labour-intensive and require figuring out how to use them (e.g., acorns are full of tannic acid, and require pounding and leaching to remove it). In fact, plants were probably not an important part of diet as the tools for their processing, such as grinding stones, show up a few thousand years after Clovis. The few traces of plant foods recovered from Clovis-age sites are mostly snack foods, such as berries (this is likely not a function of preservation; Kitchel and Mackie 2022).

So we proposed that arctic hunters, after entering North America south of the ice sheets would have continued their arctic adaptation and focused on hunting. This would have been a viable adaptation in the terminal Pleistocene when North America contained a variety of large game (which soon became extinct, possibly due to human predation but we will leave aside that contentious issue). We have solid evidence that Clovis hunters took mammoths, mastodons, and bison; and indirect evidence they took horses, camels, and sloths.

Clovis hunters would have found themselves in a world of naïve game, animals who had never experienced human hunters before. There are not many parallel cases, but there are some in which human hunters (or wolves reintroduced to Yellowstone National Park) experienced naïve game (Kelly and Prasciunas 2007). In these cases, the kill rate is very high. But the animals respond and within a few years learn to avoid the new threat. In a world where there are no other humans beyond the colonization front, hunters would know that they could do better, i.e., achieve a higher return rate, if they simply moved to new territory.

The catch-22 is that those hunters did not learn about their current environment's unique properties, including the location, seasonal timing, and processing needs of plant foods. Hunting allowed them to move into new environments, but the high return rates of naïve, non-depleted herd animals coupled with the availability of land devoid of humans and populated by naïve game also encouraged Clovis people to keep moving into new environments across North America. The implication, of course, is that the same cultural group carried Clovis technology across the continent.

Foraging models support this reconstruction. Without going into detail (see Kelly 2013), a base model predicts that hunter-gatherers will move before exploiting all foods that are economically obtainable from a settlement. For example, if one could forage at an economic gain up to 6 km, then the camp might move after using food within 3 km of camp (assuming Binford's [1980] "half-radius" foraging area; this follows what is known as the "marginal value theorem", which has been ethnographically demonstrated [Venkataraman et al. 2017]). However, a model in which the return rate declined as a function of occupation of a camp, e.g., under conditions where animals respond to hunters and make themselves more time-consuming to hunt, encouraged movement at an even shorter distance. In other words, we expect a colonizing population dependent on hunting to move quickly across a landscape populated by naïve prey.

The theoretical model focused on daily behavior. We "upscaled" the model to inform us about movements of territory, something that happened perhaps every few years; that is, we treated a territory as if it were a camp. Is this a proper thing to do? That's where archaeology provides a test. And unfortunately, no one has yet made that test as it requires copious dates on sites across the continent. However, an alternative is that migration was driven solely by population growth, with daughter groups moving away when local carrying capacity was reached. Modelling suggests this would require a population growth rate far above that recorded for prehistoric hunter-gatherers, which hover around 0.04%, not the higher rates observed among ethnographically-known hunter-gatherers (Zahid et al. 2016; Prasciunas and Surovell 2014). Consequently, the movement was probably not driven by population growth alone.

The geographic scale of social relations

Let me return to another question of concern, namely, the scale of social relations because our reconstruction implies widely scattered Clovis residential groups. Ethnographic data show that nomadic hunter-gatherers live most of the year in groups of about 25 people, with short-term seasonal gatherings of larger groups. The figure, 25, seems to be true almost regardless of the environment (Hamilton et al. 2007). Bruce Winterhalder (1986) showed that it appears to balance depletion of the local foraging area with the need to have enough hunters in a group so that someone is successful (at large game hunting) at a rate that will feed the group (see Kelly 2013). But 25 people is too

small to maintain reproductive viability. Under conditions of extremely low population density, which was the case for the population that colonized the New World, a residential group had to stay in touch with enough other widely spaced residential groups to avoid extinction. Is there any evidence of these larger groups during the colonization of the North America?

This is a tough question. For later time periods, archaeologists conduct social network analysis with ceramics as the basic data. For the Clovis time period, we mostly have stone tools; network analysis can be applied to the lithic raw materials from which those tools were fashioned. Doing so with Clovis assemblages, Buchanan et al. (2015) found three major networks: one in the northeast that stretches from west of the Great Lakes, south to Missouri, over to South Carolina and north to Maine. A second covers Texas and the southern and central Plains, and a third covers the northern Rocky Mountains out to the western north plains. These regions receive some support by concomitant regional differences in Clovis projectile point form as well (Buchanan et al. 2014).

I do not mean to imply that these (large) regions were ones in which every individual interacted with every other person. They might, however, be regions where individuals were more socially connected, in a spider-web way, than they were between regions; thus, they tell us something about the geographic scale of social connections during colonization. And the message here is that these groups of 25 or so foragers were each embedded in a geographically large social network, as Bird et al. (2019) point out for modern Aboriginal Australians. But let's not trade one stereotype for another: it is clear that as population grew in North America, and economic organization shifted, in many places from hunting and gathering to agriculture, that the geographic extent of groups – in other words, their scale – also changed. The geographic scale and social content of groups are part of the adaptive process. We should not expect them to be always the same everywhere.

Men and women

Even with a gross temporal and spatial scale, can archaeology inform us about such things as the division of labor? Ethnographic data support a division of labour, regardless of environment, in which men hunt and women gather. This is most likely due to the need to breastfeed children – sometimes for several years – and consequently for mothers to keep children with them.

Since childcare is more compatible with plant gathering than hunting, women gather (see discussion in Kelly 2013).

However, it's possible that a hunting-focused adaptation south of the ice sheets produced different relations between men and women. Haas et al. (2020) argue that the number of hunters in early western hemisphere foraging groups might have consisted of nearly equal numbers of men and women. The analysis is difficult as it relies on a very small sample of human burials, as well as on the interpretation of grave goods (which is a fraught exercise, given the highly symbolic nature of burial ritual). Nonetheless, using the presence of hunting equipment as a guide to what people did in life, Haas suggested that 30-50% of women were likely to have been hunters.

Archaeologists have long denigrated a reconstruction of Clovis life that portrayed men out hunting mammoths while women sat at camp, breastfeeding children. Women were no doubt doing many other things. In fact, using ethnographic data, Waguespack (2005) found that women in foraging societies do an increasingly larger percentage of tasks other than direct food acquisition as the percentage of meat in the diet increases. These tasks include childcare, but also firewood collection, and especially clothing manufacture. But the ethnographic dataset of meat-dependent groups is biased toward arctic peoples, i.e., groups that have significant clothing requirements. What would women do in a hunting society of say, the southeast US, with much lower clothing requirements? One guess is that they could be incorporated into the hunt, and shift a group's tactics from individual stalking, or sit-and-wait hunting, to communal hunting, which might have increased the per capita return rate. In these cases, women (childless, past reproductive age, or able to leave an infant in camp in the care of another) could have been armed with atlatls and used to drive and even kill game. We do not know the answer yet. But I would not expect a hunting-dependent culture outside the arctic to look exactly like arctic cultures.

The First African Frontier

The first African frontier in and outside of Africa would have been similar and different to the North American case. Those intrepid hunter-gatherers who ventured out of Africa would not have been coming from the arctic, quite the opposite in fact, and so they may have been less focused on game. Those foragers were also probably not entering land with naïve fauna since Europe and Asia were already occupied by Neanderthals and Denisovans. There is also

no grand barrier to movement like the ice sheets in North America that might have made "rearward" movements more difficult.

On the other hand, evidence for intensive plant food processing also appears late in the Old World, and all optimal foraging models place game animals as the top-ranked foods (balancing search and harvest/processing costs with the calories acquired). Those moving out of Africa were probably hunting-focused, though maybe not to the extent of Clovis hunters. This suggests that the migratory wave out of Africa was driven more by population growth and subsequent (slow) territorial depletion, than by the attraction of higher hunting return rates in an unpopulated region with naïve fauna. This is a process that could be modelled using, e.g., the approach recently employed by Klein et al. (2021). Combined with an environmentally informed "ideal-free distribution" foraging model, we could also predict which regions would be occupied first, second, and third. This does not mean the model is right, but, tested against archaeological data, it provides us with knowledge of when we are wrong, when factors other than the simple ones entailed in foraging models are not driving the pattern as revealed by archaeology's "strong signal".

Conclusion

Scale is a fact of social life; it is also a fact of research, driven largely by the nature of our data. The large-scale patterns revealed by archaeology, its "strong signal", are most likely revealing issues of ecology, human subsistence, and reproduction. The patterns we observe in the archaeology associated with the movement of palaeolithic hunter-gatherers across a continent is most likely to be explained by those factors communicated in archaeology's strong signal. This does not mean that other factors were not at work, only that they are difficult to discern at the scale palaeolithic archaeology can record information. Nonetheless, the interpretation of large-scale patterns uncovered by archaeology are complemented and potentially tested by small-scale studies at those archaeological localities amenable to research that retrieves fine-grained information, something more than just the "strong signal". They can also be hypothesized through agent-based models incorporating social variables (see Widlok and Henn, this volume). I think HBE is a useful research strategy because it is suited to large-scale archaeological data and, by making archaeologically testable propositions, provides a way to know when it is leading us astray. There is not much more we could ask for in a research strategy.

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Comment by Graeme Warren

Small town England in the early 1980s had an overriding aroma of cheese and onion crisps, damp concrete, and failure. For the young teenage me, the bold, brash American Football that was being shown on a new once-weekly TV show on Channel Four was a bright and shiny escape from this grey world. I became mildly obsessed. Aside from Channel Four, American Football was barely covered in the UK media, but, I had one other form of access, an unexpected benefit of living near to the Cold War era American cruise-missile base at Greenham Common. On a Sunday evening I could, just about, pick up the radio broadcasts of the American Armed Forces Network from the base. Live commentary on games! The reception was very poor, fading in and out. The presenters used unfamiliar technical terminology, and many cultural references sailed over my head. But with persistence, some inference and lots of learning, I could listen to the radio broadcasts and keep track of the games. Making sense of these exotic, uneven and inconsistent broadcasts was even enjoyable.

I tell this story, of course, because of the analogy made to the radio in Robert Kelly's paper 'What Good is Archaeology?'. This is a stimulating short

paper, and there is much that could be discussed about many aspects of it. But in keeping with the aims of this volume, I will focus my response on scale.

Kelly argues that whilst the radio signal of prehistory is sometimes clear, it is more frequently 'poor and full of static'. Because of this, we should develop strategies to use this static as our archaeological 'strong signal'. At times, Kelly argues that we might be able to discern a male, English speaking voice on the radio. But our focus should not be these moments because they don't offer any *systematic* data. Instead, we should work out how to engage with the static. What a curious way of listening to the radio!

As I argue in my paper in this volume, archaeology is characterised by mulit-scalar temporal data. Sometimes we have 'static' – to stick with the radio analogy. But sometimes we can make out the gender of the speaker, and the language they are communicating in. This is non-trivial. It matters if the speaker is communicating in English, French, Somalian, Mandarin or a Khoisan language. It matters if they are male, female or neither. Kelly and I are both male and we speak English. But if you listen to our voices you would also identify further differences between us. We should not dismiss these kind of insights.

Archaeological data also involves moments of sharper chronological resolution – moments where, perhaps, when we can hear the words of the broadcasts. In this regard, an array of new analytical techniques are enhancing our ability to provide details, and refining the resolution available to us. Bayesian modelling of radiocarbon dates, for example, means that rather than working on a 60-120 year resolution as claimed by Kelly, we can sometimes approximate to generational time frames. This puts us within a similar time scale to some long-term ethnographic studies.

Kelly suggests that archaeologists can't identify 'details of kinship' and preferred marriage partners. But this is not true. Kinship is increasingly something we do discuss. Recent genomic analysis of individuals buried in the Early Neoltihic Hazelton North Chambered tomb in southern England (Fowler et al. 2021), for example, shows that patrilineal descent appears to have been a key determinant of inclusion in the tomb, but location with the chambers was determined by female descent. Step-sons appear to have been incorporated into the lineage.

These finer grained aspects of our data matter. They matter because such details offer points of engagement with the humanity of the past. But they also matter because of how the archaeological record is formed. Numerous studies have demonstrated that hunter-gatherer groups create, use and deposit

material culture according to specific ways of understanding the world: Peter Jordan, for example, has highlighted that the deposition of animal remains by the Khanty is spatially complex and bound up with negotiations between hunters and animal spirits (2003). We need detail to reconstruct these cultural practices and to understand how our sites are formed.

In this context, an archaeological retreat to the largest of scales and the claim that the abundance and distribution of sites (or animal or plant remains) is 'first and foremost telling us something about the abundance and distribution of people' is missing a key step. The abundance and distribtion of sites is, first and foremost, telling us something about the formation of the archaeological record. This arises from an interplay of activities in the past and in the present which create the material evidence we work with. Making sense of this record requires that we are sensitive to the multiple scales of our data: the smaller scale is our only hope of understanding how practices in the past influenced site formation, which is the basis of the evidence which we can use at larger scales.

The movement to the large scale in archaeology has been fed by many factors. In part, this is a reflection of the nature of (some of) our data, but the increasing availability of big data, the processing power to deal with it, and broader trends in academic publishing are also part of this trend. We should be careful what we lose in pursuing it (Cunningham & MacEacharan 2016). The large scale and the long term isn't the only thing that 'Archaeology is Good For'. Choosing to listen to the static and not the other parts of the archaeological 'radio signal' feels like a counsel of despair. Kelly argues that archaeology is "less good at consistently and systematically obtaining the minute detail that makes humanity interesting". That is not a description of archaeology that I recognise. It does not match my experience of working with students and volunteers and seeing their reaction to the recovery of a lithic or pot sherd. The excitement at a moment of connection with the past is a moment of attenuated interest in the character of a material, and the lives it was once bound with. Archaeology is much more than just static. And uneven quality radio signals can still be listened to. How else could I keep track of the scores?

References

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Comment by Brian Codding

In this provocative essay, Kelly asks "what good is archaeology?" To be sure, as a collection of methods applied to the study human behavior, archaeology is limited. Specifically, Kelly notes that "archaeology cannot infer many of the abstract elements of past human cultures that ethnographic research shows us matter…" Given this, is it "possible to get anything of value from archaeology at all?"

Central here is a recognition of what we can, and cannot, "see" with archaeological data. Archaeology cannot "see" the scale of past social organization with the precision of an ethnographer, but it is equally sure that there are aspects of social organization in view. Acknowledging this limitation is a first step in identifying what good is archaeology.

This is in some ways reminiscent of Christopher Hawkes' 1954 essay on "Archaeological Theory and Method: Some Suggestions from the Old World" featured in American Anthropologist (Hawkes 1954), in which he outlines his (in)famous "ladder of inference". The rungs on the metaphorical ladder move from the observed unit of study, material remains, upward to those aspects of human society most removed from material objects, such as religious beliefs. With each rung, the archaeologist makes an inferential leap, creating less and less certainty about the claims being made. With this ladder in mind, we too can focus on what archaeology can do well, and what it should perhaps leave to the ethnographers.

However, moving beyond identifying limitations, Kelly highlights how we can bolster our inferences as we climb Hawkes' rungs by leveraging theory, specifically, theory from ecology. As Kelly notes, "ecology must exert a strong influence on hunter-gatherers" (and on post-industrial society as well, as is clear from global climate change, and the current pandemic). Using this fact and theory designed to amplify it, Kelly argues that we can help resolve some of the issues with archaeological data.

Leveraging these tools, Kelly examines how high we can reliably climb on Hawkes' ladder to understand social patterning among early colonizers of the Americas, even from course grained archaeological data. For examples, he reviews how the general approach has elucidated the geographical scale of past social interaction spheres, and the relative dietary contributions of women and men.

This reflexive turn rewards the reader with insights about how we can best examine the scales of hunter-gatherer social organization in the past, inferred

from the material remains they left behind. In so doing, Kelly illustrates the good archaeology can provide.

References

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