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Response

Kim Sterelny & Trevor Watkins

We would like to begin by thanking the commentators for their thoughtful, informed and, for the most part, generous responses. Even Ofer Bar-Yosef, while clearly disagreeing deeply with our whole approach, has invested precious resources of time and thought in our paper. We shall respond commentary by commentary, but there are some common themes in the responses, and we will note those as we go through.

Bar-Yosef

It is obvious that we have a different view of the Neolithic transition than that of Bar-Yosef. That said, our views are not quite so opposed as he imagines.

First: of course we do not suppose that that we know that 'all past societies were "mobile bands of fluid membership"'. For one thing, there is considerable complexity and variation in the social organization of forager groups. In particular, Binford famously distinguished between 'residential' and 'logistic' mobility. Logistic mobility is a form of social organization in which a base camp is moved rarely, but specialized work groups pursue targeted resources, often over considerable spans of space and time (Binford 1980). We suspect logistic mobility was a response to the expanding range of resources that foragers exploit; for when foragers exploit many resources, these attenuate at different rates and spike seasonally at different places. Logistic mobility minimizes the costs of exploiting different resources, while also lowering the movement costs of mothers with young children. For them, the less often base camp moves, the better. If logistic mobility were a response to a broadening resource portfolio, it would gradually have become more important as a form of social organization over the last hundred millennia. Moreover, some forager societies were sedentary. The classic examples are the so-called 'complex forager societies' of the Pacific Northwest. But we argue that Near Eastern farming emerged out of foraging (harvesting) for storage, and that form of foraging requires at least a partially sedentary lifestyle. That said, sedentary foraging does require particular and unusual ecological conditions: a rich, predictable, geographically concentrated flow of resources. Since such resource flows are rare, while we recognize variation in forager lives, and the many uncertainties in our knowledge of the past, we stand by the claim that most forager lifestyles depend on some form of mobility. So we and Bar-Yosef see the forager record differently, but the gap is not as wide as he supposes.

The same is true of our views of social learning and teaching. We do not think social learning and teaching is a feature only of relatively recent human social worlds. To the contrary: one of us has written a monograph defending the idea that the evolution of social learning and teaching is deep and important, shaping hominin evolution over the last three million years (Sterelny 2012). However, we do think that the demands on these mechanisms have increased over the last hundred millennia, as the resource envelope expanded and material culture became more complex. That is especially true if social life became more complex too, with rituals and norms becoming more extensive and important. Material symbols are an undeniable feature of the archaeological record only over the last 120,000 years (perhaps less), and this fact strongly suggests an increase in social complexity (Henshilwood & d'Errico 2011). We think this has been an accelerating trend: the informational load on human agency, and hence on the mechanisms of learning and teaching, has increased, increasingly fast, though obviously with much local variation.

Bar-Yosef chastises us for not using the names of the 'industries' and 'cultures' that are traditional for the south Levantine culture sequence, because, he says, we thus deny identities to the prehistoric 'people with no name'. One of us has argued the opposite of this view (Watkins 2008), and we did not rehearse that argument here. We think that he has failed to recognize that we have throughout referred to those people as 'communities' (rather than as archaeological 'sites' or 'settlements'), who engaged in intensive networks of sharing and exchange (rather than as archaeological cultures, in the terms inherited from Gordon Childe and Kathleen Kenyon).

We do disagree about group size and its importance. At what group size does social stress become important? If we trust the results of evolutionary models of cooperation, to the extent that cooperation is based on reciprocity and investment in reputation, cooperation is difficult to sustain in groups of larger than 25 or so (these models are summarized and discussed by Bowles & Gintis 2011). We think those models are too pessimistic, underestimating the ease with which accurate information flows through groups and underestimating the importance of partner choice in stabilizing cooperation. Hence we think that Dunbar's estimate of around 150 might be a better ballpark estimate. Note, though, that this is not Dunbar's estimate of the upper limit of group size, or his estimate of the upper bound at which individuals know by name every member of the group (see his various papers in Dunbar *et al.* 2010; 2014). Rather, it is the upper bound at which cohesion can be maintained by intimate mutual knowledge. Of course, larger groups are possible; they are actual, and Dunbar and his colleagues have done interesting work on how these higher levels of integration are managed (e.g., Hill *et al.* 2008; Read 2010; Stiller & Dunbar 2007). But are they cohesive by virtue of intimate mutual knowledge alone? Given the role of ideology in the *kibbutzim* movement, we doubt that these are counter-examples to Dunbar's estimate. So we stand by the claim that, as group size increases, so too do potential stressors. Unless new social technologies are developed to manage tensions, those groups are apt to fragment.

Finally, we are somewhat confused by Bar-Yosef's view of niche construction. He is clearly very sceptical. But at times the scepticism seems to be that this is old and obvious news, already incorporated into archaeological thinking (as 'anthropogenic affects on the environment'); cultural niche construction, however, is much more than this. At times the scepticism seems to be that there are not enough well-documented cases of specific genetic changes in response to specific niche construction effects. It is indeed true that there are few cases as clean and well documented as lactose tolerance. But that is because simple relations between genotype and phenotype are the exception, rather than the rule, and because the genetic bases of most human phenotypic traits remain unknown. Does Bar-Yosef really think that there were no genetic changes in the hominin lineage as a result of the invention of fire and cooking; the emergence of language; the expansion of toolmaking and tool use; the changes in human diets over the last 100,000 years? Cultural niche construction theory is more than a list of specific examples of gene–culture co-evolution; it may be obvious, but it is surely important and pervasive. In our paper we have tried to draw attention to another kind of co-evolutionary feedback loop within a cultural niche construction framework: the recognition of the potential to construct a cultural niche that powerfully interacts with human cognition.

Zeder

We do not have any serious disagreement with the perspective that Melinda Zeder articulates in her commentary. In our response, we aim to make explicit both overall agreement, but also the differences in detail and emphasis. First, we agree that the emergence of the Neolithic in southwest Asia depended on large-scale climatic changes. We did not emphasize this in our paper, because we believe that there is a broad consensus that the more equitable and stable climate of the Holocene was a precondition of dependence on domesticates (see, for example, Richerson & Boyd 2013; Richerson *et al.* 2001). But it is clearly not sufficient: elsewhere in the world, dependence on domesticates emerged deep into the Holocene. So these climatic and environmental changes were an enabling background condition. We also agree that local environmental conditions, the 'mosaic of resource rich environments', also played a crucial enabling role; for in our view, storage foraging—which does indeed depend on local, seasonal, resource richness—was the midwife of the Neolithic transition. Storage foraging built the technical, economic and social preconditions for the gradual increase in dependence on domesticates, and storage foraging itself depends on the character of the local environment. Perhaps in contrast to Zeder, we also think that it is very likely that a shift to storage foraging would fuel demographic expansion. It eases constraints on birth rates faced by mobile foragers: constraints imposed by the costs of moving dependent, immobile toddlers; and by the low seasonal resource bottleneck. But it also changes the costs and benefits of larger families. As Becker notes in his classic analysis of the European demographic transition, from a quite young age children are a valuable source of labour in subsistence economies, in virtue of the low-skill character of much farm labour (Becker 1960; Shenk *et al.* 2010). Foragers' children are more expensive than farmers' children.

Third, and perhaps most importantly, she and we view niche construction as a framework for exploring the interaction of environmental, social and cognitive factors. We see that interaction as crucial in understanding not just the Neolithic transition, but hominin evolution in general. That said, there are differences in emphasis and detail. For example, she has a more opportunity driven, less demographic pressure driven, picture of increasing investment in ecosystem engineering; in enhancing local productivity. We accept, of course, that the 'the explosive increase in the size and density of communities, the privatization of access to resources, the specialization of economic and social roles, together with dependence on domesticated resources, emerged incrementally and over a significant time period'. That said, we suggest that some elements of this matrix became important early, perhaps earlier than Zeder would place them. In particular, she does doubt that settlement size stressed social mechanisms early in this process. But even if earliest Neolithic community size never exceeded a 150–200-person threshold (and we deliberately did not commit ourselves to estimating settlement size), we have argued that settled communities face new problems of conflict management, if only because they lose an important forager mechanism, that of costlessly shifting away from those who annoy you. Robin Dunbar has also pushed this point hard (Dunbar *et al.* 2014). New collectively produced and maintained infrastructure and enhanced collective food storage practices presented collective action and resource division challenges that would further aggravate the problems of conflict management.

Finally, we think we need to clarify the strategy of our paper. It focused on the informational and conflict-management challenges of the Neolithic transition, rather than on the environmental and economic context. But that is not because we think those material factors are less important, or that the transition itself is explained by ideology, explained by new ways of thinking and organizing social life. Bogaard seems to read us as over-estimating the role of these cognitive factors, too. In his commentary, Stephen Shennan mentions that both Hodder and Cauvin urged an ideology-driven model of neolithization, and recognizes that our view contrasts with theirs. He is right; indeed, one of us has developed an (informal) individual economic choice model of the transition from foraging to farming in the Near East (Sterelny 2015). Rational economic agent models—models that see agents as making optimal decisions, given the options they face—rest on three sets of assumptions. One set is about the environmental context. Another is about the patterns of choice made by other agents. Interaction is strategic: the payoff to one choice depends on the choices others make. A third set are assumptions about agents' access to, and ability to act on and evaluate, information about other agents and the environment. These cognitive and social assumptions are often not made explicit in models of rational economic choice. But they are essential to these explanations of agents' action. We have focused on these cognitive and social factors in the target paper, not because they are more important, but because they are under-explored.

Bogaard

We begin our response to Bogaard's thoughtful commentary by echoing one aspect of our response to Zeder. It is true that our target article emphasized cognitive factors in the Neolithic transition, but that is because we think those factors have been somewhat neglected; not because we think ecological and economic factors are unimportant. We agree with both Bogaard and Zeder that the transition to farming presupposed storage: a more sedentary life replaced mobile foraging before the extensive use of managed resources. Our aim was to integrate these into a single qualitative model. In explaining the Neolithic transitions, it is important to explain agents' access to social and informational resources, not just their access to material resources.

So as with Zeder and Shennan, we see ourselves as disagreeing with Bogaard only on matters of detail and emphasis. One of those concerns the pace of the transition. Bogaard suggests that the early use of managed resources imposed less motivational stress than we suppose, because cultivation was less intense. The earlier phases of cultivation demanded less in preparation and cultivation. In her view, throughout this whole period, the use of domesticates was part of a mixed subsistence strategy. So our picture of early farming as a 'horrible way of making a living', posing all manner of temptations to offload the job onto others, is overstated.

Bogaard may be right in thinking that dependence on domesticates developed more gradually than we supposed. Let's suppose she is right. Even so: she herself recognizes a late pre-pottery Neolithic tipping point 'at which cultivators began to invest more labour in farming and to claim private ownership of the resulting produce'. That tipping point signals the social stresses of proximity, and collective action combined with private ownership around which our paper is organized. We suggest that the establishment of recognized private ownership in land and its products was likely to be the result of a long and difficult social negotiation, for the sharing and egalitarian norms that we take to be typical of forager lifeways would not have disappeared instantly. But they would have been stressed in larger, more permanently co-residential groups. This social change would have been especially difficult if land, this most critical resource, was privatized, just as communal collective investment in the built environment increased. We take it that Bogaard would accept this point, as she links the monumental structures of Göbekli Tepe to these stresses: to the 'transitional phase in which sedentary foragers faced the stark trade-off between communal and private investment'. So while there may be some disagreement about the timing of the cognitive and social stresses imposed by the Neolithic transition, and about their exact nature, there is no disagreement about their existence and importance.

Moreover, even if Bogaard is right in thinking that when managed resources were collectively stored (earlier in this transition) those resources were not critical (because they were part of a mixed subsistence strategy), communal storage still poses a trust problem. Agents will still want their share, and suspect others of taking too much. Boehm (2012) reports extensive ethnographic evidence of minor but persistent forager squabbling over food. Others report quite complex norms to manage these stress points (Alvard & Nolin 2002; Gurven 2004). Conflict is possible even over resources that are not critical to survival. Finally, we stand by our views of the intrinsic nature of early farming work. Even if farming was for many millennia part of a mixed strategy, someone has to grub out the weeds, clear the rocks, turn the soil, plant the seed. The fact that others have better jobs—they get to fish and hunt—makes that more likely to be a source of social stress, not less likely.

Shennan

As we read him, Shennan agrees with the approach proposed in our paper, but thinks that a niche construction approach that recognizes the causal importance to the Neolithic transition of the interaction between many factors is in danger of degenerating into an ecumenical holism; to a view that is too complex, and too under-specified, to make testable contact with either data or formal models. Somewhat surprisingly, Bar-Yosef seems to read us the opposite way, taking us to defend a linear, single-factor causal model of the Neolithic transition. Shennan reads us right: we do defend a view of this transition that depends on positive feedback loops among environmental, cognitive and social factors, and that makes Shennan's concern that the picture is too complex to be testable legitimate. But we do not see our scenario as intractably complex. The perspective we develop would clearly be sharpened and made more testable if conjoined to models that (for example) explored the risks involved when a community becomes dependent on the storage of just a few resources. How reliable must storage be? How stable must the year-by-year pattern be, for this to be viable for a single community, not part of a regional network? How vulnerable is storage-dependence to community breakup and the loss of access to crucial resources? Appropriately constructed models could give us much better insight into the risks and stresses communities faced in the early stages of this transition. Likewise, our perspective would be much sharpened with quantitative estimates of the investment in utilitarian and ideological infrastructure at Near Eastern sites. We show in the target article that this investment at Göbekli Tepe is very considerable, but we do not have any estimate for the man-years (say) a cycle of construction on a single, circular enclosure, or of its infilling, represents. The detailed analysis and dating of the construction history of the site awaits the completion of the large shelters that are essential for the protection of the enclosures if they are to be fully exposed. The first exploratory examination of Enclosure C identifies a programme of construction, modification, extension and reconstruction, prior to the comprehensive back-filling (Piesker 2014). So, though our paper is not formal, we see formal methods as directly relevant to the causal drivers we have identified; because these methods are indeed relevant, we do not think we have slipped into empirically empty storytelling.

That said, we do have reservations about the particular class of models with which Shennan chooses to illustrate his points. We agree of course that investment in farming—and especially longer-term investment, in clearing stones and weeds, rotating crops and improving soils—is rational only when those investing can reasonably expect to harvest the benefits of their investments (see also Sterelny 2015). We agree that some form of recognized property right is the most likely basis of that security. But we remain unconvinced that the co-evolution of farming and property rights is linked to intergroup competition and group selection in the ways Choi and Bowles (2007) suggest. There does not seem to be any signature of pervasive group-on-group competition and conflict in the archaeological record (see, for example, Bar-Yosef 2010; Ferguson 2013): we do not see investment in fortifications, the strategic location of settlements for defence, rather than for access to resources, the production of specialist weapons technologies, or the physical markers of lethal wounds on human skeletons (signs as listed by Knüsel & Smith 2015). It would be natural to read the monumental structures of Göbekli Tepe as costly signals, credibly warning potential aggressors of the power and cohesiveness of the Göbekli Tepe communities. But there is no archaeological signal of a competitive environment that would warrant that level of investment; indeed, this absence appears to be an anomaly that one of us (TW) is now investigating.

Moreover, models are rarely direct tests of historical hypotheses; models purchase formal tractability at the price of abstraction and idealization (the classic statement of the tractability–realism trade-off is Levins 1966). Models play a crucial role: they identify causal factors that are likely to be critical. Our own paper depends on models of this kind: we rely on models of the evolution of cooperation that identify the critical importance of group size, and of accurate, inexpensive information about the social profile of other agents in the group (see, for example, Binmore 2006; 2010). Models tell us what to look for: they identify factors likely to be crucial. As we are confident that Shennan would agree, testing historical scenarios requires integrating information from models with ethnographic and archaeological information.

Comparative data are also important, and that takes us to a final point, for we are also sympathetic to Shennan's concern about particularism. Our paper is focused on southwest Asia. But if we are right, the shift to a larger social environment, a sedentary social organization, and one becoming based on agriculture, intrinsically generates informational and motivational challenges, ones that threaten the local social contract. This aspect of our model should apply to any Neolithic-like transition, anywhere in the world. So comparative data from other transitions are relevant in testing our hypothesis. The response to these challenges might vary from case to case after all, collapse in the face of these challenges is clearly one possibility—but, if we are right, we should see some signal of challenge and response. We are hostage to comparison.

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