Commentary on Andersson and Read's "The evolution of cultural complexity: not by the treadmill alone"

they enjoyed in a cold, wet climate: their dentition was considered near perfect and their skins unblemished (except by outbreaks of disease plausibly attributed to European contact); insulated by red ochre skin applications, a uniquely efficient fire technology, and a well-chosen diet, they avoided damp clothes and rheumatism and slept happily naked under the stars.

Henrich's publication was heavily biased, not only in its failure to cite the principal sources on Tasmanian ethnography (such as Ling Roth et al. 1899), but also in its privileging of the polarized archaeological views of Rhys Jones (e.g., Jones 1977). Jones had already been heavily criticized in his interpretations by Richard Cosgrove and coworkers, in work also ignored by Henrich (e.g., Cosgrove 1999; Holdaway and Cosgrove 1997; see now Cosgrove 2014; and for the broader background of diachronic cultural complexity and adaptation in Australasia, see Hiscock 2008). Jones had followed Tylor in seeing Tasmanian aborigines as "representative of the Paleolithic age" (Cosgrove 1999:359), but the fact that material cultural complexity in these small-scale communities had been further reduced after Tasmania became geographically isolated was seen as a proof of the "treadmill model."

The counter idea, that Tasmanians at contact were legatees of a material culture which they themselves had consciously refined in the most literal sense, choosing a path of expedience over a more costly and risky increase in entailment, is something which (having argued it myself) I am obviously glad to see positively commented on (Taylor 2010). My alternative account was part of a broader argument that rejected the "dual inheritance" theory of hominin evolution out of hand, and I have since sketched more of the background to that rejection (Taylor 2012). Central here is the idea of essentialist "units" of culture—such as "making fire"—that the next generation passively imitates.

Tasmanians, while they could probably make fire ab initio, Polynesian style (Backhouse Walker 1900:69–70), typically made it from a curated flame—a fire log or fire stick that the group always carried (with no danger to clothing, as they were naked). When that went out, they either found more (forest fires, for example) or, especially in the wet season, negotiated for it (not giving fire as a gift, even among actively feuding groups, was a universal taboo, and fire was the first gift they gave to the French; see Taylor 2010:143 with references). The "fire-making skill" was thus not a single indivisible transferrable entity, although the preferred mode (curation) was the one that produced fire most reliably and quickly. Similar intentional considerations can be argued to underpin all the other so-called deficits in Tasmanian culture, and whether such arguments are accepted as valid or not, they nevertheless demonstrate that our data are underdetermined when not downright imponderable. Such ambiguity is, as Andersson and Read demonstrate, fatal for the strong, law-like version of the "treadmill model."

When V. Gordon Childe wrote that "the environments to which societies are adjusted are worlds of ideas that differ not only in extent and content, but also in structure" (Childe 1949:22), he based his judgement on knowledge of a very broad range of cases. These included those where large-scale, complex societies had effectively put the brakes on technological development primarily for ideological reasons. The centralized control of bronze production in the Near East, he argued, had stifled progress, while the tribal societies of the central European Bronze Age raced ahead, producing an astounding range of complex metallurgical and metallic innovations. States tended toward theocracy and were prone to stagnation in the realm of material innovation in a way that small-scale and flexible social formations were not. On the other hand, states explicitly prided themselves on producing the material conditions believed to please their gods; that is, their elite members viewed themselves as "skilled" in this connection. In such a hall of mirrors, attempting to account for differences in cultural complexity using a neutral algebra is, at best, limiting.

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24 VIII 15

Andersson and Read's article is a welcome antidote to the uncritical enthusiasm for the models of Henrich and of Powell and colleagues. Since I am in strong agreement with pretty much everything Andersson and Read write, I will just try to strengthen their case here and offer three additional arguments against the treadmill model.

The first relates to the ethnographic data. Andersson and Read are entirely right that the Oswalt, Inuit, and Oceanic Island data sets do not (Oswalt and Inuit) or only weakly (Oceanic Island) support a relationship between population size and complexity of hunter-gatherer tool kits. But even if a (significant) relationship would be found, that would by no means imply population size to be a driver of cultural complexity. To confirm such a causal relationship, one should (minimally) observe an association between demographic and cultural change—an association which, by definition, cannot be inferred from data sets such as those just mentioned, which contain population and complexity numbers taken at only one point in time—for a correlation between absolute numbers is perfectly consistent with complexity being driven by nondemographic factors (e.g., social or cognitive innovations and adaptivity to environmental conditions), with population size acting merely as a passive constraint.

As to the second addition, Andersson and Read critically review the evidence regarding the Tasmanian case. Although that evidence is of the right kind (i.e., it concerns a possible association between demographic and cultural change), An-

dersson and Read convincingly argue that the archaeological data do not license us to construe the case as one of cultural decline. But it is just as doubtful that demographic change was involved. While Henrich assumes that, before the sea level rise of the Holocene, the foragers inhabiting what is now Tasmania formed a pool of interacting social learners with groups from what is now Bass Strait and mainland Australia, there is no archaeological evidence to suggest any social connections between the regions that would become the mainland and Tasmania. For example, no exotic Tasmanian artifact raw materials, such as Darwin glass, brecciated chert, or blue chert of Late Pleistocene age, have ever been found in Victorian mainland sites dated to the same period (Cosgrove 2015; Cosgrove et al. 2014; Hewitt and Allen 2010; K. Vaesen, M. Collard, W. Roebroeks, and C. Cosgrove, unpublished manuscript, 2015).

A third challenge comes from evidence concerning the Upper Paleolithic transition (as targeted by Powell and colleagues). One may reasonably wonder whether the remarkable cultural developments of the Late Pleistocene really correspond to an increase in cultural complexity—or more specifically, following Powell and colleagues, to an increase in transmission inaccuracy. Yet, even if this were so, a direct association with demography seems ill-supported. Confronting population estimates (adopted by Powell, Shennan, and Thomas [2009, 2010] from Atkinson, Gray, and Drummond [2008]) with estimated dates for the arrival of fully modern human behavior (FMH) in various parts of the world (estimates by Powell, Shennan, and Thomas [2009, 2010]) yields quite a few nontrivial anomalies. Concerning Sub-Saharan Africa, populations grow steadily from ~160 kya onward, yet FMH appears only around 90–75 kya, and FMH disappears, despite population growth, between 75 and 40 kya. Population growth in North and Central Asia starts ~55 kya, whereas the first elements of FMH (namely microliths) emerge only ~43 kya, and FMH in full evolves only ~22 kya. Southern Asian populations increase very markedly 55–45 kya, after which they stabilize; it is in the latter period, not during expansion, that FMH gradually develops. In Australia, FMH arrives fairly suddenly ~20 kya, much after the pronounced population increase 50–45 kya. Another type of anomaly concerns events after the arrival of FMH. In Sub-Saharan Africa, Europe, North and Central Asia, and the Middle East and North Africa, populations continue to expand, whereas they tend to stabilize in Southern Asia and Australia. To salvage their model, Powell, Shennan, and Thomas thus must demonstrate that complexity further increased in the former parts of the world, while Southern Asia and Australia went through a period of cultural stasis.

Powell and colleagues argue that some of these anomalies may be due to the low resolution of single-locus coalescent inferences (i.e., the method used by Atkinson, Gray, and Drummond 2008). However, while a recent multilocus study by Schiffels and Durbin (2014) resolves some of said anomalies, it gives rise to a set of new ones. In Africa, FMH would first appear at a time when populations were shrinking. In Europe, FMH would arrive at a historic low. Furthermore, population curves for Asia and Europe follow a trajectory that is almost identical, which is at odds with the variation between these two regions with regard to the timing of the appearance of FMH.

To conclude, let me be clear that I do not deny that demography may have a bearing on the mode and tempo of cultural evolution. I merely claim that cultural evolutionists have been too quick in identifying the mechanisms underlying this causal relation, an error that gives rise precisely to the failed predictions described above. On a more positive note, I am convinced, but cannot argue here, that a more promising approach is to revive a tradition that currently seems to have fallen out of favor, namely the tradition set in motion by Thomas Malthus and Ester Boserup.

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Reply

We are pleased with the positive endorsement of our article by all of the commentators. This gives us the opportunity to expand our article in the directions that they introduce in their comments. We find it striking that all reach essentially the same point, although from a variety of directions: namely, that the models (formal and informal) aimed at accounting for change in cultural complexity need to be restructured to bring them into accord with relevant ethnographic research. In our reply, we first make a few observations about their comments, and then we consider whether, as Gilligan suggests, we should have rejected the treadmill model outright. In so doing, we are led to discuss further the pervasive conceptual problem with the treadmill model introduced by having the same parameter, α, relating sometimes to skillfulness and at other times to complexity.

With regard to the comments, Eriksson, the first commentator, focuses on the fact that good mathematical modeling requires the modeling to be grounded in rigorous ethnographic research that delineates the properties of the processes being modeled mathematically. All too often, he notes, and especially when modeling cultural evolution, the emphasis is on the dynamics encompassed within a model but without having first established the connection between empirical observations and the theoretical processes incorporated in the model. Without a solid empirical foundation, he indicates, models such as the treadmill model are premature. When the empirical connection is found to be wanting, as has been documented with the treadmill model, all too often the response of the modelers and