

Interactive comment on “Endogenous change: on cooperation and water in ancient history” by S. Pande and M. Ertsen

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I have reviewed the paper by Pande and Ertsen and have read the comments by Prof. van der Zaag. One of the benefits of the interactive discussion procedure you have instituted is that it allows for iterative assessment of the paper under review, thus eliminating the need for duplication of some observations.

In this case, let me state at the outset that Prof. van der Zaag (who I'll refer to from now as PVZ) has made my task much easier by expressing some of the chief concerns I have—though no doubt in a far more expansive and articulate way than I might have done.

To begin, I note that the subject the authors have chosen—the relationship of cooperation and water management in ancient times—is an important one and can provide useful insights into analogous contemporary issues. Moreover, the two case studies selected for examination are aptly chosen. Both concern well-known and well-studied civilizations, both flourished in arid-to-semiarid environments, and both exhibited a high degree of control over their (scarce) water resources. As well, the topic fits well within the theme of the special issue—*notwithstanding* PVZ's caution that the period predates the Anthropocene.

The authors, having consulted the considerable literature on the Harappan and Hohokam civilizations (but not very much of the primary-source material and perhaps relied too heavily on a few selected secondary sources, some of which may be dated), set out to fit a theoretical model to the evidence of water-shortage-related decline of each culture. The model they've chosen is the theory of endogenous change. The first 20 percent of the paper is allocated to a detailed discussion of this and other theoretical concepts. Not being an anthropologist/archaeologist (I believe that is the discipline from which this section is drawn), I found the discussion unnecessarily dense and difficult to access, partly because of its reliance on disciplinary jargon. In fact, after reading this section, I wasn't sure where the authors stood in regard to the various constructs they discussed—that is, which notions did they agree with and which ones were merely presented as part of the contextual discussion? All this is particularly relevant to the readers of this journal, who will not likely be familiar with this mode of analysis and its associated forms of expression.

But rather more significant is the substantive question of whether the authors, having selected their preferred model, have made the case that (1) this model is appropriate to the cases at hand, and (2) that it helps explain the collective behavior of either society. I came away somewhat unconvinced, partly because, as PVZ points out, the data that would support the authors' hypothesis are largely absent. Yes, they've cited paleocharge records of the Indus and dendrochronological data from the U.S. Southwest, but these are somewhat cursory consultations and they left me unconvinced that they

reveal the full story of how climate varied and changed during the periods in question in the two regions.

I know from the work of some of my distinguished colleagues at the University of Arizona that the tree-ring record (which is far richer than the paper suggests; Ni, et al., is more than a decade old; I suggest consulting recent work by Connie Woodhouse and her associates—see <http://cwoodhouse.faculty.arizona.edu/content/curriculum-vitae>) is richly supplemented by paleopalynological evidence, rodent-midden data analysis, surviving pottery, and other varied primary-source archaeological research (see, for example, the Southwest Social Networks in Late Prehistory Project (<http://www.archaeologysouthwest.org/what-we-do/investigations/networks/>)). All these would add depth to the insights provided in the present paper. I imagine the same sorts of additions would strengthen the section on the Indus Valley civilization, though as PVZ notes, that is the better explicated and documented of the two cases.

And if the climatic record is incompletely explored, what of the social record? I found little discussion of the role of social and political organization, cultural and religious practices, or other aspects of each civilization that relate to trans-subregional cooperation. There are few if any mentions of actual institutions in either case. Were there formal institutions? Who organized them? How did they function? Were they effective? This is a particularly weak link in attempting to demonstrate a link between climatic variability, water-management practices, and cooperative practices.

I will not add stylistic comments to the list PVZ has provided, though I did find a few small typographical problems every so often. These would need to be corrected in a final copy-edit.

I thought the maps provided for the Indus case were very nice and of higher quality than the ones for the Hohokam case.

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