

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

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This is an interesting yet challenging paper, which fits the scope of the special issue, i.e. “Predictions Under Change: Water, Earth, and Biota in the Anthropocene”. The paper attempts to show how two specific societies developed and specialised (during the periods 4550–3950 BP and 1000–800 BP, respectively) under increasing water scarcity conditions, and subsequently collapsed, purportedly because of the success of the increasingly complex societies (e.g. with stronger cultural and trade linkages at relatively large spatial scales), giving rise, in both cases, to population increases that eventually could not be sustained when scarcity conditions further intensified, and apparently surpassed certain critical points which these societies could not address (e.g. because

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further technological innovation did not occur or evolved at too slow a pace).

Despite of the fact that the authors warn for a reductionist approach that would single out water manipulation as the single most causal factor in stimulating social development and complexity (p.4830, lines 16-19; see also p. 4831, lines 2-4), and despite that they develop quite a sophisticated theoretical framework to allow them to analyse and explain endogenous change in a non-reductionist and dynamic way, the reader cannot escape the impression that there is some climatic determinism underlying the storylines of both case studies. I am not saying that this is necessarily problematic, but the authors should recognise this.

So this contribution to the theme of the special issue is limited in two ways:

(a) The cases effectively pre-date the anthropocene era (which is considered by many to have started by the onset of the industrial revolution around the end of the 18th century). This does not, in my view, disqualify this paper for this special issue; it rather adds value in a particular way, namely how historical (Holocene) societies also have been dynamically enmeshed with their biophysical and climatic environments.

(b) The driving force of societal development and change seem to have been primarily conceptualised as climate with hydrology as a secondary factor; societies respond to these conditions and thus are reactive. It remains unclear to what extent the case study societies have been able to manipulate their environments to such an extent that they could neutralise the climate signal and create an illusion of standing “on top of” nature. In that sense there is no full “symmetry” in dealing with social and biophysical aspects. The claim made in the paper, namely that “Human action ... [is often] not considered as conduits of feedback themselves” (p. 4831, lines 24-27), is therefore in my view not fully refuted by this paper.

Before delving deeper into the paper I would like to state that I am not a historian, nor am I familiar with both cases analysed in this paper. So the value of this review is limited, and my criticism of the methodology adopted may not be shared by historians.

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I therefore sincerely hope that also historians will review this paper.

Having stated this I now proceed with my four major comments. I have also minor comments that I present towards the end of the review.

My first major comment refers to method. Here I should be very careful given the obvious paucity of data. What I think the authors do is, for both cases, to correlate two independent data sets, one is climatic, the other is social. They then impute causality. And this latter is in my view problematic. The best they can do, given the lack of (qualitative) data that can explain the correlation, is to postulate hypotheses with respect to causal relationships, no more and no less. I find the formulations in the Indus case more cautious and appropriate than in the Hohokam case.

In light of the above I consider sentences such as the following problematic:

“The weakening of winter monsoon during the mature phase and ever decreasing strength of the summer monsoon created the avenue for cooperation” (p.4840, lines 26-27). Wouldn't a formulation in the form of: “It is hypothesised that ...” be more appropriate?

“Such short time scale fluctuations must have interrupted the progress of institutions to maturity” (p.4847 line 27 – p.4848 line 1). Perhaps the word “might” could have been more appropriate.

“This added further strain on the personal relationships and exacted another cost of personal nature on the coalition structure, weakening it over time” (p.4849, lines 19-21). Why not rephrase the beginning of the sentence: “This may have added ...”

My second comment concerns the Hohokam case. I find this case less convincing than the Indus case, for four reasons:

First, figure 14 needs to be carefully explained: how was it manufactured? This remains unclear, yet the entire story hinges on the fact that there was a disjuncture between rainfall and stream flow availability over time.

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Second, this case requires a more robust analysis of the assumed change in climate variability. The paper states: “This degree of stochasticity in annual water resource availability represents uncertainty that adds to the cost of building a coalition under water scarce conditions.”(p. 4848, lines 1-3). We are talking about a society that developed since 450 AD. The problem I have is that the text suggests that a fundamental change in variability occurred sometime around 1000 AD, but no evidence is given; the paper only states that the variability was very high. But perhaps it was also very high when the society was formed starting in 450 AD *(and that thus the very fabric of that society was adapted and “trained’ to that high level of climate variability). So statistical data that start in 1000 AD, as presented in this paper, are insufficient to clarify this point.

Third, what I find confusing is the sub-argument on quickly alternating wet and dry periods (see e.g. the quote by Ni et al, 2002, on p. 4846 lines 1-3; and then the refutation of this claim by other authors; while the paper at several instances distinguishes “wet to dry conditions on one extreme and extremely dry conditions on the other extreme (p. 4848 lines 5-7, see also p. 4847 lines 26-27), without explaining what this means (for this reader it remained completely unclear)). The reader is left wondering why also this is salient and necessary to report. Is this a key dimension of the problem at hand? If so, why?

Fourth, this case heavily relies on secondary sources that are extensively cited. Approximately 20% of the case description consists of literal quotes from one author (Abbott).

My third major comment relates to the fact that both cases have not only very different time scales (p. 4847 line 19), but also different spatial scales. The latter is important in that spatial heterogeneity is likely to be greater at larger spatial scales but also more functional (different climatic zones, ecological niches, allowing the beneficial exchange of goods, and allowing different buffering functions when faced by climate shocks, potentially making the entire system more robust). The above implies a question: has the

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Hohokam case been studied at the correct spatial scale; did indeed no long-distance trade links exist? If so, are there explanations for this absence? If indeed the correct (limited) scale was used, then the conclusion in the Synthesis session is quite obvious (see p.4848 lines 22-23).

My fourth major comment is not a criticism but a wish: I wished the cases contained more detailed descriptions and analyses of technological innovations that occurred in the two cases (and I fully realise that my wish may be impossible due to lack of relevant data). That would have more specifically highlighted how environmental conditions and human societies interlocked, and how humans actively re-shaped both their social relations and the biophysical environment in which and upon which they operate and on which they relied.

Some minor comments of a more editorial nature:

p.4830 line 1: The title is in my view too general (and perhaps also not entirely correct, since the climate signal is exogenous). What about a title like: “Cooperation and water in ancient history – scarcity triggers more complex cooperative arrangements at hydrological scales”

p. 4830: Key words missing

p. 4831, lines 4-5: Is water indeed the most precious of all resources? This statement can be disputed. Why not simply state that it is a vital resource for human survival. This does not diminish the argument.

p.4832, line 9: Check year of the reference to Ambec and Sprumont

p.4834, line 14-19: Sentence does not read well; there may be a grammatical flaw.

p.4835, line 4: Shapley, 1967 does not appear in the reference list

p.4835, lines 3 and 8: Are the concepts of “cooperative cores” and “core stability conditions” in the cases? Are these concepts needed?

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p.4836, line 13: Check spelling of settlements

p.4837, line 16: 3900-3200 BP would be more correct

p.4839, line 7: What are “administrates”?

p.4840, lines 12-13: The sentence lacks a word; I suggest “evident”, i.e. “It is instead evident in the...”

p.4840, line 24: “subsistance”; the correct spelling is subsistence

p.4841, line 24: The words “such as in the farming system” can in my view be omitted

p.4842, line 11: The numbering of paragraphs in the Hohokam case differs from that of the Indus case. That does not convey a coherent approach.

p.4842, line 20: “re-known”; is “renowned” meant here?

p.4842, line 21: “and their apparent disappearance after roughly 1450.” Not clear who “their” refers to.

p.4843, line 12: It would be in order to explain in one or two sentences the phenomenon of “ball courts” and why it is a good indicator for this society.

p.4844 line 13: Two times “their” in one sentence; replace one by “the”

p.4846, line 28: “period” should be plural

p.4847, line 2: “..a different allocation of the population”??? Not clear. Is “distribution” meant?

p.4847, line 24: There is awkward use of the word “the” here; also on p. 4848 line 10,

p.4849 line 1 and line 3.

p.4848, line 10: “its”? Is meant “their”?

p.4848, line 20: “It”: not clear to what this refers

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p.4848, line 25: “emergences” should I think be singular

p.4850, lines 10: Two times use of the word “all”: was it indeed necessary for “all” settlements to cooperate, or could it have also been “most”?

p.4860 and 4861, Figures 6 and 7: Confusing that the Y-axis of both figures are different. How come? Quite odd in my view.

p.4868, Figure 14: See above. This figure needs a proper explanation.

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