

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

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We thank the Editor for her comments. We are pleased that the Editor finds the theory of endogenous change in context of water that we have proposed a valuable contribution to the special issue. We again thank the editors of the special issue for providing us with such honest and critical reviews of our manuscript. We welcome the Editor's decision to send it out to review again; we look forward to the referee responses. In this document we will discuss the different comments made by the Editor.

We start with a remark that any further clarification that may be needed for bringing out our argument in a more convincing way will of course be included by us. However, we do have some observations on what we have done in that respect so far. On the

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issue of the comparative case study method, we believe we have addressed the issue of explaining the basis of our comparative analysis in our revision in the first paragraph of our concluding remarks as well as our discussion. We agree adding some extra text in the paper is relatively easy, but we strongly believe that such addition would only marginally contribute to the overall paper.

We believe we have included enough data from both archaeology and climatology. We have provided a discourse on the debate on the exact cause of Harappan dispersal between archeologists and paleoclimatologists and that our theory supports the arguments of the former. The figure illustrating the chronology of Harappa against paleoclimatic proxies of water resource availability provides the evidence. In the case of Hohokam we have now added a new figure that clearly illustrates the departure of scarcity conditions (human induced) from hydroclimatic variability.

We do not believe a theory like the one we propose can be ‘proved’ through empirical evidence. We can only find evidence that either supports the regularities predicted by a theory or falsifies it. At best, empirical evidences can ‘disprove’ existing theories. In both the case studies we find evidence that supports the argument (as posed by the theory of endogenous change) that societies mature under increasing scarcity condition but disperse when scarcity is exceptionally extreme. Note here that the theory suggests that increasing scarcity condition is a sufficient condition for maturity but not necessary (which means that there may be other conditions under which a society may mature). We think we have included sufficient words of caution about any ‘inevitability’ our approach may suggest – we certainly do not suggest such inevitability.

We have to emphatically disagree with the editor that “the archaeological evidence of dispersed settlements is not really sufficient to prove lack of cooperation - they might simply be a symptom of a dying civilization with no surpluses to trade and populations migrating in search of food rather than lack-of-cooperation as a rational response to scarcity”. Also we did not read it so from the referee comments. We would think that finding a ‘dying civilization with no surplus to trade or population migrating in search of

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food' is evidence of the collapse of a cooperative structure. Nowhere do we claim that a collapse of cooperative structure 'causes' the symptoms mentioned! The symptoms, as the word itself suggest, are of the collapse of cooperative structures. The theory of endogenous change poses the conditions (or the causes) that triggers collapse of cooperative structures, 'symptoms' of which might be several including those mentioned by the editor.

Regarding the jargon and the language and the need to refer back to Greif and Laitin paper, we have attempted in our revision to further define the concepts in the paper. We however would not be able to remove all of the concepts (jargon) since the notion of endogenous change is not limited to the contribution of Greif and Laitin, though the concepts of quasi-parameters, sequence of punctuated equilibria etc were borrowed from them. Endogenous change theories have been proposed in several contexts, such as economic growth, technological change, climate change etc. Why we insist on retaining the concepts is for a complete discussion of the theory of endogenous change for hydrologic systems. For example, the theory of endogenous change in the context of water is formally incomplete without specifying what is meant by change and under which conditions such a change may occur. These conditions need to be explicitly stated since the organization of water dependent societies is uniquely shaped by the properties of the natural system they reside in.

Finally, we would like to highlight that a change in a society in response to 'exogenous' change in hydroclimatology can still be 'endogenous'. The organization of a society for a given hydroclimatological context is endogenous, i.e. it is not solely driven by external factors given to it as such (such as climate). Societal response is shaped by choices of and based on (endogenous) interactions between societal members. We have not included this discourse in the revision since it invokes additional socio-political and economic arguments.

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