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Hydrology and Earth System Sciences
Discussions

Interactive comment on "A journey of a thousand miles begins with one small step – human agency, hydrological processes and time in socio-hydrology" *by* M. W. Ertsen et al.

Anonymous Referee #1

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This paper presents an interesting discussion of the importance of temporal scale in the modeling of coupled human-water systems, focused on ancient irrigation systems in the Southwest USA. Examples are provided of long-term climate reconstruction and archaeological analysis of medium-term canal system development, followed by a short-term model simulation examining the impact of different canal operation scenarios on daily irrigation system performance. The authors argue that these three temporal scales are integrated and fundamentally linked. They suggest that modeling of human agency must capture the short-term dynamics, but also set these within the medium- to long-term evolution of the system. This analysis of the issue of temporal

scale in modeling of human agency is undoubtedly pertinent and highly relevant to socio-hydrology. The paper therefore has the potential to provide a valuable contribution to this special issue. However, to reach this potential a number of areas would need to be significantly improved. These are discussed in the comments below.

1. The interoperability of the analysis in Section 3, 'Climate reconstruction: the archaeological long-term view', is reduced by a number of specific errors:

i. The portion of Fig. 3 comparing monthly simulated and observed precipitation is missing from the manuscript. This makes it difficult to judge the accuracy of the model to adequately simulate precipitation.

ii. Variables on p.14276 lines 10-12 are incorrectly defined as "temperature" when in fact they should refer to precipitation.

iii. Equations on p.14276 lines 2-3 appear to be incorrectly defined. On the basis of the description given on p.14275 lines 26-30, my interpretation is that these equations should calculate the simulated monthly climate variable (precipitation/temperature) at the study site using observed monthly climate variability at the upland site (CD2/Fort Valley) plus the annual ratio/difference in that climate variable between the upland (represented by the tree-ring reconstruction data) and lowland sites (given by observed data at CD6/Chandler Heights). However, you are actually calculating (according to the equations and variable definitions) the simulated monthly climate variable (precipitation/temperature) at the study site using observed monthly climate variability at the lowland site (CD6/Chandler Heights) plus the annual ratio/difference in that climate variable between the reconstructed upland (represented by the tree-ring data) and observed upland (given by observations at CD2/Fort Valley) records. Exactly which calculation has been conducted needs to be clarified and justified as currently it is confusing.

2. My second point also relates to aspects of Section 3. Specifically, whilst the use of the climate factor (CF) approach is detailed for the period where observation data are

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available, it is less apparent how the CF method has been used to reproduce the long-term paleo-climate record for the Hohokam region. Have monthly temperature and precipitation estimates been simulated for the full length of the tree-ring record and, if so, what additional steps and assumptions did this involve? Specifically, my impression is that use of the CF method would require the assumption that climatic variability has not changed over time. If so, how reliable is this assumption? Furthermore, can any of these simulated trends in long-term climate be associated with evidence of short- to medium-term changes in irrigation and human agency in the region (thus illustrating the integrated nature of temporal scales)? This is hinted at towards the end of the last paragraph on p.14277, and further discussion would strengthen the paper.

3. In terms of Section 4, 'Canal system geo-archaeology and micromorphology', I will largely defer to other reviewers who will hopefully be able to bring more substantive knowledge of this subject area than myself. However, I will note that you should be careful to avoid the use of technical terms which may not be immediately obvious to readers of a journal such as HESS. Examples would be "sherds" (p.14279 line 24) or "vughy structure" (p.14281 line 16). These were, however, limited and in general I found this section well written.

4. Section 5 presents a novel example of using archaeological canal network data to parametrise a hydraulic model of a small-scale irrigation management system. However, I feel more evidence needs to be provided to justify the validity and relevance of the way this model was then applied. Whilst recognising that necessary data may be absent, some specific comments include:

i. The analysis is focused on assessing the impact of different control scenarios on irrigation delivery times. However, it was unclear if the archaeological evidence supports the assumption that control structures were used to manipulate flows in such irrigation systems. If it does, this should be stated more explicitly. If data is available it would also be interesting to note how these controls are likely to have been operated. Would they have been managed by individual irrigators or some sort of controlling organization?

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How frequently would farmers have been irrigating? This would help to provide context for the design of model simulations and implications of the results.

ii. It is stated that applying controls reduced total irrigation time to some 5 to 6hr. However, it is not shown what value this was reduced from. It would be valuable to report the results for a no-control scenario to illustrate both the variability within different control scenarios and between control and no-control.

iii. I agree that the results clearly show lower delivery times and greater stability under certain control scenarios. However, I remain somewhat unconvinced that the differences in irrigation delivery times between scenarios would have translated into markedly different societal/individual benefits. My suggestion would be to elaborate further on the sentence "higher benefits in terms of stability and lower demands in terms of coordinating actions required" (p.14286 lines 11-12) to strengthen this conclusion.

iv. It would also be helpful to know if the longer total delivery times resulted in higher volumes of water being needed to fill up all the fields or whether the water just took longer to reach all fields. If it was the former then obviously this could be very significant if it meant some irrigators could not fill their fields when inflow was scarce.

5. Finally, some more general comments which could improve the overall quality of the manuscript:

i. Sections 1 and 2, whilst making a number of very valid points, are unnecessarily wordy. This obscures the key aims of the research and may discourage the reader from continuing further. An example would be p.14268 line 14 – p.14269 line 9, which could be expressed in less space if the quote was omitted and the rest of the text simplified. Similarly, I think the contribution of the paper to the wider literature (e.g. what can archaeological analyses bring to the field of agent-based modeling?) could be made clearer through some careful restructuring of these two introductory sections.

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ii. While some figures were very visually appealing and informative, others, most notably Figs. 1 and 9, were of poor quality and should be improved. As previously noted, Fig. 3 is also incomplete.

iii. A number of assertions are made which are not supported with references. An example is "Agent-based models have been applied successfully in rain-fed agriculture" (p.14271 line 3). These would need to be supported by citations in future revisions of the paper.

iv. A number of grammatical and typographical errors are present within the text which would need to be corrected in the final copy-edit.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 14265, 2013.