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Discussions

Interactive comment on "A virtual water network of the Roman world" *by* B. J. Dermody et al.

Anonymous Referee #2

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Summary: This paper uses a hydrological model, a dynamic agent based model and present and past input/forcing data for these model and comes up with a virtual water trade analysis under climate variability for the Roman world around 200 A.D.

Comments: I read this article with great enthusiasm. Virtual water trade is an important topic, especially in context of socio-hydrology and water footprint assessment. Yet I have several concerns with the underlying methodology. Following are my comments:

1. The paper appears to have been condensed from another version. This process appears to have been rushed through. Selective examples include reference to figure 4 (line 25, page 6573)when they meant figure 3, reference to Wilensky, 2010 that does not exist in the reference list, reference to a dynamical agent-based network which this referee was not able to find, insufficient explanation of this agent based model itself, passing reference to socio-hydrology without further exploring how this paper fits into or

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complements recently published literature on socio-hydrological modeling, in particular historical socio-hydrology, etc.

2. The authors do not make a clear distinction between socio-hydrology, which is the science of people and water that studies two-way feedbacks between humans and water systems, and water resource management. The authors mention “emergence” of complex socio-hydrological responses to climate variability but none are found in the manuscript.

3. It is not clear which agricultural commodity is facilitating the virtual trade of water. References to trade in grain have appeared in several places.

4. The modeling effort appears to be disproportionate in resolution/complexity. The authors model the hydrology using a 3 (or 4 layer landsurface model, not clear) and landcover resolved at 30” resolution. A “suitability” algorithm is even used to downscale HYDE data set that probably been derived from proxies using GLCC. No details of how HYDE dataset has been reconstructed is provided. This perhaps is an overkill, given the uncertainties in HYDE is data set. The authors also fail to justify why the suitability algorithm is a suitable algorithm to downscale HYDE data set. No underlying assumptions have been provided by the authors to gauge its value in analyzing the VW trade in the Roman world. Present day climate forcings for the study have also been used. It is hard to believe that such detailed modeling can compensate for the uncertainties present in the reconstructed HYDE dataset or for that matter Orbis network structure data set. The VW deficits that the authors calculate can as well be estimated using very simple mass balance models. This mismatch between model complexity and information availability casts doubts on the conclusions drawn.

5. To continue on point 4, the authors use MIRCA dataset to divide cropland into rainfed and irrigated. They then use the complex hydrological model to estimate how much of blue water demand can be met. Using HYDE population data set, the authors then estimate the food deficit (and hence VW deficit). The authors failed to realize that

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MIRCA dataset refers to a period of 1998-2002 and heavily depends on existing agriculture infrastructure (Area Equipped for Irrigation, AEI). Linear transfer of AEI figures from present to the past is erroneous and connects to another comment that appears later. Further unvalidated output of the complex hydrological model is used to estimate how much of blue water demand is met, assuming AEI can be linearly interpolated.

6. Available area under irrigation requires infrastructure. Depending on the scale of irrigation, infrastructure development requires relatively heavy investments and often central planning. This inturn tends to go hand in hand with institutional development of societies, often nonlinearly. Linear transfer of AEI from present to past is therefore misguided and ignores the relevant two way feedbacks between humans and their water system in the Roman world.

7. How VW redistribution is calculated is missing. I will therefore assume that one starts with the most VW poor, allocate VW from VW rich nodes ordered by their edge costs, update the VW deficits and repeat the same with the second most VW poor and so on. If this is the case or it be any other algorithm, no rationale has been provided why agricultural trade will happen in this fashion. Why should the VW rich nodes trade with the closest (in edge cost sense) VW poor node. At present there is no reason to believe that "struggle against distance" is an appropriate mechanism for VW trade.

8. Continuing further on point 7, the VW network structure does not appear to evolve over time. No support has been provided for the assumption that all the edges of the Orbis network were active around 200AD. Perhaps the VW redistribution algorithm may also activate or deactivate certain edges based on the distribution of VW deficits but it preserves the structure of the network. Without the flexibility to adapt the structure, one is unable to see how the presented paper allows us to explore complex emergent socio-hydrological responses. In fact, not knowing whether all the edges of Orbis were active during the period under study, one may even overestimate the resilience provided by the VW network.

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9. Last paragraph, page 6573 and page 6574: It is confusing that the arguments about the variation in yields with increasing temperature/precipitation are interchangeably made in space and time. Though it is based on spatial analysis averaged over 50 years of simulation.

10. The argument that import cost is closely related to node degree is perhaps a consequence of the VW redistribution algorithm. One may wonder if it may even be independent of the underlying network structure. In this referee's opinion, it is not something that "emerges" from the model but is a consequence of the VW redistribution algorithm itself. Given the lack of information on the VW redistribution algorithm, it is unclear whether this is indeed the case.

11. The authors have ignored the point that the network structure that existed during Roman times possibly emerged alongside the spatial distribution of affluence, labor specialization and emergence of social classes. Thus high cost edges from interior nodes existed because those nodes can afford it for one reason or the other, either because they were the ones who controlled the hubs, trade routes, inland trade routes, who controlled the political power etc. A resilience test of a socio-hydrological system in context of virtual water trade therefore does not reside in sensitivity of import costs to climate variability and population growth for a given network structure but how the network structure itself adapts to changing conditions and to what extent can it adapt. Population itself may adapt alongwith the network structure and it is this possibility of co-adaption of human and water system to climate variability that defines the resilience of a socio-hydrological system.

12. Finally, the authors appear to invoke the relationships between stability and inefficient information transfer to counter the discrepancy between model simulations and historical records of the export regions of VW to Rome. The trade in grains probably occured for reasons not limited to what the agent based model of the authors prescribes. These include wage rates, opportunity cost of land and water, type of governance, trade in other commodities etc. The authors appear to have missed such critical

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aspects of VW trade. It is in this sense that the analysis is unbalanced: a complex hydrological model with a "suitability" algorithm to downscale HYDE but being inadequate in their modeling of VW trade. The scope of the study is to study the resilience of the socio-hydrological system to climate variability, which warrants a more complex representation of the VW trade but perhaps that is incompatible with the current complexity of the underlying land surface hydrology.

13. There appears to be a lack of coherent structure to modeling the underlying socio-hydrological system. The authors appear to have incrementally contributed to the manuscript based on their expertise (and appear to have been guided by that and not by the research question), which probably has led to a mismatch between the resolution of land surface modeling, VW redistribution modeling and available historical information.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 6561, 2014.