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Discussions

Interactive comment on "Moving sociohydrology forward: a synthesis across studies" *by* T. J. Troy et al.

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The paper presents a nice overview of the different contributions on sociohydrology (SH) in the special issue of HESS/ESD. As such, I only have a few remarks on elements in the text which might ask the authors to push their argumentation just a little further. First on the issue of complexity. I am not too sure the general definition of complexity is very helpful. The idea of multiple interactions is not so clear anyway, it is how these are defined and conceptualized. On page 3322, two interesting remarks are made that I would see as examples of phrasing (and framing) that would require some more thought. How can there be scale mismatches between systems (line 15)? Either systems are related or they are not, and I am certain not all the processes are to the

liking of all, but mismatches suggests there would a good match possible. For whom is that to judge? In addition, why is two-way coupling necessarily a slow process (line 25). That seems to presuppose certain types of changes.

These opening remarks from my side link to the issue of one-way or two-way couplings. I would argue that all our SH systems are two-way coupled, or, as the authors correctly state, there are always multiple couplings. I would like to draw the attention to the concept of Human Niche Construction, which builds on the concept of Niche Construction in stressing that in changing their selective environment organisms change themselves too. Human NC simply argues that humans do so as well. In stressing the importance of human agency, HNC comes close to the concept of Evolutionary History (EH). Evolution is everywhere, happening all the time and humans have played an enormous role – conscious or not – in shaping evolutionary processes. HNC and EH relate changes across four dimensions: 1. material environment – modified by human agency; 2. social arrangements – when modifying the environment and responding to the changes; 3. genetic structure of the human group – as a result of modifications; and 4. genetic structure of other groups than humans. No, whether these changes are short term or not, and how extensive they are, is not easily to defined before any research. The issue which level of coupling to go for would but only be a matter of methodological possibilities or limitations to engage fully with the fully coupled system. There is only one way to go in theory, but practical limitations might require distinguishing between more or less integral coupling. This would mean we need to think about two-way coupling, water-human one-way coupling, and water-human one-way coupling as methodological issues, not as conceptual issues.

The paper suggests a few times that whether areas are wet or dry matters (page 3324 for wet, 3325 for dry). I have no problem with bringing material conditions in the analysis of SH, not at all I should say, but the whole concept of SH forces us to rethink what dry and wet mean. The material conditions are no longer external anymore. Quite often, the issue is not wet or dry, but linking different rhythms and the manipulations to

realize certain rhythms. The paper also suggests a few times that wetness or dryness shapes behavior or preference (page 3339 for example); a similar relation between rich and poor people's preference is suggested on page 3331. The observation that certain sites have something does not make it a preference. Collective outcomes are not to be confused by people's outcomes. Societies do not make any change, people do. We should not confuse outcomes with actions. Obviously, I am flattered that the authors use in their conclusion the choice between two approaches as discussed in one of my own contributions to the field , but I would like to suggest that several approaches in the special issue are either not making that choice or do things that go against what I wanted to suggest when making the statement in the first place.

A main concern I have about the field of SH (and social complexity in general) is how human agency is taken on board. This issue is discussed in the review paper, but I would suggest some more on it along the lines below – either in agreement or disagreement. Many times, predefined responses are used, or known responses are copied by an algorithm. This includes quite a few studies on the Murrumbidgee system, but also the work on flooding. The unraveling of feedback seems to rather difficult in such work, as the feedback mechanisms have been predefined. What is there to unravel when the outcome is already known? Basically, the approach that shows that it can mimic behavior that was expected (which is pretty good perhaps) does not provide a way to be surprised. The problem in complexity sciences to me is the two-way issue of assumptions and pattern-repetition. What we think will happen is modeled to happen. This is a huge problem and we should strive for avoiding doing exactly that in SH. Whether economic sciences (or sociology for that matter) will help much is not clear to me. Several fields of economy have been pretty successful in using predefined behavior as input – claiming the predefined agency to be the desired one was well. If there are fields of scholarship the SH community could engage with it is (environmental) history and archaeology. In those disciplines, ideas about the value of data from the past, from human-environmental interactions, about proxy's and analysis of change, are much better developed than in the hydrological community – and if you

ask me also in much of the complex sciences field, which is heavily driven by economy and psychology.

I have added a scan with my original hand-written comments, as these have some more detail.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/12/C1289/2015/hessd-12-C1289-2015-supplement.pdf

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