

Interactive comment on “Socio-hydrology and the science-policy interface: a case study of the Saskatchewan River Basin” by P. Gober and H. S. Wheeler

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This paper describes in fairly general terms recent developments in a large closing river basin – the Saskatchewan. Such a descriptive case study is valuable in its own right. But I doubt whether this paper makes a significant contribution to the central theme of the special issue, namely how water and humans are dynamically linked. The paper does make mention of an interesting feedback that occurred in the state of Alberta, namely when this state was faced with a severe drought “that triggered crisis conditions in 2001-2002” (p.6681), and society responded (and I guess it had to respond) by changing the manner it viewed water (a paradigm shift), and thus also

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its strategy towards water management. But the paper does not analyse this case in any detail – what role did hydrologists play in this process, if any, and what role social scientists? Did the crisis not only trigger a paradigm shift in policy, but also in academia? Did indeed new concepts similar to that of “socio-hydrology” emerge, and/or did it trigger “hydro-social scientists” to engage with policy and with hydrologists (see e.g. the work by Swyngedouw, Bakker, Linton etc.)? How did actors make use of the policy window (p. 6681, line 20). Did so-called policy entrepreneurs (Huiteima and Meijerink, 2010) emerge and exploit this opportunity? Who were they, and how were they related to “conventional” water actors? And to larger political processes and power plays? Who were the losers? How does this case relate to other literature on closing river basins (see e.g. Molle et al. (2010) for an overview). We don’t know. I consider this is a missed opportunity.

The Saskatchewan is a very interesting case that has the potential to contribute to our understanding of how social scientists, hydrologists and engineers interpret phenomena, argue their perspectives, clash, and may cross-fertilize and hybridize their understandings. But this is not described nor analysed in this paper. The paper rather describes challenges to water security, focusing first on environmental change, then on population growth and economic development and subsequently on governance, and finally making a rather perfunctory reference to socio-hydrology and the science-policy interface. But everything remains framed in rather general terms, and as such this paper does not advance our understanding of the interaction between water, earth and biota, which is the subject of this special issue.

What I find telling in this context is that socio-hydrology is introduced as a “deus ex machina” solution (to be introduced from outside), rather than an endogenous phenomenon that may have emerged because of the water crisis. I find this a conventional approach that seems to sit uneasily with the very essence of the concept. This is my major reservation that I have with this paper.

In all the paper brings little news. This is also exemplified by the fact that the paper

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ends with two sentences that are of a highly general nature and not directly informed by the empirical material presented in the paper.

A second reservation I have is with the way the paper, and its argument, is structured. It consistently first introduces issues related to climate change and then issues related to other human induced changes, suggesting (a) that climate induced changes are more significant, and (b) that these came first. Neither is the case. The changes induced by river diversions (for irrigation and urban water use) and by constructing large reservoirs for hydropower have been enormous, which is aptly summarised by Figure 3, but these are not quantified. A water balance indicating the magnitude of water diversion on the hydrological cycle would be very useful. The impact of climate change is also said to be significant (“influencing the magnitude and timing of river flows” p. 6671), but its magnitude is also not quantified. For example, how has it influenced river flows as presented in Figure 2? I would have preferred that the paper consistently first deals with the direct anthropogenic changes and thereafter the indirect anthropogenic changes (not “anthropocentric”, line 7 on page 6674).

A third reservation has to do with the presentation of the paper. I am surprised that the introduction section fails to introduce the paper, and its objective. Also the section that follows fails to clarify the structure of the argument. So the reader cannot know what the paper sets out to achieve.

There are some minor issues of a more editorial nature that the authors may wish to address.

The use of acronyms is problematic. First, it is not a good practice to start the first sentence of a paper with an acronym without clarifying it (p.6670, line 25). Second, for the basin the following acronyms are used: SRB, SSR, NSR, SR, NSRB, SSRB. This is for the reader confusing. I guess SRB, NSRB, and SSRB would be sufficient? Or is there a difference between SR and SRB, etc.?

Although the paper is generally well written I find some formulations problematic, or

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they may need explanation:

“Governance and policy thus would need to be dictated by conditions in dry, rather than normal, years” (p. 6676 lines 27-28). I find the word “dictated” odd in the context of governance and policy (and adaptive management).

“Boiled water advisories” (p. 6679, line 22); please explain; readers not familiar with the USA may not understand what these are.

“... and over-rely on results of prior outcomes” (p.6680, lines 25-26). Not clear what this means.

Finally, two comments about the figures:

Figure 1 is not legible. I suggest that the authors draw a new map that only contains details that are relevant in the context of this paper.

Figure 2: Why does this time series stop in 2002, when the paper suggest that many impacts related to climate change have only become evident during the last decade?

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