

Interactive comment on “Hydrologic effects of land and water management in North America and Asia: 1700–1992” by I. Haddeland et al.

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The paper quantifies an important and largely ignored aspect of human alterations of continental hydrological cycles. Much of the focus in global water management research has dealt with the direct alterations of water flows through human withdrawal of water for irrigation as well as changes due to infrastructure development (e.g. dams and canals). This paper links it with the indirect changes in hydrological flows as caused by land use/land cover changes. Although land cover changes are well known to have hydrological effects, there have been very few studies quantifying the effects over larger areas.

The paper is well written, and very well suited to the theme of this journal. I therefore

recommend the paper for publication although I have one general and some more detailed comments that I would like the authors to address:

General comment

1. Methods: I would appreciate a more detailed summary of the model used, in particular in relation to major assumptions on land use - water interactions. Now you have to read the previous manuscripts to understand how this is done. Maybe this could best be done with a brief description of the main parameters that results in hydrological alterations (e.g. how is vegetation class related to LAI related to ET and stream flow). This could either be done by adding text or a figure. I would be particularly interested in how conversion from grasslands to croplands alters the parameters that determine hydrological changes. In the reported results in this paper this seem to result in decreased stream flow, but I am not convinced this is happening in general in reality, why it would be good to have the underlying assumptions in the paper.

Detailed comments:

1. Line 2 page 2904: I would suggest not using 'scenarios' in the title of the section since this seem to refer to a forward looking exercise.
2. Table 1. I would like to see a comparison between the data/land use used in this study and that used in Ramankutty and Foley to see how much this study deviates from theirs. Could a comparative column be added to the table (preferably quantifies, but otherwise how the classes differs)? Also, the classes in table 1 is fewer than in figure 4 - which ones were used?
3. Line 25, page 2905: What is meant by 'only dams built within the simulation year is included'? Should it be 'only dams built before the simulation year'?
4. Line 7 page 2908: Note that the Gordon et al 2005 paper compares potential and actual vegetation, and does not use the 1700 data as a starting point. In 1700 there has already been land converted to crop/grasslands in East Asia (see the Ramankuttney

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and Foley paper).

5. The first paragraph in the Conclusion section could be re-written, as it is a bit unclear at the moment. For example the authors go from saying that runoff in North America has decreased (from replacement of grasslands) to that irrigation cancels out the increased runoff.

6. Line 12 page 2910: What is the implications of 'simulated water use is only about half of the water requirements' for the results?

7. Figure 2. Please provide more detailed text and reference. Figure 3. Reference missing. On line 15 page 2905 you could also refer to this graph when discussing how you got historical data for 1950.

8. Figure 6. You have one point which is 'water assumed freely available'. However this is not really referred to in the text. It would be good to mention somewhere why this make the result so different (this is related to how the ET from irrigation is estimated and also to the groundwater discussion referred to by reviewer 1).

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