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Interactive Comment

Interactive comment on "One-way coupling of an integrated assessment model and a water resources model: evaluation and implications of future changes over the US Midwest" *by* N. Voisin et al.

Anonymous Referee #1

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This manuscript presents the first results from a coupling of an integrated assessment model with land surface hydrology and water management models. The paper is generally well written, and the subject and findings are a contribution appropriate for publication in HESS. I would suggest some changes to clarify the approach, and an augmented discussion of the importance of the approach to advancing our understanding of the earth system response to climate disruption.

Specific comments:

1) Abstract: I found the effort pretty interesting, but from reading the abstract it almost sounds as if the authors are bored by their own work. Statements like "The supply deficit seems to be driven by the change in flow. . ." seem so obvious that it is unclear why any experiment would be needed to ascertain this. I cannot understand what the last sentence is trying to say. Much later in the paper (for example, conclusions part 2b) there are things that were found here that would not be easy to discover with other techniques, illustrating the advantage of coupling an IAM to a water model. If the abstract clearly stated why this coupled model is needed, and featured more of these sorts of unique insights into the response of the water supply and demand, that would be an improvement.

2) p. 6365, line 9, the 'pseudo grid cell' of each subbasin represents on average 120 km2. It might be worth noting that this is roughly the same as a 1/8-degree grid cell, making the resolution comparable to the NLDAS effort, which shows up later in the paper.

3) Section 2.2, the modeling chain is discussed, which is somewhat confusing. The CLM implementation is discussed, along with its atmospheric forcing data (sect. 2.2.2). While some of the shortcomings are discussed in Section 5, it would be helpful to explain why there is a land surface model used, when there is already a land surface component in the IAM. Some mention should be made regarding the types of errors that may be introduced when taking one set of output from an IAM and feeding it into a one-way coupling that includes a component (the CLM) that can no longer feedback into the earth system dynamically. Is there any correspondence between the land use in the IAM and that in the CLM? It should also be clarified why atmospheric forcing data were needed, since up to this point it sounds like that would be obtained from the IAM.

4) p. 6366, line 15, calling the B1 scenario "middle of the road" is incorrect. It is a very optimistically low projection, and the lowest of the SRES scenarios.

5) p. 6367, line 13, Similar to prior comments, the questions raised by assumptions

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should be at least mentioned up front, rather than relegating them to the end of the paper. Specifically, “return flow is not explicitly simulated” would seem to be a serious shortcoming, as in some basins this is a significant component of the managed water supply.

6) Section 3 I found confusing. It is explained that the IAM operates at a 5-year timestep. In the temporal downscaling (sect 3.2) annual water demand is linearly interpolated to obtain annual demand values. But then irrigation (section 3.2.1, p. 6370, line 13) demands are apparently computed on an annual basis using estimated crop coefficients. Are the demands obtained from the IAM only lumped values at 5-year intervals? Is the separation by source (the remained of section 3) only used to allocate the totals from the IAM?

7) Another concern regarding the 5-year IAM timestep and the linear allocation of demand between time steps is how cyclical events would be smoothed. For example, ENSO variability is largely removed with a 5-year aggregation. Changes in intensity, extent, or duration of, for example, 2 year wet or dry periods, with their concomitant changes in water demand (as farmers dynamically adjust some demand to accommodate supply) would be missed completely by this analysis, it would seem.

8) p. 6370, line28, Similar to comment 7, the monthly distribution of irrigation demand would change with variation of temperature and precipitation from year to year. Is that simulated here?

9) Equation 1, the subscripts do not seem to be used consistently. The subscript i is for month, but does that mean there are 12 of them, or one for each month is the simulation? Should Ratio have a subscript of i as well?

10) Equation 7, similar to eq. 1, T is monthly temperature; should it have an i subscript too?

11) p. 6374, line 20, the operating rules are stated as static into the future. Again,

some mention of this is included later in the paper, but a justification at this point would help. Since adaptation of reservoir operation is already being promoted for relicensing (e.g. Viers, 2011. JAWRA 1-7. DOI: 10.1111/j.1752-1688.2011.00531.x) this is not realistic. It should be justified as a necessary simplification at this point.

12) p. 6375, section 4.1, historic changes over the period 1984-1999 are evaluated for the coupled modeling system. Analyzing changes over this short period would be highly vulnerable to natural variability. Furthermore, since it represents just 3 IAM timesteps, looking at changes over that period would not seem to be a very meaningful exercise. Or is it just the variability that is being analyzed?

13) p. 6376, line 5, regulation drives a 24% loss in annual discharge, which seems far too large – is that including diversions?

14) Section 4.2.1, Some more information on these changes would be helpful. For example, for the Missouri, demand increases up to 60% over the irrigation season. Is that driven by expanded area, higher temperatures creating increased PET? Are changes in irrigation efficiency included? Direct effects of CO_2 on stomatal conductance? There are so many things wrapped up in these numbers, an expanded interpretation of what is driving these changes would show some of the value of using these models. Similarly, section 4.2.3 could be expanded to provide a deeper understanding of the projected changes.

15) p. 6380, line 24, as in the abstract, the line "changes in supply deficit are driven by a combination of changes in demand and runoff" (repeated in conclusion number 2d) is too bland and general. How much of the deficit is driven by changes in demand vs supply? How has this study provided clues to this response that have not been available with the tools used for climate-water impacts up until now?

16) p. 6382, line 14, the "regulated flow is projected to increase" and the next line down says " supply is also projected to increase." Aren't these essentially the same thing?

17) A final thought – a lot of flow regulation in this area is done for barge navigation purposes. How are requirements/demand for navigation represented in the IAM or in the offline models coupled to it?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 6359, 2013.

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