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Discussions

Interactive comment on "A global hydrological simulation to specify the sources of water used by humans" *by* Naota Hanasaki et al.

Anonymous Referee #3

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Summary: This Discussion Paper presents on model development in the H08 Global Hydrological Model intended to expand the model's representation of human water appropriation. The reported model enhancements are important in an age of large and increasing human modification of the water cycle, and the authors' approach to model structures and data will be of interest to HESS readership. The Discussion Paper presents both the implementation of model enhancements and the results of model simulations globally and for major river basins. As a presentation of model methods I find the paper to be a strong and useful contribution to the literatureâĂŤit is clearly written, appropriately detailed, and reports an impressive set of model development and data wrangling efforts. GHM of this type are not my primary research focus, so I cannot comment on the completeness of referencing and the place of this paper in the broader

GHM literature, but it appears that the authors have taken care to put their work in the context of related efforts with different models. As a presentation of simulation results I believe the paper succeeds to some extent. The authors present reasonable estimates of water sources at basin and global scale, and they discuss potential sources of error and uncertainty. Perhaps inevitably, however, both model evaluation and quantification of uncertainty are quite thin. As a result one is left with point estimates of large quantities with significant uncertainties, where it would be considerably more informative to have ranges reported on the basis of some kind of quantitative error estimate. That said, I believe that the Discussion Paper is important in that it presents on a large investment in model development for H08, and my comments below are suggestions for improvements rather than requirements for final publication.

Suggestions:

1. P. 4, line 31-2: This is the first instance I noted in the paper of parameters presented as single point estimates with no sensitivity test and little in the way of justification. The same happens at several otehr points in the text, either explicitly or implicitly via citations. This left me wondering how sensitive model results are to the choice of parameters that are, at best, only roughly constrained by data. Later in the paper the authors identify a number of major sources of uncertainty, but parameters within the hydrological model or management modules are not specifically discussed. Would it be possible to perform targeted sensitivity tests of some of these parameters? Not only would this enhance confidence in model results, but it would provide useful guidance for later model development regarding the relative importance of various parameters to model results.

2. p.5, line 27-28: The assumption regarding the division between surface water and groundwater is quite a large assumption, and it doesn't account for regions in which farmers use groundwater to make up for surface water shortfalls. I don't have any better idea about how to deal with these complications in a global simulation, but it would be useful if the authors could spend a sentence or two justifying the assumption

and explaining potential limitations.

3. Table 4 presents some model evaluation, but no significance tests are presented to show whether ALL is significantly different from NAT for each basin, or whether either simulation is significantly different from observation. Please provide tests of significance for these differences, accounting for temporal autocorrelation as appropriate.

4. Irrigated area: Perhaps this is covered in an earlier H08 publication, but how does the model decide on what fraction of area equipped for irrigation is active in any given year? In my own work I've found this to be a challenge, particularly when it comes to interannual variability in irrigation demand under extended droughtâĂŤe.g., when farmers fallow irrigation fields due to water shortage. Is this addressed in the model, particularly when it comes to trends in water stressed regions?

5. Comparisons with GRACE: the authors have compared to a single GRACE product. While I expect that different flavors of the spherical harmonics GRACE simulations will be similar in most basins, the more recent mascon solutions have emerged as likely more reliable for terrestrial applications (http://onlinelibrary.wiley.com/doi/10.1002/2016WR019494/full). The authors should consider adding a mascon analysis to their evaluation, both to quantify observation-based uncertainty and because the mascons might indicate that the TWS trends are actually larger than the spherical harmonics solutions indicate, and are in better agreement with ALL simulation results.

Minor comments:

Abstract: The “R” in GRACE stands for Recovery, not Retrieval.

Section 2.2.2: A few words on the WATCH methodology would be helpful for those of us not familiar with it.

Section 3.2.2: Were scaling factors applied to the GRACE data?

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