

Interactive comment on “An integrated model for the assessment of global water resources – Part 1: Input meteorological forcing and natural hydrological cycle modules” by N. Hanasaki et al.

Anonymous Referee #3

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The authors have attempted to develop (and describe) an integrated model for global water resources management. The model has six major modules and physically models the more important processes that have a direct relevance on the availability of water. Given the global scope of the work, the model development is no doubt very challenging and it is commendable for the authors to tackle it. However, I feel that the the paper, in its current form, is just not reviewer-friendly or reader-friendly to elicit constructive critique of the work towards further improvement. The paper presents an overload of information, modeling anecdotes/problems, in an unorganized fashion without pin pointing the gist and the hypothesis behind the work. Even if all the numer-

ous allusions and model findings are to be taken at face value (which I do since it is impossible for me to cross check individually each reference and verify), I still think that the paper needs to be drastically altered, trimmed and made scientifically focused if it is to be reviewed carefully. So I suggest that the authors spend a good amount of time working on converting their excessively long paper (over 10,000 words).

The following are my general comments that the authors could address while downsizing the paper:

- 1) Clarify who/what will be the biggest beneficiary for the development of such a global water resources management model.
- 2) Why is such a development needed in the first place when we have had a number of models to do the same (even though they may not have seen global application)? For example, as the authors rightly mention - VIC, UNH work based on KINEROS, Coe's global model (not mentioned) etc.
- 3) On 2) - the scientific hypothesis/basis needs to be gistly highlighted. We understand that global calibration is almost impossible given the state of availability of calibration data. But many existing models such as VIC could also be implemented using extensive proxy data from ECMWF, FAO, satellite products etc. It seems that the calibration issue alone is a rather weak reason. Whatever the motivation - I suggest that they be itemized in the form of bullets or identifiable writing that draws the readers attention (instead of getting lost in vast amounts of text).
- 4) The project is too ambitious - I personally think the anthropogenic module is a difficult one to simulate except for very simple cases. And th reality is that regulated rivers do not have simple water regulation any more (like just withdrawal at some variable rate based on demand). Treaties and transboundary issues have a highly complex effect on global water resources management. With so many transboundary basins and lack of basin wide treaties in many places, the effectiveness of a global anthropogenic module is questionable.

5) Hap hazard literature review shows up in many places when the authors' describe a method/module. I think the review needs to be more organized and appear in the beginning.

6) As a bottom line - the authors really need to market their model in a more focused manner, clearly defining the client base of users for such a model (would UNESCO, ADP or the Mekong Secretariate want to use a model?).

7) Provide a flow chart of conceptual linkages (modules) and processes in the model development. This helps the reader understand the work.

8) As for the appropriateness of the paper in HESSD, I am not entirely convinced that it fits completely well within the scope of HESSD. I think there are lots of other avenues out there that focus more on model development than global nature of hydrology where this work could go.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 4, 3535, 2007.

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