

Interactive comment on “An integrated model for the assessment of global water resources – Part 2: Anthropogenic activities modules and assessments” by N. Hanasaki et al.

Anonymous Referee #2

Received and published: 17 November 2007

This manuscript documents a huge effort to establish a global water resources model, which is per se an important contribution to water resources and climate change assessments. However, the present manuscript appears to be way too long, so that it is hard for any reader (and reviewer) to get an idea of the relevant things (in terms of model conception and results) and to sort out the less relevant details. This is especially true since there is a part I to this paper which also contains a lot of model descriptions (so I would expect to see more results in the present part II). The model description gains too much room as compared to the more interesting results (i.e. the comparison of water stress indicators and the emphasis of the seasonal results). Be-

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sides, the authors are generally a bit too defensive as for the shortcomings of their approach; certainly the caveats and compromises need to be discussed and the results compared to previous estimates, but in the present version of the manuscript the "good" things are hidden in extensive descriptions of comparisons and problems. A way out may be to summarize in a dedicated section the potential importance for the global water stress assessment of the simplifications and omissions that had to be made.

In general, I like the Introduction as it sets the stage for a global water assessment that should consider seasonal dynamics – this is a worthwhile research question, but the results presented here are somewhat hidden in the very long model description. I recommend to focus the paper more on these results (sections 5.1 to 5.3, perhaps including an analysis on monthly basis) and reduce the other parts of the paper (model description) to the essential things. Section 5.4 is very short and not very informative – my suggestion is to extend this analysis considerably and combine it with the analysis of the seasonal water stress indicator (recommended) or to leave it out (as well as leaving out the long description of the module in the Appendix B, as it seems that this module is already described in another paper). The analysis of seasonal water stress would benefit if the implications for environmental flow requirements was discussed in more detail (e.g. how would the present CWD indicator look like if the flow requirements were (not) considered? – it is not clear at the moment whether these are considered or not in the preset analysis). Only then it makes sense that this method is described in such a detail here. Also, would it be possible to say anything about the implications of the (diverse measures of) water stress for crop growth? Any such result would justify why the crop module is described so extensively.

In the following I provide recommendations for what passages should be shortened or deleted.

– Section 2.1: There is a need to provide a table with the crop growth and irrigation parameters, this would help a lot in understanding this module (as described in this

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section and in Appendix A). Also, this section contains descriptions of other models that follow different ways to module crops globally – I don't think this is necessary here.

– Section 2.3: It is not necessary to describe the Smakhtin approach in detail, because it is not applied here.

– Section 3.2: many things are said twice here. The passage on p. 3594 (from "One might consider ...") can be completely deleted.

– Section 3.3 (Assumptions): These assumptions should be mentioned where appropriate in the methods section. There is no such special section required, especially since not all assumptions/caveats are considered in that section.

– Section 3.4: The different simulations NAT, CAL and IRG are mentioned, but it is not obvious why these were made. Why not just provide a description of, and results for, the full simulation? Also, the strength of the coupling between the different modules is not very clear: Fig. 1 suggests that it is mostly one-way couplings, except for a feedback of agricultural water withdrawal to soil moisture (which is not explained in the text, at least I do not find it). If it is mostly one-way couplings between two modules, or a cascade of modules, then I would not say that this is an "integrated" model.

– Section 4.1: This section is far too long, the results are described extensively although shown in the figures: Better just mention the highlights, the details can be depicted from the illustrations.

– Section 4.2: Also too long. Also, at least the first sentence of this section I have read somewhere else in the manuscript. Indeed, the whole manuscript needs to be checked for other redundancies and further potential for shortening to make it more succinct.

Further specific comments:

– Section 2.1: This section suggests that there are only the few global crop and water balance models that are mentioned; in fact, there are more : at least the models of Bondeau et al. 2007 and Osborne et al. 2006 or 2007 may be mentioned here (both

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published in Global Change Biology). P. 3587 line 27: It doesn't always start from 0%, does it? How are evaporation, transpiration and interception computed?

– It is not clear how irrigation affects plant growth – does the model allocate the water withdrawal to the fielding order to allow for optimal growth? And what happens if there is not enough water stored in rivers and reservoirs?

– page 3595 line 9: What are primary and secondary crops?

– page 3596 lines 6-8: Was there a criterion to restrict the growing periods to a certain length?

– page 3598 line 15: Are the results shown, i.e. the improvement by irrigation?

– page 3599 line 18: Is there an explanation for the misestimates in warmer countries?

– Fig. 2: Which simulation is this, the full one? Also, it is not astonishing that some of the dots lie outside the observed range, because the observations refer to country-scale values. The text suggests that these are bad estimates, but in fact this may not be the case (since the country average consists of diverse values within a country). Better plot the simulated country totals rather than the individual cells' values?

– Sections 4.3 and 5.1: Note that differences to other studies are probably also due to the fact that you used other climate input.

– Table 4 is very short and all results are mentioned in the text, so delete it.

– page 3604 lines 2-6: This interpretation is rather weak. Lines 19-26: I do not understand this paragraph.

– Fig. 5: Please use different colors and more classes – the red tones are not distinguishable (it's either red or white). The last sentence of the figure caption is already mentioned in the text, avoid these doubles.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 4, 3583, 2007.