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Hydrology and Earth System Sciences Discussions

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.

N. Hanasaki et al.

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Thank you very much for your comments. We are very pleased to have a number of constructive and useful suggestions.

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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 es-

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pecially 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).

> We understand your concern that our manuscript is too long to grasp the whole picture of our research. We are grateful that you suggested a number of detailed and concrete ideas to shorten our manuscript. Our general policy for revision is: 1) cut redundant description on model and its validation as much as possible; 2) move them to the appendix part if necessary; 3) focus more on the results and discussions of model application.
>
> Please notice that some of the reviewers concerned the validity and feasibility of modeling anthropogenic activities. Therefore, we need to leave a considerable amount of descriptions how we modeled, and how it performed in order to convince readers.

Besides, 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.

> Thank you very much for your encouraging comments and good suggestions. We will add a section you mentioned.

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

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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.

> We analyzed the impact of reservoir operation module and environmental flow requirement module on global water resources assessments, by turning off these two modules respectively and comparing the results with that of the control simulation. The results are summarized in Section 5.4 and typically Table 5, however, as you pointed out, the discussions might be too short. We will expand this sub-section in revised manuscript. It will answer your question.

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.

> As we described in the conclusion section, we believe that simulated water stress for crop growth can be a useful indicator in water resources assessment (Section 6, 2nd Paragraph). However, for this application, we

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need considerable additional works on the crop growth module to validate crop yield response on water deficit. Therefore, we just mentioned this topic as a future research need. Nonetheless, the crop growth module is quite important in our integrated model, because it estimates planting date, harvesting date, and growing period. These variables critically influence the amount and timing of irrigation water requirement.

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 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.

We will add parameter lists for crop growth module. About the review part, we will delete, or move to appendix.

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

The environmental flow requirement module which we used is firstly appeared in journal publication. Since the estimated environmental flow requirement is quite different from that of the pioneering work of Smakhtin et al. (2004), we need to mention it. Because we also understand your concern, we will move this comparison to the appendix.

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

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Actually, this question was raised by a well known global hydrologist when we discussed this model. The balance between spatial and temporal resolution is quite important in hydrological simulations. We will revise this part to make it clear, but we want to leave it.

- 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.

We will move the first two assumptions on withdrawal to "2.4 Anthropogenic water withdrawal module" section, and the last assumption on crop type to "2.1 Crop growth module" 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.

First, the NAT simulation generates the model climatology of some key variables, the CAL simulation generates the crop calendar, and the IRG simulation generates the potential demand of irrigation water. The outputs of these simulations are indispensable to the full simulation, and described in detail in Section3.4. We will revise this part for further clarity.

Second, Fig. 1 shows the water flow of a single grid cell. Notice that our model is a grid-based hydrological model, and a large number of basins

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consist of more than one grid cells. The withdrawal or reservoir operation in the upper stream influences the water availability of the lower stream. This process creates feedbacks among cells and modules. We will add this discussion to the text. Also, as we mentioned in Part 1, we integrated natural hydrological cycle and major anthropogenic activities processes, and this is why we call our model integrated. (Part 1, Introduction, 2nd paragraph)

- 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.

We will trim down these sections as much as possible, and just mention the highlights.

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 published in Global Change Biology).

Thank you. We will cite these works.

- P. 3587 line 27: It doesn't always start from 0 percent, does it? How are evaporation, transpiration and interception computed?

Yes, if the soil moisture is above the target content, irrigation water is not applied. We will add this point to the manuscript. Because the land

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surface hydrology model is based on the bucket model, evaporation and transpiration are unified, and interception is not considered.

- 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?

In our model, water deficit affects the growth of biomass, and consequently, the harvest. Irrigation is applied to keep soil moisture of irrigated area at 75 percent of the field capacity throughout a cropping period.

In reality, crop growth changes LAI, roughness length, albedo, etc., and they influence evapotranspiration, although this feedback is NOT considered in our model. The roughness length and albedo is independent to irrigation application in our model. We will add this point to our manuscript (Appendix A).

If there is not enough water, the applied irrigation water falls below demand, and finally, the CWD index (an indicator for subannual basis water resources assessment) of the grid cell decreases.

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

They are described in "3.3 Assumption" subsection. We will move this part to "2.1 Crop growth module" subsection in order to concentrate all assumptions on the crop growth module in a single section.

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

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We set only a criterion that the cropping length must be less than 1 year (365 days). In a small number of grid cells, mainly in mountainous area, the growing periods become very long (larger than 200 days), but we left as is, because of the lack of reliable observation data. We will add this discussion to the text.

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

No. The original non-irrigated results are shown. We will emphasize this.

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

Air temperature is an important clue to narrow down the potential cropping period. In warmer countries, however, air temperature is above the base temperature during a year, and can't use as a clue. We will add this discussion.

- 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 countryscale 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?

First, let us describe what Fig 2 shows. Fig 2 shows the simulated harvesting date and planting date, both are the outputs of SIM simulation. This planting/harvesting date information is common to IRG and FULL simulation. As described in the caption of Fig 2, the green dots show the simulated

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planting date of individual grid cells in a country, and the green box shows the observed range of planting date. If the green dots are within the green box, then it suggests that the estimated planting date is within the observed range.

Second, let us explain why we showed the range, not a single-country averaged value. As is shown in Fig 2, the observation has wide range in both planting and harvesting date, reflecting the climatic variations in the countries. We showed the variability of both observation and simulation.

- 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.

Thank you. We will add it to the text.

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

We will put the all information in to the text and delete Table 4.

- page 3604 lines 2-6: This interpretation is rather weak.

We will add more discussions on the interpretation of the global distribution of CWD index.

- page 3604 Lines 19-26: I do not understand this paragraph.

What we wanted to say is that CWD is more informative than CQ90. We will revise this paragraph.

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- 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.

> We will change the color scheme of Fig 5. We will delete the final sentence of the figure caption.

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

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