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Hydrology and Earth System Sciences Discussions

Interactive comment on "A global water scarcity assessment under shared socio-economic pathways – Part 2: Water availability and scarcity" *by* N. Hanasaki et al.

Anonymous Referee #3

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With interest I have read this manuscript which is overall well-written, well-structured and clear. The paper is among the first to use the shared socio-economic pathways to assess the impacts of global change on water resources but otherwise follows the methodology of earlier assessments closely. Overall, it is well-suited for publication in HESS. Still, the paper has some weaknesses that should be addressed before the paper can be published. But none of these impair the validity of the paper and therefore a moderate revision is in order, considering the following points:

1. It is understandable that the authors split the assessment in two parts. However,

this obscures the consequences of the non-quantitative aspects of the SSP scenarios for this assessment (13935, line 25 ff). It would be good if this influence could be substantiated in this paper. Without clarification the claim that this paper is different other than assessments with the previous generations of SRES scenarios does not hold as these scenarios do also include a narrative of social-economic change and quantified by e.g., population growth and GDP development;

2. The statement on previous assessments only using population and GDP (p13935, 24) and the use of an annual time scale (p 13936, 1) need to be substantiated by references. Note that the latter contradicts the later reference to the work by Alcamo, Hoekstra, Wada and their co-workers;

3. The paper refers both to consumption and withdrawal and uses the latter to assess water scarcity, if correctly defined. But this quantity is the gross abstraction from the stream. Thus, daily water scarcity, as assessed in this paper, is an overestimation as it ignores the return flows. In order to get a more realistic estimate, return flows need to be included on an appropriate time scale (e.g., applied irrigation water percolating to the groundwater) and the non-consumed water added to the stream flow. It is crucial that the magnitude of the return flows involved is estimated robustly in light of the changing environmental consciousness (e.g., environmental flow in Figure 1) or changing water scarcity. At least, the underlying simplifying assumptions, their validity and sensitivity of the outcome should be discussed;

4. The cumulative withdrawal to demand ratio helps to overcome some of the limitations of the water scarcity index in assessing the temporal characteristics of water scarcity. The CWD itself is not free of limitations either, as it is fixed to an arbitrary start date and does not reveal the impact of the incurred water shortage and the recovery from it. Besides the oversight of alternative water resources such as groundwater, the CWD also ignores the technological possibilities to fully exploit stream flow. This results in a larger availability that may offset partially the overestimation of water use that is incurred by using the withdrawal but this is fortuitous at best. It can be argued that

while the WWR is less powerful, it partly compensates for these aspects by using the arbitrary limit of 40%. As such, the superiority of the CWD over the WWR is not obvious and largely dependent on the way how the latter one is applied (p. 13960, 20);

5. Do the changes in the environmental flow conditions/withdrawals affect the flood wave propagation in this study? Or is the timing with which it travels downstream is independent of the changing resistance and gradient along the stream?

6. Given the above discrepancies in the analysis, it may be worthwhile to consider model uncertainty in addition to scenario uncertainty. Although its contribution to the overall uncertainty may be small, as the authors imply, it would be an advance if this part of the uncertainty could be formally identified;

7. The choice to restrict the visualization of spatial data to merely one GCM is understandable but it is unclear to me whether the local differences may not be larger for one of the other GCMs as both precipitation and potential evaporation are affected and runoff scales accordingly as a function of soil moisture. It would be informative if the regional differences between the scenarios could be highlighted;

8. The statement (p 13959, 14) that "preparing scenarios for these terms is very challenging and maybe impossible" seems a bit far-fetched given the large steps that are taken elsewhere in this paper to translate the SSP scenarios into parameterization. Also, it makes one wonder that on this ground an important resource as groundwater can be left out of the equation;

9. In a similar vein, what other parameters than those listed in Table 3 are used and how were these derived (e.g., soil)?

10. While this paper uses new scenarios, it does not address the question on what these new scenarios add to our knowledge on the impact of global change. While hard to compare, it would be good to show what the gain in information is that stems from this exercise. Do these new assessments show a new direction or magnitude

of change and are these different –in terms of confidence limits- of earlier estimates based on SRES scenarios? Overall, the differences in climate change between the SRES-based estimates and the newer RCP ones are not that large.

All-in-all, this paper is interesting but needs some additional explanation and discussion with regards to a number of choices made if it wants to substantiate the findings that arise from applying these new scenarios of socio-economic and climatic change.

Good luck with your revision

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