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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 #4**

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The paper, the second part of a two-part paper, presents an assessment of global water scarcity using a global hydrological model H08. This study uses the latest climate projections from CMIP5 and socio-economic scenarios SSP1-5. To my knowledge, this is the first study that incorporates SSP scenarios and CMIP5 climate projections to quantify the number of global population under water scarcity. The manuscript is overall well-written based on good modeling efforts. However, I do have some concerns regarding the methodologies used in the paper and the presentation as detailed in the following.



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Discussion Paper

1. As pointed out by Referee #3, splitting the assessment in two papers obscures the overall aims of the papers and the consequences of the water use scenarios based on SSPs that are developed in the first part of this two-part paper. The authors should consider to merge this two-part paper into a single paper. In the first paper, the development of water use scenarios is presented, but substantial parts of the methodologies (paper 1) are mostly literature review in relation to previous modeling efforts to develop water use projections (they can be in supplement). These parts can be considerably shortened. Based on previous studies, the authors used a revised approach to project water use, but novel aspects of the paper 1 are limited. Merging this two-part paper into a single paper describing new aspects of global water scarcity assessments will make a paper much stronger and novel. Many parts including literature review in the first paper can be omitted or shortened, and some parts in the second paper can also be considerably shortened, e.g. literature review on global water scarcity, the assessment of global water scarcity with WWR ratio.

2. As also suggested by Referee #2, a literature review on global water scarcity assessment (2.1) and water scarcity index (2.2) can be substantially shortened or mostly unnecessary. The Cumulative Withdrawal to Demand (CWD) ratio has already been extensively discussed in Hanasaki et al. (2008a,b).

3. How do you treat missing values in industrial and domestic water withdrawals that are often present in the FAO AQUASTAT data base? And please change to irrigation water withdrawal/consumption if livestock sector is not included (confusing).

4. Some of the non-meteorological variables are based on previous studies that were published nearly a decade ago, e.g. irrigated area, irrigation efficiency. Why not use updated or the latest data? What is the motivation using those data?

Irrigated area, crop intensity: Portmann, F., S. Siebert, C. Bauer and P. Döll (2010), MIRCA2000 - Global monthly irrigated and rainfed crop areas around the year 2000: a new high-resolution data set for agricultural and hydrological modelling, Global Biogeo.



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Cyc., 24, GB1011.

Irrigation efficiency: Rohwer, J., D. Gerten, and W. Lucht (2007), Development of functional types of irrigation for improved global crop modelling, PIK Report 104. Potsdam Institute for Climate Impact Research.

5. The author used 0.10 and 0.15 respectively to convert potential water withdrawal demand for industrial and municipal to a consumption from the work of Shiklomanov (2000). These values are generally too optimistic for developing countries with limited water recycling technology. Are there any country statistics to support these values? Also, are these values subject to change during the future simulation? In principal, these values (water recycling) should improve along with economic and technological advancement. Is technological improvement reflected in water recycling ratios? Or the authors used the fixed ratios throughout the simulation? The authors should clarify this point and at least describe the assumption and uncertainty therein.

6. Section 4.4, 6.5, and Appendix A are unnecessary. What is the motivation using the annual WWR ratio to assess the number of global population under water scarcity, having known that this ratio neglects seasonality and underestimate the population under water scarcity, which is already discussed in Section 2 and Hanasaki et al. (2008a,b)? Also, the difference and relationship between WWR and CWD have been already extensively discussed in Hanasaki et al. (2008a,b). The authors should exclude the WWR ration from their analysis, and focus on the CWD ratio and the findings.

7. 'Section 6 Uncertainty' reads more discussion rather than describing the uncertainties of each simulation and model component. The authors should change to '6 Discussion' unless some quantitative uncertainty measures are provided.

8. The authors should at least briefly describe any improvement obtained from the overall analysis with the latest CMIP5 climate projections and SSP scenarios compared to previous water scarcity assessments that are based on the IPCC SRES scenarios.



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Minor comments:

Page 13938L20-21: “monthly discharge is higher than the Q90 value for 90% of the time” should be “mean monthly discharge that is exceeded 90% of the time”?

Page 13947L15-18: “The results were converted into the withdrawal base by using water use efficiency which is the ratio of water consumed over water withdrawn including return flow and delivery loss. H08 simulates the former, while the latter is widely recorded allowing us to convert water consumed into water withdrawn.” The conversion using irrigation efficiency is already described in Section 3.3. Please remove these sentences.

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