Response to Reviewer #3

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

Thank you.

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;

Although the implications of water resources assessment for each SSP are shown in Section 5.3 (discussion on implications), the relationship between water scarcity and narrative scenario is not very sufficiently discussed in the current form of draft. We are going to add discussion in Section 5.3. One of the fundamental differences between SRES and RCP/SSP is that the former fixes the combination of socio-economic and emission scenario one by one, while the latter allows flexible combinations of them. This enables us to assess the challenge of mitigation and adaptation under various socio-economic conditions and climate stabilization targets. This study developed a scenario matrix (i.e. combination of climate and socio-economic scenarios) shown in Fig 3 of Part 1, and showed the results of water scarcity assessment in Fig 10-12 and Table 14 for every combinations. Because this point is also not effectively emphasized in the current text, we are going to add description in Abstract, Introduction, Discussion, and Conclusion.

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;

We are going to revise this part so that readers would not misunderstand anything. What we wished to convey here are two methodological characteristics of this study. One is we boldly interpreted the narrative scenario of SSP. Actually, we partly supplemented information in order to develop water use scenarios. The other is we utilized the CWD index which is suited to assess the impact of climate change on hydrology and water use.

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;

Reviewer #2 gave us a similar comment. The term "withdrawal" is currently used in two meanings in this paper. One is abstraction of water from river channels in the H08 model for <u>consumptive</u> water use. The other is the standard use of this term, taking water from various sources including return flow. Because it is quite confusing, we are going to term "water abstraction" for the former, and strictly distinguish from the latter. Note that the former was used for CWD calculation and the latter for WWR. Therefore, return flow was not included in the CWD index and water abstraction was not overestimated.

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);

CWD is not a perfect index, and includes some shortcomings. They are intensively discussed in Section 6.5 (uncertainty of index). It is already mentioned in the section that groundwater withdrawal is not included in the current simulation. We are going to further add limitations of CWD in Section 6.5 which Reviewer #3 mentioned, such as neglecting of technical potential of water withdrawal, and not specifying the period of water shortage.

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?

The river sub model used in this study is a simple one (well described in Oki et al., 1999), and the flow velocity was neither affected by water withdrawal nor settings of environmental flows. We are going to add this in Section 3.1 (introduction of model).

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;

The uncertainty in the H08 model should be discussed as well. However, in order to investigate it, we need to add considerable amount of sensitivity tests because this study already covers vast area of hydrology and water use. Although quite important, we believe this is beyond the scope of this paper. We just mention the needs of model uncertainty analysis and sensitivity tests in Section 6.4 (uncertainty of model).

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;

In order not to increase the number of figures, we only showed the results of MIROC-ESM-CHEM in the current form of paper. Because we will not be able to triple figures (i.e. we used three GCMs), we are going to add discussion on the differences of GCMs and their consequences in Sections 4 and 5 (results and discussion).

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;*

What we meant here was it is quite difficult, if not impossible, to know the exact location and period of individual projects of reservoirs and irrigation in the future. We are going to revise this part so that our intention is clearly conveyed.

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

We used the default parameters of the H08 model for this study which is shown in our previous work of Hanasaki et al. (2010). We are going to add this in text.

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.

The key characteristics of using SSP/RCP are as described in 1). Additionally, RCP enables us to conduct climate stabilization experiments, which was quite difficult under SRES scenarios. We are going to add this in Introduction.

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

Thank you. We believe our manuscript will be substantially improved by your helpful comments after revision.

Reply to Reviewer #4

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

Thank you.

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.

Reviewer #4 mainly raised two points here. One is the objective of this work becomes obscure because we separated our work into two papers. The other is the paper contains less novel information but more literature reviews. These concerns are well taken, but due to the reasons below, we wish to keep the paper double-featured, and not to remove literature reviews too much.

First, we separated our work into two papers mainly three reasons. First, the total number of words is more than 20,000, and it is apparently too long for one paper. Second, Part 1 (projection of water use) and Part 2 (water resources assessment using the projected water use) are closely related each other but having different key drivers (i.e. water use is primarily driven by socio-economic factors, while hydrological cycle is by climatic factors). Third, partly because of the difference in key drivers, we believe that the readership of two parts may be slightly different. Part 1 reports one of the first examples of interpretation and application of SSP scenario. This may attract integrated assessment modelers who developed SSP, and climate change impact assessment modelers who potentially interested in using SSP. On the contrary, Part 2 focuses more on the advancement of climate change impact study on global water resources. This may attract hydrological model developers who are interested in advantages and limitations of techniques, and water resources experts who are looking for the latest water scarcity assessments. We would like to provide sufficient information for both of the potential readers. One of the reasons why the objective looks obscure could be, as Reviewer #3 pointed out, the results of Part 2 was not clearly related to the narrative scenario of SSP which is the core of Part 1. We are going to add this discussion in Section 5.3 (discussion on implications).

Second, please note that we didn’t necessarily select the latest and most complex modeling techniques for this study. Rather we carefully chose the best ones for SSP. Indeed, in some cases, we needed to select simple and robust models for limited input data. We are going to shorten text in order not to give an impression of redundancy, but we believe fundamental modeling backgrounds should be shown as well.

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).

We are going to further revise Section 2 not to give any impression of redundancy. One of the key objectives of Part 2 is to show the advantage of CWD in climate change impact assessment compared to WWR. As Reviewer #4 pointed out, CWD was first proposed in Hanasaki et al. (2008b) and already applied to the historical period of 1986-1995. However, CWD has not yet been applied to climate change impact assessment, and we believe this is the first paper reporting it.

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).

As Reviewer #4 pointed out, AQUASTAT frequently contains missing numbers. We added text how we dealt with these missing numbers in Section .We also change the term agricultural water into irrigation water, because it doesn't include live stock sector.

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. 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.

We referred Döll and Siebert (2002) for the global distribution of irrigation efficiency and crop intensity. We have noticed the new datasets of Portmann et al. (2010) and Rohwer et al. (2007) suggested by Reviewer #4. The new dataset will be examined and utilized in the near future, but according to our preliminary sensitivity test, the use of new datasets has limited effect to the overall results of water scarcity assessment.

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.

Thank you. We fixed the ratio of water consumption to water withdrawal for industrial and municipal water at 0.10 and 0.15 throughout this study. This is due to the lack of water consumption data of these sectors. However, we totally agree with Reviewer #4 that this is a quite important assumption of this study. We are going to add discussion in Section 3.3 (non-meteorological data and scenario).

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.*

As mentioned above, the advantage of CWD cannot be effectively discussed without showing the results of WWR. We are going to add explanation why we need to show WWR in Introduction and Results Sections.

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.

We are going to consult English Editor what would be the most appropriate title for Section 6.

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

A similar comment was given by Reviewer #3. We are going to add discussion on the fundamental differences between SSP/CMIP5 and SRES/CMIP5 in Introduction and Conclusion.

About two minor comments, we are going to correct them accordingly.