Response to Reviewer's comments on submission to *Hydrol. Earth Syst. Sci. Discuss., 8*, 7399-7460, 2011 (doi:10.5194/hessd-8-7399-2011)

Note: textual remarks, inconsistencies and minor errors have been updated in the new text wherever applicable. References refer to those used in the manuscript.

First of all, we would like to thank the Anonymous Referee #2 for his/her valuable comments and thoughtful suggestions, which helped to significantly improve the quality of this paper. Our detailed responses to the comments of the Referee #2 are presented below.

Response to major comments raised by the Anonymous Referee #2:

Referee's comment (1): The methodology section (2) is rather lengthy and could be shortened. For example, section 2.6.2 can be shortened, and only the updates over Wada et al. (2010) can be highlighted.

Response to the comment (1): As suggested by the Referee, we have revised the methodology section and moved parts of the methodology section including Table 2, 3, 4 and 7 to an appendix. However, Sect. 2.6.2 describes a new approach to downscale country groundwater abstraction rates to 0.5^{o} spatial resolution. We have shortened the first paragraph of Sect. 2.6.2 but kept the remaining part as it describes a new approach.

Referee's comment (2): Eq. 1, section 3.4, and Table 9: you compare your results with Kummu et al., (2010) which, as you have indicated, is the only study assessing the past development of water scarcity. However, there are numerous studies on water stress around the year 2000 or the mid-1990s, e.g., Vorosmarty et al., (2000), Oki and Kanae (2006), Hanasaki et al. (2008b), Alcamo et al. (2007), all listed in the references. As you account for desalinated water and groundwater abstractions in calculating water stress (Eq. 1), I would expect your estimates of population living in water-stressed areas would lie on the lower limit of these previous estimates. However, your value of 2.6 billion (43%) is well above the most previous estimates. Please compare your results for the year 2000 with the above listed studies in section 3.4, and also add discussion in the final section.

Response to the comment (2): As suggested by the Referee, we have added a comparison between our estimates and previous estimates for the year 2000. Our value of 2.6 billion is indeed higher than those of previous studies even though we have accounted for desalinated water use and groundwater abstraction. The main reason for this is that we have computed water stress per month rather than per year. As a result, our sub-annual assessments capture seasonal variations of water stress and return higher values than annual assessments as also indicated by Wada et al. (2011) (see Table 1 and 9). We have added further texts to clarify the difference.

Referee's comment (3): Section 2.5 and Table 3: it would be interesting to see how the use of desalinated water affected your WSI, particularly in countries using huge amounts of desalinated water such as the Saudi Arabia.

Response to the comment (3): We concur that desalinated water use might have a large impact on water stress, particularly in a country such as Saudi Arabia. We include an analysis to quantify the impact of desalinated water use on water stress in an appendix.

Referee's comment (4): Section 2.6.1: what is the unit of RIrr? In Eq. 7, why do you multiply the latter term by Tirr,i? I wouldn't know but the first term here is a flux (per day) and the latter one is given in terms of total volume for the cropping period. Please make sure that it is correct. I would suggest using 'irrigation return flow' rather than 'artificial recharge' because apparently only irrigation is considered here. Also, P7415,L1: irrigation water infiltrates at a rate of unsaturated hydraulic conductivity of the top soil layer, but recharge to the groundwater storage (gravity drainage) would be equal the unsaturated hydraulic conductivity of the bottom soil layer, am I right? Going back to P. 7410, L20: how do you consider the domestic/industrial return flows? Are they directly added to surface runoff?

Response to the comment (4): The unit of R_{Irr} in Eq. 7 is m^3 day^{-1}. We have left out T_{Irr} from Eq. 7 as the latter term should also be m^3 day^{-1}. We agree that the term 'artificial recharge' is ambiguous. We have replaced 'artificial recharge' to 'additional recharge from irrigation' throughout the revised manuscript. Recharge to groundwater should indeed be equal to a rate of unsaturated hydraulic conductivity of the *bottom* soil layer (we have used the rate of unsaturated hydraulic conductivity of the *bottom* soil layer for our calculation). We have corrected the text. For return flow from domestic and industrial sectors, we computed the amounts by using recycling ratios (see Sect. 2.3.3) and calculated net water demand by subtracting the amounts of return flow from gross water demand. We then subtracted local net water demand from local runoff to compute downstream blue water availability. This subsequently simulates the effect of upstream water consumption and reduction of downstream river discharge (see Sect. 2.4). The part of gross water demand which subsequently returns to river system is thus not subtracted from surface runoff in this study.

Referee's comment (5): Table 8: what are the differences here with the previous estimates (Wada et al, 2010)?

Response to the comment (5): Compared to the previous work by Wada et al. (2010), we included additional recharge from irrigation in our recharge estimate. This increases global recharge by 420 km^3 yr^{-1} and reduces the amount of non-renewable groundwater abstraction from 309 to 275 km^3 yr^{-1}. This improvement subsequently mitigated or removed some of hotspots, notably in South California and along the Indus, where the amounts were overestimated by Wada et al. (2010).

Referee's comment (6): Section 3.5 and 3.6: in these sections, you highlight the climate- and anthropogenically-driven water scarcities and discuss the historical drought events. As you have pointed out, in many emerging economies, and also globally, irrigation water demand is the major cause of heightened water stress. Irrigation demand simulated by your model is largely dependent on precipitation received during irrigation period. In that sense, the anthropogenic cause here is not totally independent of the climatic causes. Please note this limitation with some discussion. Also, specify the definition of drought in your study.

Response to the comment (6): We performed two simulation runs for computing water stress to distinguish the impacts of water demand and climate variability (see Sect. 2.2). The first simulation was done with transient water demand (1960-2001) and water availability (1960-2001) and the second simulation was done with water demand of 1960 and transient water availability (1960-2001). For the second simulation with the water demand of 1960, we computed irrigation water demand for the irrigated areas of the year 1960 but with inclusion of long-term climate variability (1960-2001). As a result, irrigation water demand varies over the period 1960-2001 due to changing climate conditions (i.e., precipitation and green water) while industrial and domestic water demand remain same over the period. This means that differences of irrigation water demand between the first and second simulation are caused by irrigated area changes over the period. Thus, the difference in magnitude of water stress between the first and second simulation is resulted by the difference in anothropogenic causes (i.e., irrigated areas, population and economic development). In some regions and countries such as Kerala (India), Turkey, Romania, Bulgaria and Cuba (see Fig. 11), water demand increased rapidly over the period 1960-2001 and heightened water stress on top of water stress caused by climate variability. We have revised the methodology section (Sect. 2) to clarify our method relevant to this point as it was not entirely clear in the manuscript. In addition, we have revised Sect. 3.6 and clarified the terms in relation to drought, water shortage and water stress. We have also added a paragraph to discuss the limitation in comparisons between simulated water stress and observed droughts.

Response to minor (editorial) comments raised by the Anonymous Referee #2 (reviewer's comment in italics, changed text between quotation marks):

(1) P. 7401, L2: why (e.g., dams)?: can't reservoirs and dams be used synonymously?
We have removed the phrase '(e.g., dams)' and used reservoirs throughout the revised manuscript.

(2) P. 7401, L8: withdrawal: is it same as the 'gross demand' defined in pp. 7403 L.2? They have been slightly touched upon in Fig. 2, but clarification in the text would be appreciated. In Fig. 2, what does 'actually available to satisfy requirements' mean? Only from surface sources or including desalination and groundwater?
Withdrawal is the amount of water that is actually extracted from available water resources including surface freshwater, groundwater and desalination, part of which is consumed or returned. Demand, on the other hand, indicates only potential withdrawal or consumption regardless of available water resources. In many (semi-) arid regions, potential demand can not be satisfied due to limited available water resources and only part of demand that can be met by available water resources is actually withdrawn. In this study, we rather use the term 'demand' to indicate that we can only estimate potential use. We have added further explanations for the term 'demand' in the introduction section (see Page 7403).

(3) P7407, L1: please confirm that all values from previous studies in Table 2 are correctly listed.
We have confirmed that all values in Table 2 are correctly listed.

(4) P7415, L1-2: 'groundwater abstraction is somewhat uncertain' contradicts with 'Estimated groundwater abstraction is subject to large uncertainties' in P7428,L20. I

think groundwater abstraction is largely uncertain. Here, You may also want to compare your results (Table 4) with the recent statistical and model-based estimates by Konikow (2011) and Pokhrel et al. (2011), respectively.
We have revised the phrases ‘somewhat uncertain’ to ‘highly uncertain’. We have added comparisons between our estimates and those of recent studies by Konikow (2011) and Pokhrel et al. (2011).

(5) Fig. 3: I would not know if it is printing issue, but the symbols denoting different years are hard to differentiate. Also, adding 1:1 line in each panel would be appreciated.
We have revised Fig. 3 to make it clearer and added 1:1 line in each comparison.

(6) P7419, L16: Why is your irrigation demand lower?
Our irrigation water demand is lower compared to that of Shiklomanov (2000a,b) due to larger part that is met by green water availability (about 50%). The ratio of 50% is consistent with other studies (see Table 2).

(7) P7420, L2: does it mean that 420 km3 (out of the net irrigation water use of 1376 km3 in Table 6) returns to the groundwater systems? In other words, (1376-420) km3 is lost as consumptive use? Please clarify.
We have added a sentence ‘420 km^3 out of 1376 km^3 returned to groundwater as additional recharge.’