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 #1 for his/her constructive comments and insightful suggestions on our paper. They helped us to substantially improve the quality of the manuscript. Our detailed responses to the comments of the Referee #1 are presented below.

Response to major comments raised by the Anonymous Referee #1:

Referee's comment 1: Your analysis strongly focuses on the grid cell scale, which implies that all the water needed by the population is available in the very area (the grid cell) where they live, which is not necessarily the case. Thus, the stress may be overestimated in some regions, e.g. where water is transferred over distances larger than a grid cell. Please discuss this issue (though it may well be that your indicator is not strongly affected by this, since you explicitly consider the actual water extraction). You may even show water stress maps with values aggregated to countries (especially since you discuss country averages in the second part).

Response to the comment 1: We agree that the (inter-basin) water diversions can be important for some regions, e.g. aqueducts in India and Central Valley Project in California to reduce the magnitude of local water scarcity or stress. We also agree with the Referee that our results may not be strongly affected by this. For instance, over India water stress per grid cell is generally very high and blue water availability in surrounding grid cells (e.g., 3 by 3 grid cells ≈ 150 km by 150 km) is also extremely limited or infeasible to provide additional water availability due to large local water demand. Water diversions, however, may have some influences on simulated water stress in other regions (e.g., USA). Yet, data for such information is very limited. In addition, it is difficult to assess the amount of water actually transferred by canals and aqueducts from their maximum capacity, e.g. Periyar Project in South India: a maximum capacity of 40 $m^3 \cdot s^{-1}$, Kurnool Cudappah Canal in South India: a capacity of 85 $m^3 \cdot s^{-1}$, Irtysh-Karaganda Canal: a maximum capacity of 75 $m^3 \cdot s^{-1}$ (World Bank, http://www.worldbank.org/; UNDP, http://www.undp.org). As a result it is difficult to incorporate water diversions in a consistent manner over the globe. We include this as a limitation to our study in the discussion section. In addition, we have provided a Table which includes country-averaged water stress values in an appendix as suggested by the Referee.

Referee's comment 2: Please also discuss the fact that you consider the blue water only, which may bias the estimate as well. In addition, it is a bit problematic that you do not consider non-renewable water resources in the indicator (thus underestimate local stress); indeed, the water stress indicator could adopt values larger than 1 if this was considered, which if shown would highlight areas where non-renewable/non-sustainable water resources are being consumed. This needs to be justified and defined more clearly.

Response to the comment 2: We agree that this is a very important point. We have indeed assessed water stress for blue water only. The main reason for this is that the absolute amount of available non-renewable groundwater resources per grid cell is unknown (the global data does not exist.). We know only the annual rate of non-renewable groundwater abstraction per grid cell from our calculation (see Sect. 2.6). As a result, we have subtracted the annual amount of non-renewable groundwater abstraction from annual net total water demand to compute annual net total blue water demand and yield blue water stress (see Eq. 1). We have added a sentence "Since no information on absolute amounts of non-renewable groundwater resources are available, we do not include non-renewable groundwater resources as a water availability." to clarify our method.

Referee's comment 3: And finally, livestock water demand is defined quite conservatively, as you only consider drinking water (while e.g. field where feed is produced may be irrigated); please emphasise this.

Response to the comment 3: As stated by the Referee, we have computed livestock water demand based on drinking water requirements per livestock type. Water demand for irrigated pasture or fodder grasses for feeding livestock has been included in computed irrigation water demand based on the MIRCA data set (Portmann et al., 2010). We have added a sentence "Water demand for irrigated pasture or fodder grasses for feeding livestock is included in irrigation water demand (see Sect. 2.3.2)." to clarify this point in Sect. 2.3.1 (Livestock Water Demand).

Referee's comment 4: You show trends in water stress including effects of demand from all sectors. This is fine, but can you say more about the individual contributions of industry, agriculture and households to these trends, in the different regions?

Response to the comment 4: We concur that the contribution of sectoral water demand to trends in water stress is an important aspect on our assessment. We have added an analysis of sectoral water demand on water stress and described the trends for the period 1960-2000 for different regions.

Referee's comment 5: In addition, and more importantly, I am a bit worried about your definition of "drought", which you mix up with moderate/high "water stress" as quantified by WSI. These phenomena (high abstraction vs. drought) may, however, not always be congruent. Please discuss this, and be careful in comparisons with other studies like the one from Stahl et al. (2009) who may have defined drought totally differently.

Response to the comment 5: We agree that the definition of drought is ambiguous, particularly in relation to moderate/high "water stress". Drought can generally be classified into four categories; meteorological, hydrological, agricultural and socio-economic drought. Our simulated water stress is comparable to a combination of meteorological, hydrological and socio-economic drought as we consider precipitation, river discharge and water demand to compute water scarcity index. We have revised Sect. 3.6 and clarified the terms (e.g., 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.

Referee's comment 6: Moreover, it is unclear methodologically how you come to the often-stated conclusion that climate often has onset (meteorological) droughts (which then have been intensified by high water withdrawal)?

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). First simulation was done with transient water demand (1960-2001) and water availability (1960-2001) and second simulation was done with fixed water demand of 1960 and transient water availability (1960-2001). While climate variability remains same for both simulations (as we used the same water availability), the difference in magnitude of water stress between the first and second simulation is thus resulted by the difference in water demand between the first and second simulation. 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 descriptions relevant to this point throughout the revised manuscript to clarify our method.

Referee's comment 7: Some sections and tables could be left out, shortened, or moved to an appendix, as the methods part is very long and many many numbers are provided, so that the main messages are partly hidden in loads of other material. Section 2.6.1, 2.6.2, 3.2 and tables 1 to 4 (showing results from other studies for the present) and 7 (largely covered by Fig. 3) are such candidates. Figures 3 and 7 are also not need, as it would be more informative to see whether the trends that you computed agree with other studies, not just the values around year 2000. I suspect that such validation plots are already shown in your earlier papers(?).

Response to the comment 7: As suggested by the Referee, we have moved Table 2, 3, 4 and 7 to an appendix. We have kept Fig. 3 as this shows correlation between computed gross water demand and reported water withdrawal taken from the FAO AQUASTAT data base for the period 1970-2000. We have also kept Fig. 7 as this shows correlation between computed gross and net total water demand and estimate water withdrawal and water consumption taken from Shiklomanov (2000a,b) for the period 1960-2000. These validation results have not included in our previous works and we think they are relevant to show that our method to reconstruct past water demand produces a reasonable result despite uncertainties caused by our assumptions to overcome a lack of historical data.

Response to minor (editorial) comments raised by the Anonymous Referee #1 (reviewer's comment in italics, changed text between quotation marks):

1) The entire text should be checked for typos and grammatical errors. For example: "account for" not "account"; "Mexico is characterized" not "Mexico characterized"…
We have corrected typos and grammatical errors if any throughout the revised manuscript.

2) Abstract: Please mention what water stress indicator you used.

We have added a sentence "We thus define blue water stress by comparing blue water availability with corresponding net total water demand by means of the commonly used, Water Scarcity Index." in Abstract.

3) I do not see mentioning of Fig. 1 in the text, please check.
Figure 1 has been referred in Line 6 (Page 7401) in Introduction.

4) Section 2.3.2: Explain what method was used for computing potential and actual evapotranspiration (in relation to crop growth and phenology).
First, reference (potential) evapotranspiration was computed by the Penman-Monteith equation according to FAO guidelines (Allen et al., 1998). Crop evapotranspiration was then calculated by combining reference evapotranspiration and crop factors and growing season lengths from Siebert and Döll (2010) over irrigated areas from Portmann et al. (2010). Using the crop factors, growing season lengths and irrigated areas as input to PCR-GLOBWB and forcing the model with precipitation and reference evapotranspiration data as described in Sect. 2.4, this yielded daily time series of actual evapotranspiration. The reduction of potential to actual transpiration is calculated based on the total available soil moisture or green water in the soil layers. Over the surface, bare soil evaporation is drawn from the topsoil and no reduction is applicable, except that the potential evaporation rate cannot exceed the saturated hydraulic conductivity of the topsoil for the saturated fraction and for the unsaturated fraction, the rate is restricted by the unsaturated hydraulic conductivity of the topsoil layer (see Sect. 2.2 of Van Beek et al. 2011). The simulated actual evpotranspiration can be seen as the evapotranspiration of the crops in the irrigated areas in case no irrigation was applied. We have revised Sec. 2.3.2 to clarify the methods to compute potential and actual evapotranspiration.

5) p. 7409: Why do you focus on Japan here? Are those values used for other industrialised countries as well?
We interpolated recycling ratios for other countries based on the past statistics of Japan according to Wada et al. (2011) since the data on country recycling ratios are very scarce throughout the period 1960-2001. Although recycling ratios are country-dependent, Wada et al. (2011) indicated that their interpolated recycling ratios based on country's economic development stages agreed with reported values for USA and China (no other validation data were found). Uncertainties might be large for interpolating recycling ratios used in our study but we think that it is vital to account for recycling ratios for industry and domestic sectors as large parts of water withdrawn for these sectors return to the river network.

6) p. 7414: "irrigation gift"? p. 7415, groundwater abstraction is not just "somewhat" uncertain but highly uncertain.
We have revised the phrases "irrigation gift" to "irrigation" and "somewhat uncertain" to "highly uncertain".

7) p. 7420: Why did inclusion of expansion of irrigated areas produce larger numbers than Kummu et al.?
Kummu et al. (2010) computed water demand based on population and assessed water shortage by using the water demand and per capita water availability at the Food Producing Units (FPU). Their methods thus likely underestimate the number of population under water shortage in regions where relatively small population sizes yet

intensive irrigation occurs such as Central USA, Central Asia and parts of Australia (cf., Fig. 3 of Kummu et al., 2010). Inclusion of expansion of irrigated areas enabled us to capture water shortage in those regions (Fig. 9 of this manuscript) and thus yield larger numbers.

8) End of section 3.5: You should mention that estimates of blue water use are uncertain in regions such as the Sahel, which may compromise robust detection of trends and their causes.
As suggested by the Reviewer, we have added descriptions of uncertainties in water stress assessment caused by computed water demand in the discussion section.

9) Fig. 11: The temporal developments are hard to depict. Better show annual or seasonal averages, or running means (to highlight droughts)?
We think that it is valuable to show the monthly temporal developments of water stress over the past 41 years (1960-2001) when water demand rapidly increased as shown in Fig. 11 and increased water demand thus intensified water stress. Temporally averaged values likely overlook a specific very dry month when high water stress occurs while other months remain relatively wet in a year. We have added legends to specify drought years for each country in Fig. 11, which may help readers to distinguish drought years we compared with observed events.