Response to Reviewer's comments on submission to Earth Syst. Dynam. Discuss., 4, 355-392, 2013 (doi:10.5194/esdd-4-355-2013)

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

We would like to thank the Anonymous Referee #2 for his/her constructive comments and helpful suggestions on our paper, which substantially improve the quality of the manuscript. Our detailed responses to the comments of the Referee #2 are presented below.

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

RC1. This manuscript deals with modeling global water withdrawal from groundwater and surface water sources. I found overall that the manuscript is well written and the results presented are interesting. However, as noted by the authors, I also found that the paper is built upon the authors' earlier modeling works, and it appears to me that there is some overlapping in terms of model development and also the results published in the earlier works. There are also shortcomings in the introduction and methodology sections because the authors mostly describe what had been done in the previous works and the model updates are overshadowed, so the readers may find difficulty in finding what is new and what is the major goal of the paper. Nevertheless, the manuscript also contains new results for water use from different sources using different forcing datasets, and also provides new analysis using GRACE satellite observations. Therefore, I believe that the manuscript provides some new insights on modeling human water use at the global scale and is worth publishing after necessary revisions. So, my overall suggestion is that the authors make some efforts to clearly outline the main goal of this paper, provide clear overview of the major improvements in the model compared to the previous version, and also highlight the major and new findings.
A1. We would like to thank for your positive evaluation on our manuscript. We concur that the new aspects of this study (our modeling framework) were rather obscure and readers may find difficulty to find what is new and major goals of the paper. To clarify the novel aspects and major goals of this study, we have revised Section 1 (Introduction) and 2 (Methods). We have also added further discussion about our new modeling framework compared to previous approaches to clarify the advantages of our approach and to highlight the major and new findings in Section 5 (Discussion and conclusions).

My specific comments are as follows:

SC1. P356, L8 and P358, L13: It confuses me when you say 'we integrate' Wada et al. (2011a,b) and Wada et al. (2010). It seems that all these previous studies are based on the land model PCR-GLOBWB; you might have implemented some components of one into the other. Please clarify.
A2. We have revised Abstract and Section 1 (Introduction) to clarify our approach compared to earlier study.

SC2. P358, L2: References seem to be missing.

A3. This was mistake. The reference of ‘Pokhrel et al. (2012)’ has been added. Thank you for pointing out this.

SC3. P358, L22: Please specifically mention what is the major goal of this study. For example, evaluate the performance of the model in terms of what? In short, please make clear what are the key differences and major advancements over your previous works.

A4. We have revised Section 1 (Introduction) and 2 (Methods) to clarify the major advancement of this study compared to our previous work. We have also added further discussion about our new modeling framework compared to previous approaches to clarify the advantages and to highlight the major and new findings in Section 5 (Discussion and conclusions).

SC4. P359, L20-26: How are these groundwater related parameters and coefficients determined? Is the model validated for river discharge after implementing the new groundwater scheme? I think readers would be interested to see how river discharge is affected even though the main focus of this paper is water withdrawal.

A5. The lithology and topography maps were obtained from the global lithological map of Dürr et al. (2005) and HYDRO1k Elevation Derivative Database (US Geological Survey Center for Earth Resources Observation and Science; https://lta.cr.usgs.gov/HYDRO1K). We then calculated baseflow (Q_{bf}) by multiplying groundwater store, S_3 [m] and the reservoir coefficient parameterized based on drainage theory by Kraaijenhoff van de Leur (1958), J [days]:

$$Q_{bf} = S_3 \times J \qquad \text{(R1)}$$

$$J = \frac{\pi^2 (k_{S3} \cdot D_c)}{4 f B_c^2} \qquad \text{(R2)}$$

where k_{S3} is the saturated hydraulic conductivity of the aquifer, f is the drainable porosity, D_c is the aquifer depth and B_c is the drainage length. The parameter B_c is obtained from the drainage density analysis. The saturated hydraulic conductivity and drainable porosity have been related to a simplified version (7 classes) of the lithological map of the world (Dürr et al., 2005) and a literature search. Unfortunately, there is no reliable information about aquifer thickness in relation to e.g. drainage distance and lithology. Awaiting better information about this in the future, we arbitrarily assumed the aquifer thickness to be a constant of 50 m, this being the order of magnitude of the groundwater in contact with the surface water at the time scale of our simulations (several decades). By crossing the drainage length map with the lithological map and using the literature values, a global map of the global reservoir coefficient (groundwater residence time) can be estimated through Equation (R2). This parameterisation can be used as an initial estimate of global residence time, which can be further calibrated by comparing models results with low flows from discharge data and tuning k_{S3} and f for each lithological class. We have added further descriptions about the calculation of groundwater related parameters and coefficients.
After implementing the new groundwater scheme, the model has not been validated for simulated river discharge (although the previous version of the model has been extensively validated in Van Beek et al., 2011; Wada et al., 2012a), but has been validated for simulated total terrestrial water storage (TWS) in Section 4.4 (The impact of human water use on terrestrial water storage change) and Figure 6. As suggested by the Referee, we have added some validation exercises for simulated

river discharge and an analysis on the impact of human water use on simulated river discharge.

Dürr H. H., Meybeck, M. and Dürr, S.: Lithologic composition of the Earth's continental surfaces derived from a new digital map emphasizing riverine material transfer, Global Biogeochemical Cycles, 19, GB4S10, doi:10.1029/2005GB002515, 2005.

RC5. P361, Section 2.3: How is the '~20 days' before harvest determined? Also, why do you assume no direct runoff from paddy field? I would assume significant direct runoff occurs when there is rainfall over flooded paddy fields in humid regions such as eastern China and south-east Asia. Overall, I would suggest further elaborating this section with clear explanation on how these formulations can be justified.

A6. We assumed no irrigation for ~20 days before harvest. This was based on irrigation practice that occurs over paddy field (Allen et al., 1998; Aslam, 1998). The duration of no irrigation period (~late crop development stage) varies depending on regions and local practices. For some regions (e.g., Asia, Africa), water is drained from the paddy field ~10 days before the expected harvest date as draining hastens maturity and improves harvesting conditions. It is also common that irrigation is stopped a few weeks before harvest over the paddy field to dry and the rice to transfer maximum nutrients into the grains (e.g., Asia). In our initial calculation, we allowed direct runoff from paddy field but the amount was negligible (probably related to our irrigation scheme). And farmers tend to irrigate much less before expected (heavy) rainy days/periods. Therefore, we assumed no direct runoff from the paddy fields. We have added further explanations about our assumptions and the uncertainty therein in Section 2.3 (Irrigation water requirement).

Aslam, M.: Waterlogging and salinity management in the Sindh Province, Pakistan, Report No-70.1a, International Irrigation Management Institute, Colombo, Sri Lanka, 1998.

RC6. P363, L1: Does 'critical level' here mean 'wilting point'?

A7. The 'critical level' corresponds to the 'soil water depletion fraction'. Water in root zone is theoretically available until wilting point, however, crop water uptake is reduced well before wilting point is reached (Allen et al., 1998). When the soil is sufficiently wet, the soil supplies water fast enough to meet the atmospheric demand of the crop, and water uptake equals *ETc* (crop evapotranspiration; Equation 6), however, as the soil water content decreases, water becomes more strongly bound to the soil matrix and is more difficult to extract (Allen et al., 1998). Thus, when the soil water content drops below a threshold value, soil water can no longer be transported quickly enough towards the roots to respond to the transpiration demand and the crop begins to experience stress. The 'soil water depletion fraction' determines the fraction of *TAW* that a crop can extract from the root zone without suffering the water stress (~*RAW*; Equation 5). We have added further explanations about the 'soil water depletion fraction'.

RC7. Section 2.6: What are the water allocation strategies? In other words, at what proportions are different demands fulfilled from different sources? P366-L19: Why is groundwater taken first rather than surface water? Wouldn't humans tap surface water first as it is easily accessible? I assume changing the allocation strategies and *withdrawal order will affect the estimated groundwater use. 7. Please explain this clearly in the manuscript.*

A8. Our scheme allocates water first from the groundwater storage (S3), however, in case reservoirs are present at local and upstream grid cells, we first allocate surface water to meet the water demand, and the remaining water demand is allocated from the groundwater storage (S3). We have clarified our allocation strategy and the assumptions and uncertainties therein in Section 2.6 (Water allocation and return flow). The discussion about the sensitivity to these assumptions has also been included in Section 5 (Discussion and conclusions).

RC8. P366-L15: In most depleted groundwater systems (in semi-arid to arid regions), there could be very little baseflow as the water table is too deep but there could be still potential to withdraw groundwater. Please clarify the reason behind this assumption that base flow can used as a proxy to infer groundwater availability.

A9. This is a very important point. To avoid no local groundwater withdrawal over arid regions (grid cells) with negligible local baseflow, we inclined to use accumulated baseflow (Equation 14). This allows regions (grid cells) with no local baseflow to extract local groundwater resources. In case of no accumulated baseflow due to very dry conditions over a catchment, surface water availability is also expected to be very small. In our allocation scheme the water demand is then imposed on (nonrenewable) groundwater abstraction (~groundwater depletion due to little groundwater recharge). We have added further explanations about our allocation scheme to clarify our approach.

RC9. P374, L10: Please compare the groundwater withdrawal of 1200 km3/yr with the other estimates; I believe there are several estimates of global groundwater withdrawals including some cited in the manuscript.

A10. The comparison of our simulated groundwater withdrawal to previous studies was provided in Section 5 (Discussion and Conclusions), P373. We took the year 2000 (~1000 km^3 yr^{-1}) for the comparison to correspond the year of their estimates. Since this is an important point, we have expanded the comparison and added further discussion.

RC10. Section 4.4: Impact of reservoir operation on water storage change has not been discussed at all. Storage on reservoirs should affect TWS much more than irrigation does in highly regulated basins. Some recent studies (e.g.: Wang et al., 2011. Water Resource Research, Vol 47, W12502) have shown that in highly regulated rivers the impact of reservoirs could be large. Please add discussion. For example, for Colorado river basin, the changes in TWS could be mainly due to reservoir regulation.

A11. We concur this is very relevant to our study and our descriptions regarding the impact of reservoir operation on simulated TWS was not enough. We have added further discussion about the impact of reservoir operation on simulated TWS in Section 4.4 (The impact of human water use on terrestrial water storage change).

RC11. Table 1: I could not confirm some numbers such as 2380 in Hanasaki et al.

A12. The value should be 1530. Thank you for pointing out this mistake.

RC12. Figure 6: Why are these particular basins selected?

A13. The selection is rather arbitrary, but these basins are heavily influenced by human activities (human water consumption and reservoir operations). We have added sentences to explain the selection of the basins in Section 4.4 (The impact of human water use on terrestrial water storage change).

RC13. Figure 6 caption: Please confirm the units. Storages should be in m or mm.
A13. The TWS anomalies were calculated with the unit of m.