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Discussions

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Interactive comment on "A global-scale two-layer transient groundwater model: development and application to groundwater depletion" *by* Inge E. M. de Graaf et al.

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Reply to comments by Petra Döll

We thank Petra Döll for the thoughtful and extensive evaluation of our work. She raises a number of valid points, additions and concerns, which we hope to address in the answers below. The reviewer's points will certainly be very useful in improving the paper.

Gradient-based groundwater modeling at the global scale is a big challenge, and it was very interesting to me to see results of such an effort in the manuscript of de Graaf et al. I would like to make some comments and pose questions regarding 1) the

groundwater modeling and its presentation in general (as there are a few aspect that need clarification) and 2) the estimation of groundwater depletion.

(1) Global groundwater modeling in general

Page 5, line 20: Regarding discharge of groundwater to local springs etc. From the wording, it is not clear if water storage S3 is from PCR-GLOBWB or the elevation of the floodplain. Please clarify. How is the elevation of the floodplain determined, and where is it as compared to DEM, HRIV and RBOT? Please indicate in the manuscript how large this local drainage component is compared to total Qbf (globally).

The water storage S3 is indeed taken from the original version of PCR-GLOBWB (the third reservoir). From the DEM and the storage in S3 (aquifer depth times porosity) we can calculate the amount of storage [L] above the floodplain elevation. From this follows the local drainage flux. Globally this is 65% of the total baseflow. We will add these descriptions to the manuscript.

Page 8, Line 4: If groundwater head falls below the bottom of the confining layer, is then the confined aquifer modelled as unconfined, and the storage coefficient set to the specific yield to Table 1 instead of 0.001?

Ideally it would, but our approach does not allow this at this time. Running MODFLOW with the option of cells being wetted and rewetted is not feasible due to convergence issues. This would result in too many outer iterations in the solver and consequently very long run times (already three weeks currently). Changing the parameters from confined to unconfined will affect the depletion estimate.

Fig. 5: Typo in figure caption: Central Valley is unconfined, the Netherland confined.

Thank you we will correct this.

Fig. 6: I was wondering whether all three graphs showing R and all three graphs showing QRE really have the same y-axis (they should!). The units for QRE are %? Regarding QRE, model performance decreases according to Fig. 6, while the two

examples in Fig. 5 show clearly the advantage of distinguishing between confined and unconfined aquifers, for the two wells shown. Why is this so? Can you localize where, in the confined parts of your model, assuming unconfined conditions leads to a better modeling of head amplitudes? I think it is necessary to include, in the conclusions (p. 14, l 12-14), that including confining layers leads to a worse simulation of head variations.

This is a valid point. We will add a sentence stating where the inclusion of the confining layer increases performance and where it decreases. We will highlight the areas where groundwater dynamics are not well represented and point out which research is needed to improve the model outputs in future. Also, we will replace "count" with "relative frequency" in the next version of the manuscript.

Fig. 9: In my understanding, Fig. 9 does not show flow paths but travel times. What exactly is shown? The travel time of the groundwater recharged at the grid cell shown? Please also clarify in the text on p. 12. I also think that your conclusion (page 14, lines 18-20) is not backed by Figs. 9 or 10. Fig. 9 shows shorter travel times in case of confined aquifer modeling, while Fig. 10 shows significant importing/exporting in mountainous areas (not flat confined areas). (Units are missing on y-axis of Fig. 10).

The figure shows both. It shows the length of the flow path and the apparent age of the groundwater along the flow paths. The flow path is calculated by releasing a particle in a cell at the location of the groundwater table and then following it (using MODPATH) to a (weak) sink. We will put a clear description in our revised manuscript. Regarding the conclusions: yes, they are not correct. We see long flow paths in case of confining layers, but not slow. Indeed travel times are shorter, so we will remove the word "slow". Regarding exporting and importing we can see in Fig. 10 that in the confined scenarios (e.g. a confined layer for the High Plain aquifer) larger- values importers and exporters are simulated compared to the unconfined scenario. This is reflected by the longer flowpaths simulated in the confined scenario crossing catchment boundaries

(2) Simulation of groundwater depletion

Page 13, lines 20-25: Please define "volume-based" and "flux-based" approaches. Do you compute groundwater depletion by subtracting heads and multiplying with the storage coefficient? Do you call your approach "volume-based? Otherwise, I would not agree with the sentence: "Figure 14 (Online Supplementary Information) shows that effect of hydraulic properties on the groundwater depletion volumes (note volumes, not heads) is considerable, which makes estimates of groundwater depletion by volume based methods rather uncertain."

Indeed, our approach is volume-based as we interpret this to mean calculating the actual volume taken out of storage (this is in our approach change in head times storage coefficient or specific yield). Flux-based approaches are the ones obtained by calculating depletion as the difference between recharge and abstraction. Volume-based approaches are certainly dependent on hydraulic properties: storage coefficient or specific yield relating head change to volume taken out of storage, but also the head change itself which depends on hydraulic conductivity and the river bed conductance of any river in the vicinity of dropping groundwater heads. In Figure 14 we show the model sensitivity using different parameter sets (listed in Table 2).

The development of the global sum of groundwater storage loss in cells in which net recharge is smaller than groundwater abstractions (as shown in Figs. 12 and 14) shows how sensitive this estimate is to parameter settings. If depletion assuming unconfined conditions only is (in 2005-2010 compared to 1960) twice the amount of that for confined conditions, I would think the model is overly sensitive. Equally important, Fig. 12, with an increase in groundwater storage before 1980 as compared to 1960, and an actual onset of groundwater depletion only in 1998 indicates to me that what you see in the first decades may be caused by the fact that 10 years of running 1960 (climate and water abstractions) on steady-state groundwater levels was not enough to get a reasonable situation of the state of groundwater heads in depletion areas in 1960. Or that the location of the groundwater table to which interaction with surface water is very

sensitive was not close enough to reality (see Fig. 7 where e.g. differences of 20 m to observations are the rule but that would already have a strong impact on gw-sw interactions). How did the flows between rivers and groundwater develop over time in the depletion areas? Regarding the temporal dynamics, your simulation results show a large depletion of 5000 km3 in only 6 years (1998-2004), with relatively little dynamic at other times. Is this due to spatial averaging?

You may certainly have a point here. The delayed response of global depletion is understandable as a result of the groundwater-surface water interaction. As we answered to questions of reviewer 1 related to the late response: "This process is still there indeed. Although the effect may be somewhat overstated. What happens is that we have the larger rivers connected to the groundwater system in MODFLOW through the RIV package, and the smaller rivers by the drain package. When one starts to pump more than is being recharged part of the abstraction will come out of storage, but in the beginning part will come from reduced discharge (to rivers and drains) too. After the drains fall dry, part of the abstraction may still be supplied by the rivers (river bed infiltration) and this part increases as the groundwater head drops. After the head drops below the river bottom this infiltration flux becomes constant. So after that most of the additional pumping must come from storage and cause increased depletion rates. Of course, this effect may be overestimated because river levels are simulated by PCR-GLOBWB and are not fully coupled to MODFLOW yet. River levels could decline more in reality causing a more gradual increase of depletion rates." However, we agree that this process and the time of disattachment of groundwater and surface water may be quite sensitive to the initial conditions. So a longer warm-up period may be a good idea, for the revised manuscript we will do so (e.g. 50 years).

In page 4, line 27 you state that after 10 years of running 1960, a dynamic equilibrium was reached? Do you mean that after 10 years, groundwater heads did not change by +- x%? What value did you choose for x? As a test for sensitivity of Fig. 12, I suggest you rerun your model with 100 years of 1960 initialization instead of 10 years,

and show the results in Fig. 14.

We agree that a longer warm-up period is a good idea. We will do so the revised paper. (See also previous comment.)

Another question regarding Fig. 14: The grey recharge-abstraction curve is more or less a straight line, e.g. the annual difference between groundwater recharge and abstraction is constant between 1960 and 2010. Can you explain why, as groundwater withdrawals are known to have increased significantly during these fifty years?

We see that something is wrong here, and the reviewer is totally right as this point. We found out that in this figure the plotted line of the abstraction-recharge deficit (abst-rch) presents wrong information; a mistake was made in summing up total recharge rates. This concerns the postprocessing and not in the actual calculation, and thus it does not change the depletion estimates of the maximum and unconfined scenario (also presented in Figure 12). We apologize for this mistake and will include the updated the figure shown in the attachment.

This figure shows an exponential curve for the abstraction-recharge deficit, which is inline what we expect as groundwater abstractions are indeed increasing over 1960-2010. Our estimate is within the same range as previous flux based estimates, e.g. this studies 2000 deficit is $\sim$19400 km3 is comparable to the estimate of Pokharel (2000);$\sim$19000 km3. We will rewrite the discussion of this figure in the manuscript (p.13, l.12-l20).

In Fig. 13 C, please add observations for High Plains Aquifer, while in Fig. 13A I suggest you use the same legend/color as in 13B, for better comparability.

We will do so.

In Fig. 11, use mm instead of km3.

We will do so.

In your comparison to other estimates of global groundwater depletion, please add a comparison to the maps and global values of Döll, P., Muller Schmied H., Schuh, C., Portmann F. T., Eicker A. (2014): Global-scale assessment of groundwater depletion and related groundwater abstractions: Combining hydrological modeling with information from well observations and GRACE satellites, Water Resour. Res., 50 (7), 5698-5720, doi:10.1002/2014WR015595. In this study, we took into account that baseflow is reduced (and then zero) in areas of groundwater depletion (but inflow of river water into groundwater is not simulated). Also, there is groundwater recharge from surface water bodies in dry areas (very rough estimate).

We will add this to our comparison. The effects conceptually modeled in your approach are implicitly accounted for when using a global groundwater flow model, although the effect of increased capture may be over-estimated in our case as we do not have a full coupling allowing for a decline in surface water levels as a result of groundwater pumping. We are preparing a paper with fully coupled groundwater-surface water interaction that does allow falling river levels as a result of groundwater pumping.

In addition we assumed, based on comparing our modeling results to many independent estimates including GRACE, that 70% deficit irrigation is done in groundwater depletion areas. This resulted in a best estimate of global groundwater depletion of 2240 km3 for 1960-2000, and of 3257 km3 for 1960-2009. So according to your study, gw depletion increases much faster after 2000 than in our study, and the total value is higher.

Indeed, we assumed optimal irrigation volumes that may explain the larger depletion rates found in our study.
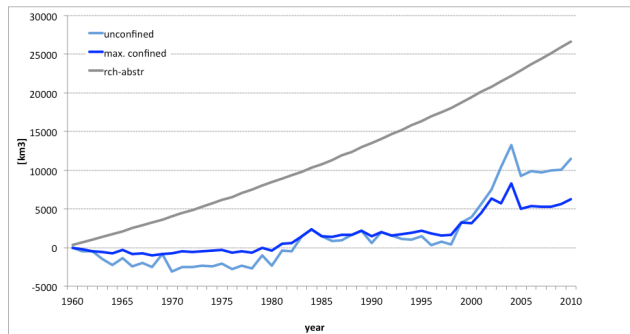
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Figure 12: Global depletion trends simulated for the maximum (max. confined) and unconfined aquifer scenarios and compared to the estimated cumulative deficit between simulated recharge and groundwater abstraction (abst-rch).

Fig. 1.