Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-121-AC3, 2016

Hydrology and
Earth System
Sciences
Discussions

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.

Inge E. M. de Graaf et al.

idegraaf@mines.edu

Received and published: 6 June 2016

Reviewer 2

We thank Reviewer 2 for the extensive evaluation of our work. He/she provides several reasons why the model should not have been made and why it does not add to previous work on global depletion. Obviously we disagree with this assessment, but do take some of his/her critique at heart.

We will start with the main points summarized at the end of the review and then address any other specific questions and remarks that have been made above.

(1) The problem is ill posed. Most abstractions are from confined aquifers, and most

water withdrawn from aquifers comes out of storage. Thus estimating the abstractions (or volumes) of withdrawal gives you one estimate (which previous authors have done) and putting those abstractions into a groundwater model does not improve only gives you head declines. To address depletion globally focus on the regions where the impact is highest.

We disagree with this assessment. Yes, many abstractions are from confined aquifers but it does not mean that there isn't any connection with the surface water system when abstracting water from a confined aquifer. A well pumping from an unconfined aquifer will tend to capture most of its discharge from the nearest steam. The presence of a confining layer between the well and the stream causes the cone of depression to extend to greater distances to capture the natural discharge required to offset pumping (e.g. Morgan and Jones 1999). Some of the current authors also authored previous assessment of global depletion (Wada et al., 2010) and the main critique of this work was not accounting for increased capture and thus over-estimated depletion. By using a groundwater flow model we now do take account of this increased capture. This is illustrated by Figure 12 that clearly shows a big difference between depletion rates (water abstracting from storage) calculated with the groundwater model and those simply obtained from abstraction minus recharge. See also the difference between Wada's estimate and ours (Table 3) where ours is lower due to taking account of increased capture.

(2) The head declines are only as good as the parameter estimates. Storage coefficients and hydraulic conductivities and thicknesses vary greatly by magnitude and in space, so the uncertainty in the results will be high, even if you can match some head observations. The fact that in all of the mountainous terrains of the world the simulated depths to water are off by more than an order of magnitude is one warning flag.

We agree that the head declines are as good as the parameter estimates. In general this is true for all models. In line with good modeling practice we present our results with clear bounds of uncertainty (see Figures 6,7, and 13). The simulation and associated uncertainty are indicative of the sensitivity of the model and of the directions in which improvements must be sought. These improvements are discussed in the discussion. If you do not model e.g. groundwater flows or depletion at all, you will never discover how uncertain one really is and which data are lacking and where. Moreover, if you model a system, even if its parameters are subject to uncertainty, one can still learn a lot from exploring the system behaviour and system sensitivities. Finally, regarding the remarks about the (too) deep groundwater levels in mountains. We explicitly state that our model does not resolve the perched water tables in hillslopes and the water tables in alluvial pockets in mountain valleys (these are conceptually modeled in the surface water model), but calculate the groundwater in the bedrock below. This is the cause of the underestimation in the mountains. We will state this earlier in the Introduction in the revised version of the manuscript to be more clear about this point. However, this does by no means disqualify our results in the larger alluvial basins coastal plains and deltas where groundwater is relatively shallow, groundwater depth matters and where groundwater is often being depleted due to the high demand.

(3) Their results, in fact, didn’t show anything new in terms of depletions. The locations and amounts of depletion are exactly where they told the model the pumping was, and the results were similar to past volume-based results, suggesting the water was mostly coming from storage. In their conclusions the authors state this is only one step in building a better global model, but building a model just for the sake of building it is not scientific progress. This model has an ill-posed application. It is not useful for learning anything new about depletions because the uncertainties are so high. It would be better suited, for example, for looking at how the water table and base flows respond to changes in climate, but it is not clear that the resolution needed for the accuracy for that application is a worthwhile endeavor at the global scale.

We concede that our study does not reveal new areas of groundwater depletion. This makes sense as the groundwater abstractions used here are closely related to those used by Wada et al. (2010), apart from that they are not taken directly from the IGRAC

Printer-friendly version

Discussion paper

data but total demand is allocated to the groundwater and surface water resource on the basis of availability (according to De Graaf et al., 2013). However, the depletion rates are different and reveal, albeit with considerable uncertainty, the effect of increased capture (see our response to reviewer 1 for the mechanism). Moreover, and this is also the answer to the first point that was raised at the beginning of the review (see hereafter), calculating depletion volumes is an important application of this model, but not the only one. There are multiple reasons for building a groundwater model instead of using a single storage-outflow reservoir in land surface models: 1) Indeed, also in case of confined aquifers there is considerable interaction between surface water and groundwater. This is difficult to parameterize in a conventional land-surface model. 2) Our ultimate goal is also to estimate head declines. This is important because one needs to know how the depth-to-groundwater develops with time. If the groundwater head falls substantially ordinary farmers' pumps cannot reach it anymore and eventually it becomes unattainable for ordinary industrial pumps as well. It therefore constitutes a clear limit to groundwater availability. 3) Even though the volumes of groundwater traveling across catchment boundaries is limited, it becomes more and more important if one goes to even higher resolutions, and certainly in case of groundwater abstractions. 4) In many areas of the world groundwater significantly contributes to evaporation through groundwater convergence and capillary rise (see Fan et al., 2007; Bierkens and Van den Hurk, 2009) which warrants modeling groundwater head explicitly.

So in conclusion: this study adds considerably to previous depletion estimates and provides the first global estimates of global groundwater head variability and head decline and is a first step toward assessing how long our groundwater reserves could last.

Hereafter we answer other specific points raised by the reviewer:

Page 1. Line 1. This sentence appears to be the justification for building a global groundwater model, but this sentence is misleading. If by basins they mean surface water basins, then lateral flow is only significant in the most surficial part of the system where >90% of the flow occurs. Most substantial groundwater withdrawals are from deeper confined and semi-confined aquifers where the total natural flow in the system is very small compared to the recharge, for example. It is true that most hydrological models do not include a groundwater flow component, but that in itself does not justify building a global groundwater model. Basically as the saying goes, “all models are wrong, but some are more useful than others”–so the authors have to demonstrate that their model is both accurate “enough” and “useful” to some degree. Unfortunately don’t believe this model is either. It is only in the ballpark where the data exist in the 10% of the world where most of the pumping is occurring (it does a very bad job in much of the world in simply fitting depth to the water table), and it really isn’t useful in the 10% of the world where the pumping is occurring because global groundwater depletion has already been quantified to a similar degree of accuracy (Wada et al, 2010, 2012; Konikow, 2011), and the groundwater model they are constructing really doesn’t improve on those estimates. Lately it seems to be that the bigger the model is, the better it must be and the more likely we are to learn something new from it. Therefore the authors are now producing a global model; but in this case the model does not produce any useful results. More on this later.

We agree with the reviewer that, albeit flowpath analysis (See Figure 9 and also De Graaf et al., 2015) shows that many flowpaths passing catchment boundaries, the associated volumes are limited. However, we also observe a trend that future land surface models will operate at increasingly higher resolution (Bierkens et al., 2014), which makes that the across boundary fluxes at the grid scale will become more and more important, particular in case of groundwater abstractions. Our modeling effort is part of preparing for that circumstance. Also, as argued above (point 3) there are many other reasons for wanting to simulate groundwater heads at larger scales. We will more explicitly provide these reasons in our revised version

Page 1, line 19. This has been known for decades-why cite a reference from 2016? Why not a UN report from the 20th century?

The reviewer is right and we will add a reference to that effect.

Page 2, line 29. Here the problem begins. Indeed most abstractions occur in confined aquifers (which is why the investigators include a second layer to represent them), but simulating head declines there does NOT improve any estimates of groundwater depletion. Most all groundwater depletion in deep aquifers comes out of storage, not from bounding surface-water bodies, and therefore the model -simulated depletions only mimic the abstractions that are put into the model a priori. Thus the model is only as good as the estimate of water abstractions that are available. The earlier global depletion studies did their best to estimate these abstractions. Putting these numbers into a groundwater model does not improve on them.

As argued above under point 1), it is not true that most of the water comes out of storage. Most of the abstractions in our model indeed stem from confined aquifers or in confining layers, but they certainly attract groundwater at the expense of baseflow or river runoff as shown in Figure 12 and Table 3.

Figure 5 horizontal axes need labels (year). “Data” is not an acceptable y-axis labels aren’t these heads, in meters?

The reviewer is right. We apologize for the oversight and will change this in the revised version.

Figure 6 is very hard to understand because the x-axes are not labeled and the y-axes are underlabeled. Count of what?

We will put the required labels in these figures in the revised version.

Figure 7. The points are too faint. It’s not clear how far the simulated heads actually fall below the measured heads. Axis labels need to include “in meters”.

The points have been deliberately plotted a bit faint to show the effects of point density in the Figure. These are thousands of points, so it is difficult to indentify individual points back. We will try to improve and put “(in m)” on the axes.

Figure 8. These results truly show how bad this model is. Only in the deserts of the world is the depth to the water table typically greater than 100 m. Yet most of the mountain ranges here show depths greater than 300 m. There are streams in most of those mountains so we know the depth to water is zero at those locations and likely a few tens of meters at the watershed divides. If so much of the world gives results this bad it does not give me confidence in the rest of the world, and there really isn't any reason to do a model of the whole world, especially when 90% of the abstraction occurs in 10% of the area. The authors will argue that these areas don't have any significant depletion anyway, but that argues that those areas really don't need to be modeled. If the time and resources were focused on the 10% of the world where depletion was really occurring, you would get better results and might learn something.

As argued under point 2 above, we clearly state that we are not able to simulate the perched water tables in hillslopes and the water tables in alluvial pockets in mountain valleys (we hope to resolve these in the near future), but calculate the groundwater in the bedrock below. The conclusion that as a result the results should be therefore bad in areas where groundwater tables are shallow is thus not at all founded, and we have the validation results to show that the model performs adequately in these areas. We do not think it is a good idea to limit the model to areas where there is depletion because: 1) we expect other areas that have yet no depletion to develop this in the future (parts of Africa, especially cities in deltas); 2) as stated under point 3 above, the groundwater model has other purposes than calculating depletion rates. Finally, in a previous paper (De Graaf et al., 2015) about the one-layer steady state version of the model we have masked the mountain areas out. We elected not to do this here, but could do this again.

Figure 11. This figure makes no sense. If you have an areal map then depletion volumes should be represented by meters, unless what they mean is cubic kilometers per square kilometer, but given that the numbers are up to 100 that can't be the case so this figure is undecipherable. In the paper on page 11 the top sentence I see now

Printer-friendly version

Discussion paper

the authors do indicate it is km3 per grid cell, but their grid cells vary in area across the globe so plotting results in this way is not consistent, nor are the number intuitive. M3/m2 would be much more meaningful.

This is a valid point: we will change this to M3/m2 in a revised version.

Table 1. The authors report using Sy values of 0.01 and higher for hard rocks, although in the text they way a storage coefficient of 0.001 was used for confined aquifer. Given the storage coefficient is the specific storage (which can vary by a couple of orders of magnitude) times the thickness (also quite variable) it is easy to imagine that the results could be off by a factor of ten. I realize the authors test the sensitivity by varying it by several factors, but given the authors are unable to quantify this uncertainty, it makes their results equally uncertain. Coming up with a "best-fit" scenario does not indicate their parameters are at appropriate values.

We calibrated the model to fit the head time series and we show the uncertainty in Figure 4. We will discuss the uncertainty more extensively in the discussion. We will refer to De Graaf et al (2015) where we already investigated the coefficient of variation of results (a measure of uncertainty) as a result of both conductivity and aquifer thickness uncertainty.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-121, 2016.