Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-121-RC1, 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.

Anonymous Referee #1

Received and published: 18 April 2016

This paper presents an ambitious attempt at hydrologic modeling at the global scale. The work builds on a previous model of the author by adding confined aquifer units and using a transient model. Given the scarcity of physical subsurface data available at the global scale, large assumptions were made about aquifer structure and parameters. The work is clearly a step in the right direction, and we need to test our ability to model these systems, but the usefulness of the results is not clear. I have two specific concerns:

1) To calibrate a global model with observations only from the United States and from one delta in Europe doesn't seem reasonable. Especially, given that a major value of the model lies in it's ability to parameterize subsurface systems or predict groundwater

level changes for the remainder of the world, where we happen to have few observations. At this point, perhaps the model should just be applied to the US and part of Europe, where the model structure can be better tested?

2) I'm concerned about the overall discrepancy between the representative model depth, and the system it aims to simulate. This stems from using surface geology to infer aquifer properties, and similarly, using surface geology to infer the presence and properties of confining units. Many primary aquifer systems are multi-layered with numerous confining units and aquifers with varying properties with depth. The objective of the paper could be to just model the near-subsurface system. However including groundwater extraction values, which in many cases are drawn from deep systems, may force the calibration process beyond reasonable limits.

Overall, it's clear that a significant amount of work went into this, and it moves us closer to having a global groundwater model. Addressing some of the comments here about model structure, calibration, and uncertainty in storage change will help clarify the value of the model and it's results.

Specific comments:

1) It would be helpful to conceptually explain the model and assumptions a bit more clearly in the methods section. Obviously the data required to model these deep aquifers is rare, certainly at the global scale – so the current project is making reasonable assumptions in order to move the understanding forward. Given that, it should be clear early in the paper what system and dynamics it expects to model reasonably well, given the data input restrictions. Broadly, the model improves upon a previous version which modeled all aquifers as unconfined. Is the current model explicitly modeling the most surficial aquifer and most surficial confining units only? The permeability values represent the surface geology, and the confining unit permeability also seems to be based only on the shallowest layer of material.

2) Most of the large-scale (irrigation, industrial, municipal, etc.) groundwater usage is

drawn from deep wells, whose regional aquifer characteristics may not be well represented in this model. Can the authors discuss how calibrating the model with relatively shallow aquifer input parameters to fit potentially deep system extraction rates may impact the model performance? It seems like there may be a discrepancy between the system modeled and the one it is calibrated to.

3) Given a two layer model, are interactions with a shallow unconfined aquifer (e.g. alluvial aquifer overlying a confining unit) lost? Are there specific areas where surface water - groundwater dynamics were not well represented, perhaps useful for guiding future research to improve our subsurface parameterization capacity in these areas?

4) Were groundwater observations from all well depths used to calibrate the model? For the confined aquifer areas, it is highly possible that groundwater observations are being made in multiple aquifers, where deeper layers would not be expected to have a direct connection with the surface as is being modeled. I understand we cannot expect this level of detail to be included in the model, I'm just curious how fitting a model to these data will impact your results.

5) The brief description of how aquifer thicknesses were calculated (in addition to the citation to the 2015 paper) is helpful. Can a similar one be provided for how thickness of the confining unit were calculated?

6) Were the parameters for the confining units assumed based on the surface unit texture? Were any measurements (or regional model parameters) used to inform individual aquifer confining unit permeability, or were they set uniformly across the globe?

7) In the methods section 2.1.2, Does "Next to the river levels" mean proximally adjacent to? Or "next" figuratively? It sound like there are fixed head boundaries being specified at sea level adjacent to all the rivers. If this is correct, can you justify why you chose to do this? Can you explain this decision with respect to Figure 8? The depth to groundwater appears to follow topography (as you say in the paper), and is simulated quite a bit deeper than observed (e.g. much of western US and Mexico).

8) If it took 10 years for the model to reach equilibrium, does that say something about the degree of disequilibrium in the groundwater system in 1960? Do you think 10 years is reasonable? If so, or not, can you infer something about how the model is functioning?

9) There are two periods of rapid groundwater depletion in Figure 12 early 1980s and 2000s. You explain the first as being delayed despite overall abstraction > recharge (is that right?) by stream capture. Is this a process that would be included in the model, without having feedback from groundwater level on surface water?

10) The total groundwater depletion is given with 4 significant figures. Can you justify this precision? Can you provide an estimate of uncertainty on the depletion estimate based on errors associated with the groundwater level simulations and storage values?

11) The conclusion that model performance is only slightly better with the inclusion of the confined systems suggest that we do not need to model the confined systems? or that they should be modeled another way?

12) Many of the figures can be tightened up: they could use subfigure labels (A,B,C,etc.), and make sure the axis labels are final (some say "data", several are missing), and that for figures with subplots that the axis line up for all figs.

There are a handful of typos, but those can be corrected with minor effort.

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