Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2018-120-RC1, 2018

Geoscientific Model Development Discussions

Open Access

Interactive comment on "Beyond the bucket – Developing a global gradient-based groundwater model (G^{3}M v1.0) for a global hydrological model from scratch" *by* Robert Reinecke et al.

Anonymous Referee #1

Received and published: 10 June 2018

The paper describes the development and application of a global gradient based hydrogeological model. I can clearly see the use of developing large-scale models. However, there are a range of significant issues related to this paper. I do not support publication at this stage. The reasons are as follows. A fundamental problem with these models is that they are difficult to verify, but this is not at all reflected in the discussion of the results. The authors state on line 24 page that these models are useful in areas with little or no data, as they allow to generate robust information. How can anything robust be generated (and how do we know its true?) in the absence of data. The hydrogeological literature is full of examples where even in the data -rich regions different models produce different outcomes.

Printer-friendly version

Discussion paper

We are presented with plots, numbers and graphs and some interpretation, but there is no credible discussion on the reliability of the result obtained. The only indication of model performance is that there is essentially no correlation between simulated and observed depth to groundwater. To me this means simply that the model cannot be used to make these types of predictions.

It is not useful to plot observed and simulated hydraulic heads over such large scales, even if its just for the sake of model comparison. It is true that other authors have also presented simulated vs observed hydraulic heads over such large scales, but this is simply misleading. Depth to groundwater is the variable that counts for calculating exchanges with surface, amongst many other processes. In this sense none of the available models on a global scale is ready yet. This must not necessarily be a problem, as long as the results are not oversold, as is unfortunately rather often the case.

The formulation of the equation 2 is for a confined aquifer. The authors justify this conceptually wrong choice on line 20, page 6: "Flow equations are for confined aquifer because it reduces convergence time. "This is a very poor argument, purely based on convenience. To what extent the model should capture the relevant physics should cannot be a question on how difficult it is to solve equations. The goal of this modelling approach is to advance the interaction between the surface and the subsurface across very large scales. Given that the direct interaction with the surface always happens with unconfined aquifers the fundamental basis of the approach is flawed on the most basic level. While for steady state simulations the term falls out of the equation it is still very concerning that that a model is developed with inadequate flow equations.

The authors will probably argue that this is a first step in model development and that unconfined conditions can be added later. However, a large part of the model is presenting model- simulations. For example, we learn how much water if flowing from aquifers to rivers on a global scale. Given the formulation of the model, these results should not be presented.

I did not understand why the authors develop a new model in the first place. They rightfully acknowledge that models such as MODFLOW exist, and these model could potentially do the job. Their argument is that MODFLOW models typically integrate geological data that is not available on a global scale. Therefore, a simplified model is developed. But this is a strange way of reasoning, as with MODFLOW one is not obliged to integrate all the geological complexity. It would have been perfectly possible to use MODFLOW for this project , with several significant advantages: for example, an unconfined aquifer (see below) could have been simulated. In this sense the novelty of the aspects concerning model development is questionable.

There are many other problems working on a global scale which are not even mentioned here but will even further undermine the credibility of the model. The three most important ones are: (1) Elevation is the wrong parameter for such a model. The data that should be used is not an ellipsoid- DEM but rather a geoid as the geoundulation is significant. (2) The density of sea-water is different, therefore there should be a density correction. (3) Steady-state conditions are inappropriate assumption that is not justified sufficiently well.

Validation is done with other macro-scale models. This is a not an ideal strategy, as these large-scale models suffer from similar deficiencies (even though on less fundamental level). For a solid assessment of model performance a detailed, catchment scale hydrogeological model should be used for a benchmark comparison. On line 28, page 7 the authors highlight that this is ok –" . . .without losing important model behavior. " Transient and steady state is significantly different in both spatial and temporal dynamics.

The description of the conductance is confusing. In MODFLOW L is not the length of the river itself, but the length of the river within a grid cell. But this might just be an imprecise formulation.

Other aspects also require more justification and discussion. Why only 8 % of wetland surfaces? Where does this number come from? What are the numerical convergence criteria, as well as a wide range of additional model parameters?

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2018-120, 2018.