HESSD

Interactive
comment

Interactive comment on "Application of an improved global-scale groundwater model for water table estimation across New Zealand" *by* Rogier Westerhoff et al.

Anonymous Referee #2

Received and published: 27 July 2018

This study presents an updated version of the global-scale equilibrium water table model (Fan et al 2013) for New-Zealand by using New-Zealand specific input data at a higher resolution than done before. This update resulted in an estimate of water table heads and depths at 200m resolution that fit observed water table depths slightly better than before.

I first read the paper to gather my thoughts and then read the comments of reviewer R1, as well as the author response, just to be efficient rather than re-iterating verbosely. I will first say I largely agree with the comments of R1.

I have a few additional comments:

- The writing, and therewith the presentation and discussion of the research, should be significantly be improved. In addition to the points razed by R1, I suggest to rewrite the abstract and introduction and specifically focus on logic of the reasoning (meaning is a statement followed by the right argument and is the argument clear) and being as clear as possible. For example, abstract L2-3 reads: Large-scale models are simplified and not used at smaller-scales, because hydrology and water policy are constrained at the catchment scale. This does not make sense. What the author meant to say is that large scale models, are not useful for smaller scale groundwater assessments yet, because of the simplifications (and the coarse resolutions), therewith are not useful for e.g water policy. The next line reads: However, …... . However, the statement in this line cannot be linked to the previous statement. Something like "for water policy smaller-scale models are more useful. However, …. ." should be included. This are just two examples within the first three lines. Also, be careful using "this" "that" "it" without a summary word.

Overall from the abstract and introduction it was not clear for me what the main motivation and goal of this research were and how it will help us to improve current modelling efforts; to improve the EWT model but also be more useful for water managers? The lack of a logical structure and the bad writing are not beneficial for a clear understanding.

- I found the manuscript very limited in discussion of previous work, methodologies, results, and relevance of the work done.

For example, on discussion of previous work: P4 L4 "many studies …." And then only one reference is a bit limited, as it is not a review paper you refer to. P4 L7 "De Graaf apply a model…. Global-scale input data" This is too generalized, it should be a bit more specific what is meant with "a model" and "input data". Especially as you give some details for the Fan et al 2013 model. 1 to 2 Lines extra focusing on the differences between the two models referred to is needed. I know the models are quit difference. I little review here will also connect to the discussion, and will help you getting your point

across why your model is better than the large-scale models available currently (see also my points later on)

P4 L17: How do you know groundwater models are less reliable in data-sparse regions as there is no data to validate the results. In the case of a model calibration, like done in this study, you can say your model performs best for the regions where you do have data to calibrate on (the whole meaning of a calibration).

Methodology and results: In section 2 it is not explained what happens when water tables hit the surface, nor is it explained that this is not simulated as a head dependent flux and river infiltration (water entering your aquifer) is not included. How realistic is this in the real world? (this should come back in conclusion/discussion as well) Also, your model result look very biased toward shallow water tables, (however not discussed in the manuscript). I think this positive bias can be explained by the way drainage is estimated (see also comment R1).

Another aspect I do not understand is the storage and the convergence criterium that is left out. I agree with R1 that 'steady-state' in combination with a timestep is a bit confusing. How I understand it, is that you run the model over 100 years forced with the same climate data until an equilibrium is reached (i.e. a steady-state). I think for this kind of procedures the term 'dynamic steady state' is used often. (I certainly would not call it transient). What I do not understand, for such a dynamic steady state you still need a storage coefficient, so how does that work? Also, it is not yet clear what you used as a criterion to stop your run. It is written that the convergence criterium is not used, as running the model beyond 100 years did not improve model performance. But how did you decide that 100years were enough; did you check your model outcomes, estimate R for water tables and when that looked good you stopped it. Or was it wall-clock time driven, or CPU time driven? I think whatever criterium you used is fine, but now it raises questions.

I fully agree with R1 on point C and more extensive sensitivity analysis should be done.

From the results it cannot be concluded which model change has the largest impact on the results.

- I agree with R1 and I found the discussion of your work very limited, and not providing enough information to fully understand and acknowledge the importance of the work done.

In my opinion, a relevant aspect of the discussion that is not/not enough elaborated is where we stand now and how it will help is further. How useful is your model in reality, as it is a steady state model approach, not simulating groundwater gradients, calibrated for New-Zeland, under natural conditions only, only unconfined aquifer systems? Are there now model that can do this maybe better, and under real world circumstances (i.e. current climate conditions and human impacts).

In other words, if you need to advice the New-Zealand water managers, how should they use the model and what do they need to know about the model structure and uncertainties to interpret the results correctly and use the model to its full potential? It for which purposes can the model not be used, and what should be improved to make the model useful for the more real world simulation (varying climate and human interactions).

Reading the authors comments on R1 point C I think the authors should be careful in saying that regions where not modelled before (is New Zealand not included in the large-scale models, I think so); stressing the computational efficiency (how efficient is the model, and how does this compare to other large-scale model efforts?).

Minor comments: In the introduction a bit more details on the modelling should be given: 1 to 2 lines saying it is a flux-based approach, simulating steady-state water table heads, using averaged climate conditions, run for 100 years etc.

Related to analysis and scatters: I would suggest to present the R2 (coefficient of determination) is stead of R

P7-L16: "drained by humans"; artificial drainage? P8-L6-7 "who . . . . ." Leave this out, it is not relevant as you do not use the parameters of Gleeson. P6 L5 "the improved NWT"; is this the same at L4 "the NWT" or is there also an improved version (leave out improved).

F8: it would be more logic to switch those scatters, so that wte, discussed first, becomes (a) and wtd (b) (same for the other scatters).

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2018-235, 2018.