Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2018-235-RC1, 2018

Hydrology and Earth System Sciences
Open Access
Discussions

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 #1

Received and published: 13 June 2018

The manuscript describes the adaptation of the global steady-state groundwater flow model of Fan et al. (2013), with a spatial resolution of 30 arc-sec (1 km) to New Zealand, resulting in an estimate of depth to groundwater and elevation of the groundwater table everywhere in New Zealand at a spatial resolution of 200 m. Adaptation was mainly done by using input data that were developed specifically for New Zealand with a higher spatial resolution than the input data of the Fan et al. model (Digital elevation model, groundwater recharge) but also using other (likely better) estimates of hydraulic conductivity. They compared model results for two regions with groundwater well data within New Zealand and found a somewhat better fit to the observations than that is achieved with the global scale model.

I suggest rejecting the manuscript for a number of reasons.

A The presentation of the research is done poorly. A1) There are linguistic weakness (e.g. in the abstract: inconsistent terms and meanings with respect to smaller-scale/small-scale/local models and larger-scale/global models, l9: "because the quality of their, coarse and global-scale, input data is large", l1: "larger, i.e. global", while larger here should also refer to national).

A2) The scientific terminology is not always used correctly, and some statements seem to be wrong (e.g. p6, l4 "ground-based, satellite-observed and modelled parameters"; p8 l16: g is not the gravitational constant but gravitational acceleration, and it is not 9.90 m2/s but 9.81 m/s2. Why "rainfall recharge" instead of "diffuse groundwater recharge"?

A3) The reason for setting parameter values are not always clearly given, e.g. p8 l27: "As cell resolution of the NWT model is 200 m, the values of a, b and fmin of Eq. A2 were changed accordingly, to 75, 150 and 4". To what extent does the cell size leads to is e.g. setting the value to 75 instead of 120 in the global-scale model?

A4) The manuscript does not explain, except in the Appendix, one major simplification of both the global (EWT) and the New Zealand (NWT) model: There is no hydraulic-gradient dependent interaction between groundwater and rivers; where the water table reaches the land surface, the groundwater is assumed to flow out. This, however, may be the main reason for the dominant overestimation of groundwater table elevation as compared to observations as river levels may be below the land surface elevation. Also, losing rivers cannot be simulated.

B Methodological weaknesses, combined with confusing presentation: In the manuscript, it is stated at various locations that the model was run in daily time steps for 100 years. However, a steady-state model cannot be run at daily time steps, because per definition there is no time variable in a steady state model, and the change in hydraulic head over time is zero. In addition, to do transient runs, one would need to set a storage coefficient, which is not mentioned in the manuscript.

C Lack of new information/innovation that is of general scientific interest The analysis is lacking components that would lead to improved scientific understanding. I suggest to analyze the specific reasons for the better fit of the NWT model results to observations. In a type of sensitivity analysis, variants of the NWT model could be run, in which only one “improved” data set is included while the other data sets remain those of the EWT model. Or alternatively where all but one data set is improved. For example, to understand the impact of the new hydraulic conductivity approach, do one NWT variant in which the approach used in the EWT model is applied. This would be a useful analysis to support the suggestion in the last paragraph of the discussion to use the hydraulic conductivity approach used for NWT (Gleeson data) also for the global-scale EWT, to improve it. But it was not shown in the study whether with the EWT hydraulic conductivity approach the fit to observations in New Zealand would have been better. Similarly, the groundwater recharge estimate used for EWT could be used as input of another NWT variant, and the resulting water table elevations could be compared to the standard NWT results to understand the importance of improved/national groundwater recharge estimates. Then, the presumably large role of the DEM in improving results would be clearer, and your concluding statements would be more firmly based.

I would also suggest adding to Fig. 8 the simulation results of EWT to directly visualize the improvement of NWT over EWT, and adding to Fig. 11 also the results of the standard NWT with a spatial resolution of 200 m, not only the LiDAR-based 100 m variant that is shown (but not indicated in caption).

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