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Discussions

Interactive comment on "Technical Note: Reducing the spin-up time of integrated surface water–groundwater models" *by* H. Ajami et al.

Anonymous Referee #1

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General comments

This paper focuses on the description and evaluation of a procedure for reducing the time of model spin-up, which is a commonly adopted strategy to initialize integrated/coupled hydrological models such as ParFlow.CLM.

In my opinion, there is a core issue within this paper related to its very basic idea, i.e., the assumption that an equilibrium state (achieved over no matter how many years of forcing data) can represent a correct (or even reasonable) initial catchment state. Although I acknowledge that this is a common assumption, I believe that not only it is not true in general, but the number of cases where this could be reasonable is limited, in theory, only to catchments where i) the land use do not change over time and ii), most

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importantly, the inter-annual variability of the weather forcing is very small. The latter point is equivalent to the assumption that a single year (or two, three) of forcing data can be considered representative of the whole climatic regime of the catchment, an hypothesis that is never realistic in practice. Unfortunately for hydrologists, catchments are always dynamic systems and never in a state of equilibrium; therefore, I am afraid that the whole procedure proposed in the paper is not worth the effort from the very beginning. Instead, the only way to achieve a correct or reasonable initial state is to use a "warm-up" procedure, where the model must be run using a long enough time-series of forcing data before the period of interest; the necessary warm-up duration will be obviously catchment-specific and can be evaluated by starting the model with two or more different initial guesses and checking that after the warm-up all the simulations converged to the same final (dynamic) state. Another issue of this paper regards the lack of important details, such as (at least) a brief description of ParFlow.CLM, and some steps of the procedure that are not described with sufficient clarity. See below in the list of specific comments.

Specific comments

Page 6971, line 20: please define "service unit".

Page 6972, section 2.1: despite the title, no description whatsoever of the model is provided, but only a description of the two catchments.

Page 6975: lines 7-19: this section is rather difficult to follow. Is the DTWT function used to re-initialize the model spatially variable or uniform? And the resulting DTWT distribution after re-initialization? Also, it is not clear how the "best performing" DTWT functions were chosen: what objective function was used to evaluate the best performance, root mean square difference, mean absolute error or the semi-variogram?

Page 6976, lines 2-3: why is the pressure head profile in the UZ adjusted with a (basically) instantaneous distribution, taken from the last day of the sixth cycle, while the re-initialized DTWT is assumed as an annual mean? I see a possible lack of consistency that should be discussed.

Page 6977, lines 1-17 and Fig. 3: I am quite puzzled by these results. From Fig. 3a, I would expect that i) the MAE of the Exp2-Catchment curve decreased with time, not the contrary, especially after year 14, and ii) the MAE of the two Exp1 curves was larger, not smaller, than the Exp2 curves. Have the authors any explanation for this?

Page 6977, lines 18-29: it is not clear how the semi-variograms were calculated. Was the mean annual DTWT used?

Page 6978, lines 20-22: this sentence is not clear, please rephrase.

Page 6979, lines 5-9 and Fig. 6: from the figure I cannot see how the adjusted vertical pressure distribution produces better results than the hydrostatic profiles, nor I can see the bias with the latter. Perhaps, would be a good idea to show the experimental pdf (hystogram) of the differences along with their spatial distribution.

Page 6979, line 16: why does the smaller Baldry catchment require more service units than the larger Skjern catchment? Is it because the former has a larger grid size due to a better DEM resolution?

Page 6981, lines 1-2: I do not agree that the proposed procedure “has the potential to assist in parameter calibration”. Due to equifinality, if a wrong initial state is used, such as the one likely to achieve by assuming equilibrium, a calibration procedure could lead to strongly biased parameters.

Page 6990, Fig. 5: I am surprised that the hydrostatic equilibrium procedure underestimates the groundwater storage even from the start of the simulation. If the DTWT at re-initialization is the same as for the adjusted pressure profile, how can the Authors explain that large bias?

Page 6992, Fig. 7: there seems to be a spatial pattern, with streaks of DTWT overestimation in the south of the catchment. How can this be explained?

Technical corrections

Page 6970, lines 18-19: change the sentence to “The issue of model initialization is important for hydrologic predictions as the initial state has a major impact on the catchment’s modeled response”.

Page 6979, line 21: correct “particluar”.

Page 6974, line 17: DTWT was 3 m only for the Skjern catchment.

Page 6977, line 20: change “semi-variance” to “semi-variogram”.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 6969, 2014.