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Interactive comment

Interactive comment on "Analyzing the impact of groundwater flow and storage changes on Budyko relationships across the continental US" *by* Laura E. Condon and Reed M. Maxwell

Anonymous Referee #2

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Overall, I find this manuscript to be interesting and in general, well written. It is also impressive to see these types of large, physically vigorous integrated GW-SW models applied towards problems that have typically been addressed with predominantly empirical methodologies. That being said, there are a number of issues with this work that need to be addressed prior to publication. Especially, I believe the authors need to address the major limitations in their approach.

Issue 1: Representation of the groundwater flow system.

As I understand from the description of the model domain, groundwater flow is primarily constrained within a single layer that extends from 2 m to 102 m below ground surface. Conceptually, this must mean that groundwater movement is restricted to a 2-

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Discussion paper

dimensional plane parallel to surface. With such simplification, can the model really be used to assess the groundwater component of the water balance within nested watersheds that range from 10's to 100,000's of sq-km? If one can assume the presence of Tothian flow systems across much of the domain, how can local groundwater flow conditions develop when in essence the local systems will be overprinted by large regional flow? If the results of the work did not have such strong dependence on the model's ability to simulate the water balance in small watersheds this would not be such an issue; however, given the large number of small watersheds that are considered in the analysis, I believe it is crucial to have a more highly resolved subsurface domain. Even one or two additional layers within the groundwater flow zone would allow 3 dimensional groundwater flow systems to develop, and hence facilitate the model's ability to capture local flow systems that are a key part of the water balance, especially in humid areas with notable topographic variability such as the Eastern and Northwestern regions in the domain. Furthermore, a graphic that depicts how the watershed nesting has been conceptualized within the model domain framework would be of value to readers.

Issue 2: Applicability of the Gleeson et al. (2011) dataset.

While it does need to be recognized that parameterizing the subsurface component of these large integrated models is challenging, and the use of large scale homogeneous datasets is particularly attractive, modellers must be cognizant of the limitations that these datasets impose on the model. This point is highlighted with the Gleeson et al. (2011) subsurface permeability map, which is an extreme simplification of subsurface hydrostratigraphy in order to facilitate global coverage. For an application such as the one of focus here, the Gleeson et al. dataset does not provide adequate spatial accuracy/resolution for credible model results to be generated for smaller-scale watersheds, and considering the large weighting that smaller-scale model results for the groundwater component of the water balance are given in the analysis, this is very problematic.

Issue 3: The use of modelled ET.

Typically, and especially in large scale models, it very difficult to accurately simulate ET. In this work the authors use simulated ET as a surrogate for measured ET across much of continental USA. Considering the extreme importance of ET in the water balance analysis, I wonder if biases or errors in simulated ET may not be skewing the results. This point may be highlighted in Figure 2d, where it appears that groundwater recharge is unrealistically high across much of the Great Plains. Furthermore, as Figure 10 highlights, the analysis results that are dependent on simulated ET show a strong deviation from the results generated from the other two water balance calculation methodologies.

A few other minor comments are as follows:

L16: be careful with use of ‘realistic’ this work is more conceptual in nature

L96: . . .a physically. . .

L120: abcd?

L135: expense,

L142: comma not needed

L150: technical feasible yes, but how realistic is it to extract local scale information from continental scale models?

Eqn 1: Are the units expected to balance?

L167: Verify units for q

L201: Ev, and. . .

L263: Is a single year really ideal?

General comment: a histogram showing watershed size distribution would be valuable.

L298: balanced

L314: for

L331: opposed to

L340-343: This statement is not really supported by Figure 2, which is presumably (authors should state this in caption) depicting ratios in annual totals. This statement is also irrelevant to main objectives, suggest deletion.

L396: one,

L410: and often

L437 onwards: these results and discussion should be supported by at least some basic curve fit statistics.

L514: correlation discussion should be supported by some rˆ2 values.

L576: higher curve numbers? Visually the results look the same, could statistics be used here as well?

L578: do you mean Fig. 6?

L603: 100? Do you not mean 1000?

L607: References needed here.

Figure 9: General observation here: As the authors state, the analysis results that focus on the larger watersheds provides a better match to the idealized curves. Could this result be at least in part due to the major issues identified earlier in this review that highlight the weaknesses in the authors approach towards simulating the groundwater component of the water balance for small watersheds?

L612: No doubt watershed characteristics are important, but what about limitations with the way groundwater flow in small watersheds is represented in the model, and the adequacy of the employed datasets for meeting such finely resolved objectives?

Summary and Conclusions: General comment for this section is that it is too long and there is too much detail provided in the reiteration of methodology and results. Suggest shortening by at least 50% and focusing more on the significance of the results.

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