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Discussions

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 #3

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Overall, I think this paper has the potential to turn into a good contribution that elaborates the influence of groundwater on the Budyko Hypothesis. The paper does not seem to have a well-described objective. I did not see a set of research questions or hypotheses to be tested. All the results presented in the paper are based on a single water year simulation in the ParFlow model, which is a fairly short time scale to convincingly report and use any groundwater related modeled variables. As I tried to figure out what the objectives of this paper might be I kept asking myself the following questions. Is the idea to:

1) develop a conceptual model for incorporating the role of groundwater (GW) to the Budyko hypotheisis (BH)?

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2) parameterize the contribution of GW in the BH by relating the w parameter in the Zhang equation to a GW variable that may be obtained from observations or models, which can be used as a simple model?

3) evaluate model results to see when a Budyko type behavior is generated in systems where GW cannot be neglected (e.g. Fig 4), by modifying the source of water in the axis of the BH plotting position?

It was never clear to the reader why three water balance conceptualizations were used and why w were calculated for all three of them (Figs 5, 6, 7). The authors need to state what their goals were.

If one needs to improve the use of the BH for regions where GW can not be neglected, one could work with the original model inputs of observed P and Q, and calculated Ep, and parameterize w = f(GW, E/P, Ep/P) and use this w in the original model and test it .. In your case E would come from PArFLow. Apparently this does not seemed to be the objective of this paper, but I felt that Fig 6b came close to this idea but stopped there.. Finding w value for the indirect method (Fig 6a) did not make sense to me as E=P-Q won't give the "correct" ET and therefore why would you calculate w using this ET/P. Please better state what you objectives are.

Interpretation of Figure 4,5,6,7 need help. The paper does not sufficiently discuss the processes that lead to patterns in these figures.

Abstracts lines 25, 26âĂŤwhat do you mean by best results? Best of what? Is the "best" represent better predicted water balance by the BH, modified in this study, against modeled water balance? Or did you develop a simpler model of water balance that gives consistent predictions with ParFlow?

Modeling methods: In this study modeled data comes from ParFlow, which was used for only a single water year (1985), starting from a steady-state groundwater configuration. Obviously the question is – why would you use a single water year.. I wonder if the system won't respond to this steady-state assumption when you start running the model with the actual climate forcing of 1985. The paper mentions that PArFlow simulations were done for historical climate in CONUS. I wonder why the authors did not use the full length of simulations and evaluate the –mean annual water balanceâĂŤwith GW contribution in the BH hypothesis, instead of just using a single year which I presume creates some rapid transient conditions in the beginning of the model run as the water table would respond to the 1985 forcing. Running the model with a historical climate forcing data and evaluating the long-term water balance with long-term-average estimated flux variables, including groundwater seems to be the logical way to go. I'm having a hard time accepting the justification of the use of a single water year. BH is ideal for long-term-aver water balance conditions as well. So logic tells me to use longer simulations.

In the methods the G term need to be more clearly explained in my opinion. In reading the paper I went back and forth a few times to make sense of what authors might have meant by G but I'm still not clear.. My intuition tells me that groundwater contribution would be the net volume of groundwater staying in the basin at the end of a water year.. I imagine G is not always a contribution as in some cases G may flow out of a basin in which case G will be a sink term. Your groundwater surface water exchange can practically be infiltration or saturation excess overland flow.. I'm not following what this definition means in the context of eq.(6). Your sign convention in the G/P plots should be explained.

Line 319– I'm not following this para.. shouldn't a positive G mean that the watershed receives flux across its groundwater boundaries and a negative G indicates net export of GW to surrounding basins.. Water that infiltrates to subsurface would just increase the storage of GW wouldn't it.. This water may stay in the watershed or exported out.. seems like concepts are a bit miss-use here or not explained clearly.. Perhaps you use a Delta Storage term in 6 and 7 and explain these referring to the storage change etc.. hard to follow here..

Line 353- I'm not clear how G was calculated.. Above you said you used eq (6).. here G/P>0 is interpreted as storage gain.. Headwater of Missouri should be recharging the system and therefore they are not GW exporters.. but the region below in ND and Nebraska area should be net exporters right..? so I was expecting to so G/P<0 in northern basins and G/P>0 toward the middle of Missouri where it connects to Mississippi.. Please better explain conceptual model. Please clarify– if G>0, there should be net input to the watershed and Effective precip should be P+G, and if G<0 there is net export from the watershed and Effective P=P-G... Is your formulation consistent with this?

Line 419.. I would not cite Istanbulluoglu et al., 2012 here. Istanbulluoglu et al showed the limitation of assuming ET=P-Q in the Budyko curve, and proposed to use ET= P-Q-DeltaS, where DeltaS is change in groundwater storage assuming no net export/import of GW.

Line 453..don't cite Istanbulluoglu et al., 2012 here

Line 525.. Istanbulluoglu et al., 2012 used the inferred ET approach to show its limitations– not as the proposed method to calculate ET from P and Q. Incorporating the contribution of groundwater in the water balance equation to calculate ET led to a more consistent trend in the evapotranspiration ratio and aridity index.. See Figs 6a,b and Fig 7a,b,e,f. I think this paragraph should better summarize their results.

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