Hydrology and
Earth System
Sciences

Discussions

Open Access

HESSD

Interactive
comment

Interactive comment on "Reinterpreting the Budyko Framework" *by* Nathan G. F. Reaver et al.

Anonymous Referee #1

Received and published: 30 December 2020

The manuscript entitled "Reinterpreting the Budyko Framework" by Reaver and colleagues highlights several misconceptions regarding recent interpretations of results obtained using the Budyko framework. The authors especially criticize the common assumption that the wealth of functional Budyko curves represent expected trajectories through the Budyko space. By considering a stochastical model and observations from several hundred catchments, it is shown that catchment behavior in time does not follow the predicted trajectories within the Budyko space. The authors further highlight specific parameters used within parametric Budyko equations do not represent catchment-specific biophysical features. The authors thus conclude that Budyko-based results should be interpreted more carefully and thoughtfully.

The manuscript is generally in good shape, overall well structured and well written. The introduction motivates the study and provides an in-depth overview of the recent research within the field. It needs to be acknowledged that this paper addresses a

somewhat heated debate on the interpretation and applicability of the Budyko framework in the context of biophysical drivers influencing the terrestrial water and energy balance. However, it is my assessment that the line of arguments and conclusions, as presented in this paper, are mostly adequate. The supporting data and examples seem valid, but I would appreciate a more in-depth justification of several assumptions. I have provided a relatively small number of comments. However, I would also like the authors to consider two more general remarks:

(i) I largely agree with the general conclusions of the paper. However, I know (from my own experience) that the debate on the interpretability of the Budyko framework is somewhat heated. Therefore, I think it needs to be noted that the Budyko framework remains a powerful concept when interpreted and applied correctly. And I don't think that you necessarily "reinterpret" the Budyko framework.

I fully agree that it needs to be acknowledged that there has been a rather large number of recent studies that overinterpreted results. Nonetheless, these studies still present results that are valid and sound within their specific setting. However, any interpretation going beyond these settings is not adequate, which needs to be acknowledged without condemning previous research. You already highlight this in your introduction, but I think you also need to be more careful throughout the rest of the manuscript (see some of the more specific comments below).

In this spirit, I would like to see a more positive evaluation of the Budyko framework per se. I think that the framework, given its adequate application and interpretation, remains super useful. It might thus help to better outline how upcoming Budyko-based research can profit from considering the limitations highlighted in this study. Your conclusions already provide some suggestions, but I still think that the Budyko framework has more potential besides being a global constraint (p. 25,l. 14), as it can also be applied within well-defined setups.

(ii) Your theoretical example using Porporatos model is neat. However, it is still an

artificial example and also needs to be interpreted as such. You use one model (Por-poratos model) to investigate the characteristics of another model (Budyko). Fine, but you need to thoroughly justify that Porporatos model is an appropriate choice in this context: Is the choice of parameter values for the different cases realistic? What kind of conditions do these parameter values represent? Is there any real-world example that would illustrate your choice?

Additionally, as the parameters might be independent within your theoretical model-ing framework, they might not be independent under real-world conditions. That also represents another problem of the large number of studies trying to identify biophysi-cal controls. There is no single parameter that controls the partitioning of precipitation into evaporation and runoff. It is rather a convoluted mess of different processes that interact with each other.

Minor comments:

p. 3, l. 7: Please also consider Padron et al., 2017. It provides a comprehensive overview of inconclusive and contradictory evidence obtained from using eq. 6.

p. 4, l. 4-7: It will be helpful to already mention those equations here.

Sec. 2.3: I think it might help to incorporate this section into Sec. 2.1?

p. 10, l. 14: It might be helpful to further explain what you mean by "Budyko-like"?

p. 10, eq. 8: Could you explain in more detail how you estimate the aridity index from this equation?

p. 11, l. 15: Why 2m? Well, this is related to my second major comment above. The choice of these parameter values needs to be justified. What kind of soil characteristics does Z0=2m represent? I know that your overall conclusions won't change when set-ting Z0=1.9, but it is important to understand what it means and what kind of real-world characteristics your choice represents.

Sec. 3.2: Why don't you include this subsection in the Background part (Sec. 2.)?

p. 14, l. 8-10: Is that true? Are Eqs. 5 and 6 considered the only valid parametric Budyko equations? Do you have more evidence for this statement?

Figure 2: I know it is hard to convey all the necessary information into one Figure, but I have to admit that this one is especially difficult to interpret. The trajectories are a big mess (and to a certain extent this is exactly what you want to highlight here). However, Figure 3a is of more value in this context. If you like to keep Figure 2, maybe consider drawing thinner red lines or introduce some transparency?

p. 25, l. 1-2: Maybe this would be a better title: A reinterpretation of explicit Budyko curves and parametric Budyko equations.

p. 25, l. 20-26: I agree that the interpretation of the parameter representing landscape features is misleading. Calling it a catchment-specific parameter is not justified either. However, even though it is a lumped parameter just existing as a mathematical necessity without any a priori physical interpretation, there might still be an a posteriori physical interpretation. You call the parameter a proxy variable for E/P, which is, in fact, also some sort of physical interpretation. That means, if you assume a constant aridity index and change E/P, the parameter changes as well. Vice versa, if you change the parameter, E/P changes as well. I think the misleading interpretation here is often more related to the assumption that the parameter somehow controls E/P, which is definitely not true.

p. 25, l. 27-28: This statement is too strong in my opinion (see also my first major comment). Any interpretation of obtained results is valid within their specific setting. However, it is the overinterpretation and generalization that is "untenable" (which is a very strong word in this context).

Additional References:

Padrón, Ryan S., Lukas Gudmundsson, Peter Greve, and Sonia I. Seneviratne. 2017.

"Large-Scale Controls of the Surface Water Balance Over Land: Insights From a Systematic Review and Meta-Analysis." Water Resources Research 53 (11): 9659–78. https://doi.org/10.1002/2017WR021215.

———————————————