The Budyko framework beyond stationarity

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Referee comment

Referee #1:

We sincerely thank Fernando Jaramillo for his positive, extensive and helpful review. We overall agree with his general comments and we will revise the text accordingly. In combination with his comprehensive list of specific comments, the manuscript will significantly benefit from his review. We will first address the general comments of Fernando Jaramillo, followed by a response to the main specific comments that are not related to the general comments and are not too technical or related to typos and grammatical issues.

General comments

1) It needs to state from the beginning that it only deals with temporal climatic intra-annual non-stationarity, not with landscape non-stationarity due to water use, land use and land cover change. The authors imply in the beginning that the term "stationarity" only relates to the climate component, but this is not true (See Milly et al., 2008, Science). Landscape non-stationarity is a fact and has been shown to have major implications for E/P at basin scales, I mention some references. I know the authors know this, but they should mention from the beginning what type of non-stationarity they are dealing with. They should mention from the beginning that their advance does not deal with the understanding of landscape non-stationarity.

This is an important point and we thank the reviewer for bringing this up. We will clarify in the revised version of the manuscript that we are primarily looking at temporal nonstationarity. (since the respective storage term in the water balance equation is a time derivative). However, we also like to mention that the additional parameter y0 is simply representing the amount of additional water (besides P) that is available to E (see Eq. 4b). In our understanding this does not necessarily exclude other processes (such as e.g. landscape non-stationarity) besides storage changes. We would like to see further investigations on this topic in future assessments.

2) The methodology is also rather cryptic and confusing. I think they are missing a complete methods section where they explain how they estimate their yo and K parameters at the global scale, boot strapping, resampling, calibration, validation, etc. If they want other scientists to use their model or "framework", they should clearly state what is the procedure to derive yo and K. Importantly, they also need to be more precise in introduction and conclusion on what ways this study differentiates from the former works (e.g., empirical Milly, 1993, Potter and Zhang, 2007, Zhang, 2008; and stochastic Zanardo et al, WWR, 2012) that also tackle climatic intra-annual non-stationarity. In other words why is their work "the robust, theoretical incorporation into the Budyko framework that is missing". Right now, it is not very clear.

Thanks! We will include additional, in-depth explanations on the applied calibration and validation techniques. We will also revise the main text accordingly. We also see the need of putting our work in context to previous assessments on similar topics and will enhance the respective part in the introduction and add a small paragraph to the conclusions.

3) Their exploration of their model (sensitivity analysis) is very complete and well developed, and the model appears to be robust based on the high correlations at the global scale and for the period 1990-2000. But in order to know really the advance that their models implies for the Budyko framework, it is necessary to show in what way it predicts E and E/P better than Fu 1981 or Zhang 2001,2004 and even better than Budyko's (1956, 1974). They indeed show good correlations between predicted and observed E at the global scale, but how much better than the previous models? For instance, Zhang's model 2004 was able to explain 89% of the variance, how much better is this one then? They should repeat their global analysis with the previous models they mention and compare. I would redo similar Figures 8 and 9 but with the former empirical models and compare.

We will update figures 8 and 9 in the revised version of the manuscript. But in this context, it is also important to argue from a physical perspective. The original approach might perform rather well on paper, but it does not at all represent the fact and the underlying physics of an exceedance of the original suppply limit. Strictly speaking, the original model is not at all applicable at monthly time scales, since the physically possible exceedance of the supply limit lies outside the physical limits of the model. The new two-parameter approach does in lieu correctly represent the points overshooting the original supply limit, but is still subject to huge data spread lowering its performance.
Hence, the new two-parameter model does not necessarily need to perform significantly better than the original model, since it is in fact physically correct at monthly time scales (thus doing the right thing for the right reason). The two-parameter approach is further flexible enough (when adjusting y0 accordingly) to also represent more complex seasonal hydroclimatological patterns.

Specific comments

Suggestion for the title: The Budyko framework beyond climatic stationarity

Thanks for the nice suggestion! We will change the title accordingly.

-Line 12- I don't think this study is a new framework but rather a good improvement or advance to Budyko's framework, or can this work compare to Budyko's framework to be also called a framework? But of course, this is the authors' choice. I think we scientists are now drowning in so many frameworks...

This is a good point and also refers to some comments of the other reviewer. We will change the wording in the revised manuscript and will introduce the new formulation rather as a modification of Fu's equation than as a new framework.

-Page 6801 line 9 Water use should be included in the list of factors affecting the scatter in the Budyko space. Position in Budyko space is a cause, but also a consequence, of movement in Budyko Space. Movement in Budyko and hence non-stationarity is also

attributed to human changes in landscape conditions by land use and water use or by changes in water phase (landscape changes; Jaramillo and Destouni, GRL, 2014), and this should be clearly stated here in this paragraph. Land use change is mentioned by the authors by including Donohue 2007, Zhang 2001,Li et al. 2013, however water use and most water phase changes are neglected. Hydropower and irrigation CAN affect, and rather substantially, the position of a basin in Budyko space, see as example Destouni et al. Nature Climate Change, 2013).

Thanks! We are well aware of the related and interesting work of the reviewer and will change the text accordingly to discuss issues of land-scape nonstationarity. See also our response to the first comment.

-Zanardo et al., WRR, 2012 deals closely with what the authors deal here, but from a stochastic point of view and should be included in the introduction, I think.

We will add a sentence on the findings of Zanardo et al. (2012) to the revised version of the manuscript.

-Page 6801. Line 23. Since the definition of "steady conditions" or "stationarity" is an important part of this study, the terms should be defined appropriately in the beginning. What do you mean by these terms? I assume the authors relate stationarity to steady state conditions. "Stationarity" is mentioned in the title of the manuscript but nowhere else in the text. Since steady-conditions instead are mentioned in several parts of the manuscript, I assume they mean stationarity as "steady conditions", i.e. no change in the storage term of the water budget. The authors relate "stationarity" to that dealt in the manuscript, i.e., that of the intra-annual climatic conditions that may change water storage at the annual scale. However, again, the stationarity assumption is affected also "by water infrastructure, channel modifications, drainage works, and land-cover and land-use change" (Milly, 2008, Science) and Review Fernando Jaramillo changes in water phase (Jaramillo and Destouni, GRL, 2014). This last work shows that changes in the landscape were responsible for non-stationarity in up to 74% of the basins of a global study once intraannual climatic non-stationarity was coarsely ruled out. Since this is not explored in their manuscript, I would appreciate if the authors could be more specific and mention the type of stationarity that their framework is dealing with, i.e. changes in water storage due to intra-annual changes in climatic conditions (Ep and P) as they mention in the first two lines of the Conclusions.

Thanks! As already mentioned in response to previous comments, we will revise the text to make clear what kind of stationarity is investigated throughout the manuscript. We will also more thoroughly explain the definition of steady-state conditions. As mentioned by the reviewer, our main intention is to explicitly represent the storage term in the water budget equation.

Page 6802, line 15 – Isn't the relationship found by the authors Eq. 9 also empirical? Please specify the difference between "empirical" and "analytical", since this is a main justification of this work.

An empirical relationship (or evidence) is usually derived directly from data or observations. Here we use very simple phenomenological assumptions, from which a mathematical relationship is derived analytically. We will clarify this in the revised manuscript.

Page 6804, line 2 – Why not <-1? Please specify for the reader.

This is based on the assumption that E<=Ep and hence the minimum value of (P-E)/Ep is -1 (if P=0 and E=Ep).

Line 11 – Again, in relation to my recent question, it should state if additions of water due to changes in the landscape conditions of water phase (melting glaciers, thawing permafrost, closing stomata by rising CO2 concentrations or systematic anthropogenic changes linked to water use, are accounted in this boundary condition y0. Or if these additions/subtractions of water are rather represented by changes in the mathematical constant k, following Zhang's w. Or if they are not accounted for at all.

This is again related to the response to the first general comment. The parameter y0 is a measure of additional water that is besides P available to E. This does in our understanding not exclude other storage components. However, investigating controls and drivers of the two parameters is a complex task and clearly beyond the scope of this study.

Figure 2 and 4 and text. There is something strange with the sign of y0 along the manuscript! In the

We apologize for this unfortunate mistake! The parameter y0 is defined between 0 and 1. The figure captions are wrong and will be corrected accordingly.

Line 7 to line 13 – Let's say I want to replicate the results. This explanation for the derivation of y0 and k for the global grid requires more wording because as it is now is rather cryptic. Forgive me if I understood incorrectly but since you use several combinations of P, E and Ep for each grid cell to minimize and thus estimate y 0 (Fig. 8 a and c), why do you then need the dataset values of P, E and Ep at all? Also, please explain in more detail the resampling, bootstrapping and least-square fit. Maybe a flow diagram of the procedure would be helpful. Also the difference between panel a and c or between b and d in Fig. 8 should be better explained.

See also our comment to the second general comment. We will include an appendix with an in-depth explanation of the methods. We further need dataset values of P, E and Ep, since y0 is minimized at each grid cell according to the given data. We basically identify the particular month (with the respective P, E and Ep values) that minimizes eq. 10 and maximizes y0.

Line 23 and 25- This procedure also requires more information, it is difficult to understand what was done here, it is cryptic: "anomaly correlations between "detrended" time series with removed annual cycles???? Explain please.

We apologize for the strange wording! We basically compute anomalies (i) by detrending and (ii) by removing the mean annual cycle of both the modeled and observed time series of E. The anomaly correlation is the correlation between the obtained anomaly time series. We will revise the text to clarify this

Line 28, 29. I do not know how to see that "...the annual cycle is well represented by the model" by looking at the four panels.

We will explain this in more detail in the revised version of the manuscript. The high correlation between both the modeled and the observed time series is basically an indication of a similar seasonal pattern (and thus an indication for a reasonable representation of the seasonal cycle in the modeled time series).