

The following is a list of technical comments and suggestions to the text and figures of the manuscript.

Suggestion for the title: The Budyko framework beyond climatic stationarity

Abstract. Line 2- “E” should be described from the beginning as “actual evapotranspiration”, not only “evapotranspiration”, for the benefit of the general audience of HESS.

-Line 8- Potential evapo-“transpi”-ration along the text

-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...

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-Page 6800-line 2. Milly, 1994 and Yang 2007 should be included here to be consequent since it is mentioned too in Page 6801 line 3.

-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*).

-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.

-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

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 intra-annual 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.

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.

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

Line 12 – "parameter"

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 y_0 . Or if these additions/subtractions of water are rather represented by changes in the mathematical constant κ , following Zhang's ω . Or if they are not accounted for at all.

Line 15 – "are" provided

Line 18 – I don't think this study is a new framework but rather a good improvement 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.

Figure 2 and 4 and text. There is something strange with the sign of y_0 along the manuscript! In the Figure caption, figure and text, y_0 is either <0 or >0 , please be consistent. Fig. 2 says that y_0 can be less than 1, e.g., $y_0=-1$ in the demand limit. So why is the sensitivity analysis of Fig 3 only in the range $0<y_0<1$ and page 6804 line 4 specifying the range of y_0 as $0<y_0<1$? Also, shouldn't the labels of the lines in Fig. 2 $y_0=0.2, 0.4, 0.6$ and 0.8 be negative? The Page 6805 line 7 says that $y_0=1$ in the demand limit when in Fig. 2 the limit is in $y_0=-1$???

Figure 3 and 4 and 6– Cosmetic comment. I think that in general red is associated with high, and blue to low, so I would suggest an inversion of the color scale as in Fig. 7. In Figure 9 it makes sense because it is a correlation, blue good, red bad.

Page 6806 line 2 – "the" demand limit

Line 10 – Again, same comment as that to Page 6804 line 11

Page 6807 line 17 – what parameter?

Line 23 – Reference missing for GPCP

Page 6808. Line 5 – what parameter?

Line 7 to line 13 – Let's say I want to replicate the results. This explanation for the derivation of γ_0 and κ 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 γ_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.

Line 11- Minimizes instead of maximizes?

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.

Lines 26-27 Please explain to what panels are you referring to in the figure and in general along the analysis of Page 6809:

Example: ...correlations are relatively large in many regions (**Fig. 8 a and c**), whereas anomaly correlations are smaller (**Fig. 8 d and d**), please refer to the figures in the text of the analysis so that the reader can follow and understand.

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.

Page 6809 and Figures 8 and 9 – Important! It would be wise to repeat the exercise shown in Figures 8a-d and 9a-d but with the original model that the authors refer to (Eq. 1; Fu, 1981; Zhang, 2004) (Eq. 1) and even Budyko's. In this way, correlations between modelled and observed E could be compared and the reader could really see the advantage of using the authors model (Eq. 8) to predict E globally based on γ_0 and κ and not ω ; Eq. 1.

Page 6809 line 10-11. Please state that you refer to the regions with negative correlation.

Page 6010 line 13-14 How did you account for seasonal variations in the parameter set?

Lines 15-19 I would not discard also changes in the landscape conditions of water phase and storage as a possible reason for this cause or land use and water use. When E is calculated directly from water budget, that means $E=P-R$ assuming change in $S=0$, then many basins locate in Budyko space above the demand limit. This means that the signal of land use and water use change and changes to of water phase (melting glaciers, thawing permafrost) are already reflected in the basin-constrained value of E. See Jaramillo and Destouni, "Developing water change spectra and distinguishing change drivers worldwide", Fig. 4a, ellipsis that although cut extend beyond the demand limit. Nevertheless, I agree with the authors that this also implies uncertainties in the parameters.

Conclusions, Lines 12 and onwards should be within the analysis, not in conclusions.

Review Fernando Jaramillo

Line 17 – What numerous studies?? References

The reference to Greve et al. GRL, 2015 at the end of the manuscript should be better included in the text. As it is now, it looks like a last minute addition.