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Comment on hess-2021-55

Anonymous Referee #1

Referee comment on "Drivers of drought-induced shifts in the water balance through a Budyko approach" by Tessa Maurer et al., Hydrol. Earth Syst. Sci. Discuss.,
<https://doi.org/10.5194/hess-2021-55-RC1>, 2021

Review on "Drivers of drought-induced shifts in the water balance through a Budyko approach" by Maurer et al. submitted to HESS

General comments

The authors compiled temperature, potential (PET) and actual (ET) evapotranspiration, precipitation (P) and runoff (Q) data for 14 Californian catchments from a 34-year period with three drought periods to analyze the dependence of ET and Q on P and PET using the Budyko framework. By an innovative approach they quantitatively distinguish drought-induced changes that would be expected within the Budyko framework ("regime changes") from "partitioning" changes that can only be explained by a shift of the curve parameter(s) (in this case, the omega parameter of the Fu equation). They find that regime changes dominate observed changes in ET and Q, while partitioning changes still add non-negligibly to changes especially in some catchments. The topic is relevant to HESS, the methodology sound and original and the results can help understand catchment responses, with the proposed methodology being a potentially useful comparatively simple tool for many other studies in the future. While a number of suggestions for improvement are given below, many of them (hopefully self-explaining which ones) are optional such that from my point of view the manuscript can be accepted after minor revisions. The maybe most relevant single suggestion is avoiding misinterpretations by readers about the degree of novelty of the approach by better acknowledging existing literature on interpreting and decomposing changes in Budyko space (see detailed comments on L84 and L263).

Specific and technical comments

L42 "wetter, monsoon region in China": something seems to be missing in sentence, check

L72: be=>been

L79: A recent study which among others also briefly looks at drought in a Budyko framework: <https://doi.org/10.1098/rstb.2019.0524>

L84: "new framing": This is a bit misleading. Although I'm not aware of your exact

methodology (way of decomposing) having been applied to your exact question (distinguishing two directions of drought effect) before, the general idea of using movement along vs. perpendicular to curves in Budyko space to distinguish processes (e.g. climate variability from land-use) is quite widespread, occasionally also including quantification efforts. It would be good to re-check the literature, cite a few examples and adapt the wording. Starting points might be e.g.

<https://doi.org/10.5194/hess-22-567-2018> (which is already cited but not with reference to the decomposition idea) and [doi:10.1029/2011WR011586](https://doi.org/10.1029/2011WR011586). It would be good to discuss somewhere how your suggested terms "regime shift" vs. "partitioning shift" relate to already introduced terms in such sources. Both, differences in methodology and scientific reasons, can justify your choice of terms (e.g. "climate" vs. "residual" in Jaramillo et al. implies a claim about the causes which it seems you could partly disprove for some catchments); but still it is important for readers that not each paper "reinvents the terminology wheel" without referring to past suggestions.

L104 PRISM may be a well-known climatology dataset in the US but the description focuses on the interpolation/regression method and does not specify the ultimate source of the original data input to the downscaling / interpolation (e.g. station observations or reanalysis?). Please add a sentence on that so readers all around the world can better judge the potential strengths and weaknesses of the data.

L105: I guess that inavailability of radiation data was the reason to choose a comparatively crude, less known, semi-empirical PET approach such as Hamon? Here or later e.g. in the discussion, it would be good to comment on the effect it might have had on results.

L107: Please add one or few sentences on the cornerstones of the ET estimation methodology of Roche et al. 2020. Together with the runoff mentioned in the next sentence, you have everything you need to "close" the water budget (i.e. check for gaps and surpluses in $P = ET + Q$) and / or quantify the Budyko input parameters P, PET, ET and Q without determining any of them residually, which is good; however, this is only perfectly true if the methodologies to quantify each of them do not implicitly use one or more of the other parameters. As far as I can judge from a quick glance into Roche et al. 2020, this is not a (big) problem here but readers should be put in the position to get a first idea without reading the reference.

L113: To build further on the comment before, it would be good to report (here, results section or supplement) how large the needed corrections to P were and how much they differed between basins, to give an idea of the overall quality of the dataset - or rather, it's weakest (most assumption-dependent) parameter, which might actually have been ET rather than P.

L119: Mention both PET/P and ET/P consistently as symbols, in words, or both.

L123: (Du et al., 2016) => Du et al. (2016). Same at L126 for Thomas and possibly more places.

L125-133: Difficult to follow. Consider rewording and/or showing the equation(s), if needed in the supplement.

L152: Remain consistent about writing omega as a symbol or a word.

L156-163 and Figure 2: The description in the text at the end of section 2.3 and the graphical description in Figure 2 b do not seem to match. I believe the text is "correct" in the sense that the regime shift is consistent with its definition ("what would be expected according to Budyko/Fu") and the partitioning shift is the rest such that both add up to the

total observed shift in $ET/(P-\Delta S)$. However for the Figure to match this, the vertical blue "partitioning shift" arrow would need to start near the tip (not the foot) of the red arrow / near the centroid of the + symbols, and its tip and the triangles (which are not to be referred to in the text, I think it should be the true observed data of the drought years?) should be further to the (upper) right on the $\omega=3$ line. The difference between these two ways of illustration matters because the distance between the two F_u lines changes with aridity index.

Figure 2: Compute more nodes of the F_u equation to make the lines smoother

L167: with respect *to* runoff?

L169: How did "amount of available storage" and the methodology used to estimate it relate to the ΔS values and abcd model used to estimate it earlier?

L175: is estimated *from* average...?

L192-193: Unclear: If you refer to changes between droughts (as opposed to between drought and non-drought), then why is there only one difference value per basin given although there were three drought periods?

L201 / S2: How can a relative error still have units of mm? In case of doubt, specify relative to what / briefly explain the methodology.

L202-203: Were these years excluded from the calibration? Not that I'd like to suggest to do so, it's just that the current wording almost seems to suggest so.

Figure 5: If regime shifts and partitioning shifts behave strictly additive (without any nonlinear/interactive terms), which it looks like and would be consistent with the methodology description near L163, wouldn't it be more intuitive to use stacked columns? E.g. plotting partitioning shift on top of regime shift - if they have the same sign, the total column length is the total shift, if not the resulting total shift could still be a point marker within the column?

Sect. 3.2 in general: While excessive, or rather wrongly interpreted, significance testing is meanwhile disputed (e.g. <https://www.nature.com/articles/d41586-019-00857-9>), could you think of a simple methodology to roughly transfer what is said from the K-M-tests about changes in the two indices (L211) to the importance comparison between regime and partitioning shifts? While the text qualitatively already tries to convey this message, inspection of the point clouds in Fig. 4 seems to suggest even more that only few catchments (maybe only Kaweah, Kern and Tule) saw a "significant" partitioning shift, while the shifts in all other catchments might be within the range of uncertainty indicated by the scatter of annual data, and thus statistically indistinguishable from "pure regime shifts". Maybe a simple way to try to do this could be to compare the partitioning shift to what would have been significant at $p=0.01$ or 0.05 in the total ET/PET shifts. A more complex way could be a Monte Carlo type approach where years are randomly removed from the drought / non-drought subset, but maybe this would be overdoing it.

L246: "Tule. northern..." => "Tule. Northern..."

L263: See comment on L84.

L2278: 10 times less: Is this mentioned anywhere in the results section or at least supplement? Sorry if I overlooked it, but it seems to come a bit out of nowhere here.

L353: "become drier" - specify in which sense (e.g. less runoff?)

Figure 6 and Table 2: Maybe I overlooked something but other than for the aridity index threshold, which was explained and discussed at L233, the thresholds for the other three parameters are poorly or not connected to the manuscript text (both in terms of explaining how they were determined and of discussing their implications).