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Comment on hess-2021-55

Ryan Teuling (Referee)

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-RC3, 2021

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

The manuscript by Maurer and co-workers addresses the issue of changes in water balance partitioning during drought. This is a relevant topic that fits well within the scope of HESS. The authors use a novel combination of methods and data, to arrive at the conclusion that not only the position along the Budyko curve changes during drought, but also the Budyko parameter reflecting the catchment functioning. The manuscript is generally well-written and nicely illustrated. However in contrast to reviewer #1, I unfortunately have some serious concerns about the robustness of the results, and the main motivation for the study, that I think need to be addressed. These are discussed in more detail below.

In the Introduction, the authors state that "A particular focus is the change or shift in the precipitation-runoff relationship during droughts, which usually results in less observed runoff per unit of precipitation than would be predicted using non-drought relationships" and that "it is not fully understood which hydrologic mechanisms trigger them". I disagree with this statement, and thereby unfortunately with the main motivation for the study. No hydrologist would claim that runoff response to a unit precipitation input should stay the same across different moisture regimes. In fact, it is well known that the runoff response is a strong and highly nonlinear function of catchment storage on short timescales (i.e. Kirchner, WRR, 2009), which is at least in part related to the nonlinear relation between soil moisture and unsaturated hydraulic conductivity (the main understanding of which dates back almost a 100 years). There is no reason why this would not work similar at longer timescales. The questions here is what we actually don't understand about drought and water balance partitioning, and how the proposed method using a highly conceptual model can provide more insight into this. I believe the authors should do a better job here in formulating a research question that truly reflects, and builds on, the current state of knowledge.

My second concern deals with the validity of the conclusions. This relates both to the quality of the datasets used and the consistency between them, and to the application of a modified Budyko framework.

The results rely heavily on the quality of the data used. Unfortunately, the selection of datasets used by the authors raises a number of questions. Firstly, the precipitation data is rescaled to force long-term average water balance closure (L113-114: "Finally, annual precipitation data were adjusted by the long-term average residual of P − ET − Q so total basin storage over the period of record was zero."). This is a highly unusual procedure, because normally P is the term with the smallest relative error. It is unclear how this procedure was implemented exactly, and how big the corrections were. In addition, it creates an inconsistency with the ET data used, which are calculated based on P which is now inconsistent with the P used in the water balance analysis. The authors should show clearly that this procedure is needed, and that its impact is limited. The rescaling might well mask larger errors in other terms, such as ET. While not much information is provided on ET, it seems to be based on statistical modeling of the relation between observed ET, NDVI, and P. The problem here is that observations of ET over forest ecosystems made by eddy covariance often are inconsistent with runoff observations (the "forest evapotranspiration paradox", see Teuling, Vadose Zone J. 17:170031. doi:10.2136/vzj2017.01.0031). I believe the authors should provide more evidence or arguments on why this ET dataset is useful in this context, and how non-forest and snow areas are dealt with. Perhaps my biggest concern is with the runoff data. Little information is provided on these, so I did some searching on the web myself instead. Based on the following document: https://www.waterboards.ca.gov/waterrights/water_issues/programs/bay_delta/california_waterfix/exhibits/docs/petitioners_exhibit/dwr/part2_rebuttal/dwr_1384.pdf, it seems that the unimpaired flows are subject to numerous calculations and assumptions that even differ between the individual basins. This raises the question if this data should be considered a model product or an observational product. My feeling after reading the document is more the former. This is particularly problematic, because any assumptions in the approach that may impact the runoff values differently in normal and drought years, will directly impact the results. The authors should show that the risk for such bias is small, otherwise their main findings might reflect an assumption made in a modeling chain, rather than an observation that tells us something new about how nature works.

My second main concern deals with the modified Budyko approach. I share the concern expressed by referee #2 that the modified framework has not been sufficiently tested or proven for the current application. Even in case the framework is valid, there is a fundamental difference between the plots with P, and P−ΔS. In the traditional Budyko framework, the aridity index PET/P reflects an external climate forcing that is decoupled from the catchment itself. Here, the Budyko parameter reflects how the catchment partitions precipitation between ET and Q, at a given atmospheric forcing. In the modified formulation, this interpretation is no longer possible because PET/(P−ΔS) on the *x*-axis now becomes dependent on catchment properties (through ΔS which is affected by optimization that is different for drought and non-drought years). This means that changes along the Budyko curve can no longer be considered as only induced by climate variation, and changes in the Budyko parameter no longer reflect changes in catchment functioning only. This potentially creates a flaw in the interpretation of the drivers of drought-induced shifts in water balance partitioning. The authors should provide convincing evidence or arguments on why the modified Budyko framework can be interpreted in the same way as the traditional framework. I also suggest to use a symbol for the Budyko parameter that is distinctively different from the "*w*" used in most papers, stressing the fact that they are not the same parameters.