Hydrology and Earth System Sciences
Discussions

Hydrol. Earth Syst. Sci. Discuss., referee comment RC2
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Comment on hess-2022-309

Anonymous Referee #2

Referee comment on "An analytical generalization of Budyko framework with physical accounts of climate seasonality and water storage capacity" by Xu Zhang et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2022-309-RC2, 2022

This study proposes a generalization of the Budyko framework beyond the use of a single average aridity index. The key idea is to better account for seasonality and the related phase lags between precipitation and radiation and also for storage characteristics. While, I like the scope of the study and agree that the proposed generalizations are really important, I think the study suffers from several short comings.

May major concern is the obvious inconsistency between the "discretization" of the hydrological year into 4 months long periods, with the conceptualization of the first partitioning stage of the Ponce-Shetty model.

The idea that precipitation equals recharge/infiltration dW of/in the subsurface store and fast "flow" Q is **only correct** during rainfall events, because evaporation and transpiration can be neglected then.

P= Q + dW.

This equation is not correct during a 4 months period, consisting of rain and fair weather periods, it simply violates the mass balance, because parts of P are released as evaporation and transpiration during this period.

This implies that a one parameter Budyko (Eq 17.) cannot be used to model this partitioning for increments of 4 months, because in this time precipitation is simply not equal to fast runoff and storage change, but parts are released as ET. This does simply violate mass conservation at the soils surface, and the problem arises from the fact that the entire model is formulated for steady state partitioning, which essentially implies that storage changes are zero. This can be easily inferred from the water balance equation for any compartment (e.g. the soil), which should be the based for any kind of model concept (which is not the case here). So I think that the entire model analysis is based on a physically inconsistent reasoning. Either you have changes in storage or have steady states, you cannot have both.

Technical points:

- The manuscript would benefit from proof reading, at least I miss “definite articles” in front of many nouns.
- I would avoid abbreviations like “E” in headers, there are better ways to keep thinks short.
- Fluxes are generally equal to storages changes in time (not to storage itselft), would be nice to have proper equations, with proper variable definitions.
- I miss units/dimensions for most of the variables.
- Equation 8 proposes that the entire stock is “active” and released as base flow or ET. This is not consistent with soil physics and soil water retention curve, which corroborate that water stored at tension larger than pF =4.2 (permanent wilting point) is not available for transpiration (and also not for base flow generation).
- Eq. 9 is not correct, see comment above, expect that the authors refer to the active storage.
- With boundary conditions you mean upper and lower bounds of the terms?
- P going to infinity doesn’t make sense to me, at least not physically.
- Soil water storage is also limited by infiltration capacity, not only by storage volume. Both factors are not necessarily correlated, think about clay soils.