We thank the Editor and the reviewer very much for their valuable comments and suggestions on our manuscript entitled "Insights into evapotranspiration partitioning based on hydrological observations using the generalized proportionality hypothesis". In the following, we provide a point-by-point response to the reviewer's comments. We have revised our manuscript based on the reviewer's feedback and our responses accordingly. We are confident that the quality and clarity of our manuscript have been enhanced after the revisions.

Reviewer 1

The authors did a good job on incorporating the reviewers' comments in the revision. The manuscript is improved. I still have a few comments that I hope the reviewers can address.

1. Lines 120-127: The introduction of parameter f and equation (4) is still not very clear to me. According to Lines 124-125, it is assumed that the remaining portion of E after deducting E0 is equivalent to the remaining portion of Et after deducting the portion f. This is a big assumption. This part needs better explanation from a physical point of view. The E after deducting E0 is usually called continuous evapotranspiration. Why is it equivalent to the amount of transpiration after deducting the fast transpiration from the top soil?

   Response: Based on the GPH definition, $E-E_0$ represents the evapotranspiration that competes with baseflow, whether we call it continuous evapotranspiration or not. Since shallow transpiration does not compete with baseflow, it should be excluded. According to the literature (as discussed in the manuscript lines 114-127 and in our previous responses), $E - E_0$ consists only of slow transpiration, because all other evaporative fluxes are included in E_0. From a physical standpoint, if we consider the evaporative fluxes that compete with baseflow, we would need to include slow transpiration and possibly deeper soil evaporation. However, the latter is typically neglected in GPH applications at long timescales, or combined with interception as in Savenije (2004), Gerrits et al. (2009), and Abeshu & Li (2021). Therefore, the only flux considered to compete with baseflow is the slow transpiration, leading to the relation:

   $$E-E_0 = E_{t\_slow}$$

   Equation 4 follows directly from this assumption. Since E_0 includes interception, evaporation from surface depression, topsoil evaporation, and shallow transpiration, the remainder of E must be the slow transpiration: $E-E_0=E_{t\_slow}$.

   For transpiration, we define fast transpiration as $E_{t\_fast}=f*E_t$, and thus slow transpiration as $E_{t\_slow}=(1-f)E_t$. Equating these two E_{t_slow} equations yields

   $$E-E_0 = (1-f)E_t$$

   By substituting E_0 with kE yields

   $$(1-k)E=(1-f)E_t$$

   which is equation 4.

2. Figures 4 and 5: The k values are pretty high. Some watersheds have k values higher than 0.9, which means over 90% of the evapotranspiration is considered as initial evapotranspiration. This is different from previous proportionality studies. For example, in Sivapalan et al. (2011) "Functional model of water balance variability at catchment scale: 1. Evidence of hydrologic similarity and space-time symmetry", the k value range is from 0 to 0.45. It seems to me that the physical meaning of initial evapotranspiration in this study is slightly different from the previous works. The authors should make clear statement about this difference. By the way, change "λ" to "k" in Figure 5.

   Response: We acknowledge that the reported k values are relatively high. However, it is important to clarify what the initial E (i.e., E_0) represents in this context. Specifically, E_0 is the portion of evapotranspiration lost prior to the competition between E and baseflow. Many components of E_0 are not accessible to baseflow, and only moisture that reaches deeper soil layers (i.e., slow transpiration, as discussed in the previous point) participates in this competition. This explains why the resulting *k* values tend to be relatively high.

   Regarding the Sivapalan et al. (2011) study, they adopted the formulation $E_0=kE_p$, which differs from other forms discussed in lines 110-112 of the manuscript. In contrast, we used $E_0=kE$. Since potential evapotranspiration (Ep) is typically much larger than actual evapotranspiration (E), it is reasonable that the k values reported by Sivapalan et al. are lower than those in our study. Furthermore, Abeshu and Li. (WRR, 2021) "Horton Index: Conceptual Framework for Exploring Multi-Scale Links Between Catchment Water Balance and Vegetation Dynamics" also reported similar or even higher k values (see their Fig 6), using the same formulation for E_0 as in our work ($E_0=kE$).

   In Section 5, since parameter f is the new parameter introduced in this study, I recommend the authors to add in-depth discussion about the connection between f and other variables. Also, it would be helpful if the authors could do a sensitivity analysis on parameter f, to show how the values of f would affect the results of the proportionality equations.

   Response: Since *f* represents the portion of fast transpiration, it affects how evapotranspiration is partitioned and may influence the E_t/E ratio. However, it does not affect the hydrological fluxes such as Q, Qb and Qd, because *f* is independent of k, and changes in k do not alter the values of *f*.

   To address this point, we performed a sensitivity analysis on *f,* as suggested, and have incorporated the results into the manuscript (Section 4.4, Sensitivity of E_t/E to *f* values and Appendix A). We found only minor differences in the resulting Et/E when varying the fast response depth between 5 cm, 10 cm, and 15 cm. These differences fall within the range of uncertainty reported in the literature for evapotranspiration partitioning methods.