<u>*Comment 1:*</u>

The study nicely integrates large datasets and modeling to elucidate the minor importance of those differences in Sr,max to hydrologic modeling. The paper is clear, well-written, and the figures are compelling. I have a few minor comments (see below), and I also think the authors could discuss further the implications of the simplification they employ to estimate Sr,max.

<u>Reply:</u>

We thank the reviewer for his thoughtful, detailed and constructive comments and we highly appreciate his positive overall assessment of our manuscript. We will address all reviewer comments in detail here below.

<u>*Comment 2:*</u>

Regarding the latter, the authors simplify daily actual evapotranspiration (E_a_daily) to be equal to daily potential evapotranspiration scaled by the decadal ratio of actual to potential ET. They then uses a daily water balance to determine the necessary Sr,max to deliver that daily evapotranspiration. Using a constant ratio to convert potential ET into actual, however, does not necessarily reflect the behavior of catchment vegetation. As a counterpoint, one might expect potential ET to be met fully during periods of low E_p (and plentiful water) and actual ET to approach zero during periods of high-demand/drought. Thus, another way that one could determine the requisite Sr,max – as opposed to equations 4 and 5 – would be to simplify the system such that

E_a = E_p if water is available in storage

E_a = 0 when the water in storage is depleted

Find the value of Sr,max such that the (long-term sum of E_a)/(long-term sum of E_p) equals that desired long-term ratio

Such an approach may better represent vegetation response (albeit a little extreme, along the lines of Milly, 1994), and would be more consistent with the complementary hypothesis for evaporation (see multiple references by Szilagyi)

It may be that the resulting Sr,max does not differ much from that determined from equations 4 and 5, due to the self-limiting process of ET (e.g., whether one removes 5 mm on day one and then zero on day two or 2.5 mm on day 1 and another 2.5 mm on day 2 may not matter). However, it would be interesting to compare and to see if there is a difference, especially for the monthly/seasonal results, where the differences may have an even larger effect.

I understand this may be beyond the scope of the paper. Nevertheless, given the significance of equations 4 and 5 on the central message of this paper, I recommend that the authors spend more time discussing those simplifications, alternative simplifications (such as that above), and the potential implications on the results and conclusions.

<u>Reply:</u>

This is indeed an import comment and a valid observation. We completely agree that the assumption of a constant ratio E_A/E_P may introduce uncertainties. In particular, during dry periods, this assumption does not account for vegetation water stress and may therefore lead to overestimation of E_A and a potential resulting inflation of $S_{r,max}$. We will discuss this and the related limitations of the method in more detail in the revised version of the manuscript

<u>*Comment 3:*</u>

Relatedly, I think the abstract and discussion would benefit from additional acknowledgment that the catchments used in this study are both snow-free and relatively aseasonal. Thus, the conclusions may not be extensible to snow-dominated watersheds and/or those with strong seasonality, such as a Mediterranean climate.

<u>Reply:</u>

Good point. We agree that many of the study catchments, and in particular those in the Meuse basin, for which we have implemented the hydrological model, are characterized by relatively little snow and little precipitation seasonality. We also agree that the effects of changing $S_{r,max}$ may thus be more pronounced in other environments. We will make this more explicit in the discussion of the revised manuscript.

<u>*Comment 4:*</u>

Given the nature of the datasets used, I think a more representative title for this work would be "Catchment response to climatic variability: Implications for root zone storage and streamflow predictions." The CAMELS datasets are catchment-based, and the authors are not isolating specific vegetation responses.

<u>Reply:</u>

Agreed, we will adjust the title to better reflect the analysis.

<u>*Comment 5:*</u>

I found it somewhat confusing that the meanings of the subscripts modifying evapotranspiration (E) and aridity index (I) were not consistent. When A was used as a subscript, it meant "actual" when modifying evapotranspiration; however, it meant "aridity" when modifying the index, which – in turn – meant it signified potential (not actual) ET. Thus, I_A was not the analog to E_A; rather I_E was the analog to E_A. Perhaps I_A could be used to indicate the evaporative index based on actual ET, whereas I_P could indicate the evaporative index based on potential ET.

<u>Reply:</u>

We agree with the reviewer that this could potentially be perceived as inconsistency by some. We would nevertheless prefer to keep it as is, as these symbols are frequently found in literature.

<u>*Comment 6:*</u>

Line 52-60: The authors present their methods of determining Sr,max from a daily water balance (see above). In essence, the Sr,max is the storage volume needed to ensure that daily ET can be met. However, that value represents a minimum value for Sr,max, which – of course – could be larger. It might be worth

a comment to that effect, especially since those values of Sr,max are then used in a hydrologic model with a very different mathematics.

Reply:

This is again a very sharp observation and we of course completely agree. We will explicitly mention this in the revised version of the manuscript.

Comment 7:

Lines 165-172: the numbering scheme used in this paragraph does not exactly match the numbering of the methods sections to which it refers.

Reply:

Thank you for pointing this out. Will be corrected in the revised manuscript.

Comment 8:

Lines 299-307: I particularly appreciate that the authors sought explanatory variables, such as aridity index, for their results. I expected aridity to be a controlling factor, and it was interesting to learn that it was not.

Reply:

We agree, we were also quite surprised.

Comment 9:

Line 431: dangling phrase, "the more equilibrated scenario A"

Reply:

Will be corrected in the revised manuscript

Comment 10:

Equation 5: as written, the equation is circular. What should be used as the argument of the inequalities on the RHS is the integral from t0 to t of (P_daily - E_A_daily) dt rather than S_D,j,i(t)

Reply:

Indeed. We will correct this in the revised manuscript

Comment 11:

The reference for Dralle, et al. 2021 is missing from the reference list

Reply:

The reference will be added in the revised manuscript.

Comment 12:

I recognize that figure 7 is intended to explain the methodology and not results. Even so, I recommend that the qualitative character of the distributions for delta_I_E reflect the results of this paper. That is, the distribution for scenario A should be narrower than that for scenario B; and the mean for scenario B could even be shifted away from zero (compare Figure 7 and Figure 11). As is, the figure gives the false impression that the uncertainty across all catchments is greater than across the Meuse watershed alone.

Reply:

Thank you! That is an excellent idea. We will adjust the figure accordingly in the revised manuscript.