Author's response to the Reviewers' and Editor's comments
Please refer to the detailed itemized responses below. Our responses are indicated in blue text and the text referring to the clean version is indented. Edits compared to the last version are highlighted in red text.

Reviewer #1
I appreciate that the authors thoroughly addressed all my comments, and the current manuscript, especially the model description, reads much better. I find the new results based on the partitioned transpiration (T) are particularly interesting, although this may not be the main focus of this study. Because observed T stands between the simulated T under full recovery and no recovery scenarios, implying a partial recovery, similar to GPP while this is not shown when using evapotranspiration (ET) data. This finding supports, on the one hand, the existence of legacy effects on T, and on the other hand, the hypothesis that the reduced T due to legacy effects could be compensated by the increased soil evaporation, leading to the near recovered ET. If authors see the position for adding more discussion on this finding, I would recommend doing so. In addition, the reference of the ET partitioning method based on uWUE should be Zhou et al., 2016 instead of Nelson et al., 2020.
Overall, I find this study nice and congratulations!
Reference:
Zhou, S., B. Yu, Y. Zhang, Y. Huang, and G. Wang (2016), Partitioning evapotranspiration based on the concept of underlying water use efficiency, Water Resour. Res., 52, 1160–1175, doi:10.1002/2015WR017766.
[Response] We thank the reviewer for this helpful suggestion. We agree that the partitioned transpiration results provide additional support for our interpretation of drought legacy effects. Following the reviewer's suggestion, we have added a short discussion in the Discussion section noting that observation-based transpiration exhibits an intermediate recovery between the full- and no-recovery scenarios, whereas this pattern is less apparent in ET. This difference suggests that enhanced soil evaporation following rewetting may compensate for reduced transpiration, thereby masking vegetation drought legacy effects when using ET alone. This can be found in lines 393-399 in the clean version.

> The quick recovery behavior of ET should be treated with caution as it contains both plant transpiration and soil evaporation, with the latter recovering rapidly after rewetting and potentially masking the slower recovery of transpiration. Supporting this interpretation, observation-based partitioned transpiration shows an intermediate recovery between the simulated full- and no-recovery scenarios, consistent with the presence of drought legacy effects in plant water use. In contrast, this pattern is less apparent in ET, suggesting that enhanced soil evaporation following rainfall may compensate for reduced transpiration and obscure the persistence of vegetation drought legacy.

Reviewer #2
The revised manuscript is very much improved on the original and especially the methods are much more complete and clear. I appreciate the author's efforts to address both mine and the other reviewer's comments, including new simulation and figures.

I have one more comment - in their reply, the authors say that their method can be applied to sites with eddy covariance data and then generalised from there but at the same time that a model with a fixed recovery limit parameter does not show a lot of improvement on the model with full

recovery. To me this is a contradiction and I would like to see a little discussion on how this could be addressed.

[Response] We agree that our previous wording could give the impression that a single fixed recovery parameter could be directly transferred to other sites. This was not our intention. Our results indeed show that prescribing a fixed recovery limit does not substantially improve the model compared with the full-recovery assumption. This finding indicates that post-drought recovery is dynamic and cannot be adequately represented by a universal constant. Instead, the best model performance is achieved when recovery trajectories are constrained by site observations through data assimilation. The purpose of this study is therefore primarily diagnostic: to demonstrate that eddy-covariance observations can be used to infer ecosystem-specific recovery trajectories and to identify the dominant mechanisms responsible for drought legacy effects. We envision that future applications would not transfer a fixed recovery parameter directly. Rather, recovery characteristics (e.g., recovery rate or maximum recovery extent) inferred from multiple flux-tower sites could be related to measurable ecosystem properties, such as drought severity, vegetation type, hydraulic traits, or remotely sensed indicators, allowing recovery parameters to be predicted for sites without flux observations. We have clarified this point in the Discussion and now explicitly distinguish the current observation-constrained framework from its potential future regional application.

Please find the revised text at lines 455-458 in the clean version:

> The present framework should therefore be viewed as a diagnostic tool for identifying drought recovery processes at observation-rich sites. Extending it to regional and global applications will require predictive relationships between inferred recovery characteristics and observable ecosystem properties, rather than prescribing a universal recovery limit.

Editor: Specific comments

Line 111 — "Specifically, nearly 64% of the area was characterized as ‘at least moderate drought’ in July as a peak." I would suggest "…in July, at its peak" or "…peaking in July.", although I am not a native speaker.

[Response] This sentence has been revised as:

> Specifically, the drought peaked in July, when nearly 64% of the area experienced at least moderate drought.

Lines 123–124: According to the FLUXNET2015 data use policy, the DOI of the datasets for US-UMB and US-NR1 should be added in the reference list or in a specific table, which I do not see.

[Response] We apologize for this missing reference information. We have added the DOI of the datasets for these two flux towers.

US-UMB: Aimee Classen, Christopher Gough, Gil Bohrer, John Lenters, Peter Curtis (2023), AmeriFlux FLUXNET-1F US-UMB Univ. of Mich. Biological Station, Ver. 3-5, AmeriFlux AMP, (Dataset). https://doi.org/10.17190/AMF/2204882

US-NR1: Peter D. Blanken, Russel K. Monson, Sean P. Burns, David R. Bowling, Andrew A. Turnipseed, John F. Knowles (2026), AmeriFlux FLUXNET-1F US-NR1 Niwot Ridge Forest (LTER NWT1), Ver. v1.3_r1, AmeriFlux AMP, (Dataset). https://doi.org/10.17190/AMF/1871141

Line 138: "…thereby capturing seasonal phenology and structural changes during 2012 drought (Figure S1)." I believe there is a missing article: "…during the 2012 drought."
[Response] Thanks. We agree.

> LAI is prescribed from 8-day MODIS observations and linearly interpolated to daily values, thereby capturing seasonal phenology and structural changes during the 2012 drought (Figure S1).

Lines 136–141: (Methods, Eq. 1 area) — The "Farquhar model" is named without an in-text citation. Although it is well known, consider to add (Farquhar et al., 1980) at first mention.
[Response] We have added the citation for the Farquhar model at its first mention.

Line 161: "The CliMA Land model incorporates several stomatal optimality models (e.g., Eller et al., 2018, Sperry et al., 2017; Wang et al., 2020)…" The reference list contains only "Eller, C. B., … 2020" (Stomatal optimization based on xylem hydraulics (SOX)…, New Phytol.), which is correctly cited elsewhere. No 2018 Eller reference exists in the bibliography. Please correct this issue.
[Response] We are sorry for this mistake. We have corrected it to Eller et al.2020.

Line 193 — "where Ψ63=2.94, b=2.52" — Ψ63 is a xylem water potential and should be expressed in MPa, and, following the sign convention used throughout the rest of the manuscript for water potential, which should be negative, i.e., "Ψ63 = −2.94 MPa." Please check the unit and confirm the sign.
[Response] We agree. The number of 2.94 should be followed by unit MPa. In last version, there is a negative sign convention in front of Ψ63. Here, we set Ψ63 to -2.94 MPa and deleted the negative sign convention in front of Ψ63.

$$\beta = e^{-(\frac{\psi}{\psi 63})^b} \qquad \text{Eq. (7)}$$

where Ψ63=-2.94 MPa, b=2.52

Reference list: (Ciais et al., 2005; Nature 437) — Ciais et al., 2005, the paper about the known 2003 European heatwave is in the reference list but not cited in the main text. Please add it in the proper place; otherwise remove it from the reference list.
[Response] We removed it from the reference list.

Line 277: "…compared to applying the stress factor to Vcmax (Wilcoxon rank text: P<0.001)." Please change "rank text" to "rank test" (correct elsewhere at Lines 276 and 285, just above and below this sentence).
[Response] We corrected it in Line 277.

Line 315 and in other places. I have seen " modeling " in a few places and " modeling " in others. Please decide on one style (British or American English) and keep consistent spelling throughout the manuscript.
[Response] We used ‘modeling’ throughout the manuscript.

Line 332: "…photosynthesis simulated by the Medyn-g1 tends to be slightly higher…". Please change "Medyn-g1" with "Medlyn-g1".

[Response] Thanks. We corrected it.

General comment: please check the reference cited in the text and correct double citations like in this example: example at line 393 -> “Our findings align with the analysis by He et al. (He et al., 2018), which found that GPP takes longer to recover than ET. However, unlike He et al. (He et al., 2018),”. For instance the text should be: ““Our findings align with the analysis by He et al., 2018, which found that GPP takes longer to recover than ET. However, unlike He et al. 2018,” . There are other manu places where this is done, please check the manuscript carefully. Here some I spotted: Line 111 (Wood et al.), Line 189 (Benson et al.), Line 220 (Wolf et al.), Lines 392–393 (He et al., appears twice), Line 400 (Yu et al.), Line 405 (Yang et al.), Line 427 (Flexas et al.), Line 431 (Drake et al.), and Line 438 (Rehschuh and Ruehr). This looks like a citation-manager/macro artifact repeated through the Discussion section and should be corrected throughout (e.g., "Wood et al. (2023)" rather than "Wood et al. (Wood et al., 2023)").
[Response] We apologize for this citation issue. We have checked and corrected the double citation throughout the manuscript in the clean version.

Line 416: As for “modelling” standardize the use of parameterize. For instance: "…these results suggest that challenges remain in parametrizing stomatal and non-stomatal limitations…" uses "parametrizing," while "parameterized" is used at Line 435 ("…parameterized a single set of plant hydraulics parameters…"). Standardize to one spelling.
[Response] We thank the Editor for pointing this out. We used ‘parameterized’ in similar conditions.

Figure 2 caption (Lines 265–269) vs. Figure 4 caption (Line 349) — The multi-year baseline period differs between the Figure 2 caption ("the 2007–2019 record") and the Figure 4 caption ("calculated over 2004–2019"), with no stated reason for the different windows. Please clarify why the reference periods differ, or correct this if it is an inconsistency.
[Response] The multi-year baseline period should be 2004-2019. We corrected it in Figure 2’s caption.