We thank the reviewer for the insightful comments and suggestions for improving the manuscript. We found all the comments / suggestions to be valid and took action as advised. Throughout this document, the reviewer's comments are rendered in black, and our responses in green. Line numbers have changed slightly, and so we have included a version with tracked changes to highlight revisions to the text. In addition to the requested revisions, a few minor edits were introduced for style and/or flow.

Reviewer #1 (Formal Review for Authors (shown to authors)):

This paper reports one major development of Community Land Model version 5 (CLM5), the plant hydraulic module. In the model development, the soil-plant-atmosphere continuum model is implemented in CLM5. The advance of plant hydraulic development is highlighted because it is more physical-based compared to original soil moisture stress, which is more empirical-based. The need to develop the plant hydraulic module in Earth System Models is well introduced in the Introduction. The new plant hydraulic stress configuration is compared with soil moisture stress configuration through simulations on tropical forest. The differences between PHS and SMS have been compared in many aspects, including soil moisture, water flux through vegetation, vegetation stress, and GPP. I was expecting some refined but meaningful conclusions from the comparison results on how PHS performs differently with SMS under drought. However, after finishing reading the results and discussions being fed with large amount of information, I was a little disappointed that I was only briefed from the conclusion that "the new model behavior differs from the default configuration in ways that are expected, ", which is perfectly correct but not useful enough. Anyways, the major contribution of this work is significant. But I still want to specify my concern:

Major remarks:

The features of PHS and SMS are not well concluded. The take home messages are still left widely open. Normally, reader may expect to know how PHS performs better than SMS compared to observation. But the authors claim they don't aim to comprehensively assess which model is better (line 765-769). I understand the claim because more mechanistic representation of plant hydraulic processes is already a significant contribution in land model development. Bias of model performance includes both parameter bias and model structure bias. To fully evaluate these biases, it needs a full length of another manuscript. However, for a model development paper, readers, some of who are potential model users, may not only be satisfied by generally knowing PHS is theoretically advanced (line 26-27), but also expect at least some details on how it performs differently compared to previous configuration (SMS). This paper actually did compare PHS with SMS but without being well summarized, which make it extremely difficult to acquire the take home messages. They compared PHS with SMS on a tropical forest drought experiment, which provides various interesting information in the Results and Discussion. But none of results have been abstracted or highlighted at a more visible place (in Abstract or Conclusion). I think a list of meaningful points or, if too many, at least one point should be concluded in the abstract. For examples, I was impressed by that PHS saves more

water in the soil under drought and the vegetation therefore experiences less stress, which can potentially imply different future climate feedback. Such sort of conclusion with ecological meanings will make more senses to readers who use CLM to study drought. Meanwhile, these impressions from the abstract help understand comparison results and further tag PHS and SMS. Otherwise, PHS and SMS do not make too much sense until we carefully study all the results, which leaves difficulty to reading. I suggest authors to think about how to summary and highlight their results from the precipitation throughfall exclusion experiment in the revision.

The authors agree with the reviewer on the need to improve conclusions and better crystallize key results and takeaways. To that end, significant revisions have been included in this version of the manuscript:

(1)
The abstract and conclusion have both been updated to better reflect the results and implications discussed within the paper. This includes the reductions in transpiration and soil moisture biases, as well as the changes in precipitation and VPD sensitivity introduced by the new model formulation.

(2)
In order to improve the flow and readability of the results, two figures were moved from the main text into the supplementary information.
Figure 3, a transpiration time-series which duplicates information in Figures 2 and Figure 13
Figure 10, which is a somewhat technical detail (regarding the sensitivity of root water uptake to soil potential) compared to some of the other main results
Likewise, explanatory text was added to three figure captions to better link the results to key points.

Minor remarks:

Line 26-28: The last sentence in the abstract has not been thoroughly discussed in the paper. I would expect further details in the discussions on how to test hypothesis? Any significant implications to climate change can be drawn from the hypothesis?
Example hypotheses that were previously incompatible with CLM model structure were discussed in the benefits section (5.5.1) under point 4. Additional text discussing hypotheses that were previously untestable was included in 5.3.1 (L641).

Line 222, 290: Recently, Anderegg et al., (2018) published a paper on Nature, which has identified an important trait "hydraulic safety margin (HSM)". I am personally interested in whether HSM can be represented by one of parameters in these equations.
This reference has been added to the manuscript and discussed in Sections 2.5.5 and 5.3.1 (L309,L645). The concept of HSM is indeed captured by the PHS parameterization and looks to be an important driver of variations in ecosystem flux response to drought. There is not a single

parameter defining HSM in PHS, but rather it is an emergent behavior of the system. To first order approximation, we suggest that:

$$HSM = P_{50} - \alpha \frac{T_{max}}{k_{max}} - \beta$$

Whereby the combination of two PHS parameters (P_{50} and k_{max}) serve to determine the HSM, subject to the climatological maximum transpiration (T_{max}). Multiple parameters govern limitations to transpiration by drought stress affect T_{max}, which include (among PHS parameters), P_{50}, c_k, and k_{max}, but would also be affected by the Medlyn slope parameter. The variables α and β include the effects of other variables including,

for α: tree height, leaf area index, percent loss of xylem conductance

for β: root resistance characteristics and climatological soil potential

While we cannot provide a simple analytical definition based on PHS parameters, HSM can easily be calculated from model output and the value of p_{50} or p_{88} derived from the percent loss of conductance parameters. Characterizing HSM in the model and its effect on drought coupling would be a very interesting follow-up study.

Line 352: Could you describe after "Six-year spin-up simulations", how much is steady state achieved? For examples, soil moisture changes less than 1% between two loops.

In the spinup simulations, soil matric potential changed by less than 0.2% (root-fraction weighted average across soil layers) between the two loops (under both the SMS and PHS configurations), which has now been included in the text.

Line 367: The tense of "compare" should be consistent in the paragraph.

Resolved.

Line 376: "all" should be "both"?

Here we are referring to the twelve instances of large error under the SMS formulation. We are trying to point out that in all twelve instances, the errors occur from underestimating transpiration. The text was updated to clarify this meaning.

Line 388-389: "PHS performs better than SMS, but yields a more pronounced high bias." Does the PHS yield more pronounced high bias than SMS? I don't see this from Figure 2 (R2 is higher and RMSE is lower in PHS). Or do you mean PHS yields a more pronounced high bias in TFS than in AMB?

Indeed we were comparing the behavior in TFE to the behavior under ambient conditions. The text has been updated to remove this ambiguity.

Line 597-599: "In the PHS simulations, this suppresses variability in GPP relative to SMS, and leads to underestimating the range of transpiration compared to sap flux observations (Figure 2)". The author attributes the underestimates of the simulated transpiration range to "suppressed variability in GPP". However, I think the attribution is too arbitrary. Because the "suppressed variability in GPP" is relative to SMS but not observation. If the "suppressed variability in GPP" is based on more physical-based mechanism, it is not very convinced and not

really encouraged that the "advanced" development claimed by the author may introduce more bias. Especially, the bias can be also contributed from other processes, such as photosynthesis, soil hydrology, and parameter errors, which are still hard to separate.
The reviewer's points are valid and have been reflected within the text. In particular, the relationship between GPP and drought status (VPD, precip) has not been identified specifically as a degradation relative to any observations. We have updated the text to acknowledge the change in GPP variability between model configurations (within the results section), while leaving for future studies a more thorough assessment of model skill.

Line 646: In "Due to constant vegetation potential (ψc)", the ψc is actually a soil potential not a vegetation potential. I understand the author tried to use it as a hydraulic analog. But I think the hydraulic analogs are only mathematically useful, but scientifically incorrect. So I suggest the explanation needs to be very careful when using the analogs. At least it should be mentioned that ψc here is only a hydraulic analog every time when using it.
This is a fair point, and the text has been updated to more carefully register the meaning of ψc throughout. This includes edits to L646 as well as L242.

Line 818: For consistency, change "through-fall" to "throughfall".
Resolved