RESPONSE TO REFEREE #1

The authors have satisfactorily addressed all of my comments. I would just suggest that they add to the manuscript a definition of live fuels, i.e. “leaf and shoot (< 6 mm diameter)”, which they have identified in their response letter, but does not seem to appear in the manuscript itself.

RESPONSE: Thank you for the comment. We have added “leaf and shoot (< 6 mm diameter)” to the manuscript a definition of live fuels.

RESPONSE TO REFEREE #3

I thank the authors for their thoughtful responses to my suggestions and revisions. Their work has helped to improve the manuscript. However, there are a few key points that I made in my previous review that were not addressed that I think are important, so I will reiterate them here. Before I get to the paper, I would like to point out that the line numbers and pages are off in the R2R. This makes it a significant amount more work to review the revisions. I hope that the authors will check this more carefully next time.

RESPONSE: Thank you for the comments and sorry that we did not completely address some of your previous comments.

One of my biggest concern that was not addressed is related to PFT diversity. I understand that the authors performed a clustering analysis on some key traits to identify unique PFTs and that this clustering analysis underlies their justification for 3 PFTs. However, implicitly underlying the clustering analysis is a hypothesis by the authors that the traits used in the clustering analysis will result in a functional difference in climate response/PFT performance. The authors did not test to see if there was a functional need to have 3 distinct PFTs through any simulations. I contend that PFT LA and PFT HA are functionally similar enough based on Figure 2 that there is not a need for all the PFTs used. This also ties into my criticism of H4 and its usefulness in this manuscript. These concerns were not addressed in the revision.

RESPONSE: To address your comment, we have tested to see if there was a functional need to have three distinct PFTs through simulations using one PFT, two PFTs, or three PFTs defined from the trait clustering analysis; results shown in Fig. S6. As shown in Fig. S6, we found that the traits used in the clustering analysis will result in a functional difference in climate response based on how many distinct PFT groups are defined. Significant percentage changes (** $p < 0.01$, * $p < 0.05$) of live fuel moisture content and fire season length between future period (2080-2099) and historical period (1960-1999) were found using three PFTs while not found using one PFT or two PFTs under climate scenarios.

My other major concern is that the authors do not do any validation related to interannual variability, which is crucial for having confidence in model predictions on centennial timescales. The authors added an additional validation figure (S5) in response to my comment in the previous revision, but this is a poorly explained figure (I suggest the authors expand the figure legend if they keep this figure) and it is also only a single year. Further, it looks like the model-predicted variation in within-season LFMC (Fig. S5) substantially underestimates the observed LFMC peaks and troughs. Given that the model is not capturing the seasonal extremes, and there is little variation across climate forcings (Fig. S5), this makes me quite concerned about the model's ability to capture interannual variability, especially since the model substantially overpredicts minimum soil moisture (Fig S3). If the authors want to make decade to centennial scale predictions, they need a longer time scale validation of some kind, despite the scarcity of data. This will help at the very least identify model biases that can be used to better interpret/caveat future projections

RESPONSE: Thank you for your valuable comments. To address you concern, we have added a longer time scale validation during 2006-2019 in Fig. S5 using the chamise species from the National Fuel Moisture Database (2021). Based on the simulated monthly mean LFMC during 2006-2019 for PFT-LA, which includes the chamise species, we found that our model can capture the seasonal and interannual variability, but underestimates the highest wet season peaks in LFMC in 4 of 14 years. This may cause biases for future projections while it would not highly affect the long-term trend of LFMC and fire season length. We have commented on this mismatch in the revised manuscript as follows (Lines 435-439, please refer to revision changes tracked manuscript):

"*Based on the simulated monthly mean LFMC during 2006-2019 for PFT-LA, which includes the chamise species, we found that our model can capture the seasonal variation and interannual variability, but underestimates the highest wet season peaks in LFMC in 4 of 14 years (Fig. S5).*

This may cause biases for future projections while it would not highly affect the long-term trend of LFMC and fire season length."

More minor comments:

Did the authors fit a linear model to the R and SR in Fig 4/5 of Saura-Mas and Lloret? That is what it appears to be from figure S4, but this should be spelled out in the methods

RESPONSE: Thank you for pointing this out. We have added the description that we fitted a linear model to the R and SR in Fig 4/5 of Saura-Mas and Lloret in Lines 285-288 (please refer to revision changes tracked manuscript) as follows.

"*Then, we estimated the LFMC within leaves and shoots (< 6 mm diameter) using the empirical equation derived from shrub LFMC and LWC data including the three regenerative strategies [seeder (S), resprouter (R) and seeder–resprouter (SR)], in summer, autumn and winter from Fig. 4 and 5 in Saura-Mas and Lloret's study (2007)*"

Re fig S3, it looks like the model substantially overestimates soil moisture relative to observations. How sensitive is the prognostic LFMC to soil moisture/ what implications does this have for future projections?

RESPONSE: Thanks for the comments. The overestimation is likely related to the soil texture information fed into the model, which we originally used based on the coarse-resolution soil survey data. For this revision, we collected some on-site soil texture data (Sun et al. 2016) with a much lower silt percentage. We have updated the soil texture information and rerun the LFMC simulations. Our results showed that the model is able to better capture the soil moisture content (See updated Fig. S5); however, the updated LFMC dynamics are very similar to the previous LFMC dynamics. This showed that our model is not very sensitive to the soil texture information. This could be related to the fact that the model uses the soil water potential to determine the water uptake for plants and the plant hydraulic traits could mostly determine the LFMC.

L 177. What do the authors mean by "recovered well"

RESPONSE: We have deleted "but has recovered well" to remove confusion.

The text is still too small in Fig 2

RESPONSE: Revision done. We have increased all the text size in Fig 2.

Figure S5 still shows a single seasonal cycle for LFMC and it seems to underestimate the within season variability of LFMC relative to the observations. This doesn't get at my question about model validation of year to year variability. Additionally, it is difficult to tell what is being shown in Fig S5 without further explanation. I suggest the authors elaborate further if they would like to keep this in the SI

RESPONSE: Revision done. We have replaced Fig. S5 with new validation figure, which showed observed and simulated monthly mean LFMC during 2006-2019 for chamise (*Adenostoma fasiculatum*) in Stunt Ranch at Santa Monica Mountain. This figure showed that our model can capture seasonal and interannual variations in LFMC during 2006-2019, except for peaks in a subset of years (see above).

Why is there a No-RH included in Figure 5? This is not discussed in the text. I mentioned this previously and my comment was not addressed

RESPONSE: Revision done. We have removed "No-RH" in Fig. 5 and also deleted the description of relative humidity throughout the manuscript.