

## ***Interactive comment on “Assessing Climate Change Impacts on Live Fuel Moisture and Wildfire Risk Using a Hydrodynamic Vegetation Model” by Wu Ma et al.***

### **Anonymous Referee #3**

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Ma et. al. use FATES (Functionally Assembled Trait Simulator) with a coupled hydrodynamic model, FATES-HYDRO, to predict LFMC (Live Fuel Moisture Content) in three newly-developed chaparral PFTs (Plant Functional Types) in a Santa Monica Mountains chaparral ecosystem. FATES was validated using local weather station data to force the model and an annual cycle of measured live fuel moisture content measurements (LFMC). The model was then used to predict LFMC and fire season length over the historical period of 1950-1999, and a future period from 2075-2099 according to RCP 4.5 and 8.5 scenarios. Fire season length and number of high fire risk days was defined according to the LFMC critical threshold of 79%. This paper addresses an important gap in wildfire modeling through developing and validating modeled LFMC.

C1

The authors then use the FATES model to understand how LFMC may change with climate change. This work has the potential to be of significant interest to Biogeosciences readers and makes important progress on mechanistically forecasting LFMC. However, I have several significant concerns related to methodological clarity and presentation that need to be addressed prior to publication.

First, there needs to be significantly more detail in the methods. For example, it is not explicitly explained how LFMC was calculated in FATES. Other necessary details such as the meteorological forcing time resolution are omitted. I assume that the met forcing is subdaily based on previous FATES-hydro papers, but this is not specified. Additionally, I am somewhat confused about the input meteorology and how the humidity relates to the authors' temperature experiments. On lines 238-239, the authors indicate that forcings include both relative humidity and specific humidity. However, in their elevated temperature experiments, the authors do not mention recalculating specific humidity so that relative humidity is conserved. This is an important step because it will impact the temperature effect on VPD and subsequent model-predicted LFMC. How did the authors handle specific humidity for their temperature scenario? More broadly, one of their hypotheses is very warming centric, but it would help the reader for the authors to focus more on the mechanism by which warming impacts LFMC in the hypothesis and subsequent and discussion. It is also not clear how the authors spun up soil moisture for their validation. These are among many questions I was left with by the lack of methodological detail.

Field data: 1) Details about the study site could be discussed more thoroughly rather than referencing the Venturas 2016 paper. These could include: topography (including hill slope and aspect), substrate, fire history, and other ecotypes present. 2) Do all PFTs follow a similar spatial distribution throughout the site? Or are they spatially separated into microclimatic or topographical/hydrological niches? It is stated that LFMC observations are weighted by relative abundance of PFTs, but the relative abundance itself and how this calculation are not discussed. 3) Leaf water potential and water

C2

content are used as metrics of LFMC. Is stem LFMC derived from these metrics?

The authors developed 3 new shrub PFTs following standard tree allometry. This is not necessarily a trivial task as shrubs don't have a dbh in the traditional sense that a tree does. In this manuscript context, this distinction might be particularly important because the different branching and pathlength patterns for stems of chaparral species could impact the hydraulics and the underlying assumption that a shrub is analogous to a small tree is not really discussed. Related to this, based on the Fig. 2 validation and particularly panels (b,d,f) R2 and slopes, it is unclear whether the authors achieve any notable increase in model skill by parameterizing three different PFTs versus one or two. Model parsimony is not examined in this context and the larger number of PFTs is not justified, rather just assumed, in the text. Note that the axes for all subpanels should all be the same scale for Fig. 2 (and for all figures).

Model validation: Given that the aim is to make long-term forecasts and understand the variability related to climate, it would be better to have at least two seasonal cycles of observed LFMC. Currently it is not clear if the model can skillfully capture interannual variability, which would be important for the long term questions the authors aim to ask

The authors do a significant amount of work running simulations across climate models, but do not discuss in the text model spread and how that plays into future fire season uncertainty. This is a clear missed opportunity and does not take advantage of the large amount of effort that the authors invested in these simulations.

The methodology for the bootstrapping calculations is unclear and needs to be described more in the methods if the authors would like to include it in their analysis. A methods section devoted to 'statistical analyses' would be appropriate,

Stylistic comments: The figures need significantly more work. Please reexamine the color schemes, increase text size, remove underscores, and standardize axes across panels within the same figure. Figure 5 is a particularly big offender. Figure S2 might be a good candidate for the main text. Figures 3 and 4 should have the same shade

C3

for the different models as they are not discussed individually. PFT names are a bit clunky. A better shorthand might make for easier reading. Figure 5 labels could be clearer. Figure 1 is unhelpful. Why is there a No-RH included in Figure 5? This is not discussed in the text.

Overall, the introduction is well written. However, H4 was not well motivated. Additionally, when the authors refer to H1-H4 in the results/discussion, it is difficult to the specific hypotheses. It would be helpful to the reader if hypotheses were written out subsequently. H1 is this VPD increases through warming? It would be helpful for the authors to explicitly say this

Given that fire season length is not validated, rather it is defined as number of days with LFMC below 79%, it would be very useful and informative for the authors to test the sensitivity of their forecasts to different reasonable LFMC thresholds.

Line specific/minor:

L162 Could the authors be more specific? Is this average max and min annual temperature?

L171-173 it would be nice to give a few more details here about the study site

L234-236 How was LFMC calculated in the model?

L238-239 what is the temporal resolution of the met data? Were historical and future data extracted just for the grid cell above the study site? Were they interpolated to sub daily? L273 does the increase in number of PFTs appreciably improve model performance? Isn't interannual variability important, particularly for long term forecasts?

L298 warming-driven VPD increases? It would help the reader if you are specific about the mechanism

L310 given that fire season length was not validated and dependent on the 79% threshold, it would be good for the authors to test the sensitivity of changes in fire season

C4

length to this assumed threshold

L319 In an ecological context, I imagine that these different PFTs are coexisting in the same location. What do the authors think that the coexistence and heterogeneity in LFMC that result will do to impact fire behavior and fire season length?

I think that it is reasonable that the authors turned off growth/mortality, but this choice is not without implications. The authors need to discuss the possibility that veg density might decrease and LFMC could be conserved under future scenarios

Thanks for the interesting read!

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