RESPONSE TO REVIEWER #2

First off, apologies it took me so long to get my review in.

The manuscript examines future changes in live fuel moisture content (LFMC) of three plant types found in Southern California chaparral shrubs, as simulated by the plant hydraulic model driven by ESM output. Factorial experiments determine if precipitation, temperature or CO2 are drivers of LFMC changes. The modelling framework suggests that LFMC is likely to fall for all plant types, driven primarily by increased temperatures and, under the more extreme RCP8.5 scenario, precipitation seasonality changes. Validation against present-day measurements of plant LFMC adds confidence to these results.

The study fills a fundamental knowledge gap required to improve fire dynamics in many fire modelling disciplines. The paper is, on the whole, very well written and was an enjoyable read. The authors make clear the aims and hypothesis, and the methodology seems to be designed to make results easy to digest - targeting each hypothesis specifically. However, a lack of (or sometimes excessive) detail in the methods made interpretation of figures difficult and would make it hard to reproduce results. Therefore, I have asked for major revisions so that methodology can be adequately assessed in another round of reviews. Though assuming the methods are sound, I suspect a lot of the manuscript will not need much changing.

Best of luck with the rest of the reviews. On the whole, it’s a great paper, and I look forward to reading a revised version.

Douglas Kelley

RESPONSE: Thank you very much for the positive comments. We have added more details in the methodology in the paper so that interpretation of figures is more straightforward to understand and easy for others to reproduce results.

General comments

Methods

I struggled to see where LFMC is calculated in the model. Maybe this is obvious to people who do more with plant hydraulics? But it would be useful to have a “dummies guide” sentence or two for people like me.

RESPONSE: Thank you very much for your suggestion. We have added one paragraph to explain how we calculate LFMC in the model in Page 14 Lines 297-314 (please refer to revision changes tracked manuscript) as follows.

"*In this study, we used measured LFMC to validate simulated LFMC. FATES-HYDRO does not directly simulate the LFMC. Thus, we estimated the LFMC based on simulated LWC. The LWC in the model is calculated as follows,*

$$LWC = \frac{fw-dw}{dw} * 100, \quad (4)$$

where, fw is the fresh weight and dw is the dry weight, which are simulated within FATES-HYDRO. Then, we estimated the LFMC within leaves and shoots 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) as follows (Fig. S4),

$$LFMC = 31.091 + 0.491LWC, \quad (5)$$

The climate in Saura-Mas and Lloret's study is Mediterranean (north-east Iberian Peninsula), which is consistent with the climate of our study area. LFMC was measured on our site approximately every three weeks, concurrently with plant water potentials in 2015 and 2016. LFMC measurement details can be found in Pivovaroff et al. (2019). For comparison with our model outputs, we calculated the mean LFMC within leaves and shoots for each PFT weighted by the species abundance (Venturas et al. 2016). Species abundance was calculated by dividing mean density of a specific species by the mean density of all species."

I *think* there are some extra details that might have crept into the model description about how the FATES could be used but not turned on this study. This made it difficult to work out what the model actually produces. For example line 175, we told that FATES cohorts and successional trajectory-based patches, and on line 180, we'd told about how it simulates mortality. Though not all these processes are used in this study? (line 210-2014 sounds like a lot of this is turned off in "reduced-complexity configuration"?) Likewise, it wasn't actually coupled to E3SM (line 183) for this study, but was there anything used from ELM, or was this turned off as well? If so, is the reduced-complexity model transient, or just it just simulates time slices of plant hydrology? If the authors want to include the extra details about FATES, it should be linked to some results or discussion points (see next general comment) and it should be made clear when introduced that those aspects of the model are not used here.

RESPONSE: Thanks for pointing out the confusion. We have modified the model description to reduce the potential confusion. Specifically, we made the following modifications. First, we have removed the unrelated process description such as mortality and growth. Second, we clarified that FATES has to be coupled to a host land model to simulate soil hydrology, canopy temperature and transpiration. Please see the description details in Pages 9-10 Lines 194-212 as below:

"*FATES is a vegetation demographic model (Fisher et al. 2015), which uses a size-structured group of plants (cohorts) and successional trajectory-based patches based on the ecosystem demography approach (Moorcroft et al. 2001). FATES simulates the demographic process including seed production, seed emergence, growth and mortality (Koven et al. 2020). Because the main purpose here is to assess LFMC, we controlled for variation in plant size structure that could arise from plant traits or climate differences between model runs by using a reduced-complexity configuration of the model where growth and mortality are turned off and ecosystem structure is held constant. FATES has to be hosted by a land surface model to simulate the soil hydrology, canopy temperature and transpiration. These host land models include the Exascale Energy Earth System Model (E3SM, Caldwell et al., 2019) land model (ELM) as well as the Community Earth System Model (Fisher et al 2015) and the Norwegian Earth system model (NorESM, Tjiputra et al 2013). In this study, we used the DOE-sponsored ELM as our host land model. The time step of FATES to calculate carbon and water fluxes is 30 minutes and it can downscale the data from 6-hourly climate drivers.*"

For running the model, gathering measurements and performing cluster analysis, the authors need to add in detail that would allow someone else to reproduce the results, which can include clear references to instructions in other papers. For modelling protocol, here are some examples:

• Is the model transient or equilibrium? If transient, where are initial conditions from? (spin-up etc)

RESPONSE: We added a separation section of "Model initialization (section 2.4)" to describe the model initialization process in Pages 13-14 Lines 287-295 as follows.

"*Our model simulation is transient in terms of soil water content, leaf water content, carbon and water fluxes. The forest structure (plant sizes and number density) is fixed and is parameterized based on a vegetation inventory from Venturas et al. (2016). The soil texture and depth information are parameterization based on a national soil survey database (https://websoilsurvey.sc.egov.usda.gov/App/WebSoilSurvey.aspx; Table S1). The soil moisture is initialized with 50% of the saturation and the tissue plant water content is initialized so that it is in equilibrium with the soil water potential. We run the model for 10 years based on 1950-1960 climate so that the simulated soil moisture, leaf water content, carbon and water fluxes are not depending on their initial conditions.*"

• What's the time step of the model? Did it match the driving data and if not, was there any disaggregation required (either pre-processing or in the model itself)?

RESPONSE: We put specifically the following sentence in the model description: "*The time step of FATES to calculate carbon and water fluxes is 30 minutes and it can downscale the data from 6-hourly climate drivers.*"

• If inputs are on a time step >1 day, is "temperature" mean, max or min?

RESPONSE: Inputs are 6-hourly and we have modified that in the climate driver section. We downscaled the MACA daily data to 6-hourly based on the temporal anomaly of the observed mean daily data to the hourly data for each day during 2012-2019.

• Where did CO2 information come from? And how was it expressed (i.e, ambient or internal concentration? Daily, monthly or Annual? etc)

RESPONSE: We added the following sentence to describe the data for the CO_2 concentration:

"*The model is driven by yearly CO_2 data obtained from Meinshausen et al (2011).*"

• What do the model outputs look like? I.e whats the temporal resolution (if transient), and how are they aggregated into the 25-year blocks in the time series (Fig 3, 4).

RESPONSE: The model outputs can be hourly to monthly. In this study, we used daily mean leaf water content and calculated the metrics based on this daily mean value for every 20-year period.

In terms of validation measurements, did they come from another study? If so, indicate which one. If you did it yourself, please provide more information:

• Were measurements taken from the same study site, or is it based on lab measurements?

• How did you measure LFMC? Was it through destructive sampling, and if so, how was water and dry weight preserved after sampling but before measurement?

• How many samples went into each measurement? And were they from the same plant, or where multiple sampled?

• How were samples/plants selected?

• Was the same plant(s) revisited every 3 weeks?

• Was LFMC measured throughout the plant(s), or just leaves?

• What equipment was used? Are there any calibration notes required?

• Please provide the date range measurements were made (From Fig 2. it looks like August to March?) and notes of any major deviations from 3-week sampling time.

RESPONSE: In terms of validation measurements, they came from another study (Pivovaroff et al. 2019). We've added this to the main text in Page 14 Lines 309-311 as follows.

"*LFMC was measured on our site approximately every three weeks, concurrently with plant water potentials in 2015 and 2016. LFMC measurement details can be found in Pivovaroff et al. (2019).*"

Finally, for clustering and parameter selection, again, indicate which study this came from or else include for e.g.:

• Which clustering algorithm was used, and what parameters (seeds, chains etc) were set to run it.

RESPONSE: Hierarchical cluster analysis was used to define three plant functional types based on allometry and hydraulic traits of eleven chaparral shrub species. Allometry and hydraulic traits data of eleven chaparral shrub species were displayed in Table S2 of the supplementary. We've added this to the main text (Page 12, Line 262).

• Which dimensions were clusters based on (i.e is it the VCmax, P50 listed in the next sentence? Any others?)

RESPONSE: Clusters were based on allometry and hydraulic traits of eleven chaparral shrub species in Table S2 of the supplementary. We've added this to the main text (Page 12, Lines 268-270).

• How was the number of clusters (PFTs?) was decided, or if it was a pragmatic choice to match the model

RESPONSE: First, the dendrogram is built and every data point finally merges into a single cluster with the height shown on the y-axis. Then we cut the dendrogram in order to create the desired number of clusters determined by a pragmatic choice based on hydraulic traits of eleven chaparral shrub species. We've added this to the main text (Page 12, Lines 263-266).

• What software was used. And include a software package reference.

RESPONSE: R's rect.hclust() function was used to see the clusters on the dendrogram. We've added this to the main text (Page 12, Lines 266-268).

• Which specific FATES parameters were estimated?

RESPONSE: All parameters of allometry, leaf and wood traits, and hydraulic traits were collected from observations shown in the Table S2 and S3 of the supplementary. We've pointed this to the main text (Page 12, line 268-270).

There are also some points about methods detail in specific comments

Linking this study to other fire measures

Results are very interesting, and goes to show that Southern California might be in some serious fire-related trouble. But it would be hard to make management recommendations or quantitative analysis of the impact this might have on future fire regimes. The perfectly okay (can't do everything in one paper), though it would give m/s a bit more impact if there was some discussion at the end of how this could be incorporated into the applications mentioned on line

117-120. For my area of global vegfire modelling, this m/s seems like a simple yet effective approach for incorporating a more mechanistic way of simulating LFMC, which could potentially improve some of the moisture-related biases found in most models when simulating (line 63/64) "combustion, fire spread and fire consumption" (see, e.g. (Forkel et al., 2019)), especially rate of spread based models stitch as (Thonicke et al., 2010). It might also have a big impact on future fire-carbon cycle assessments (Kloster and Lasslop, 2017). I'm sure there would be benefits for fine-scale fire behavior and landscape-scale fire disturbance models as well that could be discussed.

RESPONSE: Thank you for your suggestions. We have added one paragraph at the end of Discussion part to discuss why it is important to incorporate LFMC into the applications of future wildfire models in Pages 23-24 Lines 510-520 as follows.

"*Application of a hydrodynamic vegetation model to estimate LFMC dynamics could potentially benefit wildfire modeling at the fine-scale, landscape-scale, and global-scale. This is because LFMC is one of the most critical factors influencing combustion, fire spread, and fire consumption while previous wildfire models mainly focus on impacts of dead fuel moisture, weather conditions on wildfire, fuel loads, and representation of live fuel moisture (Anderson & Anderson 2010; Keeley et al. 2011; Jolly & Johnson 2018). The implications of this are that fire potential will vary with plant water potential and uptake from soils, photosynthetic and respiratory activity, carbon allocation and phenology with variability across species and over time (Jolly & Johnson 2018). Therefore, future work to incorporate LFMC dynamics in wildfire models could potentially play a vitally important role in the future studies of wildfire modeling under climate change.*"

Specific comments

Line 52 & again on line 121/122: The authors state that models simulate fire "from climate and dead fuel moisture" only. This is not strictly true for global-fire models (though I'm not so sure about the rest), where many do have live fuel representation (Hantson et al., 2016; Rabin et al., 2017) and some related empirical/stats studies use proxies of live fuel moisture (Bistinas et al., 2014; Kelley et al., 2019). The knowledge gap for these models is that the representation of LFMC is often quite crude, and are based on i.e top layer soil moisture (Thonicke et al., 2010) or supply/demand indices (Bistinas et al., 2014; Kelley et al., 2019) and not plant hydraulics.

RESPONSE: Revisions done. We have added more explanations to state that related empirical studies use proxies of live fuel moisture and live fuel representation in Page 3 Lines 57, 62-66 as follows.

"*Limited studies have applied proxies of live fuel moisture in global-fire models.*"

"*In global-fire models, related empirical studies use proxies of live fuel moisture (Bistinas et al. 2014; Kelley et al. 2019) and live fuel representation (Hantson et al. 2016; Rabin et al. 2017). While previous studies provide great insights into fire risks with changes in climate, dead fuel moisture, fuel loads, and representation of live fuel moisture*"

Line 70: Is this meant to be "(Dennison and Moritz, 2010)"? (It might be me that's got the wrong year). I *think* (Dennison and Moritz, 2010) was based on Southern California Chaparral as well, right? If so, state that here, cos it makes clear the "79%" is specific to the region. Also, is there an uncertainty bound on this that needs mentioning?

RESPONSE: Revisions done. The citation should be (Dennison and Moritz, 2009) if you check the downloaded paper. Additionally, we have made clear the "79%" is specific to the Southern California Chaparral in Page 4 Lines 80-82 as below. Furthermore, there is no uncertainty bound on the threshold that needs mentioning based on the paper (Dennison and Moritz, 2009).

"*Dennison & Moritz (2009) found strong evidence of a LFMC threshold (79%) for southern California chaparral shrubs, which may determine when large fires can occur in this region.*"

Line 71: Is "but" the right word?

RESPONSE: Revision done. We removed the word "but".

Line 82: "The sensitivity of LFMC to climate change..." and CO2 concentration? Line 86: replace "should" with "could"

RESPONSE: Revisions done.

Line120: (Hantson et al., 2016; Rabin et al., 2017) are better references, as they detail most global-scale fire models. (Thonickeetal., 2010) can be used anywhere when you are talking specifically about rate-of-spread fire models (see general comments).

RESPONSE: Revision done. We have replaced (Thonicke et al., 2010) as (Hantson et al., 2016; Rabin et al., 2017).

Line 121: Only some global fire models use “fire danger indices”. Replace with something like "the fire measures these simulate".

RESPONSE: Revision done.

Line 122: I’m not sure what “dynamic prediction” means in this context? Line 122-124, sentence starting “One key limitation...”: Yes! For my research at least, this is the exciting thing about this paper.

RESPONSE: Revision done.

Line 130-142: I wonder if the model allows for the non-additive impact of precipitation and temperature? If you’re not sure, you don’t necessarily have to test this. But maybe somewhere (perhaps discussion) acknowledge that this is a model result, and the models ability to simulate non-linear impacts of climate needs to be assessed to strengthen confidence in this result.

RESPONSE: Revision done. Yes, the FATES-HYDRO allows for the non-additive impact of precipitation and temperature.

Line163: Maybe add the mean number of dry days in the dry season if that information is available?

RESPONSE: The mean number of dry days in the dry season could be difficult to define due to the non-standard criteria on dry days.

Line 166-171: Reference Fig. 1 so we can see which PFTs they’ve been added to.

RESPONSE: Revision done.

Line 222-228: I think Biogeosciences allow bullet lists? (a question for the editor?) If so, PFT definitions as a bulleted list might be a little easier to read.

RESPONSE: PFT definitions have already been abbreviated and we will check with the editor to see if a bullet list is allowed.

Line 244: sentence starting “The MACA dataset”: Is this sentence saying MACA regridded EMSs from their native grid to 1/24th degree grid?

RESPONSE: Yes, this sentence says MACA regridded EMSs from their native grid to 1/24th degree grid.

Line 247: I’m not sure what the last sentence of this paragraph is saying. What was the “training data” training?

RESPONSE: Revision done. We have removed “As training data for MACAv1/v2-METDATA” to reduce confusion.

Line 248: I’m not sure you’ve told us what “v2-METDATA” is yet?

RESPONSE: Revision done. We have removed “As training data for MACAv1/v2-METDATA” to reduce confusion.

Line 255: Why were 1950-1999 and 2075-2099 used as historic and future periods? Was it a citable recommendation? AR5, I think, used 1981-2000 and 2081-2100 (Collins et al., 2013). I don’t think there’s anything technical writing using that period, just 1950 seems quick a long time before the present day.

RESPONSE: Thank you very much for your suggestion. We have changed the historic and future periods to 1960-1999 and 2080-2099.

Line 263: Any particular reason why 1986-2005 was selected? Wouldn’t it make more sense to use the same historic period.

RESPONSE: This is because the MACA climate datasets (https://climate.northwestknowledge.net/MACA/data_csv.php) are divided as historical (1950-2005) and future periods (2006-2099), thus we selected historical 20-year period (1986-2005) to replace every 20-year for the future period.

Line263: was this cycled data detrended? If not, was there a “jump” in the climate input for factorial experiments? And what impact (if any) would that have on the model?

RESPONSE: The reason we used 20-year period is to detrend this cycled data because 20-year period is long enough to avoid locating in the historical drought period and no obvious trend was found in this 20-year period.

Line 272: It might be worth including an NME or NMSE as well (e.g. (Kelley et al., 2013)). There appear to be some biases (see intercept in regression in Fig2) which R2 wouldn't pick up. It might be worth commenting on the models ability to simulate LFMC below the "79%" threshold used for fire season length. MPCH, for example, seems to struggle to get the very low LFMC values (Fig 2)

RESPONSE: Revisions done. We revalidated the FATES-HYDRO and updated the Fig. 2. It captures low LFMC values better for three PFTs with the updated results. We also mentioned the model's ability to simulate LFMC below the "79%" threshold used for fire season length.

Line 278: Is "daily mean" meaned over the decade, year or the dry season. From Fig 3, it looks like 25-yearly averages?

RESPONSE: "daily mean LFMC" was meant to be daily LFMC over the period.

Line 334-338: The "strong relationship between observed and simulated" does look good, but it might be worth acknowledging the (necessarily) small amount of validation data used. I.e, if validation of the models ability to simulate long term trends due to changes in climate/CO2 (either from histic record or FACE/lab experiments) would add confidence to the model results, if and when required data becomes available.

RESPONSE: Revisions done. We have acknowledged the small amount of validation data were used in this study. Furthermore, we also added 95% confidence interval to the model predictions.

Line 355-360: I'm not sure I follow this argument. Surely the model could also be underestimating the CO2 effect?

RESPONSE: Revision done. We have mentioned that the model could also be underestimating the CO_2 effect.

Line 364: Have you checked that there is a change in seasonality in precip or a reduction in dry season precip in the future climate inputs? It would be hard to see that there isn't given the precip factorial experiment, but maybe a quick plot of dry season precip (and for that matter, temperature) time series would help check the model is responding as expected. Maybe something for the SI?

RESPONSE: Yes, we have checked that there is a reduction in spring and autumn precipitation in the future climate inputs from the Fig. S2.

Line 396: Please acknowledge and cite any software and software packages/libraries used in this study. Also, please check with data providers about any acknowledgement statements that might be required.

Please provide either a repository link and doi or code availability statement for models used and analysis performed. Also, please provide a data link or statement of any model input and output.

RESPONSE: Revisions done.

Fig2: What was the cause of some of the big uncertainty bars? And was the measurement uncertainty included in the regression on the right-hand plots?

RESPONSE: Revision done. The big uncertainty bars display confidence interval around smooth in smoothing function lm(y ~ x). We have removed uncertainty bars in the updated figure in order to reduce confusion.

Fig2: Are the axes the correct way round on the plots on the right? It looks like the y-axis "FATES-HYRDO" range in b, d, f match the "Field" blue lines in a, c, d?

RESPONSE: Revision done. We have renewed the figure where the axes are the correct way round on the plots on the right.

Fig 3: You've included factorial experiments for the impact of change in RH, but I can't see where you presented in the text? If you didn't, why not? Or why did you include it in the figure?

RESPONSE: In the Discussion and Conclusions parts, we have presented that relative humidity was the least important drivers among four climatic variables in the text.

References

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