RESPONSE TO REVIEWER #1

This manuscript examines changes in live fuel moisture content (LFMC) under climate change. As the authors note, most similar studies focus on changes in dead fuel moisture or weather. I note that similar studies have also included fuel load, which was not mentioned here, see for example Clarke et al. (2016, doi:10.1007/s10584-0161808-9).

RESPONSE: Thank you for pointing this out. We have added the fuel load studies in Page 3 Lines 55-67 (please refer to revision changes tracked manuscript) as follows.

"So far, prior studies have mainly focused on impacts of dead fuel moisture, fuel loads, and weather conditions on wildfire. Limited studies have applied proxies of live fuel moisture in global-fire models. For example, dead fuel moisture is found to be related to fire ignition and fire spread potential (or potential area burnt) (Aguado et al. 2007), specific weather conditions such as increased vapor pressure deficit (Williams et al. 2019) can lead to a vast increase in fire activity (Goss et al. 2020), and wildfire fuel loads are projected to increase under climate change (Matthews et al. 2012; Clarke et al. 2016). In global-fire models, related empirical studies use proxies of live fuel moisture (Bistinas et al. 2014; Kelley et al. 2019) as well as the specific representation of live fuels (Hantson et al. 2016; Rabin et al. 2017; Jolly & Johnson 2018). While previous studies provide great insights into fire risks with changes in climate, dead fuel moisture, fuel loads, and representation of live fuel moisture, there is still limited understanding of how climate change influences live fuel moisture content (LFMC) and the consequent wildfire risks."

Examining LFMC dynamics under climate is novel, and of broad interest for predicting changes to fire risk under climate change. So I was very much looking forward to reading this manuscript. The work presented is indeed novel, and it is exciting to see this type of research going forward.

RESPONSE: Thank you very much for the positive comments.

However, the authors did not provide any detail on how LFMC was modelled, which makes it difficult to assess the validity of the methods used, or for other researchers to build on this work, or apply similar methods in their own study systems. On lines 208-209 the reader is referred to Christoffersen et al. (2016) for the formulation of LFMC. However, Christoffersen does not explicitly examine LFMC. In most ecophysiology models, relative water content, rather than

LFMC, is modelled. While the two are related, they are different, with LFMC being dependent on leaf dry matter content. I would strongly urge the authors to devote a significant section to detailing how they went from RWC to LFMC, and how exactly they modelled LFMC. Since this is the aspect of their work that is most novel, the authors should describe the equations used to model LFMC, and their derivation. On a related note, on line 333, the authors note that LFMC is calculated on foliage and fine branches. Given that RWC generally is only calculated for leaves, can we assume that there is a mis-match between the LFMC that is modelled, and that which is measured? There needs to be more detail in the methods on how LFMC was defined, (e.g. what size diameter class of twigs, or just leaves?), and how this was dealt with in the modelling study. The reader should not have to go to related papers to find out this fundamental information.

RESPONSE: Thank you for pointing this out. Regarding the mis-match between the observed and simulated LFMC, we have clarified the definition of observed and simulated LFMC in Page 14 Lines 297-314 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,*

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

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),

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

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.*"

Furthermore, we also revalidated FATES-HYDRO using the updated LFMC, which was estimated from the simulated LWC, to compare with observed LFMC (Fig. 2). Then we updated all corresponding results throughout the manuscript.

Overall the discussion was satisfactory, but the authors could have gone further. I'm intrigued by the potential implications of leaf senescence and indeed whole plant mortality on flammability. Dead fuels decline in moisture content far below those of live fuels, so understanding changes in canopy die-back and mortality will be important for understanding changes in vegetation flammability. Could the authors comment on whether their modelling approach could be extended to examine this? I'm not suggesting the authors do this for this study, but rather discuss the potential to examine these factors which are also likely to be important. Further, the discussion would also be improved by acknowledging the potential for vegetation transitions under climate change, and discussing the potential implications for flammability. An explicit examination of this is beyond the scope of the study, but some discussion is still warranted.

RESPONSE: We have added a paragraph of discussion focusing on the potential implications of leaf senescence and indeed whole plant mortality on flammability and the potential for vegetation transitions under climate change in Page 23 Lines 497-509 as follows.

"*Because the moisture content of live fuels (~50–200%) are much higher than that of dead fuels (~7–30%), leaf senescence induced by drought stress and subsequent mortality are potentially vital factors to cause large wildfires (Nolan et al. 2016, 2020). Thus drought-induced canopy die-back and mortality could largely increase surface fine fuel loads and vegetation flammability, which can increase the probability of wildfire (Ruthrof et al. 2016). Since growth and mortality are turned off in model runs by using a reduced-complexity configuration, it is possible that vegetation density might decrease and LFMC could be conserved under future scenarios. In addition, potential vegetation transitions (e.g., shrubs to grassland and species composition changes) might substantially affect flammability and thus fire intensity and frequency. In this study, we used the static mode of FATES-HYDRO to simulate LWC dynamics under climate change. If we need to assess how the leaf senescence and vegetation dynamics will* *impact the fire behavior, we can use the same model with dynamic mode to assess their impacts on fire behaviors under future drought and warming conditions.*"

I have some additional minor comments below. Line 54: you've cited Caccamo et al. 2012a when talking about dead fuel moisture, but this study focused on live fuel moisture.

RESPONSE: Thank you for pointing this out and we have moved the citation (Caccamo et al. 2012a) to describe live fuel moisture.

Line 68: Here, I'd suggest referencing some of the more recent published literature on LFMC rather than relying on a PhD thesis: Nolan et al. (2016, doi:10.1002/2016GL068614) Yebra et al., (2018, doi:10.1016/j.rse.2018.04.053) Pimont et al. (2019, doi:https://doi.org/10.1071/WF18091) Rossa et al. (2018, doi:10.3390/fire1030043)

RESPONSE: Thank you for providing the references and we have added these more recent published literatures on LFMC in Page 4 Lines 78-79.

Line 101: strictly speaking drought is an irregular period of water deficit, rather than a predictable, annual period of low rainfall. I'd suggest re-wording this to avoid the term "drought" when referring to annual climate patterns. Particularly since the extreme fire behaviour really isn't seen on an annual basis, but only during severe drought periods.

RESPONSE: Revision done. We have changed "annual summer drought" to "annual dry season".

Fig. 4 is difficult to read with so many different climate models. It's not entirely clear what all the different climate models are, since they are not described in the methods. I would suggest the authors pick the models that are most appropriate for their study region, and just present those. This should limit the number of lines to a handful at most. This information should also be provided in the methods.

RESPONSE: Thank you for your suggestions and we have improved Fig.3 and 4 to display results more clearly. Specifically, for each panel of figure 3 and 4, we only include mean value and 95% confidence interval of all the different climate models.

Line 333: did you just look at LFMC in leaves though, and not small branches? What diameter size class was used?

RESPONSE: The live fuel moisture content include leaf and shoot (< 6 mm diameter) water content. We've estimated the total live fuel moisture from the leave moisture content based on an empirical relationship (see our response previously: Page 14 Lines 297-314).

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

Bistinas, I., Harrison, S. P., Prentice, I. C. and Pereira, J. M. C.: Causal relationships versus emergent patterns in the global controls of fire frequency, Biogeosciences, 11(18), 5087–5101, 2014.

Collins, M., Knutti, R., Arblaster, J., Dufresne, J.-L., Fichefet, T., Friedlingstein, P., Gao, X., Gutowski, W. J., Johns, T., Krinner, G. and Others: Long-term climate change: projections, commitments and irreversibility, in Climate Change 2013-The Physical Science Basis: Contribution of Working Group I to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change, pp. 1029–1136, Cambridge University Press., 2013.

Dennison, P. E. and Moritz, M. A.: Critical live fuel moisture in chaparral ecosystems: a threshold for fire activity and its relationship to antecedent precipitation, Int. J.Wildland Fire, 18(8), 1021–1027, 2010.

Forkel,M.,Andela,N.,Harrison,S.P.,Lasslop,G.,vanMarle,M.,Chuvieco,E.,Dorigo, W., Forrest, M., Hantson, S., Heil, A., Li, F., Melton, J., Sitch, S., Yue, C. and Arneth, A.: Emergent relationships with respect to burned area in global satellite observations and fire-enabled vegetation models, Biogeosciences, 16(1), 57–76, 2019.

Hantson, S., Arneth, A., Harrison, S. P., Kelley, D. I., Prentice, I. C., Rabin, S. S., Archibald, S., Mouillot, F., Arnold, S. R., Artaxo, P., Bachelet, D., Ciais, P., Forrest, M., Friedlingstein, P., Hickler, T., Kaplan, J. O., Kloster, S., Knorr, W., Laslop, G., Li, F., Melton, J. R., Meyn, A., Sitch, S., Spessa, A., van der Werf, G. R., Voulgarakis, A. and Yue, C.: The status and challenge of global fire modelling, Biogeosciences, 13(11), 3359–3375, 2016.

Kelley, D. I., Prentice, I. C., Harrison, S. P., Wang, H., Simard, M., Fisher, J. B., and Willis, K. O.: A comprehensive benchmarking system for evaluating global vegetation models, Biogeosciences, 10, 3313–3340, https://doi.org/10.5194/bg-10-33132013, 2013.

Kelley, D. I., Bistinas, I., Whitley, R., Burton, C., Marthews, T. R. and Dong, N.: How contemporary bioclimatic and human controls change global fire regimes, Nat. Clim. Chang., 9(9), 690–696, 2019.

Kloster, S. and Lasslop, G.: Historical and future fire occurrence (1850 to 2100) simulated in CMIP5 Earth System Models, Glob. Planet. Change, 150, 58–69, 2017.

Rabin, S. S., Melton, J. R., Lasslop, G., Bachelet, D., Forrest, M., Hantson, S., Li, F., Mangeon, S., Yue, C., Arora, V. K. and Others: The fire modeling intercomparison project (FireMIP), phase 1: experimental and analytical protocols, Geoscientific Model Development, 20, 1175–1197, 2017.

Thonicke, K., Spessa, A., Prentice, I. C., Harrison, S. P., Dong, L. and CarmonaMoreno, C.: The influence of vegetation, fire spread and fire behaviour on biomass burning and trace gas emissions: results from a process-based model, Biogeosciences, 7(6), 1991–2011, 2010.

RESPONSE TO REVIEWER #3

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.

RESPONSE: Thank you very much for the positive comments.

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.

RESPONSE: Revision done. We have added one paragraph to explain how to calculate LFMC in the model in Page 14 Lines 297-314 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,*

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

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),*

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

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."

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.

RESPONSE: Revision done. We have specified that the FATES-HYDRO model was forced by 6-hourly climate data in Page 15 Lines 316-317 as follows.

"*We forced the FATES-HYDRO model with 6-hourly temperature, relative humidity, precipitation, downward solar radiation, and wind components.*"

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.

RESPONSE: Sorry for the confusion. The model only used the relative humidity and thus we removed the specific humidity from the text. Thanks for pointing this out and agreed that it is important for testing the hypothesis.

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.

RESPONSE: Revision done. We have added Fig. S3 to show soil moisture validation at 5-cm depth and specified soil texture information for Stunt Ranch in the Supplementary Table S1. The model is able to generally capture the pattern but has some underestimation/overestimation. As we know the model tends to have underestimation/overestimation of soil moisture at the surface layers due to numerical scheme and thus we are not too much concerned. Unfortunately, we have no soil moisture data below 5cm.

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.

RESPONSE: Revisions done. We have added more descriptions of the study site, including topography, 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.

RESPONSE: We assumed that all PFTs follow a similar spatial distribution throughout the site and they do not spatially separate into microclimatic or topographical/hydrological niches. In addition, how relative abundance calculation are discussed as follows.

"*Species abundance was calculated by dividing mean density of a specific species by the mean density of all species.*"

3) Leaf water potential and water content are used as metrics of LFMC. Is stem LFMC derived from these metrics?

RESPONSE: Thanks for pointing this out. We have added one paragraph to explain how to calculate LFMC in the model as mentioned in our previous response (see Page 14 Lines 297-314).

The authors developed 3 new shrub PFTs following standard tree allometry. This is not necessarily a trivial task as shrubs dont 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 path length 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.

RESPONSE: Thank you for this good point. In our model, we assumed that the allometry of a shrub is analogous to that of a small tree but we did modify the code so that there are several key difference. We have added the following paragraph to make these points to the main text (see Paraph below and Page 12 Lines 250-261).

"*For this study, we assumed that the allometry of a shrub is analogous to that of a small tree. However, we did make several important modifications to accommodate the allometry of shrub as their height and crown area relationships to diameter could be different from trees. First, instead of using the diameter at breast height as the basis for allometry to calculate the height, crown area and leaf biomass, we used the basal diameter as the basis for shrubs. Second, in the allometry of trees, the diameter for maximum height (d1: Fates_allom_dbh_maxheight, Table S1) is the same as the diameter for maximum crown area (d2: Fates_allom_d2ca_max, Table S1). As our data showed that d1 and d2 are different for shrubs, we have modified the codes so that the d1 and d2 can be set for different values. It is possible that different branching and path length patterns for stems of chaparral species could impact the hydraulics compared to trees; however, FATES-HYDRO treats all the aboveground xylem as a single pool and thus it should not affect our model simulation results.*"

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.

RESPONSE: We have added one paragraph to justify three PFTs based on the hierarchical cluster analysis of allometry and trait data as below (see Pages 12-13, Lines 262-277):

"*Based on a hierarchical cluster analysis (Bridges 1966) of allometry and trait data, there are a clear separation among the shrub species. 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 (Fig. 1). R's rect.hclust() function (https://www.rdocumentation.org/packages/stats/versions/3.6.2/topics/rect.hclust) was used to see the clusters on the dendrogram. 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. According to the principle of model parsimony, we do not want to classify the species into more than 3 PFTs. Meanwhile, we also want to differentiate the fundamental plant growth and water use strategies that will determine plant transpiration rate and the corresponding LFMC. If we choose to classify the species into two PFTs (based on the solid horizontal line in Fig. 1), then we will not be able to differential species with aggressive and conservative hydraulic strategy in the second group and not be able to test H4. Therefore, the chaparral shrub species were classified into three PFTs (based on the dotted horizontal line in Fig. 1 and Table S3), that are able to differential plant growth and hydraulic strategy that is critical for the simulation of LFMC.*"

Note that the axes for all subpanels should all be the same scale for Fig. 2 (and for all figures).

RESPONSE: We have updated all figures to make sure that the axes for all subpanels are the same scale.

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.

RESPONSE: Good suggestion! We have limited observed LFMC data for eleven shrub species so we only can capture interannual variability for several months. We did add a figure including seasonal cycles of observed LFMC for Santa Monica Mountains Chamise (*Adenostoma fasiculatum*) in supplementary (Fig. S5) and cited in the main text (Page 17, Lines 366-367).

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.

RESPONSE: That is good point. We have updated figures to display temporal changes in daily mean values and 95% confidence interval under climate scenario RCP 4.5 and 8.5 with 20 Earth System Models. Furthermore, these uncertainty have been used to test if there are significant difference among historical period and the future period. We also mentioned in the main text "*Our results also showed that the spread among models increase with time, suggesting a larger uncertainty in the projection into the future*", when present the results for Fig. 3 and Fig. 4.

The methodology for the bootstrapping calculations is unclear and needs to be descried 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.

RESPONSE: Good suggestion and we included a new statistical analysis in the methodology as follows (see Page 16, Lines 342-348):

"*We used a bootstrapped approach (Jackson 1993) to test if the mean of LFMC or fire season duration are significantly different between these two periods. Specifically, we randomly draw 10000 samples from the simulated residuals of LFMCs or fire season durations estimated by 20 ESMs for these two periods under the null hypothesis that there is no difference in the mean. We then calculated p-values by comparing the simulated mean difference to the empirical distribution of difference estimated from these 10000 samples. See Supplementary section 5.2 within Xu et al. (2019) for the details.*"

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 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.

RESPONSE: Thanks for your comments. The figures are not clear enough as I copied the figures in PDF format on Word file, which makes figures blurry. Actually the original figures in PDF format is very clear and finally we will provide these clear figures to the journal. In addition, we have significantly improved all figures. For figures 3 and 4, we did not standardize y axes as we would like to make the trend clearer.

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.

RESPONSE: Revisions done. We tested H4 hypothesis as three PFTs were defined based on very different hydraulic strategies thus it is worth testing that if impacts of climate change on LFMC would be varied for different PFT. Specifically, in the introduction, we have the following section in the introduction to motivate the H4: "*Meanwhile, the "tolerators" typically build xylem and leaves that are more resistant to cavitation so that they can tolerate more negative water potential and continue to conduct photosynthesis under water stress. Therefore, compared with the tolerators, the avoiders normally have a lower sapwood density and higher plant water storage capacity in their tissues to avoid cavitation (Meinzer et al. 2003, 2009; Pineda-Garcia et al. 2013). Because the avoiders rely on water storage capacity as one way to avoid cavitation thereby maintaining a relatively high LFMC, and water loss from storage should increase with warming, LFMC could be more sensitive to climate change in avoiders relative to tolerators.*"

Furthermore, hypotheses were written out subsequently in this improved manuscript. We have added the discussion that warming would substantially push Vapor Pressure Deficit (VPD) higher and decrease LFMC and strongly increase the fire season length. We have also explicitly explained given that fire season length is not validated, rather it is defined as number of days with LFMC below 79%. It is a good suggestion to test the sensitivity of forecasts to different reasonable LFMC thresholds while we were aimed to focus on climate change impacts on live fuel moisture so this would be a good direction in the future research.

We did make a sensitivity test of our results to different thresholds (e.g. 60%) and we get very similar results. As the 79% threshold is supported by previous analysis (Dennison & Moritz 2009), we would like to focus on the results from 79% in this paper to avoid the large number of figures.

Line specific/minor:

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

RESPONSE: Revision done. We have modified "The average maximum temperature is 31.5 °C and the average minimum temperature is 4.6 °C" as "The mean annual temperature is 18.1°C".

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

RESPONSE: Revisions done. We have added more details about the study site.

L234-236 How was LFMC calculated in the model?

RESPONSE: Revisions done. We have added one paragraph to explain how to calculate LFMC 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?

RESPONSE: We used daily timescales for the climate data. Yes, historical and future climate data were extracted just for the grid cell above the study site based on its coordinate.

L273 does the increase in number of PFTs appreciably improve model performance? Isnt interannual variability important, particularly for long term forecasts?

RESPONSE: We did not compare the model performance for one PFT and three PFTs. While we assumed it should be more reasonable to classify eleven shrub species as three PFTs based on their hydraulic traits. Additionally, interannual variability is very important while we have limited observations to make validation.

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

RESPONSE: Revision done. We have added one sentence to explain the mechanism as follows.

"*This is because warming is pushing Vapor Pressure Deficit (VPD) ever higher, resulting in increased fire season length*".

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 length to this assumed threshold

RESPONSE: Revision done. We have displayed temporal changes in fire season length and 95% confidence interval from 1960 to 2099 under climate scenario RCP 4.5 and 8.5 with 20 Earth System Models to this assumed threshold in Fig. 4.

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?

RESPONSE: Revision done. We have added discussion that "*However, the three different PFTs were coexisting at the same location in model simulations, coexistence and heterogeneity in LFMC might 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

RESPONSE: Revision done. We have added a new paragraph in the Discussion in Page 23 Lines 497-509 as follows:

"*Because the moisture content of live fuels (~50–200%) are much higher than that of dead fuels (~7–30%), leaf senescence induced by drought stress and subsequent mortality are potentially vital factors to cause large wildfires (Nolan et al. 2016, 2020). Thus drought-induced canopy die-back and mortality could largely increase surface fine fuel loads and vegetation flammability, which can increase the probability of wildfire (Ruthrof et al. 2016). Since growth and mortality are turned off in model runs by using a reduced-complexity configuration, it is possible that vegetation density might decrease and LFMC could be conserved under future scenarios. In addition, potential vegetation transitions (e.g., shrubs to grassland and species composition changes) might substantially affect flammability and thus fire intensity and* *frequency. In this study, we used the static mode of FATES-HYDRO to simulate LWC dynamics under climate change. If we need to assess how the leaf senescence and vegetation dynamics will impact the fire behavior, we can use the same model with dynamic mode to assess their impacts on fire behaviors under future drought and warming conditions.*"

Thanks for the interesting read!