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Biogeosciences Discussions

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

D. I. Kelley (Referee)

douglas.i.kelley@gmail.com

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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

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.

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.

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)
- 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)?
- If inputs are on a timestep >1 day, is "temperature" mean, max or min?
- Where did CO2 information come from? And how was it expressed (i.e, ambient or internal concentration? Daily, monthly or Annual? etc)
- 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).

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.

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.
- Which dimensions were clusters based on (i.e is it the VCmax, P50 listed in the next sentence? Any others?)
- How was the number of clusters (PFTs?) was decided, or if it was a pragmatic choice to match the model
- What software was used. And include a software package reference.

- Which specific FATES parameters were estimated?

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 veg-fire 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.

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.

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?

Line 71: Is "but" the right word?

Line 82: "The sensitivity of LFMC to climate change. . ." and CO2 concentration?

Line 86: replace "should" with "could"

Line 120: (Hantson et al., 2016; Rabin et al., 2017) are better references, as they detail most global-scale fire models. (Thonicke et al., 2010) can be used anywhere when you are talking specifically about rate-of-spread fire models (see general comments).

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

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.

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.

Line 163: Maybe add the mean number of dry days in the dry season if that information is available?

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

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.

Line 244: sentence starting "The MACA dataset": Is this sentence saying MACA re-gridded 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?

Line 248: I'm not sure you've told us what "v2-METDATA" is yet?

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.

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

Line 263: 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?

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)

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

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.

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

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.

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?

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?

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?

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