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

Anonymous Referee #1

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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/s10584-016-1808-9).

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. However, the authors did not provide any detail on

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Discussion paper

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.

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.

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.

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)

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.

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.

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

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