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).