

Reply to reviewer comments (original reviewer comment presented in bold)

This is my third review of this study. First, the authors have greatly improved the manuscript, which is remarkably more clear than the previous version. Furthermore, the reduction in length of the Discussion section allows the key results to stand out better, and also improves their contextualisation within the current literature.

On the other hand, I identify a statistical concern in the removal of the FWI component from burned area, which the authors refer to as the “deweathering” process. In my view, the procedure is difficult to justify from both a statistical and a climatic perspective, as detailed in my Major comment. Unfortunately, changing the “deweathering” procedure would affect all the subsequent results, and therefore require the model simulations to be repeated. Although I suspect the resulting differences to be small, I do not consider the current regression procedure to meet the expected scientific standards. Whether the manuscript can be accepted despite this issue is ultimately a decision for the editor. My role as a reviewer is to highlight this concern and to note that, in its present form, the procedure is not sufficiently well supported scientifically. I also note that this issue might have been identified earlier if my comments from the very first round of reviewing had been more fully addressed in the previous revision iterations.

Apart from this statistical concern, I believe this study represents a valuable contribution to fire model experiments, and that the manuscript is close to publication standards, pending only minor textual corrections. My review is separated in my Major comment, and Specific comments. Line numbers refer to the revised manuscript without tracked changes.

We thank the reviewer for this third review of the manuscript and appreciate the positive feedback and recognition of this valuable contribution to fire modelling.

We acknowledge that there was ambiguity in the methodological description of the “deweathering” procedure. We have revised this section to ensure that the description is consistent with our intention. Our objective is to remove the long-term trend component in BA associated with the long-term trend in fire-weather conditions, as represented by FWI, rather than to remove short-term weather variability or the full influence of fire-weather conditions on burned area.

We thank the reviewer for the detailed and constructive comments and have addressed all specific points below.

Major comment: the “deweathering” process

The statistical regression procedure applied to burned area (BA) appears problematic (Section 2.2).

The authors remove the FWI temporal trend ($\beta_{i,j}^{FWI}$) from burned area according to:

$$\widehat{BA}'_{i,j}(t) = \beta_{i,j}^{FWI} t + \alpha_{i,j}^{BA} \text{ (their Eq. (2))}$$

$$BA^*_{i,j}(t) = BA'_{i,j}(t) - \widehat{BA}'_{i,j}(t) + \widehat{BA}'_{i,j} \text{ (their Eq. (3))}$$

Note that the prime symbol denotes min-max scaled variables. I do not understand the rationale underlying Eq. (2). If the objective is to remove the variability arising from FWI, then the correct approach is:

$$\widehat{BA}'_{i,j}(t) = \beta_{i,j} \times FWI'(t) + \alpha_{i,j}$$

Alternatively, if the objective is to remove the variability arising from the trend component of FWI, then the correct approach is:

$$\widehat{\text{FWI}}_{i,j}^{\text{FWI}}(t) = \beta_{i,j}^{\text{FWI}} t + \alpha_{i,j}^{\text{FWI}}$$

$$\widehat{\text{BA}}_{i,j}^{\text{BA}}(t) = \beta_{i,j} \times \widehat{\text{FWI}}_{i,j}^{\text{FWI}}(t) + \alpha_{i,j}$$

I am not aware of previous fire-activity studies employing the “deweathering” approach proposed by the authors (Eqs. 2,3). Nor am I aware of standard statistical approaches recommending this type of detrending methodology. Furthermore, directly removing the linear trend component of FWI from BA does not appear physically or statistically justified, and the manuscript does not currently provide any motivation for this unusual procedure.

As a side note, adding $\alpha_{i,j}^{\text{BA}}$ in Eq. (2) is not necessary. This term cancels out through $\widehat{\text{BA}}_{i,j}^{\text{BA}}(t)$ in Eq. (3). Therefore, to improve clarity, I recommend removing α^{BA} from the formulation.

As mentioned in the introduction of my review, I realise that modifying the computation of $\widehat{\text{BA}}_{i,j}^{\text{BA}}(t)$, and therefore of $\text{BA}_{i,j}^*(t)$ would affect all subsequent results of the study. I expect these impacts to be quantitative rather than qualitative. Nevertheless, I consider that the current “deweathering” approach is not justified.

We thank the reviewer for identifying the ambiguity in the methodology description. We agree that our use of the term “deweathering” was misleading and likely created confusion regarding the objective.

Our intention was not to remove short-term weather variability or the full influence of fire-weather conditions on burned area (BA). Instead, the purpose of the method is to remove the long-term trend component in BA associated with the long-term trend in fire-weather conditions, as represented by FWI.

We agree with the reviewer that, if the objective were true deweathering (i.e., removing weather-driven fluctuations or variability), then a direct regression of BA against FWI would be an appropriate approach. However, this is not the aim of our study.

Instead, we construct a first-order representation of the BA trend component associated with changing fire-weather conditions by using the temporal trend in FWI as a reference. This trend component is then subtracted from the BA time series, while preserving interannual variability.

To clarify this procedure, we have revised the manuscript (lines 181 to 210) to explicitly state that our approach relies on the following assumptions:

1. The long-term evolution of fire-weather conditions can be approximated by a linear temporal trend over the study period
2. The long-term component of BA associated with changing fire-weather conditions can be represented as a linear trend consistent with the temporal trend in FWI;
3. The procedure removes only the long-term trend component associated with FWI and does not affect interannual or short-term weather variability.

Specific comments

L10. JULES-INFERNO+HDI has not been defined yet, so I recommend using wording such as 'JULES-INFERNO with the HDI implementation'.

Thank you for the suggestion. This has been addressed in the revised manuscript (line 10).

L53. Typo in the first word of the sentence.

Thank you for highlighting this. This has been addressed in the revised manuscript (line 53).

L61. ESM has not been defined.

Thank you for this observation. This has been addressed in the first occurrence of Earth System Models revised manuscript (line 38).

L146. Typo: "(2018)s".

Thank you for noticing this. This has been addressed in the revised manuscript (line 146).

L196. Typo: "an revised".

Thank you for highlighting this. This has been addressed in the revised manuscript (line 146).

L218. Two typos: "log(BA)".

Thank you for noticing this. This has been addressed in the revised manuscript (line 227).

L229-230. The meaning of this sentence is unclear.

Thank you for the helpful remark. This sentence has been revised manuscript to ensure clarity (line 238).

L236. The slope is for the log-trasnformed BA. So, the units of % mentioned are incorrect.

Thank you for noticing this. This has been addressed in the revised manuscript (line 244).

Figure 6 caption. Typo: two dots "INFERNO+HDI..".

Thank you for the observation this. This has been addressed in the revised manuscript

L625. The meaning of this sentence is unclear regarding TENA.

Thank you for the helpful remark. This has been addressed in the revised manuscript (line 633).

L643. This subsection heading is unclear. Should this be 'Model sensitivity to external drivers'?

Thank you for the suggestion. This has been addressed in the revised manuscript (line 651).

L670-674. I believe that this paragraph should belong to Section 4.2 instead of 4.1.

Thank you for the suggestion. This has been addressed in the revised manuscript (line 684).

L703. Typo: "improvesmodel".

Thank you for highlighting this. This has been addressed in the revised manuscript (line 711).