Review of "Assessing the Impact of the Human Development Index on Historical Trends in the INFERNO Fire Model" by Teixeira et al.
Reviewer: Vincent Verjans

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.

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 ($\beta_{i,j}^{\mathrm{FWI}}$) from burned area according to:
$\widehat{\mathrm{BA'}}_{i,j}(t) = \beta_{i,j}^{\mathrm{FWI}} t + \alpha_{i,j}^{\mathrm{BA}}$ (their Eq. (2))
$\mathrm{BA}^{*}_{i,j}(t) = \mathrm{BA'}_{i,j}(t) - \widehat{\mathrm{BA'}}_{i,j}(t) + \overline{\widehat{\mathrm{BA'}}}_{i,j}$ (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:
$\widehat{\mathrm{BA'}}_{i,j}(t) = \beta_{i,j} \times \mathrm{FWI}'(t) + \alpha_{i,j}$
Alternatively, if the objective is to remove the variability arising from the trend component of FWI, then the correct approach is:
$\widehat{\mathrm{FWI'}}_{i,j}(t) = \beta_{i,j}^{\mathrm{FWI}} t + \alpha_{i,j}^{\mathrm{FWI}}$
$\widehat{\mathrm{BA'}}_{i,j}(t) = \beta_{i,j} \times \widehat{\mathrm{FWI'}}_{i,j}(t) + \alpha_{i,j}$

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 $\alpha_{i,j}^{\mathrm{BA}}$ in Eq. (2) is not necessary. This term cancels out through $\overline{\widehat{\mathrm{BA}'}}_{i,j}$ in Eq. (3). Therefore, to improve clarity, I recommend removing $\alpha_{i,j}^{\mathrm{BA}}$ from the formulation.

As mentioned in the introduction of my review, I realise that modifying the computation of $\widehat{\mathrm{BA}'}_{i,j}(t)$, and therefore of $\mathrm{BA}_{i,j}^{*}(t)$ 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.

Specific comments

L10. JULES-INFERNO+HDI has not been defined yet, so I recommend using wording such as 'JULES-INFERNO with the HDI implementation'.
L53. Typo in the first word of the sentence.
L61. ESM has not been defined.
L146. Typo: "(2018)s".
L196. Typo: "an revised".
L218. Two typos: "log(BA$^{)}$".
L229-230. The meaning of this sentence is unclear.
L236. The slope is for the log-trasnformed BA. So, the units of % mentioned are incorrect.
Figure 6 caption. Typo: two dots "INFERNO+HDI..".
L625. The meaning of this sentence is unclear regarding TENA.
L643. This subsection heading is unclear. Should this be 'Model sensitivity to external drivers'?
L670-674. I believe that this paragraph should belong to Section 4.2 instead of 4.1.
L703. Typo: "improvesmodel".