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 second review of this study, following the first round of revisions. First, the authors have greatly improved the narrative of the manuscript. The analysis is now more aligned with the results, and the claims are more accurately reflecting the actual model performance. Additionally, references to existing literature are better contextualised.

On the other hand, I believe that some of my comments related to methodological aspects have not been addressed. I previously raised these comments as “Minor”, since I thought that they only required some clarification. While the authors have expanded the descriptions, some of the methods are still unclear, confusing, and mathematically inconsistent. This raises concerns about potential fundamental issues with the results. For this reason, I now raise the methodological issues of Section 2.2 as a Major comment. In addition, this review includes a Minor comment about the length and repetitive structure of the manuscript, and some Specific comments. I encourage the authors to address my comments, as well as potential comments from other reviewers, to make their manuscript up to the standards of *Earth System Dynamics*. Line numbers refer to the revised manuscript without tracked changes.

Major comment: Section 2.2

As mentioned above, these are mostly comments that I already made in my first review, and that have not been properly addressed.

(a) The burned area is “normalised” (L211). Please specify with respect to what it is normalised. If this refers to removing the mean and dividing by the standard deviation, then this process is called standardisation, not normalisation.

(b) Equation (2). This equation suggests a detrending with respect to time, as t is the predictor. It does not regress out FWI, since this variable is not used as predictor of $\widehat{\mathrm{BA}}$. Instead, the authors seem to make the regression parameter (β^{FWI}) a function of FWI. First, this approach is a needlessly complex manner to regress out FWI from BA. Second, the explanation of how β^{FWI} is computed is unclear (L212): “A linear regression of the FWI time series is fitted to estimate the local climate-driven trend”. Is fitted to what? What are the predictor and predictand of this linear fit? Third, Why do the authors not simply regress out FWI using the following linear fit?
$\widehat{\mathrm{BA}}_{i,j}(t) = \beta_{i,j}\mathrm{FWI}(t) + \alpha_{i,j}$

(c) Remaining correlation between FWI and deweathered BA. Figure 2a shows that the correlation between FWI and deweathered BA is different from zero. This proves the point above: FWI has not been properly regressed out of BA. When a predictor is linearly regressed out of a variable, then the Pearson correlation between the predictor and the residuals is zero by definition.

(d) Significance testing in Figure 2. In the previous manuscript version, the authors had not implemented any correction for multiple hypothesis testing in their significance assessment in Figure 2. I raised this issue in my first review, and the caption now specifies that “Stippling indicates grid points where the Pearson correlation is statistically significant at the 5 % level after controlling for the false discovery rate using the Benjamini and Yekutieli (2001) procedure”. However, I compared the updated Figure 2 with the one of the previous manuscript version that had no false discovery rate correction. It appears that the stippling has not changed. This is impossible if the false discovery correction is applied. I hope this is an oversight from the authors.

(e) Bayesian linear regression of BA on HDI. The authors have expanded the description of their Bayesian linear regression method. Worryingly, the additional descriptions do not clarify the confusion. The equation concerned is:
$\log(BA^*) \sim BA_0 + \delta BA \times HDI$
There are many inconsistencies with respect to this equation.

- The authors write that they assign the prior (L245) "$\delta BA \sim \text{LogNormal}(0, 10)$". This means that δBA is constrained to be positive (elementary property of the LogNormal distribution). In turn, this implies that $\log(BA^*)$ increases with *HDI*, and therefore that BA^* increases with *HDI*. However, Figure 3 shows that BA^* decreases with *HDI*.
- The regression equation shown in Figure 3 is: $y = -6.57x + 19.42$. Since the authors write in the caption that "the results are presented here in natural space for interpretability", I interpret this equation as: $BA^* = 19.42 - 6.57 \times HDI$. If we plug in the value $HDI = 1$, this gives $BA^* = 12.85\%$. However, the fit in Figure 3 shows that $BA^* \approx 0.1\%$ at $HDI = 1$. This issue is not because of the interpretation of natural- versus log-space, because if $\log(BA^*) = 12.85$, then $BA^* \approx 4 \times 10^5\%$, which is clearly unrealistic.

For all these reasons, I am concerned that there are fundamental problems with the deweathering of the BA values, and with the Bayesian regression on HDI.

Minor comment: text length

The revised manuscript is excessively long. In particular, the Section 4. Discussion & Conclusions is 6 pages long. I strongly recommend to shorten Section 4 to maximum 3 pages, and to shorten other sections of the manuscript by removing details that are not necessary to the key messages of this study.

Section 4 can easily be shortened as there are a lot of repetitions. I list here some of them.

- L656-694: this is a repetition of the Results, and should be strongly shortened.
- L685-L688: this is the same information as in L750-757.
- L704-712: this repeats almost exactly L116-124. The Discussion should not be a 2nd version of the Introduction.
- L728-730: this is the same information as in L706.
- L741-745: this is the same information as in L676-680.
- 774-778: this is the same information as in L750-757.
- L800-806: this is the same information as in L702-712.
- L809-811 and L816-817: these two sentence are quasi identical, only a few lines apart.

In addition to these repetitions, a lot of unnecessary details can be removed from the Discussion, which often reads more as a literature review than a focused Discussion of the present study.

Specific comments

First, please note that I do not provide Specific comments on Section 4, as I expect the authors to thoroughly modify this section (see Minor comment).

Abstract. This abstract is too long. Please reduce it to a number of words typical of abstracts in *Earth System Dynamics*.
L30-33. These sentences should not be individual paragraphs.
L47-48. This sentence has a wording issue due to “with...and...and”.
L54. Remove “(mathematical representation of a real-world system)”.
L55. Replace “data-driven” by observational.
L87. Rephrase to: “correlated with the magnitude of inter-annual variability in burned area”, otherwise the statement is misleading. As it is now, the sentence suggests that inter-annual BA variability is temporally correlated with inter-annual HDI variability.
L109. “this is increasingly urgent” is a structural error in the sentence.
L155. “socio-economics” should be socio-economical.
L177. Typo: “raging”.
L166. I believe that this should be normalised GNI per capita.
L171. I believe that the description of the derivation of the GDP data is irrelevant to this study, and can be removed.
L182. Typo: “bunrt”.
L195. Add a comma after “dataset”.
L195. Use either “e.g.,” or “such as”, not both.
L196. Replace “driven by” by from.
L199. Replace “forces” by forcing.
L201. Replace “These moisture codes” by These fuel moisture levels.
L232. “Southern Africa” should be Central Africa.
Figures 3 and 6. It is unclear what the blue and red curves represent. Are the red curves posterior samples? And what are the blue curves?
L268. “weather”: this is not true, as fire spread is not decoupled from weather because F_{PFT} depends on weather.
Figure 4. “*ppl*” is not defined.
Figure 7. In (a), the histogram of GFED4s agrees with the slope shown in Figure 3 (centered on -6.57), but the one of JULES-INFERNO+HDI does not agree with the slope shown in Figure 6b (not centered on -5.40). Why?
Figures 9 and 13. Please use an irregularly space color bar to improve visibility (as in Figure 8).
L494. Typo: “burnrt”.
L495. “exacerbates” should be exacerbating.
L456. The numbers provided for JULES-INFERNO and JULES-INFERNO+HDI should be swapped.
L457. “additional suppression”. I believe it could also be due to reduced ignitions (Eq. 6).
Figure 12. Please also show statistics for NHAF, SHAF, EQAS, and SEAS. There is no reason why these 4 regions should be discarded from this Figure.
Figure 12. Correlation should not have units.
L499. Wording error: “with initial where”.
L504. More precis wording would be: any relative difference in $\mathrm{STD}/\mathrm{STD}_{\mathrm{GFED4s}}$ between both are small.
L553-558. This is vague. Please remove, and refer to the Discussion for limitations.
Figure 14. Why are there no confidence intervals on experiments other than control? If this is a

choice from the authors, it should be specified in the caption.
L583. “their reference”: should this be the control?
L588. “can have opposite effects”: this is unclear. Opposite to what?
L595-597. Figure 14 shows that Ndep is a significant driver of burned area trends in GLOBAL for JULES-INFERNO+HDI.
L607. “AUS” should be AUST.
L610. Figure 14 does not show that precipitation is the dominant driver in MIDE for JULES-INFERNO.