Interactive comment on "The biomass burning contribution to climate-carbon cycle feedback" *by* Sandy P. Harrison et al.

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General comments

In this paper, Harrison et al. use temperature records in combination with (proxies for) biomass burning emissions to derive the contribution of vegetation fire to the land-atmosphere climate feedback over the recent past (since 2000) and on the centennial to millennial scale (1–1700 CE). Through the use of robust regression techniques and a simple box model, they show that biomass burning is responsible for a sizeable portion of the total land-atmosphere feedback, with increased burning both causing and being caused by increased temperatures. The authors also demonstrate—seemingly for the first time—that worldwide charcoal records can be

safely used as a proxy for global fire carbon emissions. These findings represent important contributions to the literature on the subject, and thus I recommend this manuscript for publication, given minor revisions. (Although analyses will need to be rerun if the authors choose to exclude a data point as I describe in my "specific comments"—something I don't consider strictly necessary.)

Note that my background is in global fire modelling with a focus on the 20th and 21st centuries. I am interested in millennial-scale trends and how vegetation fire might affect the climate, but I have never become proficient with the datasets and terminology used in such analyses. Thus, although I am convinced that their methods are sound, I may not be capable of fully evaluating the appropriateness of the methods relating to how the paleodata (methane, charcoal, and temperature) were chosen or pre-processed.

Specific comments

Approaching this paper as someone not used to the sorts of questions posed or methods used here, I found it hard to keep track of what analyses were being performed and why. Drawing a diagram—the accompanying figure—helped my comprehension (although I couldn't fill it in completely; see pink). The authors should consider including such a diagram so that the logical flow of analyses—and the fact that the satellite-era and millennial-scale analyses used parallel frameworks—is clear, especially to those not steeped in this sort of analysis. By including the resulting values, such a diagram would also be useful as a summary of the different components of the biomass burning contribution.

Methane might act as a bit of a red herring to the reader, because the introduction does not explain why the authors even need to consider it. Charcoal data are

described as more abundant with higher temporal resolution, with no downsides mentioned—so why not just go ahead and use the charcoal data? Presumably the authors bring in the methane data, which have hemispheric- to global-scale coverage, to test the charcoal data because the latter (a) have only watershed-to-regional coverage and (b) are not evenly available across all biomes or continents. I only realized the sort of auxiliary nature of the methane–charcoal regression once I made the above flowchart and methane was missing. Whether my interpretation is correct or not, the authors should spell this out in the introduction. Also: On my initial reading, I missed the novelty of the methane–charcoal regression (i.e., showing that trends in the charcoal record are reflective of trends in biomass burning emissions). The authors may want to consider highlighting this more.

Other comments:

- Sect. 2.1: The authors say that they excluded the years 1997–1999 from their analyses even though they are available in GFED4s, because those years' data are derived from older satellite sensors. However, the Readme for the GFED4s data says, "In general, ATSR and VIRS data was used before 2001, MODIS after 2001." This is presumably because the only MODIS data available from 2000 are for November and December. In addition to 1997–1999, then, the year 2000 should be excluded and the analyses rerun. (This would also alleviate the concern that the 2000 data point appears to be a strong outlier, although the authors do thoroughly demonstrate the robustness of their results in the supplementary material.) Alternatively, the authors should note and justify the inclusion of 2000 despite its use of (at least) ten months of non-MODIS data.

- Land vs. global temperature

  – Section 9 of the Supplement should be slightly expanded (or text should be added elsewhere) to explain how the resulting regression was used to

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convert "feedback strength vis-a-vis mean land temperature" to "feedback strength vis-a-vis mean global temperature."

  – The land vs. global temperature regression for the satellite era should have an equivalent to Section 9 of the Supplement. Or at the very least, there should be some indication of what dataset is being used. (Presumably the same NOAA data, but this should be specified.)

- Sect. 3.3, L19-23: The authors should offer some explanation as to why their estimate of δ_m turned out to be so small in magnitude, and whether/how that may affect their analyses.

- P2 L24-26: The part of this sentence after the comma rests on the relationship between fire and temperature being positive, but this has not yet been established in the text. I recommend mentioning that in the first part of this sentence.

- P3 L20-23: Is the implication that peatland only burns as a result of human intervention supported by the literature? Certainly one might think that is the case with most present-day peatland burning, but not necessarily all. Also, the use of "natural sources" is problematic, since these can still be heavily human-modified landscapes (and indeed, can have human-ignited fires).

- The Supplementary Information is highly detailed, which is excellent for the purposes of comprehension, learning, and reproducibility. However, that also means it's very long. Thus, when referring to it in the main text, please also mention which section of the Supplement is being referred to. A table of contents at the beginning of the Supplementary Information would also be helpful, especially if paired with PDF bookmarks. Finally, page breaks between sections of the Supplement would enhance its readability.

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Technical corrections

- P1 L31: "... N_2O, **and** ozone precursors), ..."

- P2 L5-8: The part of the sentence before the en dash doesn't sound complete and should be reworked.

- P2 L11: This semicolon should be a comma.

- P2 L20: There should be a comma after "period".

- P2 L29: There should be no comma after "length".

- P3 L27-29: More information about the linked temperature product should be provided here.

- P4 L2: Closing parenthesis of the Marlon et al. 2016 citation is blue and underlined.

- P4 L6-7: Is it c_t or c_t^*? Presumably the latter, with the asterisk signifying the Box-Cox transformation.

- P5 L14: Provide Supplementary Information section number.

- P7 L22: Provide Supplementary Information section number.

- P7 L27: "A regression of land versus global mean temperature for 2000-2014" is not what Fig. 2a shows.

- P7 L29: Define "climate sensitivity".

- P8 L5: "deforestion" should be "deforestation".

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- P9 L7: "A regression of land versus global mean temperature based on the 500-700 CE data" is not what Fig. 2a shows.

- Fig. 4: Y-axis labels should be added to 4a and 4b. Alternatively, to save space, the panels could be arranged horizontally, with only the leftmost panel having a Y-axis label. The Y-axis label should specify observed charcoal normans.

Please also note the supplement to this comment:
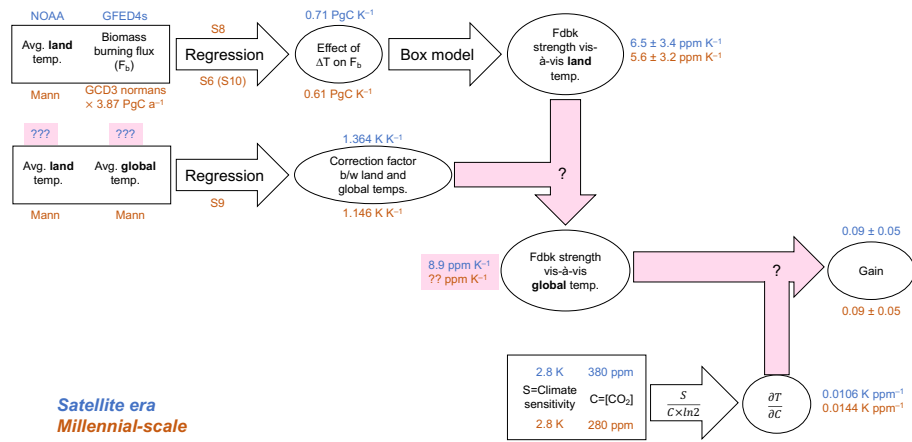https://www.earth-syst-dynam-discuss.net/esd-2018-11/esd-2018-11-RC3-supplement.zip

Fig. 1. Diagram of analytical flow in the paper.