Key findings & recommendations

1) This report represents a superb scientific achievement by the authors. It is an extremely impressive compendium of the rapidly changing nature and impacts of fire on Earth. Congratulations.

2) I do not think the report currently derives maximum impact from this work as a tool of communication beyond the research community. I am not sure the report fully delivers on its stated aim "to deliver actionable information to *policy and practice stakeholders and wider society*" (my emphasis). I recommend that:

    a. The authors begin the report with a 2-page (max) executive summary that communicates absolute key findings as concisely as possible, with internal hyperlinks to relevant report sections. This content could be drawn from the excellent section 7.
    b. The authors substantially revise the background section to make it clearer, cleaner and simpler to non-specialist audiences.
    c. The authors produce 1 or more high impact visual summarise of the report: e.g., an infographic executive summary or infographics summarising their 4 case studies.
    d. The authors consider reducing the complexity of figures in the main report text, with secondary findings moved to appendices. Some figures are too small to be easily read, in any case.

3) There are significant limitations in the handling of direct human impacts on fire regimes. These should be addressed before publication. I recommend that:

    a. The authors conduct a literature review of human fire use & management for each of their focal regions and use findings from this to inform discussions of how human factors may drive model-observation divergence or act as confounding factors in attribution. An existing meta-analysis such as DAFI (see end of this document) could provide a quick index that allows rapid identification of relevant literature.
    b. And/or the authors ask for input from their expert panels on this, particularly in section 4, where human factors are postulated as potential confounding issues explaining model-observation error.
    c. The authors redo model runs for section 5, ignoring population density changes [hold them constant] and instead focus on land cover change as a directly attributable global change process within the limits of current models and data.

Examples of the consequences of this in the substance of the report include:

- No mention of the civil conflict in the DRC as a potential contributor to the fire anomaly.
- No mention of the legacy of over-suppression in the California fires. Where suppression is discussed, it is discussed as having an immediate negative forcing on burned area, neglecting this longer-term fuel feedback. Lack of account for this kind of longer-term interaction is a substantial limitation to the discussion of results and attribution.
- Tendency towards weakly substantiated comment on policy interventions around direct human impacts on fire, and / or, general discussion of human factors as confounders without reference to specific knowledge and data on the focal regions [timing of different forms of agricultural fires in the Amazon basin, shifting cultivation in the Congo, e.g,].

4) There is a potential conflict of interest regarding discussion of carbon markets that should be disclosed. This is primarily a matter of due process & transparency rather than substance, with one exception that is noted below.

Major comments

1) **Science**

- The science of this report is generally excellent. Highlights include: assessments of which aspects of fire regimes (spread rate, numbers of fires, fire size) drove BA anomalies, the use of expert panels to identify focal events, the novel methods used to identify damages and exposure, the use of PoF to hindcast focal events. Etc.

- One important point is I think it is vital that you give due weight to regions that do not experience anomalous fire years. E.g., Eurasia, section 2.2.1.5. Just as in the sections explaining the fire regime context of anomalous burned area, can you show that there were factors constraining the impact of climate change? Were there fewer fires? Or, were there anomalous wet years? It's great that you report these "no results" for the year, but a little focus on their constraining factors would be good as well to ensure a rounded presentation of all the data.

- In general, I think it is very reasonable to focus on large and extreme fires (wildfires). However, there are times when a lack of explicit differentiation between such events and agricultural fires, e.g., is problematic. This is particularly true, in my view for your (otherwise excellent) population & asset exposure calculations. You note the region with most people exposed is Uttar Pradesh (India), however it is extremely well documented (and indeed reflected in the GLOCAB/GFED5 crop fires product) that such fires are predominantly generated through rice-stubble burning. These are not climate driven events, nor wildfires as such. This lack of distinction may partly explain the large difference between exposed and displaced persons. Similarly, in the DRC (where you note recurrent exposure), this is likely related to widespread shifting cultivation fires.

- The hindcasting section (4) is generally excellent. It is reasonable, in my view, to acknowledge that difficulties in understanding the direct human drivers of constrain the certainty of results (e.g. lines 1788-1797, 1911-1918). However, I think you need to do better than speculate on potential human causes of model-observation divergence, and rather point to explicit documented evidence about land use practices in the focal regions. Evidence around human fire use & management is increasingly systemised (see Kasoar et al., 2023 Table 1 for an overview; https://doi.org/10.1016/j.crsus.2024.100128). Such systematic data could be used to identify studies of possible direct human drivers in each focal region. At a simple level, the omission of mention of the armed conflict in the DRC as a potential driver of fire, e.g., is notable. This may explain the unattributed anomalies in northeastern DRC (line 2177). Similarly, there are data

available on the seasonality of shifting cultivation fire use in the region (see refs at end), which seems to be a partial explanation of the difficulty of forecasting there.

- The attribution section (5) is generally very good. However, human population density has an ambivalent relationship to human fire use and management (Perkins et al., 2024; doi https://doi.org/10.5194/gmd-17-3993-2024). These are much more driven by land user objectives and socioeconomic context; factors which are not accounted for in your attribution. By contrast, land cover change does have some more predictable impacts (croplands being suppressive of large fires, e.g.). Therefore, I think it would be much stronger simply to attribute burned area to land use changes, and ignore population density. Whether you include pop density or not, you cannot currently claim to be attributing changes in burned area to "socio-economic factors", because population density is a fundamentally unsuitable measure of these. But you can do so for land use change. You note this issue in lines 2619-2630, but as a structural limitation in the modelling it would be better to account for this systematically by narrowing the focus of your attribution, in my view. In the minor comments I have noted specific pieces of comment on results that I think are not sound for these reasons – in my view you should limit your comment to those aspects of the system you are robustly modelling.

2) Communications

- Overall, I am not sure the report rises to its stated aim "to deliver actionable information to *policy and practice stakeholders and wider society*" (my emphasis). Overall, the key messages & headline findings of the report are not clearly enough communicated for non-specialist audiences, in my view. This is not a problem unique to the SOW – IPCC reports have been critiqued by the policy community along similar lines, after all.

- The background section is too dense for non-academics, and I don't think it would provide such readers with a clear sense of how fire is changing globally and how this year fits in. Page 5 is stronger in this regard than page 4, which suggests to me the issue is in being crystal clear on the absolute headline message of the report and how you wish to communicate it.

- I wonder if you have mapped your key audiences & key messages for the report? The Abstract reads to me a bit like you are framing this in the context of the Carbon budget. But is this the most important message for your stakeholders (the feedback of fires on further climate change)? The key findings in section 3

may be more attention grabbing. I have had a go at providing an overall report message in the minor comments. Feel free to disregard.

- In any case, I would replace the abstract with a 2-page high level executive summary that tries to nail down your key findings and messages, accompanied by 1 or 2 high-impact graphics. This could be, in effect, a crunched down version of (the excellent) section 7.

- A contents page would be extremely useful as well!

- Similarly, your figures are scientifically logical and appropriate. However, some of them they could be more concise to make the information in the report more impactful. E.g., Figure 2/3 – how much information is added by the Global Administrative regions panels? The figure is too small to see clearly and without looking it up, an average reader is not going to know what these are. I might argue the same for ecoregions, which could also go in a supplementary figure. Once you get into the details of attribution I understand that more detailed figures are required.

3) Transparency of competing interests

- Finally, I would expect to see authors from BeZero Ltd note their role in the industry in competing interests, given explicit discussion of carbon projects (which is itself very useful). A phrase such as “High-integrity forest carbon projects can help to mitigate global climate change, and we find some evidence that these interventions are also reducing fire risk locally.” (lines 1540-1541) may well be true. However, it is also in line with the commercial interests of BeZero to make such a claim -> i.e. promoting the value of *high integrity* carbon projects and hence the importance of ratings. Readers of the report should be aware of this potential conflict so they can transparently evaluate the claims made. In addition, if the specific claim regarding reduced local fire risk is to be made, it needs substantiation & should be placed in the context of documented cases of carbon projects having led to displacement of fire-dependent livelihoods in the global South (e.g. Croker et al., 2023; https://doi.org/10.1029/2023EF003552).

Minor comments

Section 1

Abstract

Given the likely very large underestimate of BA in the MODIS product (dealt with from line 477), I don't think it makes sense to quote the headline BA numbers in km2. A % change relative to the historical trend would make more sense to me. Alternatively, use the GFED5 number for the headline totals, and global fire atlas metrics for other aspects of the regime?

The sentence about Southern California (lines 144-146) is a bit confusing – are you describing some theoretical modelled and observed burned area here?

I don't think it's accurate to describe SSP370 as middle of the road. "Medium-high" is a better description. SSP245 is explicitly "middle of the road" in the narratives. This change should be made throughout the report.

Introduction

I don't think this framing (lines 174-177) is quite right. After all, global burned area is still seemingly declining, so at least on one measure, the potential for wildfires isn't growing (the phrase "potential of wildfires" is itself a little ambiguous to me). I think a more accurate/sharp key message could be: "There is a global transition from low-intensity, low-emission and low-harm grassland fires, to high-intensity, high-emissions and high-harm forest fires… This is a long-term transition, driven by a combination of climate change and land use change, which continued forcefully in 2024-25." You do make this point, but not until the first paragraph of your results!

More broadly, to me, describing lots of increases in burned area in paragraph 1, but then contrasting this with the overall declining global trend (paragraph 2) is confusing to the non-specialist reader.

The first two paragraphs are also too long for a non-specialist audience. Important messages, such as that globally aggregated BA is increasingly less important as a measure of contemporary fire regimes get hidden in the middle of paragraphs and likely skipped over.

The opening of the second paragraph mixes discussion of the trend towards destructive fires in fuel-rich environments with the overall declining trend. This all needs reframing for clarity, to my mind.

Could we have some figures in the background section to capture these key trends? Simple clear charts to communicate the key overall dynamics/trends of contemporary fires (e.g., grasslands vs forests, tropics vs extra-tropics?).

On page 5, paragraphs 2-4 (lines 228 – 267) are much stronger than the opening, to my mind. Each with a simple clear message succinctly communicated. These could all be a symbol & key message in an infographic.

Finally, I think some really key, fundamental points are missing. I know that we (researchers) all know that fire is both a natural ecosystem process & man-made, but you should make this somewhere in the material about declining burned area in grasslands.

Objectives of this Report

These objectives are very useful. Bear in mind that "suppression" is a jargon term to non-academics, who may know this as "fire-fighting".

The key methodologies section belongs lower down the report, in my view. I would instead replace this with a clear indication of the logic of the *structure* of the report, and how this delivers against your objectives. You could then include a table summarising the objectives, the relevant report section (s) and the methods used.

The paragraph 367-372 is a bit repetitive of para 319-322. We should know what the report is trying to achieve by line 367.

Section 2

Methods

This all seems very good and appropriate.

The use of expert panels to complement EO data is impressive.

As noted above, it of course makes sense to use global fire atlas data as the primary source in a report focused on extreme fires, which enables the excellent discussion of fire spread rates and sizes etc. However, I am not clear why you do not report total burned area from GFED5?

To my mind, given that you used summary values for FRP (if I have understood correctly) it belongs in section 2.1.2.1, not 2.1.2.2.

Results

Overall

See major comment on Eurasia & providing context for negative results.

Why are selected ecoregions chosen for Figure 1, and then long tables for administrative regions? Figure 1 is much more impactful communication.

Similarly, Figure 4 contains lots of interesting information, but is too messy for me to understand in current format. Fewer panels, bigger panels, more aggregated regions, or some combination, please.

Line 1205, I think "intriguing" is the wrong word here – "important".

South America

The point about fast fire spread driving increased BA across Brazil vs increased fires in the Amazon is fascinating. To my mind that speaks to fire for explicit deforestation vs uncontrolled wildfires spreading purely on climatic factors.

America

The section on drivers of extreme fire anomalies is extremely interesting, as per South America.

Africa

The increase in numbers of fires also seems likely to me to reflect shifting cultivation fires (the dominant mode of human fire use in the region; see end refs) growing out of control due to anomalous climate conditions. They were likely undetectable by MODIS before, hence the apparent increase in fire number – which may actually be due to increased fire size & hence increased detection.

Section 3

This section is generally excellent. Figure 6 is very good as a high-level summary.

See major comments on wildfires vs agricultural fires distinction & carbon markets section.

Section 4

See major comment on human dimensions of fire in your hindcasts.

It is fine not to have detailed human factors in your models (because nobody has yet built this). However, given it seems to play an important role in PoF & ConFLAME model error [in part because the models do an excellent job with biophysical factors] I think you should consider being more explicit about this limitation in the methods section, similar to the excellent section on EO product uncertainty. At present there are several scattered references to missing human processes in the results (e.g., suppression in California [but only as an immediate not long-term forcing], lines 2157-2159, Panantal lines 2128-2131). It would be cleaner and clearer to address this upfront in the methods.

Furthermore, I think where you note possible human factors as explanations of model-observation divergence, you should do so in specific reference to published literature. E.g., in the Amazon (lines 1788-1797), there is well-documented evidence of escaped pasture fires as drivers of forest-edge degradation (see refs list). This matches your pattern of ignitions driving fires at the eastern edge of the Amazon (Figure 10). To dig deeper, the seasonality of pasture vs agricultural residue fires in the region could be identified from field data to see whether these match the error in model seasonality predictions (see refs).

Incidentally, there is an interesting & important discussion to be had about whether current fire activity in the Amazon case study is indicative of deliberate deforestation, or perhaps more worryingly, of the transition towards forest degradation and inherent instability under a changing climate (see Lapola et al., 2023; https://www.science.org/doi/10.1126/science.abp8622)

Section 5

See major comment on how what exactly you are able to attribute on the human dimensions. I have noted sections where this limitation leads to, in my view, unjustified comment in your results section, below.

As a general comment on the Pantanal case study, in meta-analyses of human fire use, there tends to have been very little in this region historically (too wet). As such, I am not at all surprised to see that it is the event most clearly attributable to climate change (lines 2649-2651).

However, because of the limitations of your socio-economic analysis, I think the comment on possible policy intervention focused on constraining direct human

influence may be true (lines 2678-2687), but cannot be justified on the basis of your analysis, which is limited to coarse patterns of land cover change.

Similarly, in the Congo, there is not much relationship between increasing population density and changes in fire use in shifting cultivation. Increased population density can shorten fallow periods (leading to increased fire use), but can also lead to sedenterisation, and hence less use of fire for field preparation. As such, your comment on socioeconomic factors broadly is not warranted (lines 2755-2760).

Again, I'm afraid I don't think the analysis in lines 2828-2840 is robust. You aren't representing socioeconomic drivers beyond land cover change. (Sorry to repeat myself).

Section 6

This section is excellent. Because you held land cover and population density constant – you are spared my comment on the limitations of this! I think this is actually better in any case as it allows focus on those dimensions of fire JULES currently captures in a robust way.

Section 7

This section is excellent & could form the basis of a revised executive summary.

However, after revising section 5, I think the sections here on socio-economic attribution need to be cut or substantially revised.

*Key relevant papers for each study region from DAFI/recent lit:

Northeast Amazonia

Cano-Crespo et al., suggests escaped pasture fires as the largest source of wildfires in the Amazon - https://doi.org/10.1002/2015JG002914

Jakimow et al., suggests that pasture burning in the Southern Amazon is focused around the main dry season (Aug-Oct) https://doi.org/10.1016/j.rse.2017.10.009

Brunel et al., suggests that pasture burning in the coastal savanna regions is clustered around December-Jan. https://doi.org/10.1016/j.rama.2021.08.003

Cammelli et al., important study to understand behavioural and economic drivers of smallholder fire use in the region - https://doi.org/10.1016/j.gloenvcha.2020.102096

Bilbao et al., 2019, details current management and policy challenges around fire, and makes strong cross-community suggestions for ways forward - https://www.mdpi.com/2571-6255/2/3/39

Congo Basin

Molinario et al., shifting cultivation and forest loss in the Congo – importantly this study highlights how subsistence farming is often synergistic with increased logging & mining (i.e. you have to feed the workers). https://www.mdpi.com/2073-445X/9/1/23; see also https://iopscience.iop.org/article/10.1088/1748-9326/10/9/094009/meta

Tyukavina et al., on shifting cultivation in the Congo basin - https://doi.org/10.1126/sciadv.aat2993

Examples of fire increasing under conflict: https://doi.org/10.1080/1747423X.2011.565372, https://www.scirp.org/journal/paperinformation?paperid=27362,

California

I recommend including citation of this modelling study on the long-term impact of over suppression on fuels. https://www.nature.com/articles/s41467-024-46702-0

Panantal

There is generally less literature in this region, owing to the wetness & lack of use of susceptibility to fire. Here are the two papers from DAFI, but perhaps indirect impacts including drainage are those to focus on (as already mentioned in the text).

Devisscher et al, - https://doi.org/10.1111%2Fgeoj.12261

McDaneil et al., https://doi.org/10.1080/08941920500248921