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Discussions

Interactive comment on "Attribution of the Australian bushfire risk to anthropogenic climate change" *by* Geert Jan van Oldenborgh et al.

Anonymous Referee #2

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Summary comments:

I was really looking forward to reviewing this paper, but it was a terrible disappointment. Overall, this comes across as a very poorly prepared manuscript, possibly related to an effort to do the work quickly and publicize it prior to peer review. The work lacks clear text to describe the analysis and clear and considered messaging of the main results; it lacks a robust assessment of attribution of fire risk is even feasible with current resources; it lacks a considered approach to climate processes relevant to fires in Australia; and it lacks clear, precise and visually appealing figures that help readers to understand the analysis and outcomes. As a reviewer it was an incredibly frustrating experience to assess this manuscript and a clear demonstration of why careful science and the review process should be prioritized over releasing rapid results for media

attention. The topic of this paper is very important, but the very poor job that has been done here undermines efforts of climate science communication and attribution science in general. My recommendation is that this manuscript should be rejected and that the authors should do a much more careful analysis and writing prior to submitting this work again.

Specific comments:

In all cases it is important to state the direction of excursions of the IOD and SAM, as opposite phases have different impacts. E.g. line 17: "…a strong Indian Ocean Dipole from the middle of the year…" should be "…a strong positive Indian Ocean Dipole from the middle of the year…"

Lines 16-18: It isn't correct to describe the IOD and SAM conditions as being in addition ("as well as") to the warmest and driest year on record. These drivers of variability were part of why conditions were so hot and dry, not an additional factor.

Line 20: Was fire activity unprecedented across all of these states individually (that is how it reads), or was the combined level of fire unprecedented? And in what way was it unprecedented? 1974/75 saw a far larger area burnt. 2009 saw a far greater number of lives lost.

Lines 23-41: The information in here is poorly supported by references. It is also not central to the background of this attribution study. Suggest shortening substantially and refocusing on the introduction required for setting up the background for this specific study.

Figure 1: This is a very misleading and inaccurate figure. The size of the dots on this image may misrepresent fire size. The blue shading doesn't only show forested areas of eastern Australia as the caption says. Why is none of Tasmania shade blue? The blue shading seems to be a very loose definition of "forest". Why start this analysis in July 2019 where it will capture many of the normal winter savannah fires which are

unrelated to the fire crisis being discussed here?

Paragraph starting line 45: This is an enormous paragraph that moves between many different concepts. Editing required.

Line 55-76: It is stated that FFDI is the index that is operationally used in Australia. Yet this index isn't used in this study. No explanation is given as to why FFDI isn't used in this study, and this would seem the most sensible choice given that it is the index used for fire danger in Australia.

Line 84: What does "in-situ sites" mean? Some of the examples given in the previous sentence seem pretty small scale (e.g. Brisbane heat during November 2014).

Line 92: How do you take a 7-day moving average of annual maximum temperatures?

Line 96: Why are these two time windows chosen for looking at precipitation, and are these choices defendable? The FFDI uses instead an accumulated precipitation deficit. Process-wise, soil moisture is important in Australia for determining the dryness of living fuels, while temperature and humidity are important for drying of dead fuels, so it isn't clear that the precipitation indices chosen are the best options for relating to fire risk. Also, how is the "fire season" defined?

Figure 2: The "Log(burned area)" is not clear. What are the units? It would also be better to show burned area in true units but give the y-axis a log scale. 2019/20 conditions should be added to figure 2-right, so that readers can see whether an extrapolation of this relationship to 2019/20 is appropriate (line 199). What are the green and grey lines in Figure 2-right?

Section 2.2: Why have an observational data section that then only says that the observation data used are described elsewhere in the manuscript? It would be appropriate to describe all of the data sources, and choices made, here.

Section 2.3: It needs to be described why these models were chosen and why others were not. These choices need to be objective, and the reader needs to know what

these objective choices were. My guess is that the choice is based on starting by selecting all models CMIP5 and CMIP6 models where large ensembles (n>?) exist, but that is just a guess and needs to be explicit in the manuscript. Is single forcing or omitted forcing within these ensembles also a requirement?

Line 156: Are these autocorrelations based on monthly data, annual data, seasonal data?

Line 160: Why 4-year smoothed? What guides this choice of length?

Line 202: Are the model spread and natural variability necessarily independent? Wouldn't this assumption be broken if model spread is at least partly due to differences in how models represent modes of variability. Wouldn't forced changes of the modes of variability (of which there is evidence) also violate this assumption?

Lines 220 to 226: It is notable that neither of the intervals of extreme heat discussed here coincided with the worst fire events that fell around New Years Day. . ..

Lines 217 and 228: Numbers for temperature anomalies in summer 2018/19 are given of 2.61oC and 1.52oC. I think that the second may be related to mean temperature and the first related to maximum temperature – but the text is not clear.

Line 232: What is the threshold behaviour of global warming referred to here?

Figure 3: Is this data averaged for the SE Australia region? Why is there data through to 2020 if this is showing July-June years? This needs to be specified in the caption. It would also be nice if a bit of extra effort went in to making this figure attractive.

Line 258: It is also notable that 1938/39 was a big bushfire year in southeast Australia. Line 271: Make it explicit here that you are talking now about southeast Australia. I was initially confused because earlier a value of 40oC was given for the hottest week, but this was nation-wide.

Lines 286 to 289: Does this then imply that previous studies that have attributed heat extremes in Australia, and have given quantitative results, are flawed/affected by this same model capability problem? If so, this is an important finding that should be made clearer.

Figure 4-7: How sensitive are these results to the choice of the 4-year smoothing for the global mean temperature dataset? I think that it is important to show results with a longer smoothing that could better account for differences in interannual to interdecadal variability, which will be random in models and cancel out over a large ensemble, compared with the single realisations of observations/reanalysis where the influence of variability will be a real part of the signal.

Lines 301 to 302: Should ACORN be included if there are issues from changing data coverage? Surely then it would be better just to use AWAP for the Australian observational dataset as it specifically resolves this issue.

Line 303: The 1929 event????

Figures 6 and 7 have data labels cut off.

Lines 335-342: This is an area where I have questions over the suitability of FWI for this study, which only considers precipitation over the last 52 days. The FFDI used operationally in Australia uses a longer-term drought index, which is particularly relevant given the multi-year timescale of droughts in Australia and the changes in soil moisture that influence fuel dryness. Multi-year drought was a factor in the 2019 extreme conditions and so should not be ignored in this way.

Line 348: The suitability of only using the lowest 20% or 30% of annual mean precipitation observations needs to be better justified. It is not clear that this is a suitable measure to be using for the attribution testing. Figure 8 still seems to show all annual data, but these should only show the 20% and 30% of data points that are actually being used in the analysis.

Line 350: What is the justification for scaling precipitation relative to smoothed global

mean surface temperature? I understand this for looking at temperature attribution, but it isn't well justified in the text that this should also apply for precipitation.

Figure 8: again text is cut off. Titles are poorly designed. My frustration at the lack of care in preparing the manuscript is rising. . .

Lines 357 to 359: This long sentence is not well constructed and hard to read/understand.

Figures 5, 6, 7, 9 , 10, 15, 16 and 17 all have unlabelled x-axes.

Section 4.5: The precipitation fails to take into account the seasonal patterns of rainfall change that are well described by Australia's Bureau of Meteorology (increasing warm season rainfall in northern regions, decreasing cool season rainfall in southern regions). Decreases in cool season rainfall may not directly influence the months when fire occurs, but is important in the context of multi-year droughts and soil moisture deficits that influence fuel dryness.

Section 4.6: Reiterating earlier concerns that this analysis doesn't actually do anything related to meteorological drought. In Australia droughts are multi-year events and so aren't and can't be described by a rainfall deficit over the scale of months to a year. The analysis also fails to address the way droughts in this region are being viewed, with southeast Australia being a region that is usually dry (often in drought) and occasionally experiences drought-breaking rain events.

Section 5: Part of the reason why the 2019/20 fires were so devastating was because of the number of extreme fire events where pyrocumulonimbus activity occurred. The metrics here do not account at all for the factors that influence pyroCb risk.

Section 5.2: The equation for DSR is given, but I don't see anywhere the equations given for FWI or for MSR.

Section 5.5, including Line 459 and Figure 16 and 17: Because all models severely underestimate the risk compared to ERA-5, surely this is an indication that accurate quantification of attributed changes in fire risk is not possible. It seems very misleading to then go on and give percent increases for 2019 and for 2oC warming based on the model output. Similarly, at line 464, it seems unwise to make a statement about the trends being non-significant when there is such disagreement between observations and models, with the models being a severe underestimate.

General comment: I didn't specifically keep track, but it seems that many acronyms are not defined.

Paragraph at line 476: The precipitation contribution can also be seen to be very much underestimated in these models compared with ERA-5. This should not be overlooked.

Paragraph at line 489: The factor of four and factor of nine numbers I think come from the lower end of ERA-5? But these values aren't the focus of the text associated with the interpretation of figure 16 and 17, which instead gave numbers based on models. This is very confusing for readers.

Section 6: This section feels like it was tacked on as an after thought. In particular, these modes of variability are part of the extremes in 2019 and so are already part of the attribution analysis carried out in earlier sections. The way section 6 is written seems like these modes are in addition to the 2019 extreme conditions, which isn't the case.

Section 6.2 and Figure 19: The analysis of the IOD fails to take into account known problems with instrumental data for the Indian Ocean. Generally it is assumed that indices of the IOD are only reliable after 1958, and even after that time there are differences in how well different data products capture the upwelling signal in the eastern upwelling region of the IOD. No information is given about what dataset is used for the analysis in figure 19, so readers can't evaluate potential problems related to the dataset used, though clearly data prior to 1958 is used and this should not be part of any analysis of IOD variability.

Figure 19 and 20: The IOD and SAM have been reported in other studies to influence Tmax, which is also important to fire risk. These should also be shown along with precipitation, and the text should investigate further why this study finds no relationship to Tmax when others have.

Figure 20: ENSO and SAM also interact with a negative correlation, so for consistency in analyses the ENSO-independent SAM relationship should be used.

Line 587: 38 million hectares in 2002/03 – this isn't correct.

Line 589: Need to specify that the 1974/75 fires were grass fires, which have different drivers to forest fires.

Section 7: This section has interesting information, though important points are frequently unreferenced. However, this section reads like a separate study that is unrelated to the focus of this paper on the attribution of Australian bushfire risk to anthropogenic climate change.

Conclusions: I've run out of steam for providing specific comments on the conclusions, but based on my critiques of the previous sections of the paper significant changes are also needed here.

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