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Natural Hazards and Earth System Sciences
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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.

Geert Jan van Oldenborgh et al.

oldenborgh@knmi.nl

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This Manuscript aims to evaluate if the exceptional fire risk associated with the bushfire during the last months of 2019 and January of 2020 was exacerbated by anthropogenic climate change, namely in the Southeastern Australia, where the fires were particularly severe. The authors analysed the exceptionally of the heatwaves and drought and how they reflected on the Canadian Fire weather Index (FWI). The analysed also, using the current climate models, the long-term trend of the above-mentioned parameter. The driver effect of the Indian ocean dipole and Southern Annular Mode was also assessed. The Overall context of the subject is very important in Australian Context Taking in account the importance of extreme climate events, such as drought and heatwaves, for the region within the context of warming tendency. It should be noted that Australia

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has a large history of tragic events despite being one the countries (maybe the first one) with better management strategies.Therefore, the work seems to be appropriate for this journal.

However the manuscript is very exhaustive, very hard to follow with an excessive number of figures and details. The analysis is very repetitive and should be organized in a more effective way in order to increase readability. Otherwise, the main achievements will be lost in somewhere.

We thank the reviewer for their review and acknowledge that the text in particular was not easy to read. This is a comment common to all the reviews received, so it justifies and motivates a thorough revision and restructuring of the text, which we will undertake. In particular, we are planning to follow the suggestion to move certain analyses to a supplementary material and just reference their results in the main text.

1 MAJOR

As I said the subject and results of this manuscript are of great interest for a wide range of readers. However, the reading of the paper is very tiring, the number of figures in the manuscript is very high and the results, synthesis, interpretation and conclusion for each topic is really tedious. Nevertheless, I recognize that to present these results is a very hard task. Therefore, my next comments are suggestions that may increase the readability and increase the number of interested readers that should be attracted to the important results of the manuscript.

1. *Some paragraphs of Introduction show a strong lack of references, namely the first ones that have only references to national reports. The same situation occurs in several paragraphs of the introduction and along the manuscript that seems to be more appropriate for a technical report than to a paper.*

We will shorten this part of the introduction and add more references for the parts that we retain.

2. *The entire paper should be reorganized. The structure should be less technical and descriptive and more similar to paper structure:Introduction, Data and Methods, Results, discussion and conclusions. Several section should be merged,and the figures reduced significantly in the maintext. The remaining figure should be moved for Supplementary Information.*

   We will restructure the paper following these recommendations, which were also expressed by the other reviewers.

3. *I understand other factors should be included in fire risk analysis. However The present manuscript is so long and the main contribution of the authors are related with Fire Weather Risk. Therefore,I suggest removing section 7 from the manuscript. A paragraph related with the other drivers may be included in Section 8. Conclusions. Consider changing the title accordingly.*

   Generally, many studies conducted by the World Weather Attribution group include a section on vulnerability and exposure, as some of the co-authors work in this field and help us put the physical results (often focused on ocean-atmosphere processes) into context of impacts on the ground. We therefore would like to keep some of the content. However, we recognize that Section 7 is too long, in particular given the length of the rest of the paper. Therefore, we will condense Section 7 and attempt to cross-reference it better in the rest of the paper.

4. *Clarify if Figure 1 shows the forested areas over the entire Australia or over Eastern Australia. Consider Move Figure 1 to Data and Methods.*

   We have redrawn Fig. 1 to show forested areas in all of Australia. Ww think it is important to show this figure in the main text to justify the choice of the study region.

5. *The option of using the CanadianFire Weather Index (FWI) instead of the FFDI is neither presented, neither justified.*

   The choice of FWI was born out of the data availability and the generally good correspondence between FWI and fires in the domain as documented in Dowdy et al. (2009). The general behavior of the two indices is also similar. However, we agree that this can be stated more explicitly, which we will do in the revised version.

6. *Consider comparing the performance of FWI with Forest Fire Danger Index (FFDI) developed and commonly used over Australia for indicating dangerous weather conditions for bushfires.*

   A comprehensive comparison of the indices for the domain of interest was conducted by Dowdy et al. (2009). They generally behave similarly, even if not identically across the full distribution of fire risk values. One general conclusion is that FFDI is slightly more temperature-sensitive than FWI. Thus, given the important role of temperature in driving the secular trend in fire weather in Australia, we again interpret our choices to be conservative and in line with our goal to provide a lower bound for the role of anthropogenic climate change in SE Australia's fire risk. Given existing extensive comparisons of the two indices in the literature, we would like to refrain from adding another index to the study. The literature also suggests that the uncertainty from the choice of index is negligible compared to the uncertainty from observational datasets and climate models used, which we are already illustrating and quantifying in detail.

7. *The author uses several different datasets for analysis the different variables and models. I would prefer to see a less wide lack of reanalysis and gridded datasets. For instance, why do not used precipitation AND temperature from CRU.*

   The CRU TS temperature dataset is monthly, whereas we needed a daily dataset to study the heat extremes, so we could not use it. More generally in our analyses we find it useful to compare different datasets to make sure that the signals we find are not artefacts of the analysis methods of these datasets. For instance in this study, we had started off using the Berkeley daily temperature dataset, and only by comparing it to other datasets such as AWAP did we find out that it did not represent the weather of this region well enough to be used. If we would have based the results only on the Berkeley dataset without using others they would have been much more clear-cut but not correct.

8. *The author use ERA5 from ECMWF to compute FWI. Why do not use the FWI computed using ERA5 by ECMWF and disseminated already by Copernicus?*

   For consistency across our study, we use our own FWI code and apply it to all datasets and model output consistently. In practice, our ERA5 FWI implementation is identical to the one provided directly by ECMWF (after they fixed a bug that gave a difference). Also, the version provided by ECMWF is not currently updated beyond 2018, so could not be used for our attribution study.

9. *Data from FFDI using ERA5 computed by ECMWF? Did The authors compare their results for FWI with the ones disseminated by Copernicus?*

   We assume the reviewer refers to the FWI, since we are not using the FFDI (see earlier reply above). We did compare our implementation of FWI with the ERA5 implementation and they are identical during the period of overlap.

10. *The option of using a window of 7 days for temperature and FWI is not fully presented and justified. Did the author make a sensitivity study to define the 7-days window? Why 7 days and not 5 days? Provide references and justification for the option made, including comparison with the widely accepted definitions of heatwaves adopted by WMO or based on percentiles.*

    The 7-day averaging interval in the Fire Weather Index was chosen to obtain a good correlation between the highest FWI value of the year and the area burned in the 2017 Sweden forest fires. A much shorter interval does not correlate well and a much longer one invalidates the use of the GEV for the statistical fit. Of course any definition has a degree of arbitrariness in it, but this one appears to work well (Krikken et al., 2019). Changing it slightly, e.g., to 5 or 9 days, did not make any difference compared to all the other uncertainties in the analysis.

Concerning heat wave definitions, in previous attribution studies we found that the ETCCDI definitions of heat waves are often not the most useful ones to describe the impacts. In particular, the percentile-based one (Tx90p) denotes warm episodes in other seasons than high summer also as heat waves, even though their impacts are very different and usually much less severe. The other definition, TXx, is more useful but does not take into account that for some impacts, e.g., mortality in countries where the vulnerable population is indoors or indeed forest fire risks, a single hot day has much less impact than a string of hot days. We chose a definition here that connects with the time scale on which we evaluate the FWI.

We will add some discussion of these considerations in the revised version to clarify our choices of indices.

11. *Figures in Figure 3 Correspond to averaged values over the southeastern Australia? Information must be presented in figure caption and main text. Consider moving Figure 3 to Supplementary information.*

    We will significantly revise all text and figure captions to be more self-explanatory. In response to other review comments, Fig. 3 has been expanded to include more datasets. At the same time, we will move most of the temperature and precipitation analysis to the Supplementary Material, as suggested by several reviewers.

12. *Lines 275 consider to present figure for JRA-55 in Supplementary information.*

We have made a new Figure 3 including all datasets discussed (attached) and indeed moved it to the Suppl. Mat.

13. *Consider reducing Figures 5-7 to one figure and moving the remaining for Supplementary information.*

    Thanks for this suggestion. Indeed, we will move the figures to the Suppl. Mat., although we may keep them separate for clarity. See reply to point 11.

14. *Line 335: Analysis of the driest month in fire season: How must considered month? Difference 2mm/day makes has a significant impact on this DMC, DC, FFMC and FWI? The impact of a delta of precipitation in a region with very low values of daily precipitation should be assessed in terms of fire weather risk. Consider providing a sensitivity analysis on this impact.*

    We are not completely sure whether we fully understand the reviewer's questions. First, this region of Australia does not have a strong seasonal cycle in precipitation; summer precipitation is very similar to winter precipitation in mm/dy. Second, we chose to use a measure based on monthly precipitation in order to maximize the number of observational datasets and model runs available, as trends in low precipitation are very model sensitive, see, e.g., Hauser et al. (2017).

    We tested two definitions: (i) for each year the driest month of the six months in the fire season September–February was fitted to a GEV function and (ii) the lowest 20% of all months in the fire season were fitted to a GPD function. Both gave very similar results.

15. *Line 339: please provide quantitative information. What are the observed values and the normal values.*

    Added the number from the BOM publication and a climatology for all Australia: '277 mm/yr (...) against a climatology of 418 mm/yr over 1900–2019'. Also added the numbers for the study area: 'with 1.28 mm/dy (467 mm/yr), against a 1900–2019 climatology of 2.25 mm/dy (820 mm/yr) in the same AWAP dataset.'

16. *Line 351: Annual mean low precipitation analysis: information about the precipitation regime over the region is desirable over a region, i.e., information about inter and intra annual variability of precipitation on the region.*

    We added a sentence about the lack of seasonal cycle and large variability to the previous paragraph: 'Note that mean monthly precipitation in the study area is almost constant through the seasonal cycle, but inter-monthly variability is very large compared to the mean, σ/μ = 49% on average, larger in summer and smaller in winter.'

17. *What are the observed values and the normal values.*

    Added 'with 0.49 mm/dy (15 mm/mo) against a normal value for December of 2.44 mm/dy (76 mm/mo).'

18. *Consider reducing Figures 9-11 to one figure and moving the remaining for Supplementary information.*

    In line with our plans to split the paper into main text and Supplementary Information, we will most likely follow this suggestion and move the analysis for temperature and precipitation to the Supplementary Information.

19. *Lines 411: Why do you use a window of 7 days for FWI and after the MSR instead of the DSR for a window of 7 days?*

    The DSR has been constructed to linearly scale with fire severity and therefore better suited for averaging over longer time scales (van Wagner, 1987). We've tested the sensitivity to either using a 7-day rolling averaged FWI or DSR and found only very little sensitivity. Hence, we choose to use the 7-day rolling averaged FWI because it is much more widely known and used. For averaging over longer time scales we did use the monthly averaged DSR (MSR) as this metric is, as previously stated, better suited for monthly and seasonal averages and also frequently used to asses fire risk on longer time scales (e.g., Malevsky-Malevich et al., 2008).

20. *Line 515 did the authors evaluate the formula of DSR for Australia region?*

    No, and this also does not make much sense as it would lump together very disparate regions with different vegetation (rain forest, Mediterranean forest, grasslands) and seasonal cycles. We are also not aware of any analysis in the published literature.

21. *Figure 13. In the figure caption describe the information for dots.*

    Dots are individual annual values. We have added this to the caption.

22. *Consider reducing Figures 15-17 to one figure and moving the remaining for Supplementary information.*

    We will consider merging these figures if it is possible to maintain readability of the figure. Generally, we are planning to keep most of Section 5 in the main paper (incl. figures), but move a lot of the material from Sections 3 and 4 to the Supplementary Material.

2 MINOR

1. *(Line 61): parenthesis is missing before 'Clarke'*

   Done.

2. *The link for each database should be provided.*

As indicated in the manuscript, all time series used in the analysis are available from one page on a public web site. These time series in turn contain links to the datasets from which they were generated.

3. *Figure 8: titles are not completed*

   Figures, captions, and titles will be revised throughout the paper.

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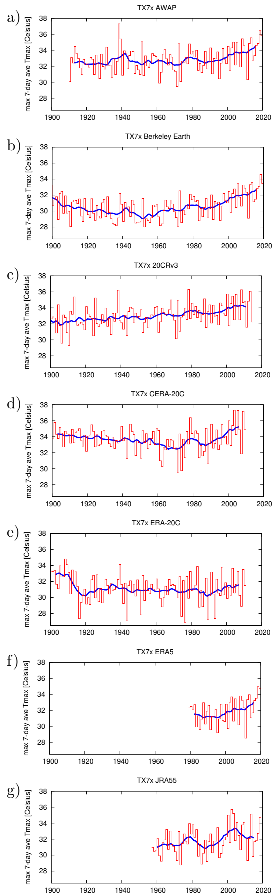

Fig. 1. The highest 7-day running mean of daily maximum temperature of the July–June year in a) the AWAP analysis, b) Berkeley Earth, c) 20CRv3 reanalysis, d) CERA-20C ensemble mean (DJF maximum), e) ERA-20C

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