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Natural Hazards and Earth System Sciences
Discussions

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

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1 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

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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.

We thank the reviewer for their honest feedback and for providing a comprehensive and thorough review despite the frustration. Indeed, the article was written relatively quickly in order to make the results available in a timely fashion during the aftermath of the fires. For example, the study's findings were used one week after the discussion paper was published in discussions of the various state commissions and Royal Commission at the Bureau of Meteorology on the link between climate change and bush fire risk, where the main messages were generally perceived as helpful and later accepted by for instance the NSW government. Our colleagues from the Red Cross / Red Crescent Climate Centre also highlighted repeatedly that a timely study is extremely helpful to inform decision-makers as windows of political opportunity often open in the brief timespan after the event but may have closed again by the time a peer reviewed study could be available. It is in the light of these windows of opportunity and with the goal to equip those decision-makers with scientific evidence that a rapid time frame becomes necessary. It of course is never thought of or aimed to replace peer-review. And in fact, this manuscript crucially builds on many peer-reviewed studies that carefully describe the methodologies that have been developed by the community.

However, it is indeed clear that we have failed to clearly highlight in the text where we use exactly the same methods explained elsewhere and clearly signpost where to find them. The current analysis is focused too exclusively on the results. Given, however that those are solid, novel and have already proven extremely useful in decision-making contexts, we are convinced that we can revise the manuscript to satisfy the concerns by, inter alia, highlighting assumptions and their justifications and more carefully describing the methodology and clearly highlight where in the peer-reviewed literature these have been applied before with broad acceptance from the scientific community. We very much appreciate the reviewer's detailed comments which helped to identify where the reasoning is not transparent enough. We found that these comments in the end did not affect any of our conclusions.

2 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..."*

  We have revised all such instances in the revised manuscript.

- *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.*

  Thank you for pointing this out. We have corrected this in the revised version.

- *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.*

  The unprecedented aspect was the extent of the fires in densely populated areas. The larger area burned in the 1974/75 fire season was mainly in sparsely populated regions. The lower number of casualties in 2019/20 in spite of more fire activity can hopefully be attributed to better warnings and protection measures. However, we agree that the statement is ambiguous and have specified in which aspect the fires were unprecedented: 'The bushfire activity across the states of Queensland (QLD), New South Wales (NSW), Victoria (VIC), South Australia (SA), Western Australia (WA) and in the Australian Capital Territory (ACT) was unprecedented in the area burned in densely populated regions.'

- *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.*

  We have followed the reviewer's suggestion and shortened this part. We also added more references.

- *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?*

  This figure is intended to be an introduction to the study domain, including some information on vegetation cover and fire occurrence during the event. It is not used in the later analysis. We added a note that the figure includes grass fires on the savannahs with completely different characteristics. Further, we now only show the fires from October 2019 to January 2020 to reduce the number of grass fires.

  Thank you for spotting the problems in the blue shading. We have included the forest in Tasmania and deleted the 'eastern Australia' in the caption.

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

  This paragraph has been revised and restructured together with the entire manuscript.

- *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.*

  The reasoning for not using FFDI in favour of the Fire Weather Index (FWI) is given a bit further down. We will point the reader towards that section in the revised manuscript. We argue that FWI is more physically-based and a better measure of forest fire risk than the FFDI citing (Dowdy et al., 2009), an employee at the Australian Bureau of Meteorology (BOM). The problem with FFDI is that the input variables to these indices are dependent. A more statistical approach is not able to disentangle the temperature dependence from the absolute humidity influence, so a more physically-based index is needed for attribution when the main changing factor is temperature. There is (unpublished) evidence again provided by BOM that the temperature dependence of the FFDI is too high for these reasons.

  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).

  We reformulated this to be clearer, 'However, at small spatial scales, human influence on extreme heat is sometimes less clear, as in Melbourne in January 2014 (Black et al., 2015).'

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

Thanks for catching this. We clarified that this refers to the annual maximum of a 7-day moving average of daily 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?*

  The Fire Weather Index uses a precipitation window of a few months (a relaxation time of 52 days to be exact, as explained in the detailed description in section 4.1 and now added to the text here). We used one month as our short time scale as it greatly enlarges the number of models that can be considered (not all provide the necessary daily output); previous studies have shown the importance of including as many models as possible for drought studies to study the systematic uncertainties due to different model formulations (see, e.g., Hauser et al., 2017).

  The annual time scale was included because the FWI is also sensitive to (part of) the rain season and at the time it was argued by many climate scientists that this was the relevant time scale, partially based on the a posteriori argument that these severe forest fires followed the driest calendar year on record. This time scale should correspond better to deeper soil moisture during the fire season, which is also approximated by the Drought Code in the FWI.

  The fire season is defined as September–February in our study region. We moved the definition to an earlier section of the manuscript and also clarified the choice of the different time scales.

- *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?*

We have updated the figure as recommended by the reviewer. The units are $^{10}\log(\text{km}^2)$.

Unfortunately, the MODIS burned area data for 2019/20 are not yet available so we cannot add that data point to the graph yet.

The grey lines denote the regression lines for the individual months, the green line for all months in the fire season. We have added this to the caption.

- *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.*

  As requested by other reviewers we have reorganised the manuscript so that the heat and drought analysis are in supplementary material. We therefore present the datasets used in the main text here, and those that are only used in the supplementary material there.

- *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?*

  Thank you for pointing out this omission, the criterion to select an initial set of models is now described more clearly. It is ultimately an opportunistic choice, guided by what models are at our disposal that have all the necessary variables at the required resolution. One recent definition of large ensembles is $n \geq 15$

(Deser et al., 2020), but there is no established definition. All model evaluation criteria are subjective to some degree, hence making them explicit is essential. We use a set of statistical tests, described in Philip et al.. This is now stated in the manuscript.

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

  We have clarified this in the text and refer to the definition above. . We consider the annual mean or driest month of the fire season, so these are annual values and the autocorrelations are year-on-year.

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

  The 4 years are chosen based on a previously developed compromise between the typical time scales of ENSO that we want to filter out and the time scales in the evolution of the global mean temperature that are externally forced and that we want to keep. See, e.g., (van Oldenborgh et al., 2009) for more background. We have added a clarification to that end: 'The smoothing is the shortest that reduces the ENSO component of GMST, which is not externally forced and therefore not relevant, but leaves as much of the forced variability as possible (Haustein et al., 2019). In particular, a longer smoothing time scale can cause issues extrapolating to the 'current climate' year, which is ususally a last one in the series.'

- *Line 202: Are the model spread and natural variability necessarily independent?* They do not have to be independent, but there is no evidence that they are not. At the regional scale, models that have a large sigma are not necessarily the ones that warm the most, and vice versa. For the seven models that pass our quality control it is indeed the case that they appear independent by showing a non-significant negative association (see Fig. 1 at the end of this reply). Equivalent conclusions are reached for the other variables in the paper.

More importantly, we effectively evaluate the models on their variability by requiring a value of the scale parameter σ in the extreme value function fit that is within the uncertainties of the scale parameter found when fitting the same GEV to the observations. This ensures that the different models we retain do not have an unrealistically wide range of sigmas. In the unlikely event of a dependence between variability and spread, this would ensure that the influence of such a dependence would be very small. We added 'largely' before 'independent' to reflect these considerations.

- *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?*

Modes of variability in general only represent a small fraction of the natural variability (see for ENSO van Oldenborgh and Burgers (2005), in which the explained variance typically does not exceed 20% ($r = 0.45$); similar figures are easily made for other modes like the IOD and SAM). We therefore consider all natural variability together rather than just the modes.

Concerning the second question, to second order, the forcing may affect the mean and variability of the natural variability. If it affects the mean, the effect is included in our analysis method. If it affects the variability the assumptions that the variability is constant (for temperature) or the variability over the mean is constant (for precipitation) is violated and our analysis is not valid. We test for this by computing the variability over time applying a running window of varying length. These tests based on observations are often too noisy to give much information, but the model experiments with much more data show whether the assumption holds or not. In the case of this analysis we found the variability did not change significantly. In other analyses we found that the variability indeed changes, albeit only noticeably in the future (Vautard et al., 2020). This test is part of the standard procedure employed in our analysis and described in detail in Philip et al. (2020), we also added it to this manuscript.

- *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. . .*

  We do not necessarily expect a week-by-week correspondence of heat and fire, as many additional factors can lead to an intensification of ongoing fires in a given week (e.g., wind, local fuel availability, etc.). Originally, this section attempted to contextualize the different heat records broken in 2019, but we recognize that much of section 3.1 is not needed as motivation for section 3, so in the revised version we have greatly shortened this section and merged it with the old section 3.2.

  It should be noted that although the analysis is done on the hottest week of the year, similar temperature increases are expected for the slightly less hot weeks and those weeks did coincide with the worst fires in the 2019/2020 season. This provides general support for the heat-fire relationship, which can be described with the entire warm tail of the distribution, not only the hottest events. It follows that the relationship between heat and fire risk is more general and not tied exclusively to the hottest week.

- *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.*

  This has been clarified in the revised version of this section.

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

  Thank you for pointing out that this was poorly written. We simply refer to the finding that heatwave changes often scale linearly with global temperature increase

(Perkins-Kirkpatrick and Gibson, 2017). No threshold behavior was implied. We have revised this accordingly.

- *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.*

  Yes, this is data averaged for the SE Australia region, which has been added to the caption. At the time of writing the peak heat season had already passed, so we were reasonably sure that the value up to that point would be the highest in the July 2019 to June 2020 year. This turned out to be correct.

  We will spend some time making the figures better-looking, as we explained in the beginning of this reply we estimated that publishing the discussion paper quickly had clear benefits in it being more useful for society, which came at the expense of having less attractive figures.

- *Line 258: It is also notable that 1938/39 was a big bushfire year in southeast Australia.*

  Thank you. We have added this information.

- *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.*

  We have added this clarification and have also moved much of the discussion on the national heat record to the Supplementary Material.

- *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.*

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We checked this for one model (EC-Earth), and found that both discrepancies, in trends and variability, are mostly confined to southeastern Australia, so would not necessarily affect the other studies done for Brisbane, Adelaide, etc. The higher trends in southeastern Australia are also clearly visible on maps of trends in one-day heat, e.g. at https://worldweatherattribution.wordpress.com/analyses/trends-in-weather-extremes-2018. This is one of the reasons why we have to take the effort of doing event attribution on a particular event, rather than just quoting general results for much larger regions.

- *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, com- pared with the single realisations of observations/reanalysis where the influence of variability will be a real part of the signal.*

The 4-yr smoothed GMST is just used as a measure of the externally forced climate signal. It is already highly correlated with the CO2 concentration and total anthropogenic forcing ($r > 0.96$). We repeated the most sensitive fits presented in the paper, that of TX7x, with a 10-yr running mean GMST and found only small differences in the fits compared to using a 4-year smoother (Fig. 2 at the end of this reply). Note that the model fits use the simulated ensemble mean global mean temperature, which is an even more robust estimate of the forced response.

Recent publications have shown that there is very little unforced variation in the global mean on these longer time scales (e.g., Haustein et al. (2019)). Also, very little of the variability over land is driven by teleconnections from lower-frequency modes of the climate system (e.g., (van Oldenborgh et al., 2012), Fig. 2). Consistent with several other papers in the event attribution literature, we concluded that a 4-year smoothing is adequate to reduce the influence of the two largest causes of interannual variability of the GMST: ENSO and the high latitude winter temperatures.

- *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.*

  Indeed the AWAP dataset resolves the issue of the varying station density (although the coastal effects may not be resolved adequately by the interpolation algorithm). However, the ACORN station dataset resolves another issue: the homogeneity of individual station time series. We prefer to show both to convince the reader that neither issue significantly affects the estimated warming trends. Leaving out the observational dataset with the lowest trend would also open the analysis up to claims of cherry-picking.

- *Line 303: The 1929 event????*

  This was a typo and should read "1939", but we will clarify in the revised version that we refer to "the exceptional heat wave of January 1939, which was not reproduced in certain datasets, but confirmed by station data."

- *Figures 6 and 7 have data labels cut off.*

  Thanks for pointing this out. We will revise the figures before resubmission, including clear labels etc.

- *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 op- erationally 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 mois- ture that influence fuel dryness. Multi-year drought was a factor in the 2019 extreme conditions and so should not be ignored in this way.*

The FWI considers in its DC code, which describes the dryness of the deepest inflammable soil layer in the FWI, past precipitation with an exponential time scale of 52 days (van Wagner, 1987). This implies that part of the winter rains are indeed included to some extent. In fact, the last three years were so dry that the winter, in which evaporation is less even in the absence of a seasonal cycle in precipitation, does not manage to replenish the soil moisture and it shows a steady decline, enhancing the FWI in the study season 2019/20 (see figure 3 at he end of this reply). We have rephrased the manuscript to make this clear.

In contrast, in our study we consider more general drought events, none of which was as extreme in the observed record. Of course those are generalities and do not rule out an influence that was only present in 2019/20.

The question how relevant 3-yr droughts were in general is separated into two parts: are multi-year droughts associated with more severe bushfires in south-eastern Australia? Do their properties diverge from consecutive independent one-year events?

Considering the first, we did a correlation analysis of the logarithm of Sep–Feb MODIS area burned against precipitation on various time scales (an extension of Fig. 2 in the article). This confirms the strong correlation with Sep–Feb precipitation: $r = -0.74$. The correlation with Jan–Dec annual mean precipitation is $r = -0.49$, so already only half as strong of a predictor, even though it includes much of the simultaneous connection and the FWI does include some winter precipitation. Finally, the correlation with the preceding 3-yr mean precipitation is $r = -0.01$. Although the uncertainties are high (95% CI on this is -0.5 to $+0.5$), this suggests that multi-year meteorological droughts are in general not necessarily a good predictor of area burned in the final year. While this doesn't rule out the possibility that in the special case of 2017-2019, the multi-year drought contributed to drying out fuels in advance of the 2019/20 fire season, it is not a relationship that is generally supported by the data.These conclusions are corroborated by Dowdy et al. (2009), who showed that the Fire Weather Index represents fire risk in Australia better than the FDDI.

Second, we did analyse one-year droughts (low precipitation in the calendar year ending in the fire season) and found that they showed no attributable trend in the study area in southeastern Australia, so it is unlikely that they influenced the long-term trend that we see in fire weather risk. Looking at the autocorrelation and spectrum of annual low rainfall in southeastern Australia (Fig. 4 at the end of this reply) we see no evidence of multi-year droughts being anything but random collections of annual droughts. The lag-1 autocorrelation is 0.15, well within the 95% CI around zero and, even if it were real, would explain only 2% of the variance. The lag-2 autocorrelation is 0.01. These numbers do not support investigating trends in multi-year droughts separately either.

Based on these results we decided that studying multi-year droughts separately would not give any new insights and have larger uncertainties. We have added a short paragraph to the text discussing the analysis here (suppl. mat.).

For completeness, preliminary results for 3-year drought illustrating the points made above are attached. The results are not different from the annual drought analysis in the paper but with larger uncertainties: neither observations nor models show a statistically significant trend. The fits are also not very stable with only 8 or 12 degrees of freedom to fit four parameters.

- *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.*

  This is simply based on the minimum number of points needed to reliably fit the 3-parameter GPD function, about 20, while still allowing to look at the low tail of the distribution, which is the one relative for the question of whether there is a trend in low precipitation years. We added this to the text.

In Fig. 8, the part that was fitted to the GPD can be deduced by where the thick lines stop and from the return time (1/20% = 5 yr). We thought it was clearer to show all points for reference how the tail connects to the rest of the distribution. We plan to make the points not used in the fit a paler shade of blue to show the difference.

- *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.*

  First, we investigate to what extent the trends in any variable are due to global warming. To do this we use the smoothed global mean surface temperature as covariate, which shows the part of the trend most related to the global warming trend. Next we use the climate models to check whether the trend is indeed attributable to global warming.

  Regarding the exact form in which the covariate is used in the fit, *scaling* the distribution with the covariate is not justified for temperature, we just *shift* the distribution for that variable, so assume the scale parameter that describes variability is constant (and check that in the observations and model data). For precipitation, the scaling (i.e, assuming μ and σ vary the same way with the covariate so that the dispersion parameter σ/μ is constant) is justified on two grounds: to avoid negative precipitation and on the theoretical arguments given in Hanel et al. (2009). It is an approximation that is widely used in the hydrological community.

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

As we mentioned before, we had what we perceived as a choice between a polished manuscript and the discussion paper being available rapidly and thus being useful to the adaptation community. We chose the latter in the hope that we will be granted the time to fix these issues in revision.

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

  We have split up and revised this sentence.

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

  Apologies. The caption mentions that these are Probability Ratios. We will add these labels in resubmission.

- *Section 4.5: The precipitation fails to take into account the seasonal patterns of rain- fall 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.*

  See reply to comment on lines 335-342.

- *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.*

  See reply to the comment on lines 335-342 above.

- *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.*

  No, we restrict ourselves to the large-scale factors in the weather that give favourable conditions to bush fire development. The small-scale feedbacks that allow the fires to grow larger cannot yet be investigated. We have added a remark on this caveat in the revised version.

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

  The FWI has a complex formulation that is given in the literature cited. The MSR is defined one sentence above the DSR as the monthly average of this quantity.

- *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 2C 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.*

  Indeed, which is why we do not provide a synthesised number for the increase in Fire Weather Index, but only separate numbers for ERA5 and the models and emphasise how different they are. In the MSR-SM discussion, we added the phrase 'in the models' to the first sentence, where it was erroneously left out. The reanalysis-model discrepancy and the fact that we only use the lower bound as a conservative estimate has been emphasised more in the revised version. As the reviewer notes, all other information is not reliable due to this discrepancy.

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

  We will do a thorough check of the introduction of all acronyms before resubmission.

- *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.*

  The trend in precipitation during the short 1979–2019 period says very little about long-term trends, one needs a century of data to be able to start to study drought trends. We added a remark that the precipitation trend in ERA5 may well be due to natural variability but does contribute to the trend in fire weather risk.

- *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.*

  Thanks for catching this. We have revised the paragraph discussing Figs 16 and 17 to also emphasize the observational numbers, so they do not appear out of nowhere when discussed later.

- *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.*

  There were a lot of questions about the specific situation in 2019 and the role of the IOD and SAM, especially from Australia where the Bureau of Meteorology had been emphasising these drivers. We thought it would be useful to address these, even though, as the reviewer remarks, they are implicitly included in the analysis of the previous sections. We emphasized that it is merely for situational context given the extreme nature of these modes in 2019, but that it is not additive to the attribution analysis presented earlier in the paper.

- *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.*

  We thank the reviewer for the additional information. The DMI index is based on ERSST v5. We added his information to the text and the caption. We have also replaced this figure with one starting in 1958. This makes the correlation to Jul–Dec precipitation slightly weaker, $r = -0.18$ instead of -0.22 and does not change any of our conclusions. The correlations to TX7x are now all non-significant.

- *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.*

  The one publication that we are aware of for the SAM that considers the same region also does not find a correlation (Perkins et al., 2015). For the IOD, the difference is that we avoid double-counting ENSO and IOD teleconnections by subtracting the influence of ENSO on the IOD before investigating the IOD connections with Australia. The ENSO forcing of part of the IOD has been extensively documented, we consider the part of the IOD that is independent of ENSO. As we mention there are significant ENSO teleconnections to this region.

We have added the temperature figures as requested.

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

  We thank the reviewer for reminding us of this. After doing this, the correlation in DJF becomes stronger but does not change our conclusion that there is no significant connection with TX7x. The Jul–Dec correlation is weaker and makes the connection to rainfall slightly weaker. The text and figures have been updated based on this analysis.

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

  Ellis et al write 'over 54 million hectares were affected by bushfires'. We have adopted this phrase in our text. (The 38 million hectares appears to come from Wikipedia, not Ellis et al. 2004.)

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

  We added this information to the text.

- *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.*

  We think it is important to discuss other factors than climate that have changed the risk of the impacts of the bushfires, the vulnerability and exposure. Even though climate change has increased the risk of bushfires, these other factors, together with meteorological factors we could not analyse such as the pyrocumulinumbus feedback, also influence the severity of the disaster. Short-term, the vulnerability and exposure are also the factors that can be addressed to decrease the risks, as stopping climate change will (at best) take a long time. This section has also been documented in the accepted companion paper, Philip et al. (2020).

- *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.*

  We are grateful for the many helpful and valuable comments by the reviewer, which undoubtedly will help us in our effort to polish this paper and make it more readable. While we acknowledge that the initial presentation of the study was suboptimal, we are glad to report that none of the adjustments to the text and the additional analyses affect the conclusions of the study.

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Interactive comment

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Discussion paper

Stott, P. A., Seneviratne, S. I., Vogel, M. M., and Wehner, M. F.: Human contribution to the record-breaking June and July 2019 heat waves in Western Europe, Environ. Res. Lett., https://doi.org/10.1088/1748-9326/aba3d4, 2020.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2020-69, 2020.

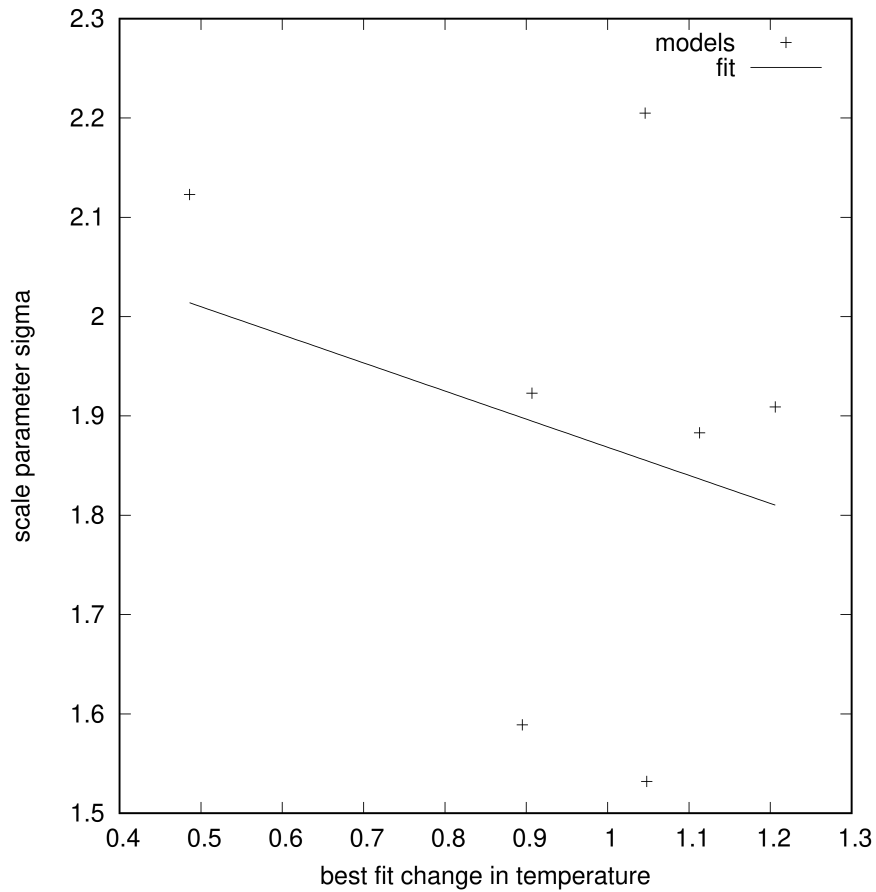

Fig. 1. Variability (scale parameter) versus model spread (best fit to trend) for the seven models that passed the quality control tests.

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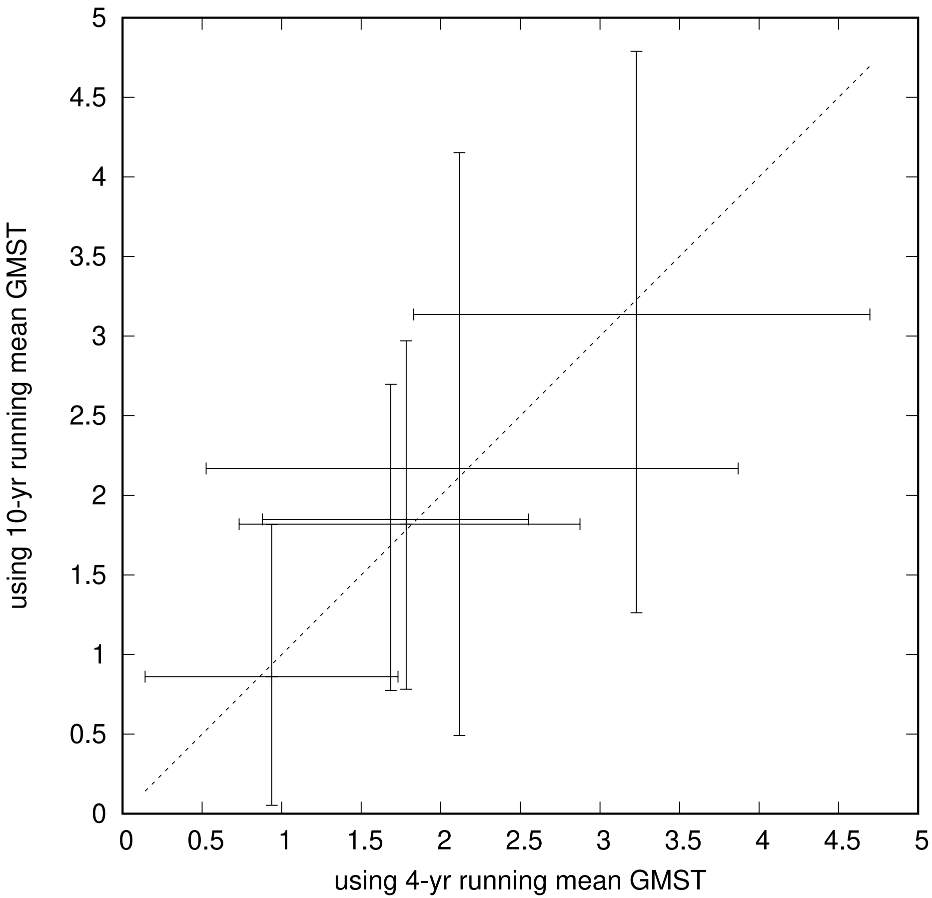

Fig. 2. Comparison of the ΔT using a 4-yr running mean and 10-yr running mean in the GMST covariate.

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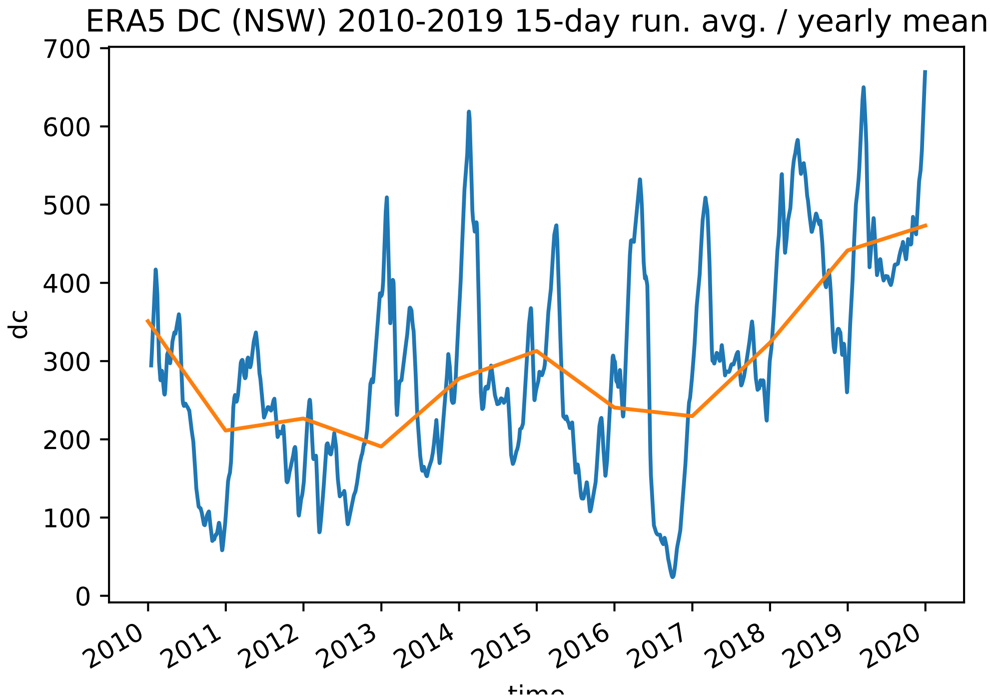

Fig. 3. ERA5 estimate of the DC component of the FWI with 12-month running mean.

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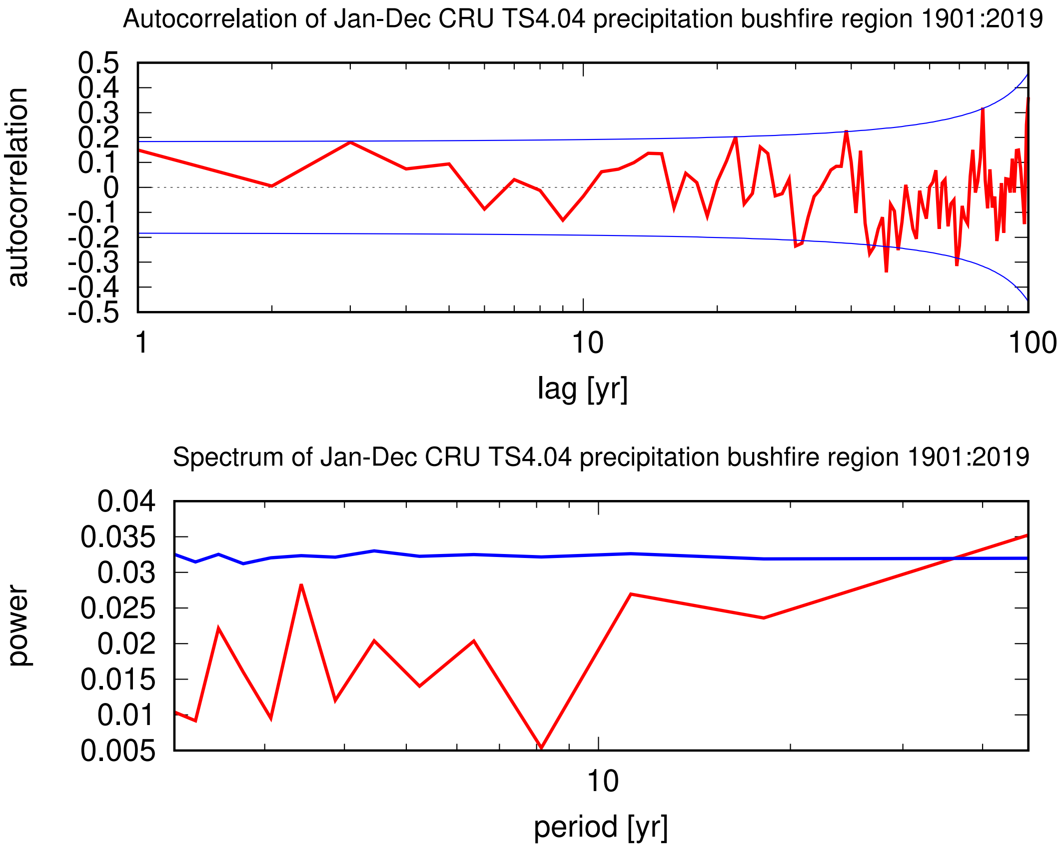

Fig. 4. Autocorrelation function and spectrum of annual mean precipitation averaged over the study area in southeastern Australia (CRU TS 4.04).

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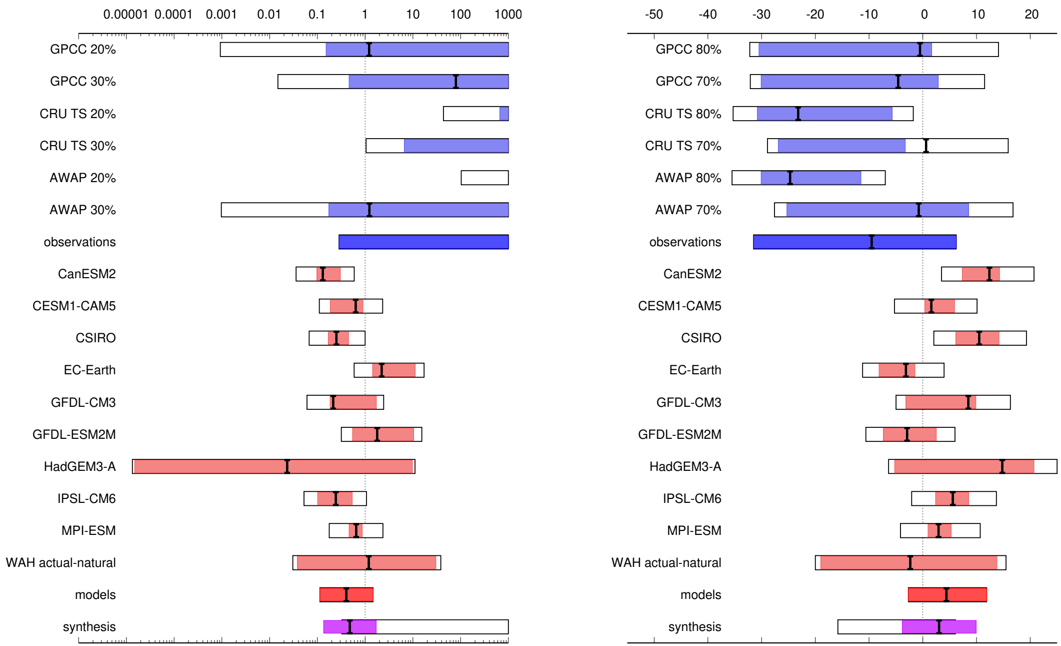

Fig. 5. As Fig. 11 in the manuscript but for 3-yr low precipitation in the study area.

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