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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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In my view, three key questions need to be addressed before we can attribute the bushfire risk to climate change, at least quantitatively.

We would like to thank Tim Palmer for his comments, which we anticipated and discussed during the writing of the paper. We do not think they substantially influence the validity of the attribution study, as we argue under each point below. We have been very careful in stating our results as a lower bound only, given that the model evaluation revealed the models tend to underestimate trends in the relevant quantities.

1. *The predominant cause of the bushfires were unquestionably the exceptionally (unprecedented) dry conditions in 2019. These were linked to the IOD (and other*

remote factors). Did climate change makes such dry conditions more likely? A pre-requisite to answering such questions is that we can simulate extremes of the IOD, the regional heat waves and the teleconnnections between the two. Studying 7-day heat waves does not address this issue adequately. We should be looking at model fidelity on monthly to seasonal timescales, where teleconnection biases are known to be substantial. The dry conditions of 2019 persisted into 2020 partly through continuation of the IOD, and partly because of the negative SAM.

   According to the bushfire experts on our team, the Fire Weather Index, which in fact uses the last 52 days of precipitation, is a good predictor of the interannual variability of bush fires, see e.g., Dowdy et al. (2009). This is confirmed by our own analysis shown in Fig. 2 in the manuscript, so it is not 'unquestionable' that the drought of all of 2019 was the predominant cause of the extreme fire season as the reviewer states. The physics behind this is that in summer the upper soil layers are typically dry and any precipitation evaporates quickly, so even an individual dry and hot season has the potential to result in high fire risk. The longer time scale precipitation deficits are important for agricultural and hydrological droughts that depend more on deeper soil moisture. We investigated longer time scales by analysing annual precipitation and half-yearly teleconnections (section 6) in case they would turn out to be relevant for the 2019/20 fire season as well.

   For the shorter time scales, a month was chosen as it enabled us to sample a larger set of large ensembles of climate models that only have monthly data available. Previous research (e.g., Hauser et al., 2017) has shown that individual models are unreliable for drought attribution and we thus prioritised investigating as many models as possible.

   Statistically, the teleconnection between the Indian Ocean Dipole (IOD; represented by the DMI) and the most sensitive half year of low precipitation, July–

December, which is also the one relevant for bushfires, is only $r = -0.22$ (our Fig. 19), implying that IOD explains only 5% of the precipitation variance. Without clear evidence for non-linearity in this relationship, the historical data hence do not show that the simultaneous occurrence of a high DMI and low precipitation is a general feature of the climate system. The next highest DMI value in 1961 was in fact accompanied by well above average precipitation. For our class-based event definition the historical connection is the important one, not the specific properties of the 2019 event (which will never occur again). This is emphasised in the paper.

For our definition of heat waves, the linear correlation to the September–November DMI index is $r = 0.22$. The time delay between precipitation and temperature response suggests that there is an important role for precipitation, with drier soils enabling higher temperatures. The simultaneous correlation in summer, December–February, between the DMI index and weekly heat is $r = 0.00$. This shows there is no dynamical connection on average from the IOD to heat waves in our study area.

Given that the IOD in the real world only explains a small fraction of the variance of key fire risk factors, model shortcomings in representing the variability or extremes of the IOD in either season are therefore unlikely to explain the factor ~ 1.7 difference between observations and models in standard deviation and trends in heat extremes that we found. The correct representation of the IOD and its teleconnections to the heat waves, drought and FWI is therefore of secondary importance to other model uncertainties.

The same holds for the Southern Annual Mode (SAM), although the correlation of $r = 0.39$ with July–December precipitation translates into a higher explained variance, $r^2 = 15\%$. There is no teleconnection to heat extremes. However, in this case we are not aware of literature that describes large discrepancies in trends in the large-scale midlatitude Southern Hemisphere circulation, as this is

exactly the kind of phenomenon weather and climate models were designed to simulate. A check of the properties of extreme low values of the SAM in most models used (we did not have the weather@home pressure fields) shows that except CESM1-CAM5 they indeed have a tail compatible with the one fitted to the ERA5 reanalysis. We already excluded CESM1-CAM5 as unrealistic. We have added this information to the main text:

'We verified that the statistics of 10–100 yr low extremes of the Southern Annular Mode averaged over July–December are represented well in the models used for the attribution analysis except CESM1-CAM5 (scale parameter too small) and weather@home (data not available).'

In summary, we have not investigated the causes of the mismatch between observations and models of a factor almost two in standard deviation and trend in extreme, but the IOD or SAM are unlikely to be the culprits as their contributions to the variability in observations are too small for that. Determining which model deficiencies cause the discrepancy with observations is an active area of research. Some literature suggests that shortcomings in the coupling to land and vegetation in the exchange of heat and moisture with the atmosphere and the representation of the boundary layers are much more likely to be the cause of the problems than large-scale dynamics. Added

'The literature suggests that shortcomings in the coupling to land and vegetation (e.g., Fischer et al., 2007; Kala et al., 2016) and in parametrisation of irrigation (e.g., Thiery et al., 2017; Mathur and AchutaRao, 2019) in the exchange of heat and moisture with the atmosphere, and also in the representation of the boundary layers (e.g., Miralles et al., 2014) are more likely to be the cause of the problems.'

2. *The Fire Index has a dependency on temperature which presumably relates to the fact that vegetation dries out more at higher temperatures. Of course, in general terms this is entirely reasonable. However, at the beginning of summer 2020, the vegetation was already extremely dry due to the exceptional conditions*

of 2019. It is not clear to me that under these circumstances additional temperature increased the fire risk much further than the critical value it was already at. In this sense the dependence of the fire index on temperature for these exceptionally dry conditions may not be correct.

The Fire Weather Index has temperature dependencies in its Fine Fuel Moisture Code representing the moisture content of fine fuels and litter on the forest floor, the Duff Moisture Code (DMC) that represents the moisture content of loosely compacted decomposing organic matter and the Drought Code (DC) representing the moisture content of deep compact organic matter of moderate depth (Dowdy et al., 2009). The dryness of the vegetation above ground is not used for the index. We checked if the relation between temperature and the FWI saturates at the high temperatures observed for this event by adding and subtracting 5K in 1K increments. We found no saturation, given that the relation between temperature and FWI was still mostly linear and positive. Added to text: 'We explicitly verified that the dependence of the FWI on temperature is almost linear in a range of ± 5 K around the reanalysis value (not shown).' Further, volumetric soil water (Figure attached) at multiple soil layers from ERA5 suggests that despite the soil already being very dry in 2018 and into 2019, the 2019/20 austral spring-summer drought caused a further drying of the soil in the study area. This indicates that the late 2019 drought and high temperatures did still cause an increase in fire risk.

The Australian Bureau of Meteorology also claims that extreme heat increases bushfire risks, 'The combination of prolonged record heat and drought led to record fire weather over large areas throughout the year, with destructive bushfires affecting all states, and multiple states at once in the final week of the year'(http://media.bom.gov.au/social/blog/2304/hottest-driest-year-on-record-led-to-extreme-bushfire-season/), so we see no grounds to speculate that the temperature dependence has saturated.

3. *The one factor where anthropogenic climate change may have been important, but has not been taken into account here, is the CO_2 fertilisation effect, i.e there was simply more vegetation to burn.*

   Recent literature from Australia shows that this effect is close to zero (Ellsworth et al., 2017; Jiang et al., 2020).

Unless we can answer these questions, then I do not think we can, or indeed should, be making quantitative estimates of the impact of anthropogenic climate change on these bushfires.

We hope to have either satisfactorily answered the questions or shown that the reviewer's concerns do not apply to our attribution statement.

Instead we must focus effort on developing a next generation of model where the regional dynamical effects of climate change can be simulated with much more confidence than is currently possible (Palmer and Stevens, 2019). Such models are likely to require much greater resolution than we have now - in particular allowing the convective rainfall anomalies associated with the IOD to be represented with the laws of physics rather than with relatively crude parametrisations.
Palmer, T.N. and B. Stevens, 2019: PNAS, 116, 24390-24395.

We very much agree with the reviewer that there is a strong need for a next generation of climate models with sufficiently high resolution to explicitly model the relevant processes, including dynamics, clouds, boundary layer processes, land-atmosphere interactions and vegetation. However, the question is whether we wait with quantitative assessments of the role of climate change in current weather extremes until everybody is sure the models are 'good enough'. We are confident that our careful attribution statement (only lower bounds) holds given our extensive model evaluation. A next generation of high-resolution climate models will hopefully provide a narrower estimate of the role of climate change in such extremes, but these relative careful quantitative statements have in general already proven to be very informative to the general public and important in decision making processes (e.g., James et al., 2019; Boudet et al., 2020). The timely availability of this particular result made it possible that it was included in the deliberations of the Royal and state commissions set up to investigate the causes of and response to these bushfires. The same uncertainties the reviewer points out are present in many attribution studies and projections generated, e.g., by the IPCC or in other national and regional climate change projections, which also are not suspended until improved climate models are available.

Figure: ERA5 volumetric soil water from multiple levels (1: 0–10 cm, 2: 7–28 cm, 3: 28–100 cm, 4: 100–289 cm, spatial average over the study area.

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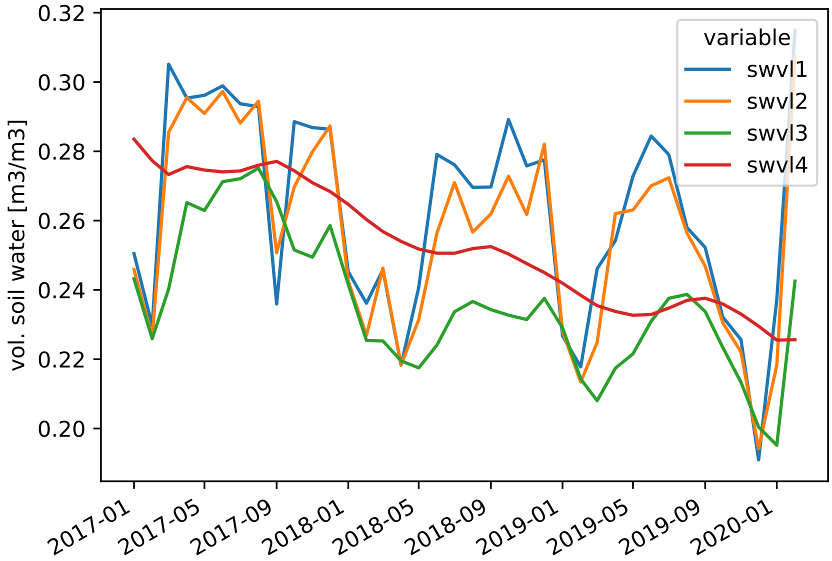
variable
swvl1
swvl2
swvl3
swvl4
vol. soil water [m3/m3]
0.32
0.30
0.28
0.26
0.24
0.22
0.20
2017-01
2017-05
2017-09
2018-01
2018-05
2018-09
2019-01
2019-05
2019-09
2020-01

Fig. 1.