Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2020-69-SC2, 2020

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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Received and published: 3 April 2020

1. The analysis in this manuscript uses a range of observational data, reanalysis products and model simulations. The authors chose to include or exclude data sets based on criteria that are neither fully documented nor properly justified. A transparent approach would be most useful.

For example, two long observation-based datasets (Berkeley Earth analysis and ECMWF's coupled reanalysis of the 20th Century CERA-20C) were dismissed because of their performance during one particular week (in January 1939) out of the 110-year period dating back to 1910. The argument here was that the results were highly sensitive to that one week which might have seen very warm temperatures. CERA-20C is

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a reanalysis based on the latest state-of-the-art coupled assimilation techniques which aims to provide a physically consistent estimate of the coupled atmosphere-ocean state and has been demonstrated to perform in general very well. If the results of the presented attribution study crucially depend on a specific week in a specific year, that should warn us about the robustness of the findings and questions the brute dismissal of the CERA-20C data set altogether.

No explanation has been given why ERA-20C, ECMWF's first atmospheric reanalysis of the 20th Century, has been excluded as a dataset.

Section 3.3. says that the ACORN-SAT station data were not available at the time of writing. But they are included in Figures 5-7. Clarifications should be given which data exactly are used in these figures and for which time periods.

For the Fire Weather Index analysis, no other reanalysis products than ERA-5 from 1979 onwards have been used – why? This presents another non-transparent choice of data sets and time periods used.

2. A related question is that of common periods between the data sets. The time periods spanned by the data sets vary considerably with some temperature data starting in the mid-19th Century, some in the early decades of the 20th Century and some only after 1950, see Table 1. The data in the FWI analysis only start around 1980 though. It is not clear to me what the impacts on the results are that stem from artefacts due to analysing data over vastly different periods, e.g. in Figure 5. If temperature trends were calculated using GEV estimates with a linear covariate relationship with global mean temperature, for data like JRA-55 assumptions must have been made for periods before the 1950s – was the temperature increase from 1900 extrapolated? Given the nonlinear nature of the trends from 1900 to today, that seems an overly strong and questionable assumption to me. The finding that ACORN, CERA-20C and ASF-20C reveal a non-stationary relationship between TX7x and GMST (presumably the others don't?) is interesting and would be worth more investigations regarding possible

implications. Perhaps the authors can comment on this.

3. As the main author of the ASF-20C atmospheric seasonal hindcast data set that has been used in this manuscript, I should clarify that the statements made around line 303 about ASF-20C are not correct. ASF-20C is not initialised from ocean reanalysis. Instead, it uses prescribed SST fields and is an atmosphere-only forecast product. The subsequent sentences are misleading at best and provide no justification for the non-stationary relationship between TX7x and GMST, as mentioned in line 300.

In line 370 it is argued that ASF-20C cannot be used for the drought analysis as the data cannot provide annual mean precipitation nor the driest month in a fire season. This is also not correct; ASF-20C does exist for all months of the year (although originating from different initialisation months).

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