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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Attribution of the role of climate change in the forest fires in Sweden 2018" *by* Folmer Krikken et al.

Folmer Krikken et al.

folmer.krikken@knmi.nl

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Anonymous Referee #2

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2-1

This paper deals with the analysis of model outputs and reanalyses using the FWI as an estimator of fire risk in Sweden.

The approach uses the standard technologies available today – climate scenarios, bias correction and of the like – to perform attribution studies and estimates of fire risk in coming decades.

We thank the reviewer for the feedback. We address the individual comments below. A pdf version of this response is attached.

2-2

Overall, however, the main result is that "In a future climate (a 20 C warmer climate relative to pre-industrial) the risk for such events to occur may increase more robustly by a factor ofâĹij2 (1.5 to 3) relative to pre-industrial climate", which is a result rather similar to many others currently available. We agree that, as stated in the manuscript [lines 274-282], our research is in line with other research with the exception of Yang et al (2015) for northern Sweden. Note however that we are the first quantitative attribution study of an extreme forest fire event in this region. Such attribution studies have proven useful in loss and damage policy (James et al. 2019).

The results is of particular relevance as high-latitude European forests are considered to be more sensitive to past and future climate variability (Drobyshev et al 2014), yet quantitative assessments of potential future changes in their disturbance regimes have been missing. The current study fills in this knowledge gap.

2-3

In addition, the authors conclude that "The increased fire risk is mostly driven by increased temperature", something couls have been expected also without refined analyses.

We disagree with this statement as the existence of an expectation is not to be regarded as existence of empirically derived evidence. In fact, many earlier fire studies put the main emphasis on dynamics of precipitation, not temperature (e.g. Lafon & Quiring 2012). An increase in temperature under future climate is indeed expected and it has the potential to yield an increased fire risk. However, just as important in fire weather risk are possible changes in precipitation, specifically changes in consecutive dry days. We mention this aspect in lines 247-255. An a priori assessment of how the

consecutive dry days will change in future climate is not possible as it requires multiple large ensembles with climate models to be assessed robustly. For Sweden, we find no clear changes in consecutive dry days (line 252-254 and figure 8). This allowed us to conclude that temperature is the main driver of the increased fire risk. We believe our analyses contribute with refining our knowledge of future fire risks in Northern Europe.

2-4

In conclusion, I am not convinced this paper is a significant addition to our understanding of fire risk assessment in future climates. If the authors intend to re-submit a new paper, I urge them to develop a critical analysis of the role of the various components (e.g., bias correction) and use different fire indicators.

We agree that a critical analysis of different bias correction methods or the use of multiple fire indicators would be an interesting follow-on study. However, we are under the impression that the manuscript in its current form provides more than enough valuable information for publication, including a number of novel and important methodological considerations. To the best of our knowledge, we are the first to use multiple reanalysis datasets in such an analysis, in which we demonstrated the large differences in FWI and associated return times. This is an important finding with ramifications for other similar research that only uses a single reanalysis product. To the best of our knowledge, we are the first to use multiple large ensemble climate models for such an analysis. Events such as the Sweden forest fires in 2018 have been, as demonstrated in this manuscript, rather extreme ($\sim$25 years return time). Hence, in order to acquire robust statistics on these events, and to extract a possible climate change signal, we need a large ensemble. This approach is particularly relevant in relation to precipitation dynamics and the resulting precipitation changes as its projections are highly model dependent. Thus for an adequate sampling of the model uncertainty we need multiple climate models. Previous research is often limited by both or one of these factors.

2-5

I think this paper is not acceptable in its current form and I suggest major revision along the lines indicated above.

We regret this assessment and humbly disagree. We hold that we are providing novel contributions to both the assessment of fire risk over Sweden as well as novel way to quantify methodological uncertainties in such assessments. We agree that exploring different bias correction schemes would be another of many additional analyses that could be conducted in a follow-up study. To address this potential, we now include a short discussion of this in the Discussion section of our paper [lines 285-289 in file with track changes, lines 284-288 in file without track changes].

We would like to sincerely thank both reviewers for many stimulating comments, which helped improve the quality of presentation.

References:

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Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2019-206/nhess-2019-206-AC2-supplement.pdf

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