

### Point to point response on reviewer 3

Dear Mr. Krikken and coauthors

Thank you very much for your interesting paper “Attribution of the role of climate change in the forest fires in Sweden 2018”. I think that your paper can be accepted after doing some minor changes.

*We thank the reviewer for investing the time for this in-depth review. Please find the point-by-point response to the comments below. Line numbers refer to the manuscript with track changes.*

#### Abstract

*Please, modify the abstract. It provides contradictory information. In the first paragraph you say “we find a negative trend of the FWI for Southern Sweden over the 1979 to 2017 time period” but the second paragraph ends with “We however do not find a clear change of prolonged dry periods in summer months that could explain the increased fire weather risk.”. Perhaps this second conclusion refers to climate projections but, in this case, you should clarify it.*

Response: Thank you for pointing this out. We have clarified this point in the text as indeed the second paragraph point to the results of the climate projections. We have now added “*in the climate models*” at the end of the abstract and clarified the text more.

*Figure 1 and so on. It is “burned area” not “area burned”. Please, modify.*

Response: We have now changed all ‘area burned’ to ‘burned area’

*Figure 1. What do you mean with “climatology over 2008-2017”? Do you refer to average annual value of burned area? What do you mean with “its individual years” Please, clarify the meaning of both terms in the main text.*

Response: The climatology refers to average cumulative values over the year, based on the data from 2008-2017. Hence, the climatology is also cumulative. The individual years all shows the cumulative values of burned area for each individual year. We have clarified this in the new figure caption on line numbers 513-514.

*“Figure 1: Burned area in Sweden. Cumulative values for 2018, average cumulative value (climatology) over 2008-2017 and the cumulative values for each individual year over the same time period (source: EFFIS).”*

*Figure 4. You only work in the paper with two Sweden regions, then you cannot say “Map of Sweden with the three regions used in this study”. On the other hand, what is the criteria of this regionalization? Are they climate regions? Please, modify the text as convenient.*

Response: Thank you for pointing this out. We believe the 3 regions in figure 4a together with only 2 lines in Figure 4b may have caused some confusion. Our analysis is indeed performed over 3 regions, as

stated in lines 149-151. For the analysis in Figure 4b we excluded Svealand (middle Sweden) because there were not enough fires / burned area to calculate robust statistics. This is mentioned in lines 184 to 185. However, to prevent further confusion on this we also added this information in the figure caption. Note that we do use Northern and Southern Sweden in the abstract to make it easier to understand for readers that do not read the complete manuscript.

The reason for splitting up Sweden is indeed not explicitly mentioned. We refer the reader to Drobyshev et al., 2012, as stated in lines 158-159. However, we agree that the manuscript would benefit in a short description. We have added the following explanation in the manuscript: *“Division of Sweden into Southern and Northern parts was justified by the analysis of observational fire statistics (Drobyshev et al., 2012) and dendrochronological reconstruction (Drobyshev et al., 2014) that revealed limited synchrony in the annual fire activity between these two regions.”*

*Line 49. You say: “found a strong influence between the recent increase in forest fires in California and the positive trend in vapor pressure deficit caused by anthropogenic climate change.” However, the temperature increase implies an increase of saturation vapor pressure and evaporation. Consequently, there is an increase of evapotranspiration and you can write “and the positive trend in evapotranspiration caused...”*

Response: Thank you for pointing this out. It has now been corrected.

*Line 59. Please, add a short explanation to justify the reason of this difference between Southern Sweden and Northern Sweden. Afterwards, in line 145 you say “Since Southern Sweden has a different fire climate than Northern Sweden” but for the reader that does not know Sweden, a short explanation is needed.*

Response: This has been addressed in the other comment about division between Northern and Southern Sweden.

*Line 91. Delete the second bracket in “(GISTEMP, (Hansen et al., 2010))”.*

Response: This has been corrected.

*Line 119. Delete the second bracket in “(CESM1, (Kay et al., 2014))”.*

Response: This has been corrected.

*Line 121. Replace RCP85 by RCP8.5*

Response: This has been corrected.

*Line 132. You say: “For the W@H simulations the GMST increase between the ‘natural forcing’ simulations and the ‘actual forcing’ simulations is 0.65°C, which is very close to the observed warming.”*

*But the last AR5 IPCC report showed that the warming was 0.85°C and nowadays it is near 1°C. The best will be to clarify in this sentence the period to which you refer when you say “observed warming”.*

*Response: The AR5 IPCC indeed reports a higher warming. This is because our ‘current climate’ is based on the 1979-2018 average, whereas AR5 IPCC bases it on the linear trend up to 2010. We chose this time period so we can best compare the climate models to the reanalysis (that cover 1979-2018). This is mentioned in lines 136-137. We have added ‘up to 1979-2019’ after line 143-144 to further clarify this.*

*Line 142. In this case references are written without comma “(Ho et al. 2011; Ehret et al. 2012)”, but in the major part of the paper you add a comma after the dot. Please, review all the references cited in the text in order to homogenize them and adding the comma if necessary.*

*Response: Thank you for pointing this out. We now homogenized all references to using a comma.*

*Lines 272-279. Please, delete or modify completely this paragraph. The non-linear model behavior observed or modeled is consequence of the non-linearity of atmospheric process and cannot be only related to the non-linearity of radiative processes. There exists a lot of discussion about this fact related with the dimming phase, mainly if you refer to the decade of 50’s when the anthropogenic climate change effect was still minor. There are a lot of factors related with forest fires production and non-linearity because they are also related with the potential combustible fuel. Vegetation has its own growth biorhythms that can depend of precipitation or temperature in other seasons of the year, and the climate change impact can be different for the different seasons of the year. I recommend you reading the papers from Turco et al., 2014 and Turco et al., 2018. The first one provides information about the different factors that can act in the forest fire risk and burned area. The second one can help you in the discussion about future scenarios*

*Turco, M., M.C. Llasat, J. von Hardenberg, A. Provenzale, 2014. Climate change impacts on wildfires in a Mediterranean environment. Climatic Change 125:369–380. DOI 10.1007/s10584-014-1183-3*  
*Turco, M., J. J.Rosa-Cánovas, J. Bedia, S. Jerez, J. P. Montávez, M. C. Llasat and A. Provenzale, 2018. Exacerbated fires in Mediterranean Europe due to anthropogenic warming projected with nonstationary climate-fire models. NATURE COMMUNICATIONS | (2018) 9:3821 | DOI: 10.1038/s41467-018-06358-z | [www.nature.com/](http://www.nature.com/)*

*Response: Thank you for clarifying this. We have decided to remove this part of the manuscript.*

*You obtain strong differences using the different models. Then it would be necessary to add a comparison of the FWI outputs from the models with forest fire observed data (burned area or number of forest fires) for the longest available period. I would recommend you in order to improve the paper and having a major impact, to add a figure with this information and discuss it in the results or discussion sections.*

*Response: Thank you for pointing this out. It is indeed very important to validate the climate models to observations. We did perform a validation on the GEV fit by comparing the shape and scale parameter of*

the GEV fit to the ones from ERA5. We found that these lie within the uncertainty estimates of the shape and scale parameter of ERA5. This was indeed not mentioned in the manuscript. We have now added this to section 2.4 (line nr 176-177): *“The models are further validated by comparing the scale and shape parameter of the GEV-fit to the ERA5 GEV-fit. All model parameters lie within the uncertainty estimate of the ERA parameters.”*

Comparing the FWI output from the model to observed fire data as suggested by the reviewer will unfortunately not provide the answers to this question. The natural variability in the climate models is not in phase with the observed climate / fire variability, hence a comparison as done in Figure 4b will not be valid beyond the trends, which are already analysed in Fig. 7.

We went through the entire manuscript again for a final check and made some small changes to correct small typos, further clarify results by including confidence intervals and to improve readability. Note that these changes do not influence the conclusions or results in any way. The changes are listed below. The line number correspond to the manuscript with track changes.

Line nr 13: Changed the correspondece person to Geert Jan van Oldenborgh

Line nr 19: Added cofidence intervals around the return time '*(90% CI > 10yr)*'

Line nr 20-21: Change to improve readibility, changed '*.. time period, yielding a decreasing probability of such an event solely based on reanalysis data.*' to '*.. time period in the reanalyses, yielding a non-significant reduced probability of such an event.*'.

Line nr 21: Removed '*given*'.

Line nr 22-23: Added text to highlight the uncertainty of the results, added '*.. give a large confidence interval around the number that easily includes no change, so..*'

Line nr 24: Removed '*on the other hand*'

Line nr 24: Added confidence interval '*(0.9 to 1.4)*'

Line nr 25: Added confidence interval '*(1.5 to 3)*'

Line nr 33-35: Added a short summary to the abstract '*In summary, we find a (non-significant) reduced probability of such events based on reanalyses. but a small (non-significant) increased probability due to global warming up to now and a more robust (significant) increase in the risk for such events in the future based on the climate models.*'

Line nr 98-99: Small correction on the data used, changed '*4th order polynomial*' to '*4yr running mean*'.

Line nr 105: Small correction. Changed '*.. dependence of the FWI the same ..*' to '*.. the dependence of the smoothed GMST the same ..*'.

Line nr 106: Added '*van der*' to the referens.

Linde nr 107: Changed '*precipitation*' to '*extreme precipitation*'

Line nr 108: Added '*to keep the FWI positive-definite*'.

Line nr 113 and 114: Changed '*will be*' to '*is*'.

Line nr 239: Textual change, changed '*The net effect on the FWI is thus a ..*' to '*The resulting net effect on the FWI is a ..*'

Line nr 245: Textual change, changed '*data*' to '*output*'

Line nr 253: Added a confidence interval, added '*factor 1.1 with a 90% CI of 0.9 to 1.4*'

Line nr 255: Added a confidence interval, added '*with a 90% CI of 1.5 to 3*'

Line nr 258: Added a confidence interval, added '(0.9 to 1.4)'

Line nr 259: Added a confidence interval, added '(1.5 to 3)'

Line nr 299: Textual change, changed 'Another' to 'a'

Line nr 309: Added a citation ('Hauser et al., 2017')

Line nr 316: Textual change, added 'that'

Line nr 326-327: Added examples to additional aspects for determining forest fire risk that were not considered. Added '*such as ignition sources, forest management and ecology*'

Line nr 330: Textual change, changed '90% uncertainty estimate ~10 years' to '*the 90% confidence interval starts at ~10 years*'

Line nr 334: Added '*an insignificant*'