Dear editors of Natural Hazards and Earth System Sciences,

Many thanks to your in-depth reviewing and valuable comments. We have now revised our manuscript according to reviewer suggestions.
Our study aims to present an approach to tackle the bias in the climate model in order to better use their outputs for impact studies. Here, we use a forest fire model, the FWI, to present the influence of multi-variable bias correction. We agree with Referee #1 that there is a mismatch between the tile and the content. We, therefore, revised the title to be "Multi-variable bias correction: application of forest fire risk in present and future climate in Sweden". Considering the suggestion from Referee #1, #2 and # 3, the manuscript has been in general shortened by removing the description of the FWI model and focusing the climate change impact in the distant future (i.e., 2071-2100) instead of coming 90 years. Accordingly, the content and figures have been changed.
Below are point-by-point answers to each reviewer comment, and we believe the manuscript is improved and hopefully ready for publication.

With best regards,
Wei Yang on behalf of Yang et al.

Anonymous Referee #1

GENERAL COMMENTS

This study illustrates the use of two climate projections driven by different forcing for forest fire risk studies and for determining future climate impacts on forest fires in Sweden. The authors showed that the raw climate model (GCM or GCM/RCM) outputs do not match very well the key weather variables in fire risk modelling, determining large inaccuracies in fire risk predictions. This is due to a range of factors, including uncertainties in observations, inaccuracies in physical process description, coarse resolution of climate models, etc. A distribution-based scaling (DBS) approach was developed as a post-processing tool with the purpose of correcting systematic biases in climate modelling outputs. The effects of the post-processing tool on precipitation, temperature, relative humidity and wind speed were analyzed. The Canadian Fire Weather Index system was then used to evaluate the influence of changing meteorological conditions on the moisture content in fuel layers and the fire-spread risk. Using DBS produces more realistic estimates of forest fire risk than using raw climate outputs. Based on these results, the approach proposed by the authors indicates that in the future southern Sweden is likely to have a higher fire risk than today, whereas northern Sweden will have a lower risk of forest fire.

The Results section includes 4 Tables and 12 Figures. Here is a summary of the main results reported in the paper: - Seasonal variations and probability functions of weather data (FWI inputs) during the calibration period (1966-1985) at Edsbyn station using observed data and raw output of the climate models. - Seasonal variations and probability functions of weather data (FWI inputs) during the validation period (1986-2005) at Edsbyn station using observed data and the “corrected” output of the climate models. - Seasonal variations of FWI indices and frequency of fire danger classes at Edsbyn station obtained using observed data, raw and corrected output of the climate models for both calibration and validation periods. - Annual mean of days with high fire risk estimated using observed data and raw and corrected output of the climate models during the calibration period for 14 stations. - Percentage changes of number of days with high fire risk during three future periods (2011-2040, 2041-2070, and 2071-2100) compared to the period 1966-1995 for 14 stations.

We thank the reviewer for these helpful comments, and respond to each of them below.

Based on the fact that wildland fire risk is largely influenced by weather conditions, more than half of the article is dedicated, throughout the MS, to discuss and analyze biases in climate models and methods that can be used to correct systematic biases in climate modelling outputs. I found a kind of mismatch between the title (Forest fire risk assessment in Sweden using climate model data: bias correction and future changes) and the content of the article. The title indicates that the focus is on fire risk assessment. The content of the MS focuses much more on climate model data and bias correction on climate model data rather than fire risk. In other words, the approach proposed to correct the bias of climate model is interesting and valid and can be used for any type of impact due to climate change, including obviously the potential impacts on wildland fire regime. However, I find that the context of the research in general is appropriate for NHESS.

Although the study seems well conducted, I suggest to reduce the number of figures and to revise the title so that it accurately reflects the content of the paper. For these reasons, I think that the MS needs very minor revision before publication in NHESS.

We have now revised the title to be: "Multi-variable bias correction: application of forest fire risk in present and future climate in Sweden". To shorten the manuscript we have focused on one future period (i.e., 2071-2100) instead of near, median and distant future to assess the forest fire risk. Figure 2 and Figure 10 are removed, and Figure 11 and Figure 12 are combined to be one figure. Thus, the latest version of the manuscript includes 4 Tables and 11 Figures.

SPECIFIC COMMENTS

- Page 844, lines 3-8. I suggest to espress measurement units in mm and mm per day rather than inches. Consequently, eq 9 should be corrected (substitute 400 for 100) a well as the moisture equivakent Q and the portential evapotranspiration V units (substitute mm and mm per day for inch and inch per day).

Technical description of the FWI models has been removed. The units used in the manuscript have been carefully proofread.

Anonymous Referee #2

The authors examine the effects of statistical bias corrections to climate model outputs on present and future fire weather over Sweden. They find that considerable improvements are made relative to raw model output on present day RCM simulation with implication for FWI projections under future warming scenarios.

This is a good paper. It is very clearly written and provides excellent methodological detail in considering the effect of bias corrections on different weather inputs to the FWI System.

Prior to publication, the paper requires more interpretation of the results in the context of other fire projections. As the authors state in the last paragraph of the Conclusions, further work must be done with, among other things, more models to start drawing robust conclusions for Sweden's future fire environment. In that vein, please relate your results to:

1. Flannigan et al. (2013). They projected an increase in the Cumulative Severity Rating (derived from the FWI) over the entire boreal region, including Sweden for 3 CMIP GCMs. All metrics considered indicate an increase in Northern Sweden, inconsistent with the results presented here. Please discuss possible reasons for this discrepancy.
2. Similarly, Dai et al. (2012) estimated increasing PDSI and decreasing soil moisture across all of Western Europe including Sweden. This is relevant given the importance of the DMC, DC and BUI to fire risk.

There are obviously methodological differences between your study and these, but the difference in sign from southern to northern Sweden (absent in the other studies) requires discussion.

We thank the reviewer for these helpful comments, and respond to each of them below.

Specific comments P841 L16: To shorten the paper, consider omitting the FWI System technical details, instead just summarizing the key features of each FWI component. In addition to Van Wagner [1987], Dowdy et al. [2010] provide a readable technical description of the FWI System.

Technical description of the FWI models has been removed.

P845 L25: the meaning of 'significant statistical properties' is unclear.

Enough long observation records (ca. > 20 years) are normally required to obtain important features from statistic point of view. The underlying concept is that the observation records should cover various climate phenomena as many as possible. In the manuscript, the "significant statistical properties" has been rephrased to be 'coverage of various climate phenomena'.

P861 L26: by 'it reflects directly' do you mean, 'it is affected directly'?

The text has been changed to 'directly influenced'.

P862 L13: change ‘sensitive test’ to ‘sensitivity test’?

The text has been changed to “sensitivity test”.

P864 L9: change ‘well reproduce’ to ‘reproduce’ and end the sentence with ‘reasonably well’

The sentence has been modified.

P867 L6: Please include appropriate caveats about possible future changes in vegetation/fuels and human activity when projecting future fire risk

Thank you for your suggestion. We have added a paragraph at Page 26, L13-23, as below:

“Forest fire activity and its spread is a result of combinations of weather, fuels and topography as well as incident management decisions. Thus, fuel bed structure and fire potential are influencing factors in addition to the changing climate. This kind of studies for Sweden has been partly done previously (Granström et al., 2000 and Granström and Schimmel, 1998). With changing climate, there may be a northward displacement of the broad vegetation belts with an increasing component of broad-leaved tree species at the expense of spruce (Koca et al., 2006). Fuel beds in the north may then shift from moss to leaf litter, with unknown effects on ignition potential and fire behavior. Apart from reducing human-caused ignition, experience concerning rescue tactics suppression methods need to be collated. An ongoing project will develop a national preparedness strategy for forest fires with consideration of changing climate.”

P867 L10: suggest discussing the Flannigan and Dai studies here.

Thank you for your suggestion. We have added a paragraph at Page 26, L24-Page27, L10 as below:

“Our results do not completely agree with the work of Flannigan et al. (2013), who found significant increases in the Northern Hemisphere by applying a combination of three GCMs and three emission scenarios. For Sweden, an overall and large increase was projected. One reason for the differences may be the way the climate change signal is treated. The DBS approach focuses on preserving the variability produced by individual climate projection, which is different from the traditional delta change (DC) approach by which the average changes are transferred onto the observations. Another difference concerns the spatial and temporal resolutions of the observed reference data. Compared to the large-scale data used in Flannigan et al., 2013, using

regional/local data is beneficial in studies including localized variables such as precipitation and wind speed.

Forest fire regimes with different climatic sensitivity in northern and southern Sweden have also been revealed in earlier studies. The results in Drobyshev et al. (2014) pointed towards the presence of two well-defined zones with characteristic fire activity, geographically divided at approximately 60º N. Such division was also reflected in Dai et al. (2012) who applied the self-calibrated Palmer drought severity index to study the global aridity in present and future climate. The calculated indices indicated drier conditions in southern Sweden than in the northern part under present climate. In the future, more precipitation was projected in northern Sweden in comparison with relative dryness in the southern Sweden. ”

Fig 8: change “1)” and “2) in x-axis label to a & b

X-axis labels have been changed. After reorganizing the figures, the content of Fig. 8 is presented by Fig. 7.

References

Dai, A. (2013), Increasing drought under global warming in observations and models, Nature Climate Change, 3(1), 52-58, doi:10.1038/nclimate1633.

Dowdy, A. J., G. A. Mills, K. Finkele, and W. J. de Groot (2009), Australian fire weather as represented by the McArthur Forest Fire Danger Index and the Canadian Forest Fire Weather IndexRep., 84 pp, Centre for Australian Weather and Climate Research.

Flannigan, M., A. S. Cantin, W. J. de Groot, M. Wotton, A. Newbery, and L. M. Gowman (2013), Global wildland fire season severity in the 21st century, Forest Ecology and Management, 294, 54-61, doi:10.1016/j.foreco.2012.10.022.

The references have been included.

Anonymous Referee #3

"Forest fire risk assessment in Sweden using climate model data: bias correction and future changes" by W. Yang et al. is a good paper, clearly constructed and giving full details for a better bias correction in the weather inputs of the national FWI assessment system. References are rich, pertinent and updated so no objection from the point of view of exposed methodology.

I have in any case some marginal comments to express:

We thank the reviewer for these helpful comments, and respond to each of them below.

1. Sweden is a member State in the UE28 since 1995 so its territory is included in EFFIS, the European Forest Fire Information System (EFFIS), which daily provides values of FWI for EU28, European non-member States and MENA countries of Northern Africa, i.e. for 42 countries. Its performances are considered very positively by all countries and help in more efficient activity of prevention and suppression.

I am therefore warning why the authors do not mention EFFIS results nor make a comparison with them, whereas they speak of an operational use of FWI in Sweden by the SMHI since 1988; in addition their thresholds for the 6 classes of FWI (which see a value of extreme for a FWI > 28) are very different from those adopted at EU level by EFFIS and also from those firstly adopted by FWI in Canada. How can this difference be explained? Why not commenting differences, if any, among the results of the two different procedures?

The major difference between the EFFIS (currently at 10 km and 16 km resolution) and national system of the FWI at SMHI at that time is their spatial resolution. Since 1998, daily forecasting data from our meteorological forecasting model, HIRLAM, have been used to drive the FWI model, which provides information at spatial resolution of 22 x 22 km and from 1999 on at a resolution of 11 x 11 km. In our work the observations we used are point stations, therefore the results are not directly comparable to the EFFIS results due to the scale mismatch.

At SMHI we have another model based on the HBV hydrological model to estimate forest fire risk. It calculates daily moisture content of upmost soil layer and provides information about water balance at a regional level. FWI classes were established so that the numbers of days of each index class roughly correspond to the equivalent number in the HBV system.

Here, we have added a bit more information regarding the national system in section 2.1.3. The details can be found in Gardelin, 1997.

2. Their explanation of FWI is interesting but excessive: it is a well-known item, covering hundreds of titles in specialized literature, so it seems useless to explain its components and

the algorithms for their assessment, which cover the whole section 2.1; also the image of FWI is useless for the same reason.

Technical descriptions of the FWI model and Fig.2 have been removed.

3. P.839, L. 1” Forest fire activity is strongly affected by two factors: weather conditions and availability of fuels” but why do fire occur? Where are they concentrated and, above all, are their origin mainly human caused or natural? Is fuel availability influenced by human activity? Do fires occur in forests, in shrub land? Do they exhibit specific characters of concentration, seasonality? Sweden is not among the countries with relevant occurrence of fires but the results of paper indirectly propose a markedly seasonal surge of events without giving information about it, just mentioning a recent large fire of which no size parameter is expressed. Some details could be of interest, also given the changing scenario of wildfire distribution as a consequence of climate change in northern latitudes

We have added a short description of forest fire in Sweden in section 1, at Page 2, L16-31.

4. Nothing is said about the origin of forest fires in the country and text itself looks rather abstract and neutral, as though fire occurrence is natural caused, which appears as the natural conclusion inferred from the read, indirectly confirmed by the statement in point 3. A short explanation of such facet could improve the paper and better capitalize its interest, since the procedures proposed and adopted by the authors could certainly be introduced in EFFIS evaluation and further improve its performances.

We have added a short description of forest fire in Sweden in section 1, at Page 2, L16-31.

Final consideration: the paper is interesting and well written; I suggest to the authors to make their country better known under the aspect of forest fire occurrence, in which it is not among the most important participants, and more clearly express the role of national system of FWI assessment (alternative or opposite to EFFIS?)

Thank you for your suggestion. Please see the answer to point 1.

Anonymous Referee #4

Wildfires are not common or widespread in Sweden, but they can be severe. It is of great important to assess the future change in the fire risk. In this study, the authors first dynamically downscaled the simulation from global climate model to a regional 25km resolution using a regional climate model, RCA3. Then the systematic errors of the downscaled results were corrected using in situ observations. Finally, the validated dynamic-statistic correction method was used to project the future change in fire risk on point scale of Sweden. The correction method developed has been proven useful. The results show important information for local government and rescue agency. However, the manuscript needed to be improved for both the scientific preciseness and the technical side before it can be accepted for publication in Natural Hazards and Earth System Sciences.

We thank the reviewer for these helpful comments, and respond to each of them below.

In what follows we suggestion several improvemnets.

Major points:

1. There are distinct climate regimes in Sweden, such as the different climate regime between northeast and northwest Sweden. Can the few stations in northern Sweden represent the different climates? How will this affect the results, especially for northern Sweden? In section 5.2, the author wrote '… Edsbyn in northern Sweden …' Does station Edsbyn a good indication of the climate for Northern Sweden?

Clearly, the results in this paper are mainly representing southern Sweden, the few stations in northern Sweden must be viewed as only indications of the future changes there.

2. The projection is largely affected by the selected global climate model. Why choose ECHAM5?

We agreed with your opinion. As mentioned in the manuscript in the next step we will apply more GCM/RCM projections to cover a larger fraction of the total uncertainty. Here, we start with the ECHAM5/RCM3 as an example to present how our approach works out. ECHAM5 is a commonly used GCM in climate change impact studies, as it is proved to be a reasonable global model representing the current climate for the Europe.

3. Number of sample is very important in statistical analysis. The simulated annual mean number of days with high fire risk is small than that from observation, especially for northern part. Will this affect the robust of the results? Will the results be the same if the authors choose FWIX≥4 instead of FWIX≥5?

When FWIX=4 is included in the analysis, the underestimation is still noticeable, but in general gets improved. As we discussed in conclusion

section, the complete production chain contains a number of uncertainties sources such as observation, climate model (i.e. GCM/RCM), bias correction approach and the forest fire risk model itself. Here, we aim to present an approach to correct the bias and preserve the climate change signal from climate projections. We agree that the uncertainties are not completely removed, which will affect the results.

4. Since the authors are talking about the impact of climate variables on the same time change in fire risk, why not focus on the climate variables for the fire season, i.e. from April to October, instead of using the traditional three seasons, from March to November?

The statistical properties are found different from season to season.

5. We lack a discussion on which process is the major impact factor to the projected fire risk for different fire season. What is the role of short term changes in weather conditions versus long term drying condition?

We agree with you that understanding the major impact factor for different fire seasons in different parts of Sweden is indeed important, and this is a major future activity. As a first step, in this study, we aim to correct the bias in driving variables in order to obtain reasonable assessment of the forest fire risk under changing climate. As a next step we will focus on the characteristics of individual events and apply in-depth analysis such as the length of dry period, type of wind, vegetation growing period, etc., to identify the dominant processes.

Specific comments:

1. In section 5.2, after 'Using the corrected data, early spring at the Edsbyn station is found to become more prone to forest fire, followed by autumn, and then summer (top panel in Fig. 13)…' We cannot get the same conclusion for station Edsbyn based on Fig. 13. For example, the author said, 'In the intermediate future, the risk in early summer becomes even lower (i.e., approximately -50 %)', while we found from Fig.13 that the maximum difference between 2041-2070 and 1966-1995 is around -20% for the DBS corrected results in summer time. Please have a check whether all the results are based on the DBS corrected simulation or not.

It should be -20%. The results have been carefully checked.

2. Are results presented in Fig. 14 focus on the fire season, Apr to Oct?

Yes.

3. What do SD/SD1 and SD2 mean?

Both of SD^1 and SD^2 explain the standard deviation of data. SD^1 is calculated for every station and then averaged over all stations. SD^2 is calculated to explain the spatial deviation, which aims to study the variations amongst stations. It is the standard deviation of the mean values of all stations.

4. Fig.1 should be Fig.2 and Fig.2 should be Fig.1 according to the text.

The mistake was corrected.

5. On page 22, line 5, the authors stated: "At station Edsbyn, the cut-off value varies from 0.85 (spring) to 1.56 (summer)." We guess the units are mm/day, or?

The unit is mm/day.

6. Please consider using difference color for Fig. 11-13.

In the figure, different line style is used. We feel it may be better for black-and-white print out.