

Interactive comment on “Evaluation of Global Fire Weather Database re-analysis and short-term forecast products” by Robert D. Field

Anonymous Referee #2

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General comments

The paper entitled “Evaluation of Global Fire Weather Database re-analysis and short-term forecast products”, by R.B. Field, addresses the evaluation of reanalysis and forecast model-derived fire weather products of global coverage to provide a baseline for their application in impact studies. In particular, two main research questions are addressed:

1. All the FWI system components, as computed using the MERRA2 reanalysis, are compared against observational data from a global weather station network (n=1746) in terms of biases and their relationship with the input variables. 2. The skill of short-term FWI forecasts (up to 8-day lead time) based on the NASA GEOS-5 weather forecasting system is evaluated, considering the global observational database as ref-

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erence.

This is an interesting paper, of undeniable scientific quality and relevance for the target journal. I would therefore recommend publication without major modifications.

I have elaborated a brief list of minor typos, and a few questions and suggestions. The article is well written and the results are presented with relevant tables and figures, and adequately discussed within the context of earlier research in the field. In my opinion it is a valuable contribution in the line of improving our understanding of the FWI system and to aid data users in its proper application, particularly when facing the need of using reanalysis data to this aim due to poor (or null) observational coverage, which is often the case in many impact and vulnerability assessment studies. The product here analyzed (GFWED) is of great relevance to this aim, and a thorough assessment of its advantages and limitations as compared to actual observational data is presented at a global scale. Furthermore, the assessment of the NASA GEOS-5 FWI forecast skill provides useful information for their application within operational and/or research context.

Specific comments

I agree with the first referee in that the manuscript would gain from a unified description of the different databases/models involved in the analysis under the Data and Methods section, so the reader can get a more straightforward overview of the data involved (GFWED, MERRA-2, GEOS-5). I also find confusing the alternative use of “GFWED” and “MERRA2 FWI” denominations throughout the text. As a suggestion to the author, it would be also interesting a very short comment on the main differences between MERRA and the newer MERRA-2 (of course, nothing too technical), and if possible to mention in a nutshell what would be the expected improvement or most relevant differences regarding the derived FWI product in both cases, apart from the citation to the work by Field et al. 2015 focused on the MERRA-based DC (L50-53).

The gaps in the input fire-weather variables TEMP, RH and WDSPD from the station

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data were completed using data from MERRA2 fields at the gridboxes of each station (and precipitation from CPC records), up to 20% gaps. This is probably the least bad option in the presence of missing data, although it is obviously “favoring” the validation results at the gap-filled stations. Are the corresponding MERRA2 data being introduced directly, or is any form of bias correction being applied prior to that, so there is a smoother transition between actual records and MERRA2 values?

It is unclear what is the verifying reference against which GEOS-5 FWI forecasts have been validated (MERRA, MERRA2?). This should be made clear early in the manuscript.

In L234 some outlying values over Pakistan are mentioned. Wouldn't it be better to just discard these data with a detrimental influence on the validation results?

The results obtained indicate the need for bias-correcting the MERRA-based FWI in many real-world applications (L371-373). With this regard, it might be worth mentioning that the correction of multi-variable indices has some intrinsic complexities that, for the particular case of FWI, have been previously addressed by other authors (see e.g. Casanueva et al. 2018)

The paper contains a lot of information from the validation of GFWD and GEOS-5 forecasts. I agree with referee #1 that the manuscript would benefit from a final conclusions section summarizing the main results and conclusions.

Technical corrections

I have also suggested a few corrections to a few typos in the text, apart from those already indicated by the Referee #1

L24 “[...] is calculated temperature, relative humidity”... Is it perhaps the word ‘using’ missing here?

L38 The reference to Cantin 2016 is missing in the reference list

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L48 needed instead of need?

Table 2. I would suggest to include this information in the legend of Fig. 1, so this table can be eliminated.

Table 3. The columns SNOWD and FIRESEASON are well understood, but these codes have not been previously described explicitly, neither in the text, nor in the table's caption, so I would suggest to explicitly describe them prior to first using them.

L188-189 [...] across stations “for?” each of the GFED regions

Fig. 13. Given the wide variability of FWI magnitude across the globe, did the author consider to use here relative instead of absolute biases?

L402 Although “at” seasonal

References

Casanueva, A., Bedia, J., Herrera, S., Fernández, J., Gutiérrez, J.M., 2018. Direct and component-wise bias correction of multi-variate climate indices: the percentile adjustment function diagnostic tool. *Climatic Change* 147, 411–425. <https://doi.org/10.1007/s10584-018-2167-5>

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2019-197>, 2019.

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