

Interactive comment on “A fire model with distinct crop, pasture, and non-agricultural burning: Use of new data and a model-fitting algorithm for FINALv1” by Sam S. Rabin et al.

Anonymous Referee #3

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General comments

The authors describe a novel fire module FINAL to the DGVM LM3 which distinguishes fires on cropland, on pasture and on non-agricultural areas in terms of driving conditions and development. Thus, the approach allows to separate fire-related emissions in terms of seasonal occurrence and cause. The procedure is well documented with the modifications applied to previous work by Li et al., coherently laid out, well structured and very understandable, especially the discussion.

Nevertheless, I have one major concern which is centered around the parameter optimization. Here, three obstacles hinder rapid publication which are partly acknowledged

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by the authors:

- The chosen algorithm may find a local minimum instead of the global one. The criterion for convergence is not clearly defined.
- For the global model only a selection of grid points is chosen for the calibration procedure without information on the selection criteria. In case of undersampling climatic conditions, the resulting parameter set may not be ideal for the neglected region or the influence of one of the drivers may be underestimated because this variable did vary between the chosen grid cells.
- The error metric is already discussed in the manuscript. It would be good to at least complement the metric by others especially designed for comparison of model results and observations.

The mentioned flaws in the design of the optimization lead to a parameter set that extinguishes one of the drivers for fire occurrence namely relative humidity. The authors should motivate the chosen method in a way that this result is convincing and the reader is not suspecting it to be caused by making an inappropriate choice. The neglect of relative humidity while strengthening the role of soil moisture usually asks for the correlation of these drivers. Please make clear why and in which way both variables play a role.

Specific comments

P1L13: 'the boreal zone suffers from underestimates', please rephrase because it is unlikely that the boreal zone really suffers.

P3L25: the argument that an MCMC approach would be too costly is understandable but maybe worthwhile when the parameter space really has to be explored. There are also other approaches like the version using generations which could help.

P4L12: 'state-of-the art' -> 'state-of-the-art'

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P5L16 and Eq. 4 and nearly all further equations: inconsistency of brackets. There should be round brackets for functions and square brackets for indices. You use both for the same expression which is disturbing.

P11L6: 'all N sample gridcells selected for the optimization'. How many grid cells were selected, how and why? What are the criteria for this?

P12L4: why were parameters from eq 12, 13 or 20 not selected for optimization?

P13L2: the symbol F_k is not explained before

P13L12: the resolution of LM3 could be mentioned earlier in the general description.

P14L21: The optimization process takes only 10 time steps. The criteria for convergence remain completely unclear and the parameter value development makes it unclear if there was a convergence. This part of the approach should be included in methods and the convergence decision should be motivated.

Fig. 5: shows clearly that fire suppression by relative humidity is gone completely but that by soil moisture is even stronger. Also population density gets a stronger influence and that of AGB becomes less with the resulting parameter set. This is mentioned in the discussion but in the results it does not become clear why this parameter set should be accepted.

P15L11: the question on a substitutional effect of soil moisture and relative humidity arises again. Could you comment on that?

P17L30: the figure may be moved to the appendix and only the numbers be included in the text to describe differences in the spatial heterogeneity.

P18L8: long-lasting fires are an interesting topic and only mentioned briefly. Could you include a short comment on expected improvements or if you intend to investigate this further?

P19L6 to L28: this part could be moved to the results.

P20L32: this is critical because you undermine your resulting parameter set. How is the artifact possible? Could it be caused by the choice of the grid cells for optimization? Why should the reader accept the chosen parameters?

P22L3: this is an interesting information. Which input data are additionally used and

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why are they not further taken into account? Do they also result in a suppression of the factor relative humidity?

P22L15: after this reasoning it is even more important to consider at least more time steps in the optimization procedure or to consider a different parameter space search.

P22L17: this is a good discussion on the error metric. Please consider to complement SSE by other metrics also in the result section (e.g. see R package QualV; <https://www.jstatsoft.org/article/view/v022i08>)

P23L18: this valuation is refreshing in its clarity and honesty but please consider the effect on the reader. Are you really not convinced that the chosen approach was successful? In this case, the optimization has to be redone with a different selection procedure for the reduced gridcell set, a different optimization algorithm and an increased number of simulations.

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