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Interactive comment on "An ensemble approach to simulate CO_2 emissions from natural fires" *by* A. V. Eliseev et al.

Anonymous Referee #1

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Major comments (in descending order of significance):

The work presented uses GFED3 emissions for model constraint (P1451, L12-13, P1452 L14-15) and talk about over and underestimates of GFED3 emissions. GFED3 emissions are based on very uncertain emission factors, fuel loads modelled by CASA and observed burned area. I do not believe the use of GFED3 emissions can be considered a model constraint by observations, rather a model constraint by the output of another model. Yes, GFED burned area is not differentiated by fire type, but this is not reason enough to choose GFED emissions instead. In many areas model-data differences for burned area are by orders of magnitude (taking the original GlobFirM publication) and it is clear enough that there is a disagreement even without knowing the contribution of non-natural fires. In addition, the distinction between natural and

man-made fires is very fluid and a matter of intense debate.

The second major issue is that validation is only against a fire product, but there is no validation at all of the climate model output. Even if burned area was used to constrain the ensemble, one would still not know if a particular ensemble version fits the observations because both climate variables and fire are realistic, or whether we are dealing with compensated errors of the climate forcing of the fire module and the fire module itself.

The GlobFirM model is not any more in much use. Reading the original paper reveals why: it was a first good approach performing satifactorily in some areas, but overall the results are often orders of magnitude away from observations (Fig. 6 in Thonicke et al. 2001, for example parts of Spain have the same fire return time as African savannas). Paragraph 3 of the abstract of Thonicke et al. (2001) states the preliminary character of this model very clearly. The authors need to make a strong point that using their approach leads to significant improvments in the peformance of GlobFirM, sufficient to make parameter esimation approaches meaningful.

In the Methods part, there is no information given on the spatial resolution of the model, making it very difficult to judge if the resolution is sufficient for some basic realism of the model to make the exercise worth-while, and if the scale gap between observations used to constain the ensemble and the simulations is not way too large. After all, the GFED3 product is based on burned area data, which by itself are based on 1km by 1km satellite data. By the standards of the remote sensing community, this is considered relatively coarse.

As this study uses a model of intermediate complexity, the question is how realistic the interannual variability of simulated fires is compared to observations, and how this affects the results.

In the introduction, instead of saying models are far from being "mature" (I agree) it would be better to give some concrete examples. These should definitely include an

assessment of how they perform against global burned-area products, e.g GFED3 or GFED4, or the estimated global burned area corrected for the impact of small fires (Randerson et al. 2012).

Minor comments:

General:

The presentation needs improvement, including language. It takes basically all of the introduction, methods and results until one understands what has been done.

Abstract

What is missing is a basic description of the fire module, including the basic of approach used. Then It is also unclear what, how and what part of the model system is constrained by what observations. Are there 5 parameters, 50 in total? In what part of the model system? How many of the parameters of the entire model system are constrained? (This amount of detail should of course in the methods section, but a basic understanding of what is being talked about here needs to be conveyed.)

P1444

L3 and further: parts of the text are past tense, parts present tense. L3-4 "reconstruction of external forcings": should be "by historical reconstructions", and also better to name of what variables. L6: -> until the year L7: what are "governing parameters"? L8: most readers won't know what the GFED3.1 data contain. L10: which -> that. What is meant by "robust", and by "within the constrained ensemble"? Does it mean the entire ensemble is constraint? I would have expected the members. L12-13: "means", "deviations" - does this mean there are several ensembles, and is the mean the mean of the means (and the same with s.d.)? Please explain. L13: -> emissions to the atmosphere L14-15 and rest of abstract: I would say that the GFED3 burned area has much more justification for being called an "observation" then the GFED3 emissions. Therefore I would not talk about "underestimates" here. L18: -> during the 21st cen-

tury. Be more precise, how is the estimate derived? As there should be some (even under-estimated) interannual variability, climatological averages, or a linear time series fit would be needed. L23: Better not reverse the order of RCPs compared to above, confusing. L24: "in year 2300". There is so much interannual variability (at least in the real world), that picking out one year does not make sense. L25-26 "all changes [. . .] mostly": this is a contradiction, either all of them, or most of them. L28-29: -> the increase of burnt area [. . .] is accompanied

P1445

L6: -> the latter. L6-7 "substantial part [. . .] is". This is not true, very few ESMs contain a description of fires. L9: "bulk characteristics", not sure what is meant by that. L11-13: The main LPJ version now contains SPITFIRE, not Glob-FirM. L15: The model by Pechony and Shindell is not a proper fire model, but a model of fire ignitions. It only predicts number of fires, but not area burned, and therefore cannot even predict emissions, which are related to burned area, not number of fires. L26: What is meant by "impact-orientated". Be more precise about what the purpose of those indices is.

P1446

L4 "lacks information". I would say it is not even the goal of those indices, they are just rough indicators without any intention to become quantitative. L5ff.: What is missing is a more concrete discussion of the performance of those model, in particular how they compare against observations. Also, the English needs attention. L11: "as proxies. . .": I don'think this is the best way of putting it. Basically, I have a model and estimate parameters in an inverse way from observations. All: Here In the introduction, it still does not become clear what kind of model we are talking about. RCP forcing could be greenhouse gases, but CMIP5 forcing hints at a simulation with an "off-line" fire model, not a GCM with fire model incorporate, as mentioned in the abstract.

P1447

L26-27 "in order to [. . .] CO2": the word to "preserve" here does not fit. Also, what output is used have is irrelevant for the presentation. Better to state the analysis will be restrict to annual means. Then, when referring to CO2 emissions, an approximation might be used where one assumes the atmosphere as whole is instantaneously mixed. This, however, needs back up by appropriate references. I am not sure the cross-hemisphere mixing time is rapid enough for that. In any case, you do not seem to make use of atmospheric conentrations.

P1448

L16-17: In Thonicke et al. (2001) there is nothing about emissions and consumption of dead or live vegetation by fire, therefore this sentence is misleading. Please explain better what has been done.

Conclusion:

The approach is interesting, but the study needs major revisions. In particular, the forcing data of the fire model needs to be subject to the same checks as the observations used to constrain the combined climate-fire model, there needs to be a better assessment of whether the spatial detail of the model is sufficient, and the model constraint needs to use observations not output of another model (CASA, emission factors). Either constaints need to be applied not only to the fire model, but the entire model system, or the forcing data of the fire model needs to be taken as given and subjected to rigorous testing, and then parameter constrant needs to focus only on the fire model. It must then be demonstrated that with this approach, GlobFirM can be made to work properly, without orders of magnitude differences between observations and model.

References

Randerson, J., Y. Chen, G. R. van der Werf, B. M. Rogers, and D. C. Morton (2012), Global burned area and biomass burning emissions from small fires, J. Geophys. Res., 117, G04012.

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