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Discussions

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

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Reply to the reviewer's comment on An ensemble approach to simulate CO_2 emissions from natural fires

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The authors are grateful to the reviewer whose comments helped to clarify the paper.

The major changes in the manuscript are as follows:

- The paper is extended by the comparison of the performed simulations with the GFED burnt area. To remove possible influence of anthropogenic fires, we we masked out the grid cells where the carbon release from anthropogenic fires, E_a, is larger than $5 \times 10^{-4}\,\mathrm{gC\,m^{-2}\,yr^{-1}}$. The latter value was chosen by a visual inspection of the GFED maps for E_a. Comparison with plots published by van der Werf et al. (2010) has shown a reasonable agreement between two approaches: the one suggested by the referee and that used here. Regional averages of the burnt area are calculated for the GFED data only for the regions in which there are no masked–out grid cell. We conclude that in many regions realistic CO_2 emissions are obtained for the burnt area which markedly deviates from observations (without account for small fires, though; see Randerson et al., 2012). In accordance to this, we redrawn Figs. 3 and 4 in the main text and Figs. S3 and S4 in supplementary information. In addition, the estimates by

Conard et al. (2002) are added to these Figures. We also included regional averages of the GFED data to Table 2 of the main text. The comparison between our simulation and the GFED burnt area is included in Sect. 3.2. Finally, the former last paragraph (now, it is the second last) in Sect. 2.3 is revised by including the description of our approach to remove the impact of anthropogenic fires on the burnt area.

- We extended the main text by Sect. *Caveats*. This Section includes a discussion of possible shortcomings of our approach. These shortcomings are related to
  - uncertainties in the GFED data: we state that their spatial resolution is insufficient for detecting small fires (Randerson et al., 2012) and that CO_2 emissions in this data set are obtained from the CASA simulations;
  - biases of our Earth system model, its coarse resolution, and simplified nature of the GlobFIRM model; in particular we state that our calibration is a calibration for the GlobFIRM model *within* a particular Earth system model;
  - assumptions behind the averaging procedure; for this, we moved and extended the respective paragraph which initially was in Sect 2.3.
- We stress that the main goal of the paper is a presentation of the ensemble approach to simulate the burnt area and CO_2 release from natural fires. An additional novelty of our work is an extension of the original GlobFIRM model by a scheme accounting for carbon release from peat fires.
- Sect. 3.3 is extended by the comparison with off–line simulations with the CLM–3.5 (Kloster et al., 2012). Our results are in general agreement with the results obtained by Kloster et al. To the best of our knowledge, no systematic comparison of the CMIP5 models concerning natural fires is publishes so far. Such a comparison is beyond the scope of the present study as well. Moreover, as we

know, our paper is the only coupled climate model assessment of possible response of natural fire characteristics to climate change occurring in the IAP RAS CM during the 21st–23rd centuries. We plan to undertake this comparison in future by employing, in particular, our Bayesian averaging procedure.

- According to the suggestion of the second reviewer, the burnt area notation is changed from S to 'BA' for the global or regional burnt area, and from s to 'ba' for the burnt area per model grid cell. In addition, k_{res} is renamed to CC (combustion completeness).
- The language is checked and ameliorated.

Below the point–to–point answer to the comments made by the reviewer are given.

General comments

- *'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.'*

We agree with this remark made by the reviewer. The discussion of these issues is included in the newly–introduced Sect. *Caveats*. We note that, while the GFED data are imperfect, these are the only global data distinguishing different fire types. The latter is important for the purpose of our study. We also extended the paper by a discussion of caveats related to the simplified nature of the GobFIRM.

- *'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.'*
  We are grateful to the reviewer for this comment. We agree that our calibration is a calibration of the GlobFIRM model *within* an Earth system model. As a result, the biases of the latter are likely affect the calibrated values of the governing parameters and, therefore, the ensemble statistics. The IAP RAS CM climatology is basically realistic, see (Mokhov et al., 2005) for the atmospheric part, (Arzhanov et al., 2008) for soil moisture, and (Eliseev and Mokhov, 2011) for terrestrial carbon stock. However, as any other climate model, our model exhibits a number of biases, which may affect our simulation of fires. We guess that the most important biases affecting the results of the present paper are underestimated daily–scale variability (due to parametrised synoptic processes) and large precipitation increase per unit warming in most regions. The latter bias, however, was partly ameliorated by accounting for land use.
  Finally, the model climatology differs very little between different ensemble members. This is true even for the vegetation and soil carbon stocks directly affected by fires.
  These issues are discussed in Sect. *Caveats*.

- *'The GlobFirM model is not any more in much use. Reading the original paper reveals why: it was a first good approach performing satisfactorily 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 improvements in the performance of GlobFirM, sufficient to make parameter estimation approaches meaningful.'*
  We agree that the GlobFIRM model is a very simplistic one. However, we stress that the main goal of the present manuscript is the presentation of the ensemble approach to simulate gross characteristics of natural fires. For this goal, the GlobFIRM model suits well. Our approach may be applied to any other scheme calculating such characteristics and even for the whole CMIP5 ensemble. In our work, the GlobFIRM model is improved by accounting of carbon release from soil during fires. In addition, we note the the complexity of the fire scheme should correspond to the overall complexity of the whole Earth system model. Our model, belonging to the class of the Earth system models of intermediate complexity, employs parametrised synoptic–scale dynamics and annual mean terrestrial carbon cycle. The model's spatial resolution is rather coarse: for instance, the whole Spain is covered by only 4 cells of our grid (but we use a mosaic approach to allow for different PFTs to co–exist in a grid cell). For our believe, the GlobFIRM model is sufficient for the Earth system model of this type. However, along with an overall development of the IAP RAS CM, we are going to replace it by a more elaborate scheme in future. These issues are discussed in Sect. *Caveats*.

- *'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.'
Upon revision, we state in Sect. 2.1 that the IAP RAS CM resolution is 4.5^o in latitude and 6^o in longitude. This is much coarser than the resolution of the GFED–3.1 data. So, the gap between measurements and the model does exist. The situation is partly improved by the usage of the mosaic approach to represent the sub–grid heterogeneity of vegetation in our model, but not completely. In the revised manuscript, an existence of such a gap is stated in Sect. 2.1.

- *'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'*
  We agree with this comment. The respective discussion is included in Sects. 4.2 and 4.3. In particular we state that the parametrised synoptic–scale processes in our model should lead to underestimation both the daily–scale and interannual variability. This might affect the results of our calibration. Its impact might be hidden for a present–day climate state, but affect projections for the 21st–23rd centuries, when climate state is markedly different from the present–day one. Our calculation of Bayesian weights lacks any information on interannual fire variability. This is a drawback, but it is consistent with the underestimation of natural variability by our Earth system model.

- *'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).'*
  We extended *Introduction* by the respective comparison. In particular, we show

that the results obtained with the CLM–3.5 model are sensitive to the choice of the parametrisation of the scheme for calculating characteristics of natural fires. However, we unable to provide any published. comparison with the GFED3 or GFED4, or the estimated global burned area corrected for the impact of small fires As we know, up to the date no relevant results are published yet. The latter is stated in Sect. 4.1.

Minor comments

- *'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.)'
  In the revised version of the manuscript it is stated in the abstract that our Earth system model of intermediate complexity includes the GlobFIRM model but extended by the scheme calculating carbon release from soil during fires. It is stated that the model is constrained by the GFED–3.1 data set.
  All other relevant information is included in Sect. 2.2. In total, we sample 7 parameters. These parameters are $m_{\mathrm{f,wood}}$, $c_{\mathrm{fuel,0}}$, and W_{e} (which are non–PFT–dependent), CC (the former k_{res}; for grasses it is permanently set equal to unity and not sampled; and all other PFTs are merged in three groups, hence, we have 3 values), and $\alpha_{\mathrm{f,s}}$ (single value, because it is sampled only for bogs/mires/fens; for all other PFTs it is permanently set equal to zero). Their ranges are listed in Table 1 of the main text. The total sample size was $K = 30$, which is approxi-

mately an order of magnitude larger then the number of the sampled parameters. No other parameters were sampled and constrained in this paper.

- *'P1444 L3 and further: parts of the text are past tense, parts present tense'*
  All sentences in the abstract are put to the present tense.

- *'L3-4 "reconstruction of external forcings": should be "by historical reconstructions", and also better to name of what variables'*
  The sentence is corrected. In addition, it is stated that we use concentrations of well–mixed greenhouse gases (CO_2, CH_4, and N_2O), sulphate aerosols (both in the troposphere and stratosphere), extent of crops and pastures, and total solar irradiance.

- *'L6: $\rightarrow$ until the year'*
  The sentence is changed.

- *'L7: what are "governing parameters"?'*
  We agree that this term may be misleading. In the revised manuscript, it is replaced by the term 'the values of parameters'.

- *'L8: most readers won't know what the GFED3.1 data contain'*
  We explicitly state that these data are for the burnt area and CO_2 release from fires.

- *' L10: which $\rightarrow$ 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.'*
  We agree that this sentence may be misleading. Upon revision, it is excluded both from the Abstract and from Sect. *Conclusions*.

- *'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.'*

  Upon revision, both terms are put in the singular.

- *'L13: $\rightarrow$ emissions to the atmosphere '*
  The preposition is changed in the abstract and further throughout the text.

- *'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.'*
  We agree with this comment. The terms 'underestimation' ('underestimated', etc.) and 'overestimatation' ('overestimated', etc) are not used for the CO_2 emissions anywhere in the revised manuscript. They are replaced by terms 'smaller than the GFED estimate' and 'larger than the GFED estimate' correspondingly.

- *'L18: $\rightarrow$ during the 21st century. 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.*
  We made our time markers more precise. In particular, we replaced 'year 2100' by the time interval 2091–2100 AD. The term '21st century' is replaced by 'from 1998–2011 to 2091–2100'. In a similar way, 'year 2300' by the time interval 2291–2300 AD. These changes are made in the abstract and in Sects. 3.2, 3.3 and *Conclusions*. The terms like 'during the 21st century' and 'during the 22nd–23rd centuries' are left only when we speak on qualitative rather than on quantitative changes.

- *'L23: Better not reverse the order of RCPs compared to above, confusing.*
  The order of the RCP scenarios is put in the way as it was for the changes from 1998–2011 to 2091–2100. This is done both in the abstract and in Sect. *Conclusions*.

- *'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.'*

We agree. Please see the second previous reply.

- *'L25-26 "all changes [. . .] mostly": this is a contradiction, either all of them, or most of them.'*
  Upon revision, we replaced 'all' by 'the simulated'.
- *'L28-29: → the increase of burnt area [. . .] is accompanied'*
  The sentence is corrected.
- *'P1445 L6: → the latter.*
  The sentence is corrected.
- *'L6-7 "substantial part [. . .] is". This is not true, very few ESMs contain a description of fires'*
  We meant that natural fires is an important biogeochemical process. The sentence is rephrased.
- *'L9: "bulk characteristics", not sure what is meant by that'*
  The phrase 'bulk characteristics' (rather awkward, we have to acknowledge), which duplicates the list of these characteristics further in brackets is removed. The latter brackets are removed as well.
- *'L11-13: The main LPJ version now contains SPITFIRE, not Glob–FirM'*
  This sentence is corrected. In the end of paragraph it is stated additionally that the SPITFIRE is a part of the LPJ now.
- *'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'*
  We agree. The paper by Pechony and Shindell (2009) is removed from the citation list.

- *'L26: What is meant by "impact-orientated". Be more precise about what the purpose of those indices is'*
  The notes of fire danger indices is not the focus of the present paper. Hence, this paragraph is removed upon revision.
- *'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.*
  We agree. Please the the response to the previous comment.
- *'P1446 L5ff.: What is missing is a more concrete discussion of the performance of those models, in particular how they compare against observations. Also, the English needs attention.'*
  We extended *Introduction* by brief notes on the published comparisons of the SPITFIRE and the CLM–3.5 with observations. The lost verb is inserted.
- *'L11: "as proxies. . .": I don't think this is the best way of putting it. Basically, I have a model and estimate parameters in an inverse way from observations.'*
  We agree that, in principle, the best way to constrain the model parameters would be to solve a relevant inverse problem by using available observations. However, this is not easily achieved in practice because to invert a numerical model is not a readily solvable task. Moreover, measurements obtained at different geographical locations and/or at different time instants, in principle, may lead to the values of the same parameter which contradict each other. In the latter case, one of two paths could be followed. The first one is to increase the complexity of a model. However, this way may be hindered by either insufficient knowledge on relevant processes or by details of the Earth system model for which a particular natural fires scheme is developed. This is valid, for instance, for the Earth system models of intermediate complexity. In this case, the second way should be followed which is to adopt a compromise in the model's performance in different regions and/or different time instants. In the present paper, we suggest an approach to

achieve such a compromise.

- *'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.'*
  In the introduction, we state clearly that our simulations are performed with an Earth system model. We rewrite the sentences about the external scenarios in order to highlight that these are the scenarios prepared to force a coupled Earth system model.

- *'P1447 output is used have is irrelevant for the presentation. Better to state the analysis will be restricted 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 concentrations.'*
  The mentioned sentence is changed according to the reviewer's suggestion. The description of the IAP RAS CM vegetation module is shortened, and the only the information related to the vegetation–fire interaction is left in the revised manuscript.

- *'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.'*
  We agree: Thonicke et al. use the fraction of individuals killed during fires. We changed this sentence accordingly. In addition we renamed k_{res} to CC (combustion completeness) in order to make its name consistent to its physical meaning.