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

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8161

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 masked out the grid cells in which the carbon release from anthropogenic fires, E_a, is larger than $5 \times 10^{-4}\,gC\,m^{-2}\,yr^{-1}$. 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 have 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.

- 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 technical description is sometimes fuzzy. The GFED dataset on carbon emissions is model output as well, this needs to be clearly stated throughout the manuscript. The GFED burned area has a separation into land cover types which is available online. For instance the croplands area specified. There is no area for the deforestation or peatland fires. But assuming that the area of deforestation and peatland fires is small compared to their emissions may be a valid approach in many regions. Natural grassland fires could be compared directly also for the burned area, as the croplands are specified and the model does not treat pastures.'*
  We have followed the suggestion made by reviewer but chose alternative strategy by excluding grid cells affected by anthropogenic fires. Please see the preface to this reply for more details.
- *'The authors should be more clear about what is the new contribution. For instance I am not aware of analysis of coupled climate carbon cycle simulations*

for the future with respect to fire. This could be highlighted better. The goal and motivation of the study should be formulated more clear.'
We list the novelties of our paper both in the Abstract and in Sect. *Conclusions*. In addition, we clarified the paragraph related to the goals of our paper.

- *'The discussion part of the paper is missing. How do the results relate to other literature. There are studies based on process models but also a comparison with statistical models could be useful to relate the results to what has been done before. For interesting it would be interesting to state, whether the variability in the ensemble is comparable to the variability that is seen between different modelling studies and approaches.'*
To compare our results with available literature, we note that our scheme to calculate the carbon release from soil is similar to that employed in the CLM–3.5. We have also compare our simulations with the off–line simulations with this model (see the last paragraph in Sect. 3.3). Unfortunately, we have no answers to other questions raised by the reviewer, basically because, up to the date, there are no available ensemble results for other models. Our ensemble standard deviation of CO_2 emissions from natural fires is comparable to the GFED estimates for the regions listed in Table 2 of the main text, but we feel that it is just a coincidence.

Specific and technical comments

- *p. 1444, l. 22: 'which other RCPs? all of them?'*
Upon revision this sentence is removed because all its information is duplicated and extended in the next sentence.
- *p. 1445, l. 12: 'currently SPITFIRE is part of LPJ (see the Thonicke et al. 2010 reference you cite later on)'*

This sentence is corrected. In the end of paragraph, it is stated additionally that the SPITFIRE is a part of the LPJ now.

- *p. 1448, l. 24: eq. 1: why do the carbon emissions of the soil not depend on the soil carbon stock, but the carbon emissions of the vegetation? any evaluation or references that can support your model?*
It is assumed in Eq. (1) that soil carbon is consumed during fires only if the soil fuel stock is sufficiently large. Based on the latter, we assume that fire development is limited by environmental factors rather than by the available soil fuel stock. For this reason, why c_s does not enter the right hand side of Eq. (1). Further, δc_v heuristically represents an overall severity of fires in peatland regions. This severity, in turn, represents an ability of fires to penetrate into the soil. Some observational support for such a representation is given by Mack et al. (2011). In the latter paper, carbon release from peat during fire is related to the consumed amount of vegetation carbon.
- *p. 1448 l. 26: reference for your threshold?*
The precise value of this threshold $c_{\mathsf{s},0}$ should depend either on the resolution of input data or on the size of the model grid cell. The ISLSCP II data for soil carbon stock interpolated on the IAP RAS CM grid resolution was reported by (Eliseev and Mokhov, 2011, their Fig. 7b). The threshold value 10 $\mathrm{kgC\,m^{-2}}$ was chosen by a visual inspection of those data. The value of $c_{\mathsf{s},0}$ should be larger at a finer resolution. For instance, the high–resolution (nominally, 150 m) data for the West Siberian Lowlands (Sheng et al., 2004) support the threshold value as large as 30 $\mathrm{kgC\,m^{-2}}$.
- *p. 1449, eq. 3: remove either ev and es or δcv and δcs if there is no difference between them*
The former Eq. (3) is removed from the manuscript, and Eq. (2) is rewritten as

$$e = \delta c_\mathsf{v} + \delta c_\mathsf{s}.$$

- *p. 1454, l. 21:why 0.01?*
  This value was chosen as an illustration. This sentence is removed upon revision because we agree that it is not necessary.
- *p. 1455, l. 5–11: this fits better into the methods part*
  This paragraph is moved to Sect. 2.3.
- *p. 1456, l. 3: BA could be a more intuitive short name for area burnt compared to S*
  The notation is changed (see also the preface to this reply).
- *p. 1461, l. 25-26: I don't understand*
  We meant that the change of the CO_2 emissions from natural fires in this region is a product of two counter–acting effects: an increase of the burnt area due to decreased soil moisture content and a decrease of the vegetation carbon stock. Upon revision, this sentence is clarified.
- *p. 1463, l. 22: which global characteristics of the carbon cycle are you referring to?*
  We meant the atmospheric carbon dioxide content. The sentence is clarified.
- *p. 1446, l. 7: "they are just tuned" instead of "they just tuned"*
  The sentence is corrected.
- *p. 1447, l. 10-11: I don't understand*
  Upon revision, this paragraph is changed, and the sentence, which is mentioned by the reviewer, is removed.
- *p. 1455, l. 3: add "are" following "which"*
  The phrase is corrected.
- *p. 1461, l. 23: typo*
  The typo is corrected.

- *figure S1: what is the x–axis, why does it go from 0 to 55 to 10?*
  The x–axis represents ensemble member. The wrong labels ('55' instead of '5') are corrected.