

## ***Interactive comment on “An ensemble approach to simulate CO<sub>2</sub> emissions from natural fires” by A. V. Eliseev et al.***

### **Anonymous Referee #2**

Received and published: 18 March 2014

Eliseev et al. perform ensemble simulations with a global coupled carbon cycle climate model for the years 850 to 2300 and analyse the fire carbon emissions. The ensemble approach is an interesting technique to estimate the uncertainty in the model and to propagate the uncertainty to the model projections.

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.

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The authors perform a CMIP5 type model simulation. How does the simulation compare to other CMIP5 model. some numbers on climate and carbon cycle variables could be useful here.

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.

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.

Specific comments:

p. 1444, l. 22: which other RCPs? all of them?

p. 1445, l. 12: currently SPITFIRE is part of LPJ (see the Thonicke et al. 2010 reference you cite later on)

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?

p. 1448 l. 26: reference for your threshold?

p. 1449, eq.3: remove either  $e_v$  and  $e_s$  or  $\delta_{cv}$  and  $\delta_{cs}$  if there is no difference between them.

p. 1454, l. 21: why 0.01?

p. 1455, l. 5-11: this fits better into the methods part.

p. 1456, l. 3: BA could be a more intuitive short name for area burnt compared to S.

p. 1461, l. 25-26: I don't understand

p. 1463, l. 22: which global characteristics of the carbon cycle are you referring to?

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Technical comments:

p. 1446, l. 7: "they are just tuned" instead of "they just tuned"

p. 1447, l. 10-11: I don't understand

p. 1455, l. 3: add "are" following which

p. 1461, l. 23: typo

figureS1: what is the x-axis, why does it go from 0 to 55 to 10?

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Interactive comment on Biogeosciences Discuss., 11, 1443, 2014.

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