

Interactive comment on “The status and challenge of global fire modelling” by S. Hantson et al.

Anonymous Referee #1

Received and published: 2 February 2016

General comments:

The paper is suitable for BG, but needs a revision: there are obvious omissions in the paper.

Specific comments:

1. Does the paper address relevant scientific questions within the scope of BG? Yes.
2. Does the paper present novel concepts, ideas, tools, or data? Yes, the paper presents a novel model inter-comparison project.
3. Are substantial conclusions reached? There is no such section as ‘Conclusions’ in the paper. Therefore, it is not clear whether this paper is aimed to reach some substantial conclusions. It seems that it is focused on “problem statement”. “There is a problem”, could be interpreted as a substantial conclusion if the problem is formulated clearly and supported by an analysis of the “state-of-the-art”. However, authors should

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do some effort in this direction. In the present version of the paper, I did not find something that could be considered as a “substantial conclusion”, although it is quite obvious for me that the paper may lead to substantial conclusions.

4. Are the scientific methods and assumptions valid and clearly outlined? The scientific methods and assumptions are not clearly outlined, and this makes it difficult to judge about their validity. The sections “4. Objective and organization of FireMIP” and “5. Benchmarking and evaluation in FireMIP” are very raw. I would recommend to add a flowchart explaining the conceptual framework of the project objective and organization, and a flowchart explaining the procedure for model benchmarking and evaluation. I also think that authors should address the following questions in the text:

A) Which of the fire modelling groups are eligible to participate in the project? B) Could any group submit its model for benchmarking and evaluation? C) Were all fire modelling groups invited to participate in the project? D) Is the proposed procedure of model benchmarking and evaluation new and original? E) Are the proposed metrics for model benchmarking and evaluation new and original?

5. Are the results sufficient to support the interpretations and conclusions? Yes.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Yes.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? I am not sure. It is not clear whether the authors propose new and original procedure and metrics for model benchmarking and evaluation or not.

8. Does the title clearly reflect the contents of the paper? Yes.

9. Does the abstract provide a concise and complete summary? Yes.

10. Is the overall presentation well structured and clear? No. The models are reviewed in somewhat chaotic manner. Some models are not mentioned at all.

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11. Is the language fluent and precise? Yes.

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes.

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? The sections 4 and 5 need major revision.

14. Are the number and quality of references appropriate? No. There are no references to the papers of some active fire modelling groups. For example, I did not find references to the papers recently published by Eliseev:

Eliseev, A.V., I.I. Mokhov, and A.V. Chernokulsky, 2014: An ensemble approach to simulate CO₂ emissions from natural fires. *Biogeosciences*, 11 (12), 3205-3223, doi 10.5194/bg-11-3205-2014

Eliseev, A.V., I.I. Mokhov, and A.V. Chernokulsky, 2014: Influence of ground and peat fires on CO₂ emissions into the atmosphere. *Doklady Earth Sci.*, v. 459, no. 2, p. 1565-1569, doi 10.1134/S1028334X14120034

Moreover, there is no one reference on the lines 136-157 where authors review physical controls of fires. This looks strange.

Interactive comment on *Biogeosciences Discuss.*, doi:10.5194/bg-2016-17, 2016.