

Interactive comment on “HESFIRE: an explicit fire model for projections in the coupled Human–Earth System” by Y. Le Page et al.

Anonymous Referee #1

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This paper describes and evaluates the development and optimization of a global fire model. A key improvement compared to current approaches is a consistent approach to model multi-day fires. The paper also contributes to the understanding of fire drivers. Therefore this work can be considered important and is suitable for publication. The methods are sound and the paper is well written. The second part of the discussion give to much detail about future plans of the authors and reads more like a research proposal from then on. I suggest to remove the parts that simply describe future plans of the authors, while general possibilities of model applications may be mentioned. One rather weak point is that the authors suggest the model to be used for future projections. While the model is evaluated in space and for the interannual variability, the performance of the model on longer time scales especially with respect to the human influence is unclear. The parameters for human ignition and suppression are

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probably strongly constrained by the current spatial patterns, but may have a strong influence on simulations for the next hundred years.

Specific comments:

Title: why earth system? the model only interacts until the level of a vegetation model, no atmospheric or biogeophysical influences are discussed.

p. 10788, l. 5: what means normalized from 30-80%? are they normalized between 0 and 1 and below (above) the given thresholds the values are 0 (1)?

p. 10791, l. 25: As far as I am aware this is also a development of the optimization metric, other studies used least squares approaches. You might add a line to highlight this modification of the optimization metric and why you chose to define the metric by using classes not the actual values.

p.10793, l.1,2: is it reasonable to change the parameters to +50% and -50%? another approach could be to increase the parameter value according to its variability, e.g. +/- its standard deviation/uncertainty derived by the optimization procedure.

p.10792, l. 7-10: please be more precise on the criteria of the gridpoints used in the optimization, a matrix table could be useful here to show the different combinations of criteria represented in the choice of datapoints used in the optimization. Which biomes, which land use densities... were represented?

p.10795, l. 11: ignition-saturated means to me that more ignitions don't lead to an increase in fire activity. I think here, it just means that more anthropogenic activity (land use) does not result in more ignitions. Moreover, do you really think that ignitions and suppression can be separated well in your approach?

p. 10796, l. 13,14: probably due to the simple representation of fuel.

p.10798, l. 17: do integrated assessment models also provide GDP? figure 1. is cut off

p.10801 l. 15 ff: In my opinion the description of your future plans should not be

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described here. The discussion should deal with the results presented here.

p.10801, l. 26/7: same

p. 10802, l. 6 ff: same

p. 10803 l. 2: I am surprised that here the interest in collaborations is expressed, I think that this is inappropriate here.

p. 10803, l. 12, whole paragraph: This whole paragraph sounds like a research proposal, I don't see the benefit of this discussion with respect to your results.

Figure 7: the IAV correlation is significant? The correlation is based on the annual values?

Interactive comment on Biogeosciences Discuss., 11, 10779, 2014.

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