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Interactive comment on "A fire model with distinct crop, pasture, and non-agricultural burning: Use of new data and a model-fitting algorithm for FINALv1" *by* Sam S. Rabin et al.

Anonymous Referee #1

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In this paper, the authors described the Fire Including Natural & Agricultural Lands model (FINAL), a fire module for the LM3 land model. One of the most important features of this model is the explicit separation between non-agricultural, pasture and cropland fires : this is a very important feature since fire seasonality is expected to differ significantly between these different fire category.

In the FINAL model, the fraction of cropland and pasture fires is directly estimated from the Rabin et al. 'unpacked' dataset, and the modelling of non-agricultural fires is based on the CLM fire module. This modul is clearly described in the article, along with the modifications done by the authors to adapt it the the LM3 land model. The parameters

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of the model, which are expected to be different from those of the CLM module, are determined with an optimization method : this optimization relies on the Levenberg-Marquardt algorithm, which minimizes the sum of squared errors between the model and the GFED3s data, for a selected sample of grid cells. The authors took care to ensure that all functions involved in the models were continuously differentiable, which is mandatory to perform such an optimization.

Because non-natural fires are directly estimated from burned area data, simulated non-natural burned area is very close to the results from Rabin et al. 2015. The results are not as good for non-natural fires, probably resulting from the strong limitation induced by soil moisture after the optimization of parameters. The results of the model, along with its limitations, are well-discussed in the article, and the authors proposed an interesting critical discussion about the optimization process. However, I still have some questions concerning the implementation of the optimization method, which need some clarifications. They are listed in the Specific Comments part.

Specific Comments :

1) You stopped the optimization after 11 steps, and said (lines 21-22, page 14) : 'By the eleventh iteration, it did not seem that allowing iterations to continue would result in much improved sums of squared errors'. I have some major concerns here. First, I think you should put the SSE subplot on Figure 4 in log scale, since the range is driven by the SSE values during the first steps and does not allow to clearly see what's happening after the fourth step. It is very common that during an optimization process, the function to minimize drops very quickly during the first steps, and then need some time to finally converge. Second, looking at the evolution of the other parameters, it is not so clear that the algorithm converged : the parameters vary more when the difference of squared errors Delta_SSE between two steps vary less. I would really like to see 4-5 supplementary steps, to see if the parameters reach a state of stability, and to ensure that the SSE is really stable after this number of step.

2) If I understand it correctly, your optimization is only done on 241 grid cells, as described in Appendix A. I think that the last paragraph of the Appendix should be included as a section 2.6.3., since it is very important for the reader to know this as he reads the methodology section, and not when he reached the discussion part : before reaching it, I thought you did the optimization on all the grid cells. I suppose this allows you to run the model much faster, but you said in the discussion : 'The deeply model-interactive setup used here – where the complete model of soil, vegetation, and fire was forced with climatic data for 19 model years – took around two hours per iteration with all gridcells being run in parallel'. But if you run the model on a limited number of cells, shouldn't it be faster ? If it is not possible to run the model only with a fixed selection of cells, then why don't you compute the SSE on a much higher number of cells ? I think you should give a clearer explanation on this choice in the article.

3) I think an important consistency check would be to specifically look at the squared errors of these selected indivual cells after the minimization process (as a second map on figure 2 for example, and, even better if you can, an histogram of the difference of SSE before/after the minimization). This will also allow to clearly check if the optimization process is mainly driven by savannas/grassland, where a small change of parameters will have huge effect on the modeled burned area, hence on the SSE in this cell (as you said in the discussion part).

4) Section 2.6.2 : not all the parameters of the model are involved in the minimization process. If it seems clear why you have chosen to optimized the parameters Beta_Ia, Beta_ROS and Beta_ROS, it is not the case for the remaining parameters. I think the authors should explicit why the have choosen these parameters (the ones driven by soil moisture), and not, for example, those driven by the temperature.

Technical comments :

1) One of the strength of the FINAL model comes from the separation of agricultural/pasture and natural fires. I think it should be more emphasized in the article. To

do so, I suggest to move the discussion about the 'unpacked' input data in section 3.2 to section 2.3. I also think that it is necessary to explain clearly what is the Fk fraction (equation 1 from Rabin et al. 2015 could appear in the article), since it is necessary to understand how the fire types are separated in Rabin et al. 2015.

2) If you decide to use capital letters to reference the figure, you should also use capital letters when you mention it in the caption or in the text. Moreover, it would be clearer if the letters were close to the titles of the subfigures.

3) Concerning the colorbar on the Figures 7,8 and 11 : I really think you should replace the dark grey (the color corresponding to 0.1 < BA < 0.5 for example) with a color 'yellow-ish' color, I think it hides too much the cells with low but non-negligible burned area fraction.

4) I think you can remove Figure 1. It is not really usefull, and there are already lots of figures.

5) Figure 12 : There is no map background for the month map, it should be added for the sake of homogeneity with other figures.

6) In figure 5 (which, I think, is really nice) : I didn't find the definition of f_supp, but I supposed that fPD = 1 - fsupp. If this is the case, I think you should either put f_PD as the axe label in Figure 5b, or explicitely write the relation between fPD and fsupp somewhere, for the sake of clarity.

7) In Table 3 : the final values should have the same number of digits as the initial values. You could even put the difference (or percentage of variation) between the two sets of value as a third column.

8) In general, there are lots of map in the article. I understand it is necessary to show separate maps for non-agricultural/pasture/cropland, but maybe you could, for example, remove the total map from Rabin et al. 2015, or the one from GFED3s, in all the figures. I do not have a strong opinion on this last item, I just think that it is easier

for the reader to focus on a smaller number of plots.

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/gmd-2017-77/gmd-2017-77-RC1-supplement.pdf

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