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Biogeosciences Discussions

Interactive comment on "A process-based fire parameterization of intermediate complexity in a Dynamic Global Vegetation Model" *by* F. Li et al.

Anonymous Referee #2

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I enjoyed reading the ms and I think it should be published. I also think it has a high potential for citations since it lists many aspects of the fire model which might be of interest in further studies.

Though I am generally very positive, I have some reservations against the way the results are presented. I also have some reservation against the way the model is build and how some of the parameter are estimated. I assume that for some of the parameter there are no better values available right now, but I think their range of uncertainty should at least be discussed.

Some of my main points are:

The results are evaluated against GFED 3, as if this would be the truth. However GFED

3 has an approach which also involves a DGVM (though additionally driven by NDVI) to estimate fire emissions. So except for the burned area part, the models are quite similar. This becomes especially visible when comparing the emissions. The emission factors are similar and the vegetation will also be relatively similar, so if the burned area is comparable the emissions are bound to be close. I think this should be noticed and not claimed to be a novel feature of the model. In that way some of the results can be shortened with respect to emissions. Also the figure 9 which is mentioned in the results section as 'Mod-new reproduces the observed decreasing ...." Referring to GFED as observation of the different burned area fraction of burned area for grasses, trees and shrubs. GFED uses CASA which is a DGVM. So there is no observation here but also a modeling. Moreover I am unclear what a "burned area fraction of grasses " is. I could see what a proportion of trace gases caused by burning trees and grasses is, but trees, grasses and shrubs are growing in the same area. So if this burns, who is contributing what? If this is meant to relate to the fractional cover of the area that burns, than this should be stated more explicitly. However, since here only the results of one model are reproduced by a different one I think this part can be removed.

I also strongly criticize the way that fire area modeling is approached in general, by assuming that a number of ignitions (depending on the population and the lighting) can spread and will be extinct (also depending on the population). This way of conceptualizing fire is very much typical for temperate and boreal zones. However, the majority of burned area globally is in Africa. There fire is used as a landscape tool. Here 99% of the fires are ignited by humans (Saarnak, 2001). These humans choose a suitable time which is different from times of naturally occurring fires since they want to be able to control the fire and ignite and extinct the fire according to the area that they aim to burn. The whole assumption of a free spread and an extinction effort based on population density is conceptually misleading here. In my opinion the concept of burned area simulation should be dichotomous depending on whether the fire happens in the temperate and boreal or the tropical zone. Given this strong difference in the concept I am very surprised to see comparable burned areas between GFED and this model. I would

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like to see the burned areas compared against GFED in a continental comparison for the different zones, looking at figure 8 it looks as if the tropics are underestimated and the boreal zones are overestimated in burned area. I would like to have these values quantified. Also looking at figure 7: to compare 4 values one does not need a diagram! However, if the diagram would separate into different continents (or regions) and not only the mean, but the standard deviation (or any measure of spread) would be given, this diagram could be useful. The total results are strongly dependent on the model parameter (or at least some of them). Most parameters are mentioned somewhere in the text. Though I think some of them are different to what I would expect (and have read, like the combustion completeness of grasses) I think it is very confusing how they are presented. A table with the parameter and the proper reference would help here. (many of the parameter have no reference mentioned in the text, but the authors might have one).

As for the Conclusions, the authors mention three points that are worthwhile improving. They already realise that the assumption that the behavior of igniting a fire does differ between different cultures, which however is treated as constant in the model. And my major criticism is partly mentioned that there are different kinds of fires like agricultural fires which are not covered. I think this sections should be largely expanded. For example all fires are considered as a kind of an accident, however in the real world at least half of the burned area is consciously ignited to treat the land. This should be reflected. Also the used data sets should be reflected upon. From table 5 I can not see what variable comes from which citation. NCEP for example has a resolution of 2.5 degree. Given that many of the forcing data variables have quite some variation within such large grid cells I would expect some effects of this. For all data the resolution should be stated. Also the fact that the LIS/OTD data is interpolated in time and might lead to a high overestimation of ignition sources before the rain season starts lets me wonder how the results are so close to the GFED burned area. I would suggest the authors quantify the amount of lightning versus human ignited fires in a map, since this would allow to evaluate the results very well. Given that this paper aims to simulate

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global fires, I also find it problematic that the fuel availability is based on correlations of 24! cells in the United States, and without any consideration of the fuel type. There should at least be a discussion as to why this should be representative for the globe.

As a conclusion I think this paper should be published, but the authors should be more clear in stating that the simulated emissions are compared to another model which is relatively similar. Or the emission part could be taken away. As for the burned area this model presents a very good approximation of the remotely sensed data. However a more critical discussion of the results would be appreciated. One example would be the different causes of fires (and it should also be fire sizes!) in boreal areas versus tropics.

Interactive comment on Biogeosciences Discuss., 9, 3233, 2012.