

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

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### **1 Major comments**

This is an overall reasonably well written manuscript. I agree with comments of the well-written review of Referee 2 so I will try not to repeat them. While, the manuscript should be published the authors must clarify and focus on the real advancements made – and that is an improvement in simulated burnt area and not simulated emissions (since GFED3 used simulated biomass anyway). Second, the lack of treatment of both agricultural fires and peatland fires must be discussed a bit more including their impact on simulated emissions.

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Other than these major comments, I have comments on how the authors interpret the Arora and Boer (2005) paper and minor comments on the manuscript itself.

## 2 Interpretation of the Arora and Boer (2005) paper

The authors criticize Arora and Boer (2005) (here after AB2005) on the basis that it will yield a maximum of 30 fire counts in a month but that in tropical savannas the actual fire count can be greater than this and hence the underestimation in burned area in this region. The AB2005 paper does not mention fire count and it was never meant to simulate fire count, so it appears that the AB2005 paper is being over interpreted by the authors. The lower burned area in tropical savannas in using the AB2005 approach as in Kloster et al. (2010) might be attributable to lower fire spread rate, for instance. An increase in fire spread rate will increase area burnt more in regions with high probability of fire. I suggest that the authors say that the AB2005 paper and its implementation by Kloster et al. (2010) does not take into account fire counts but please do not over interpret AB2005 to calculate fire counts.

On page 3237 the sentence, “In addition, CTEMFIRE does not include estimation of trace gas and aerosol emissions due to biomass burning, which may lead to incorrect estimation of greenhouse gas and aerosol forcing of climate in global change projections using ESMS” implies a structural/design issue with CTEM’s fire module. Estimation of non-CO<sub>2</sub> trace gases and aerosols is simply obtained by multiplication with corresponding emissions factors so this is an implementation decision and not a model structural or design issue.

Other than this, I welcome the minor inconsistencies discovered by authors in description of the elliptical area burnt parameterization.

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### 3 Minor comments

Page 3238, lines 10-13. Please consider revising this sentence as follows – “In CTEM-FIRE (Arora and Boer, 2005), daily burned area is extrapolated to a grid cell based on calculated fire occurrence probability ( $\leq 1.0$ ) and a representative area of 1000 km<sup>2</sup>”

It took me a while to realize that  $f_m$  (equation 7) is different from  $F_m$  (as used in equation 14). It might be useful to use a different notation here. In addition, it might be useful to mention that the root zone wetness index ( $\beta$ ) is based on wilting and field capacity soil moisture contents in AB2005.

It is unclear what  $10^{-6}$  does in equation 18.

In equation 19, shouldn't the units of fire carbon emissions be (gC/time step) rather than just gC.

On page 3247 two different ways of treating fire-related mortality are described (equations 20 and 21, respectively). It wasn't clear to me which approach was used by the authors.

Emission factors mentioned in Table 3 have no units. Please include units as mentioned in the text (g species/ Kg dry matter). Also, shouldn't the species emissions calculated in equation (22) be g/time step rather than just g.

Page 3250, lines 10-14 about the CLM4 surface data set are unclear. If the purpose of these lines is just to mention that the simulated vegetation distribution is reasonable then just mention this with a reference – why talk about CLM4's vegetation data set.

Section 4 or Table 5 does not mention the spatial and temporal resolution of the forcing data. Were forcing data spatially and/or temporally interpolated?

The term “Mod-new” is mentioned for the first time in Section 5. Please introduce this term before using it.

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Please reword the sentence “It is better than the Glob-FIRM and the old fire module” on page 3253.

Please clarify the sentences at the end of the manuscript (page 3257, lines 2-5). To what extent is this important? Since CLM-DGVM updates biomass at the end of the year it likely has high biomass during the dry fire season (than if the seasonality were taken into account) so the resulting emissions are likely overestimated. Can the authors comment on this?

In Appendix B, do lines 15-16 imply that  $f_{\theta} < 0.05$  when  $\theta > \theta_e$ . It is hard to follow this sentence. Please consider rewording.

In Appendix B, the sentence “First, a sample for the parameter calibration is selected from the above three global datasets” is unclear. Please reword.

## 4 References

Arora, V. K. and Boer, G. J.: Fire as an interactive component of dynamic vegetation models, *J. Geophys. Res.*, 110, G02008, doi:10.1029/2005JG000042, 2005.

Kloster, S., Mahowald, N. M., Randerson, J. T., Thornton, P. E., Hoffman, F. M., Levis, S., Lawrence, P. J., Feddes, J. J., Oleson, K. W., and Lawrence, D. M.: Fire dynamics during the 20th century simulated by the Community Land Model, *Biogeosciences*, 7, 1877–1902, 15 doi:10.5194/bg-7-1877-2010, 2010.

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

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