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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Studying the impact of biomass burning aerosol radiative and climate effects on the Amazon rainforest productivity with an Earth System Model" *by* Florent F. Malavelle et al.

Anonymous Referee #1

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This study uses a coupled modelling framework based on the HadGEM2-ES Earth System model to quantify the effect of Amazon region biomass burning aerosol on the terrestrial carbon cycle through changes in direct and diffuse surface radiation and feedback from climate adjustments. Assessing the ability of Earth System models to fully simulate such effects is very important and this study is a timely and welcome addition to existing work in this area - especially as other coupled model studies investigating diffuse fertilisation effects are based on a different model. The manuscript has a very good structure and is generally well written.

I have a few comments and recommendations that I would like to see addressed before publication.

Specific comments:

1. It is not clear why the authors chose to use the CLASSIC aerosol scheme to represent aerosols in their model, instead of the modal aerosol microphysics scheme (GLOMAP-mode) which has already been implemented, tested and widely used in HadGEM models. As shown in Bellouin et al. (2013), GLOMAP-mode provides a better agreement with aerosol observations and re-analysis products than the CLASSIC scheme. The same study also found substantial differences in aerosol direct radiative forcing estimates between the two schemes, i.e. -0.49 Wm-2 for GLOMAP-mode vs. -0.18 Wm-2 for CLASSIC; such differences are likely to have an important impact on diffuse fertilisation effect estimates. If it is unfeasible to repeat simulations also using the modal scheme, could the authors comment on the potential uncertainty associated with the use of the CLASSIC scheme and maybe even estimate this uncertainty?

2. I might have misread Section 2.2, but according to the first paragraph, year 2000 fire emissions have been used in all simulations. Record low fires have been recorded throughout the Amazon region during year 2000, see e.g. Table 7 in van der Werf et al. (2010): 137 Tg C yr-1 of Southern Hemisphere South American fire emissions, i.e. $\sim$50% of the 271 Tg C yr-1 1997-2009 mean. Please clarify exactly what fire emissions have been used in your simulations (also stating the regional amounts of fire emission). If it is only year 2000, than a detailed discussion is needed on interannual variability and the extent to which the low fires from year 2000 are representative for present day.

3. To what extent is the order of switching off the 3 mechanisms important (Table 3 showing the experimental design)? What is the magnitude of the 3 effects if you estimate them in a different way: for example on page 14, retrieving delta_NPP_clim by contrasting NPPˆBBAx1_clim.aer,Tot.aer,FD.aer with NPPˆBBAx0_clim.aer,Tot.aer,FD.aer?

3a. Regarding your assumption on page 12, line 25, namely “neglecting the interdependency between the three terms”, to what extent is this true? Can you confirm with your simulations that the overall effect is the sum of the 3 individual effects?

4. Page 4, lines 12-13 & lines 30-31 and paragraph starting on p 22, l 27: why was the ozone damage effect not included in your simulations? As you mention on page 5 line 2, the Pacifico et al. (2015) study used a similar modelling framework (same model), so one would expect that including the effect should be relatively straight-forward?

Technical corrections:

- p 1, l 23-28: please clearly state the time period (year) the estimated values correspond to.

- p 3, l 21: “did not accounted” -> “did not account”.

- p 7, l 11-12: please revise sentence – an “and” is probably missing.

- p8, l 24: what is the reason behind applying the “multiplication factor” only for South American sources?

- p11, eq(1): please explicitly state what you mean by “dL”.

- p11, l 26: “analyse” -> “analysis”.

- p12, l 24-25: I think deltas are missing, when you want to define “delta f_d”, “delta TotPAR” and “delta clim”.

- p15, l 12-14: saying that the revised configuration provides a better estimate of global GPP is probably too strong a statement considering the actual values. Also, since your study is restricted to the Amazon region, it would be good to say something about how the simulated GPP/NPP values over this region compare to FLUXCOM, Shao et al (2013) and MODIS. Figure 1 a-d suggests an over-prediction of the model (despite an under-prediction of global GPP)?

- p 15, l 27-30: already described within the figure caption, so no need to also describe within the text what each line represents.

- p19, l 21: a bit confusing, as in the Fig. 8 caption you describe the line as “grey” rather than “black”. Again, best to describe figure in the caption and only discuss in in the text.

- p 20, l 17: you might have reversed ABS_OP and DIFF_OP (less/more scattering).

- p20, l 23-27: please give exact values here, as it’s not clear what you mean by “significant change” so it’s best to have actual values here.

- p20, l 24 and p 21, l 1: “do” -> “does”.

- p23, l 18: “quantity” -> “quantify”.

- p37, Fig 1 caption: looks like a “(reference)” for the EDMI project is missing.

- p44, Fig 8 caption refers to some missing dashed lines.

References:

Bellouin, N., et al. (2013), Impact of the modal aerosol scheme GLOMAP-mode on aerosol forcing in the Hadley Centre Global Environmental Model, Atmos. Chem. Phys., 13, 3027–3044.

van der Werf, G.R., et al. (2010), Global fire emissions and the contribution of deforestation, savanna, forest, agricultural, and peat fires (1997–2009), Atmos. Chem. Phys., 10, 11707–11735.

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