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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Biomass burning aerosol over the Amazon: analysis of aircraft, surface and satellite observations using a global aerosol model" *by* Carly L. Reddington et al.

Anonymous Referee #3

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The manuscript by Reddington et al. presents an analysis of biomass burning aerosol abundances over the Amazon during the SAMBBA campaign period using a widely used global aerosol model and a collection of observations in order to assess the performance of the model, but also to provide insight into the processes that contribute to common model biases (primarily the underestimation of biomass burning aerosols). Different state-of-the-art emissions datasets are used for driving the simulations so as to explore the sensitivity of model performance to emissions. A discussion of other potential sources of error is also included. The study is a useful contribution to our understanding of why biomass burning aerosols are currently not well captured in modelling. The manuscript is clearly written and certainly within the remit of Atmospheric

Chemistry and Physics. I find the results worthy of publication, following only some minor improvements that I describe below.

SPECIFIC COMMENTS:

Page 2, Lines 27-33: Worth mentioning here which is the wet and which is the dry season, for non-expert readers. Also, in the last sentence of this paragraph it seems that the brackets need to close.

Page 4, Line 1: "using monthly mean 3-D fields at 6-hourly intervals" – what does this mean?

Page 4, Line 5: What about isoprene?

Page 4, Line 7: "Size-resolved emissions of mineral dust are prescribed from daily varying emissions fluxes" – Not clear what this means. Are they dependent on the model's meteorology?

Page 4, Lines 21-25: It is worth giving some additional (brief) information on the fire emissions datasets, e.g. on how they are produced (e.g. based on burnt area or fire radiatve power etc.). Also, any ideas from the literature on why they differ the way they do over different parts of the Amazon (as discussed in the subsequent paragraphs)?

Page 7, Line 13: I realised after finishing reading Section 2 that the start and end date of the simulation as well as the spin-up period have not been mentioned.

Page 7, Lines 22-23: I wonder what it is that made all of them miss the fire emissions just for that part of the season and not for the rest of it. Is it not possible that this could be due to the atmospheric modelling? For example due to the meteorological conditions not being captured well?

Page 8, Line 17: "or that other sources of sulfate are overestimated in the model" – the fact that before the fire season there are no overestimations probably suggests that non-fire emissions are not responsible?

Page 8, Line 25: “and is likely due to a deeper BL over grassland vegetation in the eastern Amazon” – any explanation or reference to support this?

Page 9, Line 12: “in during” -> “during”.

Page 11, Line 7: The biases are, however, smaller when comparing with AERONET measurements. Worth mentioning, and perhaps commenting on. What is the uncertainty in observations themselves?

Sect 3.5 (general): Given that this section is a substantial fraction of the manuscript, it would have been nice to support it with some figure or table in the main text. Maybe one simple thing to do is move Table S1 to the main part of the manuscript.

Page 12, Lines 28-29: Size distribution and composition are not discussed substantially in this analysis of possible factors. Is there anything more that could be said about them? Also, what about meteorological quantities other than humidity, e.g. wind or temperature?

Page 13, Lines 18-19: “but not fully resolve the negative bias in model AOD” – please add “. . .which is of the order of XX%” to give a sense of how far the results would still be.

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