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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 #1

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This study uses a combination of modeling (with various emission inventories) and observations to explore the importance of biomass burning aerosol over the Amazon. The study does a nice job of bringing together different observations and the comparisons against various inventories are enlightening (if only to understand how very uncertain biomass burning emissions are!). There are a few areas that should be improved to enhance the robustness of the study and its utility to the community before final publication:

1. The authors discuss a number of factors that could influence their comparison with observations, but disappointingly, they don't perform any sensitivity simulations to test

these. It would be nice to see the impact of (a) injecting all fire emissions at the surface and (b) decreasing the size of emitted biomass burning aerosol in the model on their results. This would enable the authors to be conclusive about these factors; without this the discussion remains largely unsubstantiated.

2. Observational uncertainties. The authors should include some information on the uncertainties associated with the various measurement techniques in Section 2.3 and include these uncertainties in their discussion of the measurement-model comparisons. In particular, aethalometer observations are highly uncertain with significant filter loading artifacts. The authors should acknowledge this and discuss what impact it might have on their results.

3. Are there sufficient number of observations in the "eastern region" to be statistically representative? It is hard to see from Figure 2 how many flights extend over this region and it might be useful to include the number of observations per vertical bin in Figure 5. The authors should acknowledge the limited sampling here.

4. Section 3.2: The authors show that the model fails to capture the decrease in aerosol concentrations from Phase 1 to Phase 2. Did the authors explore what role, if any, meteorology might play (temperature, wind direction, precipitation)? The emission inventory may not adequately reflect changes in burn conditions.

5. Figures 7 & 8, and page 11, lines 3-11: The authors suggest that Figures 7 and 8 are consistent, but they do not appear to be so. Figure 7 shows a clear underestimate in mean MODIS AOD over the western and eastern Amazon by all of the models, by a factor of $\sim$2. Figure 8 shows that at least some of the models adequately capture (and sometimes overestimate!) the AOD observed at AERONET sites and by MODIS at these sites, with the exception of the early part of the Alta Floresta record. Therefore, the statement on page 11, line 7 "the model consistently underestimates..." is clearly false. Given the reasonable agreement between AERONET and MODIS at these sites, does this analysis suggest that the MODIS AOD observed in other regions of the Ama-

zon is biased high? The authors need to correct their conclusions and discuss this more fully.

6. Section 3.5.3: The authors make a clear case that uncertainties associated with water uptake have a large impact on simulated AOD, but appear to show that this cannot explain the discrepancy between observations and their model (if they use an alternate approach the bias gets worse). Thus, as I read this, the authors fail to come up with any explanation for why simulated AOD appears biased low when mass concentrations are captured by their model. If so, the authors should be clear that this is not resolved and modify their conclusions and abstract accordingly. If they are saying that the uncertainty associated with water uptake could increase the AOD, they should show this result.

Other Minor Comments/Corrections

1. Page 2, Line 2: It seems that the goal of this study is to quantify the impacts of biomass burning emissions, not the emissions themselves (i.e. this isn't an inverse modeling study and the authors did not present a best-estimate of emissions from fires in the region). Please re-phrase.

2. Section 2.1: The model description is missing a few items: (a) the year of meteorology, (b) a description of aerosol removal (wet & dry), and (c) production totals of biogenic SOA for the region to compare to Table 1 emissions (also: did the simulation not include isoprene SOA?? This seems like an oversight).

3. Section 2.2: The authors focus their comparison of inventories on the OC emissions. They should comment somewhere in the text on whether the differences in BC emission (shown in Figure 1) are similar.

4. Page 5, line 29 & page 6, line 9: typo: Figure 2

5. Page 6, lines 12-13: briefly describe the main features of the plume removal algorithm, and how much data was removed using this filtering.

6. Section 2.3.4: include more information on the retrieval and relevant product including appropriate references.

7. Page 7, lines 28-30: This text is confusing. Figure 4a shows maximum concentrations of 100 ug/m3 (not 30-40 ug/m3) – did the authors mean to refer to Figure 3 here? Also the ACMS+BCeq in Figure 4 is not "consistently lower" than PM2.5 in Figure 3 as it is clearly higher on Sep 14 and 22. Please correct this text.

8. Page 8, lines 1-3: might differences in measurement technique (beyond size cut-offs) also be a factor?

9. Page 8, lines 13-15: how much of simulated OA is biogenic SOA?

10. Page 8, line 14: is the NH4+NO3+Chl contribution relatively consistent in the observations or are there days where these species make a larger contribution to total measured PM2.5?

11. Page 8, lines 13-15: how well does GLOMAP capture the speciated mass concentrations? i.e. what is the R2 between simulated and observed OA and BC?

12. Figure 5, 6, 7, 8: would be more legible if the authors included a legend

13. Page 8, line 25: Is the boundary layer deeper in the eastern Amazon than western Amazon in the model? Please comment on this in the text.

14. Page 9, line 12: typo "$\sim$ 0.5 ug/m3 during Phase 2"

15. Page 9, lines 24-29: Do the differences in observed BC:OA mass concentrations indicate anything about differences in fuel type or burn conditions in the western vs. eastern Amazon?

16. Page 10, line 11: why use only "straight and level runs"? How many measurements are included in these averages?

17. Page 13, lines 9-12: Have the authors compared the highest RH? These are the

values that will disproportionately impact water uptake and aerosol growth. How well does the model capture observed RH > 90%?