

Interactive comment on “Characterising Brazilian biomass burning emissions using WRF-Chem with MOSAIC sectional aerosol” by S. Archer-Nicholls et al.

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I realize I am the last reviewer of four (I think). I don't have many criticisms in addition to what has been pointed out by previous reviewers. In general, however I like this paper, and am rapidly becoming a fan of the Journal. Often papers like these don't get a lot of citations except by the authors themselves to point to previous work. But I think these kinds of papers are increasingly important so that research is more tractable and reproducible. What is most important is to document failures as well as successes, which is in part done here. Of my own personal interest is the plume rise aspect. Here at NRL we have largely dismissed the use of such algorithms for the mainline

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aerosol runs citing inherent issues in fire FRP observability and overall predictability of the thermodynamic structure of the atmosphere. Many of our concerns are outlined in Reid et al., (2009). Bottom line is the face value and deterministic view of such things as emission and plume rise and how it relates to model outcome is fraught with great peril. But that does not mean that others should not pursue it deterministically. It is just ultimately such comprehensive “top down versus bottom up” comparisons so common in the community will likely come to a bad end. Even so, as long as the current paper does a best effort describing what is going on, I have no specific complaints. Most importantly what they did is largely well documented. Although, I do endorse previous reviewers comments. Other minor comments are below:

1) Where does navigational error fit into things like Table 1? As shown by Hyer, in Brazil attribution between forest and field is not straightforward as the fires tend to be along the tree line. 2) Can you please elaborate a bit more in regard to the derived optical properties on page 6070? Instead of just saying look at Fast et al. (2006) could you please say if BC is in an external, internal or coated sphere model? This may have ramifications later on for W_o comparisons.

3) It is a minor (and personal point) but in regards to the discussion on page 6078 on secondary organic aerosol production and all of its subsequent, Vakkari et al (2014) cited, but Reid et al., 1998 was the first to make this point I believe. . .8”)

4) I am quite keen on knowing a bit more on the comparison between the model and trmm such as in Figure 3. These are week+ comparisons. Any chance we could get several 1 day comparisons also included? I just don't pick the best. . .;) Model representation of precipitation is a big deal in aerosol modeling and inversion.

5) Figure 5. Please label on the figure which is which. It is not entirely clear from the caption. Although, I wonder if there is a calibration bias between terra and aqua here even more apparent than what can be seen in Figure 6. Maybe a scatterplot is in order?

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6) 6) Figure 10. Please show the volume distributions too. You can't hide behind a log scale. . . .

Best regards, Jeffrey S. Reid, US Naval Research Laboratory.

Hyer, E. J., and J. S. Reid (2009), Baseline uncertainties in biomass burning emission models resulting from spatial error in satellite active fire location data, *Geophys. Res. Lett.*, 36, L05802, doi:10.1029/2008GL036767. Reid J.S., P.V. Hobbs, R.J. Ferek, J.V. Martins, D.R. Blake, M.R. Dunlap, and C. Liousse (1998), Physical, chemical, and radiative characteristics of the smoke dominated regional hazes over Brazil, *J. Geophys. Res.*, 103, 32,059-32,080. Reid, J.S., E. J. Hyer, E. M. Prins, et al., (2009), Global monitoring and forecasting of biomass-burning smoke: Description and lessons from the Fire Locating and Modeling of Burning Emissions (FLAMBE) program, *J of Sel. Topics in Appl. Earth Obs. and Rem. Sens*, 2, 144-162.

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