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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modeling South America regional smoke plume: aerosol optical depth variability and shortwave surface forcing" *by* N. E. Rosário et al.

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I have reviewed "Modeling South America regional smoke plume: aerosol optical depth variability and shortwave forcing" by Rosario et al. for publication in Atmospheric Chemistry and Physics. The paper presents a study of the radiative properties of smoke aerosols over Brazil simulated during the 2002 burning season. It is generally shown that the model does a reasonable job in simulating the evolution of aerosol optical thickness (AOT) compared to AERONET observations except when the observed AOT is very high, suggesting the model is missing intense events. Surface irradiance is also computed and compared to observations, showing similar behavior. These factors

highlight deficiencies in the model that must be corrected to improve the simulation of the energy budget.

The paper needs some work.

First, it needs a thorough read for English and grammar, starting I think with the title. “Modeling the South American regional smoke plume: Aerosol optical depth variability and shortwave surface forcing” reads better than the original title. There are numerous sentence fragments that need restructuring. The clarity of the paper really suffers for this.

There isn’t really a key result that I can see in the paper, except to say that the model doesn’t do a particularly good job under high AOT events or at the Cuiaba site. I think the paper would be much improved if these deficiencies were addressed more. They suggest some of these deficiencies are related to errors in the satellite based emissions used. While I’m sure there are errors there I’m not convinced those are the sources of the error. For example, without any comparison of events to satellite observations it is not clear that the transport of the plume (here I mean local plumes) is very good. So it could be that these high AOT events are missing not due to errors in emissions but because they are local and either not adequately resolved in the model or else just misplaced in the model. Likewise I think there needs to be more explanation for what is wrong with the Cuiaba site other than that the AERONET observations are not the highest quality there. Fine, but I get a hint that it is a different kind of site at least due to the vegetation burned. How does that tie back into errors in the emissions or difficulties in modeling that particular site?

Below are some specific minor comments.

Page 17469: Please restructure the model description. I realize references are given, but some very basic things could be presented. For example, I have no idea what satellite data is used to arrive at the biomass burning emissions. Likewise I don’t understand the nature of the aerosol simulation: are you carrying the mass of smoke

ACPD

12, C6443–C6446, 2012

as a single tracer, or are you simulating the particle size distribution? You imply in section 2.1 that at one point you tried the full CARMA mechanism but that it was too costly, so it would be useful to know what aerosol mechanism you are using. Also, please provide a reference for the global model providing boundary conditions. What is the spatial resolution of the global model?

Page 17470, lines 16 - 17: In this section you seem to be describing the solution to the radiative transfer equation, and not the aerosol mechanism itself. I think the proper reference for this is Toon et al. (1989), same as used later in the text, and not the references given here.

Page 17471, line 10: For use by other models could you please include a table of the particle size distribution and refractive index parameters used in your simulations.

Page 17472, line 24: Please include a reference for the MODIS aerosol products.

Page 17473, line 3: AERONET level 2.0 data is not available for the Cuiaba. You use this as a rationale later to exclude Cuiaba from the analyses presented. Maybe that lack of post field calibration means the AERONET data is no good, but are there perhaps other reasons that this site is not useful? It's a different ecosystem (Table 1) and so the assumptions of the emission properties may be very different, for example. Or maybe there is something about the environment (terrain?) that makes modeling distributions here difficult. Please elaborate.

Page 17473, line 16: Your grid size is 35 km, but you consider a window 90 x 90 kmˆ2 centered at each AERONET site as an averaging area. Do you actually mean something that is a multiple of your grid box? And how do you arrive at this particular window size (about 3 x 3 grid boxes)?

Page 17475 - 174766: You attribute most of the discrepancy between the simulated and observed AOT to emission errors. Certainly that plays a part, but what about the role of transport? Without showing satellite data it is hard to know if the model is even

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12, C6443–C6446, 2012

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putting the plumes in the correct locations.

Page 17478, line 1: Please clarify that what you are calling the “independent empirical estimate” is what is labeled as “Observed” on Figure 8.

Page 17478, line 13: You refer to “major differences” between the two figures in Figure 9. I can tell the figures are different, but they don’t really look very different to me. What should I be seeing? Maybe you could put a box around the region where you see major differences. Or else you could add a third plot which is the difference between the two realizations.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 17465, 2012.