

Editor's comments on acp-2014-431: Biomass burning related ozone damage on vegetation over the Amazon forest: A model sensitivity study, by F. Pacifico, G.A. Folberth, S. Sitch, J.M. Haywood, P. Artaxo, and L.V. Rizzo

The manuscript has been improved substantially, and all the reviewers' points have been addressed in the revised manuscript and/or the rebuttals. Unfortunately, some fairly important information has been relegated to the Supplement, where few readers will see it. In fact, the supplementary text is very brief, and there is no reason why it should not be incorporated into the main text, while the supplementary figures can be kept as a supplement.

Specifically I would like the authors to do the following:

- 1) Move the text "Model evaluation of surface temperature..." to the main text, probably best after the "Description of..." section. Some comments about how well the seasonality of precipitation is captured would also be in order. There is a tendency for models either to capture the distribution of precip, or the seasonal cycle in the Amazon, but not both. The text can then refer to supplemental figures, labeled S1, etc.
- 2) The same applies to the section on Model evaluation of NPP.
- 3) As far as I could tell, the Model evaluation of O₃ only appears in the supplement. This is crucial information and should be prominently visible. The reader should also be told, in the main text, that the model performs much more poorly in the tropics than the ACCENT mean (Figure S4). On a positive note, the reader should be told that agreement is better in the few tropical continental profiles than the marine ones. This might also be a good opportunity to drive home the point that we really need more long-term continental ozone measurements at tower sites to remove the near-surface bias.
- 4) The response to my comment on O₃ deposition missed the point. I had asked for a comparison of deposition velocities, not deposition fluxes. The former are a characteristic of the model and are concentration independent. The text in the supplement is also very brief and does not give a proper evaluation. It is also not clear, which "Table 1" is referred to. I didn't find a Table. I expect a more detailed response to this issue.
- 5) In their response 6 to referee #1, the authors do not address the referee's question about biofuel. They should state the percentage contribution of biofuels to the fire emissions.

- 6) Referee #2, response 2: This response about the use of diurnal cycles is honest, but not very satisfying. I won't ask to rerun the model with an improved mechanism, of course, but just for your information, there are relatively easy ways to accomplish this, for example in: Konovalov, I. B., Berezin, E. V., Ciais, P., Broquet, G., Beekmann, M., Hadji-Lazaro, J., Clerbaux, C., Andreae, M. O., Kaiser, J. W., and Schulze, E.-D., Constraining CO₂ emissions from open biomass burning by satellite observations of co-emitted species: a method and its application to wildfires in Siberia: *Atmos. Chem. Phys.*, 14, 10,383–10,410, doi:10.5194/acp-14-10383-2014, 2014. Using a diurnal cycle and a better intra-monthly variation is likely to result in significant improvements. At least it did in our study.
- 7) The quality of the figures in the pdf I received is still poor, with fuzzy text and graphs. This may just be the effect of Copernicus compressing excessively. Make sure that the final graphs are clean and crisp.