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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Sensitivity of tropospheric loads and lifetimes of short lived pollutants to fire emissions" *by* N. Daskalakis et al.

Anonymous Referee #2

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This paper describes the impact of biomass burning emissions on the loads and lifetimes of atmospheric constituents. Based on different biomass burning inventories and different height distributions, sensitivities are calculated. Many plots are presented, both in the main paper and in the supplement, which systematically present the results, e.g. as fractional changes.

The paper reads smoothly and the results seem logical and valid. However, both the validation and the discussion of the results fall a bit short. After reading the paper, it remains unclear what we have learned from the paper. I think the authors should try to improve both aspects of the paper along the lines discussed below.

Validation:

The validation seems rather ad hoc. For instance, for ozone, figure 3, only two stations are shown. Why? I could imagine that some stations are selected based on the way they are impacted by biomass burning. Also, we only look at the surface. I could imagine that the height of emissions impacts ozone formation in the upper layers of the atmosphere. To investigate this, it would be instrumental to analyze also the information that is collected by ozone sondes. The same holds for CO and NO2. Some validation with satellite data (MOPITT, IASI, OMI) would certainly differentiate the different model runs and could hint towards quality differences: which emission inventory performs best, and which emission height distribution leads to the best comparison?

Additional discussion:

One aspect that is particularly interesting in the paper, is the indirect effect of the biomass burning emissions. For instance, the lifetime and burden of isoprene is impacted, because biomass burning seems to enhance the oxidizing capacity of the atmosphere. However, once the results become a bit more complicated, the discussion in the paper tends to stop. For instance, figure 6e and 6f show what happens with OH and isoprene when biomass burning emissions are omitted from the model. Over biomass burning areas this leads to reductions in OH and increases in isoprene. However, outside the biomass burning regions, OH increases, e.g. at high northern latitudes. It might be, that these differences (in %) are totally irrelevant, but this should be discussed in the paper. In general, the authors should comment on what we have learned, and also quantify better how additional measurement strategies should look like. So, also sonde, and satellite observations should be involved in the validation and discussion.

Comments on textual issues and suggestions can be found in the annotated manuscript.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/14/C7719/2014/acpd-14-C7719-2014-

supplement.pdf

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 22639, 2014.