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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.

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Section 2.1. Please include a table that shows the temporal and spatial resolutions of all emission inventories. Natural emissions such as those of isoprene are a function of meteorological variables. Biomass burning also has large temporal variations. These variations seem to have been omitted in this study. Please state what is omitted, give the reasons for doing so, and indicate potential effects on the model results.

Information on the temporal and spatial resolution of the inventories used in the present study has been added in Table 2. We have chosen to account for monthly mean emissions since not all inventories have higher temporal resolution. This is the reason we have also chosen to validate the model results comparing to monthly mean observa-

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tions. A relevant comment has been added at the end of section 3.

(2) P. 22644, Line 9. How is chemical aging of OA computed in the model? How is BC hygroscopicity computed?

For chemical aging of OA, see our reply to reviewer 1. In our model, BC emissions are by 20

(3) P. 22644, Line 24-25. Why are MEGAN estimated emissions scaled to the PEGA-SOS estimate? What are the changes?

The biogenic emission inventory used is described in Sindelarova et al. (2014). Changes have been made in the manuscript to avoid confusion.

(4) Table 2, why is AWB NMVOC emission total twice as large as the global non-AWB biomass burning emission (compare GFEDv3 to GFEDv3-ECLIPSE)? The total (mass) amount of AWB burning is much less than non-AWB burning globally.

Rephrasing and corrections have been where necessary, as explained in our replies to reviewer 1.

(5) P. 22646, Line 7. How do the assumed plume heights compared to the observations by Val Martin et al. (2010)? A figure will be very useful.

A figure would give no clear information, since the Dentener et al. (2006) vertical distribution of fire emissions that we use here, assigns a specific percentage of the emitted compounds in pre-specified layer from 0 to 6000m, while Val Martin et al. (2010) has plume retrievals from satellite (MISR) data providing mean and maximum height values. Taking into account the regions defined by Dentener et al. (2006) (3 regions for North America, Boreal, Temperate and Tropical) and the regions used in Val Martin et al. (2010) (6 areas, Boreal Forests, Boreal Shrublands, Boreal Grasslands, Temperate Forests, Non-Boreal Shrublands and Tropical Forests), we only qualitatively compare the two studies. Thus, according to Dentener et al. (2006) approach boreal fires emit higher than what MISR sees over North America, since there 50

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(6) Figure 1 does not give enough information on spatial differences of the inventories. Please add a figure.

A new figure with spatial information of BC emissions has been added in the supplementary material (new Fig S2).

(7) P. 22647, Line 9-11. This is a very good point. It would be good to provide some suggestions in the conclusions.

Suggestions are provided in the conclusions as earlier explained.

(8) Section 4.1. This is a section that I am mostly concerned. The observations used are obviously incomplete. With a few exceptions, the comparisons show that the model cannot simulate the observed OC and that the observations cannot be used to evaluate if biomass burning emissions or the assumed plume heights are correct. I would have concluded that there is essentially no observational constraint on model simulation results by looking at Figs. 2-4. I would suggest that the writing of this section focuses on if the observations are useful to evaluate model, not on how model results differ among themselves. Model result differences are clear in the sections that followed. At the end of this section, some comments on what if any observations can be used to evaluate biomass burning simulations would be useful.

In addition to the suggested changes in section 4.1 by the other reviewer, and our reply to the previous comment of the reviewer, we have rephrased parts of this section to point out that no simulation and thus no emission database stands out for its performance in reproducing the observations.

(9) P. 22651, Line 16-19. I am also very concerned with this result. It is obviously an important result, emphasized in the abstract and conclusions. I think it is necessary to provide more detailed explanation on why a relatively small change (5-10

This section has been further developed as explained in our replies to reviewer 1.

(10) P. 22653 Line 13-14. The NOx lifetime difference seems to be much larger than

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that of OH (Fig. 6e?). I would have guessed that they are on the same order. Figure 6e (new figure 7e) shows load differences and not lifetime differences, Hydroxyl radical lifetime (that we know it is extremely short) is not calculated here. Lifetimes are provided in Table 6 and differences in lifetimes in Figures 9-11 that do not include OH. Furthermore, in Figure 11 we refer to NOy lifetime as reported in the figure caption and not to NOx, as erroneously reported in the text. This is now corrected.

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