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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 #3

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The TM4-ECPL model is used to study the effects and uncertainties of biomass burning emissions. The model results provide useful guidance in understanding the chemical impacts of biomass burning. There are a number of details that need to be added before publication of this paper. I also suggest that the large uncertainties in the model results are emphasized in the abstract and conclusions.

(1) 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.

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

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

(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.

(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.

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

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

(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.

(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%) of OH (Fig.

6e) would lead to a 40% change of isoprene SOA. The formation mechanism of SOA is still very uncertain under low NOx and high isoprene conditions. It is difficult to verify this result using the observations but the model mechanism for this large change can be clearly understood. It may even be possible to point out what future observations should look for.

(10) P. 22653 Line 13-14. The NOx lifetime difference seems to be much larger than that of OH (Fig. 6e?). I would have guessed that they are on the same order.

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