Atmos. Chem. Phys. Discuss., 13, C9611–C9613, 2013
www.atmos-chem-phys-discuss.net/13/C9611/2013/

Atmospheric Chemistry and Physics Discussions
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ACPD
13, C9611–C9613, 2013

Interactive Comment

Interactive comment on "Sources contributing to background surface ozone in the US Intermountain West" *by* L. Zhang et al.

Anonymous Referee #2

Received and published: 9 December 2013

This analysis provides incremental improvements to the GEOS-Chem simulations for surface ozone in the western US for the period 2006-2008, as described previously (Zhang et al 2011). The model improvements include improved lightning emissions and a daily emission inventory for wildfires. The changes in lightning emissions appears to make a significant improvement over the earlier work, especially for the southwestern US. However overall, it is not clear if the model is improved overall, it seems like it must be. It would be helpful to state this explicitly in reference to Figure 5 (eg what is the bias and r value from the earlier work?).

As for the changes in biomass burning impacts, it is not clear whether the daily inventory results in any significant improvement. The inventory is still computed on a 1x1 degree grid, which is far too coarse to adequately represent wildfires. The emissions

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appear to ignore influence from flaming vs smoldering combustion, which can occur in the same region at the same time. The chemistry simulation appears to be quite incomplete and relies on unrealistic tricks, such as emitting PAN directly with other primary emissions. How can this accurately simulate the chemistry? It is not surprising, then, that the model fails to reproduce the observations in fire impacted regions. The large over-estimate in ozone production shown (Figure 8) likely reflects a combination of too much NOx and instantaneous dilution into a 1x1 degree grid cell. The Glacier NP case shown, clearly shows evidence of NOx titration that is not evident in the model. Yet, the authors use the model failure here, to argue that wildfires never produce ozone. Seems like an odd argument and it's directly counter to dozens of papers that show wildfire ozone production from measurements. I certainly agree that the ozone production from wildfires is complex and not fully understood. In that light, I think it would be more instructive to find cases where observations demonstrate ozone productions from wildfires and then examine the model behavior for those cases.

Overall, this paper is a useful, if incremental, improvement in our understanding of ozone. I believe the changes in the lightning analysis is the most robust and useful contribution. The improvements in the model calculated wildfire influence do not seem as robust. Other comments below:

P 25872, Line 10: The statement about CASTNET observations not showing evidence for O3 production is a bit limiting. Why don't you look at one of the dozens of papers where observations show O3 production. This statement is at odds with an extensive literature on O3 from wildfires.

25876, line 26: The 1x1 resolution for the fire emissions seems to be quite a problem, since most wildfires will be much smaller. How does this compare to typical fire size?

25881, line 20: Others have suggested that PAN is central to the ozone production. Can you describe how PAN is produced in GEOS-Chem and whether this version incorporates any changes to the PAN chemistry that I have seen discussed by E.Fischer?

25882, line: I agree that the value of 3 g/NO per kg fuel is too high. But note that it is also highly variable from fire to fire. You should discuss this variability. What would you expect for a RANGE of emission factors across the region? Certainly this is a significant limit to the model simulations.

25882, line 8: I am not convinced that a model run with such unrealistic emissions (PAN, HNO3) tells us anything about O3 chemistry. O3 production will depend critically on the HOx/NOx/ROx chemistry in the plume, so emitting these species directly seems to me to be too unrealistic to provide anything useful.

25882, line 26 and following paragraph: The hypothesis that the large scale temp-fire-O3 correlations is related to BL height is interesting and plausible, but certainly not proven. It is going to be very challenging to untangle these comingled variables. While the GEOS-Chem simulations provide some helpful clues, given the weakness with the Chemistry simulations, I don't think we can discard any of these hypotheses at this time.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 25871, 2013.