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Interactive Comment

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

Anonymous Referee #1

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This paper uses a high-resolution version of the GEOS-Chem model and data from the EPA CASTNet ozone monitors to quantify the contribution of lightning, wildfires, stratospheric intrusions, and California anthropogenic pollution on background surface ozone in the US Intermountain West. This study uses an updated estimate of lightning NOx emissions based on the National Lightning Detection Network (NLDN) and a daily estimate of wildfire emissions built from fire reports from the natinal Fire and Aviation Management WEB (FAMWEB). The authors find that their improved lightning NOx estimates corrects previous ozone overestimates over the Southwest US in summer, and that lightning results in a mean 10 ppbv enhancement of ozone in the intermountain west. They find that stratospheric intrusions are responsible for the highest ozone concentrations observed at CASTNet sites, and that while GEOS-Chem underestimates

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the impacts of stratospheric intrusions on surface ozone, this bias is predicatable. Differences in stratospheric influence between Zhang eta l. (2011) and Lin et al. (2012) are mostly due to differences in definition of "stratospheric influence." Finally, the authors find that while GEOS-Chem predicts large enhancements of ozone from wildfires, the CASTNet data does not show any corresponding increase. The authors suggest that previous correlations between CASTNet ozone and fire occurance are due to the correlati0on of both with surface temperature, rather than a causal relationship.

This is a well-written paper on an important topic, the methods used are reasonable, and the conclusions are generally well-supported. The new techniques used for lightning NOx emissions clearly improve model performance at selected sites. I have a few questions about the methods and results, as detailed below, but nothing serious enough to prevent publication. I think the paper should be accepted following minor revisions to address the issues listed below.

Comments:

L19, P25876: On my first read through, it wasn't clear where the reduction in NOx yield per flash came from, but I'm now assuming that is due to the change in the location of the tropics/extratropics boundary? If so, please make that clearer in the text.

L8-9, P25877: I'm not sure this is correct. I think GFED2 only gives the total carbon burned/emitted, and then emission factors for specific trace gases and particle types (CO, NOx, VOCs, OC, BC, etc.) are taken from literature reviews like Andreae & Merlet (2001) or Akagi et al. (2011). What was the EF source for this study?

L12-14, P25877: Is this because GFED2 is missing small fires? Is there evidence from Yue et al. (2013) or elsewhere that this daily inventory is more accurate than GFED2 for the US from 2006-2008?

L21-25, P25877: Did Barrett et al. (2012) use the same meteorology and other relevant model settings as in this study?

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L18-20, P25879: Is this result about summer exceedences being due to regional anthropogenic pollution discussed anywhere else in the paper? I know the focus is on background ozone, but still it seems odd to have this conclusion here without presenting evidence to back it up. I'd expand this discussion, maybe even include a figure showing the correlation with anthropogenic CO.

L20, P25880: I'd like to see more explanation of this discrepancy between GEOS-Chem and CMAQ. Why does GEOS-Chem show a larger lightning influence? How does your approach for lightning NOx emissions differ from that of Kaynak et al. (2008)? Do you have evidence that the GEOS-Chem result is more accurate than the CMAQ one?

L5-7, P25881: Have you examined whether the discrepancy at these sites could be due to a transport error in the model, so that the modeled smoke is hitting the wrong receptors? Do you have other tracers measured at or near these sites (like CO, HCN, OC, etc.) that would show that wildfire smoke was present at the times the model predicted, but still there was no impact on surface O3? Also, what is the size of the excess O3 to excess CO ratio in the modeled smoke plumes (if available)?

L10-12, P25881: Why do you average the OC and O3 results over the domain? If fires are depleting O3 near the fire (through NO titration) and increasing it downwind, wouldn't your domain averaging simply wash away that signal?

Also, the locations of the IMPROVE and CASTNET sites are not always the same, and IMPROVE has much larger coverage according to Fig. 4. Could this influence your results? What does the OC correlation look like if you only include the IMPROVE sites nearest to the CASTNet sites?

L2-5, P25883: I am confused at how Figure 10 relates to Figure 9. In Figure 9 we have no correlation between domain-average ozone and carbon burned in the (preceeding?) five days for the summers of 2006-2008. But Figure 10a shows that summer- and domain-averaged area burned and ozone are correlated with temperature between

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1990-2008. If this is so, why is there no O3 to carbon burned correlation in Figure 9? Aren't these two results inconsistent?

L10-11, P25885: You say the difference is "in part" due to the different definitions of stratospheric influence. Does that mean there is a significant difference remaining even when GEOS-Chem uses the Lin et al. (2012) approach? If so, how much of the difference remains?

L15-16, P25887: You should also mention the lightning NOx yield changes due to moving the tropical/extratropical border here.

L25-27, P25887: I'd be more specific here, saying that the domain-average CASTNet ozone data showed no correlation with wildfires in the domain, in contrast to domain-averaged OC from the IMPROVE network.

Typos and style comments:

L18, P25873: I would think background generally would mean the absence of any anthropogenic influences, not just local.

L5, P25874: The wording here is a little awkward. I'd suggest changing this to "Understanding the natural sources contributing to elevated ozone in the Intermountain West is of crucial importance for policy."

L 3, P25876: Expand "OTD/LIS"

L25, P25876: I've generally seen "GFED2", not "GFED-2"

L24, P25880: Given that you conclude that fires had little noticeable impact on surface O3 during this period, I'd change this to "In the model, wildfires increase ozone by up to 20 ppbv" to make clear from the beginning that you think it is a model error rather than a correct estimate of the impact of fires.

L18, P25886: You don't need to redefine "CTM" here.

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P25900, Fig 4: The caption implies that the sites discussed in the text should be labeled on the map, but I don't see the labels in my version.

P25903, Fig. 7: The title on panel (d), "Stratosphere (transported)" is misleading, as what is actually plotted is the Lin et al. (2012) definition of stratospheric influence which includes both chemical production and transport, if I understand it correctly.

P25904, Fig 8: It's hard to see the data points once all the other curves are plotted on top. Could you plot the black curve and points on top of all the others to emphasize the CASTNet data more?

P25906, Fig. 10: The caption should mention that the bottom panel is only for 2006-2008.

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