

Interactive comment on “Wildfire influences on the variability and trend of summer surface ozone in the mountainous western United States” by Xiao Lu et al.

Anonymous Referee #2

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This paper uses back trajectories from the Lagrangian particle dispersion model FLEX-PART and estimated fire emissions for the years 1989-2010 to define a Fire Index for 13 CASTNet sites in the Intermountain West. This fire index and various meteorological parameters are used as predictors in a multi-linear regression (MLR) model that predicts daily MDA8 O₃ at these sites. The estimated impact of the fire index terms in the model is then used to determine the influence of wildfires on the MDA8 O₃, and this estimate is compared to estimated of the Eulerian chemical transport model GEOS-Chem. The authors find that wildfires enhance the summer mean MDA8 O₃ by 0.3-1.5 ppbv, with episodic daily increases of 10-20 ppbv at individual sites. They find that GEOS-Chem tends to over-predict the near source formation of O₃ and underpre-

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dict the downwind formation, consistent with previous Eulerian model studies. Finally, they find that the influence of wildfires is especially important on high O₃ days, where 31% of the days with MDA8 O₃ over 70 ppbv would not have occurred in the absence of wildfires.

This is a well-done, innovative study and a well-written manuscript. The development of the fire index and the MLR both help to understand the complex influence of fire emissions on O₃ in the intermountain west and to expose errors in Eulerian models of this process. The methodology is generally sound and the results are consistent with our understanding of fire chemistry. While I have a few concerns that I would like to see addressed before publication, as detailed below, in general this is a very nice study that should be published.

Major Concerns:

I have concerns with two of the conclusions of the paper:

1. The abstract states (P2, L32-33) that wildfires contribute 15% of the measured increasing but statistically insignificant trend in MDA8 O₃, and this is also stated in the conclusions section (P26, L461-462). However, as neither trend is statistically significant, I disagree with including the 15% value as a major conclusion of the paper, where it might be erroneously quoted without proper context. Thus I recommend that the abstract and conclusion statements be removed from the paper, but the discussion in Section 4.3 remain, as the trend results are given proper context there.
2. P20, L357-360 states that the interannual variability in MDA8 O₃ appears to be more controlled by interannual variations of the meteorological parameters, as the meteorological variables can account for “most” of the interannual variability in the MDA8 O₃, even without fires. I do not think this conclusion is adequately supported by the presented evidence. The fact that the MLR for the met parameters has roughly the same interannual variability as the measurements could be just a statistical artifact of the MLR procedure, with the interannual variability incorrectly accounted for by the meteorological predic-

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tors. The conclusion would be more convincing if evidence were presented of the interannual variability of specific meteorological parameters (Temperature, RH, etc.), and if the highs and lows in the summer means of these raw variables were consistent with the highs and lows in MDA8 O₃.

Minor Concerns: P7, L122: Please add the latitude-longitude or ID number for the Sal Lake City site you are using.

P9, L156: 250,000 is a huge number of particles to track, and is probably overkill. Usually 500 particles per receptor (time step and location) is sufficient. How many time steps are there each day, and how many particles are released in each time step?

P9, L157-158: Is this e-folding time supposed to account for the deposition of smoke along the path? How is this done – is a number on each particle decreased, or do some of the particles actually disappear during the simulation?

P10, L169: My understanding is that MISR observations suggest that plumes go above the boundary layer 20-25% of the time, so “often” seems a little vague and misleading.

P10, L170: It is true that 5 km and 5 days generally gave the best correlation, but the change in the fit wasn’t very significant compared to 2 km (PBL height) and 5 days.

P10, L175: I’d like to see an equation for variable $tr(i,j)$ as well, that shows how the residence time for a single layer is calculated and how the layers are integrated vertically.

P11, L189: Since Table S1 defines the variables, I think it should be moved to the main paper.

P11, L198: I understand the choice of MLR limits what you can do to look at non-linearity, but why did you choose the square root of the index instead of, say, the square of the index?

P11, L202: You should briefly discuss how the model doesn’t include interaction terms between the predictors, and the effect this might have on the model performance.

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P14, L236: Do you mean this is just a reanalysis of the Zhang et al. (2014) output, or did you rerun the simulations? You note later that the NO_x emissions in this simulation are too high – why didn't you use the lower value here?

P14, L250: I think you mean “all except for GRC” show weaker correlations, or there is an error in Table S2.

P15, L267: Can you explain why you get poorer correlations for Salt Lake City than in the Jaffe et al. (2013) study? What does this imply for your other results?

P15, L270: I think this dependence of the performance on altitude makes sense, but a scatter plot of R² versus site altitude in the supplement would help to prove it.

P15-16, L287-L290: Since you include the fire index as a predictor, the fact that the residuals don't correlate with TFI just shows that the MLR procedure is working as expected, right? The second clause of this sentence, that underestimates occur even in the absence of fires, seems like more convincing evidence to me.

P21, L367-L371: I didn't understand what you were trying to say here – please elaborate or rephrase?

P21, L377: Can you please explain why you chose these percentile ranges?

P24, L420-422: I'd suggest cutting this sentence, as the context of the study is already established in the introduction and this statement is incomplete – Eulerian model errors are not just about resolution, but about errors in amount, location, and timing of biomass burned, error in emission speciation, errors in chemistry, numerical diffusion errors, etc.

P25, L435: Make clear how this average R² is calculated.

Typos and Wording Suggestions: P7, L112-L113: I'd suggest making this a single list: “ozone, organic carbon (OC) aerosols, meteorological parameters, and wildfire area burned data”

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P7, L115: Expand CASTNet acronym and provide a little more descriptions than just the website.

P11, L189: Period should go after the parentheses, not before.

P14, L244: Figure S2, not S3.

P14, L247: Need a comma after “strong”

P16, L280: “as would be expected” delete “it”

P21, L364: Say “summer mean” to be as clear as possible.

P22, L389: Don’t need comma after “Strode et al.”

P23, L411-412: I suggest putting parentheses around the phrase “accounting for 22% of the summer days”

P25, L437-438: I suggest cutting everything after the R2 value - these references are already discussed in the main text and do not need to be repeated here.

P25, L442-443: I don’t see much consistency at all between the MLR and GEOS-Chem predictions, so you need to make clearer what consistencies you see.

P27, L474: “model in” instead of “model to”

P33, L677: Add unit ‘(m)’ of terrain elevations to caption, as it is not on the figure color bar.

P34, L689: “those from the GEOS-Chem” instead of “those by the GEOS-Chem”

P38, Figure 6: The wildfire trend values are very hard to see – maybe plot on a secondary y axis? In addition, since the trends are generally not statistically significant perhaps this could be moved to the supplement?

P38, L734: remove “S” from “SMLR” for consistency with the rest of the paper.

P40, L741: Need a space between “relative humidity” and “(RH)”

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Figure S1, Caption, L3 and 7: “residence time” not “resident time”

Table S1, Footnotes, L37: Should say “m (PBLH, HGT)”, delete the rest.

Table S1, Footnotes, L38: change to “mean represents the average”

Table S3, Footnote c: Put the explanation for the bold text in the figure caption, not the footnote.

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