

Interactive comment on “The impact of aged wildfire smoke on atmospheric composition and ozone in the Colorado Front Range in summer 2015” by Jakob Lindaas et al.

Anonymous Referee #1

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Author(s): Jakob Lindaas et al.

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General Comments

Overall, this is a good example of a case study, with relevance specifically to Colorado air quality and which, more generally, speaks to air quality concerns relevant to the western United States, where wildfires are a substantive air quality issue. In Colorado, there is an active community of regulators at the State and Federal level who have been debating the very issues discussed in this paper for well over a decade. There is an extensive network of monitoring and also substantive photochemical modeling

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address this issue from a policy perspective. This case study has merit, but the danger here is that a paper will over generalize a case study and overstate its own importance. The authors should be careful in this regard. From a regulatory perspective, actual exceedances of ambient standards for ozone (70 ppbv for 8 hours) are relevant and anything below this is generally not relevant. Even so, a non-attainment designation is based on more than a single exceedance at a single location. The authors should demonstrate that knowledge and perspective in the body of this work. It is well established that wildfire smoke can enhance ozone formation, especially air masses that have been aged for several days. Despite an extensive monitoring network and a concentrated field study, measurements are sparse, as is evidenced here by the use of data from several monitors remote from the BAO tower. This paper would be strengthened immensely by the use of satellite imagery showing the wildfire smoke haze during the periods of interest and also by the use of additional State and Federal agency data to establish that wildfire smoke was the cause of the ozone enhancements observed. In my mind, this is not unequivocally established by the observations presented here. For example, could CO enhancements be caused by Asian air mass transport? I encourage the authors of this work make substantial improvements to this work before I recommend publication of this work in Atmospheric Chemistry and Physics. Also, in this work, the authors contrast data from two “smoke” events with that from non-impacted periods during the same period. However, much of the data from the first fire plume is disregarded. It’s excluded from much of the text and the figures. In my opinion, the first fire plume should be included in all analyses, even if the results are diminished. If this is really a fire plume, it should not be dismissed and excluded. Fire plumes are variable, and that is an important point. Sometimes they make a case easier, sometimes more difficult, but this is a reality in a complex world.

Specific Comments

1) The Title and Abstract should more strongly indicate that this is a single case study showing influences from remote wildfire smoke on one location downwind

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2) The Abstract should mention if nearby official monitors showed ozone exceedances to put this case study into context (see additional comments to this effect below).

3) Introduction. I recommend that the authors extend the background discussion to include policy relevant discussions and demonstrate knowledge of the extensive regulatory work that is ongoing on this topic in the west related to ozone exceedances. For example, only one exceedance of the NAAQS for ozone occurred for all of July and August at the measurement site. How does this compare to the exceedances for the entire State for 2015? Was the entire State in non-attainment this year? Was the event mentioned here a contributing factor? Or, did it fall much farther down the list other exceedances of the standard in the Front Range in 2015. These questions are very relevant to policy and should be discussed in some detail to place the study in a larger context.

4) Introduction/Smoke events: How does the climatology of the study period compare to Colorado's as a whole? Was it a cool year? A hot year? A wet year? Was there evidence of pollution transport besides fire smoke from other areas of the US and Internationally?

5) It is not evident why values from the study site were compared with very select other monitoring sites (in this case CAMP, ROMO and Walden) and not others. This gives the impression that supporting evidence has been picked rather than evaluated broadly. Why, why for example, is the PM from the CAMP site (20+ miles from the BAO tower) used, while the CO measurements from CAMP have not? Why are other Front Range ozone measurements not evaluated? Why were Walden and Rocky Mountain ozone sites used, while other data (e.g. CO data from the Storm Peak Lab) were not? This needs to be address directly in the text.

6) Was there satellite imagery from this period that showed the wildfire smoke haze? This data is often widely available, and so should be included if possible.

7) Line 102. The GC-method needs to be summarized in more detail. What is the

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integration period? What is the frequency of measurement? The overall method can be referenced from another paper, but those parameters are important and should be included here.

8) Line 147 and Figure 1. CO and PM2.5 data from other surrounding monitors should also be included. Ozone data from other Front Range Non-attainment area monitors should be summarized and discussed.

9) Figure 1. Add CO from CAMP at minimum. There are other CO monitors nearby, do they agree? Add PM2.5 from locations closer than CAMP if possible. Address the latter points in text if they cannot be added to the figure.

10) Lines 147-157. CAMP also has O3. The data from that station's O3 needs to be included/shown here, and any place PM2.5 is used from that site, especially given the 35 km distance between sites. Also, there needs to be a space between 35 and km on line 150. The paper states, "PM2.5 was similarly elevated during the smoke-impacted periods at CDPHE monitoring sites across the Colorado Front Range (not shown)." Why is it not shown? It should be. Lastly, the spikes in figure 1 data are of equal magnitude as the spikes within the defined periods, why are these smoke free?

11) Figure 2. Recommend that satellite imagery of smoke added as additional figure to make the case that the plume was smoke and widespread.

12) Section 4.1. Measured data, especially for VOCs, should be tabulated and summarized. Please insert a relevant table of species measured with relevant max, min, median values and standard deviations.

13) Figure 3. Labels not clear. Add text labels. Why is first fire period excluded?

14) Figure 3 is hard to understand without a table or plot of VOC concentrations.

15) Lines 178-181. The fact no biomass burning specific VOC speciation was done at all seems a bit off. This is surprising given the title, and conclusions, and likely impacts of this paper.

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16) Line 188. Section 4.1, Figure 2: Unclear how the statement "... suggests that the age of smoke impacting the Front Range during the August smoke-period was 2-3 days." That is not apparent in the figure.

17) Figure S1-S2. These plots are jumbled. Add legends. If your point is that the boundary layers at 0Z are more variable than the ones at 12Z, you should make that point in the text. The data contradict the conjecture you make around line 218. It's not clear why the sondes are included here. The surface temperature data is presented in Figure 8, so why show the sondes? Perhaps these figures should be revised to be simpler and more concise, or removed. If you must show the soundings then perhaps have two panels, one for smoke free and one for smoke and then a solid gray-area representing all the data, and a line for the average, or even a vertical box/whisker plot.

18) Line 216. "Not shown" in reference to the diurnal cycles. Diurnal cycles should be shown.

19) Line 218. A lower PBL height during the day is exactly the opposite of what is observed and this directly contradicts data from figures S1 and S2. This speculation should be removed.

20) Line 231-2. This statement does not make sense. Abundances decrease over what time period? Please clarify the wording.

21) Line 232. Diurnals not shown. Conclusions in this section could use the support of the diurnal cycles and as is it's hard to follow without them.

22) Paragraph including lines 230-246. Trends are not explained and speculation here is spurious. A table presenting the measured values could easily replace this table. Also, why would isoprene behave differently than other alkenes? Are the changes in alkenes even significant given that they are near their detection limits? A table would suffice here rather than trying to explain trends in ways that mean little. It is unclear what the conclusion of this paragraph is. It's also unclear what the take home point is

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or and how the evidence supports the conclusion.

23) Figure 4. Include first fire period. This figure does not appear to be referred to in the text? It is unclear what 95th percentiles mean. In the legend says quantile and not percentile. Clarify. If it is not referred to in the text, it should be eliminated.

24) Figure 5. Indicate what shaded regions are. Are they percentiles? Of which measurements? Note that almost never does red line leave the grey shaded area, except for PAN and NO_x. Discuss in text. Show solar noon on the plots for clarity.

25) Line 308. Please include more detail about the analysis you did related to traffic impacts.

26) O₃ does indeed have a positive correlation with surface temperature as referenced often in this paper. However for the Front Range region, this should be tempered by the fact that the almost parallel rise in temperature and ozone starts dropping off after the air temperature hits about 86-90F (30C-32C). Some evidence of this can be seen in Figure 6. The reason for this is that once surface temperatures begin to exceed this threshold, a westerly wind component usually becomes dominant. These westerlies will often be gustier and not allow the cyclical terrain-driven circulations that normally enhance ozone concentrations across the Front Range. As referred to in this paper, the Reddy-Pfister study of 2016 expands on this and concludes that 500 mb heights and 700 mb winds hold a stronger correlation to ozone concentrations than surface temperature for Front Range locations. Perhaps this is irrelevant since the air temperature during the "smoke" periods did not get very hot, but maybe an explanation of this phenomena should be included if surface temperature is being emphasized as being more important than the other variables mentioned above.

27) Figure 6. Include first smoke impacted period on this chart in another set of colored box and whiskers if they are different from the second smoke period. Also, in this figure, the gray bars are indistinguishable from the gray circles, they blend together. Perhaps use black bars instead of gray. The same is true for Fig 8, S4, and S9.

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28) Figure 7. The point highlighted in mid-August where temperature is low and O3 is high looks interesting, why is this not considered smoke influenced given the paper's hypothesis?

29) Figure S3. Clarify if the data shown is for one or both smoke free periods. Show both, using different are lines, if they are different from each other. It is hard to see what's happening at lower values due to so many points. Or figure could be revamped showing quantiles with error bars and all data in gray behind.

30) Figure S4. Combine this figure with Figure 6.

31) Line 323. Figure 5d doesn't appear to show a very significant difference in ozone between the black and red shaded areas. Perhaps, the figure needs to be edited to make the true difference clearer; otherwise, it seems overstated in the text.

32) Line 330-333. O3 production with temperature levels off at high temperatures particularly in the Front Range due to the wind speed and direction associated with these high of temperatures. This should be addressed in the text.

33) Lines 334-335. Things like black lines or red lines descriptions should be in the figure legend and caption, not text body.

34) Line 361-363. The chosen altitude limit makes sense, but the Denver cyclone and in-basin wind patterns do contribute to ozone and recirculation. This should be emphasized more and discussed. The authors should include the wind field reanalysis data to show surface winds on their chosen day of interest in each smoke period.

35) Line 340. How did the weighting occur? Insert a reference or elaborate.

36) Does the "synoptic scale transport" discussed at the end of page 11 and start of page 12 also account for the possibility of Asian pollution influence? The HYSPLIT back trajectories on page 20 both suggest that at least a portion of the air mass may have originated in Asia. It would be interesting to see just how much, if any, influence Asian pollution may have had when comparing the smoke and non-smoke air masses.

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37) Line 352. It is unclear how just referencing the geopotential height paper (include citation at this location) leads to the conclusion that was “no evidence” of meteorological factors in ozone enhancement. This is a very broad generalization and needs supporting evidence and specific discussion if it is to be included here. Is the point you are making that the lack of meteorological factors that correlate with ozone implies that all the ozone was due to fire? If so, make this case strongly and state it clearly. Is absence of evidence meteorological driven ozone production even acceptable evidence? I’m not so sure it is. At best, it is supporting evidence.

38) Figure S5-7. The authors should explicitly discuss how the data in these figures supports their argument. This is a good supporting point, but there is need to flush out the discussion and figures better. Devoting 3 figures vague scatter plots to this is excessive. Could they be layered in 3 dimensions on a single plot? Alternatively, make one 3-panel figure or remove entirely and only quote the R2.

39) Line 368. Is this flow discussion where you should refer to Figure 4?

40) Figure 7. This figure needs to be put into context. Did the Front Range exceed the NAAQS this year? Was this one of the four maximum values that put the region into non-attainment for the year? Or was it much further down the list? This is valuable context information that should be discussed in the text. One exceedence is generally irrelevant to the overall policy discussion, but if this is not the case, it is certainly worth discussing in more detail.

41) Paragraph lines 373-375. Why did you pick 65 ppbv MDA8, when this is not the standard? This seems arbitrary. Please use the current standard and put into the correct context of this year’s ozone for the entire area as mentioned in a previous comment. Please also adjust your conclusions accordingly.

42) Figure 8. Same comment as for Figure 6. This needs to include the first fire period. Also, why were these sites chosen? Is it because they are remote? If so, why was an example of a nearby monitor not included? Was your point to show that the smoke

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was widespread? Pick more, not less sites. Was Colorado Springs impacted? It is not necessary to show all the sites, but just clarify your rationale and pick sites to make your point and then say why you picked them.

43) Lines 414-416. What are you trying to say here? Can you refer to Figure 4? Are you trying to say the smoke was widespread? If so, say that and present evidence.

44) Line 424-6. You should state this much more strongly and earlier on. It is a major conclusion of the paper. You have direct evidence of this variability.

45) Figure S9-S10. Are these figures needed? Could they be combined with Figure 4 and used together as supporting evidence?

46) Figure S11. Is this figure needed? Could you just state the values in the text? An entire figure for two data points with error bars is excessive.

47) References. Please include reference showing where the public data you used came from (CDPHE, Forest Service, NPS).

Minor Issues/Typos

1) Line 62. The use of the pronoun “they” is vague. Please clarify the wording.

2) Line 76. The term “This region” is vague and should be made more specific and the wording should be clarified.

3) Line 220. There should be a comma after “however”

4) Line 243. A comma is needed after “Thus”

5) Line 278. A comma is needed after “Thus”

6) Line 286. This is important and should be emphasized more if possible, rather than burying it deep in a paragraph. Perhaps making the PAN and alkyl nitrate discussions separate paragraphs would clarify enough.

7) Line 300. Phrasing is a bit confusing. Rather than saying “fewer days” which is a

little vague here, rephrase saying that period 1 had a shorter duration than period 2, or the equivalent.

8) Line 302. “more significant changes”...than what? NO? Clarify the wording here.

9) Line 310. The phrase “is one hypothesis” is awkward. I suggest rephrasing this sentence.

10) Line 317. A comma is needed after “In this section”

11) Line 419. “Very high” is not specific enough. Include the value here.

12) P 6, line 161-166. Section 3: Things like red triangles, black lines, etc. should be in the figure caption, but not paper text. Only science/discussion should be in the paper body. Also, what are the blue circles? Not in legend or caption. There should be a space between 1000 and m on line 166.

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