RC2: 'Comment on egusphere-2024-1938', Anonymous Referee #3, 25 Sep 2024

We would like to thank the reviewer for their comments which are reproduced here in boldface italics along with our responses in plain text.

General comments:

The manuscript describes an unusual winter ozone event in Colorado during a day with high photochemical activity, which was enhanced by a snow-covered surface. The analysis uses a combination of surface measurements, lidar, ozone sondes, HYSPLIT back trajectories, and models to show the origin of the enhanced O3 observed. While the paper is structured differently than most other measurement papers (no separation between methods and results), it guides the reader through the meteorological conditions needed for such an event, observations of O3 at multiple sites around Boulder, Colorado, and results from a chemical box model for one of the sites, where a large suite of VOC data was measured. It would have been interesting to see chemical box modelling for multiple sites to see if the O3 measurements at the highest exceedance site could be reproduced, however, without having measured VOCs at this location during this event it seems unrealistic.

My recommendation is to publish the manuscript after taking into account the minor corrections/comments below.

Minor comments:

Line 156-159: In the text you write that "The measurements from the high elevation (>2.5 km above mean sea level, a.s.l.) "baseline" monitors at Blackhawk (BHWK), NWR, and Rocky Mountain National Park (RMNP) are plotted in black with the measurements from those monitors that recorded higher (lower) MDA8 O3 on the 17th plotted in red (blue).", which sounds like all monitors that measured higher (lower) MDA8 O3 on the 17th than the baseline monitors are plotted in red (blue). However, in the plot it is only the highest (lowest) measurement of MDA8 O3 on the 17th plotted in red (blue), which is described in the following sentence. All other monitors are plotted in grey.

The text has been revised to eliminate the confusion.

Figure 4: The horizontal dashed lines indicating the 2015 NAAQS should be the same colour in both panels.

Fixed.

Figure 6: It is difficult to read the potential temperatures in the red shaded areas because of the lines being very close to each other (and the numbers therefore overlapping) and due to the text disappearing in the dark red shading.

The stratospheric potential temperature values are not important for this study, but we have enlarged the figure to improve the readability**.**

Figure 9: In the figure text you write that the dashed lines are the 200 m PBL height, but in panel (c) the line is not at 200 m. From above in the text, it sounded like the PBL height was higher on the 14th than on the 17th, so the text should either specify both heights individually or just say that it is the PBL height at the time of the hysplit trajectory initiation.

The figure caption has been amended.

Figure 10: In the figure text about panel (a), the dashed lines for the hysplit trajectories from the BOS station are not mentioned anywhere. They are mentioned in the text, but the difference between solid and dashed lines is not.

The figure caption has been amended.

Line 354-358: It would be good to mention that the altitude profiles mentioned here are shown in Fig 10c.

Done.

Figure 13: For extra clarity the text should be "Scatter plots comparing the LUR n-butane (top) and ethene (bottom) measurements to the propane (left) and ethyne (right) concentrations." The year of the Pollack reference is written as 2021 in the text (line 434-440), but 2022 in the figure text. And a description of which hours the red data covers could also be explained in the figure text.

Done.

Line 458: You write that the co-mingling of traffic and O&NG emissions is implied by the back trajectories in figure 10, however, only one of the DSRC trajectories in figure 10b crosses the I-25 mentioned in the text. Could there be another traffic emission source than the I-25 involved? Or is it better to describe it as mixing of urban and O&NG emissions as you do in line 570 when referring to figure 14?

We have revised the sentence to read "…co-mingled traffic and O&NG emissions from *Longmont*, I-25, and the WGF implied…"

Line 478-480: From the text it is implied that the ratio of 2.41 is typical for traffic emissions of i-pentane/n-pentane, however, it would be good to add some context to the sentence such as what the focus of the CalNex campaign focused on instead of implying that it measured traffic emissions.

CalNex was a major field campaign with both airborne and ground-based elements that involved dozens of researchers. A comprehensive description of the multiple objectives is not really relevant here, but we have added a reference to the overview paper by Ryerson et al. for those who are interested.

Figure 15: In the text it is mentioned that the dotted line represents the Pasadena measurements, however, in the figure text the dotted line is not mentioned despite Pasadena being mentioned: "The long- and short-dashed lines show relationships derived from previous measurements at the BAO, DSRC, and Pasadena, CA (PSDA) (Gilman et al., 2013)."

Fixed.

Figure 13, 14, and 15: Figure 13 show that the LUR site predominately measures O&NG emissions based on the good correlation between n-butane/propane and ethene/ethyne and Figure 14 shows that the DSRC site measures a combination of O&NG and traffic emissions during the afternoon/evening of the 17th based on good correlation between n-butane/propane and ethene/ethyne as well as n-butane/ethyne and ethene/propane. However, in figure 15 the correlation between i-pentane/n-pentane at the DSRC site looks closer to the O&NG ratio than the LUR site. How do you explain that?

The O&NG influence was much smaller at the DSRC, but the COVID shutdown and recent snowfall impacted the local and commuter traffic in Boulder more than the commercial traffic on I-25 so that the *relative* contribution of the O&NG sources was larger. The reviewer may have overlooked the different scales in the two plots so we have added the phrase "Note the different scales on the two plots" to the figure caption.

Line 524-525: My understanding of the scaled OH reactivity mentioned is that it is used to show the O3 producing potential of the measured air is higher on April 17th since O3 production is dependent on sunlight. Could the purpose of scaling the OH reactivity be added to the text if that is correct? If it is not correct, then some additional explanation is needed to understand why you scale the OH reactivity to the solar radiation.

The solar flux was actually very similar on the 14th and 17th, but we have scaled the solar flux to emphasize the greater photochemical production on the clear days (14th and 17th) compared to the cloudy days (15th, 16th, and 18th). We have revised the text to make this clearer.

Figure 16: Would it be clearer if the different OH reactivity contributions were stacked on top of each other so you can see the total reactivity from the plot as well?

The total reactivity is already plotted (heavy black lines) in panels (a) and (b), and panel (c) shows the total O&NG contribution in red.

Technical corrections:

Line 152: The ")" after DM/NFR should be removed.

Fixed.

Line 279: Insert comma: "… potential temperature (theta), relative 280 humidity (RH)…"

Fixed.

Line 327: Insert "the": "Even the trajectory launched from the RFN monitor…"

Fixed.

Line 364: Delete one of the "passing" in the sentence.

Fixed.

Line 546: Change format of reference from "... that of Rickly et al. (Rickly et al., 2023)…" to "… that of Rickly et al. (2023)…"

Fixed.

Line 612: NOx should be NO_x

Fixed.

Supplementary information:

Page 4, line 2: "in-situ" should be "in-situ"

Fixed.

Page 4, line 6: The Rickly reference is not in the reference list and assuming it is the same reference mentioned in the manuscript, the year should be 2023

Fixed.

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