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

The analysis of the ozone exceedance event on April 17, 2020, in the Boulder-Fort Collins region contains several intriguing elements. While the individual aspects of the analysis appear sound, the combination of them appears to be problematic. I suggest the authors focus on the most compelling aspect and conduct a more in-depth analysis. This focused approach would likely yield more robust and meaningful conclusions.

We do not understand what the reviewer means when they suggest we "…focus on the most compelling aspect and conduct a more in-depth analysis." What do they consider most compelling? We would argue that all components of our already "in-depth" analysis are essential to understanding this episode.

This April event differs significantly from previously reported winter ozone episodes. While winter events have recorded ozone levels up to 150 ppb, this springtime occurrence peaked below 80 ppbv. A key factor in winter high ozone episodes is a shallow boundary layer. It would be beneficial to compare its boundary layer height (BLH) with those of winter episodes. This comparison could help explain whether the typically higher springtime BLH contributes to the lower observed ozone levels. A more appropriate paper title would be "An Unusual Winter-like Ozone Event in Colorado."

The reviewer is correct that the episode described here differs from those that occur in O&NG basins of northeastern Utah and southwestern Wyoming in several important ways. The primary reason for the lower peak O_3 levels in the present case was not the *depth* of the boundary layer, but rather its *persistence*. The basin-like terrain of the Utah and Wyoming O&NG fields supports the formation of stable shallow cold air pools that can persist for days or even weeks until they are disrupted by a frontal passage. This allows O_3 to accumulate near the surface over time, and in the example described by *Edwards et al.* (2014), the O_3 concentrations increased from ≈80 ppbv, on Day 1, which is similar to that observed in the short-lived event described here, to more than 120 ppbv on Day 5.

The assertion that "statically stable lower stratospheric air suppressed the growth of the convective boundary layer" requires further substantiation. Figure 8 indicates a BLH of 200m, which is inconsistent with a convective boundary layer.

The term "convective boundary layer" refers to the region above the surface where the air is well mixed, whether by free convection (thermals) or forced convection (mechanical turbulence). The BL depth of 200 m is entirely consistent with the latter.

If stratospheric air indeed descended to such a low altitude (200 m), one would expect significantly higher ozone concentrations than those shown in Figure 7.

Not necessarily if there was limited mixing at the top of the convective boundary layer as we show to be the case in Fig. 8.

A more obvious explanation for the shallow BLH could be the low surface temperature shown in Figure 2. Introducing a complex mechanism involving stratospheric air seems unnecessary without stronger supporting evidence. The authors should either provide more robust data to support this claim or consider alternative explanations that align more closely with the observed conditions.

if the snow cover and low temperatures alone had been sufficient to create the 200 m deep boundary layer on 17 April, then we would have expected to see a very similar boundary layer depth on 14 April when the temperatures and snow cover were similar. Instead, we measured a significantly larger depth of 630 m. We contend that the capping of the boundary layer by the stratospheric intrusion was a key factor in the evolution of the 17 April event and that this is thoroughly documented by the lidar and ozonesonde measurements presented in the manuscript. There is nothing particularly "complex" about the mechanism we describe here. Stratospheric intrusions form above the western U.S. during the passage of most cyclonic systems in late winter and spring, and many examples have been documented by the TOPAZ lidar in Boulder. Most of these intrusions descend only into the middle free troposphere, however, and very few penetrate all the way to the surface (see *Langford et al.* 2009 for an example of the latter).

Please include "baseline" ozone concentrations at the NWR site in Figure 17. It appears that model simulated ozone concentrations at the DSRC are not much higher than NWR on April 17 and 18. Is this modeling result appropriate for diagnosing the high ozone event shown in Figure 4?

We have attached a modified version of the figure that includes both the "baseline" measurements from NWR and the highest O_3 measurements from BOS. The reviewer is correct in that the simulated (and measured) O_3 concentrations at the DSRC are only about 8 ppbv higher than the "baseline" concentrations. This may not seem like much, but it is much greater than the difference of ≈1 ppbv measured on 14 April when the temperatures, snow cover, and insolation were similar.

Several additional questions arise from the F0AM model analysis:

1. ***How does the significantly lower VOC concentration at DSRC compared to LUR and BOUR (Figure 12) impact the model's sensitivity to VOCs, especially given that NOx levels are comparable across sites (Figure 11)?***
2. ***Would F0AM simulations using NOx and VOC concentrations from LUR and BOUR reproduce the observed ozone concentrations at those sites?***
3. ***Can the variations in NOx and VOC concentrations explain the lower ozone levels at LUR and higher levels at BOUR relative to DSRC?***
4. ***If these concentration differences do not fully account for the observed ozone variations, what is the justification for using the model results to predict an increased frequency of high ozone events in the future?***

We have limited the F0AM model analysis in our manuscript to the DSRC because there were no measurements of the boundary layer depths at the BOUR or LUR and the limited measurement

suites at these monitoring stations did not include formaldehyde, acetaldehyde, or any of the other oxygenated compounds that were found to dominate the O_3 production.

The attribution of VOCs to oil and natural gas (O&NG) sources and NOx to motor vehicles in this event raises an important question: Why aren't similar ozone exceedance events observed more frequently in nearby regions with comparable source combinations? The co-location of O&NG fields and major highways like I-25/I-70 is not unique to this area. This scenario suggests that additional factors beyond the presence of these emission sources must be at play.

Indeed. We would argue that the additional factor in this case was a stratospheric intrusion.

References

Langford, A. O., Aikin, K. C., Eubank, C. S., and Williams, E. J.: Stratospheric contribution to high surface ozone in Colorado during springtime, Geophys. Res. Lett., 36, doi:10.1029/2009GL038367., 2009).

Edwards, P. M., et al.: High winter ozone pollution from carbonyl photolysis in an oil and gas basin, Nature, 514, 351-+, 10.1038/nature13767, 2014.