Thank you for the comments. Below, point-by-point responses (bold).

The authors put a great deal of effort into revising the paper. They recalculated everything more stochastic to provide a broader range of possible outcomes.

Here are a few more comments to consider:

Figure 5. The plot layouts aren't consistent. For instance, the middle panel includes other percentiles.

All figures contain only percentiles 10, 50 and 90. Some contours are hard to see, so we just added symbols to better visualize the percentile contours when needed. We added this clarification in the captions of Figures 5, 8, and 9.

Line 455-465. This part of the discussion is oblivious to the vertical mixing mechanism. A more expanded PBLH should indeed dilute the ozone concentration only if we assume that there is no non-local vertical mixing between upper PBLH with air parcels close to the surface (a subsidence compensation for the rapid vertical mixing, see Figure 1 in https://www.atmos.albany.edu/facstaff/rfovell/NWP/pleim-2007.pdf). If the concentration of our targeted compound decreases with altitude, this way of mixing (which is faster when surface kinetic heat fluxes are large, resulting in expanded PBLH) will further dilute the concentrations because high concentrations of the species near the surface will be rapidly exchanged by lower values aloft. This condition can be completely reversed for a species whose concentrations generally increase by altitude. Perhaps, when surface ozone could reach 200 ppbv near the surface, the vertical profile of ozone during these events followed the first example, but given the fact that ozone levels are much lower in your case and your ozonesondes observations indicate higher ozone aloft, an expanded PBLH should lead to more ozone, fully overpowering the dilution effect. If the authors want to discuss the relationship between ozone concentration and meteorology, they need to be very thorough and not pick and choose the part of the picture to partly validate their claims. There are also no discussions about the regional background of ozone and wind patterns. These cities may receive different air masses with varying background ozone concentrations during the season. Dry deposition is another component to consider. Maybe some discussions about LAI or varying surface resistance between two cities can help (https://acp.copernicus.org/articles/19/14365/2019/acp-19-14365-2019.html).

This comment contrasts with the comments we received from two other reviewers who considered it relevant to include a comparison between the depth of the boundary layer and boundary layer evolution between the two cities to explain differences in dilution conditions (please see comments by Reviewer2 and Owen Cooper in the interactive discussion). We followed these valuable suggestions, and we found that often the boundary layer in Quito is deeper and the first order dilution constant is faster than in Santiago. We included this insightful information to strengthen the discussion, as requested.

In this paragraph we do not show any figure, but the reviewer mentions that we make claims based on a picture (number not specified) with ozonesonde observations. This is not accurate

because we do not present such information in this paragraph. Perhaps the reviewer is referring to the previous version of the paper in which we described the shape of the ozone profiles collected in Quito (i. e., https://doi.org/10.1002/asl.829). In this regard, it seems that the reviewer only focused on the shape of the 7 am profiles, which are not representative of the noon and afternoon profiles that show mixing when the boundary layer has reached its maximum depth. In the early morning, ozone becomes titrated by traffic NO during the morning rush hour. Under these conditions, of course ozone gives the impression of being lower at the surface and higher aloft, but this is just an effect of the morning ozone titration with NO. If you look at the noon and early afternoon profiles in the same paper, ozone is lower at higher altitudes. Thus, the argument presented by the reviewer in the sense that lower ozone aloft mixes with higher ozone at the surface, precisely holds for the ozone profiles we have observed at noon in Quito. In any case, we do not discuss ozonesonde morphology in this paper. Also, the surface ozone value mentioned by the reviewer (200 ppbv) is completely out of range for Quito. Please refer to the air quality discussion on the present paper.

Regarding the regional background of ozone, we do not address this aspect in this paper. As indicated before, this study deals exclusively with the outcome from a photochemical box model. A regional/urban scale model that allows for a detailed inclusion of boundary conditions or upwind precursor sources needs to be performed in the future to assess transport and background ozone effects.

The reviewer also mentions dry deposition. As stated in the methods, we only considered chemical sources and sinks of ozone. In any case, it is important to realize what the order of magnitude of this physical sink is with respect to the order of magnitude of the chemical production of ozone. For example, for a typical ozone concentration in Quito at noon of 50 ppbv and boundary layer height of 1500 m, with deposition velocity for ozone of 0.4 cm s^{-1} (Seinfeld and Pandis, 2016), the dry deposition is 0.48 ppb/h. This quantity is two orders of magnitude lower than the chemical production of ozone found in our study.

Having analyzed carefully this comment, we do not find a solid reason to make modifications to this portion of the text.

L 470. Two indicators aren't consistent for Quito for a less photochemically active environment. I suggest including only times when enough photochemistry is involved to have meaningful discussions about the chemical conditions. In light-limited regimes, it doesn't matter what these ratios are because the response of PO3 to its precursors becomes muted. Maybe you should cut the x-axis to 10 AM to 4 PM.

It is hard to interpret what exactly the reviewer means by inconsistent indicators for "less photochemically active environments". There is also mention of "light-limited regimes". It seems that the reviewer is under the impression that there is not enough sunlight in equatorial Quito. As demonstrated in the previous set of answers, this is not the case for this city located right on the equator at high altitude. In addition, we only deal with photochemistry under sunny conditions in both cities. Please refer to the first set of answers in the interactive discussion and to the Methods.

According to the indicator thresholds described in the text (references given), both figures, 6a and 6b, show that the regime is VOC-limited for the model output obtained with the range of VOCs used for Quito. The reviewer suggests modifying the axis and only showing the output between 10 am and 4 pm. However, this range is already visible in the current figures, making this modification unsubstantial.