Review for: Measurement report: Vertical and temporal variability of near-surface ozone production rate and sensitivity in an urban area in Pearl River Delta (PRD) region, China.

In this manuscript, the authors have presented a detailed study on ozone production rates in an urban area in the Pearl River Delta region of China, including an investigation into the vertical and temporal variability of ozone, it's production rate and its precursors. The subject area is in the scope of this journal and would be of interest to the urban air quality community. I would recommend this manuscript for publication, provided the following comments have sufficiently been addressed.

Overall comments:

I am not completely convinced that comparing measured and modelled O_3 mixing ratios is a good way to validate the model, or to decide on any dilution factors (e.g Line 601). Measured O_3 mixing ratios will be influenced by transport, whereas modelled O_3 will not so you cannot draw a direct comparison here. In my opinion, the authors should address the caveats with using this to method to derive your dilution rates in the text more clearly or use a different compound which is formed from secondary chemistry, such as glyoxal, to determine their dilution rates / model lifetimes. Alternatively, they could use a value quoted in the literature and discuss the caveats of this instead. However, comparing measured PO_3 vs modelled PO_3 seems reasonable.

The authors need to be much clearer why understanding the vertical distributions are important to the air quality community. It seems that daytime O_3 is well mixed, and so ground level O_3 measurements would be representative of the vertical column. Is the key message that although this is the case, the VOC profile is different at different heights, meaning that if the chemical box modelling community is constraining to ground-level concentrations, they may not be accurately representing in situ O_3 production in the vertical column? If this is the case, it would be a very interesting conclusion and should be outlined in the text.

Minor comments:

Line 44: Type, "either" included twice. Remove one instance.

Line 190: Do you mean Table S3? At first, I went to figure S3 but I think you meant table S3. Please clarify in the text.

Line 203: Same as above comment – do you mean Table S4?

Line 232: Please clarify why you only used VOCs measured during the H_3O^+ mode.

Line 288: I wouldn't call these "conventional" pollutants. Please rephrase. Perhaps "inorganic pollutants"?

Line 295: Please clarify how you have decided on these background concentrations. Can you reference anywhere else this has been used?

Line 377: Now referred to as "ozone" in the text. It's better to ensure that either "O_3" or "ozone" is used consistently throughout the text.

Line 400: In Table 1, episode II O_3 mixing ratios are lower than non-episode II O_3 mixing ratios. Please explain why this is the case in the text.

Line 784: I'm confused on which VOCs fall into which categories. I looked at Table S2 (although if that is where the reader should be looking, please direct them here in the text), and many of the VOCs fall under two categories (e.g. AVOC and NMHC). Does that mean some species are repeated in different categories in Figure 9?

Line 820: It would be nice to know which specific AVOCs/OVOCs might be key, so that potential sources that can be targeted for reduction could be identified. The conclusion to this section is fine, but very general and doesn't really add any new details.

Line 841: "The TVOC and OFP exhibited variable trends with increased height during both daytime and nighttime" – what are the implications of this to the modelling or measurement community?

Line 847: I don't think you performed a test for statistical significance in this part, so perhaps rephrase.

Line 899: Could you expand a bit more in your conclusions on why an in-depth understanding of vertical variability of O_3 formation mechanisms is important? What could this new knowledge mean for the air quality community?