

Atmos. Chem. Phys. Discuss., referee comment RC1  
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## Comment on acp-2022-399

Anonymous Referee #1

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Referee comment on "Composition and reactivity of volatile organic compounds in the South Coast Air Basin and San Joaquin Valley of California" by Shang Liu et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-399-RC1>, 2022

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This study contrasts airborne VOC concentrations and reactivity in the SJV and SOCAB regions of California with a focus on the implications for O<sub>3</sub> in the region. The study is straight-forward and the manuscript was a pleasure to read – well organized and clear. I have only a few comments and suggestions.

- Rather than "total" OHR, I strongly suggest that the authors use the term "calculated", as in for example, Thames et al, ACP, 2020. The manuscript does not include OHR measurements and referring to "total measured OHR" as in the abstract (line 20) could be misleading.
- This study is based on only two flights. It raises the question of representativeness of the measurements shown here. I suggest that the authors discuss this directly in the conclusions and also consider softening some statements, particularly those that rely on the distribution of values. For example, line 140 "O<sub>3</sub> in the SoCAB may be more"
- The authors provide some contrast with previous studies, including CalNex (lines 259-261), but it would be informative to include even more discussion of how the SoCAB measurements in 2019 compare to the conditions in 2010 (and the earlier surface measurements in California listed in Table S1). In particular, it would be useful to discuss the relative decreases in VOCs and NO<sub>x</sub>, the impact on OHR, and any implications for the O<sub>3</sub> production regime. There were also a couple of specific interesting differences from CalNex (as described in Heald et al., 2020) that would be useful to discuss the potential causes of: (a) ethanol:methanol > 2 in CalNex whereas this study reports that methanol concentrations exceed ethanol in the SoCAB in 2019 (b) limonene is below DL in this study (but comparable to methacrolein in 2010) – this seems interesting in light of the emphasis that this study places on the need to understand BVOC impacts on O<sub>3</sub>.

Minor comments/corrections:

- Line 25: "BVOCs were important"
- Line 42: "relatively high O<sub>3</sub> background" – relative to what? I suggest that the authors modify to provide specific numbers (i.e. Fig 1 of Parrish et al. indicates 20-40 ppb, varying by season). Parrish et al., 2017 also show that the background is decreasing which would be worth stating here as well.
- Figure 1: it's not really possible to distinguish the red and blue lines for flight 1 and 2 given the data overplotted. Perhaps for clarity the authors might consider removing those lines in Figure 1 and adding a figure in the SI that more clearly shows the two flight tracks.
- Table 1, entry #1: first name is "Thomas"
- Line 88-89: why is the slope on Fig S1 1.07? One would not expect mean values to be modified by merging to a different time base. Does this also reflect the filtering? If so, perhaps clarify this in the text.
- Line 93: were the higher altitude points also removed from Fig 1? If so, I suggest stating that here.
- Table 2: either footnote c or the top row should indicate that KOH is given at 298K. Footnote c should also state that references for reaction rates are given in the text, as readers may look for those here.
- Figure 4 discussion: The NO<sub>x</sub> OHR vs O<sub>3</sub> relationship is very different between SJV and SoCAB – given that this is shown on the figure, it bears some discussion in the text.
- Line 253: you showed in Figure 3 that "the overall OHR in the SoCAB was 30% higher than in the SJV", so perhaps "confirming as in Fig. 3" rather than "suggesting" would be appropriate here.
- Line 269 vs lines 239-240: Earlier in the text the authors suggest that it's difficult to compare ground and airborne, but then here in line 269 the airborne measurements are compared to two ground sites and a model(!) in other regions of the world. If the authors stand by their statement on 239-240, then perhaps a caveat is needed here.
- Line 276-277: The Steiner et al. (2008) study is representative of conditions over a decade prior to this study, and clearly emissions have changed substantially in California over that time period (as shown here in comparison with previous work), so I suggest re-rephrasing "our measurements support" to "our measurements are consistent with...from over a decade prior."
- Line 346: says "studies" but only one citation provided. Modify for consistency.