

Interactive comment on “Assessment of NO₂ observations during DISCOVER-AQ and KORUS-AQ field campaigns” by Sungyeon Choi et al.

Anonymous Referee #2

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General Comments

This paper assesses NO₂ measurements from two field campaigns for their usefulness in interpreting satellite remote sensing observations. The paper focuses on what these field campaigns can tell us about how spatial resolution and a priori NO₂ profiles affect interpretation of satellite observations. These are important and actively researched questions in the satellite retrieval community. The paper is well written, and the analysis is generally well supported. I have a couple questions listed below, but otherwise I recommend this paper for publication.

Specific comments

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In section 2.1.2, the authors describe how aircraft profiles are linearly interpolated to the surface to meet the surface monitoring measurements. Figure 2 indicates that the merged aircraft profiles decrease towards the surface, while both models show increasing NO₂ towards the surface. It seems that the decrease towards the surface in the merged profiles is due to the interpolation – is there concern that the merged profile decreases towards the surface (which is generally unexpected over polluted areas), or that it has such a different shape than the model? AMF calculations for the satellite observations are sensitive to the profile shape in the lower troposphere, so what impact does this interpolation have on AMF calculations?

I was surprised to see the results discussed in the paragraph beginning line 304, which showed that monthly mean profiles capture the local variability as well as the daily profiles. Using monthly means rather than daily profiles would simplify future retrievals, so I would be interested to hear whether the authors think that this result is particular to the observed locations and seasons or if it can be applied more generally. For example, the only winter observations are in California (which isn't really that wintery), so could monthly mean profiles still be useful in these and other cases?

In section 3.2, the authors derive tropospheric columns from Pandora measurements by subtracting the OMI stratospheric column from the Pandora total column. I wonder whether this approach may be partially responsible for biases between Pandora and the aircraft observations. As a space-based instrument, OMI is more sensitive to stratospheric NO₂, while Pandora has a greater tropospheric sensitivity. How do the authors account for differences in vertically-resolved sensitivity between the instruments? Also, what is the possible effect of subpixel variability in stratospheric NO₂ within the OMI pixel? There's nothing in the discussion that describes potential errors in the stratosphere-troposphere separation.

Lastly, there's a recent paper by Judd et al (<https://doi.org/10.5194/amt-12-6091-2019>) that may be of interest to the authors (I am not affiliated with this paper, but thought it was relevant).

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