Editor Decision: Publish subject to minor revisions (review by editor)

(25 Jan 2021) by Michel Van Roozendael
Comments to the Author:
The comments raised by referee 2 are fully pertinent. There is an obvious flattening of the trends in NO2 and SO2 in recent years, such that linear trends evaluated from the last decade (2011-2019) cannot be used for extrapolating reference values in 2020. Please consider the suggestion of using trends evaluated over the restricted time period 2019-2019 as a basis for extrapolation in 2020. In any case, revise the main conclusions so that quantitative estimates of the COVID impact clearly reflect the flattening of the trends.

We appreciate the considerable patience of the Editor and Referee 2 while making these revisions. For consistency, these required additional analysis and more substantial revisions to the text, which we hope have addressed the concerns about the choice of background and trend period.

We have moved away from emphasizing any single starting year as a basis for background averaging or extrapolation, which was based previously on the R^2 of the trend. Throughout the paper, we have instead emphasized the sensitivity of 2020 differences to the starting year of the background or detrending period, identifying when and where the 2020 differences were significant, the range of these differences, and with what caveats. This shift was made to avoid the difficulties of choosing any single starting year for the baseline period. For NO_2, for example, choosing the 2016-2019 period as a basis is difficult because it is bookended by two years with higher NO_2; which of 2015, 2016 or 2017 to use as a starting year was not obvious to us and inevitably subjective.

This shift can be seen in the revised Results section and summarized in the first five paragraphs of the revised Discussion, with the Abstract revised accordingly. In several cases, we have pointed out flattening for more recent periods, for example NO_2 (L349) and AOD (L360) over central east China since 2016, and SO_2 since 2014 (L633) and AOD for more recent years (L651) over southern China. In Figures 5 and 7, the statistics for what were previously the 'strongest' trend years have been removed. The Supplementary Information now contains Tables S1-S8. Each has the background mean, 2020 difference from background mean, trend information, and expected 2020 differences from the trend, all for starting periods between 2005 and 2018, providing expanded information underlying Figures 6 and 8.

We hope these changes have addressed the concerns of the 2nd Referee and Editor. Overall

Referee 1:
- No comments

Referee 2:
Review of the revised manuscript by Field et al. "Changes in satellite retrievals of atmospheric composition over eastern China during the 2020 COVID-19 lockdowns"

I am glad that the authors acknowledge the obvious flattening of the observed decrease of SO2, NO2 and AOD in recent years. Nevertheless, they persist in using their wrong reference period (2011-2019) as basis for their extrapolation to 2020. I agree that they do present an analysis of the impact of different reference periods (Fig.6). But their abstract and conclusions are still centred on results which ignore the flattening of the trend, e.g. "OMI NO2 in 2020 over central east China was (...) only 17% less than what would be expected from trends", which is misleading.

The flattening is not an accident. Among several possible explanations for it, the most straightforward is that the potential for further reduction obviously decreases when the column decreases. This is something that the assumption of a linear decrease simply cannot capture. A better regression model would be an exponential decrease. This is simple to achieve: only perform a linear fit of the logarithm of the column, as for example in the recent study of Diamond and Wood (2020). Note that the linear trend of -0.056 DU yr-1 inferred for SO2 (Figure 5) corresponds to a relative trend of -14% yr-1 in 2011, which increases to -100% yr-1 in 2018 and explodes in 2019. Using such a basis for extrapolation to 2020 is meaningless (I acknowledge that negative columns are common in DOAS retrievals, but if, as the authors suggest, they are likely "below detection limits", then I think they should not be used for any extrapolation). Things are less dramatic for NO2, but qualitatively similar, as the NO2 column decreases by almost a factor of 2 between 2011 and 2019. Interestingly, the NO2 trend calculated from 2016-2019 data is 3 times lower than the trend calculated from 2011-2019. This implies that even the relative trend has diminished in amplitude over the period. The 2016-2019 period is very likely a much more realistic basis for extrapolation to 2020 than 2011-2019.

In conclusion, I do not require further analysis of the data, but I strongly recommend that the authors do better emphasize the existence and importance of the flattening. One important consequence is that the best estimate of the difference attributed to COVID is larger than is currently claimed in this article.

Reference : Diamond, M. S., and Wood, R., Geophys. Res. Lett., 47, e2020GL088913, https://doi.org/10.1029/2020GL088913, 2020.