

Editor Decision: Reconsider after major revision

09 Aug 2021

Please take into consideration the remaining remarks of the referee, in particular the need to better emphasize the existence and importance of the flattening of the observed decrease of SO₂, NO₂ and AOD in recent years. One important consequence of this, is that the COVID impact estimate is larger than currently claimed in the manuscript.

We appreciate the editor's point about placing more emphasis on the flattening seen in most retrievals during recent years. The Discussion and Conclusions have been changed accordingly; where appropriate, the emphasis has been placed on the differences between 2020 and background means during the different 'flat' periods. The Abstract reflects these changes. At L486, we now conclude by suggesting that follow-up work with additional years of data will help to determine whether this flattening is permanent. The reviewer discussed using an exponential decay curve for regression model. While not 'requiring' this for the revisions, we have suggested this as something to consider in future work at L488.

Regarding the SO₂, we have stated at L292 in the Results that the expected 2020 SO₂ extrapolated from recent trends is not particularly meaningful, which is followed by the discussion of the effects of calculating the daily SO₂ from the median rather than mean retrieved values over the region. We have concluded at L419 that no SO₂ changes could be robustly detected in 2020 in either region.

We hope that these changes have struck the right balance between establishing the need to consider past trends and variability, but that COVID-19 related changes in 2020 occurred against a recent flattening during the preceding years.

(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 NO₂, for example, choosing the 2016-2019 period as a basis is difficult because it is bookended by two years with higher NO₂; 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 NO₂ (L349) and AOD (L360) over central east China since 2016, and SO₂ 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.

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.