

**Fan et al., “Variability of NO<sub>2</sub> concentrations over China and effect on air quality derived from satellite and ground-based observations”, submitted for publication in ACP. Revision 2.**

**Response to interactive comments to revision 1:**

We thank the Editor and Referees for their time to carefully consider our manuscript and provide comments. We have carefully considered the comments and revised the manuscript accordingly, in both rounds #1 and #2, and feel that the manuscript has been improved substantially. Our comments are provided below. The referee comments for round #2 are inserted in red, and our comments to each comment are written in black. In addition to responses to referee comments, some changes have been made in the revised text, and these are also added using “track changes”.

**Referee #1**

I think the author seems to have misunderstood my previous comments. Original comments said “the content of this article hardly supports the title”. Authors should supplement the content of the manuscript to echo the title, not reduce the content and change a title. Compared to the original manuscript, the current version is indeed largely up to the standard of publication, but still far from the ACP standard. The biggest shortcoming is that the current manuscript is not innovative enough. It is more of a continuation exercise than a new breakthrough. Unfortunately, I cannot recommend accepting this manuscript.

In the first revision we have responded to the comments of both reviewers and explained the revisions in our rebuttal. We have acknowledged that the previous “title did not fully cover the content”. In such case, either the content could be changed, or the title. We decided to do the latter. In addition, as suggested by both Referees, also the content was reduced and structured.

In the comments to revision 1, Referee #1 mentions that “the current manuscript is not innovative enough” but does not explain what “enough” is and that “it is more of a continuation exercise than a new breakthrough”. The referee has not mentioned this in the first review, and we strongly disagree with this comment. We have clearly listed our research questions and described in detail the approach taken to answer them, then discussed the results and provided conclusions to each research question (points 1-6 in Sect. 5). Indeed these may not be a breakthrough, but they are a solid piece of research, starting from earlier work and providing new results which are of interest for the wider AQ research community. We also discuss the implications of these new results. They are applicable, and addressing differences, over a large area with a variety of conditions (socio-economic and geographical and meteorological and climatological) and the results are also applicable over other areas than China. Hence, we do not understand these comments by Referee #1. In particular, we disagree that the manuscript does not meet “the ACP standard”, and unfortunately the Referee has not indicated what this is either.

**Referee #2:**

The authors have made an excellent job both in addressing and replying both reviewers’ comments as well as altering their manuscript to now better demonstrate the status of air quality over East China. I highly commend them on that. Having said that, I am still concerned about the presentation of their findings.

We thank referee #2 for the kind and constructive comments to further improve the manuscript. We feel that indeed the suggested changes have contributed to better presentation of the study and its results. Responses are provided below, after each comment. Line numbers refer to the tracked pdf version of the revised manuscript (file: Fan-et-al-ACPD-Rev2\_Final-tracked)

1. I would expect to first see the back-in-time discussion for the NO<sub>2</sub> levels, i.e. Section 3.1.2, on OMI, and then what happened during 2020 and COVID, by TROPOMI, i.e. Section 3.1.1. It makes more of a natural flow of information this way, especially since you do not have back-in-time analysis for the AQI/surface concentrations.

Thank you for this comment. Indeed it makes sense to first go back in time and then concentrate on recent events. Also as regards the reduced concentrations in response to policy measures and the sudden change in response to COVID-19 measures. We have switched Sections 3.1.1 and 3.1.2 as suggested. For clarity, we have not tracked changes while switching. For further changes in these texts we did use “tracking”: after switching, changes were needed in these 2 sections for a logical and chronological storyline. Also some text was added to the Introduction to further emphasize this (lines 73-80). Figure numbers have been changed and we have carefully checked that also in the rest of the text these changes were made.

2. Figure 4 shows OMI NO<sub>2</sub> up to and including months of year 2020, these should be excluded since you do not plan to discuss them. Overall the quality of the graph is poor, make sure all labels increase in size, are made bold, and so on.

Thank you for this comment. The quality of Figure 4 (Figure 2 in the revised version) has been improved and the time series now ends in December 2019.

Furthermore, when a trend is calculated from such a seasonally-varying species as is NO<sub>2</sub>, the time series has to be de-seasonalised beforehand. This is not explicitly mentioned in the text. I strongly suggest that you first de-seasonalise and then provide trends in the relevant Table.

Thank you for this comment. Instead of de-seasonalizing we had used annual averages. In the current version (revision#2), we have de-seasonalized the monthly time series using a centered moving average with a period of 12 months, before calculating annual mean values (now mentioned on line 294-297). We decided for the annual mean values to reduce effects of variations due to other influences than the seasonal effects. Time series plots of the annual mean NO<sub>2</sub> TVCDs, without the trend lines, are presented in Figure A1, to better illustrate our point that there are differences between the time series in the north and in the south. Then the trends were re-calculated for certain intervals (Table 2, and described in the text) added to the annual mean time series (Figure 3 in the revised version) and the new trend values are presented in table 2. The features in the time series and trends were similar to those in the previous version, with some small changes.

3. Since in the rest of the text you show weekly timeseries for the AQI/surface concentrations for the COVID weeks, I strongly recommend you also show, in the TROPOMI section, similar timeseries. If not, again your results appear to be “un-connected” i.e. you first have one satellite giving you the 10 year NO<sub>2</sub> trend, then another giving you what went on during COVID for the 2020 weeks, then you have surface concentrations of NO<sub>2</sub> and other species, plus AQI, only for the COVID 2020 weeks. I am not suggesting that you compare TROPOMI NO<sub>2</sub> to the surface concentrations, this task is a paper on its own. You should however show if similar patterns appear. These patterns cannot be seen from Fig. 3 & Fig A1 which are demonstrational only.

Thank you for this suggestion, it was a very good idea and Section 3.3 has been added with a Figure 9, showing the weekly averaged NO<sub>2</sub> concentrations as time series for week -3 to week 20, both for TROPOMI TVCDs (averaged over each region) and the ground-based data (replotted from Figures 6-8 and 10). We do this comparison only for the selected regions in each of the groups 1-3, and Beijing, for clarity and easy comparison. Adding more regions made Figure 9 too full to be clear. The comparison is only qualitative since a quantitative comparison would require the consideration of meteorological condition (through a model). Nevertheless, the qualitative comparison shows some interesting differences and similarities, as described in Section 3.3. We have also added some text in the Abstract (line 37-39), Introduction (lines 150-151) and Summary and Conclusions (lines 827-829) to this effect.

Overall, from the time series provided, I note that the AQI is mostly influenced by PM<sub>2.5</sub>, rather than the variation on NO<sub>2</sub>.

This indeed is true, and was mentioned in Sect. 4.3 lines 735-736 (“However, the latter were not reflected in the AQI, which followed the variations in PM<sub>2.5</sub> but remained low when O<sub>3</sub> concentrations were high”). We realized that this comment was somewhat hidden in the text and therefore we have added some words reflecting this at other places: Abstract (line 37-39), Discussion (lines 770-771) and Summary and Conclusions (Lines 873-874).

Technical: throughout the text there are references to a Figure/Table in the supplement, I assume this was a typo error

Thank you for this observation: indeed it was a typo remaining from our initial editing, before we had read in the Instruction that it should be called Appendix. We have checked the manuscript and corrected these typos.

Figure A1 should be made with a colour bar that reflects differences, i.e. going from blue to white - which means 0- and then to red

This is not correct, we subtracted the TVCD map for each week from that for week 0, and hence green colours indicate a decrease with respect to week 0, red colours an increase, but it is still a TVCD. This was also explained in the text in Sect. 2.1 (lines 165-166), where we describe Fig. 1. To make it clear, we have added this information to the caption of Figure A2.

Figure A2 is of low quality and should be improved.

Thank you for this comment. We have replotted Figure A2 (Figure A1 in the revised version) similar to Figure 5 (Figure 5 in the revised version).