Response to the interactive comments

Response to the interactive comments of anonymous Referees #1 and #2 on "Does reduction of emissions imply improved air quality?" by Cheng Fan et al. on behalf of all co-authors.

New title: Variability of NO_2 concentrations over China and effect on air quality derived from satellite and ground-based observations

Interactive comment on "Does reduction of emissions imply improved air quality?" *by* Cheng Fan et al.

Anonymous Referee #1

Received and published: 29 December 2020

I have reviewed the manuscript "Does reduction of emissions imply improved air quality". In this work, authors used TROPOMI tropospheric NO2 together with ground- based monitoring data in 11 regions around large cities to evaluate the evolution of their concentrations during 19 weeks after the Spring Festival and their effect on air quality. Based on the review criteria of ACP, my comments are as follow.

R: We thank the reviewer for the time spent on thoroughly reading the manuscript and providing constructive comments. Below we repeat the comments and we structured them in separate paragraphs to systematically provide a response to each of them (in italics and red).

Because of the many changes, including both new text and deleting text and figures, the inclusion of all changed parts would require to append almost the whole revised text. Therefore, we provide line numbers referring to the clean version of the revised text, which will be uploaded when the editor invites us to do so. Shorter texts are copied below.

General comments:

1, the content of this article hardly supports the title. The title of this paper has an implicit meaning that a significant reduction in emissions occurred during the closure period. Generally speaking, it is right. However, the difference in the degree of reduction of emissions of different species directly determines their impact on the atmospheric environment. This work only provides evidence on the reduction of NOX, which has already been reported in many previous works. Reductions or variations of other pollutants are necessary to evaluate Changes in emission sources.

R: We agree with the reviewer that the title did not fully cover the content. Although several publications point at the reduction of emissions, we did not determine emissions, we only used concentrations (ground-based), or tropospheric column densities (from satellites OMI and TROPOMI). We disagree that we only showed reduction of NO2, we also showed other species in the ground-based data. However, we do feel that indeed not all species show a clear effect on the air quality (or AQI). Therefore we removed SO2, CO and PM10 (including figures) and only focused on NO2, O3 and aerosols (PM2.5) which are interconnected through chemical processes as explained in the revised introduction, while also the effects of socio-economic changes during the lockdown were different for NO2 and PM.

In response, we have ***changed the title*** *to "Variability of* NO_2 *concentrations over China and effect on air quality derived from satellite and ground-based observations" and* ***rewritten most of the Introduction*** *(Lines 42-127) with a better focus on the actual work presented in the manuscript, and removed most of the general descriptions of the lockdown effects. We also* ***changed and shortened the abstract*** *(Lines 18-41), and made* ***quite some changes throughout the whole text****, and Sect. 5* ***Conclusion*** *was changed to Sect. 5* ***Summary and conclusion*** *(Lines 581-657). We* ***moved the many AQI time series plots to the Appendix*** *(Figure A3).*

2, This work posed more questions than it answered. Actually, in February and March 2020, many researchers in China, even including many average Chinese, had already intuitively noticed that the air quality does not seem to have improved significantly after the pandemic lockdown, and there are even signs that it is getting worse. After June, such a feeling was confirmed by many research papers. Now, people are now curious as to why this phenomenon has occurred. I would encourage authors to answer this question but not to "discover" it again.

R: We agree that many papers have been published on the lockdown effects of the concentrations of pollutant concentrations in China and indeed some of them specifically focused on the reduction of air quality. We have referenced the papers that we were aware of (probably not all of them and we may have missed some, we have added some more recent papers in the revised version). We also note that these papers discussing the AQ getting worse, mainly dealt with the air quality in the NCP: different reasons for the reduction of air quality, in spite of the enormous reduction of NO2 concentrations (or TVCDs), were proposed. These were mentioned in the initial version of our manuscript, but probably not explicitly enough. We certainly did not want to claim that we "discovered" this phenomenon. In the revised version we discuss this subject more extensively in the introduction (first paragraph), together with references to published papers (Lines 54-69).

Furthermore, we notice also (Introduction) the connection between the reduction of NO2 concentrations and the increase of O3 and the secondary formation of aerosols, which is the reason to focus in the revised version on these three species. We realized that the abundance of information on other species was distracting from the main work, and therefore removed most of that. (Lines 54-69; 109-114).

We also have explained more explicitly the reasons for extending our study (Fan et al., 2020) but we removed much of the detail. The shorter version can be found in lines 76-94. At the end of the introduction we summarized these reasons in four clear objectives (Lines 115-122)) and refer to these in the conclusions (Lines 607-648).

In summary, we do not "discover" again the lack of improvement of air quality, but present the data in detail and discuss the effects of pollutants on AQ (as expressed by AQI), i.e. whether there was indeed improved AQ and if it was improved, how long did it last?

Specific comments:

1, the introduction is lengthy and lacks the necessary logic, which makes it a bit hard to follow. Besides, Lines 69-81 are not relevant with this work. Maybe, authors can supplement a table in the Appendix or section 2 to describe lock- down measures and period in different regions.

R: As mentioned in our response to the general comments, the introduction has been rewritten extensively with a focus on the actual study presented in this manuscript. Less relevant text has been removed including summaries of the spreading of the COVID virus and the lockdown measures. In fact we did not really use this detail in the study and so we decided that it is not useful to provide it. The detail can be found in other publications and we cited some which are most relevant.

2, the study area is called as "east China" as shown in figure 1. I think the co-called "east China" covers parts of North, Northeast, Central, East, South, Southwest and even Northwest China. When I saw the term "east China", I thought this work focused on Yangtze River Delta.

R: In the submitted version, east China was defined as the part of China East of the Hu line. This was a bit hidden in the text and probably the reviewer did not notice this. Therefore, we now define the study area explicitly and have also drawn the Hu line in Figure 1: "In the current study we focus on the part of mainland China east of the HU line (Figure 1), further referred to in this paper as east China, where 94% of the Chinese population lives (Chen et al., 2016)." (lines 130-131, Figure 1)

3, regarding extraction of baseline of NO2, authors should utilized some professional analytic tools rather than simply averaging. See "Long-term trend and variability of atmospheric PM 10 concentration in the Po Valley".

R: We are aware of this publication by Bigi et al. (2014). However, we used methods which are commonly used by others for the purpose of determining trend lines (e.g. Sogacheva et al., 2018, https://doi.org/10.5194/acp-18-16631-2018). We note that the current study is not intended to provide accurate trends but rather to indicate the effect of the use of earlier data on the determination on a baseline value.

4, similar with comment 3, it is necessary to carry out hypothesis tests to draw conclusions on whether air quality returned to the normal level. Simply showing the change curve does not lead to any statistically significant conclusions.

R: The "normal level" differs for each country and region. Here we do not refer to AQ standards but to the AQ before the lockdown, where we use the levels in 5 previous years (2015-2019) as reference. And likewise for the concentrations of species contributing to AQ. This is mentioned in Sect. 3.2.1 (Lines 34-336): "AQI time series for the same weeks in the five previous years (2015-2019) were plotted to form a plume which serves as reference for the 2020 time series.") and 3.2.2 (Lines 362-364: "AQI time series for the same weeks in the five previous years (2015-2019) were plotted to form a plume which serves as reference for the 2020 time series.").

5, the quality of figure 5 is too low to be published in an academic journal.

R: We apologize for the bad quality of the original Figure 5. We have re-drawn Figure 5 and added some explanation in the caption. We hope that the reviewer is satisfied with the current version. In addition, we also improved Fig A2, in the same style as Fig. 5

6. This work hardly provided evidences regarding changes in meteorological conditions. As far as I know, meteorological conditions play a role in determining air quality that is nearly as significant as that of emissions.

*R: We have mentioned the effect of meteorological conditions many times throughout the whole manuscript. We have not explicitly taken meteorological effects into account. This requires the use of a transport model which is not in the scope of this study. Instead, we have used averages to reduce effects of meteorology on our results, such as the 5-year plumes in the ground-based time series (see response to comment 4) or the annual averages in the decadal time series for NO_2 TVCDs (Lines 282-284: "*To further investigate trends in different regions and the differences between them, time series were plotted for each region and, to reduce effects of short term (monthly) variations, this was done for annual mean NO_2 TVCDs."). *Furthermore, a brief discussion was included on meteorological influences. To give this more emphasis, the title of Sect. 4.2 has been changed to "*4.2 Long term trends, trend reversal and meteorological influences on the estimation of lockdown effects". Meteorological influences are discussed in lines 4982-498.

Interactive comment on **"Does reduction of emissions imply improved air quality?"** ***by*** **Cheng Fan et al.**

Anonymous Referee #2

Received and published: 2 January 2021

We thank the reviewer for the time spent on thoroughly reading the manuscript and providing critical comments. Below we repeat the comments and we structured them in separate paragraphs to systematically provide a response to each of them (in italics and red).

Because of the many changes, including both new text and deleting text and figures, the inclusion of all changed parts would require to append almost the whole revised text. Therefore, we provide lines numbers referring to the clean version of the revised text, which will be uploaded when the editor invites us to do so. Shorter texts are copied below.

We do agree with many comments and in response have substantially edited the manuscript, whereas we do not agree with some other comments. However, these comments may have been made because the initial version of our manuscript was not clear enough. With the revised version, in response to both reviewers, the text more clearly explains our reasons for the research undertaken, resulting in 4 objectives, how these were addressed, and what we concluded from this work in relation to the initial research questions.

The introduction section is too long and moves from one topic to the next, mixing them at points in a confusing manner; there is a discussion on the current state of the air quality over China, a discussion on other COVID-19 related works, a discussion on aerosols, SO2 and CO, mixed with a discussion on how the measures were enforced, all of them stating names of cities and provinces with which most readers are not familiar with. In lines 82 to 92, references are given to other works as big chunks when, most of these works do not report similar findings, nor are they based on similar methodologies. Then exhaustive details are given from the Fan et al., 2020a, paper which again mention other species and confuse the issue, in a rather long paragraph. As a result, I am already perplexed as to what this article is adding to our current knowledge on air quality effects in China due to COVID-19.

R: Thank you for this comment. We agree that the Introduction was too long and not to the point. We have ***revised most of the introduction and removed much text*** *to provide a more clear and logical storyline to introduce our work (Lines 42-127). We also* ***changed the title*** *to better reflect the contents of the manuscript: "Variability of NO_2 concentrations over China and effect on air* *quality derived from satellite and ground-based observations". The manuscript now focuses mostly on NO2 variations on decadal (2011-2019) and weekly (the lockdown period and thereafter) scales. In addition we added O3 and PM2.5, for reasons explained in the text (variation, effect on AQ) (Lines 54-69; 109-114) and removed SO2, CO and PM10 which do not add information or do not show much variability (CO and SO2) (Lines 54-69; 109-114). We also* ***changed and shortened the abstract*** *(Lines 18-41), and made* ***quite some changes throughout the whole text****, and the* ***conclusion*** *was changed to* ***summary and conclusion*** *(Lines 581-657). We* ***moved the many AQI time series plots to the Appendix*** *(Figure A3).*

*We realize that readers may not be familiar with names and locations of regions or cities in China. However, a map is provided in Figures 1 and names are listed in Table 1: "*The names of the regions shown in Figure 1 and their geographical locations are listed in Table 1 and include well-known centers such as …" (Sect. 2.1, Lines 140-143). *In the revised version we have made frequent reference to this information.*

In the text, SO2 is greatly discussed and I fail to see why the author's didn't also analyse SO2 as part of this study, in line with the manuscript title which refers to air quality in general.

R: SO2 figures and text have been removed as indicated in the response to the previous comment, and as discussed in more detail in the revised manuscript (Lines 88-93).

Two different satellite sensors' tropospheric NO2 VCDs were used as data input for the manuscript's hypothesis. It is well known that the two sensors have a bias in their findings, not only due to the different spectral/spatial characteristics, but also due to the algorithm. How was this bias accounted for? Even for cases of the same algorithm being used on different satellite records, a careful homogenisation is needed to be able to discuss trends in a meaningful manner, see for e.g. the work of Georgoulias et al., 2019, ACP - Trends and trend reversal detection in 2 decades of tropospheric NO2 satellite observations (copernicus.org). The section on satellite data is hence mismatched. The TROPOMI are shown as maps of periods in 2020, the OMI data as timeseries analysis over different regions for a different time period, not including 2020, without the two actually coming together to a coherent conclusion.

*R: We agree that if we would use the two instruments in the same analysis we should first homogenize these data sets. Especially since these two instruments have another resolution, a homogenization as in Georgoulias et al. (2019) would be needed. However, in our study the measurements of the two instruments are not combined. TROPOMI data is used to analyze 2020 data only, to study the effects of the COVID-19 regulations in section 3.1.1. In section 3.1.2 only OMI data is used to derive trends for the period 2011-2019. This information is added in the (new) pre-amble to Sect. 2.2: "*Two satellite products were used in this study, i.e. the tropospheric NO_2 vertical column densities (NO_2 TVCDs) from OMI and TROPOMI. These products are briefly discussed in the following sub-sections. The OMI NO_2 TVCDs were used for time series analysis over the period 2011-2019, the TROPOMI NO_2 TVCDs, with better spatial resolution, were used to visualize weekly averaged spatial variations of the NO_2 TVCDs and discuss their evolution over the study area. OMI and TROPOMI products thus provide complementary information for different periods of time and were used for different purposes.*".*
To avoid any misunderstanding, the source of the data (OMI or TROPOMI) is now indicated in the captions of Figures 2-5.

The manuscript then provides a long discussion on air quality over different Chinese locations based on the AQI calculated from ground-based stations. This section does not merge in the least with the rest of the text, nor with the abstract and the satellite section. Overall the feeling is that this manuscript started as an AQI-based paper with the satellite analysis added on top without the two merging.

R: Thank you for this comment. In fact, the study started out with the satellite work, in particular the TROPOMI data which formed the basis for the further continuation of the study, with questions on the temporal resolution (months, weeks) and the actual starting point which provides the base line for the evaluation of the lockdown effect and how to separate this from the reduction of the initial reduction of the NO2 concentrations during the Spring Festival holidays. We then added the ground-based data for more detail on the effects on AQ.

*Probably this was not clearly presented in the original manuscript and in therefore, in the revised manuscript we have described this more clearly (Lines 97-101: "*In the current study we address the question what is "normal", using satellite observations over the last decade over selected regions, extending to 16-20 weeks after the 2020 Spring Festival. In addition to satellite data, we use ground-based observations from the Chinese air quality monitoring network providing detailed information in different regions, and compare those for 2020 with similar observations in the last 5 years (2015-2019).").

The discussion section reads out of sequence as well, since the reader is forced to go back and forth to figures presented in previous sections. Many parts are also non-sequiturs where they should appear first in the text, such as the meteorology effects. The continuous references to the work of Fan et al., 2020a, leads the reader to think that this manuscript aspires to be a Part 2 of that work.

*R: We do not quite understand this comment. It is common to first present the data and explain them in the "Results" section, after which they are discussed in the "Discussion" section. This follows the instruction on the ACP website (https://www.atmospheric-chemistry-and-physics.net/submission.html#manuscriptcomposition), point 5: "**Sections**: the headings of all sections, including introduction, results, discussions or summary must be numbered".*

*Meteorological effects are not a main part of the work but need to be accounted for. Therefore, we have mentioned the effect of meteorological conditions many times throughout the whole manuscript. We have not explicitly taken meteorological effects into account. This requires the use of a transport model which is not in the scope of this study. Instead, we have used averages over the 5 previous years as reference, i.e. the 5-year plumes in the ground-based time series, where the plume includes effects of meteorology, or the annual averages in the decadal time series for NO_2 TVCDs. This is mentioned in Sect. 3.2.1 (Lines 34-336): "*AQI time series for the same weeks in the five previous years (2015-2019) were plotted to form a plume which serves as reference for the 2020 time series.*") and 3.2.2 (Lines 362-364: "*AQI time series for the same weeks in the five previous years (2015-2019) were plotted to form a plume which serves as reference for the 2020 time series."). *Furthermore, a brief discussion was included on meteorological influences. To give this more emphasis, the title of Sect. 4.2 has been changed to "*4.2 Long term trends, trend reversal and meteorological influences on the estimation of lockdown effects". Meteorological influences are discussed in lines 4982-498.

This study follows up on our earlier work we published in Fan et al (2020) so it is logical that it is referenced. However, we have substantially reduced the text which summarized Fan et al. (see Introduction, line 76-94).

I suggest to the co-author team to consider what the main take away message should be and re-write/re-present their work accordingly.

*R: At the end of the "Introduction", Lines 115-122, we have formulated 4 objectives and lines 123-127 describe how these are addressed. The "Discussion" sections follows the same structure. In the final section "Summary and conclusions" we start with a brief summary of work done and then structure Sect. 5 "Summary and conclusions" with bullets addressing these objectives. In particular we wrote: "*To answer these questions, satellite and ground-based data were analyzed for 11 regions in east China, leading to the following conclusions.*" (Lines 605-607), and then listed these bullets (Lines 608-649).*
