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

Anonymous Referee #2

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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. In the text, SO2 is greatly

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Discussion paper

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. 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. 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. 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. 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.

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