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Atmospheric Measurement Techniques Discussions

Interactive comment on "Can a regional-scale reduction of atmospheric CO_2 during the COVID-19 pandemic be detected from space? A case study for East China using satellite XCO_2 retrievals" *by* Michael Buchwitz et al.

Anonymous Referee #3

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The manuscript "Can a regional-scale reduction of atmospheric CO2 during the COVID-19 pandemic be detected from space? A case study for East China using satellite XCO2 retrievals" attempts at detecting a significant anomaly of Fossil Fuel (FF) CO2 emissions in East China due to the impact of the covid-19 crisis from available satellite XCO2 data. The study proposes a relatively simple approach to derive monthly FFCO2 emission from averages of spatial anomalies in XCO2 data.

It properly assesses the significance of the derived changes in emissions at the beginning of 2020 by comparing results from different XCO2 datasets and by analyzing

the temporal variability of the estimates of monthly emissions over different years. It raises careful conclusions regarding the current potential to detect large decreases in the emissions such as that caused by the covid-19 crisis. This study should contribute to the understanding of the current capabilities and needs for the monitoring of the CO2 emissions from space.

However, I have several major concerns regarding the analysis and I think that they should be addressed or at least discussed properly before the study can be published.

1) The main one is about the linear regression used to convert averages of spatial anomalies of XCO2 into monthly FF CO2 emissions. The result of the regression (p7 l209) shows an offset (7.1GtCO2/year) which is close to the total emissions of the studied area (p6, l181). Therefore, the term proportional to the spatial XCO2 anomalies is much smaller. Assuming that natural fluxes (and other types of anthropogenic emissions) are null during the period of analysis, the spatial anomalies of XCO2 should be proportional to the FF emissions in the area. The explanations for getting an offset should be, in principle, perturbing factors such as some atmospheric transport effect or the other types of fluxes. However, I feel that these could hardly explain such a large offset dominating the total FF CO2 emissions estimates. I may have misunderstood something, but I think that the authors need to provide some explanations for this large offset. Actually, lines 212-221 p7 stress rather than explain the problem. If this offset challenges any physical explanation for the linear regression, the authors should justify why the linear regression could provide meaningful results.

2) I hardly understand how the derivation of spatial anomalies of XCO2 based on the crude extraction of median values over latitudinal bands could provide useful quantifications. The complex spatial distribution of fluxes, the complex combination between fluxes and atmospheric transport and the complex distribution of satellite data in space and time (far from homogeneous) makes it hardly meaningful physically, unless, again, I have misunderstood a critical point regarding this computation. "The background" is a misleading term which do not really mean anything by itself. Line 155 p5: which

"trends and seasonal variations" do they want to remove ? why would it isolate "CO2 source/sink signals" ? I do not find any real rationale behind p5 l158-159 ("the median was chosen because..."). I assume that such anomalies can be highly misleading as strongly suggested by figures 1 and 2. Should we read from line 27-28 p1 or line 172-173 p6 together with these figures that some of highest sinks of CO2 occur in Himalayas and Sahara ? and that there is not any large sink region within North America ? I would be curious to see where are the locations corresponding to the median values per latitudinal band, and in particular the comparisons between these locations in CT2019 and in the XCO2 data. A computation of the anomalies of XCO2 over East China against upwind areas or at least against concentrations in neighbour regions could probably make more sense. Could the offset arising from the linear regression (see (1)) be a consequence of this computation of the anomalies ? The fact that previous publications have used such computations does not appear as a sufficient justification to me. I think that the authors should better support the suitability of this approach for the analysis conducted here. Given its simplicity, its lack of theoretical basis and its weaknesses, does this computation really deserve a label such as "DAM" which suggests that it is a well proven standard approach for analyzing XCO2 data ?

=> (1) and (2) feed the conclusion raised by the authors themselves that their method could have been too simple. This could appear as the main reason for the lack of ability to extract a clear signal from the covid-19 crisis. The authors need to have and share a stronger confidence in the relevance of these computations.

3) a) Even though it is mentioned from time to time in the discussion (it often comes too late), the spatial and temporal scale of analysis is not really properly characterized, justified, and discussed while it has critical consequences on the outcomes and conclusions of the study. The introduction provides numbers at scales that are not really defined, probably mixing different scales e.g. at lines 100-102 and l110 p4. Comparisons between the FFCO2 signal "at regional scale" and the noise on individual XCO2 data hardly makes sense (e.g. l103-104 p4 and 358-360 p12). Stating that averaging

is needed to decrease the random noise (line 105 p4) is surprising: there is no need to average data over the whole East China and 1 month to ensure the strong reduction of the noise, and appropriate analysis at fine scales should denoise the data much better than simple averaging. The large scale ("regional") XCO2 fields are often dominated by the signal from CO2 natural fluxes and the signal from FFCO2 emissions is generally sought at fine spatial scales. Enlarging the scales of analysis can bury the signal from FFCO2 emissions in that of natural fluxes. One could have negative conclusions with "regional scale" analysis and positive ones from analysis at finer scales as acknowledge a bit late by the last lines of the manuscript. b) Even if East China has a relatively high large scale signal from FFCO2 emissions and even if the analysis are focused on the late fall - early spring period, figure 4 strongly suggests that large patterns from natural fluxes overlap the area of analysis, which could strongly bias the linear regression. Biogenic fluxes and anthropogenic emissions follow a seasonal cycle with a very similar phase and thus the regression may be able to fit the general variations of FFCO2 emissions by assimilating data with a strong signal from the biogenic fluxes. A better care for such a source of uncertainty needs to be taken.

4) The authors claim several times that their method is "data driven" and does not rely on a priori knowledges about the fluxes and atmospheric transport. This is contradicted by the use of the CT2019 system to derive the linear regression between XCO2 spatial anomalies and FFCO2 emissions. This is a critical point: the authors do need to simulate the link between emissions and concentrations using a proxy for the atmospheric transport and some assumptions regarding the spatial distribution of the FF emissions and the other fluxes. Rather than ignoring the atmospheric transport and a priori knowledges on fluxes, they use a crude proxy for it, which could bring large uncertainties to the analysis.

5) In principle, the coarse spatial resolution of CT2019 could be a problem for the analysis of the signal from FFCO2 emissions in XCO2 data which have much finer spatial footprints. How are the XCO2 values derived from this model ? When applying the

"DAM" to CT2019 and then deriving the linear regression from this system, is CT2019 subsampled at the satellite locations ? if yes, why would there be one equation only for the linear regression while the authors apply it to four satellite datasets with different samplings ? if not, can the linear regression from full emission and concentration fields really apply to the satellite data, which have a sparse spatial sampling ?

6) Some parts of the text have been written too quickly (with some lack of clarity, repetitions, copy paste of paragraphs from other publications to describe the model or methods, some typos...). The main text should not detail the color code of the figures (the figures should be clear enough). I think that there are too many figures inserted in the main text. Some could be moved to supplementary material. Others could be merged together into synthetic figures.

Secondary points:

- the authors indicate that this study is the "first attempt to determine whether a regional-scale reduction of anthropogenic CO2 emissions during the COVID-19 pandemic can be detected using space-based observations of atmospheric CO2" (p1 l20) -> given the speed of some scientific studies and the rapid publication of papers, especially during the covid-19 crisis, this claim may not hold. It may depend on the precise definition for the term "regional scale" used here. In any case, such a fact should not lower the need for strong justifications for the simple and quick analysis methods applied here (see (1) and (2)).

- a source of misunderstanding when discussing increase of decrease of emissions due to covid-19 is whether comparisons apply to previous months or to identical calendar month of the previous year(s). Things are even more complex here where changes from year-1 to current year of changes from month-1 to current month of emissions are analyzed. Even though it may hamper the fluence of the text, the description of the anomalies should systematically be clear regarding this if the context of the sentence does not help (e.g. line 42 on p2, line 83 p3, line 352 p12).

- Should we understand from this paper that the anomaly of FFCO2 emissions due to covid-19 can be quantified from the TCCON network (line 94 p3) but not from the XCO2 GOSAT and OCO-2 data even though the TCCON network is too sparse to assess the satellite data accuracy at regional scale in East China (end of introduction) ?

- The "complementarity" between OCO-2 and GOSAT (line 126 p4) could be further discussed. So far, I feel that there has not been much expectations regarding the ability of GOSAT to allow for the quantification of FFCO2 emissions, while various studies have attempted at quantifying FFCO2 emissions based on the OCO-2 data.

- l162 to 169 p5-6 display a loose discussion on whether the width of latitudinal bands is important or not, with some opposition between l163 vs. l162 and 168-169. The "good agreement" discussed on line 169 sounds qualitative while the anomalies will be used for the quantification of emission temporal variations in East China. Some quantitative assessment of the sensitivity to the width of latitudinal band would have made more sense.

- isn't the notation "deltaXCO2FF" for the estimate of the emissions misleading ? this is an estimate of the total FF emissions, not of the FF component of the XCO2 anomalies.

- table 1: should not the labels "generated by the authors (#)" be changed ? table 3 is useless, the coordinates should just be given in the text.

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