

Interactive comment on “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” by Michael Buchwitz et al.

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Many thanks for taking the time to review our manuscript and for providing very useful feedback. Your comments and the comments from the other two referees have been carefully taken into account when generating the revised version of our manuscript. Please see below our response to each of your comments.

Referee: 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 us-

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ing 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 analysing 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.

Author's response: For the revised version of our manuscript we did our best to consider all your comments, and our detailed point-by-point response is given in following.

Referee: 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

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authors should justify why the linear regression could provide meaningful results.

Author's response: We agree that this requires additional explanations. You are right that "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 problem is in fact related to the biogenic fluxes. To better address this, we will replace the relevant figure (Fig. 5) with an improved one which also shows other relevant quantities contained in the CT2019 data set, especially we will add XCO₂ anomalies due to fossil fuel (FF) and biogenic (BIO) fluxes separately, and we will also modify the text significantly. The reason for the large offset is the influence of the biosphere. We will show in the revised version of the paper that during October to May the XCO₂ anomalies are mostly dominated by FF emissions but of course the BIO fluxes are not zero. Especially around May the uptake of atmospheric CO₂ due to the biosphere is so large that the XCO₂ anomalies due to BIO fluxes are on the same order (but with an opposite sign) as the XCO₂ anomalies due to FF emissions (which are always positive), i.e., the XCO₂ anomalies can be close to zero despite positive FF emissions (or the FF related XCO₂ anomalies). To deal with this, the offset is needed. Of course our method is not perfect and we have to quantify the error of our method (and we do this). Also this aspect will be improved for the revised version of our paper.

Referee: 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 1158-159 ("the

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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 ?

Author's response: We have defined in our paper how "The background" is computed and how it is used. We therefore cannot see why this is "a misleading term which do not really mean anything by itself". Nevertheless, for the revised version of the paper, we will add additional information to further improve this aspect. Concerning "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.": Hakkarainen et al., 2019, show detailed comparisons with model simulations (from multi-year averages to seasonal maps etc.) and we see no need to repeat this. However, the comparisons reported in Hakkarainen et al., 2019, are mostly qualitative. Therefore, in our publication, we have investigated to what extent the resulting XCO₂ anomalies can be used quantitatively to obtain emission estimates. For this purpose we used the CT2019 model data set and processed this data set in the same way as we process the satellite data. Concerning your proposal "A computation of the anomalies of XCO₂ over East China against upwind areas or at least against concentrations

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in neighbour regions could probably make more sense.”: Computations of upwind area anomalies is an option but would require an entirely different method including addressing the questions how to define upwind area anomalies, etc. But to address your second proposal we will add results from a second method, not based on latitude band medians. Instead we compute the background from the area surrounding the target region. This makes it even more important to label the approaches. In addition to the DAM method we will refer to the second method as TmS (Target minus Surrounding) in the revised version of the paper. When the linear regression is applied to the anomalies computed with the second method, we still obtain a large offset. The answer to your questions “Could the offset arising from the linear regression (see (1)) be a consequence of this computation of the anomalies ?” is therefore No (we will show that the numerical values of offset and scaling factor change somewhat but not dramatically). As explained above, the main reason for the large offset are the large biogenic fluxes.

Referee: => (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.

Author’s response: Concerning “The authors need to have and share a stronger confidence in the relevance of these computations”: Please see our response to your comments (1) and (2) above. Concerning: “This could appear as the main reason for the lack of ability to extract a clear signal from the covid-19 crisis.”: We do not think that this is the main reason. As explained in our paper, we think that a main reason is the weakness of the signal (which is only 0.1-0.2 ppm). After submission of our manuscript other publications appeared using different approaches to find out to what extent satellite XCO₂ retrievals can provide COVID-19 related CO₂ emission reduction information (see Chevallier et al., 2020, Tohjima et al., 2020, and Zeng et al., 2020, now also cited in the revised version of our manuscript). The findings of these studies are consistent with the conclusions drawn in our manuscript. This is not a proof that

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the limit has already been achieved via our analysis, but this is a strong indication that a significant information loss due to the simple data-driven analysis method used in our publication is not very likely.

Referee: 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 I110 p4. Comparisons between the FFCO₂ signal "at regional scale" and the noise on individual XCO₂ data hardly makes sense (e.g. I103-104 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.

Author’s response: Concerning “the spatial and temporal scale of analysis is not really properly characterized, justified, and discussed”: We will specify this now better

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in the Introduction section where we will write: “Here we use an ensemble of satellite retrievals of XCO₂ to determine whether COVID-19 related regional-scale (here ~20002 km²) CO₂ emission reductions can be detected and quantified using the current space-based observing system.” Furthermore, the coordinates of the East China region is specified in Tab. 1. Why we selected this region is also explained in our paper. The scales mentioned at lines 100-102 is consistent with our definition. The scale relevant for the sentence line 110 is a different one as we here refer to individual satellite ground pixels (footprint). We will improve this sentence to avoid misunderstandings. We will also revise the part where the noise of the satellite data is mentioned. You are right. Noise is not the main reason. The main reason for the selected spatial and temporal resolution is that our method cannot deal with high-spatial and high-temporal XCO₂ variations. We use averages to eliminate high-frequency variability. As explained above we will add more information on biogenic fluxes. We will add to the revised Fig. 5 (using the CT2019 data set) XCO₂ anomalies due to fossil fuel (FF) emissions and biogenic (BIO) fluxes in addition to the (total) XCO₂ anomalies. A comparison of these three different XCO₂ anomaly time series permits to identify the contribution of the FF and BIO components to the (total) XCO₂ anomaly. We will show that during October to May FF (typically) dominates but the BIO impact is not zero. The non-negligible BIO impact introduces an error, which we quantify.

Referee: 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.

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Author's response: We established the link between the XCO₂ anomalies and the CO₂ FF emissions using CT2019. We use CT2019 to get the two parameters (offset and scaling factor) of the empirical linear relationship used to convert the satellite-derived XCO₂ anomalies to FF emissions. Furthermore, we use CT2019 for error analysis. Apart from these two parameters we derive the emissions and their changes entirely from the satellite Level 2 data products without any additional modelling. This is a major difference to more traditional inverse modelling approaches which require very detailed modelling and typically several assumptions, e.g., related to the a priori fluxes and their variances and co-variances. We therefore think that the terms “data driven” is an appropriate characterization of our method.

Referee: 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 ?

Author's response: No, we do not sub-sample CT2019. For the revised version of the paper we will write this when describing the use of CT2019: “The XCO₂ has been computed by vertically integrating the CT2019 CO₂ vertical profiles (weighted with the surface pressure normalized pressure change over each layer). The model data are sampled at local noon, which is close to the overpass time of the satellite data sets used here. The spatio-temporal sampling of a specific satellite XCO₂ data product is not considered here, i.e., we use the CT2019 data set independent of any satellite data product apart for the sampling at local noon.”. As our East China target region is large compared to the resolution of the CT2019 data set, we do not think that the resolution of CT2019 causes a significant problem. Note also that the CT2019 data set

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only covers 2015-1018 whereas we use satellite data also in 2019 and 2020. As will be better explained in the revised version, we focus on differences between the October 2019 to May 2020 period and previous October to May periods to find out to what extent the October 2019 to May 2020 period is significantly different compared to the other periods taking into account the standard deviation of these differences. We do this for all satellite products and in the revised version even using a second alternative method (to not only rely on the DAM method). The scatter of the results provides us with error bars considering year to year variability but also shortcomings of our method assuming that our model is not able to capture all variability (which is unlikely the case due to the simplicity of our model). We aim at carefully considering variability and uncertainty when formulating our “careful conclusions” (as written by you in your first paragraph).

Referee: 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.

Author's response: We replaced all quotes from other publications by appropriate short summaries. We also revised many figures. We think, however, that all figures are relevant and we therefore have not been able to reduce the number of figures. However, we simplified several figures (Figs. 8-12) by slightly modifying our method (the OND anomalies are now computed one stage earlier (Fig. 7)). As a consequence, most of the numerical values listed in Tab. 4 also changed slightly. The overall conclusions are however not affected.

Referee's Secondary points:

Referee: - 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 pan-

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demic 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)).

Author's response: In the meantime, additional publications have been published. We added references to these publications to our manuscript and revised the “first attempt” statement (we now plan to write: “Our study is one of the first attempts . . .”).

Referee: - 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).

Author's response: For the revised version of our manuscript we will aim at improving our manuscript also with respect to this aspect.

Referee: - 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) ?

Author's response: No. This is a misunderstanding. We will revise our manuscript to avoid this.

Referee: - The "complementarity" between OCO-2 and GOSAT (line 126 p4) could be

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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.

Author's response: There are also several publications using GOSAT data for this purpose, see, for example, Matsunaga and Maksyutov, 2018, cited in our manuscript.

Referee: - 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.

Author's response: We will replace "good agreement" with a more appropriate term. To address these comments (and the related comment listed above) we have now also used a second method not depending on latitude bands and will also report results obtained with this alternative method in the revised version of our manuscript.

Referee: - 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.

Author's response: Agreed. For the revised version of the manuscript we will use a different notation.

Referee: - 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.

Author's response: Agreed. We will modify the table along the lines suggested.

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