First of all, we thank Ray Nassar (reviewer 1) for his efforts in carefully reviewing our manuscript and his constructive comments.

Point-by-point answers to the comments of reviewer 1

Specific points

Reviewer 1: *Page 2, lines 15-17 is a jumble of references to different techniques and different types of measurements (satellites and airborne). Since the rest of the paper is about satellite observations, the reference to the airborne measurements and associated emission estimates of Krings et al. 2011 and 2018 should either be removed or some explanation is needed as to why they are relevant here.*
Authors: We cited the publications of Krings et al. (2011, 2018) as examples where assumptions on source position and plume formation have been made. However, as they are not analyzing satellite measurements, we removed them from the revised manuscript.

Reviewer 1: *It would also be useful to better distinguish between studies that quantified/estimated emissions versus identification.*
Authors: In the revised version, we cite only studies related to quantification/estimation of emissions which is more appropriate in this context: "... and the quantification of anthropogenic emissions a challenging task. Usually, the latter requires knowledge of the source position and assumptions on plume formation (e.g., Nassar et al., 2017; Heymann et al., 2017) or statistical approaches applied on larger areas and/or time periods (e.g., Schneising et al., 2013; Buchwitz et al., 2017)."

Reviewer 1: *It might also be helpful to point out the studies that took advantage of atmospheric imaging capability, which is crucial to the current work.*
Authors: Here we are aiming to directly lead to emission estimates which make use of simultaneous NO_2 measurements. The benefits of imaging capabilities are discussed at p15 l16 of the original manuscript.

Reviewer 1: *P2, L27. Specifying that the S5P launch was in October 2017 would help to clarify for the reader why only observations from 2018 were used in this work.*
Authors: Done.

Reviewer 1: *P3, L8: "eight parallelogram-shaped footprints across track with a spatial resolution at ground of <= 1.29 km x 2.25 km"*
Authors: Done.

Reviewer 1: *P3, L13: It would be helpful to add a statement along*

the lines of "The OCO-2 v9 data set has an improved bias correction approach that results in reduced biases particularly over areas of rough topography."
Authors: Done.

Reviewer 1: *P4, L2: It would be helpful to provide the SNR or perhaps a relative precision instead of just the random noise, since most CO2 specialists will not have a good grasp of the magnitude with these units.*
Authors: We added: "(enhancements near sources often exceed 10^{16} molec./cm^2)".

Reviewer 1: *P4, L14: "50 minutes" According to the figure labels, the time differences range from 6 minutes (Medupi-Matimba) to 35 minutes (Nanjing). Perhaps it would be more informative to state: "each scene observed by OCO-2 is also observed by S5P with a temporal offset ranging from 6 to 35 minutes"?*
Authors: About 50 minutes is the maximum co-location time difference for the vast majority of possible OCO-2 observations. 6 to 35 minutes is only the range for the presented cases (which are described later in the paper).

Reviewer 1: *P5, L5-10: The method described is interesting and very sensible and is one of the strengths of this work.*
Authors: Many thanks.

Reviewer 1: *P5, L29 – P6 L1: The manual adjustment to wind direction but not windspeed is similar to the approach of Nassar et al. (2017) which would be worth acknowledging.*
Authors: We added: "The manual adjustment to wind direction but not wind speed is similar to the approaches of, e.g., Krings et al. (2011) or Nassar et al. (2017)."

Reviewer 1: *P6, L7: This constant factor of 1.44 taken from Varon et al. (2018) to treat the vertical dimension is a major oversimplification in this work. Varon et al. (2018) simulated CH4 plumes that might be typical of CH4 leaks from infrastructure, thus they deal with smaller spatial scales and little to no temperature contrast. The effective vertical height of emissions will likely be very different when dealing with smokestacks, urban areas or wildfires, as in the present work. In fact, a new paper (Brunner et al. "Accounting for the vertical distribution of emissions in atmospheric CO2 simulations" Atmos. Chem. Phys., https://doi.org/10.5194/acp-19-4541-2019) that also has links to the Copernicus candidate CO2 Monitoring mission, describes the relevant factors for the vertical distribution of emissions, where different vertical emission profiles for point sources (i.e. a power plant) or area sources (i.e. an urban area) are discussed. The temperature of the emissions and the season are also shown to be important factors. Although detailed study of plume rise is complex and beyond the scope of this paper, and the use of column data reduces the importance of these issues, surely it must be too simple to use a single factor of*

1.44 times the 10 m wind speed to represent plume rise from the diversity of source types and geographic locations studied in the present work. According to equation 3, errors in emission estimates will be approximately proportional to the error in wind speed, so getting a realistic wind speed is important.
Authors: As discussed at p6 l9, the focus of our study is "on demonstrating the benefits of simultaneous NO_2 and XCO_2 measurements rather than on most accurate flux estimates" and we "recognize that uncertainties resulting from our estimate of the effective wind speed's normal may be reduced in the future by improved wind knowledge". At p6 l26, we acknowledge the differences of our study compared to the study of Varon et al. (2018) and account this by enhancing the introduced uncertainty. Within the revised version of the manuscript, we cite Brunner et al. (2019) and the paragraph starting at p6 l5 now reads: "As discussed by Brunner et al. (2019), the plume height (and subsequently the wind speed in plume height) depends on many aspects like emission height, stack geometry, flue gas exit velocity and temperature, meteorological conditions, etc. Some of these parameters are not known for many sources and their explicit consideration would go beyond the scope of this study focusing on demonstrating the benefits of simultaneous NO_2 and XCO_2 measurements rather than on most accurate flux estimates. Varon et al. (2018) proposed to approximate the effective wind speed within the plume from the 10 m wind by applying a multiplier in the range of 1.3–1.5. Therefore, we decided to use a multiplier of 1.4 for convenience. This empirical relationship accounts, e.g., for plume rise and mixing into altitudes with larger wind speeds. For the present, we consider this approximation adequate for this first study, but we recognize that uncertainties (see next section) resulting from this estimate of the effective wind speed's normal may be reduced in the future by improved wind knowledge."

Reviewer 1: *P7-11, It would be most useful to have the wind direction adjustments clearly stated for every case either in the text or a table.*
Authors: We added this information for each scenario to the text of the corresponding sections. The adjustments were always between 17° (Baghdad) and 1° (Moscow).

Reviewer 1: *P8, L7: "larges" -> "largest"*
Authors: Done.

Reviewer 1: *P8, Sec 3.2: The enhancement near Lipetsk is huge and the fit is very good. Are there other sources in addition to the gas-fired power plant and the steel plant that could be relevant, for example, what about the city of Lipetsk (population 500,000)?*
Authors: Of course, there are also other CO_2 emitting industries in Lipetsk plus traffic etc. However, we do not have access to a emission data base on facility level for Lipetsk. In order to not give the impression that the steel plant and the power plant are the only two emitters, we have rephrased the first sentence of the chapter: "... shows the surrounding of Lipetsk (approx.

0.5 million inhabitants) with, among other industries, the Novolipetsk steel plant and the Lipetskaya TEC-2 gas-fired power plant ...". Additionally, we added the approximate population also to the Moscow, Baghdad, and Nanjing section.

Reviewer 1: *P9, Figure 1: I assume the hashed/shaded region is the Moscow urban area but I am not sure? Can the authors clarify in the figure caption?*
Authors: We added to the figure caption: "The hatched area corresponds to the urban area (World Urban Areas dataset, Geoportal of the University of California, *https://apps.gis.ucla.edu/geodata/dataset/world_ urban_ areas*)."

Reviewer 1: *P9, Sec 3.4: The OCO-2 flyby of the Matimba and Medupi power plants used in this work is over 80 km away. Nassar et al. (2017) also estimated the emissions from Matimba using OCO-2 data (but version 7) from a direct overpass in 2014 and a close flyby (7 km away) in 2016. Daily emission estimates from Nassar et al. converted to annual values are 12 MtCO2/a.*
Authors: Our flux estimate corresponds to the combined signal of the Matimba and Medupi power plant. Nevertheless, we added to our discussion: "Nassar et al. (2017) also estimated the emissions from the Matimba power plant (but not Medupi) using OCO-2 XCO_2 v7 data. For a direct overpass in 2014 and a close flyby ($\sim$7 km away) in 2016 they found fluxes, converted to annual values, of $12.1\pm3.9\,MtCO_2/a$ and $12.3\pm1.2\,MtCO_2/a$, respectively."

Reviewer 1: *P10, Sec 3.5. The Australian wildfires are clearly an example of an area source not a point source. The NO2 data show structure/heterogeneity in the area source. This makes it a poor candidate for the modeling approach applied that represents the plume with a Gaussian function. Furthermore, it makes little sense to report emissions in an annual unit in the case of a wildfire, which lasts on the order of days to weeks and would demonstrate temporal variability even over that limited time scale. For fossil fuel CO2 emissions from power plants or cities, there is also periodic (diurnal, weekly and seasonal variability) and non-periodic (plant shutdowns, heating/cooling linked to weather, etc.) variability, which also makes reporting emission rate estimates for shorter time scales more exact from a single overpass.*
Authors: We discuss that "the NO_2 (and less obvious maybe also the XCO_2) cross-section has two maxima" and that "the Gaussian fitting function cannot account for this". However, this is "not reflected in the overall good fit quality ($\chi^2 = 0.6$)" for the XCO_2 fit. This means, within the noise of the XCO_2 data, we cannot expect to significantly improve the XCO_2 fit by a more complex plume model. We rephrased the corresponding section which now reads: "The NO_2 (and less obvious also the XCO_2) cross-section has two maxima which cannot be accounted for by the Gaussian fitting function. However, this is not reflected in the good XCO_2 fit quality ($\chi^2 = 0.6$), but should be taken into account when valuing the results." The unit $MtCO_2/a$ is not unusual especially

for fluxes of power plants or other anthropogenic CO_2 emitters. Likewise driving a car at 50miles/hour does not mean that you actually drive the car one hour at that speed, our flux estimates are snapshots for the time of the overpass and are not necessarily representative for the annual average. This is particularly true for a wildfire but of course also for, e.g., a power plant if emissions vary in time. Within the section summary and conclusions we state that "our estimates are valid only for the time of the overpass...". Additionally, we emphasize this point within the sections for the Australian wildfires ("for the snapshot of the overpass, we computed a cross-sectional CO_2 flux ..."). The revised version makes this point also clear in a modified caption of Tab. 1: "Note that the cross-sectional flux results correspond to the instantaneous time of the overpass' whilst EDGAR and ODIAC emissions are annual or monthly averages..."

Reviewer 1: *P14, Figure 6. Nanjing seems to show two maxima in the NO2 image, which is also problematic to represent with a Gaussian function.*
Authors: As we do not fit the entire plume of CO_2 or NO_2 with a Gaussian plume model, additional upwind maxima in the NO_2 image pose no principle problem for the analysis of the plumes cross sectional flux. If an additional maximum is coming from an additional source, the source attribution becomes difficult. If it results from non steady state meteorological conditions (e.g., accumulated NO_2 during calm conditions), the plumes cross sectional flux may still be correctly derived but may become a poor estimate for the actual emission. For the Nanjing scene, EDGAR and ODIAC emissions suggest multiple significant emitters and we discuss in the section summary and conclusions: "... the scene includes a larger area of overlaying sources, making source attribution difficult."

Reviewer 1: *P14, L13: Why is 0.5/MtCO2/a the chosen minimum value?*
Authors: For most meteorological conditions, a source of 0.5 MtCO2/a per grid box is typically well below the detection limit of OCO-2. Additionally, given a limit of 0.5 MtCO2/a, the shown maps include a reasonable small amount of non-empty grid boxes so that the reader is easily able to find the largest emitters.

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