Review of "A method for estimating localized CO_2 emissions from co-located sdaellite XCO_2 and NO_2 images"
By Andrade et al, 2023, submitted to AMT

Description and Reccomendation

This paper describes a relatively automated plume-detection and plume flux inference using combined TROPOMI NO_2 and OCO-3 XCO_2 data. In this manuscript, they specifically apply their method to the Belchtow power plant in Poland, which is one of the largest coal-fired power plants in the world. They obtain generally good agreement between their inferred fluxes and those estimated by a standard bottom-up method (using data from the power plant itself to inform its instantaneous emissions).

They find generally good agreement with a completely different approach described recent in Nassar et al. (2022). Their approach uses a cross-sectional flux approach, wherein the emission plume is intersected with a number of cross-sections at varying distances downstream of the emitter; the Nassar et al. approach uses a Gaussian plume model fitting technique.

Overall, this manuscript is extremely well-written and gives an appropriately level of detail to a relatively complicated method with many steps and multiple data inputs. I recommend publication after my relatively minor comments have been addressed.

General Comments

As the authors point out, numerous papers have tried to use CO_2 simultaneously with NO_2 to quantify co2 emission rates from power plants. In principle, you can use CO_2 alone (as Nasser at all, 2017, 2022 does) or NO_2 alone (by assuming you know the emission rate). There as thus many ways to "mix and match" the information provided by the NO_2 observations and the CO_2 observations. A small discussion of the different assumptions one could make – and their associated "pros" and "cons" - would be most welcome somewhere in the paper, and what assumptions you personally chose. It appears that you (Page 6, line 148) only use NO_2 to identify a region containing an emission plume – i.e. it is only used in a purely qualitative sense. Please clarify this in the paper. Do you use NO_2 to localize the spot of the source? Or do you use the well-known coordinates of the Belchatow station? For instance, Hakkarainen et al (2023) in their "Building a Bridge" paper seem to simultaneously fit Gaussians to both the NO_2 and CO_2 CS's, and then use them to somehow construct an effective NO_x-to-CO_2 ratio, which is (somehow) used constrain the power plant CO_2 flux. But even their paper is not very clear on this point.

It would be useful to expand your discussion of the pros and cons of your cross-sectional method (similar to that of Hakkarainen et al but more automated) and the more conventional Gaussian-plume model fit. Since the latter "fits all the data at once" it seems intuitively like it might avoid some of the errors your method is subject to, such as those caused by dispersion. But perhaps not – it seems very different to say. It implies an OSSE study with LES-modelgenerated plumes might be warranted, to investigate the pros and cons of these different techniques.

Comment on how much of this was automated vs. “Done by eye”. It seems like the authors are trying to largely automate the technique but this is not entirely clear. A seemingly big advantage of this study is the amount of automation put into this work, such that it could potentially be applied to future sensors such as CO2M. I strongly recommend emphasizing the automation aspect of this work in the abstract.

Specific Comments

Sect 2.2: Your equations are all in terms of VCDs. You seem to be assuming locally flat ground. What is the topography in the region is significant? The VCD will generally scale proportionally to the surface pressure, assuming CO_2 & NO_2 are well mixed in the boundary layer. This will imprint topography on the VCD map. Can you please comment on how you account for this?

P8, L165: Will this NO_2 smoothing potentially make the plume wider than it really is? Did you consider this effect, or can you comment on if it negligible?

P213, near L275: You should also mention that the wind speed & direction can easily vary over 1-3 hours. It seems like this is quite common and will certainly distort the plume and lead to flux inference errors. I would think this would lead to fairly coherent variability of the type that figure 5b exhibits. You take great pains to estimate the error due to variability in the source flux F; why not do the same for potential variations in the wind over the 1-3 hour period defining the plume length? At the very least, this should be commented on. Note that this is somewhat different than a mean bias error in the wind speed, such as you consider in section 2.3. Although perhaps this is inherently taken into account by your “dispersion uncertainty calculation”, wherein you empirically estimate the covariance function from the semi-variogram of the data itself?

Section 2.3: I think you also should include a measurement uncertainty term. Currently there is no term that represents potential biases in the measurements, or if there is, I can’t see it. According to Nassar et al’s work, this term is not usually large, but warrants at least commenting on it in the paper.

Section 2.3.1 & Figure 9: It seems like your dispersion uncertainty can often be extremely large and dominate the overall uncertainty. The discussion in this section, however, is fairly technical and contains no plots that give the reader evidence that you’ve done this correctly. I think plotting semi-variograms and the overlaid fits would help, especially if you compared two very different cases, such as 17-April-2020 (very large dispersion uncertainty) and 20-June-2021 (small dispersion uncertainty).

Section 2.3.2: I’m not crazy about 0.5 m/s as a 1-sigma mean wind uncertainty. I prefer the Nassar et al (2017,2022) approach using MERRA-2 – ERA5, though it would be better to include a floor of 0.5 for the reasons you state. It would be nice if you could comment on this methodological difference in this subsection.

P25, L517: The location of the lignite pit is poorly represented by the v10.4 OCO-3 digital elevation map, and leads to time-independent biases over those locations. This general issue is discussed in Jacobs et al., 2023 (https://amt.copernicus.org/preprints/amt-2023-151/). At the end of these comments, I've attached a figure made by the OCO team showing the effects of a change in DEM on this feature. Therefore, I think it is fully warranted to filter out these spatial samples around the pit by hand, or the full case-filtering approach you describe.

Technical Comments

P2, L38: need "e.g." before Reuter et al, Nassar et al citations. There have been many such publications and these are simply examples. This is the case in many places throughout the paper. Such as P2, L43; I doubt Ciais was the first author to note the long lifetime of CO2 in the atmosphere! The rule I use is this. If a paper was the first to say or show something you cite it directly. If the paper is merely an example of many such papers saying the same thing, and was not the first, you need "e.g.".
P7, Fig3: It is VERY hard to see the "grey lines" of the SAM scan. Please modify to make a little more visible.

P25, L515: "in 0.3" → "by 0.3"

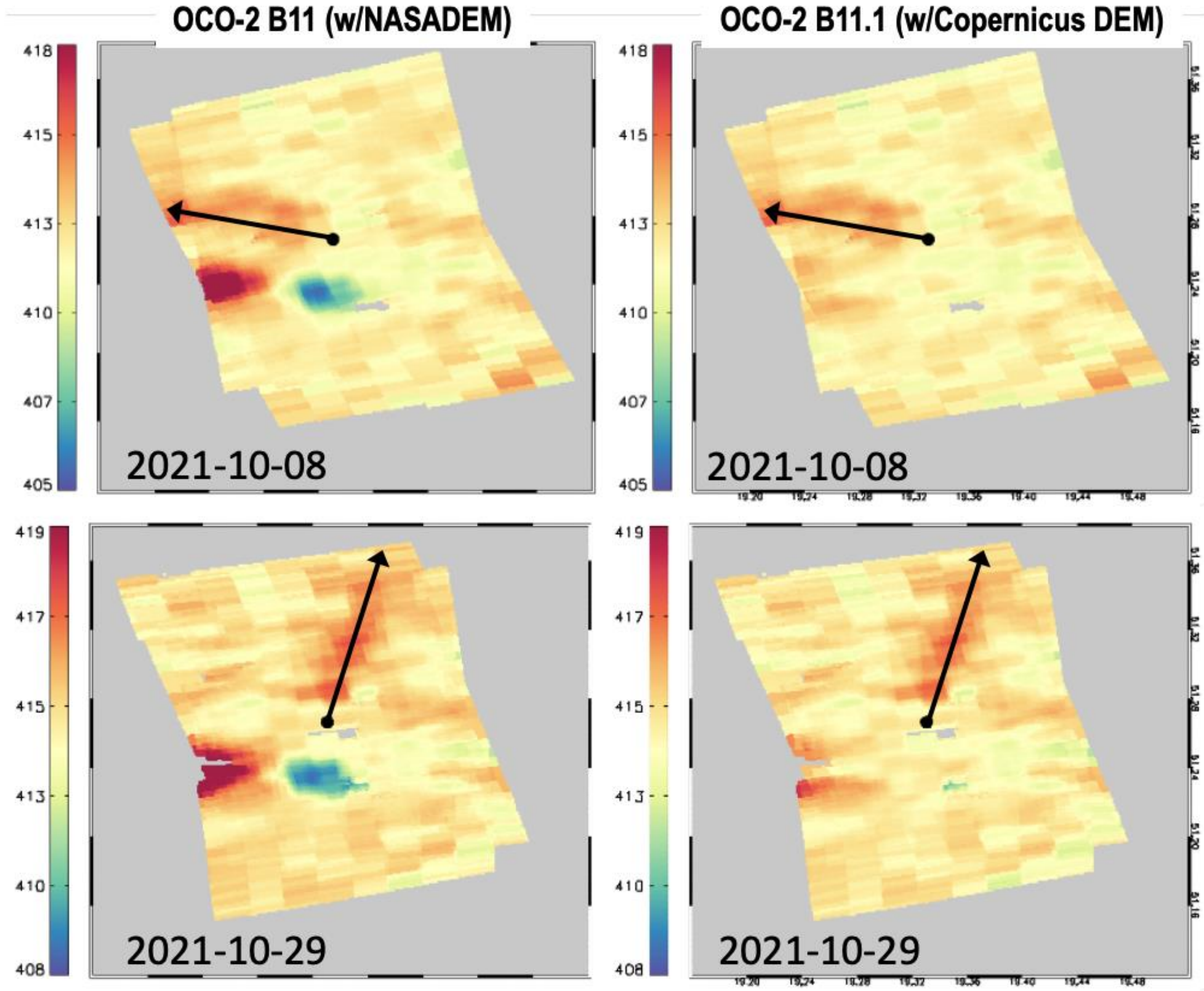

Figure 1: Two sets of OCO-2 version 11 and version 11.1 retrievals of Target-mode observations near the Belchatow power plant. The lignite pit to the southwest of the power plant (black circle) exhibits biases in both retrieval versions, though the use of the Copernicus DEM greatly reduces the size of the bias. It is likely the Copernicus DEM is still not accurate enough for this area, which may have undergone anthropogenic changes in the surface since the Copernicus DEM data were acquired (roughly 10 years ago). The prevailing wind direction of the plume is shown as a black arrow. Cases identified by Ray Nassar at ECCC.