## General comments:

The authors have made an impressive effort to improve the manuscript. The visual appearance of all figures has improved significantly. The text also shows significant improvements. Many stumbling blocks for the reader have been removed.
Nevertheless, some comments remain incompletely addressed. In particular, there are individual comments which the authors claim to have incorporated, but did not do so completely in the manuscript or did so incorrectly.
Concerning the improved Figures, I have to apologize. I've noticed that in my previous comments I've sometimes mixed up the terms "colormap" and "colorbar". At the current state I wonder why for some figures the colorbars cover different values. In general, it is obvious that $TXCH_4$ shows stronger enhancements than $XCH_4$. But for example, Figures 5 (a) and (b) should have the same range of values. (d) and (e) should also cover the same values. Otherwise, the visual comparison of the plots side by side creates a false impression. The same holds true for Figure 7, Figure A-2 and Figure A-3. If there is a reason to not use that same range of values in the colorbars, please indicate so in the respective captions. Like you did in Figure 6, where you're only want to compare spatial pattern.

Comments of Referee #1:

- 1.1: I strongly agree with Referee #1. The CAMS data was used solely for the validation of the wind-assigned anomaly method. The term "using" suggests that the CAMS data was actually used for your emission estimates, which they where not. I would even go one step further than Referee #1 and not include the CAMS data in the title at all: "Quantifying hard coal mines $CH_4$ emissions from TROPOMI and IASI observations using the wind-assigned anomaly method". However, this is only a recommendation. If the authors feel differently I suggest discussing it with the editor.
- 2.1: for the sake of completeness, a study on measurements on HALO should also be cited, as HALO was the flagship of the campaign. I recommend Galkowski et al. 2021 (https://doi.org/10.5194/amt-14-1525-2021) on in situ observations on HALO and Wolff et al. 2021 (https://doi.org/10.5194/amt-14-2717-2021) on airborne lidar observations. Also, I want to raise your attention to Andersen et al. 2021 (https://doi.org/10.1016/j.aeaoa.2021.100135) on UAV based emission estimates in the USCB and Luther et al. 2022 https://doi.org/10.5194/acp-22-5859-2022. If it seems fitting to you, you could include these two publications at an appropriate location of your manuscript. But of course, only as an option for you.

## Specific comments:

- SC 0 (new comment):
    - In the abstract in lines 21-27 validation results are given. I'm in big favor of giving results in the abstract, but only the main results, i.e. the emission estimates based on the satellite observations. Here, it is sufficient to simply state, that the wind-assigned anomaly method is validated using CAMS forecast data, showing good agreement to the CAMS-GLOB-ANT inventory. You don't have to give numbers. For the reader the results of the validation distract from the main results, which are supposed to be the highlight in the abstract.
    - The same applies for the last paragraph of the abstract (i.e. lines 37-43). The sensitivity analysis of wind speed is a method for determining a contribution to the uncertainty in the emission estimates. The results of this analysis should be reflected in the given uncertainty of the emission estimates. For the abstract it is sufficient to state that a sensitivity analysis of wind speed for different altitudes has been made.

This is part of your chosen approach and should be stated before giving the main results.

- SC 3: In line 201 it now says “wind regime sector”. In Table 1 it says “wind area”. Please recheck the manuscript, if all your changes are applied to your will.
- SC 4: It still says “simple plume model” in the title of subsection 2.3, in lines 131, 185, 277, 400, captions of Fig. 6 and A-1. Please review your entire manuscript for consistency and use only “cone plume model” or “simple cone plume model”.
- SC 5: If there are such high spreads and uncertainties in your estimates for the individual years, I don’t understand how you come up with such low uncertainties in your estimate of the three-year period. Please comment this in the scope of SC 24/27 below.
- SC 6: Eq. 6. I’m a bit baffled by the mix of equation and free text. I have to admit that I’m not sure if that’s formally allowed or not. You might think of a variable for “wind-assigned anomaly”. Something like “$\delta XCH_4$“ or similar. I realize that you would only need this variable at this point so this is only a recommendation. You could also wait for a comment from the type setting of the journal.
- SC 12: I’m afraid you didn’t add your statement to the manuscript. At least in the “track-changed” it is unchanged (see 256 ff). Moreover, you still haven’t answered the question. How did you come up with exactly 7 km? Why not 6 or 8 km?
- SC 13/14: “This cone plume model only considers a simple linear proportion of wind speed and emission strength. Huge biases are expected in a simple day or in a short period. But these biases can be compensated over a long-term period.” If you do not explain how these biases come about, it is hard to understand why they are compensated over a long period.
  I do not understand why you expect such huge biases. Most plume models (e.g. Gaussian plume model) are recursively linear with wind speed and linear with emissions rate. Usually, in plume models some parameter accounts for turbulence. The only possible representation of turbulence in your model is in the angle $\alpha$=60°. In some cases, this angle will be too small, in some cases it will be too large. This effect might be canceled out, as you suggested, over the long observation period. But it seems as your cone-model is either showing lower $XCH_4$ enhancements, or supposedly too narrow plumes. As your overall goal is to be representing for the overall observation period this is no show-stopper, but please openly discuss/explain the limits of the cone plume model to the reader.
  In your former publication you showed plots of the cone plumes for different values of $\alpha$. While you derived 60° as the best fit to the $N_2O$ plumes for Madrid, I’m not convinced that 60° is necessarily the optimal fit for the USCB, too. At the very least should investigate to what quantitative extent variations in $\alpha$ impose uncertainties on your emission estimate. As far as I can see this has not yet been considered in your uncertainty analysis Sect. 3.3.
- SC 16 Figure 6: Why do the colorbars start with the value 5 and not 0? If the colorbars are extended (i.e. $\Delta XCH_4$<5 is the same color as $\Delta XCH_4$=5), an extension arrow at the colorbar should indicate so. The same applies for the upper end of the colorbar. But, as mentioned in the general comment above, I strongly recommend using the same range of values for all colorbars. If the spatial pattern in (c) would not be recognizable anymore you can leave the colorbar as it is now., but at least start your colorbars in (a) and (b) with 0.
  Also indicate the different colorbars in the caption. Currently it seems like the modelled plumes fit perfectly to the CAMS forecast and the TROPOMI data. Which they do only in spatial appearance, not concerning the magnitude of XCH4 enhancement.
- SC 17: Sorry, in my first review I mixed the terms „colormap“ and „colorbars” by mistake. Here, I was actually referring to the colorbars and the different range of values covered by it. As mentioned in SC 16 this is ok, if you mention in the text why you did so.
- SC 20: I actually think that these two plots are of high explanatory value. Please consider including them in the manuscript (optionally). Especially, because I now realized that I

misunderstood, that by NE/SW in the title of your figures you mean wind coming from NE/SW and not plumes propagating in NE/SW-direction. My bad, but if you want to make sure that this will not be misunderstood by the reader, you could include these two plots in the manuscript.

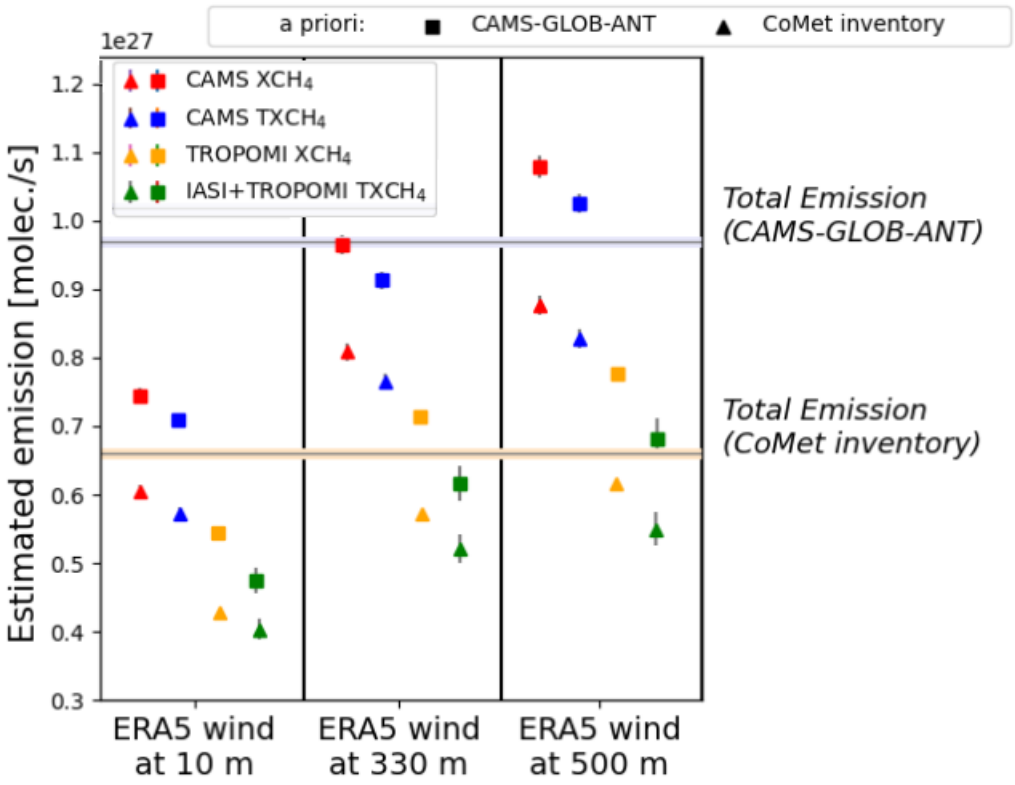
- SC 24/27: In the abstract you give your emission estimate with 479±4 kt/yr and 437±18 kt/year. Then you share the results from your sensitivity analysis regarding wind speed, separately.
  The uncertainties given your sensitivity analysis should be included in the uncertainties of your emission estimates. The $CH_4$ exhaust from the ventilation shafts is released at a height of approx. 20 m. Propagating downwind it will be carried upwards by convective eddies and thereby distributed in the entire boundary layer. Have a look into the video supplement of https://amt.copernicus.org/articles/14/2717/2021/#section11 or the Figure A2. While the highest concentrations of $CH_4$ will, for sure, be advected in the middle of boundary layer, or even closer to the ground, you'll need to include the vertical variations of wind speed in your emission estimate uncertainties in some way!
  In your uncertainty analysis (Sect. 3.3) you analyze three sources of uncertainty. Within the analysis the emission results vary strongly from the validation emission estimates given in the abstract and conclusion. Considering this, I'm confused how the uncertainty in your overall emission estimate can be so small. Please state the relative contributions of the sources of uncertainty to the overall uncertainty, including uncertainties induced by the selection of cone opening angle α (see SC 13/14)

## Technical comments:

- TC 50: In the caption it still says "simple plume model" although the authors have confirmed to switch to the term "plume cone model". Please watch out for consistency.
- TC 51: In Eq. 2 the indices "i" are still not subscripted. Actually, d and $\Delta CH_4$ are functions of the location. So $x_i$, $y_i$ should not be subscripted, only the index "i":

$$\Delta \mathrm{CH}_4(x_i, y_i) = \frac{\epsilon}{v \cdot d(x_i, y_i) \cdot \alpha}$$

- TC 57: caption Fig. 5: "colorbars in (d) and (e) are different from that for $XCH_4$". Actually, all four colorbars cover different values. To me it makes sense to have different colorbars for $XCH_4$ and $TXCH_4$, as $TXCH_4$ generally shows higher enhancements. But why different colorbars for (a) & (b)? And why different colorbars for (d) & (e)? See general comment.
- TC 67: My apologies, by my phrasing "… among each other …" it was not clear what I actually meant. I thought of something like that:




So that triangles are actually vertically aligned with the squares. In this plot the reader should become aware that CAMS-GLOB-ANT emissions are always higher than from the CoMet inventory. By plotting them vertically aligned this becomes more obvious.

- TC 71: "We use $NE_{1/2}$ for 0°-90°, $SW_{1/2}$ for 180°-270°, $NW_{1/2}$ for 270°-360°, and $SE_{1/2}$ for 90°-180°". My recommendation was to use the subscript "1/2" everywhere when the wind field is divided into two halves (i.e. everywhere before Sect. 3.3.2). The subscript "1/4" was supposed to be used, when the field is divided into quarters (previously designated by "_narrow"). This indexing is of course only necessary when talking about the narrowed angular wind regimes in Sect. 3.3.2. So, if you don't want to include an index in the manuscript before this Sect. it's fine by me. But in Sect. 3.3.2 it should be "1/4".