Round 2 Review of "*Local to regional methane emissions from the Upper Silesia Coal Basin (USCB) quantified using UAV-based atmospheric Measurements*"

General comments:

I am pleased to see that the authors made significant efforts to address each of the concerns that I raised in my first review. The manuscript now feels much clearer and more robust, overall.

The one issue where I do still have concerns is the CO_2 analysis. I do appreciate the authors' efforts to reframe this analysis away from the "1% missing inventory source" towards a simple reporting of the emissions estimate. However, I would like reassurance that the large uncertainty introduced by the linear fit for the ratio itself (2.9/4.6 = 63%) is being accounted for in the uncertainty values calculated and reported in the CO_2 analysis. I have the impression that this may have been overlooked, and will lead to much larger ranges on the estimates. These large uncertainties, then, would leave me finding it difficult to draw much meaning out of the analysis. Additionally, there appears to be little reference to the justification for the CO_2 analysis or to its conclusions in the abstract or introduction. (Though I suspect these may just be unintentional oversights.) Considering the potentially large uncertainties with such low emission values when compared to available regional estimates, I again raise the question of whether this CO_2 analysis is better off left out of the manuscript. That said, if the authors maintain that they want to keep this analysis in, I am much more comfortable with the conclusion at the end of the discussion section. There it states that, given how small and variable the CO_2 emissions are, these results support the idea that CO_2 emissions from coal mines are not significant, even from mines with rather significant CH_4 emissions. (Perhaps a comparison to a single automobile's expected annual emissions or something, for context, may help.) So if that is the takeaway that gets included in the abstract/intro, then I have no objection (after my question with the uncertainty calculations is addressed).

On the whole, I believe that this manuscript is in good shape to be a valuable contribution to the community, and will be ready for publication following some minor adjustments.

Specific comments:

Line 59: Here or somewhere else in the intro, it would be good to include the explanation for how and where CO_2 emissions come from during the coal extraction process, as motivation for their inclusion in this study. (Currently I see that one sentence had been added to Section 3.5, following a question I raised in the first round of reviews, but would be good to see that here, as well, and expanded on a bit.)

Line 97: Perhaps there should be some reference what comes out of the CO_2 analysis, too, if it is to be included in the manuscript?

Line 114: Somewhere in this paragraph, can the authors include a sentence or two about the time of day the measurements were taken and how well-mixed the atmosphere would be expected to be, including how that may affect the expected measurements?

Line 170: If the difference between the on-board met station and the off-site meteorological station was this high, was that incorporated in the uncertainty calculation for the plume calculations (in both approaches) for the flights that did not have the on-board met station (flights 5-33)?

Line 291: Can the authors include some examples for what causes optimization failures here?

Line 310: It may be good to mention here that attempts were made unsuccessfully to track down the real answer here, which is the reason for making this assumption.

Lines 350-357: Would be a good idea to specify in the caption descriptions which are the shaft-averaged measured emissions and which are the shaft-averaged inventory emission estimates. Otherwise using just "shaft-averaged emissions" for both axes gets confusing for figures b), c), and d).

Figure 8: Please specify with a legend which fit is being displayed here with the red line. Additionally, are the R^2 values really the same between c) and d)?

Line 378: I would like to see the authors include some additional possible explanations for the systematically low estimates here. Couldn't they also result from uncertainties in the quantification approaches or some poorly-accounted-for physical process? For example, the potentially large uncertainties in the wind speed and direction (especially for flights 5-33 that did not have truly local met data)? Or perhaps non-Gaussian plume behavior due to local turbulence?

Lines 388-9: Could there be some indication either in the text or in the appendix of which of the flights were kept for this analysis? Looking at the figures, I can take a pretty good guess about which ones were probably thrown out, but it would be better to state it plainly.

Lines 396-397: It's still not clear to me whether this is claiming that the CO_2 and CH_4 trends themselves are similar, which I would think would be baked into the use of the linear relationship for deriving the CO_2 values, and thus not a surprise.

Figure 10: Can the authors confirm that their uncertainty estimates incorporate the uncertainty associated with the CH4/CO2 slope? The 63% uncertainty on that linear fit slope is quite large, so I'm surprised to see some small error bars in the figure, e.g. in the Pniowek V chart.

Line 456: Can the authors articulate any other potential explanations for why the values presented here appear to be lower than in Fiehn et al.? (Maybe seasonality of emissions, for example?)

Line 481: Would specify for clarity "Estimated regional CO_2 emissions *from these coal mines*"

Line 517: I believe this should read that the "coal mine ventilations shafts **are** a minor contributor to the regional CO_2 emissions".

Lines 519-526: Maybe should include reference to seasonal changes, as well, and thoughts about the representativeness of 2 weeks out of the year to upscale an entire year's estimates.

Figures B1-B4: Can some indicator be included to highlight which flights were included vs. excluded from the different analyses? (Or if that's too much trouble, can this be stated in the text of the figure captions?)

Figures B5-B9: Similarly, can some indicator be included to highlight which flights were included vs. excluded in the analyses? Or can this be stated in the text? Additionally, please add the corresponding R2 values to each figure. And why are flights 31 and 56 missing?

Technical comments:

Line 23: Please define "IG" and "MB" before using the abbreviations

Line 67: Need a space between "Turnbull" and "et" in the citation

Line 104: There is a blue period and quotation mark here

Line 109-110: This sentence is repetitive of line 104

Line 112-113: This sentence about the "curtain" is also repeating what was just said a few lines above

Line 180: "downwind of the plume"

Line 283: "presented in the next section"

Line 355: There needs to be a space between "of" and "active"

Line 378: I'm not sure what happened to the spacing between the letters in the phrase "or the possible biases of"…?

Line 387: The M_{CO_2} subscript needs to be fixed

Line 393: Unpaired quotation mark at the end of the line

Line 439: There is no need to include the "a" in "Fig. 11a"

<u>Figure 11:</u> The caption is counting the bars wrong after the second bar. It seems like the dark and light yellow were both counted as one bar, so all subsequent bars are described as one less than they really are (e.g. teal is actually bars 4 and 5, etc.)

<u>Line 489:</u> "an MB approach"

<u>Line 504:</u> Again, the ppm_{CO2} subscripting looks weird

<u>Line 524:</u> Again, the spacing between the letters of the phrase "a single goal mine" does not match the rest of the text

<u>Line 534:</u> "complex" not "complexed"

<u>Line 539:</u> "of an individual shaft"

<u>Figures B1-B4:</u> Please either move the x-axis label to the bottom of the last row of figures or include it in all figures