In response to Jaroslaw Necki's comments from December the 23rd, 2020.

Dear Jaroslaw,
We greatly appreciate your very valuable comment on this manuscript. We will reply inline:

The authors of the article titled "Estimating Upper Silesian coal mine methane emissions from airborne in situ observations and dispersion modelling" presented the unique results of the direct measurements of methane concentration in the air over the USCB region. The article is very interesting and aims at very important target: experimental verification of the methane emission estimate basing on momentary measurements – in this case airborne. For that purpose analyses performed with QCL/ICL instrument were coupled not only with mass balance technique (already successfully used and presented in publication Fiehn at al 2020 (https://doi.org/10.5194/acp-20-12675-2020) but with the dispersion model. Beside estimates of total momentary emission, it allowed for a partition of the observed signal to particular ventilation shafts. This is the first attempt of such an initiative in case of USCB. Its hard work as the density of sources is very high and population density is also enormously large implying other sources like landfills and natural gas leakages taking part in the methane budget. In my opinion, the "wall pattern" methodology is not sensitive enough to scale correctly emissions in the group of sources lying in the radius of few km with an application of dispersion modelling. This would require further circle flying or at least more flying along other walls to obtain additional information reducing the large degree of freedom.

As stated in Sect. 3 the primary goal of this mission was to obtain regional emission estimates from the entire USCB region. We do agree, that encircling individual shafts might be an alternative approach towards estimating single shaft emissions, albeit only under certain conditions, that could not be met in the USCB region. It is mainly the lowest flight altitude and the required sharp turns, that hinders such an attempt, as the plumes are not sufficiently buoyant to reach the prescribed minimum flight altitude in the shafts vicinity. Furthermore, the plumes are not well-mixed and turbulence has to be taken into account close to the ventilation shafts, which certainly makes it even more difficult to obtain reliable emission estimates.

Although I appreciate the acknowledgement of authors for providing advice on the ventilation shafts location, I would like to underline the role of uncertainty analysis in the validation of model results for such complex source structure as occurs over USCB. The discussion of the result with the rational data of shaft activities would increase the value of this article.

We do agree with your statement on the importance of an adequate uncertainty analysis. In fact we tried to be as rigorous as possible with the uncertainty analysis, as described in Sect. 4.3 and Sect. 4.6.

My comments are related mostly with chapter 4.5 and figure 10, which in my opinion is the heart of the article.

In our opinion, the value of this article lies in the method described, as well as the results for the entire USCB region. Obtaining estimates from individual shafts from these large area flights is certainly an added value, that should be focused upon in upcoming studies. As stated in our reply to Referee #1, we concede that section 4.5 of the original manuscript might have been misleading the reader to think that we can accurately estimate individual shafts from two downwind walls. We rephrased in a way to make clearer that - while formally reporting individual shaft emissions - there is large uncertainties and correlations among the emission estimates.

Before I will comment on the results for particular shafts, where there is a substantial difference between E-PRTR value and model estimation, I would like to divide the USCB region to the West and East part. There is not much comment for the results from a western part but for some reason, the eastern part makes the article very problematic. Authors are aware of this problem as they stated: "The discrepancy between model and observation at the lowermost (first in time) flight leg, corresponding to the southernmost trajectory section in Fig. 7 (right panel) can not be reproduced by any of the included emission sources. A possible source is urban CH4 emissions of Krakow, located to the east of the USCB region" Unfortunately, the hypothesis of Kraków city influence is unrealistic basing on the wind data presented (from 3 different location in USCB, where Windcubes *were installed) and the angle between the city and flight route (larger than 65deg from the southernmost point– Fig1). However, if it happened due to the variation of wind direction or mass trajectories than the whole chapter 4.6 is not correctly calculated Wind dir and displacement distances uncertainties are underestimated.*

Using a particle dispersion model driven by a high-resolution meteorological model that assimilates three local wind-profiles from Doppler lidar measurements is among the best one can do in terms of calculating plume dispersion. Variations in wind direction and speed are de facto taken into account in the simulations, but not so in other methods, e.g. mass balance, Gaussian plume model. We also believe that our uncertainty analysis is representative of the actual uncertainties in the meteorological parameters.

Figure 10 presents the emission fluxes of the shaft in order of their name. It would be much more informative for readers if the authors would place the emissions according to the position of the centre of plume encountered by an aircraft during flights. This would give them a possible explanation of the reason why the scaling factors optimized by the model is substantially biased for eastern UCSB.

We considered other options for plotting Fig. 10 (ordered by name, emission rate, position of encounter) but after discussion with the team we decided to keep the ordering by name.

Comments for specific shaft emission estimates: Janina I (25kt/y - incorrectly) – the coal mine Janina is currently operated by Tauron Wydobycie company. It is the mine opened in 1907 but still containing one of the biggest profitable coal resources in Poland. Currently, there are 3 coalbeds excavated in this mine (at the relatively shallow level in comparison to other active coal mines) and none of them contains high methane amount. The highest (only one from 4 !)methane content of the coal is 0.2m3/MgCoal [table 20, https://www.tauronwydobycie.pl/sites/default/files/Za%c5%82%c4%85cznik%20nr%201.5%20Janinatekst1.pdf], what is 100 times less than the coalbeds in another mine operated by the same company (KWK Brzeszcze). This mine was always treated as "non-methane" mine from the methane explosion point of view and no methane incident was recorded throughout its history. Attribution of the wrong scaling factor is clear in this case. The possible reason why the model enhanced this direction compared to the measurement data lies in the existence of very big landfill in Balin county (approx. 7km NNE) which can release up to 10ktCH4 yearly.

This is interesting information that could explain this attribution to Janina I, as the a-priori is also very low in that case. Thank you for pointing out the presence of a big landfill in this region.

Brzeszcze VI (0kt/y - correctly) – This shaft was closed (filled with gravel and clay) in the year 2017 but has not been used since 2015, so the decomposition of yearly emission from the mine should include only 3 shafts (no II, IV and XI) not 4. This will increase the agreement in case of Reference run and E-PRTR database and in other databases where the emission is reported by this mine. Brzeszcze II (0kt/y - incorrectly)- All these shafts are located approx. 800m from each other looking perpendicular to wind axis (in reality II and IV are 1.9km apart, II and IX are 2.4 km apart). In this case, it is doubtful that model will attribute so drastically different scaling factors for this shafts (for II the estimated emission is 0 while for IV and IX it is close 20 kt/year) if it works correctly. I, personally measured emission by this shaft in the year 2018 at the comparable level with other two, however, Gaussian plume model is also unreliable for such estimations – so I can't reveal it for comparison.

Piast IV (5kt/y - unc. 100%)– this is strictly non-methane coal mine shaft, where the amount of VAM is negligible like another mine in the region (Ziemowit). For this reason, the result of scaling factor optimization is unreliable for both shafts (Piast IV and Ziemowit I (15kt/y - incorrectly). Jaworzno-Traugutt (30kt/y – incorrectly) – Currently the shaft should be named Sobieski-Traugutt as the mine is under the control of Tauron Wydobycie company and was renamed to ZG Sobieski. The coalbeds excavated in this mine are methaneless, it is non-methane mine. The value proposed by the authors is exaggerated by few orders of magnitude as it would require a high content of methane in the coal, which is absolutely not a *case here [e.g.: chapter 1.1.6.5, http://bip.katowice.rdos.gov.pl/files/obwieszczenia/53136/Obwieszczenie_RDOS_Katowice_WOOS_4235_5_2015_KC_21. Murcki Czułów (40kt/y – incorrectly) – Coal mine Murcki was connected with coal mine Staszic and subsequently closed. It is not operative now but its shafts are used for ventilation of part of the Staszic excavation works. The attribution of methane release to basically closed mine might, in this case, be reasonable. However, decision of the SRK (current owner of Murcki) and Staszic mine – the Czułów shaft has been closed in 2015 and since 2016 hasn't been used for ventilation purpose anymore (shaft was filled with gravel and clay) and finally in August 2018 it was dismounted [https://nettg.pl/news/152132/gornictwo-runela-wieza-szybu-czulow]. It is not possible that this amount of methane would be released by this shaft in June. So, the model has optimized the scaling factor wrongly, and surprisingly this was the highest methane emission rate attributed to any of the shafts. Murcki Zygmunt (0kt/y – incorrectly)– In opposition to shaft Czułów, this shaft is still operating and took all the ventilation work in relation to new longwalls work by Staszic mine in one of the deepest coalbeds containing a large amount of methane. In this case, however, the model has attributed scaling factor to zero with no methane emission. Comments regarding few westerly located shafts: Pniówek IV (0kt/y – incorrectly)– The active shaft belonging to one of the biggest methane emitting mines over USCB. Direct measurements recorded in the same time when the flight was performed allow to confirm that this shaft was operating and releasing VAM (methane associated with ventilation) on usual level (approx. 0.2% CH4 with 10000m3/min of air). For an unknown reason, the scaling factor reduced this emission to negligible values. Chwalowice V (17kt/y – might be correct) – The shaft belongs to the mine with two levels of exploitation. Shaft V was renovated in the year 2009 due to the demands of a new coalbed being planned for excavation. For this reason the shaft release much more methane than shaft no.VII which ventilates the older coalbeds containing less methane. Here, the proximity of the shafts to flight route allowed for the rational application of optimized scaling factors. However, methane emission of the mine is at the lower or same level as Marcel, Rydultowy mines, where deeper coalbeds are excavated (contain more methane). Summarizing, in the eastern part of USCB there is a mismatch of emission attributed to particular shafts at the summary level of 100Kt/y with the biggest value 40kt/y for the single shaft. I agree with authors that the momentary value should not be directly compared with yearly release reported by the coal mines as the day to day (or even hour to hour) emission my change by 100% (e.g. increase from 10 to 20 m3/min). For this reason, all the modelled values should be expressed in short time units (minutes or hours). But attributing large emission to a closed shaft or non-methane shafts are beyond of such explanation. In my opinion, the authors overestimate the accuracy of the dispersion model by an order of magnitude. Obviously, it has not enough spatial resolution to distinguish between particular shaft in case of single-wall pattern measurement.*

This is very valuable information and it is nice to see, that even under the very challenging situation of only 10° mean wind direction delta between the morning and afternoon flights, the algorithm is able to determine a magnitude of emissions from the shafts to match your information. We would like to encourage you to share your findings or your measurements with the scientific community as these data may be of interest for many others not reaching this comment. Based on your advice and the suggestion of Reviewer #1 we included this information into our a-prioris as "local information". The a-prioris for the closed mining shafts as indicated above have been set to $1\,\mathrm{kt\,yr^{-1}}$. The change of a-prioris, coupled with introducing correlations in the observational covariance (as suggested by Reviewer #1) led to some negative correlations in the a-posteriori correlation matrix. Furthermore uncertainties for individual shafts are enhanced compared to the version neglecting correlations. Lagrangian particle dispersion models like the one used in this study apply griding only after the simulation and only on the simulation output. Hence, the only spatial resolution that comes into play is the one of the diluted plume measured at the aircraft and the gridded meteorological input data. If you refer to the gridded input data, than achieving better simulation results would require knowledge on the turbulence in one grid cell, which is not available. In the end further campaigns and further research is needed to increase the level of confidence on the model side and to gather data at different wind conditions.

I would like to give the appreciation for the authors to organize the COMET campaign and invest a lot of time to obtain great and novelty results which certainly are valuable material for publication.