This article provides coal mine methane emission estimates for three regions of the Shanxi province (China), using the recently-developed wind-assigned anomaly method with methane concentration observations from the TROPOMI satellite instrument. The results suggest that commonly-used emission inventories overestimate coal mine emissions in the area. The sensitivities of wind-assigned anomaly results to several of the method inputs and parameters are described.

The article is concise (too much actually, see below) and its English reads well. I think it is a relevant addition to the literature because (1) it confirms previous results on Chinese coal mine methane emissions with an original method; and (2) it builds more confidence and understanding of the wind-assigned anomaly method and its sensitivities.

I recommend the publication in ACP once all the following comments are addressed.

Significant comment on structure, method and data description, and naming

While concision is indeed a quality when writing a scientific article, the authors must be careful to provide enough information so that it can still be read as a standalone piece. In its current state, this article cuts too many corners in describing their datasets and methods to be read smoothly and requires, on this matter, a significant adjustment.

Data set descriptions

The text keeps referring to “the bottom-up inventory” (in the abstract !!, line 20, and at lines 141, 147, 166, 171, 183, 228, 232, 236, 258 and 275) which is different from commonly-used EDGAR or CAMS-GLOB-ANT, but without ever properly presenting this different inventory. The reader has only the captions of Figures 2 or 4 to rely on to guess that “the bottom-up inventory” is actually work by Qin et al. (2023). Considering that the Qin et al. (2023) bottom-up inventory is a significant discussion reference, it needs to be clearly presented in the abstract, and presented and described in the main text.

For clarity, I would suggest to gather the descriptions of all three emission inventories (Qin et al. (2023), EDGAR and CAMS-GLOB-ANT) in a dedicated subsection of Section 2 “Data and method”.

In addition, regarding naming, the expression “the bottom-up inventory” which is repeatedly used to refer to Qin et al. (2023) may be confusing to some readers as EDGAR and CAMS-GLOB-ANT can also be understood and referred-to as bottom-up inventories (e.g. Janssens-Maenhout et al., 2019). I would suggest to use the actual citation or a defined abbreviation/ acronym to refer to the Qin et al. (2023) bottom-up inventory in the text, and in Figures captions and labels.

Method description

Subsection 2.2 named “Wind-assigned anomaly method”, only briefly provides the Tu et al. (2022a) reference that actually gives the description of the wind-assigned anomaly method, and then just details the approximate “cone plume” model.

These few elements are insufficient for a standalone reading and understanding of the work performed in this study. While it is unnecessary to reproduce all the description and equations provided in the main text and appendices by Tu et al. (2022a), a 1-2 paragraph digest description of how the method works is at least expected.

The reading would be greatly improved if such a 1-2 paragraph digest description of the wind-assigned anomaly would mention and provide the minimally-required information on: (1) the background estimation and removal; (2) the principles of averaging TROPOMI data for two different wind field segmentations and making the difference of those averages; (3) the fitting of modelled against observed wind-assigned anomalies to estimate an emission scaling factor; and (4) uncertainty bar calculation.

Besides, the discussion of using a Gaussian plume instead of a cone plume model included in this work shows that the cone plume model, in itself, is not essential to the wind-assigned anomaly method. For clarity, I would thus suggest to separate the descriptions of the wind-assigned anomaly method principles and approximate plume models in different sub(-sub?)sections, and so also move the description of the Gaussian plume model from Sect.3 to somewhere in Sect 2.

<u>Significant comments on Results and discussion</u>

The Results and discussion section can be improved on three different aspects, detailed below.

<u>Discussion on elevation features, approximate plume models and their opening-angle parameters</u>

To my understanding, this application of the wind-assigned anomaly method in Shanxi brings a new additional interesting aspect that is not currently discussed in the paper. Works by Tu et al. (2022a,b) previously studied locations where methane can be transported in plumes over relatively flat terrains, along elevation features. However, this new study area in Shanxi has elevation features all around the target sources, methane is being blown against these elevation features and piles up at the bottom of valleys sources.

I would expect that approximate plume models struggle more to reproduce realistic enhancements in mountainous areas with complex elevation features such as Shanxi, compared to flatter terrains like the ones near Madrid or around the Polish coal mines. Interestingly, the discussion in Sect 3.4.1 shows for Changzhi region that changing the approximate plume model from cone to Gaussian improves the wind-assigned anomaly result comparison to Qin et al. (2023). Furthermore, it also shows that increasing the opening angle of these approximation models from 60° upwards improves the comparison even more.

Does that also hold for Jincheng and Yangquan regions? Could the Gaussian plume model with wider opening angles, to some extent, be more appropriate to approximate transport in such mountainous areas with complex elevation features compared to the cone model with the lower opening angle of 60°? What could be explored to test such an hypothesis? Could test experiments with N2O help, like what was done near Madrid in Tu et al. (2022a) to show that wind-assigned anomaly works?

I do not obviously expect that all these questions will be precisely answered after completing the review process. However, I think that additional discussion elements on the appropriateness of different approximate plume models and/or of their opening angle parameter values, possibly in relation with complex elevation features in Shanxi, can be an interesting and valuable addition.

Revising uncertainty estimates to account for method-related errors

Currently, the uncertainty estimates, which seem to correspond to the error bars in Figure 6, amount to only a few percents of the total emission estimates: 1.9%, 1.4% and 3.7% for Changzhi, Jincheng and Yangquan regions, respectively. From Sect. 2 (see significant comment on method description above), I can only guess that these uncertainty estimates include the contributions of background estimation error and satellite data noise, as performed in Tu et al. (2022a,b).

In addition, Sect. 3.4 discusses the impact of (1) changing the approximate plume model to the Gaussian plume model, and perturbing the opening angle; (2) changing the wind product from ERA5 to NCEP, and changing the direction segmentation; and (3) changing the a priori inventories. Besides, other parameters may influence the results as well, such as the height of the wind speed, as discussed in Tu et al. (2022b): why is 100m chosen in this work, whereas 10m was used for Madrid area in Tu et al. (2022a), and 330m for Poland, in Tu et al. (2022b)?

Overall, the sensitivity tests reported here result in emission rates changing from -5% up to +12%, which is larger in magnitude than the maximum of the currently reported uncertainties. As the choice of many method inputs and parameters can be somewhat arbitrary (ERA5 winds against NCEP, Gaussian against cone plume, 60° against 80° fov, wind speed height, etc), the uncertainty estimates provided in this work need to be revised to account for the contribution of these method-related choices and uncertainties.

For example, ways to account for these method-related uncertainties can be through the definition of a comprehensive quantification ensemble, which explores reasonable ranges for different method inputs and parameter values, such as done by e.g. Schuit et al. (2023), or to sum different contributions in quadrature, such as done by e.g. Cusworth et al. (2021).

Revised uncertainties are expected to be higher. However, these larger uncertainties may actually help to better compare with emission rates reported by Qin et al. (2023), and to assess how significant the difference found between EDGAR/CAMS-GLOB-ANT and wind-assigned anomaly results is.

Discussion of results against previous satellite-based top-down estimates

Satellite-based estimates of methane emissions are currently being studied and developed at different scales, with different datasets and different methods, by several groups across the world. For example, Chen et al. (2022) report a downward correction of coal mine emissions in China compared to UNFCCC reports, partly driven by Shanxi; or Zhang et al. (2021) report a 30% decrease in their posterior estimates for China, 60% of which are attributed to coal mines, and provide an extensive list of other studies supporting a consistent result.

These previous studies and/or others need to be mentioned in this article. They could for example be included in the Introduction, and their relevant messages cited and discussed when presenting the wind-assigned anomaly results. As they give a similar picture of overestimated Shanxi coal mine methane emissions in EDGAR/CAMS-GLOB-ANT, the overall article message would be even more highlighted, while at the same time building more confidence in the wind-assigned anomaly method.

Minor corrections and questions

- Section 2.1 “TROPOMI dataset”: Please provide the data quality filters applied to select the TROPOMI data included in this work.

- Line 148: Please delete the “latest” adjective for EDGAR v7, as EDGAR v8 has just been released (https://edgar.jrc.ec.europa.eu/dataset_ghg80).

- Figures 4, 6 A-3 and A-5: Please use a consistent label to designate emissions from Qin et al. (2023), “bottom-up inventory” on one side, and “shaft emission” on the other.

- Figure 7: Please start the y-axis range to 0 in order to facilitate comparison with Figure 6.

- Sect 3.4.3: It is unclear whether the “calculated average” (line 231) refers to simulated or observed averaged enhancement, please precise. If it is simulated, the fact that enhancements are lower with CAMS-GLOB-ANT whereas this inventory prescribes nearly twice as high emissions is quite counter-intuitive and surprising. Is this explained by the sentence lines 234-235 about similar background estimation errors that compensate in the wind-assigned difference? If so, could you please reformulate this explanation and move it a few lines earlier in the text, when “calculated average” values are compared?

- Line 236: I think there is a typo, isn’t 8.5 x 10^27 supposed to be 8.5 x 10^26 instead? Otherwise, it would mean an order of magnitude difference…

- Figure A-10: Are marker supposed to be missing in the scatter plot (right panel). If not, can you please add them, otherwise explain why there is no marker in the scatter plot?

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