

We thank the reviewer for the comments and constructive critique. We have responded to all comments in detail and have revised the manuscript to reflect the insights of the reviewer.

Campbell et al. present an approach to incorporate high-resolution satellite data in the prior emission estimate used in a TROPOMI-based inversion. They show that posterior wetland emissions end up being lower because of the updated oil/gas prior emission estimate used in their inversion. While this is in principle a relevant topic and approach, the methodology used raises questions and the inversion itself is done over such a short time period that I have questions about the significance of the results. I would only recommend consideration for publication after major revisions.

My specific comments are included below.

P1L26-L27 I think the sentence on aquatic and wetland ecosystems appears to overstate their importance considering recent budget papers such as Jackson et al. (2024) and Sanois et al. (2025) show anthropogenic emissions make up two thirds of the global budget.

We have added the suggested references and context to the sentence. “ While aquatic and wetland sources remain significant recent budgets allocate more of these emissions to anthropogenic sources through eutrophication, reservoir emissions, and warming (Jackson et al. 2024). Despite reduction in methane attributed to aquatic and wetland ecosystem they remain the greatest uncertainty in global budgets (Saunois et al. 2025).” From the perspective of this paper informing direct monitoring of these emissions and the allocation to anthropogenic drivers only increases the need for robust methods remote sensing of methane emissions.

P2L42 Figure S1 seems to have some odd gaps, for example over Canada where I think wetland and oil/gas emissions are much more widespread than shown here?

There may be gaps in the underlying dataset and they are cited in the caption. As the caption says this is an overlap analysis of oil and gas fields as classified by OGIM 10.1. The comment raises a good point that this does not encompass all production facilities. The map provides good insight into hotspot areas for wetlands and oil and gas fields. We do believe its limitations are clear from the caption.

P2L57 I think EDGAR does not use the UNFCCC-reported emissions, but calculates national emissions using a globally consistent approach based on IPCC methodology?

We adjusted this to clearly state that UNFCCC is in reference to GFEI and EDGAR 6.0 uses IEA data primarily. “These emissions are estimated using either country-reported emissions by sector primarily from the International Energy Association and the United Nations Framework Convention on Climate Change (UNFCCC) for Emission Database for Global Atmospheric Research (EDGAR) and the Global Fuel Exploitation Inventory (GFEI), respectively (Crippa et al. 2021; Scarpelli et al. 2022).”

P3L66 It would be good to already in this sentence be clear that this consistency exists on the level of a state vector element.

Added clarification that this consistency exists on the level of state vectors.

P3L72-L73 This sentence seems to mix the varying definitions of a super-emitter between different papers. The aircraft data from the references would be able to detect much smaller emissions than GHGSat could capture.

While we disagree that detection limit matters for the definition of super emitter, we agree that the cited proportions are not useful and have rephrased. We rephrase the definition to be more general “the small number of point sources that contribute a large portion of global methane emissions” and add Cary (2006) to the citation to support the sentence at a different scale of monitoring.

Cary, I., 2006. EPA Phase II Aggregate Site Report Cost-Effective Directed Inspection and Maintenance Control Opportunities at Five Gas Processing Plants and Upstream Gattling C0111pressor Stations and Well Sites.

P4L101 It would be good to clarify that these metrics are calculated between the prior/posterior simulation and the observations.

Clarified that these metrics are calculated by comparing to the TROPOMI observations.

P4L103 Here and elsewhere, the description of the inversion system is very short, it should for example explain whether a scaled version of the geological seeps inventory is used, how the state vector compilation was done, and what the used uncertainties were.

We have added additional details both geological seeps, state vector clustering, and uncertainty estimation.

P4L112 In Table S2, what are the latitude/longitudes given? Are they scene centers for the observations without detected emissions?

Altered the table caption to include that Lat/Long for non-emission observations are scene midpoint.

P4L118 Are all ‘oil and gas facilities’ (processing plants, wells, and so on) treated as equivalent? What exactly constitutes a facility?

Clarified the sentence as represented by flaring detection as we did not infrastructure maps for the region. A major issue is treating all detection equal and we explore this with our analysis of impervious surface to TROPOMI.

P4L122 The use of flaring data is confusing to me, were emissions only found at locations with flares? Why would flares be a good predictor of emissions? The authors appear to share some of these doubts in Section 4.1 but do not argue why they did make this choice here.

Flares were ultimately utilized due to limited publicly available information on oil and gas infrastructure for the region. We agree there are limitations and we discuss these but flares to correspond with oil and gas infrastructure and in cases where a flare goes out emissions may

occur.

P5L129 Is the use of flaring detections not inconsistent with the earlier counting of infrastructure (P4L126) that was used to get to emission factors?

The extrapolation from emission factor determined by oil and gas within a GHGSat scene (P4L126) to flaring is necessary to get update priors for the entire region of interest. The determination of oil and gas infrastructure was done for the three GHGSat sites but would be prohibitively time consuming and expensive for the region.

P5L133 I guess the authors later argue that the GFEI emissions are very small compared to their estimates but how do they in general prevent double-counting of emissions between GFEI and the GHGSat-based estimates with this approach?

Yes, there is a risk of double counting. Our assumption is that fugitive emissions are mostly lacking from GFEI emissions for this region. We have added additional analysis of this point to the discussion.

P6L149 Two months is a very short time to do an analysis over such a complicated region. The authors should show and explain that there is enough data to perform a meaningful analysis. Additionally, was a spin-up performed before this time window?

There was a month of spin-up performed in the analysis. We have added this detail to the text. We also added a sentence pointing to the number of TROPOMI observations (121,591) and our studies modest goals of conducting a preliminary assessment of fugitive emission priors from GHGSat justifying the short model runs. We agree that longer model runs with more sophisticated extrapolation are justified for the region, but our study presents the need for that further study.

P8L178 Why do the authors put much more weight on the facilities in Observation 3 compared to the other ones? As indicated by their next sentence, this changes the outcome of the analysis by a factor 4.

Our small number of GHGSat observations meant making assumptions and we make clear what the assumptions are. If we were to run additional models the priors would be updated with the lower of these occurrence rates but just lowering emissions for the site would not demonstrably change the result instead it is the distribution of the emission factors that is our focus in subsequent analysis that we bring in during the discussion.

P8L184 This result appears to be strongly inconsistent with the emission occurrence at the facilities/sites that were covered by GHGSat observations (P7L174)?

Yes, if the average of the scaled annual rates were applied to all 305 flaring sites the prior would be more than double. Due to the inconsistency of fugitive emissions and the apparent observation bias of the GHGSat observations, it was evident during our iterative model development that assumptions to reduce our fugitive prior were necessary.

P10L213 A large part of the results appears to be driven by very large (spatially) state vector elements, is that correct?

Yes, and while this does impact the results -we believe the general conclusions on the potential of GHGSat, the need for more data, and approaches to extrapolating fugitive priors in the region are relevant regardless of what state vectors were chosen.

P11Table1 The posterior RMSE is worse after the addition of GHGSat data, can the authors reflect on why this happens?

The discussion includes some points to why this likely occurred, but we have expanded that sentence to include state vector clustering. “The failure of the inversion to improve on this in the posterior model suggests a disconnect between the spatial distribution of our updated priors, the TROPOMI XCH₄ observations, and clustered state vectors.”

P11L227 It would be good to show prior and posterior model – observation mismatches to support this point as the constraints are relatively weak.

We have clarified that this is directly referencing the bias calculations. And added discussion of scaling factor following the point that highlight some of our approaches issues.

P11L228-L231 I think the authors should reflect on their confidence in this value (which is also included in the abstract). They strongly increased the prior oil/gas emissions and most of that increase was corrected away by the inversion, leaving a relatively small difference that then affected the wetland emissions. Do they have confidence in this 0.6 Tg/yr difference?

This is a fair assessment. The abstract has been edited to deemphasize the result and highlight the uncertainty of it. We think it is worth highlighting in the discussion but have added additional discussion of its uncertainty. Our added sentence “This finding is highly uncertain, but our approach demonstrates that fugitive methane emissions from oil and gas exist across the region and correctly modelling their magnitude and spatial distribution would reduce the modelled wetland methane emissions.”

P12L260 I think it would be good if the authors evaluate whether there is a correlation between the TROPOMI data and surface albedo to make sure that does not drive (part of) the identified relationship. See (for example) Lorente et al. (2023).

We have added a citation to Lorente to this part of the discussion. Given the analysis has been done and the relationships are well understood, we believe including this in our discussion is adequate.

P13L271 Several other papers have incorporated high-resolution satellite data in emission priors; examples include Naus et al. (2023) and East et al. (2025). This body of work should be reflected somewhere in the publication.

We agree that these publications should be included and have incorporated them into our discussion. Thank you for pointing us to them.

P14L284 Good to clarify ‘coarse resolution’ refers to ‘spectral resolution’ here.

We have clarified to “coarse spatial resolution” – we are particularly thinking that coarse resolution would be more likely to overlap these peatlands and adjacent water bodies making successful retrievals less likely. We agree that coarse spectral resolution also makes retrievals less likely.

P14L286 Do the authors mean the GHGSat plumes can be seen between the waterbodies?

Yes, and the method for calculating plume emissions is resilient to slight speckling of the plume in pixels that have no retrieval due to being over water. This is evident in Figure 3, where small sections of the southern plume is not visible due to the river but not impact on the emission estimate is expected.

P15L312 Based on the IMI v2.0 paper from Estrada et al. (2025), which should be cited, the IMI uses glint observations over water now, but I think these would not occur at high latitude so I am not sure what the authors are referring to here.

We are referring to the option in the IMI configuration for >2.0 UseWaterObs, which if flagged will be less restrictive in what observations are filtered. We have revised the sentence to make this clear. We are not referring to glint observations.

P15L331 The data given on the GHGSat analysis are rather shallow, only detected emissions are given. Could at least plume images/data be made available?

Unfortunately, we cannot provide the commercial data but have provided all necessary data to evaluate the current study. These data were acquired through the NASA commercial satellite data acquisitions program. That program provides data access to NASA funded researchers. The ESA data partnerships program also provides access to GHGSat data.

References

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Reviewer 2.

We thank the reviewer for their constructive critique and insights. We have sought to improve the manuscript following their guidance.

Campbell et al. have developed a framework using methane plume detections from the GHGSat high spatial resolution mission in order to improve prior emission estimates from fossil fuel sources in wetland areas. This prior estimate is in turn used to improve regional methane flux inversions of West Siberian Lowlands (WSL) based on coarse resolution satellite data from TROPOMI and the Integrated Methane Inversion (IMI) system.

I think the study illustrates well the potential of high resolution data from point source imagers to improve regional flux estimates. The manuscript is well written and presented, and the topic fits well into AMT.

I would like to request the authors to address the following points before the manuscript is accepted for publication:

1. Motivation: in my understanding, the goal of the paper is to illustrate the benefit of using a better prior, as enabled by point source data, for regional inversions with the IMI. The WSL seems just a convenient study case, as there could be many others. Also, wouldn't the same approach and conclusions apply to other basins and sectors different from wetlands? For example, Naus et al. followed a similar approach to improve regional methane emission estimates from Algeria's oil & gas fields <https://pubs.acs.org/doi/10.1021/acs.est.3c04746> (paper not cited, by the way).

I think the motivation and range of application of the proposed methodology should be better presented.

We agree with the reviewer's insight. We have now cited Naus et al. 2023. We have revised the manuscript introduction to better articulate the motivation of the work and range of applications.

Added sentences which clarify motivation and scope of the study added to the introduction “Our methodology, using point source data from satellites to improve prior emission estimates is applicable beyond wetlands to any sector or region where emission sources spatially overlap, as demonstrated by similar approaches for Algeria (Naus et al., 2024) and globally (East et al. 2025). The Western Siberian Lowlands serves as an illustrative case study where we explore extrapolation of observed emissions to regional priors such frameworks could reduce sectoral misattribution between anthropogenic and wetland sources.”

2. Further point source data: the authors write “Our approach demonstrates potential but needs more data to reduce the uncertainty.”. The study is restricted to point source data from GHGSat. This is certainly the mission with the largest data archive (if one can get access to it) and the higher sensitivity to point sources, especially under non-ideal observation conditions such as those in the WSL. However, there are other publicly available plume datasets from high resolution missions that the authors could combine with GHGSat in order to extend their database (e.g. IMEO <https://methanedata.unep.org/map?fs=&e=#mcoord=1.82/10/15>, EMIT <https://earth.jpl.nasa.gov/emit/data/data-portal/Greenhouse-Gases/> and Carbon Mapper <https://carbonmapper.org/data>). The MethaneSAT mission also detected large plumes in the WSL (Fig. 13 <https://egusphere.copernicus.org/preprints/2025/egusphere-2025-4666/egusphere-2025-4666.pdf>)

Could the authors check those other methane plume databases and include any useful data in their study?

Thank you for providing the detailed comment directing us to a wealth of additional plume and methane emission data. We have reviewed all data sources and incorporated the Carbon Mapper data into our discussion of plumes in the WSL. Unfortunately, all other datasets lack plume data for the WSL. The reviewer is correct that the Carbon Mapper data would have been invaluable to our study’s modeling but incorporating them now is not feasible instead we have incorporated these data along with our discussion of SRON plume data.

(3) Improving the O&G prior: I am not sure I understand how GHGSat data are used to build the prior. Why the point source data from GHGSat are solely related to flares? And how do the authors account for emissions below GHGSat’s detection limit (which may be quite large for the challenging observation conditions of the WSL)? I think more details on this part of the processing are needed.

We have rewritten our prior development section. We updated methods used to estimate the plume emissions by GHGSat and our methods for extrapolating these to region emission priors for this study. Additionally, in the discussion we have added more detail on the limitations of our study and the potential for further analysis.