We thank the two reviewers for their comments on our manuscript "Assessing methane emissions from collapsing Venezuelan oil production using TROPOMI". We provide our answers to the reviewers (in gray) below. Changes to the manuscript (in *italics*) are marked in ***bold***. In addition, we have included an additional acknowledgement and added Lucas Estrada as a co-author who was left out of the original submission by accident. The additional co-author, corrected references, and change in the figure order are not captured in the tracked changes and are highlighted instead in the provided manuscript with changes.

Reviewer 1

General Comments:

This is an innovative paper on two tough and important questions – how can we assess methane emissions a) in nations that have not signed the Global Methane Pledge? and b) in places where strong cloud cover makes satellite observation of emission plumes difficult or intractable? Question a) has obvious global significance in trying to persuade nations to help restrain the growth in the methane burden to within Paris Agreement limits, while Question b) has wider implications for assessing not just human emissions but also natural emissions in places like the Congo basin or much of Amazonia where wet season cloud cover is deep and very long-lasting.
The paper focusses on the Lake Maracaibo area, perhaps one of the most intractable places on the planet for satellite assessment of methane emissions, and gets around the observational problem by exploiting the previously unprecedented level of coverage provided by TROPOMI, coupled with WRF modelling. The analysis is state-of-the-art.

We thank the reviewer for the positive words on our manuscript, we have addressed the specific comments below.

Specific Comments:

Very little is said in the paper about wetland fluxes. There are some major wetlands here, especially in the Ciénagas De Juan Manuel National Park. Moreover, Lake Maracaibo itself is very shallow and subject to widespread eutrophication, especially where heavy oil pollution is significant. The paper does address wetland fluxes by using WetCHART, which is briefly mentioned on line 171. But is that approach sufficient, in this location where very large wetland emissions may be occurring?
Concerning the emissions from natural wetlands, in Bolivian Amazonia, France et al measured much larger fluxes than perhaps expected. France, James L., et al. (2022) Very large fluxes of methane measured above Bolivian seasonal wetlands. *PNAS* 119 e2206345119. Similarly, note that in Africa, Shaw et al. (2022) measured very large fluxes and, referring to models like WetCHART, commented that "The models may not be adequately representing important CH4 production and emission processes for vegetation typical of African wetlands." [Shaw, J.T.(2022). Large methane emission fluxes observed

from tropical wetlands in Zambia. *Global Biogeochemical Cycles*, *36*(6), p.e2021GB007261.].

We have added the regional importance of wetland emissions and the difficulty in modeling them to the introduction:
*"**The region also harbors large wetland emissions (Bloom et al., 2017), which are difficult to model as evidenced by studies focused on other regions (France et al., 2022; Shaw et al., 2022)**"*

> Might this comment by Shaw on model failure also be valid for the Ciénagas De Juan wetlands? This is one of the world's cloudiest places, with intense deep thunderstorms. Inevitably, any remote sensing observation when the clouds are absent must be during unusual weather – presumably on these days the surface waters in wetlands are hotter because the sun is shining. That heat will drive methane emissions. Can the authors be sure that what they observe during clear-sky intervals is typical? Or are they observing unusual high-emission episodes? For anthropogenic emissions, there probably is little variation between cloudy and sunny days but I'm particularly concerned by that wetland emissions might be larger when it is hot.

We already stated in the results that we find a large fraction of wetland emissions cannot be effectively constrained by TROPOMI. We have added a note on a potential clear-sky bias.
*"We find that for both oil and wetland emissions a large fraction of emissions at the national level can not be constrained by the TROPOMI data **(Quantification of average wetland emissions is further complicated by the fact that we only have observations on clear-sky days)**."*

> As for the emissions from eutrophication of Lake Maracaibo, is WetCHART able to handle these unusual human-influenced emissions – it must be one of the worst places on the planet for oil-linked pollution.

We now mention the eutrophication of the lake in the introduction.
*"Compared to 2018, oil production in the Lake Maracaibo area decreased by 60% in 2020, to 135 thousand barrels a day (Rystad Energy, 2022), and abandonment and decay of the production infrastructure **as well as strong eutrophication of the waters** have been widely reported (AP News,2019; NASA, 2021)."*

> Thus my point – does the paper adequately discriminate between 1) direct emissions from oil & gas extraction, 2) emissions from natural wetlands; and 3) emissions from polluted shallow fringes of the Lake.

As mentioned above, we nuance our ability to estimate wetland emissions on the national scale. For the local optimization, our results are mainly focused on the oil production hot spot and our manuscript mentions:
"Emissions from the other source sectors (predominantly livestock and wetlands) show small increases that are insignificant compared to the prior and show relatively worse constraints from

TROPOMI as a larger fraction of the emissions occurs outside of the well-constrained hot spot area."

We have now added the limited information obtained on wetland emissions to the conclusions:
"Especially emissions over the eastern Orinoco production basin show little TROPOMI sensitivity. ***While the inversion only provides limited information on wetland emissions,*** *it does show sensitivity to the Lake Maracaibo area."*

> I suspect that without both local overflights to give better geographic resolution and parallel surface or low-altitude measurement to collect d13C(CH4) and D/H(CH4) isotopic information, it is not going to be possible to discriminate between these three types of sources.

We have added a note on the use of methane isotopes in the conclusions:
*"Our work can be used to target future analysis including extending our analysis for later years and incorporating facility-scale methane observations from high-spatial-resolution satellites and suborbital observations (****including of methane isotopes****) to give additional insight in the (evolution of) local emissions from different source sectors and serve as an independent verification of satellite-based inversion results."*

> That concern does not mean this paper is unpublishable or even needs major revision – it's a very knotty problem and the authors have made a very creditable stab at cutting the knots. But they do need to recognise the complex problem of local microbial wetland emissions in the study domain and make it clear just how complex that problem is.

We hope to have addressed the reviewer's concern with the additions described above.

> Finally, as a minor concern, the paper does not mention the large emissions from the oil and gas industry and wetlands nearby to the east in Colombia. I have flown much of the length of the Magdalena River and was impressed by just how much was going on there in methane terms. I appreciate the transport modeling should deal with this but maybe not. It should be mentioned if westerly winds were active on the boundaries of the domain.

We have added a sentence to the Data & Methodology section now specifically mentioning emissions in Colombia, which are accounted for in the optimization of the background ``buffer" cells of the state vectors:
"The buffer zone elements mainly serve to correct the background concentrations of air floating into Venezuela. ***These include, for example, wetland, coal, and oil emissions in neighboring Colombia that can be seen in Figure 1a.****"*

Conclusion

This is an important and innovative paper. I recommend publication after moderate revision.

We again thank the reviewer for the positive evaluation of our manuscript.

Reviewer 2

General comments

I find the manuscript by Nathan et al. interesting and well-written. It is important to have reliable quantifications of methane emissions, and Nathan et al. assess methane emissions in a country with large emissions (Venezuela). The area is challenging to observe by satellites, and the authors use inversions based on two different models to quantify emissions in the area. In addition to providing revised emission estimates, the demonstrated methodology provides useful information on the capabilities of combining satellite data and inverse modelling. However, I have included several comments below, where my main comments concern the description of the WRF-Chem simulations, which needs a lot more detail in order for the results to be reproducible, and the interpretation of the wind fields in the different datasets.
I recommend that the manuscript is suitable for publication after minor revisions.

We thank the reviewer for the positive evaluation of the manuscript. We have updated the manuscript following the specific comments below, including an expansion of the description of the WRF-Chem simulations.

Specific comments and technical corrections

L5: I suggest replacing “regional” with “local” or similar, to make clear that the area studied with WRF is smaller than the national scale area studied with IMI (the term “regional scale” could be misinterpreted as e.g., a whole continent).

We have replaced all instances of “regional” with “local”.

L9: Similarly to the above comment, I suggest replacing “regional result” with “result over the same region” or similar.

We have made the requested change.

L23 “two chemical transport models”: WRF-Chem is not a chemical transport model as it computes its own meteorology.

We have replaced "chemical transport model" with the more general "transport model".

> L27: The longitude range for the Lake Maracaibo area should be given as degrees W(est) and not S(outh). This is also the case on L104, L119, L287, and footnote b of Table 1, maybe also in other places.

We have replaced all erroneous instances of degrees South with degrees West.

> L81-82: See earlier comment about the use of national vs. regional.

This has been resolved in a previous comment.

> L83 "Weather Research and Forecasting model": I would add "with chemistry"

We have added "with chemistry" to both mentions of WRF-Chem.

> L117: OH is the major sink for methane and highly variable in time and space. How are the OH fields generated – are they also calculated by GEOS-Chem? If so, there should be some information on emission data used for compounds such CO and NOx, which are important for OH.

We use the GEOS-Chem methane simulation, which employs offline chemistry using archived OH fields. We have included a sentence clarifying this in the manuscript:
*"Methane has a long atmospheric lifetime compared to its residence time in our IMI simulation but nevertheless the IMI includes sinks of methane from oxidation by OH and Cl (**based on archived concentration fields from a full-chemistry simulation**), stratospheric loss (Maasakkers et al., 2019), and soil uptake (Murguia-Flores et al., 2018)."*

> L124-134: There are numerous options for chemistry in WRF-Chem, yet there is no mention of even the chemistry scheme used. Please provide more details. I feel this is particularly important here because WRF-Chem is not used to a large extent in methane studies.

We have now updated the text to specify that we simulate methane as a passive tracer in the WRF model:
"**Because of the short residence time for methane in the domain compared to the atmospheric lifetime, we use a passive tracer model for our simulations.**"

> L127-128: Figure 1 shows that the extent of the outermost WRF-Chem domain is quite large, and the horizontal resolution of 27 km is comparable to that of GEOS-Chem. In principle, I would think that these simulations could be used in an inversion for the whole Venezuela region, to see how that compares to IMI, or is there a reason not to? It looks like WRF-Chem outer domain results are shown in Supplement Figure A3c,d.

The reviewer is correct that, in principle, the WRF simulations could have been set up in a way that the inversion covered all of Venezuela. However, we think our WRF setup is best utilized in focusing on quantifying emissions from the Lake Maracaibo area at high resolution. Our optimization of the surrounding areas is aimed at providing adequate boundary conditions for our domain of interest. Optimizing emissions at native resolution for the entire grid would also have been computationally infeasible. We have added a sentence to the description of the WRF-chem model, in order to clarify:
"These outer domains are set up to allow for an adequate representation of the background in the innermost domain."

We have also further clarified this in the caption of Figure 1c:
"The different WRF domains can be discerned based on the resolution of the state vector elements, ***where the outer domains were set up to get an accurate representation of the background in the center domain."***

> L128: What is the vertical resolution / number of vertical layers?

We now specify that we use 33 vertical layers.

> L130-132: For what compounds are CAMS initial and boundary conditions being used? Only CH4 or also other compounds, e.g. those important for OH production?

We now specify that we use CAMS for the initial and boundary conditions on methane concentrations. As mentioned above, we do not simulate other species, which we clarified in the manuscript.
"We use the Copernicus Atmosphere Monitoring Service (CAMS) global forecast at 0.4◦ × 0.4◦ and 6-hr resolution to provide initial and boundary conditions ***on methane concentrations*** *(Koffi and Bergamaschi, 2018), which we also optimize in the inversion."*

> L132-133: Why is the soil sink from the IMI subtracted from the emissions used in WRF-Chem? Is it because WRF-Chem does not include the soil sink for CH4? I would think such a subtraction could cause problems due to negative emission values in some places? Please also specify what other CH4 sinks are treated / not treated in the applied WRF-Chem setup (OH oxidation, Cl oxidation, stratospheric loss).

We included the soil sink used in the IMI in our WRF simulations. While it is co-optimized with emissions in our WRF optimization and not in the IMI optimization, we have accounted for the soil sink in both posterior results such that we compare equivalent emission-only totals. As noted in the text, the total value for the Lake Maracaibo region is only 0.02 Tg a^{-1}, so the sink should have a negligible effect on our results. We have clarified the text:
"We ***use*** *the soil sink from the IMI but the effect is small (0.02 Tg a^{-1})"*

> L169: Please check the "Commission et al., 2021" reference, there seems to be something wrong with the author names here.

We have corrected this formatting error.

> L173: “Emission maps” -> “Emission totals and maps” ?

We have changed the text to:
*“**Total emissions and maps for individual source sectors** are included in Table 1 and Supplemental Figure S1.”*

> L252 “Supplement Figure A4”: The supplement figures should be in the correct order (Supplement Figure A3 has not been cited yet).

We have reordered the supplemental figures to match the order of their first mention in the text and the text has been updated accordingly.

> L257 Supplemental Figure A3 “Changes between the prior and posterior are small visually...”: I am not able to identify any differences between panel a and b (they look identical to me), even in the Lake Maracaibo area where there is a large increase in emissions in IMI posterior vs. prior.

While we acknowledge that it is difficult to identify the differences between panels a and b in this figure because the background is already captured in the prior simulation, there are some visible local differences though. We have added a sentence to the caption to this effect:
“The clearest differences can be seen at the northern edge of Lake Maracaibo.”

We realized there was an error in figure titles, which we have now remedied.

> L263-265: Could highlight that the difference between WRF and IMI inversions is very large for oil (almost a factor of 2 according to Table 1).

We have added this highlight to the text:
*“Lake Maracaibo emissions are estimated at 1.2 (1.0 - 1.5) Tg a^{-1} (Supplement Figure A5), dominated by oil production (51 (44-58)% of total emissions) and lower than the results based on the IMI inversion, 2.0 (1.6 - 2.4) Tg a^{-1}. **The difference is mainly related to the two inversions’ oil estimates** (Source sector emission totals are included in Table 1).”*

> L282-285: Is it certain that these other differences between the models can be neglected – e.g., could potential ozone precursor emissions in the region affect local OH concentrations and thereby methane loss in the models?

OH loss only has a very minor effect as the emissions are concentrated and the residence time of methane in the region is very small compared to its lifetime. We have clarified this with the earlier addition about simulating methane as a passive tracer in WRF-Chem.

> L286 “WRF output 10-m wind speed based on NCEP”: Normally, WRF is creating its own meteorology and NCEP is only used as initial and boundary conditions, is that not the case here, or is strong nudging towards NCEP data applied?

We specify in the text now:
“… based on NCEP **boundary and initial conditions** …**”**

> L287-293: The lower winds in WRF sounds like a viable reason for the difference between WRF-Chem and IMI-based inversions, but it seems like the lower winds in WRF have been attributed to lower winds in NCEP, which is not necessarily true. The initial conditions from NCEP would basically have no influence on the WRF winds after 1 month of spin-up, and the influence of NCEP as boundary conditions are also limited because the lake is far from the boundaries of the outer domain. Have the winds in WRF been compared against NCEP? As NCEP is only used as initial and boundary conditions (unless nudging is applied – see my comment above), I would expect WRF winds to potentially deviate a lot from NCEP, e.g., due to different physics schemes (PBL scheme, surface layer scheme, etc.). I am also wondering if the difference in wind speed taken at one location (over the lake) could be a result of the different resolution of the different datasets (NCEP/GEOS-FP/ERA5/WRF). Could it be that WRF calculates lower wind speeds because of its higher resolution, which would better account for terrain effects, land-sea breeze, etc.? It is of course difficult to evaluate which dataset performs best without any wind observations, but some more discussion around this would be useful.

We have improved the wording to specify which parts of the wind comparison were performed against the output WRF winds and which were against the NCEP data that drove the boundary conditions:
*“To further investigate the differences in transport, we compare the WRF output 10-m wind speed based on NCEP **boundary and initial conditions** to the GEOS-FP 10-m wind (used in the IMI) over the lake (sampled at 9.8 N◦, 71.5 W◦). We find that at the overpass of TROPOMI (∼18:00 UTC), the GEOS-FP average wind speed of 2019 is 2.8 ± 1.2 m s^{-1} (standard deviation), a factor 1.9 larger than the **WRF-derived** wind of 1.5 ± 0.8 m s^{-1}. The independent ERA5 reanalysis (Hersbach et al., 2020) gives a wind speed of 1.9 ± 0.7 m s^{-1} for the same time and location. Similarly, we find winds between 975 and 800 hPa in GEOS-FP are a factor 1.6 larger than in NCEP, **which is used to drive the WRF boundary conditions**.”*

We have additionally added a sentence to address the questions raised by the reviewer:
*“**While the lower wind speeds calculated by WRF may partly result from the higher resolution of the model as it attempts to resolve around the local terrain, the large difference in the data driving the boundaries is considered to be the most likely culprit.**”*

> L330 “NCEP winds used...”: I would add “as initial and boundary conditions”

We have updated this sentence to reflect the reviewer’s input.

L491 & 506: Please correct the web addresses.

We have corrected both web addresses.