Atmospheric Chemistry and Physics Discussions

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Comment on acp-2021-1056

Anonymous Referee #2

Referee comment on "Quantification of methane emissions from hotspots and during COVID-19 using a global atmospheric inversion" by Joe McNorton et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-1056-RC2, 2022

My comments on the paper are put below and also can be found in the pdf document

Review of the paper McNorton et al.

Quantification of methane emissions from hotspots and during COVID-19 using a global atmospheric inversion

General comments

The paper deals with the estimates of CH4 surface emissions by using a short window (24-h) 4D-Var global inverse modelling system based on the ECMWF Integrated Forecasting System (IFS) within the Bayesian framework. The system uses solely satellite retrievals of the total column of CH4 concentrations (XCH4) to constrain the surface fluxes of CH4. First, the authors performed a suite of sensitivity experiments to identify an appropriate prior flux uncertainty. Thus, by using several prior estimates, they carried out inversions and used their forward model to compute the relevant optimised concentrations that are confronted to XCH4 measurements from the global TCCON network. Second, by using their best prior flux information, they performed estimates of CH4 fluxes at global and regional scales for some months of the years 2019 and 2020. Furthermore, they investigated the feasibility of their system in addressing different CH4 emission hotspots in several parts of the world. Finally, the authors assessed the impact of COVID-19 lockdown on the fate of CH4 surface emissions.

Results show that the system is able to estimate the fluxes at both regional scales provided appropriate prior fluxes and relative dense observations. Their investigation of the feasibility of the system in addressing the CH4 emission is more challenging. However, results show that the system is capable of detecting part of the blowout events when enough observations are available. There, maybe the limit of the system does not depend only on the prior information, as the authors often stated in the text, but also on the lack of pertinent observations during such rapid events due to the smoothed nature of the satellite retrievals (in time and space). In fact, for such rapid events, the use of in situ surface measurements (if available and especially at a high temporal resolution) in addition to the satellite retrievals may give better estimates.

They are two points to clarify:

1) To compensate for the lack of uncertainties in the inverted fluxes, the authors considered only pixels having a reasonable number of satellite retrievals to calculate the posterior fluxes. Doing so is reasonable, but this can introduce some uncertainties when comparing your results to other published estimates.

2) Why did the authors perform the inversions only for some months of the two years? Indeed, doing this is enough for the COVID-19 lockdown, but this may also introduce some uncertainties when comparing the results of this study at annual scale to the previous analyses.

This is a comprehensive work on the estimates of CH4 surface emissions at global, regional scales and source point events. I have very much appreciated the part of the work that investigates the feasibility of the system in addressing source point emissions. Indeed, this is a challenging subject, but the results seem to indicate that the system is promising. I have also appreciated the fact that the authors underline the strengths and weaknesses of their system (e.g., missing uncertainty on the posterior fluxes) by giving some ideas for its improvement. As an example, they plan to increase the size of the short assimilation window of their system. Here, it is not easy to know if the increase of this window would be beneficial? However it is worth implementing it. As I mentioned above, maybe also to envisage the inclusion of in situ surface measurements in the system that may enrich the satellite retrievals in terms of peaks (high temporal resolution in situ data) for some of these rapid source point events. Moreover, in general, the inclusion of in situ measurements may help to correct some biases in the satellite retrievals.

Overall, the methodology is sound and quite well described and especially the results are well presented and discussed. Moreover, the manuscript is well written and easy to read. After the clarifications of both the two issues highlighted above and some suggestions in my specific comments below, I would recommend the manuscript for publication in ACP

Specific comments

Abstract

Lines 17-18: …, *but without accurate prior uncertainty information, were not well quantified.'* Accurate? This is a bit misleading. This would mean that you have compared this uncertainty to a reference value. Maybe use as in the text: ' .. with appropriate prior uncertainty or your best prior information

Lines 18-20: As already mentioned in my general comments, I do not understand why you performed the inversions only part of these years (i.e., 2019 and 2020). The comparison of the posterior estimates to the prior ones for the same period is fine, but the numbers for each of these years may be uncertain owing to the missing months.

- **Introduction**

Lines 26-27: '*Changes in atmospheric chemistry, not investigated here, may have contributed to the observed growth in 2020'*: How? The decrease of OH? Please elaborate.

Line 80: '…. *at a high spatial and temporal resolution'* by at both high spatial and temporal resolutions ?

- **Methods**

Lines 88-90: *'These were performed from January to June of 2019 and January to September of 2020..*'. I have not understood why the other months of these two years are not considered. Please elaborate

Line 133: Why do you consider the flux of the model LPJ-WHyme (page 96) and then you use the uncertainty from WetCHARTs. It is possible to consider both the mean estimate of CH4 emissions from WetCHARTs and the uncertainty as you do here

Lines 149-151: '*TROPOMI uncertainties provided as part of the CH4 product were applied within the minimisation routine and averaging kernels were use*d.' Give a reference of this data. May be the reference like Otto Hasekamp et al., (2017) [https://sentinel.esa.int/documents/247904/2476257/Sentinel-5P-TROPOMI-ATBD-Methane-retrieval] or an updated version?

Line 150: *'Additional CH4 observations from the …'* by Additional XCH4 observations from the …

- **Results**

Lines 158: '… most of countries...' by most of the countries in the world.. ?

Line 170: '*When evaluating XCH4 concentrations simulated with optimised emissions, the all-site average lowest standard error (6.8 ppb), absolute mean bias (7.52 ppb) and highest R-value (0.74) was found for the mapped prior error described in section 2.2.2*.'. Please give the range of R-value instead of only the highest R-value, otherwise give the lowest R-value.

3.2. Global estimates.

In this paragraph, I have not understood why you performed the inversions only for some months of the years 2019 and 2020. You compared your numbers to those of other studies in literature. Your numbers might include some additional uncertainties due the lack of these months. Moreover, the way you sample your optimised fluxes can introduce additional uncertainty in the numbers. In fact, what can be the contribution of the pixels discarded to the total numbers? I do not question the way you sampled your data, just be aware that doing so can introduce additional uncertainty and this needs to be quantified/mentioned in the paper. In fact, a rough estimation of the uncertainty due to your sampling method can be estimated by considering all the pixels to generate the numbers and compare them to those derived from your sampling method. I do not want to repeat these above remarks in other parts of the paper, hence please consider them and address them in the other parts of the paper when relevant.

3.3. Emission estimates for Regions and Point Sources

Line 205: *'To filter posterior estimates which provided little or no updated information we omitted daily grid cells associated with poor observation constraints (see supplement figure 1)*.' Again, how much these pixels may contribute to the numbers? This remark is valid for the other parts of the study.

Lines 226-227: *'While it is difficult to diagnose the cause of the difference in posterior estimates, one possibility is the larger prior uncertainty used in Zhang et al. (2020)'*. Yes, but not only the prior information can explain this. In fact, the inverse modelling system, as it is built, with a good observational coverage it may be possible to infer the fluxes from the surface whatever the prior information. Hence, here both prior information and observations (here about 50% of the data are used and maybe no information in some pivotal areas) are meaningful to explain the deficiencies.

3.3.4 Point source emissions- …. Bracket after Australia to be deleted

3.5. CH4 emissions during the COVID-19 period

Page 390: '*These 390 reduced emissions were likely caused by large scale droughts which occurred in early 2020 (Marengo et al., 2021'*. The droughts events may have decreased the water table?

- **Conclusion**

Last paragraph: lines 459-464

Please adapt the text to the CH4 inverse modelling system. Here, you may mention only the new developments of the CO2 inverse modelling system that are relevant for the CH4 inversion system

Tables

OK

Figures

Figure 2: C). The title of y-axis should be '% Change in Emissions 100.*(Posterior-Prior)/Prior

References

Overall fine, only at page 16 move Courtier et al. 1994 after Cheewaphongphan et al. 2019

Supplement

Figure 1: Quality flags. Please specify (>50% or larger ?)

Please also note the supplement to this comment:
https://acp.copernicus.org/preprints/acp-2021-1056/acp-2021-1056-RC2-supplement.pdf