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

The authors would like to thank both reviewers for taking the time to go into the details of the manuscript and believe the comments provide constructive criticisms which have improved the quality of the manuscript. Please find below a point-by-point response to each review comment to accompany the updated and track change manuscript.

Review 1

Overview:

The manuscript "Quantification of methane emissions from hotspots and during COVID-19 using a global atmospheric inversion" by McNorton et al. describes the results of a high-resolution atmospheric inversion of methane (CH4) emissions during 2019 and 2020. There is focus of many individual case studies of various scales, and investigation into the effect of the COVID-19 pandemic on global and regional emissions of CH4.

Overall the manuscript is fairly well written, although there are some technical corrections necessary. The figures are clear and appropriate. The model simulations carried out for this work appear to produce useful and interesting results, and future improvements to the system will further refine such outputs. In the main text, details are often obscure and some sections need to be rewritten with more clarity. My main issue is that assertions are sometimes made without sufficient evidence to back them up, and I don't agree that the authors have clearly demonstrated that one of their main results is sufficiently robust.

If the revisions detailed below are sufficiently addressed, in particular those regarding the conclusions that the authors make regarding the effect of the global pandemic on methane emissions, I am happy for this manuscript to be published in ACP.

We thank the reviewer for their comments and agree with the issues raised. We have attempted to improve the clarity of some sections. We have attempted to include additional evidence or remove previous claims where evidence was lacking. The details are described below.

Abstract and throughout: Many emission values are given as annual totals (e.g. 'CH4 emissions for 2020 were 5.7 Tg yr-1 (+1.6%) higher than for 2019) but I think that the differences in these totals must be based only on the first six months of each year, as the inversions do not cover the full years. This is misleading and should made clear throughout.

We agree and whilst it is confusing to present emissions as $6mo^{-1}$ we agree that the results are misleading by providing values as annual totals. We have updated the text where appropriate stressing that these are emissions for the first half of the year for both 2020 and 2019. We hope this adds more clarity and transparency to the results.

Line 16 - Without context the phrase 'basin-wide' is confusing. A gas basin? River basin? Wash basin?

Reference to basin has been removed, now it just refers to regional and point specific.

Line 22 and later on: Your assertion that the large atmospheric growth rate in 2020 would have occurred with or without the pandemic slowdown can not be supported for numerous reasons. The reasoning for this statement is not properly explained anywhere. As far as I can tell, it

We agree the conclusions reached may have been lacking in explanation. The main motivation for comments on the impact of the slowdown come from a relative comparison of January/February for both 2020 and 2019 alongside a comparison with the remaining 4 months. From this the results indicate larger 2020 emissions than 2019 both pre-slowdown and during. Given the relative change for both time periods is similar we conclude the slowdown impact is small, however as mentioned we may be neglecting other influences and have added this caveat to the text (2nd point) and as discussed below, for China, February might not be a suitable 'business-as-usual' month.

In response to the 1st point, as previously mentioned we have updated the text to explain better the results only represent half a year and not a full annual growth nor do they extend beyond 12 months into the continued 2021 slowdown.

In response to the 3rd point. According to the COVID-19 stringency index, cited within the manuscript, the slowdown period in China begun on 23rd January, so we agree that taking February emissions as business as usual may not be appropriate for China. However, for the rest of the world measures were typically introduced in March. The relative differences in derived emissions for January 2019 and 2020 when compared with February are very similar, further suggesting that the impact of the slowdown was small (assuming other factors did not contribute). The caveat to our approach is then, assuming other factors beyond the slowdown did not have significant impact on emissions, we conclude the continuation of emission increases were not significantly impacted by the slowdown either nationally for China or globally. We have included this in the text reiterating the point of the reviewer that direct comparisons are challenging and attributing changes to the slowdown alone neglects other factors.

The assumption is not that Jan/Feb is equivalent to the expected value for the rest of the year, but rather the relative change from 2019 to 2020 for each month is expected to be the same or similar in a business-as-usual scenario, which is what we find. We have added several references in the text to the assumptions made in drawing this conclusion.

Line 24: 'small' in what sense? Emissions were higher in 2020 than in 2019 in each of these months. How are you quantifying the effect of the slowdown?

Whilst emissions were found to be higher in 2020 than 2019 the relative change in the pre-slowdown months (January and February (exc. China)) is similar to the change during slowdown months, hence why we conclude the impact is small. This is the case both globally and for most major emitting regions (see figure 11). We agree this is not well described in the text and have added this into the main manuscript.

Line 25: Generally, descriptions of future work do not belong in an abstract.

Thanks, this has been removed.

Line 41: Is there any uncertainty included in the value of 14.7 ppb?

We have now included the reported uncertainty.

Line 43: Does this statement about venting/flaring conflict with the previous statement that oil and gas emissions reduced by 10% in 2020 (IEA)? It seems to, as written.

It does conflict with the other findings and we have included this now with "An alternative hypothesis is that reduced demand could have…". This is a hypothesis given here and not supported by other studies.

Line 46: I think that Weber et al. seem to suggest that the effect of changes to OH in 2020 on CH4 have an upper bound of approximately 2 ppb on the observed growth rate. Since the difference in growth rates between 2020 and 2019 is approximately 4.7 ppb yr-1, the OH effect is maybe not so small?

We agree and have updated the text to state Weber et al. show changes in OH alone could not account for the observed growth, although the importance of the OH trend is discussed in detail later in the manuscript and we recommend it is explored further.

Line 47: The first part of this sentence is confusing. Do you mean that we have accurate measurements, or that theoretically, given accurate measurements, inverse modelling is possible? It should be rewritten.

We agree this is confusing and have rewritten to explain that assuming we have accurate measurements then inverse modelling is possible.

Line 54: State the start date that SCIAMACHY data is available from, as you have for GOSAT.

Now added.

Line 56 - 58: IASI measurements have been used in the inversion, and should therefore also be mentioned here.

Now added.

Line 69: 'greater observability' - briefly explain why?

This has been slightly rephrased to better explain what is meant.

Introduction: The results of Forster et al. (2020) should be referenced somewhere.

We agree and this has now been added.

Line 88: What is the justification for simulating a longer period during 2020 than in 2019? Is this taken into account when comparing e.g. the global annual total fluxes in the two years later on? (2019 posterior fluxes will have reverted to the prior for July - September whereas 2020 posterior fluxes will not have done so).

For global comparisons between 2020 and 2019 we only use this first 6 months as described in the relevant sections. The additional months for 2020 were only used for the case studies.

Line 117: Between each 24-hour window, the initial 3D mixing ratios are included in the state vector and therefore total mass of CH4 is not conserved in the model. This is a justifiable consequence of the 4D-Var method with these short windows, but do you expect that it would affect your posterior flux estimations to a significant extent? If the system can 'reset' the mixing ratios to some extent every day, is it possible that some model-observation mismatch that are in reality due to emission changes can 'go missing' in the initial mixing ratios? How large were the prior uncertainties applied to the 3D grid and were error covariances included in this? This should be briefly discussed in the manuscript.

We agree this is a limitation and the total mass is not conserved as a result. For observations made nearby sources we would expect this impact to be small; however, on a larger spatial scale the impact could be considerable. Further investigation into the impact will be done in future work which will consider a long-window 4D-Var (e.g., several weeks or months) that improves mass conservation. The prior uncertainties in the 3D-state were temporally fixed but spatially varying based on an ensemble of forecasts, which is used in the CAMS operational system. A comment on this has been added to the text. We have also included a new figure to the supplementary material showing the prior uncertainty in the 3D-state.

Line 149: It would be good to have a map of the applied observation uncertainties also included in the supplementary material if possible.

We agree that quantification of these observation uncertainties was lacking and have instead added a value to the text.

Line 149: Was the satellite data filtered in any way before use?

Yes, the satellite quality flag is used in the assimilation as described in the Massart paper referenced. We have included a comment on quality flags.

Line 150: Whilst I acknowledge that it might have been too much detail for this manuscript, it would generally be good to quantify the impact of the TROPOMI observations in the inversion over just using the IASI and GOSAT observations. Would the major conclusions about COVID-19, for example, have been any different without the TROPOMI data?

We agree this is an interesting area to explore and was recently discussed by Qu et al. 2021 (Global distribution of methane emissions: a comparative inverse analysis of observations from the TROPOMI and GOSAT satellite instruments). In their study, despite TROPOMI having ~100 times more observations, it is found to have less impact than GOSAT. However, their system operated at a much coarser resolution so a direct comparison may not be appropriate. The question of how much TROPOMI data informed our results is indeed interesting, follow-

up studies will investigate this further by removing sensors from inversions to test the sensitivity.

Line 170: How different were these values from those in the control simulation? Is the improvement from optimising the emissions significant relative to the observation uncertainty? Is the model performance degraded at any TCCON sites by optimising emissions?

A subsection of these comparisons are shown in the supplementary material, although not stated in the text the standard error in the control is 7.1 ppb, relative to 6.7 ppb in the inversion. The significance of this is difficult to quantify; however, given systematic TCCON uncertainty most simulations fall within this uncertainty range. The model performance is degraded when using the mapped prior for 7/21 sites; however there is always at least one prior error which outperforms the control. We therefore conclude that whilst the inversion improves the comparison, the prescribed prior error must be reasonable.

Line 179 - 182: OK, I get that you want to only analyse properly-constrained grid cells. But does using this method have any impact on what you are quantifying? Are emission totals for 2019 and 2020 directly comparable, or do they have different spatial representations? Are regional and global totals in this manuscript comparable to other studies?

For case studies we include "observable" days for transparency. We find that, as expected, days without observations have posterior values close to the prior. For comparison of 2019 and 2020 the spatial coverage of observations is typically similar for both years. Within a one-month period, in which the posterior estimates are averaged, the entire globe is well observed. Assuming there is not large variability within each month, the two years should be comparable. The regional and global totals are comparable to other studies and typically only show a small but important deviation from the bottom-up CAMS inventory. We have stated similarities with other studies e.g. Deng et al., 2021.

Line 184: Similarly - are posterior estimates for only the first six months of each year included here? Or do the two years have posterior totals included for different numbers of months? If only limited numbers of months are included for each year, what exactly does the value of 528.2 Tg yr-1 in 2019 represent and is it really accurate to say that emissions in 2019 were 4.7 Tg yr-1 smaller than in the prior? It is important to be clear with your language here.

We agree, our original statement was confusing and, as mentioned above, these comparisons are for just the first 6 months of each year and we have improved our description in the text.

Line 190: This figure suggests that the majority of countries' total anthropogenic emissions are quantified to within 1% by the prior emission inventory. Is this really likely, or is it a result of strict prior uncertainties applied to these countries in the inversion?

For the regions shown (the 8 major emitting regions), indeed 4 do fall within 1% of the prior estimate when averaged over a 6-month period. It is feasible that the total prior over this period is reasonably accurate, although, perhaps more likely the prior uncertainty at a daily timescale should indeed be inflated beyond the values used (as discussed for several of the case studies). Without sufficient prior information this is indeed a problem for inversion studies and there is on-going work in the COCO-2 project to further develop these prior uncertainties.

Line 192: What does 'other anthropogenic' cover? It should be noted somewhere, although not necessarily in this line.

We agree and have now added this into the text.

Updated

This is incorrectly referenced in the text and should refer to the province of Inner Mongolia, the text has been updated.

We agree that this is not such a small trend and the statistical significance should be remarked on. We have now added this. The units are also updated.

The sampled region is the same, we have intentionally selected the same domain.

Additional comparison has now been included with the Lyon et al. study. With some similarities and differences highlighted.

Yes, this is specific to this region.

We agree this is confusing and have re-worded it to explain that the data is variable but has positive growth. This is essentially a trend over a 1.5 year period Jan 2019-Jun 2020, although we understand this is unclear in the way it is currently worded. We believe the analysis of a trend period is dependent on the contributing factors and here, given the rapid expansion of activities it is reasonable to infer a trend. We have modified the text for a clearer explanation.

Like our study, Schneising et al. range in values are based on the variability between days/observations and not just the uncertainty in the observations/methodology. We do agree

that these lead to a large range of values, but it is difficult to deduce what fraction of this is flux uncertainty and what is caused by emission variability.

This is incident ID 48996, Break, Leak or Malfunction of Equipment. They quote 5,000m^3 of gas released, making it one of the larger incidents in the report. Our estimates are, however, noticeably larger than those reported. We didn't want to attribute this event with the estimated emission increases but merely consider it a possibility.

We agree this section is lacking quantative comparisons with the other study. However, given the period is different a direct comparison of values might not be sensible, instead the emphasis is on the prior inventories used generally being low. The intention of this section is to show the possibility to quantify natural emissions even given the caveats; however, despite this being a heavily caveated result we still wanted to include it to show the possibilities of the system.

This is mainly caused by the Illizi being a larger domain, the observable days which are available should still be enough to draw the conclusion that the Ilizi Basin is a larger CH_4 source.

We agree this is notable, the reason for this is unknown and it would be more difficult to draw conclusions on missing sources than the larger increase seen in 2020. Also, the 2020 increase seems to be more sustained.

L223 states the uncertainties represent the daily variability not the posterior uncertainty. We understand this is unclear and wanted to add it at the start of the analysis for clarity. We have updated the text to highlight this.

We agree this message is slightly confusing and have reword several sections. Importantly, the comparison is made with 2019 and not with January and February of 2020. Each month is compared with the equivalent month of the previous year to remove the seasonality. The hypothesis is a relative change for a given month in 2019 and 2020 should be similar across all months unless the slowdown acted to alter emissions. Given the difference between 2019 and

2020 is similar for each individual month, we therefore conclude the impact is small. There are several assumptions made here though and we have tried to clarify this in the text.

Line 425: How do you know that it compared well?

The bias alone in the prior is consistent, so whilst not fully quantitative the prior (which is similar) is still adjusted in the same direction.

Lines 452, 453 and 455: There is no evidence provided that the slowdown was the cause of reduced growth in emissions in May/June. Similarly, it can't be said that the overall impact of the slowdown was small as there is no counterfactual. Finally, if changes in the sink played a significant role, then it's even less possible to say with such certainty what impact the slowdowns had on methane emissions - perhaps emissions were in fact lower during March/April than in Jan/Feb but this could not be captured in the model.

We agree that it is not conclusive that the further increase in emissions derived in Mar/Apr relative to the increase seen in Jan/Feb is attributed to the slowdown, however, this does seem the most plausible explanation. The same is true of the relative decrease in the longer-term increase seen for May/Jun. We believe the slowdown is the most likely explanation for this; however, as this is not conclusive, we have included this uncertainty in the updated manuscript. Assuming no other change, there is no clear shift in emissions over the entire 4-month period (Mar-Jun), we believe this is a reasonable assumption excluding the possible influence of OH changes.

Line 457: Has there been any research using bottom-up methods to compare to your results for emissions during the global slowdown? (E.G. the IEA data referenced in the introduction).

The IEA report a 10% reduction in emissions, however this is not seen here. To our knowledge there are no reliable mapped bottom-up estimates, but studies using simple assumptions about proxy data, e.g. Forster et al. (2020) estimate a slight reduction in emissions during the early stages of the slowdown. These have been included in the text. Doumbia et al. 2021 also provide an updated emission dataset; however, this does not include CH4.

Line 466: It would be much more beneficial to the scientific community if data were put in a public repository.

We agree; however, given the large size of the model files it is not feasible to store them in a public repository, all results are hosted on ECMWF MARS archive and available to those with access, we are happy to retrieve specific data from the archive for the wider-scientific community who cannot access MARS.

Figure 2C: does the x-axis here show the prior or posterior annual emissions? And is it the actual annual emissions, or the first six months' emissions scaled to Tg yr-1?

We agree this is unclear and have updated the caption, it is prior emissions and the emissions are based on the first 6 months alone, the flux is now given in units of months.

Figure 3 onwards: it might be beneficial to show prior uncertainty in these figures (with shading/dashed lines) if it does not affect clarity too much.

We agree it would be helpful; we have updated figures 3-10 with prior uncertainties

We agree and for several of these plots we have now added a zoomed out map of the region inset to the plots.

Whilst pie charts can be an effective way to visualise the components of a whole we have acted on the reviewers opinion and removed the pie chart and replaced it with a regional map.

All technical corrections were applied to the manuscript and once again we thank the reviewer for their detailed feedback.

Review 2

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.

This is a good point, and specifically true for studies which use observations from different sources as many of those used in the text do. We have included this additional source of uncertainty to comparisons in the text.

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.

We agree and this was also mentioned by the other reviewer. We only ran for a limited number of months due to the computational cost of the system. We agree providing the results as an annual emission as a result is misleading and have updated this in the text. Furthermore, as mentioned it makes comparisons with other studies slightly less consistent and this also now noted in the text.

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.

We agree that given the short-window used here high temporal resolution in-situ observations with low errors would improve results for specific hotspots nearby those available observations. On a global scale we expect this impact to be small; however, if further developments to the system were to focus on specific hotspots with in-situ observations then those would be included in any future work.

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

We agree and have updated the text with your suggestions.

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.

We agree and have tried to add clarity that the values given represent the flux over a 6 month period and not an annual flux. For ease of reading we have kept the values as CH4 per year but have tried to emphasise the point that these are the fluxes for 6 months not a full year. We hope this is now clear enough.

1. **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.

We discuss these changes in more detail further down in the text but agree here we can also mention more specifically OH changes, this has now been included.

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

Updated thank you.

2. **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

We have now included the justification being computational cost.

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

This is a good point, ideally we would have used the uncertainty and prior from the same source however the operational CAMS system has been well evaluated using the LPJ fluxes but an ensemble of fluxes is not available from the system. This is indeed a slight inconsistency with our assumption and we have now noted this in the text.

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?

Thank you, this has now been added.

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

3. **Results**

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

Updated, thank you.

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.

This has now been updated.

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.

We agree that reporting annual emissions was unclear and have adjusted this to report only monthly emissions. Given the reported values are for monthly averages, and assuming little variability within a month, we assume a subset of dates where observations are available within a month to be representative of the entire month. We agree this might introduce additional uncertainties and this is now updated earlier in the text. We have checked this assumption and whilst it does not always hold true, it is typically robust and the variability within a month is small.

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.

We accept this may introduce uncertainty and have updated the text accordingly.

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.

We agree and have added this in along with other possibilities regarding the period of study.

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

Updated.

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?

Yes, we think this might be the case

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

This has now been updated, thank you.

Tables

OK

Figures

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

Updated.

References

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

Updated.

Supplement

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

Updated