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 (CH_4) 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 CH_4.

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.

Comments:

Abstract and throughout: Many emission values are given as annual totals (e.g. ‘CH_4 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.

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

Line 22 and later on: Your assertion that the 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 seems to be based on the fact that the global emission growth in May-June 2020 over 2019 is smaller than the growth in the pre-slowdown period in January-February 2020, and acts to cancel out the ‘extra’ emissions in March - April.

My issues with this logic are as follows. First, only the first half of the year 2020 has been modelled in this work, so no definitive conclusions about the whole year's growth rate can be made. Second, without carrying an inversion for a counterfactual world in which there was no pandemic (which is obviously not possible), you can't say what would have happened to emissions during summer 2020. It is possible that they would have been equal to, or lower than, those in 2019 and the global slowdown was in fact still acting to increase emissions during this time. You cannot therefore allocate any change in emission growth during this time to only the global slowdown. Third, much of China had lockdowns during January and February 2020, before the global slowdown began in earnest. Many of these were lifted in March and April. This Jan/Feb period therefore does not entirely represent 'business-as-usual' for comparison to later parts of the year. In my opinion these statements need to be more thoroughly examined and explained, or removed from the document.

Line 23: 'below expected pre-slowdown levels'. Again, this statement assumes that the observed emission growth in Jan/Feb is equivalent to an expected value for the rest of the year.

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?

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

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

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.

Line 46: I think that Weber et al. seem to suggest that the effect of changes to OH in 2020 on CH_4 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?

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.

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

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

Line 69: ‘greater observability’ - briefly explain why?

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

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).

Line 117: Between each 24-hour window, the initial 3D mixing ratios are included in the state vector and therefore total mass of CH_4 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.

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

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

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?

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?

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?

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.

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?

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

Line 193: You state 'multiple inverse studies' and then only reference one. Add 'e.g.' or more references.

Line 195: Confused by reference to Outer Mongolia here for two reasons - first, there is not currently a state or country of this name. Do you mean the state of Mongolia? Second, in Figure 2b, there doesn't really seem to be any observable change to CH_4 fluxes in Mongolia.

Line 226: This trend doesn't sound that small to me (about 6% yr^{-1})? Although it might not be significant given the data spread. I'm also not sure that a 300 kt change from the prior to the posterior should be described as considerable but a 150 kt/yr trend be described as small. Finally, I think units of the trend should be in kt yr^{-2}, or preferably Tg yr^{-2}.

Line 227: Different periods are covered in the two studies too. Is the sampled region the same between the studies?

Line 229: The reference from the introduction (Lyon et al., 2020) found reduced emissions from the Permian Basin in April & May 2020. Why not compare to this reference here too?

Line 233: I assume this means oil & gas production specifically in this region has increased?

Line 240: The phrasing here is very confusing: 'Our posterior results for 2019 … show a variable but positive trend'. Do you mean that the trend is variable? Or that the variability of the data is large but has a positive trend? Is the trend statistically significant? Generally, talking of trends when discussing a 12-month period is odd, and even if you're describing a trend between 2019 and 2020 this is still a very short period to derive trends over.

Line 242: The value found by Schneising et al. does have relatively large uncertainty attached, which your prior estimate and posterior estimates for 2019 and 2020 both fall well within.

Line 248: Please clarify somewhere which incident you mean? Incidents on September 4th in this file don't appear to be particularly large compared to those on other days, as far as I can tell.

Lines 251 - 262: I feel that this section concerning Lake Chad needs to be either expanded or removed, as it does not add much to the study in its current state. Comparing to a previous study that does not provide quantitative results for the region in question does not inform the reader of much. Perhaps contact the authors of that study? Meanwhile, the limited number of days with data assimilated does not provide information on the seasonality of local fluxes.

Line 340: and also noted that a greater number of days are included in deriving the emission rate for Illizi than Hassi Messaoud.

Line 351: Very low emission rates in early 2019 also notable?

Section 3.5: What do flux uncertainties in early paragraphs here represent?

Line 364: Again, this only holds if you assume that emission growth would have remained at the Jan/Feb value all year, which is not necessarily true. The slowdown might still have been increasing emissions in May & June relative to a world in which the pandemic did not happen.

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

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.

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).

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

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}?

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.

Figure 3 onwards: it would be helpful if the maps in these figures had an inset panel or similar, showing their location.

Figure 6A: 3D pie charts are a terrible way to display data, and the one here is certainly unnecessary. The 88.9% figure could just be stated in the main text, or a stacked bar chart could be used if you really want to plot this information.

Technical corrections:

Throughout: For some reason superscripts and subscripts have been omitted throughout the manuscript (e.g. those in yr^{-1}, CH_4, CO_2). Whilst not vital at this stage, the text would have been easier to read had they been included.

Throughout: the mixing of units through out the text again makes some of the discussion more difficult to follow. You should not be switching so often between mass units of t and kt along with Tg, particularly as this is often within the same sentence or figure panel.

Throughout: links to section titles etc. in the main text should be capitalised (e.g. Section 3.1.1, Supplementary Figure 1, Table 1).

Line 14: newly available -> newly-available

Line 16: form -> from

Line 30: CO2 -> carbon dioxide (CO_2)

Line 39: remove ‘when’

Line 40: oil producing -> oil-producing

Line 64: remove ‘a first version of the’

Line 64: ECMWF should be defined during first use.

Line 69: fix ‘allows to benefit’

Line 91: 3-hourly

Line 137: delete ‘a’ before ~80 km.

Line 139: should this read something like ‘limited occurrences of co-located emissions from…’?

Line 175: Should be ‘business-as-usual’ as this is adjectival.

Line 181: ‘To this aim’ -> ‘To this end’/‘With this aim in mind’, etc.

Line 201: Previously-documented

Line 202: Barre et al. (2020)

Line 223: Change to ‘The uncertainty value here represents the standard deviation of the daily fluxes and not the posterior uncertainty’.

Line 267: remove unnecessary comma after ‘and’.

Line 271: remove unnecessary commas after ‘derived’ and ‘fugitive-only’.

Line 272: business-as-usual

Line 359: Add + before 0.4.

Line 575: Double spaced.

Figure 6 caption and others: sector-specific

REFERENCE:

Forster, P.M., Forster, H.I., Evans, M.J. *et al.* Current and future global climate impacts resulting from COVID-19. *Nat. Clim. Chang.* **10,** 913–919 (2020). https://doi.org/10.1038/s41558-020-0883-0