General comments

This manuscript uses satellite retrievals of CH_4 from the TANSO instrument onboard GOSAT to optimize CH_4 fluxes globally, but with a focus on South America and, specifically, the Amazon Basin and Brazil. The optimized CH_4 flux for the Amazon Basin shows a positive trend, which is strongest in the wet season, which the authors suggest could mean that the trend is driven by wetland emissions. The authors find that grid-cells with a flux positive trend generally coincide with grid-cells with a positive temperature trend, but there is also some coincidence with grid-cells with decreasing wetland fraction. Overall, the methodology appears sound and the results are well presented. However, there are a few issues that should be addressed before publication.

We thank the reviewer for their comments, which have significantly helped to improve the paper and clarify our results and message. We hope that we have addressed these concerns appropriately. Our point-by-point response is given below, highlighted as blue text.

The main issues are outlined as follows:

The authors emphasize that the positive flux trend in the Amazon Basin is strongest in the wet season and suggest that this is likely a result of wetland emissions. However, the fraction of wetland area in the Amazon has also been decreasing over the same time period. Based on the data presented it is not possible to conclude anything about the cause of the trend, since it could be also due to an increase of biomass burning and/or agricultural emissions, which may be increasing more during the wet season than during the dry season. I think the authors should expand the discussion mentioning other possible causes of the trend and be clearer that with the data presented they cannot draw any conclusion about its cause.

Yes, this is true, and we have rewritten much of our discussion and conclusions in order to make this more clear. We have now stated throughout that we are not able to draw definitive conclusions regarding the cause of the increase and expanded our explanation of the possible causes to include biomass burning and agricultural emissions. We continue to include our analysis using a bottom-up wetland model, but now clarify that this model's poor performance in matching the inversion results could be due to non-wetland sources being responsible for the variations.

Although the inversion was run globally, there is no mention of the global CH_4 budget before and after the inversion. I think it is important to present the values of the global total source a priori and a posteriori, as well as the total calculated atmospheric sink. The global budget is especially relevant when discussing the Amazon emissions and their trend in the context of the global emissions.

We agree that this is important and have now included the *a priori* and *a posteriori* global source values at the start and end of the study period within the main text. The prior values are in Section 2.2.2, whilst the posterior values are in Section 3.2. We have also now included the values of the total atmospheric sinks due to OH (494.5 Tg in 2009) due to O1D and Cl (19.5 Tg in 2009) and due to the soil sink (33.9 Tg in 2009).

Specific comments

L64: The authors could also cite Thompson et al. Geophys. Res. Lett. (2018) who like Worden et al. (2017) found an increase in both microbial and fossil fuel emissions. Thompson et al. (2018) also simultaneously optimized OH but found no significant OH trend.

We now have included this reference.

L150-152: By "the GOSAT averaging kernels were averaged similarly to the XCH4" do the authors mean that the averaging kernels (AKs) of all retrievals falling within a single model time-step and grid-cell were averaged (as was XCH4)? I think this should be specified to avoid any confusion.

Yes, we have now clarified this.

L155-166: The authors state that they compare CH4 mixing ratios, from a previous inversion using only the surface network of flask sampling sites, to GOSAT XCH4. They then fit a quadratic function to the observation-model differences as a function of latitude and add the calculated bias error to their prior modelled XCH4 in the inversion with GOSAT XCH4. I see one problem with this approach. That is, an offset between the optimized XCH4 and GOSAT XCH4 is expected since the information from the surface observations is limited, especially in the tropics (e.g. Fig. 1). Ignoring for the moment any atmospheric transport error, this means that the information from GOSAT (i.e. model-observation difference between the prior modelled XCH4 and GOSAT XCH4) is reduced.

We agree that this method could potentially lead to a reduction in accurate information gained from the GOSAT data, but we believe that it is the best way to simultaneously assimilate the surface observations along with the satellite data. Due to a combination of biases in the model transport and chemistry and the satellite retrievals, there is a persistent offset between the model and satellite even when the surface is well-constrained by flask observations alone. The latitudinal function that we include is an attempt to account for these persistent biases and removes the conflict between the two sets of observations. This method has been previously employed in similar studies (e.g. Bergamaschi et al. (2009)) The fact that the bias function varies only in latitude, and is constant longitudinally and in time, means that information content along these two axes is preserved. We have added a short amount of text to reinforce this message.

Bergamaschi, P., et al. (2009), Inverse modeling of global and regional CH_4 emissions using SCIAMACHY satellite retrievals J. Geophys. Res., 114, D22301, doi:10.1029/2009JD012287

L159: I think the authors should state here that they fitted a quadratic to the model-observation error as a function of latitude.

This is stated a little further down in this paragraph.

L198-191: The authors should specify that INVICAT is an inversion framework which uses the forward and adjoint models of TOMCAT.

Yes, we have now included this information.

L233: For completeness, the authors should state what “scaled as in McNorton et al.” means.

We have added ‘to apply an increasing global linear trend for the period after 2012’.

L235: The authors should state what “in a configuration described in McNorton et al.” means.

We have added ‘using four separate carbon pools to drive methanogenesis’.

L236: The authors should also state what scaling was applied to the rice emission estimates. Furthermore, rice emissions are already included in the anthropogenic emission estimate from EDGAR-v4.2FT2010. Was there a double counting of rice emissions in the prior estimate?

No, we did not include the EDGAR estimate of flux from rice agriculture, so there is no double-counting. We added the information that the scaling applied to the rice emissions was 0.75.

L236-237: The authors should specify what other natural sources were included in their prior emissions estimate.

Done.

L245-247: There are a number of recent studies addressing the possible trend in OH and OH variability related to ENSO (e.g. Zhao et al., Atmos. Chem. Phys. 2020 and Anderson et al., Atmos. Chem. Phys. 2020). The authors should mention the possible ENSO influence here.

We had missed these references and others, along with some discussion of ENSO-driven OH variability, and appreciate that they are important. We have now included this discussion here as suggested.

L252: This sentence is a bit confusing, do the authors mean that the simple bottom-up model only provided a climatological (i.e. with no year to year variability)? Or do they mean that they used meteorological (and other) driving data? Later on (L278-279) it sounds as though it is the latter.

Yes, we have changed this to ‘meteorological and ecological input data’.

L286-287: What do the authors mean by “we consider only the wet season NAT + AGR + BB emissions . . . which we assume to be almost entirely from wetlands”? The BU model described is only for wetland emissions, so I think AGR + BB are in fact ignored?

This was poorly phrased, as we intended to say that we considered the NAT + AGR +BB posterior inversion flux, which we assume to be mainly from wetlands, for comparison with the B-U model output. We have clarified this in the text.

L378: The authors should specify what they mean by “performance”, i.e. mean bias and correlation.

Yes, we have clarified this.

Table 2: In the caption, by “optimal” the authors mean the “better” statistic is in bold. I suggest changing “optimal” to “better” or similar, as “optimal” could be confused with being from the optimization.

Good point, we have changed this as suggested.

L381-382: In fact the posterior correlation is better for observations <1.5 km at all sites except ALF. Only the bias increases (more positive) for all sites, except SAN.

Yes, we have clarified this point.

L457-461: The INVICAT results used to optimize the BU model are the total of the sectors “NAT+AGR+BB”, while the BU model only considers wetland emissions. Therefore, is it realistic to think that the BU model can reproduce the trend or variability seen the INVICAT results, even when only the wet season emissions are considered? In other words, is the assumption that the emissions during the wet season are dominated by wetlands reasonable? Could the positive trend seen in INVICAT “NAT+AGR+BB” which appears to be approximately spatially correlated with a positive temperature trend (and with a negative wetland fraction trend) be driven by biomass burning or agricultural emissions rather than wetland emissions? I think it is not possible to draw any conclusions about which sector is driving the positive trend based on the data presented here.

Please see our earlier response regarding this comment, which was generally applied to the whole discussion.

Fig. 8: I suggest changing the title of (c) to “NAT+AGR+BB flux trend” as in the caption.

Yes, we have changed this panel title.

L458: By “curve fitting program” I think the authors mean a multiple linear regression was used to determine the values of q10, a1 and a2, I think this should be stated more clearly. Also, with only 3 variables, and given that the model is only for wetland emissions, whereas the observation, i.e. the INVICAT result, is for NAT+AGR+BB, it is somehow to be expected that the optimized BU model cannot reproduce the INVICAT result.

We have now clarified the details of the curve-fitting program, and made it clear that if wetlands were not able to reproduce the INVICAT result, then it suggests that non-wetland sources could have been responsible. We have generally changed the previous tone of the text which suggested that wetlands were responsible for observed changes.

L544: Related to the above comments, I don’t think the authors can conclude that wetland emissions are likely driving the positive trend based only on the fact that the trend is strongest during the wet season. I think there needs to be more analysis of the possibility of biomass burning emissions increasing during the wet season, and trends in agricultural emissions.

Yes, see previous comments.

Technical comments

L125: should be “Data from these sites are assimilated. . .”

Done.

L341: I think this should be “. . .emissions in Brazil are nearly constant over time. . .” and not “consistent”

Done.

L588: should be “. . .a period during which there was widespread flooding.”

Done.