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Interactive comment on "Exploring Constraints on a Wetland Methane Emission Ensemble (WetCHARTs) using GOSAT Satellite Observations" *by* Robert J. Parker et al.

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Response to RC2 Review Comments

(Original Comment, Our Response, New Manuscript Text)

Firstly, we would like to express our gratitude to the editor and reviewers for providing a thorough review of our paper. We appreciate their efforts, especially in these difficult times.

[pg 4 Line 27] When using the EDGAR database - how are years beyond the range of the dataset represented (2012)? Has any trend being included to cover 2009-2017 pe-

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Discussion paper

riod? This is also annual dataset so have any seasonal cycles associated with different regions been included?

The EDGARv4.2 database runs up to 2012, and we repeated the 2012 emissions for the remaining years, with no seasonal cycle applied. For the wetland regions in which we're interested, any local seasonal cycle due to anthropogenic flux is likely very small compared to the natural sources, but we now note this possibility in the text.

The EDGARv4.2 database runs up to 2012, and we repeated the 2012 emissions for the remaining years, with no seasonal cycle applied. As we focus primarily over wetland emission areas, the local seasonal cycle due to anthropogenic fluxes is likely very small compared to these natural sources. We do however note the possibility that this effect could be a source of uncertainty.

[pg 4 Line 27] Similarly which data has been used for GFED 4.1s to cover 2009-2017? Has the beta release data been used for the latter years?

We used the GFEDv4.2 emissions for the correct year up and including to 2016, and used a climatology for 2017 and 2018. We did not use the beta release due to availability when this study commenced. This may affect the modelled seasonal cycle in some regions for the latter two years but will not affect any conclusions.

We used the GFEDv4.2 emissions for the correct year up and including to 2016, and used a climatology for 2017 and 2018.

[pg 5 Lines 11-14] Are any corrections made to the GOSAT dataset based on the comparison to the TCCON network? If so, is this a global correction or are any regional variations taken into account?

We will include this statement to clarify this: After performing extensive validation to TCCON, we subtract one global offset from the GOSAT data. This value is typically small. For v7.2 of the data (as used in this study, the value was 7.71 ppb). For our latest data, v9.0, the value is 9.06 ppb (see Parker et al, 2020, under review)

[pg 5 Line 29-31] "This makes the assumption that wetlands dominate the uncertainty in interannual variability of the CH_4 emissions and the remaining CH_4 sources are in comparison far less uncertain" - is this a valid assumption for all regions? Could biomass burning have larger uncertainty in some regions?

This is a good question, and whilst it is generally true that the uncertainty regarding wetland CH_4 flux is much larger than that of biomass burning, it could be the case in some regions that the local biomass burning uncertainty is not insignificant. We have carried out full global inversions of CH_4 flux using this GOSAT data product and this chemical transport model (paper in prep). Whilst it is difficult to separate out the fire emissions from other emissions sectors using this methodology, our findings suggest that flux changes in burning regions in South America and Africa during the burning season generally change relatively little from the prior, compared to nearby wetland regions. However, it is true that in some extreme years (e.g. 2010 drought in S. America), there are more significant changes to the GFED prior derived by the inversion. Although wetland and burning regions are often spatially distinct, this could affect some of our results to a small extent and we have highlighted this possibility in the main text.

It should be noted that there is the potential for our assumptions regarding biomass burning emissions to interfere with our derived wetland seasonal cycle. However, we have carried out full global inversions of CH_4 flux using this GOSAT data product and this chemical transport model (paper in prep) which suggest that this is not a significant issue. Whilst it is difficult to separate out the fire emissions from other emissions sectors using this methodology, our findings suggest that flux changes in burning regions in South America and Africa during the burning season generally change relatively little from the prior, compared to nearby wetland regions. However, it is true that in some extreme years (e.g. 2010 drought in S. America), there are more significant changes to the GFED prior derived by the inversion. Although wetland and burning regions are often spatially distinct, this could affect some of our results to a small extent.

[pg 6 Figure 2] Why is there are 1.00 correlation between ensemble members

1913,..and 2913,.. (and 3913,..)? These different ensemble members represent those where the global scale factor has been altered - so is this expected? It would be worth clarifying this as the different ensemble members look very well correlated overall but this is do only perhaps a bit misleading if this is just due to a scaling factor difference in some cases.

You are correct in that these ensemble members (e.g. 1913, 2913, 3913) do only differ by a scale factor. We will add a statement in the text to highlight this to the reader so that it is clear. It should also be noted that a change to the global scale factor does not necessarily just result in a completely linear change in the atmospheric concentrations (e.g. due to non-linearity in transport, OH sink, etc). We do think, regardless of this, this plot is informative and provides useful information on the inter-relation between ensemble members. It also provides justification for taking representative ensemble members in some of the latter analysis.

It should be noted that the high correlation between certain groups of ensemble members is expected (e.g. for members 1913, 2913, 3913 where the only configuration difference is the global scaling factor).

[pg 7 Figure 3 / pg 9 Line 5-8] Are the defined East and West Amazon (and perhaps the Congo) regions, traversing the equator? How does their regional behaviour relate to the hemispheric differences seen? May be easier to see this if the equator/tropics were plotted in Figure 3.

This is a good point. We will add the equator line on to Fig 3 for clarity. When examining these regions which do (slightly) cross the equator we see no significant North-South gradient on the scale of the region.

[pg 8 Figure 4] What do the ranges here represent for the box and whisker plot? e.g. Is this presenting the mean or the median? Is the box representative of 1 sigma uncertainty or the inter-quartile range or something else? Please just clarify this within the caption (and does this match to Figure 6 and 7?)

We will clarify this in the text. It is the median and the 25th/75th percentiles.

The median and 25th/75th percentiles are indicated.

[pg 10 Figure 5 pg 14 Figure 10] "Colours indicate the different groupings" – which groupings do each of the colours represent? There seems to be different colours for panels 1 and 2 than panel 3 (e.g. pink)? Without additional clarification here this does not make it clear which ensembles show better correlation / lower standard deviation.

We will endeavour to explain this better in the text as we accept this style of figure requires more explanation. As we are holding two parameters fixed and only plotting the data associated to a change in the third parameter, the number of joined data points will depend on how many dimensions that parameter space has. As an example, we have 2 wetland extent maps, so 9 out of the 18 total ensemble members for each. This results in 9 sets of lines for the 3rd panel. In comparison, the other two parameters have 3 variables resulting in 6 ensemble members each and hence 6 sets of lines in the first two panels. The colours of these lines are arbitrary and only really intended to distinguish the lines from each other within a panel. There is no link between colours in different panels and we will make this clear in the text. We considered making this figure black/white but as some lines closely overlap each other, we felt it was useful to be able to see the separation of lines by colouring them.

The line colours on these plots are to indicate the different combinations within each panel. Due to the nature of the plot, there is no link between colours in different panels as they represent different pairs/trios of data.

[Section 8, Case study 2: Congo] The implication here is that the low correlation with temperature dependence in the Congo could be due to the high magnitude. Since globally, and for most other regions, some temperature dependence is seen, would this explain the difference in this region?

This is difficult to give a definitive answer and future studies focused specifically on the

Congo region may help. We will add the following statement in the manuscript:

Various published atmospheric inversions of our CH_4 data that have used WetCHARTs as the prior, all indicate that the Congo emissions are over-estimated by WetCHARTs and reduced when confronted with observations. This highlights the large uncertainty over this region. Once the necessary MsTMIP (or similar) model data becomes available and it is possible to extend the WetCHARTs Full-Ensemble (i.e. all respiration models) to this time period we will revisit this question in a future study.

[Section 9, Case Study 3: Sudd] Would be useful to clarify what the underlying temperature database being used is. Is there any reason why this would be misrepresenting the temperature variations in this region?

The data used to drive WetCHARTs is as described as in Bloom et al., 2017 (https://doi.org/10.5194/gmd-10-2141-2017). The temperature is ERA-Interim skin temperature. Again, once a more complete Full-Ensemble of WetCHARTs is available, it should allow a future study to further disentangle the effects of these different parameters.

ERA-Interim skin temperature is used as the underlying temperature driving data. One limitation of skin temperature is the assumption that heterotrophic respiration is sensitive to top of soil temperature. We advocate for an expansion of the WetCHARTs ensemble to include subsurface soil temperature estimates - in place of surface skin temperatures - to explicitly represent the representation uncertainty associated with the soil temperature dependency of methanogenesis.

Technical corrections:

[pg 9 Line 14] Commas should be added around "however" –> This, however, is not the case

Noted and modified in the text.

[pg 14 Figure 10] "ror" –> "for"

Noted and modified in the text.

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