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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Large and increasing methane emissions from Eastern Amazonia derived from satellite data, 2010–2018" *by* Chris Wilson et al.

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Review of the article: "Large and increasing methane emissions from Eastern Amazonia derived from satellite data, 2010–2018" by Wilson et al.

The manuscripts presents estimates of methane emissions, focused on Brazil and the Amazon Basin, using a top-down inversion approach, which is validated against independent measurements, and compared to a bottom up model. The manuscript is thorough and detailed, which will undoubtedly be informative for future studies. The conclusions are supported by the findings of the study, which are suitably appraised. As such, I hope to see its eventual publication in ACP. There is however, a major error

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in the methodology – as it is stated in the paper – which may or may not have a large impact on the results. This must be addressed before the manuscript can be considered acceptable for publication. In addition, below are a number of suggestions for revisions to improve the manuscript, followed by technical comments.

1. Line 150-151: "For accurate comparison between the retrieved XCH4 and those simulated by the model, the GOSAT averaging kernels were averaged similarly to the XCH4 and applied to the model vertical profiles." This approach is not mathematically sound and therefore, as an absolute minimum, it must be checked that it has a negligible effect. Otherwise it could lead, quite likely, to an underestimation of the modelled XCH4 and thus overestimate emissions. The reason for the error here is as follows. If we simplify the maths to just two variables, and let X be the concentration at each of the i the model levels, and A be the diagonal of the averaging kernel, and Y be the XCH4, then we can say that $Y = \sum_i a_i x_i$. If we take the mean of each observation j of the total n observations in a grid cell then we want $Y_{grid} = \frac{1}{n}\sum_j Y_j$, which is equal to $\frac{1}{n}\sum_j \sum_i a_{ij} x_{ij}$. As there is only one X per grid cell in the model, we can simplify this to $\frac{1}{n}\sum_j \sum_i a_{ij} x_i^{grid}$. From the text, it states that instead the average of the averaging kernel was applied to the modelled profile, so $Y_{grid} = \sum_i \frac{1}{n}(\sum_j a_{ij}) x_i^{grid} = \frac{1}{n}\sum_i x_i^{grid} \sum_j a_{ij}$, which is not equivalent. Please let me know if this needs more clarification.

2. Line 197: Although each has completed 40 iterations, how do you check that the optimisation routine has converged within 40 iterations?

3. The paper is thorough and detailed, although – as a matter of opinion – it is in places quite arduous to read. I suggest making use of supplementary material and moving some of the analysis here. For example, at line 533: This section should be moved to the supplement, and referred to in line 531. The application of the B-U model as a whole and its discussion would be better placed in the supplement to improve the conciseness of the manuscript. This could also apply

to the validation against the site and aircraft data – refer to the outcomes in the main text but the details can be moved to a supplement. This should get across the key points of the paper, i.e. the emissions and their sources, better to the reader.

4. Code and data availability: It would be much more beneficial to the community if the results of this work (posterior emissions estimates in space and time) were publicly available. E.g. placing the spatial maps for the mean posterior emissions for Brazil and the posterior emissions estimates for Brazil each year in a public repository in e.g. netcdf format. I see that this has also been suggested by the topical editor, but I would like to reiterate its importance.

Technical comments:

Throughout: Units for ACP should be expressed in exponential form, i.e. Tg yr^{-1} and not Tg/yr.

Throughout: Be consistent with Tg(CH4)/yr and Tg/yr (e.g. line 81). I would recommend stating explicitly in its first occurrence that Tg/yr refers to Tg(CH4) and from thereon just writing Tg/yr.

Throughout: There is an error in the nomenclature used throughout the manuscript. The manuscript often refers to the prior/posterior mole fraction when referring to a single parametric estimate; the prior/posterior are distributions of values. A better usage is a priori and a posteriori, or more explicit, the prior/posterior mean.

Line 74: Basin is capitalised (not consistent with earlier use of basin).

Line 75: This sentence is confusing: it reads as though fires contribute to the number of wetland sources. Consider splitting this sentence as e.g. “as well as a number of other wetland sources in S America, emissions from. . . .also contribute to methane emissions.”

Line 77: Overlap in which sense? The emissions processes? In space? In stochastic error?

Line 79: Consider “variability” rather than “variance”, so as not to confuse with statistical variance.

Line 79: ‘Earlier estimates’ is inexact. Is this when the research was carried out, or the emissions from the year(s) in question? Specify the time period that you are discussing.

Line 142: ‘. . .and found that the two agreed within their respective errors’. This sentence is meaningless without a description of the probability content in which these dataset agree (e.g. the 1 s.d. uncertain regions for both datasets overlap).

Eq(1): Why are the numbers in bold, as well as the brackets? Please remove.

Line 170: Specify that it is the inset of Figure 1.

Line 171: ‘until 2014’ should not be in parenthesis.

Line 172: Until when? Present?

Line 178: Space needed between 500 and m

Line 190: It is worth mentioning the species here, considering it is only 4 citations.

Line 194: Are these 5.6 degree square grids? Horizontal is vague.

Line 196: Use the latex command `\citep[ERA-I][]{dee_reference}` here.

Line 215: They are given 250

Line 222: What was the functional family of the spatial correlation imposed? The most defensible choice here is a Matérn covariance structure (see Stein 2012, Interpolation of Spatial Data: Some Theory for Kriging), although it seems that this is not the case here.

Line 223: Again, probability content of this uncertainty needs defining.

Line 226: L-BFGS is a general minimisation routine, not a method to explicitly derive a covariance matrix in the context of uncertainty. This needs rewording.

Line 228: The 'cost function' is introduced for the first time here, and needs expanding upon for readers unfamiliar with the method.

Line 229: This should be the other way round – the lack of off-diagonals would give smaller emissions uncertainty than expected when including off-diagonals i.e. $var(\sum_i(x_i)) = \sum_i(var(x_i)) + \sum_{i \neq j} cov(x_i, x_j)$.

Line 237: Again, use `\citep[MeMo][]{ref}`.

Eq 2.: Brackets are bold, when they shouldn't be.

(Paragraph starting 274: This paragraph reads well and is explicit. Ideally much more of the paper should read like this.)

Line 301: 'The posterior error-weighted mean residual model-satellite mismatch' needs defining. It's unclear what this is.

Figure 2: Consider earlier comment about the use of the terms prior/posterior. Note that the red-blue colour bar is not colour blind friendly.

Line 310: Is this the mean of the prior/posterior means?

Line 318: This should say the spatial distribution of the posterior mean.

Line 345: What correlation do you assume when you make the assumption that they are highly correlated between years?

Figure 5: These colours are very difficult to differentiate. I suggest revising the shadings/colours in the figures.

Line 458: The curve fitting programme (note 'programme' if using British English) needs elaborating on. What is the programme? What curve does it fit?

Line 462: If talking about 'no significant trend', the (statistical) significance of this trend

must be given.

Line 474: Does ‘here’ refer to Section 4? If so, this should be stated as such: “Section 4 also shows. . .”

Line 551: Use `\citep[e.g.][]{ref}`

Line 585: An extra closing bracket is present.

Line 586: Consider ‘substantial’ instead of significant.

Line 598: Which other models besides Bloom et al.?

Appendices Figures: Consider making these colour blind friendly.

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