

Interactive comment on “A global wetland methane emissions and uncertainty dataset for atmospheric chemical transport models” by A. Anthony Bloom et al.

A. Anthony Bloom et al.

abloom@jpl.nasa.gov

Received and published: 28 February 2017

We thank the reviewers for their constructive feedback and suggested corrections. Below we have addressed each individual comment from reviewers 1 and 2, as well as comments by executive editor Astrid Kerkweg (reviewer and editor comments are shown in italics; our responses to the reviewer comments are shown in bold). We believe that the following revisions have substantially improved the overall quality of our manuscript.

Referee 1

The authors aim at providing a dataset for methane emissions by wetlands which

Printer-friendly version

Discussion paper



includes not only estimates of fluxes by biogeochemical models (the bottom-up approach) but also information on error covariance patterns. This information may be useful for performing inversions of methane fluxes through atmospheric data assimilation (i.e. for the top-down approach).

General comments

To my knowledge, this is the first time it is explicitly attempted to provide information on the uncertainty patterns together with bottom-up estimates of methane fluxes. As I am working with atmospheric data assimilation, I think the method and results of this study are very interesting. I have nevertheless two main remarks:

(1.1) the 6-member ensemble is too small to allow for statistics, so I would recommend only mentioning that “more classical” (i.e without the uncertainty patterns) methane flux estimates are available in EE for those who need a long period of time - and simplifying the text and figures accordingly in the Results and Discussion Sections

We acknowledge that relative to FE, the EE ensemble size may limit statistical representations of wetland CH₄ model uncertainty. We now elaborate on this point in the discussion section of the revised manuscript: “ due to the smaller ensemble size and the use of only one carbon model (see Table 1), the 2001-2015 EE emission variability should be interpreted with caution, and - where possible - evaluated against the FE ensemble during the 2009-2010 period”.

However, we have chosen to keep the EE evaluations, as these may be beneficial for the users of EE. We also note that - in response to comment 1.8 - we have expanded the EE ensemble (EE ensemble size = 18) to explicitly represent the uncertainty of the global wetland CH₄ source. Finally, at the request of the second reviewer, we have now included an evaluation of the EE emission inter-annual variability in the revised manuscript. Given these changes, we are confident that the inclusion of EE evaluations is fitting for the main body of the manuscript.

(1.2) The figures are potentially very nice and informative but presently difficult to read, even with a large zoom on a screen (see below for more specific comments on each).

We have addressed the reviewer's comments relating to figures below; where appropriate, we have also increased font and panel sizes to improve figure legibility.

Specific Comments

General

(1.3) The means of the ensembles are used. Why not use the median?

For gridded and zonal emission estimates (Figures 1-3 and GEOS-Chem simulations), we chose to report the mean values in order to maintain consistency with the prescribed global wetland CH₄ source (166 Tg CH₄ yr⁻¹ during 2009-2010; see comment 1.4). We found that gridded and zonal FE and EE median values amount to substantially less than 166 CH₄ yr⁻¹; in contrast, mean FE and EE emissions amount to exactly 166 CH₄ yr⁻¹ during 2009-2010.

(1.4) See if it is possible to update your Kirschke et al. (2013) reference with Saunois et al. (2016) (available at <http://www.earth-syst-sci-data-discuss.net/essd-2016-25/>) in the whole text. In Section 2.1, p.7, l.17-18: would propagating the new smaller uncertainty for global mean wetland methane emissions (i) take much time and (ii) significantly change the results?

As suggested by the reviewer, we have now updated the global mean wetland CH₄ emission estimate and the associated uncertainty (166 Tg/yr +/- 25% or 124.5 - 207.5 Tg/yr), which spans the Saunois et al., (2016) mean (166 Tg/yr) and range (125 - 204 Tg/yr) of 2000-2009 wetland CH₄ emission estimates. We have updated the text, figures and the GEOS-Chem model simulations accordingly. We did not find any substantial changes in the results or evaluation of our dataset.

Section 1 Introduction

Printer-friendly version

Discussion paper



(1.5) p.3, l.20-21: uncertainties are often formulated using correlation lengths in space (e.g. at the global scale, 500 km on land) and sometimes also in time (e.g. still at the global scale, one or two months) over a percentage of the prior emissions. This is especially done to take into account large patterns in the errors due to underlying controls as is the case with wetland emissions. Please check your references here and adapt the text. This does not change the fact that correlation lengths are always an issue because the value at which they are set is derived from expert-knowledge, which is mainly valid at the global scale.

We acknowledge our oversight: we omitted to mention the use of prior spatial and temporal correlations on total CH₄ emissions, such as those used in the Bousquet et al., (2011) and Pison et al., (2013) studies. We have rephrased the sentence to better summarize the use of prior error covariances among inversion efforts: “typically CH₄ inversions do not explicitly formulate wetland CH₄ emission uncertainty correlations: rather, prior wetland CH₄ uncertainty correlations are either absent or implicitly prescribed through space-time correlation lengths on total CH₄ emissions.”

(1.6) p.4, l.8-11: I don't understand here what is meant by the “further constrained” ensemble. Do you mean that the top-down approach could retrieve the controls of biogeochemical processes instead of fluxes from atmospheric data assimilation? It seems it is what is meant in Section 4.2, p.15, l.6-8.

We have now re-worded this sentence: “Top-down CH₄ emission estimates can then be used to quantify (a) the probability of individual ensemble members; and (b) the combined probability distribution of carbon models, CH₄:C temperature dependencies and wetland extent scenarios.”

Section 2.1

(1.7) p.5, l.20: using the word “ensemble” for a six-member sub-set and deriving statistics over such a small number of members does not seem very appropriate. See if it

[Printer-friendly version](#)[Discussion paper](#)

is possible to leave EE aside for most of the paper and only mention it as a “more classical” set of estimates (see also General comments).

We have revised our manuscript text and figures to better convey the limitations of the extended ensemble (see response to comment 1.1). We also note that the EE ensemble is now comprised of 18 members (see response to comment 1.8).

(1.8) p.7, l.18-19: why 1000 perturbations?

In response to the reviewer’s question, we investigated the impact of the number of perturbations, and found no substantial impact on reported FE percentile intervals and error correlation values. In light of this, we now use a simpler approach to explicitly represent global wetland source uncertainty: we expand both FE and EE emission ensembles by deriving three scaling factors corresponding to global annual wetland emissions of 124.5, 166 and 207.5 Tg/yr; these span the range of the Saunois et al., 2016 emission estimates. The number of ensemble members for FE and EE are now 324 and 18. We have amended Table 1 and the methods section accordingly, and we have removed the description and calculation of FE_{exp} , since it is now obsolete.

Section 2.2

(1.9) p.9, l.4-6: I understand the idea of keeping mostly sites where the vertical mixing in the model is not too much of an issue but using only the altitude (a.s.l.?) of the site seems to be too simple. Could you detail a bit more?

Our statement was erroneous, as we did not actually use an altitude threshold (the text was a remnant of a previous analysis); we have corrected the sentence accordingly.

Section 3

(1.10) see if it is possible to leave EE aside (see above and General comments)

See response to comment (1.1).

(1.11) p.10, l.5: could you quantify “considerable”?

We now explicitly state the peak CH₄ emission month for each dataset.

(1.12) p.10, l.19-21: could you explain in more detail why you expect inter-annual variability to be smaller than your uncertainty?

In the revised manuscript, we have revised this sentence to clarify that our expectation is based on previous wetland CH₄ modelling efforts. We also support our statement with a quantitative comparison between the FE and EE uncertainties and the maximum inter-annual variability of the 1993-2004 WETCHIMP models.

(1.13) p.11, l.16-seq.: this paragraph is difficult to read with all the figures embedded in the text. Could you put them in a Figure or Table?

In the revised manuscript, we have split the paragraph into two (as two separate figures are being presented), and we have omitted the 5th - 95th percentile results, as these are depicted graphically in Figure 8.

Section 4.1

(1.14) p.14, l.8-11: more and more atmospheric data of mixing ratios of methane isotopes are available and data assimilation systems try to make use of these and isotopic signatures of the various sources to improve the inversion of methane fluxes. Do you think not only the total methane fluxes but also the isotopic composition could be improved?

We agree that our ensemble estimates can be used to better represent the biogeochemical process uncertainty in isotopic CH₄ studies; however, we have chosen not describe the potential advantages of using our datasets in isotopic CH₄ investigations, as these are beyond the scope of our manuscript. In case the

reviewer is specifically referring to lakes and wetlands: to the best of our knowledge, lake and wetland isotopic CH₄ signatures are not sufficiently distinct to resolve between the two sources.

(1.15) p.14, l.16: *the global uncertainty is always smaller than the smaller scale uncertainties, could you quantify “substantially”?*

We have revised this sentence and now explicitly state that regional scale uncertainties (shown in Figure 4) span a factor of 2 – 148..

Technical Corrections

General

(1.16) check “Kirschke” everywhere (and not “Kirshke”)

Done

(1.17) check all references in the form of “based on the Bloom et al., (2016) methodology”: it looks like there shouldn’t be a comma before the year between parenthesis.

Done

(1.18) “primary” is used for “main” or “dominant” e.g. p.7, l.26 or p.11, l.20, and it seems a bit strange to me, non-native English speaker.

In reference to the “primary uncertainty” estimates, we have now replaced “primary” by “dominant” throughout the revised manuscript.

Section 1 Introduction

(1.19) p.4, l.9: “based top-down CH₄ emission estimates” → *based ON top-down CH₄ emission estimates?*

This sentence has been edited in the revised manuscript

Section 2.1

GMDD

Interactive
comment

Printer-friendly version

Discussion paper



(1.20) p.5, l.10: “heterotrophic respiration at time for a ” → delete “at time”?

Now changed to “heterotrophic respiration per unit area at time t ”

(1.21) p.6, l.18: h should be h ?

h is now written in bold-italics as “ $h_{*,j}$ ” is a vector. We note that in the revised manuscript mathematical notations are now consistent with the Geophysical Model Development journal requirements.

(1.22) p.7, l.4: “freshwater bodies in $w_i(x)$ section 4” → delete $w_i(x)$?

Typo corrected

Section 3

(1.23) p.9, l.18: if FE emissions are intended, it seems that it should be Figure 1a; if it is Figure 1b which is commented, it should be “High-latitude EE emissions”.

In the revised manuscript we have re-worded this paragraph in order to (a) correctly reference the spatial distributions of FE and EE emissions; (b) add explicit references to panels c and d (as recommended by the reviewer in comment 1.24).

(1.24) p.9, l.19-20: add references to panels c and d in Figure 1 to guide the reader.

Done

(1.25) p.9, l.22: “(EE) s ; the FE” → delete “ s ”?

Typo corrected

(1.26) p.10, l.14: Chang et al. (2014) should be “Alaska Wetlands” to be consistent with the whole sentence.

Done

(1.27) p.10, l.24-25: “dominated carbon decomposition” → “dominated BY carbon decomposition”

Printer-friendly version

Discussion paper



Done

(1.28) p.13, l.8: “is able capture” → “is able TO capture”

Done

Figures

(1.29) Figure 1: difficult to read, even on a screen. Larger maps and discrete colour scales would make it easier. It seems that panels e and f are never referred to in the text.

As recommended by the reviewer, we have now enlarged the font sizes, increased the map sizes, and have used a discrete (9-color) scale. We have also included explicit references to panels e and f in the results section of the revised manuscript.

(1.30) Figure 2: - it is almost impossible to distinguish pale grey fine lines from darker grey larger lines! - put FE ensemble and FE mean in the legend since it is used in the whole text (instead of “Ensemble” / “Mean” alone in the top panel or “This study” in the bottom panel) - if following the recommendation of not commenting too much on EE in the body of the article, the top panel could be in Supplementary material (or Appendix?)

For the sake of clarity, we have now removed the top panel in Figure 2; we now show the means and full ranges of FE and EE emissions in a single panel along with GC and BL emissions. We have updated the figure legend accordingly.

(1.31) Figure 3: a discrete colour scale would make it easier to read, together with larger panels if possible.

We have now revised the figure to include a discrete color scale; as recommended, we have also expanded the panel sizes.

(1.32) Figure 4: you may use box plots to make the legend clearer and shorter; could

GMDD

Interactive
comment

Printer-friendly version

Discussion paper



you enlarge the map?

As recommended, we have substantially enlarged the map in the revised figure and shortened the legend.

(1.33) Figure 6: the colour scale is a bit strange since the ticks every 0.2 do not fit the limits of shades

We have revised the color scale to match the ticks.

(1.34) Figure 8: a discrete colour scale would make it easier to read C6

The revised figure includes a discrete color scheme.

(1.35) Appendix B: - p.16, l.10: “ensemble. the “cor()”” → “ensemble. The “cor()””

Typo corrected

- p.16, l.11: “For Figure 6, Al,m we aggregate”: do you mean that Fig 6 shows the Al,m coefficients?

Sentence revised: the start of this sentence now reads “For Figure 6, we aggregated...”

(1.36) Appendix C: p.17, l.4: “R100,1,1” should probably be R100,3,1

Typo corrected

Anonymous Referee 2

This study describes and evaluates a new global dataset of CH₄ emissions from natural wetlands. The method follows an ensemble approach, which has the advantage that the computation of uncertainties, including spatio-temporal covariances, is straightforward. The dataset as meant to serve as a first guess in inverse modeling for which the uncertainty quantification has a clear advantage over other methods.

(2.1) It is not entirely clear what the evaluation using the GEOS-CHEM model brings,

other than the notion that this dataset is in reasonable agreement with datasets that were used in the past. Obviously, flux measurements are better suited to test the performance of a methane emission model, although scale dependencies complicate that approach also. Since this holds for the other datasets as well, it would nevertheless provide additional information.

We agree with the reviewer that - relative to the regional flux constraints (Figure 4) - the GEOS-Chem evaluation only provides supporting evidence on the plausibility of wetland CH₄ emissions relative to previous datasets: we now clarify this point in the revised manuscript. In addition, we now recognize the value of in-situ methane CH₄ measurements in the revised manuscript. While the direct comparison between global-scale fluxes and in-situ measurements is a challenging task (and beyond the scope of our work) we now highlight the importance of measurement-based regional estimates for the evaluation of global wetland CH₄ emission ensembles..

(2.2) Otherwise I was missing the dimension of inter-annual variability, which brings a clear advantage for the EE dataset - although it remains unclear what that variation looks like and how realistic it is.

We have now incorporated an evaluation of the EE inter-annual variability (IAV), including an additional manuscript figure: in particular, we compare 2001-2015 EE IAV against 2009-2010 FE emissions and the WETCHIMP model ensemble during 2001-2004. We also include an evaluation EE IAV against observationally constrained Alaska CH₄ emissions during 2012-2014 (Miller et al., 2016) and Amazon emissions during 2010-2011 (Wilson et al., 2016). Our evaluation indicates that EE IAV is broadly consistent with both modeled and observationally constrained wetland CH₄ emission estimates.

Otherwise I have only a list of technical corrections, which should be relatively easy to tackle.

[Printer-friendly version](#)[Discussion paper](#)

(2.3) page 4, line 9: 'based top down'

This sentence has been edited in the revised manuscript

(2.4) page 5, eq. 20: *Mention that there are 6 scenarios for EE (which helps the reader to make sure he/she understands table 1 correctly)*

Done: we note that EE now consists of 18 ensemble members (see response to comment 1.8)

(2.5) eq 2: *what is done when $w(x)$ is not covered by $h(x)$ and vice versa?*

We have revised the description of equation 2 to clarify that $w(x)$ represents the wetland extent fraction, while $h(x,t)$ is the relative temporal variability. In the revised manuscript, we also clarify that when $h(x,t)w(x) > 1$, $A(x,t)$ is set to a maximum value of 1.

(2.6) eq 3: *how is this done for the EE time series, every year 175Tg/yr or just the mean over the whole period? In the latter case: how do the global emissions compare for 2009-2010? It would also be useful to know how much of a correction is needed to get to 175 Tg/yr.*

For both EE and FE, s_c is derived such that each ensemble member's global emissions amount to an average annual flux of 124.5, 166 or 207.5 Tg/yr during the 2009-2010 time period (previously 175 Tg/yr; see comment 1.8). We have now clarified this in the revised manuscript. In response to comment 2.2, we also include an evaluation of EE throughout 2001-2015. For the sake of brevity, individual FE and EE s_c values (spanning 4 - 100% relative to the maximum s_c value) will be included as part of the final manuscript dataset (the dataset will be linked to the manuscript via a digital object identifier; see comment 3.1).

(2.7) page 7, line 26: 'uncertainty. The derivation' i.o. 'uncertainty; the derivation'

Done

(2.8) page 8, line 11: 'been in a'

Done

(2.9) page 8, line 15: 'Commission' i.o. 'Comission'

Done

(2.10) page 8, line 17: '). The non-wetland' i.o. ') .The non-wetland'

Done

(2.11) page 9, line 23: 'significantly lower (with' i.o. 'significantly (with' and remove 'lower' in the next line.

Done

(2.12) page 10, line 20: 'estimated' i.o. 'estimate'

Changed to “emission uncertainty estimates are”

(2.12) page 10, line 25: 'by carbon' i.o. 'carbon'

Done

(2.13) page 11, line 15 - bottom: This part is hard to read due to all the numbers. It would be better to put the numbers in a Table.

See response to reviewer comment (1.13)

(2.14) page 13-14: How much emissions are derived from rivers/lakes using the current approach?

As discussed in the “model limitations” section, we are unable to report emissions from rivers and lakes as we have insufficient information to disentangle the relative CH₄ contribution of non-wetland freshwater bodies within each grid-cell. We have re-worded this section to clarify this point in the revised manuscript.

(2.15) page 28: 'Contribution' i.o. 'Contribution'

Done

(2.16) figure 1: bottom panels: how can the units be compared?

All color bar labels now include units in the revised version of Figure 1.

(2.17) figure 3: the legend title misses a unit area

textbfThe wetland emissions ($\text{Tg CH}_4 \text{ yr}^{-1}$) are totals within each region shown on the inset map. We have modified the figure caption to clarify this.

Astrid Kerkweg

(3.1) *In my role as Executive editor of GMD, I would like to bring to your attention our Editorial version 1.1: <http://www.geosci-model-dev.net/8/3487/2015/gmd-8-3487-2015.html>. This highlights some requirements of papers published in GMD, which is also available on the GMD website in the 'Manuscript Types' section: http://www.geoscientific-model-development.net/submission/manuscript_types.html. In particular, please note that for your paper, the following requirements have not been met in the Discussions paper:*

"Inclusion of Code and/or data availability sections is mandatory for all papers and should be located at the end of the article, after the conclusions, and before any appendices or acknowledgments. For more details refer to the code and data policy" (Editorial v1.1, Appendix A1)

"Papers describing data sets designed for the support and evaluation of model simulations are within scope. These data sets may be syntheses of data which have been published elsewhere. The data sets must also be made available, and any code used to create the syntheses should also be made available." (Editorial v1.1, Appendix A5).

For these papers the same criteria as for model description papers apply, i.e., "The main paper must give the model name and version number (or other unique identifier)

GMDD

Interactive
comment

Printer-friendly version

Discussion paper



in the title.” (Editorial v1.1, Appendix A2) In this case the “model” is the “data set”

Please add a data availability section and include the data sets name and version number in the title in your revised submission to GMD.

The data availability section is now positioned at the end of the article before the acknowledgments and appendices sections. We have now added a model name and version number (WetCHARTs version 1.0) in the title of our manuscript. We have also added a data availability section in the revised manuscript, and we will be making the code available in the supplementary material. The final dataset (>50 MB) will be submitted to the Oak Ridge National Laboratory Distributed Active Archive Center (ORNL DAAC), and will be linked to the manuscript via a digital object identifier (doi). The data availability section has been expanded to include information on the ancillary datasets used in this study.

Additional changes

In the revised manuscript, Figure 1e now correctly shows the FE 5th - 95th percentile values (in the discussion manuscript, the figure was inadvertently showing FE standard deviation values).

References not included in the discussion manuscript:

Miller, Scot M., et al.: A multiyear estimate of methane fluxes in Alaska from CARVE atmospheric observations. Global Biogeochemical Cycles 30 (10) 1441-1453, 2016

Saunois, M., et al.: The global methane budget 2000-2012, Earth Syst. Sci. Data, 8, 697-751, 2016.

Interactive comment on Geosci. Model Dev. Discuss., doi:10.5194/gmd-2016-224, 2016.

GMDD

[Interactive
comment](#)

[Printer-friendly version](#)

[Discussion paper](#)

