Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-273-AC1, 2017
© Author(s) 2017. This work is distributed under
the Creative Commons Attribution 3.0 License.

Atmospheric Chemistry and Physics Discussions
Open Access

ACPD

Interactive comment

Interactive comment on "Inverse modelling of European CH_4 emissions during 2006–2012 using different inverse models and reassessed atmospheric observations" *by* Peter Bergamaschi et al.

Peter Bergamaschi et al.

peter.bergamaschi@ec.europa.eu

Received and published: 13 July 2017

Major comments - "2.1 Wetland hypothesis"

I do not find these arguments convincing. The arguments, as presented, are inconclusive at best. The region where we would expect the largest wetland emissions is Northern Europe, however in this region the inversions consistently point to a reduced seasonal cycle compared to WETCHIMP. The EU-28 seasonal cycle in WETCHIMP is 10 Tg/yr which is roughly the same as the top-down seasonal cycle in their inver-

Printer-friendly version

Discussion paper

sions. But, again, their inversion pointed to a decrease in the seasonal cycle in Northern Europe where the bulk of the wetland emissions should be. So why do we think this is due to wetlands? Because other sources are assumed to be atemporal? The authors acknowledge that other sources could have seasonal cycles (e.g., manure emissions are temperature dependent, enteric fermentation could have a seasonal cycle due to variations in the herd size, etc).

Although the WETCHIMP model ensemble estimates large CH4 emissions for Northern Europe (1.9 (0.8-3.5) Tg CH4 yr-1 (mean, minimum, maximum); excluding Norway), this data set estimates significant wetland emissions also for western Europe (1.6 (0.4-3.1) Tg CH4 yr–1), eastern Europe (0.3 (0.03-0.9) Tg CH4 yr-1) and southern Europe (0.6 (0.01-1.1) Tg CH4 yr-1). Excluding Northern Europe, the sum of the WETCHIMP CH4 emissions for western, eastern, and southern Europe is 2.5 (0.4-5.1) Tg CH4 yr-1, corresponding to 12.5% (2.2%-25.6%) of the total reported anthropogenic CH4 emissions for EU-28, which highlights the potential significant contribution of wetland emissions also for western / eastern / southern Europe.

While the inversions of TM5-4DVAR, TM5-CTE, TM3-STILT yield indeed a smaller seasonal cycle for Northern Europe compared to the mean of the WETCHIMP models (but similar amplitude for TM5-CTE), they derive significant seasonal cycles also for western / eastern / southern Europe, broadly consistent with the range of seasonal variations of the WETCHIMP ensemble. Our interpretation of this result is that indeed the spatial distribution of wetland emissions of the WETCHIMP ensemble (within Europe) is not fully consistent with the inversion results, but we consider the considerable derived seasonal variation for western / eastern / southern Europe as indication that wetlands could contribute significantly also in these sub-regions.

This interpretation is indeed based on the assumption that anthropogenic CH4 emissions have only very small seasonal variations. To our knowledge, only very few studies investigating the seasonal variations of the anthropogenic emissions are available (and have been discussed in the discussion paper). Clearly further studies on this topic will

CC BY

be required.

We will emphasize more clearly in the revised paper the caveats of the hypothesis of significant wetland emissions.

There is little-to-no discussion of the background used for the region (see next comment), could errors in the background be driving this?

The global models assimilate also global observations from the NOAA ESRL global cooperative air sampling network. The model simulations outside Europe have been further analyzed for TM5-4DVAR, showing in general very good agreement with observations at global background stations (similar as shown in previous papers, see Bergamaschi et al. [2013], Figure S4). Therefore, it seems unlikely, that errors in the background are driving the derived seasonal variations of European CH_4 emissions.

There is no mention of the methane sink, is the OH correct? If OH were too low then you may have an artificially low seasonal cycle in the global simulations (which would, again, impact the background concentrations).

The global models apply OH fields that were calibrated against methyl chloroform measurements [Patra et al, 2011; Bergamaschi et al., 2010; Houweling et al., 2014]. Since the global models assimilate global observations, potential deficiencies of the global OH fields are likely to be largely compensated by (artificial) increments of the global fluxes. As mentioned above, e.g. TM5-4DVAR reproduces the measurements at global background stations very well (the performance of other global model at global sites were not further investigated in this study). The impact of different global OH fields on derived European CH_4 emissions has been investigated by Bergamaschi et al. [2010], which showed only a very small impact.

It's unclear to this reviewer why the authors did not just perform an inversion with atemporal emissions and compare the posterior seasonality to the prior seasonality. This would show how much of this derived seasonality comes from the data instead of the

Printer-friendly version

Discussion paper

CC BY

prior. It would allow them to say which regions have significant seasonal cycles. The authors could have achieved much of this by looking at the seasonal cycles in their case with homogenous prior emissions.

Also the inversion results from inversion S3 (which was performed without using detailed bottom-up inventories as 'a priori'), show significant seasonal cycles in derived emissions. This confirms that the derived seasonal cycle is driven by the observations, and not by the a priori emissions. This was not mentioned in the discussion paper but will be included in the revised paper.

Major comments - "2.2 Poor description of methods makes it difficult to gain any insight"

The description of the various inversion systems is poor. There is a single paragraph in the main text describing the inversions. There is no mathematical description of the inversions. This is quite surprising since, at it's core, this is an inversion paper.

The inverse modelling system are described in the supplementary material (SM), section 1 "Atmospheric models" (summarizing the main elements of each system). Furthermore, all seven inverse models are described comprehensively in separate specific papers (see references in the SM). For most models used in this study only smaller updates were applied (compared to previously published applications). Therefore, we had chosen to put the model descriptions in the SM (and would prefer to keep this in the SM also in the revised version). However, we will somewhat extend the general description of the models in the main paper (section 3.2 "Atmospheric models") in the revised version.

At the bare minimum, the author's should state the assumptions for their inversions (e.g., Gaussian errors?).

Most inverse modelling systems applied in this study use Gaussian probability density functions for the uncertainties of the emissions (in case of TM5-4DVAR a 'semi log-

CC BY

normal' pdf is used; see SM section 1.1). We will add the applied pdfs in the model description for those models where this information is missing in the discussion paper.

There is additional text in the supplement (1 paragraph per model) but it is difficult to synthesize the models. Some of the models are regional but it's not clear where the boundary conditions are coming from.

It is clearly stated in section 3.2 ("Atmospheric models"; page 5, lines 23-25) where the boundary conditions are coming from: "The regional models use boundary conditions from inversions of the global models (STILT from TM3, COMET from TM5, CHIMERE from LMDZ, or estimate the boundary conditions in the inversions (NAME), using baseline observations at Mace Head as 'a priori' estimates." Furthermore, the boundary conditions are described also in the SM for all regional models (STILT, NAME, CHIMERE, COMET).

Some of the models are estimating the covariance matrices from the data, some are not.

We assume that the reviewer refers here to the observation covariance matrix. The uncertainties of the observations (diagonal elements of the covariance matrices) include both the measurement error and the model error. Most models use the "working standard repeatability" (see section 2 of main paper) as observation error. However the estimates of the model errors are very different in the different inverse modelling systems (and generally based on simplified assumptions). For most models the assumed uncertainties of the observations is described in SM section 1 - for those models where this information has been missing (CHIMERE, COMET), it will be added.

It is extremely difficult for the reader to understand why these inversions are performing differently. For example, it seems that the boundary conditions are coming from global models in the case of some regional models, how independent are these different inversion systems (especially the global/regional ones)? Are we comparing apples to apples?

Printer-friendly version

Discussion paper

CC BY

The global models providing the boundary conditions for the regional models are generally largely independent from the regional models (apart from the fact that the different models may have some features in common, e.g. use of same or similar meteo data sets).

How much of the differences are due to assumptions vs transport vs something else? It's extremely difficult to understand the differences without clearly laying out the key differences between the models.

Given the very high complexity of the different inverse modelling systems, it is indeed very difficult to understand where the differences in the derived emissions are coming from. But this is actually not the goal of this study (and would require further specific modelling experiments). The objective of this study is to use the model ensemble to provide more realistic overall uncertainty estimates (from the range of the inverse models) and to evaluate the model performance by validation against independent observations.

I would point the authors to the Henne et al. (2016) paper as an example of a paper that does a good job of explicitly highlighting the differences between their inversion systems and allows the readers to actually gain insight from the ensemble of inversions. Table 2 from Henne et al. (2016) is a particularly good example of how one can demonstrate the major differences between inversion frameworks.

The fundamental difference between the study of Henne et al. (2016) and our study is that Henne et al. use one single inverse modelling system, varying various input parameters / settings of this system as compiled in their Table 2. In contrast, our study uses very different inverse modelling systems, which makes it inherently more difficult to highlight the differences between the systems (which are largely independent systems and which differ in many aspects). Important parameters (model resolution, meteorology, a priori emission inventories, applied station sets are compiled in Tables 1, 2, and 3. We will include also the applied baselines for the regional models in Table

CC BY

3.

Also, the phrase "no a priori" is, almost certainly, using incorrect terminology. The posterior probability is proportional to the product of the likelihood and the prior probability: Posterior probability / Likelihood × Prior probability. Using a homogenous distribution of emissions is still including a prior, it just isn't based on a bottom-up inventory. To actually use "no a priori" would be "Maximum Likelihood Estimation" where one simply finds the parameters that maximize the likelihood term

In section 3.1 we have described S3 as: "Inversion S3 was performed without using detailed bottom-up inventories as 'a priori', in order to analyse the constraints of observed atmospheric CH4 on emissions independent of 'a priori' information (using a homogeneous distribution of emissions over land and over the ocean, respectively, as starting point for the inversions in a similar manner as in Bergamaschi et al. [2015])." The short notion "no a priori" has been only used in Table 2. We will add a footnote in this table to refer the reader to the above description in section 3.1

Major comments - "2.3 'Novel' Bias method"

This "novel" bias method is, essentially, what an inversion already does. . . They are just plotting the model-data mismatch averaged over different parts of the atmosphere. This is hardly a "novel approach". (mathematical derivation not repeated here) From this, it's quite easy to see how cobs−cmod = ∆cobs − ∆cmod. So, as I stated above, all the authors have done is plot the model-data mismatch (cobs − cmod) averaged over two parts of the atmosphere. It does not strike this reviewer as particularly "novel".

We do not agree with the statement of the reviewer that our approach to estimate the bias in the derived emissions is "essentially, what an inversion already does", since we look at independent observations that were not used in the inversion - which is a common method to validate inverse models (see e.g. Michalak et al., [2016]). Commonly, however, such analyses are performed to diagnose qualitatively, if the inverse models have biases. The novel aspect of our method is that we use the baseline in

Printer-friendly version

Discussion paper

order to extract the signal which comes from the European emissions. Integrating the enhancement of the model simulations compared to the background over the entire boundary layer or the entire column of the lower troposphere (and comparison with the corresponding observed CH4 enhancement) provides a measure of the total CH4 emitted by European emission. The ratio of the simulated vs. observed integrated enhancements provides a first order estimate of the relative bias in the model emissions.

As explained in section 4.2, the validation against independent aircraft profiles is very important, since the inverse models assimilate only surface observation. Therefore, potential errors in the vertical mixing of the models can introduce significant biases in the derived emission.

There are novel approaches that attempt to account for systematic errors in inversions in a rigorous manner. Weak-Constraint 4D-Var (Tremolet, 2006) and Hierarchical Bayesian inference (see Ganesan et al., 2014 and references therein) are two good examples of this.

We agree that the "Hierarchical Bayesian inference" is an interesting approach to provide more realistic uncertainty estimates for individual models (i.e. estimates within the individual inverse modelling systems, corresponding to the error bars in our Figure 3). Nevertheless, validation against independent observations will remain indispensable as independent evaluation of the inverse models. Also the mentioned "Weak-Constraint 4D-Var" is certainly a very interesting technique - but to our knowledge so far only applied in some cases for data assimilations, but not in inverse modelling systems.

References

Bergamaschi, P., et al., Atmospheric CH4 in the first decade of the 21st century: Inverse modeling analysis using SCIAMACHY satellite retrievals and NOAA surface measurements, J Geophys Res-Atmos, 118(13), 7350-7369, doi: 10.1002/jgrd.50480, 2013.

CC BY

Bergamaschi, P., et al., Inverse modeling of European CH4 emissions 2001-2006, J. Geophys. Res., 115(D22309), doi:10.1029/2010JD014180, 2010.

Houweling, S., et al., A multi-year methane inversion using SCIAMACHY, accounting for systematic errors using TCCON measurements, Atmos. Chem. Phys., 14, 3991–4012, doi: 10.5194/acp-14-3991-2014, 2014.

Michalak, A. M., Randazzo, N. A., and Chevallier, F.: Diagnostic methods for atmospheric inversions of long-lived greenhouse gases, Atmos. Chem. Phys., 17, 7405-7421, https://doi.org/10.5194/acp-17-7405-2017, 2017.

Patra, P. K., et al., TransCom model simulations of CH4 and related species: linking transport, surface flux and chemical loss with CH4 variability in the troposphere and lower stratosphere, Atmos. Chem. Phys., 11, 12813–12837, doi: 10.5194/acp-11-12813-2011, 2011.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2017-273, 2017.