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Atmospheric Chemistry and Physics
Discussions

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.

Anonymous Referee #1

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1 Overview:

Review of “*Inverse modelling of European CH_4 emissions during 2006–2012 using different inverse models and reassessed atmospheric observations*” by Bergamaschi *et al.*

Bergamaschi *et al.* present an ensemble of top-down emissions estimates for European methane sources for 2006–2012. The main scientific finding is that wetlands are a significant contributor to the European methane budget. I do not support publication

of this manuscript in it’s present form. This is mainly because (1) I find the wetland hypothesis wholly unconvincing, (2) the methods description is poor, making it hard to gain any insight from the different inversions, and (3) it’s not clear to this reviewer that their “novel” approach to estimate bias is actually an advancement. As such, it’s not clear to this reviewer that this manuscript contributes much to the current literature.

2 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 $\sim$10 Tg/yr which is roughly the same as the top-down seasonal cycle in their inversions. 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). There is little-to-no discussion of the background used for the region (see next comment), could errors in the background be driving this? 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).

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 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.

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

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

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.

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: $\text{Posterior probability} \propto \text{Likelihood} \times \text{Prior probability}$. 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.

2.3 "Novel" Bias method #1

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".

The likelihood term ($\mathcal{L}$) in the inversion, assuming Gaussian errors, is typically written as the norm of the difference between the observations and the modeled concentrations:

$$\mathcal{L} = ||\mathbf{c}_{\text{obs}} - \mathbf{c}_{\text{mod}}|| \tag{1}$$

Most atmospheric inversions assume Gaussian errors, leading to the following expression:

$$\mathcal{L} = \frac{1}{2}\left(\mathbf{c}_{\text{obs}} - \mathbf{c}_{\text{mod}}\right)^T \mathbf{R}^{-1}\left(\mathbf{c}_{\text{obs}} - \mathbf{c}_{\text{mod}}\right) \tag{2}$$

However, the modeled concentration can be broken up into the contribution from the emissions and the background: $\mathbf{c}_{\text{mod}} = \Delta\mathbf{c}_{\text{mod}} + \mathbf{c}_{\text{mod,bkg}}$ where $\Delta\mathbf{c}_{\text{mod}}$ is the modeled enhancement due to emissions within the domain. This can then be used to rewrite the likelihood as:

$$\begin{aligned}\mathcal{L} &= ||\mathbf{c}_{\text{obs}} - \mathbf{c}_{\text{mod}}|| \\ &= ||\mathbf{c}_{\text{obs}} - (\Delta\mathbf{c}_{\text{mod}} + \mathbf{c}_{\text{mod,bkg}})|| \\ &= ||\mathbf{c}_{\text{obs}} - \Delta\mathbf{c}_{\text{mod}} - \mathbf{c}_{\text{mod,bkg}}||\end{aligned}$$

$$
\begin{aligned}
&= ||\mathbf{c}_{\mathsf{obs}} - \mathbf{c}_{\mathsf{mod,bkg}} - \Delta\mathbf{c}_{\mathsf{mod}}|| \\
&= ||(\mathbf{c}_{\mathsf{obs}} - \mathbf{c}_{\mathsf{mod,bkg}}) - \Delta\mathbf{c}_{\mathsf{mod}}|| \\
&= ||\Delta\mathbf{c}_{\mathsf{obs}} - \Delta\mathbf{c}_{\mathsf{mod}}|| \qquad (3)
\end{aligned}
$$

From this, it's quite easy to see how $\mathbf{c}_{\mathsf{obs}} - \mathbf{c}_{\mathsf{mod}} \equiv \Delta\mathbf{c}_{\mathsf{obs}} - \Delta\mathbf{c}_{\mathsf{mod}}$. So, as I stated above, all the authors have done is plot the model-data mismatch ($\mathbf{c}_{\mathsf{obs}} - \mathbf{c}_{\mathsf{mod}}$) averaged over two parts of the atmosphere. It does not strike this reviewer as particularly "novel".

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.

3 References:

Ganesan, A. L., Rigby, M., Zammit-Mangion, A., Manning, A. J., Prinn, R. G., Fraser, P. J.,. . . Weiss, R. F. (2014). Characterization of uncertainties in atmospheric trace gas inversions using hierarchical Bayesian methods. *Atmospheric Chemistry and Physics, 14*(8), 3855-3864. doi: 10.5194/acp-14-3855-2014

Henne, S., Brunner, D., Oney, B., Leuenberger, M., Eugster, W., Bamberger, I.,. . . Emmenegger, L. (2016). Validation of the Swiss methane emission inventory by atmospheric observations and inverse modelling. *Atmospheric Chemistry and Physics, 16*(6), 3683-3710. doi: 10.5194/acp-16-3683-2016

Tremolet, Y. (2006). Accounting for an imperfect model in 4D-Var. *Quarterly Journal of the Royal Meteorological Society, 132*, 2483-2504. doi: 10.125/qj.05.224

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