Biogeosciences Discuss., 11, C7182–C7187, 2014
www.biogeosciences-discuss.net/11/C7182/2014/

Biogeosciences
Discussions
Open Access

Interactive comment on "Natural and anthropogenic methane fluxes in Eurasia: a meso-scale quantification by generalized atmospheric inversion" *by* A. Berchet et al.

Anonymous Referee #2

Received and published: 7 December 2014

This study makes use or continuous measurements of methane collected at several sites in Siberia to constrain methane emissions in the West Siberian lowlands using inverse modelling. To do this requires a regional modeling approach with a relatively high-resolution model. This is what has been in this work, for the first time, which is what makes this study scientifically interesting in my opinion. Unfortunately, the focus of the paper is not so much on the methane fluxes themselves, how they influence the measured signal, and what are the consequences for our understanding of Siberian methane sources. Instead, much of what is presented is a rather technical discussion about the application of the Marginalized Bayesian Inversion method for his specific problem. At times the discussion is difficult to follow, because another paper that is still

under review is referenced for details. Although I browsed through that manuscript, this was insufficient to judge the details of the method – which other reviewers are currently taking care of. Any corrections to the method, in response tot their comments, should somehow find their way in this work.

My main criticism about this study is that the method consists of several steps, which require decision criteria that are not quantified. This makes it very difficult to judge to validity of the approach. Yet the abstract claims that the method is more objective than conventional methods. This may be true regarding the quantification of prior uncertainties, but it seems that the problem is rather moved to elsewhere in a way that is less transparent in the end. With a focus on the method, I would have expected some evaluation of the performance of the new compared to conventional methods. This way we would learn about the added value of the technique that is used. Right now this is unclear, in particular since the evaluation that is done (against GOSAT data) is qualified as unsuitable in the end. Then what is it that we learn here? For this manuscript to be suitable for publication there should be a clear answer to his question. The comments listed below will hopefully provide some guidance.

GENERAL COMMENTS

It is not clear why the minimum measured value per day is used as data filtering method. To exclude night time data, and take PBL height as selection criterion is fine. Something like an afternoon averaged value has been proposed in earlier studies. However, to select a minimum measured value will almost certainly bias the inversion towards too low emissions. A low-resolution model averages high frequency variability, not only the highs, but also the lows. In the model, the grid box where the measurement side is situated will almost certainly have emissions. Because of this, the simulation will not just represent to undisturbed background, but also local emissions.

Besides the comparison with independent data it would be useful to know how well the inversions reproduce the measurements that are used in the inversion. The risk of

the rigorous sampling that is applied is that the measurement coverage becomes very irregular. How realistic are the optimized seasonal cycles at sites with only few data points? This will be hard to judge from the limited data themselves, but nevertheless this cycle should look plausible, and not too much perturbed at times when data are available. Such comparisons could also serve as an evaluation of the data selection procedure. How poor or how well is the agreement between the model and data that were filtered out?

Among the poorly quantified criteria that I mentioned earlier are the following: What are the criteria is used for,

- Aggregating fluxes.

- Aggregation of the boundary conditions.

- Plumes that can or cannot be resolved by the transport model.

- The rejection of unconstrained regions?

- The rejection of regions that are influence by the boundary condition.

- The statement that the observations constrain fluxes within a radius of 500 km.

- Separation between natural and anthropogenic fluxes.

As detailed below the use of language is rather inaccurate in several places. I only mention the most important examples, but strongly recommend some further action from the group of co-authors to improve the readability of the manuscript.

SPECIFIC COMMENTS

P14590, line 3: Methane influences OH also directly (thus not only via O3)

P14591, line 26: What is meant by the 'absolute composition in CH4'?

P14592, line 7: What is meant by 'relative contributions to the fluxes'?

P14592, line 14: What is meant by the 'data assimilation Bayesian theory'?

P14592, line 17: 'the likely under-estimation .... some inventories": a reference is needed here

P14592, line 18: 'However, atmospheric ... local constraints surface fluxes': This depends on the kind of site and is not true for example for a station like South Pole.

P14593 line 1: It is kind of obvious that uncertainties in reproducing transport come from transport errors.

P14594 line 8: 'To do so, we look for the pdf of the state of the system with some knowledge about the atmospheric composition and on the state distribution.': What are we supposed to learn from this sentence?

P14599, line 27: How can the method account for sampling bias. I didn't find that information back in section 2.1

P14601, line 13: A version number and reference is required for GFED.

P14603, line 15: 'state vector' i.o. 'observation operator'.

P14603, line 19: What matters is not the mean residence time of the air, but the amount of methane that is oxidized within the domain. This could easily be a few Tg/yr. If it cannot be assigned to an atmospheric sink, then it will be accounted for as a reduced source. I understand that you don't want to optimize the sink, but I don't think that the sink can be completely ignored.

P14604, line 14: The statement that GOSAT is the only remaining source of data seems incorrect. For example, NIES has also an aircraft program in Siberia.

P14604, line 27: 'and associated to ... priori profiles' What is meant here?

P14609, line 15: 'Then, amongst ... from each other". What criterion is used to make this distinction?

P140610, line 10: ‘indicating that . . . anthropogenic from wetland emissions”. What is the criterion for deciding that fluxes can or cannot be separated?

P140610, line 22: ‘to the real fluxes’. I guess you mean to the inversion-estimated fluxes?

P14611, line 10: which co-located emissions? And how do you know that the regional flux estimates are accurate?

P14612, line 8: How are ‘significantly constrained’ fluxes extrapolated.

P14613, line 17: ‘This could explain . . . August’ I think it is worth checking how well the inversion resolves anthropogenic and natural sources for this month using posterior covariances.

P14614, line 3: How is the 27TgCH4 derived?

P14615, line 10: ‘We observe a mean shift . . .’ But the GOSAT data were corrected for a mean offset with the model. Therefore it depends on which model estimate you take for this correction whether the shift that is discussed here leads to an improved agreement with GOSAT or not.

P14615, line 11: A mean shift cannot be compared with a single column retrieval precision.

P14617, line 1: But the MERLIN mission doesn’t yield measurements at high spatial resolutions (only a narrow line of $\sim$50 km length per aggregated sample).

TECHNICAL CORRECTION

P14590, line 2: ‘climate forcing’ (without ‘s’)

P14591, line 26: ‘literature’ (remove a ‘t’)

P14591, line 26: ‘composition <of> CH4’

P14592, line 1: ‘variation’ (without ‘s’)

P14593, line 13: ‘in-/out-coming to/from’: What?

P14593, line 19: ‘The maximum likelihood criterion’

P14600, line 2: ‘AT night or WHEN the PBL is thiNner than’

P14603, line 11, ‘hot spotS’

Figure 3, caption: ‘On the left column, . . . boundary conditions” This sentence is broken, and needs revision.

Figure 4: This figure needs resizing. Right now it is difficult to see what they are supposed to represent.

Interactive comment on Biogeosciences Discuss., 11, 14587, 2014.