Geosci. Model Dev. Discuss.,
doi:10.5194/gmd-2016-181-RC2, 2016

Geoscientific Model Development Discussions
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EGU

Interactive comment on "Development of CarbonTracker Europe-CH_4 – Part 1: system set-up and sensitivity analyses" *by* Aki Tsuruta et al.

Anonymous Referee #2

Received and published: 12 October 2016

Tsuruta et al. describe a series of sensitivity tests performed on the data assimilation system Carbon Tracker Europe-CH4. The authors explore the impact of model parameters such as assimilation ensemble size, prior emissions estimates and the number of observations used. From these tests, two configurations are proposed to be used for longer-term studies in a companion paper.

Although such sensitivity tests are undoubtedly important, the authors need to be clearer about how the outcomes of this work are of benefit to the wider inverse modelling community. In order to make the work more generally applicable perhaps the authors could provide some comparison of the relative importance of the input parameters, through a global sensitivity analysis for example. As it stands, the paper attempts

to provide a justification for a particular model set-up to be used in the companion paper, but I wonder whether this is enough to justify a paper of its own, or whether this information should rather be included as a supplement to the companion paper.

Overall, I found the manuscript to be a little vague on what the outcomes of the sensitivity tests are, with a focus on qualitative rather than quantitative discussion. The manuscript was let down slightly by a number of grammatical errors or poorly constructed sentences, which may be why the key messages of the work are not clearly conveyed.

General comments:

The paper focuses on sensitivity tests of various model inputs and parameters, and selects two models as a consequence of these tests. However, all tests assume the same model-data mismatches (mdm), which raises two major issues:

1. Clearly the mdm values are another input to the inversion which will change the form of R and thus the cost-function minimization. The impact of the mdm values on posterior emissions has been examined many times before (e.g. Michalak et al., 2005; Trudinger et al., 2007), with the studies commenting on the importance of these error terms. It seems a little odd therefore that this crucial component of the inversion is ignored in the sensitivity tests, given the somewhat arbitrary nature of their assignment. Furthermore, the observation error correlation structure would also impact on the solution, but I was unable to find any discussion of this in the manuscript.

2. The decision to select the 2 chosen models S1 and S5 appears to be dependent on the posterior mismatch to the observations. However, given this posterior is itself dependent on the chosen mdm, it is conceivable that under a different set of assumptions one would select a different model instead of S1 or S5. Since the values of mdm appear to be entirely down to investigator choice, I cannot see how the paper can propose an "optimal" inversion system that would be applicable beyond the specific case examined here.

In the introduction it is stated that the aim of the paper is to "introduce the set-up…for an optimally working methane inversion system." However, I am not convinced that this is achievable from only seven different inversion configurations. Comparatively, one configuration may be better than the other six, but it would require a much more in-depth analysis to find the "optimum" configuration. For instance, some combination of configurations S2, S4 and S7 could provide a better match to the observations. However, performing the sensitivity analysis in the localised way of this work means that such a conclusion cannot be reached.

It may be that the choice of S1 and S5 is justified but any clear evidence to support this conclusion was either lost in the text or not present. In fact, Figure 7 would appear to suggest that there is very little to distinguish between the majority of configurations at both the global and continental scale.

Specific comments:

Page 2. Lines 28-30: "All of these impacts…" These sentences were hard to make much sense of. I suggest they are rewritten.

Page 7, Lines 5-7: What was this "information provided by the experimentalists"? How many continuous day or night time observations were assimilated per site per day? What was the form of the observation error covariance matrix (e.g. diagonal?) If it is diagonal, is this assumption justified? In general some key details appear to be missing.

Page 7, Lines 14-16: Given the TM5 model is run at higher resolution over Europe, one might assume this would lead to a reduced representation error for European stations, i.e. 1x1 degree boxes might be able to represent a point in space better than a 4x6 degree. However, the mdm values appear to be the same whatever the model resolution at each site. Is there a reason for this?

Page 7, Line 17: "…for the sites that appeared problematic in the inversions…" What

is meant by "appeared problematic"? Tuning the mdm values post-inversion is surely unacceptable as a violation of Bayes rule. What is the justification for 75 ppb? If the data is problematic why not just discard it completely?

Page 14, Lines 1-3: "S3 posterior mole fractions matched the observations best…in other words the additional information from the continuous observations was useful in gaining better agreement with NOAA observations." But according to Table 2, S3 is the configuration with discrete observations only, so the above statement cannot be right. It doesn't seem very surprising that S3 matches the NOAA observations better when these are the observations that have been used to constrain the emissions. Surely it would be more helpful to compare to an independent dataset rather than those that have already been used to derive the emissions.

Page 14, Lines 24-25: "Indeed the additional observation uncertainty increased the emissions uncertainty also." I fail to see how increasing the number of data points (however uncertain) would increase uncertainty, and this is not backed up by Table 4 which shows the inversion with only discrete observations (S3) has the highest emissions uncertainty.

Page 16, Line 7-8: "Thus, improving the prior estimates is important even when using an inverse model in the absence of observations." I assume this is a case of poor phrasing, but I am intrigued as to how one would perform inverse modelling in the absence of observations.

Page 16, Lines 23-24: "We choose CTE-CH4 with S1 settings and S5 settings…" Why? This seems to be a main outcome but no justification is given.

Figure 2: I appreciate the Asian tropical plot is supposed to show the large variability with an ensemble size of 20, but is there a way of conveying the same message without it looking quite so messy? It might make the plot a little easier to interpret.

References

Michalak, A. M., Hirsch, A., Bruhwiler, L., Gurney, K. R., Peters, W., and Tans, P. P.: Maximum likelihood estimation of covariance parameters for Bayesian atmospheric trace gas surface flux inversions, J Geophys Res-Atmos, 110, 2005.

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