**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Technical Note: Methods for interval constrained atmospheric inversion of methane" *by* J. Tang and Q. Zhuang

Anonymous Referee #1

Received and published: 6 October 2010

Tang and Zhuang describe three different interval constrained optimization methods for atmospheric trace gas inversions and use a pseudo data inversion of methane to evaluate the interval constrained inversions and their unconstrained counterparts. Due to the limited number of observations available to constrain inverse estimates of methane and other atmospheric species, it is common for atmospheric tracer inversions to estimate physically unrealistic fluxes unless some form of regularization is applied, such as aggregation to large regions, correlations between regions, or strong constraints from a priori estimates. The authors suggest that these non-physical results could be alternatively eliminated using an interval constraint approach. This topic is potentially of great interest to the atmospheric tracer inversion community, and the work is promising, but I cannot recommend the paper for publication without substantial revisions as described below.

Most importantly, the Results and Discussion section was thin, with very little analysis of the results of the synthetic data inversions. Tang and Zhaung show a series of figures in which they plot the a priori methane fluxes against the true fluxes used to generate the methane mixing ratios used in the inversion, and then fit these data to a line. If the inversion were working perfectly, all the points should line up on a line with a slope of 1 and an offset of zero. However, all of the pseudo data inversions have slopes that are much less than one, meaning that they all systematically under-estimate the methane emissions (with slopes of 0.63-0.75). The reasons for this are never discussed or explored, even though the under-estimate common to all the inverse methods tested here appears to be a much larger than the differences between inverse methodologies. I suspect it may be related to biases from the a priori estimates, which have a slope of 0.44. If so, the extent to which the priors bias the inversion is an important point and should be discussed, particularly if the interval constrained technique could be used to reduce reliance on the a priori estimates.

In addition, I found Figures 1,2,3,5, and 6 to be a bit unclear. Since the purpose of this paper is to compare the effectiveness of the optimization, it would be easier for the reader to judge how accurate a particular approach was if all of the plots were on the same scale, the x and y axes of a given plot were on the same scale, and the "perfect inversion" (e.g. 1:1 line) were shown. If feasible, it would also be interesting to see regional groupings of points highlighted in different colors to try and understand if there is some spatial coherence in the errors and where the outliers are coming from.

Furthermore, the paper does not stand on its own well. Interval constraints are not widely used in the field of atmospheric tracer inversions, and very little background is provided in the paper. Likewise, I wasn't able to fully understand the methods section without first reading the methods section of Tang and Zhuang [2010] in detail, which was also unclear and difficult to follow in places. While I appreciate that material published elsewhere should not need to be repeated in full, a more thorough, detailed discussion of interval constrained inversions in the introduction and methods would be

appreciated.

In addition to these broader issues, I have the following minor suggestions:

p. 19982, lines 15-18: This is already a somewhat method-specific description of atmospheric inversions. Please broaden to a more general description. In this paragraph or the one that follows, it would be helpful if you expanded on the errors and biases inherent in other inverse methodologies or regularization techniques that might be improved or eliminated by an interval constrained approach.

p. 19983, lines 1-5: I'm not aware of any inverse studies of methane or other greenhouse gases that report non-physical inverse estimates. This is because inverse modelers generally recognize this problem and use some form of regularization (e.g. constraints based on a priori estimates, co-variances between parameters, etc.). The authors should recognize this and instead build a case for why the interval constraint approach might be better than these alternatives, as suggested in my previous comment.

Throughout the paper, the authors refer to the inversions without interval constraints as "unconstrained" inversions. This was a bit confusing on the first read, because the inversions are constrained by observations (or at least synthetic observations), they just don't include an interval constraint.

Figure 1: I was surprised by the large bias in the priors, since the authors say that the priors are obtained by adding random perturbations to the true fluxes (19992, lines 8-10). If these errors are truly random, then shouldn't Figure 1 have a slope of 1?

Finally, there were quite a few English grammatical mistakes in the paper that should be corrected before resubmission.

This study is potentially quite promising, and I look forward to reading it again once the authors have addressed my concerns.

References:

Tang, J. and Q. Zhuang (2010), Technical Note: Methods for interval constrained atmospheric inversion of methane, Atmos. Chem. Phys. Discuss. 10, 19981-20004, doi:10.5194/acpd-10-19981-2010.