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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Towards better error statistics for atmospheric inversions of methane surface fluxes" *by* A. Berchet et al.

Anonymous Referee #2

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This is a very interesting piece of work, addressing a commonly recognized weakness in the use of inverse modeling for estimating sources and sinks of greenhouse gases. It takes more than average effort to understand what was done, but it is worth the investment. Except for some inaccurate formulations, this has more to do with the complexity of the problem than the description of it by the authors. Below some suggestions are made to further improve the readability. The most important concern I have is that while the proposed methods are interesting to read about, it is difficult to grasp where the joint information provided by the a priori fluxes, the transport model and the measurements end up in the solution. It is interesting to see the techniques applied to a real problem, but it makes it very hard to objectively evaluate their performance. After all we have very limited means of verifying the results of inversions. As explained below, some worrying results, in my opinion, should have received more attention. Else, the

study would benefit from an extended discussion concerning the recommended use in inversions with large state vectors. With these topics addressed, the study should be acceptable for publication.

GENERAL COMMENTS

Differences between the techniques for estimating B and R are mostly discussed in terms of uncertainties rather than the impact of the actual solution. In the end only a single method is selected for discussing the estimated fluxes, where it would have been interesting to have a comparison between the three techniques. It would allow evaluating the relevance of the difference between the methodologies, which is important for application to larger inverse problems (what are the returns of larger computational investments?). Of course, the solution can only be judged in relation to its (posterior) uncertainty. There I was much surprised to read that the uncertainties in Figure 7 do not exceed 1%. Given that the estimated uncertainties are supposed to account for contributions from transport and representation uncertainty this is a very surprising outcome. It is hard to imagine that it is really possible to estimate the emissions at this level of accuracy. One necessary test, in my opinion, is if the other two methods yield solutions that are really within that uncertainty range. If not then how consistent are the posterior solutions and what does it say about the realism of the estimated R and Bs?

In the discussion some loose statements are made comparing the inverse methods, which estimate prior R and B uncertainties versus those that rely on expert judgment. The claim is made that the former approach gives better results, without providing any evidence. In my opinion such a comparison is essential to assess how important it is to invest in the optimization of R and B. The question is also how to measure the relative performances, given that we don't know the true solution. Without further explanation and demonstration it is not possible to judge value of these statements. Currently, the ND method is favored over the other methods. To apply this method to large inverse problems, however, seems impossible. The question remains if the cheaper methods

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are still preferable over the expert judgment approach.

Further assessment is needed of the signals in the data that determine the estimated uncertainties. The signals that have been identified are PBL height and the passage of frontal systems. They are interpreted as a confirmation that the methods do what they are supposed to do, which is reproduce known sources of transport model uncertainty. The question, however, is whether or not the results would have been any different for a perfect transport model. Another possibility is that the algorithm has the tendency to reduce the weight of any conditions that cause high variability (of which PBL dynamics and frontal passages are important examples). Part of the variability is signal, which the inversion is supposed to translate into fluxes. Reducing the weight of signal will therefore limit the performance of the inversion. It will probably require dedicated OSSEs to determine how successful the methods really are in separating flux uncertainty from model uncertainty. Until then, I believe more careful formulations are needed.

SPECIFIC COMMENTS

P3742: The state vector elements don't seem to add up.

P3748, line 8: What physical considerations?

P3750, line 9: Design on B. The underlying assumptions are not clear to me. What are d_bˆa and d_bˆ0 in equation 9?

P3750, line 14-17: Please explain in more detail (I'm getting lost here).

P3751, line 12: What is meant by 'results'? The R matrix? What is to be expected in this case depends on the off diagonals (size and sign). It is hard to make a comparison, if matrices differ not only by off diagonals but also the diagonals. Therefore it is not clear to me that this outcome was expected.

P3751, line 25: Is this cause hypothesized or has it really been tested?

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P3752, line 6: So for ML the observational and the background errors are smaller than for DS. How in that case can the overall chi2's be comparable (i.e. satisfy the chiˆ2 criterion)?

P3752, line 8: what is meant by 'no clear behavior'?

Table 2: I'm surprised by the low p values given the generally low r values, to make this clearer it would be helpful to include to number of data.

P3762, line 28: To make use of nighttime data using increased uncertainties assumes that model errors associated with the simulation of the nighttime PBL are random, which is likely not the case.

TECHNICAL COMMENTS

P3750, line 3: 'convergence' instead of 'divergence'?

P3751, line 21: 'as' instead of 'then'

P3752, line 19: 'as a' instead of 'by way of'

P3753, line 18: interquartile 'range' instead of 'gap'

P3755, line 22: remove 'issue'

P3760, line 1: 'as' instead of 'than'

P3760, line 22: 'assumptions' instead of 'hypotheses'

P3761, line 3: 'of' instead of 'to'

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 3735, 2013.