Atmos. Chem. Phys. Discuss., 14, C6067–C6072, 2014
www.atmos-chem-phys-discuss.net/14/C6067/2014/

Atmospheric Chemistry and Physics Discussions

ACPD

14, C6067–C6072, 2014

Interactive comment on "Greenhouse gas network design using backward Lagrangian particle dispersion modelling – Part 2: Sensitivity analyses and South African test case" *by* A. Nickless et al.

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Received and published: 19 August 2014

The authors would like to thank the reviewer for considering the manuscript and for positive comments and criticism. The following points and statements have been extracted from the review, together with responses to each point:

Even though some parts of the manuscript are very well written, most of it is difficult to read.

Response: As stated in response to the first review, the manuscript has been rewritten and restructured to place firm emphasis on the sensitivity analyses. The writing has been tightened up so that it is clearer, and misuse and inconsistencies of terminology have been rectified. Particularly, the use of "prior flux covariance matrix" and "observation covariance matrix" have been clarified throughout the manuscript to avoid any ambiguity. We have also clearly stated what is implied by the term "uncertainty" and how we have used the posterior flux covariance matrix to assess the uncertainty in the flux estimates.

Organization of the manuscript is unfavourable.

Response: The paper has been restructured to improve readability and to follow a more logical order. Moreover, we have clearly made the sensitivity analyses the focus of the paper, and avoided repeating explanations and equations which have been presented in Part 1 of the paper (Ziehn et al., 2014), ensuring that only the necessary equations applicable to the sensitivity analyses are included in Part 2. We have also expanded on the methodology section to present statistical tests which we use to better compare between network design solutions from the different sensitivity tests. In particular we have focused more on the uncertainty reduction that a network can achieve, and used statistical measures of spatial clustering and similarity to compare between solutions, rather than trying to assess the network based on some preconceived notion of what the optimal network would look like.

To test the influence of the concentration from the boundary Equation (8) is used: "The average value for the square root of the sum of all the diagonal elements of Cb for all stations was only 0.073 with a standard deviation of 0.026 in January, and 0.070 with a standard deviation of 0.031 in July." This is difficult to understand, but I presume Cb is calculated per station, otherwise one could not compute an average value and a standard deviation. The average value over the stations are of less interest than the maximum value. Also the maximum diagonal element is of interest. The other and probably more important point to note is that Equation 1 obviously uses an uncorrelated error of the concentration at the boundary. In fact, at the model resolution, one would expect high error correlations in space and time, which would magnify Cb.

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Response: Yes, **C**b is calculated for each station. Better descriptive statistics, as suggested by the Referee, have been used to assess the results from the boundary contribution tests. Moreover, as suggested by the Referee, we have considered correlation between the boundary terms, to determine how this would impact on the uncertainty contribution from the boundary concentrations.

The tests of the influence from the ocean pixels need to be better explained.

The section on the influence tests has been reconsidered since it does not in its current form contribute to the optimal network design, and since we are already assessing the contribution from the ocean pixels in the sensitivity analysis and dealing with the non-South African terrestrial flux uncertainties in the study. The analysis of the contribution from the boundaries has been further developed as discussed in the previous point.

The solution of the network design problem must be independent of the optimisation algorithm that is used. Otherwise the term "robust" that the authors use is not justified. Here the IO and the GA yield different networks. This is a serious problem of the study.

Response: Although both the IO and GA optimisation algorithms are seeking to find the network solution which best reduces the posterior error of the fluxes, they operate off very different philosophies, which is why we can expect to obtain different results. IO, although by far a more efficient algorithm than the GA, can potentially never find the best solution for a multimember network, due to the incremental approach, which always considers only one additional station at a time. But due to the nature of the optimal network design problem, where regions of large flux uncertainty tend to be fragmented, surrounded by large regions of low uncertainty, incremental optimisation has successfully been implemented. This method has also been favourably compared to other optimisation techniques, such as simulated annealing, which simultaneously optimises parameters and is capable of finding a global minimum in a multi-parameter problem (Patra and Maksyutov, 2002). We are assured though, for a specific configuration of parameters, that the IO will result in only one network solution. The GA on

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the other hand attempts to optimise the network placements simultaneously, therefore it does have the potential of finding the global optimum for multi-parameter problem. It relies on pseudo-random numbers at every iteration to determine what new population members will be. Therefore, for every run of the GA, different combinations of population members may be considered each time. It may therefore by chance not include the global optimal solution to compare to other solutions. It is up to the user to ensure that the settings of the GA are such that the probability of finding the best solution is high, although it can never be guaranteed.

What we have emphasised in the new manuscript is the comparison between networks. Although the IO may find a solution which is slightly inferior to the best solution found by the GA, we have assessed just how different the solutions really are. Similar conclusions are drawn from both optimisation procedures where the gross features of the network are maintained, as for the study of Patra and Maksyutov (2002), and therefore the IO algorithm still has merit. Additionally, the way the solution is formed has added advantages for practical implementation of the network. It presents a list of the all of the potential stations at each increment, providing each station's potential reduction of the posterior error at that stage. Therefore, it provides not only an network solution for the total number of stations required, but also an order of prioritisation, as well as a list of best alternatives if a selected location is found to be unfeasible.

After considering the issues related to using a method like the GA for network optimisation, an additional short paper recently submitted has considered what occurs if the GA is run several times, and under different GA settings. We determined that in this particular example, when the prior flux covariance matrix elements were large, the chances of converging on the global minimum were much lower, even when the population size and number of iterations were doubled. But comparing the features of the network solutions with the spatial similarity index revealed that they were still similar in nature, and that the IO solution was very close to the best solution found by the GA for both the winter month and the summer month.

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Phrases like "The standard design assumed that there was zero variance from the ocean sources as we wished to emphasize the importance of the terrestrial uncertainties in the network design.", "The resolution of the spatial grids should be in line with the number of stations added to the network and the size in subregion for which fluxes could be estimated over the domain of interest given the available number 5 of stations.", or "if the objective of the network is to reduce the overall uncertainty for a large area, like South Africa, having a high spatial resolution for the network may result in an over-concentration of sites in high activity areas, leaving large parts of the country undersampled." indicate a misconception: The setup of the flux inversion must include the main sources of uncertainty in the system (including aggregation error) instead of being driven by the desired outcome.

Response: As alluded to by Referee 1, Referee 2 is requesting that we do not allow our preconceptions of the optimal network interfere with the interpretation of the results. We have ensured that the interpretation of the network solutions from the different sensitivity analyses are based on statistical comparisons, and the discussion now pertains to these results. As also mentioned by the Referee1, we have included an assessment of aggregation error (Kaminski et al., 2001), and rerun analyses where applicable.

There are many strange expressions. I only list a few examples:

"The magnitude of the boundary condition to each potential observation site..."(abstract)

Response: The Referee stated "condition", but what was written in the manuscript is "contribution". What was meant here is that we determined the size of the boundary contributions to the posterior flux covariance matrix, where each potential observation site was considered.

"Since the transport model is not assigned a covariance matrix, the uncertainty is transferred to the observations".

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Response: What was meant here is that, due to the known problem of modelling night time atmospheric transport, the covariance matrix of the observations was adjusted in the sensitivity analysis to consider larger night time observation errors. This follows from the development in Tarantola (2005) (eq. 1.101) showing that model errors can be added to observational uncertainties in the case of Gaussian errors.

"The actual measurement uncertainty at the sites has a much smaller uncertainty.."

Response: What was meant here is that a conservative estimate of the observation uncertainty was used.

General response: In the re-writing of the manuscript we have endeavoured to avoid any ambiguity, and ensured that all terminology is used correctly and consistently.

References:

Kaminski, T., Rayner, P. J., Heimann, M., and Enting, I.G.: "On aggregation errors in atmospheric transport inversions", J. Geophys. Res., 106, pp. 4703-4715, 2001.

Patra, P. K. and Maksyutov, S.: "Incremental approach to the optimal network design for CO2 surface source inversion", Geophys. Res. Lett., 29, 1459, doi 10.1029/2001GL013943, 2002.

Tarantola, A.: Inverse Problem Theory and Methods for Model Parameter Estimation, Society for Industrial and Applied Mathematics , Philadelphia, 2005.

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