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Atmospheric Chemistry and Physics Discussions

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.

Anonymous Referee #2

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General comments

The manuscript presents a model study on the design of a network sampling atmospheric CO2 to constrain the CO2 surface fluxes over South Africa. The network design exercise consists in adding five sites to a base network of two existing sites. This instrumentation for these addiditional sites has been purchased, i.e. there is some practical relevance of this study. The study also investigates the sensitivity of the optimal network with respect to choices in the setup, which is very interesting.

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

to read. In part this is due to constantly changing or wrong terminology. For example, on p 11306 Cf0 is correctly introduced as “prior error covariance matrix”. Later (e.g. in the title of section 2.2) it is often referred to as “prior covariance matrix”. At the bottom of this there is a confusion of objects. For example, on p11309, line 3, authors write “fluxes are not independent”, when they mean their errors are not independent. In the abstract just “covariance matrix” is used. At other spots the term “uncertainty” is used for “error”. Another example are the expressions for “T” of Equation (1), which keep on changing between “Jacobian”, “transport matrix”, “influence function”, and “s-r relationship” throughout the manuscript. Then there are strange phrases (see detailed comments for examples) and many typos. Also the organisation of the material is unfavorable.

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 C_b 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 C_b 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 C_b.

The presentation of the test of the influence of errors in the ocean fluxes is incomprehensible. Is the 1kg C/m^2/week used for the diagonal of C_B of Equation (8)? In which case it would be the (square of) the error instead of the source.

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.

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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 aggregaton error) instead of being driven by the desired outcome.

In summary, the paper needs to be rewritten and the above methodological problems need to be addressed. It may be that more problems become apparent when the presentation is more comprehensible.

Detailed Comments

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

- “The magnitude of the boundary condition to each potential observation site...” (abstract)
- “Since the transport model is not assigned a covariance matrix, the uncertainty is transferred to the observations”.
- "The actual measurement uncertainty at the sites has a much smaller uncertainty ..."

Just two further (of many possible) comments:

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Equation (3) or (4) is sufficient while Equation (2) is not needed, and may even confuse the reader, because this study does not minimise J(f) but the cost functions in Equations (9) and (1).

p 11306, line 24: Could rather reference Rayner et al., Tellus, 1996.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 11301, 2014.