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Interactive comment on "Adjoint inverse modeling of a CO emission inventory at the city scale: Santiago de Chile's case" *by* P. Saide et al.

Anonymous Referee #2

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The paper Adjoint inverse modeling of a CO emission inventory at the city scale: Santiago de Chile's case by P. Saide, A. Osses, L. Gallardo, and M. Osses addresses the problem of emission rate estimation by inverse modelling in general, and the specific problem of collocation of sources and observation sites. The topic which is addressed in this study is of utmost importance, as emission inventories and emission estimates form the basis of air quality modelling and control. The authors use a variant of the three-dimensional variational approach to address this problem.

General remarks: The general design of the paper is good and many parts of discussions are instructive, Especially the often missing discussion of meteorological effects is laudable.

Regrettably, the paper has shortcomings in demonstrating the value of specific parts of

the methodology, namely those addressing the "co-localisation problem". The straight forward approach to solve this problem appears to be the specification of those off-diagonal elements in the (multivariate) emission error covariance matrix B, which describe spatial correlations. Further, the effect to confine optimised emission rates to the grid cells with observations can be overcome by extension to spatio-temporal inversion techniques, which link cause (emissions) and effect (observed concentration levels) by modelling. It appears, that in this sense, the approach presented in this study could be regarded as a complexity reduced technique with simplifications from more complex algorithms.

Overall, a publication may be reconsidered after major revisions.

Specific remarks: Abstract and Page 6339, beginning of 2nd paragraph, also to be considered introduction of factor matrix F, page 6335: In particular, the central role of a proposed F factor matrix, to be multiplied with the background error covariance matrix, is pointed out. This method appears to be inspired by Issartel et al 2007, but is claimed as own development. The description includes elements of implementation, however, a mathematically rigorous optimality criterion and an associated algorithm to achieve this is lacking. The authors should add a proper description, justification and derivation of their method. Also the abstract, when addressing this issue, is unclear on what will be provided in the paper.

page 6331: Inflow boundary conditions are really zero, without any background values? If so, how is it justified?

Description 4.1.1 : The description of the sensitivity matrix H calculation is unclear. Basically, the direct model calculates the forward sensitivity, and not the backward, as the adjoint of it, and hence both cannot serve the same purpose, (except perhaps for applications when linked by scalar products). The approximation of the adjoint by calculation of the wind field backward in time can be taken as approximation for the adjoint, but how this is finally used in an inversion algorithm, which basically needs

both, the forward/tangent-linear and the adjoint, is not made clear. Also, the figure caption Fig. 7 does not help and should be reformulated accordingly.

Page 6337: The description of the L-shape method is insufficient. It should be noted that the quantities calculated with Eq. (10) are not error terms, as they are not the basis for calculating the error variances ïĄşobs/par, but the logarithms of inverse error covariance weighted squares of analyses-minus-background discrepancies. The plausibility of the result presented is difficultto assess. So, given reasonable estimates of R and B, the results of Eq. (10), prior to logarithm application, should be of order of the number observations (trace of the unit matrix in observation space) and of order of the number of horizontal grid cells (trace of the unit matrix in surface grid space) (for further explanation, if needed, see a posteriori validation papers from Talagrand 1998 (ECMWF proceedings) or div. papers of Desroziers et al.) . The plausibility of the values of ïĄą (from Eq. (9)) given in Figure 8, and the final result of 1.4e+7 should be demonstrated by means of discussion of observation and emission rate errors. Further, the related Figure 8 axes should explicitly express logarithmic scaling, as is used.

Page 6340, first paragraph of 4.2: Typically, the complexity of variational data assimilation algorithms, is not dominantly dependent on the number of observations and not controlling the computing time dominantly. Also the description of a reduced inventory should be reformulated, to make it clearer.

Page 6342, 1st line: what is the “time average of the morning/afternoon hours” ? Please make clearer.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 6325, 2009.