**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Regional-scale geostatistical inverse modeling of North American CO$_2$ fluxes: a synthetic data study" *by* S. M. Gourdji et al.

Anonymous Referee #2

Received and published: 11 January 2010

This study investigates the optimal set-up of a geostatistical inversion system for the estimation of regional sources and sinks of CO2. It is a very systematic, statistically sound piece of work. The experiments are highly relevant steps that need to be taken before a system can be exposed to real data. In this sense the methodology could serve as a good example for other studies. However, it remains at a rather technical level. It is difficult to get a feeling for the importance of the results, the extent to which these findings were to be expected in light of previous studies and how the performance of the optimal set-up compares with what others have published before using different methods. More emphasis on these aspects would make it more useful to a wider group of scientists than experts in geostatistics only. Several of the comments and sugges-

tions made below are meant to make the manuscript easier accessible. I personally experienced some difficulty understanding the terminology. Some more work will be needed in these directions as explained in further detail below. Besides this, however, in my opinion there is no fundamental problem that should prevent final publication.

GENERAL COMMENTS

It is difficult to judge the overall performance of the inversion without definition of a requirement for a regional inversion for Northern America from the perspective of carbon cycle research. Right now only some vague suggestions are made that this simple set-up is performing reasonably well already. It is not easy to judge the value of an estimate within 10% for North America on the basin of an inversion, that, if I understand correctly, only represents a temporal aggregation error on the side of the data uncertainty (it was not clear to whether or not the results in figure 6 and 9 included transport error, which makes me assume this is not the case).

In the abstract it is mentioned that the results are likely applicable to other inversion methods. It is not clear why this should be the case. Actually it is not even clear how robust the findings are to the specific set-up that is chosen for the presented geostatistical inversion. For example, Figure 5 indicates that the relative performance of the cases depends quite a bit on the treatment of transport model uncertainty. If those errors are accounted for the difference between the cases doesn't look that big. What about its significance? The question arises how relevant the discussion on flux and data aggregation really is for a real-world application in which systematic transport model uncertainties are likely to dominate the error budget. The question of robustness is important because it is chosen to perform these simulations in a simplified inversion set-up.

On page 21 it is mentioned that transport errors destroy the skill of the inversion in estimating the Q covariance parameters. How then do you explain that the posterior uncertainty and the extent to which it represents the true uncertainty is almost not

affected by accounting for transport model error (as shown in table 5)?

SPECIFIC COMMENTS

Page 9, equation 4: How do you obtain beta-hat?

Table 1: This table doesn't provide much information. The text lists several more differences between the inversion cases than mentioned in the table. (such is a longitudinally varying drift factor, difference in the number of drift factors etc). It would be useful to have a table that summarizes all the differences between the inversion cases.

Section 3.1.2: The first paragraph mentions that the skill of the inversion to recover the temporal aggregation error and transport model error is tested. In didn't find that back anywhere in the results. Table 4 and 6 list inversion inferred transport errors. From the text I learned that this is the estimate refers to the "true" transport error (which I think should have been mentioned in the table caption too). Also in the caption of figure 3 and 4 it helps to remind the reader whether we are dealing with inversion recovered or "true" aggregation errors and that these are errors in the simulated concentration due to temporal aggregation of fluxes.

Table 5: It is mentioned somewhere in the text, but it helps to remind the reader in the title that the numbers refer to 8-day fluxes.

Page 26, line 25: What is the meaning of a negative transport error? This should briefly be clarified.

Page 23, line 1: "discussed, . . . shown in Figure 3" I see no "non-afternoon measurements" in Figure 3 . . .

TECHNICAL CORRECTIONS

Page 23, line 2: 'except for with exceptions'

Figure 3: 5 instead of 6 cases

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 22407, 2009.