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Comment

Interactive comment on “Regional-scale geostatistical inverse modeling of North American CO₂ fluxes: a synthetic data study” by S. M. Gourdj et al.

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General comments

The paper by Gourdj et al. presents a range of synthetic data inversions for continental North America using a geostatistical method. While the case presented is rather limited in its scope (only one summer month and one network of sites), the paper endeavours to identify general principles from this that might be applied in a wider range of cases. I am concerned that the paper is too detailed and therefore too long, with the result that the most important findings tend to get lost. I would make two suggestions that might allow the manuscript to be reduced in length by 20%.

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Interactive Discussion

Discussion Paper



(1) The authors remove some material that they consider to be less important. It seems to me that each of the tests they have performed is valid, but perhaps not everything needs to be presented? Deciding what can be left out of a paper is often as important as deciding what needs to go in a paper. Perhaps it is sufficient to just note that some of the tests were performed, with a single sentence to describe the main result. I realise this is already done to some extent, such as for the cases with and without night-time data, but I think there may be other places where a shorter presentation of the results may be justified. Some of the findings I thought were most interesting were the difficulty in estimating the spatial covariance when transport error was included, the impact of including temporal covariance in improving the uncertainty estimates and what seemed like rather poor results at the ecoregion scale compared with the earlier metrics used to assess the inversion. The authors may agree that these are worth highlighting or they may want to choose other points to focus on.

(2) The paper might read more easily if the results of each test were presented along with the description of that test, e.g. sec 3.1/3.1.1 followed by sec 4.1, sec 3.1.2 followed by sec 4.2 etc. This allows a reader to only focus on one set of tests at a time, and may mean that some repetition in the text can be removed as readers no longer need to be re-introduced to a topic etc.

Specific comments

I think the introduction could be reduced in length e.g. previous work might be described more briefly (especially p22410-22411). Is all the GIM description needed in the introduction since some is covered in the methods section as well? The last paragraph of the introduction also seems to repeat some points that have already been made.

Given that the test case is for a relatively short period (1 June to 8 July 2004), it might be good to comment on the feasibility of the method for longer periods in relation to calculating the trajectories (p22416, line 26-28) as well as for the inversion itself. Per-

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haps in the final section you might also want to speculate on whether the inversion performance might be expected to be different for winter than summer.

The diurnal variation in the covariance structure of the 3 hour fluxes seems interesting (p22427). Given that the night-time variance is lowest, does this imply that the potential disadvantage of night-time sampling i.e. the smaller footprint, might be offset by the fluxes being representative of a larger area at night? Do you think this would be the case for the 'real world' as well as the CASA model world?

As noted above, the inability to estimate the spatial covariance parameters when random transport error was included seems to be rather a concern (p22427). Would this be worse for non-random transport error (which could easily be the case in the real world)? Do you have any evidence for the suggestion that more data points would improve things - given that with your different temporal aggregations, you have quite a large range in the number of data points that you are currently using? If more data points doesn't help, what other options do you have to estimate these spatial covariance parameters?

The first paragraphs of sec 4.4 and 4.4.1 seem to say similar things. Try and combine the information. Perhaps the first paragraph of Sec 4.4 should just say that three refinements were tested ... spatial covariance parameter choice, adding temporal flux covariance and allowing model-data mismatch to vary across sites.

Figure 9 and associated text (p22434). The text and figure caption says that the mean north american flux is on this figure, but it doesn't seem to be. These ecoregion results seem rather disappointing - to have only half the region estimates to be within 2 sigma of the true flux would suggest that the flux uncertainties are too small. The sentence (line 22) "However, among the two cases, ..." doesn't seem to agree with the figure. To me Fd looks closer than F8d to the true flux for the desert region and in the tropics both inversions seem to be more than 2 sigma from the true flux. Figure 9 is the first time that any absolute uncertainty numbers are presented - I wonder whether a root mean

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Discussion Paper



square uncertainty would make a good comparison with the RMSEs presented in Fig 5?

p22436, line 9-17: the improvement from using separate model-data mismatches seemed fairly small to me.

p22437, line 1-14: I'm not sure whether this discussion of the Law et al. results is really needed here. The Australian case is more dependent on coastal sites which could behave rather differently to the continental sites that you are reliant on.

Technical corrections

p22414, line 4-6. Suggest removing sentence "The naming convention ..."

p22414, line 7: Suggest adding "(grid-scale)" after $1^{\circ} \times 1^{\circ}$

p22414, line 13-14: Suggest removing sentence "For the remainder of this paper .."

p22416, line 13-14: Remove "which are maintained Administration."

p22416, line 19-21: Remove ", or other readily ...noaa.gov)"

p22416, line 28: Suggest adding "these" after "integrating"

p22431, line 1: check sec number, need 4.4.1 not 4.3?

p22431-22432: check sec numbers - 4.2 should be 4.3? in a couple of places?

p22432, line 25: add "(0.0 in Table 6)" after negative (if this is true)

Figure 5: I suggest extending the y axis to 0.00. As plotted, the difference between cases is overemphasised compared to the total error.

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

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