**Biogeosciences
Discussions**

Interactive
Comment

Interactive comment on "Structure of the transport uncertainty in mesoscale inversions of CO$_2$ sources and sinks using ensemble model simulations" *by* T. Lauvaux et al.

Anonymous Referee #1

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General Comments

This manuscript addresses an important part of the error budget in inversions of CO2 sources and sinks. The inversion community has always realized that transport error is significant, but little has been done to actually estimate this error component. This study is therefore highly relevant and presents a good effort to estimate at least part of the transport error component and especially the error correlations. I recommend this paper for publication, but would like to see the items below properly addressed.

Specific Comments

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Page 4814, line 7: This sentence is formulated a bit awkwardly. Please consider rephrasing it. My suggestion would be '... meso scale model (Meso-NH) simulations, which uses CO2 fluxes from the ISBA-A-gs land surface model.'

Page 4814, line 10: I would like to see a sentence here saying that only part of the model error is considered in this study. As said later in the manuscript, the model error of the meso scale model itself is not taken into account.

Page 4814, line 13: Why was the ensemble size taken as small as it is? Is computational cost really the only reason? Do the authors have an idea how large the ensemble has to be to avoid the problems with noisy error statistics?

Page 4815, lines 3 - 4: Please be consistent with your use of brackets; also, it is better readable when the tracer is named before the reference.

Page 4815, line 14 - 16: This sentence is not very clear. Could the authors please rephrase it.

Page 4815, line 17: I wouldn't say 'relatively simple'. The estimation of prior covariances is still a big challenge. For surface fluxes we only have local observations that are of good enough quality to represent the truth in limited areas of the world. Over the ocean, there is very little. This makes it especially difficult to estimate the error correlations. In my opinion, the estimation of prior flux errors needs at least as much attention as the estimation of the transport model errors.

Page 4815, line 22: Use 'estimation' instead of 'description'

Page 4816, line 22: I am not entirely convinced that the forecast error growth is of importance in this context. Inversion transport models are usually constrained with NWP analyses that are available every 6 hours and not with medium-range forecasts. Therefore, it is the uncertainty in these NWP analyses that really counts (apart, of course, from the intrinsic model error of the mesoscale model itself).

Page 4817, line 1: What does the acronym PEARP mean?

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Page 4817, line 3: I guess the authors mean 'in the Discussion' instead of 'at the output'.

Page 4818, line 18: Coming back to my previous comment, why do the authors use 102 h forecasts from the Arpege model instead of the analyses? Common practice for flux inversion models is to use analyses or short-term forecasts (up to 12 hours) to constrain the transport, not long forecasts that are not fully constrained by meteorological observations. Generally, the large scale advection is well constrained by observations and therefore well represented in the analyses. A more significant model error comes from the parameterizations of vertical mass transport (both in the global analyses and the meso-scale model itself) and the representation error of the used model resolution (especially, in areas where the orography plays a significant role). Unfortunately, the authors have chosen not to estimate these more intrinsic model errors.

Page 4820, equation 1: The variables in this equation are not fully explained in the text. What is for instance the difference between M and Mx?

Page 4820, line 21: The title of this section is not very clear. The authors probably mean something like 'The model part of the observation error covariance'.

Page 4821, line 10: What is 'ergotic'?

Section 2.3: I got lost in this section. This is probably partly due to my lack of knowledge of the specific details, but I hope the authors can find a way to better explain what they do and why.

Page 4822, line 17: Do the authors have an idea what the effect is of ignoring negative correlations. Especially in time, one would expect significant negative correlations (the authors actually show that in their plots as well). Does this not limit the validity of the approach?

Page 4825, line 20: Please add 'During the day' at the beginning of the sentence.

Page 4827, line 6: Do the authors have any ideas to explain the differences?

Page 4829, end of Section 3.5: The error reduction depends significantly on the prior error distribution, but there is no reference to that at all. How do for instance the prior errors over ocean compare to those over land. This is important for better understanding the presented results.

Most figures are not very well readable. The fonts are small and often not very clear. Also, the coast lines are not very clear. It would help if these could given a different colour (maybe white) and be a bit thicker. It is important for better understanding the discussion about orographic effects.

Technical Comments

Unfortunately,the spelling is rather poor. After a few pages I stopped correcting these many (mostly small) errors. Could the authors please go through the manuscript themselves and correct these mistakes.

Interactive comment on Biogeosciences Discuss., 5, 4813, 2008.

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