Biogeosciences Discuss., 5, S3270–S3273, 2009
www.biogeosciences-discuss.net/5/S3270/2009/

Biogeosciences Discussions

BGD

5, S3270–S3273, 2009

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 #2

Received and published: 5 March 2009

The contribution by Lauvaux et al. address an important topic. Transport uncertainty has been shown by numerous authors to be the main barrier to determining carbon sources/sinks from CO2 concentrations. However, I have some major questions regarding the adopted methodology that I would like to be addressed before the paper is published.

MAJOR COMMENTS

1) The paper presents a methodology to quantify transport errors by perturbing synoptic boundary conditions to drive mesoscale models. However, as the authors have correctly pointed out in the paper, generating ensemble realizations of the same model

by no means guarantees that the ensemble is an adequate characterization of the transport errors. In the Introduction, it is asserted that "In the absence of further data to demonstrate this we are forced to treat the statistics of an ensemble of models as if they are the statistics of the difference between the model and the truth."

I disagree that there is an absence of observations to characterize uncertainties in the transport model. Why not use direct observations of transport fields (from, e.g., radiosondes and wind profilers) to derive uncertainties in the model? This, in fact, has been carried out before. An example is: "Vertical mixing in atmospheric tracer transport models: error characterization and propagation" by Gerbig et al.: Atmos. Chem. Phys., 8, 591-602, 2008. In this paper direct comparisons between transport models and transport observations were presented, including covariance lengthscales–a key result that the current manuscript attempts to derive. Such a paper needs to be referred to.

2) It is unclear exactly how the ensemble perturbations from the global model (ARPEGE) were coupled with the boundary condition of the mesoscale model (Meso-NH). More details are necessary here. Are the Meso-NH state variables being nudged toward the ARPEGE values at the boundaries? Is a "nudging coefficient" being used? If so, what is its strength? Also, are there "sponge layers" at the domain boundaries to prevent reflections? An equally important question: could any of the "transport errors" be an artifact of the physical inconsistencies between the two models?

3) As indicated by Fig. 1, the ensemble dispersion grows with simulation time. So wouldn't the "transport error", as suggested by the methodology, also grow with simulation time? If so, why was an arbitrary cut-off timeframe of 4 days chosen? Wouldn't the "transport errors" and their correlation structures as derived in the manuscript be sensitive to how the authors chose the timeframe?

4) Since the variance of transport error is mostly underestimated by the manuscript's method, the key result would be the covariance characteristics. The covariance structure is affected by the Gaussian diffusion operator, as can be seen by contrasting Fig.

5ac versus Fig. 5bd. While I can understand that a small ensemble is statistically noisy and requires a smoothing operation to reduce the noise, I am concerned that the retrieved covariance lengthscales, for instance, are partially artifacts of the Gaussian diffusion operator. Can some of the error structure be smoothed out? Can a stronger justification be presented, other than that the mountains showed up more clearly?

5) The abstract does not currently contain a statement specifying that the variance as represented by the methodology is underestimated–a key result that the authors mention in the Discussion. In fact, the ensemble spread in the nighttime (Fig. 1b) does not appear to include the "true" observed CO2–a clear indication of the underestimation of variance. This should be pointed out.

6) Fig. 1b is an indicator of the *systematic* (rather than random) error existing in the model. It appears that the manuscript did not attempt to grapple with systematic errors. Yet as many groups attempting to simulate high-frequency (hourly) CO2 concentrations have shown, the failure to capture the nighttime CO2 buildup is a systematic error in pretty much all of the current transport models. How would the methodology deal with systematic errors?

MINOR COMMENTS

1) Introduction: "In atmospheric transport inversions the model contribution usually dominates the measurement error...". Should cite references to make this point.

2) Sect. 2.1: Why are there only 11 ARPEGE simulations, if there are 16 singular vectors?

3) Sect. 2.2, Eq. 1: What is the variable "x"? Please explain.

4) Conclusions: "There are strong vertical correlations in the boundary ayer, particularly at night." It appears from Fig. 3 (RHS) that this should be the daytime. Is this a typo?

BGD

5, S3270–S3273, 2009

Interactive Comment

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

BGD

5, S3270–S3273, 2009

Interactive Comment