

Interactive comment on “Bayesian statistical modeling of spatially correlated error structure in atmospheric tracer inverse analysis” by C. Mukherjee et al.

Anonymous Referee #2

Received and published: 6 April 2011

The paper discusses ways to implement spatially correlated observation errors, as well as non-Gaussian a-priori flux probability densities, in atmospheric trace gas inversions. For demonstration, the method is applied to an inversion of carbon monoxide measured by the MOPITT satellite.

Both correlated data errors and non-Gaussian priors are relevant and open issues. Therefore I think this is an interesting contribution. I would like to recommend this paper for publication, though I feel that some revision is needed to make it more accessible to practical application, as suggested here:

The example used to demonstrate the method is a case with very few adjustable de-

C1542

grees of freedom. How feasible is the method also in cases with many degrees of freedom? This question is relevant because satellite inversions seem to be more common with many degrees of freedom such that the high spatial resolution of the data can actually be exploited, which is not the case for the example in this paper.

In addition, I'm not sure whether the “simply overwhelming statistical evidence” (p 1687 line 6) is actually a property of this example case with few degrees of freedom, and would look very different in a more relevant case with more degrees of freedom.

The covariances of the model-data error implied by the presented method correspond to a simple decorrelation with growing distance. How realistically does this describe the actual errors of satellite retrievals and/or model simulations? A discussion of this point seems to be essential, as it touches the applicability of the method.

In many places, the paper is written in rather complicated sentences (e.g. p. 1676 line 29, p. 1677 line 9-11, and many more), that are difficult to follow. There are also many filling words, and repetitive argumentation, which should be cut.

The motivation in abstract and introduction is very general and vague. It would be much more useful (and also make a more appealing entrance into the paper) if it would describe specifically what the paper will be about.

Minor comments:

Just for interest: In the synthetic tests with known L , how does the a-posteriori estimated correlation length (i.e., that implied by the estimated ϱ and τ_c) compare to the known L ?

Eq (4): I suggest to write the condition in curly brackets, $\epsilon_i | \{\epsilon_j, j \neq i\}$, to indicate that it is the set of all other j .

p 1681 line 2: avoid “common” because there are many other choices in the literature.

p 1681 line 6: Explain “indicator function”.

C1543

p 1681 line 21ff: Why didn't you just calculate the missing lines of K from the CTM?

p 1682 line 2: "de facto standard" - this may be the case in certain fields but certainly not in biogeochemistry.

p 1684 line 1: What is the 20

p 1685 line 8: Shouldn't "LearningRatio > 1" always be the case by theory?

p 1686 line 3: Probably can drop "logged".

p 1687 line 12: Where does the noise come from here (prior fluxes, transport/chemistry)?

p 1690 line 2: Has τ_n been explained anywhere?

Figs 7 and 8: Maybe 2 values of L are enough.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 1671, 2011.