Atmos. Chem. Phys. Discuss., 13, C12476–C12478, 2014
www.atmos-chem-phys-discuss.net/13/C12476/2014/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Characterization of uncertainties in atmospheric trace gas inversions using hierarchical Bayesian methods" *by* A. L. Ganesan et al.

Anonymous Referee #2

Received and published: 26 February 2014

This study considers methodological aspects of the estimation of trace gas fluxes from atmospheric measurements. The a-priori probability distribution in the Bayesian framework is not fixed but partially adjustable through hyper-parameters. Unlike previous studies, these hyper-parameters are adjusted simultaneously with the unknown fluxes, considering a chain of conditional probability distributions. To my knowledge, this is indeed a clean way to implement this concept. In my opinion, this is an interesting step in atmospheric trace gas inversions.

However, I see several important caveats not mentioned in the text:

- Even though some correlation length scales and variances are not fixed from "expert knowledge" in this work, the functional form of the a-priori covariance according to Eq (6) is still as simplistic as in most "traditional" studies. Of course, these simplistic forms are generally chosen because the full complexity of the "true" covariance is difficult to handle, but choosing a simple form means that it cannot actually be expected to become substantially closer to reality, thus heavily limiting the achievable gain from varying hyper-parameters.

- If I understand correctly, the MCMC run of Sect 2.1 comprises 50000 calls of H*x, which is a computational demand more than 2 orders of magnitude higher than in most published atmospheric trace gas inversions studies. For many (if not most) regional or global applications, this will be prohibitive.

- Though I agree that missing information about the a-priori PDF represents a major problem in atmospheric trace gas inversions, there are further problems with potentally larger impact, in particular errors in the modelled transport, and undersampling by the available data. I feel that potential improvements through the presented method need to be set into the context of these remaining problems. For example, biases in transport cannot possibly be detected by any hyper-parameter, and therefore represent an additional and potentially limiting error in the results.

While I still think that this is a worthwhile study and should be published in ACP, given these caveats I do feel that the text substatially overrates the gain of the proposed method. The revised version should openly discuss these caveats, and already clearly name them in the abstract.

Minor comments:

- Eq(1): All symbols should be riefly explained. Also mention that x,y etc are vectors.

- p33406 line 15: Even if the "NHB" calculations are much less costly than "HB", many applications are certainly not of "low computational expense" depending on number of

data and unknowns (see caveat above).

- p33406 line 18: The statement "the derived fluxes . . . strongly depend on these parameters" is true in many applications where data are sparse, but is not true in well-constrained situations. This means it is not a feature of Eq(2) per se.

- p 33410 line 5: Similarly to the comment above, the assumption of Gaussian model errors is certainly a great simplification of reality.

- p 33411 line 19: I may have misunderstood, but isn't rho(x | mu*, sigma*) the "true distribution" and not rho(x|y)?

- p 33412 line 8: Formulation "shown trough Eq" is unclear.

- Sect 2.2: How did the actual emission estimates (as the primary result) compare to the truth?

- p 33413 line 11: Formulation "monthly diurnal" is unclear.

- p 33415 lines 19-22: This sentence is not fully clear to me.

- The assumption of constant SF6 fluxes is a potentially problematic one - if real SF6 fluxes vary within the month, estimates will have biases not accounted for in the error estimate (aggregation error).

Fig 4: The inset should be described if present, but I rather feel the figure would win clarity (and not loose information) if the inset would be removed.

Typos:

- p 33413 line 23: due to

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 33403, 2013.