Atmos. Chem. Phys. Discuss., 15, C5475–C5478, 2015
www.atmos-chem-phys-discuss.net/15/C5475/2015/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Constraints on methane emissions in North America from future geostationary remote sensing measurements" *by* N. Bousserez et al.

Anonymous Referee #1

Received and published: 30 July 2015

The paper presents an application of the method developed by Bousserez et al. (2015) for Bayesian posterior uncertainty quantification. The paper is well-written and contains some interesting parts, but a series of simplifications severely limits its value. For instance neglecting correlated model errors for the assimilation of profile retrievals makes the whole discussion about the multi-spectral instrument useless. The other results alone are not enough to populate a paper. Another example is the test about boundary conditions: assuming that their uncertainty results in a single continental offset for the whole period does not look like the real world. More details are given hereafter.

Detailed comments

1. p. 19018, l. 14 and elsewhere: why is there an "s" at the end of DOF when the plural is not used? Also note that the DOF is defined again in p. 19023 and 19026.
2. p. 19021, l. 5-10: the authors suggest that nobody has used Monte Carlo or numerical approximations of the Hessian because of their "prohibitive" cost, but looking at the results shown by, e.g., Meirink et al. (2008) or Cressot et al. (2014) with them, such approaches look straight-forward.
3. p. 19022, l. 6: providing -> provided.
4. p. 19023, l. 16: why is **B** diagonal? I understand that this conveniently simplifies the algorithm but the authors should explain why it makes physical sense. Why would the diffuse emissions seen in Fig. 1 have uncorrelated prior errors every $\sim$ 50 km? I note that the two references above used a 500 km e-folding correlation length.
5. p. 19024, l. 19: Does the 40% relative error apply to grid cell emissions or to the whole domain? Does this number correspond to 1 or 2 σ ? In any case, the authors should clearly indicate the monthly error budget integrated over their domain and give some indication of its realism. This point is particularly important for a study of uncertainty reduction.
6. p. 19025, l. 1: The authors assimilate profile retrievals. For such a product, model errors are highly correlated between levels and accounting for them is critical (which actually explains why everybody assimilates columns as far as I know).
7. p. 19025, l. 3-6: the two sentences should be developed to better explain what the authors have used.

ACPD
15, C5475–C5478, 2015

8. p. 19025, l.18: The authors write “This value [8ppb] is consistent with GOSAT column errors reported in Parker et al. (2011).” The reader may guess that the value corresponds to 1 σ, but in this case the link with Parker et al. is weird. Parker et al. actually write: “from comparisons to TCCON observations we have inferred a single sounding precision for our CH_4 retrievals of 0.4 – 0.8% with estimated biases between -17 ppb and 2 ppb (0.1 to -0.9%)” (their §32). Basically the authors have taken the smallest value in the range for the standard deviation and have neglected the large biases reported by Parker et al.

9. p. 19025, l. 22-25: the authors rightly warn the reader against model errors, but such errors are spatially correlated while the authors neglect observation error correlations (p. 19026, l. 10). Also note that retrieval errors themselves are correlated in the real world.

10. p. 19026, l. 17-18: for a given instrument, the retrieval errors vary with the satellite altitude. How is this dependency accounted for?

11. p. 19027, l. 17-19: this artifact and the accompanying remark suggest that the control vector is not defined appropriately.

12. p. 19027, l. 28-29: This claim is tied to the realism of the modeling framework and may therefore not be reliable.

13. Section 3.2. What about the initial state of the simulation? How is it accounted for here and what is the impact of a biased initial state? What happens with more realistic error structures (e.g., decoupled errors at the edges both in space and time)?

14. p. 19029, l. 25: the estimate may be mathematically rigorous, but not so realistic. The word “rigorous” is therefore not appropriate.

ACPD
15, C5475–C5478, 2015

References

Cressot, C., et al. (2014), On the consistency between global and regional methane emissions inferred from SCIAMACHY, TANSO-FTS, IASI and surface measurements, Atmos. Chem. Phys., 14, 577-592, doi:10.5194/acp-14-577-2014.

Meirink, J. F., et al. (2008), Four-dimensional variational data assimilation for inverse modeling of atmospheric methane emissions: Analysis of SCIAMACHY observations, J. Geophys. Res., 113, D17301, doi:10.1029/2007JD009740.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 19017, 2015.