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Interactive Comment

Interactive comment on "Sulfur hexafluoride (SF_6) emissions in East Asia determined by inverse modeling" *by* X. Fang et al.

Anonymous Referee #2

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This paper presents an inversion of SF6 emissions for the East Asian region using three monitoring sites. Although there are some major issues with the analysis, the paper is generally well written and the results seem mostly robust. My main criticism of the paper is that the presented advances could be considered quite small: a well-known inverse method is used to estimate emissions of a gas that has been well studied in this region. The main innovation, compared to previous studies, appears to be the use of two additional monitoring sites in the analysis. In light of this, I suggest that the authors could make some improvements to the paper, by addressing the following weaknesses in their approach and making the paper more focussed. I will provide minor corrections in the revised submission.

Major comments

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- The a priori emissions field used in this work seems to be primarily based on previous “top-down” emissions estimates that have already incorporated some of the same measurements (e.g. Gosan station, South Korea). The starting point for the Bayesian method used is that the observations are independent of the prior, but the chosen emissions do not fulfil this criterion.

- The authors apply a scaling factor to their emissions to address the apparent ‘step-change’ in derived emissions between the periods before and after the Gosan observations began. This approach is highly questionable, given that the sensitivity of the Gosan observations to the surrounding emissions field will be non-uniform, and potentially variable from year-to-year. Therefore, I would find any method to ‘correct’ for a lack of observations difficult to justify (indeed, if it were possible to do this, we wouldn’t need observations every year, and could instead extrapolate results from previous or subsequent years). If the uncertainty quantification is robust, the derived a posteriori uncertainties should accommodate changes in derived emissions before and after the addition of a measurement station (i.e. if there is an unphysical step change, it should be within the derived uncertainties). If this is not the case, I suggest the authors need to take another look at their uncertainty quantification.

- The assumption (section 4.7) that the sensitivity tests can be considered independent estimators of the “true” emissions field is very difficult to justify. For example, every test uses the same observations, many share the same a priori emissions, etc. It would be interesting if the authors could propose a different method for dealing with the influence of this type of sensitivity information on the derived emissions. At the very least it should be noted that these tests merely approximate an uncertainty in their methodology.

- When analysing the a posteriori emissions from some regions (section 5.2), year-to-year fluctuations are derived, or regional patterns of increase and decrease in neighbouring regions are noted. This looks like potential ‘dipole’-like behaviour. I suggest that the authors analyse the a posteriori uncertainty covariance to test for the presence

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of strong anti-correlations between parameters. If significant correlations are present, this would suggest that averaging or summing of correlated regions or time periods should be performed.

- The authors should justify the assumptions used in deriving the a priori uncertainties, which have a significant impact on the derived emissions (e.g. at the start of section 4.2 it is stated that the emissions scaling factor uncertainty is 0.5 and 1.0, with no justification). Furthermore, the method for estimating the (equally important) model-data mismatch uncertainty is not given.

- Given the amount of new material in the paper, I think it is too long in its current form. I would suggest moving much of the non-essential information to the supplement. In particular, some material covers well-known ground (e.g. Figure 4 describes the improvement in RMSE as the prior uncertainty is increased, which is an trivial outcome of any Bayesian inversion).

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 21003, 2013.