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Atmospheric Chemistry and Physics
Discussions

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

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We thank the reviewer for his/her very constructive comments. We are wondering, however, if the reviewer has looked at the most recent version (doi:10.5194/acpd-13-21003-2013), or perhaps only the version which was first uploaded (i.e. before it became a discussion paper)? Some things are strange, like the reviewer refers to Fig. 4 and section 5.2 which doesn't exist. We made substantial improvements before submitting to ACPD. We reply to these comments below and will modify the manuscript as outlined.

1) "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."

Reply:

The reviewer is correct in stating that the inversion method used is well-known. However, our study adapts this proven method and applies it to the problem of quantifying SF6 emissions in East Asia. The novel aspects and improvements over previous studies are outlined as follows:

(a) Better quantification of SF6 emissions from non-Annex I countries such as East Asian countries is crucial for better understanding the global budget of SF6. The increase of global SF6 emissions after 2000 was suspected to be driven by non-Annex I countries (Levin et al., 2010; Rigby et al., 2010). This study gives information on the contribution from SF6 emissions in East Asian to global totals.

(b) This study provides an independent validation/comparison of the reported national estimates based on the bottom-up approach. There are many bottom-up estimates for countries in this region, but adding top-down estimates is in urgent need.

(c) Emissions are estimated for 7 years, from 2006 to 2012 thus making it possible to examine the emission evolution in East Asia. Previous top-down estimates provide results for only one year or a short period, e.g., a maximum of 3 years (Rigby et al., 2011).

(d) A large number of sensitivity tests were performed in this study for the influence of the measurement network, the meteorological data, the inversion resolution, the inversion geometry and etc. We were thus able to identify the factors which had the strongest influence on the results and our uncertainty estimates should be more robust than more traditional error propagation methods alone would have allowed.

2) 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.

Reply:

Completely independent bottom-up estimates were used in the a priori emission estimates in our reference inversions for China, the Taiwan region, South Korea and Japan (Table S1 in our SI and shown below):

(a) Bottom-up estimates for China were derived from an SF_6 emission inventory specifically for China (1990–2010) (Fang et al., 2013)

(b) Bottom-up estimates for the Taiwan region were derived from Second National Communication of the Republic of China (Taiwan) under the United Nations Framework Convention on Climate Change (Taiwan, 2011).

(c) Bottom-up estimates for South Korea were derived from Korea's third national communication under the United Nations Framework Convention on Climate Change (Republic of Korea, 2012) and CDM project information (UNFCCC, 2012).

(d) Bottom-up estimates for Japan were derived from National Greenhouse Gas Inventory Report of Japan 2012 (GIO, 2012).

The reviewer may have been referring to the global a priori emissions estimates, which are indeed top-down estimates. Although at the global scale, this a priori estimate is not completely independent of the observations, at the scale of East Asia, it is independent as we have adopted specific bottom-up estimates for most East Asian countries.

3) 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.

Reply:

The "correction factors" used for East Asian countries in our original manuscript are close to "100%" except those for North Korea and South Korea. Originally we wanted to get rid of the influence of changes in the measurement network on the trend of national emissions. But it's not really important. As mentioned in the comments, the derived a posteriori uncertainties accommodate changes in derived emissions before and after the addition of a measurement station. The reviewer does have a point and therefore, in our revised manuscript, we will not apply these corrections.

4) 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.

Reply:

In each sensitivity test, the influence from one factor on the inversion results was examined, respectively. So each test uses the same observations and the same prior (except in the test for the influence of the prior information) as the reviewer states. It is not possible to calculate the true uncertainty, however, our tests examine the sensitivity of the results to certain assumptions and thus give a more realistic picture of uncertainty than that derived from the posterior error covariance matrix alone i.e. $A = (H^TRH + B^{-1})^{-1}$ where H is transport operator, R is the observation error covariance matrix, and B is the prior error covariance matrix.

In our revised manuscript, we will modify the text to state: "these tests merely approximate an uncertainty" in our methodology as suggested in the comment.

5) 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 neighboring 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 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.

Reply:

Thanks for this suggestion. We carried out the Significance Tests between the emissions of the different regions in China (the sample size is small with just seven data points). And results show that there are no significant anti-correlations between the emissions of these regions (see Table 1 in the supplement to this Reply letter; there are two positive significant correlations). We would say there aren't any anti-correlations and thus no dipole behavior.

6) 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.

Reply:

As for the assumptions (actually Section 4.2 mentioned in the comments is Section 3.2

in our manuscript), unfortunately, no information on uncertainties of the emissions in individual grid cells is actually available. The bottom-up emission inventories simply do not provide such information that could be used for error propagation or, in this case, sensitivity studies. Therefore we set approximately $\sigma_x^j = p*\max<0.5x_j,1.0x_{surf}>$ as the description of a priori uncertainties for each box. While this is certainly a subjective choice, it is one that has been used in many previous studies (e.g., Keller et al. (2012), etc.).

As for the method for estimating the model-data mismatch uncertainty, we adopted the same method as described by Stohl et al. (2009) and as used in the later study of inversion for East Asia (Stohl et al., 2010). Model-data mismatch uncertainties are determined as the root mean square (RMS) error between a priori model output and observation, averaged for each station. In our revised manuscript, we would add the description of this issue in Section 2.3.

7) 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 a trivial outcome of any Bayesian inversion).

Reply:

We are wondering if the reviewer has looked at the most recent version (in ACPD), or perhaps only the version which was first uploaded (i.e. before it became a discussion paper). Actually, before we submitted the paper for ACPD, following this reviewers' comments, we had already tried our best to shorten our manuscript from 11106 words in the original version to 9190 words (ACPD version), and moved two figures and one table into the Supplement. Some materials covering well-known ground were shortened and the corresponding figure (e.g. the original Figure 4 describing the improvement in RMSE as the prior uncertainty is increased) was moved to the supplement.

We believe that already the ACPD version has well addressed this point of the reviewer. However, we will further work on improving the quality of the presentation and on further shortening of the paper before submission to ACP.

Reference

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Development Mechanism (CDM), 2012.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/13/C8710/2013/acpd-13-C8710-2013-supplement.pdf

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