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Atmospheric Chemistry and Physics Discussions

Interactive comment on "History of atmospheric SF_6 from 1973 to 2008" *by* M. Rigby et al.

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We would like to thank the anonymous referee for their helpful comments. We will briefly outline our response and describe changes made to the manuscript following each comment, which is presented in italics.

Referee 2: "This paper extends the measurement record of SF6 concentration history by several years compared to Levin et al. (2010). Moreover, as pointed out by one of the 'Short comments' on their paper regarding the urgent need for regional source estimation by inverse modeling, this paper achieved that goal to a great extent. The paper is well written and suitable for publication after minor corrections and some additional checks as suggested below."

Author response: We thank the referee for the very positive evaluation of our work.

Referee 2: "p.13540, 2nd para: I tend to agree that EDGAR4 is more accurate than the UNFCCC reporting, if one takes some lesson from the ratio of fossil fuel CO2 emission ratio aggregated for the North America and the rest of the world. I think the author should put more faith on their estimation; otherwise value of the whole paper will be undermined and wouldn't probably warrant a publication."

Author response: We have estimated the uncertainties and covariances in our estimation of both global and regional emissions, and have attempted to state conclusions that accurately reflect these uncertainties. We have also modified the figure showing regional inversion results (see Figure 1 in short comment "Response to Dr I. Levin's Review"), and now feel that it more concisely shows the major conclusions to the work.

Referee 2: "It should also be possible to validate whether this estimation or EDGAR is right by comparing the model and observed concentration gradients between the sites in the latitude belt of North America around the globe. For example, if you put 0.6 Gg/yr source over North America, you would find MHD – NWR/THD differences by model at odd in comparison with observations."

Author response: This is essentially what the inversion does. If discrepancies are found between the modeled gradient between MHD and THD, then the model sensitivities are used to adjust North American emissions accordingly. The inversion solves this problem for every site simultaneously.

Referee 2: "Also how did Hurst et al. take in account the CO lifetime as a function of longitude, latitude and altitude? The mismatch in CO and SF6 lifetime could lead to large error in their estimation."

Author response: Hurst et al. (2006) treated CO and SF_6 as inert tracers over the 15

day back-trajectories calculated by the STILT model. This may lead to a small error in the derived SF_6 emissions as a result. The following sentence was added to the manuscript to highlight this:

"Potential sources of error could include biases in either transport model used, a bias in the EDGAR prior influencing our derived emissions, or errors in the CO inventory or assumptions about CO lifetime influencing the Hurst et al. (2006) estimates"

Referee 2: "In the 3rd paragraph of p. 13540, you mention that you do not have confidence on MOZART's capability in simulating SF6 synoptic variations. I think, some of the earlier studies you cite have already looked in to this aspect, and I strongly believe you should test the model synoptic variation using the continuous measurements. This should have been the first step before running the inverse model."

Author response: We have carried out extensive comparisons of the MOZART SF_6 with AGAGE and NOAA observations. These comparisons, as in the second inversion presented in the paper, do use continuous measurements, and are shown for each measurement site in the auxiliary material. We agree that previous studies found good agreement at some monitoring locations (e.g. Gloor et al., 2007). As our auxiliary material shows for the weekly continuous measurements, agreement is excellent at some locations (e.g. Mace Head, Ireland), but poor at others (e.g. Gosan, Korea). We find that the correlations between the model and the observations weaken at shorter timescales.

Referee 2: "p. 13524, l.1 : '. . .-ESRL flask. . .' Also give a citation to HATS group paper."

Author response: References to Dlugokencky et al. (1994), Geller et al. (1997) and Hall et al. (2007) are now included here.

Referee 2: "p.13535, l.15-17: It would be better to compare the % of NH emission for the same time period here and in Maiss et al. The reference to 100% in comparison with 94% a bit misleading. Could you separate the % of NH emission for two time periods?"

Author response: This sentence has been changed to:

"EDGAR places a higher percentage of emissions in the NH than previous estimates, being between 96% and 100%, depending on the year (for example Maiss and Levin, 1996 estimated a 94% NH source between 1978 and 1994, compared to an average of 97% in EDGAR during this period)"

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