

## ***Interactive comment on “History of atmospheric SF<sub>6</sub> from 1973 to 2008” by M. Rigby et al.***

**Anonymous Referee #2**

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This paper extends the measurement record of SF<sub>6</sub> 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.

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 CO<sub>2</sub> 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.

It should also be possible to validate whether this estimation or EDGAR is right by

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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.

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 SF<sub>6</sub> lifetime could lead to large error in their estimation.

In the 3rd paragraph of p. 13540, you mention that you do not have confidence on MOZART's capability in simulating SF<sub>6</sub> 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.

p. 13524, l.1 : ‘...-ESRL flask...’ Also give a citation to HATS group paper.

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?

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