

## ***Interactive comment on “Nitrous oxide emissions 1999–2009 from a global atmospheric inversion” by R. L. Thompson et al.***

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This manuscript has been very carefully prepared. It deals with sources and sinks of  $\text{N}_2\text{O}$  and assesses trends and variability over the period 1999–2009. As such it explores the added value of long-term monitoring of atmospheric mixing ratios. Understanding the climate variations that drive  $\text{N}_2\text{O}$  emissions is important to make reliable predictions of the future.

I have very few comments. One remark is about section 3.1. Here, the authors explore whether an emission pulse from the tropics can be picked up by the network. Figure 3 shows the atmospheric response to a positive and negative anomaly in the tropical sources. My objection here is that the inversion is driven by concentration gradients,

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and strictly speaking the 0.3 ppb perturbations in the concentration do not necessarily drive tropical flux adjustments. Similar perturbations to the background could be caused by flux perturbations at mid-latitudes. A more robust estimate comes from figure 4, the error reduction. Aggregated over the tropics (i.e. table 5) quantifies how well the tropical fluxes are constrained by the network. I would therefore suggest to leave out figure 3, and possibly add an inversion with some key tropical stations removed. One would expect e.g. less error reduction over Africa (compared to the reported 40–43%).

Another point I would like to raise is the role of stratosphere-troposphere exchange in the Southern Hemisphere (SH). On page 15710, line 13, the authors refer to unpublished work to increase the SH errors by 1 ppb. I would expect that this error is not random, but systematic (too excessive exchange bringing down low mixing ratios from the stratosphere). It would be good to present some of the unpublished work to better quantify the important effect of stratosphere-troposphere exchange: why is this not an issue in the Northern Hemisphere (NH)? Why is it not an issue with transport from the NH to the SH? What made the authors decide that this extra error was required?

A final point, which is probably hard to address: the error is based on a 1 year Monte Carlo analysis. Can this error be safely assumed for the other years also?

### **1 Minor issues**

Page 15701, line 4. Reference to Xu-Ri? Also  $T_g$  should be  $T_g \text{ a}^{-1}$  I guess.

Page 15701, bottom. I recently learned that if you use "secondly", "first" should be "firstly".

Page 15710, top. Here the errors for SH land were scaled by 0.66 .... to allow a greater reliance on the prior estimate. I do not get this. Smaller errors on the measurements

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means more reliance on the measurements? Also, how does this relate to the 1 ppb extra error?

Page 15718, 2 and 3: "low" should be "negative" and "high" should be "positive".

Table 1: unit Altitude is missing.

Figure 5: Only the mean is shown. Maybe it is worthwhile to add a similar map with the IAV?

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