Although the authors have dismissed most of my comments as being nonsense, they have reworked the paper substantially and improved the writing and structuring in such way that they answered most of my major concerns in the process. In particular, thank you for acknowledging the need for providing a methodology section, which now clarifies a few things for me, including the source of the e90 tropopause (a concept that was known well to me indeed, but not to the general ACP reader who may cross this manuscript, the sole reason I asked for this information to be added). I thus am happy to recommend this manuscript for publication after simply noting the following points.

1) Re: your answer to the following comment (which I don't repeat here due to its unduly length):

"L400-435 [of initial manuscript] Hegglin and Shepherd (2007) have used ACE-FTS O3/N2O correlations in an extensive comparison to a CCM, so should be cited here. This study reveals how sampling issues can affect the interpretation of tracer-tracer correlations using ozone. In particular, the ACE-FTS instrument exhibits a strong sampling bias with unequal sampling of seasons and hemispheres. Undersampling the full correlation space (since your monthly ACE-FTS data will not have sampled all latitudes in your considered latitude range evenly) is likely to impact your results. The differences (or even apparent agreement!) in the slopes between observations and your model may thus be at least partially explained by this sampling bias. A discussion of the limitation of your approach should thus be added."

As any reader of this comment can ascertain themselves, I was not asking for you to cite this paper for the quantification of STE. Instead, I wanted you to merely acknowledge the fact that the ACE-FTS slopes were used for model transport & chemistry evaluation before and that they were shown to be very sensitive to sampling. I noted in fact that your model slopes have changed from one version of the manuscript to the other without justification, an indicator for such sensitivity? Furthermore, anchoring the endpoints of the slopes in the tropospheric value (another addition to your revised manuscript I noted) was another methodological peculiarity highlighted by Hegglin and Shepherd (2007) but not referenced in your (rather informed) use of ACE-FTS data.

Yes, we agree and added a sentence reflecting their work on Line 433.Regarding the change in slopes in the revised paper, our lengthy effort on revisions found an error in how we calculated those slopes. The previous version incorrectly plotted N_2O kept at 320 ppb (with emissions) while the correct version now shows N2OX (no emissions) scaled to a fixed 320 ppb.

Inserted this Line 433

.... N2O in the lowermost stratosphere to establish the ratio of the two STE fluxes. **The ACE-FTS O3:N2O slopes were used for model transport & chemistry evaluation (Hegglin and Shepherd, 2007) and found to be very sensitive to satellite sampling, except in the lowermost stratosphere.**

Inserted this line 446

Our method described here for deriving the slopes from the ACE-FTS data is slightly different from that of Hegglin and Shepherd (2007; e.g., we do not anchor the tropospheric point), and we have the advantage of a longer record.

2) L714-728 Your statement that CMAM and observations agree seems not correct to me when looking at Hegglin and Shepherd (2007) Figures 4 and 13. These both indicate that N2O and ozone/N2O slopes in the model are too high when compared to the observations. Without a proper evaluation of the CMAM N2O flux estimates, this discussion seems thus on somewhat weak grounds. I realise here that it is not clear how the authors use N2O from Aura-MLS. Since v3.3 should only be used above 100 hPa (see SPARC report no 7, page 115) and while earlier versions may be more useful but still associated with much larger uncertainties in the LMS, this uncertainty should also be properly documented and accounted for in the discussion. Note, again what you exactly do with MLS is not clear to begin with since an appropriate description is lacking.

This section does not evaluate the N_2O abundances in the lowermost stratosphere, only the O3:N2O slopes, which we took from H&S Fig 13cd. H&S Figure 4 is absolute abundance vs latitude and this is not related to the STE fluxes. We realize that taking the O3:N2O slopes from their figure, especially considering the large seasonal range, and so we revised the numbers in Table 1 (used in this paragraph) to be a range: e.g., -23±2 instead of the overly precise -23.0. These are now moved into the text. Looking at Table 1, we believe that this is pretty good agreement in modeling the LMS slopes compared to ACE-FTS observations, and we can thus make the comparison of what the STE flux ratios should be.

We apologize for the MLS confusion we created here, and have tried to make clear that Aura-MLS N2O is ONLY for P < 100 hPa to calculate the loss of N2O and hence the 'implied' STE. Thank you also for the constructive notation (Aura-MLS) to avoid confusion with the LMS acronym. Since the CMAM N2O looks "normal" in the mid-stratosphere, we assume they have the near correct lifetime (based on MLS-N2O) or the SPARC lifetime report, e.g., 110-120 yr, and thus have N2O STE fluxes that are "typical".

Revision Line 553

For UCI we calculate NH:SH fluxes of O3 (208:182 Tg-O3/yr) and N2O (5.1:6.4 Tg-N/yr). Thus the mole fraction slopes in the lowermost stratosphere should be -23.8 (NH) and -16.6 (SH). Our model O3:N2O slopes are -23.2 (NH) and -17.5 (SH). Given the seasonal variability and scatter in correlation plots (Figure 7), we count this as consistent. For CMAM, the modeled O3:N2O slopes, **-23±2 (NH) and -18±3** (SH) are similar to ours and **also** to the ACE-FTS observations as analyzed by Hegglin and Shepherd (2007), **-22±4** (NH) and **-14±3** (SH), or by us, -19 (NH) and -15 (SH).

Revision below Line 560

"CMAM does not report the **implied** STE N2O fluxes **derived from their photochemical loss of N2O**, but their model seems to match observations of N2O in the middle stratosphere, and so we assume that the **Aura-MLS derived** N2O fluxes are a close estimate (12.9 Tg-N/yr). **Note we are using Aura-MLS N2O values here to calculate the photochemical loss, which occurs**

in the middle to upper stratosphere.

Revision Line 565

We do not view this as a critical assessment of CMAM since it involves us combining diagnostics from two separate publications **and possibly different model simulations,** ...

Table revision

LMS O_3:N_2O slope*	-23.2	-17.5		UCI model
	-19.4	-15.3		ACE-FTS observations
	-23±2	**-18±3**		CMAM model, Fig 13 of (Hegglin & Shepherd, 2007)
	-22±4	**-14±3**		ACE-FTS observations, ibid
			-20.0	(Murphy & Fahey, 1994)
			-22.0	(McLinden et al., 2000)
STE				

note at bottom of table:

* LMS = lowermost stratosphere **only**. For UCI model, months are selected for highest STE (FMAM in NH, SOND in SH, Fig. 1). For CMAM, **the monthly ranges** from their Fig. 13cd **are estimated**. Where no reference is given, the source is this paper.