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Atmospheric Chemistry and Physics Discussions

Interactive comment on "TransCom N_2O model inter-comparison – Part 1: Assessing the influence of transport and surface fluxes on tropospheric N_2O variability" *by* R. L. Thompson et al.

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We thank the reviewer for his/her very thoughtful and constructive review. Please find our responses to all questions and comments below.

Page 2318, L17: The south to north gradient is not clear from Fig. 2. You may want to revise the color scale to make this feature evident in Fig. 2.

We have now revised the colour scale in Fig. 2, however, it is not possible to maintain a linear colour scale and make the gradient much clearer owing to the large range of values.

Page 2319, L4-6: Is this classification of lower and upper stratosphere also valid for the tropics where tropopause could be located above the 380 K isentrope. I would suggest adding a black line showing the tropopause height in Fig. 2 so that readers can better recognize lower and upper stratospheres.

We have now included a dotted black line indicating the position of the annual mean tropopause.

Page 2319, L16: Reichler et al. (2003) also suggested that average lapse rate between the level (at which lapse rate becomes less than 2 K km-1) and all higher levels within 2 Km should not exceed 2 K km-1. This condition removes the probability of false tropopause detection. Did you check this condition as well? If yes, please add this information to the manuscript.

Yes, we included this condition as well, i.e. following the method of Reichler et al. (2003). We have now included this extra information in section 3.1.1, second paragraph.

Page 2320, L13-14: Why is N2O sink larger in ACTMt42l32 while it was stated in section 2.1 (Page 2313, L16-19) that loss rates in all models are scaled such that global annual total loss of N2O was about 12.5 Tg N?

It was suggested in the experiment protocol that the global annual loss of N2O should be approximately 12.5 TgN, while most models were able to achieve this, it was not the case in ACTMt42l32. The reason for the larger sink is most likely due to a combination of the strength of the photolysis rate and how it is distributed with altitude. To achieve the recommended total loss requires tuning this.

Page 2320, L18-19: Is N2O lifetime calculated as (column 4/ column 5) of Table 7? If yes, N2O lifetime in TOMCAT comes around 108 years. In addition, the lifetimes shown in Table 7 and Fig. 3 are different (e.g. blue cross in Fig. 3 says lifetime of N2O is less than 90 y while Table 7 says it is 92 y). Please check these calculations again and accordingly revise the discussion.

We apologise for this mistake. We had updated our calculation in Table 7 but had forgotten to update Fig. 3. We have now corrected Fig. 3 for the updated values and also corrected the mistake for TOMCAT in Table 7.

Page 2325, L23-24, Why does model show such a larger amplitude compared to observations at MLO?

The models all show a much larger seasonal amplitude at MLO than is observed. This is owing to an overestimate of the influence of stratosphere to troposphere transport (STT) at this site and to the unrealistic later summer maximum in soil emissions (i.e. in OCN) in the northern mid latitudes. The minimum in the modeled seasonal cycle, i.e. circa May, is consistent with modeled STT, in which stratospheric air, depleted in N2O with respect to the troposphere, is transported across the tropopause with a maximum occurring in winter (December to February, in the northern hemisphere) and approximately a 3-month delay for transport to the lower troposphere. When the BWM soil emissions are used instead of OCN (BWM has no seasonality) the seasonal cycle is still overestimated by the models at MLO, albeit to a lesser extent as with OCN. We have now added this to section 3.3.1.1, end of the first paragraph.

Page 2328, L13-14: Is not this the case for PFA and ULB too? Can you please elaborate how did you conclude that modeled STT influence is stronger only at Hawaii?

This is related to the answer to the above question. Since the amplitude of the modelled seasonal cycle at MLO is much larger than observed, and considering that the seasonal cycle in the model is strongly determined by STT, this together indicates an overestimate of the importance of STT at MLO (and similarly at HAA, which is also in Hawaii) in the models.

Section 3.3.2: It appears that the emission scenario BWMN04 leads to best agreement between the models and the observations. This information may be included in the

abstract. Given this, please justify your choice of OCNPIC as control simulation over BWMN04. How the results presented in the previous section would have differed if BWMN04 were used as the control simulation?

We chose OCNPIC as the control scenario as at the outset we thought that the ecosystem model, OCN, would better reproduce the real N2O emissions from soils, considering that this model is driven by climate data and resolves the emissions seasonally. Had we instead chosen to use BWM for soil emissions, the result that would have changed in Section 3.3.1, is that we would have seen an improved agreement in the phases of the model and observed seasonal cycles in the northern mid to high latitudes. It would have also improved the fit to the seasonal cycle at MLO somewhat, owing to the absence of the late summer peak in emissions in the northern mid-latitudes, which also influences the modeled mixing ratio at MLO. Moreover, we could not have known this before having done the model runs and analysis.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 2307, 2014.