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Interactive comment on “TransCom N₂O model inter-comparison – Part 1: Assessing the influence of transport and surface fluxes on tropospheric N₂O 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.

General comments

This study investigates the influence of surface emissions, tropospheric transport, and transport from stratosphere to troposphere (STT) on the variability of atmospheric N₂O concentrations through observations (NO₂, SF₆, CFC-12) and their equivalents from 6 different transport models and two model variants. All the models underestimate

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[Interactive Discussion](#)

[Discussion Paper](#)



the inter-hemispheric (IH) (i.e., south to north) gradient of N₂O concentrations, while models that have provided results for SF₆ reasonably capture this gradient. Focusing on the seasonality and the inter-annual variabilities of the studied species, the authors show that the surface emissions and/or the STT in the Northern Hemisphere (NH) are the causes of the underestimate of the observed inter-hemispheric gradient by the models. Indeed, the authors show that the seasonality of CFC-12 (which has emissions reasonably known in NH and has its sink in the stratosphere) is well captured by the models that provided results on CFC-12, but the STT seems to be more vigorous in the models. Regarding the Southern Hemisphere (SH), all the models fail to simulate the seasonality of both N₂O and CFC-12 concentrations. The authors conclude that the STT is not well reproduced by the models. Interestingly, the authors point out the potential deficiency in the Brewer-Dobson model in explaining the seasonality of STT in SH based on the observations. Most of these results are consistent with some recent studies reported in literature. The study clearly shows that there is still a room for such work since as inherent in the model intercomparison exercise, the identified issues of each of the studied models cannot be investigated in details. Hence, as a future work, I suggest to the authors to focus on each of these models to quantify the contribution of each of the identified issues (when possible). The authors have fully considered the main comments of my first review relevant for ACPD. The paper is now clear and the results are clearly explained. Hence, I recommend it to be published in ACP after considering the few minor comments reported below:

Specific comments Page 2311, line 5: recent studies instead of a recent studies?

We have corrected this.

Page 2311, line 20: ... spatial and temporal variabilities?

We consider that “variability” is correct and thus have not changed this.

Page 2316, line 2: ... forward? I understand what the authors mean, but it seems for me that this word does not add anything here. If they want to use this word, they need

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Interactive Discussion

Discussion Paper



to explain it

We have now removed “forward” and written the following: “Six models and two of their variants participated in the inter-comparison of modelled N₂O mixing ratios..”

Page 2316, line 11: You say that the models LMDZ4 and TOMCAT provide concentrations relevant for the closest model time-step to the observations. Since the temporal resolution of these two models is not reported, it is hard to appreciate how far the model data are from the observations in time. Please, clarify

We have now included the model timesteps for LMDZ4 and TOMCAT at p2316, l12.

Page 2317, line 5: GC-MS. Please define the acronym MS

We have now defined this on line 5.

Page 2317, lines 12-15: The computed mean biases (i.e., calibration offsets) are subtracted to the observations of the relevant sites? Please clarify

We forgot to mention that the calculated offsets were added to the observations for the model-observation comparison. This has now been clarified on p2317, l15.

Page 2318, lines 12-13: ‘This is a particularly simplification for species such N₂O and CFC-12, which have a source to the troposphere and stratosphere sink’. The sentence is not clear for me. Please clarify

What is meant by “This is a particularly useful simplification for species such as N₂O and CFC-12, which have a source to the troposphere and stratospheric sink” is that the N₂O (or CFC-12) budget can be simplified for the troposphere/stratosphere by considering only the source to/loss from the troposphere/stratosphere and the flux across the 380K isentrope. We have tried to make this clearer in the following formulation:

“This is a particularly useful simplification when considering the budgets of species such as N₂O and CFC-12, which have a source in the troposphere and sink in the stratosphere”

Full Screen / Esc

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Interactive Discussion

Discussion Paper



Page 2319, lines 25-28: Since all models use the same prior fluxes (OCNPIC), differences in the modelled growth rates are due directly to differences in the net cross-tropopause N₂O flux, which depend (not s here) on the upward and downward mass fluxes and the above and below tropopause N₂O mixing ratios. The authors can add that these differences are linked to the meteorology used in each model and also the vertical definition of the models.

We have changed this to the following:

“Since all models use the same prior fluxes (OCNPIC), differences in the modelled growth rates are due directly to differences in the net cross-tropopause N₂O flux, which depend on the upward and downward mass fluxes and on the above and below tropopause N₂O mixing ratios; factors that are determined by the meteorological data used as well as on the vertical definition of the models.”

Page 2321, lines 5-8: The authors should put these acronyms on Figure A2. This help to easily follow their demonstration in the text.

We have added these site acronyms to Fig. A2.

Page 2321, line 17 and elsewhere when relevant: The authors should fix the use of CTM or ACTM.

We have corrected this now and use “CTM” throughout the manuscript.

Page 2329, line 17: .. to a lack . . .

We have corrected this.

Tables and Figures

Table 7: The period of study is 2006-2009 instead 2007-2009 as specified in the text? Please clarify

This was a mistake in the caption of Table 7. It should be 2006 – 2009. This has been

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corrected.

Figure 1: As already mentioned above, please fix the use of CTM or ACTM

This has been corrected to “CTM”.

Figure A2: Legend. It is the map that shows the locations of the observational sites. Also, you should put the acronyms of the sites

We have now added the site acronyms.

Figure A3: You state that you subtract the mean mixing ratio (model/obs). Are mean values computed at the global scale? Please clarify.

Yes, we subtracted the global mean. This is now stated in the caption.

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