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Atmospheric Chemistry and Physics Discussions

Interactive comment on "TransCom N_2O model inter-comparison, Part II: Atmospheric inversion estimates of N_2O emissions" *by* R. L. Thompson et al.

Anonymous Referee #1

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This manuscript presents a comparison of five models, meteorology, and inversion frameworks in estimating nitrous oxide emissions. The objectives are to quantify N2O emissions globally and regionally and to identify the potential causes of discrepancies between models/inversion frameworks. The paper is well-written and reaches important conclusions, for example, about the representation of stratosphere-troposphere exchange in models and the shift in emissions from the tropics to mid-latitudes, and should be published in ACP.

General comments:

1. I do not understand some of the results, particularly from the MOZART model. The model mole fractions simulated by the posterior fluxes do not appear to match of observations (as shown in Figures 2,3, A1, A2, A3). If the inversion is doing what it should, incorrect fluxes may be derived but the simulated mole fractions would come close to the observations (at individual stations as well as in the growth rate). Is there a problem with the inversion?

The authors state that there is a problem with having too high initial conditions, but this should be "absorbed" into the derived emissions. While the degree to which the emissions are tied to the prior is governed by the prior emissions and prior uncertainty, the results still indicate that the inversion did not reach meaningful conclusions (i.e., poor validation as there is not a good fit to the observations). If the derived fluxes are then incorrect, this would also significantly affect the posterior simulation of spatial distributions and seasonal cycles – therefore, it is not clear to me why the model can be excluded for emissions estimation but used for other analyses, which are still based on the posterior fluxes. In any case, would it not be straightforward for the inversion to also solve for the initial condition as part of the inversion to minimise these issues?

2. The authors do mention that each CTM could be used with any inversion framework but it would be good to stress in the conclusions that the results do not necessarily indicate issues with the model specifically but the combination of the two as well as other parameters (e.g., type of observation assimilated – monthly, weekly, etc, as well as the choices in prior and observational uncertainties).

3. Does the general uncertainty analysis only include the spread in the models or does it account for uncertainties from the inversions (for example, on a regional level, the uncertainties derived in each inversion could be large but the spread small)? Please make this more clear in the text.

4. All figures and text should be made clear that simulated values for each model come from the posterior fluxes.

5. It would be useful in the figures showing comparisons to observations to also show the observational uncertainty (e.g., Figs 3, A1, A2, A3). For example, in Fig A3, the LMDZ growth rate is also a bit low but it might be within the observational uncertainty so that would be helpful to see.

Specific comments:

1. Page 5273 Line 24 The citation of Forster et al. 2007 for N2O being the third most potent greenhouse gas is not correct (it has it as the fourth). There is updated work showing N2O surpassing CFC-12 (e.g., NOAA Annual Greenhouse Gas Index).

2. Page 5277 Line 26 How does each model/inversion choose the prior and measurement uncertainties?

3. Page 5280 Line 16 Even measurements from the same station could have systematic errors when calibration gases are changed (step changes in time series).

4. Page 5280 Line 22 Why do only certain models/inversion frameworks have the capability to solve for inter-calibration offsets? Couldn't all models/inversion frameworks solve for these offsets as additional parameters?

5. Page 5281 Line 11 Be more clear about where this Rˆ2 value was calculated from (is this comparing the mean of 2006-2008, all of the models, etc.)?

6. Page 5281 Line 11 If the initial conditions are incorrect, this will also affect the gradient (see general comment above).

7. Page 5283 Line 11 The notation of STT is suddenly used, I think, instead of STE. Is STT meant to be something different (if so, it is not mentioned)?

8. Page 5284 Paragraph 1 To be more clear, the discussion of the global totals and the connection with how the observations were assimilated should be expanded on. It is not inherently to do with the model or the inversion methodology but how the data was assimilated. The three models (MOZART-I, ACTMt42l67-I and TM3-I) could assimilate

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at the sampled time, in principal. It should be mentioned that the low totals that result from using monthly means, for example, is largely due the fact that NOAA flasks are filled for “background” conditions so averaging the model’s simulation into a monthly mean will tend to be higher than a background value.

Second, the statement of low MOZART emissions being a result of high initial conditions should be moved before the discussion of the temporal resolution of the data assimilation. It seems to me, from the way it reads now, that the primary reason for the low global emissions is because of assimilating monthly means but the high initial conditions are likely a major reason (again pointing to the general comment above).

Additionally, there are some typos in the ACTMt42l67-I naming. On this page, it is labeled ACTMt32l67.

9. Page 5285 Paragraph 2 This is a nice discussion of the effect of inter-hemispheric mixing time. Could you expand on how that would affect the “other hemisphere”? For example, TM5-I over-predicts the SH emissions because of a too slow mixing time. This should cause the NH emissions to be under-predicted. Similarly, the reverse should be true for LMDZ-I. Also a diffusive PBL in this model should result in higher concentrations simulated in the upper troposphere. How does this compare with other models and HIPPO?

10. Page 5287 Line 17-18 Please remind the readers about what the first and second criterion mean. Is this the definition of “significant” starting on the previous page?

11. Page 5288 Paragraph 2 South Asia experiences a double maximum in April and September. The authors argue the peak in April could partially be an artifact due to too strong STT. What is causing the September peak?

12. Page 5289 Paragraph 1 For South and Tropical America, what could cause the maximum in September?

13. Page 5289 Paragraph 2 LMDZ predicts a much larger amplitude than any other

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model for the 90-30S ocean. What could cause this?

14. Page 5290 Line 11-13 How was the 16.1-18.7 TgN/yr global emissions calculated? Where does this uncertainty of 0.7 TgN/yr come from? Does it include the spread in models as well as the uncertainties from the inversions? Is it an average of all of the years? If this uncertainty is based on the range in the inverse methods/models, then it cannot really be considered a “true” estimate of the uncertainty because the models do not form an ensemble of independent estimates (see general comment).

15. Page 5300 Table 2 What does the ‘11 regions’ under the ‘scale length in B’ mean for the ACTMt42l67 model?

16. Page 5308 Fig 2 The figure is not plotting the gradient but the zonally averaged mole fraction. Also, the sentence ‘The grey shaded area shows the range of values for the model using the prior fluxes’ is difficult to understand. Is it the range from each model aggregated together?

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 5271, 2014.