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Reply on RC1

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Author comment on "Modelling the growth of atmospheric nitrous oxide using a global hierarchical inversion" by Angharad C. Stell et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-513-AC1, 2022

We thank the reviewer for their helpful comments. In this document, we reply to each comment, providing extra detail and outlining how we have updated the manuscript.

There is no discussion of positive atmospheric growth rate anomaly in 2020 and no mention of the emissions and sink in 2020 and how these may or may not be different with respect to the previous 9 years, although this is alluded to in the abstract (L7). Figure 7 shows the source per latitudinal band for the analytical and hierarchical inversions: the hierarchical inversion indicates a positive source anomaly in 2020 in the band 0-30N, but the source in the analytical inversion is significantly smaller. From the analytical inversion it does not appear that there was any significant source anomaly in 2020. A discussion about how and why the hierarchical and analytical inversion results differ in their source estimates and atmospheric growth rates, especially for 2020, would greatly add to the paper.

We have added a discussion on the increase in 2020 emissions by adding the following text to line 276 (now line 288):

``Whilst it is difficult to deduce the cause of the emissions increase in 2020 from this study, several factors could play a role. It is likely that natural cycles (e.g. the El Niño–Southern Oscillation (ENSO) (Ishijima et al., 2009; Thompson et al., 2013; Ji et al., 2019; Patra et al., 2022)) contribute to the emissions increase in 2020, alongside the longer term trend in increasing emissions, which has been attributed to a non-linear response of nitrous oxide emissions when nitrogen input is high (Thompson et al., 2019) or an increasing emissions factor due to warming and the redistribution of emissions (Harris et al., 2022)."

A discussion about how and why the hierarchical and analytical inversion results differ is provided in Sections 3.3.2 and 3.4. We have added a discussion of why the 2020 results differ to Section 3.3.1, after line 276 (now line 293):

``The impact of the hierarchical inversion can be seen by comparing to an analytical inversion within this work, as shown in Fig. 5. On the global scale, there is very good agreement in total emissions between the two inversions performed in this work. The only year where the analytical result falls outside of the 95 % credible interval of the hierarchical result is 2020. This is because on a global scale, nitrous oxide emissions are well constrained by the observations so the inversions give consistent solutions. However, there are fewer observations to constrain the emissions in 2020 (as 2021 observations were not available). The hierarchical inversion moves further from the prior because the uncertainties in the inversion can be adjusted (Sect. 3.4). The two inversions do not agree on the land and ocean emissions for 2011--2020 as well as for the global total emissions over the same time period. The land and ocean emissions are less constrained by the observations than the global total emissions, and so differences in the uncertainties in the inversions (Sect. 3.4) lead to different results. This is also the case for the zonal emissions which are discussed in Sect. 3.3.2."

L4: The authors should replace "non-Gaussian" with "truncated Gaussian", which is what is described in section 2.3 for the prior uncertainties, and "Gaussian" for the model-measurement discrepancies as described in section 2.4.

Thank you for pointing this out, it has been changed as suggested.

L24: The reference Solazzo et al. 2021 is not really appropriate here since it is the natural sources of N2O that are being discussed, whereas Solazzo et al. discuss only anthropogenic emission estimates derived using emission factor approaches.

We agree and the reference to Solazzo et al. has been removed.

L29: It sounds as though NOAA and AGAGE are the only ones measuring N2O, which is not the case. Please at least add “among other laboratories” (e.g. ICOS network, which has continuous measurements of N2O since about 2018).

We agree and have rephrased this sentence to read:

``The atmospheric abundance of nitrous oxide is monitored by several laboratories, and in this work we use measurements taken by the National Oceanic and Atmospheric Administration (NOAA) (Dlugokencky et al., 2021; Sweeney et al., 2021) and the Advanced Global Atmospheric Gases Experiment (AGAGE) (Prinn et al., 2000, 2018)."

L33: It is not “meteorology” that is driving the growth rate, rather (and this is the conclusion of Ruiz et al.) climate oscillations, in particular, the Quasi-Biennial Oscillation, are an important (but not the only) driver of variability in the tropospheric growth rate of N2O.

``Meteorology" has been replaced by ``climatic variability".

L67: N2O fluxes on land can be negative, however, the sink is thought to be rather small (Tian et al. 2020). Therefore, please change the end of this sentence to “we expect land emissions to be predominantly positive”

We have rephrased this to ``we expect land emissions to be predominantly positive", as the land sink is very small.

L143: According to Eq. 1, excluding the error term, the true flux is modelled as the sum of the prior flux and a scaling of the basis function (phi) (not a scaling of the prior flux)

The basis functions are the prior fluxes in the different TransCom regions, as stated on line 142 (now line 146), so the original phrasing is correct.

L260: There is no "record" of the emissions, but only of the growth rate. Please change this to "the first paper to report emissions for 2020, the year which had a record growth rate"

This has been rephrased to:

``... this is the first paper to report emissions for 2020 which are likely to be the highest in 2011--2020."

L310: In this study, the posterior fluxes in the NH, 30-90N peak in January-February, which is earlier than found in the cited previous studies, which find a maximum in spring, around March-April. I strongly suspect that this winter peak

in emissions is due to model-transport errors, and Fig. S1 shows that the model does not capture the phase of the seasonal cycle in atmospheric N2O in the northern latitudes (phase is approximately 6 months out of phase with the observations), although some of this mismatch may be due to the missing or incorrect seasonality in the prior fluxes. A winter (January-February) maximum in the emissions for the latitudes 30-90N is very difficult to reconcile with what is understood about the drivers of the emissions, which include management (e.g., timing of fertilizer application) and environmental factors (e.g., soil moisture and temperature).

In this study, the posterior fluxes in the NH 30-90N typically peak in March, in agreement with the cited previous studies. This has now been explicitly stated in line 310 (now line 336). Additional gridlines have also been placed on the figure to make this easier to read.

L333: The authors say that their error budget for observation-space uncertainties is smaller than in previous variational inversions, but the observation number and frequency between this study (monthly observations) and other inversions (hourly or afternoon averages) is very different and thus the observation-space uncertainties also need to be different to reflect this.

We agree and this has been rephrased to:

``This means the error budget in this work is smaller than a non-hierarchical inversion would have imposed. Therefore, a non-hierarchical inversion for the same number of data points and uncertain parameters would be less data-constrained than our framework."

L335: I think the result for the error budget scalar in the extra-tropical SH is not that surprising considering the large model-observation differences there.

We agree, but starting with no prior knowledge of the system, we would describe it as ``counter-intuitive". This has been clarified by rephrasing to ``somewhat counter-intuitive".

L337: I think the authors mean "or the inter-annual variation" not "all the inter-annual variation"

This has been changed as suggested.

L338: Concerning the reason for the cause of poorer agreement with the observed seasonal cycle and interannual variability for the extra-tropical SH, this is very likely also due to the large error budget scaling factors in this region, which means that the observational constraint is weaker. Furthermore, the Antarctic region (i.e., Transcom region T00) has very likely negligible emissions, and the variability in atmospheric N2O at the extra-tropical SH sites is driven by atmospheric transport, including stratosphere-troposphere exchange, ocean fluxes, and to a smaller extent fluxes over the small amount of land in the SH extra-tropics.

The error budget scaling factors are estimated in the inversion, so the reverse explanation for the poor agreement in the extra-tropical SH is also possible: that the error budget scaling factors are larger because of the poorer agreement. We have rephrased and added to this section to include the importance of atmospheric transport:

``One of the most likely causes of the large error budget scaling factors and observational mismatch is an inadequate prior without enough flexibility to change as a result of solving on the scale of TransCom regions. TransCom regions are particularly restrictive in the Antarctic circle (where the largest error budget scaling factors are found), as the TransCom region for Antarctica also includes Greenland and the Mediterranean Sea (see Fig. S1), limiting the potential for the fluxes in this area to adjust. Another factor causing the large error budget scaling factors and observational mismatch could be that the extra-tropical Southern Hemisphere stations generally have lower error budgets before the scaling factor is applied, because of the lower spatial and temporal variability in their mole fractions. Additionally, because of the low emissions in this area, the variations in atmospheric nitrous oxide mole fractions are mainly driven by atmospheric transport, which the inversion cannot adjust."

L351 (and L5-6): The statement "we show that the recent atmospheric surface growth rate fluctuations are partly driven by emissions but also by inter-annual variability in transport" is not very well supported. The authors do not discuss or quantify the contribution of variability in emissions to that in the atmospheric growth rate, nor do they quantify the contribution from inter-annual variability in transport.

This is demonstrated in Fig. 4 and discussed in L242-250 (now lines 250-259). Running the model with repeating 2015 meteorology changes the atmospheric growth rate, as does changing the emissions. Indeed, we have not quantified the exact contributions, but we have amended the text slightly to ``we show that the recent atmospheric surface growth rate fluctuations are likely to be driven both by emissions and also by inter-annual variability in transport".

L354: Concerning the phase of the posterior seasonal cycle in the NH, I think this result may not be very robust given the uncertainties in the modelled atmospheric transport, therefore, the authors should include as a reason for this shift in phase "errors in modelled atmospheric transport".

We believe this comment comes from the reviewer's misreading of the seasonal maximum in their comment about L310, and as such is an unnecessary change. We have clarified the misunderstanding in the response to the comment about L310.

L361: The statement that this inversion is “more data constrained” compared to previous studies is not quite true. The observation constraint is not only determined by the observation uncertainties but also the number of observations, and there are previous examples of N2O inversions using vastly greater numbers of observations (e.g. afternoon mean observations).

We agree and have rephrased this to:

``Our uncertainties are estimated by the inversion are generally smaller than those that would be used in a non-hierarchical inversion for the same number of data points and uncertain parameters, and therefore our inversion is more data-constrained."