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Reply on RC2

Angharad C. Stell et al.

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-AC2, 2022

We would like to 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.

Line 27-29: There are other networks that have routine measurements of N2O. Why not mention or include them in this work?

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)."

Line 44-45: This statement makes it seem like additional observations are the main requirement for constraining regional inversions, but that is not problem being addressed by this work. Consider rephrasing here to better motivate what's actually being done in this study.

This has been rephrased to:

``These discrepancies suggest that new measurement or modelling approaches are required to constrain fluxes at the regional scale in global inversions."

Line 58-67: This paragraph is helpful in that it mentions some of the shortcomings of past work that might be addressed in this study, but I think it could use a more definitive take-home statement about what is being done here and how the main findings will be presented. This way the reader will have a clearer idea of what to expect before they read on.

The following has been added to this paragraph:

``In this work, we investigate nitrous oxide emissions on a global and zonal scale using the hierarchical inversion. To help examine departures from previous inversions and explore the benefits of the hierarchical framework, we compare to results from an analytical inversion."

Line 67: "as we expect land emissions to be only positive." Ok, I guess by definition emissions are positive, but I think soils can also be a sink for N2O?

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

Section 2.1: There are other networks that measure N_2O, did none of those data meet the criteria to be included in this study?

There are potentially a small number of other sites in other networks that could have made our selection criteria. However, many of these stations are located in the same or similar places as the NOAA/AGAGE networks so do not add much independent information to the inversion. Whilst developing the current inversion, we did experiment with including/excluding slightly different sites and there was not a substantial change in the emissions inferred. This is likely because the main focus of our paper is global and zonal scales, rather than regional scales, where additional measurements will be important.

Line 109: "starting from an atmosphere with a constant nitrous oxide model fraction." Does this constant mole fraction extend into the stratosphere as well? If so, has any analysis been done to see how well the model represents stratospheric N_2O? It not, do the authors expect their spin-up approach to affect the results, given that stratosphere-troposphere exchange drives the seasonality of surface N_2O at remote sites away from continental sources?

This constant mole fraction does extend into the stratosphere, but we have checked that stratospheric nitrous oxide is adequately represented. This was done by examining the simulated zonal mean atmospheric nitrous oxide distribution with height as well as the lifetime. We have expanded the sentence starting on line 109 (now line 112) to make this clearer:

``The resulting initial condition field matches surface nitrous oxide observations to within a few ppb, has a zonal and annual mean latitude–altitude cross section of nitrous oxide mixing ratio that matches other models (Thompson et al. (2014b), and also gives a nitrous oxide lifetime of 120 years, in good agreement with Ko et al. (2013) and Prather et al. (2015)."

Line 131: In the abstract and introduction the authors describe the prior emissions as non-Gaussian, but here they are described as Gaussian truncated at zero. Please update the earlier descriptions to be consistent.

This has been done as suggested.

Line 132: Here the authors mention that they use a 3-year moving window to reduce computational cost of the inversion. It would be really helpful if the authors included more information about the computational cost of this approach versus analytical or 4D-Var, for example, to put this statement in context.

We have added some information about the computation time to line 204-5 (now lines 210-212):

``The method of Zammit-Mangion et al. (2022) is too computationally expensive to run over the long time period in this study. We instead use a moving window approach, which reduces the computation time from weeks to days, but is still much longer than the seconds it would take to solve analytically."

Line 142-143: "the true flux in a region is modelled as a scaling of the prior flux in that region." But, equation (1) shows the true flux as a sum of the prior flux and a scaling of the basis function, so I'm unclear what exactly is begin done. If equation (1) is correct, it seems the prior scaling factor would be zero, and then positive or negative posterior scaling factors are allowed? Is that correct? Note: I found the answer to this question later in Section 2.3.4, where it states that the scaling factors are greater or equal to -1. Suggest moving this info earlier, or at least pointing the reader to Section 2.3.4 here.

This has been added as suggested.

Line 153-156: Here in the description of the response function construction it would be a helpful to talk more about the computational cost of the inversion. It sounds like it requires 3,036 perturbed forward runs of the model? How long does that take? How does it compare to the cost of the analytical inversion?

Yes, 3,036 perturbations to the model are required, as well as an original ``base" run. The perturbed simulations are used to construct the basis functions used in both the analytical and the hierarchical inversions.

Apart from the ``base" simulation, which must be run first to derive the initial conditions, the perturbed simulations can all be run in parallel, greatly reducing the computation time. The computational burden can be further reduced by using tagged tracers within the GEOS-Chem model for emissions from each of the TransCom regions. With these tweaks, only 132 perturbed forward simulations are required. On our HPC system, the ``base" simulation took about a week, followed by another week for all the perturbed simulations.

To express this within the paper we have added the following:

``Running the perturbed simulations is computationally expensive, but can be reduced by running simulations in parallel and using tagged tracers within GEOS-Chem for the emissions from the different TransCom regions. These model runs are required for both an analytical inversion as well as the hierarchical inversion."

Line 163: Can the authors clarify what is meant here by "similar error properties"?

This phrase has been removed as it is unnecessary.

Figure 3: Could some text be added to the graphical model to clarify the processes that are represented by the different arrows? I don't find the model particularly helpful to the reader as is.

The arrows, as is customary for graphical models, do not represent processes but statistical dependence between the nodes (random variables). This explanation has now been added to the caption:

``The arrows represent the statistical dependence between the variables."

Lines 204-209: Here it is mentioned that the moving window was used for computational efficiency for long-term inversions. Can the authors quantify how much computational cost is saved using this approach?

This has been done as specified in the response to the comment about line 132.

Lines 242-250: It sounds like the authors are concluding that interannual variability in the N2O growth rate is driven by meteorology from their test in Fig. 4d. The authors mention the QBO as a potential driver, and I think this discussion could be expanded upon (by citing additional peer-reviewed literature) to elucidate the processes that would impact surface N2O year-to-year. For example, do different phases of the QBO result in different rates of

strat-trop exchange, or does it more have to do with differences in horizontal transport in the atmosphere?

We have expanded this to read:

``Previous studies have suggested that the Quasi-Biennial Oscillation (QBO) is an important driver of the nitrous oxide growth rate, as it modulates the stratosphere to troposphere mass flux (Ray et al., 2020; Ruiz et al., 2021)."

Line 260: The authors should avoid overinterpreting the 2020 results here, given that they are less constrained than other years in the inversion.

We have mentioned that this year is less constrained in line 209 (now line 216), but this should be accounted for in the uncertainty in the emissions, and the emissions are still well constrained on a global scale. When considering the emissions split between land and ocean, the larger uncertainty for 2020 can be seen in Fig. 5. We have rephrased the line in the paper to:

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

Figure 7: In the monthly plots it's quite difficult to discern the timing of the peaks. Suggest making these plots larger or at the very least adding more tick marks to denote the years and months.

We have added annual gridlines and it is now stated which month the seasonal maximum occurs in the text for Northern extratropics.

Line 337: “The most likely cause is an inadequate prior…” Other studies have attributed the poor model representation (and, thus, high error scaling factors in this work) of southern hemisphere N2O to uncertainties in model transport. Can the authors discuss this possibility here?

We have altered this as suggested:

``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 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."

Line 350: “Likely due to different chemical transport models…” I think the authors could do more to discuss the differences between their work and other studies. If they are just due to differences in chemical transport models (which I assume means differences in their representation of model transport?) then what is this different inversion framework adding? Might we expect the results to be different from other studies using the hierarchical inversion?

Our hierarchical and analytical inversions use the same transport model with somewhat different inversion methods and get similar results. This suggests that if a transport model from another study was used, we might get similar global and zonal results to that study. However, there will also be differences caused by using different optimisation regions and prior means. There are greater differences between our two inversions on a regional scale (as seen in the Supplement), but we chose not to discuss specific regions due to the problems described in Sect. 3.3.1. What the hierarchical inversion adds to the study is described in detail in Sect. 3.4 (uncertainties that are informed by the data and vary greatly across different stations and regions). Whilst, for global scale inversions of nitrous oxide emissions, the hierarchical and analytical inversion produce similar global and zonal emissions, that is not necessarily true for other gases and scales of inversion.

This has been rephrased to:

"... likely due to differences in atmospheric chemical transport models and optimising emissions for different regions, rather than the inversion method."

Lines 353-358: The findings listed in this paragraph are all broadly consistent with what other inversion studies have found. Suggest instead highlighting the new findings of this work and what recommendations can be made from the error budgets derived (e.g., which regions are the least constrained by our global N2O observing network?)

Whilst our conclusions are indeed broadly similar to other studies, we believe they are still worth stating here. It is hard to say which regions are the least constrained, because the likely answer is the regions which have stayed very close to the prior, but alternatively, the prior could just been correct in those regions. We chose not to discuss specific regions due to the problems described in Sect. 3.3.1.

Lines 361-362: "therefore our inversion is more data-constrained." I'm not sure the authors have demonstrated this here. For one, they use fewer observations than other N2O inversion studies have used. Also, does a lower prior observational uncertainty budget necessarily mean a higher data constraint? I typically think of data constraints as being quantified via posterior error reduction or degrees of freedom from the inversion. Can these kind of metrics be calculated from the hierarchical inversion framework?

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."

Line 19-20: This sentence is awkward. Consider revising.

This has been rephrased to:

``Additionally, nitrous oxide is currently the largest contributor to stratospheric ozone depletion, when considering ozone depletion potential-weighted anthropogenic emissions (Ravishankara et al., 2009)."

Line 137: Consider making Fig. S4 into Fig. S1 since it is the first supplemental figure referenced.

This has been done as suggested.

Line 190: Delete second instance of "uncertainty" here.

This has been done as suggested.

Line 244: Change “are impact” to “are impacting”

This has been done as suggested.