EGUsphere, referee comment RC2
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Comment on egusphere-2022-513

Anonymous Referee #2

Referee 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-RC2, 2022

This paper presents estimates of global nitrous oxide fluxes from 2011-2020 using a hierarchical Bayesian inversion framework employing the GEOS-Chem CTM and surface measurements of N2O. In this framework the error statistics are informed by the measurements, which is a new contribution of this work. The results suggest that increasing N2O emissions, particularly from 0-30 N, contribute to the observed growth rate of surface N2O, although significant interannual variability is derived. As in other studies, regional emissions (particularly from the oceans) are poorly constrained in this framework.

This manuscript is generally well-written and expands upon the limited body of work on global N2O inversions. However, I think the authors could do a better job highlighting the unique contribution of this work throughout the paper. There are also a few places where the conclusions made are not well-supported by the analysis as it is presented currently, or where additional clarification is required. I've noted some specific instances in the comments below. After these are addressed, I would recommend publication.

Specific comments:

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

- 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.
- 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?
- Section 2.1: There are other networks that measure N2O, did none of those data meet the criteria to be included in this study?
- 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 N2O? It not, do the authors expect their spin-up approach to affect the results, given that stratosphere-troposphere exchange drives the seasonality of surface N2O at remote sites away from continental sources?
- 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.
- 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.
- 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.
- 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?
- Line 163: Can the authors clarify what is meant here by "similar error properties"?
- 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.
- 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?
- 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?
- Line 260: The authors should avoid overinterpreting the 2020 results here, given that they are less constrained than other years in the inversion.
- 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.
- 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?

- 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?
- 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?)
- 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?

Technical comments:

- Line 19-20: This sentence is awkward. Consider revising.
- Line 137: Consider making Fig. S4 into Fig. S1 since it is the first supplemental figure referenced.
- Line 190: Delete second instance of “uncertainty” here.
- Line 244: Change “are impact” to “are impacting”