Comment on egusphere-2022-513

Anonymous Referee #1

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-RC1, 2022

General comments

This manuscript presents new global estimates of N2O surface fluxes for the period 2011-2020 using an hierarchical Bayesian inversion framework. The inversion framework presents some innovations, namely the use of a truncated Gaussian distribution for the prior flux uncertainties, and a moving window for the inversion. The study also adds to the small number of global inversions of N2O. The work is generally well presented, however, there are some shortcomings in the discussion (see below) and a number of specific points to be addressed (see Specific Comments).

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.

Specific comments

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.

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.

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).

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.

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"

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)

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"

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).

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.

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.

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

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.

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.

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".

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).