Responses to reviewers' comments on "Global Nitrous Oxide Budget 1980–2020" (manuscript number essd-2023-401)

We would like to thank the reviewers for their thoughtful and insightful comments. The manuscript has been revised accordingly, and our point-by-point responses in blue color are provided below, and our new/modified texts in the revised manuscript are indicated in red color.

Reviewer 2:

Review ESSD-2023-401, Global N2O budget

Remarkable compilation of many N-relevant processes. Hard to review, particularly because they have many experts in their author list. Definite merit for ESSD, but needs key improvements. Basic approach: For important N-molecule, one needs to pay attention to a wide variety of sources, sinks, and biochemical reactions. This paper represents best efforts. They itemize important sources, sinks, and reactions. They draw conclusions by source and by regions. They list abundant uncertainties but rarely in numerical terms. Instead, authors focus on geographic patterns/biases of these uncertainties. They recommend that uncertainties get resolved ("reconciled") in future observations and from future models.
For examples: Line 1173: "large uncertainty in the estimates of agricultural N2O emissions"; Line 1209: "large discrepancy in natural soil emissions among NMIP2 models exists"; Line 1391: "variability of these emissions remains uncertain" (referring to emissions from continental shelf regions); etc.
They miss (fail to address) test-able hypothesis: that uncertainties remain so large that conclusions prove speculative (at best). Reader finds zero discussion of cumulative numeric uncertainty. In very practical terms, this reader suggests that uncertainties mapped geographically in Figure 21 overwhelm any regional signals authors might hope to identify in Figure 13.

Response: Our study. has developed the most comprehensive assessments possible given the observations and modeling capability available to date. Following the IPCC approach, we build robustness of what we know on the global N_2O budget and its component fluxes by bringing multiple and partially/fully independent lines of evidence to constrain temporal and spatial fluxes. The diversity of data and approaches used in this effort has limitations in the way we can treat numerically uncertainties, and so we ensure we provide at all times range values for each budget flux as the best expression of uncertainty we can provide. As it is done in other GCP global budgets, agreed protocols and community acceptance of best practices and data dictates what is used for this assessment to ensure the highest quality, and not a simple review of everything that has been published.

In GCB, reader immediately (abstract) learns that those authors present all data with consistent specified uncertainty of +1 sigma. In global methane budget, reader confronts ranges (min/max) but those authors write (explicitly, in second paragraph of Introduction): uncertainties in regional emissions may reach 40%–60% (of global mean). For N2O, where obs come directly from same sources as for CH4, where microbial processes intervene in both sources and sinks, and where models have similar (plus, additional?) weaknesses as for other chemical or transport models, one expects

similar quantitative uncertainty info? Should readers assume that we know N2O concentrations and fluxes with better-than-CH4 uncertainty, or with worse? Reader here gets no hints. This particular reader assumes 'worse'! Not because authors have done a poor job (instead, they seem to have done a very good job) but because N2O represents a more difficult, complicated reactive molecule than e.g. CO2 or CH4. If the target remains more elusive, authors must demonstrate and exercise BETTER methods to identify and quantify. Disparaging statement above (e.g. that uncertainties remain so large so as to preclude conclusions on specific sources or assigned to specific regions) represents a clear refutable hypothesis. Unfortunately, authors never pose nor address such a hypothesis. Validation remains conceptually and quantitatively difficult for these global budgets. Other ESSD products showed good success by using 'leave-one-out' techniques. Because these authors report multiple atmospheric data sources, multiple model outcomes, etc., they might choose to pursue similar leave-one-out strategies? Or, and this would prove relevant to uncertainty issues, authors feel reluctant to identify one obs data set or one model as 'reference'?

Response: As the reviewer is aware, the Global Carbon Budget provides a much more mature, and process and datasets and modeling are more mature and established. Separate assessments are available for CO_2, CH_4 and now also for N_2O, that are characterized by the respective properties, emission sources and processes typical for the respective gas. It is not the aim of either of these activities to compare against each other, but rather to point out the distinctive conditions. The present manuscript on N_2O uses a large range of available sources to provide an overall assessment, to provide results of fluxes into and out of the atmosphere, with the ranges derived from the differences from individual sources. Each individual approach may provide their own uncertainty analysis, based on the respective input parameters. As a guidance to the scientific community, we understand that providing the ranges will be much more useful than attempting a quantitative analysis of uncertainties, which may be a topic of a separate piece of work. Identifying where the uncertainties are well constrained or very large can be viewed as a success of our effort, which we hope will guide future research.

(Just for your information, in our opinion, the geographic uncertainties of N2O vs. CH4 budgets are about the same because we rely on similar bottom-up and top-down data constraints. For differences among sectors, we would say that we are more confident about N2O sources and sinks, because agriculture is clearly the dominant source and the stratosphere is clearly the dominant sink. In contrast, CH4 has multiple important sources in the range of 10-30% of the total. For CH4, tropospheric OH is the dominant sink, but the methanotrophic sink could still be bigger that currently estimated, whereas it is very unlikely that soils represent more than a tiny sink for N2O. We don't think that it is appropriate to devote space in the manuscript to compare N2O and CH4 uncertainties, but we suppose we could make these arguments in our response to the reviewer's comments).

I identify many small but necessary changes below. Until, however, readers gain a complete quantitative discursion on uncertainty, I doubt that suggestions that follow pertain. Manuscript needs serious overhaul; small fixes unimportant on that scale.

Line 84: "accumulating in the atmosphere since the pre-industrial period". No evidence

presented here. Most data in this paper start in 1980. No citation for statement, in next sentence, that N2O concentrations "from 270 parts per billion (ppb) in 1750". How do we know pre-1980 N2O concentrations? In other publications, GCP considers 1750 as start of industrial period? 'Pre-industrial' seems unsupportable and too vague. I know what authors intend here but many ESSD readers will not?

Response: We have revised the first two sentences in the abstract to avoid the picky argument regarding pre-industrial period and to be more precise regarding troposphere vs whole atmosphere: "Nitrous oxide (N_2O) is a long-lived potent greenhouse gas and stratospheric ozone-depleting substance, which continues to accumulate in the atmosphere. The mole fraction of tropospheric N_2O has …" . For N_2O concentration in 1750, we have added the following citation to the revised manuscript.
Reference :
Macfarling Meure, Cecelia, et al. "Law Dome CO_2, CH_4 and N_2O ice core records extended to 2000 years BP." *Geophysical Research Letters* 33.14 (2006).

Line 92: if fluxes increased by 40% over 1980 to 2020 but concentrations increased only 25% (from 1750 to 2022, line 85) then something must also have changed in sinks? E.g. Fig 1 shows at least three sinks (downward arrows, including one massive downward arrow); atmospheric concentrations must represent some balance of these processes? Not clear here, nor elsewhere in thus manuscript. Authors job to compile and present best data (good on them) but also to explain basic balance / imbalance of global N2O budget? This reader might understand subtle differences here but many readers will not? Somewhere, in abstract or exec summary, readers need to find concise summary?

Response: N_2O emission reported here represents the annual amount of N_2O emitted to the atmosphere (concept of flux, unit: Tg N/yr). Atmospheric N_2O concentration is directly proportional to atmospheric N_2O burden (concept of pool, unit: Tg N). Since the initial atmospheric N_2O burden (pool) is not zero, it doesn't change proportionally with N_2O flux. We agree with the reviewer that the stratsopheric sink has increased as the concentration of N2O has increased, because it is concentration-dependent. Prather's papers have discussed this point and cited in this manuscript (Prather et al. 2023). That explains how fluxes can increase more than concentrations, because the concentration-dependent sink has also increased somewhat.

Lines 107 to 116: Good summary here! Compares recent work (1980 to 2020) to ice core records. No mention of 'industrial' or 'pre-industrial'. Revise abstract in light of what you have here? Also no mention of stratospheric processes (O3 impact) or sinks but these processes emerge later? Or, somewhere, a sentence that this budget focuses (necessarily) primarily on tropospheric processes?

Response: Thanks for the suggestion. We have revised the sentences as follows: "..land cover change. Ice core data show relatively constant tropospheric N_2O mixing ratio over the past two millennia (Canadell et al., 2021; MacFarling Meure et al., 2006; Fischer et al., 2019), from about 270 ppb in 1750 to well above 300 ppb. The tropospheric N2O mole fractions,, .." Regarding the stratosphere, we would refer the reviewer to line 157 to 160.

Line 110: "It" You mean 'these'? Or, 'these concentrations'? Eliminate this sentence,

because you do not need two successive mentions of ice core records?

Response: We are sorry for the unclear statement. "It" refers to the tropospheric N_2O mole fraction in 2022. We have revised this sentence:
"The tropospheric N_2O mole fraction in 2022 is higher than at any time in the last 800,000 years."

We mentioned the ice core records two times because the first sentence focuses on the tropospheric N_2O mole fraction, and the second focuses on the growth rate of atmospheric N_2O mole fraction. We'd like to express that both the current N_2O concentration level and its growth rate are unprecedented in the last 800,000 years.
Here, you use and cite units in fluxes of ppb per year. In abstract and next (emissions) paragraph you instead use Tg N per year. Settle on most useful or most appropriate set of units? Or, include a table that helps readers quickly convert?

Response: As stated in the manuscript, "ppb" is the most appropriate unit for atmospheric N_2O mole fraction, and "ppb yr^{-1}" is the unit for annual change rate of atmospheric N_2O mole fraction. "Tg Nyr^{-1}" is the most appropriate unit for N_2O fluxes. We added one sentence stating the converting factor from "ppb yr^{-1}" to "Tg Nyr^{-1}" in the revised manuscript: "The conversion factor from the unit "ppb yr^{-1}" to the unit "Tg Nyr^{-1}" is 4.79 Tg N ppb^{-1} (Prather, et al., 2012)."

Line 195: "most emitted"? Most often emitted? Most emitted by net (atmospheric) concentration or by flux? Most reactive? Need slight clarification here.

Response: Sorry for the unclear statements. We have revised the sentence as follows:
"which is the most important depleting substance of stratospheric ozone (World Meteorological Organization, 2022)"

Line 198: "mole fractions have increased by more than 25% since the pre-industrial era, from 270 parts per billion (ppb) in 1750 to 336 ppb in 2022" But, figures here only show data since 1980. If increase since pre-industrial values is true (as I accept), reader needs a citation or source for such data?

Response: Thanks for the suggestion. We have added the citation to the revised manuscript.
Reference :
Macfarling Meure, Cecelia, et al. "Law Dome CO_2, CH_4 and N_2O ice core records extended to 2000 years BP." *Geophysical Research Letters* 33.14 (2006).

Line 200: detailed and well-referenced sentence starting 'The 20th century rate' renders the previous sentence moot; reader does not need to see both. ESSD/Copernicus impose some punctuation standard for '20th century'?

Response: We have revised the sentences as follows: "The increase rate of atmospheric N_2O in the 20^{th} century is unprecedented over the past 20,000 years".
We think it's better to keep both sentences, because the first sentence focuses on the atmospheric N_2O mole fraction, and the second focuses on the growth rate of atmospheric N_2O mole fraction. We'd like to express that both the current N_2O

concentration level and its growth rate are unprecedented in the last 800,000 years.

Line 204: "growth rate of atmospheric N2O, the mean annual growth" need changed punctuation here, e.g. growth rate of atmospheric N2O: the mean annual growth.

Response: Thanks for the suggestion. We have revised accordingly.

Line 217: "Reducing N2O emissions is a required net-zero greenhouse gas (GHG) emissions and the recovery of stratospheric ozone" Something missing here? Required to meet GHG targets, and to allow (or foster) recovery?

Response: We are sorry for the grammatical error. We have revised the sentences as follows:

"Reducing N_2O emissions will contribute to the mitigation of global warming and the recovery of stratospheric ozone (Jackson et al., 2019)."

Line 218: Complete "N2O mitigation measures"? I think Pier's paper pointed out that remaining CO2 targets/budgets for 2C disappeared into the 'noise' of other GHG mitigation efforts (e.g. for N2O) but did not address N2O mitigation impacts directly?

Response: We have improved and make more complete statement based on a recent paper reviewing the needs for non-CO_2 greenhouse gas emissions reductions compatible with a number of temperature stabilization targets based on IPCC AR6 data (https://dhttps://doi.org/10.1038/s43247-023-01168-8). The revised sentences are as follows:
"Significant reductions of N_2O emissions are required along with net CO_2-emissions to stabilize the global climate system. For pathways consistent with the remaining carbon budget of 1.5°C, 1.7°C and 2°C stabilization, global N_2O emissions need to be reduced by 22%, 18% and 11 %, respectively, by 2050 (Rogelj and Lamboll 2024)."

Line 221: "Implementing N2O mitigation" you already said this in previous sentence. This sentence seems redundant?

Response: We have revised the sentence as follows: "All in all, implementing N_2O mitigation will contribute to achieving a set of United Nations Sustainable Development Goals (United Nations, 2016)."

Line 223: Nitrification and denitrification might both impact N2O production but, with one a source and one a sink, they can't both "contribute". Awkward phasing for most readers.

Response: N_2O can be produced and emitted in both nitrification and denitrification processes (Butterbach-Bahl et al., 2013; Gruber & James, 2008; Kuypers et al., 2018; Frestons and Davidson 1989). We have added the references to the revised manuscript.

Line 225 and following: good list but punctuation should change to semi-colon between each of 21 factors? Proof readers will know.

Response: We have revised accordingly.

Line 225 and following: check Fig 3 to ensure tight correlation with list here, by exact terms and directions of arrows? I think I counted 21 fluxes in Fig 3 but with uncertainty about whether to count bidirectional arrows as one or two terms?

Response: We have revised Fig3 by adding a red arrow from the coastal waters box back to the atmosphere. Each bidirectional arrow corresponds to one term and indicates that this term can either positively or negatively affect terrestrial N_2O emissions.

Line 262: But, no red arrow (indirect anthropogenic impact) from coast oceans box of Fig 3?

Response: This was an omission from our side and a red arrow from the coastal waters box back to the atmosphere has now been added to the revised figure. This change is consistent with the text that accounts for aquaculture (part of it being coastal) as a direct anthropogenic source and coastal emissions induced by N leaching as an indirect anthropogenic source.

Line 266: Good list but, strictly speaking, these should not fit in the category of terrestrial natural ecosystems?

Response: Sorry for the incorrect statement, these fluxes are in the category of "anthropogenic fluxes". We have changed "terrestrial natural ecosystems" to "terrestrial ecosystems".

Line 284: "multiple BU (BU) and TD (TD) methods" something missing or awkward here?

Response: Thanks for pointing this out. We have deleted "(BU)" and "(TD)".

Line 302: "all possible" but readers just learned that you had to ignore termite sources for lack of data? Perhaps all 'plausible'. Or, all 'quantifiable'? Change wording to reflect availability of reliable data? Lists and categories that follow seem reasonable and well-documented. Later (Line 1443) authors devote an entire paragraph to "missing fluxes". "All possible" remains confusing and/or inappropriate.

Response: Thanks for pointing this out. We have changed "all possible" into "all quantifiable".

Line 316: very important if slightly confusing paragraph. Put this in a table, instead? Ala Table 1 in GCB? Fluxes, change rates of fluxes, atmospheric concentrations: too much for reader to remember without a reference table?

Response: Thanks for the suggestion. We have added the unit table to the supplementary material.

Line 341: 'are' rather than "is"?

Response: We used "is" because "Which" refers to "the total N_2O emission".

Line 364: to " to develop" and "quantified" in same sentence? Need some attention to tense here?

Response: Thanks for pointing this out. We have changed "quantified" into "to develop".

Please ensure to define bottom-up (BU) and top-down (TD) once and only once, then attend to all subsequent uses of abbreviations to ensure coherence. E.g. to this reader, text in legend to Figure 4 (line 379) seems confusing?

Response: Thanks for the suggestion. We have revised this sentence as follows:
"We use both BU and TD approaches, including 20 BU and four TD estimates of N_2O fluxes from land and oceans."

Here, confusion threatens overall merit of this work. By this point reader has confronted 21 types of N2O fluxes, recasting of those types into six broader categories, definition of units (fluxes, change rates of fluxes, concentrations), parsing across geographical regions (including to a few specific countries), and - finally (!?) elucidation by 31 (more if one counts same model run at two different spatial resolutions) inventories and global, regional or process models. Huge effort by authors to compile all this! Please keep readers well-informed and cognizant of which source (or sink) estimates apply to which categories. The category 'Shelf' for example, which authors intend as one depth-limited region of perimeter oceans, remains confusing as used. Authors will know best what they need to report and how but, unfortunately, present parsing and arrangement implies perfunctory approach while authors prefer to project entire effort as careful and complex. Some better way to convey complexity and uncertainty? Not clear for me. I plead for better overall arrangement or at least an ongoing outline to help readers? Table 1 represents a comprehensive list, without reliability designation? Figure 4 complicates when it might clarify?

Response: We appreciate this comment by the reviewer, which highlights the complexity and completeness of this assessment. The reviewer very well identifies the challenges we were confronted with when compiling the multiple sets of data. We have been largely able to sort out the multiple approaches. Without seriously misinterpreting the individual approaches, it proved impossible to fully harmonize the terms used differently, and to fully bring into agreement the respective system boundaries used. Although it is not clear how to reorganize such a volume of information, as the reviewer acknowledges, we have highlighted in the text Figures 1 and 2 (overall budget and infographic) and Table 3 (more detail and numbers for the same), which were developed to guide the reader thru the multiple fluxes and numbers. A good reference to return to when the reader needs to place the information in context. Likewise, Figures 13, 14 and 15 were developed to guide the reader through the regional information. We think this guidance will assist the reader in navigating the paper. We are convinced - as also indicated by the reviewer - that the present manuscript provides a useful compilation of the available information.

Line 415: Copernicus publisher adheres to standard mechanism to handle 'submitted' references. Not this one, unfortunately; authors and editors need to correct.

Response: We have updated this reference:

Li, Ya, et al. "Increased nitrous oxide emissions from global lakes and reservoirs since the pre-industrial era." *Nature Communications* 15.1 (2024): 942.

Line 417: Which "observation-based analysis"?

Response: We have revised the sentence to avoid confusion:
"The analysis of Rosentreter et al. (2023) is observation-based and includes the contribution of coastal vegetated ecosystems,"

Line 423: percent of what?

Response: 44% of the total N_2O emissions from inland waters. We are sorry for the unclear statement. The results in Yao et al. (2020) suggested that 56% of the total N_2O emissions from rivers, reservoirs, estuaries and lakes were attributed to anthropogenic N additions, and the resting 44% of the total N_2O emissions were from natural sources. To avoid confusion, this sentence has been revised as:

"Using this approach, we estimated that N_2O emissions from natural sources of rivers, reservoirs, lakes and estuaries accounted for 44% (36%−52%) of the total emissions from inland waters."

Line 424: "Shelf processes" in Table 1 actually represent "continental shelves" as specified here?

Response: Yes. We have changed "Shelf products" in Table 1 to "Continental shelf products" to avoid confusion.

Line 434: If authors already listed all pertinent (hi-res) ocean biogeochemistry models in Table1, does reader need a second list here? Again, this reader wishes for some clarification: unable to assign priority to any one model, authors have chosen to use them all, to use a mean, to use a median? How do uncertainties from individual models penetrate into overall global estimates?

Response: In this paper we look at an ensemble of ocean biogeochemistry models. Therefore we use the ensemble mean of models as the "best estimate" and use the range of ocean model results (min/max) as an estimate for the uncertainty. Using an ensemble can give a better indication of the uncertainty compared to the uncertainty from a single ocean biogeochemistry model. That's why we do not provide uncertainty bars for each inversion result. Here we listed the names of ocean models to follow the convention that had been used in the section above on the land models - which had listed model names and main references

Line 455: First reports on deriving uncertainty info from source materials? Reader of ESSD needs more of the same?

Response: Thanks for the suggestion. We have added more descriptions of uncertainties in FAOSTAT, EDGAR, and NMIP2 estimates to Section 4.2.1.

Line 465: More useful to list FAO general reports first, then to deal with FAOSTAT specifics on fire types after? E.g. helpful to readers to change order of last and next-to-

last sentences?

Response: Thanks for your suggestion! We have changed the order of these two sentences according to your suggestion.

Line 470 and following: In this section authors need to include continental shelves in a more general 'coastal' category? Needs explicit mention/explanation?

Response: Here we include continental shelves in a more general "ocean" category which includes both open ocean and continental shelves. We have revised the sentence as follows:

"The emission from 'N deposition on ocean' was provided by Suntharalingam et al. (2012) which includes emission from both open oceans and continental shelves,"

Line 481: not clear where "56%" comes from?

Response: We are sorry for the unclear statement. The results in Yao et al. (2020) suggested that 56% of the total N_2O emissions from rivers, reservoirs, estuaries and lakes was attributed to anthropogenic N additions. Empirical methods (empirical models and meta-analysis) adopted this ratio to calculate long-term average anthropogenic N_2O emissions from inland waters, consistent with Tian et al. (2020). This sentence has been revised as follows:

"The anthropogenic emission from inland freshwaters estimated by Yao et al. (2020) considered annual N inputs and other environmental factors (i.e., climate, elevated CO_2, and land cover change). The results in Yao et al. (2020) suggested that 56% of the total N_2O emissions from rivers, reservoirs, estuaries and lakes was attributed to anthropogenic N additions. Empirical methods (empirical models and meta-analysis) adopted this ratio to calculate long-term average anthropogenic N_2O emissions from inland waters, consistent with Tian et al. (2020)."

Line 484: "low" or "act as a sink". Should reader assume you included these sources/sinks or ignored them as insignificant?

Response: We have deleted this sentence to avoid confusion.

Line 486: "SH1-SH7" and line 487 "SH1-SH8" From supplement reader learns that SH1 etc. represent set-up parameters for models involved in NMIP2 but readers need that informations sooner, e.g. here?

Response: We have moved the "Table A4. Simulation design of NMIP2." To "Table 2. Simulation design of NMIP2"

Line 491: "book-keeping" approach may account for deforestration/reforestration but you lost readers on broader issue of overall issue of land-cover changes on indirect (perturbation?) emissions?

Response: The reviewer is correct that bookkeeping methods could not account for indirect effects of environmental changes or emissions from some management

practices (e.g., fertilizer application) after land use change (e.g., climate effects). However, these effects are considered in other N2O emission sectors through the NMIP2 experiments. Such a use of the bookkeeping approach in our study exactly follows the methodology adopted in global carbon accounting (Friedlingstein et al. 2022).

Line 495 and following: Not clear what authors concluded here: should they use older estimate for NH3 oxidation and lightning production or do they ignore these processes as "small" and inconsistent or unquantifiable?

Response: We are sorry for the unclear statement and calculation error. For NH_3 source of N_2O: we used the mean value of two estimates : 0.4 Tg N yr^{-1} (Kohlmann and Poppe, 1999) and 0.6 (0.3-1.1) Tg N yr^{-1} (Dentener and Crutzen, 1994), the mean value is 0.5 Tg N yr^{-1} and the range is 0.3-1.1 Tg N yr^{-1}. For N_2O emission from lightning production, the estimate is 0.05 (0.02-0.09) Tg N yr^{-1}. Therefore, for N_2O emissions from lightning and atmospheric production, the estimate is 0.55 (0.32-1.19) Tg N yr^{-1}. We also have revised the description and updated the number in the global N_2O budget table (Table 3 in the revised manuscript).

Line 515: ppb to Tg conversion factor buried here, should appear more prominently in a 'units' table?

Response: We have added one 'unit' table and added the statement of conversion factor to the notes of Table 3.

Line 517 and following: Did authors use + 1.4% uncertainty or IPCC AR5 uncertainty? One applies to concentrations and other to concentration changes? Not clear to this reader.

Response: Sorry for the unclear statement. We have revised the sentences as follows:
"Combining uncertainties in measuring the annual mean surface mole fraction, which are <1 ppb (Dlugokencky et al., 1994), with those of converting surface mole fractions to a global mean abundance, we estimate a ±1.4 % uncertainty in the absolute burden (Prather et al., 2012). The uncertainty in the ppb-to-Tg conversion does not affect the trend uncertainty. This uncertainty is estimated to be ±0.2 ppb or ±1 Tg N between any two years over any recent period, based on the combined NOAA and AGAGE record of surface N_2O taken from Table 2.1 of the IPCC AR5 (Hartmann et al., 2013). Thus, the uncertainty in the burden change between two decades (e.g., 2000s to 2010s) is bounded by ±1 Tg N (<0.1 %)."

Line 524: another example of a process (tropospheric loss) too small to appear in overall N2O budget?

Response: Yes, tropospheric chemical loss occurs at a very low rate, thus we did not include this item in the budget.

Line 533: 'is' rather than "as"?

Response: Should be "was" rather than "is". Sorry for the grammatical error, we have corrected it.

Line 548: additional uncertainties introduced by these interpolation or re-gridding steps?

Response: This interpolation doesn't affect the global statistics. Sorry for the grammatical error, we have corrected it.

Line 553: Back at line 319, we read "Unless specified, uncertainties are reported in brackets as minimum and maximum values of all estimates". Data presented here (e.g. 315.8 ("315.5-316.2) ppb in 2000 to 335.9 (335.6-336.1) ppb in 2022") follow this convention? Values in parentheses represent min and max? But, each source (e.g. NOAA, CSIRO) will have gone to lot of effort to identify uncertainties of their N2O measurements, reported not as + min/max. Min/max tells readers very little about distributions or uncertainties? Please can authors adopt, and adhere consistentily to, better more reliable more informative uncertainties?

Response: Yes, uncertainties in the atmospheric N_2O mole fractions also follow this convention, using minimum and maximum to represent uncertainty. We acknowledge that min/max can't tell much information about the distribution of uncertainty. However, considering that many of the identified N_2O sources only have 2 or 3 products/estimates, we can't get much information about the distribution of uncertainty or just simply assume the normal distribution of uncertainties. Considering these facts, using minimum and maximum to represent uncertainty is the best choice for our study.

Line 556: "was" implies singular but this sentence refers two (plural) years, 2020 and 2021?

Response: Sorry for the grammatical error. We have corrected it.

Line 556: "30% higher than the average value in the decade of the 2010s" Not sure that readers can confirm this information from Figure 2? Inset of Figure 2, which purports to show annual growth rate, has no uncertainties, no demarcation of decades, nothing to help readers follow (or, dispute) authors' conclusions. Help, please.

Response: The inset of previous Figure2 shows the growth rate of N_2O dry mole fraction at a monthly resolution. We have revised it into the annual resolution and added dash lines to indicate the levels of N_2O growth rate in the 2000s and 2010s.

Line 563, Figure 5: No uncertainties in obs nor in model outcomes?

Response: We have added a table in supplementary material (Table SI-3) reporting the uncertainties in observed atmospheric N_2O concentrations and the future predictions.

Line 571: "with large uncertainties (Figure 6)." What uncertainties? + min/max as above?
Something different here? No information! Nothing in panels or figure legend?

Response: Yes, we used min/max to represent uncertainty. We have deleted ", with large uncertainties" to avoid confusion. We have also added the following statements in the caption of Figure 6"

"For each sub-figure, the line represents the mean N_2O emission of different estimates, and the shaded area shows minimum and maximum estimates."

Line 579, Figure 6: Except for panel C (Other direct anthropogenic emissions), all uncertainties (if shown by color ranges) appear to increase, 1980 to 2020? Not a positive report? Should readers assume that capability to construct N2O budget decreases, because uncertainties increase? Not what this reviewer would have expected as measurements and models all improve? If end of 2020 total of 6.7, with range of 3.3 (minimum or ?) to 10.9 (maximum or ?), how can reader trust anything that follows about sectors or regions? If authors expect readers to accept time-dependent changes in N2O sources or sinks, those readers will need to trust authors' handling of received as well as generated uncertainties. No evidence provided here.

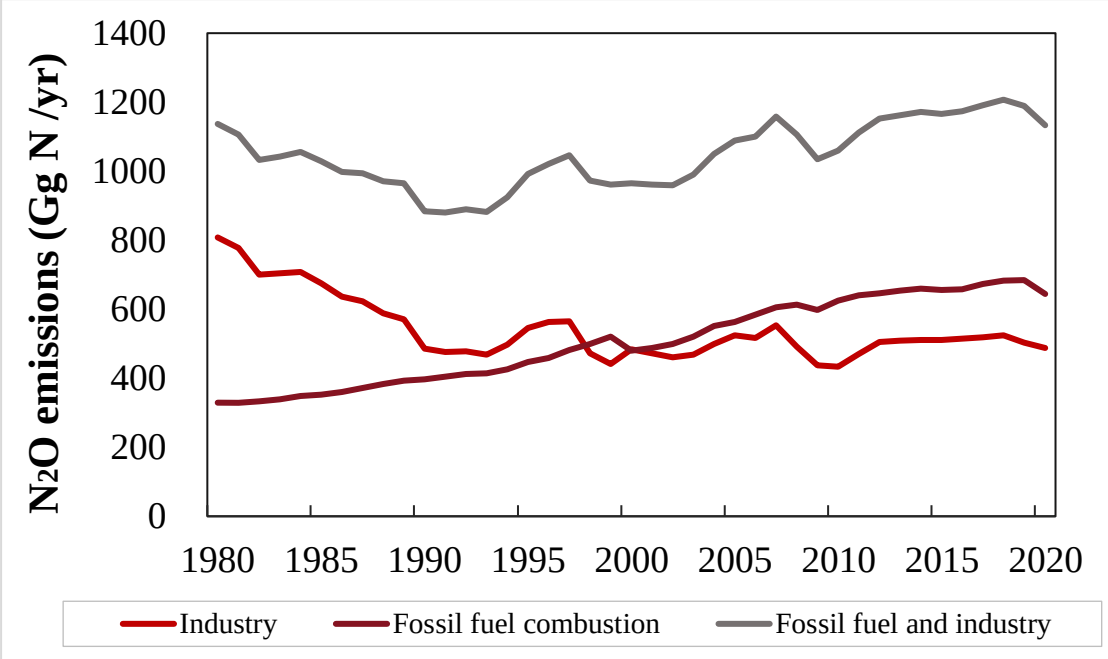
Response: We acknowledge that the range of maximum-minimum estimates increased for most sectors (panel B, D, E), however, this is mainly caused by the increase in the overall magnitude of emissions. For the relative uncertainty ((maximum-minimum)/mean), direct emission from agriculture and indirect emission (panel B and D) only show slight increase, other direct emission (panel C) shows significant decreasing trend. The increase in the estimated ranges particularly increases reflects the increasing in the number of multiple sources of emission data. Our analysis has followed the principle that a larger member ensemble, in this case of estimates coming from diverse approaches, provides a more robust and higher confidence mean value, than using a smaller number of data sources, but with the downside of having larger ranges. This approach is consistent with the IPCC Guidance for Consistent Treatment of Uncertainties in its assessment reports, which we have followed in our analysis. We think this is best approach available to us at present given the diversity of data, and we acknowledge the need for improvements in future the way uncertainties are expressed in complex data syntheses.

Line 583 and following, sections on agriculture, other direct, etc.: Lots of work here, compiling and reporting data from trusted sources NIMP2, EDGAR, FAOSTAT, etc., but not one mention of uncertainties. Do all these sources produce 'perfect' data? I know, and readers will know better, that each source spends a great deal of time and effort to identify and report uncertainties. All of that effort and info lost here? Not one error bar or uncertainty envelop in any of these figures? Authors need to provide readers a basis to trust these conclusions, to respect authors' good efforts, but nothing presented here provokes nor supports such respect. Fossil fuel N2O emissions have remained unchanged for four decades? Weakness in reporting? True? How can any reader know? We need to trust authors to provide explanation with documentation and uncertainties! Instead, nothing provided!

Response: Uncertainties in estimates of FAOSTAT, EDGAR, and NMIP2 are as follows: (1) FAOSTAT: from Tubiello et al. (2013): Fig. 3 pg. 7, uncertainty in FAOSTAT N_2O emissions is ~ 60% across typology. In fact, it is asymmetrical, following 2006 GL values and IPCC uncertainty formulae, with umin ~-30% and umax ~90%; (2) EDGAR: the global uncertainties in EDGAR for N_2O are provided in Solazzo et al. (2021) (Table 6) for all sectors. The uncertainty in N_2O emissions ranges for Energy between 113.3% and 113.3%, for IPPU between 15.7% and 12.4%, for agriculture between 301.7% and 224.9%, for Waste between 202.6% and 159.0%, and

for Other sectors between 111.8% and 111.8%. We would like to highlight the fact that N2O emissions from agriculture in EDGAR are very uncertain. (3) NMIP2: we also calculated the uncertainty in NMIP2 estimates of direct agricultural emissions, the maximum estimate is about 60% higher than the ensemble mean, and the minimum estimate is about 40% lower than the ensemble mean. We have added the uncertainties of FAOSTAT, EDGAR, and NMIP2 into Section 4.2.1 and 4.2.2.

In the manuscript, we reported that N_2O emissions from fossil fuel and industry (including emissions from fossil fuel combustion and industry) only slightly increased during 1980-2020, mainly because that the increase in N_2O emissions from fossil fuel combustion was largely offset by the decrease in N_2O emissions from industry (see the following figure), including emissions from chemical processes, solvents and products use as described in EDGAR database (Solazzo et al. 2021). N_2O emissions from fossil fuel combustion significantly increased during the past four decades.



Line 633, "both DLEM and book-keeping approach suggested increasing uncertainties in postdeforestation pulse effect". Really? Increasing uncertainties? Expressed as min/max, 95CI, per cent of total, + x sigma, what? If authors want readers to accept this contention about perturbation fluxes, those readers will want to have seen consistent approach to uncertainties up to this point and will need more details here.

Response: We apologize for the inaccurate description. The text has been revised as "The spread between different estimates (DLEM and the bookkeeping method) on post-deforestation pulse effect increased from the 1980s to the 2010s". Such a spread increase features the discrepancies of the two methods in accounting for the long-term changes in N_2O fluxes in deforested areas.

Lines 648 and following: Reader would like to accept that natural N2O fluxes have not changed over four decades but a) this reader doubts that contention and b) authors have given no indication of their skill or knowledge to back up such contentions.

Response: Sorry for the unclear statement. Among all sources, natural emissions from shelves, inland waters, and lightning and atmospheric production were assumed to be constant during 1980-2020. According to BU approaches, the total natural emissions from these sources were 1.7 (0.9-3.0) Tg N yr^{-1}. Emissions from other natural sources including soils and open oceans kept relatively steady throughout the study period 1980-2020, with mean estimates fluctuating between 9.9-10.3 Tg N yr^{-1} (minimum estimates: 6.2-7.1 Tg N yr^{-1}; maximum estimates: 12.8-13.6 Tg N yr^{-1}). We have revised Section 3.2.2 as follows:

"Emissions from natural soils and open oceans kept relatively steady throughout the study period 1980-2020, with mean estimates fluctuating between 9.9-10.3 Tg N yr^{-1} (minimum estimates: 6.2-7.1 Tg N yr^{-1}; maximum estimates: 12.8-13.6 Tg N yr^{-1}). Natural emissions from all other sources including shelves, inland waters, and lightning and atmospheric production were assumed to be constant during 1980-2020. According to BU approaches, the total natural emissions from these sources were 1.7 (0.9-3.0) Tg N yr^{-1}. The mean value of global N_2O emissions from all the above-mentioned sources fluctuated between 11.5-11.9 TgN yr^{-1}, with an average of 11.7 TgN yr^{-1}. Global natural N_2O emissions have a large uncertainty, with the maximum estimates (15.8-16.6 TgN yr^{-1}) roughly double the minimum estimates (7.0-8.0 TgN yr^{-1})."

Line 650 and following: do authors now (or, again) present minimum and maximum values? Doubtful, but no information given to help readers decide? Readers would like to accept authors' contentions on all these natural N2O fluxes but, without uncertainty information and specifications, how can we?

Response: Yes, as stated in section 2.1: "uncertainties are reported as minimum and maximum values of all estimates". In section 3.2.2, we have reported the minimum and maximum values of estimates for emissions from different natural sources.

Line 685, TD estimates: Inversions, as published, include substantial uncertainties. As presented (plotted) here, however, readers get no indication of authors expertise at assimilating and assessing TD info. No error bars on any plot, can't be true? What valid signals emerge from what noise? No sense of that provided in this section.

Response: In this paper we look at an ensemble of inversions and use the range of inversion results as an estimate for the uncertainty. Using an ensemble can give a better indication of the uncertainty (due to atmospheric transport and to the specific parameters used in the inversion, e.g. choice of observation and prior uncertainties) compared to the uncertainty from a single inversion framework. That's why we do not provide uncertainty bars for each inversion result. Also, not all these inversion frameworks provide uncertainties.

Line 778, Figure 13: Total N2O emissions (panel A), look nothing like panel A of Figure 6 (total anthropogenic). Total here represents Fig 6 plus non-varying natural total of 12 (Line 651)? 7 plus 12 gives 19, perhaps within uncertainty (?) of this Figure? If this reader has made too simple assumptions or additions, authors have not given sufficient information to prevent my errors? What do uncertainties shown here represent? Cumulative for BU and TD? From where? If we can't understand panel A, why should any reader give any credence to panels b to t? If I calculate instead from numbers given

in Table 2 (again, with min and max?), I get 18 BU or 17 TD total N2O sources, minus stratospheric sink of 13 or total atmospheric sink of 14 to get a residual in atmosphere of 3 to 5? Compared to 6 as provided authors? Sorry, does not compute correctly. In notes to Table 2 one reads that uncertainties follow AR5 with detailed notes provided in Supplement. Sorry again, this reader will not comb through supplement to elucidate uncertainty info. Many readers will simply give up at this point.

Response: We have double checked Figure 6 and Figure 13 (a), there is no calculation error. Figure 13 (a) looks different from Figure 6, mainly because it (total emission) includes natural emission (mean: 11.6 Tg N yr^{-1}, min-max: 7.2-15.9 Tg N yr^{-1}), which makes uncertainty range larger. For (a)-(t), "the blue lines represent the mean N_2O emission from bottom-up methods and the shaded areas show minimum and maximum estimates; the red lines represent the mean N_2O emission from top-down methods and the shaded areas show minimum and maximum estimates." We added the above description to the caption of Figure 13 to avoid confusion.

Lines 790 to 1084, regional extrapolations. More hard work by authors but basically bogus without uncertainties or some mechanism to validate?

Response: From Line 790 to 1084, we provide the uncertainties of most fluxes and their change rates (minimum and maximum). It's not objective to describe our regional analyses as "without uncertainties". Since it's not feasible to directly measure or observe N_2O fluxes at regional level, we are unable to really validate the estimates of fluxes reported in our paper. Therefore, we compare the top-down estimates with bottom-up estimates to give readers the information of reliability of our reported numbers. This paper synthesizes state-of-the-art N_2O flux products to estimate global and regional N_2O budget, we don't think it's appropriate to say our results "bogus".

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