Comments on essd-2026-109

The manuscript by Nazir and colleagues introduces a database on global aquatic N_2O emissions that includes both inland and marine systems. To this end, the authors compiled data from different sources for the period 1980–2023 and applied statistical analyses with the aim of quantifying the overall emissions and provide mechanistic insights on the main drivers of N_2O production and fluxes across the air-sea interface.

While the relevance of a continued improvement on our understanding of the cycling and emission of N_2O across ecosystems and over different time scales is clear, the observational basis, line of argumentation and methodological approach of this manuscript are insufficient to substantiate the claims regarding the global distribution and fluxes of N_2O; in particular, for the marine realm.

The major issue is that in their compilation the authors excluded an overwhelmingly large amount of data on N_2O concentrations which are indeed available. This is especially evident when comparing the data coverage of marine systems on Fig. 4 and published work by e.g. Yang et al. (2020) and Resplandy et al. (2024) (which was indeed cited by the authors themselves). As can be seen on the manuscript, the data coverage of the global ocean is restricted a handful of data points, which clearly does not reflect the state of the art. Since a few of the manuscripts cited by the authors (e.g. Resplandy et al., 2024) explicitly refer to large databases such as MEMENTO, it is surprising that the authors opted for not using them. Other examples of databases which are openly available and have been used for work on N_2O, such as GO-SHIP and PANGAEA, were also not considered.

Although –as discussed above- the data scarcity is particularly evident for open and coastal seas, even for inland aquatic ecosystems, recent work (see e.g. Zheng et al., 2022) does provide a compilation of observations which was disregarded in this manuscript. With regards to anthropogenically-driven emissions, which is a topic explicitly addressed by the authors, there is recent work by e.g. Tang et al. (2024) who provides solid data on the contribution of such settings where natural and human-impacted areas are adjacent.

In the absence of a reasonable explanation for the omission of data from both inland and marine aquatic systems, it is unsettling to read that a "comprehensive" emission data base (as stated in the title) is presented, and that "(…) A formal validation against an independent global reference product is currently not possible due to the lack of a globally consistent benchmark dataset for aquatic N_2O observations across all major water bodies (…)." (see l.289–290). This, especially since the authors themselves cite the work by Tian et al. (2024), in which the current figures for different systems are included. Admittedly, that work is not a database per se; yet, stating that the proposed dataset is novel and complete is not factually correct and suggests a lack of rigour in the conception and execution of the study.

In going through the manuscript, it is apparent that whereas most of the results and discussion are devoted to inland waters (in which the data deficit is still visible, yet reduced), the emissions and microbial controls for marine systems are marginally interpreted, ignoring the large body of available literature on the topic. Considering that the authors state the ocean and coastal regions to be the “largest contributors to global aquatic N_2O emissions, with oxygen-deficient zones disproportionately responsible for more than half of the total flux” (see l.67–69), it is then surprising that later in the discussion, the ocean is depicted as a sink with minor relevance (e.g. Section 3.3, l.529–531, l.740–741). In addition to the under-representation of the marine observational component in this study, which is already problematic, the interpretation of the results is misleading. For instance, the authors explain the low N_2O fluxes from oceanic regions by suggesting that N_2O is produced in oxygen minimum zones and as those have a limited extent in comparison to the global ocean, this supports their conclusions (see l.549–553). Such a statement is not only insufficiently substantiated by the scarce data provided in this manuscript, but also suggests that the authors are unaware of the disproportionally high share of coastal upwelling systems, open ocean (equatorial upwelling) and the associated, underlying oxygen minimum zones to the global budgets of greenhouse gases, including N_2O. There are many sources to illustrate this point, but succinct overviews such as Codispoti (2010) and Capone and Hutchins (2013) suffice to put in evidence that the discussion in this manuscript is, at the very least in this regard, flawed.

In addition to the observational constraints and thereby resulting, unconvincing interpretation, the framing of the study and its relevance has several inconsistencies that can be misleading for the reader. For instance, presenting values for global N_2O emissions based on a study that explicitly considers only streams and rivers (Yao et al., 2020; see l.41–43) is not representative in this context. Likewise, the comparison of bottom-up emission estimates with Tian et al. (2024) does not make sense, as the latter does include both inland and marine sources (l.43–44). Other example of the incorrect usage of references include Rees et al. (2022; a paper dealing with polar, well-oxygenated waters), which is used by the authors to discuss the role of oxygen minimum zones for N_2O production. Like this, there are examples throughout, that show inconsistencies in the study and put in evidence lack of rigour in its preparation.

The crux of the paper is mounted on three major objectives (see l.94–96):

“(1) construct a harmonized database for data acquisition and observation”

“(2) characterize the distributional patterns of N_2O concentrations and fluxes across various aquatic ecosystems”

“(3) identify existing data gaps and propose future research directions to advance the understanding of N_2O dynamics in aquatic ecosystems”.

With regards to (1) it can be argued that while such a database for inland and marine waters does not exist, individual products are readily available and steps towards harmonization are in place (see e.g. Wilson et al., 2018; Wilson et al., 2020; Petrescu et al., 2021).

With regards to (2), there are plenty of manuscripts (see e.g. Rosentreter et al., 2021; Zheng et al., 2022; Aho et al., 2023; Bange et al., 2024), which address this point. Although none of those aims to address the distribution in all ecosystems at the same time, the global vertical and zonal distribution is rather well known. Insights that would have been novel could have addressed, for instance, the uncertainties in the extent of the fluxes, their seasonal variability and/or the variability in cycling pathways. Based on the statistical analyses presented in this manuscript, it is not clear how (2) advances our knowledge on the matter beyond significant correlations amongst a set of predictor variables.

As for (3), and considering the aspects highlighted above, it can be argued that the identification of gaps from this study lacks the robustness needed to substantiate such claims.

Because of the points above, the manuscript does not provide conclusive evidence that the proposed database delivers the products listed in l.103–105.

Regarding the methodological approach, aspects which are worth mentioning include:

- The data compilation includes master and doctoral theses, as well as "selected reports", which according to the journal's rules (available under: https://www.earth-system-science-data.net/submission.html#references) are not allowed: "Works cited in a published manuscript should be published already, accepted for publication, or available as a preprint with a DOI."
- In section 2.1 (specifically l.120–124), the authors describe the procedure followed to search for data in bibliographic databases and repositories. After trying to mimicry that approach with some of the key words listed in the manuscript, the results show that the relevant literature (and databases) which are missing in the study, do appear. Under this scenario, it is unclear why these were not used for this manuscript.
- In l. 124–128, the authors explain the exclusion criteria used for determining which datasets would flow into the study. Based on that description, it is unclear if one, several or all of the criteria have to be met in order for the dataset to be included. If all criteria would have needed to be met, this would explain (at least partially) why so many data sets were omitted. However, even then, it does not appear justified to do so, as there are other mechanistic relationships between, e.g. nutrients, oxygen and N_2O, which could have allowed creating e.g. a gridded product.
- In l. 143–144, the authors indicate that the proposed database "builds upon analogous compilations in aquatic GHG research". However, it is unclear which are those compilations.
- In l.185 and following, the authors indicate that data points with minimal differences in coordinates were merged and used as a single site. It is unclear what exactly does this mean (average values, gridding procedure, etc.). While this seemed to refer to horizontal variability, for vertical profiles the authors state that multiple points or depths were averaged to provide a representative value. Under circumstances in which a system is very shallow and/or stratification is not present, that might be a valid assumption. However, this is not the dominant situation in aquatic ecosystems. Indeed, variability across gradients in e.g. salinity and oxygen are typically hotspots for N_2O production. Because of this, this assumption bears a serious risk of masking some of the natural variability in many of the datasets.
- In l.253–256 it is explained that flux data derived from different sources was used for the compilation. Yet, a discussion on the compatibility of those methods (e.g. chambers vs. estimating air-sea gradients) is not provided. Also, although recalculation of gas transfer coefficients (k) based on previous syntheses are

mentioned, neither the exact procedure nor the corresponding citations were included.

- l.257–260: Based on the current description, it is unclear how including isotopic data helps increasing the measurement accuracy and comprehensiveness of the proposed database. One could generally agree in that having isotopic evidence provides more insights into processes than what could be gained by evaluating concentrations alone. However, having the data on a compilation per se does not result in that effect.
- The procedures followed as part of the quality assurance are superficially (and not quantitatively) mentioned, which makes it challenging to judge on the credibility of the approach.

Given the points raised above, detailed commenting on the results and discussion sections would be redundant, as the observational basis is incomplete.

It is worth noting that the caveats presented above go beyond missing citations of relevant literature and are of fundamental nature. The lack of data which was indeed available (especially in the marine realm), as well the lack of novelty, led this study to provide conclusions which are not adequately substantiated, and present a misleading view of global aquatic N_2O emissions. While, overall, the idea of a combined product of N_2O emissions across several aquatic ecosystems is certainly interesting, the approach followed in this study does not suffice to achieve that goal.

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Databases

https://memento.geomar.de/de.html

https://library.ucsd.edu/dc/object/bb3391883k

https://www.pangaea.de/

Kind regards,

Damian L. Arévalo-Martínez & Hermann W. Bange