Review of essd-2026-109, Nazir et al., submission

This manuscript presents the Global Aquatic N_2O Emission Database (GANED) by compiling over 5,000 concentration records and over 7,000 flux measurements from 426 data sources, including peer-reviewed articles and academic theses/dissertations. The effort of this synthesis is commendable, and the dataset will be a valuable community resource for constraining aquatic N_2O budgets. In fact, there has been an existing MEMENTO database (https://eos.org/science-updates/counting-oceans-greenhouse-gas-emissions) for oceanic N_2O concentration and fluxes. The authors should cite the previous work and to acknowledge the researchers working in this field. And there are justification to be presented about the improved aspects for this database compared to previous ones. At present form, several methodological and interpretive issues need major clarifications.

Below are major comments, mostly related to data processing and interpretation:

1. Lines 120–125: The description of literature search across Web of Science, Scopus, and Google Scholar lacks screening of duplicate records, as the same article may be recorded in different databases. The authors should make sure one record is counted only once. The number of unique sources retained from each database after duplicate removal and screening should also be briefly summarized.
2. Lines 226–252: It seems that the authors combined flux data derived from concentration-gradient calculations and chamber-based measurements. These two methods differ not only in parameterization but also in sampling scale and measurement principle. It remains controversy whether these datasets, having different inherent biases, are comparable, or can be combined as a whole for subsequent analyses. Lacking these information makes it difficult to evaluate whether the main conclusions are robust.
3. Lines 270–275: The removal procedure of outlier datapoints needs justification. Given the size of dataset, and the skewed distribution nature of N_2O concentrations and fluxes, excluding values greater than three standard deviations from the mean within each aquatic type may remove real “hotspot” observations rather than erroneous records.
4. Line 315: Calculating the proportional contribution of different aquatic systems to total N_2O emissions is unclear. I am confused whether these percentages are based on simple aggregation of observations, spatially weighted estimates, or upscaled global fluxes.
5. The temporal trends in Figs. 6 and 7 may be affected by changes in the sampling density and locations. For example, annual means for “rivers” may reflect varying proportions of headwater streams, agricultural drainage channels, or urban-impacted rivers, rather than realistic temporal changes in N_2O levels. This limitation should be considered when interpreting the temporal trends.

6. Fig. 9 shows large variabilities in N_2O concentration and flux within an aquatic system; take "River" for example, N_2O concentrations could span nearly six orders of magnitude. This raises uncertainty about how much of the total variation is actually explained by the different categories, or simply just because the sampling density? The manuscript should discuss the limitation that within-type heterogeneity may be driven by finer subcategories such as polluted rivers, background rivers, agricultural drainage channels, or urban-impacted systems.
7. Line 305, Fig. 10 and Fig. 2: The data-processing procedure used for the correlation analysis is insufficiently described. It is unclear how missing values, repeated observations, aggregated records, different flux measurement methods, and potential non-independence among records from the same site were handled before calculating the correlations. The correlation should distinguish more clearly between R and R^2. R^2 cannot indicate positive or negative associations; yet Fig. 10 and explanatory texts use R^2 together with correlation. This statistical notation should be made consistent throughout the main text and figure.
8. In fig. 10, the non-significant relationship between N_2O flux and TN needs to be clarified, as N_2O flux is reported to be strongly positively correlated with NH_4^+, NO_3^-, and NO_2^-, which are components of TN.
9. Based on the dataset, the Random Forest model may be limited in the predictive performance, having an overall $R^2 = 0.13$ (line 481 - 482) and low or negative R^2 values for some aquatic systems (stream, reservoir and ocean). The statement about the RF model having "robust predictive capability" appears to be overstated (line 481). The RF model may be useful in identifying the most important predictor, e.g. DIN. In table 3, the "Feature Importance" should be interpreted more cautiously, as they indicate relative importance within a weak predictive model rather than strong evidence for dominant controls on N_2O flux.
10. Section 4.3 provides a mechanistic discussion of temperature effects across aquatic systems, but the relationship between water temperature and N_2O flux in a global setting is weak and non-significant (as in Figure 10). System-specific temperature effects may exist, but the current analysis does not provide sufficient explanations.
11. The authors attributes the strong heterogeneity in GANED mainly to real differences in N_2O production and emission processes (Section 4.4). However, it should be noted that the database inherits the data variabilities from sampling design, observation frequency, and measurement approaches. Without separating these sources of variability, the interpretation of heterogeneity as an inherent process-level feature may not be justified.

Minor comments:

1. The abstract should contain a brief explanation about how these heterogeneous datasets were harmonized, standardized, and quality-controlled before cross-system comparisons and correlation analyses.
2. Line 39-40: There appears to conflate atmospheric N_2O concentration with N_2O emissions; ppb is not an emission unit.
3. Line 128: As pH is not included in subsequent analysis of predictor, the authors needs to explain here.
4. Lines 145–150: It is unclear whether concentration and flux measurements collected at the same site and date can be directly matched at the record level.
5. Lines 165–170: The details of the authors' own data need to be described, i.e. the size of dataset, and whether including these data could have changed the interpretation in certain aquatic systems.
6. Table 1 lists several special site or channel types that may influence N_2O concentrations or fluxes, but the distinction between coded and uncoded sites is not clearly defined. It is also unclear whether these codes were used in the subsequent analyses; if not, the rationale should be stated.
7. The number of flux records is inconsistent across the Results section. The database is first described as containing 7,386 flux records, whereas Fig. 9 is a compilation of 6,456 flux measurements. If this difference results from filtering missing or invalid values, the filtering criteria should be stated clearly.
8. Lines 315–320: The authors indicate that only 2% of sites have more than 10 concentration observations and 4% have more than 10 flux records, suggesting very limited temporal coverage. The meaning of "total = 1629 observational counts" and "total = 3807 counts" should be clearly stated, and the manuscript should better justify how such sparse long-term observations support the subsequent temporal trend analyses.
9. Lines 450–465: The interpretations of correlation analyses appears to be inconsistent. DOC is described as positively associated with N_2O flux, whereas the abstract and Table 2 report a negative correlation; water temperature is described as having an inverse relationship, whereas Table 2 shows weakly positive and/or non-significant relationship. These inconsistencies need to be double checked throughout the text in the next round of revision.