In this manuscript, the authors present newly estimated global ocean N_2O flux to the atmosphere and its confidence interval using observations and two submodels of N_2O production. The paper provides interesting insights but the writing could be improved to make the manuscript clearer. The main problem of the paper, as I see it, is that there are not enough details to assess the validity of the model and results. Below are some major comments and questions, followed by minor edits.

Major comments/questions:

It is unclear how the authors calculate the best estimate of N_2O production using observations (l. 82). How is the range obtained in this case? I thought that the authors might be using the maximums and the minimums of each factor to calculate the range but that does not seem likely.

I am having hard time understanding the equation 1. How is this equation derived and why are such large significant figures used? This equation does not account for the latitudinal dependence of pN_2O - wouldn't that be a problem? Isn't it better to use atmospheric model results validated by atmospheric measurements of N_2O?

I think there might be a mistake in equation 2. Otherwise, I do not see how a value of 2 could mean that the model deviates form the observations by a factor of 2 in either direction. $10^{(10\log 2)} = 1024$ and it is nothing close to a value of 2. Please explain. Perhaps, the standard mathematical notation (summation and the number of observations n rather than "average") would be more appropriate here.

It would be useful if the N_2O flux calculation in section 2.7 is explained in a little more detail, rather than stating that it "is calculated with the piston velocity from Sweeney et al. (2007)." I am not familiar with this calculation and would love more explanations on how the ocean N_2O flux is estimated but the Sweeney et al. (2007) is not listed in the references either.

I am not sure how equation 3 is used to determine the global air-sea flux of N_2O that best fits the ΔpN_2O data, if RSS/RSS_{min} just depends on the number of observations. I do not understand how different model simulations would have different values of RSS/RSS_{min} if the number of observations is the same.

As for equation 4, I think that its application should be described within the methodology section, rather than just mentioning a little in the discussion section.

Also, how did the authors optimize various model parameters? And is it not a problem that the optimized oxic $\Delta N_2O/AOU$ slope of 12.7 μmol N_2O (mol O_2)$^{-1}$ is so different from the global average given earlier in lines 77-78 (81.5 ± 1.4 nmol/mmol)? What is the value for this parameter in the prognostic model?

Minor comments
1. L. 24 "It also currently" → "It is also currently"

2. There are several places in the text, where more detailed or clearer explanations would help readers understand the paper better. For example, l. 53-56 is unclear what the sentence means. Do the authors mean that $\Delta N_2O/AOU$ slope becomes negative under suboxic conditions and that leads to the ambiguity of how much N_2O is produced in this region? Please clarify.
3. L. 71 "observationally derived" $\rightarrow$ "observationally-derived"
4. L. 75 Since not all readers of this paper are experts in oceanic biogeochemistry, it would be helpful to explain that the f-ratio is the fraction of total primary production by nitrate.
5. L. 79 What is the "-O_2:C ratio"? What is the dash for?
6. L. 233 "N cycle based" $\rightarrow$ "N cycle-based"
7. L. 242-246 "This estimate…" run-on sentence and needs to be rewritten.
8. L. 263-267 "It should also…" run-on sentence and needs to be rewritten.
9. L. 286 "140 pm" $\rightarrow$ "140 ppm"
10. L. 290-294 "On the one hand…" run-on sentence and needs to be rewritten.
11. N-cycle data database used in this paper are shown as embargoed in the data source pointed by the authors (https://www.uea.ac.uk/green-ocean/data). Will the data be publicly available?