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Interactive comment on "Oceanic N_2O emissions in the 21st century" *by* J. Martinez-Rey et al.

Anonymous Referee #2

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Review of "Oceanic N2O emissions in the 21st century" by Martinez-Rey et al.

The manuscript by Martinez-Rey et al. uses a current-generation Earth System Model to predict changes in N2O emissions during the 21st century under the RCP8.5 business as usual emission scenario. N2O is an important greenhouse gas that affects the atmosphere's radiative and ozone budgets. Hence, understanding how natural sources of N2O will evolve under a changing climate is an important question. N2O emissions depend on biogeochemical sources, ocean circulation and air-sea exchange. ESM provide a natural framework to represent these processes in a physically consistent way.

The main findings of the paper is a (minor) decline in N2O production and emissions and increase in N2O inventories in the simulations, resulting from compensating changes in oceanic sources (following warming, declining export and nitrification, gen-

eral deoxygenation), and a decrease in air-sea fluxes driven by increased stratification. Increased stratification dominates the overall transient response, producing the most robust results. The predicted decline in marine N2O emissions is nearly equal to the projected N2O increase from terrestrial sources, potentially offsetting it.

ESM projections as the ones presented by the Authors are necessary but difficult, and suffer from large uncertainties. These include model biases, shortcoming in parameterizations, and results (e.g. N2O production changes) that often depend on the compensation between opposite but largely uncertain terms. Clearly framed simulations could help disentangle the role and magnitudes of the various mechanisms at play. In this prospective the Author's work is welcome. However, aspects of the work are not systematic enough to entirely support all the conclusions, and clarifications are necessary. I also worry that some of the conclusions might be model-dependent and hence not robust enough. On the other hand, the work highlights several aspects of N2O cycling where additional research is needed.

The manuscript is well structured and written, and generally clear. Similarly, the figures are clear and support the analysis.

Specific comments:

- My first concern is the use of the IPSL-CM5A-LR model, mostly because of its seriously deficient O2 simulation. The Authors clearly state that most current ESMs have a hard time getting the right O2 patterns (especially low-O2 regions). However some models perform better than others. In the upper ocean (0-1000 m), IPSL-CM5A-LR strongly overestimates O2 (on average by 50-100 mmol/m3). Hence it underrepresented quite dramatically the extent of low-O2 waters where most of the enhancement of N2O production in the low-O2 pathway takes place. Similarly, anoxic waters in IPSL-CM5A-LR are almost missing, biasing the representation of the (already uncertain) N2O dynamics related to denitrification. Finally, most low-O2 waters in IPSL-CM5A-LR are found below 1000m in the deep North Pacific, where they would intercept very

little organic matter fluxes. Figure 3C acknowledges some of these biases, but the discussion in the manuscript is lacking. The Authors should be more upfront about these biases, and should put more effort in discussing how they could affect the results, especially the claim that changes in the low-O2 pathway are negligible. Given how small OMZ are to start with, especially in the upper ocean where most nitrification takes place, I'm not surprised that the model puts so little emphasis on this pathway.

- The same goes for the projections to 2100, especially related to the evolution of OMZ in the tropics. As the Authors point out, the tropics are regions of disagreement among ESMs. IPSL-CM5A-LR seem to predict an O2 increase in the Atlantic tropical OMZ, and a more complex pattern in the Pacific, with overall O2 increase above $\sim$100 m and decrease below. In the pacific OMZ, this is at odds with many other models that predict O2 increase. Hence N2O projections of the low-O2 pathways could be not robust when the model is put in a larger prospective.

- Similarly, IPSL-CM5A-LR seem on the large side of models' NPP decrease prediction - up to twice as large as many other models (e.g. Bopp et al., 2013, Fig 9). This would overstate the role of nitrification decreases.

- Overall, the title and abstract should reflect the model-dependent aspects of the study - e.g. ". . . in a Earth System Model" or ". . . in IPSL Earth System Model" in the title, etc.

- A second concern relates to the choice of the two N2O production parameterizations, which seem somewhat arbitrary. The Authors identify 3 major processes controlling the evolution of N2O sources. These are: decline in nitrification rates (because of less export and remineralization), warming, and deoxygenation. The first process decreases N2O production, the last two increase it, hence opposing the first. What is confusing is that the Author use two alternative parameterization of N2O production (P.TEMP and P.OMZ) where decline in nitrification is compensated by either process. This makes it hard to compare the two parameterizations, and assess which one is more representative of the real ocean - where perhaps all factors are at play. As a sensitivity study, two

simulations only are not enough to bracket the range of possibilities of the mechanisms proposed, and separate their effects.

- For P.TEMP, I am not sure what the reference (and background) for equation (1) is - especially the temperature dependence, which seems a little bit ad hoc. This should be more clearly discussed, because in this simulation the temperature effect appears strong enough to almost compensate entirely for the decreases in nitrification sources by 2100. I also note that IPSL-CM5A-LR predicts a temperature increase by 2100 of around 4 K which is on the high end of ESM prediction ($\sim$2-3 K). This might overstate the role of warming in increasing N2O emissions.

- Regarding P.OMZ, the Authors should write down the exact equation used for f(O2). While they say it is a step-like function, it appears more complex in Fig S1. Also, how was the partitioning between 75% high-o2 pathway and 25% low-O2 pathway calibrated? I assume that was done by adjusting alpha and beta, but this seems a bit arbitrary. Don't existing parameterizations based on measurements (e.g. Nevison et al. 2003, GBC, etc.) provide a more data-based way for this partitioning? How does the final parameterization used here compare to the existing ones? Perhaps some discussion on how these choices impact the low-pathway results and sensitivity could be added.

- p. 16731, ll. 27-28. These correlation coefficients seem quite small - corresponding to R2 of 0.18-0.24, that is around 1/5th of the data variance. . . Overall I'm not impressed by the model N2O simulation (again Fig 3a-b), and I disagree that even P.OMZ has a good correlation with the model (p. 16714, l. 9). No model is perfect, but the specific shortcoming in the N2O simulation should be clearly laid out and there should be a discussion on how they could affect the conclusions.

- Part of the N2O emission changes are transient. If the system were to stabilize (e.g. to a warmer climate), air-sea fluxes would again match interior production. Perhaps the distinction between transient and long-term responses could be discussed, as it would

matter for the long-term climate effects of N2O.

- Conclusions: p. 16724, ll. 12-16. I'm confused by this sentence. Saying that differences between the P.TEMP and P.OMZ are modest and translate into non-significant differences in model projections, seems inaccurate and contradicts many of the finding discussed before. Just by looking at the trajectories of production and fluxes (Fig. 4-5) the models respond quite differently - with much larger production and flux decline in P.TEMP. I disagree that the biogeochemical differences are negligible between the two models. Rather, my take is that purely physical responses (through air-sea exchange reduction) dominate - hence the (somewhat) homogeneous response of emissions in P.TEMP and P.OMZ. This comment somewhat echoes some confusion throughout the paper of what is driven by physical changes, and what by biogeochemical changes. These are well-separated by construction in the box model, but not as well in the 3D models.

- A recent paper by Zamora and Oschlies (2014, GRL) suggests that N2O production by nitrification in the euphotic zone could be a large and an overlooked source of uncertainty for N2O emissions. Such a source term would respond similarly to the 'high-o2 pathway' and decline with declining productivity, but the Authors should reference it in the paper.

- The paper by Zamora and Oschlies (2014, GRL), and others before, pointed out the large uncertainty stemming from parameterizations of N2O sources. If uncertainties figures where attached to Martinez-Rey results, would climate-induced changes in N2O production and emissions be distinguishable from zero? Changes in inventories might be more robust. They would also be the easiest to detect if we were to monitor N2O over the next century and put Martinez-Rey and coauthors' predictions to a test. I have the impression that the inventory increase is the most robust result of the paper, and should be highlighted as such in the abstract.

Technical comments:

- I'm confused by the units and values of some of the box-model parameters. k should have units of 1/time, and represent a global integral of a piston velocity, but is listed as a concentration ratio in Table S1 - this is confusing. Also Table S1 should include the value of v.

Interactive comment on Biogeosciences Discuss., 11, 16703, 2014.