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Biogeosciences Discussions

Interactive comment on "Factors controlling the community structure of picoplankton in contrasting marine environments" *by* Jose Luis Otero-Ferrer et al.

Anonymous Referee #1

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The manuscript investigates the relationship between nutrient supply estimated by measurements of turbulent mixing and nitrate gradients and the community structure of picophytoplankton (including both autotrophic and heterotrophic groups). The take-home message of the work is that studies that use ambient nutrient concentrations as a proxy of nutrient availability could be misleading as in many regions of the ocean the supply of nutrients by turbulent diffusion is not registered in bottle samples of nutrients. This is an important message that needs to be communicated to the wider marine science community.

The manuscript is well written and provides a nice overview of the ecological literature

of marine picophytoplankton. The dataset of turbulent mixing, nitrate concentrations and picoplankton community structure is novel and covers a variety of hydrographic and trophic regimes.

I have a few questions and comments concerning both the estimation of nutrient supply using combined MSS and nutrient profiles and the choice of sampling stations used in the analysis which I hope the authors may be able to address.

NUTRIENT FLUX ESTIMATES Although the authors correctly point out that concomitant datasets of turbulent mixing and picophytoplankton community structure are rare, this may be due in part to the lack of high-quality estimates of nutrient flux from microstructure profile measurements in the surface ocean with coincident depth-resolved nutrient profiles (required to obtain a robust estimate of the nutrient gradient near the nutricline). The vertical resolution of the nutrient profiles within the dataset is unclear (only a range between 3 and 9 depths is provided, but it could be tricky to use 3/4 depths to provide a good estimate of the nutrient gradient). Could the extent of the density gradient be over/underestimated in cases where the depth resolution is low and by how much? It would be good to have a frequency histogram for the dataset showing the number of depths per profile so the reader is aware of the vertical resolution of nutrient concentrations across the dataset.

DENSITY AND NITRATE RELATIONSHIPS The authors also mention that for some of the stations nutrient data was not available, and instead of nutrient bottle data, a relationship between density and nitrate was used. Again it is not clear how robust the relationship between nitrate concentration and density was for the relevant stations. Could the authors provide supplementary plots of the nutrient versus density relationship that was used to estimate nitrate gradients, similar to that of Williams et al. (2013a GRL 40:5467-5472; 2013b Limnol. & Oceanogr.: Fluids and Envs 3:61-73)?

EPISODIC NATURE OF MIXING Mixing events in some regions can be episodic, yet short-term vertical pulses of nutrients can trigger significant shifts in community struc-

ture. In some oceanic regions tidal mixing can also be important. The authors mention that 3-10 profiles were taken, but it would also be helpful to know the time interval over which these profiles were made and how this varied between the three study regions (again a frequency histogram documenting this would be helpful). Could it be that for some regions the flux of nutrients could be significantly underestimated given that such short-term events may not be captured in MSS profile data? Given the general audience of the journal, both the strengths and shortcomings of this method of estimating nutrient supply should be provided.

PICOPHYTOPLANKTON BIOMASS The authors report the estimates of picophytoplankton biomass and ratios of biomass, but I was unable to find how the authors convert from cell abundance to carbon per unit volume. This is quite important, as there are several group-specific carbon conversion factors in the literature fand for the larger eukaryotic cells it is likely a biovolume conversion factor may provide a better estimate, as the size range within this subgroup can be significant.

EXTRAPOLATION TO THE GLOBAL OCEAN I was surprised to see that over half of the stations used were from coastal embayments. One could argue from many points of view that these regions may not be representative of the open-ocean eutrophic environments (likely different taxonomic diversity within these gross cytometric groupings, potential supply of nutrients from terrigenous sources, different light environment caused by attenuation by CDOM and sediments, need to correct for advective flux). Perhaps the authors have supporting literature/data that would help convince the reader that these embayments broadly reflect the environmental conditions of offshore stations, but even with such supporting information they should also highlight the need for data from open-ocean meso- and eutrophic environments that would help further resolve the global relationship between mixing and picoplankton community structure.

The dataset is largely confined to a specific geographic region (40N-30S, covering a limited number of biochemical provinces in the Atlantic Basin), yet the authors use relationships from this study to predict future changes in picoplankton community structure

across the entire globe. Would the authors consider limiting their predictions of future community structure to the geographic regions/ latitudinal gradients that are used to develop the predictive models? Given that the global ocean covers a variety of biogeochemical regimes, some of which may not be limited by nitrate, restricting the geographical scope of the future predictions may be advisable, even though the overall patterns tend to broadly resemble those from other global studies.

There is also very limited information on how the global ocean model simulations of nitrogen flux from Lewandowska et al. (2014) were used to estimate percentage change in cyano:picoeuk biomass ratio. Also information on the predictive model setup and assumptions (physical and biogeochemical) would be helpful.

USE OF MOREL MODEL TO ESTIMATE DEPTH OF PHOTIC LAYER Is the light attenuation observed in the Galician coastal stations largely a result of phytoplankton or other optically-active substances? I mention this because the model of Morel used to estimate euphotic depth is restricted to Case-1 (open ocean) waters where light attenuation is dominated by phytoplankton.

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