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Biogeosciences Discussions

Interactive comment on "Longitudinal contrast in Turbulence along a ∼19S section in the Pacific and its consequences on biogeochemical fluxes" *by* Pascale Bouruet-Aubertot et al.

Anonymous Referee #2

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Overview: The manuscript describes an impressive set of turbulent microstructure data in an attempt to assess near-surface turbulent mixing in a generally oligotrophic region and the main driving mechanisms for the turbulence (inertial or internal tide shear). The measurments are used to estimate the supply of nitrate and phosphate to the euphotic zone, with some interesting consequences suggested by non-Redfield fluxes in the surface layer. The data is very strong, and the aims are novel and important. My main suggestions focus on some more quantitative analyses to support the claims made.

General points: A key aspect of the quantitative analyses of the turbulence data is the

demonstration of turbulent dissipation alongside regions of close-to-critical Richardson number, supporting the suggestion that turbulence was generated by shear instability. (e.g. Page 5 line 16: reference is made to subcritical Ri). This is really difficult to see in Fig. 3. The coloured dots overlap considerably, which makes the profiles of dissipation and diffusion difficult to see. The shading of Ri does not really convey useful information. Assuming the critical Ri is being taken to be about 1, then it needs to be clear where that is. The log scale makes Ri=1 roughly white I think, but I cannot see what the text on page 5 discusses. Why does Ri use a log scale? You are really only interested in changes in Ri about the value of 1. Why not do a more quantitative analysis – for instance, what does a scatter plot of turbulent dissipation versus Ri look like? The evidence as presented is not a convincing case for shear instability. Perhaps one of the most interesting an important analyses is that relating turbulent dissipation to the energy in the subinertial flow at inertial and semi-diurnal frequencies (pages 6 and 7). However, this analysis lacks any real quantitative evidence. It is based largely on a qualitative comparison of vertical profiles in Figure 10, which is not adequate in supporting the assertions made on the drivers for turbulence (particularly as an important suggestion is that the higher dissipation in the west is not driven by the most obvious candidate of rougher seabed topography and more internal tidal activity). This analysis needs to be strengthened. Also, the dissipation profiles in Figure 10 (and Fig. 7) would benefit greatly from having the 95% uncertainties added alongside the mean profiles (e.g. bootstrapping the profiles at each station – there is plenty of them), which would better highlight just how strong the contrasts are between the stations.

Specific edits and smaller suggestions/queries: 1. The title should really be "...along a 19ïĆřS section....." 2. Line 2 in the Abstract, if it is necessary to have the Moutin & Bonnet reference here, then include the complete reference. 3. Line 3 Abstract: "...hydrographic and current measurements at fine scale...". What is meant by fine scale? The horizontal spacing of the CTD profiles could not really be described as "fine", and while the vessel ADCP data could be at fine scale, it is not used as such. 4. Line 6 abstract: "....with stronger turbulence in the west, i.e....." 5. Line 8 abstract:

“. . .pattern was correlated with the energy. . .” 6. Line 13/14 abstract: Turbulent nitrate fluxes are described asgreater in the west because of the increase in eddy diffusivity. What proportion of the increase was because of Kz, and what caused by changes in the nitrate gradient? 7. Line 16 abstract: “. . .organisms that were seen to be the main contributors. . .” 8. Page 2 line 1: “. . .increasing oligotrophy to the East.” Presumably “. . .increasing oligotrophy towards the east and the centre of the gyre”? Oligotrophy would lessen if you kept going east. . . 9. Page 2 line 18: The dissipation is reported for the “stratified ïĄ¿300m”. I am not sure what this means. Is it an average dissipation over the upper 300 metres? 10. Page 2 line 31: “the main purposes of. . .” 11. Page 2 line 33: trichodesmium should be Trichodesmium (capital letter and italics). 12. Page 2 line 34: “. . .turbulent diffusion was found to make a negligible. . .” 13. Page 3 line 1: “leads to the question of the sources of other nutrients to the euphotic layer that could sustain. . .” 14. Page 3 lines 4/5: “The aim is also to provide insight into the main mechanisms. . .” 15. Page 3 line 6: “. . .dynamics influence biogeochemical. . .” 16. Page 3 line 9: French (capitalised). 17. Page 3 line 10: the short duration stations are described as “24 hour”, and are later described as having “a few profiles” (line 24) of microstructure. Most of these short stations only had 1 profile, which I assume took a lot less than 24 hours and does not count as a “few”. This should be clarified. 18. Page 3 line 18/19: “. . .yielding processed currents. . .” 19. Page 3 line 27: “. . .which allowed validation of the estimate. . .” 20. Page 3 line 28: dissipation is described as being calculated in 1 metre bins and then averaged over 8 metres. Is this a standard analytical procedure? 21. Page 3 line 29: “level is 5. . .” 22. Page 4 line 7: was N also calculated on 1 metre bins before the 8 metre averaging? 23. Page 4 line 8: “ïĄĞ has generally been set to. . .recent findings of Shih. . .” 24. Sections 3, 4 and 5 each constitute Results and Discussion on 3 different topics. I suggest use a general section 3 Results and Discussion, and then subsections 3.1 Spatial pattern of Turbulence, 3.2 Possible impact of internal waves, etc. The section on Spatial pattern of turbulence is in need of splitting into coherent paragraphs – at the moment it is a fairly dense section of text that makes it hard work for the reader (well, at least this reader). 25. Page

4 line 24: by “depth averaged” below the mixed layer, I assume you mean average between the base of the mixed layer (how defined?) and the deepest reached by the profiler? 26. The longitude axes of the data continues to increase in value past 180ïĆřE. I know this makes life much easier for plotting the data, but fundamentally it is not the correct horizontal coordinate system. Specify in terms of correct longitude, and include “degrees” or “ïĆř”. 27. Page 5 line 11: how was shear, S, calculated? From the LADCP or SADCP? What depth bins? What time averaging (i.e. how many raw profiles)? 28. Page 7, lines 13,14. The changes in vertical nitrate flux are attributed entirely to changes in Kz. This looks reasonable, based on Fig. 11, but are there any changes in the strength of the vertical nitrate gradient that might also contribute? 29. The caption to Fig. 12 is incorrect. 30. Fig. 13. Is the dashed line in each panel the mean value? The top of the nitracline has been defined I assume on the basis of an interval with one end pinned by nitrate reaching undetectable concentrations. If the euphotic zone were defined in terms of the 1% irradiance, would that change the results. 31. It would be useful to provide some context for the values of the nitrate flux measured – how do they compare with other published values (e.g. Planas et al., Limnol. Oceanogr. 1999, 44, 116-126; Lewis et al., Science, 1986, 234, 870-873; Stevens et al., Limnol. Oceanogr. 2012, 57, 897-911).

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