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.
We could not present a point to point correlation between epsilon and Ri since measurements were not performed at the same time and location (due to the drift of the VMP). We agree that the spatial pattern of Ri and epsilon illustrated with longitude depth section in Figure 3 provides qualitative information that is now complemented with quantitative information following your suggestions: Histograms of Ri and epsilon in the Western part (West of 170W) and the Eastern part as well as percentage of critical Ri values and median epsilon as a function of latitude now displayed in a new figure, figure 5. A table, Table 2, giving statistics on Ri, epsilon and Kz in these two regions, has been added thus providing quantitative evidence of the longitudinal contrast in turbulence.

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.

We strengthened the analysis of the long duration stations as suggested with quantitative analysis provided in Table 3. Kinetic energy and shear variance in different frequency bands, sub-inertial, inertial and semi-diurnal, underlines the energetic niw at LD-A, the weaker niw signal at LD-B and LD-C but still slightly higher than the semi-diurnal internal tide. The energetic niw at LD-A, triggered by a strong shear in sub-inertial flow, is clearly correlated with epsilon. At the other stations, it is difficult to say what drives the turbulence, the near-inertial signal may be as well of significance as it is the most energetic and with larger shear.

The context of the OUTPACE cruise with significant niw generated by a cyclone is very specific, of general interest for the studied region, where these meteorological phenomena are frequent at the end of the summer). It hides the more continuous influence of internal tides as a turbulence driver. Our measurements show only a slightly larger semi-diurnal kinetic energy in the West at LD-A compared to LD-B and LD-C (Figure 9 of the manuscript) but suggests a larger contrast in shear variance (Figure A below).

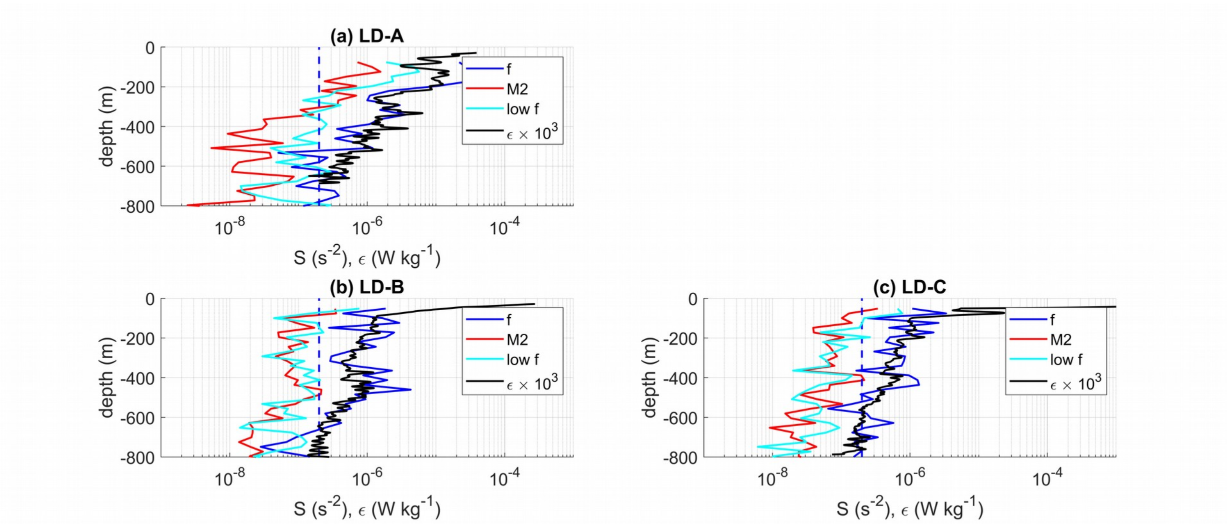

Figure A: Vertical profiles of epsilon, shear variance for the sub-inertial frequencies, the inertial and the semi-diurnal M2 frequencies at the long duration stations, LD-A (a), LD-B (b) and LD-C (c). The noise level for shear is shown with a blue dotted vertical line.

Also, the dissipation profiles in Figure 10 (and Fig. 7) would benefit greatly from having the 95% uncertainties added along side 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.

We added the 95% uncertainties for the mean profiles displayed at the long duration stations in Figure 7 which highlights the strong contrast between these stations. We did not modify Figure 9 (previously Fig.10) since these uncertainties on epsilon are already shown in Figure 7. In this figure, different profiles are displayed in subplot for each long duration station and adding the 95% uncertainties on epsilon would overload each subplot and, to our point of view, would make it unclear.

Specific edits and smaller suggestions/queries:

1. The title should really be ":::along a 19ïC.rS section::::.."
ok

2. Line 2 in the Abstract, if it is necessary to have the Moutin & Bonnet reference here, then include the complete reference.
we deleted this reference which was not necessary

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.
fine scale refers to the vertical resolution, fine scale correspond to ~10m scale, "vertical" has been added for clarity

4. Line 6 abstract: "::::.with stronger turbulence in the west, i.e.."
"higher turbulence level" replaced by "stronger turbulence"

5. Line 8 abstract: "::::.pattern was correlated with the energy::::."
"correlated to" replaced by "with"

6. Line 13/14 abstract: Turbulent nitrate fluxes are described as greater 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?
Thanks for mentioning this point. There is in fact an opposite effect of K_z and nitrate gradient in the longitudinal flux variations: the increased westward K_z leads to a nitrate flux W/E difference of the order of 84% whereas the lower nitrate concentration gradient in the West is responsible for a nitrate flux W/E difference of -20%. We added this information in a table with statistics for the full transect as well as for the long duration stations (Table 5). We did not modify the abstract due to length constraints but added some comments and tables in the manuscript.

7.

Line 16

abstract: "::::.organisms that were seen to be the main contributors::::."
"were evidenced as the main" has been replaced by "were shown to be the main"

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:::.
absolutely "to the East" => "towards the East and the center of the gyre"… done

9. Page 2 line 18: The dissipation is reported for the "stratifiedï A¿300m".I am not sure what this means. Is it an average dissipation

over the upper 300 metres?
We replace the text by “below the mixed layer down to 300m” to make it clear

10. Page 2 line31: “the main purposes of ...”
done

11. Page2 line 33: trichodesmium should be Trichodesmium (capital letter and italics).
done

12. Page 2 line 34: “...turbulent diffusion was found to make a negligible:::.”
done

13. Page3 line 1: “leads to the question of the sources of other nutrients to the euphotic layer that could sustain:::.”
“able to” replaced by “that could sustain”

14. Page 3 lines 4/5: “The aim is also to provide insight into the main mechanisms:::.”
“to” replaced by “into”

15. Page 3 line 6: “:::dynamics influence biogeochemical:::”
we have replaced “these small-scale dynamics influence” by “this small-scale dynamics influences”

16. Page 3 line 9: French (capitalised).
done

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.
The beginning of section 2 describes the full duration of the stations, not that of the VMP profiles. The latter is given in subsection 2.2: we changed “a few profiles at each SD station" into “at least one profile at each SD station except at SD13”

18. Page 3 line 18/19: “:::yielding processed currents:::”
corrected

19. Page 3 line 27: “:::which allowed validation of the estimate:::”
modified

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?
this is for consistency with LADCP measurements, of 8-m vertical bins

21. Page 3 line 29: “level is 5:::”
corrected

22. Page 4 line 7: was N also calculated on 1 metre bins before the 8 metre averaging?
yes, as stated in the text

23. Page 4 line 8:"ï.G A.has generally been set to:::recent findings of Shih:::."
corrected

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).
Thank you for the suggestion: We divided the sections into sub-sections to make it easy for the 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 BGD the profiler?
The depth range is now specified in the text as well as in the figure caption

26. The longitude axes of the data continues to increase in value past 180ï ´C.rE. 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 Interactive include "degrees" or "ï ´C .r".
We changed the labels of the longitude axis to make it consistent with geographical coordinates.

27. Page 5 line 11: how was shear, S, calculated? From comment the LADCP or SADCP? What depth bins? What time averaging (i.e. how many raw profiles)?
Shear was computed from LADCP profiles that have the finer vertical resolution compared to SADCP data. Details were added in the figure caption: "S is inferred from LADCP data and N was vertically averaged over 8-m length vertical intervals for consistency."

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?
Variations in the vertical nitrate concentration also contribute significantly to the variation of the nitrate turbulent diffusive flux. We now provide some statistics of these variations within the nitracline in Table 5. The eastward nitrate concentration vertical gradient, inducing an eastward increase of the flux, counter balances slightly the westward increase of the flux resulting from the westward gradient in K_z.

29. The caption to Fig. 12 is incorrect.
thank you, corrected

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.

Yes the dashed line is the mean value, this has been added in the figure caption.
The top of the nitracline has been defined as the depth where nitrate concentration is zero based on an extrapolation from the last detectable concentration (> 0.5 µmol.m^{-3}) assuming a constant vertical gradient above this depth (see Moutin et al, 2018). It is only at long duration stations that the top of the nitracline has been defined in isopycnal coordinates: this allows to get rid of a varying depth of the nitracline because of vertical displacements of isopycnals induced by internal waves, particularly by internal tides. The euphotic zone has been defined in terms of PAR and matches within a few meters with the top of the nitracline most of the time except in the eastern part. For instance at long duration stations, at LD-A and LD-C the top of the nitracline matches with the euphotic zone depth within a few meters whereas at LD-B a significant difference is observed as a result of the subsurface bloom with 55m for the euphotic zone depth and 120m for the nitracline (see Moutin et al, 2018). Histograms in the euphotic zone displayed in Figure 13 reveal the strong contrast with the distribution in the nitracline.

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).

Thank you for pointing out these references: we now refer to Lewis et al, 1986 and Stevens et al (2012) in the last section.