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.

BFC Fernández-Castro (Referee)
bieitof@iim.csic.es

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This manuscript describes a set of microstructure turbulence measurements along a longitudinal transect in the western South Pacific sub tropical gyre. An interesting longitudinal gradient in both the intensity of turbulent dissipation and the mechanisms responsible for that dissipation is found. The biogeochemical implications of this gradient are also explored with the calculation of nitrate and phosphate diffusive fluxes across the base and within the photic layer. The longitudinal variability of nutrient supply was coherent with an increasing degree of oligotrophy to the east and the different degree of penetration of the phosphate and nitrate fluxes into the surface ocean was related to the activity of nitrogen fixers. The dataset presented in the manuscript fills a substantial gap of knowledge, providing microstrucutre measurements in a largely undersampled area. The analysis of the turbulence generation mechanisms is promising and the implications for the biogeochemistry are very interesting. However, I have found a number of problems in the manuscript, mainly related to the presentation and discussion of the results and to the lack of some important information in the methods section. In my opinion the manuscript is not suitable for publication in the present form and a major revision would be required.

GENERAL COMMENTS:

The text needs to be revised in order to improve the communication of the results. Many parts of the manuscript are difficult to follow or contain grammatical and typo-graphic errors.

We worked on the text to improve the communication of the results.

Sections 3 and 5 are structured in single extremely long paragraphs and are very difficult read. These sections need to be restructured and split into paragraphs. Some of the figures contain errors, for example wrong axis labels (Figure11)

Subsections and paragraphs were added in sections 3 and 5. Errors in the figures were corrected.

-There is general lack of methods information. In particular,I was not able to find a description of the method used to obtain the tidal forcing(Figure5)and, more strikingly, there is no explicit mention to how the nutrient fluxes were computed. In this sense, one of the most outstanding results from a biogeochemical perspective is that, contrary to nitrate, the phosphate upward diffusive flux does not drop to zero above the DCM. This is probably because the nitrate gradient above this depth is virtually “zero” but the nutrient distributions are not shown and the calculation method is not reported. I guess that there is some “noise” in both the nitrate gradients and the Kz and that the fluxes are not actually “zero” but below some “noise” level. How was “zero” defined?

Details on the method of computation of the tidal forcing, inertial energy flux and turbulent diffusive nutrient fluxes are now given in sections 2.4 and 2.5. The noise level for the concentration and the computation of the noise level for the flux are now detailed.
We added the vertical sections of nitrate and phosphate concentrations in Figure 10.

Another important point is that, in my opinion, the use of a fixed averaging interval (20-80m)for the calculation of the nutrient fluxes within the photic layer (Figure 14) is not the best choice because the photic layer dimensions change with longitude. From my point of view, the photic layer fluxes should be calculated in a depth range consistent with this variability, as done for the fluxes across the nitracline.

The average is now performed within the photic layer, taking into account its variation with longitude: Figure 14 (now Figure 13) has been modified accordingly. The euphotic zone depth is now displayed in Figure 11 of the time depth sections at the LD stations.

-In general I miss more quantitative information in the text. The description of the results is mostly based on the qualitative description of the figures. I believe that reporting some quantification of the average TKE dissipation rates and nutrient fluxes in the different situations/regions (namely in sections 4 and 5) would help to structure the text and communicate the results more effectively. In the particular case of section3, I would suggest to define a separation between the eastern and western parts of the section based on longitude (e.g. at 190E) or bathymetry and obtain some statistics for the different parameters in both parts (eg. mean or median values of epsilon, K, percentage of subcritical Ri bins, etc.). Also, the results from LD stations highlighted the impact mixing intermittency, with implications for biogeochemical fluxes. I find this information novel and very valuable, and it could be better illustrated with some numbers/statistics. The quantification of the N:P ratios of the diffusive nutrient fluxes across the nitracline and within the photic layer could also be helpful for the discussion the biogeochemical implications.

Following the reviewer suggestions we have added four new tables in the revised manuscript showing statistics over the two regions, with 170W longitude chosen as the separation between the two.
In particular, we have reported:
- in Table 2 the percentage of Ri <1, mean and standard deviations values of epsilon and Kz, , computed over the first 500m excluding the mixed layer;
- in Table 3 the kinetic energy and vertical shear in the sub-inertial flow, the inertial frequency and semi-diurnal constituent at the long duration stations in both the 100-500m layer and the 50-250m layer;
-in Table 4 the mean values of the turbulent nitrate and phosphate diffusive fluxes within the euphotic layer in the eastern and western sub-domains as well as at each long duration station;

- in Table 5: The impact of the intermittency of turbulence on biogeochemical diffusive fluxes is now quantified with the percentage of flux variation induced by Kz variation that is compared to that induced by variations in the vertical concentration gradient.

Moreover, we have added Figure 14, where a comparaison of the time average vertical profiles of nitrate and phosphate turbulent diffusive fluxes is performed in order to highlight the biogeochemical implications: NO3 depletion in the photic layer and significant PO4 sources through turbulent diffusion that are likely to provide the required conditions for the growth of N2 fixers in the Melanesian archipelago. Please, note that at the long duration station LD-C,i.e. in the gyre, phosphates are not used because the iron availability is not sufficient for the growth of N2 fixers (Blain et al., 2007; Moutin et al., 2008,2018; Guieu et al., 2018).

- One of the main focus of the manuscript is to demonstrate that the spatial patterns of dissipation rates are related to the west-east gradient in the intensity of internal wave generation (and dissipation). However, in the first part of section3(lines 4-17 of page 5) and in the conclusions, the authors mention shear instability as a possible driver of the longitudinal asymmetry, based on the distribution of the Ri numbers along the section. It is not entirely clear to me whether the authors want to suggest that the presence of subcritical (and patchy) Ri derives from a mechanism other than internal waves (i.e. low frequency flow), as it seems to be pointed out in the conclusions. I think this point needs some clarification and better justification. The authors could add some more insights to the discussion of Figure 10 or include a similar decomposition for shear variance.

We agree that the spatial pattern of Ri and epsilon illustrated with the longitude depth section in Figure 3 provides qualitative information on the likeliness of shear instability as a driver for turbulence onset. This information is now complemented with new quantitative analyses.
Table 2 gives statistics on Ri, epsilon and Kz West of 170W and East of 170W thus providing quantitative evidence of the longitudinal contrast in turbulence.

The new table 3 provides an insight on the onset of shear instability driven by the low frequency flow or the internal wave field thanks to the quantitative analysis at the long duration stations. Kinetic energy and shear variance in the different frequency bands: sub-inertial, inertial and semi-diurnal, underline 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 LDA , triggered by a strong shear in sub-inertial flow, is clearly correlated with epsilon. At the short-duration stations, it is difficult to say what drives the turbulence, the near-inertial signal could also be of significance as it is the most energetic and has larger shear than the internal tide.
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). This 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 but suggests a larger contrast in shear variance. These comments have been added in the discussion to clarify what drives turbulence : shear instability of the low frequency flow or internal waves.

The shear profiles in the different frequency bands displayed here below in Figure A shows a similar behaviour as that of kinetic energy displayed in the manuscript: a dominance of the near-inertial shear at all long stations LD-A, B and C that is correlated with larger epsilon. Information on shear is given in Table 3, as said above. We chose to display only kinetic energy profiles as this signal is always above the noise level as opposed to the shear.

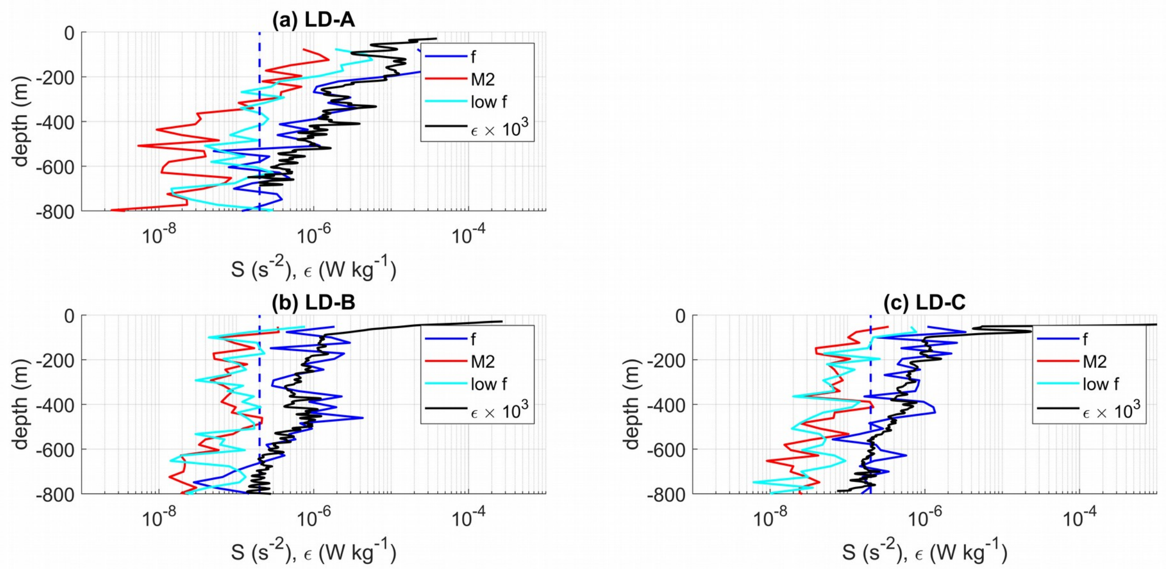

Figure A: Vertical profiles of epsilon, shear variance for the sub-inertial frequencies, the inertial and the semi-diurnal M2 frequencies at the long duration stations, LD-A (a), LD-B (b) and LD-C (c). The noise level for shear is shown with a blue dotted vertical line.

SPECIFIC COMMENTS:

Page 2, lines 24-27: "Global maps of the energy flux into inertial motions is enhanced at mid-latitudes as well as around a SE oriented track from the Equator to 40S and within â´Lij 180 - 200E longitude (Alford and Zhao, 2007, Fig.9)". The authors may specify whether "mid latitudes" refers to the south Pacific basin in particular or to the global ocean.

"Mid-latitudes" is for all the oceans but here the small band is observed in the South Pacific only; the text has been modified accordingly.

Sometimes the authors refer to inertial waves and others to near-inertial waves. This is confusing to me. For example, in this sentence (Page 1, line 25)" Global maps of the energy flux into near-inertial motions show enhanced semi-diurnal tide energy conversion in the western part of the subtropical South Pacific [...]"it seems that you are referring to the M2 internal tide as near inertial. At a latitude of 20S, the inertial period if about 35 hours(if I am no wrong).
Is it correct to say that semi-diurnal internal tides are near-inertial?
Sorry this was an error, it is indeed very confusing: the semi-diurnal tide energy has been taken out.

Page 3, lines 3-5: "The purpose of this paper is to characterize three-dimensional turbulence along the OUTPACE transect with microstructure measurements performed at both one-day short duration stations and at long duration stations lasting three inertial periods." I would say that you characterize the "spatial variability" of microstructure turbulence rather than three-dimensional turbulence. (It also applies to the abstract)
"the spatial variability of turbulence" is now specified p3 and we deleted "three-dimensional" in the abstract.

Page 3, Section 2: were all the instruments (CTD, VMP) deployed in all stations? Is LADCP data used in this manuscript? I could not find it in the figures or text.
Yes, all instruments were deployed at all stations except at SD13 for the VMP (see Table 1).
We have added "These measurements were performed at all stations with a typical 3 hours time interval between each deployment" in the text.
LADCP data is used for the computation of Ri and it is now mentioned in the Figure 4 caption.

Page3, line 17: add reference for the Visbeck inversion method
done

Page 3, section 2.2. Indicate the approximate maximum depth of the microstructure sampling. Indicate the approximate number of profiles in long and short duration stations, eg. 30 and 1-3 profiles.
1 to 3 profiles were performed at each SD stations and about 30 profiles at each LD station. We added some details in the text and a references to Table 1.

Page 3, line 29. More detailed information about the microstructure data processing would be desirable. For example, how was the noise level estimated? From which depth were the epsilon data considered reliable? Was there any noise removal procedure applied? How was the information from the two shear sensors merged?
More detailed information on the microstructure data processing has been added in section 2.2.

The noise level was inferred from Ferron et al (2014). Note that our epsilon values are always far above this estimated noise level. Note also that, when the epsilon value is below the threshold level, the shear spectra does not follow the Nasmyth spectra, a situation not encountered here.

Shear measurements were processed using the routines developped by Rockland: the spikes in the shear data are first removed and the spectral coherence between the shear sensors and the accelerometers is used to remove vibrational contamination.

The first 20m below the surface were not considered to avoid any contamination from the ship wake as well as the 20m at the end of the profile because of the decreasing vertical velocity there. More generally, epsilon values were excluded when the vertical velocity gradient, dW/dz, was larger than 2.5 10^{-2} s^{-1} .The averaged epsilon from the two shear probes was taken provided that the ratio between the two estimates was smaller than 2, otherwise the epsilon value with the smallest depth variation (compared to the neighbouring upper and lower epsilon values) was considered.

More details are now given in section 2.2 and reference to Ferron et al (2014) for the full procedure is made.

Page 4 line 24-25, indicate the approximate depth range for epsilon and Kz averaging as in Fig.1 caption (100 to 800m). What was the mixed layer depth? You could add the distribution of MLD to Figure 3.

The depth range (below 100m depth) is given both in the text and in the figure caption; note that the mixed layer depth has been also added in Figure 3.

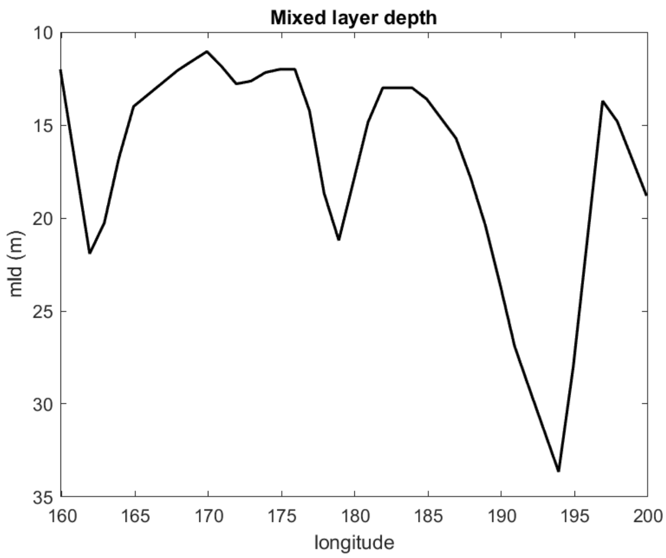

Figure B : mixed layer depth as a function of longitude

Pages 4-5, section3: This section is described in a extremely long single paragraph. I would suggest to split the section into 3-4 paragraphs to facilitate the reading

done

Page 5, line 5-6: " More insights on turbulence are given with vertical sections of epsilon and Kz in Figure 3a and b." What is represented in this figure, station-averaged profiles?

The station averaged profiles are now displayed in Figure 3a and b for better visualization. This information has been added in the figure caption.

Page 5, line 6: This sentence: "The range of epsilon values covers 3 orders of magnitude, typically below the mixed layer down to 300m depth,

and presents a typical patchy pattern with spots of intense turbulence with values up to â´Lij10..8Wkg..1down to 500m. Most of these events are observed in the West and their occurrence decreases eastward and downward (Fig.3a)." is not clear to me. Please be more precise.
The text has been modified and reference is made to Table 2 for statistics of epsilon and Kz West of 170W and East of 170W.

Page 5, lines 12-13. Add E. I don't appreciate the absence of a marked pycnocline in 180 and 180E. Could you explain better? In general, the description of the vertical and horizontal distributions of N, S and Ri could be improved. Sometimes it is difficult to know for sure to which depth intervals the authors are referring.
we agree that the pycnocline is not weaker at 180E and have corrected this. We improved the description of figure 3 with more details to make it clearer (see p.6)

Page5, lines 18-19: How was this information obtained? From which source? A more thorough explanation of this analysis is definitely required, similar to that given for the input of near-inertial energy (lines 25 to 29).
Details on the computation of the tidal forcing and of the energy flux into inertial motions is now provided in section 2.

Page 5, line 25. What is the inertial period in the study area?
The inertial period is of about 36h as now specified in section 2.

Page 6, line 21.
Figure 9 is abruptly introduced here without any specific explanation. Panels (a) -(c) are not mentioned at all. On the other hand, panels (d)-(i) introduce redundant information already present in Figure 6. In my opinion the authors could just drop this figure and sustain their argumentation with Figure 6, which is already familiar for the reader.
Figure 9 has been dropped and reference is made to Figures 5 and 6 as suggested

Page 6, line 28 onwards and Figure 10: How were the energies associated with the different frequencies calculated? A similar frequency-decomposition of the shear variance could be useful to better separate the processes contributing to shear instability (internal waves vs. low frequency) [*]
Frequency spectra of kinetic energy were used to infer the kinetic energies within the different frequency bands, similar information was computed for the shear as already mentioned above. Details are now given in the text (see page 8)

Page 6, line 31: "[...] a wave mean flow interaction (i.e. critial level)." Perhaps a reference is needed here
A reference has been added: Soares et al, 2015.

Page 7, line 4-5. "The contrast in turbulence between the three stations is mostly confined in the upper few hundred meters as a result of an energetic niw and its interaction with the strongly sheared subinertial flow." Are you refering to Figure 10a where you can see a decrease of low frequency energy with depth? A direct quantification of shear variance in the different frequencies could help to visualize this. See previous

comment [*]
Yes, this is now detailed in the text with additional information in Table 3.

Page 7, section 5. The nutrient distributions are not shown and the sampling and methodological details are not reported in the manuscript. You must at least provide a reference where this information can be found. The methodology used to calculate the diffusive fluxes is not reported either. Were the VMP and nutrient sampling vertical grid coincident? Was some interpolation required to match the vertical resolution of both variables? Again this section is too long to be written in a single paragraph.
- the nitrate and phosphate concentrations have been added to Figure 10 (previously Figure 11);
- the methodology for the computation is now detailed in section 2.5, an interpolation was indeed required in order to infer turbulent diffusive fluxes;
- the section has been organized into different paragraphs as advised (5.1 overview along the outpace section, 5.2 focus on LD stations, 5.2.1 nitrate input at the top of the nitracline, 5.2.2 new primary production sustained by phosphate turbulent diffusive fluxes at the western stations? 5.2.3 turbulent diffusion and the oligo to ultra oligotrophic conditions encountered during the outpace cruise)).

Page 7, line 14: "Large variations are noted, that result from the strong variability of Kz (Fig.11b).". Specify that these variations are in the "short-scale" in contrast with the large-scale longitudinal gradient.
"at small scales" has been added; this paragraph is now more detailed, with information on the flux variations with longitude that detail the Kz and the cz contributions.

Page 7, line 17. The authors may state that the nitrate flux is zero above the DCM because the gradient/concentration is zero.
The description of figure 10 is more detailed (see p.9).
The addition of the nitrates and phosphate concentrations, makes it indeed more clear, indicating that no flux can be calculated in the areas where their concentrations are below the quantification limits. These quantification limits were introduced in the method section.

Page 7, lines 17-20. change "[...] of the nitrate diffusive flux within the Redfield ratio [...]"to"[...]of the nitrate diffusive flux by afactor of 1/6 corresponding to the Redfield ratio [...]". I think "followings" is not correct in English.
done

Page 7, line 30-33. What is the euphotic layer depth and how does it relate to the nitracline? You could show the calculation interval in Figure13
The euphotic zone depth (EZD)(sinon on marque souvent Zeu) was determined on board from the photosynthetically available radiation (PAR) at depth compared to the sea surface PAR(0+, and used to determine the upper water sampling depths corresponding to 75, 54, 36, 19, 10, 3, 1 (EZD), 0.3, and 0.1% of PAR(0C+) (Herbland et Voituriez (1977) Moutin and Prieur, 2012). EZD is now displayed in Figure 11 (dashed black line) and can be compared with the top of the nitracline. It is most of the time shallower than the top of the nitracline, as often observed in ultraoligotrophic environments.

Page8, line1: "The mean nitrate turbulent diffusive flux is far larger [...]" What is the mean flux? "Far" is not quantitative. Give some numbers

We referred to figure 12 (now Figure 12) for quantitative information, this information is now given in the text.-

Page 8, lines 5-6: give numbers

Values are given in the text and a table has been added.

Page8, lines 7-10.From your data I would not say that the nitrate flux into the photic layer is negligible in the Malasian Archipelago (LD-A). The depth of the photic layer is usually some meters below the DCM which is located at 80-100 m in LD-A. At this depth the nitrate fluxes are not zero (Figure 12c). If it is negligible in comparison with N2 fixation, could you give some typical value of N2 fixation rate to compare.

We now give some values inferred from Moutin et al (2018), table 6; the term “negligible” was not appropriate: the contribution is small 46 µmol m^{-2} d^{-1} compared with the N2 fixation rate of 642 µmol m^{-2} d^{-1}, namely 7.2 %.

Page 8, lines 10 – 15: According to your data, the nitrate flux vanishes above the base of the euphotic zone and the phosphate flux reaches shallower depths, potentially fueling nitrogen fixation. I find this result very interesting. Now this question raises to me: is the supply at the base of the DCM Redfieldian (N:P ~ 16), and, thus, net production at the DCM results in a preferential uptake of nitrate (N:P>16), such that the nitrate flux gets exhausted first, or, on the contrary, the nutrient supply is already nitrogen-depleted at the base of the DCM, i.e. the N:P ratio of the diffusive flux at the nitracline is <16? It is just for my personal curiosity, but it might also be interesting to discuss that in the manuscript. You could show the phosphate fluxes as well in Figure 13 and report the mean N:P values in the text. You could compare these N:P ratios with those at shallower depths (Figure 14).

It is true that according to our data, the nitrate flux vanishes above the base of the euphotic zone and the phosphate flux reaches shallower depths, potentially fueling nitrogen fixation. It is now clearly shown in the additional Figure 15 where the scale were appropriately defined to allow a direct comparison of N:P fluxes following the classical Redfield proportions (N:P = 16:1). In this figure, the fluxes are superposed if they follow the Redfield proportion. At depth, N03 and PO4 fluxes follow the Redfield proportion. Closer to the surface, N03 flux decrease and reache zero before PO4 fluxes creating a potential ecological niche for N2 fixing organisms (Excess P). We will not discuss further the results for several reasons. The upper nitracline and phosphacline do not depend only on uptake processes but rather depend on complex interactions between uptake and remineralization processes resulting in such « equilibrium » states. Moreover, in oligotrophic environments, the DCM mainly results from photo-acclimatation and rarely corresponds to a maximum in biomass. Such subjects would necessary have to be discussed if we try to better interpret our results and, although interesting, we think this is out of the scope of this paper.

Page 8, lines 16-28: In my opinion the choice of a constant interval for the flux integration within the photic layer is not the best choice here

because the different stations exhibit different photic layer depths, with an eastward deepening of the DCM. The use of a fixed interval results in zero nitrate fluxes in LD-C, but not in the others. This might be reflecting only the different dimensions of the system but not substantial differences in nutrient cycling dynamics. The authors might refer the lower limit of the interval to the depth of the top of the nitracline or the (upper) DCM, as in Figure 13.
As suggested we now perform the flux integration within the photic layer taking into account its longitudinal variation.

Page 8, lines 22-24."While at LD-A the phosphate turbulent diffusive flux is of the same order of magnitude as that of the nitrate turbulent diffusive flux at LD-A (Fig.14b and c) there is at least an order of magnitude difference between phosphate and nitrate turbulent diffusive fluxes at LD-B (Fig.14e and f).". The comparison between the nitrate and phosphate fluxes would be better done in terms of the Redfield ratio, otherwise it is confusing.
The ratio between the scales for the nitrate and phosphate concentrations and fluxes is equal to the Redfield ratio in figures 10-14 to make the comparison clear.

Page9, lines1-7.It is not entirely clear to me if you suggest that the shear instability mixing, based on the distribution of the Ri number along the transect, derives from a mechanism other than internal waves, i.e., strongly sheared mean sheared currents as you seem to point out here. Your Figure 10 indicates that the most energetic currents correspond to the semidiurnal and inertial periods, with a generally minor contribution of the low frequencies, at least in the upper 400-500 m. Is it possible that the patchy Ri patterns derive from internal waves becoming shear-unstable and not due to shear
in the mean currents? The separation between the two processes is not sufficiently argued, from my point of view. See a previous comment [*]
We have clarified in the discussion how the contribution of internal waves was evidenced, based on the long duration station, see previous reply p4-

Same lines: There is an extensive work on shear-driven equatorial turbulence by W. D. Smyth, J.N. Moum and colaborators. The authors could possibly include some reference to their work. Eg: "Smyth, W. D., Moum, J. N., Li, L., & Thorpe, S. a. (2013). Diurnal Shear Instability, the Descent of the Surface Shear Layer, and the Deep Cycle of Equatorial Turbulence. Journal of Physical Oceanography, 43(11), 2432–2455. https://doi.org/10.1175/JPO-D-13-089.1" or "Smyth, W. D., & Moum, J. N. (2013). Marginal instability and deep cycle turbulence in the eastern equatorial Pacific Ocean. Geophysical Research Letters, 40(23), 6181–6185. https://doi.org/10.1002/2013GL058403"
We knew these two references. We had decided not to refer to the first one as it is quite specific of the diurnal cycle, of which we do not have any observational evidence in the OUTPACE experiment. But we thank you for reminding us of the second one and have cited it p.9 consistently with the discussion on the seasonal variation in equatorial turbulence.

Page9, lines 22-23: "Phosphate turbulent diffusive fluxes mean values were significant in the euphotic layer with the exception of the most

eastern station." What does "not significant in the eastern most station" mean? What are the confidence intervals?
The confidence interval for concentration is 0.05µmol.l-1 (section 2.5); the confidence interval for the turbulent diffusive flux is estimated to 0.4µmolm^{-2}d^{-1} taking the molecular value for Kz. This information has been added in Section 2.5.

TECHNICAL COMMENTS

Page1 Title and throughout the manuscript: add degree symbol to 19S. Sometimes "S" and "E" are shown in italics, which I believe is not correct.
ok

Page 1, line 7: What does "surface layer" mean here. The longitudinal differences in turbulent dissipation reach 400m. I would not call this a "surface layer"
It means the layer near the surface as opposed to the bottom boundary layer where the turbulence is enhanced. We agree that this formulation may be misleading and have deleted "surface".

Page1 Line 14: Averaged nitrate turbulent diffusive fluxes*ACROSS THE BASE OF THE PHOTIC ZONE* were at least twice as large at the western station than at the two eastern stations due to the *LARGER*vertical diffusion coefficient.
We specify the factor between the western and eastern parts, both in a 100-m layer starting from the top of the nitracline and within the euphotic layer

Page 2,Line 14: I would rather start a new paragraph after "Ledwell et al., 2008"
we agree, done

Page 2 Line 27: There is no Figure 9 in "Alford, M. H. and Z. Zhao, 2007: Global patterns of low-mode internal-wave propagation. part ii: Group velocity. Journal of physical oceanography,37 (7), 1849-1858."Is this the correct reference? I believe the authors intended to refer to "Alford, M.H. and Z. Zhao, 2007: Global Patterns of Low-Mode Internal-Wave Propagation. PartI: Energy and Energy Flux. J.Phys. Oceanogr., 37, 1829-1848, https://doi.org/10.1175/JPO3085.1"
yes, thank you, modified

Page 2, Line 27 and throughout the manuscript: the format of the references to the figures is incoherent. Many different formats are used, eg. Fig.9 (Line 27), Fig.1(Line 12), Fig6d (Line 33). Please uniformise.
Yes, done: when within "()" Fig.xx otherwise Figure xx

Page 2, Line 31: purposeS
Yes, done

Page 2, Line 32 and throughout the manuscript: in "N2 fixation", "2" should be subscript as in Page 1, Line 16
done

Page2, Line 33. Italics: Trichodesmium
done

Page 3, line 19 and throughout the manuscript: there is no space between units and the corresponding figures(eg. 2min).I would suggest to add a space here
done

Page 3, line 29 and throughout the manuscript: Units should not be in italics
done

Page 4 line 20: the molecular viscosity was already defined in line 11, move "= 1.2 × 10..6m2 s..1." to line 11.
done

Page 4, Line 26: "...West of 185E"
modified into 175W to be consistent with longitude

Page 5, line 10: end paragraph here?
yes, done

Page 5, line 17: end paragraph here?
yes, done

Page 5, line 21: remove "...of our study area"
yes, done

Page 5, lines 29-30: In this sentence "The maps reveal a striking longitudinal contrast in inertial flux until mid March (Fig.6a-e)", striking might be too strong.
"striking" replaced by "strong"

Page6, line9.In "... the shear is far larger" remove far*
done

Page 6, lines 6-7: In the sentence "Turbulence at LD-A is by far the largest down to 400m depth with contrasted mean epsilon and Kz between LD-A on one hand and LD-B and LD-C on the other hand (Fig7a and b), within a factor of 5..10 for epsilon and Kz." the authors intended to describe both the vertical distribution and the variability between stations, which makes the reading and interpretation very difficult. Also,"by far" is imprecise here. I suggest to split the sentence into sentences and report some mean epsilon or Kz values to better quantify the differences. I am not a native English speaker but I have the impression that it would be better to use "by a factor of 5-10" instead of "within a factor of 5-10". At least it is easier to understand, from my point of view.
A table, Table 3, has been added to specify these variations, with depth-averaged values, at each LD station. We followed your recommendation and replaced "within" by "by".

Page 6, line 28. Maybe change to "The enhanced epsilon at LD-A is *coincident with an energetic niw *at 50-200 m (Fig.10a)."
done

Page 8, lines 2-4. This sentence is too long. Consider splitting.
done

Page 8,line 14:I can't figure out the meaning of "locally" in this sentence
"locally" was replaced by "in a few spots"

Page 8, line 16. Consider to introduce a new paragraph here.
Yes, done

Figure 1:
-Add epsilon and K_z symbols in the caption. Eg. "Log values of dissipation rate of turbulent kinetic energy (epsilon, Wkg ..1)".
-Remove "(log scale)", this information is repeated.
-Add "longitude(E)" in the xlabel. The same in the following figures
-The caption states "Time-averaged values at long duration stations, LD-A, LDB and LD-C are displayed with diamonds while values at short duration stations are displayed with circles.",however, I could not see any diamond in this figure
The figure has been modified accordingly with circles for all epsilon and Kz values.

Figure 2:
-Magenta symbols and lines are not easily visible for me in this figure (and others).I would suggest to use a different color
done for Figure 2, magenta replaced by red in (b)

-In the Methods sections the reported SADCP frequencies are 150 and 75kHz. According to the Figure caption velocity data were obtained with a 38kHz SADCP. Is it a different instrument?
Thank you for noticing, there was an error in the Methods section, that we have corrected.

Figure 3:
-I would suggest to represent the mixed layer depth
-Indicate whether the represented profiles are station-averages or individual profiles
-Circles overlap with each other more than I would like to. In this way it is difficult to interpret the vertical patterns. I would suggest to make the figure larger in the vertical dimension in order to reduce the overlap.
The mixed layer depth has been added in the figure (magenta curve), profiles at the long duration stations have been time-averaged for better visualization. The figure has been enlarged as well.

Figure 4:
-Panel c: the authors could highlight somehow the Ri values <1 or <0.25, to stress the areas of instability. If the information is the same as represented in Figure 3,
you could also use the same color scale to avoid confusion.
As advised by the other reviewer, we display N2-S2 instead of Ri and use a linear colorscale that highlights the regions where Ri<1; the same colorscale is used in Figure 3 and 4.

Figure 7:
-Caption: specify with which instruments N2 and S2 were obtained. add something like that:"[...] were inferred from the rosette-mounted CTD and LADCP instruments/SADCP(?)"
done

Figure10:
-Could you specify to which SADCP each line corresponds in the legend as well?
done

Figure 11:
-The x-scale of the subplots is different. I am also confused by the number of profiles shown in panels (b-c). There are more profiles shown here than stations in BGD the cruise (18) but less than the total number of profiles (>100). How is that possible? Are they station-averaged profiles? If not, what does the x-axis represent?
-What do the shaded areas represent? Zero vertical gradient (= zero flux)? Indicate
-There was a problem with the x-scale: the scale is longitude and should be the same for all subplots, this has been corrected
-the vertical lines displayed refer to the LD stations
-the shaded area represent regions with no measurements

Figure 12:
-Caption: "Longitude depth sections of ..." Longitude-depth is not correct. comment Does not the x-axis represent time in days as in Figure 8?
yes, the x-label corresponds to time in days, this has been corrected, thank you for noticing that.

Figure 13:
-Add phosphate fluxes. You could also add mean (or median) values and confidence intervals
The mean values are now displayed on the figure, the error for the flux is given in the method section and is inferred from the threshold concentration and molecular diffusion.

Figure 14:
-You could add mean (or median) values and confidence intervals
Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2018-170, 2018.

The mean values are now displayed on the figure.