Response to the first reviewer.

First of all we would like to thank the reviewer for his very careful review of our work including relevant scientific comments, many editorial comments and English corrections. We believe that the reviewer comments helped to improve this manuscript.
We have included our reponse within the reviewer comment.

General Comments
In this paper the authors characterize turbulence from microstructure measurements in the upper 100m within 3 anticyclonic eddies in the Mediterranean collected during the BOUM experiment. They then test a finescale parameterization of the turbulent kinetic energy dissipation rate based on internal wave scale shear and strain against the microstructure measurements. This parameterization is then applied to deep finescale measurements to characterize vertical mixing within the eddies' full depth range as well as along the east-west transect that was made as part of the BOUM survey. The key results are:

1. a report of a high (O(10-6 W/kg)) level of the turbulent kinetic energy (TKE) dissipation rate in the seasonal pycnocline and a moderate level (O(10-8 W/kg)) below based on microstructure measurements in the upper 100 m of 3 anticyclonic eddies in the Mediterranean sea;
2. an (inferred) significant increase of the TKE dissipation rate at the top and base of eddies associated with strong near-inertial waves, and enhanced (inferred) turbulent vertical mixing in these regions and in the weakly stratified eddy core, each inferred from finescale measurements of internal wave shear and strain and the application of a finescale parameterization for the TKE dissipation rate;
3. a picture of the spatially variable distribution of (inferred) turbulent dissipation and mixing across the BOUM Mediterranean transect. In particular shear and dissipation are enhanced near the surface and near the bottom especially at the base of eddies and in a straight suspected to be strongly influenced by topographic effects. Inferred turbulent mixing is characterized by a steady increase with depth.
Overall the paper presents new direct and inferred measures of turbulent mixing and dissipation rates and emphasizes the spatially varying nature of these quantities. Measurements of these rates and their spatial variability will certainly be interest to the Biogeosciences community. However, it is my opinion that the paper requires significant improvement before it is of publication quality. Key to there improvements are revisions related to the language, style and presentation, however there are also some shortcomings in scientific quality that also require attention. Details, both general and specific, follow below.
My general comments with respect to the principal evaluation criteria are as follows:
1. Scientific significance:
It is my option that the manuscript is adequate with respect to the substantialness of its contributions.
It reports on new data, specifically the microstructure measurements and the deep finescale data at the 3 long-duration stations, as well as the 26 stations, and emphasizes the spatial variability revealed by this large survey. This in itself means this paper makes a unique and worthwhile contribution.
However, it is my opinion that the significance of its contribution could be greatly enhanced through improvements to the quality and breadth of the analysis. See comments regarding scientific quality below.
2. Scientific quality:
In general the scientific approach and applied methods are valid and adequately explained. There are however some important shortcomings in the discussion of the data and methods section regarding the finescale parameterization that need to be corrected. See Specific Comment [1] below.
Also, I was concerned that several discussions/statements were disproportionate to the results being reported. I would recommend caution in making overly grandiose claims. For specific examples see Specific Comment [2] below.
Finally, it is my opinion that the analysis could in many places be extended to gain more insights from the available data. This I feel would increase the significance of the results reported. For details see Specific Comment [3].
3. Presentation quality:
Here also there is an important need for improvement. The discussion is often vague and imprecise, sometimes so much so that the meaning is unclear. A number of examples are given in the list of technical corrections. Imprecision (for example referring the to turbulent kinetic energy dissipation rate epsilon (W/kg) as simply "energy dissipation" and suggesting that the microstructure measures the TKE dissipation rate due to internal wave breaking) undermines confidence in the authors understanding of the methods being applied.
There are issues with English language fluency, mainly missing articles. I have tried to note them in the list of technical corrections below, but there are numerous occurrences and I have certainly missed some. Having the manuscript carefully proof-read by a native English speaker if possible would help. Less excusable are the existence of various typos, undefined symbols and reference values, and inconsistent notation. Again I have tried to note these in the list of technical corrections, but I recommend careful proof-

reading before the manuscript is re-submitted. Figure captions are especially prone to typos and inaccuracies. Finally, throughout inconsistent assumptions are made regarding the reader's familiarity with the BOUM experiment. I understand that this is a contribution for a special issue on BOUM, but am unclear about the background that will be provided. Regardless the presentation could be improved by assuming a consistent familiarity with BOUM (for example it is not consistent to define the broad goal of the experiment and many details about the cruise but earlier in the Introduction not be specific about which basin you are referring to. Additional examples are provided in the technical corrections.

Specific Comments:

1. In the discussion of data and methods there are some important additions/corrections that need to be made. The impact of these omissions and errors (potentially resulting from imprecise language and not the authors' meaning) is significant as it undermines the reader's confidence in the authors' understanding and hence application of the methods.

Examples include:

- the omission of any discussion of the wavenumber dependence: energy dissipation will scale like the energy of the internal wave field at the critical wavenumber

The scaling of dissipation rate as proposed by Henyey at al (1986) and adapted by Gregg 1989 Polzin 1995 or Kunze 2006 et al parameterization is a E^2N^2 dependence where E is the total internal waves energy, not the energy at the critical wavenumber. However it is true that some assumptions on the critical wavenumber are made in practical applications of this scaling. For instance In the Gregg 1989 parameterizations (used here with the addition of the frequency content dependence) the energy dependence is translated as $E=E_{GM}(S_{10}$^2/S_{GM}^2), where S_{GM}^2 is the Garret Munk shear variance integrated between kz=0 rad/m and the critical wavenumber kc=0.6rad.m^{-1} (corresponding to a λ_c=10m wavelength following observations of Gargett 1981) and S_{10}^2 is the shear estimate from 10m velocity difference . S_{10} is also mathematically equal to a 10m running average of the shear which is a low pass filter of the shear, therefore S10 does not represent the shear at the critical wavenumber but will rather receive contributions from all wavenumbers below kc. As noted by the reviewer the critical wavenumber depends on the energy level of the internal waves field. An issue raised by Gargett 1990 is that Gregg 89 may underestimate the dissipation for energetic internal waves field for which the roll off wavenumber would occur below kc=0.6rad/m (or to be more specific below kc/2=0.3 rad/m which represents the half power point 10m running average filter (Polzin 1995)). However in this study wavenumber shear spectra from the first 500m of the water column do not show evidence of any roll off below kc/2=0.3rad/m so there is no contradiction to apply the Gregg89 scaling there. For deep profiles shear spectra below 500m are above the GM and the critical wavenumber is likely reduced. But for noise issues the signal is applied for wavenumbers smaller than knoise/2=0.06 rad/m and no evidence of roll off is observed below this low wavenumber.

We have added much more details about the Gregg 89 parameterization and we added a small discussion on the critical wavenumber in section 2.3

in the discussion of the "drawback of the G89 parameterization" (page 8967) it is stated that the main G89 shortcoming is the tendency to underestimate epsilon in the ocean interior. This is just a consequence of the drawback that you describe, namely the failure of the parameterization to take into account the frequency dependence of the wave field. The shear-to-strain ratio dependent correction factor you describe that is used to correct for this shortcoming should be referenced to Polzin et al. 1995.

We agree with this point we have reformulated this part and referred to Polzin 1995. See in the technical comments below.

- in the discussion of the higher frequency part of the spectra (page 8967) it is stated that strain dominates over shear. Instead I think strain variance is proportionately more important, but the shear to strain variance ratio will still be greater than one.

We agree and we have corrected this point as a response to the reviewer technical corrections below

A number of other examples are listed in the technical corrections.

2. I believe some statements are disproportionate to the results reported.

For example:

- I believe it is not valid to claim that you validate the finescale parameterization in that a test against microstructure measurements between 25 and 100 m depth in three eddies does not constitute a validation for the range of depths and different environments to which it is later applied. In particular I think including "Validation" in the title is not appropriate. I think some comment about the validity of the "validation" in its extension to other depths and environments is warranted.

Ok we changed the the title to "Characterization of turbulence from a fine-scale parameterization and microstructure measurements"

However we believe that the parameterization conditions at larger depth z>100m within the eddies is valid, since the shear spectra shape does not evolve significantly with depth as shown in Fig.4. Only the ratio to GM canonical level shows some evolution an effect that is taken into account in the parameterization. It is

true however that the parameterization was not validated for deep profiles outside of the eddies, there we can only claim the application of a classical parameterization in conditions where it should apply.
- in the Introduction it is stated “the results are discussed in relationship with the specificity of internal wave dynamics in these environments.” Is this done? Where?
We discuss the increase of dissipation related to increase shear at the base of the eddies A and C in section 4. In the discussion we propose some hypotheses for the increase near inertial shear at the eddies A and C base, namely trapping of near inertial waves. Further discussion on the internal waves environment was also added as a response to the point 3 of the reviewer below.
- in the Introduction it is also claimed the analysis of the transect measurements provide “a first insight of the impact of internal wave mixing at the basis scale”. I believe you do not discuss the impact of the mixing but rather report on its magnitude.
Ok we corrected this sentence to « provide a first overview of the internal wave mixing rate at the basin scale »
Similarly, the discussion does not characterize processes from the microstructure measurements as it is claimed but rather the rates of the turbulent kinetic energy dissipation rate. A number of other examples are listed in the technical corrections.
Ok this sentence was changed to characterize turbulent dissipation from the background internal waves field shear and strain

3. Finally it is my opinion that there are several areas where the analysis could be extended to gain more insights from the available data. This would increase the significance of the results reported.
Examples include:
- a more through/quantitative analysis of the relationship between microstructure measurements of turbulent dissipation and finestructure measurements of the internal wave field. How do regions of enhanced turbulent dissipation (quantitatively) relate to internalwave shear and strain variance? Discussing the link between the microstructure measurements and the finestructure observations of the internal wave field will allow you to back up the claim that “turbulent processes” are characterized, not only turbulent rates of mixing and dissipation.
We added a new figure and discussion relating the evolution of the turbulent dissipation as a function of the fine scale shear and buoyancy scaled strain in section 3.2. (see Fig.10 at the end of the review)
- a more detailed discussion of the observed internal wave field from finestructure measurements. For example what are the observed internal wave energy levels? What is the observed spectral shape of internal wave energy, shear and strain? Is variance enhanced at certain wavelengths, and do these scales give hints as to internal wave sources and/or evolution? Similarly do metrics such as the shear to strain variance ratio or the polarization of the wave field give further insight into internal wave sources, evolution or governing dynamics? This would validate the claim “The results are discussed in relationship within the specificity of internal wave dynamics in these environments”.
The wavenumber and frequency shear spectra are already provided in Fig;2 and 4 and their shape/level is compared to GM spectrum we make a new reference to Fig.2 in section 4 in order to illustrate the dominance of a wavenumber around 125m which is related to the vertical wavelength of near inertial internal waves. In addition we added a new panel on fig.11(now Fig.12) showing the profile of shear to strain ratio Rw for stations A, B and C and we discuss its value in section 4.
More specific examples are provided in the list of technical corrections.
Technical corrections:
[title] I suggest “Characterization of turbulence and validation of THE fine-scale parameterization in the Mediterranean Sea during THE BOUM experiment. We corrected the title to “Characterization of turbulence from microstructure measurements and a fine scale parameterization in the Mediterranean sea during the BOUM experiment”
[p 8962]
[lines 1 to 5] This sentence assumes a degree of familiarity with the BOUM experiment (the meaning of its acronym; that “western and eastern basins” are those of the Mediterranean Sea). This may or may not be appropriate but should be consistent with other descriptions of background information regarding BOUM.
[line 7] 100 m OF THE WATER COLUMN ok corrected
[line 7] “we focus here on the characterization of turbulent mixing induced by internal wave breaking.”: An important point is that microstructure measurements measure the turbulent kinetic energy (TKE) dissipation rate due to all sources of turbulence, not just internal wave breaking. The finescale parameterization infers the TKE dissipation rate from measures of internal wave shear and strain and assumes the observed wave energy is dissipated due to internal wave breaking. The placement of this sentence here in the middle of discussion of the microstructure results (and before the discussion of the fine-scale parameterizations) makes it unclear about whether the authors appreciate this distinction and confuses the reader regarding what the microstructure vs. finescale observations represent.

The authors understand very well this distinction, of course the microstructure measurements can't discriminate a specific mechanism generating the turbulence. We assume that most of the dissipation is generated from internal waves breaking, an assumption that is largely accepted for the ocean interior. However we agree to correct this sentence in order to avoid any confusion on the nature of measurements and its distinction with a parameterization..
[line 10] "energy dissipation mean values" should be the "mean turbulent kinetic energy dissipation RATE values". Nowhere in the discussion is the energy dissipation referred to as a rate, but a rate (in the units of W/kg) is being reported. Imprecise language such as this undermines the reader's confidence in the authors' expertise.
Ok we abusively used dissipation for dissipation rate, this has been corrected throughout the article. The authors have a good expertise on the subject and have even work to improve the methods for determining dissipation rate from SCAMP measurements they do not confuse these quantities.
[line 11] "mimic energy dissipation produced by internal wavebreaking" : This is too informal and imprecise. The parameterizations infer a TKE dissipation rate assuming a downscale energy cascade of the energy of the observed wave to the wave breaking scale. Ok corrected
[line 11] "wavebreaking" should be "wave breaking" Ok corrected
[line 13] It is my opinion that "validated" is too strong a description of the tests performed. Further "parameterizations" should not be plural. Only one form of the parameterization was tested. Ok we changed validated to compared
[line 13] "a parameterization" is vague. Specify which form of the parameterization is applied. I have seen this referred to as the latest incarnation of the Henyey et al. (1986) parameterization as applied in Kunze et al. (2006) (or something more recent). Ok corrected
[line 13]: "mixing" is vague. You infer the rate of turbulent vertical mixing. Ok corrected
[line 14]: "thus providing an overview": an overview of WHAT? Ok the sentence was modified
[abstract general] please also include a summary of the results relating to the characterization of the turbulent dissipation and mixing rate along the E-W transect. Ok a sentence was added
[line 19]: "dynamical vertical transport". The word dynamical is not required. Ok corrected
[line 21]: "in order to represent adequately biogeochemical processes": To represent in WHAT? Ok changed to characterize
[page 8963]
[line 1]: "is of particular importance": Important to WHAT?
[line 4]: "anticyclonic eddies have focused much attention": The eddies do not focus attention, they are the subject of our focused attention. Ok corrected
[line 5]: "intrinsic processes": intrinsic to WHAT? the eddy dynamics? Yes corrected
[line 8]: "THE seasonal thermocline" Ok corrected
[line 9]: "uplift of A nutrients-enriched deeper layer" Ok corrected
[line 10]: "THE eddy motion" Ok corrected
[lines 10-12]: The description of the process of near-inertial wave trapping is relatively sparse compared to the discussion of eddy pumping. Can more details be added here?
Ok details have been added as:
"Indeed the negative vorticity ζ of the anticyclonic eddy can shift the effective inertial frequency to a lower value feff=f+zeta/2 (Kunze, 1985) therefore near inertial waves which evolve in the frequency band f>feff will encounter their turning points when propagating away from anticyclonic eddy centers (Bouruet-Aubertot et al, 2005) and remained trapped in the eddy core".
[line 13]: "affected by a significant mesoscale dynamics" is awkward. Perhaps "in which mesoscale dynamics is important"? Ok corrected
[line 15]: "specific" is awkward. Perhaps "unique"? Ok corrected to particular
C4840
[line 16]: specify INTERNAL WAVE energy Ok
[lines 17-25]: Here is a very general description of the BOUM goals. Will this not be covered elsewhere in this special issue? This is a good example where the authors assume very little familiarity with the BOUM experiment in contrast to other places where they assume a high degree of familiarity.
Ok there is an introduction paper y Moutin et al therefore we deleted this sentence describing the general goals
[line 23]: "determine THE physical forcing" Ok corrected
[line 28]: "estimation" is awkward. Replace with "estimates"? Ok corrected
[page 8964]:
[line 1] "ON" not "in" the plateau Ok corrected
[line 1] "THE mixing processes" Ok corrected
[line 3] "we focus on the characterization of turbulent dissipation and mixing RATES" Ok corrected
[line 4]: "resulting from internal wave breaking": Please be aware that the dissipation

rates measured from microstructure do not necessary have to be due to internal wave breaking. Inferring dissipation rates from the finescale parameterization used assumes the observed internal wave energy is dissipated by internal wave breaking. Of course, see above our answer to this question, we suppressed "resulting from internal waves breaking" to dissipate any source of confusion on the microstructure measurements.
[line 5]: "turbulent processes are characterized from microstructure measurements": No. Turbulent dissipation RATES are characterized by the microstructure measurements. Characterizing processes requires further interpretation of microstructure (and likely other measurements). Ok changed
[line 6]: I feel "validate a parameterization" is too strong a statement for the tests reported on. Ok changed to "favorably tested"
[line 8]: "within THE eddies' full depth range"
[lines 8-9]: "the results are discussed in relationship with the specificity of internal wave dynamics in these environments": Do you actually discuss the internal wave dynamics? I don't think so. Also this is a very awkward sentence. We indeed discuss high level of dissipation rate considering the possibility of internal waves trapping, following the reviewer comments we also add some discussion on the influence of the shear to strain ratio Rw. The sentence was changed to "This parameterization is then applied in order to characterize vertical mixing within the eddies full depth range and effect of possible internal waves trapping is discussed"
[lines 11-12]: "providing a first insight of the impact of internal wave mixing at the basin scale": Again I believe you discuss RATES not IMPACTS. Ok this is true impact was changed for rates.
[paragraph 1]: Is it important to relate the goals of this study to the larger BOUM goals here? This paragraph was deleted
[lines 14-21]: Again this is very general information about the BOUM experiment. Is it not presented elsewhere? It is not consistent to give this information but not say define the basins referred to in earlier discussions. This paragraph was deleted
[line 21]: A year should be provided for the Moutin et al. reference. Ok
[line 23]: "for each STATION" Ok corrected
[line 24]: "binS" Ok corrected
[line 25]: "measured BY" Ok corrected
[page 8965]
[line 2]: change to "vertical profiles of current velocity at 8 m resolution". Ok corrected
[line 4]: "stationS" Ok corrected
[line 11]: "energy dissipation rate (epsilon)" is more precisely "the turbulent kinetic energy dissipation rate" Ok corrected
[line 12]: "THE vertical diffusivity" Ok corrected
[line 16]: "Estimation of dissipation" is more precisely "Estimation of the dissipation RATE" Ok corrected
[lines 25-26]: "this appeared to be more efficient when noise was larger ...". Do you mean "efficient" or "effective"? What basis underpins this statement? More details please. Some details have been added as :
"Ruddick et al. (2000) method includes a model of the noise as a part of the model spectrum that is fitted to the experimental curve. For very low chiT the temperature gradient spectra is dominated by noise and small inaccuracies in the noise model results in large errors in the estimation of chiT. In Luketina et al (2000) method the high wavenumber part of the spectrum dominated by noise is simply discarded, this appeared to be much more effective when noise variance was larger than chiT"
[line 2]: "inferred from THE TURBULENT kinetic energy dissipation RATE" Ok corrected
[line 4]: "the ratio between THE buoyancy flux and THE turbulent production RATE" Ok corrected
[line 7]: "have shown that THE Osborn relationship" Ok corrected
[line 9]: "REGIME" not "regimeS" Ok corrected
[lines 7-13]: Is there other work that uses/validates the Shih et al. (2005) modifications that can be cited here? Yes, Jardon et al (JGR 2011), Fer and Widell (2007) , Van der Lee and Umlauaf JGR(2011), for instance uses Shih et al modification. The reduction of mixing efficiency is also consistent with some decrease of mixing efficiency observed for high turbulent intensities (Etemad Shadidi and Imberger 2005, or Moum 1990 for instance). However there are certainly some remaining issues regarding to which value the mixing efficiency should be set (cf Ivey et al Annu.Rev. Fluid. mech. 2008) we use here the state of the art parameterization. A paragraph was added to cite these studies.
[line 11]: define the symbol v ok
[line 12]: add year to the Shih et al. reference ok corrected
[line 14]: "THE Osborn relationship" ok corrected
[line 16]: provide a justification for the choice of 7
ok "Shih et al (2005) also show that turbulent diffusion is inefficient below epsilon/nu N^2<7".
[line 17-18]: I am confused as to whether Kturb is a diffusivity for density or temperature. Is it strictly correct to combine this with the molecular diffusivity for heat? Also isn't

the molecular diffusion negligible compared to the turbulent diffusion? Some comment here to this effect seems fitting.

Kturb is a turbulent diffusivity for density. This turbulent diffusivity is generally much larger than the molecular diffusivity when the turbulence intensity is strong enough. However according to Shih et al 2005 when the turbulent intensity is smaller than 7 turbulence is unable to mix the fluid and the diffusivity is then equal to the molecular diffusivity. We use the molecular diffusivity of heat for the molecular diffusivity of density since the density is primary set by temperature for the water masses considered here and because the molecular diffusion of salt is ten times smaller. We have specified this point in the text.

[line 19] "Fine scale" is sometimes written "fine-scale". Please make consistent ok corrected throughout.ok we choosed fine-scale

[line 20]: "classically" is inappropriate. Perhaps "commonly" or "typically"? ok corrected

[line 21] " A fine-scale parameterization" ok corrected

[line 22-6 on following page] "Basically, this relationship depends on the dynamics of the internal wave field that controls energy transfers towards small scales..." This description is arguably inappropriately general, informal and imprecise.

No reference is made to wavenumber dependence or the critical wavenumber.

Ok changed to:

'This parameterization assumes a steady state spectrum of internal waves where wave-wave interactions transfer energy from large to small scale motion. Internal waves eventually break when they reach a critical wavenumber .'

"IGW" and "reference energy level" are not defined. It is stated that here the energy level of the internal wave field is comparable to the reference energy level, but no diagnosis of the observed wave field energy levels are presented.

In practical applications of such parameterizations it is generally the shear level of the internal waves field rather than the energy level which is compared to GM (Gregg, 1989, Polzin 1995). Shear spectra of Fig.4 shows that the spectral shape and shear level is comparable with Garret and Munk. We have reformulated this whole paragraph from p6 l23 to p7 L22 to clarify this point and avoid any confusion.

[line 8]: "reasonably" is inappropriately informal and imprecise. Changed see above

[line 12-13]: "THE vertical derivative..." Changed see above

[line 17]: "This is consistent with the main assumption": I don't understand what you mean by this. Changed see above

[line 22]: It is inconsistent with your treatment of other reference values to not give the numerical value of No.ok corrected

[lines 24-25]: "The main drawback of the G89 parameterization is its under-estimate of epsilon values in the ocean interior. There, as internal wave breaking comes into play as well, the higher frequency part of the wave field, for which strain dominates over shear, is not properly taken into account." Again this description is arguably inappropriately informal and imprecise, and some statements are incorrect. A main drawback of the G89 parameterization is that it, as it stated later in the discussion, it fails to take into account the frequency content of the wave field. What is meant by "There, as internal wave breaking comes into play as well"? As well as what? What does this statement mean? Also, the shear to strain ratio is smaller for higher frequency waves but strain does NOT dominate over shear (the shear to strain ratio is still larger than 1).

Ok the corresponding paragraph p7 l23 to p8 l5 has been completely reformulated, we notably clarified the link between Rw and the frequency content.

[line 2]: The symbol R_omega should be defined. Ok done

[line 6]: It is not appropriate to begin this paragraph with the word "However". Perhaps "Due to the fact" or "Because"? Ok corrected

[line 8 and also in previous discussion re. 10 m shear]: I am confused about whether the "10 m smoothed strain" is obtained by differentiating the vertical profile of strain over a interval of 10 m, or whether it is computed from the spectral power at 10 m wavelength. This should be made explicit. In fact, in general the wavelength dependence of the estimate of dissipation from finestructure is not very explicit in these discussion. Should somewhere it be discussed that a 10 m wavelength is chosen as it represents the critical wavenumber below which waves break and produce dissipation? The 10 m scale is of course an approximation as the actual critical wavenumber is a function of the magnitude of the shear variance.

What we call a 10m smoothed strain is a 10m running average of the strain, which is in fact mathematically equal to a 10 m difference of the isopycnal displacement (which is consistent with the 10m velocity difference). But practically we do not compute the isopycnal displacement, but directly the strain as zeta_z=(N2-<N>2)/<N2>, therefore we get an equivalent of the 10m difference of the isopycnal displacement with the 10 m running average of zeta_z. Note that such a computation of strain is consistent with the 10m difference based estimate of the shear. We have specified this in the text and a discussion on the critical wavenumber has been added at the end of this section (see our response to the general comment 1 of the reviewer)

[line 9]: Where exactly in the water column is the strain-based estimate applied? This is not clear. Why if the shear is not properly measured for z>25 m is the average shear to strain ratio for 20<x<100 m used? The upper limit of 20 does not seem consistent with the 25 m cut-off.

Yes it was 25m and was corrected

In addition don't you mean z < 25 m not z > 25 m in line 6? Yes corrected

[lines 6-11]: As you mention, due to the lack of shear measurements and the strong deviation from GM79 conditions, the parameterization estimate in the upper 20-25 m is uncertain. Does it make sense to include this with the more accurate/less approximated estimates of the parameterization in the deeper water column? It concerns me that approximately 1/4 of the depth range over which the finescale parameterization is "validated" against microstructure measurements is expected to be uncertain for multiple reasons.

Such strain based parameterizations have been applied by Kunze et al (2006) where shear measurements were lacking and Wijerska et al (1993). The uncertainty associated to the application of such a parameterization is clearly stated in the text. Moreover the parameterization is mainly used to compute dissipation rate at larger depth where shear is available.

[lines 10-11]: Can you specify why deviation from GM conditions occurs in the z < 20 m near-surface depth range? I am surprised you define this by a fixed depth as opposed to a physical condition.

The GM model is strictly valid "far" from boundary and internal waves energy sources, practically these conditions are verified below the pycnocline. During BOUM the pycnocline has an average location at 15m depth and evolves between [10-20m] this is why we state this condition, we have changed p 8 l9 to l11 to clarify this point.

[lines 12-13]: Somewhere it should be stated this is noise in the LADCP VELOCITY MEASUREMENTS. Ok corrected

[line 17]: "To turn around this problem" is informal and imprecise language. Perhaps "To account for this" or "To incorporate noise considerations" Ok corrected

[line 17]: "low pass filtering..." OF WHICH signal? Velocity LADCP or SADCP signal, this was specified in the text

[line 21]: I recommend using "observed" in place of "experimental" but this is my personal preference. Ok corrected

[line 23]: "THE fitting process" should be "A fitting process" unless more details about the procedure are provided. Ok corrected

[line 23]: specify as the "VERTICAL wavenumber at which noise ..." Ok corrected

[line 6]: "We used A FIR filter..." Ok corrected

[line 10]: "Roughly equivalent" is imprecise. What does this mean? In Kunze 2006 parameterization computations are performed in the wavenumber space and spectra are simply cut above kc, here we rather use low pass filtered signal since formulation of Eq.3 is local in space, the lines 4 to 9 p9 were modified to specify this point.

Moreover we have now adopted a new formulation for the normalization by the GM shear variance for low pass filtered LADCP velocity field. Indeed an analytical expression for the GM shear variance for wavenumber smaller than kc can be simply obtained as : S_{GM}^2=(3*pi/2)*j*E_{GM}*b*N0^2*kc*(N/N0)^2 (Gregg 1989 annex A). Where kc is the actual cutoff used to filter observed LADCP velocity field. This expression is straightforward and more accurate than the low pass filtering of stratification previously used.

[line 11]: "in THE Kunze et al. (2006) parameterization" Ok corrected

[line 13]: "if both are LOW PASS FILTERED at the same CUT-OFF wavenumber". Ok corrected

[line 15]: "Figure 2c, d and e SHOW examples ..." Ok corrected

[line 15]: "long duration stations" should be consistently refereed as such Ok corrected

[Section 3 Title and 3.1 Subtitle]: It is perhaps more helpful to specify that these observations relate to the conditions inside the 3 anti-cyclonic eddies as opposed to being an analysis of the Long Duration Stations. We agree the title was changed to "Stratification and dynamics within the three anticyclonic eddies"

[Section 3 Title]: I suggest "Observations: Direct estimation ..." Ok corrected

[line 23]: "This 3 HOUR interval between profileS allows A CHARACTERIZATION OF BOTH the background state..." Ok corrected

[line 24-26]: This definition of the background state is not clear. Do you mean the background state is defined by the low-pass filtered record of stratification and currents that includes sub-inertial frequencies only? What do you mean by "super-inertial oscillations as well as the lower frequency band of the internal wave spectrum"? The lower band of the internal wave spectra includes super-inertial oscillations does it not? As well, what do you mean by the "lower frequency band"?

We have modified this paragraph we did not refer anymore to the low pass filter and lower frequency band, we simply define the background state as subinertial currents and time-mean stratification (averaged over 3 inertial periods) we also indicate the number of inertial periods covered for each long station. We did not make a thorough analysis of the background state as this was done by Louis Prieur and is available in the introduction article by Moutin et al 2012)

[line 26]: By "mean" do you mean the background state defined in the previous sentence?

We meant the time mean location of the pycnocline, it has been specified in the text

[line 1]: "with values _3.10ˆ{-3} rad sˆ{-1}": Values of WHAT? Buoyancy frequency I assume? This needs to be specified. Yes it has been specified

[line 2]: "Stratification next": is imprecise. I suggest "Below the pycnocline..." Ok corrected

[line 3]: "best evidence" is not I think what you mean. I suggest "The most pronounced example" Ok corrected

[line 4]: The notation [depth 1, depth 2] should be defined (as well as earlier in the text where it is used). The notation has been defined

[line 7]: "within [100, 200] m DEPTH" Ok corrected

[line 8]: "The stratification VERTICAL PROFILE presents..." Ok corrected

[line 9]: "which constitute robust barriers limiting vertical transfers": Is this your expectation or based on observational evidence? This should be clear.

This is a general expectation for strong gradients, however in on our case the results show that the vertical diffusivity does not decrease because the strong gradient is counter balanced by high inertial shear and enhanced dissipation. We have deleted this statement here as this is discussed later.

[line 12]: It is not necessary to begin this sentence with "As well". Discussion of the velocity measurements should perhaps be in a new paragraph. Ok corrected

[line 12]: I assume that "mean" is that same as "background state" defined at the bottom of page 8969? Please be consistent and explicit in your language. Ok corrected

[line 12]: "will provide" should be simply "provide". Future tense is not appropriate here. Ok corrected

[line 14]: "along the vertical" should be "IN THE vertical" Ok corrected

[line 17]: Is it possible to reference the eddy position from satellite (or other) measurements? Analysis of the eddy position and characteristics was performed by Louis Prieur and is available on the introduction article by Moutin et al (in revision the BGD is available) we now refer to their article in this paragraph

[line 18]: "We next examined" is too informal. "Time-depth sections illustrate temporal variability and highlight a dominance of variability at the inertial frequency." Ok corrected

[line 18]: "higher frequencies" compared to WHAT? I assume the sub-inertial frequencies included in the definition of the "background" state. This should be made more clear. We have now referred to the internal waves band (rather than higher frequencies) which has already been defined earlier

[line 20]: Reference to Figure 3 should be made here. Ok corrected

[line 20]: It is more accurate to refer to these as time-pressure sections. Ok corrected

[line 21]: Why is it necessary that the waves be baroclinic? Is it because of the observed variation with depth in the coherent propagating signal? Yes as opposed to the barotropic signal which is constant with depth, this was specified in the text

[line 26]: "Also the spectral resolution limited by the duration of the stations..." Indicate here that this is the lower limit of the spectral resolution. There is only one definition for the spectral resolution ($\Delta f=1/T$) which is only fixed by the duration T of the observation. Maybe the reviewer has confused here spectral resolution and min and max of the resolved frequency band. The min frequency is always zero (mean state) and we have already defined the max frequency (half the sampling frequency)

[page 8971]:

[line 1]: "the existence of sub-inertial waves": You have not demonstrated that these motions are waves. "sub-inertial motions" is more accurate.

In fact the peak at 0.8f is associated with near inertial waves but the effective inertial frequency is shifted to $f_{eff}=f+0.5\zeta= 0.8f$ because of the negative vorticity $\zeta \cong -0.4f$ of eddy A. This is discussed later in the discussion section. So we deleted the reference here to subinertial waves as it may be confusing and we simply speak of a shift in the inertial peak.

[line 5]: should be "SHOW a flatter slope" ok corrected

[line 9]: "fine-scale parameterizations" should be "a fine-scale parameterization" as only 1 parameterization model is tested. ok corrected

[subtitle Section 3.2] "Fine scale" is sometimes written as "fine scale" and sometimes as "fine-scale". Please be consistent. Ok fine-scale is now used throughout the article

[line 14]: "long station" should be "long duration station" to be consistent with previous labeling. We have now introduced earlier the acronym LD for Long Duration stations which was used in the whole text

[line 14]: "in the background fine scale strain internal wave field" is awkward. Perhaps

“An indication of the background internal wave field is given through a visualization of background strain from fine-scale measurements”?
the end of the sentence was changed to “the background strain obtained from fine-scale measurements is also shown.”
[line 16]: “neat” is informal and imprecise. neat was changed to strong
[line 19]: “under internal waves heaving” is awkward. Perhaps “due to internal wave heaving”? ok corrected
[line 24]: “IN Fig. 5...” ok corrected
[line 24-25]: “the strain appears clearly related to internal waves induced isopycnal displacement, this is most obvious for station C where a strong near inertial signal is observed.” What is the basis for this statement? The correspondence of high strain and isopycnal displacement does not necessarily implicate internal waves. When you say “is most obvious for station C where a strong near-inertial signal is observed” - is this in reference to the enhanced power at the near-inertial frequency displayed in Figure 4? How do you know the strain/isopycnal displacement in Figure 5 is related to the near-inertial peak for station C in Figure 4?
The dominant feature of Fig5 upper panel is an oscillation of the isopycnal position, the period of these oscillations is 0.85 day (considering 2 oscillations between day 178.7 and day 180.4) which is in close agreement with the inertial period of 0.89 day. We have now specified this in the text.
[lines 27-28]: “the strain values are generally maximum in the pycnocline region which suggests internal wave strain importance in breaking processes...”. Shouldn’t shear also be evaluated and compared before making this statement about the relative importance of strain? We do not mean that strain is the dominant process but that strain should be considered as it is done in the formulation of the parameterization we used. Our aim here was mainly to illustrate dissipation values with some variables representing the fine scale variability in background, we used isopycnal displacement and strain because shear is not available in the pycnocline. However following the revewer comments we also show now a more quantitative analysis of the dependence of dissipation vs shear (below the pycnocline) or strain at the end of this section .
[lines 2-3]: “Still, no clear phase relationship is apparent between internal waves strain and dissipation here.” Do you mean phase relationship in time or the vertical?
Both since internal waves phase is constant along a $z=c_z t$ characteristic
What is the basis of this statement? It just a visual inspection unfortunately dissipation sampling is too sparse to confirm or infirm quantitatively such a relationship. This was specified in the text
[lines 2-3] “internal waves strain” should be “internal wave strain” ok corrected
[the discussion of averaging the turbulent dissipation rate on pages 8972-8973]: This discussion of background understanding is very detailed and does not seem to fit in this section which reports on results. In my opinion it is best largely omitted, with references to the key works provided, or perhaps incorporated into Section 2 on data and methods.
The discussion was largely shorten we now mainly refer to earlier works
[line 10]: “turbulent dissipation” should be “the turbulent dissipation rate” ok corrected
[line 11 and later uses]: lognormal is more conventionally written as log-normal. It should not be capitalized in line 13. ok corrected
[line 15-16]: “within the framework of homogeneous isotropic turbulence (Kolmogoroff, 1967) multiplicative cascades for homogeneous isotropic turbulence”. This sentence does not make sense. Surely the phrase “homogeneous isotropic turbulence” needs only to be used once. This paragraph was deleted
[line 17 and all other references]: “dissipation” should be at least “dissipation rate”. ok corrected throughout the article
[line 21]: ‘criticize THE Baker and Gibson (1987) approach” This sentence was deleted
[line 25]: “AN arithmetic mean...” This sentence was deleted
[page 8973]
[line 5]: “were distinguished”... HOW? The first region corresponds to the depth range variation of the pycnocline
[line 8]: “Maximum Likelihood” should be defined or referenced. A reference (Priestley, M. B., 1981: *Spectral Analysis and Time Series.* Academic Press, 890 pp) was added .
[line 11]: “the PDFS show two dynamical regions. For data...” . ok corrected
[line 16]: “which may result partially from A lack of convergence...” ok corrected
[line 16]: I prefer “observed” over “experimental”. ok corrected
[line 16]: “PDF” should be “PDFs” ok corrected
[line 19]: “All in all” is informal. Perhaps “Overall”? ok corrected
[line 24]: “THE arithmetic mean ...” ok corrected
[line 25]: “is lower at station C WHERE IT IS ON THE ORDER OF 10ˆ(-7) W kgˆ{-1} than at station B and A WHERE IT IS ON THE ORDER OF ...” ok corrected

[line 26]: “PDF” should be “PDFs” in both instances ok corrected
[line 27]: “shows” should be “show” ok corrected
[page 8974]:
[line 3]: “THE MLE estimate...” ok corrected
[line 12-13]: “Davis (1996) advise” should be “the advice of Davis (1996)” ok corrected
[line 15-16]: “In order to reduce dispersion of epsilon and kappa_z values...” What do you mean by this? We just mean that the data are smoothed by the running mean, we rephrased the sentence.
[line 21]: “pretty constant” is informal and imprecise. Perhaps “approximately constant”? ok corrected
[line 23]: “from more” should be “by more” ok corrected
[line 26]: “largest” should be “large” ok corrected
[line 27]: “neat” is informal and imprecise. Perhaps “near-constant level”? ok corrected
[page 8975]:
[line 1]: “fairly good agreement” is informal and imprecise. Perhaps just “good”? ok corrected
[line 3]: “interval of SCAMP measurements” should be “interval of THE SCAMP measurements” ok corrected
[line 4]: “when THE average...” ok corrected
[line 4]: “performed” is awkward. Perhaps “computed”? ok corrected
[line 8]: “Punctually observed” is awkward. I’m not sure what you want to express with the adjective “punctually”. ok corrected
[line 10]: “The whole profile set” is awkward. Perhaps the “all Long Duration Station average”? ok corrected
[line 14]: “Station averages” are more clearly referred to as “Individual station averages” ok corrected
[line 17]: “due to the lack of statistics”: What is the basis of this statement? Is it more accurate to say “likely due ...” ok corrected
[line 18]: “according to THE Shih et al. (1995) classification...” ok corrected
[line 20]: “regime” should be “regimeS” ok corrected
[line 20]: “dominates” should be “dominate” ok corrected
[line 21]: “around 40” should be “around 40 m depth” ok corrected
[line 21]: “corresponds to region” should be “corresponds to A region” ok corrected
[line 22]: “where THE molecular diffusion regime ...” ok corrected
[line 23]: “THE overall average VALUES OF ...” ok corrected
[line 23]: “falls” should be “FALL” ok corrected
[page 8976]:
[line 2 and future references]: “epsilon” should be “epsilon_param” to be consistent with previous notation. “K_z” should also indicate it is inferred from the parameterized estimate for epsilon ok notations epsilon_param and Kz_param are now used
[line 3]: “domain” is awkward. Perhaps “range”? ok corrected
[line 7]: “most striking evidence” is awkward. Perhaps “This is most apparent in eddy C”? ok corrected
[line 8]: “highest values OF DISSIPATION” ok corrected
[line 10-12]: “This increase in dissipation can be related to the high shear values ... that results both from the mean current profile and from strong near-inertial internal waves for eddies A and C...” “can be” is awkward. Perhaps “is” or “may be” depending on the evidence. Also shear from the mean current profiles does not likely contribute to the shear taken into account in the finescale parameterization is on 10 m vertical wavelength scale. Is this true? Finally “results” should be “result”.
The parameterization is not on a 10 m wavelength but based on a first order 10m difference ,which is rather a low pass of the shear signal and is therefore affected by low wavenumber shear and possibly the mean current shear. However the dominant signature on the shear comes from the near inertial internal waves with a ~100 -150m vertical wavelength at the base of eddies A and C (Fig. 3 (a, c ,d ,f) and which corresponds to the peak at 8.10-3 cpm (125 m wavelength) on spectra of fig 2 c and e. The mean current has likely a much smaller signature on the shear. L10-12 where changed to explain this.
[lines 12-13]: “This impact of near inertial internal waves on dissipation is best evidenced...” Explain how. We have now related the increase in dissipation at the eddy base compared to the eddy core to the strong near inertial internal waves shear
[line 13]: Here inconsistent notation is used to express depth ranges. Ok corrected
[line 14-15]: “Stratification comes into play” is too informal. Perhaps “due to the impact of stratification”? Ok corrected
[line 17]: “are of the same order as those at the base of the eddy associated with waves”. This sentence troubles me. All estimates of epsilon/K_z from the parameterization are in theory associated with waves, as that is what the parameterization model assumes. Statements such as this underpins confidence in the authors’ comprehension

of the methods applied. Is “associated with enhanced wave activity” the meaning
that the authors’ intend? Yes, we wanted to refer here to the strong near inertial signal at the eddy base. We changed the end of the sentence to : “where strong near inertial internal waves are observed” to avoid any confusion.
[line 18]: “at” is not required.
[line 19]: “snap shot of dissipation and mixing”. It should be specified that this is an estimate/inference of dissipation and mixing based on the finescale parameterization model. Ok this was specified
[line 21]: “evidenced by” is awkward. Perhaps “is seen in the depression...” Ok this was specified
[line 21]: re “isopycnes”: Do you mean “isopycnals” yes corrected
[line 22]: “THE same featureS ARE observed ...” Ok corrected
[line 24]: please provide a reference for the observations of the Ierapetra eddy
We in fact refer here to our Fig.12
[line 25]: “THE highest shear and dissipation values ...” Is this shear at 10 m vertical wavelength? This is not the shear at 10 m vertical wavelength but this is the shear base on a 10m finite difference, this is now specified
[line 27]: “LADCP data” (stations is not required). LD stands here for Long Duration not LADCP
[page 8978]:
[line 1]: “is also evidenced”is awkward. Perhaps simply “is also seen”? Ok corrected
[line 2] “LD station profiles”: Have you defined what LD stands for? Yes it has been defined
[line 5]: “is likely associated with topographic effectS”: What is the basis of this statement?
We have changed likely to possibly. Increase of internal wave induced dissipation rate are generally observed in regions of complex bathymetry: larger barotropic baroclinic conversion rates, scattering of low mode internal waves or enhanced bottom drag.
[line 7]: “ A slight enhancement...” Ok corrected
[line 9]: “diffusion” should be “diffusivity” Ok corrected
[line 15]: Should there be a paragraph break here? No corrected
[line 16]: “nutrient” not “nutrients” fluxes Ok corrected
[line 19]: “in THE G89 parameterization” Ok corrected
[line 22]: in THE north-western Mediterranean sea. ” Ok corrected
[line 22]: “fluxes estimations” is awkward. Perhaps simply “flux estimates”? Ok corrected
[line 23]: “the slightly adapted G89 parameterization we propose here...” Propose is the wrong verb. The shear to strain ratio dependence adaption was proposed by Polzin et al 1995. Here you USE the adapted G89 parameterization. Ok corrected
[line 28]: Do you mean between the base of the seasonal pycnocline and 100 m depth? Yes corrected
[page 8978]:
[line 3]: “directly estimated from SCAMP measurements and parameterization” is confusing. Perhaps “Directly observed by SCAMP measurements and estimated from the fine scale parameterization”? Ok corrected
[line 4]: “is in between THE Copin-Montegue (2000) and Moutin and Raimbault (2002) values...” Ok corrected
[line 6]: “obvious” is informal and awkward. Perhaps “pronounced”? Ok corrected
[line 9]: “induce a negative background vorticity” is incorrect. They make a negative contribution to the background vorticity.
We understand the formal distinction made by the reviewer but we don’t think the distinction between the two statements is necessary and that it would be confusing for the reader. If an eddy can be identified and characterized (as it is the case for the three eddies in BOUM) it means that its vorticity is largely dominant over some background vorticty not associated with the eddy.
[line 10]: “which influenceS inertial wave propagation.” Ok corrected
[line 11]: “estimated the eddies’ vorticity” Ok corrected
[line 26]: “theses” should be “these” Ok corrected
[line 27]: “coarse FREQUENCY resolution ” Ok corrected
[line 28]: “spectacular increase” is informal and imprecise. Replace with “large”, “order of magnitude” etc. Ok corrected
[line 29]: “more particularly” is awkward. Perhaps “more specifically” Ok corrected
[page 8979]:
[line 2]: “experimentally observed” is awkward. Perhaps simply “observed” Ok corrected
[line 4]: “THE radiation... THE baroclinic adjustment” Ok corrected
[line 5]: “get insight of this near-inertial waves generation” should be downplayed to something like “gain insight into the possibility of near-inertial wave generation...” No evidence of wave generation is actually presented. Ok corrected
[line 7]: “Vertical mixing estimate” is awkward. Perhaps “Estimates of vertical mixing” ?
[line 9]: “integrate” not “integrateS” Ok corrected
[line 11]: “Regarding turbulent mixing it is an advantage because...” is awkward. Perhaps

“This is an advantage for estimating turbulent mixing... Ok corrected
[line 16]: “Whereas” is not required. Ok corrected
[line 18]: “In THE BOUM experiment...” Ok corrected
[line 21]: “pretty weak” is informal and imprecise. Perhaps “relatively weak”? Ok corrected
[line 25]: “from THE Gregg-Henyey (1989) parameterization”. Is this distinct from the G89 parameterization refereed to elsewhere. If so, how? If not, why is it referred to differently here? There is no difference it is now referred to G89
[page 8980]:
[line 1]: You did not propose a slightly adapted G89 parameterization. You used the adapted G89 parameterization. This is a very important distinction. Ok corrected
[line 2]: “validated” to me implies a higher level of test than that performed here. Perhaps downplay to “compared”? Ok corrected
[line 4-5]: It is ok to say that the finding of a weak value of background parameterized dissipation is consistent with the energy sources for internal waves being weak in summer. It does not seem ok to say the energy sources for internal waves are weak in summer, therefore the background parameterized dissipation is weak. You did not present results related to the energy sources for internal waves.
Ok corrected
[line 8]: I presume you mean large near inertial SHEAR? yes corrected
[line 8-9]: “at eddy top and bottom” is informal and imprecise. Perhaps “at the surface and the base of the eddy defined by ...”
[line 10]: “increased” should be “increases”. Keep the tense consistent. Ok corrected
[line 12]: “likely resultS” or “may result”. On what basis can you claim this is likely? Ok corrected
[line 16]: “implicating BOUM experimental results” is awkward. Perhaps “using BOUM observations” Ok corrected
[line 16]: “using numerical modelS..” Ok corrected
[line 17]: “will allow to charactorize thoroughly” is awkward and “charactorize” is misspelled. Perhaps “will allow a through characterization”? Ok corrected
[line 18]: “THE statistical distribution...” Ok corrected
[line 20]: “biogeochemical modelS” Ok corrected
[line 21]: “that THE vertical dinitrogent turbulent fluxes” Ok corrected
[Table 1/2]: Why are some MLE estimates of epsilon NA?
Because the shape of the distribution was too far from log-normal or perhaps to poorly resolved and the mean estimates were inconsistent. In these cases we only provide arithmetic and geometric mean
[Figure 1]: Is bathymetry relevant to the discussion in the text? Is it possible to map the location of the three anti-cyclonic eddies from say altimetry or cruise hydrography measurements?
The location of the three eddies is mapped by the position of the 3 LD stations (A, B, C) at the scale of the figure the exact position of the eddy centers can’t be distinguished with the location of these LD stations, More accurate map of the location of the three eddies can be found in the introduction article by Moutin et al (2012)
[Figure 2]: It should be indicated that these are VERTICAL shear spectra. The y axis should be labelled. Also, indication of the number of spectra averaged should be given. This information should be incorporated into an indication of the 95% confidence interval for the spectral estimate.
24 spectra were averaged for each LD stations, while the spectra for all SD stations is based on an average of 29 spectra, a 95% confidence interval is now indicated.
Also why does the red line not appear in the lower panels:
were composite spectra not fitted to the LD station ensemble averages? Why does the dashed cyan line (noise fitted spectrum) not appear in panels c and d?
As opposed to SD vertical wavenumber shear spectra the LD vertical wavenumber shear spectra were strongly affected by a peak around 0.008 cpm corresponding to the strong near inertial internal waves wavelength found within eddies. Because of this peculiar spectral shape a fit could not be achieved. We therefore assumed that the noise level was comparable to the one found for SD stations over the first 500m (same depth range as LD stations). The noise spectrum (as found from SD stations) was added to the figure and the caption was completed.
I am surprised that the upper ocean (panel A) shows GM shear variance levels while the deeper ocean shows variance levels more than an order of magnitude larger that GM levels. Does the GM model spectra used incorporate depth-dependent Nˆ2 information?
The GM model incorporated a depth-dependant N^2 information
Yes the GM model include a N^2 dependence. Although the observed shear is lower in absolute below the 500m it is indeed much stronger than the GM value which strongly decreases with N^2.
Is there a reason that the lower panels are displayed in C-B-A order?
This corresponds to the chronological order of the measurements it is the same order in Fig.3

[Figure 3]: These should be labelled as time-pressure plots. The structure of N(z) may be more clearly seen if it is on a log scale.

We tried the log plot at first, but the linear plot with this adapted colorscale seemed to be the better representation. Otherwise the sharp thermocline is less clearly identified. We have labeled it as time-pressure.

[Figure 4]: Why is there no black line for eddy B? Please indicate confidence intervals.

The black line represents spectra in regions with strong near inertial internal waves at the bases of eddies A and C, such region was not identified for eddy B because its base was deeper than max depth of the measurements. Confidence intervals have been added.

[Figure 5]: "Gray" is misspelled in the figure caption. "Dissipation" is more accurately the "TKE dissipation rate". How is strain calculated? What (vertical) scales does it include?

On this figure the strain is calculated from (N^2-<N^2>)/<N^2>) base on a 1 m resolution of N from the CTD. This is now specified in the text

[Figure 6]: "Green" is misspelled in the figure caption. Caption should indicate what panels a, b, c and d represent. The units of epsilon in the axis labels and the figure caption are inconsistent. Why are there sometimes one fit and sometimes two fits in each of the panels?

The figure caption has been corrected, there is no fit when the quality of the fit was too poor either because the distribution is poorly resolved or too far from lognormal and resulting estimates of the mean inconsistent with the measurements.

[Figure 7]: Does this provide any new information from this analysis applied to epsilon in Figure 6? Why is the fit only shown in panel b?

The Kz values is important for biogeochemists, the way its averaged is estimated is also an issue

[Figure 8]: Why is a confidence interval provided only in panels 2 and 3. How is the 95% confidence interval defined (for example, how many degrees of freedom are assumed?)

We used the bootstrap resampling method (Efron B., Tibshirani, R.J., *An Introduction to the Bootstrap*, Chapman & Hall/CRC, 15 mai 1994) which is the standard method for estimating a mean and associated confidence intervals from a limited number of samples and a priori unknown distribution. This method as also been used by previous authors to estimate mean dissipation from SCAMP (Sharples and Coates Ocean Dynamics 2003, Macintyre et al L&O 1999) .The 95% confidence interval was more specifically estimated using the percentile bootstrap method. We added the reference to Efron book in the text.

[Figure 9]; Again, does this provide new information compared to this analysis applied to epsilon in Figure 8? Again why does the confidence interval appear only in panels 2 and 3? The values are off-scale in panels 4 and 5.

The Kz values and dependence with depth is important for the biogeochemists implied in the BOOM project. The values are not of scale but at Kz=1e-7 m^2/s (molecular diffusion regime)

[Figure 10]: (now Fig.11) These plots are noisy and the "filled contour" visualization may not be appropriate. Have you tried a flat shading?

Does binning over larger depth bins allow one to see more clearly the large scale vertical structure?

What is the depth bin size and what is the justification for this choice?

"energy dissipation" is more precisely the "TKE dissipation rate". Plots are shown as a function of pressure not depth.

A part of the noisy aspect of the plot comes from the noisy values of N at depth we therefore applied a 10m running average to N. We also use now a flat shading as suggested by the reviewer. Larger binning is not adequate because it blurs the sharp transition in dissipation values at the pycnocline or at eddy C base. Legend caption was corrected.

[Figure 11]: (now Fig.12) I assume this is the parameterized estimate for epsilon? This should be indicated. Is it possible to add confidence intervals to the arithmetic mean profiles?

It has been indicated that epsilon is parameterized

[Figure 12]: (now splitted inFig.13 and Fig14) Shear squared on what vertical scale? This is important. "dissipation rate"
is more precisely TKE dissipation rate. Is SD defined? "Isopycne" I believe should be "isopycnal". The plots should be bigger so that the spatial structure is easier to see.

Again the filled contour visualization leads to the impression of unresolved structure

This is based on a 10m vertical shear (as used for the paramterization), SD is defined at the beginning of the article on the Hydrographic and current measurements section. We have splitted figure12 in fig12-13 in order to increase panels size. We have also opted for a scatter plot, that allows to distinguish smalls scale structure and to represent the exact horizontal spatial sampling.

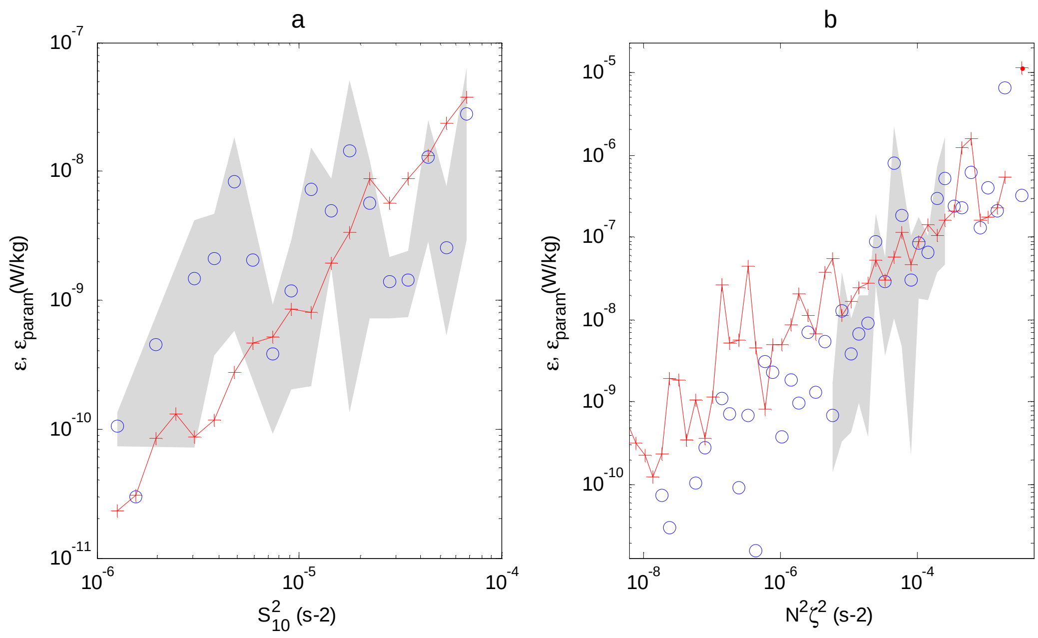

Fig10 Averaged dissipation rate ϵ (blue circles) and parameterized dissipation rate ϵ_{param} (red line) calculated in bins of shear S_{10}^2 and buoyancy scaled strain $N^2\zeta^2$, the gray shading represents the 95$\%$ confidence interval calculated when at least 10 data point were averaged in a bin.

End of the response to the first reviewer