Response to Reviewer 1 comments on Stevens (2017) Turbulent length scales in a fast-flowing, weakly-stratified Strait: Cook Strait, New Zealand. (original reviewer comments in black).

This is an interesting paper reporting measurements of turbulence in a very energetic flow through ocean straits, in this case Cook Strait N.Z. Such measurements are relatively rare and this therefore represents an interesting addition to the literature on direct measurements of ocean turbulence in energetic flow. That said, the paper is poorly presented with some important details about the measurements not included in the paper, and even some typographical errors. These issues need to be addressed before the paper is suitable for publication.

I thank the Reviewer for their very helpful and clearly knowledgeable comments and suggestions. The recent emergence of multiple papers in the literature on aspects of this topic is further evidence that this research theme is important and widely applicable. The following responds to their points. The lack of inclusion of measurement details is responded to below and describes the associated modifications to the revised manuscript. With regard to the point about poor presentation, a number of typos have been cleaned up. As well as this, their comments have motivated a substantial number of improvements.

Detailed comments:

- P1,L24 Waterhouse et al 2014 - *thanks, corrected.*

- P1,l27 Wesson and Gregg (1994) report measurements in Straits of Gibralter, so why is this "… (a) coastal environment". Koch-Larrouy et al (2015) (DSR, 106:136-153) is also relevant here. – *fair enough, I think the point of difference relates to what is a coastal environment and the mechanics of "influence". The initial reference was to bring attention to the effect of strong tidal mixing but I am happy with the reviewer's suggestion as well and additional reference that connects to high trophic levels (Scott et al. 2010) and have modified the text accordingly.*

- P2, L20 . Energy bearing scale. Why is *LT contained* by *LO* , they are independent lenghtscales? *This is of course a key question to ask and is at the heart of the study and many others. The Reviewer asks in what sense are they independent? LT is empirical and L_{Oz} is a scaling argument - but of the same mechanics. What about "constrained by the L_{Oz}"? The relationship between LT and L_{Oz} is key to the manuscript and many papers that seek to quantify dissipation rate from overturn scale. I have changed to "constrained" and return to this dependence in the Discussion – "The calculated L_{Oz}, on the other hand, is not actually physically constrained and in several instances it exceeds the water depth".*

- P5, L18- "The microstructure data were processed in the usual ways resolving the dissipation" is insufficient. Is the author speaking of using the Naysmith empirical spectrum? More detail is needed here. Bluteau et al (2017, JTECH, 34: 2283-2293) provides an extensive review of processing methods for free-fall profilers, and also provides insight into how to process fast-response temperature measurements, and it may well be possible to apply these ideas here. See below. *Reviewer Two highlights this also. This is a debatable point as the field has evolved that there is now a consistent set of hardware and data processing available. For example, the canonical Wesson and Gregg 1994 paper addresses such points of clarity and so the numerous studies in the intervening quarter of a century fill in many of these issues. For example, Bluteau et al. 2017 refers to an earlier paper for shear microstructure methods. On reflection, I should not have used the phrase "in the usual way". The Reviewer is correct in that there are always points of clarity and interest in following through on these aspects. In acceding to the Reviewer's point, I now include the reference to Bluteau et al. (2017) which was published after the initial submission of the present manuscript and include additional information regarding the processing. I do note that the original manuscript included a figure and discussion of variability in drop speed which is rarely discussed in available studies. I have made this contribution clearer in the revision.*

- P5, l23 what is xxx? *Thanks for spotting this as there were some version control issues. This returns to the point above about the relationship between LT and LO. The amended text now says "One might expect overturns, as identified using the LT, to be equal to, or smaller than LOz. Dillon (1982) observed the ratio to be LT/LOz =0.8. This calculation struggles with regions of weak stratification where locally-small N2 drives a very large scale. This makes sense as weak stratification fails to retard turbulence. However, it can lead to non-physical outcomes as the scale will eventually exceed water depth."*

- P6, L4 Ranges of Γ are missing. See Bluteau et al (2017) and references therein. *Thanks. The ranges weren't missing, they were not specified. This is helpful as the Bluteau reference was not available at the time of writing the initial manuscript. Although to be fair this reference doesn't clearly identify the ranges specifically – it's concluding remarks say "The estimated Rif varied over almost two orders of magnitude with a median Rif not significantly different from the canonical value of 0.17. The median Rif obtained from either technique did not differ significantly from this value, although the median Rif obtained from the fitted chi estimates were slightly larger the median Rif obtained from the integrated chi estimates". The text has now been amended in a number of places to highlight the results of Bluteau et al (2017).*

- P7, L5 The fact that the Strait is not well mixed suggests that the vertical diffusion time scale *H*2 *Kz* is long compared to advection times in the Strait? Assuming here that advection is re-establishing the vertical gradient? This is discussed later in paper, but argument is confusing. *The point is important because, in an applied sense, this is a key aspect of the location and experiment. A number of references assume because it is fast flowing and clearly turbulent that it homogenizes the water* *column. This is not supported by the observations. These observations come from the strait narrows and so presumably represent the most energetic conditions. The Reviewer suggests that it is restratification due to advection. This is possible, but given the spatial heterogeneity and relatively fast transit time it is also possible that the water column simply doesn't have time to homogenize as suggested by the scaling in the discussion. This point is now clarified in both the Results and Discussion which has been amended to say "This suggests that, at these most energetic of mixing conditions, we should not expect to see a stratified water column as it should get mixed over the multiple tidal cycles it takes for water to clear the strait. The bulk top-bottom observations (**Error! Reference source not found.**) counter this as, for some of the year at least, there is clearly a scalar gradient. Possibly, the observations need to be restructured and collected drifting with the flow to better follow the evolution of mixing."*

- P7, l20. The usual argument is the dissipation rate is dependent on the intensity of the background shear S. Why is it dependent on N? *The N is used here to delineate layers in the water column (at least on a profile-by-profile basis). The text originally was designed to indicate that the dissipation rate and stratification structure were consistent. I agree with the Reviewer that this terminology could be misleading and have reworked the text to not imply direct causality.*

- P7, l23 One has to wonder how meaningful is the calculation of the Thorpe scale L_T in this situation. It is a strongly advective situation and vertical stratification is (relatively) weak, so how do these effects conspire here? Some estimates of accuracy of L_T scale calculations would be useful, particularly as here we find the scales are large compared to the total depth? *This is an excellent point and one that has been explored in the wider analysis of the problem but not included in the initial manuscript. I believe the Reviewer is getting at the issue that such a large overturn will have time to be affected by the background flow. It is not clear to me that it affects the "accuracy" of the LT but rather it affects what the LT actually means. This is now considere*d in the Discussion which says …"*While the LT never approaches the full water depth, they are large given the flow speeds. Stevens (2014) measured velocity shear at bulk scales (i.e. resolved from 8 m ADCP bins) reaching as high as 0.01 s-1. The velocity variation over an eddy of LT=100 m in a flow with a velocity shear of 0.01 s-1 is 1 m s-1. This is comparable, but not greater than, background speeds suggesting that it might influence the degree of isotropy by straining eddy structure in the horizontal direction.*"

- P8, l2 But how is K_Z computed here? Large values of $K_Z = 10^{-1}\ m^2 s^{-1}$ have been reported by Bluteau et al (2017), but they argue these high values are much more reliably estimated from the temperature spectra than the velocity spectra. As Bluteau et al (2016, JTECH, 33:713-722) argue integration methods are only robust if $\varepsilon \leq 10^{-6}\ m^2 s^{-1}$. Author should consider this point carefully. I assume in all the processing that the author has used $\Gamma = 0.2$? While on average this may be globally

true, the flow in Cook Strait seems very unusual with very high mean velocities and very high values of Re*b* in Figure 11, for example. The point being that consistently here possibly Γ ≠ 0.2 and it may be very misleading to assume that in the present observations – see Bluteau et al Fig 4.? So in Figures 7,8 and 9 is *KZ* to be believed? There seems only one way to check this: independently compute *KZ* from the temperature field, without any a priori assumption on the value of Γ.
The Reviewer rightly picks up on one of the major themes in ocean turbulence – the efficiency of mixing – this is too big for this manuscript and dataset which focuses on the LOz/LT question. The Reviewer also picks up on the unusual nature of the flow with its high mean velocities. The changes I have made in response are to remove panel (b) of previous Fig. 10. (the Kz distribution) and replace the axes in Fig 7, 8 & 9 with 0.2eps/N2 and then expanded the Discussion. Given the bounds suggested by Bluteau et al 2017 there should still be meaning given the dynamic range observed. This enables the later discussion to be augmented as well as emphasised what future work is required. The revised text now all considers the related point made by Smyth et al. 2001, based on DNS of patches, which demonstrates the order of magnitude variability in LO/LT over the lifetime of the turbulent event.

- P9, l7 The range of Re*b* estimates is 2 orders of magnitude? Figure 11 suggests more than 4 orders of magnitude?
  Agreed, the original text was misleading and has now been clarified. It now states… "In the present Cook Strait data, the majority of Reb estimates exceed 100, with the peak of the distribution being around 5x1^04 two orders of magnitude with the peak of the distribution around 5x10^4. (Figure 11). However, maximal values exceed 10^7, which is primarily due to the small N."

- Fig 12 suggests a very poor correlation between Lo and Lt – its log-log after all! *I accept the Reviewer's point that the best-fit distribution is centred on some widely spread data points. As noted by the Reviewer these variables are "independent" in the sense that they are derived from different components of the profile data. However, this level of variability is consistent with the spread of results of Wesson and Gregg (1994) all the more so because we calculate LT using the microstructure sensors allowing for a much smaller minimum lengthscale. Given that these data are at one limit of ocean energetics I believe we have to be careful about rejecting data because they don't conform to expectations. I have added material to the first subsection of the discussion on this point.*

- P11, l13 Maybe it simply means that the gamma is not 0.2, irrespective of the Re? *Is the Reviewer suggesting that the Kz is different enough to make the scaling argument not useful? Given that the scaling is linear in Gamma and the present homogenization time is 25 hours, this suggests that the Kz might be out by a factor of 5 say - i.e. homogenization takes 5 hours. The text is now amended to connect this point with that raised above around Gamma=0.2. It says "The* Γ*=0.2 "constant" is a clear point of contention in the literature (e.g. Bluteau et al. 2013; Mashayek et al.*

2013). Bluteau et al. (2017) develops an approach that takes microstructure profiles and resolves the diffusivity "directly" fitting a model for dissipation of thermal variance to the convective-inertial subrange (i.e. lower wavenumbers than the dissipation scale). The Bluteau et al. (2017) analysis suggests that improved estimation of the thermal diffusivity indicates that the fixed mixing coefficient might underestimate mixing by a factor of 5 in the mean especially for the more turbulent events. Extending this by applying the Osborne diffusivity method sees an average diffusivity is around 0.04 m2 s-1 and exceeding 1 m2 s-1 (Figure 10b). One might expect a 300 m water column to then be homogenised in a time (L2/Kz=) 3002/1=25 hours, but this might be as little as 5 hours if the Bluteau et al. (2017) increased estimate of Kz were to hold. "

- P14 line 10 where is the Hogg reference cited.? *Thanks for spotting this missing reference. This has now been included (Table 1 caption and Discussion).*

References

Scott, B.E., Sharples, J., Ross, O.N., Wang, J., Pierce, G.J. and Camphuysen, C.J., 2010. Sub-surface hotspots in shallow seas: fine-scale limited locations of top predator foraging habitat indicated by tidal mixing and sub-surface chlorophyll. Marine Ecology Progress Series, 408, pp.207-226.