

**Response to Reviewer 2 comments on Stevens (2017) Turbulent length scales in a fast-flowing, weakly-stratified Strait: Cook Strait, New Zealand. (original reviewer comments in black).**

- The manuscript discuss direct measurements of turbulent quantities in Cook Strait. Considering that such data are relatively scarce in oceanography and can be interpreted in a broader context, the reported data are valuable.

*I am pleased the Reviewer sees the value of the data being in a wider context and not site-specific. Their comments have motivated a substantial number of improvements.*

- The central issue of this manuscripts and its main message is about the comparison between the Thorpe and Ozmidov scales. Unfortunately, the discussion itself is rather short and poorly documented (three references). More efforts should be put in the analysis around figures 12, 13 and 14. The manuscript will be greatly improved by a better focus on this scientific issue. In particular, can the data shed new light on the claim (Mater, 2013) that  $LT \sim LO$  with  $N.k \sim \epsilon$ ?

*I have now expanded the Discussion. The limited number of references discussed originally was partly related to the limited directly related studies. In addition, one of the key references in the Discussion is a synthesis (Mater et al 2015) so there is implicit connection to a wider literature. The discussion now includes material on the relative magnitudes of the LT and LO scales. It is difficult to make a direct dynamical comparison as the two studies do no overlap in scale – as is to be expected with the DNS approach. Certainly, the field observations do not exhibit the roll-off at larger LO. To be fair these authors, in the later 2015 paper, do move their perspective to ocean scales and this is where the present manuscript picks up the comparison. One of the additions made in response to the Reviewer's point, also from DNS – this time Smyth et al. 2001, demonstrates the order of magnitude variability in LO/LT over the lifetime of the turbulent event, something that the Mater et al 2013 paper doesn't clearly respond to.*

- The measurements themselves are presented with (too) many figures, but basic information is missing. Little is said, for instance, on the timing of the collected 34 profiles covering a very large period of time of 5 years. Processing of the microstructure data must be described or documented in a much more precise way than with sentences like "in the usual way" or "An approach".

*As noted in Response to Reviewer #1, additional details on sampling and microstructure analysis are now included. The profiles come from only a short window of 12 days within this longer period. Also, the driver is strongly tidal so periods between measurements should be irrelevant especially as the separation is between scene-setting broad scale information in the earlier data collection (where previous work demonstrated consistency from year to year) and the later microstructure work.*

*Details on the profiling timing is contained in Figure 6 . This was erroneously not specifically referenced but was talked about in the previous version. I thank the Reviewer for spotting this and have now augmented the Methods section. The figure has been moved forward to Fig 2 and the associated text now says “The timing of the profiles during the 2012 sampling is shown in Figure 2. Long periods of contiguous sampling is difficult because a vessel suitably manoeuvrable to conduct the experiments is prone to weather limitations. Sampling over three days in 2012 centered on periods spanning northward, turning and southward tidal flows (Figure 2).”*

*In addition, The number of figures has been reduced by one – removing the Kz distribution and combining the eps and Re\_b figures.*

- Considering the fast flows in this region and the irregular topography, three-dimensional effects (e.g. horizontal advection) are anticipated and should be discussed.

*The Reviewer makes a good point. This partly overlaps with a comment by Reviewer 1 about the veracity of the LT in such fast flowing waters. The text has now been amended to include discussion on this at the end of the first subsection in the Discussion. I argue, however, that the topography is not irregular at least in the region where data were collected. It is actually, over the distance travelled in any one tidal cycle, reasonably “regular” in the sense that there are no major changes in channel orientation and no submarine ridges running transverse to the flow (Fig. 1c). Evidence of this (i.e. lack of cross-strait eddies) is contained in the Strait being considered something of a bioregional barrier limiting across-strait connectivity (Forrest et al 2009). This is now included in the Discussion which says “the Strait has been identified as a dividing line in terms of ecological structure (e.g. Forrest et al 2009). The implication is that there is not a great deal of across strait transport. This supports the focus of the present work on the vertical structure. Furthermore, over the time it takes to drift through the strait all vessel tracks tended to be on an axis aligned with the strait. Over these scales of time and space the strait itself is bathymetrically reasonable consistent. It remains to conduct a study that will adequately quantify across-strait mixing and the associated drivers.”*

- Some of the (many) typos and formal problems to be fixed.
  - Page 3, line 4 : "velocity Sh" ! velocity shear ? *fixed*
  - Page 3, line 8 : "Do we actually observe high dissipation rates?" *fixed*
  - Bottom of page 5 and first paragraph of page 6 : please fill the gaps "xxx" and "X" "Y". *fixed*
  - Please carefully check references : some of them are missing or unused (Gregg and Oszoy, 2002; Matter et al, 2003 or 2005; . . . ) *both now included.*
  - Figure 6 seems not to be cited / discussed in the text. *Thanks for spotting this – this has now been moved to Figure 2 and discussed at some length.*
  - Many figures are provided but with very little discussion. The ratio number of figures to Length of discussion seems to be rather low. *The revised Discussion is expanded to provide a more lengthy treatment of the data and the issues. In addition, the manuscript has been reduced by one figure.*

- In conclusion, the data are interesting but the manuscript should be better focused to avoid wild discussions of many details (e.g. on individual profiles taken at different (unknown) location and times) and come to solid conclusions.

*I thank the Reviewer for their suggestions and critique. The modified Discussion now focuses more specifically on the questions posed in the introduction and provides additional insight into the mechanical context and how this relates to the observed data shown in the figures. I do defend though the descriptions of the individual profiles which are there to serve as a context to talk about some of the actualities that can get lost when considering a scatter diagram covering many orders of magnitude and many hundreds of realisations. This helps the reader keep in mind that oceanic turbulence structure cannot be considered to be a series of disconnected experiments (as implied in Mater et al. 2013) but connected parts of a continuum. As noted above, to be fair these authors, in the later 2015 paper, do move their perspective to ocean scales and again this is where the present manuscript picks up the comparison. In regard to the examination of selected individual profiles I took inspiration from the canonical Wesson & Gregg paper that included a small number of individual profiles to ground the later synthesis within the context of the source data. I note also that Mater et al 2015 look at selected patches. I certainly wouldn't argue that the entire manuscript be filled with such examination, but I believe it helps put the synthesis into better context. In addition, thanks in part to the points raised by the Reviewer, I believe the manuscript now comes closer to contributing to the "solid conclusions" they seek for questions at the forefront of ocean turbulence for some time now.*

## References

- Forrest, B.M., Gardner, J. and Taylor, M.D., 2009. Internal borders for managing invasive marine species. *Journal of Applied Ecology*, 46(1), pp.46-54.
- Smyth, W.D., Moum, J.N. and Caldwell, D.R. The efficiency of mixing in turbulent patches: Inferences from direct simulations and microstructure observations. *Journal of Physical Oceanography*, 31(8), pp.1969-1992, 2001.