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Ocean Science
Discussions

Interactive comment on "Observations of turbulence beneath sea ice in southern McMurdo Sound, Antarctica" *by* C. L. Stevens et al.

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We thank the Reviewer for the time and effort taken to consider our manuscript. In the following, we present a response to both major and minor comments, which refers to the revised version to be posted on the interactive discussion website.

Comment: p 1408, around line 10: "The small-scale turbulent energetics lie on the boundary between isotropy and buoyancy-affected. This will likely influence the formation and aggregation of frazil ice crystals within the supercooled layer" I thought the important factor here was ratio of crystal size to Kolmogorov scale, not turbulent Froude number.

Response: Both scaling arguments are important – but to different aspects of the prob-

lem. If the viscous limit is comparable or larger that the crystal size then shear will be evenly distributed across the crystal and will likely influence growth in a different manner than if LK were smaller than crystal size so that turbulent fluctuations could penetrate to the crystal scale. The Froude number perspective is invoked because at the larger scale turbulence influences crystal growth, aggregation and transport. A sentence has been included in the abstract and the point is emphasized in the Discussion. The actual difference in effect are beyond the scope of the present work but something we are keen to develop – as are other groups.

Comment: p 1498, lines 17-18: useful to expand on what the unique properties are.

Response: We added a sentence and three references (Jacobs et al.; Holland et al. & Payne et al.) to give the reader more details relating to the production of very cold water.

Comment: p 1408, line 26: refer to Figure 1 here?

Response: Again, this was correct in the original manuscript, but the change was missed in the proof reading.

Comment: p. 1411, line 11: while as far is I know this is the first use of hydrodynamic diagram in polar domain, it has certainly been used more than a "few" times elsewhere. Response: The text has been amended to reflect this.

Comment: p. 1414, line 6, formula formatting problem (pg 1413?) Response: This was a typesetting error that was identified in the proofing.

Comment: p 1415, line 28, mean "ensemble-averaged profile" here? Response: Yes, the Reviewer is correct here - changed.

Comment: p 1416, line 12, sentence starting here is not clear to me

Response: This has been re-written and now says "There were several isolated events generating high dissipation rates in a single isolated bin for a single profile. These were

typically at mid-depth (Fig. 7b) and located in the centre of the density structure where the large scale velocity shear is typically concentrated and where there is likely the greatest variability".

Comment: p1416, I am interested in the perceived effectiveness of the ensemble-averaging procedure. . . .. if the authors could make a more definitive statement on the approach.

Response: This is an important point to examine. We believe the Reviewer is asking should we look at time-averages of instantaneous flux calculations or fluxes calculated using time-averaged properties. The main issue here is probably "what is the relevant timescale for variability in turbulence production?" Shaw et al. (2009) analysed the extensive SHEBA experiment dataset (>12,000 profiles!) and used 3 hr ensemble-averaging to determine turbulent transport parameters from temperature microstructure. We think it's important to use some form of averaging – not so much in the turbulence energetics term (here epsilon or in the case of Shaw et al., chi_T) but definitely for the gradient term, N2 or (dT/dz)2. In the SHEBA experiment the variability is largely associated with the under-ice tide+circulation boundary layer at short timescales or variability at larger space and time scales due to seasonal and coastal influences. Here, however we have substantial tidally-driven variability that results in rapid transient effects and some substantial internal wave-generated variation in vertical structure. Thus we have chosen an experiment-average ensemble but conducted the averaging in isopycnal coordinates. We have explained this in the revised text and included the Shaw et al. 2009 reference.

Comment: p 1418, line 26, this sentence is repetitive.

Response: The sentence has been re-structured. "Furthermore, with regard to direct measurements, by not profiling the bottom 10-20% of the water column, it is possible that we underestimate the total dissipation (St. Laurent, 2008)."

Comment: p1419, line 1, this sentence is pretty vague -

Response: this has been reworded to be more specific. "On the other hand, with only moderate stratification and substantial vertical velocities implied by the rapid changes in depth of the pycnocline, this high level of diffusivity is not surprising."

Comment: Appears that Figure 8 is referenced out of order.

Response: Corrected.

Interactive comment on Ocean Sci. Discuss., 6, 1407, 2009.