**Ocean Science
Discussions**

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

C. L. Stevens et al.

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We wish to thank the Reviewer for the time and effort taken to consider our manuscript. Their thoughtful analysis of our work has correctly identified what we believe to be main thrust of our work. 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. In general we have clarified several issues relating to scales and included the Shih et al. parameterization of K_rho. This last point was implicit in our first draft but the Reviewer's suggestion encouraged us to extend it more fully. The Reviewer's comments also resulted in the inclusion of two new figures.

Comment: (1) Discussion of scales. There is little to criticize here from the perspective of applying principles garnered from the fairly extensive literature on small scale

turbulence studies in the stratified open ocean (although more on eddy diffusivity below). However, the static stability of this regime is comparatively low, and I would point out that there is also a fairly extensive background on turbulence scales from underice ocean boundary layer (IOBL) studies that are sometimes at odds with the conventional approach. For example, in a significantly turbulent regime during flood and ebb tides, it is probably necessary to consider the scale imposed by rotation. There is ample evidence from IOBL measurements (McPhee 2008, ch 5) that for small (negligible) stratification this goes as $\sim 0.03 u^*/f$, and that for low stress this is particularly important (M2008, fig 5.7).

Response: The main point here with respect to improving the manuscript is that the manuscript ignored the planetary rotational scale limit. This is because we believe that our fast ice system is likely dominated by the influence of nearby topography (Fig. 1). We agree that we did not pay enough attention to the IOBL literature here and now include the rotational scale in Section 2 for completeness and return to the point in the Discussion. We are presently working on validating this assumption with a different dataset that considers this effect of local topography.

Comment: With measurements in the upper ocean (except in the region where the turbulence scale is Kz – the uppermost measurements here are beyond this zone– see p. 16), this provides a direct estimate of stress by assuming production equals dissipation. I would posit that this scale is an upper limit for Le in much of the domain considered.

Response: Our observations captured overturn scales in excess of this scale (2 m) – e.g. new Fig. 9. This is potentially an effect of the coast line dominating over Coriolis' steering and/or the result of the large amplitude "internal wave" activity.

Comment: By the same token, the Ozmidov scale is basically dimensional analysis assuming the limiting scale when buoyancy is in play is controlled by the density gradient and dissipation. For weak stratification where the planetary and buoyancy scales

(Obukhov length) compete, an alternative is given by McPhee (2008, eqns 4.25&4.26). Again, this item is meant more as an opportunity for discussion instead of a direct critique.

Response: Agreed – however we felt our paper is not sufficiently broad and does not encompass enough of the heat flux aspect of the problem to enable us to explore this aspect in detail.

Comment: (2) I am puzzled by the authors' choice for estimating K. It seems to me that by far the closest analog to this study is the Fer and Widell work, which shows comparable levels and weak stratification (although slightly stronger than here). Obviously when N2 is small, the Osborn eddy diffusivity will be large. Although mentioned in the discussion of supercooled water outflow where it is posited that the Osborn estimate is an upper bound, it seems to me that this warrants more discussion. According to FW, the Shih et al. (2005, JFM) approach is applicable when eps/(nu.N^2) >100, and they show that this much better describes their results. For the mean eps from Table 1 and mean N2=2×10−6, I get something like 3000 for that parameter and something like 3 x 10-4 m2 s-1 for K. This means that the heat flux estimated from the potential temperature gradient would be much smaller than the 7 W m-2 mentioned. So whereas I have no reason to question the dissipation measurements, it seems to me that the diffusivity estimates might well be way off.

Response: We agree and note we did reference Fer and Widell and consider the Gamma issue through the factor of 40 decrease as part of the uncertainty in the temporal persistence of the plume. The Reviewer's estimate falls within this bound. We chose the established approach for the initial calculation as being conservative because in the present context the higher heat flux then means the plume of supercooled water persists for a shorter distance. However, following the Reviewer's advice we now (i) use the Shih et al. approach at the outset and (ii) compare the Obsorn and Shih results (new figure – Fig. 8). In a sense, the focus on the lower Shih et al. estimate reduces the wide range of quoted values and helps address the Reviewer's point about

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being "obtuse" below.

Comment: (3) Paragraph starting p.11, line 22. McPhee and Stanton (JGR, 1996) made direct comparisons of stationary and profiling microstructure measurements at the edges of freezing leads, and at depths within range of the measurements described here. They were able to estimate heat flux from thermal variance measurements and got reasonably good agreement for eddy diffusivities from quite different perspectives. Response: Agreed, we were a bit strong on the suggestion of a lack of work – we certainly didn't mean to imply there have been no studies rather that given the importance of the issue there has been little work. We've turned the text around to focus on the few studies that have looked at this important comparison.

Comment: (4) Figure 5 has many fascinating features. As I interpret the discussion, there are tidal bores that traverse the measurement site, and much of the activity in the central part of the water column seems to occur as the water column relaxes back after the bore passes. The clearest example is the 2nd segment, from sta 12 to 21. It is notable that the maximum dissipation seen for the whole series occurs just above what seems to be a descending pycnocline, relatively deep in the water column. I would like to see a little more discussion and perhaps expansion of this part of the paper. Just an offset time sequence of the density profiles from 12 to 21 would be quite instructive. What is the source of the enhanced epsilon? Could it be from breaking internal waves riding the bore?

Response: We now include the suggested figure augmented by colour coding the density profile with dissipation rate (new Fig. 6 - attached here). As the Reviewer suggests it clearly locates the high dissipation events and also illustrates the complex layering that is presumably responding to a combination of mixing and waves. In our description we include the suggestion with respect to the turbulence being related to breaking internal waves. We think it's not quite as "straightforward" as internal bore-driven instability but leave this wider topic for a different study.

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Comment: (5) I really did not understand the arguments in the paragraph starting at line 21, p. 14. Maybe I am just missing something obvious, but if upward heat flux is fixed (7 W m-2), why is it that delta theta drops out?

Response: The confusion arose because, while we have an instantaneous observation of the diffusivity driving the heat flux, the model argument assumed the flux was driven by a Fickian process so that the flux and the heat content both scaled with delta theta. Thus it dropped from the argument. We now clarify this in the text.

Comment: The authors allude to uncertainty in K_rho here, but the conjecture about how far out into the Sound supercooling and frazil production will reach seems pretty obtuse. Response: Certainly we agree that the estimate is not tightly constrained but (i) it is a very important quantity from the perspective of the regional ice-ocean system and (ii) as it's not been looked at from this perspective before. By focusing at this stage largely on the Shih et al K_rho estimate, as per the Reviewer's suggestion, we have removed the uncertainty arguments from the text so the text at least is less obtuse.

Details:

Comment: page 7, line 6 equation error. Response: This was a typesetting error that was identified in the proofing – We will look for it next time.

Comment: page 8, line 25 I see this for 8 but not for 21. Response: this is less apparent for profile 21 because of the colour table. The text has been amended to clarify that profile 21 is less obvious in terms of rise in isopycnals, however reference to it is maintained as it sustains very high epsilon. Also with the new figure 6 this change is clarified.

Comment: page 15, line 2 Units for time? Response: "days" – corrected.

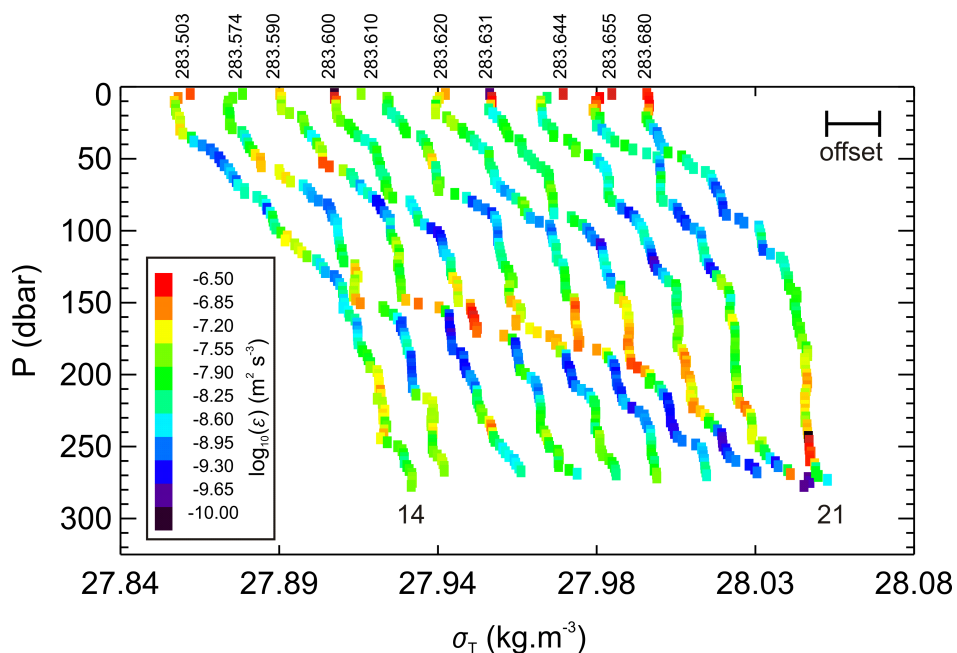Interactive comment on Ocean Sci. Discuss., 6, 1407, 2009.

Fig. 1. Waterfall plot showing offset density profiles from Friday the 10th of October, 2008 where each profile is colour coded with dissipation rate.