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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.

M. McPhee (Referee)

mmcphee@hughes.net

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Please see attached pdf for equations and ungarbled characters

Review of Stevens et al., Turbulence in southern McMurdo Sound.

Overall assessment: The paper reports interesting results from measurements in a demanding environment that has been little sampled, one that includes supercooled water. It deserves publication, but I would recommend that the authors address some of the following issues.

(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 stratiïňĄed 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 food 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$0.03u*/f , and that for low stress this is particularly important (M2008, ïňĄg 5.7). With measurements in the upper ocean (except in the region where the turbulence scale is κz – 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. 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 stratiïňĄcation 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.

(2) I am puzzled by the authorsÊij 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 ε levels and weak stratiïňĄcation (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 outïňĆow 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 ε/νN2 >100, and they show that this much better describes their results. For the mean ε from Table 1 and mean N2 $\approx$2$\times$10 ˆ$-$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 ïňĆux 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.

(3) Paragraph starting p.11, line 22. McPhee and Stanton (JGR, 1996) made direct comparisons of stationary and proïňĄling microstructure measurements at the edges of freezing leads, and at depths within range of the measurements described here. They were able to estimate heat ïňĆux from χ (thermal variance) measurements and got reasonably good agreement for eddy diffusivities from quite different perspectives.

(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 proïňĄles from 12 to 21 would be quite instructive What is the source of the enhanced ε? Could it be from breaking internal waves riding the bore?

(5) I thought the comparison between Kolmogorov and frazil crystals scales to be very appropriate and interesting. However, 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 ïňĆux is ïňĄxed (7 W m-2), why is it that $\Delta\theta$ drops out? The authors allude to uncertainty in K here, but the conjecture about how far out into the sound supercooling and frazil production will reach seems pretty obtuse.

(6) Details: page 7, line 6 equation printed wrong page 8, line 25 I see this for 8 but not for 21 page 15, line 2 Units for time?

Summary The paper is publishable with fairly minor corrections, but might gain appreciably by some attention to the issues raised above.

Please also note the Supplement to this comment.

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