Ocean Sci. Discuss., 12, C1604–C1613, 2016
www.ocean-sci-discuss.net/12/C1604/2016/

Ocean Science
Discussions
Open Access

Interactive comment on "Turbulent heat transfer as a control of platelet ice growth in supercool under-ice ocean boundary-layers" *by* M. G. McPhee et al.

M. G. McPhee et al.

c.stevens@niwa.cri.nz

Received and published: 10 February 2016

Reviewer Summary: This manuscript reports observations of turbulent ocean heat fluxes in supercooled waters under sea ice, in a setting that may promote platelet ice growth. Time series of ocean current, temperature, and salinity are described alongside turbulent flux measurements in the boundary layer over the course of several tidal periods. The observed turbulent fluxes are shown to be well characterised using standard bulk formulae, based on the observed supercooling and the inferred friction velocity at the ice base. The friction velocities are used to argue that the platelet ice has a greater roughness length than alternative settings for heat transfer under sea ice.

The manuscript is clearly written, subject to a few technical clarifications. In my opinion the article provides useful observational data and constraints on bulk heat transfer correlations for settings with platelet ice growth, that are worthy of publication. One concern is that whilst the supposition in the title and last sentence of the abstract that the turbulent heat transfer controls platelet ice growth seems plausible, I would argue it is not yet firmly supported by the analysis in the present version of the manuscript. The results demonstrate turbulent heat transfer consistent with interaction with a freezing boundary, but have not yet shown that this flux is as significant, or more significant than other potential sources of heat transfer as detailed below. This conclusion needs to be either better supported by some further analysis/information, or else the discussion modified accordingly. Some suggestions for how to better evaluate this hypothesis follow below, along with a few other requests for technical clarification.

Author Response: We thank the Reviewer for their useful comments and we are pleased that that they found "useful observational data and constraints on bulk heat transfer correlations for settings with platelet ice growth, that are worthy of publication". We are presented with something of a conundrum in that Reviewer #2 recommends that we actually strengthen the language around our results and conclusions. The Reviewer raises several issues which we address in the following material. We have now modified the Discussion as requested and separated out our conclusions into a separate section and strengthened our justification for the conclusions with several new references that target points made by the Reviewer.

Specific comments:

Reviewer Comment 1. The title, last sentence of the abstract, and comment on page 2818, line 16-17 suggest that this manuscript has demonstrated that the ocean heat flux is providing a strong control on sea ice growth in this location. However, the present version of the manuscript arguably only demonstrates that the ocean turbulent flux is consistent with transfer between a boundary at the insitu freezing point, and a supercooled bulk fluid. It is less clear how significant this flux is as an overall driver of sea ice growth. Is there any evidence to demonstrate that this is indeed a strong control on the sea ice growth at this location, in comparison to other potential heat fluxes due to some combination of conduction up through the ice interior, lateral advection in the surface ocean, and relief of supercooling in the surface ocean over time by ice growth? If there were independent estimates of ice growth rate, these might be usefully compared to the ice growth expected if all of the downward ocean heat flux were used to remove latent heat of solidification. It may also be possible to produce scaling estimates for the heat flux conducted up through the sea ice if ice thickness and the upper and lower ice surface temperatures could be estimated.

Author Response: It would appear Reviewers 1 and 2 are opposed on this point. We view it as a likely hypothesis that needs more investigation. We lacked measurements for viable estimates of conductive heat flux in the ice column. Still, in agreement with Rev 2, if the product of ustar x deltaT limits (1-d) heat transfer away from the horizontal ice base, it provides an important limit on platelet growth. It seems that crystals grew more readily on objects suspended in the supercooled water because the heat can be diffused and advected away continuously in all directions. Our experience with time series of ice temperature profiles suggests that thermal memory in the ice precludes using upper and lower temperatures to estimate conductive flux on time scales as short as here. We have therefore modified the discussion significantly by rewriting the paragraph indicated above (page 2818, lines 11-18) and also paragraph that was on page 2817, lines 7-16. In page 2818 paragraph, we now reference Purdie et al. (2006) and Gough et al. (2012) who performed the calculations for sea ice growth as suggested by the reviewer here and obtained estimates of the amount of ice growth through negative oceanic heat flux. We did not have a thermistor probe installed at the site of our measurements, so cannot repeat the method of those authors, but since this is a similar location and with similar ice, this is sufficient in our view.

Author: we have included the working revised manuscript as a supplemental pdf.

Reviewer Comment 2. The authors make several references to ice nucleation on the moorings and masts, and in particular that they have carefully discarded any of the ADV measurements that may have been contaminated by freezing. Based off your observations, is it possible to rule out any freezing onto instruments also impacting the temperature and salinity measurements, or whether such artificially induced freezing might have played a significant role in the heat budget for the region of the water column that is being measured?

Author Response: Yes this can have an effect on the measurements and this issue is addressed in McPhee et al., JGR 2013. The text is modified to clarify this and now states “This can affect both ADVs and conductivity sensors. We used the criteria identified in McPhee et al. (2013) to remove affected data”.

Reviewer Comment 3. Estimate of z0 between equations (7) to (8). Some of the details of this calculation were not clear to me - can you provide further details? In particular, at what value of z is U(z) evaluated when estimating z0? Also, taking log(z0) in equation (8) needs a more consistent treatment of the physical units - has there been some non-dimensionalisation here? Minor clarifications/suggestions on presentation:

Author Response: We have added a qualifier "for U measured at 1 m" which addresses this (i.e. log 1 = 0).

Reviewer Comment 4. I didn’t find definitions of the directions of u0, v0 and w0 before first use in equation (1), or an explicit definition of the turbulent dissipation rate above equation (2).

Author Response: The text has been amended so that it now states . . .” currents averaged over each realization were rotated into a reference frame such that mean vertical and cross-stream horizontal components vanished, from which the velocity perturbation components were resolved (, and). Linear trends were then removed, then “area-preserving” (weighted) spectra were calculated. . .”. Reference to ïĄě is now made in the opening paragraph of section 2.2.

Reviewer Comment 5. It might be worth providing a background reference(s) for the justification of equations (2), (3) and (7), for readers less familiar with the relevant parts of turbulence theory.

Author Response: We now reference the landmark text Tennekes and Lumley 1972.

Reviewer Comment 6. The scaling estimate in equation (3) assumes that buoyancy-driven convective turbulence is not significant in modifying the boundary layer structure. It might be useful to mention this here, but then note later (e.g. near to p.2815, lines 10-15) that the very good comparison between the two estimates of turbulent eddy length-scales in figure 5b provides support for your hypothesis of a shear-dominated boundary layer.

Author Response: The assumption that buoyancy is not influencing production of turbulence is implicit in the existing text which said “then TKE production rate by current shear is...”. The text has now been amended to say – “it is possible that buoyancy effects are also contributing to the turbulence and this can be examined by comparing production and dissipation rates.” Further below, where the two terms are compared the text now states - “This supports the hypothesis that buoyancy-induced turbulence is minimal in the present conditions.”

Reviewer Comment 7. Is there a typo in equation (4)? If I equate the production in equation (3) to the dissipation rate so that _ _ u3_=(_ jzj) and substitute for _ jzj _ _ = c_=kmax, I end up with u_ _ (_c_=kmax)1=3.

Author Response: We thank the Reviewer for spotting this – we’re not sure what went wrong in the drafting but the equation got restructured somehow. It has now been corrected.

Reviewer Comment 8. p2816, line 8/9 “negatively increasing”. Would “decreasing” be easier to read?

Author Response: Possibly, but the wording was chosen to emphasize that the thermal

OSD
12, C1604–C1613, 2016

Interactive Comment

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

forcing increases in a negative sense. As a compromise the text now says "(Fig. 6a and c). The departure from the freezing point temperature also exhibits the trend of becoming larger (i.e., increasingly negative) with time during the observation period."

Reviewer Comment 9. p2816, line 11. Can you give a standard error (or other error bar) on the estimated value of cH to allow a better estimate of it's similarity or difference to the other values? Also, I think there is a typo here as c changes from lower to upper case between lines.

Author Response: This could potentially be achieved by adding and subtracting the std dev error bars associated with the measured quantities, but this might be misleading for a record this short. The data from 2 tidal cycles are suggestive, not definitive.

Reviewer Comment 10. p2817, lines 8-16; discussion of congelation vs platelet ice growth. Could this be reworded to more clearly emphasise that the key difference between congelation and platelet ice growth is that a supercooled ocean allows a significant part of the released latent heat to also be removed into the cooler ocean in the case of platelet ice growth, whereas congelation growth cannot conduct heat into the ocean when the ocean is warmer than the freezing temperature at the ice-ocean boundary.

Author Response: This paragraph is now changed to read: "The ocean turbulent heat flux was negative (downward) throughout the entire measurement period (Fig. 6a). Sea ice in this region is typically forms as congelation ice early in the growth season, then incorporated platelet ice towards the end of the growth season (e.g., Smith et al., 2001). Congelation ice grows when the latent heat released during phase change is conducted from the relatively warm ocean to the relatively cold atmosphere. In this context, relatively cold means below the freezing point temperature of seawater. Platelet ice formation occurs in supercooled seawater and when this occurs near the ice/ocean boundary, the latent heat released can either be conducted upwards through the main ice column or transported downwards by turbulent heat flux into the ocean boundary layer. The latter process of negative oceanic heat flux does not occur for congelation ice because the ocean in that case is warmer than the freezing point temperature at the ice-ocean boundary."

Reviewer Comment 11. p2817, lines 11 and 12 "congelation growth in water at freezing temperature requires a small upward ocean heat flux to compensate for salt release" Can you provide a reference, or more detailed justification to support this statement? It isn't immediately clear to me that such a heat flux is always required (especially if salt were segregated into the pore space within the sea-ice interior during congelation growth, rather than being rejected at the sea ice interface with the ocean, and there is some delay in the subsequent drainage of brine out of the ice back into the ocean).

Author Response: The reviewer identifies an important aspect of the data. The appropriate reference is McPhee, Morison, Nilsen 2008. In order to keep the mixed layer at freezing as salt is added requires heat extraction. The argument does neglect the "mushy layer" concept but it is worth noting such layers are not always present. An example of this with a small upward heat flux consistent with downward salt flux is seen in Fig 6.14, McPhee (2008).

Reviewer Comment 12. p2818, line 25-26. "u_ will be modulated primarily by tides". Is this universally true, rather than flows induced by ocean currents or wind-driven ice motion? Worth adding a qualifier?

Author Response: Agreed, the text now says "u* will be modulated primarily by tides as direct wind forcing is effectively absent in the present fast ice situation."

Reviewer Comment 13. Figure 2. The labels are small and hard to read in panel (c).

Author Response: The figure has been modified in response to this and to Reviewer Two's comments.

Please also note the supplement to this comment:

OSD

12, C1604–C1613, 2016

http://www.ocean-sci-discuss.net/12/C1604/2016/osd-12-C1604-2016-supplement.pdf

---

Interactive comment on Ocean Sci. Discuss., 12, 2807, 2015.

OSD
12, C1604–C1613, 2016

Interactive Comment

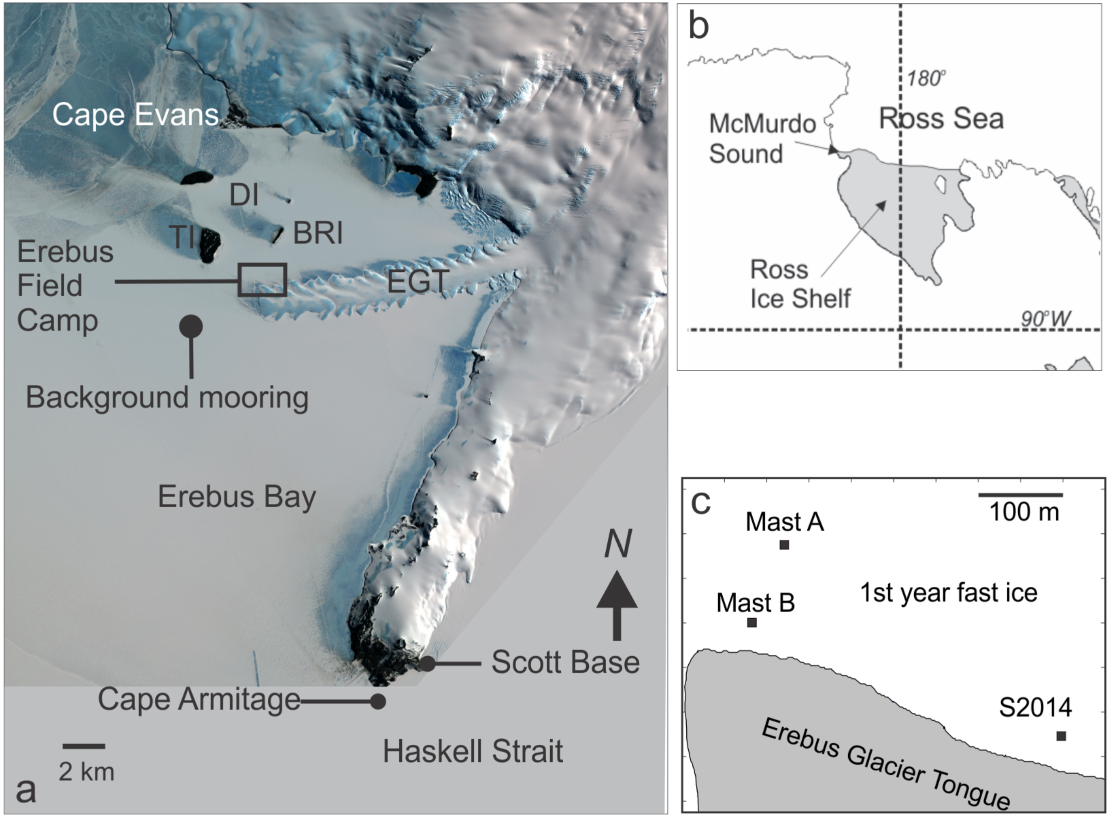

Fig. 1. revised Fig 2

OSD

12, C1604–C1613, 2016

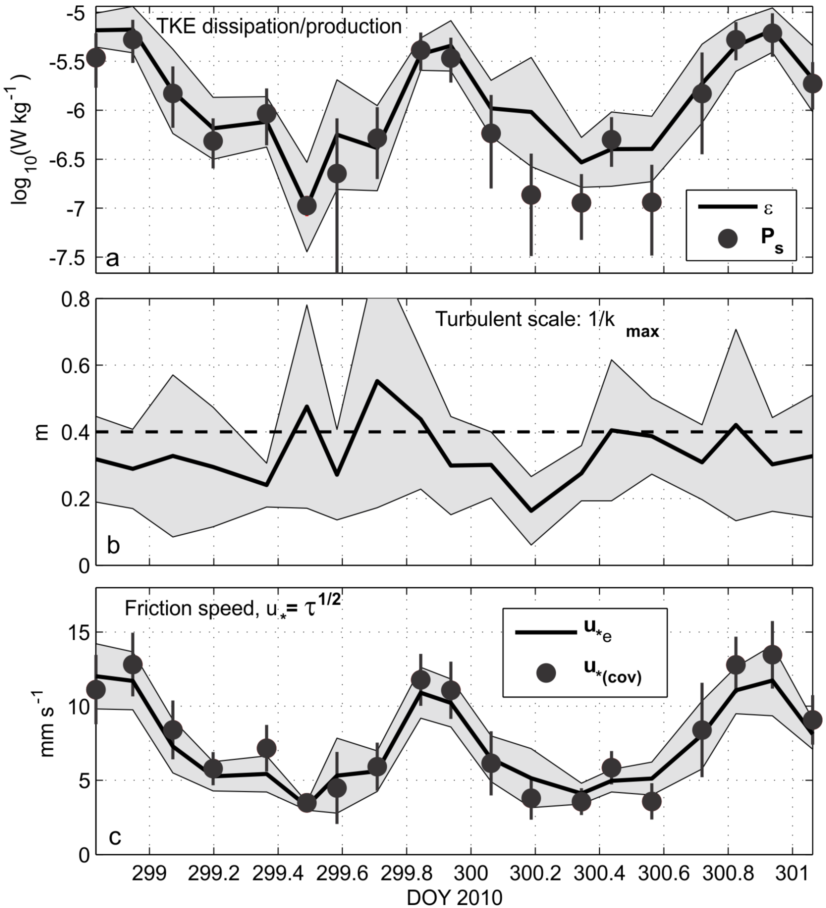

Fig. 2. revised Fig 5