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Ocean Science
Discussions

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.

Anonymous Referee #2

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Review of "Turbulent heat transfer as a control of platelet ice growth in supercool under-ice ocean boundary-layers" by

M. G. McPhee et al.

This paper presents a nice data set of turbulence under fast ice in Antarctic waters. In contrast to "the normal" situation in polar waters the heat flux is downward into the water below the ice. The unique data set and the clear way it is presented makes this a valuable contribution. The changes suggested before publications are minor in my view.

I have one substantial scientific comment, regarding the conclusions, consistent with

the other reviewer. Contradictory to the other reviewer I think you could make stonger conclusions based on your results though. At least we agree that a conclusion section should be added. The conclusions given in the discussion are very modestly formulated in my view. Perhaps my view is guided by my experience and that I thus find the proposed hypothesis likely.

Given the conclusive data set and that the first author is one of the most experienced researchers in this field, I would suggest that more firm conclusions should be stated. Given the very similar C_H values found here and in other studies, I think it is appropriate to fully conclude that the process has been explained, and that you can go beyond "hypothesizing" and "postulating".

Minor comments:

Abstract: Line 9-10: You hypothesize that platelet growth is rate limited by turbulent heat transfer. It seems to me that you should you be able to answer this question fully.

Introduction: On the Weddell side large ice crystals were detected quite deep down in the water column (Dieckmann et al 1986), but at a location where super cooled Ice Shelf Water was present. This appears to be the same type of crystals as the platelets. My point is that large crystals have been found elsewhere outside the Ross sector, and that that given presence of super cooled water such crystals have been found a few decades back. The process studied here is thus more general than what the introduction appears to describe, and this should be included somehow.

Methods:

Page 6, line 19. "nonsensical" is new to me, I guess you mean "erroneous" or "invalid" or "wrong"?

Results:

Page 7, line 19. What data do you mean here? I think it would be better to say "The presented data comes from spring tide. . .." But you also present data over several days

(Figure 4,5,6 three days). So I think you need to re-write this part a little.

Page 8, line 4: How do you know the water column was isothermal down to 40 m? Did you do CTD casts – if so you should state this. It would be OK to do this without showing the figure if this will be used in a different paper. Also the statement for a super-cooled water column down to 15 m depth needs to be supported by either data or a citation.

Page 8, line 12. Please define DOY when it is used the first time.

Page 8, line 16 – 18. Ok here comes the part explaining why you focus on the spring tide, so this should somehow be blended with the initial text on page 7, line 21.

Page 8, line 23. YD should be DOY? Also the section break here seems wrong because the section above and this one cover the same.

Page 8, line 25. I have not seen “slack water” before. Perhaps my tidal vocabulary is limited, but it also sounds very American. Is there a better and more precise term to use in a European English journal?

Page 9, line 7. Delete “with”.

Discussion:

Page 11, line 11-12. “congelation .... release” This sentence is not meaningful to me. Ice growth leads to salt release, but in what way is an ocean heat flux required? Probably some text is missing here?

Page 12, line 14 – 16. Again you draw conclusions extremely carefully. What else than delta T and friction velocity could contribute to the heat flux? I understand the difference in time scale between variation in u_* and the heat flux, but still think that you can pose proper conclusions based on your observations. If you can’t state that this has been explained “well enough” then who can?

There is supercooled Ice Shelf Water (ISW) water below the crystals, and these large

crystals could not appear from the smaller ISW plume, because such large crystals would be bouyant enough to leave the ISW plume (Jenkins 1995, Smedsrud 2004). So they need further heat loss in situ to grow that large, and the heat flux through the thick fast ice must be small as stated in the cited work. Some more reasoning around this issue could perhaps convince the other reviewer, that might be less familiar with the physical setting here, but probably has a better grasp of the turbulent heat transfer.

Figures:

Figure 1: I think it is much better to NOT use abbreviations in a figure, because people might look at it independently. There is plenty of room in the figure. Abbreviations could be given in the figure caption if needed.

Figure 2: This figure is definitely too small. It is not possible to see the names and features in the image. The middle image should be larger, and the two insets could be placed inside this one. With a larger figure the names can be spelled out properly as well. Also the square box in the upper figure seems to have no purpose.

New suggested references:

Dieckmann, G., G. Rohardt, H. Hellmer, and J. Kipfstuhl (1986), The occurrence of ice platelets at 250 m depth near the Filchner Ice Shelf and its significance for sea ice biology, Deep Sea Res., Part A, 33, 141–148.

Jenkins, A., and A. Bombosch (1995), Modeling the effects of frazil ice crystals on the dynamics and thermodynamics of the ice shelf water plumes, J. Geophys. Res., 100, 6967–6981.

Smedsrud, L. H., and A. Jenkins (2004), Frazil ice formation in an ice shelf water plume, J. Geophys. Res., 109, C03025, doi:10.1029/2003JC001851.

That’s it!

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