

Interactive comment on “Dynamics of Large Pelagic Ice Crystals in an Antarctic Ice Shelf Water Plume Flowing Beneath Land-Fast Sea Ice” by Craig Stevens et al.

Anonymous Referee #1

Received and published: 11 November 2020

Summary:

This manuscript summarizes measurements made under fast ice during a field campaign in McMurdo Sound. The authors find relatively large and thin ice crystals with an average diameter of about 100 mm directly below the main sea-ice layer. They provide visual and echo sounder evidence for particles further down in the water column and use the echo sounder data to derive vertical velocities of these particles. They argue that parts of these particles in the water column are the same ice crystals as observed directly under the sea ice. In addition, they report measurements of horizontal velocity and turbulence in the water column below the ice and attempt to relate the turbulence

C1

measurements with the ice crystal size and vertical motion.

General comments:

This manuscript attempts to address 4 key questions around the growth, movement, and aggregation of large ice crystals or platelets in the water column as stated at the end of the introduction. The first two key questions related to the ice crystal size and their dependence on turbulence could in principal be addressed to some extent by the data and methods presented. However, the analysis lacks depth and conclusions seem largely unsupported. Much of the analysis and description are qualitative rather than quantitative and choices of depth levels, profiles, example images, ranges, etc. seem somewhat arbitrary. The results are structured in the order of the instruments that were deployed rather than addressing the scientific question. In many instances I am missing the context of a specific measurement and its overall use to support and address the key questions. The first key question is in my view not really novel, as large ice platelets have been observed in this region before. The second key question of how the crystals and their deposition depend on the vertical motion and turbulence would be novel, but the analysis appears not to be conclusive and provide new insights. The third and fourth key question on the source of crystal growth and its influence on the large scale are not even addressed by the analysis and only part of a somewhat inconclusive literature discussion. Overall, while being an interesting research topic, the analysis presented in this manuscript lacks scientific insight, support for its arguments, and a clear formulation of its conclusions.

The manuscript suffers from poor scientific writing with numerous incomplete thoughts and speculations. Rather than guiding the reader through the story line, results, and argumentation, the text lacks clarity and context. In many instances the context is implied rather than explicitly formulated, which makes it overall very difficult to read and understand the manuscript. It reads more like a report from the field campaign rather than a scientific paper.

C2

I provide a number of specific comments illustrating the above mentioned issues below. But this list of comments is far from complete given the overall concerns that I have with the manuscript. In my view, the manuscript would need to be re-written much more carefully with the development of a scientific story line, a narrower focus on the actual subject and questions that can be addressed with the data available, a much deeper analysis, and a clear formulation of the conclusions that can be drawn from the analysis. At this point I recommend a rejection of the manuscript.

One possible recommendation to the authors would be to make better use of the echo sounder data to only address their question 2, which seems to be the only question that can be addressed with the presented data. They could provide a more in-depth assessment of uncertainties and more information of the apparent classification that has been performed, the particle size estimation from these data, and the attribution to ice crystals vs. other particles in the water column. There is a lot of unnecessary discussion that could be substantially reduced to strengthen the focus on this aspect of the paper and provide supporting evidence.

Specific comments:

1. The abstract completely misses any context, problem statement, or conclusions. It is rather a list of the measurements made with some results.

a. Line 11: What is “outflow” referring to? I assume Ice Shelf Water.

b. Line 13-14: “Advecting” horizontally or vertically? How can they be advected if they are already in a depositional layer? I assume this is not meant but the writing is ambiguous.

c. Line 17-18: There is no evidence provided in the manuscript that the flow is really of “tidal” nature. Is 0.1 m s⁻¹ referring to vertical or horizontal velocity? The turbulence in the boundary layer can also result from other factors than tidal flow.

d. Line 19-21: How do class 1 and 2 differ? Both appear to be large particles. There is

C3

no description in the manuscript of how these classes are being derived and how one of them is being attributed to ice crystals.

e. Line 21-23: Large ice crystals (platelets) have been observed before in this region. This is not a new finding. There is no evidence in the paper that these crystals are depositing as compared to growing locally. It is unclear what is meant by “fully grown”. Is there a limit in the size to which the particles can grow? And how is this determined? There is no evidence in the paper for the evolution of growing particles and an upper limit.

f. Line 23: The histogram (Figure 4b) suggests a larger size than the 30-80 mm reported here in the abstract. The main text states an average of about 100 mm. All these numbers appear to be inconsistent.

g. Line 24: The “settlement” is not being clearly addressed in this paper. What are the implications? If there are any they should be explicitly stated here.

2. The introduction lacks clarity and focus. It is unclear what the gap of knowledge is, how it relates to the larger picture, and how it is being addressed.

a. Line 28-29: I am not sure how Antarctic sea ice variations are “confounding communication of key issues to stakeholders and decision makers” and what the formation of platelet ice has to do with it? This argument seems a bit arbitrary and poorly motivated.

b. Line 29-30: The warming on the deep water that is responsible for the increased melting of some of the ice shelves has not yet been attributed to “anthropogenic” sources. Thus, this statement is not quite correct. In addition, I do not know of any evidence that the Ross Ice shelf and the study site have been affected by this process.

c. Line 35: Not all ice shelf water rises buoyantly to the surface. In some regions the water flowing out of the cavity is actually denser than the surrounding water and sinks along the bottom (e.g. in Filchner Trough).

d. Line 47-48: The relation between ice growth, viscosity, and advection is unclear in

C4

this sentence.

e. Line 53-56: It is unclear what is meant with this sentence.

f. Line 65-74: The purpose of this paragraph is unclear. I assume that it aims at providing context for ice-nucleation particles, but this is not clear to the reader at first. In addition, there seems to be irrelevant information and colloquial language. The reasoning implied by the last sentence is not clear to the reader. Isn't it well known by in-situ observations that there is supercooled water under the ice shelf? What is the relation between the supercooled water and the marine sediments at the surface of the ice shelf?

g. Question 1 is already known. There are large ice platelets in this region. Question 2 seems like the most appropriate for this study. Questions 3 and 4 are not addressed by this study.

3. The method of how the vertical velocity is being derived from the echo sounder data is not sufficiently described to be reproducible (lines 134-141). How large are the vertical segments? How are features identified and tracked? How well does the feature tracking from one time step to the next perform and what is the estimated error? The meaning of the sentences in lines 138-141 is unclear and needs clarification.

4. The importance and relevance of the background conditions described in section 3.1 for addressing the key questions is not evident from the text. How do the T and S profile, and current measurements affect where and how the ice crystals could grow in the water column? What is the role of different flow speeds at depth and close to the surface? Why are the depth levels chosen this way? How does the rising of the ISW plume cause a shear in the flow (line 175-176)? Doesn't the below surface freezing point temperature throughout the water column suggest that ISW is present in the entire water column? How else would it be possible to form waters with a temperature below -1.9degC down to 500m? It is unclear why tides should be the main driver of the horizontal flow (lines 176-181)? How about pressure/density gradients, wind forcing,

C5

and large-scale circulation?

5. Section 3.2 contains many unsupported statements.

a. There is no apparent evidence for the reader and no scientific analysis provided in the manuscript that there is a "constant supply of crystals from depth" or their size (lines 188-189).

b. The authors do not provide evidence for the claims made in lines 190-195.

c. How is it known what fraction of the signal derived from the echo sounder results from ice crystals (line 196-197)? How is it known that the signal also includes biological particles? How is the background defined (lines 198-201)? There is no evidence provided for rising particles (line 201-203). No analysis of video sequences supporting the identification of ice crystals (line 203) is provided.

d. Is the positive shift of the histogram towards an upward directed velocity statistically significant and larger than the uncertainty (lines 204-205)? If not, no such claim can be made.

e. How is the crystal size (7 cm, line 205) of the particles in the echo sounder estimated? Why is no histogram provided for those particles? What is the range of Reynolds numbers given the range in velocity and particle size?

f. What is the depth range over which the echo sounder data is evaluated (line 196ff)?

g. Lines 213-226: It is unclear how the discussion of the temporal evolution in these signals relates to the question addressed in the paper.

h. It is unclear how the conclusion is drawn that the "current meter spectrum is dominated by the tide" (line 228-229) and how this relates to the issue addressed in the paper. What is the role of the frequency spectrum for the ice crystal formation and transport and how is this analysis motivated?

6. Line 265: There is no evidence provided in the paper for the visual observations

C6

referred to here.

7. Line 277-279: This sentence is not supported by the analysis. There is no evidence provided for the upward transport of large ice crystals and their origin.

8. Line 280-282: How is this separation obtained? Why is there no analysis showing the statistics of the different classes? What criteria are used to separate these classes.

9. Line 396: There is no evidence provided in the manuscript for a substantial sediment load in the region of ice crystal formation.

10. The manuscript is missing a clear formulation of the conclusions at the end.

Technical comments:

- "Pelagic": I am confused by the use of this word in the context of ice crystals. It is typically used for marine habitats and (if I am not mistaken) is a greek word for "open ocean". So, what is the intention of classifying ice crystals as "pelagic"? I thought the key aspect of the paper was to look at large size of the ice crystals occurring in this region.

- Line 48-49: "thought to exist" requires a reference.

- Line 78-80: These claims require references.

- Line 187: there is no figure 4c

- Line 409-410: A quote requires accurate referencing.

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2020-249>, 2020.