

## ***Interactive comment on “An Antarctic monitoring initiative for fast ice and comparison with the Arctic” by P. Heil et al.***

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### **Summary**

This manuscript discusses the importance of fast ice as a component of the global cryosphere, in particular with regard to its usefulness as an indicator of global change. The authors present details of a nascent fast ice observing network in the Antarctic (the Antarctic Fast Ice Network, AFIN) and discuss some of the differences between fast ice conditions in the northern and southern hemispheres. A comprehensive fast ice observing network in either hemisphere would facilitate significant advances in our understanding of the response of sea ice to climate change. Moreover, since there are pronounced differences in fast ice regimes both between and within the Arctic and

Antarctic, a detailed description of fast ice from a global perspective would make a valuable contribution to the literature.

Unfortunately, this manuscript fails to meet its full potential, largely through a lack of detail and supporting data. The authors provide insufficient detail regarding the challenges and techniques of monitoring fast ice in the Antarctic to fully realize the contribution of this manuscript as a methods paper promoting AFIN. Similarly, the authors' broad generalizations based on limited data result in a simplistic comparison between fast ice in the northern and southern hemispheres. I therefore recommend that substantial revisions be made to this manuscript before it is accepted or publication.

I have listed other general and specific comments below, but my overarching comment is that the authors have set themselves too high a goal for the material they present. In my opinion, the main reason this manuscript is not ready for publication is that its broad scope is not supported by enough data. The results presented in manuscript represent at best a case study demonstrating the potential of AFIN. Given the amount of work that would be required to assemble the comprehensive datasets necessary for a useful comparison of Arctic and Antarctic fast ice regimes, I recommend that the authors omit any reference to the Arctic and resubmit the manuscript as a detailed methods paper describing AFIN with specific case studies from Davis and Finbul. Instead of being used as the inadequate basis for a hemispheric comparison of fast ice, these results could be used to support the more useful discussion of the technical and logistical challenges involved in establishing the much-needed AFIN program.

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## General comments

### 1. Restructuring of manuscript

In line with my recommendation above to resubmit this paper as a methods paper with case study results, I suggest that the authors restructure the manuscript to better distinguish the methods and results sections. In its current form, the manuscript presents results in section 3.2 (which I see as more of a methods section) and section 5.3 (which is part of the results section). In restructuring along these lines, I believe it would become easier to provide more methodological detail when describing the overall network and its core components. The results can then be presented as case studies from specific stations. Omitting the Arctic vs Anarctic comparison (in line with my overarching comment above) would allow the authors to expand the remaining discussion to more thoroughly address the physical, technical and logistical challenges of establishing and expanding AFIN.

### 2. Standardize hyphenation

The manuscript contains multiple uses of “fast-ice” and “fast ice”. Please standardize the use of hyphenation. My preference would be to omit the hyphen in all cases.

### 3. Standardize use of terminology

The authors appear to use the terms “dendrites” and “platelets” to refer to the ice crystals that grow and/or collect beneath the fast ice. I recommend that the authors either distinguish between the two terms or just use one. If just using one term, I recommend “platelet”.

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## Specific comments

In the comments that follow, I have used **bold** and *italic* formatting to highlight text to be inserted or deleted, respectively.

Page 2438, Line 6

“... seasonal **undeformed** fast-ice

Page 2438, Lines 15-18

These lines exemplify the simplicity of Arctic-Antarctic comparison presented in the manuscript. Moreover, these last two sentences of the abstract appear at odds with the last line of section 6.3 (p2453, lines 25-29). In line with my overarching comment in the summary, I recommend these two lines be removed.

Page 2442, Lines 1-2

“... the physical quality of fast ice **and its** spatio-temporal persistence”. Also, “physical quality” is a rather ambiguous term, please describe in more detail what is meant by this.

Page 2442, Lines 26-27

Please provide more details on how the “degree of erosion (honeycombing)” is observed. Is this a subjective visual estimation of the depth of honeycombing, or is it based on an instrumental or crystallographic measurement?

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Page 2442, line 27 – Page 2443, line 1

The length of any dendrites brought up with the auger will be at least partly a function of the size of the auger used. Please specify how these data will be interpreted beyond determining the presence of absence of dendrites.

Page 2443, line 9

Is “vertical fast-ice extent” any different from fast ice thickness? If not, please use the latter, more common term. If there is a difference, please describe it.

Page 2443, line 18 – page 2444, line 3

This material belongs in section 5.1.

Page 2443, lines 18-24

These lines belong in the results section

Page 2445, lines 23-26

This statement needs to be clarified and supported by a reference. At casual reading, it appears that the authors are claiming that platelets incorporated into the consolidated portion of the fast ice thickness can be identified based on their oxygen isotope ratio. If this is true, then it is an important result. However, I wonder whether the authors are instead alluding to platelet identification from crystallographic analysis, with isotopic analyses being use to distinguish between snow ice and sea ice.

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Page 2446, line 1

“Historical data”

Page 2446, line 4

“... in support of station *support operations* ...”

Page 2446, lines 17-21

These lines should be move to the results/case study section, in line with my general comment 1.

Page 2448, line 8

“*On the other hand, there* **Instead**, the interannual variability ...”

Page 2448, lines 18-27

I recommend that this entire section be removed unless the authors present actual results from the AFIN network here. Otherwise the material would be as background in the introduction in the context of the kinds of processes that the AFIN network needs to observe.

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Page 2451, line 6

I assume the use of “equatorward” is an attempt to generalize between the Antarctic and Arctic. However, in both cases, the majority of the pack ice lies northward of the majority of fast ice. I recommend simply omitting the term “equatorward”.

Page 2451, lines 16-21

This is important background information that I feel would be more useful if it were included in the introduction or the methods sections.

Page 2451, lines 24-25

This is an example of where I feel additional detail would enhance the value of this paper. The authors are clearly drawing on experience of dealing with the challenges of carrying out observations in supercooled ocean conditions. A description of any calculations to estimate the amount of heating required and the means of supplying this heat to the dome would be most welcome.

The following comments are examples from the text supporting my recommendation that the comparison between the Arctic and Antarctic should be removed from the manuscript.

Page 2543, lines 2-5

This is an example of a sweeping generalization based on limited data. At the least this sentence should be changed to make clear that it applies only the central Arctic.

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This another example of a statement that needs to supported by a reference, data or both. Moreover, while the statement may be true, its value is diminished by the lack of any discussion of what contributes to interannual variability in fast ice thickness (for example, large scale sea level pressure patterns or river discharge).

Another sweeping generalization that is not specifically supported by data or references. Not only is there no explanation of how fast ice “establishes itself” (do the authors mean lateral expansion away from the coast, or thickening, or both?) but the inference is that Arctic fast ice is not subject to intermittent removal by cyclone action, which is not true (e.g, fast ice in the Bering, Chukchi or Beaufort seas):

Mahoney, A. R., H. Eicken, and L. Shapiro (2007), How fast is landfast sea ice? A study of the attachment and detachment of nearshore ice at Barrow, Alaska, Cold Regions Science and Technology, 47(3), 233-255

Mahoney, A. R., H. Eicken, A. G. Gaylord, and L. Shapiro (2007), Alaska landfast sea ice: Links with bathymetry and atmospheric circulation, Journal of Geophysical Research-Oceans, 112, C02001, doi:10.1029/2006JC003559

This statement is not true. Multiyear ice can be found along the Taymyr Peninsula in the Russian Arctic:

Reimnitz, E., H. Eicken, T. Martin, 1995, Multi-year fast ice along the Taymyr Peninsula, Siberia. Arctic, 48, 359-367

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This is another simplification that not only neglects the differences identified in the last two sentences of the abstract, but also fails to consider the relative importance of icebergs and grounded ridges, river discharge and the complexity of the coastline.

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