

Interactive comment on “Frazil ice growth and production during katabatic wind events in the Ross Sea, Antarctica” by Lisa De Pace et al.

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Received and published: 29 November 2019

This paper describes autumn-winter measurements within an Antarctic polynya during katabatic wind events. These data have been collected in extraordinarily unpleasant conditions and the authors are to be complemented on the number and quality of their measurements. Given the time and place and circumstances under which they were collected, such data are unique and valuable. This paper contributes to our scientific understanding of these important, but rarely observed, katabatic events, making direct observations of how ice formation takes place in these violent conditions. The authors add value by comparing their in situ measurements to those derived from other sources (model, satellite etc).

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However I have a number of comments regarding the presentation of the data which I elaborate on below.

Comment 1: The notation used in the equations and particularly in the supplementary material are not consistent throughout the paper, leading to confusion. For example line 400 states that the total mass of frazil is $Mass_{ice}^S$. However line 81 of Supplemental states that the total mass of frazil is $Mass_{wai}^T$. Some work is required to please ensure consistency of definitions of symbols throughout the Supplementals and the main body of the text.

Comment 2: Is it important that the stations retain their station number from the field campaign? It would be easier for the reader to see patterns in the tables and Figure 10 if there was a simple and intuitive ordering of station numbers, say from the coast outwards.

Comment 3: Please consider the number of significant figures used in estimated values throughout the paper. For example in Tables 1 & 2 estimates are given to 4 significant figures and 2 decimal places which greatly exceeds the uncertainty in the estimate.

Comment 4: I very much appreciated the detailed laying out of calculations in the Supplementary material. However, while I followed Supplemental 1, I could not understand the derivation of $Conc_{ice}^{salt}$ in Supplemental 2 and 3. I do not understand why you use the quotient of the integrals (S3.3) to represent the integral of the quotient (i.e. the integral of (S3.2)). Please could you clarify.

Technical Corrections

line 36: I'm not sure what is meant by "one to two orders of magnitude better insulated"? Does it mean that the heat flux to the atmosphere is one to two orders of magnitude lower?

Line 54: "eutectic freezing point" ? None of the cited works use the word "eutectic". I don't know if this is strictly incorrect but I did find it confusing since the "eutectic

temperature” for sea ice is about -36°C (Vancoppenolle et al., 2019)

Vancoppenolle, M., Madec, G., Thomas, M., & McDougall, T. J. (2019). Thermodynamics of sea ice phase composition revisited. *Journal of Geophysical Research:Oceans*, 124, 615–634. <https://doi.org/10.1029/2018JC014611>

Line 54: “Dmitrenko”

Lines 57-58: These are observed sizes so why not cite observations. Heorton & Feltham, 2017 and Wilchinsky et al., 2015 are modeling studies. Note Wilchinsky rather than Wlichinsky.

Line 62: incomplete sentence.

Line 64: Heorton & Feltham, 2017 and Wilchinsky et al., 2015 would fit well here. Additional relevant observational study that may be of use.

Ito, M., Ohshima, K., Fukamachi, Y., Simizu, D., Iwamoto, K., Matsumura, Y., . . . Eicken, H. (2015). Observations of supercooled water and frazil ice formation in an Arctic coastal polynya from moorings and satellite imagery. *Annals of Glaciology*, 56(69), 307-314. doi:10.3189/2015AoG69A839

Line 66: Suggest reference for statement re dense water formation; such as Oshima et al 2016.

Ohshima, K.I., Nihashi, S. & Iwamoto, K. Global view of sea-ice production in polynyas and its linkage to dense/bottom water formation. *Geosci. Lett.* 3, 13 (2016) doi:10.1186/s40562-016-0045-4

Lines 96-98: Suggest also compare with satellite observations, e.g. Oshima et al, 2016.

Line 115: Typo Petrelli et a;., 2008

Line 151: What is the implication of being deployed from the starboard Baltic Room?

More importantly what sort of issues arose because of sampling in supercooled waters? The very recent paper of Robinson et al (2019) may be of interest.

N.J. Robinson, B.S. Grant, C.L. Stevens, C.L. Stewart, M.J.M. Williams, Oceanographic observations in supercooled water: Protocols for mitigation of measurement errors in profiling and moored sampling, Cold Regions Science and Technology, 2019, 102954, doi.org/10.1016/j.coldregions.2019.102954.

Lines 169-171: Care needs to be taken because the magnitude of the supercooling depends on the standard used. For example Nelson et al (2017) state “in situ supercooling is larger (~ 0.003 K) when using TEOS-10 compared with EOS-80.”

Nelson, M., Queste, B., Smith, I., Leonard, G., Webber, B., & Hughes, K. (2017). Measurements of Ice Shelf Water beneath the front of the Ross Ice Shelf using gliders. Annals of Glaciology, 58(74), 41-50. doi:10.1017/aog.2017.34

Line 179: How were data normalized to 10 meters? I assume log boundary layer.

Line 190: Suggest “near katabatic winds ($> 10 \text{ ms}^{-1}$) lasting”

Section 3.1: The reader would have more confidence in this section if the sampling protocol was detailed (see comment on line 151).

Line 220: “plots (a-k)”

Fig 4 & Fig 5: Again more description of the temperature of the instrument when it enters the water is needed in order to interpret these figures.

Lines 248 & 250: What was the uncertainty in determining the baseline for temperature and salinity?

Line 254: Consult Nelson et al (2017) and Robinson et al (2019)

Line 258: Incorrect citation. Should be (Skogeth et al, 2009)

Lines 265-268: Check procedures with respect to Robinson et al (2019)

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Line 293 & 302: remove hyphen in “super-cooling” for consistency as used “supercooling” in other places.

Line 296: “0.5 to 1 ‰ ” NOT “0.5 to 1 ‰ ”. This may mean that the statement on lines 303-305 needs to be reconsidered.

Line 298-299: Consider the number of decimal places in relation to the error in the measurement.

Section 3.5: I’m not sure if this section is necessary.

Line 311: “Ice Shelf Water” is not defined. Also later in paper ISW is used and this also needs to be defined.

Line 312: “(Rees Jones & Wells, 2018)” NOT “(Jones & Wells, 2018)”

Lines 313 & 315: “Robinson et al (2014)” NOT “Robinson et al (2017)”

Line 340: Remove “?”

Line 342: “and movement”? of pack ice

Line 363-364 and 398-399: What is the “starting location”? Why 10 m? Why does 10 m eliminate selection bias? Please consider rewriting.

Equation (2): Is $Conc_{ice}^{temp}$ the same as $Conc_{ice}^T$ in Table 1? Please be consistent with notation.

Line 381: lower case “w”

Line 393: “Supplementals 2 and 3”

Line 400: This is an example of Comment 1 above.

Equations (3) and (4): What is H? Is this z_S in the Supplemental?

Equation (5): $Conc_{Ice}^{salt}$ the same as $Conc_{ice}^S$ in Table 2? Please be consistent with notation.

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Lines 424-426: Surely you could argue that the humidity was high because of evaporation.

Table 1: Please see Comment 3.

Line 477: “Robinson et al (2014)” NOT “Robinson et al (2017)”

Line 479: ISW is not defined

Lines 486-487: I understood that the smallest eddies controlled the rate of dissipation. However the arguments of the energy cascade equate the rate at which energy was injected at the largest scales to the rate of energy dissipation at the smallest scales (e.g. see Fig 8.3 Cushman-Roisin, 2019). This I agree with equation (6).

Line 488: “Cushman-Roisin, 2019” NOT “Cushman-Rosin, 2019”

Line 490: Insert “TKE” after “turbulent kinetic energy”

Equation (8) & (11): I find the use of * to mean \times very confusing.

Line 518: what does roughness class 0 imply? It does seem very small.

Line 534: delete “.”

Lines 544, 555, 562, 563, 587: please italicize variables

Line 551: How is an “active depth layer” defined?

Line 562: insert space

Line 573: replace “A log-linear fit” with “A linear fit on a log-log scale”

Line 578: replace “A logarithmic linear fit” with “A linear fit on a log-log scale”

Lines 616-617: See Comment 3. I suggest rounding to 69, 28 and 10.

Line 621: “This other variations. . .”??

Line 624: Insert “CI” after “confidence interval”.

Line 628: Delete” bin averaging”

Table 2: Please see Comment 3.

Table 2, column TKE diss: Why to the power “-05”? Why not just “-5”?

Table 2: Insert a note “MLD= mixed layer depth” – if it does??

Line 643: See Comment 3. I suggest rounding to 26.

Section 6.2: Note that from satellite studies Oshima et al (2016) quote an ice production rate of 8.4 m yr^{-1} (from Mar-Oct) which is about 35 cm day^{-1} . This is close to your result.

Fig 10: This is a very interesting figure - I found it difficult to see and read the colors on top of the bathymetry color bar. I was not sure why bathymetry was needed. I wondered why it was so deep on the southern side of the Drygalski Ice Tongue? A simpler figure, an intuitive numbering of stations, and rounding of data would all make this figure have a higher impact in my opinion.

Line 721: Roisin

Line 784: D.W. Rees Jones

Line 792: Ross Sea

Line 809: Arctic

Supplementals: Please see Comments 1, 3, and 4.

I think Equation (S1.5) is meant to be in $Conc_{ice}^{temp}$

Personal dislike of use of * to mean \times “times” in Supplemental 2 and 3

What is x in Table S3? I assume that \times “times” is meant.

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2019-213>, 2019.