Review 1 Comments and Responses: Comments are in black and responses are in red

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).

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 MassSice. However line 81 of Supplemental states that the total mass of frazil is MassTwai. Some work is required to please ensure consistency of definitions of symbols throughout the Supplementals and the main body of the text.

Thank you, all equations have been fixed and reviewed for internal consistency.

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.

We recognize that a sequential numbering system for the stations would be more logical. However, we also think there is a lot of value in being able to relate these data back to the hydrographic data that is stored in the public domain. For this reason, we argue that it is worthwhile to retain the original numbering so that they would match the station numbers in the public repository.

We have included a sentence in Section 2.2 to explain the enumeration: "CTD station numbers follow the original enumeration used during NBP17-04, so they are more easily traceable to the hydrographic data, which is archived as described below in the Data Availability section."

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.
Thank you, we have corrected the number of significant figures used throughout the tables.

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 Concsalt in Supplemental 2 and 3. I do not understand why you use ice

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.

Thank you for catching the error on our derivation of the frazil mass from the salinity anomaly. We agree with your assessment that we had applied the integral incorrectly when going from Step S3.2 to Step S3.3. Supplemental 3 has been changed to correct the formula. All calculations were redone and code was double checked.

The correction led to minor changes in the mass of ice and the concentration of ice, but those changes in the bulk inventories were not large enough to alter our interpretations.

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?

Thank you, edited for clarity.

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 −36oC (Vancoppenolle et al., 2019)

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

Thank you, edited for clarity and removed.

Line 54: "Dmitrenko"

Thank you, corrected.

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.

Thank you, corrected.

Line 62: incomplete sentence.

Thank you, corrected.

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 Arc- tic coastal polynya from moorings and satellite imagery. Annals of Glaciology, 56(69), 307-314. doi:10.3189/2015AoG69A839

Thank you, corrected and added.

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

Thank you, corrected and added.

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

We added this paper and a few other satellite observation papers.

We have heavily revised section 6.2 – the discussion of previous sea ice production estimates. That section includes this paragraph on remote sensing: "Overall, these ice production estimates from in-situ data are larger than the seasonal production estimates derived from remote sensing products. Drucker et al (2011) used the AMSR-E instrument to obtain a seasonal average of 12 cm day^{-1} for years 2003-2008. Oshima et al, (2016) estimated 6 cm day^{-1} of seasonal production for the years 2003-2011, and Nihashi and Ohshima (2015) determined 7 cm day^{-1} for years 2003-2010. Finally, Tamura et al (2016) found production rates that ranged from 7-13 cm day^{-1}, using both ECMWF and NCEP Reanalysis products for 1992-2013, reflecting a greater degree of consistency in successive estimates, likely because of consistency in the estimation methods. "

Thank you for pointing us to the paper. We have added the comparison to microwave sensing production rates.

Line 115: Typo Petrelli et a;., 2008
Thank you, corrected.
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 wa- ters? The very recent paper of Robinson et al (2019) may be of interest.

Thank you. This was a great paper to review and has been added to our references. The paper outlines two potential sources of bias that are a concern for us that are explained in detail here and have been added to Section 3.2:

1. Self-heating where the thermistor reads warmer than the water because of the heat that remains in the housing, etc. We did keep the CTD rosette at room temp, so there is a risk of this.
2. Ice formation on surfaces in the conductivity cell. We don't see this as a risk because of (1) - the sensor was warm before it went over the side.
3. In the first draft, we examined and discussed the potential for frazil ice crystals passing thru the conductivity cell.

We think (2) did not take place because the cell was filled with saltwater prior to deployment. Freezing did take place at the beginning of the expedition, but this can be very damaging to a conductivity cell so steps were taken to avoid it.

Additionally, conductivity/salinity was increasing in our profiles. This is opposite the trend that Robinson et al (2019) observed. We address this question in more detail within section 3.2.

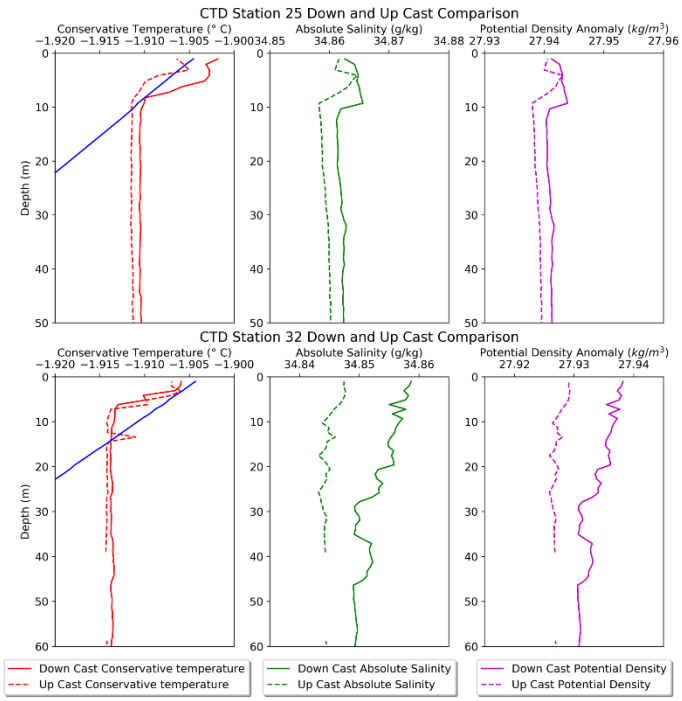
The protocol was to complete 2-3 minutes of soak time at around 10 m, until the spikes in the conductivity cell have completely gone away. We believe this dissipates much of the thermal

inertia, although a 10 minute soak time would have been better, the results suggest that 2-3 minutes will dissipate 70-80% of that excess heat in the sensor body.

While Robinson recommended using upcasts to avoid the thermal problem, this is complicated because the CTD sits at the bottom of the rosette, so the upcast can be influenced by turbulence and smoothing around the large 24 bottle, 2 ton rosette package.

Nevertheless, we examined upcasts and found that many of them were consistent with the results form the downcast – both for temperature and salinity. We have included a figure in the supplemental that highlights this.

The below figure was added as Supplemental Figure 1. For Station 25 you can see the salinity and temperature anomaly is reduced for the up cast which we attribute to the wake. For station 32, there is missing data in the up cast which would have impacted our calculations.



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 super- cooling 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
We have added a sentence in Section 2.3 stating that the choice of empirical relationship can affect the absolute freezing point calculation and we have included this citation, thank you for pointing this out.

Line 179: How were data normalized to 10 meters? I assume log boundary layer.
Thank you, correct we used a logarithmic wind profile.

Line 190: Suggest “near katabatic winds (> 10 ms−1) lasting”
Thank you, corrected.
Section 3.1: The reader would have more confidence in this section is the sampling protocol was detailed (see comment on line 151).
Thank you. We have added more details to our sampling procedures.
Line 220: “plots (a-k)”
Thank you , corrected.
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)

Please refer to our related responses above and in the revised manuscript.

We have revised Section 2.3 to be more descriptive with the CTD sampling procedure.

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

Lines 265-268: Check procedures with respect to Robinson et al (2019)
We did not find any reference to or guidance on averaging procedures Robinson et al (2020). As discussed, we investigated the effect of averaging over different vertical intervals and found no systematic influence.

Line 293 & 302: remove hyphen in “super-cooling” for consistency as used “supercool- ing” in other places.
Thank you, corrected
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.
Thank you, corrected. I updated it to use g kg-1 to be consistent throughout the paper.
Line 298-299: Consider the number of decimal places in relation to the error in the measurement.
Thank you both reduced to the appropriate number of significant figures.
Section 3.5: I’m not sure if this section is necessary.
Thank you eliminated in favor of shortening the article.
Line 311: “Ice Shelf Water” is not defined. Also later in paper ISW is used and this also needs to be defined.
Thank you, defined. Section Removed.
Line 312: “(Rees Jones & Wells, 2018)” NOT “(Jones & Wells, 2018)”

Thank you. Section Removed.

Lines 313 & 315: “Robinson et al (2014)” NOT “Robinson et al (2017)”
Thank you. Section Removed.

Line 340: Remove “?”
Thank you corrected.
Line 342: “and movement”? of pack ice
Thank you, sentence removed in favor of shortening the article.
Line 363-364 and 398-399: What is the “starting location”? Why 10 m? Why does 10 m eliminate selection bias? Please consider rewriting.
Thank you, reworded. The variance in the temperature and salinity was less than the order the precision of the instrument. We cited the precision in Section
Equation (2): Is Conctemp the same as ConcT in Table 1? Please be consistent with ice ice notation.
Thank you corrected.
Line 381: lower case “w”
Thank you corrected.

Line 393: “Supplementals 2 and 3”
Thank you corrected.

Line 400: This is an example of Comment 1 above.
Thank you corrected.
Equations (3) and (4): What is H? Is this zS in the Suppplemental?
Thank you corrected to reflect the integral from the surface to zS. H was removed for clarity and consistency.
Equation (5): Concsalt the same as ConcS in Table 2? Please be consistent with Ice ice notation.
Thank you corrected.
Lines 424-426: Surely you could argue that the humidity was high because of evapo- ration.
Thank you edited for clarity.
Table 1: Please see Comment 3.
Line 477: “Robinson et al (2014)” NOT “Robinson et al (2017)”
Thank you corrected.
Line 479: ISW is not defined
Thank you corrected and 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).
Thank you. Clarified and corrected.

Line 488: “Cushman-Roisin, 2019” NOT “Cushman-Rosin, 2019”
Thank you corrected.
Line 490: Insert “TKE” after “turbulent kinetic energy”
Thank you corrected.
Equation (8) & (11): I find the use of * to mean × very confusing.

Thank you. It has been removed from all equations.
Line 518: what does roughness class 0 imply? It does seem very small.
Roughness class 0 implies a ocean or sea surface
Khalfa, Dalila & Abdelouahab, Benretem & Herous, Lazhar & Issam, Meghlaoui. (2014). Evaluation of the adequacy of the wind speed extrapolation laws for two different roughness meteorological sites. American Journal of Applied Sciences. 11570583. 570-583. 10.3844/ajassp.2014.570.583

Line 534: delete "."
Thank you corrected.
Lines 544, 555, 562, 563, 587: please italicize variables
Thank you corrected.

Line 551: How is an "active depth layer" defined?
Thank you, edited for clarification.
Line 562: insert space
Thank you corrected.
Line 573: replace "A log-linear fit" with "A linear fit on a log-log scale"
Thank you corrected.
Line 578: replace "A logarithmic linear fit" with "A linear fit on a log-log scale"
Thank you corrected.
Lines 616-617: See Comment 3. I suggest rounding to 69, 28 and 10.

Line 621: "This other variations. . ."??
We have revised the wording.

Line 624: Insert "CI" after "confidence interval".
Thank you corrected.

Line 628: Delete" bin averaging"
Thank you corrected.

Table 2: Please see Comment 3.
Table 2, column TKE diss: Why to the power "-05"? Why not just "-5"?
Thank you corrected.
Table 2: Insert a note "MLD= mixed layer depth" – if it does??
It does, Thank you added.

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.

Thank you the figure was modified to make it easier to read the sea ice production rates.

We have revised this section (6.2) significantly to discuss the ice production rates.

Line 721: Roisin
Thank you corrected.

Line 784: D.W. Rees Jones
Thank you, reference removed since the context was removed.
Line 792: Ross Sea Thank you corrected.

Line 809: Arctic Thank you corrected.

Supplementals: Please see Comments 1, 3, and 4.
I think Equation (S1.5) is meant to be in Conctemp ice Thank you corrected.

Personal dislike of use of * to mean × "times" in Supplemental 2 and 3. Thank you, all removed.
What is x in Table S3? I assume that × "times" is meant. Yes, italics removed and spaces added.
Thank you