Dear Mr. Peterson:

Thank you for submitting the manuscript "Desalinating young sea ice enhances turbulent ocean heat flux" (2017GL072968) to Geophysical Research Letters. I have now received 2 reviews of the manuscript. Based on the reviews and my own reading of the manuscript, I am declining the manuscript for publication in Geophysical Research Letters. Both reviewers feel that the paper could perhaps be a GRL paper in the future (or perhaps better to be submitted to JGR to expand more on methods/results), but at the moment the treatment of the results is too brief and the high salt flux numbers are not backed up by other evidence.
To give you more time I am rejecting this for now, but you may resubmit to GRL if you can adequately address the reviewers comments or it may be better to expand this and submit to JGR.

I am enclosing the reviews, which you may find helpful if you decide to revise the manuscript and submit to another journal. I am sorry that I cannot be more encouraging at this time.

Thank you for your interest in GRL.

Sincerely,

Julienne Stroeve
Editor
Geophysical Research Letters

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Please see the following link for information regarding GRL's Science and Presentation Category Ratings: http://publications.agu.org/author-resource-center/author-guide/grl/

Reviewer #1 Evaluations:
Science Category (Required): Science Category 2
Presentation Category (Required): Presentation Category B
Key Points (Required): Yes

Reviewer #1 (Comments to Author (shown to authors):

I have looked at the manuscript "Desalinating young sea ice enhances turbulent ocean heat flux" by Algot Kristoffer Peterson [Paper # 2017GL072968]. It looks at a subset of turbulence (TIC) data from a well-studied but rich dataset and focuses on saline plumes beneath an Arctic ice floe.

This is an intriguing snippet of data. I have no doubt that properly understanding the nature of the salt flux from these drainage channels and their influence on the upper water column is very important in getting the whole ice-ocean story right. Also this manuscript gives balanced weighting to the ocean and ice - which is not so common.

However, without making continual reference to the large JGR paper that is in press there is lots here that is lost to the new reader. There's no sense of setting. It appears quite important

that we have an understanding of floe size and motion but this is not addressed. Is it 10 m to the floe edge or 500 m? There is also a seasonal perspective but again this is not really covered.

What I don't get is the TIC is fixed in the floe reference frame and these drainage channels don't particularly wander. How can we unpick variation in ice-ocean relative motion vs straight plume width? I think there is a significant component missing in the concurrent velocity data.

The TIC is fixed, and the drainage channels don't move, but as the floe drifts with varying speed and changing directions, my interpretation is that the TIC will sometimes sample plumes meandering past the measurement volume. We cannot know for sure if we measure the core of the plume (the full diameter), and I do not attempt to pick out the plume width.

I think the analysis needs more development. This is manifest in the Results section reading more like a Discussion Section. Words like possibly and imply appear.

The manuscript has been substantially updated, and the analysis is extended.

It would be pretty useful to see some upper water-column stratification profiles - possible? This would give some idea of the baroclinic timescales for transmission of signals say from the flow edge.

In the updated manuscript, figure 2 shows upper 30m temperature along the drift track, and four selected stratification profiles from MSS profiles. I calculated the vertical modes from the four stratification profiles now given in Figure 2. The phase speed of the first baroclinic mode is between 0.25-0.43m/s. With a distance from the ice edge of ~200m, the travelling time for a baroclinic signal from the floe edge would be ~10minutes. This is comparable to the 15minute segment length used in the flux calculations, and could thus potentially introduce errors. However, these are signals that would be violating Taylor's hypothesis, which is something that has already been systematically checked (as described in the preceeding study Peterson (JGR, 2017). From the analysis done there, any segements violating Taylor's hypothesis are already flagged and removed from analysis.

If Fig 2 is actually from a coherent plume structure then it would seem useful to reverse the plume equations to work out what is actually happening at the source? You have a width and a distance and a buoyancy anomaly?

I take it that plume equations are those described by Turner (Buoyancy Effects in Fluids, 1973). I think taking the equations back to the source is problematic, and prefer not to dive deep into this. The plumes predict that both w and g' become infinity as z approaches 0, which is more of a virtual point source than the actual source. If we made an assumption of the radius at the source, the calculations could be done, but the equations are still only applicable for a fully developed 'steady state' plume, and not in the developing region close to the source. Determining the entrainment coefficient is also problematic, particularly as the plumes move in a sheared flow.

As Fig 4 notes shear mixing is a key element in the story and the TIC will provide nice data on this but it is not included here. Also this figure implies that the ocean is stagnant. As noted elsewhere the horizontal advection story that is missing here must surely be key?

Friction velocity from the TIC is now included in Figure 1. Also included is the ice drift speed and velocity measured by the TIC. Figure 4 is updated to how that the plumes are sheared, necessary to explain how they can in fact be observed by the instrumentation.

Figures
Fig 2 I'd be curious to see horizontal velocity over this time?

Horizontal velocity anomaly is now included in the figure, which shows a corresponding drop during the plumes. I've also looked at absolute velocity, and although variability in the background velocity is large, a marked decrease is visible there as well.

Fig 3 OK this had me initially confused - these profiles are in the ice presumably... Ice? Snow? Vertical axis label says distance from ice-ocean interface but 0 cm seems to be at ice snow interface... ? I think I worked it out in the end but it shouldn't be that hard.

The figure was unfortunately incorrectly labeled, but is now updated and should make more sense.

Fig 4 - the color shading to represent temperature is pretty subtle - profiles simpler and clearer? It would be interesting to include the TIC in the panels to give the reader a true picture of exactly the setup and what the measured signal might mean in that context.

Temperature profiles are also shown, in addition to the color indication for temperature in the background. The subtle difference at the ice-ocean interface is also labeled 'meltwater', pointing to the reason for the temperature elevation at the interface. The figure is updated to include the turbulence mast, since there are several vertical scales indicated that can confuse the reader.

Reviewer #2 Evaluations:
Science Category (Required): Science Category 2
Presentation Category (Required): Presentation Category A
Key Points (Required): No

Reviewer #2 (Comments to Author (shown to authors):

Evaluation of « Desalinating young sea ice enhances turbulent ocean heat flux » by Algot K. Peterson, submitted to GRL

Feb 28, 2017, Martin Vancoppenolle

Peterson presents eddy-covariance derived estimates of under-ice turbulent heat and salt fluxes, during about a week of measurements, in June 2015. Their finding corroborate high downward salt fluxes and upward heat fluxes associated with these plumes. The authors argue that such salt fluxes can be explained by sea ice desalination above a freshwater layer, which I think makes sense. The paper is well written and I enjoyed reading it. However, I think it is not ready for publication (science category 2-3), for the following reasons.

* The statements in the abstract are too strong as compared with observational evidence. In particular the large salt flux seems very very large (nearly impossible) and most importantly is unsupported by nearby ice core evidence and previous studies.
* Systematic biases in the experimental setup or instrumenting device are not discussed.
* There is no context information about sea ice conditions and water column conditions.
* The discussion of sea ice desalination mechanisms is imprecise
* The supplementary material is useless and not referenced

The dataset is definitely interesting, and the paper is not poor (Presentation category A) in present stage. The paper would gain in interest if rather than seeking for large impacts, the authors would focus on better discussing physics and uncertainties. In that state, I encourage the authors: it could make an excellent contribution to GRL or maybe rather JGR, provided these four points are properly addressed.

More detailed comments

*** The numbers for salt fluxes seem particularly large, and there is no reason to believe they would be so large.

1) All numbers measured are really high, in terms of velocity, salinity and temperature fluctuations, as compared with those of Widdell et al (2006), which performed a similar analysis in a Svalbard fjord. There is no physical explanation as to why all these fluctuations would be so large.

The most important reason for the large difference between this study and Widell's is that the sea ice is melting rapidly in the present study, as opposed to little or no melt in the fjord case. The melt causes brine pockets to be exposed directly, in addition to desalination within the ice. This is stated more explicitly in the updated manuscript.

2) In particular, the large salt fluxes are not supported by ice core evidence. The numbers would imply a very large sea ice desalination (a loss of at least 7 g./kg m in salt content), whereas nearby ice cores would suggest ~3 g/kg. The authors say turbulence measurements and ice cores are consistent. In reality, I would say they are consistent in terms of order of magnitude, but not in terms of actual values. Hence the conclusions you can draw are qualitative and not quantitative as done in the abstract.

There is no particular reason why one should expect a perfect match between salinity flux and salt content of the ice cores, for several reasons. Firstly, the ice cores are not necessarily sampled from the same place as the plumes originate, and there is considerable inhomogeneity even in level sea ice. Second, ice coring is a destructive sampling method where brine is often lost from the core upon retrieval. Extrapolating to cover the gaps indicate salt flux about twice the salinity reduction between the ice cores. Agreement within a factor of two is definitively acceptable. The updated manuscript better emphasizes the discrepancy and has a more qualitative focus.

Besides, my own computations based on the mean salt flux presented in the table (0.19 * 10-4 g/kg m/s during 8 days) give even a larger value: >13 g/kg, which could be more

representative of what could happen if a different account for data gaps was made.

More generally, 7 g/kg means that sea ice with a bulk salinity of 7, which is in the high range for this time of the year, and a thickness of 1 m, would desalinate entirely during 8 days, which I would tend see as (completely) unrealistic. At the very least, I don't think such sea ice desalination has ever been observed.

During the course of these observations, the sea ice melted nearly completely from more than 1m thickness. When the ice melts it is to be expected that the salt is released, and complete desalination is not so strange in my opinion. What is more extraordinary about this, is that the observations indicate that much/most of the salt actually penetrates past the very surface layer, reaching the instrument at 1m below the ice.

This does not mean that data should be thrown away, but then the confidence in the numbers is not so high.

3) I'm not an expert in turbulence measurements, but I wonder why - in the presence of suspiciously large salt fluxes - a systematic bias has not been considered and discussed. I don't understand why the impact of observational uncertainties have not been explored (not only in a statistical sense).

The data set had already been thoroughly quality checked as reported in the JGR paper, including tests for stationarity, which would be the most likely candidate to corrupt the flux calculations. This was not discussed in the manuscript to keep it short, but now I shortly refresh the quality control, and look more into possible biases in a separate section, such as the potential effect of the hydrohole on the measurements.

Recommendation: the author should give more thorough discussion as to why salt fluxes could be so high, more generally on the confidence we can have in the numbers. I would explore potential systematic biases in the observations. Or provide more insight on how sea ice or oceanographic conditions would favor such large vertical current, salinity and temperature fluctuations, giving these really high salt fluxes.

*** There is not enough context explanations on:
1) what the sea ice conditions looks like (supplementary material is insufficient and not cited)
2) what the water column looks like
3) how the turbulence device was set-up under the ice. If a hole was dug recently before sampling, this could well have intensified brine fluxes.

In this context, the reader cannot appreciate what could have made conditions specific to support large salt fluxes.

*** The discussion of the detailed sea ice desalination mechanisms is imprecise, to the best of my knowledge. Drop off.

There are imprecisions. Just two examples.
- there is no need for melt ponds to have « flushing », percolation of surface meltwater is sufficient, and there is abundant evidence in the literature to support that (works of Hajo Eicken in the 2000's).
- l. 221: gravity drainage occurs when buoyancy exceeds friction. -> it is dissipation

(including dissipation by diffusion)

The rest of the sea ice desalination analysis is pure speculation to me: you cannot conclude anything precise based on two ice cores without a more physically-based analysis.

Recommendation: since the paper does not provide any ice physically-based ice-desalination analysis (except 2 ice cores), I would recommend to keep the discussion on sea ice desalination simple. During the melt period period both gravity drainage and flushing are possible, and they would contribute by about 2-3 g/kg.m overall, for typical first-year ice. This level of discussion would probably be nearly sufficient for interpretation of your turbulence measurements.

The discussion of the desalination process is not concluding, but rather explores the processes that can be active, and that criteria for those are met. I believe that the observations can be caused by a combination of flushing (because of flooding), gravity drainage (helped by melt at the interface) and directly through melt exposing the brine pockets. I believe the updated manuscript gives a more balanced mention of the different processes.

*** Conclusions: key statements / abstracts is unsupported by paper material

Finally, the wording and the paper would gain in rigor if the statements in the abstract were tuned down in tone.

The conclusions and abstract are toned down a bit, and focus is now more qualitative than quantitative, reflecting uncertainties in measurements.

« The timing of salt release ... can have great impact on sea ice melt » is a general statement that is not really supported by your data, because the heat flux generated by plumes, at least as your data tell, is much smaller than what other sources would give (solar heats and fronts). I think a more rigorous version would be « Our observations suggest the salt release during summer melt is significant but likely not the dominant process ».

- « Unprecedented observations of salty plumes »... I would not say your observations are unprecedented, since the paper of Widdell et al already documented such process, but for fast ice. I would say that your study expand the findings of Widdell for drift ice, which is good enough!

- I think the focus of the abstract should not be on the big numbers, especially the salt flux, given the probable uncertainties discussed above. The claim « The total observed salt flux accumulates to 7.6 kg/m2 and compares well with the salt reduction found between two nearby ice cores » is not supported, because you would need a very strong sea ice desalination that is not really confirmed by ice core evidence and that sounds nearly impossible. The paper would gain in quality by admitting this salt flux estimate is probably too large. The sole fact that salt can be exported below the surface layer during sea ice melting is interesting enough.

- The last sentence that « increasing fraction of younger ice suggests the impact of desalination in increasing in the new Arctic » is not supported or true. Yes, more desalination in the future Arctic can be expected, but when you budget freshwater and salt, the dominant effect on sea surface salinity is more freshwater release, which increases stratification

- Finally, none of the key points is supported, in my view
* key point 1: the observations are not really unprecedented (unprecedented is not the point actually)
* key point 2: there is no quantitative match between ice cores and turbulence
* key point 3: unlikely speculation