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Interactive comment on "Observations of brine plumes below Arctic sea ice" *by* Algot K. Peterson

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The author thanks the reviewers for their good and constructive comments. The comments are reproduced below, followed by the authors response and any changes to the manuscript. Author responses are indented and typed bold fonts.

Some additional minor changes to the manuscript are listed below. See also the track-changes pdf attached for minor edits and the updated figures.

- **Added a short explanation of Taylor's hypothesis since it has been pointed out to me that some readers may be unfamiliar with turbulence theory: "Taylor's hypothesis assumes that an eddy is essentially unchanged as it passes the measurement volume, allowing temporal measurements to be translated to spatial measurements."**

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- **Caption Figure 3. Changed description of the temperature data to simplify the description: “Conservative Temperature (colors) data are derived from a combination of vertical MSS profiles in the upper 30m and TIC time-series measurements from 1 and 5m.”**

The manuscript looks at a subset of turbulence (via tried and tested TIC instrumentation) data from a well-studied but rich dataset and focuses on saline plumes beneath an Arctic ice floe. This is an intriguing dataset and analysis. 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 (Arctic) ice-ocean story right. Also this manuscript gives balanced weighting to the ocean and ice – which is not so common. I think this is a useful study.

My main comments/concerns/thoughts are: The setting is now framed nicely in the informative, if rather complex, figure 3. I think I've worked it all out. Should the triangles colour-code to the N2 profiles? Daily ticks (crosses) are hard to see. What are the dotted lines? Are the 3D view and bathymetry really required given that we are looking at the upper 1m and the ocean is 1000-2000m deep?

I agree that the figure is rather complex, but I think it is useful to gather the environmental context in one figure like this. Color-coding of the triangles is a good idea. The dotted lines are ice edge (50In the updated Fig 3 the following changes are made:

- **Triangles are color coded to match the profiles (in b)**
- **Daily ticks are made larger**
- **The label ‘ice edge’ is placed together with the dates for ice-lines**
- **The contours 2000 and 1000m are labeled**

What I don't get is the TIC is fixed in the floe reference frame and these drainage channels don't particularly wander (yes/no?). How can we unpick variation in ice-ocean relative motion vs straight plume width? I think there is a component missing that could be accessed from the concurrent velocity data. Fig 5 helps with this.

As I understand it, drainage channels do not wander, but the plumes do – in response to turbulence and ice-ocean relative motion. The TIC observes plumes as they meander through the measurement volume, allowing us to observe some cross-section of the plumes. These cross-sections, however, cannot be assumed to be the diagonal across the plumes that could be converted to a plume width. For this reason, I decided not to try to pick out the width of the plumes. See also my response to a comment on scale of the plumes by the second reviewer.

The horizontal speed is correlated with the plume downward vertical velocity – this has me confused. If it's flowing horizontally faster wont the boundary-layer be more turbulent and tend to mix the plume before it arrives at the TIC?

I agree, faster horizontal flow tends to mix the plumes – which is also observed in Fig 5b. When horizontal velocity is higher, fewer of the plumes are supercooled. The plumes are flowing in a turbulent environment, and as I see it, these observations show the resulting fluxes of plumes and boundary layer turbulence combined.

Is it possible to take the vertical and horizontal velocities and back-trajectory to see where the plumes are actually coming from? Picking a mean horizontal speed of 0.15 m/s and taking a peak vertical velocity w' of -0.05 m/s so it will take 20 s for a "new" plume to get to the sensor suggesting a source radius of 3m. How many drainage centers are likely in this area?

This kind of 'back-trajectory' is already included in the manuscript (p.15, lines 1-8), estimating that the plumes typically originate from a 2-5m diameter from the

measurement position. Such an area should contain a great number of drainage channels. We would typically find drainage channels in all ice cores we made (10cm diameter). I don't have numbers for this drift (it was not measured), but e.g., Cole Shapiro (JGR, 1998) found that brine inclusion aspect ratios in the horizontal plane was typically 1:2-5 in May (near Barrow, Alaska).

Fig 5b is one of the cleanest results I've seen in boundary-layer observations. Despite this it seems to warrant only a few lines of text and no real exploration. It is really curious that the fast horizontal flow should generate the strongest vertical flows also.

You are right that this result might deserve a little more mention. I did not find the result so strange though, and have extended the paragraph (p9 l4-10) to clarify: "The peak in vertical velocity increases with increasing current speed (Figure 5b). This does not mean that plumes are stronger during fast drift, but rather that wmax is determined by w' of the larger turbulent eddies. The plumes are thus mixed more efficiently into the ambient water."
As I see it, this mechanism is also consistent with less super-cooling for higher drift speed, also discussed in that paragraph.

Pg 13 line 25 doesn't make sense to me. I might have thought increased boundary layer turbulence might have mixed the plume and reduced the peak in the plume by increasing the width.

See the response to the previous comment. We agree that the increased turbulence mixes the plume – and the large wmax reflects the size of the eddies, not that plumes are intensified. I think this picture comes across better with the changes made in response to other comments.

Possible to look at the width of the plume as a function of velocity? It would seem so but I think this then reveals an issue in that the nice plume structure shown in Fig 4 is controlled by the horizontal velocity.

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This is possible, of course. Plotting the 15-minute mean horizontal current against duration of the plumes (from the objective criteria stated in the manuscript) does show a correlation, with wider plumes during stronger currents. I remade figure 4 using only plumes where $U_{mean} < 0.1$ m s^{-1} (see attached figure). It seems that only panel (a), u', changes in character. The same plume structure is present for both strong and weak currents. It could be argued that the plumes during strong currents should be disregarded, as they are more mixed away by the turbulence, but I think it makes more sense to include them. Even during strong currents, plumes are identified that contribute to the (inversely correlated) fluxes.

I believe a bit more connection to plume mechanics would help. There's a significant set of literature on this (e.g. reviewed by List 1982; Woods 2010). If Fig 4 is actually from a coherent plume structure then it would seem useful to reverse the plume equations to work out what is happening at the source? You have a width and a distance and a buoyancy anomaly? You could see if it is normal in a plume for the horizontal u' to be greater than the w' (and the u' width seems greater?)?

There are several issues that needs to be considered to apply plume equations to these observations, which is why I have decided to leave this out. First, the stratification between the source and the observation point is not known. Second, the plume equations assume a point source (somewhere behind the actual source), so knowledge of the source diameter is also needed, which is also unknown. Furthermore, the entrainment coefficient is also unknown. A fair treatment of these issues would require a thorough sensitivity study to the variables, and preferably a numerical simulation, which I consider to be outside the scope of this manuscript. If someone would like to perform such a study, I would be more than happy to contribute/share my data.

Pg 9: line 8 "supercooled" – might be good to clarify that this is brine-induced supercooling and tied to the plume source, as opposed to pressure-induced supercooling that might be found in ice shelf affected waters.

Agreed. I changed the last half of this paragraph to the following:
"For low drift speed, many of the plume observations carry water that is supercooled relative to the ambient. The supercooling is caused by the lower salinity-determined equilibrium temperature of the brine within the sea ice. Supercooling decreases with drift speed, and is not observed for plumes where the mean current exceeds 25cm/s, consistent with stronger mixing of the plumes during high drift speed. Plumes associated with high maximum heat fluxes are more often supercooled than not."

pg 15 line 10 "dissolve" not sure I'd use this term. They entrain and grow in scale but weaken in terms of buoyancy anomaly.

Agreed. Changed this to "...as one would expect plumes to gradually expand in size, but weaken in terms of buoyancy anomaly, with distance from the ice."

Please also note the supplement to this comment:
https://www.ocean-sci-discuss.net/os-2017-27/os-2017-27-AC1-supplement.pdf

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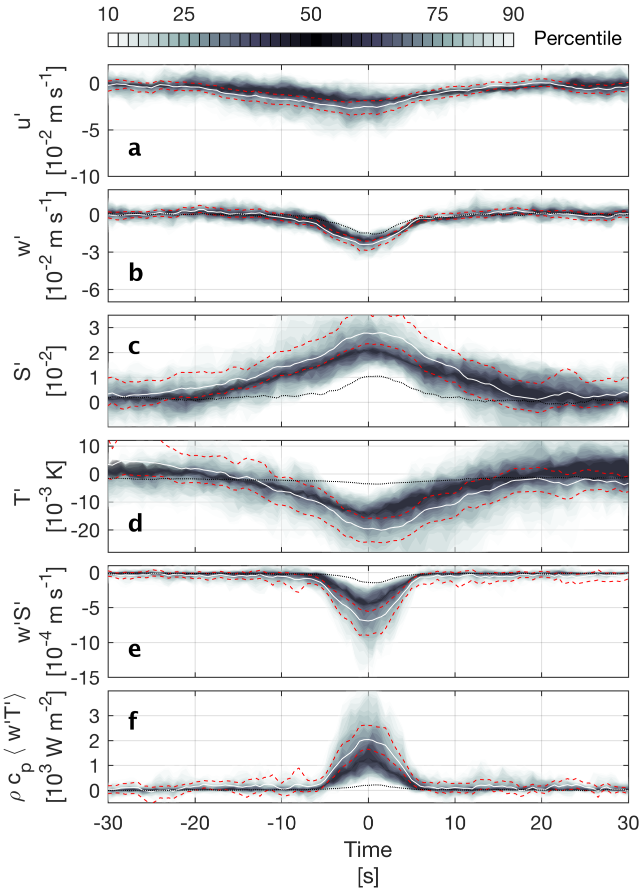
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Fig. 1.