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Ocean Science Discussions

Interactive comment on "Ventilation of the Northern Baltic Sea" *by* Thomas Neumann et al.

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First of all, we would like to thank the anonymous referee for the thorough review of our manuscript.

The common concern of all referees is that the amount of data collected during a cruise in the sea ice covered northern Baltic Sea is not sufficient to justify any of the hypotheses raised in the manuscript. Indeed, only three stations could be explored and we hardly can increase the number of in-situ observations. However, the referees proposed an option to save the manuscript by a complementary modeling approach.

We decided to follow this line and will perform a model simulation for the winter season 2016/2017. We will set up a model for the Baltic Sea with earmarked water masses allowing us to identify the origin of water which eventually arrives in the deep water of the Bothnian Bay. Nevertheless, we want to stress that recent ocean models are

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not able to reproduce a possible haline convection due to brine release. However, this approach will prolong a revised version of our manuscript and will include substantial changes.

In the following, we respond to the referee's specific remarks. Remarks are shown and followed by our response.

Review #2:

1.) The layer close to bottom at one station (station 9) is marginally interesting. Clearly one would have wanted more than one station to show the persistence of any suggested process. This water is high in oxygen, but also 0.5 deg C above freezing. This indicates that it is not related to sea ice formation, or brine driven convection.

Here we disagree with the referee. When brine mixes with surface water and vertically subsides, it preserves most probably the surface temperature. If the new water mass then horizontally spreads or moves downslope, it mixes with warmer deep water.

2.) So why is all the sea ice observations included? Really – it has no use as the paper is written at the moment. Obviously the authors would have liked to find evidence for the "brine hypothesis" – but they have not.

Yes, this was one of the motivations for the sea ice observations. We wanted to test the hypothesis whether brine contributes to deep water formation in the northern Baltic or not. We will formulate the hypotheses more pronounced in the revised manuscript.

3.) There is generally a small number of citations given. While it is good practice not to overflow with too many, here it is on the sparse side. And one suspects that the authors have spent a limited effort on finding relevant studies. A good example is for the experimental studies in polynyas (Page 2, line 5). Clearly there are many more observations available from polynyas, both in the Arctic and Antarctica. As noted by the other reviewer are also some literature on the down-flow required. Examples on earlier polynya studies are given at the end. In general is there also much more

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available studies of Baltic sea ice available, where the few seas ice samples could be compared to.

We will increase the number of relevant literature.

Specific Comments:

4.) Page 2, line 7. The Arcic is a name, should always be spelled with capital A. Correct throughout.

Will be done in a revised version.

5.) Page 3, line 6: Use of "Fast ice" is wrong. Fast ice means sea ice frozen onto the shore. Here I think you mean pack ice? https://www.jcomm.info/index.php?option=com_oe&task=viewDocumentRecord&docID=14598

Pack ice is the right term.

6.) Page 3, line 11 It is not clear how you sample the brine. You state that:" holes approximately half the depth of the ice thickness were drilled to collect brine". Do you mean that you take out the core, and wait for water to drain back into the whole? How do you know this is the brine? The brine salinity is very tricky to sample, and conditions here are very special with the super low surface salinities.

The brine sampling was like the referee assume. We will describe the sampling procedure more explicitly. We are quite sure that we sampled brine. What else could have caused the high, measured salinity? Of course, the sample could be polluted by snow. However, we took care to remove snow from the vicinity of the sampling station.

7.) Page 4 – lower 4 lines. You simply state you used the (standard) Guildline's Autosal 8400B and the accuracy. It is a standard procedure in the field.

Thanks for this hint.

8.) Page 4 –line 8: Are you sure you closed the bottles on the way down? With higher

pressures this would lead to the bottles imploding, so the standard is to do this on the way up.

Yes, we are sure; exception is the surface bottle because of possible air bubbles. Doing so, we prevent samples from the wake of the down cast. Pressure differences are not so large in the shallow Baltic Sea.

9.) Page 6 –line 7: What do you mean by; “CTD and salinometer measurements of the melted ice core water are very close and, therefore, the CTD measurements appear to be reliable”? How can you take a CTD measurement of the melted ice core? A CTD needs to be fully submerged in ocean water to work, and measures the conductivity over a much larger volume of water that is inside the conductivity cell. . .

We used the handheld (mini) CTD for these measurements. The conductivity cell is very small compared to the sample water volume. We will describe the procedure more explicitly.

10.) Page 6 – line 12; “The mean sea ice bulk salinity in the Bothnian Sea is about 0.6g kg-1”. This is a very strong claim when you have ice cores from 3 locations. . ...

We will reformulate this statement: - mean sea ice bulk salinity of the sampled cores -

11.) Page 8, Figure 4 caption: ice sheets – this means the large piece of ice on Greenland and Antarctica. You may mean “ice core”?

We will call it “ice core slices” to prevent misunderstanding.

12.) Page 10. Figure 7. The mini CTD observations appear close to the ship-born CTD. If they are plotted in the same figure – then one could see if there are any differences – but this appears not to be the case. This figure is not valuable – unless there are some significant differences – and then these should be shown in Figure 6.

We will check whether there are any differences. The measurements are not exactly from the same location. Mini CTD observations are directly from below the sea ice

while ship-borne observations are from open water and the surface layer could be disturbed by the ship itself. If there are no differences, Fig 6 can be withdrawn and we will mention mini CTD results in the text.

13.) Figure 8: Is this the ship CTD data? Why then is not the warmest water on Station 10 and 12 about +3 deg C visible? And

Since we are interested in the water body with oxygen concentration close to saturation, we excluded all TS data points with oxygen well below saturation.

14.) Page 12 – line 10. Please use one temperature throughout a paper. It is fine to use the new conservative temperature, but then you should use it throughout.

Here we use data from a satellite imagery product given in deg C. We will it either mention in the text or convert the data into conservative temperature by assuming a typical salinity for this region.

15.) Page 13 – line 1: "we do not have information on surface salinity or currents." This is exactly the main problem. Very little data is available, and then one cannot really conclude on the suggested processes either. A numerical model could have amended this in a nice way.

We intend to use model simulations in the revised version.

16.) Page 13 – line 6: Also here; "there are some indications that surface water from the Bothnian Sea have been mixed with Bothnian Bay water forming the observed bottom water at station 9". Some anecdotal indications are not really enough to claim that one has new findings worth publishing in an international journal.

See above.

17.) Page 14, line 11-16: While I am no expert in biological processes it is clearly possible that there is growth of organic mater in sea ice, and this should be discussed. A fairly new paper (Assmy et al 2017) also finds that phytoplankton can also grow below

a snow cover.

DCOM in the northern Baltic Sea constitutes of refractory, terrestrially gelbstoff in high concentrations. Autochthonous CDOM plays a minor role only. Therefore, CDOM can be used as a conservative tracer in the northern Baltic Sea.

18.) Page 14, line 18: polyniyas is spelled wrongly.

Will be corrected.

19.) Page 14, line 17 – Page 15, line 7. While this is possible in the Bothnian Bay – you do not have any observations that indicate that this is going on. IF you added some simulations that this is likely, then this text could remain – otherwise it should be deleted.

We will check when the model simulations are done.

20.) Page 15, line 8 – 20. This section finally contains some calculations about the brine water "hypothesis". The calculations appear OK - but does not use a proper range in forcing and boundary conditions. How representative is the 0.2 m of ice thickness? Is there any freshwater discharge during winter?

In the revised text, forcing and boundary conditions will be checked carefully and justified.

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