Ocean Sci. Discuss., 7, C704–C710, 2011
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Ocean Science Discussions

Interactive comment on "Measurements of bubble size spectra within leads in the Arctic summer pack ice" *by* S. J. Norris et al.

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1) *p. 1744, Ln 3-5, Text starting with 'Bubbles appear..." and ending with "...such as algae." It would be good to show an image where images of bubbles and algae are clearly distinguished. Could be included as Fig. 3a. Or, you may cite a paper where such images have been shown. In Ln 25 'pronounced spike in the size spectra" also would be good to show a size spectrum with such spike(s). As your measurements are in such a quite environment, the problem of multiple counting would be an issue specific for this kind of measurements. To my knowledge, bubble size distributions suffering from multiple counting have not been published. So, it would be instructive to demonstrate multiple counting to the readers. This figure could be Fig. 3c (the current Fig. 3 would become Fig. 3b).*

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Interactive Discussion

Discussion Paper

We have added both sample images of marine biota and bubbles (new figure 3). Also a separate figure showing an example of a contaminated bubble spectra before and after correction for multiple counting (new figure 4).

2) *p. 1746, Lns 5-8: The correlation coefficients reported here are relatively low; it is fair to state in Ln 6 "...are weakly correlated both..."* Applied.

3) *p. 1746, Ln 23, "no bubbles were observed larger than 560 micrometer". Make a point that this is well below the upper detection limit of the instrument.* Applied.

4) *p. 1747, Ln 25, 'a slight bias'. Here 'slight' is a qualitative descriptor, could you show a number?* Applied.

5) *A comment and suggestion: You restrict your discussion here to the transfer of organic matter to the atmosphere as sea-spray aerosol. I believe that the discussion on the chemical composition of those particles could be expanded with the following. You mention (in the Introduction, p. 1740) that the sea-spray aerosol particles are hygroscopic thus effective as CCN. This is mostly true for particles containing sea salts, not so much for particles with large organic fraction (a reference should be cited). Because Bigg and Leck et al have reported mostly organic sea-spray particles, the claim for CCN effectiveness of bubble-mediated particles is somewhat diminished. Meanwhile, strong depletion of ozone in Arctic from reactive halogens is well documented (e.g., Gilman et al., 2010 and the reference there in). As we all know, sea-spray aerosol particles from breaking waves in open ocean are relatively large and they cannot be transported far (a reference should be cited). Even if transported, they would age and be already de-halogenated (a reference should be cited) when reaching Arctic. Though there are other sources of reactive halogens in Arctic (a reference should be cited), the sea is perhaps the main source. So, the halogens that deplete Arctic ozone should come from bubbles like those you observe. But, again, Bigg and Leck et al papers say bubble bursting in Arctic produces many small, often purely organic particles. How to reconcile the facts that there are sea-salt aerosols providing reactive halogens in Arctic and they*

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are able to act effectively as CCN because of their hygroscopicity, but such particles are not readily observed? Perhaps the observations until now show large organic fraction and less sea salts in the sea-spray aerosol because the de-halogenation proceeds faster in Arctic than in other places (note that this is an opinion of a nonchemist) and is difficult to observe the initial stages of the sea-spray aerosol. Whatever the reasons (perhaps they need to be discovered), the point is that you may speculate (no enough data for clear deduction) about all these open questions; either in a paragraph following the first one in Section 4.1 or in an additional brief Discussion section."

Speculation on the chemical composition of the aerosol and their potential as CCN is not really appropriate for this paper and is not the purpose of the paper. The introductions review of Arctic aerosol and CCN is included to put the present interest in Arctic bubbles into context. The introductions review already addresses some issues raised by the reviewer about CCN effectiveness. At the end of the introduction we state: "Our purpose is to evaluate the bubble measurements only; we do not attempt here to assess any of the – very extensive – aerosol measurements made during the field campaign. Detailed studies of the physical and chemical properties of particles sampled from the air (below, within, and above cloud), the ocean surface microlayer, and from collected fog/cloud droplets are underway and will be presented in future papers."

6) *'Substantial' is a subjective and descriptive; better use a number.* Applied.

7) *In Fig. 7, I see a clear linear increase with time for both groups of data, open and covered surface (solid and 'white dot' circles) within the range of their daily variability (small dots). None of the variables in Fig 4 show systematic change in time. Fig. 8 (aside from your discussion of the changes of N for positive and negative heat fluxes) shows linear trend for both data groups when the flux changes over the full range of values is considered, from less negative to 0 to more positive. This hints that perhaps over the period of the experiment the atmospheric stability changed systematically in time. If so, perhaps a graph of deltaT in Fig 4 will be useful; currently you say 'not shown' (p. 1747, first paragraph). I am not sure how significant this change in time*

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is, but in my view it is clearly seen; something changed systematically with time. In connection with this, I would like to add this comment. When the effect of environmental conditions (e.g., atmospheric stability, surfactants, etc.) on whitecaps is discussed, usually the sentiment, rightly, is that the effects of these conditions are important for low to moderate winds (3-10 m/s), while at higher winds these effects are wiped out. If your data show clear correlation between total N and deltaT, they present evidence for the importance of the environmental conditions to bubble concentrations and indirectly to particle production. In the view of the latter, your data could be considered an extension of Mårtensson et al. (2003) study (which focused on the effects of SST and salinity on particle production) in two aspects: (i) adding the effect of atmospheric stability on particle production; and (ii) confirming in the field implications derived in laboratory investigation.

The reviewer notes that there is a discernable general increase in bubble concentration with time for both groups of data – open water and ice covered; but cannot discern any such trends in the mean conditions in figure 4 (old figure numbers). We are not convinced that the time trends are significant in themselves, see figure 7 (old figure numbers) in the discussion paper. For the open water cases there is almost no increase between August 20 and 27 and then a decrease to August 28 – the trend is not a well defined and robust linear increase. If August 31 is excluded the trend is less than the scatter, and entirely unconvincing. In the ice-covered cases the trend is a little more convincing, but we should separate August 17 and 18 from the others since the flow comes from below the main ice floe rather than the lead, and thus has a very difference recent history. Furthermore, there IS a strong trend of decreasing air temperature with time visible in figure 4c (old figure number) – though slightly disguised by the cold period around August 21-22. This is associated with the transition from summer melt to autumn freeze up (see figure 1 below). The reviewer suggests that the data 'hints' that 'atmospheric stability changed systematically in time' and might be important. This is exactly what is shown by the relationships with surface heat flux – which is determined largely by the surface-air temperature difference, and thus increases as Tair decreases

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(see figure 2 below). The range of Twater is just 0.032 °C, from -1.688 to -1.72 (during bubble sample periods), a negligible variability. This makes a plot of deltaT against time somewhat redundant since all the significant variability results from Tair (ranging from -0.02 to -7.47°C) which is already shown. Given the relatively short fetch over the lead, the local heat flux rather than a more general atmospheric stability parameter is likely to be more important since the internal boundary layer directly influenced by the lead surface may be only a few metres deep. The discussion in section 3.2 has been extended to clarify this issue.

Interactive comment on Ocean Sci. Discuss., 7, 1739, 2010.

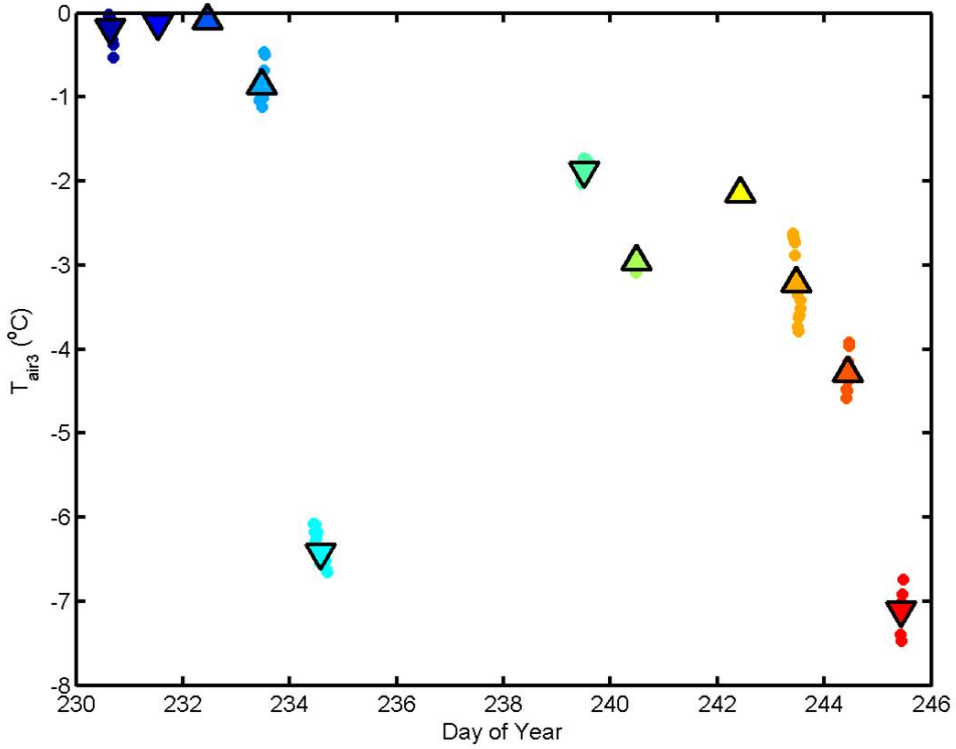

Fig. 1. Time series of the air temperature just for the periods of bubble mearsurements.

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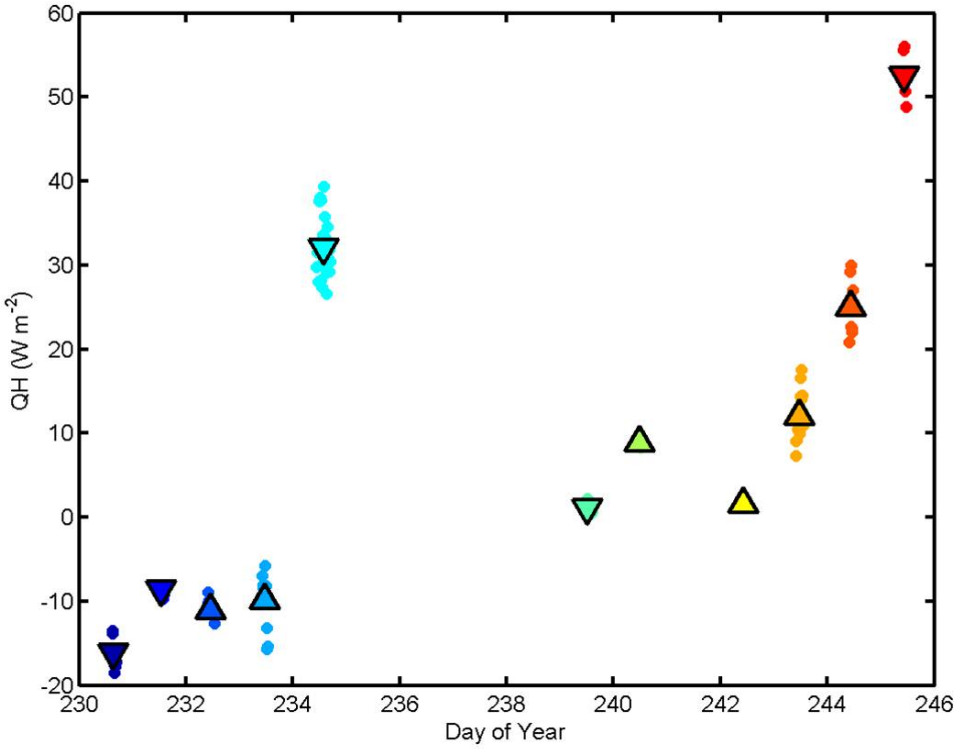

Fig. 2. Time series of the surface heat flux just for the periods of bubble measurements.

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