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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.

Anonymous Referee #1

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Summary:

The manuscript describes measurements of bubble population in open leads in Arctic with video-imaging system. Size distributions of bubbles produced by processes other than wave breaking are presented for the first time. As much as the collected data allow, the influence of the environmental conditions on the bubble concentrations is investigated. Implications on aerosol production and mechanisms of bubble production are discussed.

Significance:

It has been known for a while that breaking of water waves is only one of several possible processes (namely, raining, snowing, supersaturaion, marine biota) producing

bubbles in the ocean and that those other mechanisms produce smaller bubbles (e.g., Blachard and Woodcock, 1957). These other mechanisms create a bubble population (sometimes called background) different from the transient bubble population which breaking waves create. Being of interest for air-sea interaction processes, and especially for the generation of sea-salt aerosols, the transient bubble population was extensively studied and documented. Much less attention was directed to the background bubble population and the particles they might create. The series of papers published by Bigg and Leck and co-workers mostly in the last decade started to change this status quo. Bigg and Leck group showed that bubble bursting generates small, often purely organic particles, but this cannot be reconciled with the particle production by bubble bursting in breaking waves where turbulence of the flow would ensure the production of mostly sea-salt particles. One possible explanation this reviewer saw while reading Bigg and Leck et al papers was that though the same bubble-bursting mechanisms (jet and film droplets) created the particles which they observed, the bubbles that burst should be different from the breaking-wave bubbles. Norris et al manuscript provides experimental evidence for a bubble population which has concentrations comparable to those observed in open ocean but created and sustained by processes different from wave breaking. This is a significant new result.

Evaluation:

The manuscript presents new data which are in the scope of OS journal and will be of interest to the oceanographic, air-sea interaction, and aerosol communities. It is concise and clearly written. Thus I recommend that this manuscript be accepted for publication. Some suggestions, questions, and technical remarks follow.

Comments and questions:

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).

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..."

3) p. 1746, Ln 23, 'no bubbles were observed larger than 560 μm". Make a point that this is well below the upper detection limit of the instrument; this is indeed a physical observation, not instrument artifact.

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

5) p. 1748, first paragraph in Section 4.1: 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 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 non-chemist) 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.

6) p. 1750, Section 5, second sentence "Substantial number of bubbles...': 'Substantial' is a subjective and descriptive; better use a number. Suggest starting the sentence with "Under low winds (up to X m/s) and in absence of breaking waves, substantial number of bubbles (up to XX) were found...'

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 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.

Minor suggestions:

1) A few additional references:

p. 1740, Ln 22, 'highly hygroscopic' needs reference, e.g., Tang et al. (1997).

p. 1740, Ln 22, 'act as very effective cloud condensation nuclei' needs reference, e.g., Andreae and Rosenfeld (2008).

p. 1740, Ln. 23-4, the statement "The radiative properties....depend strongly on their droplet size...' needs reference.

2) The authors often use the adjective 'very', sometime in place, sometimes redundantly and sometimes imposing interpretation/opinion. It is better to state a fact (or give a reference) and leave to the readers to decide for themselves if it is 'very' something or not. Examples where 'very' should be removed are:

p. 1740, Ln 22, 'very effective' (opinion, the facts in a reference would be more convincing)

p. 1741, Ln. 3, 'very poorly represented' (redundant, 'poorly' is enough)

3) Acronym ASCOS. First mentioned in the body of the manuscript on p. 1742, last line without spelling it (the spelling in the Abstract does not count). Meanwhile its first introduction in Section 2.1 (p. 1743) is the right place. Suggest avoid using 'ASCOS' on p. 1742, change it to say 'made during the field campaign' or "field experiment' or the like.

4) Punctuation: Comma is needed when starting a sentence with a weak clause (Rule 9 here www.grammarbook.com/punctuation/commas.asp). Examples in the text:

p. 1743, Ln 7, remove the comma before 'between' and add comma after '2008'.

p. 1746, Ln 18, add comma after '200-560 μm'.

p. 1747, Ln 23, add comma after 'able'

p. 1747, Ln 26, add comma after 'ice-free'

Please, check the text for other cases.

5) p. 1749, the title of Section 4.2: Suggest change to "Bubble production mechanism" to clearly distinguish from "aerosol production" in the title of Section 4.1

6) p. 1753, you may already know, but just in case note that reference Fuentes et al, 2010b is already published, update from ACPD to ACP.

7) Figures

Fig. 1: Suggest adding '(blue line)' after 'The ice edge" in the second sentence.

Fig. 3: Comment 1 above suggests adding Fig 3a and Fig 3c with the current Fig 3 becoming Fig. 3b.

Fig. 5: PL96 should be PL98. With the current symbols, it is difficult to distinguish: i) open ocean vs surf zone data; ii) different orientations of the triangles, especially when placed on curves. To avoid both problems, I suggest use of open (for open ocean)

and solid (for surf zone) symbols. Then you may repeat some symbols (circle, square, diamond, triangle) and avoid triangles with so many orientations.

Fig. 6: Suggest adding '(thin lines)' after 'bubble size spectra' and adding '(thick lines)' after 'their means.' This will remove any guessing for the reader.

References

Blanchard, D.C., and A.H. Woodcock (1957), Bubble formation and modification in the sea and its meteorological significance, Tellus, 9, 145-158.

Andreae, M. O., and D. Rosenfeld (2008), Aerosol-cloud-precipitation interactions. Part 1. The 2070 nature and sources of cloud-active aerosols, Earth-Science Rev., 89, 13–41.

Tang, I. N., A. C. Tridico, and K. H. Fung (1997), Thermodynamic and optical properties of sea salt aerosols, J. Geophys. Res., 102(D19), 23,269–23,275, doi:10.1029/97JD01806.

Gilman, J. B., Burkhart, J. F., Lerner, B. M., Williams, E. J., Kuster, W. C., Goldan, P. D., Murphy, P. C., Warneke, C., Fowler, C., Montzka, S. A., Miller, B. R., Miller, L., Oltmans, S. J., Ryerson, T. B., Cooper, O. R., Stohl, A., and de Gouw, J. A.: Ozone variability and halogen oxidation within the Arctic and sub-Arctic springtime boundary layer, Atmos. Chem. Phys., 10, 10223-10236, doi:10.5194/acp-10-10223-2010, 2010.

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