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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Wintertime Arctic Ocean sea water properties and primary marine aerosol concentrations" *by* J. Zábori et al.

Anonymous Referee #2

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The paper presents a series of interesting laboratory measurements on the effects of bulk water properties on the generation of sea-salt aerosol by bursting bubbles. The results from two different types of conditions are presented (with the water warming or cooling for several different types of seawater samples). The results are presented on a mainly phenomenological basis, and there is little attempt to relate the observed changes in aerosol production rates between the various experimental conditions and seawater types to fundamental physical or chemical processes. This is especially frustrating since it seems (to this reviewer at least) there were two significant oversights in terms of supporting measurements. First, the authors attempt to correlate bubble and particle production to simply the dissolved oxygen concentration. However, they neglected to measure the total gas tension, or sum of the dissolved oxygen and nitrogen concentrations, which is probably more relevant to bubble production than DO. For

example, there have been laboratory measurements in a tipping bucket tank (which is similar I think to the weir system used in the experiments discussed in the manuscript) that have shown bubble production for the smaller bubble size ranges (which presumably would be more important for particle production since they dominate the number-size distribution) is a function of total dissolved gas, not just dissolved oxygen (Asher and Farley, 1995). That the authors did not measure nitrogen concentrations is no excuse for ignoring the role of dissolved N2 and O2 on bubble formation.

A second problem is that the authors claim that the physical properties of the seawater (surface tension, viscosity) dominate the observed changes in aerosol production rates (see page 16108, lines 11-14). However, they provide no justification for this assumption in terms of direct measurements of those properties showing they are at least correlated with the changes in aerosol production. Although it is relatively easy to envision heuristic models for why the two are related to aerosol production (thinking along the lines of the bubble film drainage rates that ultimately lead to the bubble fracturing and generating aerosol particles), it would be interesting if the authors could provide some concrete numbers to these assumptions in terms of calculated effects of viscosity on the drainage rate. There is probably a wealth of information on the subject in the chemical engineering literature.

Finally, the authors provide some very simple conclusions on how their measurements will impact aerosol production in a warming arctic. However, they completely ignore the fact that whitecaps are integrally related to this process, and it is known that sea surface temperature also affects whitecap coverage at a particular wind speed. Furthermore as the huge anomalous arctic storm the week of August 9, 2012 demonstrates, wind speed over the Arctic Ocean may be far more important as a cause for aerosol variability than small changes in production rate as a function of water temperature.

Overall, the results in the paper are interesting and in general support the conclusions from previous studies in this area. As such they represent a new data point but the paper as written only marginally advances basic understanding of how changing envi-

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ronmental conditions will affect sea salt aerosol generation in the arctic. If the paper could be strengthened by addressing the summarized points below it would be suitable for publication.

1. The authors did not measure the partial pressure of dissolved N2 along with dissolved O2. Both are important for bubble formation in weir bubble generation.

2. There should have been some attempt made to constrain the effect of naturally occurring surfactants on bubble populations. If surface tension is an important parameter as the authors claim, surfactants have a much larger potential effect than temperature and the effect should be accounted for, rather than assumed unimportant.

3. The authors might want to review the modeling work of Jaeglé et al. (2011), who modeled global distributions of sea salt aerosols, including high latitudes.

4. Relative humidity is very much lower than in the tank than in the atmosphere, 10% in the tank versus maybe 50%-60% in the arctic. Since the rate at which the bubble film thins is related to when it breaks and therefore how much aerosol is generated (and the size range of that aerosol), there should be some discussion of how RH is related to the measured size distribution.

5. No discussion of the relationship between water temperature and whitecap coverage, and how changes in whitecap coverage might be of more relevance in a warming arctic than small changes in bubble or aerosol populations.

References: Asher, WE, and PJ Farley, 1995, Phase-Doppler anemometer measurement of bubble concentrations in laboratory-simulated breaking waves, J. Geophys. Res. 100: 7045-7056.

Jaeglé, L, PK Quinn, T Bates, B Alexander, and J-T Lin (2011), Global distribution of sea salt aerosols: New constraints from in situ and remote sensing observations, Atmos. Chem. Phys., 11, 3137-3157, doi:10.5194/acp-11-3137-2011.

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