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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Comparison between summertime and wintertime Arctic Ocean primary marine aerosol properties" *by* J. Zábori et al.

Anonymous Referee #2

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The authors present an accompanying study to Zabori et al., Atmos. Chem. Phys., 12, 10709-10724. The paper of discussion now tackles the relationship between sea surface temperature and primary aerosol production at a site at Kungsfjorden at Svalbard. However no direct atmospheric studies but an atmospherically representative experiment was chosen to be able to intercompare different sea surface compositions and temperature using particle free air and a bubble burst creator. They transport water from different places within and around Kungsfjorden into this temperaturized (28 degree Celsius) PET volume for intercomparison of Western Svalbard Current (WSC) waters, Kungsfjorden waters of different depths and glacier run-off water at different seasons, i.e. winter- and summertime to test if the results match for the same liquid phase temperatures of the original water. To do so, they observed the particle size distributions by two instruments (DMPS and OPC) because of different particle size

ranges, averaged their observations and normalized these. The resulting median particle diameters of the distributions were plotted versus the sea surface temperature and a hopthesis with respect to climate couplings between sea surface temperature and CCN productivity is proposed.

In general I do like this approach and the work of the authors quite much and the article is well structured. Sometimes the English displays some grammatical challenges that should be corrected using a good spell and grammar checking program (not Microsoft Word for sure!).

However there are some points worth clarification and discussion before this study desires publication in Atmos. Chem. Phys. Those are:

* The authors use two different aerosol particle sizing instruments, i.e. a DMPS for sub 0.3 micrometer particle sizes and an OPC for the range above. However those do not seem to have been matched. p. 31162, 2nd para.: "The first two OPC bins were not used in the analysis of the winter data, and subsequent calculations, but are presented for completeness. There is evidence that for the overlapping size range of the DMPS and OPC instruments, the DMPS measurements provide higher quality data." Why is that? One questionable assumption for the particle instruments is the particle density, which requires a certain assumption for all particles, although each particle mass may display a different one. I am not totally sure that coarse particles display the same as submicron ones. The aspect of water content is as well critical. Was the water removed or is this a cocktail out of aerosol particle and water? Is there a method discriminating the water droplets from particles? Since both instruments use different inlet systems, dilutions and temperatures this might affect the overlapp, too.

* The focus has been set to produced aerosol number concentrations, but the information on volume or mass is missing. This is questionable, since aerosol dynamics might have transfered the particles formed to larger sizes, i.e. the same amount of water soluble material but a smaller amount of particles formed. The aerosol dynamics has

been removed in a variety of scaling and normalizing steps, which are certainly helpfull to make the big variety of size distributions comparable. Nevertheless this point might impact on the very hypothetic conclusions drawn.

* Have the authors tried Gaussian fits to the size distributions measured? This would provide additional information to the median diameter, which is certainly a valuable information but the mode width might provide additional information.

* Different flows and heights of inflow during summer- and in wintertime: What about the influence of this on the results? Are there any tests about this aspect?

* Figure 3: Two points: (1) Why is there an "outlier" for 6 degree Celsius in summertime (plots b and c)? Have the datasets been checked for outliers? Since commonly multiple charge corrections are applied this feeds back into the results of other sections. (2) I can see a decoupling of larger and smaller particle numbers during summer with no clear temperature dependency during (subplot d). Perhaps this results of the inflow of glacier water, vertical mixing inside the Kungsfjorden or an additional source during summertime for smaller particles. Actually the results stated in the text (p. 31163, top paragraph) should be provided with standard deviations. Checking the summertime values it seems both values could match still within their uncertainty range.

* As pointed out by a different reviewer there are some curiosities among the size distributions shown: (a) In Fig. 4 each winter particle size distribution shows an "outlier" at around 280 nm in diameter. This is strange and would point on a certain primary particle source of a distinct size especially if it is being observed for four different experiments. (b) It comes even more strange in this figure to see an identical particle size distribution for all four cases in wintertime (hypothesis of mine) with every small up and down reproduced in every of the subplots. The feature is a little bit hidden in subplot a) as the summertime values display a higher magnitude. Has one mean measurement been used for all of the intercomparisons? This would explain parts of the results and appears even more pronounced in Fig. 5.

* Fig. 6: What is the way of errorbar calculation, i.e. standard deviation or counting error? This applies in detail for the DMPS errorbars beacuse of the small charged fraction of particles.

* (Comment) Figs. 7 and 8: It is a good idea to plot the particle size distributions for different water temperatures of the original samples. The striking difference between the behaviour during winter and summertime is worth more discussion with respect to the implications for the presented hypothesis. One would expect similar behaviour.

* This indicates that the total feedback loops are more complex than captured so far by a simple temperature based particle production. The authors really discuss their results in a good way. However the hypothesis is still fragile. Cerainly I agree with the authors that more research is needed and that this paper presents a hypothesis to work on, but the present data delivers only indications but no final proof, since studies have been conducted in an arbirary environment (PET volume) with filtered air.

* What happens if there is an additional feedback loop including rising atmospheric background particle numbers with temperature that originate elsewhere and which are transported towards the site of interest? Their prospective lifetime would be sufficiently long. What about the size of air bubbles with rising temperatures? Size the solubility changes with temperature, those may become larger and compensate partially the observations being made. What about the surface wind speed initiating the wave breaking and the sea salt particle production (Table 1)? Does this force more intense vertical mixing during winter or what about its impact on the conclusions drawn?

* I evidently miss the inclusion of the chemical informations of the former article of the group (Zabori et al., 2012) that provides salinity, dissolved organic compounds etc. Do these parameters change with sea surface temperature, capable in explaining the observations? Therefore I'd like to see a broader discussion of potential effects if such a hypothesis is being concluded.

I am aware of the multitude of comments made. The study is of notable interest and re-

quires detailed checks since a feedback loop has notable impacts on the Artic climate. As a summary it can be concluded: The article is worth to be published but only after carefully checking the points mentioned.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 31153, 2012.