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## ***Interactive comment on* “Potential source regions and processes of the aerosol in the summer Arctic” by J. Heintzenberg et al.**

### **Anonymous Referee #1**

Received and published: 7 May 2015

The paper by Heintzenberg et al. tries to uncover potential aerosol source regions by using clustering techniques. It is an informative paper capitalising on numerous research cruises into the Arctic ocean on board the icebreaker Oden. The Arctic Ocean is an important region both due to proximity of Northern Hemisphere land masses all of which contribute to aerosol pollution perturbing otherwise pristine marine environment. At the same it is quite difficult to separate those contributing sources due to their diversity and intersecting air masses. The paper would be suitable for publication in this journal after addressing rather numerous comments. Last but not least, English of the manuscript makes following the text rather difficult.

Major comments

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The main weakness of the paper is that it lacks clarity enormously throughout. Even the title is not very precise as the paper presents data not just of particles, but precursor gases as well (DMS). The whole paper is in-concise, lacking focus and containing weakly supported statements. Overall, the paper discusses source regions more than aerosol processes which are often implied or invoked.

The third sentence of the abstract states about identification of five source regions and three aerosol types, but they are not listed/named. Instead there are long passages discussing selected sources or processes. The main hypothesis at the end of the abstract is badly worded: the long travel time over pack ice and?? open water cannot control formation of ultrafine particles, but instead authors are probably arguing for fragmentation process taking place. Moreover, identifying a specific region with distinct aerosol size distribution does not necessarily mean a source region, but instead may indicate certain processes taking place in those specific regions or en-route to them: BL dynamics, aerosol activation and deposition, nucleation, fragmentation, primary , secondary production, so on and so forth.

Introduction is supposed to be a brief summary of the latest findings and experimental techniques to be developed, but instead there are lengthy paragraphs arguing in favour of already published peer-reviewed papers. Many sections of the introduction should be moved to discussion while a short summary of relevant results should be mentioned in the introduction. No overview of clustering techniques is provided in the introduction despite the fact that clustering algorithms are numerous, results abound and they are quite central to the paper.

Line 93. What inconsistency the authors are talking about? Statistical interpretations arise from analysing direct observations, so that is one and the same. The derived result cannot contradict the original, otherwise something is wrong with the statistical technique.

Line 108. How can DMS directly condense on the particles? DMS derived products

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like SO<sub>2</sub> or H<sub>2</sub>SO<sub>4</sub> can either directly or through cloud processing (aqueous reactions) become incorporated into droplets.

Using 5day long trajectories is quite inaccurate when it comes to their origin. Typically, trajectory uncertainty can be anywhere between 15-30% of the travelled distance ([http://www.arl.noaa.gov/faq\\_hg11.php](http://www.arl.noaa.gov/faq_hg11.php)) and consequently travel time over pack ice or open water highly uncertain too for trajectories of e.g. 1000km or longer. While the authors acknowledged the uncertainty (without reference, only by assumption) there is no discussion about the implications on the time trajectory spent over water or pack ice.

Section 4. The section title is missing clustering type (trajectory, I guess). This section demands that the title of the paper is modified to include “gaseous aerosol precursors”. Despite obvious connection of DMS with aerosol particles there is no discussion of that relationship. Incidentally mentioned Tables 2&3 contain relevant information of particle size distribution clusters, but these are not discussed in connection to DMS while they should be.

Section 5. The section starts with optimistic note that clustering worked, but missed to name them accordingly which makes the following text difficult to follow. There is little discussion of the observed differences between the clusters. For example, can the difference between the number concentrations of clusters 4&5 be at least partially attributed to anthropogenic activities? And many other similar questions: when the particles are called aged (line 487) are they biogenic or anthropogenic and which substances exactly became aged?

Section 6. For comparing size distribution data between ice-breaker and e.g. Zeppelin station it is imperative to have a connected Lagrangian flow. Was the time lag applied considering the distance between the two sampling points? If not, spectral differences are difficult to interpret as to what was the cause and the outcome rendering any connection to aerosol processes. The whole section needs much more careful wording as

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to not overstate the findings.

Conclusions and synopsis should not include lengthy discussions with references to the Figures and Tables as those sections belong to discussion. I suggest breaking section 7 into two: Discussion (7) and Conclusions (8). The latter will summarise the findings and will inform the abstract which is very loose at the moment.

Minor comments

Line 62. I don't understand the sentence "A plume to be entrained. . .is brought down to its top".

Line 65. The sentence belongs to discussion and above all is highly unclear. There are numerous papers demonstrating traces or a more significant pollution carried to the Arctic environment. Not measuring light-absorbing carbon particles was due to the lack of sensitivity of measurements? Even in a far more remote Antarctica there are measurable levels of light absorption.

Line 84. Please correct "presence of bubbles" to "bubbles generated by wave breaking/air entrainment". Bubbles are not just present in water they appear there.

Line 97. The sentence starts with "the same. . ." when biological processes were not discussed previously.

Line 118. The sentence refers to unspecified time period. New particle formation events do not occur as an increase in particle concentrations, but rather manifesting themselves as an increase.

Section 2 Database should be renamed to "Sampling techniques on ice-breaker Oden" as there is no database mentioned here.

Line 215. Please specify data cleaning procedures. Why was it necessary if sampling section refers to pollution controller?

Line 445. The ice maps are called "controlling factor" without proving it first. Controlling

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factor may be ok in the conclusions, not at the start of discussion.

Line 630. How anything measured during different times can confirm? The observations may be indicative or supporting, but not confirming.

Line 634. There is no inconsistency when the measurements do not agree with the mechanistic model, but rather point to knowledge gaps.

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Interactive comment on Atmos. Chem. Phys. Discuss., 15, 8429, 2015.

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