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Interactive comment on “The Arctic Summer Cloud-Ocean Study (ASCOS): overview and experimental design” by M. Tjernström et al.

Anonymous Referee #1

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This is an overview paper for the Arctic Summer Cloud-Ocean Study (ASCOS), a campaign held in late summer 2008 to study relationships between ocean composition, aerosol, clouds, radiation and meteorology in the central to high Arctic. The measurements were primarily taken from the Oden icebreaker when tied to an ice flow at 87 N. In addition, nearby measurements were made/attempted at an open lead, on the ice flow, from a tether sonde and from a helicopter. In many respects, the campaign was a tour-de-force with an extremely extensive set of measurements attempted, and the organizers should be given substantial credit for that. Measurement campaigns of this nature are not straightforward to pull together, and my impression is that this one was very successful.

That all being said, I have comments on the overview paper that will hopefully

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strengthen it and give it better balance. To start, it strikes me that a paper of this type could be written in two ways. Many overviews attempt to be all encompassing, consisting of overall campaign goals, logistical details, lists of measurements made, and a summary of all the findings. The other approach is to attempt to synthesize the new science into a coherent story. It strikes me that this paper attempts both approaches, and problems arise with each.

Specifically, as a complete overview, the paper does a good job of describing the goals of the campaign and of the philosophy behind the types of measurements required and the logistical details, such as choosing the ice flow to attach to. I found this philosophical approach quite refreshing and believe it will be valuable to others planning such campaigns. In an Appendix it also describes the measurements performed in a lot of detail, which will be useful as reference for future papers.

However, I find the paper does a patchy job of summarizing the science. While the meteorological (7.3) and aerosol-cloud interaction sections (7.4) were nicely written, the entire discussion of the aerosol chemistry and physics (7.2) is focussed on primary oceanic particles whereas no attention is given to an overall discussion of the aerosol present at the site. Likewise, there is little discussion given in the ocean chemistry measurements to the biogeochemistry of the system, and instead it is only bubble size spectra and turbulent mixing that are discussed in the water column section (7.1). The Discussion section is similarly weighted, with much discussion about the primary organic particles. While I do not dismiss the novelty of the bubble measurements and detection of EPS substances in aerosol and cloud water, the paper does not justice to the field by ignoring the other aspects of the science. What is particularly bothersome is that there are some results from this study that point to the primary particles of oceanic origin NOT being the only source of CCN material. In particular:

i. The aerosol flux studies of Held et al. (2011) described in the paper indicate only a small source from open leads.

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ii. The work of Leck et al. (Size resolved airborne particulate polysaccharides in summer high Arctic C. Leck, Q. Gao, F. Mashayekhy Rad, and U. Nilsson Atmos. Chem. Phys. Discuss., 13, 9801-9847, 2013) has indicated that measured polysaccharide levels do not fully match the observed levels of organic aerosol measured during ASCOS).

iii. The work of Chang et al. (Aerosol composition and sources in the Central Arctic Ocean during ASCOS, R. Y.-W. Chang, C. Leck, M. Graus, M. Müller, J. Paatero, J. F. Burkhardt, A. Stohl, L. H. Orr, K. Hayden, S.-M. Li, A. Hansel, M. Tjernström, W. R. Leaitch, and J. P. D. Abbatt, Atmos. Chem. Phys., 11, 10619-10636, 2011) has evidence for a continental signature in the aerosol sampled during ASCOS, in addition to a long-range biogenic aerosol source with MSA as a major component. This paper should be discussed more in the overview as it is the only paper that I am aware of that assesses the source of air to the site during the campaign. As written, the overview implies there is no transport of aerosol to the site (for example, top of page 13579) but that is apparently not the case, given the results from Chang et al.

iv. Nucleation events were observed during ASCOS, when aerosol particle concentrations were low (page 13580, line 3). This is clear evidence that atmospheric nucleation of new particles was occurring, i.e. a primary source from the ocean would not be dependent on pre-existing aerosol surface numbers.

v. The CCN closure experiments indicate that the organics present in the particles are insoluble (section 7.2), i.e. if primary organics are present then they are not CCN active, counter to the general emphasis of much of the paper. In fact, the belief is that the organics are likely sugars, which would be expected to be highly soluble.

Instead, I recommend a balanced presentation of the results for primary oceanic particles, alongside a discussion of the evidence for in situ production of sulfate and MSA from DMS and the role of aerosol transport from lower latitudes. If this is not possible within the scope of the overview, then I recommend pulling out the science conclusions

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entirely and leaving the paper as a technical overview of the campaign. A second paper could be written on the sources of aerosol to the ASCOS field site.

Finally, I found it exciting to read through the Appendix about all the novel instrumental approaches that will have information on the nature of the aerosol present, such as the radioactive tracers, single particle studies, thermal analysis of aerosol volatility, ATOFMS, etc. However, scant results from these instrumental approaches were presented. Why not?

Additional Specific Points

1. Introduction – very nicely written. Indeed, the writing style in the entire paper is excellent.
2. In Section 3, where the types of measurements required is discussed, more emphasis should be given to gas phase studies. Indeed most aerosol scientists (and I put myself into this category) often don't give the gas phase the emphasis it deserves. If the goal is to really determine the balance between primary oceanic particles and secondary atmospheric particles formed from DMS (or isoprene, e.g.) oxidation, then the gas phase precursors AND oxidants have to be measured in situ. If there is a weakness to the instrument suite on the Oden, it was the lack of emphasis given to these gas phase measurements.
3. In Section 4, the measurements from the helicopter could be downplayed as it turns out there were not so many made. Also, I found the final few paragraphs in this section about pollution from the ship and the operating conditions to be too lengthy.
4. It is said in Section 5 that fog was frequently encountered. Given the aerosol processing that fog will do, I would have been interested to read about the potential effects that it could have on aerosol processing, but this was not discussed.
5. I really liked the work of Mauritsen et al. described in section 7.4. It is nicely summarized here and this might be one of the major findings from the ASCOS study.

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 13541, 2013.

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