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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Contributions from DMS and ship emissions to CCN observed over the summertime North Pacific" *by* L. Phinney et al.

Anonymous Referee #2

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In this paper, sources of CCN in a remote marine environment are investigated based on high-time resolution chemical and physical aerosol measurement. The investigation clearly goes beyond earlier studies on this topic, so I consider this paper as a useful contribution to scientific community. The manuscript is scientifically sound and well written. In spite of its good quality, a few minor issues should be addressed before its publication in ACP.

Scientific comments

The authors make a strong point that during their measurements, there was one day during which marine boundary layer (MBL) nucleation contributed to CCN formation and one day during which organic ship emissions contributed to CCN. How about rest

of the days? Apparently, sulfur compounds deriving from DMS oxidation played a central role in providing CCN, but where the seed particles were coming from. Were they i) particles nucleated in the free troposphere and entrained from there into the MBL, ii) primary particles emitted by the ocean (sea salt and organics), or iii) long-range transported anthropogenic particles? The authors should discuss this issue explicitly and address whether their measurements could give any hints on it.

The study suggests that secondary aerosol formation (via addition of sulfur species into pre-existing smaller particles) plays an important role in the CCN budget of a remote MBL. The author could discuss briefly how/whether differs from CCN sources in a continental boundary layer (e.g. primary vs. secondary aerosol sources).

Minor/technical comments

I am slightly puzzled with the CCN measurements? It remains unclear how quantitative they are. Do real CCN concentrations depend linearly on deltaV? Are CCN values at different supersations comparable to each other (i.e. if deltaV is higher by a certain factor at 0.34% supersaturation as compared with 0.19% saturation, are real CCN concentrations higher by the same factor)? I do not know wheter issues have been addressed in the paper by Shantz et al. (2008), but they should be briefly mentioned here as well.

I doubt that particles in a remote marine boundary layer would grow by condensation larger than 1000 nm diameter (page 321, line 4).

I can not follow the discussion on page 321 (lines 22-28). How would particle number size distributions alone tell anything about effective CCN radius?

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 309, 2009.