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Interactive Comment

Interactive comment on "Unexpectedly high ultrafine aerosol concentrations above East Antarctic sea-ice" *by* R. S. Humphries et al.

Anonymous Referee #3

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Comments to the paper Humphries et al. Unexpected high ultrafine aerosol concentrations above East Antarctic sea-ice.

The paper is concerning the changes in particle populations in the ranges 3-10 nm (CN3-10) and >10 nm (CN10) observed during an oceanographic cruise from Tasmania to Casey Station (I think) in East-Antarctic sea-ice areas. The paper is well written and the data and results are in depth discussed with good consequentiality and consistency. A large quantity of work is made in producing evidences (meteo analysis, back trajectories, pressure fields) that could demonstrate the Authors hypothesis on a fundamental role of Antarctic Free Troposphere (AFT) in supplying ultrafine particles in Marine Boundary Layer (MBL). For this reason, I think the paper is surely suitable to be published on APC but I'd like to point out some possible weak points (in my opinion)

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Interactive Discussion

Discussion Paper

here below described. The Authors have already published other results concerning the ultrafine aerosol concentrations measured during the same cruise, especially concerning the new particle formation in sea-ice areas. In particular, the 18 Oct nucleation event shown in this manuscript (figure 2B) is under discussion on ACPD. Therefore, in the present manuscript, the discussion is mainly focused on the evaluation of the possible location of nucleation and of the main source of ultrafine particles measured in the sea-ice region. Authors strongly support that AFT is the best candidate for these processes but, in my opinion, all the evidences here discussed can be considered just as “circumstantial evidences” and the role of the MBL is, almost a-priori, ruled out by the Authors. I completely agree that air-masses fluxes from the AFT are relevant and are well described by the back-trajectory and pressure fields analysis but, anyway (as the same Authors show), air masses move at sea level in the MBL for a mean of 36 hours before to reach the measurement sites. The Authors rightly assert that “local” nucleation is demonstrated by a delay between the CN3-10 and CN10 number increase and steady-state particles increases could be a signature of previous nucleation, possible occurred in the AFT. However, the Authors report only two events that can be attributed to present or past nucleation (Figure 2 A and B) whose just one (here described) does not show such a delay (09 Oct). Just one event cannot be significant and, more important, the nucleation event could be occurred some hours before the air masses reached the measurements site. In this possible circumstance, a quite distant (not near the ship, I agree) MBL area could be the location of the nucleation event that caused the 09 Oct signature. Indeed, the delay between the number increase of CN3-10 and CN10 in the 18 Oct event (classified as non-AFT event by the Authors) is very few hours (about 2-3 hours, from Fig. 2 B). Therefore, the nucleation causing the 09 Oct event could be occurred in every MBL site in which the air masses moved during the 24 h (back-trajectories 12:00, 14:00 and 16:00 Fig. A5) before to reach the measurement site (i.e., the day before). Besides, the trend of the CN3-10 – CN10 ratio is intriguing. Figure 2 shows a very constant ratio along all the period (excluding the nucleation events and with some little differences after these events) in spite of the several cyclonic phases

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occurred in the same period (Table 1). If the AFT is the major source of background ultrafine particles, due to the relevant differences in the altitude of the air masses origin during cyclonic activity (up to 6.000 m a.s.l. – Fig. 6A), it seems to be unlike that the ratio remains essentially undisturbed. On the contrary, if the MBL is the main particle source, CN3-10 and CN10 particle concentrations are controlled by the MBL dynamics during high and low pressure phases (by changes in the mixing-layer height), while their ratio should remain constant, in absence of new particle formation. The presence of a relevant background value of the ultrafine particle population in the MBL could be justified by the same processes described in the "Discussion" section: DMS and iodine compounds emissions, other than VOCs emissions by phytoplanktonic activity and organic water-soluble compound emitted by film-drop breaking in the sea-ice borders. Indeed, if I have well understood, the ship was often anchored to drifting ice floe. If pack ice drifts, then sea-ice coverage (the percentage of sea-ice area really covered by sea ice) in not 100% and the area could be classified as a sea-ice marginal area (not necessary sea-ice edge), where open seawater spots and, possibly, polynyas, could act as powerful particle sources (by primary and secondary processes). Finally, the particle concentrations seem to be too high with respect to previous measurements (and for the AFT, too) and, in my opinion, the Authors do not sufficiently explain (by cyclonic-activity frequency) this evidence. Indeed, if "local" MBL sources are not relevant and the ultrafine particle concentration is so high in the AFT, then similar values should be measured during similar periods and in similar areas.

In conclusion, before the possible publication on ACP, I would suggest:

1. Authors should add in the introduction some of the information reported in the "Discussion" section concerning the formation of new particle in the MBL (DMS, iodine compound, etc..). This is relevant because at least one (18 Oct) of the two nucleation events here shown represents a local nucleation. 2. Authors should verify if the MBL dynamics (evaluated by PBL models) can modulate the changes in particle concentrations (while their ratio remains constant) observed during the measurements period.

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3. Authors should be careful in attributing only to AFT the main source of ultrafine background particles, as well as the location of new particle formation, addressing the discussion on the possible role of MBL, too. 4. Authors should better explain the very high ultrafine particle background concentrations here measured, with respect to the values measured in similar period and in similar areas. High concentration of nanoparticles has also been observed in the Nansen Ice Sheet in condition of low wind speed and low friction velocity (Contini et al., JGR 115, D16202, doi:10.1029/2009JD013600, 2010). It could be useful to show the correlation between concentrations and wind velocity (other than temperature, as shown in Fig. A6).

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 29125, 2015.