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Interactive comment on "Natural new particle formation at the coastal Antarctic site Neumayer" *by* R. Weller et al.

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First of all we would like to thank referee #2 for his effort in evaluating our manuscript (ms)! According to his comments, we reconsidered and rectified our ms. In a nutshell: We added a detailed case study and considered in more breadth auxiliary data from Neumayer.

1) Instrumentation used during 2012 is not directly comparable to instrumentation used in 2014. When focusing solely on qualitative definition of presence and absence of NPF, it should not play a major role, however, for comparison of growth rates and size distribution dynamics, direct intercomparison of both systems is necessary and should be presented. It is not uncommon that aerosol size spectrometers vary from

each other significantly [Wiedensohler et al., 2012] as well as cut off characteristics of CPCs. Also using GR calculation and size ranges with two decimal precision has no realistic meaning.

Unfortunately a surely desirable intercomparison was not possible, because we merely have had one classifier available. In order to check the consistency of particle growth rates (GR) between both set-ups contemporaneous measurements would have been indispensable but was not feasible. The CPCs have been checked for consistency as described in Weller et al. 2011a and both instruments were calibrated by the manufacturer recently before the campaign. We are confident that the compulsory change of our set-up did not significantly restrict our analysis and our main conclusions. GRs are intrinsically difficult to compare, because they are usually determined within different size ranges (see table 1). We cannot anticipate that GRs are independent of particle size, even though we could not detect such a dependency due to the large uncertainty of the derived GR (table 1). Apart from this, GRs derived from 11 out of 13 NPF events were measured with an identical experimental set-up! We agree, that presenting GR in two decimal precision is meaningless and corrected the values accordingly.

2) I understand that it is very demanding on resources and logistics to carry out measurements at such a remote place and it is difficult to run extensive instrumentation set up there. But authors did not explore even which they have available. Data analysis will be more robust if local meteorology and other aerosol and trace gas observations at Neumayer will be better linked to NPF observations. How different are conditions between class I and class II event? How different are conditions between NPF and non NPF days? Can importance of marine air on NPF be better assessed? Authors have available data from local meteorology, radiation, cloud cover, BC and scattering levels, OPC and two CPC data which are part of core program. Trace gases: Rn222 and O3 are observed with good temporal resolution, daily data about reactive trace gases. Can authors link air mass origin using trajectories with other observations to assess time spent over the sea/coastal Antarctica for NPF and nonNPF cases? Authors can

also try to use water vapor as an air mass tracer of marine and continental air masses. Neumayer is a GAW station and potential of observations conducted there was not explored sufficiently in this manuscript.

Our ms seems to leave a mark of not having appropriately considered available secondary data. In our revised ms, we tried to clarify this important point, e.g. presenting a detailed case study, summarizing so far as possible and meaningful, typical meteorological and trace compound characteristics during NPF event and non-event days (new Table 2; we could not found significant differences of auxiliary parameter between case I and case II events). It should be mentioned here, that in terms of meteorology we particularly relied on 2 m and 10 m data from the meteorological mast, i.e. temperature, relative and (calculated) absolute humidity, wind velocity vector as well as on the BSRN radiation data (focusing here on actinic uv-radiation). As mentioned in chapter 4.3 (now 4.4), rH tended to be lower during NPF, but this was simply due to the fact that in those cases bright weather prevailed. On the other hand, there were plenty of days with meteorological conditions typical for NPF days exhibiting no particle nucleation. Apart from the shifted diurnal variations between NPF and uv, no correlation between uv radiation and GR (or particle concentrations within the nucleation mode) was given. Surface ozone, black carbon (BC) and 222Rn: Again, we found no remarkable relationship between particle concentrations within the nucleation mode or periods with NPF and the concentration of continuously measured surface ozone, BC and 222Rn concentrations. Particularly 222Rn data are difficult to interpret at coastal Antarctica and there is no unequivocal link between 222Rn concentrations and the characteristics of the air masses (continental or marine) as discussed in detail in Weller et al. 2014. On the other hand BC concentrations were in the lower ng m-3 range throughout, except spikes during very rare contamination events. As for surface ozone, our now more than 30 yearlong continuous record showed some cases where the typical ozone depletion events (ODE, usually sporadically occurring between August through October each year) were accompanied with enhanced CP concentrations, but this was not the case within the relevant measuring period. Apart from this, a discussion of this finding

is another story and clearly beyond the scope of the present ms. Finally, apart from the CPC employed by the SMPS, only one independent CPC was running (and not two, as assumed by the referee). Optical aerosol properties of the aerosol were measured by an integrating nephelometer and are considered in the revised version of our ms as well as some more words on IO measurements at NM. Our experience in interpreting the Neumayer 222Rn time series (Weller et al., 2014) indicated, that assessing the time air masses spent over the sea/coastal Antarctica would need sophisticated Langrangian model calculations considering sea ice coverage, which is beyond the scope of our present ms. In our revised ms we highlighted the somewhat equivocal outcome of trajectory analyses. The dependence of rH from air mass history on the other hand is quite complicated and again (highly) equivocal. If at all it is possible to distinguish between periods of cyclonic and katabatic impact by rH values.

Minor comment: In Introduction on page 15656 authors present a picture of aerosol having decisive role in radiative forcing. GHG have decisive role, aerosols have largest uncertainty and we do not know how decisive role they actually play.

We agree that our statement is somewhat misleading: the decisive role of aerosols in radiative forcing is mainly due to their role in acting as cloud condensation nuclei. This point is now clarified.

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