

Interactive comment on “Aerosol size distribution seasonal characteristics measured in Tiksi, Russian Arctic” by E. Asmi et al.

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We greatly thank referee for their comments and wish to acknowledge the excellent and very detailed work done for reviewing our manuscript which helped us to improve it and clarify the results. We modified the manuscript according to the suggestions as detailed below and we hope that referee finds our revisions satisfactory.

MAJOR COMMENTS

I. First of all, the clustering of DMPS data seems counter-intuitive. First data are clustered, then a combination of factors is proposed why the clustering produces the observed results. Why not use those factors (season, air mass origin, wildfires, etc) as parameters to sort the size distribution data? While clustering can be a useful tool (eg

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for trajectories), its advantages over a more intuitive approach seem unclear in this case. Why group data around a mathematical concept when actual, physical parameters are readily available? Unfortunately, the clustering itself is hardly described at all in the manuscript; it is thus hard to assess the (presumably existing) deeper motivations for this approach. Also the clustering results produce more questions than answers. While data points inside a cluster should be rather similar, the numbers in tables 1 and 2 suggest that inner-cluster variability with regards to N is as large or at times even larger than inter-cluster variability. Of course this is a result of normalizing the data, BUT: the reader cannot assess variability with regards to size distribution shape as this information is not given (figure 3). And this would be the point of all the normalizing, right? Especially terrible is the number of cases in some clusters in tables 4 and 5. When some clusters have 8000 cases and others merely 8 the discussion of differences between clusters doesn't seem all that relevant. I find it highly doubtful if those are clusters in the actual meaning of the word at all. They seem merely singular cases which don't represent much else than themselves. Finally, four (4) different clusterings of the same data that are not based on actual physical characteristics seem a not-too-great idea when it comes to manuscript readability. Just compare "the normalized number cluster 6" to "the spring-time continental aerosol under cloudy conditions"; only one of those has some tangible meaning. In any case, the manuscript attempts to characterize the clusters in those terms - why then not start with them in the first place? I would very much prefer the manuscript without the whole cluster business. I know this would mean substantial changes but I'm really not a fan. And considering the above issues it might be the easiest solution.

OUR REPLY: The idea of clustering was to present the variability in size distributions in Tiksi and to find different type of size distributions to see if those are related with any physical parameters: e.g. if a distribution of strong nucleation mode is more frequent in marine in comparison to continental air mass, and to what type of weather parameters certain modes are usually connected. Making this analysis starting from physical parameters as referee suggests, i.e. plotting distributions for a range of weather pa-

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rameters, or for a range of air mass origin, for us seems more to create a big mess, instead of creating something useful. Since most of the data present quite homogeneous bi-modal distributions, it's likely that those would be dominating the analysis. So in this matter, we don't agree with the referee. We do agree that the observations could be more equally distributed into different clusters, but again, if the data are quite homogeneous with some 'events' only changing the distribution shape, then for us this is a result as well. We also agree with referee that clustering got way too much emphasis in the manuscript. For this reason, we cut most of this analysis out, and made only one tiny clustering experiment with five (5) clusters found. Additional parameter of aerosol black carbon concentration was added to explain these clusters. And eventually, we used these clusters to pin-point the size distributions connected with different events, further analysed later in the manuscript (i.e. fire events, inversions, secondary particle formation). We hope that referee finds our solution is improving the manuscript quality.

II. Given that the manuscript is not terribly huge, I wonder why new particle formation is kinda completely ignored. The abstract promises "valuable information on [...] sources of Arctic aerosols." Surely NPF is a source?

OUR REPLY: We agree. Our original idea was to focus on size distributions and only present the different sources, not to thoroughly analyse them in one manuscript. But by cutting out some of the cluster analysis, we could add a chapter making a complete analysis of new particle formation including event frequencies, and particle formation and growth rates. This is actually a very interesting addition to the manuscript, also to our view.

III. Wildfires. Given that they raise pollution levels to almost Chinese levels (page C6050 18125) I wonder why the manuscript doesn't include a proper investigation dedicated to them.

OUR REPLY: Thank you for this suggestion, which is good indeed. However, we find that this could be a topic for future manuscripts since making this analysis in fact needs

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a lot more additional data (gases, satellite products), and is a bit out of the scope of this manuscript. Here, we just wish to present that these wildfires have potential to affect size distributions also in the Arctic, but not yet to make a complete analysis to give quantitative numbers of their effect. We hope that referee understands this choice. To improve the manuscript in this respect, we however separated those wildfire cases which clearly came out in clustering analysis, using them as examples of the impacts of wildfires (e.g. in Fig. 10 in revised manuscript, Fig. 7 in previous). Now the impact of those isolated wildfire cases for particle mass and CCN numbers are analysed separately. We hope that this further emphasizes the point we're making.

IV. The supplementary information is technically extremely poor. Unzipping produces (among other things) an eps file of gargantuan proportions (100+ MB). And the captions are somewhere else. Just make it a nice and easy pdf file with all the information in one place.

OUR REPLY: Yes, we apologize for this. Now the supplementary is all presented in one pdf.

DETAILED COMMENTS

1. The abstract has all those numbers, it thus also needs information on the DMPS size range.

REPLY: This was added.

2. Is arctic haze an aerosol source? I would rather call it a phenomenon.

REPLY: Yes, we agree, this is corrected.

3. 18110/11: "biogenic emissions in summer which affects the smaller, nucleation and Aitken mode particles." - should maybe be: "biogenic emissions in summer which affect the nucleation and Aitken mode particle concentrations." or so

REPLY: Yes, modified. The sentence is now: "These seasonal cycles in number and

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mass concentrations are related to isolated processes and phenomena such as Arctic haze in early spring which increases accumulation and coarse mode numbers, and secondary particle formation in spring and summer which affects the nucleation and Aitken mode particle concentrations.”

4. 18110/14: "significant" - how significant? 50%? 100?

REPLY: This was clarified with an additional sentence: “increase of both the particle mass and the CCN number with temperature was found higher than in any previous study done over the boreal forests region.”

5. 18110/17: "nearby" -> "local"

REPLY: Corrected.

6. 18113/16: "theoretical diffusional losses during the particle transport are taken into account in the data inversion." - while i agree that this is somewhat trivial and doesn't need equations and such, at least a reference would be nice.

REPLY: Reference to Hinds, 1999 was added and a mention that the losses for laminar flow were calculated.

7. 18114/2: "charge of the regular maintenance done weekly" - what is this regular maintenance?

REPLY: This is basically just adding butanol to CPC and checking that flow rates are ok and concentrations seem reasonable.

8. 18114/4: "quality checked weekly" - what is this quality check?

REPLY: This is a visual inspection of inverted data and all measurement parameters (T, RH, pressure, flows, etc.).

9. section 2.2.1: the nominal cut-off of the 3772 is 10 nm if memory serves correctly. how do you measure down to 7 nm? sure, most readers can guess. but it should still

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be mentioned.

REPLY: Yes, the temperature difference between saturator and condenser was modified to 23C to reach a cut-off of size of 7 nm. This was verified in calibrations using silver nanoparticles. This explanation was added in manuscript text.

10. 18115/1-5: so how do the data differ? i suppose you have some overlap where data from both locations are available.

REPLY: We compared the winds for these two locations and added some text on the results of the comparison: To study the homogeneity of wind observations at station and cabin sites, we excluded calm cases (wind speeds less than 1 m/s) the same way as was done for the other analysis here. Compared to the wind speed observations at station, wind speed at the cabin was on the average 71 % (correlation coefficient 0.934) and 79 % (correlation coefficient 0.972) lower in summer and winter, respectively. Wind directions were within 30 degree in 93 % and 87 % of the cases in summer and winter, respectively.

11. 18115/14: "additional 36%" - 36% of 74% or 36 %-points?

REPLY: 36% of 74%

12. 18117/about the APS: you focus on a comparison of counting efficiency. how do you know that APS sizing is reliable? and if it isn't, what sort of error does this introduce?

REPLY: By experience, the APS was calibrated and the flows were regularly checked. We believe the APS is accurate in measuring the aerodynamic size of the particles, however, for different atmospheric compositions it's generally known that the detection efficiency in lowest channels can be poor.

13. 18119/4: "median values extending from 3 to 5ms-1." - what exactly does this mean? daily medians? weekly? monthly? i also wonder if the std.dev wouldn't be a better metric to describe how stable wind speeds have been.

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REPLY: Stable here refers to the lowest quartiles as presented in Fig. 2. Median is the median of all the observations.

14. 18119/5: "sudden onset" - i don't see anything sudden in the figure. and really, why would there be?

REPLY: Agree, wording was changed removing "sudden onset".

15. 18119/6: "RH decreases" - this all refers to RH_{water}, right? or is the picture the same for RH_{ice}?

REPLY: Yes, it's RH with respect to water, and we do understand that with respect to ice would change the picture completely.

16. 18119/12: "median wind speeds" - median what? daily? weekly? monthly?

REPLY: Median of all the observations, which were taken in half an hour averages.

17. 18119/26: "each size distribution were normalized with its maximum concentration" - what does that mean? i.e. what is the max. concentration? the bin with the highest concentration? is that really the best way to bring out differences in size distribution shape? why not use total N instead?

REPLY: Yes, it's the bin with maximum. But in revised manuscript normalized size distributions are no longer presented or used.

18. 18120/3: what's the "peak mode"? i mean, it is quite intuitive but i don't think it's actually a thing. if it is, as i assume, the mode with the highest concentration, than one should probably say that the highest concentrations are found in the aitken mode which has a diameter of xx nm. or whatever.

REPLY: Agree. Term 'peak mode' is no longer used in revised manuscript.

19. 18120/first paragraph: the paragraph really doesn't say anything about nucleation? simply looking at the numbers, one would think that those clusters represent different

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times after nucleation with 3 being the freshest and 1 the oldest. or is there a reason not to think that way?

REPLY: Yes, agree. But again, this analysis was completely removed from revised manuscript.

20. 18120/2nd paragraph: these lines just state the minimum information from the figure. some interpretation would be nice.

REPLY: This part was removed.

21. all 18120: in addition to medians, i always find it nice to know something about the range within a data set. which means that quartiles would make a suitable piece of information. probably also in other sections.

REPLY: Agree on this in principle, but too many numbers (medians with ranges) would have made the already complicated chapter even more difficult to read, in our point of view.

22. 18121/first paragraph: lacks a statement of what is typical for continental and marine clusters. of course, referring to the major remarks in the beginning, this is a backwards approach. to me, it would be much more intuitive to start by clustering the trajectories and then separate the aerosol data by trajectory cluster.

REPLY: Here it was meant, the percentages of marine vs. continental air were suggesting the most marine out of those clusters found.

23. 18121/last paragraph: these four modes are standard textbook fare. i am not quite sure what you mean by saying you "identified" them.

REPLY: This paragraph was removed.

24. 18122/2: "Total particle number concentrations (Fig. 4a and Table 3) show two annual maxima in March and in July." - really? i don't see it. the attribute i would use for the data in figure 4a is non-descript or something along those lines. to say there is

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an annual cycle in Ntot seems like an over-statement

REPLY: The figures are now larger, hope the annual cycle becomes more clear. Also looking at table 1 can help.

25. 18122/3: "A similar interannual cycle is seen for each particle mode separately" - while the previous comment may be a matter of taste, this isn't. Nnuc has neither regular march nor july maximum. one is shifted to february half of the time, the other one sometimes happens in june. and to call these tiny differences cycle is too optimistic. similar things could be said about the other modes. the thing of course is that the figure is an unfortunate choice if cycles are to be presented. log-scale diminishes the differences one would like to see (and which the table says exist). also, if it's a cycle we want to see, why not make a figure that actually shows a 1-yr-cycle instead of the full time series? i would ditch figure 4 AND the table and make a new figure with the data that is now in the table.

REPLY: I hope this becomes more clear now with enlarged figures. The idea of the figure is not to repeat the information in table 1. The typical annual cycle can be seen using both the figure and the table. The idea of the figure is to present that, as typical to the atmosphere, there are always some year-to-year variability, but the text wishes to explain the general variability (as for those using global models would appreciate to know the overall features and not values for any particular year). In our view, the inter-annual variability is rather similar for all the four years, with maximums in spring and summer, minimum in late autumn.

26. 18123/8ff: once again, i fail to see the benefits of this cluster business. what information do we gain from this detour?

REPLY: This analysis was kept here, but separating only 5 clusters. The idea is, by starting from differences in size distributions to see if they actually can be connected with differences in environmental conditions. As explained in the beginning, doing this analysis vice-versa would not give the same information.

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27. 18125/1ff: i have said this before but i want to point it out again: mass concentrations of 65 just show how silly clustering can become. "cluster" 4 is not a cluster but a single event that produces shanghai-level pollution in siberia. it is no doubt a very interesting event (that would deserve a bit more attention in the manuscript) but it's not a cluster in very much the same way that one person is not a group.

REPLY: Agree, but in this analysis, it presents one special case of size distribution that is not similar to 'typical' conditions, yet, very interesting observation.

28. 18126/17: "y = 0.014x + 0.12" - units?

REPLY: Units were added. The text is now stating 'y represents the aerosol mass [ug/m³] and x the time [s] spent over the continent'.

29. 18127/25: "In addition to natural vegetation sources, another temperature dependent aerosol source in summer Siberia are the intense fire events, as already discussed in Sect. 3.3." - yes! but where is the proper analysis? why not identify all trajectories that came through wildfire and then have a closer look? it would also be beneficial to exclude them from the current analysis to determine how much of the temperature dependence is actually related to BVOCs.

REPLY: We now analysed further those five BB cases that were found using clustering analysis. A more detail and quantitative analysis of fires is left for future manuscripts since this is out of the scope of this paper. Here the main idea is to present that there are some fire events which do affect both particle mass and CCN numbers in the Arctic.

30. 18128/1: "the maximum average CCN100 concentration" - average over what?

REPLY: Average of all the CCN100 concentrations in each of the temperature ranges shown in the figure of CCN vs. T (Fig. 11 in revised manuscript, Fig. 8 in previous).

31. 18128/3: "only 13 cases were available" - which would make it all the more important to determine which of those are wildfire-related.

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REPLY: Impact of strong fires to CCN was analysed previous figure (Fig. 10 / Fig. 7) where it can be seen that fires affected mainly CCN at lower T. However, only 5 fire events were isolated for this analysis. This will be a topic of future papers to develop methodology to better identify also weaker fires and to elucidate their impact for particle properties.

32. 18128/section 3.4.2: it would be nice to see a little investigation (a case study if nothing else) how these pollution events develop.

REPLY: Agree. However, the manuscript is already fairly large and we feel in this context it's sufficient to mention this phenomena exists and affect the concentrations. We're not sure if presenting a developing pollution event in Arctic is of particular use but will add this if it's found necessary.

33. 18129/7: "interannual" - i guess it should be iNNeRannual. iNTerannual variability (fig. 4) is not that large.

REPLY: Yes, thank you. Corrected.

34. figure 1: the map is terribly small. the photos, on the other hand, are not that essential.

REPLY: We enlarged the underlying topography map and added a more general map in supplementary material.

35. figure 3: as said before, it is unclear how the data for the figure were normalized.

REPLY: Figure 3 was removed completely.

36. figure 4: too messy, too small, labels hardly readable.

REPLY: These were divided into 2 separate figures, hope they are more readable now.

37. figure 6: the temperature information should also be in the figure, not only in the caption.

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REPLY: Added.

38. figure 7: fit quality information for 7a is missing. also: how did you determine that linear and quadratic fit are the only options? why not try an exponential fit? this figure and the fit would probably benefit from an exclusion of wildfire-related episodes. the same is true for figure 8

REPLY: Fit quality information added. Quadratic fit removed and fire episodes isolated and presented separately.

39. figure 9: this is not terribly important, could move to supplement. a figure that describes a haze event could be nice though (as said above).

REPLY: This was moved to supplement. Haze event is difficult to present in one figure, we feel the text and tables (with annual cycles) above presents those events relatively well.

40. supplement: make a single pdf.

REPLY: Yes, done.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 18109, 2015.

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