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Atmospheric Chemistry and Physics Discussions

Interactive comment on "In situ measurements of aerosols optical properties and number size distributions in a subarctic coastal region of Norway" *by* S. Mogo et al.

Anonymous Referee #3

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Review of manuscript by Mogo et al. ("In situ measurements of aerosols optical properties and number size distributions in a subarctic coastal region of Norway")

GENERAL COMMENTS

The manuscript by Mogo et al. presents aerosol data measured on Andoya in the summer 2008 roughly from mid of June until end of August (it's actually not clearly stated in the manuscript). The measurements comprise aerosol microphysical (total number concentration >2.5 nm, size distribution by SMPS and and APS) as well as optical properties (aerosol absorption and scattering coefficients at three wavelengths by PSAP and integrating nephelometer). Furthermore, the authors discuss derived

optical properties like single scattering albedo and Angström exponents representing the wavelength dependence of the optical properties.

The data are of interest because there is always a need for good quality measurement data of aerosol microphysical and optical properties in the high latitude regions. The data are of particular interest in the context of the IPY POLARCAT initiative. However, in my opinion, the manuscript fails to convincingly discuss this data set. I don't find many substantial and well founded conclusions. Physical explanations of findings in the data sets are lacking in many cases. Many statements are not properly discussed.

For instance, the authors mention a couple of times that their data compare well with other measurements in the Arctic. References have been given, but other than that no details are presented. The authors should explain explicitly how numbers compare, if they want to draw any conclusions! It cannot be left to the reader to go into the cited literature and test the hypothesis that data compare nicely. (And anyway, what would be the conclusion?)

I am not convinced of the statistical presentation of data. In general, the manuscript states at many occasions in the text min/max values of the time period (based on hourly means, apparently), arithmetic (?) averages and standard deviations. For some data this is followed by details on the median, sometimes also other percentiles and the actual frequency distribution. I believe the information on averages and in particular standard deviations is quite meaningless. It is stated that medians are higher than averages. It is obvious when studying the frequency distributions which are usually asymmetric. The discussion should be the other way round: Discuss first frequency distributions, than argue how to statistically best represent the data set, and consequently use median and percentiles to characterize the "mean" aerosol properties throughout the measurement period.

The entire paper lacks a discussion of measurement errors and in particular errors of the derived optical properties.

ACPD

11, C15807–C15818, 2012

All numbers for aerosol related quantities should be presented consistently with, I suggest, two significant digits. In text and Table 2 one finds concentrations given like 2463 1/cm3, but also 0 or 1 1/cm3. This is both not very meaningful considering the typical measurement error I expect.

The discussion of possible sources of particular aerosol types observed in the data set I do not find convincing as well. Back trajectories were calculated by the authors (but are not shown!) and according to this analysis air mass types have been grouped into a couple of sectors. Based on this classifications some of the observed aerosol properties have been discussed. However, as a reader I don't get any feeling for the transport pathways and actual source region (and source region distance) the authors are referring too. The other reviewer has noted this similarly and I therefore refer also to his comments in this aspect.

The authors mention briefly the role of different (local) meteorology during the measurement period, but it is not explained well. It is also not discussed in connection with the trajectory classification which should be done. It would help if periods of different meteorology and different air mass origin would be actually shown in the time series graphs. It is very difficult for the reader to grasp the connection of certain aerosol properties (e.g. high or low concentrations) to meteorology and air mass history.

The authors have mixed data presentation and discussion of the observations. The clearness of the paper suffers considerably from this. The final conclusions section does not present any conclusions, just a mere summary of what has been said before. The introduction section is not well structured. It does not explain well the aims of this research and how it relates to open scientific questions. The introduction section contains a fairly large part on the methods (instruments) which is repeated in the method section.

The authors mention at the very beginning of the manuscript the relevance to determine aerosol optical properties because of the question of climate effects. I am lacking in the

ACPD

11, C15807–C15818, 2012

conclusions an assessment of their findings in the light of the climate effect question. What is the role of summer time aerosol at a place like Andoya? Is it so clean that any effects on the radiation budget can be rated as actually negligible? Was the summer in 2008 typical for other summers in terms of meteorology and aerosol properties? What kind of air masses are typically dominating the summer aerosol? Is Andoya a special place? Would it make sense to continue observations there? What would the authors expect if they would look at other seasons of the year?

The paper, if it is going to be revised, would need a thorough check by an English native speaker. In terms of scientific terminology, it should be noted that there is in aerosol science to my knowledge no common use of a term "numerical" size distribution or "numerical" concentration. It should read "number" size distribution etc.

The manuscript in its present form is overall not well structured. It lacks many explanations. It lacks any explicit illustrations of details on comparisons with other published data. It lacks conclusions and it lacks well-founded conclusions. It should not be published. I leave it open to the editor to reject the paper or ask for major revisions.

SPECIFIC COMMENTS

Title: aerosols –> aerosol. Also: The title should say that the study deals with the summer season.

Abstract: See general comment above. Is it meaningful to refer to arithmetic averages and standard deviations?

Abstract: The meaning of the sentence starting with "Whereas..." (line 13) is very unclear.

Abstract: Last paragraph. "...presented lower optical parameter values..." (line 4) and similarly in line 6. Be more specific!

Introduction. Page 32926, line 2. Estimates of what? Line 5. "these important optical properties" - which ones? The paragraph is strange because it starts general but

ACPD

11, C15807–C15818, 2012

actually refers to the study region only. For sure there are a large number of aerosol microphysical surface in situ studies available in the Arctic. Furthermore, the study area should be actually mentioned.

Line 8. Define "ultrafine".

Line 12."different size distributions" related to "origin of air masses" - this is a statement which is too much simplifying. Many more factors determine the size distribution obviously.

Introduction, in general. I am missing a statement qualifying the location of Andoya in terms of atmospheric properties. In the abstract the station is qualified as "rural" - is this correct? Is it not in general still under the control of westerlies and therefore rather marine air masses? Are the measurements of this study always taken inside the boundary layer? If not, this needs to be discussed!

It should be pointed out more clearly: Why were there measurements made at this site? Why in the summer season? Why only in the summer season? To which objectives of POLARCAT does this study refer to?

In general, I don't find the structure of the introduction section to follow a clear pathway. It should begin more general and become towards the end more specific towards this study. For instance, the last paragraph in the introduction goes back to reporting general observations of Saharan dust and urban aerosols. This just does not fit there.

The aim and role of the paper, the open questions, the current knowledge and how this paper tackles any of the open questions should be more clearly developed in the introduction. The conclusions section in the end gives the opportunity to validate if the study has made any progress on open questions posed in the introduction.

Methods. Page 32928, 1st paragraph. Sample air was heated to achieve RH below 40 %. If ambient RH was 68 % minimum as stated elsewhere, it was always heated, right? Then it should be stated this way. Furthermore, where does the cut-off diameter of 10

ACPD

11, C15807–C15818, 2012

μm come from? Measurement by the APS? Is it constant? Was the inlet characteristics possibly modified if there were high wind speeds?

Methods. Page 32928, 2nd paragraph. Discussion of detection limits of the PSAP needs to be added. Were all measurements above detection limit? If not, how were the data treated in the following analysis?

Methods. Page 32928, line 27. The correct term is: differential –>mobility<– analyser. How was the SMPS operated? Full scan in how much time? What is the size resolution? What about charge correction? What about size calibration?

Methods. Page 32929, top line. "number" –> "number concentration".

Methods. Page 32929, 2nd paragraph. I don't understand, why the SMPS and APS data sets were not combined to provide one size distribution data set. Aerodynamic diameters can be converted into mobility diameters. The gap between the size ranges is not that large, so why should it not be possible to interpolate between 390 and 500 nm?

Methods. Page 32929, 3rd paragraph. Were the nephelometer data used to correct according to the formula given by Bond et al. If yes, than it should be stated. How were the different wavelengths matched between PSAP and IN? The meaning of the sentence in lines 19-22 I don't understand. Lines 23-24. How was the correction made? Line 26. Which error threshold? Errors are not discussed. They should be discussed.

Methods. Page 32930. Formula (4) can be removed. It is obvious. And the sizing of the intervals is anyway a matter of setup of the SMPS instrument (and not known to the reader).

What does the sentence "The nucleation, Aitken and accumulation modes are also fully described" mean? How were the three modes defined in this paper? (I can guess, but I did not find it.)

ACPD

11, C15807–C15818, 2012

Do I understand correctly from later in the text, that the nucleation mode in the context of this paper is referring to the size range >10 nm (10-30 nm?). I don't think this is appropriate. The nucleation mode extends to below 10 nm. Strangely, the authors do have a CPC measuring down to 3 nm. Why is the not used to define a nucleation mode by the measurements? Substract the integral of the SMPS measurements from the total CPC concentration. Does this not work? If not, why?

By the way, elsewhere in the text the authors refer to nucleation events. Many aerosol scientists would rather prefer to speak of new particle formation events. The nucleation takes place in the size range not covered by the instruments.

Page 32930, lines 20-22. I don't understand this. Low values of what? How does the averaging over one hour relate to low values? Are there any measurements below detection limit?

Page 32931, 1st paragraph. Why should 5-min values not be available? Again, what values? And why are values missing at all? How is the data coverage related to weather conditions? This is not explained. Under which weather conditions did instruments not measure and why? Would this not introduce a bias to the results discussed later in the paper?

Section 3.1. The first sentence is a results or actually a conclusion and should occur at the very beginning of Section 3. It should be shown in the paper that this is the case.

I refer to my previous general comment on the statistics. Is it useful at all to discuss averages and standard deviations? I suspect median and percentiles are more meaningful. If the median is lower than the mean, as stated a few times, it just indicates that the mean is not very useful. Furthermore, if the numbers are all given in table 1 anyway, they don't need to be repeated all in the text. Is there any conclusion on the variabilities? If yes, it should be stated. If not, it is not necessary to state every standard deviation (or range of percentiles) in the text.

ACPD

11, C15807–C15818, 2012

Interactive Comment

Page 32931, lines 23-26. It should be explained in detail how data compare to other published data sets. It cannot by just stated in passing. This could be a discussion section on its own right. Same is true for the single scattering albedo discussion in lines 7-8, page 32932, and later on again for the discussion of the Angström coefficients.

Page 32932, lines 27-28. Is there any support to that sentence?

I am missing in the entire discussion physical explanations for the findings. Are the findings as expected or not? Which aerosol types would explain the findings? What is the role of sea salt aerosol? Again, the question if all measurements were generally inside the boundary layer should be addressed. The authors say on page 32933 that Saharan dust events explain the negative Angström coefficients. Is there any reasoning behind this? Why is this not shown? It looks in the text to be purely speculative! Which back trajectory data and MODIS images were analyzed? It should be shown.

Page 32934, lines 2-4. This I don't take as an argument. If one compares the integral in the size ranges of 100-390 and 100-500 nm, there should be no significant difference. Just check the size distributions in the figure 13 which have their maximum below 100-200 nm.

Lines 14-19. The authors identify three types of weather conditions based on observed aerosol concentrations (not based on meteorological data!). It is not possible for the reader to follow this. Where is the proof? Furthermore, it would be helpful if these periods could be identified in the time series graphs (Fig. 2, 4 and/or 5).

Line 20. "The six-day period selected for further analysis" - what does this refer to? All the following analysis in the paper? I don't think this is the case. Or is it? It is a very misleading statement.

Page 32935, line 4. As stated before, what is the justification to define the nucleation mode beginning at 10 nm? Especially if there was a measurement going down to 3 nm?

ACPD

11, C15807–C15818, 2012

Interactive Comment

Same page, 2nd paragraph. This is strangely structured. Sentence lines 21-23 should come before sentence lines 19-21. Before that, what is most likely origin of the coarse mode peak? Why is there no time series data for the coarse mode (APS based) data like for the data of the SMPS and CPC? Lines 25-26. Why should this be a characteristic feature of the station? What is the physical explanation behind that? What is the usefulness of Figure 7? And again there follows a statement that the findings in this study are similar to other observations, but it is not shown (lines 24-27). Next paragraph: again there is a comparison mentioned which is not performed in the manuscript. Even if it were true that data at ALOMAR fit to other Arctic stations data, what is conclusion on aerosols affecting all these stations? Same transport patterns, same aerosol types, same source regions?

Page 32936, line 11. "fit for each correlation" - what correlations are the authors referring to?

Line 14. Explanation for the slopes of the lines being dependent on particle size? Actually, the authors have made particle size measurements. They should be able to give evidence for their statement.

Line 17. How do marine aerosols come into play here? What is the argument?

The entire section in lines 14-21 - is this speculation or conclusion from the data? If the latter, the authors need to be be much clearer in their line of arguments. How do these statements refer to their own data? Which figure illustrates the statements? If they argue with the regression parameters R they need to give R values for the correlation with coarse mode particle concentrations for both cases, the case with smaller slope line and the case with higher slope. The same is true for the correlation with N100. In the present text they are not comparing the same thing and one cannot follow the argument.

Page 32937, 2nd paragraph. The statements appear to me extremely speculative. What are the continental urban sources relevant for the area of Andoya? Why is the

ACPD

11, C15807–C15818, 2012

entire discussion in this section not linked to the trajectory study which follows in the next section. Maybe the trajectory results can support some of the claims in this section. The discussion is not well structured in this respect.

In the previous part of the manuscript there was some discussion of observations with respect to the different weather periods. In this part of the manuscript this differentiation is apparently not the case. Why?

Lines 20-23. I doubt that this is a general rule (low single scattering albedo relates to small particles). What is the physical explanation? In any case, the authors have made size distribution measurements, why don't they validate their statements with their own data?

Page 32938, line 1. I can't follow the conclusion that local air is extremely clean. And how often did episodes with long-range transport of small particles occur? What about small particles formed as a consequence of gas-to-particle conversion processes (mentioned elsewhere) - that would occur rather locally, right?

Page 32938, discussion in last paragraph. What is a micrometer fraction? Is it possible that inlet cut- off effects are of relevance if the scattering coefficient is dominated by super-micron particles? If there are super-micron particles present at all, how does that fit to the previous notion that the ALOMAR site was characterized as being extremely clean?

Moreover, I don't find it surprising at all that if there are coarse mode particles present, that they contribute to the scattering. What is the message in this context? One could use Mie theory together with the measured size distributions to actually calculate the size segregated contributions to the total scattering coefficient. Of course, total CN does not correlate with scattering. Was this expected?

Page 32939, 11-13. Analysis was done separately for sub-micrometer and super-micrometer fractions. But why? This is a weakness of the paper which I don't under-

ACPD

11, C15807–C15818, 2012

stand.

Line 20. Physical explanation?

Page 32940, section 3.4 on trajectory analysis. Back trajectories should be calculated for the real altitude of the measurement location. Why use high altitude trajectories for long-range transport analysis? What would be the relevance to measurements discussed in this manuscript? Can the authors give evidence to the statement that back trajectories arriving at 500 m are inside the boundary layer?

Next page. Air mass origin is not well defined by the sector classification. A source region for aerosols could be far away - or close - for the same sector. It is hardly possible to follow the arguments given in this section. Furthermore, it is not helpful that this section on back trajectories comes at the end of the manuscript. It could be used in the very beginning together with meteorological information to characterize the entire measurement period and to base further analysis of the aerosol properties on this classification.

Conclusions section. This is rather a summary and it suffers therefore from many of the inconsistencies and lack of explanations in the previous chapter. What is the "spectral shape" referred to on page 32943, line 4. That results were typical for the summer in this region is an interesting conclusion, but this is not supported at all by the previous discussion. In line 12 on page 32943 the authors say that they compared their data with those from other Northern European locations. Maybe they did, but this is not included in the manuscript. What is the relevance of saying that the ratio of Aitken to accumulation mode was larger 1? On page 32944, lines 8-10 authors state that they could clearly distinguish between two aerosol types. I missed this in the previous text. Anyway, what are these aerosol types? How does this fit to the "extremely clean" argument?

Table 1. Why no P16 and P84 percentiles given like in Table 2?

ACPD

11, C15807–C15818, 2012

Table 2. Significant digits should be the same for all quantities and resemble the typical uncertainty.

Figure 2. Panel a) represents a derived quantity and should rather be positioned below the measured quantities.

I would prefer if all time series data of Figures 2, 4 and 5 could go in one (big) figure with all data on the same time axis. One can hardly compare the different properties the way it is now. I would also stretch the time axis to the maximum allowed by the page width. Weather periods as discussed in the text should be highlighted. Can one incorporate the classifications results of trajectory study in the time series plot?

Same is true for the frequency distribution plots. They could also be bundled into one figure.

Fig 5a needs to be properly explained in the figure caption.

Figure 6. I have serious problems with this way of showing the size distribution results. Why not logarithmic scale on the y axis? It is necessary to combine the data in panels a) and b) into one plot!

Is the secondary maximum in panel b) at 2.5 μm a significant and a real feature? The text mentions a maximum in the coarse mode fraction of 3300 nm. I don't see this. Is the maximum in the APS data around 600 nm a real feature?

Figure 9b. Defined regions can be hardly identified. The plot is too busy.

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ACPD

11, C15807–C15818, 2012

Interactive Comment