Our point-by-point responses to reviewer #3 are presented below. We accepted most of the suggestions presented by this reviewer and improved the manuscript accordingly. In this response, we explain why not all the suggestions were incorporated into the final version of the manuscript.

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.

> *Data were available from 13 June to 26 August, 2008, and this information was provided in the original version of the manuscript in section 3.2, p. 32930, last paragraph. These data are now shown on p. 10, 2nd paragraph.*

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.

> *We improved the manuscript by incorporating the advice of the 3 reviewers. The physical explanation and discussion of the results now provide further depth than in the previous version, as the reviewer will observe in the responses to the specific comments that we present in this response.*

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?)

> *Many references and comparisons to other stations were presented (for both the optical and microphysical parameters); however, it is true that we only presented a qualitative comparison to these stations, and no values are given. Our intention was simply to provide context for our station with respect to the nearest known reported measurements. Our work was not intended to present a review of the values corresponding to these parameters. The presentation of quantitative comparisons for all of the measured parameters would only increase the length of the manuscript, which contradicts the recommendation of reviewer #2.*

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 reviewer says that the data are "based on hourly means, apparently". In the manuscript, we clearly stated that we decided to work with hourly means (and justified our decision on p. 32930, last paragraph of the original manuscript), and therefore we do not understand the reviewer's doubts. In the new version of the manuscript, the caption of Table 1 includes information regarding the means to clarify further the fact that we are working with arithmetic averages.*
> *With respect to the statistics, we disagree with the reviewer on this point. The statistics are adequate for the form of data presented and are typically used for this type of study. See, for example, Aaltonen et al. (2006), Asmi et al. (2011), Garland et al. (2008), Virkkula et al. (2011) and many others.*

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

> *The measurement errors were calculated for the absorption and scattering measurements, and these values were considered in the analysis of the respective alpha parameters together with the fit error, as explained on p. 32937, line 3 of the original version. The calculated errors are not presented in the figures for the sake of clarity. The limits of detection and noise of the instruments were analyzed prior the campaign, and calibrations were performed when necessary, as described below in the specific comments. Information regarding the limits of detection of the PSAP instrument was not presented in the previous version of our work, but we now present this information in the new version (see the specific comments). We have been performing measurements using well-known commercial instruments that have been previously characterized in the references presented in the main text. In addition, we refer the reviewer to the previous comment regarding common practices used in similar studies.*

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.

> *Usually, I prefer to maintain a coherent presentation of digits for results. However, in this work, we present very different types of data obtained using different instruments, and the precision of each presented quantity is dependent on the measuring instrument. Therefore, we determined the precision according to the quantity measured, as well as the instrument used for the measurement.*

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.

> *Example trajectories are shown in the new version of the manuscript (Fig. 1). Reviewer #1 also expressed several concerns regarding this point. We have fully incorporated his suggestions, and we feel that this revision is one of the main improvements in the new version of the manuscript. This point is now clearly stated. Please see the specific comments for details regarding this point.*

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.

> *Three different situations are now identified in the time series graph (Fig. 2 of the new version): periods with new particle formation events, periods with other concentration increases (not related to new particle formation) and clean conditions. These three types of periods are also discussed in the text.*
> *In the new version of our work, the back trajectories are discussed in connection with the presented data (according to the suggestions of reviewers #1 and #2), and we think that this point strongly improves the manuscript.*
> *However, we recognize that in this work, the connections between the synoptic weather conditions and particle events have not been described in detail. We realize the importance of such an investigation, but we leave this analysis to future studies in the region using additional available days for the analysis. A three-month campaign did not provide sufficient data for a detailed study of this nature.*

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 paper has been restructured, and the back trajectories are now discussed together with the optical and microphysical parameters (according to the suggestions of reviewers #1 and #2). The introduction and the conclusion sections were rewritten to address the reviewers concerns. The methods (instruments) were moved to the methods section, and the primary objectives of our work are clearly stated in the introduction. The conclusions were also improved by presenting the main findings of our work.*

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 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?

One summer is not a sufficient timeframe to answer all of these questions, which is a good reason to continue those observations. The comparison of the ALOMAR station with the nearest stations presented several similarities (as described in detail in the text), although there were a number of specific characteristics that may warrant further interest in these data. We have plans to perform measurements at the ALOMAR station and determine the radiative forcing by aerosols and its contribution to climate effects; however, this determination is clearly beyond the scope of the present study.
The extrapolation of our data to other summers or other seasons of the year is overly risky. We have no reason to believe that this summer was an atypical one. The other seasons of the year have fewer sunny hours; in addition, this region has low aerosol levels, and therefore the effect of aerosols on the radiation is probably negligible.
During the summer of 2008, the most common air masses were those from the north (58 %) and from the west (22 %): section 3.4, p.32940 in the old manuscript and section 2.4, p.11 in the new version.

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.

We would like to point out that the manuscript was revised prior to submission, and it will be revised again before submission of the final version. None of the authors is a native English speaker, and therefore we will not argue whether it is correct to write "numerical size distribution" instead of "number size distribution"; however, several published references use this term. Of course, we changed this terminology throughout the manuscript, although the suggestion that the paper should be checked by a native English speaker is questionable, considering that reviewer #1 referred to our previous version as a "well-written analysis".

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.

We have significantly improved the manuscript with respect to the main concerns of this reviewer, as well as reviewers #1 and #2. We hope that the reviewer is able to change his mind after reading the new version of our work.

SPECIFIC COMMENTS

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

2. Abstract: See general comment above. Is it meaningful to refer to arithmetic averages and standard deviations?
   The reviewer criticizes our statistics, but he doesn't make a clear point. Does he think that we

should have based our analysis on the median instead of the mean or that the standard deviation is not useful here? This statistical treatment is adequate for the type of data presented and is also typical of studies presented in this field. See, for example, Aaltonen et al. (2006), Asmi et al. (2011), Garland et al. (2008), Virkkula et al. (2011) and many others.

3. Abstract: The meaning of the sentence starting with "Whereas..." (line 13) is very unclear.
   This sentence is not included anymore. The abstract presents a new structure.

4. Abstract: Last paragraph. "...presented lower optical parameter values..." (line 4) and similarly in line 6. Be more specific!
   In the abstract this sentence is changed to: "... the relationships between the air mass origins and the parameters studied allow us to describe two characteristic situations: that of the northern and western air masses, which were predominantly composed of marine aerosols and presented the lowest optical and microphysical values observed, indicating predominantly non-absorbent and coarser particles; and that of the eastern and southern air masses, in which continental aerosols were predominant and exhibited higher values for all parameters, indicating the presence of smaller absorbent particles." In section 3.4, pp.22, we specify how higher values were observed, with this information added to the text: "Sectors 1, 2 and 3 (S, SE and E) analyzed as a whole yield higher σa and σs coefficients and higher αa and αs exponents than in sectors 4, 5 and 6 (σa, σs, αa and αs are 27 %, 37 %, 5 % and 29 % higher, respectively, for sectors 1, 2 and 3 than for sectors 4, 5 and 6)."

5. 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 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.
   We believe, the reviewer means page 32925. The entire paragraph was removed to shorten the text, as recommended by reviewer #2. The introduction was restructured, and only one sentence from this paragraph (the one the reviewer refers to in line 5), located in the end of the introduction, is retained in the new version: "Whereas columnar aerosol properties are presented in Toledano et al. (2006), Rodríguez et al. (2011) and Rodríguez et al. (2012), no surface measurements appear to have been reported for the in situ properties of aerosols in our study area."

6. Line 8. Define "ultrafine".
   "Ultrafine" particles are those with diameters less than 0.1 μm (include the nucleation and accumulation modes).

7. 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.
   Of course, there are many contributing factors in the determination of the size distribution. We meant that there is a strong correlation between similar distributions and the origin of the air masses. Similar distributions are often derived from the same origin. The corresponding sentence was corrected in the main text: "Previous works (Birmili et al., 2001; Tunved et al., 2005) have observed that different size distribution properties can be related to the origin of the air masses, with similar distributions often arriving from the same origin."

8. 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!
The location is classified as "rural" because this area is not urbanized and has a low population density. This classification is not related to the location relative to the interior of the continent, as the reviewer appears to think. This location is influenced by westerlies, and the predominant air masses are marine, as demonstrated in the frequency histogram (Fig. 8, new manuscript). 80 % of the air masses presented a marine origin during the summer of 2008. See section 3.4, p.32940 in the old manuscript (section 2.4, p.11 in the new version). The measurements are taken inside the boundary layer, and therefore discussion of this fact is not needed.

9. 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?
This study refers to the objective of POLARCAT, which is related to the quantification of the optical properties and radiative effects of aerosols, as well as the impacts on surface albedo. As we explain in the introduction, in situ measurements were not previously available within a large area surrounding ALOMAR, and the location of the station (i.e., above the Arctic circle and with few local sources of anthropogenic particles) provided an excellent opportunity to study remote background aerosols. The summer season was selected due to the number of hours that the sun is above the horizon, as well as the objective of having the largest possible number of days with available measurements. In the other seasons of the year, there are fewer sunny hours, and, being a predominantly unpolluted region, the radiative effects of aerosols are probably negligible.

10. 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 introduction was rewritten to address the reviewer's concerns.

11. 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.
Also the conclusion section was rewritten according to the suggestions of this reviewer and reviewer #2.

12. 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.
No, this heating is not always observed. The minimum ambient RH is a daily average. During the day, the RH values can decrease to less than 40 %, and the sample air is not heated in these cases.

13. Furthermore, where does the cut-off diameter of 10 µm come from? Measurement by the APS? Is it constant? Was the inlet characteristics possibly modified if there were high wind speeds?
No, the APS could measure particles up to 20 µm and was not responsible for the cut-off

diameter. Our inlet system was calculated to have particle size cut-off of 10 µm. The calculation of the cut-off for the inlet system took into account the average wind speed. Strong deviations from the average wind speed could, of course, change the collection efficiency of the system.

14. 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?
    This information was added to the manuscript, section 2.2, pp.7: "The detection limit of the instrument was investigated by Springston and Sedlacek III (2007) and Müller et al. (2011) and for an averaging time of 160 s, the expected value approach 0.05 Mm^{-1}." And on section 2.3, pp.10: "... measurements taken during rainfall were typically below the detection limit of the instruments and were thus eliminated..."

15. Methods. Page 32928, line 27. The correct term is: differential –>mobility<– analyser.
    Corrected.

16. How was the SMPS operated? Full scan in how much time? What is the size resolution? What about charge correction? What about size calibration?
    As stated in the manuscript, 12 measurements were conducted per hour, which means that a full scan was performed every 5 min. The size resolution was 16 channels/decade, and a multiple charge correction was made using the TSI software. An intercomparison of our system and other in the Pallas Station (Finland) was carried out at the end of May 2008, prior to the ALOMAR campaign. Good agreement between instruments was found for 0.6 l min^{-1} polydisperse aerosol flow. Using this aerosol flow and 6 l min^{-1} for the sheath flow, the data were acquired in the size range of 10.7 - 392.4 nm.

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

18. 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?
    The study of the correlation between the parameter αs and the size of the particles can only be accurately analyzed if the supramicrometer and submicrometer fractions are separated because the consideration of all sizes biases the correlation, thereby giving more weight to the small particles, which are less sensitive to scattering. This was explained in the manuscript, section 3.3, p.32939 (p.21 in the new version).
    In addition, due to the low concentration of particles registered by the APS instrument, the error resulting from interpolation and conversion of the diameter values is not advantageous. This point is now clarified in the manuscript: "... This decision, together with the low concentration of particles registered by the APS, prevented us from merging the SMPS and APS data because the gap between the upper channel of the SMPS, 390 nm, and the lower channel of the APS, 500 nm, would require interpolation between these sizes and a conversion of the aerodynamic diameters provided by the APS to the mobility diameters provided by the SMPS, with associated errors resulting from both procedures."

19. 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.
This paragraph states that the data were, in fact, corrected according to the formula given by Bond et al. (1999)! The details regarding this correction were presented in Bond et al. (1999), and we do not see the purpose in repeating the explanation of this correction that was previously presented in their work.
The absorption coefficients were extrapolated to the wavelengths of the nephelometer using the Ångström exponent (this information has been added to the main text).
The corresponding sentence in lines 19-22 was rewritten as follows: "The data were corrected for these dependencies using the procedure described by Bond et al. (1999), as the algorithms presented by Bond et al. (1999) and Virkkula et al. (2005) agreed with the higher ω0 and smaller σa values and as no other values of σa > 6 Mm^{-1} were observed at the ALOMAR station during the measurement period."
Regarding the backscattering values, it was our decision not to present the data because for the majority of the measurement period, the values remained below the detection limits presented in the main text.

20. 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).
This formula was included to explain the size intervals considered in the calculations. This formula is now embedded in the text.

21. 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.)
This sentence means that we studied the concentrations of particles separately in each of these modes. The values of 30 nm and 100 nm are used as the cut-off values of the diameters between the modes, as previously stated in section 3.2, p.32934, line 13 (it is now the p.15, 3rd paragraph).

22. 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?
Yes, the nucleation mode extends below 10 nm, and when we present the range of 10-30 nm, we are referring to the size range in the nucleation mode that we could analyze using the SMPS data. The CPC counts particles of all sizes, including sub-micrometer, supra-micrometer and those in the gap at 390-500 nm that occurs between the SMPS and the APS data.

23. 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.
The reviewer is right and we corrected the manuscript accordingly.

24. 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?

Low values of all the parameters were registered by all of the instruments! We decided to work with hourly averages because if the values were averaged for the entire day, we would miss local events (e.g., new particle formation events). We stated in the manuscript (p.32931, line 2) that the data coverage was ~72 % due to the weather conditions. Gaps in the data were mainly attributed to the weather conditions; for example, during rainfall, the measurements were typically below the detection limit of the instruments and were eliminated. Also, a relatively small amount of data were lost as a result of handling of the instruments.

25. 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?
This is already answered in the previous point. We added this information to the new version of the manuscript: "... measurements taken during rainfall were typically below the detection limit of the instruments and were thus eliminated..."

26. 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.
The sentence was removed from this section. It is a conclusion from our data and it is presented in the conclusions now.

27. 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.
As we said before, we disagree with this reviewer's opinion on the statistics. See our response to comment 2.

28. 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.
This question was already addressed in the general comments. Please see the 3rd comment on p.1 of this response document.

29. Page 32932, lines 27-28. Is there any support to that sentence?
As we stated in the general comments, the study of the air mass origins is now greatly improved, and we think that this investigation is one of the main strengths of the new version of the manuscript. We provided further support of this sentence by adding reference examples of these situations (according to the suggestion of reviewer #1). The main text was changed as follows: "... These values may be due to long-range transport episodes from Central Europe and Russia. In Fig. 1, the air mass arriving from sector 3 (day July 14 - black line) is presented as an example of transport from Russia. The σs and σa values at 550 nm were 8.36 Mm^{-1} and 0.67 Mm^{-1}, respectively, while the αs and αa values were 2.10 and 0.37, respectively. For the sample trajectory arriving from sector 2 (blue line), σs and σa reached values of 9.53 Mm^{-1} and 0.61 Mm^{-1} , respectively, and the αs and αa values were 2.01 and 0.44, respectively. A sample trajectory arriving from Belgium (day June 21 - magenta line) is presented for sector 1. The σs

and σa parameters reached values of 11.48 Mm^{-1} and 0.74 Mm^{-1}, respectively, and the αs and αa values were 1.88 and 0.33, respectively."

30. 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.
    This question was already raised by reviewer #1 and we added this explanation to the manuscript: "... if the extinction Ångström exponent was lower than 1.1, the day was classified as possible aerosol dust event (Rodríguez et al., 2011). The back trajectories and MODIS images were then analyzed to confirm the dust events." During the period reported in this paper, no evident dust event was observed and the possible dust that arrived at the station was always mixed with pollution or other particles making it difficult to show the exact source. However, a more clear event was detected on 31 May-5 June 2008 (Cimel data), before we started the in situ campaign and another event was detected in the same station and reported by our group the year 2007. This event is fully described in Rodríguez et al. (2012), including the dust aerosol optical depth provided by the NAAPS model, the aerosol optical depth and the Ångström exponent provided by the Cimel sun photometer and the MODIS-Terra sensor. The reference to this event is also added to the manuscript, section 3.1.

31. 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.
    The reviewer is right, and we are not trying to justify that our value is lower than that obtained by Asmi et al. (2011) at Pallas; we simply call attention to the fact that we are not exactly comparing the same range of sizes.

32. 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).
    We did not say that we identified three types of weather conditions! We identified three types of situations, whose characteristics were specified in the following sentences, and the situations that we observed were usually associated with the same weather conditions. These periods are now identified in the time series graphs according to the suggestion of reviewer #1.

33. 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.
    The complete sentence says "The six-day period from 27 June to 2 July was selected for further analysis because it exemplifies the first situation, "clean conditions"." Therefore, if one reads the full sentence and knows that we are studying 3 situations, it becomes clear that this phrase refers only to the first situation.

34. 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?

The study of new particle formation events was based on the SMPS data, and these data are only available for greater than 10 nm. The reason that we did not use the CPC data for this study was previously explained in our response to comment 22.

35. Same page, 2nd paragraph. This is strangely structured. Sentence lines 21-23 should come before sentence lines 19-21.
    The reviewer is right and we changed the structure according to his suggestions.

36. Before that, what is most likely origin of the coarse mode peak?
    Our data don't provide enough information to answer this question.

37. Why is there no time series data for the coarse mode (APS based) data like for the data of the SMPS and CPC?
    We did not include the APS time series because it provides little additional information due to the low values registered. However, we have now included these data as supplementary material accompanying the paper.

38. 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?
    Fig. 7 is now included as supplementary material. This analysis is based on the work of Asmi et al. (2011), who studied the scatter plot of N30-100 versus N100 for several European stations and found that the shape of the scatter density contour can be interpreted to show the typical relationship between concentrations of the two modes for specific types of stations. The comparison of our Fig. 7 with the scatter plots presented by Asmi et al. (2011) is intended to compare the ALOMAR station with the other European stations studied by these authors.

39. 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?
    We emphasize that our goal of comparing the ALOMAR station with other stations is not intended to be exhaustive. We are only looking for similarities and differences with respect to other stations to understand the local situation, and a qualitative comparison is sufficient to accomplish this goal. If the reviewer does not believe us when we say that our parameters are higher or lower than those of other authors, he will also not believe us if we present the actual values from the other authors. Furthermore, the addition of these data would only increase the size of the manuscript.

40. Page 32936, line 11. "fit for each correlation" - what correlations are the authors referring to?
    Corrected to: "In this way, the slope of the linear fit for each correlation between the channels is the respective Ångström exponent."

41. 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.
    We present the evidence in the following sentence of the manuscript on p.32936, lines 16-21 (p.18 in the new version): "The line with smaller slope is associated with larger particles, ... The data that contribute to this line have a strong correlation with the number concentrations

of the coarse particles Nmicrometer (> 500 nm) as demonstrated by the value R = 0.9. The line with the higher slope is associated with smaller particles, ... and its data present a correlation of R = 0.6 with the concentration of particles in the accumulation mode, N100."

42. Line 17. How do marine aerosols come into play here? What is the argument?
The large size of the particles and their optical properties are likely to suggest the marine origin.

43. 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.
We added the following information regarding the correlations: "The line with smaller slope is associated with larger particles, most likely marine aerosols (also dust particles during desert events). The data that contribute to this line have a strong correlation with the number concentrations of the coarse particles Nmicrometer (> 500 nm) as demonstrated by the value R = 0.9 (R = 0.2 for the correlation with the accumulation mode). The line with the higher slope is associated with smaller particles, i.e., continental or polluted aerosols, and its data present a correlation of R = 0.6 with the concentration of particles in the accumulation mode, N100 (R = 0.3 for the correlation with the Nmicrometer)." This relationship between the optical parameters and the size of the aerosol is further explored in Fig. 6a and 7c, as specified in the main text of the revised manuscript.

44. 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 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.
This paragraph was completely rewritten, and the statements are better justified. The trajectory study supports the interpretations of regions A and B of the graph as being predominantly continental and marine, respectively. The continental urban sources relevant to the area of Andoya are Russia and central Europe, and example trajectories are presented for these situations in section 3.1, p.13 (new version).

45. 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?
We did not discuss different weather periods but simply observed that the weather conditions consistently coincided with specific periods related to the concentrations of the particles. The same type of relationship with the weather conditions was not obvious for the optical parameters.

46. 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?
We said "for a given σa value". To justify this observation, we classified the data in Fig. 6a into

two groups, αs < 1.3 (coarse particles) and αs > 1.3 (fine particles). In Fig. 6d, ω0 is also compared to αs to search for information regarding the size of the aerosols and to examine whether the optical data support the size distribution measurements. The joint analysis of Figs. 6a and 6c shows that for a given σa value, lower ω0 values correspond to higher αs values, and higher ω0 values correspond to smaller αs values. This result agrees with that obtained by Clarke et al. (2007), and considering that αs is higher for smaller particles (see Fig. 7c and its explanation in the text), one can conclude that for a given σa value, lower ω0 values correspond to smaller particles, and higher ω0 values correspond to larger particles. This explanation was added to the main text.

47. 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?
    Yes, it can occur but it is not due to that reason that the local air is no longer clean. This classification is derived from the fact that low values were registered in the optical and microphysical parameters together with the low frequency of occurrence of the episodes with long-range transport of small particles. The frequency of each air mass origin is presented in Fig. 8.

48. 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?
    We meant "supra-micrometer", and this has been corrected. As we explained earlier, our inlet was predicted to present a cut-off diameter of 10 µm. However, based on the APS data, we observed that the largest particles were typically less than 5 µm, and even those particles were detected at very low concentrations.

49. 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?
    Of course, one can use Mie theory and the size distributions to derive the scattering. However, as the reviewer has pointed out many times, we have our own data. We measured the microphysical and optical parameters, and in this section, we are comparing our experimental data to the expected theoretical results.

50. 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 understand.
    We explained this point earlier in this document (see comment 18). We do not consider this separate analysis to be a weakness. We chose this option because, as we explained previously, there are advantages to studying both fractions separately, whereas no advantage is attained by combining the fractions.

51. Line 20. Physical explanation?
    This subject was already addressed while responding to comment 46.

52. 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?
The high-altitude trajectories were only used in cases of uncertainty to understand better the sources and paths of the aerosols. In section 2.4, we explain that the mixed layer depth was obtained from the Hysplit model, and the air mass was considered to be a source of the aerosol particles that arrived or formed close to the measurement point if the 500 m air mass was inside the boundary layer for the majority of the time. This was confirmed using the Hysplit model.

53. 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.
This section is no longer located at the end of the manuscript, and the section is linked to the presentation of the data according to the suggestions of reviewers #1 and #2. To provide a sense of the distance traveled by the aerosols before arriving at our measurement point, example trajectories for each of the considered sectors are presented in Fig. 1, as suggested by reviewer #1.

54. Conclusions section. This is rather a summary and it suffers therefore from many of the inconsistencies and lack of explanations in the previous chapter.
The conclusion was rewritten and this problem is solved now with many explanations added to the main text.

55. What is the "spectral shape" referred to on page 32943, line 4.
At this point we are talking about the single scattering albedo, so it is the spectral shape of the single scattering albedo. But we rearranged the sentence like this: "The spectral dependence of the single scattering albedo was also analyzed and it mainly decreased with wavelength."

56. 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.
This point has already been answered in the general comments and in the specific comments 28 and 39.

57. What is the relevance of saying that the ratio of Aitken to accumulation mode was larger 1?
This conclusion arrives from the comparison with the study of Asmi et al. (2011) and the full explanation can be found in our response to the specific comment 38.

58. 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?
Regions A and B on Fig. 5f (new version of the manuscript). As explained in the main text, Region A, which has higher exponents due to the presence of smaller particles, presents the

characteristics of continental polluted aerosols, which may originate from continental urban sources. Region B, which has lower exponents due to the presence of coarse particles, which are clean and less absorbent, may be of marine origin.

59. Table 1. Why no P16 and P84 percentiles given like in Table 2?
    Table 1 and Table 2 are shown together now and P16 and P84 are presented for all parameters measured and calculated.

60. Table 2. Significant digits should be the same for all quantities and resemble the typical uncertainty.
    Different instruments were used for measure different quantities and the significant digits were decided according to the instrument.

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

62. 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?
    Done.

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

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

65. 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!
    We decided to use linear scale on the y *axis because we are comparing our distributions with those of Asmi et al. (2011), Garland et al. (2008) and Virkkula et al. (2011). All of them use linear scale.*
    In face of the low concentrations observed for the coarse mode, no difference would be visible in the combination of panels a) and b). Please note the scale 0-3600 cm^{-3} for the SMPS data on panel a) and the scale 0-6 cm^{-3} for the APS data on panel b).

66. 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?
    The reviewer is right about the significance of these maximums as we are talking about really low concentrations of particles, however, they were strong enough in some days to be noticeable in the mean distribution. It wouldn't be noticeable if we combine the SMPS and the APS data. The mention to the 3300 nm mode was a mistake in the manuscript, it should be 2300 nm.

67. Figure 9b. Defined regions can be hardly identified. The plot is too busy.
This figure is now grouped in the new figure 5 and we improved the plot for better visibility.