Interactive comment on "Seasonal variations in physical characteristics of aerosol particles at the King Sejong Station, Antarctic Peninsula" by Jaeseok Kim et al.

Anonymous Referee #1

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We thank the referee for valuable comments that we have used to improve our manuscript. We have considered the comments and have modified the manuscript accordingly. Our detailed responses to the referee's comments are below.

General remarks

The authors report the measurements of aerosol number concentrations, cloud condensation nuclei (CCN), black carbon, and meteorological conditions over a six-year period.

The authors claim in the Introduction that "it is necessary to have long-term observations at different regions because aerosol particles vary temporally and spatially." It seems to me that the authors do not make full use of the data measured for a relatively long time (March 2009 to February 2015).

Given extensive data available, interest in the paper would be enhanced if the authors had obtained more significant results. For instance, in addition to a seasonal trend, the authors should check if an annual trend exists for aerosol number concentrations, CCN, and air temperature.

I will discuss in detail several points which need to be broadened, analysed and corrected.

Authors' response: We did analysis for the annual trends of temperature, CN, and CCN concentrations as shown in Fig. S1-S3 of this response. As displayed in Fig. S1-S3, no clear annual trends of temperature, CN, and CCN concentrations are observed during the six-year period, mainly due to a relatively short observation period. For the analysis of long-term trends, authors reached a conclusion that longer term observations are needed, and not to include in the manuscript.

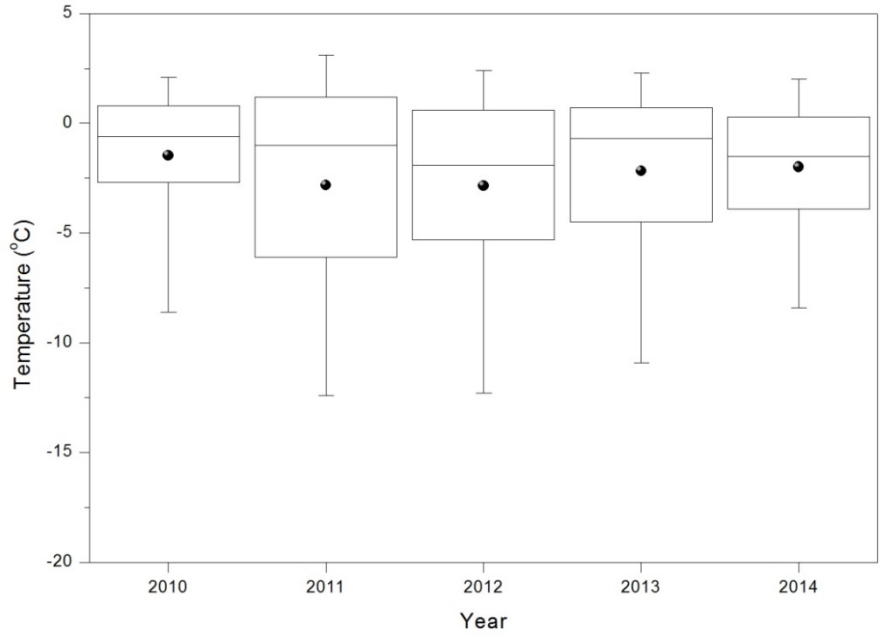

Figure S1. Box plot of annual variations of temperature during whole observation period. Lines in the middle of the boxes indicate sample medians (mean: circle), lower and upper lines of the boxes are the 25th and 75th percentiles, and whiskers indicate the 5th and 95th percentiles.

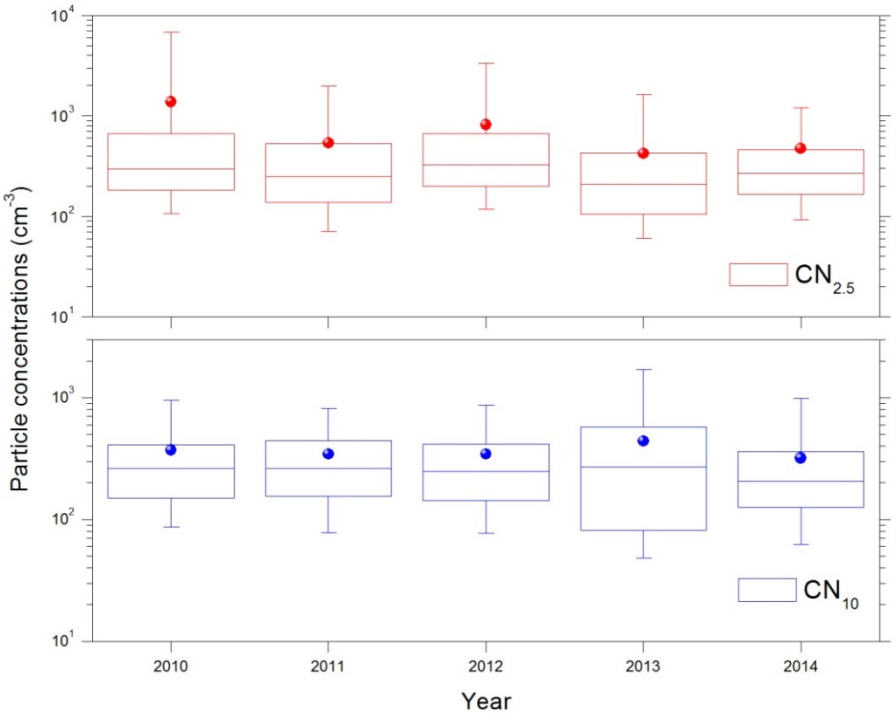

Figure S2. Box plot of annual variations of $CN_{2.5}$ and CN_{10} concentrations. Lines in the middle of the boxes indicate sample medians (mean: circle), lower and upper lines of the boxes are the 25th and 75th percentiles, and whiskers indicate the 5th and 95th percentiles.

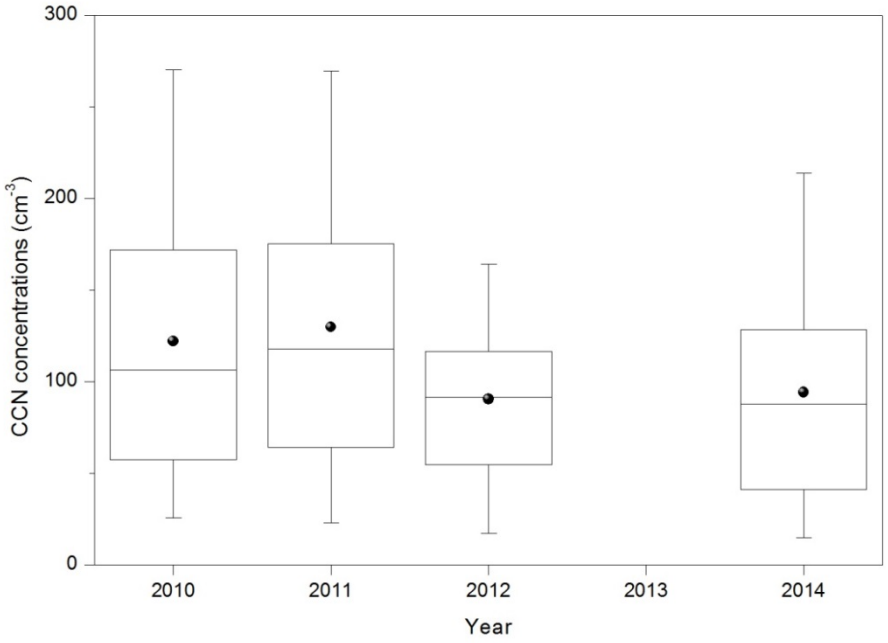

Figure S3. Box plot of annual trends of CCN concentrations. Lines in the middle of the boxes indicate sample medians (mean: circle), lower and upper lines of the boxes are the 25th and 75th percentiles, and whiskers indicate the 5th and 95th percentiles.

Specific comments

A) Page 5

Line 12 and following:

The experimental part should be broadened highlighting the important points. The authors should clarify:

a) If the relative humidity (RH) of the sampled air was adjusted (e.g. at 40%) or not at the inlet of the SMPS.

Authors' response: The aim of this study is investigate physical characteristics of aerosol particles in ambient condition. The RH controller was not used at the inlet of instruments.

To clarify that we did not use dehumidifier to readers, we added the following sentence in Page 5 Line 24:

"To maintain the ambient condition, any drying system was not used during sampling."

b) The length and diameter of the main tube and the tubes connecting the stack with the sampling devices, showing if the flow is laminar or turbulent.

Authors' response: Based on GAW aerosol measurements guidelines and recommendations, we installed cylindrical stainless common inlet. The diameter and length of the main common inlet were 0.1 m and 5.2 m, respectively. In order to calculate Reynolds number in the common inlet we used average values of air pressure and temperature. They were 98.8 kPa and -2.4 ^{o}C, respectively. The Reynolds number in the main tube was 2388. It represents that the flow in the main common inlet is transition regime (2000<Re<4000). For sampling, short L-bend tube made of stainless steel was placed at center of the main common inlet. Sampling was done by connecting instruments and main common inlet using conductive tubing. Diameter and length of the conductive tubing connecting the stack with the sampling devices are 3/8 inches and 0.6 m.

In the revised manuscript, we added the following paragraph on Page 5 line 13 to clarify the sampling method:

"Based on Global Atmosphere Watch (GAW) aerosol measurements guidelines and recommendations, we installed cylindrical stainless common inlet. The common inlet was placed on the roof of the observatory (Fig. 1). The diameter and length of the common inlet were 0.1 m and 5.2 m, respectively. In order to understand flow condition in the common inlet, Reynolds number was calculated. We used mean values of air temperature and pressure measured over the period from March 2009 to February 2015. The mean values of temperature and pressure were -2.4 ^{o}C and 98.8 kPa, respectively. The total flow rate of sample air was maintained as 150 lpm. The Reynolds number in the common inlet was 2388. It represents that the flow in the common inlet is transition regime (2000 < Re <4000). For sampling, short L-bend tube made of stainless steel was placed at center of the common inlet. Instruments were connected with the common inlet using conductive tubing to minimize the particle losses. Diameter and length of the conductive tubing connecting the stack with the sampling devices are 3/8 inches and 0.6 m, respectively."

c) If the total counting efficiency of the system (main line and sampling lines) was computed.

Authors' response: We estimated the total counting efficiency of the common inlet. First of all, we calculated inlet efficiency. We used average values of air pressure and temperature. They were 98.8 kPa and -2.4 ^{o}C, respectively. The inlet efficiency of aerosol particle range from 2.5 nm to 5 µm was about 1 (Baron and Willeke, 2001; Hinds, 1999). Then, we calculated efficiency of transport loss. We considered diffusion and sedimentation for calculating the efficiency of transport loss. We ignored loss from thermophoresis and coagulation. The efficiency of the sedimentation loss for aerosol particles range from from 2.5 nm to 5 um was about 1. And the efficiency of diffusion loss of aerosol particles was 0.92 for 2.5 nm particles and was about 0.99 for larger than 10 nm particles. Thus, the

total counting efficiency of 2.5 nm particles was 0.92 in the common inlet system used in this study, whereas ~~it~~ for larger than 10 nm particles was about 1. All sampling line except for the common inlet was conductive tubing. The conductive tubing has been used to minimize the known particle loss. Authors think above mentioned total counting efficiency do not have to be included in the manuscript.

d) If CPC and DMA were calibrated before and during the campaigns, which lasted about six years.
Authors' response: We always have extra CPCs *in-situ* which were maintained and calibrated by the manufacturer. If CPC had problems during observation period, overwintering researchers replaced bad CPC with extra CPC. The CPC in bad condition was sent to the manufacturer for maintenance and calibration. Status of instruments was checked every day by overwintering crews. Overwintering researchers regularly measured flow rate of CPCs (UCPC 3776 and CPC 3772) and calibrated zero count test for particle counter at the observatory. If flow rate and status of instruments were weird, we eliminated data during that period to improve data quality. The DMA was cleaned and calibrated for flow rate of sample and sheath air.

e) If particle, CN and CCN concentrations are shown in standard conditions.
Authors' response: CN and CCN concentrations of aerosol particles in ambient condition were measured by instruments in good condition. We filtered data when there are instruments error and malfunction symptoms. The dataset used in this manuscript are believed to be reliable data measured with the well running CPCs and CCNC.

Line 18 and following:
"The aethalometer was used to measure the concentration of light absorption particles at two wavelengths (370 and 880 nm). In this study, we used the results obtained by measuring light absorption at 880 nm to determine the BC concentrations."
Please insert the manufacturer of the aethalometer. The authors should clarify why they take into account aerosol absorption at 880 nm.
Authors' response: In this study, we used AE-16 model manufactured from Magee Scientific. In the revised manuscript, we insert the aethalometer model accordingly. Although we got results at two wavelengths (370 nm and 880 nm) from the instrument, manufacturer recommended that results obtained by measuring near-infrared wavelength (880 nm) for analyzing BC concentrations. Data obtained from 370 nm wavelength were usually used to analyze aromatic organic species. Because data at 370 nm wavelength do not have enough sensitivity for analyzing BC concentrations, 370 nm

wavelength of the aethalometer has not been mentioned in the revised manuscrip

We modified text to following text in Page 6 Line 4:

"The aethalometer (Magee Scientific, AE16) was used to measure the concentration of light absorption particles at 880 nm wavelength."

B) Page 7

Line 3 and following

"Fig.3 depicts monthly variations of the meteorological parameters measured from…

The authors should discuss possible correlations between the considered parameters, and possible variations in these parameters (e.g. temperature trend) during the period considered (2009 – 2015).

Authors' response: we added discussion of variations in temperature trend accordingly. As shown in Figure S1 in this response, the temperature variation does not have meaning due to relatively short period to verify the temperature trend. In addition, trend analysis of temperature and solar radiation are out of the scope of this manuscript. Instead of the trend analysis of temperature and solar radiation, we focused on correlation analysis between solar radiation and CN concentration, which is described in Figure 5 and section 3.2.1.

We added the following sentence at Page 7 Line 25:

"No clear annual trends of temperature are observed during a six-year period due to a relatively short observation period. In this manuscript, we focused on correlation analysis between temperature (or solar radiation) and CN concentration."

Line 6 and following

"…the observation site was relatively humid and warm condition compared to other Antarctic stations…"

The statement should be changed to: "..the observation site was relatively humid and warm compared to inland Antarctic stations..".

Authors' response: text was changed accordingly

Line 16:

"(DJF)…..(JJA)"

Should be changed to: …"maximum in the summer (from December to February, DJF) and minimum in the winter (from June to August, JJA)"

Authors' response: text was changed accordingly.

Line 20:

"There are no significant anthropogenic sources of aerosol particles in Antarctica, therefore, our results were in good agreement… "

The statement should be changed to: "Our results were in good agreement with the results…".

Authors' response: text was changed accordingly.

C) Page 8

Line 2 and following:

"The major compounds of aerosol particles found at a coastal Antarctic regions were non-sea salt sulphate and methane sulphonate (MSA) derived from oxidation of DMS produced by phytoplankton (Weller et al., 2011).

Weller et al.'s conclusion (2011) is different. They write referring to Neumayer station: "From thermodenuder experiments we deduced that the portion of volatile (at 125°C) and semi-volatile (at 250°C) particles which could be both associated with biogenic sulphur aerosol, was maximum during austral summer, while during winter non-volatile sea salt particles dominated.".

Atmospheric marine aerosol consists prevalently of primary aerosol (organic material, sea-salt) produced on the ocean surface by bubble bursting and wave crest disruption, and biogenic secondary aerosol (non-sea-salt sulphate and methanesulphonic acid from oxidation of DMS emitted by phytoplankton, and ammonium from biological reduction processes of N-cycle compounds).

During the 2002-2003 summer season, Fattori et al. (2005) reported that the coastal site ("Mario Zucchelli Station" in Terra Nova Bay) was affected by primary and secondary marine input: the sea spray contribution was dominant in the coarse fraction whereas the biogenic source prevailed in the fine fraction.

Authors' response: We understand that the text in the 1st version manuscript was not clear enough, possible to mislead the intention what we wanted to explain. Following referee's suggestion, we changed the sentence as to make the meaning clearer. Weller's conclusion is very well acknowledged, in this manuscript, we mean that highly CN2.5 concentrations during the austral summer season (DJF) most likely to be related to nss-sulphate and MSA derived from oxidation of DMS produced by phytoplankton since secondary formation aerosols play an important role in CN2.5 concentration.

To make clear the intention and meaning, we changed sentence in Page 8 Line 15 as:

"The high $CN_{2.5}$ concentrations during the austral summer season (DJF) should be related to non-

sea-salt sulphate and methanesulphonate (MSA) derived from oxidation of dimethyl sulphide (DMS) produced by phytoplankton (Weller et al., 2011).”

Line 8 and following

“The CN concentrations typically increase in the summer due to high biological activity, while they decrease in the winter when biological activity is low.…”

The CN concentration is related not only to biological activity, but also to primary aerosol. Primary aerosol includes inorganic salts, inorganic and organic mixture, and biological particles. The contribution of primary aerosol to the total aerosol concentration depends mainly on wind speed and the season, and it is higher in winter and lower in summer. In general, factors affecting total particle number concentrations are the air mass type, meteorological conditions, and whether or not nucleation-mode particles are present.

Authors’ response: Our intention was that difference between CN2.5 and CN10 concentrations typically increased in the summer season, whereas those in the winter decreased. Our hypothesis is that the trends of difference should be related to secondary aerosol formation caused by biological activity. Because temperature and solar radiation play an important role in the biological activity, we focused on the correlation between CN2.5 concentration and temperature, and between CN2.5 concentration and solar radiation.

To clarify our intention, we changed text to following sentence in Page 8 Line 21:

“The difference between $CN_{2.5}$ and CN_{10} concentrations typically increased in the summer season (DJF) due to high biological activity, whereas those in the winter season (JJA) decreased when biological activity is low. Our hypothesis is that trends of the difference should be related to secondary aerosol formation caused by biological activity.”

Line 14 and following

“Our results suggest that CN2.5 concentrations may be more closely coupled with solar radiation intensity than with temperature”.

The problem is more complex. An important parameter could interfere, i.e. the ocean temperature, which is different from air temperature. For instance, at the Antarctica research station Aboa Virkkula et al. (2009) observed that the annual maximum daily-averaged particle concentration was later, in February, than the maximum in solar radiation intensity. They concluded that the particle concentrations are more closely linked with the ocean temperature than with solar radiation. Peak sea temperature in polar regions is reached in late summer. As the authors measured both solar radiation

and CN concentration, it could be important to point out if there is a delay between the maximum CN and the maximum solar radiation.

Authors' response: Virkkula el al. (2009) compared daily average particle number concentration and solar radiation from December 2003 to April 2004. Compared with our data-set, it seems that their observation period is not long enough to clearly verify correlation between particle concentrations and solar radiation. Cayan (1980) reported that sea surface temperature and air temperature have roughly same variance. We assume that seasonal trend of ocean temperature should be similar to those of air temperature. We also compare monthly variations of CN2.5, temperature, and solar radiation, as can be seen in Figure S4 of this response. CN2.5 concentration sharply decreased from March, while temperature decrease occurs later, say in May, whereas solar radiation gradually decreased from February. Although temperature gradually decreased in the winter, in addition, CN2.5 concentrations were stable as well as solar radiation. Correlation between CN2.5 concentrations and temperature, and between CN2.5 concentrations and solar radiation would be different month by month. However, relationship among monthly mean values of CN2.5, temperature, and solar radiation was investigated and explained in this study (see Figure 5 and discussions of the manuscript).

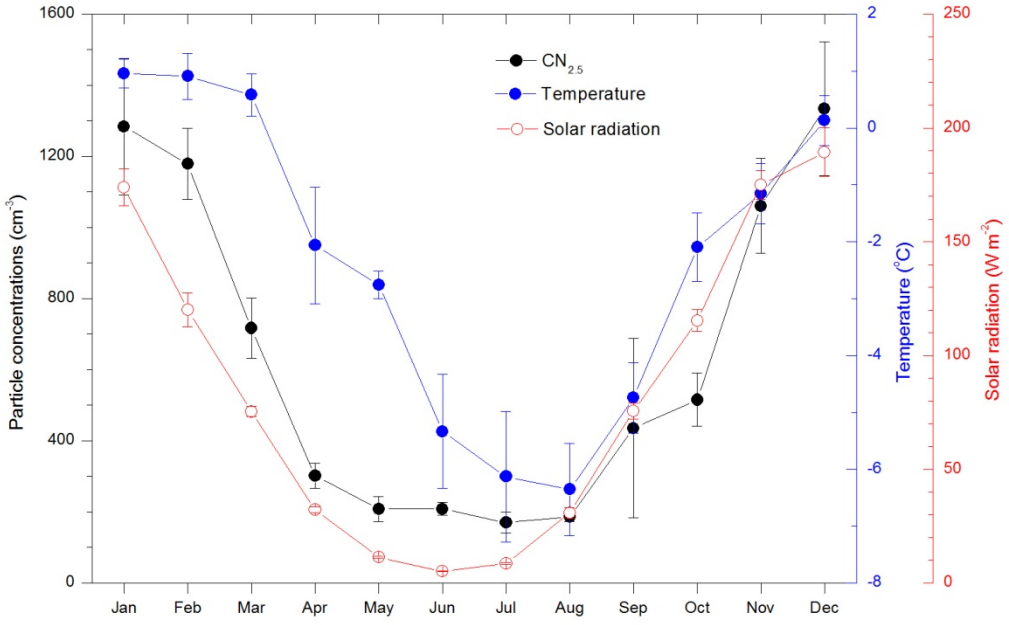

Figure S4. A comparison of monthly variations of $CN_{2.5}$ concentrations, temperature, and solar radiation during whole observation period.

In the summer 2014 the CN2.5 and CN10 concentrations are much lower compared to remaining

considered years (Fig.4), but the solar radiation (Fig. 3) remains roughly stable during the summer in the years 2010-2015. These data should be explained.

Authors' response: The reasons for lower CN2.5 concentrations in the summer 2014 could not be explained by solar radiation and temperature because solar radiation and temperature did not show any distinctive variation compared with other years. Other metrological parameters such as wind, air pressure, and RH could not explain the lower CN2.5 concentrations neither. The possible reason is the type of air masses reached to the sampling site. As shown in Figure S5, Case IV where air mass was originated from the South Pacific Ocean was dominant in the summer. Remarkable results in the summer 2014 were that frequency of Case II (air mass was originated from the South Atlantic Ocean) was high and frequency of Case IV was lower than other years. Based on the air mass back trajectory analysis as explained in Sec 2.2 of the manuscript, frequency of four types of air mass were compared in summer season. Seasonal trends of CN2.5 concentrations were different according to air mass history as shown in Figure 13 in the manuscript. In case of Case II, peak CN2.5 concentrations were in November, while maximum CN2.5 concentrations of Case IV were in February. Therefore, it is that increasing frequency of air mass originated from the South Atlantic Ocean (case 2) would explain this lower CN2.5 concentration.

To explain lower CN2.5 concentrations during the 2013-2014 summer season, we added the fowling paragraph on Page 9 Line 7:

"Unique results of $CN_{2.5}$ concentrations were observed as shown in Fig. 4. The $CN_{2.5}$ concentrations in the summer season of 2013-2014 were much lower than other years. Unfortunately, the reason for the lower $CN_{2.5}$ concentrations could not be explained by solar radiation intensity and temperature because the solar radiation and the temperature did not show any distinctive variation compared with other years. The possible reason is type of air masses reached to the sampling site. Although air mass originated from the South Pacific Ocean (Case IV: descriptions of the cases I, II, III and IV are described in section 3.3) *was dominant in the summer, based on the air mass back trajectory analysis as explained in Sec 2.2, frequency of air mass originated from the South Atlantic Ocean (Case II) in the summer of 2013-2014 was higher than other years and frequency of air mass originated from Case IV was lower than other years. In case of Case II, peak $CN_{2.5}$ concentrations were in November, while maximum $CN_{2.5}$ concentrations of Case IV were in February. Therefore, it is*

that increasing frequency of air mass originated from the South Atlantic Ocean would explain this lower $CN_{2.5}$ concentration."

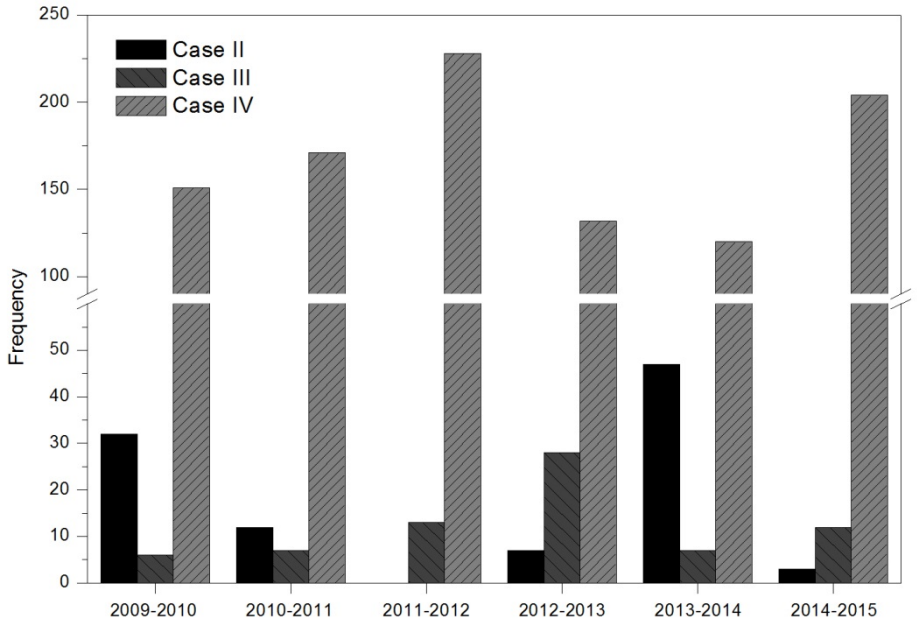

Figure S5. Frequency of air masses reached to sampling site depending on air mass history in the summer only

Line 17 and following

"The monthly mean CN concentrations increased from September to February…"

Several papers report the annual variation of CN with the maximum in summer and the minimum in winter in coastal areas (Bigg et al., 1984; Gras and Adriaansen, 1985; Gras, 1993; Jaenicke et al, 1992) and in inland stations (Bigg et al., 1984; Samson et al., 1990). A few references should be cited.

Authors' response: We add a few references.

D) Page 9

Line 10 and following:

"The clear seasonality of CCN concentrations is probably caused by the seasonal trend of CN concentrations…"

The statement should be changed to: The clear seasonality of CCN concentrations follows the trend of CN.

Authors' response: text was changed accordingly.

E) Page 10

Line 3 and following

"The fraction at the SS value of 0.2% during the winter(JJA) was similar to those measured in Mace Head and Finokalia, which are regions representative of marine environment."

"Our observations suggests that the major components in the aerosol particles that are activated to CCN at an SS of 0.2% should be hygroscopic sea salts during winter, while compounds less hygroscopic than sea salt would be dominant during the summer.".

Comparison with the Finokalia site appears inappropriate, as Bougiatioti et al. (2009) performed measurements at Finokalia from mid-June to mid-October (i.e. summer and autumn), not in winter. In addition, Bugiatioti et al. state that "Finokadia is located at a unique "crossroad" of aged aerosol types (marine boundary layer, Saharan desert, European sub-continent, biomass burning events) during the summer period." In the case of King Sejong Station, only a few cases of air masses originated from the continent of South America were shown. The authors' conclusion appears to be oversimplified and should be better explained. In addition to sea-salts, sea-spray aerosol includes organic material (prevalently water insoluble) which possesses a low hygroscopic growth factor, while simultaneously having a CCN activation efficiency higher than soluble non-sea-salt sulphate (Ovadnevaite et al., 2011).

Authors' response: Paramonov et al. (2015) compared results from CCNC measurements 14 sites around world. The sampling sites were just grouped according to location in their study. For instance, Finokalia, Mace Head and RHaMBLe campaign were representative sites for marine environment. However, Bougiatioti et al. (2009) measured physical and chemical characteristics of atmospheric aerosols according to air mass history during the short-term campaign at the Finokalia site. Although they showed different characteristics of aerosols depending to origin and pathway of air masses, it is not clear enough due to results from short-term measurements. To clarify, we have removed the comparison with Finokalia data in the revised manuscript.

In the revised manuscript, text was changed on Page 11 Line 14 as:

"The fraction at the SS value of 0.2% during the winter (JJA) was similar to those measured in Mace Head, which is a representative site of a marine environment (Paramonov et al., 2015)."

CCN activation efficiency depends on the constituents of aerosol particles (Dusek et al., 2006; Ovadnevaite et al., 2011). As only CCNC dataset are available in this study, there exists a limitation to infer the chemical compounds of aerosol particles. Thus, we discarded the citation of chemical

compounds (e.g. sea salts) of aerosol particles in the revised manuscript.

We changed sentence to following text in Page 11 Line 17:

"Although CCN concentrations were low in the winter, our observations suggest that aerosol particles that are activated to CCN during the winter season should be more hygroscopic than those during the summer period."

Line 8 and following

"Fig. 10 illustrates the seasonal variations in the mean activation ratio of CCN concentrations at an SS of 0.4%..."

As in the previous paragraph, the authors discuss the trend of CCN concentration at SS= 0.2 %, I expect the authors considered the seasonal variations of the ratio between CCN and CN concentrations at supersaturation 0.2%, instead of 0.4%.

Authors' response: The main purpose of this section is to see the seasonal variations of CCN activation ration at SS=0.4%.

To avoid confusion for readers, we divided the section on Page 11 Line 20:

"3.2.3Activation ratio and Fitting parameter of CCN."

F) Page 11

Line 16 and following

"The BC concentrations observed at our station were slightly higher than those at other stations in Antarctica…." "Additionally, no clear seasonal patterns were observed in our study throughout the entire observation period.".

The BC concentrations measured by the authors are much higher than those at other Antarctic stations. Please compare the concentrations shown in the paper concerning South Pole, Halley, Neumayer, and Ferraz station (0.65 ng m-3, 1.0 ng m-3, 2.6 ng m-3 and 8.3 ng m-3. respectively), with those measured at King Sejong Station. The very high concentration measured needs to be explained. In addition, it appears to me that a seasonal trend can be noted from Fig.12, i.e. prevalently lower values during winter.

Authors' response: The main scope of this manuscript is to understand seasonal trends of CN and CCN concentrations. Data from CN and CCN concentrations when BC concentrations were higher than 100 ng m-3 were discarded to improve data quality, while we used raw BC data for analysis of trend of BC concentrations. For this reason, mean BC concentrations were slightly high (64.68 ng m-

3). If we discard data when BC concentrations were higher than 100 ng m-3, the mean BC concentrations sharply decreased as 27.43 ng m-3 during whole observation period. However, mean BC concentrations in this study were higher than those measured at other Antarctic stations (e.g. South Pole, Halley, and Neumayer). The reason for the higher BC concentrations might be related to location of sampling site. There are nine permanent on-site stations on the Baton Peninsular of King George Island. In particular, six stations are located within a 10 km radius from the King Sejong Station. There should be extra bias of data from BC measurements due to effect of other stations.

We changed the Figure 12 and modified text of manuscript on Page 13 Line 3:

"To eliminate effect of local pollution on observations, in this study, data where BC concentrations were higher than 100 ng m^{-3}, were discarded. The BC concentrations varied between 1.07 ng m^{-3} and 75.97 ng m^{-3}, with a mean of 27.43 ± 4.98 ng m^{-3}."

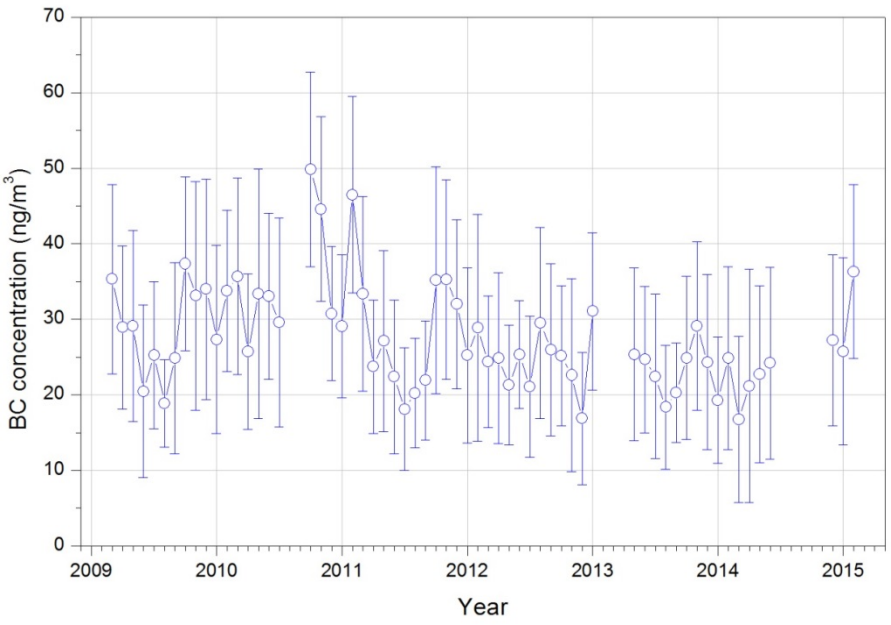

Figure 12. Monthly mean concentrations of black carbon over the period from March 2009 to February 2015. Here the error bars represents the standard deviation of the measurements from the mean value.

In the revised manuscript, we added fowling text to explain high BC concentration compared with results from other Antarctic station on Page 13 Line 9:

"The reason of the higher BC concentrations might be related to location of sampling site. There are nine permanent on-site stations on the Baton Peninsula of King George Island. In particular, six

stations are located within a 10 km radius from the King Sejong Station. There should be extra bias of data from BC concentrations due to effect of other stations."

As already mention of response, it is not main aim of this manuscript to understand BC trend. Data of BC concentrations were used to remove effect of local pollution on CN and CCN analysis. It would seem that there was seasonal trend of BC concentrations during short-term. For example, BC concentrations in the winter were lower than those in the summer during 2011. However, the BC concentrations in the winter were the highest in 2010 and no clear trend of BC concentrations was monitored during 2012. Because the variation of BC concentrations does not have meaning due to relatively short period to verify the BC trend, thus, we do not comment on seasonal trends of BC concentrations in this manuscript.

G) Page 12

Line 6

As mentioned earlier in Sec. 2.3

Please change to: As mentioned earlier in Sec. 2.2

Authors' response: Thank you for correction. Text was changed accordingly.

Line 8

"Although they are unreliable due to the low observation frequency…."

I suggest changing this statement to: "The very few cases of air masses originated from the continent of South America show the highest BC and CCN concentrations (Table 1).

Authors' response: text was changed accordingly.

H) Page 14

Line 3 and following

"The activation fraction of aerosol particles at the King Sejong Station….was lower than at the Arctic sites indicating that less hygroscopic compounds in aerosol particles should be dominant".

No reference is shown for Arctic sites. The conclusion appears superficial. I recall Ovadnevaite et al.'s paper (2011) which shows that seaspray aerosol enriched in primary organic matter (prevalently hydrophobic) possesses more CCN activation efficiency than more soluble particles dominated by nss-sulphate.

Authors' response: We add a reference in the manuscript. Lathem et al. (2013) showed CCN activation efficiency measured in Arctic area with aircraft during summertime. Based on physical

properties of aerosol particles, it is impossible to exactly understand chemical compounds of aerosol particles. Chemical characteristics of aerosol particles can be just deduced. We modified text to eliminate misunderstanding to readers.

In the revised manuscript, we modified text to following on Page 15 Line 26:

"It suggests that aerosol particles in Antarctic Peninsula should be less hygroscopic than those in Arctic."

Line 13

"Although the BC and CCN concentrations were the highest when the air mass originated from the South American continent, the results are not significant because only a small amount of data was analyzed.".

I suggest changing this to: "The very few cases of air masses originated from the continent of South America showed the highest BC and CCN concentrations."

Authors' response: We changed a sentence as referee's suggestion.

"The very few cases of air masses originated from the South American continent showed the highest BC and CCN concentrations."

I) Page 21

Figure 1 shows devices like OPC and Nephelometer, not used in the measurements.

Authors' response: In this manuscript, we didn't show date form OPC and Nephelometer. We modified Figure 1 to reduce confusion.

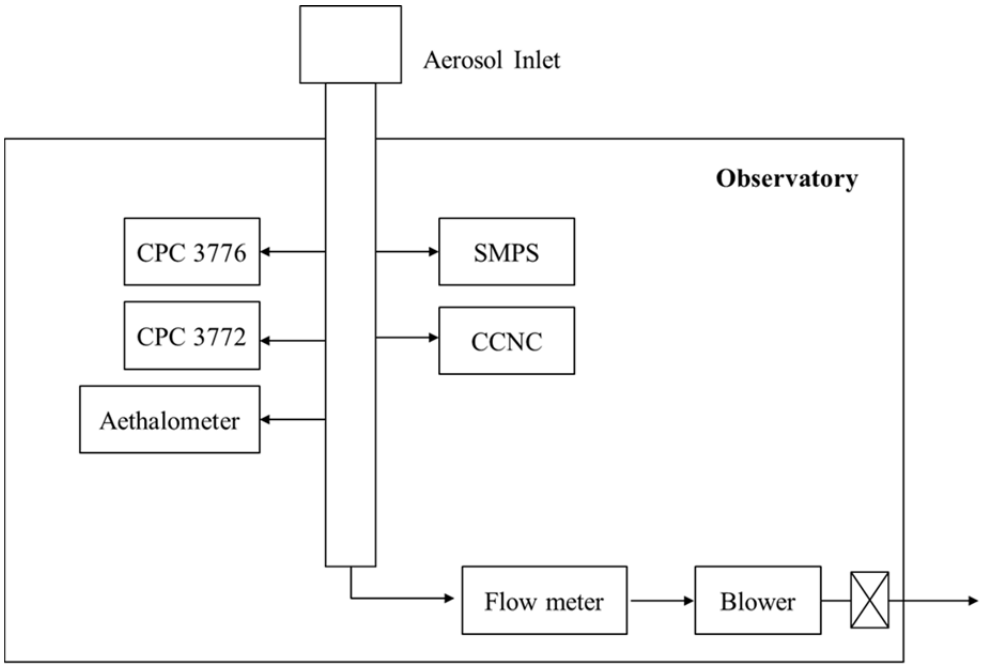

Figure 1. A schematic diagram for the observation methods used in this study.

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Interactive comment on "Seasonal variations in physical characteristics of aerosol particles at the King Sejong Station, Antarctic Peninsula" by Jaeseok Kim et al.

Anonymous Referee #2

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We thank the referee for valuable comments that we have used to improve our manuscript. We have considered the comments and have modified the manuscript accordingly. Our detailed responses to the referee's comments are below.

This manuscript summarizes measurements of aerosol physical properties (size distributions and number concentrations) as well as CCN concentrations performed at a research station in Antarctica. These data are needed to establish quantitative points of reference for a region that may face dramatic changes as a result of climate change. Because of this, this manuscript fulfills an important role and is suitable for publication in ACP.

I do feel that the authors have missed some opportunities in their presentation of the data to dig a little deeper into their data. I would like for the authors to comment on some of the more obvious issues to me:

(1) Very little is mentioned about the particle size distributions. For example, if new particle formation is expected (and there are several references to suggest this in the text), then what do the SMPS data tell us about the nature of the particle formations events. To address this question the data from the size distribution shown in Fig. 8 could show "box-whisker" data that better account for less frequent new particle formation events.

Authors' response: The main purpose is to understand general physical characteristics of aerosol particles at the King Sejong Station in Antarctic Peninsula. Because the study on new particle formation events is out of scope, in the manuscript, brief description about the particle size distributions was done and the particle size distribution data were used to support explanation for monthly trends of CCN concentrations as shown in Fig. 8. In accordance with referee's suggestion, we also showed "box-whisker" data of particle size distribution as shown in Fig. S1. Although there is much outlier in summer season, we don't know that it indicates frequency of new particle formation events. We are preparing other manuscript related to new particle formation events and are

analyzing deeply and carefully about results of the particle size distribution.

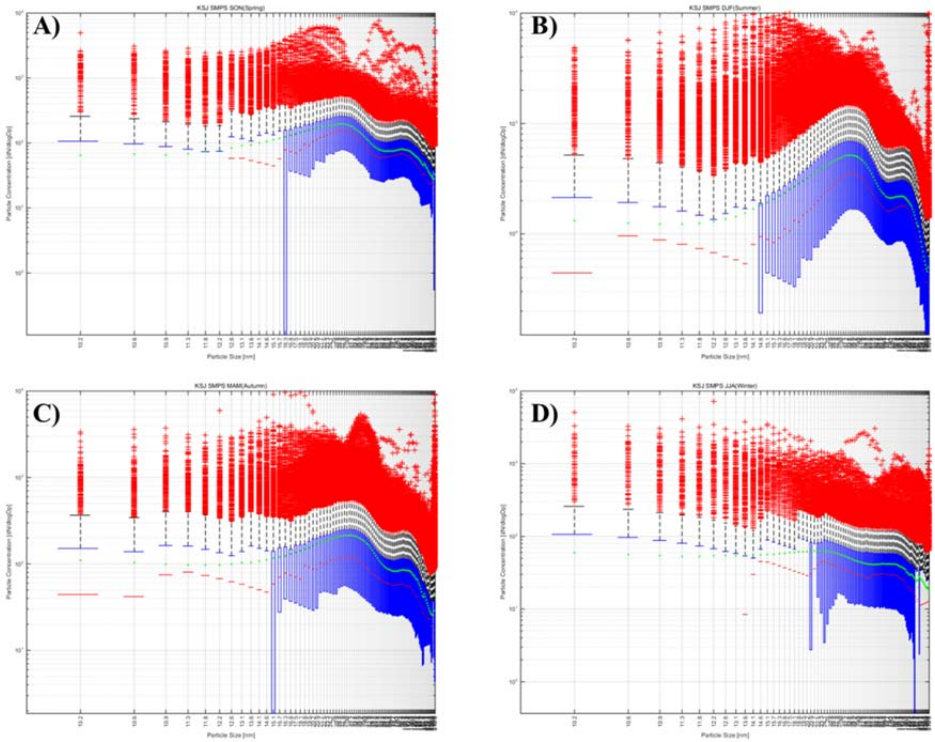

Figure S1. Box-Whisker plot of particle size distribution during (a) spring, (b) summer, (c) autumn, and (d) winter season. Red bars in the boxes indicate median values (mean values: green circle), whiskers indicate the 5th and 95th percentiles, and red cross out of whiskers represents outliers.

(2) Another missed opportunity is the lack of a thorough analysis of the CCNC data. Other than reporting concentrations at specific supersaturations, the "spectrum" of CCN activity is not really discussed in this paper. Some fitting parameters, namely "C" and "k", are presented but one of the most widely used parameters, kappa from k-Koehler theory, is not even mentioned. While I do not demand consideration of these issues as a condition for publication, I do urge the authors to consider how a somewhat minor investment in time might add great value to this publication.

Authors' response: If chemical constituents of aerosol particles are size-dependent, it is complicated to estimate CCN concentrations by using particle size distribution. Because size-dependent chemical information of aerosol particles is not available in this study, an empirical parameterization using in-situ CCN measurements was used in this study. The CCN concentrations as a function of SS were fitted with an equation of the form $Nccn=C(SS)^k$, where Nccn is CCN concentration at a certain SS value, and *C* and *k* are the fitting parameters. The fitting was done separately for each SS cycle of CCNC data. Besides that we have focused and found the fitting values c and k values, we tested

single hygroscopicity parameter, kappa in κ-Köhler theory, as suggested by referee. We roughly estimated kappa by using monthly mean CCN concentration at SS of 0.4% and monthly mean total particle concentrations obtained from SMPS data. The kappa is approximately 1.18 in December. The possible reason for the high kappa value would be explained by limitations of measurements available. For example, we decided the critical diameter (d_{crit}) by comparing CCN concentration with the integrated SMPS data. In addition, CCN concentrations were measured without size-selection. We decided not to mention the kappa value in the revised manuscript.

The following are a list of edits and concerns, with the page and line numbers listed:

P2, line 5: lager -> larger

Authors' response: text was corrected accordingly.

Line 10: This abstract should not assume that the reader understands what C and k refer to. If you want to include these in the abstract you need to explain what they are.

Authors' response: We agree with referee's opinion. To help reader's understanding, text was added to explain what the *C* and *k* are.

We added following sentence on Page 2 Line 9:

"Based on measured CCN data at each supersaturation ratio (SS), empirical parameterization were also fitted using formula expressed by power-law function ($N_{ccn}=C\times(SS)^k$), where N_{ccn} is the CCN concentrations at a given SS, and *C* and *k* are the fitting parameters."

Line 17: improper use of semicolon. Replace with colon

Authors' response: text was corrected accordingly.

P3, line 9: "unascertained" may be a word (I'm not sure) but many simpler words like "undetermined" or "unknown" are better suited.

Authors' response: text was corrected accordingly.

Line 10: remove "their"

Authors' response: text was corrected accordingly.

Line 21: e.g. not i.e. (this is not an exhaustive list of physical properties).

Authors' response: text was corrected accordingly.

P6, line 1: "decided" should be replaced with "determined"

Authors' response: text was corrected accordingly.

Line 2: delete the second "only"

Authors' response: text was corrected accordingly.

Line 7: what model of DMA was used?

Authors' response: We used a cylindrical DMA similar to the TSI 3081 model. The length, inner diameter, and outer diameter of the DMA were 44.42 cm, 0.953 cm and 1.905 cm, respectively.

We added following sentence in Page 6 Line 17:

"The length, inner diameter, and outer diameter of the DMA were 44.42 cm, 0.953 cm, and 1.905 cm, respectively."

Line 12: "Besides" might be better replaced with "in addition" (this is clearly a style suggestion)

Authors' response: text was corrected accordingly.

18: "were" should be "was"

Authors' response: text was corrected accordingly.

P7, line 8: Please review this sentence and correct grammar

Authors' response: text was corrected accordingly.

"The solar radiation varied from 2.3 W m^{-2} to 375.4 W m^{-2}, with a mean value of 81.2 ± 38.9 W m^{-2}."

P9, Line 1: Please explain why, for this analysis, a supersaturation of 0.4% was chosen

Authors' response: Anttila et al. (2012) measured cloud droplet number concentration (CDNC) and CCN concentration at five different SS values (0.2, 0.4, 0.6, 0.8, and 1.0%) during the third Palls Cloud Experiment (PaCE-3). The campaign was performed from 11 September to 11 October, 2009. According to their results, they found high correlation between CDNC and CCN concentrations at a supersaturation of 0.4%. The CCN concentrations at less 0.4% supersaturation were lower than CDNC, while the CCN concentrations at higher than 0.4% supersaturation were higher than CDNC.

Based on this result, variation of CCN concentrations at the supersaturation of 0.4% was analyzed in this study.

We added following sentence on Page 10 Line 7:
"Anttila et al. (2012) measured cloud droplet number concentration (CDNC) and CCN concentrations at five SS values (0.2, 0.4, 0.6, 0.8, and 1.0%) during the third Palls Cloud Experiment (PaCE-3). They showed correlation between CDNC and CCN concentrations at each supersaturation. The relationship between CDNC and CCN concentrations at the SS value of 0.4% was approximately linear, while CCN concentrations were lower than CDNC when the SS value was lower than 0.4% and CCN concentrations at upper 0.4% higher than CDNC. Based on this result, in this study, the supersaturation of 0.4% was chosen to investigate seasonal variations of CCN."

Line 10: is this redundant with the previous sentence in line 5?
Authors' response: We agree with referee's opinion. Because meaning of two sentences was similar, in same paragraph, we removed the sentence in line 5.

In the manuscript, we remove following sentence in Page 9 Line 5:
"It was similar to the seasonal cycle of the CN concentrations."

Line 19: any evidence of growth from Aitken to nucleation mode?
Authors' response: If the referee raises an issue if there is any evidence of growth from Aitken mode particle to CCN size, we cannot provide any direct evidence of this. Nevertheless, we compared monthly mean particle concentration measured from SMPS with CCN concentrations. The calculations showed that the number of accumulation mode particles cannot explain the measured CCN concentrations. It means that the rest of CCN are from the growth of smaller than accumulation mode particles, say from the Aitken mode, or residuals of cloud process. We could not provide direct evidence to distinguish these processes, we modified the sentence as:

Page 11, line1 has been changed to
"Accumulation mode particles can easily act as CCN (Dusek et al., 2006), hence CCN concentrations increase during the summer and decrease during the winter."

P10, Line 15: correct spelling of Arctic
Authors' response: Thanks. We checked spelling and changed.

P11, Line 5: how well did this model fit the data? There is no mention of this.

Authors' response: We estimated *C* and *k* values by using daily mean CCN concentrations at each SS value. The average correlation coefficient, r, was 0.978. We think it was a good fit.

In the revised manuscript, we added following sentence in Page 12 Line 18:

"The average correlation coefficient, r, was 0.978."

Line 17: period (not periods)

Authors' response: text was corrected accordingly.

P13, Line 6: no comma after "for"

Authors' response: text was corrected accordingly.

P14, Line 5: all mention of CCN concentrations need to state the SS

Authors' response: In summary section, we missed SS values for explaining CCN concentrations. It can give readers confusion. Thus, we modified sentence in the manuscript.

To clarify we modified sentence to following text on Page 15 Line 22:

"In addition, we presented the clear seasonal trends of CCN concentrations at the supersaturation of 0.4%."

Line 10: again, I don't think the reader knows immediately what C and k refer to. If the authors want this section to summarize results I would suggest explaining this to the reader.

Authors' response: We agree with referee's opinion.

To clarify meaning of *C* and *k*, we added following sentence on Page 16 Line 3:

"The C and k are constants were estimated using approximate formula expressed by a power-law function ($N_{CCN}=C\times(SS)^k$) (Twomey 1959)."

Figure notes: Figs 4, 8, 10-13: what do the error bars represent? This needs to be in the caption.

Authors' response: The error bars represent a standard deviation.

We add caption in Figures 4, 8, 10-13.

"Here the error bars represent the standard deviation of the measurements from the mean value."

Figure 9. Why not normalize to total N?

Authors' response: To investigate seasonal variations of fractions of CCN concentration at each SS value in aerosol particles activated CCN at a SS of 1.0%, we normalized it to CCN concentrations at a SS of 1.0% without total N.

Fig 13: seems statistically the same to me.

Authors' response: In this Figure, we found that seasonality of $CN_{2.5}$ concentrations were different in accordance with the air mass history. For instance, the $CN_{2.5}$ concentrations originating from the South Atlantic (Case II) were the highest in November, whereas the $CN_{2.5}$ concentrations originating from the South Pacific (Case IV) were the highest in February as can be seen in Figure 13. This is probably due to difference in chemical compounds that contributed to aerosol formation processes and/or in variations of biogenic activity according to the origin and transport pathway of air masses. This analysis has been explained in the section 3.3. Unfortunately, we don't have chemical data of aerosol particles depending on air mass. To verify our hypothesis, further studies on chemical compositions of aerosol particles need to be carried out in the future.

Fig 14: needs to show something about the variability of the size distributions.

Authors' response: we showed the variation of modal diameter and number concentrations of the size distribution as can be seen in Table 2. In the revised manuscript, the following sentence on Page 14 Line 20 was mentioned.

"The modal diameters with standard deviation and number concentrations are summarized in Table 2. It is obvious that the modal diameters during the summer are larger than those during the winter for both Aitken and accumulation modes; ~~0.023 μm in the winter and 0.034 μm in the summer for the Aitken mode and 0.086 μm in the winter and 0.109 μm in the summer for the accumulation mode.~~ The number concentrations for the summer are also higher than the ~~value~~ for the winter for the Aitken and accumulation modes, 49.16 ± 3.88 cm^{-3} during the winter and 304.36 ± 20.10 cm^{-3} during the summer for the Aitken mode and 44.78 ± 14.24 cm^{-3} in the winter and 140.25 ± 10.64 cm^{-3} in the summer for the accumulation mode."

Reference

Anttila, T., Brus, D., Jaatinen, A., Hyvärinen, A. P., Kivekäs, N., Romakkaniemi, S., Komppula, M.,

and Lihavainen, H.: Relationships between particles, cloud condensation nuclei and cloud droplet activation during the third Pallas Cloud Experiment, Atmos. Chem. Phys., 12, 11435-11450, 10.5194/acp-12-11435-2012, 2012.