We would like to thank all three reviewers for their constructive comments that we believe will certainly improve this manuscript. Our responses to the reviewers' comments are presented below in separate sections. The replies are formatted such that we first show the reviewers' comment in italic and after this present our replies. Revisions made in the manuscript are highlighted by ' in the beginning of the inserted text and by ' at the end. In the revised manuscript, sections that have been altered are marked in yellow, with the exception of minor technical corrections.

Anonymous Referee #1

SPECIFIC COMMENTS:

Abstract: 'indirect aerosol effect' has typically referred to an influence that aerosol have on cloud albedo. Here you really look at two effects: (1) a direct microphysical effect of aerosol on droplet size and (2) an indirect aerosol effect on precipitation intensity. Try to be precise. The use of these poorly defined terms is too often used without clarity.

We have rewritten this sentence in the abstract. The sentence has been changed from 'The merged data are used to examine the indirect aerosol effects on convective clouds over the Nordic countries.' to 'The merged data are used to examine how aerosols affect cloud droplet sizes and precipitation from convective clouds over the Nordic countries.' (Lines 19-21 revised manuscript)

Abstract: Clouds with greater vertical extent have the highest precipitation rates and are most sensitive to aerosol perturbations

We don't understand this comment but this sentence has been removed from the abstract since other results have been added (see specific comment 3 by reviewer #3) and we want to keep the abstract from becoming too lengthy.

Sec 2.2. 'Therefore, the Level 1B data from band 31 (10.780-11.280 m) and 32 (11.770-12.270 m) have been used to calculate the CTT at a 1 km horizontal resolution.' You need to provide the details or references for this calculation yourself.

We have provided a reference to Rosenfeld and Lensky (1998). (Lines 152 revised manuscript)

Sec 2.5: 'The 30th percentile of the variation in re with height has been studied here since this represents clouds early in their development, which are less influenced by ice formation'. It is not clear to me what this means.

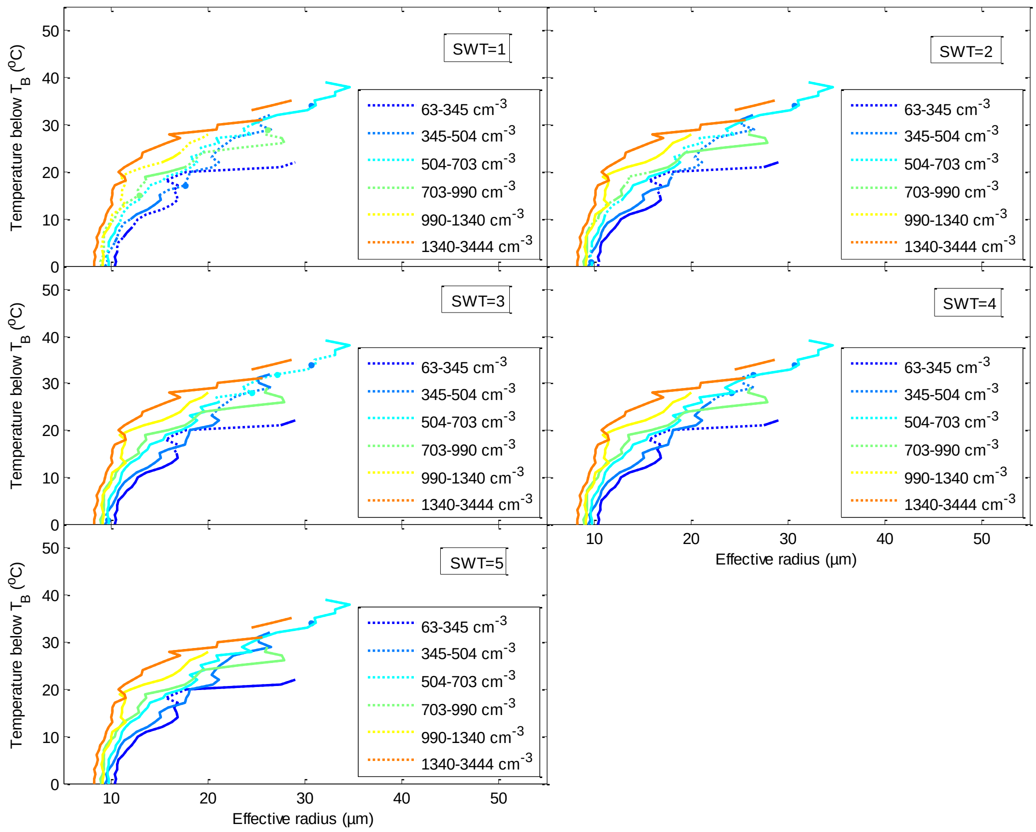
The 30^{th} percentile of the variation in r_e with height is used rather than the median. We follow the method of Freud et al. (2008) who used the 30^{th} percentile rather than the median. Ice particles absorb more at 3.7 μm and they are also larger than water droplets with the same mass. By choosing the 30^{th} percentile we reduce the chance of overestimating the r_e in the profiles due to ice.

Sec 2.5: 'Clouds with less ice formation are preferred, since the measurements at both stations focuses on CCN'. Does this mean that 'ice' clouds are removed from the analysis? Be clear. The word preferred is very inexact. I have the same complaint with sec 2.2.
Ice clouds are not removed from the analysis. This sentence refers to the previous sentence. See previous comment. We have changed the 'preferred' to 'desired' in Section 2.5 and to 'favored' in Section 2.2.

Sec 3.2: 'However, the cloud profiles do seem to be affected by the humidity at 1000 hPa. A lower SH results in smaller droplet sizes at lower levels of the profiles (Fig. (4j) to (4l)) and dividing the profiles according to RH produces similar results (not shown)'. I think that this actually strengthens your argument for aerosol effects on re because the dependence of re on SH and N80 is of opposite signs but table 4 shows that SH and N80 are positively correlated. You should discuss this.
We discuss this somewhat in the discussion Section 4: 'There is also a stronger positive correlation between N_{80} and SH at Hyytiälä (Table 3) which could mask some of the effect the aerosols have on the droplets. A low SH can reduce the size of the droplets as can be seen in Fig. 4j-l.' and also: 'The entire effect of the SH may not be visible in the data because of the positive correlation with N_{80}.'.

Sec 3.2 and Figure 4: I can see that the profiles are different. You should really demonstrate that these differences are statistically significant. Possible they are significant at some levels and not at others. You could draw dashed lines for the insignificant heights and solid lines for levels where the differences are significant.
We believe that what the reviewer suggests is a good idea, but showing statistical significance here is a bit more complicated than that. If we are to test significance independency between the profiles, the question between which profiles arises. If we only test the significance to the closest intervals we are only investigating a small subset of the data and we believe that the significance testing should be done with respect to all the other profiles. This however becomes very difficult to illustrate in a figure and we have not been able to think of a way to do this. If the editor or the reviewer has any suggestions on how this could be done however, we would be very interested to test them. We also believe that Figure 4 is complicated enough as it is without adding more information to it. We have nevertheless done the significance testing and in the figure below we show an example of it where the effective radius profiles from Vavihill are divided according to N_{80}. The profiles have solid lines where the profiles are significantly different from each other and dashed where they are insignificant. If the line is solid it means the profile is significantly different with a 95 % confidence interval from profiles from intervals with higher N_{80}. SWT in the figure stands for significance with respect to, and hence in the first subfigure the profiles are tested for significance with respect to the profiles in the closest interval and in the second subfigure the profiles are tested against the profile 2 intervals away etc. We can see that profiles are not statistically different from each other at all heights when compared to the closest intervals but significant differences are obtained when the profiles are compared to intervals further away. Since we don't believe that we can incorporate these results into Figure 4 in the manuscript we have instead added the

following sentences to the text in Section 3.2: ‘It has been investigated whether there is a statistically significant difference between the profiles according to a t-test with a 95 % confidence interval. Every profile was compared to the other 5 profiles at each level of the profiles. For Fig. 4a and c, the profiles are statistically different from each other at most levels except when the profiles are compared to the profile from the closest intervals. For Fig. 4b statistical significant difference between the profiles occurs at most levels when the profiles two intervals away are compared.‘ (Lines 308-314 revised manuscript)



Section 3.3: ‘The strong correlation between T14 and dT is expected since the vertical extent of the profile also limits the T14 and this correlation is hence an artifact of the method’. I don’t undersand this statement. Can you clarify please?
dT is the extent of the entire profile and therefore ΔT_{14} can never be larger than dT.

Section 3.3: ‘The w’. Define w. Why specifically talk about w here as opposed to the other variables. The message of this entire paragraph is not entirely clear to me.
We believe that the reviewer here refers to Section 3.2 paragraph 5. (The section 3.3 was called 3.4 in the manuscript before it was typeset which may have caused some confusion). w is the vertical wind velocity which was defined in paragraph 3 of Section 3.2. We have however moved the definition of w to the method section (2.3) since the other reanalysis parameters are defined here. The reason we discuss w here is that it is one of the parameters that is significantly correlated with ΔT_{14} at both stations.

Table 3: The table is overwhelming. I think that this table might be easier to look at if it were 2 tables; one for each location.
We believe that it is easier to compare the values between the two stations in one table and therefore prefer to keep it as it is.

Sec 3.3: 'The COT profiles were also divided according the same parameters but neither aerosols nor meteorological parameters were found to separate the profiles from each other to any great degree.' How can this be? Figure 4 shows an apparent strong correlation between re and N80. If the differences in fig 4 are not significant you need to mention this. Also if you can't show a statistically significant relationship between N80 and re then the whole premise of the paper unravels. This seems like a major problem or miscommunication that needs to be dealt with.
We do not know why the dependence of the r_e on N_{80} does not appear in the COT profiles. This is however not the first investigation to find that the increased aerosol loading is associated with decreasing droplet sizes but no increase or, even a decrease in COT see eg: (Costantino and Breon, 2013;Brenguier et al., 2003;Twohy et al., 2005). We have decided not to investigate these results further but instead focus on how the dependence of the r_e on N_{80} affects precipitation. Concerning the results in Fig. 4 please see the previous comment regarding this figure. See also our reply below to Anonymous Referee #3, Specific Comment 3.

Abstract: 'Furthermore, an increase in aerosol loadings results in a suppression of precipitation rates' Your data in Table 4 are not significant and don't support this.
We have provided p-values in Figure 4 to demonstrate whether the correlations between the N_{80} and the precipitation intensities are significant. We agree with the reviewer that not all these correlations are significant. For the upper part of the table where the cases with no precipitation is included 42% of the correlations are significant while in the bottom part where the non-precipitation cases are omitted 67% of the correlations are significant. Since there is a large gap in the dbcz-values between the precipitating cases and non-precipitating cases we think that the results in the lower part of the table should be primarily studied. We hence consider that we have significant results supporting our claim that a decrease in precipitation intensity from convective clouds is associated with an increased boundary layer aerosol loading at the sites studied here.

Fig 8: You need to provide significance testing here.
The solid lines in this figure denote significant correlations and the dashed lines denote that significant correlations do not occur. The exact p-values and correlation coefficients for this figure are provided in Table 4.

Sec 3.4: 'Furthermore, an increase in aerosol loadings results in a suppression of precipitation rates'. Again you need to do significance testing.
This sentence comes from the abstract. Significance testing was performed and is provided in Table 4. We have nonetheless changed the sentence in the abstract to: 'However, lower

precipitation rates are associated with a higher aerosol loading for clouds with similar vertical extent.‘ to avoid discussions regarding the causality (in response to Anonymous Referee #3, Comment 2). (Lines 35-36 revised manuscript)

Anonymous Referee #2

Comments:

1. Table 3 shows poor correlation between N80 and precipitation, while other indications show a much clearer relation. Please explain the apparent contradiction. In my view the signal appears only when looking at partial derivatives of the relations, i.e., under narrow ranges of conditions while holding everything else near constant.

We agree, Table 3 shows poor correlations between N_{80} and precipitation while Figure 8 and Table 8 indicate stronger correlations. Convective clouds of different vertical extent cannot be expected to produce the same precipitation intensities due to diverse dynamics. We have therefore divided the dataset according to vertical extent in Figure 8 and Table 4. When we do this, the correlation between N_{80} and precipitation appears. This treatment would resemble the referee´s partial derivative.

2. Please explain how TB is calculated.

This is explained in Section 2.5 ‘assuming that the highest CTT within a satellite scene +2 °C equals T_B’. We had however forgotten to add the +2 °C in the original manuscript. The number 7 on page 13862 line 14 (original manuscript) has also been changed to 9 because of this. See also Specific comment 1by reviewer #3.

3. Page 13865 line 18: The drops do not grow faster when the mixed phase is reached. Rosenfeld and Lensky (1998) wrote that the development of ice particles is indicated as larger cloud drop effective radius, when the calculation of re assumes water drops.

We have re-written the sentence and it now reads: ‘When the mixed phase is reached, coalescence and mixed-phase precipitation formation cause the r_e to start growing rapidly with height’. (Lines 328-330 revised manuscript)

4. Page 13866 last line: Same as the previous comment.

We have replaced ‘droplets’ with ‘r_e’.

5. Page 13870 lines 5-1. The dbzc is in fact a logarithmic transformation of the rainfall intensity. For consistency, the authors should test the correlation with respect to the lograithmic transformation of the other rainfall datasets.

We tested the correlation between N_{80} and the logarithmic transformation of the other rainfall datasets, but the correlations did not change to any great degree. Below we provide a table of correlations with and without the logarithmic transformation.

Vavihill			Hyytiälä		
	Normal	Logarithm		Normal	Logarithm
N_{80}-GBP	-0.03	-0.12	N_{80}-GBP	0.16	0.16
N_{80}-rr3h	0.02	-0.05	N_{80}-rr3h	0.04	0.03

6. Page 13870 lines 15-16. According the invigoration hypothesis (Rosenfeld et al., Science 2008) the invigoration requires warm cloud bases. Li et al. (2011) showed that invigoration *does occurr for cloud base temperature>15C but not for lower cloud base temperatures. Cloud base temperatures at the study areas are lower than 15C. Therefore, the observations here cannot be considered as going against the invigoration hypothesis and previous observational studies that support it.*

This section has been rewritten such that it now clarifies that our results agree with the study by Li et al. (2011). (Lines 445-447 revised manuscript)

7. Figures 2 and 3: The units of the SH are mixing ratio (unit less) and not g/kg. If the authors want to show g/kg they have to multiply the scale by 1000.

We have corrected Figures 2 and 3 by multiplying the values with 1000.

Anonymous Referee #3

Specific comments

1. Profiles. The method used for creating vertical 'profiles' has been used in other studies. However, the term has the potential to be misleading, particularly in the abstract. The authors clarify their methodology in Section 2.5. It would be good to also include some clarification in the abstract, emphasising the the re is cloud top re for different clouds in a given scene. Maybe this could be done by achieved by re-writing the sentence at 13854.7 as follows: 'From the satellite scenes, vertical profiles of cloud top cloud droplet effective radius (re) are created by plotting retrieved cloud top re against cloud top temperature for the clouds in a given satellite scene.'

We have changed the sentence in the abstract according to the reviewer's suggestion but we have however removed the first 'cloud top' in the sentence to avoid writing cloud top three times in one sentence. (Lines 21-23 revised manuscript)

I would also question the assumption that the highest cloud top temperatures really represent TB (13862.18), and hence whether dT is really a good indicator of vertical extent for a given cloud. Another possible interpretation of dT would be that it is a measure of inhomogeneity between clouds in a given scene. It is good that the authors state their assumption about TB at 13862.18, but they may want to provide further clarification of this when they discuss dT and T14.

We want to clarify that we had forgotten to write that T_B is calculated by taking the warmest cloud pixel +2 °C, see comment 2 by reviewer #2. We have followed the method by Rosenfeld and Lensky (1998) which has been used in several peer reviewed articles since then and we believe that dT is a good indicator of the vertical extent of clouds in a satellite scene. We have however added the following sentences to the discussion: 'dT is calculated by taking T_B minus the cloud top temperature and hence any uncertainty in T_B will be transferred to dT and also to ΔT_{14}. T_B is set to the warmest cloudy pixel +2 °C and may be underestimated if no low clouds are present in the satellite scene. This is not believed to be common here since convective clouds usually have highly variable cloud top heights and clouds with dT below 9 °C were excluded from the study.' (Lines 448-453 revised manuscript)

2. Correlation vs causation. The authors find N80–re and N80–dbzc relationships. There can be many reasons for observed relationships between aerosols and cloud properties. (See e.g. introductions of Quaas et al, 2010, doi:10.5194/acp-10-6129-2010 and Grandey et al, 2013, doi:10.5194/acp-13-3177-2013 for a discussion of possible reasons for correlations between aerosol properties and cloud fraction. Many of these may also apply to re and precipitation rate.) The authors attempt to account for many of these. For example, the use of ground-based aerosol measurements avoids the problems of cloud contamination associated with satellite-retrieved aerosol properties; and interpretation of observed relationships can be difficult if large spatial scales and many cloud types are chosen, a problem that this study avoids by focusing on two locations and one particular class of clouds. Meteorological conditions are also considered. The fact that the re profiles are more closed associated with

N80 does provide evidence that the aerosols may have a significant impact on re. And stratification by dT is a step towards accounting for the effect of meteorology on the N80–dbzc correlations. However, I am not convinced that these analsyses conclusively prove that the relationships are indeed due to aerosol effects on clouds. In particular, it is very difficult to completely account for the impact of meteorological covariation. Furthermore, satellite retrievals of re may be unreliable (see point 4 below), and seasonal covariability may exist (see 7f below).

At times, the authors make strong statements about the causal effects of aerosols on clouds. For example, the abstract contains statements such as 'aerosol number concentrations result in smaller re' and 'an increase in aerosol loadings results in a suppression of precipitation rates'. I would caution the authors against stating such strong conclusions, both in the abstract and elsewhere in the manuscript.
We have softened the language in our statements concerning the effects of aerosols on clouds in the abstract (Lines 28-29 and 35-36 revised manuscript), Section 3.2 (Lines 305-307 revised manuscript) and Section 4 (Lines 414-416, 458-460, 500-501 and 505-507 revised manuscript).

3. Null results. In the abstract and final summary, it could be good to mention the null result that no relationship between N80 and cloud optical thickness was found (13867.1). The lack of any observed convective invigoration (13870.16) could also be mentioned in the abstract, as could the null results for two of the precipitation datasets (13870.6).
The abstract has been rewritten and now contain the three null results. (Lines 29-30 revised manuscript)
In the final summary, the following sentence has been added: 'The aerosol loading was, nonetheless, found to affect neither the COT profiles nor invigorate the clouds.' We have already included the null results from the other two precipitation datasets in the sentence: 'However, the meteorological parameters rather than aerosols control the total amount of precipitation that reaches the ground.' But we have emphasized that this refers to the total amount of precipitation by adding 'total' to this sentence. (Lines 305-307 revised manuscript)

4. Reliability of satellite-retrieved re. The possibility of errors in the re retrievals is acknowledged in both Section 2.2 and and Section 2.5. Indeed, the authors make an attempt to select only the more reliable data. Further discussion of possible errors would be beneficial. In particular, retrievals of re generally assume plane-parallel clouds, so the retrievals are likely to be more reliable for status and stratocumulus cloud fields than they are for broken cloud fields with more complicated 3-D geometry, such as the convective cloud fields studied here (Marshak et al., 2006, doi:10.1029/2005JD006686; Vant-Hull et al., 2007, doi:10.1109/TGRS.2006.890416). It is possible that 3-D effects may be more problematic for high solar zenith angles, so there might be seasonal cycles in these errors for high latitude locations like the two sites used in this study. Other useful references include Zinner et al. (2010, doi:10.5194/acp-10-9535-2010) and Bréon and Doutriaux-Boucher (2005, doi:10.1109/TGRS.2005.852838).

We have added a paragraph in section 4 where we address these issues. This paragraph starts with: ‘Studying convective clouds using satellite retrievals that assume plane-parallel clouds introduces uncertainties in the r_e which can be overestimated in cloud fields with much sub-pixel scale variability (Zhang et al., 2012).’ (Lines 434-442 revised manuscript)
We also examined the effect of solar zenith angles and found it to be of minor importance in our data sets, see comment 7f by reviewer #3.

5. Vague criteria in methodology.
(a) Only vague criteria are provided for the selection of satellite scenes at 13859.11-20. More specific details should be provided.
We have rewritten this paragraph somewhat to make it clearer (Lines 157-160 revised manuscript). The cloud scene selection is divided in a first manual (subjective) step followed by a more rigorous step based on fixed criteria. The selection criteria for the first manual subjective part of the method cannot be made more specific than already described. However, the screening for the profile analysis is automatic and more rigorously done. Only 49% (Vavihill) and 59% (Hyytiälä) of the original scenes (see Table 1 in the manuscript) are included in the analyses. We hence consider the data included in the study as being suitable for the analysis.

(b) At 13862.8, the specific cloud optical thickness and cirrus reflectance thresholds should be specified.
The threshold values have been specified and the sentence now reads: ‘Pixels with COT<25 and cirrus reflectance>0.02 have therefore also been removed before creating the profiles.’ (Lines 233-234 revised manuscript)

6. Introduction. A few relatively minor suggestions:
(a) The authors may want to mention the semi-direct effect, maybe at 13855.12.
We have changed the sentence regarding the indirect aerosol effects to: ‘The latter includes increased cloud albedo due to smaller but more numerous droplets (Twomey, 1974), suppression of drizzle, increased cloud lifetime (Albrecht, 1989), increased cloud height (Pincus and Baker, 1994) and cloud burn-off (Ackerman et al., 2000).’ (Lines 49-52 revised manuscript)

(b) The review of previous studies is currently in the following order: aircraft, then models, then satellite. A more logical order might be models, then aircraft, then satellite, so the observational/satellite studies can be more closely grouped together.
The order of the paragraphs has been changed according to the reviewer’s suggestion.

(c) Modeling paragraph: the review paper by Khain (2009, doi:10.1088/1748-9326/4/1/015004) would be a useful reference.
The reference has been incorporated into the text. (Line 88 revised manuscript)

(d) Aircraft paragraph: at the beginning of 13855.20, 'suggested' is probably preferable to 'shown'.
'shown' has been changed to 'suggested'.

(e) Satellite paragraph: it could be good to emphasise that correlations between satellite-retrieved cloud and aerosol properties are not necessarily due to aerosol effects on clouds.
We have decided not to include this suggestion in the introduction since most of the studies we mention in this section do not use satellite derived aerosol products.

(f) 13856.21-23: Stevens and Feingold (2009, doi:10.1038/nature08281) and Tao et al (2012, doi:10.1029/2011RG000369) are possible references for this sentence.
We have added a reference to the Stevens and Feingold paper. (Line 90 revised manuscript)

7. Miscellaneous suggestions.
(a) 13854.18 and 13870.2: when mentioning the precipitation rates/intensities here, specify that it is radar reflectivity, a measure of precipitation rate/intensity.
We have changed the manuscript on line 13870.2 according to the reviewer's suggestion: 'However, when taking dT into account, the increased aerosol loading is associated with a decrease the radar reflectivity, which is a measure of the precipitation intensity (Fig. 8).'
In the abstract (*13854.18*) we mention that the precipitation intensity comes from the weather radar dataset in the first paragraph. We have therefore chosen to keep the sentence as it is in order to keep the abstract from becoming too lengthy.

(b) 13854.17-23: in the abstract the relationship between precipitation and aerosols is mentioned before a statement saying that meteorological conditions are more closely related with the precipitation. In contrast, in the concluding paragraph (13871), the relationship between precipitation and meteorology is mentioned before the relationship with aerosols. I think the order in the concluding paragraph is clearer, and a similar order could be adopted in the abstract.
We have changed the abstract according to the reviewer's suggestion. (Lines 32-36 revised manuscript)

(c) 13858.8-10: does the discontinuity in the upper size limit affect N80? It could also be good to introduce N80 in Section 2.1 – when re-reading the paper, many readers (myself included) may refer back to here, rather the Results section, when looking for a definition of N80.
We do not believe that the discontinuity in the upper size limit in Hyytiälä data affects the results because the aerosol number concentrations at this size range are low hence N_{80} will be dominated the smaller size ranges, see Figure 4 in Asmi et al. (2011). We have moved the introduction of N_{80} to Section 2.1.

(d) 13864.1-3: it could be good to quote the correlation coefficients of 0.94 and 0.98 from Table 2 to convince the reader that N80 is indeed a good proxy for CCN.

We have added the correlation coefficient values to the text:' When N_{80} is compared to the CCN data (Table 2), it best correlates (correlation coefficients: 0.94-Vavihill, 0.98-Hyytiälä) at a supersaturation of 0.4%,' (Lines 279-280 revised manuscript)

(e) 18864.12: the authors state that the 'meteorological parameters used in this study do not seem vary significantly with air mass origin (Fig. 2c to h).' This does not appear to be true. The directional patterns in Figs. 2d and 2h appear to be as strong as in Fig. 2b.
This section has been rewritten such that it now reads: 'The meteorological parameters for Vavihill do not seem to vary significantly with air mass origin (Fig. 2c,e and g). At Hyytiälä however, the T_B and SH (Fig. 2d and h) are higher in southerly air masses, similar to the aerosol number concentrations in Fig 2b.' (Lines 289-292 revised manuscript)

(f) As a further step to account for seasonal covariation, it could be advantageous for the authors to repeat the analysis for JJA.
We have repeated the analysis for JJA and found the results to be similar to the results reported in the manuscript. The values in Table 3 change somewhat when only the JJA data is used, but the significance levels are the same as in the manuscript for almost all correlations. Similar results are also obtained when Figures 4 and 6 with all data and only JJA data are compared.

Technical corrections

Throughout: where possible, 'amount' should be replaced with more accurate words. e.g. 'number' at 13863.18, and 'number concentration' at 13863.26.
The word 'amount' have been replaced by 'number' where it has been found suitable including the two places mentioned by the reviewer.

13854.3: change '2' to 'two'.
We have changed '2' to 'two'.

13857.20: 'has' to 'have'.
We have changed the manuscript according to the reviewer's suggestion.

13857.23: 'measure' to 'measures'.
We have changed the manuscript according to the reviewer's suggestion.

13857.25: 'is' to 'are'.
We have changed the manuscript according to the reviewer's suggestion.

13858.12: 'and has' to 'have'.
We have changed the manuscript according to the reviewer's suggestion.

13859.3: 'at a 5km' to 'at 5km'. Similar at 13859.6.
We have changed the manuscript according to the reviewer's suggestion.

13859.23: ‘conditions, affect’ to ‘conditions affect’.
We have changed the manuscript according to the reviewer’s suggestion.

13859.25: ‘data is’ to ‘data are’.
We have changed the manuscript according to the reviewer’s suggestion.

13860:14: consider moving the time resolution sentence to the 13860.19 or 13860.21, so that it is next to the sentence about the timing definition of each day. Also consider deleting the ‘however’.
The sentence has been moved so that it is next to the sentence about the timing of the day and has been changed to “The time resolution of both precipitation datasets is only on a daily basis.”

13860.27: ‘was’ to ‘were’.
We have changed the manuscript according to the reviewer’s suggestion.

13861.3: ‘could’ to ‘may’.
We have changed the manuscript according to the reviewer’s suggestion.

13861.18: ‘method is assumes’ to ‘method assumes’.
We have changed the manuscript according to the reviewer’s suggestion.

13862.3: ‘contaminations’ to ‘contamination’.
We have changed the manuscript according to the reviewer’s suggestion.

13862.14: ‘has’ to ‘have’.
We have changed the manuscript according to the reviewer’s suggestion.

13862.15: ‘hence’ to ‘implying’.
We have changed the manuscript according to the reviewer’s suggestion.

13863.6: ‘not available during all days’ to ‘not always available during days’.
We have changed the manuscript according to the reviewer’s suggestion.

13863.15: ‘normalizing’ to ‘offsetting’ (if I’ve understood this correctly).
‘Normalizing’ has been changed to ‘offsetting’.

13864.2: ‘correlates to’ to ‘correlates at’.
We have changed the manuscript according to the reviewer’s suggestion.

13865.15: ‘above’ to ‘colder than’.
We have changed the manuscript according to the reviewer’s suggestion.

13869.29: 'strongest correlated to' to 'more strongly correlated with'.
We have changed the manuscript according to the reviewer's suggestion.

13870.4: 'is' to 'are'.
We have changed the manuscript according to the reviewer's suggestion.

13871.4: 'datasets the clearly' to 'datasets clearly'.
We have changed the manuscript according to the reviewer's suggestion.

13871.20: 'is' to 'are'.
We have changed the manuscript according to the reviewer's suggestion.

13871.21: 'distributions' to 'distribution'.
We have changed the manuscript according to the reviewer's suggestion.

References:

Ackerman, A. S., Toon, O., Stevens, D., Heymsfield, A., Ramanathan, V., and Welton, E.: Reduction of tropical cloudiness by soot, Science, 288, 1042-1047, 2000.
Albrecht, B. A.: Aerosols, Cloud Microphysics, and Fractional Cloudiness, Science, 245, 1227-1230, 1989.
Asmi, A., Wiedensohler, A., Laj, P., Fjaeraa, A. M., Sellegri, K., Birmili, W., Weingartner, E., Baltensperger, U., Zdimal, V., and Zikova, N.: Number size distributions and seasonality of submicron particles in Europe 2008–2009, Atmos. Chem. Phys, 11, 5505-5538, 2011.
Brenguier, J. L., Pawlowska, H., and Schuller, L.: Cloud microphysical and radiative properties for parameterization and satellite monitoring of the indirect effect of aerosol on climate, J Geophys Res-Atmos, 108, -, doi: 10.1029/2002jd002682, 2003.
Costantino, L., and Breon, F. M.: Aerosol indirect effect on warm clouds over South-East Atlantic, from co-located MODIS and CALIPSO observations, Atmos Chem Phys, 13, 69-88, 2013.
Freud, E., Ström, J., Rosenfeld, D., Tunved, P., and Swietlicki, E.: Anthropogenic aerosol effects on convective cloud microphysical properties in southern Sweden, Tellus B, 60, 286-297, 2008.
Li, Z., Niu, F., Fan, J., Liu, Y., Rosenfeld, D., and Ding, Y.: Long-term impacts of aerosols on the vertical development of clouds and precipitation, Nature Geoscience, 4, 888-894, 2011.
Pincus, R., and Baker, M. B.: Effect of precipitation on the albedo susceptibility of clouds in the marine boundary layer, Nature, 372, 250-252, 1994.
Rosenfeld, D., and Lensky, I.: Satellite-based insights into precipitation formation processes in continental and maritime convective clouds, B Am Meteorol Soc, 79, 2457-2476, 1998.
Twohy, C. H., Petters, M. D., Snider, J. R., Stevens, B., Tahnk, W., Wetzel, M., Russell, L., and Burnet, F.: Evaluation of the aerosol indirect effect in marine stratocumulus clouds: Droplet number, size, liquid water path, and radiative impact, J Geophys Res-Atmos, 110, -, doi: 10.1029/2004jd005116, 2005.
Twomey, S.: Pollution and Planetary Albedo, Atmos Environ, 8, 1251-1256, 1974.
Zhang, Z., Ackerman, A. S., Feingold, G., Platnick, S., Pincus, R., and Xue, H.: Effects of cloud horizontal inhomogeneity and drizzle on remote sensing of cloud droplet effective radius: Case studies based on large-eddy simulations, Journal of Geophysical Research: Atmospheres, 117, D19208, 10.1029/2012jd017655, 2012.