Dear Dr. Cziczo,

Below are our responses to the referee comments submitted during the open discussion phase on our paper, “Ice water content of arctic, midlatitude, and tropical cirrus – Part 2: Extension of the database and new statistical analysis”. We would like to thank both reviewers for their time and for providing comments and insights that will improve the manuscript and more clearly convey its message to the reader. The reviewer comments are included here in their entirety, followed by our responses in a different font. We trust that you will find our responses appropriate and adequate, and we look forward to your favorable decision to invite a revised manuscript for publication in ACP.

Kind regards,

Anna Luebke, Martina Krämer, and Linnea Avallone

Ice water content of arctic, midlatitude, and tropical cirrus

Part 2: Extension of the database and new statistical analysis

A. E. Luebke, L. M. Avallone, C. Schiller , J. Meyer, C. Rolf , and M. Krämer

Responses to referee 1:

First of all, we thank the referee for the constructive comments that helped us to improve the manuscript. We have conscientiously treated all suggestions and hope the new manuscript and the point to point responses will lead to publication in ACP.

General Comments:

1. (a) *First, the authors choose to explore the variability in IWC as a function of temperature. There have been many such parameterizations since Heymsfield and Platt's seminal 1984 publication. While illustrating temperature relationships is valuable, calling it a parameterization implies that it should be used verbatim in models. I don't think this makes sense at this point when models are moving toward much more explicit microphysics.*

   Response: See answer to specific comment 14.

   (b) *The authors slice their data by latitude, ignore anvil cirrus, and then attempt to show how the IWC-temperature relationships vary. All of this is done without any mention of the statistical significance of their results. Essentially, the authors argue that their sparsely sampled data sets accurately represents the statistics of the full atmosphere in a particular latitude belt. While this may be true, it is incumbent on the authors to defend this position. For instance, while there may be some 10's of thousands of points in each PDF in Figure 6, how many independent samples are there? Clearly, data points collected from one day to the next are likely independent, how independent are data points collected a few seconds apart in a given cirrus cloud?*

   Response: To demonstrate the statistical significance of the dataset, we included a new Figure (Fig. 5) in the manuscript and inserted the following text (section 3.1, new fourth paragraph):

“Altogether, the dataset used in this analysis encompasses the latitude range from 22 degrees South to 68 degrees North, the altitude range from 5 to 20 km and the temperature range 182 - 249 K. It represents 38.2 hours (27,500 kilometers) of cloud sampling (137,409 data points; for more details of the 13 field campaigns between 1999 and 2008 see Table 1).

The statistical significance of the dataset is demonstrated in Fig. 5, where all observed IWCs are shown as a function of temperature. The color scheme refers to the frequency of data points with respect to the total number of sampled points. It can be seen that the large temperature and IWC range of cirrus is completely covered by the measurements. The dataset includes data points collected only one second apart, which might not be regarded as independent of each other. However, we believe that the reliability and statistical significance of the data set is illustrated by the spread of IWCs over several orders of magnitude sampled in each temperature interval.”

We hope that with this new analysis we can resolve the doubts of the referee about the significance of the dataset. Here we would like to add that it currently represents the largest in-situ cirrus data base, where the physical space of the phenomenon is thoroughly explored and all major possible cirrus states are well covered (though we should note that the TTL cirrus at temperatures < 200 K are probed with a little lesser frequency than the others due to the fact that fewer field campaigns took place in the tropics). Therefore, deriving assertions about cirrus clouds from this dataset is meaningful. As a closing remark we want to note that the sparseness of observations is always a problem for in-situ measurements. Additionally, the assessment of the independence of adjacent data points is challenging, especially given the unknowns regarding microscale structure within clouds, and not something we felt important to address in this paper.

2. *Second, the authors seem not to be aware of the argument that has gone on in the ice crystal measurement community in the last number of years regarding the problem of ice crystal shattering with OAP measurements. This issue has been largely resolved and it has been shown that number concentrations of ice crystals in cirrus greater than 1/cc are exceedingly rare and very transient. Yet, the authors show just the opposite in Figure 8. They then proceed to use this relationship to draw their primary conclusion that IWC is mostly dependent on particle number and not on particle size. Basically, the independence on*

particle size in figure 8 and the dependence on particle number arises from the fact that the particles measured by OAPs have all been ground down to a similar size by shattering.

Response: We thank the referee for this comment. We are aware of ice crystal shattering and should have discussed that point in the paper, or at least referred to the discussion on shattering from the original work presenting this particle data set (Krämer et al., 2009, ACP). The present comments have inspired us to strengthen our findings by analyzing cirrus measurements performed with particle spectrometers where ice crystal shattering is accounted for.

N_{ice} frequencies: The results of the N_{ice} analyses are shown here in Figure 1. The left panel shows the same N_{ice} (number concentration of ice crystals) data set that is presented in the lower left panel of (old) Figure 8 in the paper, but for the full temperature range (205 - 240 K). In the middle panel, N_{ice} frequencies of cirrus clouds sampled with a CAPS probe during the COALESC campaign in 2011 out of Exeter, UK are shown. CAPS is the combination of two instruments, CAS and CIP, and samples particles in the size range 0.6 – 1000 µm. It is equipped with the so-called PBP - option, which means that the interarrival times of the particles are stored and shattered fragments of ice particles can be sorted out based on their clustering together. In the right panel, cirrus clouds measured with the 2-DS instrument (size range 15 – 1000 µm) during MACPEX 2011 out of Houston, USA, are shown (the referee mentioned this data in comment 18). This data is also corrected for shattering.

When comparing the frequency distributions, it becomes apparent that the overall structure of two peaks can be seen in all panels, though the peak height and N_{ice} varies. Possible artifacts of shattered crystals in the FSSP data set are highlighted by a purple oval. Indeed, these are the ice crystal concentrations >1 cm^{-3} mentioned by the referee. However, the important message from Figure 1 is that the bimodality of the N_{ice} frequency distributions is not caused by shattered ice crystals, though it is also visible in cases where shattering might have occurred. Further, it is interesting to note that the bimodality is seen in the MACPEX data set, though crystals smaller than 15 µm are not included the in N_{ice} data from 2-DS.

We have also included the analysis of the COALESC cirrus for the two available temperature intervals in the revised manuscript (here Figure 2, in the paper new Figure 9). It can be seen that the same temperature dependence as presented in (old) Figure 8 of the paper is observed during COALESC, namely that the N_{ice} peaks shift to higher values with increasing temperature.

Thus, we believe that the new statements that “the IWC is influenced by particle number” and “the peaks at larger N_{ice} concentration frequencies shift to higher values with increasing temperature” are better supported now than they were in the first version of the paper.

R_{ice} frequencies: Under the assumption that the N_{ice} concentrations > 1 cm^{-3} in the FSSP measurements are caused by shattering, it is unlikely that the mass mean size PDFs for the three temperature ranges would still be that close to each other as shown in the lower right panel of (old) Figure 8. We cannot calculate the mass mean sizes for the COALESC data set, since the IWC, which is needed for the calculation, was not measured.

Thus, we have altered the statement that “the IWC is mainly dependent on particle number and not on particle size” to “the IWC is influenced by particle number”, as mentioned above.

You'll find the changes corresponding to this response in the version of the revised manuscript with tracked changes (see especially the new section, 3.2.2 Ice water content and crystal number).

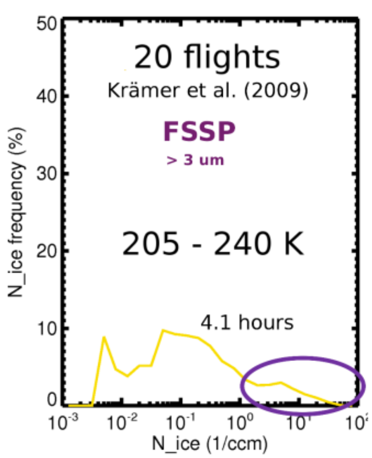

Figure 1: N_{ice} frequency distributions of cirrus in the temperature range 205 -240 K from FSSP (not corrected for artifacts from ice crystal shattering), CAPS and 2-DS measurements (both shattering corrected).

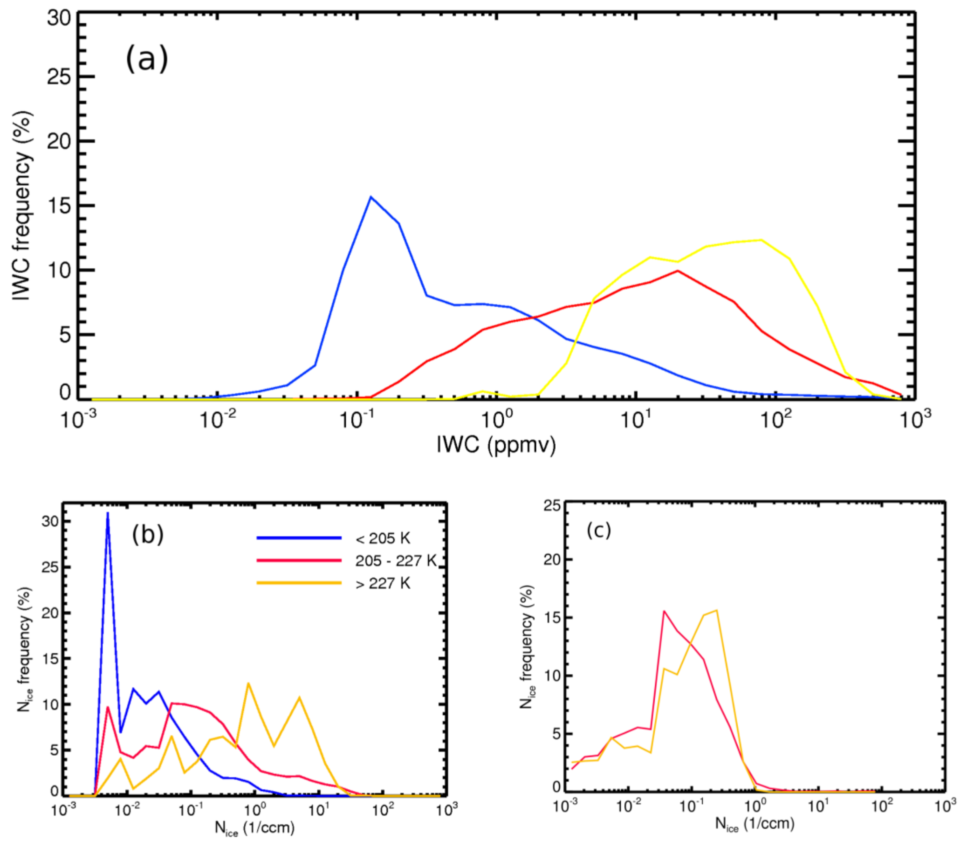

Figure 2. New Figure 9 of the revised manuscript.

Specific Comments:

1. *Page 29445, Line 5: The statement that the "microphysical properties of an individual cirrus cloud ... determine whether absorption or reflection will dominate for a particular cloud" is not true. The macroscale properties (IWP and temperature of the top) are the zeroth order determinants of whether an ice layer is going to have a net warming or cooling effect on the atmospheric column. After this, forward scattering becomes important - i.e. microphysics.*

   Response: We understand the distinction that the reviewer has made in terms of degree of importance of different properties. The text will be changed to convey that microphysical properties "contribute to" determining whether net warming or cooling will occur.

2. *Page 29449, Paragraph starting at line 5: Need citations for these field programs where available.*

   Response: This comment has been taken into consideration and references have been included in the paper where available.

3. *Page 29449, Line18: Should explain the implications of the FISH IWC using saturation mixing ratio. Does this mean that no ambient vapor measurement was made and ice saturation was assumed to difference the total water to get IWC?*

   Response: In those cases no ambient water vapor measurement was available for use. However, while it is true that the assumption of saturation mixing ratio was used to process some of the data from Schiller et al. (2008), only IWC data as calculated using water vapor measurements was used in our analysis. This is explained on page 29449 L14-17. As this is not a method included in the analysis presented here, no further comments on this subject will be included in the paper.

4. *Page 29451, Line 20: How much data are rejected? What are the criteria for rejection?*

   Response: The criterion for rejection is a large scatter between the adjusted and measured values indicating a poor agreement between the total water and water vapor instruments in question (Kraemer et al., 2009). This brief description will be included in the paper for clarification purposes. As described in the paper on page 29449, line 26 – 28, the data from FISH had been processed previously, so only the data from the CLH was subject to the data quality check. No data was rejected as a result of this particular test. Other data was rejected prior to the beginning of the analysis based on criteria such as instrument issues, missing correlative data, etc.

5. *Page 29451, Line 26: What does the ratio of eRH-ice to RH-ce give in circumstances where a cirrus fall streak may be sedimenting through subsaturated air? Unlie liquid cloud droplets, Ice crystals can exist in such subsaturated air for quite some time. See Heymsfield and Donner (1990) for instance. Some data suggest that a sizable fraction of all ice in cirrus reside in subsaturated air. This id likely not well represented by the in situ data because the aricraft tend to spend most of their time near cloud top attempting to measure the interesting dynamics where particles are formed.*

Response: Cirrus in subsaturated air can be found by the same procedure as those in saturation or supersaturation as long as RHice,enh > 100% and RHice,enh/RHice > 1. The method for finding cirrus in cases where RHice,enh < 100% is described in Krämer et al. (2009), section 2.1.2 Cloud detection:

"Cirrus regime (b) whereby RHice,enh/RHice > 1 and RHice,enh < 100 %. This situation may be caused by a subsaturated cirrus, but might also be the result of the scatter of the water vapour. Here, we define this as cirrus only when RHice,enh/RHice > 1.3."

We think that with this criterion we can catch most of the subsaturated situations that we have observed.

In response to the question of whether cirrus in subsaturated environments are well represented by the in situ data: in field campaigns dedicated to cirrus, the flight strategy is normally to have flight legs lying close together in order to obtain measurements that represent the vertical structure of the clouds. This also includes legs below and above the cirrus. However, very thin evaporating cirrus are not easy to findsince they become sub-visible in the last stage of evaporation. That kind of cirrus might be underrepresented in in-situ observations (see also Kübbeler et al., 2011, ACP).

6. *Page 29453, Line 25: extinction is really 2 times the cross sectional area of the particle ensemble for these particle sizes and wavelength combinations. Number and effective radius are just different moments of the particle size distribution.*

   Response: This is completely right. The cross sectional area depends on the particle size distribution as well as the IWC and the effective size. We will reformulate the text to make it clearer.

7. *Page 29454, Line 5: Where does this relationship come from? What is the uncertainty in the relationship? the parameters a and b are going to be functions of particle habit and will vary widely.*

   Response: The relationship is provided by Heymsfield et al. (2005) (mentioned in the text) to determine IWC values from Calipso lidar and CloudSat satellite instruments. An uncertainty is not given in the publication so unfortunately we cannot state this here.

The ratio IWC/σ is proportional to the effective radius and increases with temperature, as stated in Heysmfield et al. (2005). The parameters a and b vary of course with temperature implying a change in particle habit at different termperatures.

8. *Page 29454, Line 10: Citation(s) are needed here.*

   Response: The available citations are already mentioned, like Seifert et al. (2007) to determine lidar ratio and extinction coefficient, and Rolf et al. (2012) for the extinction determination procedure including multiple scattering correction.

9. *Page 29454, line 20: Declarative statements like this with no citation should not be made. Where does 12% come from? Is it a theoretical value or derived by comparison to in situ data like the FISH? Perhaps both? The error is a constant percentage over 5 orders of magnitude? Does it account for the lidar ratio uncertainty? For the multiple scattering correction uncertainty? For the uncertainty relating area to mass in a particle size distribution?*

   Response: The uncertainty in the extinction coefficient is obtained by considering the uncertainties in the backscatter signals, the lidar ratio, and Rayleigh backscatter coefficient. By converting the extinction into IWC values through the use of the aforementioned parameterization (Heymsfield et al., 2005) most IWC values have an error of around 10%. In order to be able to state just one value we choose the mean error of all IWC cirrus observations, which has a value of 12%. Single IWC measurements with a relative error larger than 100% were neglected and not considered in the climatology. The value of 12% is now specified as the mean uncertainty in the text.

10. *Figure 2: I'm not sure what this plot is supposed to be telling the reader. What is agreeing? I suppose it says that the lidar and in situ probes are generating quantities that lie within several orders of magnitude of each other and agree that IWC varies as a weak function of temperature. Beyond that, it is difficult to draw much from this plot.*

    Response: The lidar cirrus measurements provide the opportunity to compare the low IWC values from CLH and FISH at temperatures above 230 K. This is important, since, as we state in section 2.2:

‘For lower IWC from CLH and FISH it is uncertain if the signal stems from instrument uncertainties or from a cloud…Thus, the inclusion of Leo-Lidar cirrus observations provides the opportunity to compare the small IWC values from CLH and FISH with these measurements…Most notable is the agreement of the data below the red dotted line, the minimum IWC that can be detected with certainty by CLH and FISH. At temperatures below 230 K, CLH and FISH measured IWCs even smaller than those detectable by the Leo-Lidar, but that is with low certainty.’

So we think that the plot provides confidence in the in-situ measurements.

11. *Page 29455, Line 14: I don’t think these conclusions can be drawn from this comparison. The relationship between lidar attenuated backscatter and iwc is complicated by all the uncertainties raised in my earlier comment.*

    Response: We understand,of course, that there are uncertainties in the determination of IWC from the lidar extinction coefficient. However, we used the algorithm published –and often cited- by Heymsfield et al. (2005). Our findings are based on the validity of the results of that study and we feel that on this basis we can draw conclusions.

12. *Why use this particlular lidar? There are longer data sets at various locations from millimeter radar. IWC and radar reflectivity are much more directly related than attenuated backscatter and iwc.*

    Response: We used this lidar simply because it is placed at our institute (Forschungzentrum Jülich). We derived a cirrus climatology from the measurements and found it to be in very good agreement to other mid-latitude climatologies (Sassen and Campbell, 2001; Immler and Schrems, 2002) in terms of altitude coverage and optical properties (i.e. optical depth). Thus, we feel confident with respect to the reliability of the measurements for the purposes of our analysis. Also, as we do not agree with the referee’s opinion about the use of Heymsfield’s IWC-lidar backscatter relationship (see previous comment), we do not see the need to use other data sets.

13. *These in situ instruments are generally flown with particle probes are they not? Why is it not reasonable to "validate" the hydrometeor occurrence thresholds with the particle probe counts? The particle probes may not be perfect but the presence of hydrometeors is* *registered with accuracy and that data set has the necessary requirement of being concurrent with the FISH and CLH.*

Response: First, cloud particle probes were not available for all flights. Second, for some of the flights where cloud particle measurements were available, we compared cloud occurrences derived from both the water and the particle measurements. The agreement was not good at cloud edges and in thin cirrus where the closed path hygrometers are more sensitive to cloud particles than the particle probes. We believe that the reason for this is that the sampling volume of the cloud particle probes is much smaller than that of the closed path hygrometers.

14. *Page 29456, Line 13: The community, I think, has moved beyond T-IWC parameterizations. The value of illustrating these relationships with this dataset is that modelers can create similar relationships from model output and see if their more sophisticated representations of cirrus produce IWC as a function of T as a function of latitude in a manner similar to these measurements.*

    Response: On page 29459, line 11-13, we state that our intent is not to provide a parameterization for use in models. We agree with the referee that the merits of a study like the one we have presented lie in the possibilities for comparison and evaluation with other instruments, approaches, etc. It is clear to us now that despite our statement of intent, any use of the word "parameterization" is misleading to a reader. Thus, the word will be removed entirely when used in reference to our own analysis and replaced with words that still accurately describe our intent, such as "relationship".

15. *Page 29456, Line 21: Isn't geography just indicative of different formation mechanisms and the processes that maintain cirrus? Even these processes are not exclusive to a region but a function of the underlying spectrum of large scale dynamics and turbulence. It would be most interesting to investigate how the large-scale dynamics within which cirrus exist vary as a function of latitude.*

    Response: We agree with the referee that analyzing cirrus microphysics as a function of large scale dynamics, turbulence, and latitude would be very interesting and potentially informative. Although that is beyond the scope of the analysis presented here, it is part of the first author's Ph.D. thesis work.

16. *Page 29456, line 25: The statistical significance of these fits need to be quantified. Are the differences meaningful given the sparsity of the data? I'm not at all convinced that they are meaningful.*

    Response: See answer to general comment 1 (b). Additionally, the main goal of this work is to present this large dataset in a way that is both manageable and meaningful. With that in mind, the results from our analysis show interesting observations and we offer possible explanations for them. While we understand the importance of quantifying the significance of our results, that level of analysis is beyond the scope of this work. We hope that our results lead to further discussion on these topics and also that researchers with more advanced statistical techniques will expand upon our conclusions.

17. *Page 29459, Line 5: Without more thoroughly establishing the statisticsal significance of the bimodality, the features and their change with temperature is an intriguing curiosity.*

    Response: See answer to general comment 1 (b) and the response to specific comment 16.

18. *Figure 8: Recent analyses of data sets collected with particle probes that can filter for shattering artifacts from TC4, SPartICus, and MACPEX show very strongly that >1/cc concentrations are extremely rare in cirrus (occur less than 1% of the time). This would argue strongly that the number and size plots in figure 8 have not been filtered for shattering and are erroneous. Any inferences drawn from these plots should be removed from the manuscript.*

    Response: See answer to general comment 2.

19. *Page 29461, conclusion 1: This conclusion arises purely from the fact that shattering artifacts have not been accounted for the particle probe data. The conclusion is wrong and has been shown to be wrong in recent analyses of data collected with probes that can account for shattering artifacts.*

    Response: See answer to general comment 2.

20. *Page 29461, conclusion 2: The validity of the bimodality needs to be established with significance testing before this conclusion is justified.*

    Response: See answer to general comment 1 (b) and the response to specific comment 16.

21. *Page 29461, conclusion 3: I have no idea how this conclusion emerges from the data presented here. Just because the number concentration is small?*

Response: We supported this hypothesis through the detailed study from Spichtinger and Krämer (2012) of Ni-PDFs in the low temperature range, which shows that the peak at low Ni is mainly caused by heterogeneous ice formation. In addition, cirrus from pure heterogeneous freezing are also connected to low IWCs. This is because they occur only at very low vertical velocities, which means that the air parcel is lifted only by a short distance and the decrease in temperature is small. The IWC of a cirrus cloud is controlled by the difference between the temperature (in terms of water vapor saturation) where the air parcel originated and the temperature where it is observed (see Schiller et al., 2008, Fig. 8). Thus, heterogeneously formed cirrus show a combination of low ice crystal numbers and low IWCs. We have included this in the explanation of hypothesis (iii):

'…both heterogeneously and homogeneously formed cirrus clouds are visible in the IWC PDFs as two modes of lower and higher IWC, where the lower and weaker Ni/IWC mode might represent heterogeneous freezing and the higher and stronger mode represents homogeneous freezing, as one would expect.'

Ice water content of arctic, midlatitude, and tropical cirrus

Part 2: Extension of the database and new statistical analysis

A. E. Luebke, L. M. Avallone, C. Schiller , J. Meyer, C. Rolf , and M. Krämer

Responses to referee 2:

First of all, we thank the referee for the constructive comments that helped us to improve the manuscript. We have conscientiously treated all suggestions and hope the new manuscript and the point to point responses will lead to publication in ACP.

General Comments:

1. *One conclusion of this study is that the IWC is determined by ice crystal number (Ni). This conclusion is reached by comparing the correlation of PDFs of IWC and Ni. However, this is not convincing. IWC is determined by the ice crystal growth from vapor deposition in cirrus. At higher temperatures with larger saturation vapor pressure, IWC tends to be large. Ice crystal radius (Ri) is certainly determined by the growth process. This may be the reason why IWC has a good correlation with Ri (Liou et al., 2008). In comparison, Ni is determined by the ice nucleation and influenced by the ice aggregation. The correlation between PDFs of IWC and Ni seen in this study may be because the analysis in this study did not separate different geographical regions. The lower peaks of PDFs of IWC and Ni are corresponding to the TTL (having both lower Ni and IWC), while the higher peaks are from other regions (having both higher Ni and IWC). Thus more solid analysis may be needed to warrant the conclusion.*

   Response: First, we want to point out here that we don't think that the Ni and IWC correlation is due to the fact that we did not separate the data by geographic region. By separating the data into three temperature ranges, we have divided the data into groups that are characteristic of certain cirrus types. For example, the data set in the temperature range below 205 K can largely be attributed to the data from the tropics, including the TTL. The data sets in the remaining two temperature ranges can be attributed mostly to the Arctic and midlatitude regions, respectively. Both lower and higher peaks can be seen in the PDFs of IWC and Ni for the three temperature ranges.

Second, in response to the remarks with respect to the influence of IWC by Ni: IWC is influenced by both the ice crystal number (Ni) and the crystal radius (Ri) via the following relationship:

$$IWC = 4/3 \cdot \Pi \cdot Ri^3 \cdot Ni$$

which is integrated over all sizes, for the case of spherical cloud particles.

The contributions of Ni and Ri to IWC are not well known, but we believe that from our analysis the correlation between Ni and IWC becomes visible. We think that the process behind this correlation is as follows:

Cirrus from pure heterogeneous freezing have low Ni and are also connected to low IWCs. This is because they only occur at very low vertical velocities, which means that the air parcel is lifted only by a short distance and the temperature decrease is small. The IWC of a cirrus cloud is controlled by the difference between the temperature (in terms of water vapor saturation) where the air parcel originated and the temperature where it is observed (see Schiller et al., 2008, Fig.8). Thus, heterogeneously formed cirrus show a combination of low ice crystal numbers and low IWC. On the other hand, cirrus that form homogeneously in higher updrafts have higher Ni and also higher IWC, since the vertical displacement (and temperature shift) of the air parcel is larger. These thicker cirrus show a combination of higher ice crystal numbers and higher IWC.

We have reduced the statements (i) - (ii) on (old) page 29461 from conclusions to hypotheses. In addition, we now say that Ni influences (not determines) IWC. Further, we have altered the explanation of hypothesis (iii):

`... both heterogeneously and homogeneously formed cirrus clouds are visible in the IWC PDFs as two modes of lower and higher IWC, where the lower and weaker Ni/IWC mode might represent heterogeneous freezing and the higher and stronger mode represents homogeneous freezing, as one would expect.'

As a result of an analysis motivated by comments from referee 1, we also removed the discussion on the interplay between Ri and IWC. We have seen that the Ri we derived in the first version of the paper might have been influenced by ice artifacts from shattering, and that, especially in the warmer temperature range, Ri could have been larger. We feel that the findings of our study are more sound with the inclusion of the additional analysis and the alterations made to the manuscript.

2. *This authors claim the lower peak of PDF of IWC is caused by the heterogeneous nucleation while the higher peak is by the homogeneous nucleation, and refers Spichtinger and Kramer (2012). However, from Spichtinger and Kramer (2012) it was suggested that the lower peak is produced from the homogeneous and a mixing of homogeneous/heterogeneous nucleation under the very slow large-scale motions superimposed with high frequency short wave. The dominant role of heterogeneous nucleation mechanism for TTL cirrus is still not concluded.*

   Response: We do not agree with the referee's interpretation of the results from Spichtinger and Krämer (2012). From their Figures 10 and 11 (shown here) it becomes obvious that the peak in the Ni-PDF at low ice concentration is caused by heterogenous+homogeneous freezing (see especially the blue curve in Fig. 10, which represents this case). Heterogenous+homogeneous freezing means that the ice nucleation is initiated by heterogeneous freezing alone, which might be followed by a second homogeneous freezing event in cases where the supersaturation with respect to ice exceeds the homogeneous freezing threshold in spite of the presence of some ice crystals. The blue curve shows a first peak caused by pure heterogeneous freezing events and a second peak at higher Ni, where additional homogeneous freezing events have occured. From Fig. 11 it becomes apparent that the observed peak at very low ice concentrations only appears in the model simulations when the heterogenous+homogeneous freezing events are mixed with the other cases and that it stems from the events of pure heterogeneous freezing. Though a small part of this peak (about 15%) is from pure homogeneous freezing (from other cases), the remaining and more dominant part is a result of pure heterogeneous freezing. Thus, we feel that our interpretation that the two modes of the Ni-PDF are separated by the two freezing mechanisms is correct.

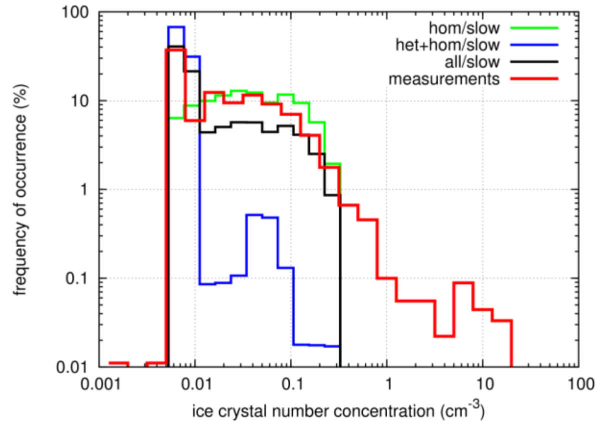

Fig. 10. Frequencies of occurrence of ice crystal number concentrations from simulations in very slow large-scale motions ($w_{LS} \leq 1\,cm\,s^{-1}$) with homogeneous freezing (hom, green), heterogeneous/homogeneous freezing (het/hom, blue) and a mixture of both (all); the red line represents the measurements of Krämer et al. (2009). Note that the lowest concentration bins of the model simulations are clipped for better comparison with the measurements.

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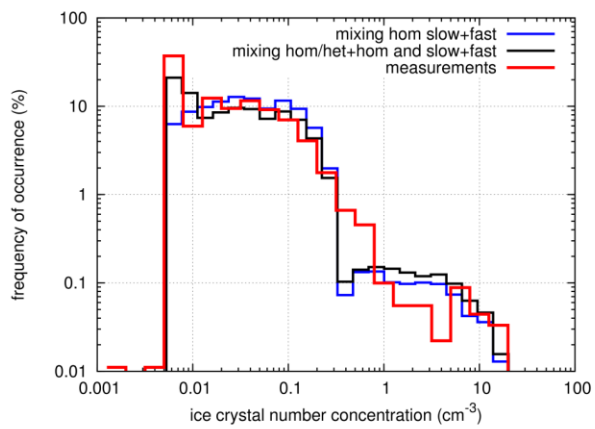

Fig. 11. Frequencies of occurrence of ice crystal number concentrations: mixture of simulations in very slow ($w_{LS} \leq 1\,cm\,s^{-1}$) and faster ($w_{LS} > 1\,cm\,s^{-1}$) large-scale motions with homogeneous freezing (hom, blue) and with heterogeneous/homogeneous freezing (het/hom, black); the red line represents the measurements of Krämer et al. (2009); the lowest concentration bins of the model simulations are clipped off for a better comparison with the measurements.

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3. *The description in section 2.1.3 for FISH and CLH analysis methods, e.g., equations (1) and (2) is not clear. This will be further noted in my specific comments below.*

   Response: See the response to specific comment 6.

4. *Some discussion in conclusion section on how the data collected in this study can be used for climate models will be valuable.*

   Response: We agree with the referee that including how our data can be used in climate model studies is useful. Given the observational nature of our data and analysis, we think that our data could be best used for model evaluation purposes. The final paragraph of the conclusion will be expanded to indicate this.

Specific Comments:

1. *P29445, L7, "owing to the number and . . .". It is unclear here what you mean for "number"?*

   Response: Here we are referring to the fact that there are several different microphysical properties that can affect the radiative role of a cirrus cloud. We have replaced "number" with "assortment" to make this clearer.

2. *P29446, L14, "and measured variability is not considered". Variability of what?*

   Response: We have changed this to "and measured variability of IWC is not considered" to make it more clear.

3. *P29446, L17, "to create these models". Here "models" should be "parameterizations".*
   Response: Specific comment 3 has been taken into account and the text will be changed to read "relationships" instead of "models".

4. *P29448, L19, "Sect. 2.3" should be "Sect. 2.1.3".*

   Response: Specific comment 4 has been taken into account and the text has been changed accordingly.

5. *P29449, L17-19. I don't understand how "water vapor saturation mixing ratio" can be used to determine IWC since it is not always saturated to ice in cirrus (i.e., it can be either super- or subsaturation with respect to ice).*

   Response: While we understand the referee's concern and agree that saturation cannot always be ensured, the assumption of saturation mixing ratio was used to process some of the data from Schiller et al. (2008). As stated on page 29449 L14-17, the only IWC data used in our analysis was calculated using water vapor measurements, not assuming saturation. Any

IWC data that was calculated using the alternative method was not included (e.g. that subset of data from Schiller et al. (2008)).

6. *P29450-29451. The description is unclear to me, e.g., equations 1 and 2. What is "enhancement factor" in the last line of P29450? If eIWC is the value calculated by subtracting H2Ogas from H2Oenh, will H2Ogas,adj = H2Ogas by inserting eIWC into equation 1?*

   Response: We understand the referee's confusion since there was not a definition given for what the enhancement factor is and which variables it is affecting, which is helpful for understanding equations 1 and 2. On page 29448 L13-16, there is a brief explanation of the enhancement and where it comes from. This will be expanded upon to make it clear that the enhancement factor is the value by which the IWC has been enhanced due to the usage of the subisokinetic inlet.

7. *Table 1. It will be good to add "latitude coverage" from Figure 3 and "temperature or altitude coverage" from Figure 4 in Table 1 for each field campaign.*

   Response: This comment has been taken into account and Table 1 now includes latitude and temperature coverage for all flight campaigns.

8. *P29455, section 3.1. It will be helpful to add some discussion on the Schiller et al. dataset. Is the dataset contained in this study (Table 1) completely different from Schiller dataset? Or are there some data in common?*

   Response: We agree that it is important that it is understood which data from Schiller et al. (2008) is included in this analysis. This is currently explained in the paper on page 29449 L14-19. However, it will be changed to reflect that the FISH data used is also found in Schiller et al. (2008):

   "It should be noted that the FISH data that was included in this analysis is a subset of that from Schiller et al. (2008), coming from the 29 flights where water vapor (H2Ogas) was measured separately and could be used to calculate IWC from the enhanced total water (H2Oenh) measurement.".

9. *P29456, L20, "additional variables to explain it". Which additional variables?*

Response: The text will be changed at this point to provide examples of additional variables: (e.g. vertical velocity, geography, etc.).

10. *P29456, L28-29, do you mean that you only sample in situ cirrus, not anvil cirrus?*

    Response: This is an important distinction to make. The text will be changed to make it clear that flights where active convection was studied were not used to develop the fit lines. Therefore, not only was in situ cirrus used, but potentially aged anvil cirrus as well.

11. *P29458, L16 and 24. "cirrus formation mechanisms". It would be good to add some details on the formation mechanisms here.*

    Response: While we understand the importance of cirrus formation mechanisms in this field of study, this paper is not about those mechanisms and therefore we do not think going into detail about it would contribute to the manuscript. Thus, no changes shall be made to reflect this comment.

12. *P29459. What is "y" in Equation 4?*

    Response: Besides x and f(x), the remaining coefficients do not have any physical meaning, which is explained on page 29459 L20-23. The text will be altered to make this more clear: "The remaining coefficients (i.e. y, t, w, A, and z) are constants…".

13. *P29461, L16. "relationship is also seen in this comparison". I am not able to see this relationship.*

    Response: The full paragraph from which this line comes has been taken out. Please see the response to general comment 1 for further details.