Responses to Reviewer

We deeply thank reviewer for the helpful comments and suggestions which present us important guidance to the improvements our researches and the manuscript. We have made corresponding corrections and added more descriptions in the revised manuscript according to the comments. Our responses below to the comments are shown in blue. Revisions in the manuscript are marked with same color. The language in the manuscript has been edited by a professional and native-English editor. In the revised manuscript, we also made some corrections on Figure 1-11 to get a well-formed and clear figure. Formats of some references were also corrected. We hope our responses and modifications can meet with your approval.

Review #1 of Properties of mid-latitude cirrus cloud from surface Ka-band radar observations durin2014-2017 by Huo et al.

I thank the authors for taking the time to respond to my comments. I am pleased with many of the responses and recognized that the manuscript has now gained in quality after this first round overview. However, I still feel that some extra work remains necessary to clarify the goal and result of this study before publication, as detailed below.

General comments:

1. The definition of "cirrus" and “ice clouds" has been updated and the paper results are consequently easier to interpret. However, the authors still completely disregard the possibility of having mixed phase clouds. Any ice cloud with a temperature higher than -38C has a high chance (depending on temperature) to include supercooled water. This, as well as the consequences on radar measurements and subsequent climatologies, should be discussed in the paper. The only time they are mentioned is in section 5, where the authors do acknowledge that a significant amount of ice clouds can have a liquid-origin, i.e. originate partly from the supercooled state of the mixed-phase layer. This proves that they also shouldn't be ignored from the previous analyses. Including these clouds in "ice clouds” is not necessarily a problem, as it is often done from remote sensing, but they should still be discussed and their impact on radar measurements detailed.

Ice clouds mostly contain ice crystals. We agree that supercooled water should exist in ice clouds. According to reviewer’s advice, we added some discussions about the supercooled water in section 2.2 (last sentence).

However, what’s the proportion of supercooled water in ice cloud over Beijing? Can it be neglected? We searched in publications but did not find useful answer. Could reviewer please give us some references? And, the identification method of supercooled water from KPDR data need to be developed at present. We hope review could allow us to make such investigations in future.

2. The authors now further discuss the sensitivity of the radar in section 2.2 but I am still not completely pleased. The reader needs more quantitative estimates of cloud types that are

discussed here or, more importantly, those who are not represented in this study. Please include an estimate of the IWC and OD thresholds, “This KPDR has strong detection capability for ice clouds "(P3. L. 68) is not sufficient. I think that the frequency of occurrence shown in Fig. 2 (about 4% or less for cirrus) demonstrate that thin clouds are not well detected, and it would be useful to know the detection limit. This threshold should be stated in the abstract as well.

Did reviewer mention to section 2.1? From the content, we think it is section 2.1 not 2.2. According to reviewer’s suggestion, we added more explanations and discussions about the capability of Ka radar in section 2.1 (line71-79).

As shown in Equation (4) and (5) in the manuscript, IWC and OD is related with the number density, particle size, particle distribution function, and particle shape. Radar reflectivity factor is also related with these parameters. The statistical relationship between IWC and reflectivity is presented in Equation (5) with various A and B for different cloud types at different temperature and places. Thus, reflectivity is also connected with the properties of cloud target. Wu et al. (2019) reported that for the 94 GHz cloud profiling radar (CPR) on CloudSat, thin ice clouds with IWC lower than approximately 0.4 mg/m^3 are invisible by comparison with MSL. CPR has a sensitivity of approximately −30 dBZ. This work gave us a reference.

Synchronous measurements by lidar are useful to validate the detection capability of Ka radar in cirrus clouds. There has been a lidar and a Ka radar making daily measurements at our Tibet observatory since last year. We will make such investigations when data are met.

3. It is still difficult to consider that the observations presented in this study are in general terms representative of all mid-latitude ice clouds". The authors acknowledge this several times within the text, and discuss e.g. in Section 5 specifically of formation mechanism "in Beiing”. I would therefore encourage (again) to change the title to "Properties of ice clouds over Beijing from surface Ka-bandradar observations during 2014-2017”.

We revised the title according to reviewer’s advice.

Specific comments:

1. The authors have changed "cirrus " to "ice clouds " but in several encounters it makes no sense(e.g 1. 20, 1. 27, 1. 78). Please correct.

According to the comments from both reviewers, more revisions were made in the Introduction (e.g. l. 22-23, l.27-32).

2. Fig 2 shows diurnal variations of ice cloud occurrence. but I’m not so sure to see the highest occurrences mentioned p. 6 l. 154. Are they statistically significant? It would be best to include at least standard deviations. Also, how are the ice cloud detections influence by precipitation, which might also occur at specific time of the day?

They are not statistically significant. These occurrence frequency were calculated from whole dataset using the equation (1) and (2), not the average values of the four years. So, no standard deviation is calculated. According to our analysis (Huo et al 2020, in Chinese), the average precipitation frequency from all clouds is about 5% during 2014-2017. The

precipitation from ice clouds is lower than 1%. Thus, the influence from precipitation is not considered in this paper since it makes little contribution.

3. p. 7 l.159: What is exactly meant by "extinction process"? If the decrease is indeed robust (see previous point) then the authors should propose some hypothesis at least.

Sorry for that. It was revised as decay process. Some hypothesis was added (please see l.170-171).

4. In Fig. 5 and Fig. 6, is there a real added value to include the Ze scatter plots rather than the mean and standard deviations? I would suggest to include only the plots (e) and (f) of both figures, together.

Figure 5 and Figure 6 were obtained from large number of data which cannot be presented wholly within a scatter plot. To keep the symmetry, we plot the mean and standard deviation in separate panel. We hope reviewer could accept them.

5. Same comment concerning Figures 8 to 11, comparing them is really difficult. Why not have only 2figures that show i)only the overall distribution for all seasons and temperature bins and ii)another similarly one with the PDFS subsetted by thresholds?

We have thought to show the frequency of four seasons in one figure. But the figure became messy when the origin-type lines were added since those lines were different in four seasons. In addition, the maximum numbers used to calculate the normalization frequency are various among seasons and temperatures. Considering these reasons, we showed them in four figures in order to present a clear result. We hope reviewer could accept them.

6. Section 5: please justify the use for a-34 dBZ threhsold, why this exact value to separate in situ and liquid-origin cirrus?

This work is based on the hypothesis that cirrus clouds over Beijing originate by two types. Then, we tried every threshold between -32 ~ -38 dBZ and found that cirrus clouds in spring and summer were separated clearly into two groups by the threshold of -34 dBZ and the two groups demonstrated different PDFs. Then threshold of -34 dBZ was selected.

7. P. 18 365: superficial really doesn't sound good, "preliminary" perhaps?

Sorry for that, it was revised according to reviewer's advice.

Review #2 Properties of mid-latitude cirrus cloud from surface Ka-band radar observations during 2014-2017 by Juan Huo et al.

For the second review, I start a new discussuion thread, because it will become too confusing to fill in comments and thoughts again in a different color in the author‘s response document that went back and forth already several times.

The authors have worked on the main two aspects of the first review, but to my opinion not in a completely satisfactory way. That means the paper still needs some improvement before it is mature enough to be published. I will outline this in the comments below concerning the two previously mentioned points and some new comments that appear with the revised version of the paper. The comments are sorted in order of appearance in the manuscript. Note that text copied from the manuscript is in italics and quotes.

1) Line 22: ‘*Cirrus clouds, composed of ice crystals, is ice cloud.*‘
First, this sentence is gramatically not correct and second, it is unclear for what reason the sentence should is placed here. Generally, the new text needs some language polishing.

This sentence was revised as “Cirrus clouds are dominated by ice crystals”. This revised manuscript has been polished by a native English editor.

2) Lines 26 – 30: *‚Ice clouds exert potential warming effects on the Earth–atmosphere energy system. Studies show that the occurrence frequency of the cirrus clouds, part of ice clouds, exhibits latitudinal variability ranging from 50% in the equatorial regions of Africa to 7% in the polar regions (Hahn and Warren 2007; Sassen et al. 2008, 2009; Stubenrauch et al, 2006).* → see ** below
Ice clouds are an important component of the planetary radiation budget in terms of magnitude; plus, they influence hydrological and climate sensitivities and affect surface climate (Lawson et al. 2019; Yang et al. 2015).‘
** Since you also deal with mixed phase clouds, you need to say also something about them at this point.

Yes. According to reviewer’s advice, we added some descriptions about the ice clouds in the Introduction (please see Line 27-31).

3) Line 45: please specify the temporal resolution.

Yes. According to reviewer’s advice, we added an example since temporal resolution changes between radars.

4) End of Section 2.1 Ka-band radar: *‚It should be noted that KPDR is more sensitive to larger particles in the cloud particle size distribution since the reflectivity is proportional to the D6 (D is particle size).‘*
You need to write a sentence here that thin ice clouds containg mostly small ice crystals are not detected!

According to the advices from both reviewers, we added more descriptions about the capability in this section (please see Line 70-79).

5) Line 78: *‚Cirrus is ice cloud‘* This is no sentence → see comment 1)

It was deleted.

6) Section 2.2 Ice cloud identification: This section is mostly the same as before, describing the ice cloud observations as they were all cirrus clouds. This needs to be rewritten to place it in the context of ice cloud observation at temperatures < -10 C and then define that those at < -38 C are cirrus.

According to reviewer's advice, we revised this section. The structure is rearranged and some descriptions are added. We hope they could satisfy reviewer.

7) Line 169-170: ‚*Both the maximum CTH (13.35 km) …*' In line 167 the maximum was defined as 12.9 km.

Does the followign values need to be corrected?

‚ *… and the highest mean CTH (10.16 km) are found in summer, whereas winter has the minimum CTH (11.25 km) and lowest mean CTH (7.66 km).*'

Very sorry for our carelessness. The old ones are forgotten to revise. They have been replaced with new statistical results. All statistical numbers in this section (section 3) have been examined and corrected in the revised manuscript.

8) Table 1: all values have slightly changed, why is that ?

In the revised manuscript, in order to select the ice clouds with high confidence, we used a new threshold ("mean cloud-top temperature less than −40°C and the maximum cloud-base temperature less than -10°C") for ice cloud identification, not the old "the cloud-top temperature should be less than −30°C and the cloud-base temperature should be less than 0°C". With the new criteria, some cloud clusters are excluded. About half of them are from summer. Therefore, statistical results based on these new ice clouds selected by new criteria changed.

9) Figure 4 (and respective text):

The maximum cirrus optical depth is reported to be between 1-3, e.g. Sassen et al., 2008; Kienast-Sjögren et al., 2016. In your Figure 4, COD up to 20 is seen, pointing to the glaciated mixed-phase clouds that you have detected. You write in lines 224-225:

‚*The proportions of CODs lower than 3 in spring, summer, autumn and winter are 46%, 36%, 49% and 52%, respectively.*'

If this portion of ice clouds is at the lower temperatures, then these are probably in-siu-origin cirrus clouds. This could be mentioned in the text.

According to reviewer's advice, we illustrate the ice clouds with cloud base temperature lower than -38 in new Fig.4. The related text were also revised (please see Line 236-243).

10) Lines 263-264: *‚At temperatures higher than −38 C, ice clouds can form heteroge- neously or homogeneously (Kanji et al. 2017).*'

This is not correct. At temperatures > −38 C, ice form solely heterogeneously from liquid cloud drops. In case liquid cloud drops are supercooled down to = -38 C, they freeze homogeneously at this temperature. I'm sure that this is correctly described in Kanji et al. (2017).

Sorry for that. It has been revised as "At temperatures higher than −38°C, primary ice clouds form only when aided by ice nucleating particles (Kanji et al., 2017).".

11) Line 291: *‚… until they are lifted to the ice formation temperature region.*'

… Until they are lifted to temperatures < -38 C.

It was revised as reviewer's advice.

12) Line 300 (and Figures 8-11):

First I want to mention that the new plots are really very intersting! But:

‚ … central temperature of −65C, −60 C, −55 C, −50 C, −45 C and −40 C…'

In Figure 6, the lowest detected temperature is -50 C, and in the previous version of the paper, Figure 8 and 9, the lowest center temperature was - 45 C. Where does the new data below -45 C come from?

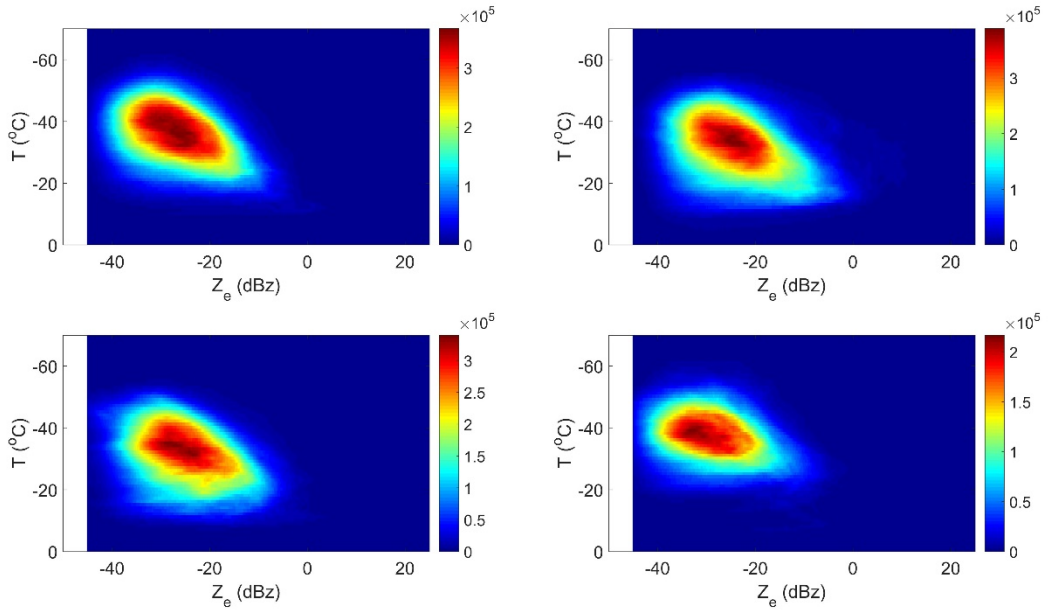
(Side comment: I do not understand that you argue in the author's response that *‚-65 C temperature occupy very small percent of the temperature range*' (your Figure RC6) to explain that there are no cirrus clouds at temperatures < -50 C in your observations – and now such

cirrus are presented. One major comment in my first reviews was that cirrus < -50 C are not present in your data set. But, as the coldest point at mid- latitudes is around – 65 C, absence of these cold, thin cirrus means that they are not detected. We discussed that back and forth and now such cirrus appear …)

Due to insufficient descriptions and inappropriate expressions, we are sorry to bring reviewer so many doubts.

First of all, we need to state that all the analysis results, including every table and figure in this revised article, come from the same dataset.

Some ice clouds with temperatures below -50° C are shown in Figures 8-11, but they are not shown in Figure 6, which is related to the plotting way of Figure 6 and the data itself. The frequency is represented by the statistical counts calculated at 0.25 dBZ and 1 ºC interval. The variation range of these counts is too wide (from 300 to 500,000). In Figure 6, the statistical counts was divided by 5000 firstly and then displayed with color by the color map for aesthetic reasons. Our naked eyes couldn't d distinguish those very low values because they are two small relative to 500,000 although they have been displayed in Figure 6. We tried other plotting ways. For example, the figure below show the original counts, not divided by 5000 with different color map). Similarly, these low counts are also hard to distinguish from the background. Thus, the ice clouds below -50 ° C shown in Figures 8 to 11 are real, but compared to the ice clouds above -50 ° C, the counts is too small to be shown clearly in Figure 6. Since purpose of this section is to investigate the dominated temperatures and reflectivity, current Fig.6 is appropriate to show the key results. In order to reduce the confusion of readers, we added relevant explanations in the text for Figure 6. Also, quantities represented by the color bar are added. In addition, the maximum counts in each panel in Figures 8~11 are also provided for comparison. It should be noted that the counts in Figures 8~11 are calculated within the range of T ±1 ° C interval while in Figure 6 it is calculated within T ± 0.5° C.



– Anyhow, the database is not consistent now, the data at temperatures < -50 C should be added to the observations shown in Figures 5 and 6 for warmer temperatures. Are they included in the analysis shown in Figures 1 – 3 and Table 1? If not, this should also be done.

As those answers above, each table and figure in the revised manuscript is obtained from same database.

– Also, in the panels of Figures 5 – 9 the number of data points (or hours of observations) should be noted to give an impresssion on the statistical significance of the observations.

According to reviewer's advice, hours of the observations are added in this section (please see line 85). That is, there are more than 28,000 hours of measurements during the period 2014-

2017.

13) Figures 8-11: In the Figure captions is would be good to note in addition that the dashed lines should correspond to in-situ-origin and the dotted lines to liquid-origin.

According to reviewer's advice, they were added in the captions.

14) Figures 8-11: The PDF's at -45 C and -40 C look very different (much smoother, no modes) than those shown in Figure 8 of the previous version of the manuscript – why is that ?

It is because that data used for analysis changed.

(1)The previous criteria for identifying ice cloud is the temperature of the cloud top should be less than −30°C and the temperature at the maximum Ze layer and at the cloud base should be less than 0°C. The new criteria in the revised paper is that mean cloud-top temperature less than −40°C and the maximum cloud-base temperature less than -10°C. There are about 300 cloud clusters are excluded. Some of them are clouds with thick depth, for example > 5 km. About half of them are from summer.

(2) Previous Figure 8 shows the PDF of all ice clouds (the "−30°C; 0°C threshold"). The new Figures 8-11 show the PDF of all cirrus clouds (ice clouds with cloud-base temperature lower than -38°C).

15) Lines 308 ff: The results are very intersting, but mainly only described in the text. It would make the paper scientifically more sound if some ^explanations for the discovered features could be offered. Here are two examples to demonstrate what I mean, but there are more places.

- '*There is no cirrus cloud detected in summer and autumn below −65 C.*'

- In Figures 9 and 10, there are no cirrus clouds below -60 C !
- Could this be because the troposphere is higher in summer and autumn because of higher sun intensity, so the highest and coldest cirrus are not detected?
- The higher sun intensity in summer, causing more convective active,would also explain a higher percentage of liquid-origin cirrus.

- *'Above −55 C, the peak frequency center in winter locates at smaller reflective value than that in summer.'*

What does that mean physically for the cirrus ?

All in all, the results are sound taking into account the underlying processes (which is great), so it would improve the paper to discuss that.

According to reviewer's advice, we added more explanations and discussions in this section (e.g. Line 329-333, Line 334-339). Please see the new section 5. Thank reviewer very much for these comments and suggestions.