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

This manuscript by Huo et al. shows a climatological analysis of ice cloud macrophysical properties based on measurements of a ground-based radar situated in Beijing, China. More specifically, the authors explore four years of radar measurements, from which are identified cirrus clusters, and statistically analyse the occurrence, cloud heights, depths and horizontal extents associated with these clusters. Microphysical retrievals are not performed but coincident cloud optical depths by Himawari-8 are used to complete the analyses. Finally, the relation between reflectivity distribution and cirrus origin is investigated.

The manuscript is rather clear and well written. This study is motivated by the large uncertainties on our current understanding of ice cloud formation mechanisms and their representation in climate models, which I agree constitute significant problems. However, I strongly feel that two points hold this manuscript back in that effort and it was quite difficult to view past them for the review. First, the definition of cirrus used by the author must be better clarified. Second, the instrumental sensitivity to the said cirrus should be quantified. Further details on these two points are provided below. Unfortunately, without providing this precise context to the study, I’m afraid the manuscript and its findings cannot be as useful to the community as they should be. There is otherwise a lot of potential in this radar dataset and the analyses performed here, particularly concerning the observed seasonal variations of cloud properties and in the cirrus origin study that is new and very interesting.

I therefore strongly encourage the authors to clarify the manuscript by responding to the following comments, and advise the editor to accept this manuscript only after substantial major revisions.

Thanks to reviewer for the comments and encouragements. We have made careful revisions according to both reviewers’ suggestions. Our responses here and below are marked in orange which follows the format of our replies to referee#2. Revision portions in the revised manuscript are marked with red. We hope reviewer can accept them.

General comments:

1. My two general comments are actually related. The first one concerns the cirrus classification. The identification procedure described in section 2.2 is twofold. First, clusters are categorised in nine cloud types following a procedure described in a procedure detailed in a previous study by the first author, and two types are selected. It would still be very useful to have a short summary of the main features of the “Cs” and “Cc” classes in this section. Then the cloud-top (CTT) and cloud-base (CBT) temperatures are used to filter out cirrus clouds. The conditions are CTT < -30ºC and CBT > 0ºC. I understand that this can be highly debated but considering the instrumental setup (see point 2) in this study I think it would be wiser to go for the stricter CTT < -40ºC threshold and avoid strong contamination by mixed-phase layers. I might have a biased perspective but when reading the title I’d expect a study about “cold” ice clouds (and their associated processes), while many of the results focus on in-cloud temperatures > -38ºC so it gives the (perhaps wrong) impression that these are largely represented in the dataset. If it is decided to keep the same threshold then an histogram of the CTT and in-cloud temperature distributions must at least be presented so that the types of cirrus explored in this study are made clear to the readers.

On a side note, I do not understand why the authors use the Heymsfield et al. (2017) reference to justify the -30ºC threshold while including again this reference when explaining that other study use a -38ºC threshold, please be consistent.

As mentioned by reviewer, there are different definitions for cirrus cloud in previous publications, some of which use the - 30 ºC, some of which use the - 10 ºC, and some use 0 ºC as a temperature threshold. We had thought that the difference may be due to the different scope of the definition, some are broad definitions and the other are narrow definitions. Our original definition standard mainly refers to the judgment standard from CloudSat/CPR, because the detection principle of CPR is similar to KPDR.

Considering the advices from two reviewers, we decided to adopt the opinions of reviewers and define these clouds as ice clouds in order to avoid confusion. And in the revised document, in order to ensure 100% ice cloud, we raised the temperature threshold standard: cloud-base temperature is required to be below -10 ^{o}C. Cirrus cloud is ice cloud. In the revised manuscript, the cirrus clouds are picked out from ice clouds according to cloud-base temperature (T_{base} < -38 ^{o}C) for specific analysis and for a contrast. For example, the occurrence frequency (see section 3.2) and formation type of cirrus (section 5) are analyzed. We hope that our revisions will be approved by reviewer.

2. More importantly, and I think critical for this paper, is the question of the instrumental sensitivity. In section 2.2 the authors remind us that cirrus are typically associated with very small optical depths (e.g. OD < 3, in this section), and one can logically wonder how many of these clouds are actually detected by the ground-based radar. And even if detected, what portions of these cirrus are actually seen in terms of vertical and horizontal extents? This has critical impacts on the determination of the cloud-top height, occurrence and horizontal extents, which are central results to the study, and must be answered to properly set the context of the study. At minimum, it is necessary to show a sensitivity analysis of this cloud radar to the cirrus optical depth (or ice water content).

Such preliminary analysis is presented in section 3.4 where the Himawari-8 COD retrievals are compared to the radar reflectivity measurements, but these have a few issues. First, CODs up to 25 are detected, how do these classify as cirrus? Second, the Himawari-8 product used here is based on visible/near-IR channels and it is well known that these are not the retrieval of choices for thin cirrus studies, where thermal infrared channels should be preferred. There seems to be Himawari-8 retrievals of ice cloud properties that use such split-window techniques (e.g. Iwabuchi et al, JMSJ, 2018) – these would be much more robust and meaningful. Additionally to the analyses currently shown in section 3.4 the authors should show distributions of Himawari-8 COD and CTH retrievals when the radar detects a cirrus but also when it doesn't, to estimate the amount of missed cirrus and biases in their CTH due to the radar sensitivity. Another possibility would be to use lidar measurements from the same region to detect the cloud occurrence, if any is available to the authors.

Referee#2 made the same comments about the detection ability of KPDR. We use four ways to confirm the detection capability of KPDR, that is, for most cirrus clouds in nature, KPDR can detect them. The confirmation process have been presented in detail in our replies to the comments (A) of reviewer#1. As the content is long, they will not be repeated here. We sincerely ask reviewer to refer to our answers to this comment (from last two paragraphs on Page 2 to Page 5).

We use Himawari-8 COD data because there are more available optical thickness data for analysis when compared with other available COD datasets. In the article, COD data is used only when Himawari-8 determines as cirrus and radar determines as ice cloud. We have no specific analysis for the data with COD greater than 25, and just regard it as the retrieval uncertainty because the COD retrieval of cirrus clouds is inherently challenging. But overall, these abnormal CODs are in the minority, and the final statistics based on large number of data are meaningful.

Reviewer suggested that clouds that AHI measured as cirrus but radar didn't could be used to investigate the radar detection capability. We think this work is a little risky because the field of view of AHI is larger than that of radar. It is very likely that ice clouds appear in the field of view of AHI but not in the field of view of radar. Therefore, it is difficult to explain the contrast results and verify the detection capability. If reviewer agrees, we will not make such an analysis.

3. On a more positive note, the two first comments do not impact the validity of some of the findings such as the seasonal variations (since the same detection threshold applies), or the origin analysis that are based on the reflectivity. There are very interesting finding worth being published and I'd be very much willing to discuss them further after proper clarification of the two aforementioned points in the revised manuscript.

Thanks a lot. We have made revisions in the manuscript carefully according to reviewers' comments. We hope reviewers could accept them.

Specific comments:

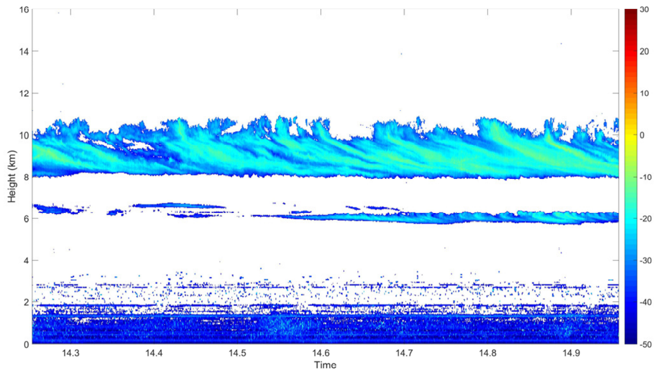
Considering the importance and impact that revising the first two points might have on the paper, I have for now limited specific comments to a couple of important points directly related to them.

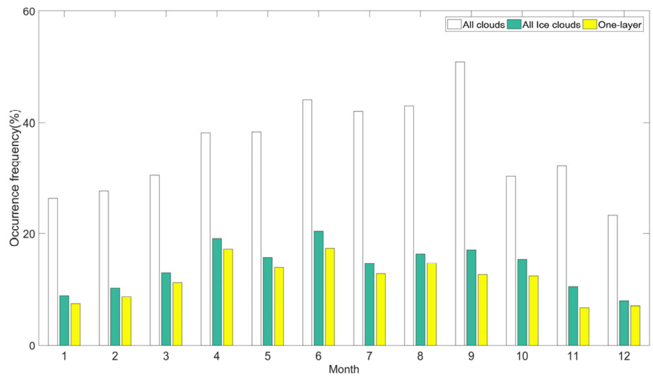
1. The introduction is very general, it would be more interesting to discuss a bit more the state-of-the- art in radar-only cirrus analyses. How did they tackle the instrumental sensitivity issues to provide usable climatologies?

According to reviewer's suggestion, we add more introductions about this portion in the revised manuscript. For example, "Radar can perform long continuous observations and has high temporal resolution, which is more advantageous than the aircraft in understanding the characteristics of cloud daily changes, the formation and development of clouds. Regular calibration of radar signals can ensure the stability of radar and support long-term data for cloud climatology research."

2. Regarding the occurrence analyses (section 3.2), could you please comment on cirrus detection in the presence of underlying liquid clouds or precipitation? Would that be a limiting factor to the statistics presented here?

If the height gap between two cloudy bins is >150 m (about five radar bins), then the cloud profile is regarded as layered. For a cloud cluster, if more than 80% profiles having single layer, the cloud cluster is regarded as single layer. Please see the left figure below where clouds have two layers. The right figure presents the monthly occurrence frequency of all clouds, all ice clouds and one-layer ice clouds. The one-layer ice clouds dominate over Beijing. Multi-layer cloud is not a limiting factor.





3. In section 3.4 and after, a "mean reflectivity" is used. Could you please define the meaning of this quantity? Is it vertically averaged?

The mean reflectivity is the averaged reflectivity of all cloudy radar bins, not only vertically but also horizontally (namely, time-averaged). We revised the sentence (line 215) to make the meaning more clear.