Review #2 of 'Properties of mid-latitude cirrus cloud from surface Ka-band radar observations during 2014-2017' 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 of review. 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 an 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.

2. The authors now furhter 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" (p. 3 l. 68) is not sufficient. I think that the frequency of occurence 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 abstact as well.

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 Beijing". I would therefore encourage (again) to change the title to "Properties of ice clouds over Beijing from surface Ka-band radar observations during 2014–2017".

Specific comments:

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

2. Fig. 2 shows diurnal variations of ice cloud occurence, but I'm not so sure to see the highest occurences 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?

3. p. 7 l.1 59: 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.

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

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

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

7. p. 18 l. 365: “superficial” really doesn’t sound good, “preliminary” perhaps?