The revised manuscript "A long-term study of cloud residuals from low-level Arctic clouds" has largely addressed most of the comments of the reviewers. However, a few comments remain not be thoroughly addressed. And I have some additional comments. I recommend the publication of this manuscript in ACP if these comments can be addressed.

1. Both reviewers mentioned making comparison for the particle number distribution in cloud and outside cloud to ensure the measurement quality from whole-air inlet (Reviewer 1 comment 3 and 7, Reviewer 2 minor general comment 6). Cloud particles may also experience possible additional losses in the whole-air inlet too due to e.g. the different size distribution compared with ambient aerosol particles. But the authors decided to not follow the comments because the authors argue that "appearance or termination of the cloud event can also be caused by a change in air mass with differences in aerosol properties." In my opinion, the information on the particle loss of whole-air inlet for cloud droplet sampling is necessary because if there were significant difference, it would affect the ratio of cloud residuals to total particle concentrations shown in Fig. 6 and Fig. 7. Although the authors showed a new comparison of cloud residual concentration vs. total particle concentration in accumulation mode, this does not necessarily mean that total particle concentration has no artefacts/free of loss and of ensured quality.
2. A related comment to the comment 1 is that the description of the whole-air inlet is lack, especially the important role of the data for this study. One cannot expect readers to follow the setup just stating "fulfils the World Meteorological Organization guideline". For example, how is the inlet heated, and to what temperature? Has this setup been validated previously regarding the particle loss? If so, references could be cited. More details would be helpful here.
3. In the new structure, for Section "3.2.1 Warm clouds" and Fig. 6, are all these data for temperature < 0 C? If so, it would helpful to clarify explicitly in both the text of 3.2.1 and caption of Fig. 6.
4. Section 3.2.2 heading "Cold clouds" seems not to be consistent with the scope of discussion that it covers. It mainly discusses the influence of temperature rather than cold clouds. Maybe a different heading would work better.
5. Some responses to reviewers should be incorporated to the revised manuscript as they will be helpful to other readers. For example, the response to the comment D (Pg 17 of the authors' response file), comment "L. 158-159" (Pg 23 of the authors' response), comment "L121-122" (Pg22), comment.
6. Some "new" statements are introduced in the "conclusions" part while how they are drawn is not straight and completely clear in the main text. For example, L663, "The cluster analysis of cloud residual size distributions showed a D50% dependence on updraft for liquid clouds (clusters 3–5)." This finding is not explicitly drawn, although the authors show that D50% depends on updraft velocity and updraft velocity of clusters 3-5 are different. A direct way to draw such a finding would be a figure showing the D50% dependence on updraft for data belonging to clusters 3–5.

   L666-667, the finding "A clear relationship between a decreasing total particle number concentration and a decrease in D50% was also observed" is also not directly drawn in the main text. In L537-538, a figure of D50% vs. particle number concentration and

updraft velocity is needed as the current presentation of the trend e.g. D50% values is hard to follow.

L668-670, it is not clear which data and discussion these statements are based on since the data were not discussed by April-October or November-March.

7. The abstract has not fully reflected the changes in the focus of the revised manuscript as much of the findings in the main text and conclusion are missed in the abstract.

Minor comments:

1. L523 and Fig. S9, why different clusters (2-5) show different slopes? Shouldn't it be that all cluster have similar slopes if the transmission efficiency of GCVI (size-dependent if I understand correctly) are correctly applied and the GVI is free of artefacts?
2. The caption of Fig. 2 can still be misleading if one reads separately from the main text. I suggest rewriting "cloud particle number concentrations derived from the FM-120 fog monitor measurements (red)" as "*corresponding* cloud particle number concentrations derived from the FM-120 fog monitor measurements *and transmission efficiency of GCVI*".
3. L445, Fig. 6 or Fig. 7?