Reply to Reviewer #1

Authors are grateful for the valuable comments of the reviewer. Responses are given below:

Comment 1: It is stated on page 9 line 3 that the measured signal-to-noise ratio was sufficient to retrieve the calibrated polarimetric variables. I think it would be useful to specify explicitly somewhere how you ensure observations of ρ hv and therefore retrievals are unbiased in regions of poor SNR.

Response: For the manuscript we only used data with SNR exceeding a certain threshold, defined by the polarization decoupling properties of used antenna and depolarizing properties of scatterers. Details are presented in the given reference (Myagkov 2016). For current study the applied threshold in SNR was about 30 dB for non-depolarizing particles. Thus, we are sure that our retrievals are not significantly contaminated by noise. The data with lower SNR is not considered in the algorithm. This clarification is added to the corrected version of the manuscript. See p.9. L19 – L23.

Comment 2: Case 4: The interpretation of the relatively low polarizability ratios as a thick internal liquid water layer is unconvincing without coincident lidar measurements (which were unavailable due to attenuation). I suggest that the sentence on page 14, line 16: "There is also a thick internal liquid layer..." be edited to reflect that this is speculative.

Response: The mentioned sentence describes Fig. 6d (in the new version of the manuscript). This figure represents lidar volume depolarization ratio. Low values of the lidar depolarization in clouds are normally related to liquid particles. Please note, that polarizability ratios of 1 are not interpreted as liquid layer in the manuscript. First of all, we only analyze polarizability ratios at cloud tops. Second, polarizability ratios in the manuscript characterize only ice crystals and NOT water particles. For clarification, we added references to Fig. 6d on page 15, L1-2.

3) Are you sure there is no seeding occurring in Case 4 (i.e. is it consistent with criterion 2 on page 9)?

Response: Please note, that for the analysis in Case 4 we consider the cloud top at 5.5 km height. As there were no clouds above, there was no seeding effect.

Technical Corrections:
Page 4, line 25: "This mode implies", should be "This mode employs"?

Response: Changed

Page 5, line 7: "As the result. . ." should be "As a result. . .". This remainder of this sentence sounds a bit awkward: "the proposed method is only applicable for data with high values of ZDR which can be only induced by strongly oblate particles and which, therefore, can be undoubtedly separated from prolate particles". Suggest this is rewritten.

Response: Changed

Page 7, line 25: " Further this radar is. . .", omit "Further", or replace with "Hereafter"?

Response: Changed

Reply to Reviewer #2

We express our thankfulness for the valuable comments of Reviewer #2. From the Reviewer's comments we infer that he considers the scope of the manuscript to be different than implied in the introduction. Before providing detailed replies to the single review comments we would therefore like to emphasize, that the manuscript under review is a follow-up of the technical description of a newly developed hybrid-mode cloud radar. Herein, we intend to present a first statistical analysis of the apparent shape of ice crystals observed in mixed-phase clouds. The focus was thus by purpose set on meteorological cloud properties than on the discussion of details of radar polarimetry (which is dealt with in the predecessor article). Responses are given below:

Comment #1: First, the introduction (on mixed-phase clouds) is mostly irrelevant to the topic of the paper.

Response: Please note, as indicated in the introduction to this response letter, the aim of the manuscript is twofold. (1) It provides an important information for modelers on the first stages of formation of ice particles in mixed-phase clouds. Therefore, in the beginning of the introduction we show to a reader why mixed-phase clouds are important and why shape information is required to study them. (2) The manuscript shows a validation of the retrieval of polarizability ratio previously described in Myagkov et al 2016 which is based on polarimetric cloud radar observations. Both pieces of information are strongly related to mixed-phase clouds. For the first point it is clear. For the second one, the validation of the retrieval in its current state cannot be done using observations of e.g. cirrus clouds, because ice particles in this type of clouds have very low apparent density (refractive index) and therefore their polarimetric signatures are not well pronounced. Moreover, in cirrus clouds ice particles, present in the same volume, often have strongly varying shapes, that hampers the validation of the retrieval.

Comment #2: An introduction of the main topic of the paper, polarizability ratios, is completely missing from the introduction, and only appears in the discussion were even then this reader could not grasp what it was. I strongly suggest that the authors replace the current introduction of the paper with a relevant discussion of polarizability ratio to position non-experts to follow the subsequent discussion of the observations.

Response: Our study on shape of ice crystals in mixed-phase clouds is based on measurements with a newly developed polarimetric cloud radar. The idea was to publish the implementation of the radar and the shape retrieval technique first (recently published, AMT Myagkov et al 2016) and then to publish small statistics with the comparison to the lab data (current manuscript). The paper on the retrieval is referenced in the current manuscript and therefore we do not see a point to show all the formulas of the polarizability again. Instead, in the end of the introduction section (page 5-page 6) we briefly explain in words how the retrieval works and give a short explanation of what polarizability depends on. We added an additional sentence giving the reference to the book of Bringi and Chandrasekar in the corrected version of the manuscript. In addition, we extended the discussion of Figure 1 and inserted a new Figure 2 to illustrate to a reader how the algorithm works in principle. We also added a sentence that the polarizability ratio is only related to actual particle shape, if the apparent density (characterizing ratio of air and ice in a approximating spheroid) of the particle is known.

Comment #3: I would argue that the title of the paper (shape-temperature relationships of pristine ice crystals) does not accurately reflect what is presented. The polarizability ratio is more akin to an "apparent shape" as it depends on the shape and "bulk" density (dielectric properties) of the ice as the authors present. This is clear from Figure 10 in Myagkov et al (2015a) which shows that other that being an indicator of oblate/prolate dominated scatterers the ratio does provide much information on the scatterers (i.e. something I can use in my cloud model to characterize ice). Unfortunately the dielectric properties of the ice in general is not know, so what the authors presented in the paper is probably the best one may hope for: the measurements are consistent with the lab data (itself a worthy conclusion).

Response: Title is changed to "Relationship between temperature and apparent shape of pristine ice crystals derived from polarimetric cloud radar observations during the ACCEPT campaign"

Comment #4: I would suggest a more accurate description of the results it that use of the polarizability ratio (as proposed by Myagkov et al. 2015) is an improvement over previous techniques to identify regions in the cloud dominated by columnar vs plate-like ice crystals.

Response: As it was mentioned in the first response, one of the goals of the manuscript is just to validate the retrieval published in Myagkov et al 2016. The current manuscript does not imply any further developments of the algorithm. A sentence that the algorithm is just a combination of two approaches proposed by Dr. Melnikov and Dr. Matrosov is given on page 5.