Comments:

The authors present an analysis of a snapshot of a riming event that took place during BAECC SNEX experiment in Finland. Overall the story is rather interesting and deserves to be published.

Major comments:

Two claims in this manuscript caused some concerns, because I am not sure that they are well supported.

- On p.2. line 8-10 the authors claim that the presented analysis presents an upper bound of what we can retrieve with profiling cloud radars. Is it really an upper bound of what we can expect? I understand what you are trying to say. You are doing an analysis that is beyond traditional use of spectral moments. But calling it an upper bound is too strong. You are not using multi-frequency and/or dual-polarization radar observations. I would argue that there is a room to improve, even given the current measurement capabilities.

- A similar type of statement is in the conclusions p 18. line 18 -25. In this study you don't use any in situ observations. It is a general knowledge that more you know a better retrieval/ analysis you can perform, but you have not demonstrated what can be gained. It does not mean that adding in situ data would not help, but the way one analyses the data may change. A much better way of demonstrating what can be gained and how in situ data can be combined with the analysis would have been to use data from the lower cloud layer, 1 km and below, and combine it with ground-based observations collected during the experiment. If you would have done this, you could have made a very strong statement on what is needed and can be gained. Actually, it is a stated goal of BAECC SNEX and surface precipitation data was collected during the event you are presenting as can be seen in (Kneifel et al, 2015).

One might also wonder how representative is the data selected for this study is. You are presenting only several minutes of data from an event that lasted several hours. As you have stated in the manuscript riming was also taking place in the lower cloud layer. Could you contrast the two? Could you carry out the proposed analysis for the lower riming case? This kind of comparison is interesting and would show applicability of the method and its robustness.

Minor comments:

p. 7 line 14. You state as a matter of fact that the cloud is a liquid-topped mixed phase cloud. How do you know that this is a liquid-topped mixed phase cloud? Only later in the text you describe how you made this inference. This statement gives an impression that it is easy to identify where liquid is, which is not as it is stated in the introduction.

p. 9, line 16 You are saying that the mixed-phase cloud extends from the surface to 3.4 km, is it really true? The sounding shows two cloud layers one from the surface to 1 km and the other from 2.5 km and higher.

p.9 line 17-18. Do you have any other support for identification of the spectrum peak as due to liquid? Could it also be the newly formed ice? In Fig. 5 you can see how ice falls out of this layer. How do you separate these? Is there an objective method of separating an ice from liquid peaks? Can one do it automatically?

In the comparison of the modeling results and observations, there is not much discussion on why reflectivities are not matching. Could you elaborate on this?

p. 15. Line 16-17. I don't think you can claim that a dual-layer Mie sphere is a good or an appropriate model that mitigates the known scattering problem. If anything, Leinonen and Szyrmer (2015) show that a more complex model is needed. I personally, do not think that in this case uncertainty in the scattering model is very important. But I don't like the statement that the dual-layer Mie sphere scattering model is an appropriate approach. It gives an unsubstantiated impression that it is more appropriate then a simple Mie sphere. If anything, I would use a spheroidal model (Hogan et al, 2012; Matrosov et al. 2005).

Hogan, R. J., L. Tian, P . R. A. Brown, C. D. Westbrook, A. J. Heymsfield, and J. D. Eastment (2012), Radar scattering from ice aggregates using the horizontally aligned oblate spheroid approximation, J. Appl. Meteorol. Climatol., 51(3), 655–671.

Matrosov, S. Y., A. J. Heymsfield, and Z. Wang (2005), Dual-frequency radar ratio of nonspherical atmospheric hydrometeors, Geophys. Res. Lett., 32, L13816, doi:10.1029/2005GL023210.