Review of Identification of multiple co-located hydrometeor types in Doppler Spectra from scaning polarimetric cloud radar observations

The paper introduces a new shape retrieval of ice particles using the Doppler Spectra of ZDR and RhoHV and a spheroidal scattering model. This retrieval is based on the retrieval of Myagkov et al. 2016 and is expanding their approach by considering the full spectra as opposed to the main peak of the spectrum. This enables them to retrieve the size of up to 5 different species, potentially giving a possibility of investigating secondary ice production in more details.

I am a bit disappointed about the poor quality of the paper, especially in terms of understandability of the method, and readability of the text. Also, while I agree that the retrieval is a valuable extension of Myagkov et al. 2016, I would have liked to see further improvements to the method (such as moving away from the spheroidal scattering assumptions). In my opinion, this would be an essential next step, in order to reduce the possibly large biases introduced by the spheroidal method, especially when considering aggregates. Nevertheless, I suggest the manuscript to be reconsidered for publication in AMT after the following major and minor comments have been addressed.

<u>Major:</u>

1. the text needs a lot of rewriting, both structurally and for a better understanding of a few sentences (which I will specify in the specific comments). In my opinion, the new method is not explained satisfactorily. I am missing a detailed explanation on why you chose to divide the spectrum into 5 parts, and how you are dividing the spectrum, which has not been mentioned anywhere. I do not know which 5 species/shapes you want to cover with that, I can only think of rimed particles, aggregates, plate-like crystals, columnar crystals. Of course you can also have super-cooled liquid water, but for that we know the shape very well, so no retrieval is necessary. I would also like to know if you are trying to “track” the different species throughout the different heights, or if the division of the spectrum is just random. Also, please explain the main peak method better, for me it was difficult to understand that. Other more “trivial” retrievals such as the vertical wind velocity are explained in great detail, even though it is not that relevant to the study and many institutions are running wind retrievals on an automatically basis. I would suggest to rewrite the paper the following way:

I like Figure 6 and 7, perhaps you can move that to the method section? Then it might be clearer how the retrieval works. If you include the full RHI scan in Figure 6 (without the separation into the 5 parts) and 7, you can first explain the main peak method and then the spectral approach in a clearer way. I would first explain in detail which peak is used in the main peak approach, indicate that in the figures and then continue on to your new approach and show how that is different and how you are dividing into 5 parts. How are you then using the 5 parts? Are you averaging along the Doppler velocity of each part? If so, it might be helpful to provide the averaged elevation dependencies of ZDR and RhoHV either in addition to the Doppler spectra parts or instead of them.
Also you should specify what the polarisability ratio and the degree of orientation are and provide the formulas. This has of course been mentioned in Myagkov et al. 2016, however, these are not standard variables and I think it is therefore necessary to explain that again. You can even use panel a and c of their figure 13 to show the polarisability ratio and orientation. When you are explaining polarisability ratio you can also mention that the shape retrieval can be used with sZDR and sSLDR.

2. I am missing in this paper a few clear statements about the problems of the method. i.e. you should critically discuss the scattering properties that you are using. You did not specify which spheroidal

method you are using, in Myagkov et al. the retrieval is based on Rayleigh. It is well known that spheroidal methods have issues of representing the scattering of ice particles accurately, especially in the Mie-region (which is reached at Ka-Band for aggregates) and for low density ice particles such as aggregates. I understand that generating a large DDA database which is necessary to do such a retrieval is difficult. However, I know that other working groups have done that and you could collaborate with them. I think that especially now that you want to retrieve the shape of multiple species a different scattering method should be taken into account. In my opinion it would have been beneficial to collaborate with other groups who have sophisticated scattering databases based on e.g. DDA. Another thing to discuss: can you be sure that along all elevation angles the species in one height doesn't change? For larger heights this spans quite a large cloud area!

3. Along with comment 3: aggregates have a small polarimetric signature, especially if you are assuming a spheroidal scattering approximation. How much more insight do you even gain when you are considering the spectral lines with aggregates? I am expecting that similar to rimed particles they will just have a polarizability ratio of close to 1. Can you even distinguish between these different particles then? If you can only distinguish between them because they have a different fall velocity is it even worth splitting the Doppler spectra into 5 parts? Would it not be beneficial to just separate between ice crystals and aggregates? I would suggest to show the theoretical polarizability ratio you would expect for all your 5 different species (which I am assuming to be rimed particles, aggregates, dendritic ice crystals and columnar ice crystals and something else). This would show that a separation into 5 parts is actually necessary and that not i.e. 2 parts would suffice.

4. Why do you need to retrieve the actual air motion? Do you use the Doppler velocity for anything and therefore a knowledge of the fall velocity of the particles are valuable? Otherwise you could just move the Doppler spectrum to 0m/s. This would save a lot of effort and space in the paper.

<u>Minor:</u>

1. discuss your figures better/more in the text. Especially Fig 4 and Fig. 9 are not discussed enough. Most readers are not familiar with all the Radar and Lidar variables and can not draw their own conclusions.

2. In this regard: why do you show the Lidar measurements? Due to the rain the Lidar doesn't even penetrate into the relevant cloud regions, so you can just omit the plots.

3. You need to refine your naming in the equations better. For example, before equation 1 you say that E_h is the horizontally polarized plane of the received wave. You never specified what E dot means. Then in equation 4 you say that E_h is the copolar element of the backscattering matrix. Please be specific with your definitions, and stick with one! (also co and cross-polar are not the same thing as horizontal and vertically polarized)

4. In your conclusions I am missing a clear outlook and a discussion of the method and its caveats.

5. The colormap in your Figures looks like jet, perhaps you could consider moving towards a colormap which does not have so many deficiencies, if you prefer rainbow you could use e.g. turbo or something similar.

6. why are you only analyzing the first time period of 7.11.2014 with your spectral retrieval? The second time period would have been interesting, since there the polarimetric variables are large!

Specific comments:

Line 47-48: you say peak signal of the Doppler spectrum, could you specify which variable you mean? Spectral reflectivity? Spectral SNR? Spectral ZDR?

Line 61: you say "the polarimetric variables exhibit sensitivities to specific fall velocities" This is not true please specify this sentence
Line 64: large aggregates do not fall faster than 1m/s, their fall velocity saturates around 1m/s. The Doppler velocity is therefore often used to distinguish between rimed particles and aggregates. Please correct!

Line 91: why do you need the LIDAR? The super-cooled liquid water layers are not visible in your case studies because the lidar doesn't penetrate the rain

Eq. 1-4: see minor comments

Line 125-126: your statement that prolate particles have a negative ZDR is not true. This is only valid if they are oriented in a very specific way. In my experience I have never seen negative ZDR that is only associated with prolate particles, it is mostly attributed to differential attenuation or conical graupel

Eq. 7 and paragraph below that: why do you describe LDR in such a detailed way? Your retrieval is based on ZDR and RhoHV and those are the relevant parameters.

Line 166: please specify the scattering model you use!

Section 3.1: as mentioned in the major comments: please introduce polarizability ratio further, how do you calculate it? How are sZDR and RHV used for that?

Line 187: Also small particles form distinct peaks in the Doppler spectrum (as you can see in your Figure 12)

Line 189-192: please rephrase this sentence, hard to understand

Line 198: if you say not more than 5 shapes can be present please name those shapes. I would even refer to this as particle types, because if you consider dendritic particles, they can have many different shapes

Line 211-212: I don't understand why you need to "harmonize" the Doppler spectra in order to derive the vertical wind, isn't it the opposite way around? You retrieve the wind from the PPI scans in order to match the Doppler spectra across different heights/elevation angles?

Figure 1: Specify which Doppler spectrum (sSNR? SZDR?). Usually Doppler spectrum refers to spectral Ze, which you are not using here correct? Also this sketch is too idealized. I have never seen a Doppler spectrum with 5 distinct peaks. How do you know that columns are falling faster than small dendrites but slower than large dendrites? In my opinion you don't need that plot, but rather a good explanation of how you separate into 5 parts!

Figure 2: it is nice to have a block diagram, however, you should also describe it in the text! e.g. in the text it is not mentioned that you are using minimum square error function which is an important detail.

Line 221-224: I don't see how you depicted turbulence in Figure 1

Equation 8: what is Vw? What is Vh?

Figure 3: I do not understand this figure! Since the wind retrieval is something that is frequently done, this is not necessary. But if you want to leave it in you have to work on that. A few things that I don't understand are: What are the dashed lines? Why is Vh in the dashed line not the same as the grey Vh line? I don't understand the beta angle or the alpha angle in this context.

Paragraph about aliasing: how do you determin n?

Paragraph horizontal wind: do you need to know the true fall speed? See major comment 4

Figure 4: Why is the 0° isotherm not at the same height as the melting layer? Looks like a 500m difference here! Also: if you want to include the lidar measurements please change the colormap you use, I can not see anything in panel f

Line 294-296: How do you know that there is liquid all the way to the cloud top? The lidar doesn't penetrate through the rain.

Please discuss the Figure in more detail. I am missing more discussion of LDR, what does that mean, mean Doppler velocity, what can that tell you about the particles,…

Line 303: what does "transition towards strong spherical particles" mean? Also: I barely see a tendency if I look at Figure 5

I would like to see plots of your RHI scans, similarly to how you have done it in Figure 6. I would suggest to either discuss the radar observations of Figure 4 in more detail to show how they are relevant, or only show Ze, LDR and then the RHI scans.

Figure 5: in your Figure 4 you show that you have the temperature information, It would be helpful to have that also in this figure to draw conclusions about the ice particles (i.e. ice particle habits are strongly dependent on temperature)

Line 314: I would not say it "effectively" identifies the shape, since you are not comparing against other measurements you do not know it that is actually true

Line 315 and following: I don't understand the discussion here. The sentences are really hard to understand. Do you mean that because there are prolate shaped particles in the time between 09:46 and 09:48 they also have to have been present earlier? What do you base that on? You are saying that the cloud changes drastically, therefore I would not compare the two time periods and draw conclusions on the microphysics that are happening.

Figure 6: I would suggest to also show the RHI of the complete spectrum (see also major comments). I would also adjust the colorbar of ZDR and RhoHV, because I can not see any tendency in the variables. ZDR for part 1-4 looks like it is close to 0 for all heights and elevations. Would it not be nice to show

the method on a case that exhibits larger polarimetric signatures? Then the benefit of having a spectrally resolved retrieval would be more obvious. Here even the slowest falling particles show barely any ZDR.

Line 328: You say dealing with noise is a challenge, yet you do not say how you deal with it!

Line 330-331: please rephrase the sentence “this diminished SNR … fails to reflect in ZDR and RHV” how do you know that?

Line 337: ZDR is always really close to 0, I do not see a tendency, so I would just assume that the particles are nearly spherical. Perhaps if you change the colormap it will be visible.

Line 341: ZDR and RHV look nearly exactly the same to me as for part 1,2.

Figure 7: While it looks nice to have all the separate doppler parts and it helps the understanding, it is really hard to see the elevation dependency of ZDR and RHV here. I would suggest to add another figure with that (I assume that you average the different parts over the Doppler velocity to obtain one value of ZDR (RHV) per elevation, so you can show that in the figure)

Line 365: do you mean part 2 and 3? Part 1 is barely existing here

Line 372: are your particles transition into spherical particles? Or is turbulence removing the small ZDR signal that was present at higher altitudes? How can you tell the difference? Or are the largest prolate particles aggregating, therefore leaving only the small prolate particles which have a smaller ZDR? I do not think that particles can change their shape if they have already developed into distinct prolate shapes. For this analysis, again the temperature information would help

Paragraph below Line 373: I do not agree with this analysis. First of all, your SNR was too low to retrieve the shape of the particles which seeded into the region below 4km. This does not mean however that there where no prolate particles that seeded. In addition, your argument that in the later period you see prolate particles is in my opinion not an argument that the particles you had 15 minutes earlier were generated the same way. The cloud clearly changed drastically between the two time steps. In the second time period it is likely that ice particles where formed via a mixed layer at cloud top. However, if the same process was present I would have expected much higher spectral ZDR values to be present at a similar height in the first time period. Especially since in the second period the LDR is really large for the newly generated ice particles! For this discussion it would be really helpful to have the retrieval also for the second time period, so that it is possible to compare the polarisability ratio for the two cases. So if you want to draw any microphysical conclusions I would strongly suggest to include the retrieval of the second time period. I agree with your statement on SIP, however only because ZDR of the slowest falling part of the Doppler spectrum is so low. If there was SIP I would expect much larger values.

Paragraph below 380: Nice discussion about SIP and the melting layer (ML), however, how is that relevant here? You do not have two LDR layers within the ML. I would rather discuss the large number of papers that have found elevated LDR/ZDR above the ML in the needle growth regime than that very special case that Dmitri Moisseev had.

Line 396-397: Why are they not able to infer any information about the “background population”? They also have the Doppler spectrum so they are able to do that. In the Doppler spectrum the particles separate due to their different velocity

Figure 9: do not include lidar here, in this case it really has no valuable information for your case study. And please discuss the figure in detail in the text, do not assume that the reader can deduce all necessary and important information by themselves.

Line 409: Why do you use the time period between 20:01 and 20:11 for the main peak approach? Did you not specify that you are using the time period from 20:37 until 20:47? In the earlier time period there is barely any LDR signal, it is an unfair comparison then to use the main peak approach on a time period where the polarimetry is expected to be low. I would suggest to use the same time period as you are using for your spectral retrieval

Line 411: the increase you see in the polarizability ratio is really small. Is that even significant?

Line 413: is your scattering approximation suited also for liquid?

Line 418: not necessary to mention the specific figure of Li et al, they are not the only ones that observe a second mode in a Doppler spectrum

Paragraph below 419: please rewrite, very hard to follow. The main point is you have a second, slow falling mode which might indicate multiple ice species, especially since the slow falling particles show a different LDR!

Figure 11: not necessary, you can get all the information from the figure 9

Fig. 14 and 15 are nice, however, there are already many figures and by now the reader has understood how the retrieval should work. So focus on the figures with the polarisability ratio and degree of orientation!

Line 462-464: I do not see the indication of fragmentation of dendrites. What are you basing that on?

Line 481-484: while I agree with the statement, my opinion is that with the provided analysis you can not draw that conclusion (see comment above)