**Atmospheric
Measurement
Techniques
Discussions**

Interactive
Comment

Interactive comment on "Towards the improvement of cloud microphysical retrievals using simultaneous Doppler and polarimetric radar measurements" *by* Y. Dufournet and H. W. J. Russchenberg

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Review of - Towards the improvement of cloud microphysical retrievals using simultaneous Doppler and polarimetric radar measurements -, by Y. Dufournet and H. Russchenberg.

This paper describes a new and innovative technique to retrieve the cloud microphysical properties (including particle habit and orientation) from Doppler and polarimetric variables measured by the TARA radar. This paper is well organized and clear. I rec-

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ommend publication of this paper once the following comments are addressed :

1) There is a need for a better discussion in the introduction about the instrumental combinations that have been explored in the past (last paragraph of p. 2 and beginning of p. 3), not only the dual-frequency and radar-lidar : radar + IR radiometry (e.g., Mace et al. 1998), radar reflectivity and air temperature (Hogan et al. 2006; Protat et al. 2007), radar reflectivity+ Doppler (Matrosov et al. 2002; Delanoe et al. 2007; Deng et al. 2006). The list of papers about radar-lidar should also be expanded (Tinel et al. 2005; Wang and Sassen 2002). There is also a new type of algorithms that treats both the radar-only, lidar-only and radar-lidar parts of the clouds under study (Delanoe and Hogan 2008; Deng and Mace 2008) that need to be mentioned to be exhaustive.

2) The description of the radar processing (section 2.2) needs to be improved. The Nyquist velocity of the radar should be mentioned. In section 2.2.4, I don't understand why additional steps are needed when the SNR is high (line 21, page 7), which should be easiest to process. Also regarding the additional clipping (end of p. 7), there is a suggestion that the threshold should be -15 dB for sLdr. But wse have no indication that this threshold works in all conditions. Is there any evidence that you could show to make the case stronger?

3) In section 3.4.2, p. 16, first paragraph. I would show the Doppler and Zdr in the zoom of Fig. 10 because you have only one case to show and because I am not convinced by the arguments presented in the text. This would allow a better discussion to be made with all the pieces of information at hand. For instance, from what I see, I don't trust the physical interpretation of the change in microphysics observed between 16.30 and 16.32. It looks more like an instrumental problem than a switch to dominant pristine habit in a region of high reflectivity that does not look so different from the surrounding profiles. This particular point deserves more study of the observed radar variables (and maybe tranmistted power if you measure it).

4) Figure 4: very difficult to understand where the signal is and the effect of clipping

and other processing when you don't have the original spectra. Could you find a way to show the different steps in a more illustrative way. Fior instance, in Fig 4b and 4c, I can't see where the signal you want to extract is and where the artefacts are !

Minor edits :

p. 3, l. 27 : "extremely noisy". Just a question : s it really instrumental noise of the complex microphysical processes at very short time scale ?

p. 4, l. 5: define "FM-CW", and maybe in a sentence the difference between a pulsed radar and an FM-CW radar.

Eq. 2.2 : define v

p. 5, l. 9-13 : At 45 degrees, the horizontal wind dominates largely the vertical component (fall speed + vertical wind). I would not say that you would be able to extract the fall velocity in any of these $45°$ measurements.

p. 6, l. 24 : please give the Nyquist interval of the radar.

p. 7, l. 21-22 (see also general comment) : I don't understand this statement : for high Z the signal to noise ratio is always high, so why would you need any additional clipping in that case ?

p. 8, l. 22 : there is a word missing in "where meteorological . . . are present". Also, l. 26, I would not call the variations "statistical". Maybe "higher-order" ? Also, l. 30-31 you don't filter out the second order fluctuations but the higher-order fluctuations, isn't it ?

p. 12, l. 8: same thing as previously, with a second-order fit you filter out the orders higher than the second ? l. 11 and l. 143, I think it is Figure 8, not Figure 7.

p. 13, l. 13 : I suggest the following change : "heaviliy rimed crystals (graupel and hail)" instead of "graupel and hail). Also, l. 24, "tilted" instead of "titled".

References : The Delanoë et al paper appeared in 2007, not 2006

Alain Protat, Guyancourt, 17/01/2011

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