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Interactive comment on “Riming in winter alpine snowfall during CLACE 2014: polarimetric radar and in-situ observations” *by* J. Grazioli et al.

Anonymous Referee #1

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General comments:

The paper is well written and provides very interesting observations of snowstorms in complex terrain. The authors do a nice job in compiling statistics for numerous events. The data presented help support and refute hypotheses from previous studies.

Overall, I have numerous concerns, though none are particularly major. These mainly have to do with some of the data interpretation, and will affect the main conclusions of the study. These are outlined below in detail.

Specific Comments:

1. Reliance on classification scheme – clearly, any algorithm meant to reduce the radar

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observations to a single classification cannot be "true". Even your particle images show that, indeed, mixtures of particles are present at small scales, let alone the large radar sampling volumes. Because the classification results are used to identify "rimed precipitation", the authors need to better qualify that this is your way of parsing the data. In reality, aggregation/riming are very difficult if not impossible to distinguish with radar alone, as the authors state in the introduction. Thus, at least some of the statistics and conclusions are sensitive to the method of classification used (i.e., there may be some differences if another scheme was used instead).

2. Page 3, line 9: You are somewhat conflating mass and flux here. Riming leads to an increase in mass because the particles are acquiring additional ice. The increased fall speed increases the mass flux, as you've stated.

3. Page 4, lines 15-20: Nice overview. There is another brand-new paper on these topics in early online release (Schrom et al. 2015, JAMC).

4. Figure 1: What does WGS84 mean in the captions? Please define.

5. Figure 1: Please check the labels of the two sites: KS and MAE. The MAE site indicates a higher elevation (2230 m) than KS (2061 m), but the KS site is located in a region of higher terrain according to the color scale.

6. Page 8, line 9: "Riming is…turbulent mechanism" is a bit odd. I get what you are saying, but it implies that riming is inherently turbulent (which is not true). Please clarify.

7. Figure 2: Doesn't event duration have just as important a role as riming degree? Some indication of event duration (or normalization – instead of using total accumulation, use maximum rate of accumulation, etc.) is needed.

8. Figure 3 and discussion in text: You do indicate event durations in the table, but no mention of it is given when discussing Fig. 3. Using your data displayed in Table 1 and estimated from Figure 3, you can show that the correlation between event duration and total accumulation is largest (Rˆ2 $\sim$ 0.72), much larger than the correlation between

PRP and accumulation shown ($R^2 \sim 0.44$). Additionally, if you take the average accumulation rate (total divided by duration), the correlation between this rate and PRP is larger ($R^2 \sim 0.60$), which is probably more important to discuss than total accumulation. All this is to say that, though PRP is important for total accumulations, it is not the most important effect- event duration is, at least for your dataset.

9. Also, to play devil's advocate here: these results are predicated on the classification scheme, which favors rimed particles over aggregates and crystals for increasing ZH. Doesn't this just imply that events with larger ZH have larger snow accumulation rates?

10. Page 10, line 7: What do you mean by "vertical column"? Over the JFJ site? It was not clear to me when reading.

11. Figures 5-6: Indicate in the captions what is depicted by the error bars (5%-95%?).

12. Page 11, lines 18-19 and Fig. 5a: Careful with assuming differences in spectral width at vertical incidence are due to turbulence. The largest contributing factor is differences in particle vertical motions, which would also arise from a dispersion of fall speeds in addition to turbulent vertical motions.

13. Figure 5: Some of these differences between "previous" and "rimed" phases discussed in the text are very subtle. Are they statistically significant? Some of them, at least, are probably not.

14. Fig 5b: The vertical velocities are not changing much between the "previous" and "rimed" phases – in fact, the fall velocities look very much like one would expect in aggregates. Is it possible that these events are simply heavier aggregation events?

15. Page 13, lines 20-22: Why not aggregation? Aggregation of crystals could provide the same type of changes in the radar variables.

16. Page 14, lines 1-5: Exactly! These large aggregates would also dominate the signal in ZH.

ACPD
15, C5950–C5955, 2015

17. Page 14, lines 7-24: This is a very nice discussion of the various hypotheses presented in the literature. For completeness, you should include Schrom et al. (2015). These authors were able to simulate (using more complex scattering calculations) distributions of planar crystals and aggregates that matched ZH, ZDR, and KDP without invoking riming or secondary ice production.

18. Page 14, line 25: Also, by your arguments, the increased density of anisotropic particles should cause ZDR should increase for riming as well, all else being equal. However, riming will cause particles to become less anisotropic, right?

19. Page 15, line 10: How can you dismiss the possibility that these particles are aggregates?

20. Fig. 8: EV3 also clearly shows some needles/columns, which may be a sign of secondary ice production?

21. Sentence spanning pages 15-16: Are these images taken from within the observed KDP enhancement? The arguments put forth in previous papers (e.g. Kennedy and Rutledge 2011) indicate that dendrites producing the KDP signature would very quickly aggregate; thus, unless truly "in-situ" within the signature, one may not expect to see pristine habits.

22. Page 16, lines 4-9: So if KDP is enhanced because of riming, why does KDP and ZDR decrease again? It seems as though you are describing a scenario in which riming initially enhances density (but minimally affects aspect ratio), and then ultimately further riming continues to enhance density but causes the particles to become less nonspherical. This should be explicitly stated in this discussion. Otherwise, some readers may be confused as to why riming contributes to both increases and decreases in ZDR/KDP.

23. Page 16, lines 20-22: This result was also described in a number of previous papers (Kennedy and Rutledge 2011; Bechini et al. 2013; Schrom et al. 2015).

24. Page 18, lines 14-15: Technically, the cold front doesn't produce snow accumulations; rather, it may initiate storms that produce heavy snow.

25. Figure 13b: recommend reducing the range of the color scale for vertical incidence Doppler velocity to bring out the updrafts (i.e., maybe -3 to 3 m/s is better).

26. Figure 13c and text: The spectral width maxima is at roughly 3.5 km between 21-23 UTC, whereas the shift in sign of Doppler velocity is above 4.0 km for this period. Wouldn't you expect the turbulent mixing to be across the shear interface, not more than 0.5 km below? Your description of mountain-induced turbulence later (page 19, lines 25-27) is more appropriate, in my opinion. Perhaps you can mention this first here? Also, why not use the RHI scan measurements of spectral width, which will be enhanced due to the vertical wind shear (more so than the vertical incidence scan, which can have a large contribution from the dispersion of fall speeds).

27. Figure 14: The KDP enhancement is interesting and different from those previously reported – this enhancement is over a very deep layer within higher Zh. Typically, it is confined to some smaller layer near -15 deg C. Is it possible to overlay temperature contours on these time-height plots?

28. Page 20, lines 8-9: But again, the Zdr enhancement is located a full km above the layer with enhanced spectrum width values. How do you reconcile this discrepancy?

29. Page 20, line 14: use of "parameter" is incorrect here. Use "variable" or "measurement" or something similar.

30. Page 21, line 20: specify "radar observations classified as riming", because these results hinge on the validity of the classification scheme used.

31. Depositional growth is not emphasized until the end and in the schematic. But, small crystals falling into a region of SLW can definitely grow rapidly via the WBF mechanism, in addition to riming (which the authors emphasize).

32. Page 21, lines 23-24: The WBF process should be mentioned here, too, as it could

also rapidly deplete SLW.

33. Page 22, lines 7-9: This result is consistent with the hypothesis of Andric et al. (2013), which should be acknowledged here.

34. Page 22, lines 10-11: Again, this result is consistent with those previously published. This should be acknowledged here.

35. Page 22, line 15: Pristine columns/needles are seen in Fig. 8 from EV3.

36. Page 22, last paragraph: Lidar observations would certainly help identify the regions of SLW.

Technical comments:

1. Page 3, line 2: Not sure quotations are needed around "from the cloud to the ground".

2. Page 3, line 9: "leads a net increase of the mass of precipitation" can be revised to read "leads to a net increase of the precipitation mass."

3. Page 3, line 17: "ice crystal" should be "the ice crystal" or "ice crystals".

4. Page 10, line 15: "global" is probably a misleading term here. Perhaps "bulk" or something similar is better.

5. Figure 10: Your caption/labels appear reversed (spectral width is panel a, velocity is panel b).

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ACPD
15, C5950–C5955, 2015