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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

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The paper presented by Grazioli et al. analyzes the microphysics of riming and its impact on snowfall accumulation on the ground using a combination of polarimetric radar and in-situ observations. The combined measurements reveal that continuous riming - which enhances the snow mass accumulation on the ground - is often connected to turbulent layers which are likely to generate the needed supply of supercooled liquid water.

Overall, I think the findings and analyses in the paper are an important contribution to our understanding of the complex processes of riming on snowfall and therefore, the manuscript should be published in ACP. However, the text itself and also some of the argumentation needs some revision before acceptance.

General Comments:

1) I find your derivation of turbulence (Section 3.2.2) based on the vertically pointing radar data not very clear and also not very convincing. First, I suggest to better explain which variable you use to derive a measure of turbulence. Often either a FFT of a vertical Doppler velocity time series is used where one can fit the -5/3 slope. Alternatively, the turbulence component can be derived from the spectrum width (e.g. Doviak&Zrnic radar text book). However, both methods are not reliable in case of precipitation. My concern is that the spectrum can be even bi-modal due to the rimed mode (e.g. Zawadzki et al., AR, 2001) and hence using the spectra to derive a measure for turbulence, i.e. disentangle the contribution from the particles and due to turbulence appears to me very problematic. Although I don't see that you try to derive a quantitative measure of turbulence, this part has to be more precise and you should better explain which observables you are using for your argumentation. To me, the best parameter to argue about turbulent motions seems actually to be your sonic anemometer data. You should also be more precise when you write "wind speed" whether you refer to the vertical or horizontal component (for example P. 18076, L. 17).

2) In the discussion part, I am missing discussion of potential orographic lifting which will also favor generation of SLW. Although the RHIs clearly show the wind shear as a potential source for turbulence, I'm surprised that the aspect of orographic lifting is not discussed more deeply. In Figure 13 where one can clearly see the wind shear in the RHIs, I can't understand why the zone of maximum spectrum width and vertical Doppler velocity variability (ca. 3.0-3.5 km) which you assign to turbulence is located below the zone of maximum shear (ca. 4km). This should be explained more carefully since it is related to the main findings of the paper. This is also inconsistent with your nice schematic (Fig. 15) where the turbulence is drawn to be between the two shearing horizontal wind arrows unlike one finds it in Fig. 13. Maybe you could even try to make a fourth panel in Fig. 13 which shows the vertical gradient of the RHI Doppler velocity and hence the strength of wind shear directly. I was also not sure what the velocity

Fig. 13 c) exactly means: Is it the radial velocity which is plotted or only the horizontal wind component? If the second is true, how did you estimate the vertical component? Using the intermediate zenith pointing observations? There is actually, also a lower-level shear zone between 3 and 3.5km visible in Fig. 13c at the end of the period. Is this shear zone also producing enhanced turbulence? The many white vertical stripes make it hard to see the details in the plot; if possible, it would be good to find a better plotting solution (e.g. maybe black colour for missing values).

3) The co-authors who are English native speakers should more carefully go through the paper and help to improve English style and punctuation. I listed many typos which I found but not all since in my opinion this is not the main job of the reviewers.

4) How much do the results depend on the riming classification algorithm? Did you investigate the sensitivity of the results to the classification method used? I think such an analysis would strengthen the reliability of the results a lot!

Specific Comments:

I found it very difficult to follow the discussion and to keep in mind which event is now a core or edge event and to always look back and forth between the text and manuscript. You could make it much easier for the reader to follow, if you would for example group events into core events (CE1,2,3) and edge events (EE1,2,3) in the plots and also give them a different acronym? That would make it much easier to follow and to find the cases you refer in the text and in the plots.

Please provide more information about the snow accumulation measurements especially whether they are established with a wind fence; blowing snow can dramatically change snow accumulation, so how did you account for this error?

P. 18068, L. 18-19: Maybe it's not so clear what you mean with "over vertical columns of snowfall". I assume you mean that they analyzed microphysical processes within a vertical column e.g. probed with vertically pointing radars? Maybe rephrase into

something like "dominant microphysical snowfall process within the vertical column".

P. 18072, L. 12: "riming leads to smoother shapes" Isn't the more important change the change in cross sectional area perpendicular to the fall direction? To my knowledge, riming increases the mass of the particle but doesn't change its size and cross sectional area much. As a result, the particle terminal velocity increases. Of course, riming also changes the overall shape but I think mass increase and cross sectional area are the main components governing the change in terminal velocity.

P. 18095, Fig. 2: In the caption you explain that the blue lines relate to riming classifications from Mitchell et al., 2009 but in the text (P. 18072, L. 18) you mention that the riming degrees where derived on the definitions in Mosimann et al., 1994. This might be confusing so please explain whether both definitions can be converted into each other.

P. 18074, L. 7: "whole vertical column" Is this really the entire vertical column? I don't know how thick the cloud systems were but I would suspect that they reached up to several km where the temperature might be far too low (e.g. below -35 degC) to allow any riming taking place.

P. 18074, L. 24-27: "upper edge of the vertical column" is not well phrased. I think you mean something like that the height of the JFJ coincided to the altitudes with maximum percentage of rimed precipitation. And hence the in-situ observations were able to capture the rimed precipitation particles, right? I would maybe simply denote the "riming cores" as "main riming region".

P. 18074, L. 28: I suggest shortening the beginning of the sentence into something like "In the following, we analyze the characteristics of the described cases by means of . . .". Right now the text sounds in several parts more like if the reader would listen to a presentation (also P. 18081, L 3). Again, something where the native speaking co-authors could help.

P. 18075, L. 17-18: “behave similar in terms of evolution of wind and turbulence”. First, what do you mean exactly with this sentence? Evolution of horizontal or vertical wind component? Where do I find information about turbulence in Fig. 5? Or do you derive turbulence from spectrum width? If yes, how do you distinguish between the PSD and turbulence component? You certainly can’t neglect the PSD term since we are dealing with riming which often shows even a bimodal Doppler spectrum. Is the spectrum width derived from the entire spectrum or just from the principal i.e. strongest peak? Please explain better. Secondly, I can’t see so much similarity in EV6, 7 in Fig. 5 except maybe for spectral width and Doppler velocity. For hor./vert. wind speed for example, EV5 and EV7 look much more similar than EV 6 and 7. See also my general comment related to the topic of turbulence. Figure 7: Very hard to read the legends even if I zoomed in my pdf. I suggest increasing font size and thickness.

P. 18077, L. 12ff: Low and high are quite relative expressions. At W-band a reflectivity between +5 and +10 dBZ would be actually quite high. I suggest to mention some numbers or ranges like you do it a few lines below for rho-hv.

P. 18077, L. 20-22: Couldn’t it be also quite realistic that riming and aggregation take place and lead together to this increase in Z? Or can you exclude a significant contribution of aggregation?

P. 18078, L. 22: If I remember correctly, Hallett and Mossop process is very much limited to the -5 degC region. Is it really realistic to assume this process to be relevant at -15 degC? I think later in the text you discuss it but here it sounds like a reasonable explanation and you should maybe exclude it here already because of the wrong temperature regime.

P. 18079, L. 14ff: If you can derive mass and size of the particles from your in-situ data, why don’t you derive the mass-size relations directly and compare it with literature values about m(D) depending on particle habit and degree of riming? That seems to me much more clear than arguing with particle masses which do not mean much

without information about their related sizes or volumes. I suggest to improve this part accordingly and to really extract this important information that you have in your data.

Figure 8 and discussion: I can clearly see needles in the EV3 and EV6 case. I am surprised that you do not discuss that in the light of expected ice splintering/secondary ice processes. Where do you get your temperature information from for the -15 degC level in Fig. 7? Again the entire discussion of the in-situ data is too qualitatively in my opinion. Why aren’t you deriving for example, the aspect ratios of the particles as function of their size? Quantitative analysis of these probe data has already been done in numerous previous studies where these in-situ probes have been flown on aircrafts. Such data are quite important e.g. for future radar simulation studies of riming; particularly, because observations of riming are in general quite rare. I can only encourage the authors to provide these more quantitative information here or at least as supplemental material.

P. 18080, L. 4: Also relating to the interesting different gradient of Z above and below the KDP peak, I suggest to provide the reader with concrete numbers, for example: The reflectivity gradient above the KDP peak ranges between. . .. dBZ/km while below it is reduced to . . .. dBZ/km. This would also allow to better compare it with Z-gradients derived in former studies.

P. 18081, L. 14: I can’t find EV8 in Fig. 10.

Figure 10: a) and b) in the caption is reversed compared to the figure legend.

Figure 13 b: In Fig. 10 the largest Doppler velocities range between -3 and 1 m/s. For better visibility, you should adjust the colorbar to the same range.

Figure 14: I think you could improve your color scaling here as well. Some of the maximum values for Zdr and Kdp only appear on the highest edges of the cloud where I suspect the SNR to be quite low already and hence the values are not quite reliable. Actually, I guess that you would like to emphasize the Zdr above the high reflectiv-

ity layer, so reducing the maximum value of the colour scale should bring out these features much more clearly, I suspect.

Conclusion section: I agree that a combination of polarimetry and in-situ is of course important and you showed how many interesting aspects can be found. But one should not forget also to extend combination of different remote sensors for future studies (polarimetric, Doppler spectra, multi-frequency, lidar, MWR). In my opinion, particularly lidar and microwave radiometers could add important new information about the distribution and quantification of SLW.

Typos: P. 18067, L. 9: "leads to"

P. 18067, L. 27: remove comma after "that"

P. 18092, Table 1: Leave "UTC" after "MM-DD HH" (third column)

P. 18072, L. 10: Add comma after "Firstly"; check if "entangle" is the correct verb here or whether you rather mean something like "remove" or "capture".

P. 18072, L. 16: Remove "that" after Mitchell et al.

P. 18072, L. 19: Why do you need parenthesis here?

P. 18072, L. 21: I suggest changing it into something like: "This can be observed in Fig. 2 which has been derived from their results."

P. 18073, L. 1: Add comma after "In this case"

P. 18073, L. 8: Add comma before "respectively"

P. 18073, L.25-26: Add comma after "In the previous section" and before and after "however".

P. 18074, L. 17: "Panel (a)" or "Upper panel"; similar for Panel b later on. Also maybe simplify the phrase "Panel b is used to show" into "Panel (b) shows" or "Panel b is intended to"

P. 18097, Fig. 4: Accumulation intensity in the legend and Mean snowfall accumulation rate in the caption. Please unify. Also maybe "time period" instead of time step? Typo: "Timesteps" at the end of the caption.

P. 18075, L. 11: Add comma after "Therefore"

P. 18075, L.13: Wrong Fig. reference, probably you mean Fig. 6a.

P. 18076, L. 14: magnitudes of the variables

P. 18076, L. 25: "was lasting about 9h, was about three. . ."

Caption Figure 10: "angles" seems to be redundant here

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