acp-2015-416

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Responses to the reviewers

With the present supplementary document we provide our responses to the comments of the two anonymous reviewers of the manuscript *acp-2015-416*, now entitled "Microphysics of winter alpine snowfall: polarimetric radar and in-situ observations during CLACE 2014".

The comments of the reviewers are reported in *italic* font. Quotations of the manuscript in its revised or in its original form are reported in blue . The figures used to support our answers to the reviewers can be found at the end of this document.

Anonymous reviewer 1

We would like to start by thanking the reviewer for his/her valuable contribution, for the many useful suggestions and corrections and for his/her constructive comments that lead, in our opinion, to an improvement of the quality of the manuscript. A recurring and probably the major question of the reviewer was about the duality of the microphysical processes of riming and aggregation. The revised version of the manuscript, as illustrated point-by-point in the following section, takes into account the concerns of the reviewer: (i) we tried to clarify when "rimed precipitation" refers to the output of a classification algorithm or to other considerations, (ii) we stressed the potential role of aggregation to explain the observed polarimetric signatures, and (iii) we underlined the synergy between dendrification, aggregation, and riming and not only their contrasts.

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 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).

   ANSWER:
   We agree with the reviewer about the need to clearly state when the term "rimed precipitation" comes from the output of classification methods. In this way the reader will be able to interpret the results of this work even when and if additional knowledge about hydrometeor classification from polarimetric radar data will be available in the literature.

   Different classification methods set different boundaries between hydrometeor classes, some classes may exist in one method but may not be covered by another one, and so on. Following the suggestion of the reviewer we compared the results of the classification method GTB2015 (Grazioli et al., 2015) employed in the manuscript with another method based on fuzzy logic: DR2009 (Dolan and Rutledge, 2009) and the results are similar. DR2009 does not provide a "rimed hydrometeors" class and we obtained it by merging the low density graupel (LDG) and the high density graupel (HDG) classes. As an example, Figure 1 in this document is the equivalent of Fig. 3 of the manuscript, but obtained by means of DR2009. Even though the correlation is lower, the qualitative message remains.

   CHANGES:
   It is clearly stated throughout the manuscript that the term "rimed precipitation" refers to the output of an hydrometer classification method and that hydrometeor classification methods have limitations. Furthermore, when appropriate, we refer to "snowfall microphysics" instead of "riming". Several sentences have been rephrased.

   In the title of the manuscript:
   Microphysics of winter alpine snowfall: polarimetric radar and in-situ observations during CLACE 2014

   In the abstract:
   Radar-based hydrometeor classification suggests that riming is an important factor to favour an efficient growth of the precipitating mass and correlates with snow accumulation rates at the ground level.

In the introduction (end of the section):
The combination of numerical modelling, radar observations and in-situ data led to the development of classification methods aiming to estimate the dominant type of hydrometeors that populate the radar resolution volumes (e.g. Straka et al., 2000; Dolan and Rutledge, 2009; Bechini and Chandrasekar, 2015; Grazioli et al., 2015).However, the complex microphysics of ice-phase precipitation cannot be fully captured by polarimetric radars alone nor simply described by a single hydrometeor type.

At the end of Section 3.1:
Similar trends were observed when a different hydrometeor classification method was employed (i.e., the method of Dolan and Rutledge, 2009, not shown here). The relation between riming and snowfall intensity was also qualitatively observed by Schneebeli et al. (2013) in a different location in the Swiss Alps. It must be noted that this results rely on radar-based hydrometeor classification (i.e., indirect measurements), and also that riming does not exclude the occurrence of other microphysical processes, notably aggregation.

In the conclusions (beginning of the section):
It was illustrated, thanks to the comparison between radar-based hydrometeor classification (i.e. indirect measurements) and actual measurements of snow accumulation, how riming correlates with snow accumulation rates. Most of the time, radar observations of precipitation classified as rimed…

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.

   ANSWER+CHANGES:
   We agree with the reviewer that this sentence was technically incorrect. We shortened and rephrased it as follows:
   Riming leads to a net increase of the mass of precipitating snowflakes (ice-phase) at the expense of the cloud liquid droplets. Aggregation contributes indirectly to this mass transfer by generating larger and faster targets for riming (Houze and Medina, 2005, hereafter HM2005).

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).

   ANSWER+CHANGES:
   We thank the reviewer for spotting the work of Schrom et al. (2015). This recent manuscript has been useful in the review process of our paper. It is mentioned in the introduction and other times throughout the discussion of the revised manuscript.

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

   ANSWER:
   The acronym means World Geodetic System 1984, and it indicates the reference system of the coordinates. WGS84 is the coordinate system used by GPS.

   CHANGES:
   The end of the caption now reads:
   The coordinates shown on the map are expressed in the World Geodesic System 1984 (WGS84) reference.

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.

   ANSWER+CHANGES:
   We thank the reviewer for spotting this problem. We investigated it and the issue was actually twofold: the colorbar was faulty and the conversion from Latitude and Longitude positions in degrees to a map

projection was not accurate enough. Both aspects have been solved. Additionally, Figure 1 is now displayed with a different color scale, more commonly used for digital elevation models.

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.*

   ANSWER+CHANGES:
   We revised the manuscript by limiting the use of expressions like "turbulent, turbulence, turbulent mechanism", when they could originate confusion. In this case the expression "turbulent mechanism" has been replaced by collisional mechanism.

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.

   ANSWER+CHANGES:
   The initial choice to display total net accumulation was initially driven by the fact that this parameter can be more unambiguously defined than the duration itself. However, the reviewer is giving here a good suggestion, that we address in the following point that is about the same topic. Please note that this figure has been removed from the revised manuscript, as the variable plotted on the X-axis was qualitative (riming degree) and was generating confusion.

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 s how that the correlation between event duration and total accumulation is largest (R^2 0.72), much larger than the correlation between PRP and accumulation shown (R^2 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 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.

   ANSWER:
   The reviewer is right when he/she realizes that duration is the most important variable, in our dataset, to explain total accumulation, at least at the scale of the entire precipitation events. We thank the reviewer for his/her suggestion to display rates instead of total accumulations. Now we show on the image and discuss in the text the average accumulation rates and not the total accumulations.

   CHANGES:
   Fig 3 (now Fig. 2) displays mean accumulation rates instead of total accumulations. The discussion in the text is now oriented to mean accumulation rates and thus the effect of duration is taken into account.

   In the middle of section 3.1 note for example the following sentence:
   It is worth noting that on average accumulation rate scales well with PRP. In particular, the events characterized by near-zero PRP are also associated with near-zero accumulation rates (e.g. events 1, 2, 11, 8), and all the events showing non negligible accumulation rates have also proportionally higher PRP.

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 Z_H. Doesn't this just imply that events with larger Z_H have larger snow accumulation rates?*

   ANSWER:
   According to the literature on the subject it is a known fact that snowfall intensity and Z_H are related. Many monotonically increasing relations between those variables have been proposed (well summarized for example in Table. 3 and Fig. 4 of Scipion et al., 2013). The reviewer is right on this aspect. It is also true that the classification methods of Dolan and Rutledge (2009) or (Grazioli et al., 2015), on average

have higher Z_H for rimed hydrometeor classes. However, this is not the only signature: on average they show also higher K_{dp} and a wider range of variation of Z_{DR}. K_{dp} in particular is an important parameter in the classification schemes because values that significantly departs from $0° \mathrm{km}^{-1}$ are rare in snowfall.

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

ANSWER+CHANGES:
In this context, "PRP calculated over the whole vertical column" means that in Eq. 1 h_2 goes to infinity, i.e., there is no upper boundary set. This sentence was unclear for both reviewers and it is therefore rephrased as follows:
…(ii) average PRP above 50% (with $h_1 = 2250\,\mathrm{m}$ and $h_2 = \infty$ in Eq. 1.)

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

ANSWER+CHANGES:
The caption now includes the following sentence:
The errorbars highlight the location of the 5% and 95% quantiles, respectively.

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.

ANSWER+CHANGES:
We agree with the reviewer about this point, some of the sentences of this section might be misinterpreted. We rephrased carefully this part, as follows. In the beginning:
The "core" events are of major interest for the microphysical descriptions presented in the present paper. $\mathrm{EV6}_C$ and $\mathrm{EV7}_C$ show similar qualitative trends in the transition between the preceding phase and the rimed phase. Horizontal wind speed, vertical wind speed, Doppler spectral width, and the variability (interquantile range) of the mean Doppler velocity are lower during the rimed phases (blue histograms) than during the respective preceding phases (red histograms) as shown in Fig. 4.

Later, towards the end of the paragraph:
It can be hypothesized that the initially active conditions of wind speed, variability, and updraft create the appropriate environment to generate rimed precipitation, that falls out efficiently during the following calmer part. Motion variability, wind gusts, updraft and turbulence are in fact important factors leading to riming by providing SLW droplets and conditions that favour collision (Rauber and Tokay, 1991; Pinsky and Khain, 1998; Houze and Medina, 2005).

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.

ANSWER+CHANGES:
The reviewer is right: not all the differences are strictly statistically significant. However, the discussion about this figure is qualitative, based on the trends (increase/decrease) of the mean values. In fact, while the rimed phase is defined by a set of conditions (see Sec. 3.1), the preceding phase is set to a fixed time span (during which precipitation is present). The text was carefully checked and we made sure that no statistical claims are associated to this image.

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?

ANSWER:
As we discuss in the revised Sec. 3.3 and 4., aggregation is probably playing a role during these events. In this case we do not fully agree with the reviewer about the mean Doppler velocities. At first, while the mean Doppler velocities may not differ a lot, the velocity spectrum is very different (if we consider together panels b and d of Fig. 5). Secondly, it is true that the values shown in the image can be associated to aggregates (e.g. Brandes et al., 2008), but also to rimed hydrometeors (Garrett and Yuter, 2014).

CHANGES:
The following sentence has been rephrased in the middle of the subsection named "Core events":
The mean Doppler velocity (panel b), influenced by particle fall velocity and air motion, shows positive values (meaning that the updraft was strong enough to lift the hydrometeors) and a larger variability during the preceding phase around mean values ranging between -2.3 and -0.5 ms^{-1}, as to be expected both for aggregates and/or rimed hydrometeors (Brandes et al., 2008; Garrett and Yuter, 2014).

In the end of the same paragraph the following sentence has been added:
Aggregation of individual ice crystals is also favoured by the variability and enhancement of particle velocities, and in turn aggregates provide larger SLW collection areas (Houze and Medina, 2005).

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

ANSWER+CHANGES:
The sentence mentioned by the reviewer has been removed. We believe that it was providing the misleading information that the increase in Z_H is only due to riming.

The role of aggregation on the radar variables is highlighted in the beginning of the section:
In fact, Z_{DR} is largely influenced by the geometry of the particle that contribute the most to the Z_H signal (i.e. the biggest ones, Hubbert et al., 2014) such that the presence of even a few larger isotropic aggregates significantly decreases Z_{DR}.

Additionally, in the middle of the section:
The conditions favouring the collision of ice crystals and liquid water droplets during riming favour also the ice-to-ice interaction and aggregation can be initiated. The newly formed aggregates will contribute to enhance Z_H while keeping Z_{DR} relatively low.

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

ANSWER+CHANGES:
We believe that our response to the previous point also addressed this comment.

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 Z_H, Z_{DR}, and K_{dp} without invoking riming or secondary ice production

ANSWER+CHANGES:
It is appropriate to mention the computational results of Schrom et al. (2015) in this section. We added the following sentence to the discussion:
Recent research (Schrom et al., 2015) suggested, by means of particle size distribution retrievals based on scattering simulations, that a population of unrimed hydrometeors uniquely composed of aggregates and dendritic crystals may be able to generate the observed K_{dp} enhancement.

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

ANSWER:
The reviewer is definitely right. In our interpretation, the conditions leading to riming (thus to collision of liquid droplets and ice crystals) are also favourable for ice-to-ice collision , thus potentially for aggregation. The creation of aggregates would not decrease too much the K_{dp} signal unless all the anisotropic crystals get aggregated but it will definitely decrease Z_{DR} overcoming the density effect mentioned by the reviewer.

CHANGES:
The following sentence was introduced in the middle of section 3.3.2:
The conditions favouring the collision of ice crystals and liquid water droplets during riming favour also the ice-to-ice interaction and aggregation can be initiated. The newly formed aggregates will contribute to enhance Z_H while keeping Z_{DR} relatively low.

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

ANSWER+CHANGES:
In fact we do not dismiss this possibility, we are only observing that the outer shapes of those hydrometeors (in the limited size range sampled by the in-situ instruments) are smooth and therefore they seem rimed. The sentence has been rephrased as:
We observe during EV6 and EV7 the presence of many particles with smooth shapes, interpreted as heavily rimed hydrometeors. Some of these hydrometeors do not have recognizable original shape while some seems to probably originate from planar crystals.

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

ANSWER:
We thank the reviewer for the suggestion. The presence of those habits may suggest HM ice multiplication while the temperature range is unusual, thus other explanations were proposed.

CHANGES:
In the section mentioned by the reviewer we only describe the measurements without discussing in depth about the microphysical explanations. Later, in the middle of Sec. 4.2, we added the following sentence:

The enhancement of K_{dp} happens in our case at temperatures between -13 and -16°C and although we observe the presence of columns and needles (as shown in Fig. 7), often produced when HM is active, this leads us to assume that other ice production mechanisms may be taking place.

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

ANSWER:
The image are unfortunately not always directly within the observed K_{dp} enhancement and therefore we cannot exclude the hypothesis proposed by the reviewer. However, the inspection of hundreds of images even during the best matched cases never revealed the presence of pristine dendrites during the timesteps showing K_{dp} enhancement. As mentioned in the manuscript, rimed planar crystal that indeed may be rimed dendrites or plates were instead often observed. In this context we agree with the hypothesis of Bechini et al. (2013), that associated the K_{dp} enhancement to the fact that dendrites will efficiently collect SLW without changing excessively their aspect ratio.

22. *Page 16, lines 4-9:*
So if K_{dp} is enhanced because of riming, why does K_{dp} and Z_{DR} 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 Z_{DR}/K_{dp}.

ANSWER+CHANGES:
The reviewer is right. What we meant here was that aggregation is probably the best mechanism to explain the signatures at this height levels, below the areas where riming initiates. This brief section (3.3.3) has been largely rephrased in this way:
Aggregation is a mechanism that explains this trend (e.g. Kumjian et al., 2014). If aggregation is dominant, individual oblate crystals will be merged together into more spherical-like aggregates at a faster rate than they are produced. The presence of larger isotropic hydrometeors leads Z_H to increase and Z_{DR} to decrease, while the decrease of ice particle concentration and the consumption of oblate hydrometeors lead K_{dp} to decrease. Further riming, that may occur together with aggregation, can contribute to similar signatures only if the anisotropy of the hydrometeors is significantly reduced.

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).

ANSWER +CHANGES:
Those works are properly acknowledged and cited at the end of the section.

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

ANSWER:
We agree with the comment of the reviewer.
CHANGES:
The sentence has been rephrased as:
The snowfall event associated with the cold front produces a significant...

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).

ANSWER+CHANGES :
The color range has been reduced according to the suggestion of the reviewer. Please note also the revised and new panels of this figure.

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).

ANSWER:
We believe that the problem with Fig.13 in the original manuscript was that the third panel, showing the summary of Doppler velocities from RHI data, was very difficult to interpret and was hiding the complex dynamics that can only be seen by looking at individual RHIs.

When two air masses with different relative motion are in contact (and the shear, in our opinion does not contradict the orographic effect but can be the result of the air trapped in the valley) we would expect the shear layer to be in fact slightly above the area of turbulent mixing. This is the case and the apparent mismatch observed in the figure is resulting from the fact that the Doppler spectral width was shown from vertically-pointing profiles while the shear observed in the third panel that was showing spatially averaged data (and therefore the spatial variability of the altitude of the turbulent mixing was not visible and was not directly comparable with the data coming from vertical profiles).

CHANGES:
Fig.13 (now Fig 12) has been edited. The third panel has been removed and two new panels, showing RHI data of Doppler velocity and Doppler spectral width around 2215 UTC have been added. Those panels illustrate, even though in a single timestep, the stratification of the atmosphere during EV3.

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

ANSWER:
Unfortunately the temperature was available only as a point-measurement and we do not have enough confidence to extrapolate this information into a contour plot. However, as mentioned by the reviewer, the cores of K_{dp} enhancement are often around the estimated -15° level (estimated from point measurements taken at the JFJ location and -6.5°km^{-1} lapse). This can be seen in Fig. 7 of the manuscript, where the estimated -15°C level is plotted as a blue horizontal line.

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

ANSWER:
We believe that this is now clarified. The revised Fig.12 shows that the mixing layer is situated at higher altitudes further away with respect to the radar location.

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

ANSWER+CHANGES:
The term variable is now used.

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

ANSWER+CHANGES:
The sentence has been rephrased as follows:
It was illustrated, thanks to the comparison between radar-based hydrometeor classification (i.e. indirect measurements) and actual measurements of snow accumulation, how riming correlates with snow accumulation rates.

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).*

ANSWER:
Depositional growth was not emphasized because this paper was focusing on "active" mixed-phase clouds processes (updrafts, turbulence) that would favour riming with respect to vapour deposition. The measurements of enhanced crystal growth in calmer meteorological condition in ice super-saturation may be the topic of a separate manuscript. In this manuscript we tried to identify case-by-case the dominant microphysical mechanism among the many that can take place at the same time.

CHANGES:
We mention now earlier in the manuscript the depositional mechanism, at the beginning of section 3.3.2:

The enhancement of Z_{DR} is often explained by the presence of ice crystals grown by vapour deposition, that promotes anisotropic shape enhancement (Takahashi, 2014; Andric et al., 2013) and that can be particularly efficient if SLW is present and the Wegener-Bergeron-Findeisen (WBF) process takes place (e.g. Pruppacher and Klett, 1997).

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

    ANSWER+CHANGES:
    We agree with the reviewer and the sentence is rephrased as follows:
    precipitation classified as rimed were following time periods of updrafts, variability of wind and particle motions and availability of SLW. When rimed precipitation was most intense, instead, the conditions were usually quieter (thus favouring the precipitation) and SLW was depleted in the cores of rimed precipitation, being probably collected mostly in the form of rimed accretion on the precipitating ice crystals and also contributing to crystal growth in terms of depositional water vapour transfer on the ice-phase at the expenses of SLW droplets (WBF process).

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

    ANSWER+CHANGES:
    We prefer not to include direct citation in the conclusion (however we acknowledge the role of these works in the revised precious sections). We propose the following rephrasing for the conclusions:
    A common feature of these cases was shown to be a peak of K_{dp}, observed and documented in other published research, associated either with relatively…

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

    ANSWER+CHANGES:
    The sentence has been rephrased as follows:
    The enhancement of K_{dp} has been shown to be related to the maximum Z_H measured in the vertical column of precipitation, as observed in previous research.

    Also, please note the end of section 3.3.4:
    he peak of K_{dp} might therefore be considered as an indication of high Z_H values at lower levels as similar relations have been documented in other locations in Europe or North America (Kennedy and Rutledge, 2011; Bechini et al., 2013; Schrom et al., 2015).

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

    ANSWER:
    Correct! We were referring only to dendritic and not all crystal habits. The sentence has been rephrased as:
    did not show any evident or dominant pristine and unrimed dendritic habit in these cases.

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

    ANSWER:
    A wind lidar was deployed close to the X-band radar during CLACE 2014 and it was indeed very helpful to identify SLW layers during precipitation. Unfortunately this manuscript was devoted to rather intense snowfall events during which the lidar signal was completely attenuated.

Technical comments

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

   ANSWER+CHANGES:
   The quotations have been removed.

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."*

   ANSWER:
   We agree with the reviewer and we make use of his/her suggestion verbatim.

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

   ANSWER+CHANGES:
   We changed to ice crystals, in consistency with the rest of the manuscript.

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

   ANSWER+CHANGES:
   This part of the sentence has been rephrased as:
   ... summarizes the bulk characteristics ...

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

   ANSWER+CHANGES:
   The mistake has been corrected.

Anonymous reviewer 2

We would like to thank the reviewer for his/her helpful comments and suggestions that are addressed point-by-point in this section.

Introduction

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).*

   ANSWER+CHANGES:
   We agree with the reviewer on some aspects of his/her comment. In the original manuscript the word "turbulence" was probably over used while it is appropriate mainly for EV3 (the event with the shear layer stable in time) and EV4. As a first change, the revised version of the manuscript makes use of terms like "wind gusts", "updraft", "wind variability", "particle motion variability" instead of "turbulence" when appropriate.

   As the reviewer said, we do not try to provide a quantification of turbulence (defined for example as the coefficient of variation of wind velocities in sonic anemometer data) and in Section 3.2.2 we want to highlight the cases where the motion of the hydrometeors is influenced by the wind, departs from free-fall and is subject to updrafts. Many sentences of this section have been rephrased and the misuse of the word "turbulence" should be solved. The beginning of Sec. 3.2.2 has been rephrased as:

   ans$EV6_C$ and $EV7_C$ show similar qualitative trends in the transition between the preceding phase and the rimed phase. Horizontal wind speed, vertical wind speed, Doppler spectral width, and the variability (interquantile range) of the mean Doppler velocity are lower during the rimed phases (blue histograms) than during the respective preceding phases (red histograms) as shown in Fig. 4. Doppler spectral width, shown in panel (a), reduces of about 50%, as well as the horizontal (panel c) and vertical wind speed (panel d). The mean Doppler velocity (panel b), influenced by particle fall velocity and air motion, shows positive values (meaning that the updraft was strong enough to lift the hydrometeors) and a larger variability during the preceding phase around mean values ranging between -2.3 and -0.5 ms^{-1}, as to be expected both for aggregates and/or rimed hydrometeors (Brandes et al., 2008; Garrett and Yuter, 2014). It can be hypothesized that the initially active conditions of wind speed, variability, and updraft create the appropriate environment to generate rimed precipitation, that falls out efficiently during the following calmer part. Motion variability, wind gusts, updraft and turbulence are in fact important factors leading to riming by providing SLW droplets and conditions that favour collision (Rauber and Tokay, 1991; Pinsky and Khain, 1998; Houze and Medina, 2005). Aggregation of individual ice crystals is also favoured by the variability and enhancement of particle velocities, and in turn aggregates provide larger SLW collection areas (Houze and Medina, 2005).

And also later in the same section:

The trends of EV4$_C$ are similar to EV6$_C$ (except for the Doppler spectral width), even though the magnitudes of the variables is very different. Also in this case a significant decrease of updrafts occurrences and horizontal and vertical wind intensities (Fig. 4) can be observed, as well as a decrease of LWC (Fig. 5,a) in the transition between the preceding phase and rimed phase.

The term turbulence is used now in the manuscript when we discuss the vertical structure of EV3. With turbulence we mean variability in terms of 3D wind speed that is able to influence the free-fall behaviour of the hydrometeors. As an example, in the first half of Sec. 4.1 the following sentence has been rephrased:

EV3, finally, shows a peculiarity: the enhancement of spectral width in this case appeared to be confined, above the radar location, between approximately 3000 m and 4000 m. In this layer the Doppler spectral width reaches values up to 2.5 $\mathrm{m\,s^{-1}}$ and mean Doppler velocities are often positive (updrafts), while at altitudes below 3000 m the range of variation of velocities and spectral width is narrower. We will refer to this as “turbulent layer”, meaning that the variability of hydrometeor motions within this area is governed by wind velocity variability and gusts overcoming the free fall behaviour of particles. Additionally, the absence of multi-modal Doppler spectra (not shown here) led us to assume that the enhancement in spectral width is indeed mostly due to atmospheric turbulence and not to variability of particle fall velocities.

We believe that the reader, aware both of the mean Doppler velocity and the Doppler spectral width should be convinced that the turbulence seen during EV3 cannot be due to bimodalities of Doppler spectra only (this is a concern of the reviewer, if we understood correctly). We present in this document (Fig. 3 at the end of the document) for the reviewer an example of genuine Doppler spectrum collected at 2138 UTC the 1st February 2014, during EV3. It can be seen that the spectra do not show evident multi-modalities, they are very wide and often large portions of them are situated in areas of positive velocities (updrafts). Most of the spectra during the intense part of EV3 showed this characteristics.

We agree with the reviewer that it was needed to clarify when horizontal and vertical wind speed are mentioned. The title of Fig.5 c has been changed to “Wind speed, horizontal component”. This is also clarified throughout the manuscript.

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.

   ANSWER:
   We thank the reviewer for his/her comment, which made us think more deeply about the role of the orographic lifting in producing high concentrations of SLW. We think that in EV3 the south-westerly large-scale flow ascending over the Alps (see figure 11 of the manuscript) has an important role in producing high concentration of SLW above the height of the Alpine crest. This is a general large-scale feature of the atmospheric flow during EV3, which is relevant not only for the snowfall production mechanisms that are observed in the valley of our study, but for other Alpine regions as well, especially those located south of the main crest. Since our study area is located in the interior of the Alps, much closer to the northern edge of the mountainous chain rather than to the southerly slopes, for orographic lifting at the mesoscale to be effective in enhancing precipitation, the lower level winds should have been from the North or North-West. The soundings of Payerne, however (see Fig. 2 of this document), and local met stations data do not show the presence of such wind impacting over the Alps at the low levels, rather the wind was south-westerly at almost all altitudes during EV3. Thus, there was no real orographic lifting at the mesoscale on the northern slopes of the Alps, but, as mentioned, it was occurring only at the large scale over the entire Alpine ridge before the passage of the cold front.

CHANGES:
Section 4.1, mid-section:
The atmospheric sounding of Payerne (Lat. 46.82, Lon. 6.94) at 12 UTC, not shown here, indicates the presence of a strong south-westerly flow above 2 km, which was probably producing high concentrations of SLW due to large-scale orographic lifting over the Alps.

Section 4.2, end of section:
Above the layer (enhanced Z_{DR}) favourable conditions exist for anisotropic crystal growth thanks to the recirculation of SLW and ice fragments from the lower levels and to the supply of moisture (or SLW) provided by the large-scale south-westerly flow.

3. *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 lowerlevel 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?*

ANSWER:
We believe that the problem with Fig.13 in the original manuscript was that the third panel, showing the summary of Doppler velocities from RHI data, was very difficult to interpret and was hiding the complex dynamics that can only be seen by looking at individual RHIs.

When two different air masses are in contact we would expect the shear layer to be slightly above the area of turbulent mixing. This is the case and the apparent mismatch observed in the old version of the figure is resulting from the fact that the Doppler spectral width was shown from vertically-pointing profiles while the shear observed in the third panel that was showing spatially averaged data (and therefore the spatial variability of the altitude of the turbulent mixing was not visible and was not directly comparable with the data coming from vertical profiles).

CHANGES:
Fig.13 has been edited. The third panel has been removed and two new panels, showing RHI data of Doppler velocity and Doppler spectral width around 2215 UTC have been added. Those panels illustrate, even though in a single timestep, the complex stratification of the atmosphere during EV3. The schematics of Figure 15 has been modified and the arrows have been removed. The important concept of the scheme is in fact the stratification between different air masses that interacts through a turbulent layer.

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).

ANSWER:
We thank the reviewer for the suggestion but, for internal consistency, we prefer to keep the white color for missing data in our plots. The vertical white lines separating the data have a specific purpose: they remind to the reader that the radar was not continuously scanning nor continuously profiling but it was interleaving this two actions.

4. *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.*

ANSWER:
The manuscript was checked before submission by a native English speaker colleague and it will be

checked again before the submission of a revised version. We will do our best to improve the quality of the manuscript from this point of view.

5. *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!*

ANSWER:
We agree with the reviewer. There is the need to clearly state that the identification of rimed precipitation relies on a radar-based classification method and to mention the results that can be obtained with other algorithms.

Different classification methods set different boundaries between hydrometeor classes, some classes may exist in one method but may not be covered by another one, and so on. Following the suggestion of the reviewer we compared the results of the classification method GTB2015 (Grazioli et al., 2015) employed in the manuscript with another method based on fuzzy logic: DR2009 (Dolan and Rutledge, 2009) and the results are similar. DR2009 does not provide a "rimed hydrometeors" class and we obtained it by merging the low density graupel (LDG) and the high density graupel (HDG) classes. As an example, Figure 1 in this document is the equivalent of Fig. 3 of the original manuscript, but obtained by means of DR2009. Even though the correlation is lower, the qualitative message remains.

CHANGES:
It is clearly stated throughout the manuscript that the term "rimed precipitation" refers to the output of an hydrometer classification method and that hydrometeor classification methods have limitations. Furthermore, when appropriate, we refer to "snowfall microphysics" instead of "riming". Several sentences have been rephrased.

In the title of the manuscript:
Microphysics of winter alpine snowfall: polarimetric radar and in-situ observations during CLACE 2014

In the abstract:
Radar-based hydrometeor classification suggests that riming is an important factor to favour an efficient growth of the precipitating mass and correlates with snow accumulation rates at the ground level.

In the introduction (end of the section):
The combination of numerical modelling, radar observations and in-situ data led to the development of classification methods aiming to estimate the dominant type of hydrometeors that populate the radar resolution volumes (e.g. Straka et al., 2000; Dolan and Rutledge, 2009; Bechini and Chandrasekar, 2015; Grazioli et al., 2015).However, the complex microphysics of ice-phase precipitation cannot be fully captured by polarimetric radars alone nor simply described by a single hydrometeor type.

At the end of Section 3.1:
Similar trends were observed when a different hydrometeor classification method was employed (i.e., the method of Dolan and Rutledge, 2009, not shown here). The relation between riming and snowfall intensity was also qualitatively observed by Schneebeli et al. (2013) in a different location in the Swiss Alps. It must be noted that this results rely on radar-based hydrometeor classification (i.e., indirect measurements), and also that riming does not exclude the occurrence of other microphysical processes, notably aggregation.

In the conclusions (beginning of the section):
It was illustrated, thanks to the comparison between radar-based hydrometeor classification (i.e. indirect measurements) and actual measurements of snow accumulation, how riming correlates with snow accumulation rates. Most of the time, radar observations of precipitation classified as rimed…

Specific comments

1. *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.

ANSWER+CHANGES:
Following the suggestion of the reviewer we added the same "C" and "E" labels of Fig. 4 also to Fig. 5, 6, 11. Furthermore, in section 3 we added a suffix to the name of the snowfall events to discriminate them directly (i.e., $EV3_C$).

2. *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?*

   ANSWER:
   Snow height measurements are usually provided by automatic stations without a fence, but in carefully selected sites. We decided to use the data of the MAE station because the behaviour observed in other measurement stations nearby (especially the ones deployed at similar altitudes) was not different. We provide for the reviewer at the end of this document (Fig. 4) the snow height accumulations of four stations close to the measurement area and daily liquid water equivalent measurements taken at the KS site. The sites that are situated at the altitudes of interest (i.e. not too low) show very similar trends and high correlations (not shown) and thus we assume that the MAE site is a good reference.

   CHANGES:
   In section 2.1, subsection "MAE", the following sentence was rephrased:
   Among other gauging stations in this area, MAE is chosen as a reference because: (i) it provides relatively high temporal resolution data (30 minutes), (ii) it is among the closest to KS, (iii) it is located approximately at the altitude of the first radar resolution volume not affected by ground clutter and radar blind range, (iv) the accumulation trends measured at the other stations in this area and during the study period are very similar (thus the wind and post-depositional processes were influencing the stations in a similar way).

3. *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".

   ANSWER+CHANGES:
   The sentence has been rephrased as follows:
   ...to formulate hypotheses on the dominant microphysical processes occurring at various altitude levels during snowfall...

4. *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.

   ANSWER+CHANGES:
   The sentence was rephrased as: Firstly, it captures SLW droplets that would otherwise remain suspended or precipitate with much lower speed. Secondly, it increases the density and smooths the edge of ice-phase hydrometeors thus leading to higher fall velocities and mass fluxes (e.g. Pruppacher and Klett, 1997; Garrett and Yuter, 2014).

5. *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.

ANSWER+CHANGES:
We agree with the reviewer that this can be seen as an ambiguity. In fact, the numerical classification (riming degrees from 0 to 5) comes from Mosimann et al. (1994), while the qualitative classification (lightly, moderately, heavily rimed) comes from Mitchell et al. (1990). We displayed both because we believed that they are complementary, especially for non-specialist readers. Note that this figure has been removed from the revised manuscript.

6. *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.

   ANSWER+CHANGES:
   In this context, "PRP calculated over the whole vertical column" means that in Eq. 1 h_2 goes to infinity, i.e., there is no upper boundary set. This sentence was unclear for both reviewers and it is therefore rephrased as follows:
   ...(ii) average PRP above 50% (with $h_1 = 2250\,\mathrm{m}$ and $h_2 = \infty$ in Eq. 1.)

7. *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".

   ANSWER:
   The reviewer is right. The in-situ observations collected at the JFJ site have a different meaning during "core" and "edge" events. During core events, they sample the environment inside the main riming region. During edge events, they sample the environment above the main riming region.

   CHANGES:
   The sentence has been rephrased as follows:
   During EV3, EV4, EV6 and EV7, PRP is close to its maximum values at the altitude of the JFJ site. In the present section only we will refer to those events as "core events" and will add the C subscript to their name, to indicate that the instruments of the JFJ site were located in the core of the main riming region. On the contrary, during EV5 and EV13, JFJ is located above the main riming region and we will refer to them as "edge events" and the E subscript is added.

8. *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.

   ANSWER+CHANGES:
   The sentence has been rephrased according to the suggestion of the reviewer:
   In the following, we analyze the characteristics of the described cases by means of radar observations and in-situ measurements.

9. *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?

   ANSWER:
   We believe that this aspect has been clarified in the revised manuscript by answering to the general

comments (especially number 1) of the reviewer. In the revised manuscript the term "turbulence" is now used more carefully and it does not appear in this section. With the sentence quoted by the reviewer we meant that the variations of the variables shown in the figure between the preceding and rimed phases is similar for EV6 and EV7.

CHANGES:
The sentence has been rephrased as:
$EV6_C$ and $EV7_C$ show similar qualitative trends in the transition between the preceding phase and the rimed phase. Horizontal wind speed, vertical wind speed, Doppler spectral width, and the variability (interquantile range) of the mean Doppler velocity are lower during the rimed phases (blue histograms) than during the respective preceding phases (red histograms) as shown

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.

ANSWER:
In the revised version of the manuscript the term "turbulence", that was used in a misleading way, does not appear any more.

Is the spectrum width derived from the entire spectrum or just from the principal i.e. strongest peak? Please explain better.

ANSWER+CHANGES:
The Doppler spectrum width of the vertical profiles is calculated from the entire spectrum (once unfolded and after noise substraction). In section 2.1, when describing the polarimetric weathr radar, we added the following sentence:
During vertical profiling full Doppler power spectra were collected, allowing to calculate directly the Doppler velocity and Doppler spectral width.

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.

ANSWER:
in the revised version it should be now clear that we were discussing about similarities of variable variations and not of the absolute values (see also above, in this same point).

Figure 7: Very hard to read the legends even if I zoomed in my pdf. I suggest increasing font size and thickness.

ANSWER+CHANGES:
Fig. 7 (Fig. 6 of the revised manuscript) actually does not include a legend. However, the size of the main annotations has been increased to ensure better readability.

10. *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.

ANSWER+CHANGES:
We agree with the reviewer and the sentence has been rephrased as follows:
... Z_H takes values around 10 dBZ, Z_{DR} is approximately 0 dB and K_{dp} is close to 0 °km^{-1}...

11. *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?

ANSWER+CHANGES:
We agree with the reviewer. Snowfall microphysics involves complex interactions between the different phases of precipitation and many processes can occur at the same time. In this case we mean that we consider riming as the dominant mechanism but also aggregation helps to explain some of the observations.

Section 3.3.2 has been fully edited in order to discuss about other microphysical processes (mainly aggregation) that may be taking place together with riming. As an example, in the middle of the section the following sentence has been added:

The conditions favouring the collision of ice crystals and liquid water droplets during riming favour also the ice-to-ice interaction and aggregation can be initiated. The newly formed aggregates will contribute to enhance Z_H while keeping Z_{DR} relatively low.

12. *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.

ANSWER:
The reviewer is right about the range of temperature of the HM process. In fact, in our manuscript we hypothesize other ice production mechanisms like hoarfrost transport (Lloyd et al., 2015) or collisional mechanisms that can occur at colder temperatures(Vardiman, 1978; Yano and Phillips, 2011).

In the sentence highlighted by the reviewer we are presenting the explanations that were given by Andric et al. (2013), referred to in the text as A2013, for the areas of enhanced K_{dp}. In the following sentences then we come back to our case and propose a different collisional mechanism than HM.

CHANGES:
The sentence highlighted by the reviewer has been slightly rephrased in order to underline that we are only reporting the explanation of A2013:
A2013 suggested instead that secondary ice production of very small oblate crystals would need to take place to explain the K_{dp} signature...

13. *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.

ANSWER:
We acknowledge that the request of the reviewer is relevant but we believe that the analysis suggested are beyond the scope of this manuscript . They would require to access and analyze much more complex (and raw) data and also to develop (or adapt) image classification methods to the current needs.

Additionally, we believe that such level of detail may be excessive in this context (at least for the ice crystals). As mentioned in section 2.1, while describing the in-situ instruments:
A 3-View Cloud Particle Imager (3V-CPI) provided images and habits of liquid and ice-phase hydrometeors in the 10 to 1280 μm size range.

This size range is still small compared to the snowfall cases that we analyzed and thus the main utility of this particle probe for our manuscript was to get qualitative information about the shape of those small hydrometeors and to quantify the total mass and total number of particles in this size range (to get for example evidences of secondary ice production if they appear in large number). We mention in the conclusions (end of section) that future studies should employ particle probes able to sample larger hydrometeors:

Future studies should include radar measurements at higher frequencies, to better capture the transition between clouds and precipitation, and in-situ particle imagers of larger maximum sampling size in order to visualize the hydrometeors that contribute most to the total Z_H signal and that are larger targets for riming.

14. *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.

ANSWER+CHANGES:
We agree with the reviewer and this is now discussed, especially in relation to EV3, where the number concentration of small hydrometeors is very high. In the the second half of Sec. 3.3.2 we added the following sentences:

We observe during $EV6_C$ and $EV7_C$ the presence of many particles with smooth shapes, interpreted as heavily rimed hydrometeors. Some of these hydrometeors do not have recognizable original shape, some seems to probably originate from planar crystals, while columns and needles are observed during EV6.

During $EV3_C$ rimed crystals are observed together with a large amount of small particles (some of them highly oblate, probably columns or needles), of various shapes.

Also later, around the middle of section 4.2:
Regarding the generation of secondary ice, H2002 presented evidence for a Hallett-Mossop mechanism (Hallett and Mossop, 1974, HM herafter), that occurs at temperatures warmer than -8°C. The enhancement of K_{dp} happens in our case at temperatures between -13 and -16°C and although we observe the presence of columns and needles (as shown in Fig. 7), often produced when HM is active, this leads us to assume that other ice production mechanisms may be taking place. A first possible explanation would involve collisional mechanisms (e.g. Vardiman, 1978; Yano and Phillips, 2011) that require only earlier stages of riming, presence of supercooled liquid water, ice crystals, and turbulence (all conditions that are met during EV3). A second explanation, recently proposed for the same measurement campaign of CLACE 2014 (Lloyd et al., 2015), would be the lifting and transport of hoarfrost crystals generated at the ground level.

Where do you get your temperature information from for the -15 degC level in Fig. 7?

ANSWER+CHANGES:
The information comes from the temperature measurements of the JFJ site, assuming a constant lapse rate of 6.5°km^{-1} and therefore this measurements is reliable only if the -15° level is close to the altitude of the JFJ (like in the cases shown in Fig. 7). We added the following sentence to the caption of Fig. 6 (previously Fig.7):
The blue horizontal line indicates the estimated altitude of the -15°C temperature level, extrapolated from in-situ measurements collected at the JFJ location by means of a lapse rate with altitude of -6.5°km^{-1}.

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.

ANSWER:
We believe we addressed this comment of the reviewer in a previous answer.

15. *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.

ANSWER:
We tried to implement the suggestion of the reviewer. However, we realized that pure differentiation of Z_H profiles lead to very noisy estimates. Thus some moving window smoothing or moving window differentiation would be necessary. This method should then be properly described and motivated (i.e. why a specific smoothing length is chosen) in the manuscript. We believe that this may create issues in terms of clarity and interpretation of the results (as it was for example the case of Fig.13 in the original manuscript).

16. *P. 18081, L. 14:*
I can't find EV8 in Fig. 10.

ANSWER+CHANGES:
EV8 was actually EV5. The sentence has been corrected.

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

ANSWER+CHANGES:
Amended.

18. *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.

ANSWER+CHANGES:
The figure has been edited and the colorbar of the Doppler velocity now ranges between -3 and 3 ms^{-1}.

19. *Figure 14: I think you could improve your color scaling here as well. Some of the maximum values for Z_{DR} and K_{dp} 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 Z_{DR} above the high reflectivity layer, so reducing the maximum value of the colour scale should bring out these features much more clearly, I suspect.*

ANSWER+CHANGES:
The range of variation of Z_{DR} has been reduced according to the suggestion of the reviewer. The scale of K_{dp} instead is already quite compact and we decided to keep it as it is. K_{dp} is a variable that mostly takes very low values and therefore the figure, that shows mean values per height level, has already a narrow range of variation.

20. *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.

ANSWER+CHANGES: We agree with the reviewer, especially about the lidar data, and we rephrased the sentence as follows:
Future studies should include radar measurements at higher frequencies, lidar data, and passive remote sensors (e.g. radiometers), to better capture the transition between clouds and precipitation. Additionally, in-situ particle imagers of larger maximum sampling size in order to visualize the hydrometeors that contribute most to the total Z_H signal and that are larger targets for riming.

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" C6605
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

ANSWER+CHANGES:
All the suggestions have been included in the manuscript, unless they were part of sentences that do not appear in the revised version.

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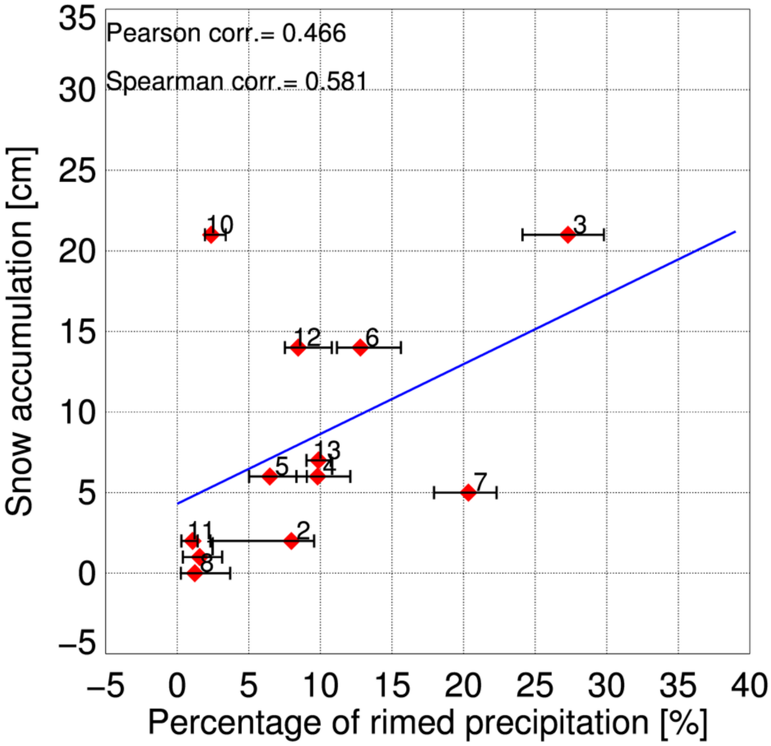

Figure 1: As Fig.3 of the original manuscript ACP-2015-416, but obtained by means of the classification method DR2009 (Dolan and Rutledge, 2009).

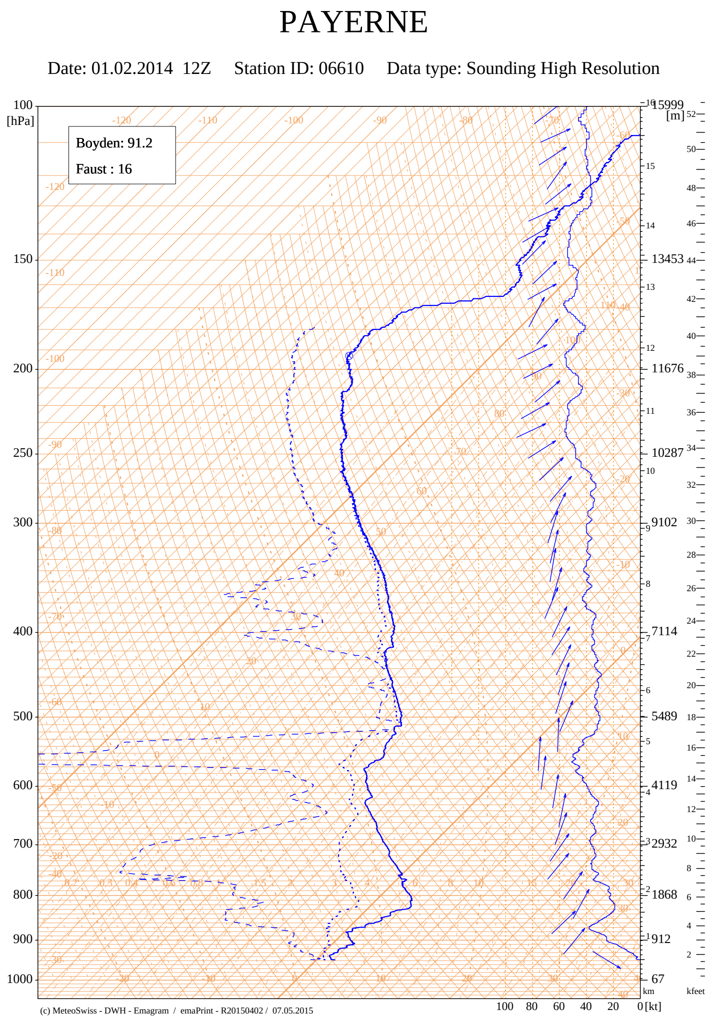

Figure 2: Sounding launched from the meteorological station of Payerne (CH) at 12Z, 01-02-2014. at the m(source: MeteoSwiss).

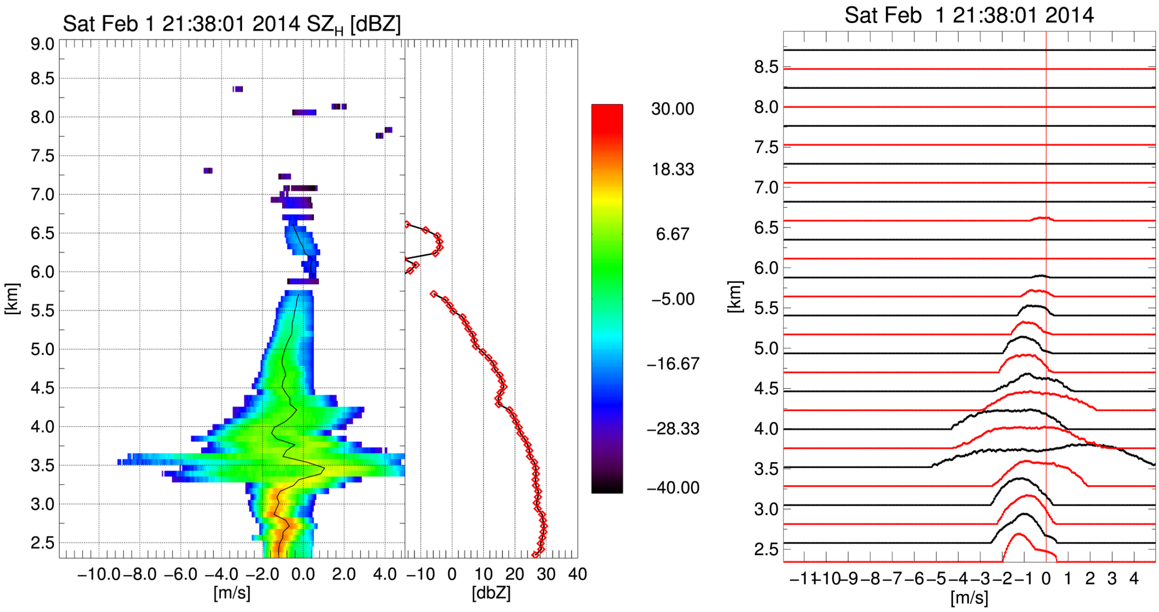

Figure 3: Example of a Doppler spectrum (spectral reflectivity factor) collected at vertical incidence during EV3. Left: color-coded Doppler spectrum, and right: stacked spectra.

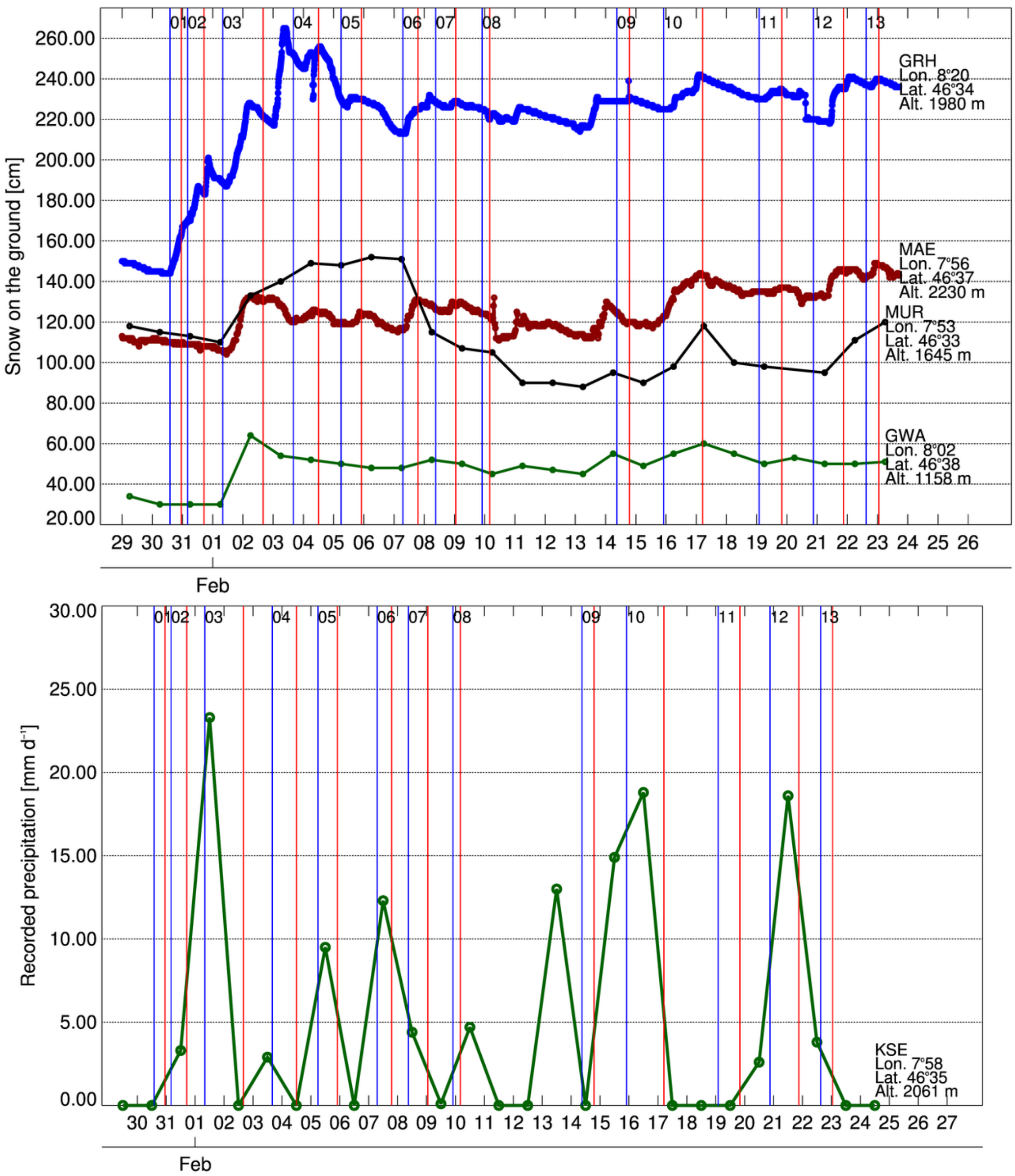

Figure 4: Top panel: snow height measurements during CLACE2014 in 4 different automatic stations. Bottom panel: daily accumulation of liquid water measured at the KS site.