Reviewer comments on 'Influence of low-level blocking and turbulence on the microphysics of a mixed-phase cloud in an inner-Alpine valley' by Fabiola Ramelli et al.

Response to Reviewer #2

We would like to thank the anonymous referee for his/her constructive and helpful feedback and suggestions on the manuscript. We incorporated the suggestions within the revised manuscript, which significantly improved the quality of the manuscript. In the following, we will address the reviewer's comments and present our responses and changes in the revised manuscript. Reviewer comments are reproduced in blue and the author responses are in black. All line numbers in the author's response refer to the revised manuscript.

General comments

1) *This manuscript uses a wide variety of ground-based observations to investigate the impact that orography can have on cloud microphysics in an Alpine environment. Understanding this impact is obviously important for increasing the accuracy of weather and climate forecasts in orographic regions, and the applications that depend on these forecasts.*

   According to the title, the major objective is to examine the effect of low-level blocking and turbulence on mixed-phase cloud microphysics, and a conceptual figure is given and discussed. The scope of the manuscript is rather broad, and tries to cover too many aspects without enough attention to detail. Many possible processes are described but, often, not enough evidence is presented in interpreting the observations. To be published, this manuscript requires major revisions. In my opinion, the manuscript would benefit from a much tighter focus, and a discussion reduced to the relevant processes backed by evidence. A major issue is that low-level blocking and wind shear are not likely to be having an impact on the formation of the mixed-phase cloud (the supercooled liquid layer at cloud top) but possibly modifying the precipitation as it falls, i.e. through seeder-feeder processes.

   Thank you for your comment and for raising several points, which helped to make the manuscript clearer. In particular, we shortened Section 4.2 and tried to focus on the relevant processes. It is difficult to provide conclusive evidence, as no in situ observations were available within the mid-level cloud and thus the analysis was based on remote sensing and ground-based ice particle observations. Furthermore, we removed Appendix B (correlation calculations) from the revised manuscript. On the other hand, following your suggestion, we extended Section 4.3 and investigated the reason for the formation of the low-level feeder cloud by relating the updraft velocity to the in situ measurements of the cloud properties.

   We agree that the low-level blocking and wind shear were not having an impact on the formation of the mid-level mixed-phase cloud or the supercooled liquid layer at cloud top; but that the shear layer likely modified the falling hydrometeors (e.g., through depositional growth, riming or aggregation) by providing an ice supersaturated environment. We modified some sentences in the revised manuscript to make this point clearer (see responses to specific comments). Additionally, we clarified that the blocked low-level flow was only responsible for the formation of the low-level feeder cloud (see Sect. 4.3).

Specific comments and questions

2) *This case study observes the passage of a synoptic-scale frontal system, and some of the features described in the manuscript can be directly attributed to the large scale motion rather than the orography. The sloping shear feature above 2.5 km in Figure 5 is common to many synoptic scale frontal systems (e.g. Keyser and Shapiro, 1986), and similar wind and shear patterns are often seen in weather radar, radar windprofiler or scanning cloud radars in fronts passing over much flatter, homogeneous terrain. The vertical wind shear values are also similar to those observed in fronts over more homogeneous terrain (Chapman and Browning, 2001).*

Thank you for this comment and the references. As described in Section 3.1 the case study was measured in a post-frontal environment on 7 March 2019 between 16 UTC and 20 UTC. The cold front passed the measurement site in Davos at around 8 UTC (see figure of the radar reflectivity below). We included a map of reanalysis data at 700 hPa height in the revised manuscript (see Fig. 2a) to give a better overview of the synoptic situation and extended the discussion of the synoptic situation (page 6, line 138-145): *"The synoptic situation over Europe was dominated by an occluding low-pressure system (980 hPa) located east of the British Isles. As the low-pressure system continued to propagate towards Scandinavia, it drove a cold front over the Alps, which passed the measurement location at 8 UTC. Based on observations, rainfall of up to 50 mm was produced on the southern side of the Alps during the passage of the cold front (not shown). By 15 UTC, southwesterly flow in the post-frontal air mass continued to advect cold air and moisture into the Alpine region (see Fig. 2a), which produced light precipitation on the south side of the Alps with some spillover precipitation on the lee side (i.e. north side) of the Alps. The case study was measured in the post-frontal air mass between 16 UTC and 20 UTC, when some spillover precipitation reached the measurement locations in the Davos region."*

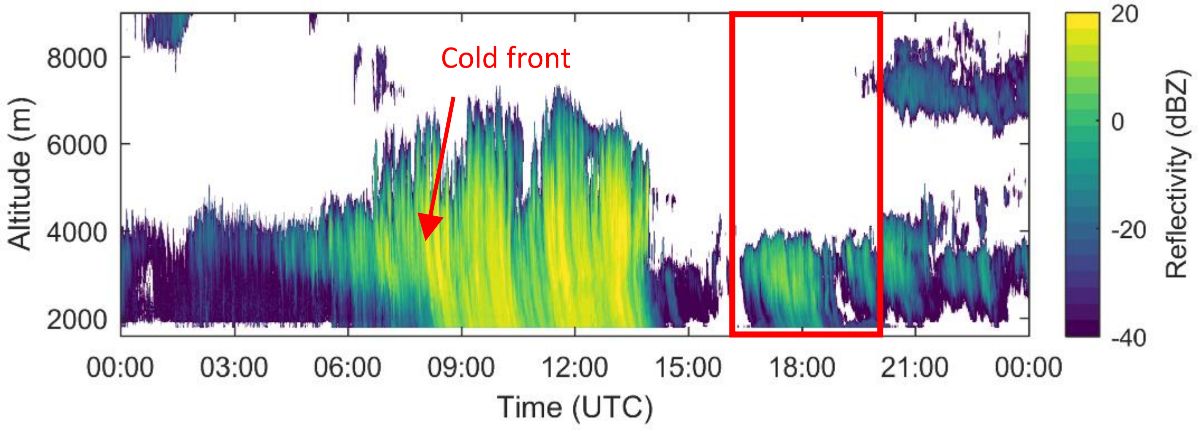

Figure 1: Radar reflectivity on 7 March 2019. The red rectangle indicates the measurement period of the present case study.

Since the synoptic frontal system passed the measurement location already in the morning, we assume that the sloping shear feature was related to the orography. This is further supported by the height of the shear layer, which was related to the altitude of the upstream mountain barrier. Since we cannot exclude an influence of the synoptic system, we extended the paragraph as follows (page 13, line 226-232): *"Sloping shear features have also been observed in connection with synoptic scale* *frontal systems (e.g. Keyser and Shapiro, 1986; Chapman and Browning, 2001), where similar vertical wind shear values have been measured (Chapman and Browning, 2001). The presented observations cannot provide conclusive evidence about whether the observed wind and shear patterns were orographically or synoptically driven. We suggest that the sloping shear feature was influenced - at least to some extent - by the orography, as the height of the shear layer was related to the altitude of the upstream mountain barrier B1 and the strength of the blocking and downward propagating cross-barrier flow (Fig. 5c, d and Fig. 7)."* Furthermore, we changed the subtitle of Section 4.1 from "Low-level flow blocking triggering wind shear and turbulence" to "Low-level flow blocking and wind shear" in the revised manuscript in order to avoid a clear assignment of a cause.

3) *As shown in Figure 8, the highest radar reflectivity values are expected at the upper boundary of the sublimation zone, before the falling ice particles start to sublimate and reduce in size. Figure 8 and 9 show that the sloping shear feature appears to coincide with this sublimation zone, with the location of the maximum radar reflectivity values lowering in altitude over time just above the 0.01 s-1 wind shear contour also lowering in time. This is what would be expected if the sloping shear feature indicates the frontal boundary between two air masses, one saturated, and one subsaturated. This correlation between the upper edge of the sloping shear zone and the maximum radar reflectivity values therefore suggests that the large scale forcing could be responsible in this case.*

   Hence, without additional observations, or using output from a high resolution numerical weather prediction model, it is difficult to determine whether the changes at lowlevel (blocked/unblocked flow) are responsible for any changes at upper levels.

   Thank you for this comment. We agree that the shear layer likely indicates the boundary between a saturated and a subsaturated air mass. However, we suggest that the flow separation (dry boundary layer in the valley/ moist cross-barrier flow aloft) occurs due to the upstream topography, since the synoptic scale-frontal system passed the measurement location already in the morning and the height of the shear layer was related to the altitude of the orography (see also response to previous review comment).

   The boundary between the two air masses was characterized by a turbulent shear layer. We suggest that the hydrometeors falling through the shear layer from above likely experience changes in the cloud properties as indicated by the enhanced radar reflectivity (increased ice growth) and the increase in LDR (change in particle shape or density). Thus, we agree that the low-level blocking did not directly influence the formation and microphysics of the mid-level cloud. Instead, the blocked layer influenced, in combination with the strength of the downward propagating cross-barrier flow, the altitude of the shear layer. We modified some sentences in the revised manuscript to make this point clearer (page 21, line 341-345): *"The ice particles encountered a turbulent shear layer while falling through the cloud, within which changes in the microphysical cloud properties were observed including enhanced radar reflectivity (i.e., increased ice growth) and LDR (i.e., change in particle shape or density). This suggests that the turbulent shear layer created an ice supersaturated environment and thereby influenced the cloud microphysics."*

4) *The wind shear values derived from the two instruments are not always consistent with each other. Is this due to the differences in spatial and temporal resolution, scan pattern or integration time? Please include the elevation angle that the wind profiler operates at and the scan pattern used by the Doppler*

lidar for deriving winds. The wind calculations assume a homogeneous wind field and it is known some scanning patterns are more susceptible to turbulence, which can mean that this assumption is no longer valid (Päschke et al., 2015). How much of an impact could the turbulent zones have on the horizontal wind and shear calculations? How about variations in the particle fall velocity?

Päschke, E., R. Leinweber, and V. Lehmann (2015), An assessment of the performance of a 1.5 m Doppler lidar for operational vertical wind profiling based on a 1-year trial, Atmos. Meas. Tech., 8, 2251–2266, doi:10.5194/amt-8-2251-2015.

Thank you for this comment. The differences in the vertical wind shear are mostly due to different vertical and temporal resolution of the wind profiler and wind lidar. The temporal and spatial resolution are specified on page 5, line 122-124: *"The wind profiler had a temporal resolution of 5 min and a vertical resolution of 200 m, whereas the wind lidar provided wind measurements with a higher vertical resolution of 50 m."* This pattern is also visible on Figure 5. Furthermore, we included the elevation angle and the scan pattern used by the wind lidar in the revised manuscript (page 5, line 124-126): *"The wind lidar operated in Doppler Beam Switching (DBS) mode with 4 beams at an elevation angle of 75° and a vertical beam. Additionally, Range Height Indicator scans (RHI) were performed every 30 minutes in four different azimuth directions (0°, 70°, 180° and 250°)."* The wind profiler had a vertical beam as specified on page 5, line 121 in the revised manuscript. DBS is a widely used technique to measure 3D wind properties. According to a manufacturer, both VAD and DBS can be used to retrieve wind speed. The scanning strategy will have a bigger impact on variance measurements (Newman et al., 2016; https://doi.org/10.5194/amt-9-1993-2016) but this is beyond the scope of this paper.

5) *Section 4.2 attempts to describe the influence of shear on the particle microphysics, but insufficient evidence is given to support this. It is obviously difficult to use the Doppler velocity values directly, as these are compromised by the unknown vertical air motion, but the Doppler spectra do show important information. Figure 10 shows one example of the Doppler spectra following one fall streak, and the broadening is consistent with changes in the particle microphysics; the broadening occurs in a temperature range that coincides with the temperature range for the Hallett-Mossop process for secondary ice production (-8 to -3 C). This increase in Doppler spectral width is clearly seen in Figure 3c between 3000 and 2500 m. However, this increase in Doppler spectral width is more or less constant in altitude throughout the entire time period, and not correlated with the wind shear, suggesting that temperature (possibly the Hallett-Mossop process) is responsible for this microphysical process, not shear.*

Thank you for this comment. As shown in Fig. 8 and Fig. 9, the maximum radar reflectivity was observed to coincide with the upper part of the shear layer. This suggests that the shear layer created an ice supersaturated environment, which enabled the falling hydrometeors to grow to larger sizes. As you pointed out correctly, the Doppler spectra (Fig. 10) contains important information regarding the microphysics. The broadening of the Doppler spectrum and the increase in the Doppler velocity (and thus in the particle fallspeed) at 3000 m suggests that riming occurred within this layer as presented in the revised manuscript (page 14, line 264-271): *"Interestingly, the increase in the LDR was collocated with the region of maximum radar reflectivity (2900 m; Fig. 10a), of maximum (negative) Doppler velocity (2900m; Fig. 10a) and the upper part of the shear layer. The spatial coincidence between maximum radar reflectivity, shear layer and increase in LDR was also observed for other fallstreaks (Fig. 3d), suggesting that the shear layer created an ice supersaturated* *environment, within which the hydrometeors grew to larger sizes. The increase in the Doppler velocity might be indicative of riming. Previous studies observed that an increase in the Doppler velocity can be indicative of riming, which leads to a higher terminal fall velocity of particles due to the rapid gain of ice particle mass (e.g., Mosimann, 1995; Kneifel and Moisseev, 2020). This is further supported by the increase in the LDR of the faster falling population of the spectrum as a consequence of the higher particle density."*

Indeed, the temperature was in the range of the Hallett-Mossop process, so to account for this we now mention on page 16, line 275-277 that the Hallett-Mossop process was potentially occurring: *"The temperature between 3000 m and 2500 m ranged from -8 °C to -4 °C and was thus in the temperature regime of columnar growth and of the Hallett-Mossop process. Thus, secondary ice particles might be produced upon riming, which could then rapidly grow by vapor deposition into column-like particles."*

6) *Note that Doppler spectra wouldn't necessarily show discrete multiple peaks with secondary ice production in turbulent regions, or if sublimation is occurring (evaporation broadens the size distributions).*

   Thank you for pointing this out. We included a sentence describing this effect in the revised manuscript (page 16, line 283-286): *"The analysis of the Doppler spectra showed no evidence of discrete multiple spectral peaks (i.e., the presence of multiple particle populations with different fall speed), which would support the occurrence of secondary ice production. However, turbulent regions or sublimation could broaden the size distributions and thus mask the presence of discrete multiple peaks in the Doppler spectra."*

7) *The conceptual picture shows ice above a supercooled liquid layer, which, although possible, is not that typical for mixed-phase clouds with relatively warm (above -27 C) cloud tops (e.g. Westbrook and Illingworth, 2011; Battaglia and Delanoë, 2013), and is not supported by the remote-sensing observations shown here. The one occasion during P2_unblocked where the base of the supercooled liquid layer is not at the top of the cloud layer seen by the cloud radar is when there is appreciable LWP. LWP of 100 gm-2 implies a liquid layer that is likely to be at least 400 m thick from theoretical adiabatic considerations (e.g. Merk et al., 2016, https://doi.org/10.5194/acp-16-933-2016), which would place the top of the liquid layer at the top of the cloud layer seen by the cloud radar. This means that the observed case study agrees with previous studies.*

   Thank you for pointing this out. Indeed, this was a technical mistake in the drawing on our part. We changed the schematic in the revised manuscript, so that the supercooled liquid layer extends up to the cloud top.

8) *The data presented does indicate that low-level blocking influenced the presence of low-level cloud in the valley. The three periods selected showed clearly that low-level cloud was present during blocked low-level flow, but not once this blocking weakened.*

   One option would be to investigate the reasons for this further. The radar Doppler velocity plot suggests that the low-level liquid layer is being formed in updrafts, as almost all Doppler velocities appear to be slightly positive (i.e upwards) for this layer. Is this the case? Or is this due to the difficulty in reading the color scale? The typical vertical air velocity in this layer could be determined from either the Doppler spectra (similar to Fig. 10) or from CFADs of Doppler velocity (similar to Fig. 8). If the air motion is upwards, it would still be weak (< 1 m s-1), so would not necessarily counter the blocked flow argument *but be a result of in-valley circulation. Does the LWP correspond to the updraft speed? How about the cloud droplet number or size (Figure 12)?*

Thank you for this comment and your suggestions. We extended Section 4.3 in the revised manuscript and investigated the reason for this transition further. Indeed, the updraft velocity seems to play a crucial role for the formation of the feeder cloud (estimated from the maximum Doppler velocity of the spectrum). Higher updrafts were observed during P1_bl and P3_bl, when a low-level feeder cloud was present (page 18, line 310-313): *"Indeed, the cloud radar indicated the presence of higher Doppler velocities and thus higher updraft velocities during P1_bl (0.5 m s^{-1}) and P3_bl (0.7 m s^{-1}) (Fig. 12). When the blocking weakened and the updraft velocity decreased during P2_unbl (0.37 m s^{-1}), the low-level cloud at Wolfgang dissipated likely due to insufficient upward motion to sustain the production of liquid water."* Furthermore, we calculated correlations between the maximum Doppler velocity and the microphysical properties in the revised manuscript (see Fig. 13 and page 18, line 313-322): *"The correlation plots between different dynamical (mean and maximum Doppler velocity) and microphysical properties (LWC, CDNC, mean diameter) in Figure 13 further support the assumption that the updrafts driven by the in-valley circulation induced the formation of the low-level liquid cloud. Moderate positive Spearman's rank correlation coefficients were observed between the maximum Doppler velocity (vmax) and the LWC(0.42) and CDNC (0.46), whereas a weak correlation was found between the maximum Doppler velocity and the mean diameter D (0.17). Similar correlation coefficients were observed between the mean Doppler velocity and the microphysical properties (not shown). The increase in CDNC with increasing updraft velocity (Fig. 13c) suggests that droplet formation was limited by the vertical velocity that generates supersaturation, i.e. velocity-limited conditions were encountered at Wolfgang. This aspect is dealt with in more detail in a paper by Georgakaki et al. (2020), where they investigated the drivers of droplet formation in mixed-phase clouds using observations from the RACLETS campaign."*
The findings suggest that local updrafts were produced by the in-valley circulation during blocked low-level flow, which were the drivers for the formation of the low-level feeder cloud.

9) *The wind direction changes and speed slows (in general) at Wolfgang during P2, which coincides with precipitation and no low-level liquid water (Figure 7). Is this just because there is enough time for the precipitation to fall before evaporating (shallower subsaturated layer)? Is this precipitation solid or liquid? What is the size distribution?*

As mentioned in Section 4.2 (page 14, line 246-249), this is likely a consequence of the lower extent of the subsaturated layer (maximum radar reflectivity moves to lower altitudes, see Fig. 9), which enabled the hydrometeors to reach the surface prior to complete sublimation during P2_unbl. The amount of precipitation recorded by the disdrometer at Wolfgang was generally low (< 0.2 mm/h). The precipitation reaching Wolfgang was solid in accordance with the temperature slightly below 0 °C (see also ice particles detected by MASC and HOLIMO in Fig. 10). A plot of the particle size distribution of hydrometeors measured by the disdrometer at Wolfgang is shown below in Fig. 2. We included the following sentence in the revised manuscript (caption Fig. 11): *"The particles measured by the disdrometer at Wolfgang were primarily in the size range between 0.5 mm and 1.5 mm (not shown)."*

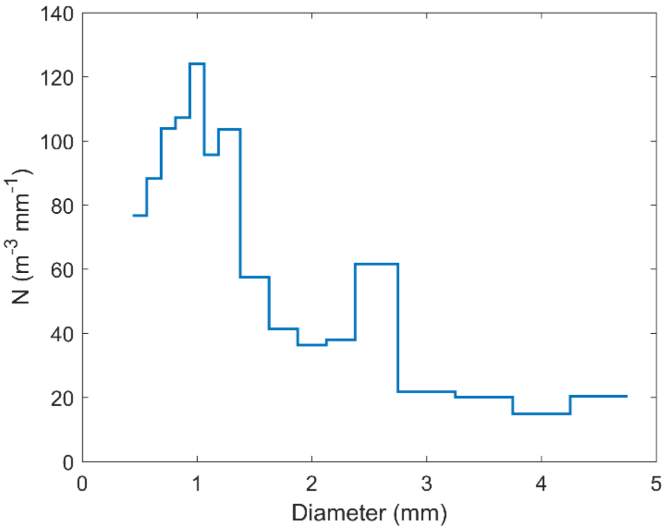

Figure 2: Particle size distribution of hydrometeors measured by the disdrometer at Wolfgang (averaged between 17:45 UTC and 18:40 UTC).

10) *Also of interest is why the seeder-feeder mechanism did not appear to operate in this particular case study, presumably due to the fact that the upper level precipitation rarely fell far enough to benefit. E.g Fig. 12 shows precipitation not quite reaching to 1800 m.*

We assume that the seeder-feeder mechanism only played a minor role for precipitation enhancement in the present case study, as the lower part of the boundary layer was characterized by subsaturated conditions and thus a significant fraction of the hydrometeor mass sublimated before reaching the feeder cloud. However, in the revised manuscript, we included a sentence stating that this effect was important in other cases of the RACLETS campaign (page 20, line 328-332): *"We assume that in the present case study the low-level feeder cloud did not play a crucial role for precipitation enhancement, as a significant fraction of the hydrometeor mass sublimated before reaching the feeder cloud. However, in other cases of the RACLETS campaign, we found that orographically-induced low-level feeder clouds could enhance precipitation through the seeder-feeder mechanism and provide an environment for secondary ice production mechanisms (Ramelli et al., 2020b)."*

11) *Figure 9. Is it likely that the TKE measured close to the surface at Gotschnagrat is representative of the turbulence in the atmosphere? Isn't it more likely to be due to local shear close to surface? What are the wind speeds at this location? The direction is the same as at the surface, but Fig. 5a suggests high wind speeds at this height, is the increase in TKE just due to an increase in wind speed close to the surface? Why would this then correlate with the precipitation rate elsewhere in the valley? How does this relate to the conceptual figure?*

Thank you for pointing this out. It is likely that the high TKE values measured by the 3D sonic anemometer were related to the high wind speed close to the surface and not representative of the turbulence in the atmosphere. Accordingly, we have removed the TKE observations in the revised manuscript.

12) *I'm not convinced of the usefulness of any the correlation coefficients described here. How do they relate to any expected dynamical or microphysical processes? The shear layer appears to coincide with the sublimation zone. For this case study, any attempt to link the surface precipitation to the maximum reflectivity in the profile should at least take the varying sublimation depth into account.*

    Thank you for the comment. We agree that it is difficult to relate these coefficients to the dynamical and microphysical processes. We removed Appendix B and the discussions related to the correlation coefficients in Section 4.2 in the revised manuscript. Instead, we focus on the relationships between the vertical velocity and the low-level cloud properties (see also response to review comment #8).

Technical comments

13) *Line 52: Is this wind shear value for wind shear in the horizontal or in the vertical?*

    Thank you for this comment. We changed it to "vertical shear in the horizontal wind" in the revised manuscript (page 2, line 53).

14) *Lines 91-92: Do you mean 'the mean ridge height'?*

    Thank you for this comment. We changed it to "mean ridge height" (page 4, line 94-95).

15) *Line 116,118: Isn't Vaisala a Finnish company?*

    Thank you for pointing this out. We change it in the revised manuscript.

16) *Figure 2 caption: This should state 'taken by the Meteosat 2nd Generation (MSG) satellite'.*

    Thank you for pointing this out. We exchanged the satellite picture with a reanalysis plot of the synoptic situation in the revised manuscript.

17) *Line 228: Cloud base? The ice cloud continues to the surface during P1 and P2. Since all ice is falling (precipitating), the ice cloud base is defined in terms of visibility, not in terms of relative humidity (changes in growth or evaporation rate). Hence it is only during P3 that there is an ice cloud base.*

    Thank you for pointing this out. We redefined it as onset/beginning of the sublimation layer in the revised manuscript.