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Interactive comment

Interactive comment on "On the relationship between total differential phase and pathintegrated attenuation at X-band in an Alpine environment" *by* G. Delrieu et al.

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First, we would like to thank the three reviewers for their detailed and constructive comments about this article.

As a main feedback, the three reviewers suggest us to consider more events in our analyses in order to strengthen the quantitative outcomes of this article, especially regarding the PIA-ïĄędp relationship in the melting layer (ML). We recognise this is desirable and feasible since we have recorded about 30 events with the ML being at the level of the Moucherotte radar. This will be however a major additional work requiring the collection and processing of the Moucherotte radar data that are not available yet to us, and will not be available in the coming weeks/months due to the covid-19 lockdown. However, we have extended the rain case study to 9 convective events (new Table 2) and the results obtained nicely confirm the analysis of the first version of the article for the July 21st 2017 convective event.

We have also deepened the methodology and redo all the calculations with a special attention on (i) the characterization of the dry-weather reference targets stability and time variability (new tables 3 and 4, figures modification to show the 10% and 90% quantiles of the apparent reflectivity of the mountain targets) and (ii) on the possible deltahv contamination of the raw psidp profiles. The regularization procedure of the raw phidp profiles was improved in this latter respect and we found it to be efficient in filtering “bumps” likely associated with deltahv contamination. Regarding the manuscript, the abstract and the conclusion were largely rewritten and the description of the MRT and polarimetry PIA estimators was also much detailed in sections 3.1 and 3.2. Two additional figures were included to better illustrate and support the analyses made. In general terms, we took great care in discussing the results and the possible influence of the various sources of error in the two different case studies. We do hope these efforts, which effectively resulted in a major revision, will satisfy the anonymous reviewers.

Our item-by-item replies are inserted below in blue within the reviewers' comments recalled in black.

Anonymous Referee #3 1 Summary This manuscript proposes a data-driven investigation of the relationship between the differential phase shift and the speciïňĄc attenuation in rain, melting snow and snow, using an original instrumental set-up consisting of two X-band polarimetric radars at different altitudes in the complex terrain around an Alpine valley. Such relationships are crucial to accurately correct for attenuation in precipitation to obtain reliable quantitative precipitation estimates at X-band. The path integrated attenuation is determined using strong (ïňĄxed) mountains echoes at various distances from the considered radar from the two considered radars. . . and provide independent estimates that can be compared to the (total) differential phase shift derived from polarimetric radar measurements. In rain, additional information about the raindrop size distribution measured by a disdrometer at the ground level is available to compute theoretical relationships. Focusing on two contrasted event (one convective and the other with a transition from snow to rain), the authors quantify the respective values of PIA and total differential phase shift from a number of mountains echoes, in rain using the lower radar, and in snow and in the melting layer using the higher radar. In this way, the speciïňĄc attenuation in the ML can be quantiïňĄed and it appears that the relationship between the PIA and the total differential phase shift is not that linear. To be more precise, in the ML (stratiform case), old Fig. 11 suggests that the multiplicative coefficient of a k-Kdp relationship (assumed to be linear) depends on the position within the ML and as such on the melting processes.

2 Recommendation The manuscript is clear, the methods are sound and properly described. Such characterization of the attenuation in the melting layer and its links with the differential phase shift are relevant to the weather radar community and to AMT readership. I have some concerns and suggestions listed below, I hence recommend to send the manuscript back to the authors for major revisions.

3 General comments 1. The main concern in my view is the limited amount of data analyzed. The representativity of these two events, and the one used to investigate attenuation and differential phase shift in the melting layer is not clearly addressed: to what extent can a reader use the numbers provided here for other locations/seasons? This is an important aspect because if not representative, the obtained results will be of limited interest to potential readers (who may not be able to reproduce the same instrumental set-up involving two radars and complex terrain). The authors touch upon this issue in the conclusions and mention that they will process more data, but this should be addressed earlier in the text, and to be honest I am wondering if they should not do so already in this manuscript. We fully understand this comment but we are not in position to extend our analyses to other ML cases right now, see our head comment. . . We have extended the convective case study to 9 events.

2. The scientiïňAc objectives of the manuscript are not very clear. What are the main take-home messages for the reader? We have tried to improve this aspect in the conclusion of the article (and in the abstract). Message 1 about the rain case study in the conclusion: “In the end, the scatterplot of the MRT PIAs as a function of the ÏŢ_dp (r_M) for all the nine convective events presents an overall good coherence with however a significant dispersion (explained variance of 77%). It is interesting to note that the non-linear k- K_dp relationship derived from independent DSD measurements taken during the events of interest at ground level allows a satisfactory transformation of the XPORT ÏŢ_dp profiles into almost unbiased (although dispersed) PIA estimates. Both estimation methods are prone to specific errors and, even if the MRT PIA estimator is more directly related to power attenuation, it is a priori difficult to say which estimator is the best. An assessment exercise of attenuation correction algorithms, making use of both PIA estimators, with respect to an independent data source (e.g. raingauge measurements) is desirable to distinguish the two PIA estimators. A specific experiment is being designed in this perspective to be implemented in the near future.” Message 2 about the ML case study:

“From this dataset, it was possible to derive the evolution of PIA(r_M) and ÏŢ_dp (r_M) values as a function of the altitude within the ML. The evolution with the altitude of the ratio of the mean value of PIA(r_M) over the mean value of ÏŢ_dp (r_M), as a proxy for the slope of a linear k- K_dp relationship within the ML, was also considered... The three variables considered present a clear signature as a function of the (scaled) altitude. In particular, the PIA/ÏŢ_dp ratio peaks at the level of the _hv peak (somewhat lower than the Zh peak), with a value of 0.42 dB degree-1, while its value in rain just below the ML is 0.33 dB degree-1. ... Although the experimental configuration for the study of attenuation in the ML presents some limitations (radome attenuation, NUBF), the preliminary results presented here will be deepened by processing a dataset of about thirty stratiform events with the presence of the ML at the level of the MOUC radar.

3. The assumption that the differential phase shift on backscatter (δhv) is negligible is not really justiïňĄed. This comment led us to a big consideration of this point during the revision process. Besides the cited literature we have searched evidence of deltahv contamination in the raw psidp profiles both for the convective cases and the stratifom one. We have found some "bumpy" profiles during some convective events that our regularization procedure is fortunately able to filter in a nice way. No bumpy profiles in the stratiform case. But we recognise that more work is required on this topic. . . Together with the possible PIA overestimation due to radome attenuation for the MOUC radar during the stratiform event, these two sources of uncertainties may affect the highlighted behavior of the ratio between the PIA and the Ψdp in the ML. This aspect should be clariïňĄed. This is actually hard to clarify. In addition to the delthv contamination, the radome attenuation is a real concern. We have considered a mountain target in the vicinity of the MOUC radar (5 km or so) but it was too unstable to provide useful information. We have tried to be more careful in our interpretations of this very limited case study ; see our new comments of the results obtained on the upper part of the ML (which could sign radome attenuation or NUBF effects). Radome attenuation is likely small for the considered case due to the low rainrates/snowrates and the fact the radome is heated.

4 SpeciïňĄc comments 1. Title: I think the exact term is differential phase shift. I recommend the authors to edit the whole text to add shift where needed. OK, done

2. P.1, l.12: rainfall and snowfall rather than rain and snow. corrected

3. P.1, l.13: "high mountain regions": the adjective high is relative... I suggest to change to "mountainous regions". Although relative, we want to keep the adjective "high" since radar QPE is particularly challenging in such regions wrt to plains or medium-elevation mountains

4. P.1, l.24: high rather than strong rain rates. corrected

5. P.1, l.24: Φdp is not deïňĄned yet. corrected

6. P.2, l.41: insert “over extended areas" between “achieved" and “with traditional". done

7. P.2, l.42-51: it would be good to support the statements by references to the literature. These statements are quite “generic” and do not require in our view specific referencing.

8. P.2, l.58: the common usage is that polarimetric means dual-polarization and Doppler... Good point, we suppressed “Doppler”

9. P.3, l.72: Kdp is the speciïňĄc differential phase shift on propagation. Please correct wherever needed in the text. corrected

10. P.4, Section 2.1: what about the calibration of the two radars? How was it checked/performed? The MOUC radar calibration was performed through the Météo-France standard electronic calibration procedure followed by a qualification of the rain products through radar-raingauge comparisons. The XPORT radar electronic calibration was performed at various occasions during the radar implementation in several campaigns. No radar-raingauge or radar-disdrometer comparisons have been made so far in the Grenoble context. In any case, a major advantage of the proposed methodology is that both the Φ_dp and the MRT PIA estimates are independent of eventual radar calibration errors.

11. P.4, l.110: missing closing bracket after “study". corrected

12. P.5, Eq.1: This equation is for a given polarization, this should be indicated using a subscript h/v for instance. The following sentence was added: Note that PIAs can be obtained from eq.1 for both the horizontal and the vertical polarizations. In the present article, we will restrict ourselves to the horizontal polarization, the study of differential attenuation being a possible topic for a future study.

13. P.6, l.168: δhv is the differential phase shift on backscatter. OK corrected

14. P.6, l.175: the units of these ranges of values (degree?) should be provided. done

15. P.6, l.179: the assumption of negligible δhv should be better justiïňĄed. A few degrees for δhv as suggested on l.175 are not necessarily negligible compared to the overall Ψdp values provided in Fig.10 for instance. As mentioned in the General Comments, the resulting uncertainty in Φdp values may affect the behavior highlighted in Fig.10 and 11. Combined with possible radome attenuation... Yes we agree, but there is little possibility to go further. . . We have expended and moderated our comments of new Figs 12 and 13 in section 4.2 and in the conclusion.

16. P.6, l.182-183: why N = 10 and N = 4? How did you come up with these values? This is empirical.

17. P.7, l.191: same here, please justify these thresholds in Zh and hv. Actually for the Φ_(dp)profile processing for the convective case (without ML interaction), we determine now the beginning and the end in range of rain cells undisturbed by clutter by using a rhohv threshold only (_hv $\geq$ 0.95) to be valid over a number of successive gates (10 gates for XPORT radar, i.e. a range extent of 342 m). We had to adapt these figures for the MOUC radar due to the well-known decrease of rhohv in the ML (_hv $\geq$ 0.80, and 2 successive gates, that is 480 m) and to consider the actual range of the first mountain gate for the determination of rM.

18. P.7, l.196: the black line in Fig.4 represents the instantaneous values of Zh, it would benice to ïňĄgure the variability of the mountain return, to give the reader an idea about the noise of such echoes (and hence an idea about the uncertainty in the derived PIA estimates). We have added a lot of material in the revision about the stability and time variability of the dry-weather mountain returns of the various targtes. We did not modify Fig.4 however for which the considered mountain returns correspond only to those available for a given radial of the considered target.

19. P.8, l.218: please provide a reference for negligible attenuation in snow. done

20. P.8, l.228: “[7]" seems to be a literature reference, but there is no number in the references. Please update. corrected

21. P.8, l.245: the co-ïňĆuctuation between the two signals does not look that bad by eye... Maybe you could compute the correlation coefïňĄcient to have a quantitative criterion? The number of pairs of points is quite low for individual targets. The results for the ensemble of targets is displayed in new Fig.11 (old Fig 9).

22. P.8, l.242-248: the possible inïňĆuence of beam broadening and radome attenuation (see l.401-406) could be ïňĄrst mentioned here. Yes, we have added a paragraph on the possible error sources at the end of section 3.2

23. P.9, l.277: change citations from numbers ([10] and [19]) to author's names... done

24. P.10, l.302 and Fig.8: I may be wrong, but I think there is an issue with the axis labels in Fig.8: PIA from polarimetry should be on the y axis while the PIA from MRT should be on the x-axis. Otherwise, there would be an underestimation from the polarimetric approach (slope > 1), not consistent with Fig.6 left. Please clarify. Very good point! But I confirm that the MRT PIA is on the y-axis and the polarimetry-derived PIA is on the x-axis. As explained in the revised manuscript, this effect is related to the different ranges of Kdp values considered in the DSD analyses and in the Kdp range profiles discretized at 34.2 m. We have checked the stability of the non-linear relation when considering 1-min DSD samples, leading to an extended Kdp range.

25. P.11, l.322-323: what can explain this variability in the ML depth? If this is due to different types of hydrometeors, is the scaling approach used here still relevant? We think this variability of the ML thickness (with high values between 2:30 and 3:00 UTC, visible on the bottom graph of Fig. 5) to be due to the arrival of the hotter air mass and some kind of atmospheric mixing. We preferred this altitude scaling to the display of absolute altitudes with respect to e.g. the reflectivity or the rhohv peak altitude since the ML thickness varies significantly and the curves of the various ML characteristic points (peaks and inflexion points of Zh and rhohv) evolve rather harmoniously during the ML rise.

26. P.11, l.342: could this less evident shift between peak in Zh and in hv be also due

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to beam broadening? As the ML is going up in altitude, it is also going further away in the PPI used to extract the polarimetric radar variables... Maybe. . . However, in the more systematic study of the ML described in Khanal et al. 2019, the shift between the two peaks has been evidenced for MLs at the altitude range of the last part of the January 4th 2018 event. We don't think this point alters the conclusions made in this article.

27. P.12, l.379: why are δhv values expressed in dB? It was a mistake, corrected, thanks!

28. P.13, l.405-406: but the attenuation due to wet snow sticking on the radome is not necessarily directly proportional to the rain rate (it can accumulates...). The assumption of negligible radome attenuation during the ML scans should be better justiïňĄed. As it could have signiïňĄcant impact on the estimated PIA values and hence on the behavior of the ratio PIA/Ψdp in Fig.11. We have to mention that the radome of the MOUC radar is heated, so that snow may not accumulate that much over it. In addition snow/rain rates were low, so radome attenuation may be low in this case. But it is difficult to be sure, and this is certainly a major limitation of our current measurement configuration...

29. P.17, Table 1: the spectral width is not recorded? Yes for the MOUC radar, no for the XPORT radar at that time; but we didn't use Doppler data in this study

30. P.19, Fig.2: it would be better to use the same y axis scale between the 2 events, to ease the comparison. Not done! The required code was not available to the main author at the time of the revision (lockdown. . .)

31. P.20, Fig.3: the underlying images are too coarse in resolution. They should be improved. done

32. P.21, Fig.4: As expected, the phase measurements are contaminated by clutter earlier (i.e. closer to the radar) than reïňĆectivity measurements. Hence the last (starting from the radar) reliable gate in Ψdp may be closer to the radar than the last reliable

gate in Zh from which the PIA is estimated. Could this introduce a bias? The Φ_dp processing (and the subsequent polarimetry-derived PIA estimation) is made on the range of gates non contaminated by close-range or mountain clutter. The MRT PIA includes “on-site” attenuation and attenuation “over” the mountain. So yes, this could introduce a bias, which is difficult to estimate, but that we think of limited magnitude in the considered examples. This is mentioned in the revision.

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