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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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1 Summary

This manuscript proposes an data-driven investigation of the relationship between the differential phase shift and the specific 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 (fixed) mountains echoes

at various distances from the considered radar 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 specific attenuation in the ML can be quantified and it appears that the relationship between the PIA and the total differential phase shift is not that linear.

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.

2. The scientific objectives of the manuscript are not very clear. What are the main take-home messages for the reader?

3. The assumption that the differential phase shift on backscatter (δ_{hv}) is negligible is not really justified. 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 Ψ_{dp} in the ML. This aspect should be clarified.

4 Specific 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.

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

3. P.1, l.13: “high mountain regions": the adjective high is relative... I suggest to change to “mountainous regions".

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

5. P.1, l.24: Φ_{dp} is not defined yet.

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

7. P.2, l.42-51: it would be good to support the statements by references to the literature.

8. P.2, l.58: the common usage is that polarimetric means dual-polarization and Doppler...

9. P.3, l.72: $K_{dp}$is the specific differential phase shift on propagation. Please correct wherever needed in the text.

10. P.4, Section 2.1: what about the calibration of the two radars? How was it checked/performed?

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

12. P.5, Eq.1: This equation is for a given polarization, this should be indicated using a subscript h/v for instance.

13. P.6, l.168: δ_{hv} is the differential phase shift on backscatter.

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

15. P.6, l.179: the assumption of negligible δ_{hv} should be better justified. A few degrees for δ_{hv} as suggested on l.175 are not necessarily negligible compared to the overall Ψ_{dp} values provided in Fig.10 for instance. As mentioned in the General Comments, the resulting uncertainty in Φ_{dp} values may affect the behavior highlighted in Fig.10 and 11. Combined with possible radome attenuation...

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

17. P.7, l.191: same here, please justify these thresholds in Z_h and ρ_{hv}.

18. P.7, l.196: the black line in Fig.4 represents the instantaneous values of Z_h, it would be nice to figure 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).

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

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

21. P.8, l.245: the co-fluctuation between the two signals does not look that bad by eye... Maybe you could compute the correlation coefficient to have a quantitative criterion?

22. P.8, l.242-248: the possible influence of beam broadening and radome attenuation (see l.401-406) could be first mentioned here.

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

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.

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?

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

27. P.12, l.379: why are δ_{hv} values expressed in dB?

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 justified. As it could have significant impact on the estimated PIA values and hence on the behavior of the ratio PIA/Ψ_{dp} in Fig.11.

29. P.17, Table 1: the spectral width is not recorded?

30. P.19, Fig.2: it would be better to use the same y axis scale between the 2 events, to ease the comparison.

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

32. P.21, Fig.4: As expected, the phase measurements are contaminated by clutter earlier (i.e. closer to the radar) than reflectivity measurements. Hence the last (starting from the radar) reliable gate in Ψ_{dp} may be closer to the radar than the last reliable gate in Z_h from which the PIA is estimated. Could this introduce a bias?