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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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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

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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 #1

General comment The manuscript entitled "On the relationship between total differential phase and path integrated attenuation at X-band in an Alpine environment" presents interesting observations of radar measurements conducted at various relative altitudes with respect to the melting layer. The two-radar set-up and the combinations of their measurements is interesting, uncommon, and surely relevant for the radar meteorology community. I believe that the manuscript is suitable for publication after a major review, following the major and minor comments proposed here. Major comments 1.

Let us take as example Figure 4, but this has to be considered as a general comment on how to present the MRT data. When the authors show the reference dry value of reïňĆectivity, I believe they should show also an indication of its variability (standard deviation or quantiles, to put some sort of error bars to the black curve). In my experience, the variability of mountain returns can be signiïňĄcant even at short time scales. This is particularly true as the radar of this manuscript is scanning and not pointing at a ïňĄxed direction. I would be pleased to see a signiïňĄcant section of the manuscript devoted to illustrate and statistically characterize the stability of MRT signals in dry weather before to discuss the analysis and the results of the two cases.

A considerable effort was done in this respect with a detailed description and illustration of the methods used to select the mountain targets and their stability and time variability in the two measurement configurations (new section 3.1). Two tables were added and the figures were modified when needed so as to show the time variability of the mountain targets. 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.

2. It would be beneïňĄcial if the authors could extend their analysis beyond the focus on two contrasted events only. It would be also more consistent with the title of the manuscript, that suggests a more global approach rather than the analysis of individual precipitation events.

As indicated above we have extended the convective case study to 9 events. We are not in position to do the same work for the ML case study in this period. We have moderated the ambition of our study by adding “Preliminary investigation of . . .” in the title of the article

3. While I found the data shown here very interesting, I could not see in the manuscript a clear research goal but rather a showcase of interesting radar observations.

We tried to improve the motivation of this study in several places. Effectively, this study is somewhat “upstream” with respect to the practical goals of the RadAlp experiment

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which concern rainfall and snowfall estimation in a high-mountain context. This is only one step in a certainly long-term process. The reviewer may recognised the importance of comparing PIAs derived from polarimetry and from direct power estimates and the need to put these 2 estimators in competition for QPE with respect to independent measurements (future step). Attenuation in the ML is also poorly documented and important for the interpretation of radar measurements, especially in our high-mountain context. Other comments 1. Abstract: I believe that the goals of this research should be better stated in the abstract.

Modification performed: "We present in this article a methodology for studying the relationship between the differential phase shift due to propagation in precipitation (Φ_dp) and path-integrated attenuation (PIA) at X-Band. This relationship is critical for quantitative precipitation estimation (QPE) based on polarimetry due to severe attenuation effects in rain at the considered frequency. In addition, this relationship is still poorly documented in the melting layer (ML) due to the complexity of the hydrometeors' distributions in terms of size, shape and density. The available observation system offers promising features to improve this understanding and to subsequently better process the radar observations in the ML."

2. Page 2, L 53: to my knowledge, the Swiss meteorology ofïňĄce has all the radars installed at high altitude, i.e. it copes with the altitude dilemma by choosing visibility over proximity to the ground. Is it right?

Yes, and this is the same for the French radar network. This is justified for the detection/monitoring of strong and localized convective events at the regional scale that are poorly sensed by conventional raingauge networks. However, we showed in a previous article that the Moucherotte radar perfoms its measurements within or above the ML in about 70% of cases of significant precipitation in Grenoble, with subsequent increased difficulties for QPE at ground level. See the following reference: A.K. KHANAL, G. DELRIEU, F. CAZENAVE and B. BOUDEVILLAIN, 2019. Radar Remote Sensing of Precipitation in High Mountains: Detection and Characterization of Melting Layer in the

Grenoble valley, French Alps, Atmosphere, 10, 784; doi:10.3390/atmos10120784

3. Page 6, L 173: please consider that in case of hail of cm size, δ can be very large at X-band.

Yes we are aware of this. No hail was reported for the convective cases considered. In addition we took great care in the revision to evidence and try to filter (with some success) the deltahv contaminations, e.g. new Fig.4 left.

4. Page 7, L 200: the clutter identiïňĄcation by means of HV should be interpreted as visual, or an algorithm is implemented to discriminate clutter from HV ?

See next point

5. Page 7, L 191: was this choice based on comparison with ground-based instruments?

No this choice is based on the ML statistics presented in Khanal et al. (2019) As detailed in new section 3.2, we flagged as noise all Φ_(dp)gates for which _hv < 0.95 for the XPORT rain case study and we determined the beginning and the end of the precipitation range considering a number of successive gates (10, i.e. e range extent of 342 m) for which this threshold was overpassed. Due to the noise affecting rhohv in the ML, we had to considerably lower this threshold (down to 0.8) for the ML case study. We have abandoned the idea of using a threshold on the reflectivity in the new version of our methodology

6. Page 6-7: is Kdp then simply estimated as gate-by-gate derivative from the clean Φdp, or an estimation method is used?

Yes, the Kdp profile is simply estimated as gate-by-gate derivative of the processed Φ_dpprofile.

7. Page 11, L 345: would it be possible to show the position of the 16 MRT targets on a map? Also, could it be clariïňĄed more in detail how those (gates? pixels?) have been

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chosen, and which are their statistical properties?

The way the targets are defined is now more detailed in the revision in section 3.1, with 2 additional tables and modification of the relevant figures

8. Section 4.2: this one is in my opinion the most interesting part of the manuscript. I would recommend to expand it, and to apply this methodology to many more precipitation events and aim at results based on a large dataset.

More details have been added to illustrate the methodology and the limitations of this preliminary case study. As explained in the head of the review, we are unfortunately not in position to extend it to other events right now. . .

9. Figure 4, please show all the polarimetric variables over the same range. For example the Ψdp proïňĄle is shorter than the ZH or HV proïňĄle. If a censoring is applied, please mention it in the caption and describe it in the text.

The Φ_dp profile processing / display is voluntarily restricted to the "rainy" region (r0, rM) free of close-range and mountain clutter while the other profiles include the mountain target. 10. Figure 5: please mind the overlapping labels on the y axis. corrected

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