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Atmospheric Measurement Techniques Discussions

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 #2 The manuscript discusses a methodology to investigate the relationship of the radar-derived PIA and the total differential phase in two different interesting precipitation regimes: rain and melting layer. I found the manuscript very well written and understandable and technically correct. That said, I feel that the manuscript lack of significant conclusions. I suggest for major revision. Main concerns.

1. The main messages to keep home for a reader seems to be i) apply a non-linear fit for k-Kdp relationship in rain to have an more unbiased estimation of PIA and ii) Melting layer attenuation can be estimated using a unique configuration that foresees the use of two radars optimally positioned in a Mountain environment. I find the first

finding not very new although useful, Yes but several publications (e.g. Testud et al. 2000, Schneebeli and Berne 2012) mention the existence of a linear relationship at X-band and the subsequent advantages in terms of QPE. Our preliminary findings seem to indicate that this is not the case and that the rain type may be an important factor controlling this relationship. whereas I find the second finding interesting although the measurement configuration is far to be generalizable. I think the Authors should add some more text where they discuss their results thinking to a practical-oriented use of their findings. For example, keeping in mind all the limitations recalled by the Authors, do you encourage the use of the parametrization introduced in figure 9 (blue curve) to a have a rough estimation of ML attenuation using a polarimetric radar? No because in this scatterplot are mixed pairs of estimates obtained from various layers of the melting layer. In the hypothesis of a linear relationship, new Fig. 13 (old Fig 11) is certainly more useful to describe the k-Kdp relationship and its variation within the ML.

2. I was surprised by the fact that having two radars operating at nearly the same frequency in a such interesting configuration, somehow one above and one below the ML, you didn't try to compare the reflectivity factors of the two to have a proxy of the ML attenuation. This is a good idea that was already explored by our Météo-France co-authors for the other Alpine X-band radars (within the RythMME project). See the following reference: Yu, N., Gaussiat, N., and Tabary, P.: Polarimetric XâĂŘband weather radars for quantitative precipitation estimation in mountainous regions. Q. J. Royal Meteorol. Soc., 144(717), DOI:10.1002/qj.3366, 2018. Note that an accurate calibration of the two radars is required and that the difference in the resolution volumes limit to some extent the interest of this approach. In addition, our colleague Nan Yu, started looking at the possibility to implement his method to the Grenoble configuration. He found out that many of the "common measurement gates" of the two radars were actually affected by side-lobe contamination for the MOUC radar.

3. Did you check the radar absolute calibration using DSD Parsivel data? The MOUC radar calibration was performed through the Météo-France standard electronic calibra-

tion 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.

4. MRT variability is never discussed in this manuscript. Do you think it can explain part of the variability in figure 8, y-axis? As requested by reviewer #1, we have added a lot of material in the revision regarding the mountain return stability and time variability. For the rain case, we believe this factor to be of very limited importance due to the very small variability of the mountain returns. More impact is likely for the ML case but we don't think this is a dominant source of error.

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