Review of the manuscript: On the relationship between total differential phase and path-integrated attenuation at X-band in an Alpine environment

By Delrieu et al.,

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 fist finding not very new although useful, 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 they 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?
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.
3. Did you check the radar absolute calibration using DSD Parsivel data?
4. MRT variability is never discussed in this manuscript. Do you think it can explain part of the variability in figure 8, y-axis?