Interactive comment on “Detection of the freezing level with polarimetric weather radar”

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1 Response to Anonymous Referee 3

The manuscript describes a technique to estimate the Freezing Level (FL) height, motivated by its practical use in downstream hydrological applications. The methods blend QVPs/VPs ideas as a convenient way to summarize the dual-polarization, vertical properties to inform this retrieval.
Overall, the manuscript accomplishes the application it sets out to perform. However, the effort seems limited in that it amends previous ideas with potentially questionable inputs. Such applications may be publishable within the scope of AMT, but this seems to require substantial edits (not a trivial re-write). The manuscript is long, yet not particularly organized in how it presents concepts, physical discussions. Most statements probably should be more conservative. It is not always clear what is original, or why this advancement matters? One radar advantage (somewhat lost with QVPs) is ability to capture FL variability spatially when compared to model output, surface, or radiosonde information. The manuscript claims originality from QVPs, but avoids when it is appropriate to use QVPs, e.g., important trade-offs for this decision. QVPs could be a smart substitution, but this choice encourages compensating errors. It is also not clear the outcome (i.e., FL estimates to match 0C Temperature) is the best target (i.e., 0C Wet Bulb Temperature)..

We thank the referee for the detailed review. The comments will be considered in the revised version of the paper. In the following, we provide below point-by-point answers (in italic) to the comments.

Major comments:

1. What accuracy does one require “FL” estimates, and is this important? What is the ‘value-added’, aka, why this specific approach? What is the advantage over existing ML ideas?

   (a) *The added value of our algorithm is its capability to detect the ML using data collected by operation weather data. Previous studies require the processing of data collected/generated by other instruments, or data not available in operational radar networks. We consider that this is helpful for radar data corrections that require the delimitation of the ML, and when running NWP models it is not a feasible option.*

2. QVPs are spatial averages that favor widespread precipitation, homogeneous fields. QVPs are clever, convenient, but one 'practical' issue is related to their generation –e.g., this requires more statements on the tolerance for 'when' (under what conditions) these are generated, e.g., 'how frequently' does this result in useful retrievals? What about 'edge case' QVPs that may be generated, but require filtering? Basically, how confident are we that all conditions that allow a QVP are also equally viable as inputs?

   (a) *We believe that a thorough review of the construction process and limitations of the QVPs is out of the scope of this work and probably the subject of a new paper. Nevertheless, during the design of our FL algorithm, and looking at a large number of QVPs, we proposed different thresholds and parameters in the algorithm that are useful to identify the ML signatures from QVPs. In fact, we analysed a larger number of QVPs (almost one year of data) to identify potential problems during the implementation of the algorithm. This helped us to develop a robust algorithm. The results presented in this paper only cover events where both radiosonde and radar data were available.*

3. QVP averaging removes ability to define regions of mixed precipitation (azimuthally) as one example issue common to FL/ML literature. The variability of the FL can be substantial, studies suggesting O[500 meters] – variability as large as the melting layer – and radar sectors where 'rain' switches to 'snow'. This argues QVPs are not suitably fine-grained, would struggle in locations where this is a concern, e.g., Boodoo et al. (2010).

   (a) *One of the main advantages of the QVP methodology is its ability to document the characteristics of the ML, as demonstrated by Griffin et al. (2018), Kaltenboeck and Ryzhkov (2017) or Ryzhkov et al. (2016). We are aware of the limitations of this methodology when large spatial variability of the FL is present in the PPI scans and this may only be mitigated using other inputs like data produced by numerical models, but previous studies e.g. (Hall et al., 2015) demonstrated the ability of radar measurements over numerical models to accurately detect the FL. That is why, we propose* *several conditions and parameters in our algorithm to only work with QVPs that are likely to contain the ML signatures.*

4. Melting onset is not at 0C temperature, rather at the 0CWet Bulb temperature. When viewing from radar, the height when one claims a melting response is typically lower, e.g., delay in measurement sensitivity to melting, but also the RH is often not 100%. A concern is if one becomes too interested in a retrieved 'match' to a radiosonde target that is not always correct. While the 0C temperature is the historical 'freezing level' definition, it is not the one (or only) hydrological applications care about associated with 'contaminated' radar signatures (e.g., 'bright band' shape also starts above with aggregation processes). This is partially why I suspect VP/velocity profiles are not as seeming useful in the offered, e.g., this is more a case of poor target/definitions than velocity not being a highly useful input.

    (a) *We relied on the idea that the radar rain measurements are related to events with relative humidity near 100%, that is why the algorithm was designed to match the Dry-bulb 0 °C measured by the radiosonde. Nevertheless, we agree with the reviewer on the necessity to analyse the relation between Dry-Bulb/Wet-Bulb temperature. We present an analysis through a year of radiosonde measurements (See Figure 1) and we found that, although the height is somewhat lower, the variation is not significant as theoretically, the Dry-Bulb and the Dry-Bulb temperatures are similar in the rain medium (as measured by the radar). We'll compare the outputs of the algorithm with this new information and it'll be included in the result Section.*

5. QVP-based dual-pol measurement profiles have different issues for interpretation; For example, ZDR profiles do not rapidly increase until onset of melting (0C Wet Bulb), whereas Z is increasing above the ML owing to aggregation. Unfortunately, where these signatures occur in altitude is complicated further when aggregation, melting are not the same spatially, then averaged in a QVP. The QVP issues are exacerbated when coupled with nonuniform beam-filling NBF issues that smear profiles. This calls into question concepts for 'combining' Z and RHOHV (or variants therein) – as in Wolfensberger et al. reference – as confusing when based on QVPs. It is not clear the order of operations, and how/when one averages, combines such fields. It makes a difference in the eventual input profile validity. Moreover, it may lead to solutions that 'work', but come to a matching answer for the wrong reasons.

    (a) *We agree with the reviewer that certain microphysical process fingerprints may be reduced in magnitude because of the averaging process on the construction of the QVPs, as showed by Kumjian et al. (2016). However, we consider that the ML fingerprints are*

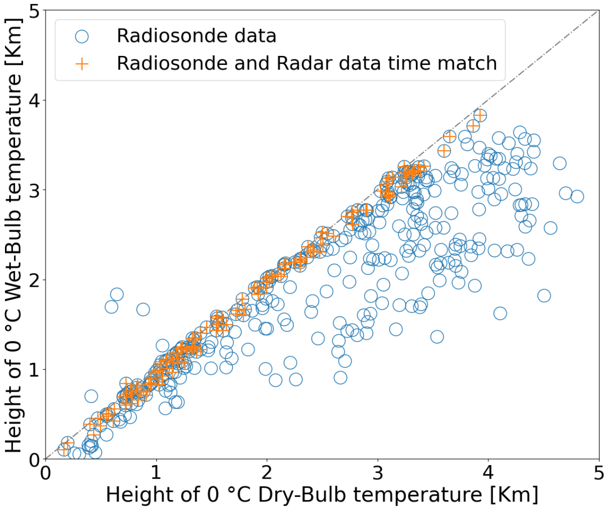

Figure 1: Heights of 0 Wet-bulb/Dry-bulb temperatures.

strong enough to surpass this limitation; this is why we analysed all the possible combination of polarimetric variables and compare them with the radiosonde data. The resultant profiles may not represent all the microphysical processes inside the profile, but a profile with enhanced ML signatures instead.

6. Effort is spent on explaining dual-polarization signatures (QVP), but the important process aspects are not too well described. Radar response to processes/properties (signatures) to include melting, aggregation, breakup/fallout, etc., are undoubtedly difficult. These processes and observed properties are smoothed/complicated further in response to known radar (system) bias, NBF, etc. Averaging and other processing details distort things further, esp. regions that preferentially feature different density or mass flux into these melting layers, RH, vertical motions, etc. There are a few resources for discussion on QVP signatures of the bright band (and reasons for its variability), e.g., Kumjian et al. (2016).
   I call attention to nonuniform beam filling NBF in particular, and melting onset expectations. Illustrations for potential offsets in radar quantities are found in Ryzhkov (2007). For intermediate tilts being used for QVPs, the expectation should be for modest biases in quantities owing to NBF (e.g., smearing) – This is complicated by the QVP averaging if the fields are not homogeneous. It is possible to model how well certain combinations of quantities may demonstrate compensating issues if one attempted to multiply those profiles, at different tilts, etc. – much of that would also arguably change on when those QVP averaging was performed (multiply before QVP, or QVP before multiply?). Again, it does not make sense (to

this reviewer) how Z and RHOHV fields can be multiplied to generate a useful profile without factoring in several details (thus, also not surprised it may not apply consistently well, either).

  (a) *We are aware of the microphysical processes related to rain events. Still, a detailed explanation of all of them based only on data collected from operational weather radar is beyond the scope of this paper. This task requires data collected from research radars with higher resolution that improves the rain process's understanding in a microphysical level. Instead, we describe the observed signatures related to the ML only, based on a long-term analysis of QVPs and VPs, as they are the algorithm's foundation. On the other hand, the NBF effect was analysed, and a threshold in the range is proposed to mitigate their effects. Moreover, the normalisation process and the algorithm's design help to mitigate the effects of the beam broadening, as the algorithm does not detect the ML using quantitative values of polarimetric variables, but the strong gradients that the ML generate in the profiles. This is why we consider that the rationale behind the algorithm's design is justified: by multiplying normalised profiles, we want to generate a new profile with enhanced gradients related to the ML, and to some extent, wash-out other peaks that difficult the implementation of a peak-finder algorithm.*

Minor comments:

1. The 'freezing level' is a poor term, persists in operations. Perhaps 'melting level', as frozen media begins to melt at that level.

   (a) *We agree with the reviewer, "Detection of the melting layer with polarimetric weather radar" could be a more appropriate title for the paper. We'll make clear the differences into the manuscript.*

2. The authors use examples for the QVP, VP profiles in several figures. Critically, I find these examples often physically nonintuitive, even when the authors imply these as only meant as examples. For example, one expects the Z peak to be higher in altitude (above) of the ZDR peak, with the ZDR and RHOHV peaks located at similar altitudes. If the authors retain the physical discussions on the dual-polarization signatures, the reasons for such relative behaviors are perhaps more important. These are also far less commonly described.

   (a) *As stated in the discussion section, the VPs and QVPs sometimes do not agree with previous studies based in RHI scans or theoretical profiles, but we will present a more detailed explanation of the behaviour of the profiles.*

3. I had an impression velocity gradient ideas were being presented as novel/unique. The authors should likely consult the profiling radar literature (e.g., works of C. Williams, other profiling radar echo classification manuscripts) that commonly use gradients of mean Doppler velocity in their efforts. As above, I suspect velocity is more accurate / informative profile input (when available) for assessing the wet bulb zero for reasons of its improved vertical resolution and sensitivity to its relative 'change point' with melting onset. I suspect open-code / python change point / inflection techniques would also apply vertically as compared to gradient ideas, too.
    (a) *We are aware of the work related to the profiling radar literature, e.g. (Tian et al., 2019; Williams et al., 1995, 2005, 2007). Still, we consider that the velocity gradient was not used as an input variable to delimit the melting layer. We'll appreciate it if the author could provide more references on this matter. On the other hand, even though that we are aware of the use of inflexion techniques and the use of the Velocity as an input of some algorithms (we even made some experiments using "raw" velocity profiles) we conclude and demonstrate that the use of the second derivative (or in this case, its complement $1 - gradV$) fits better into the design of our algorithm.*

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