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Interactive comment on "Detection of the freezing level with polarimetric weather radar" *by* Daniel Sanchez-Rivas and Miguel Angel Rico-Ramirez

Anonymous Referee #3

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Reviewer Comments on "Detection of the freezing level with polarimetric weather radar", by Daniel Sanchez-Rivas, and Miguel A Rico-Ramirez, AMT-2020-375.

Overview Comments:

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

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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).

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?

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?

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).

4) Melting onset is not at 0C temperature, rather at the 0C Wet Bulb temperature. When

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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.

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.

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, break-up/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).

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.

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.

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.

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