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

Daniel Sanchez-Rivas and Miguel Angel Rico-Ramirez

November 2020

1 Response to Anonymous Referee 1

This paper describes a melting layer detection technique from vertical Profiles (VP) and quasi-vertical profiles (QVP) from polarimetric radar observations. Examples are given from a C-band operational weather radar in SE England. Apart from Zh, Zdr, phi_dp, and rho_hv, the technique includes mean Doppler velocity and the gradient of the vertical Doppler velocity. The paper can be published in AMT but it needs to be written in a more coherent manner. Sentences don’t follow each other in some cases, and more clarification is needed in some cases.

We thank the reviewer for the insightful review of the manuscript. We modified the manuscript through a careful review of the language. Please note that considering the reviewers’ comments, we modified the paper title to ‘Detection of the melting level with polarimetric weather radar’. In the following, we address all their point-by-point comments in blue, outlining our response and how we modified the manuscript. The changes refer to the marked-up version of the manuscript.

1. At the end of Intro, insert a paragraph outlining what this paper is trying to achieve and how the paper is structured
    (a) *The requested paragraph was added in Lines 133-138.*
2. Line 95: By Doppler velocity, do they mean the mean radial component?
    (a) *Corrected in Line 152.*
3. Line 115: What does ’visible signatures’ mean? Can you quantify?
    (a) *We rephrased this statement in Lines 203.*

4. Line 128: The authors say “Based on the profiles of vertical velocity [V], we propose a new variable: [gradV]..” - What about spectral width? Is this available from routine scans?
   (a) *The Spectral width variable was not available in the analysed radar datasets. We added an statement about this in Line 192.*

5. Figure 2: For the VP plots on the left side, the y-axis should go from 0 to 8 km to be consistent with the QVP plots. What about panel (j)? Why is the 0 to 1 km omitted?
   (a) *As described on Lines 188-190, data collected at vertical incidence is contaminated by spurious echoes. Still, we modified the plot so the y-axis is consistent on both sides, enabling a straightforward comparison.*

6. Line 144: Define 'normalised' at this point.
   (a) *We added further information in Lines 222-223.*

7. Line 147: should 'estimate' be 'detect'?
   (a) *Agreed and corrected.*

8. Line 148: What does 'enhancements that the ML bring-up into the variables' mean?
   (a) *We modified the text as outlined in Line 229-230.*

9. Line 154: By 'elevation' do they mean 'altitude a.g.l'?
   (a) *Corrected in Line 188.*

10. Lines 156-159: Grammar needs to be improved, and also the text is ambiguous; the sentence doesn't make much sense.
    (a) *The text was modified to improve its readability, as shown in Lines 242-246.*

11. Lines 163: convective events are associated with different microphysical processes so ML doesn't apply.
    (a) *Agreed and corrected in Line 254.*

12. Line 168: Doesn't the radar perform 'bird-bath' scans routinely?
    (a) *Yes, the bird-bath scans are the VPs. Please note that in Lines 277-278 we explained the need to know first the height of the melting level (ML) to apply a bias-correction to* Z_{DR}.

13. Line 168: The sentence beginning 'Hence the Zdr ..' requires much more clarification.

    (a) *We added further explanation about this variable in Lines 273-276.*

14. Section 3 is verbose, not very technical and not well-written at all. Please rewrite. Also explain clearly why the peaks in Z_H, Z_{DR} and ρ_{HV} are at different heights above ground level and explain the difference between BB and ML.

    (a) *We rewrote Section 3 to improve its readability. Also, we added further explanation and discussion regarding the peak heights in Lines 235-241. As mentioned, the difference between the profiles generated from our datasets and previous studies relies on the type of profiles and the average process for constructing the profiles. This can be seen in Revision Figures 1 and 2. The difference between melting level, melting layer and the bright band is now explained in Lines 19-25.*

15. Line 237: Once again, explicitly say how the normalisation is performed.

    (a) *The normalisation process is described in Lines 347-352.*

16. Explain how equations (2) and (3) were derived. If published elsewhere, then insert reference for the derivations.

    (a) *Clarification about the derivation process of Equations (2) and (3) is provided in Lines 388.*

17. Line 267: Explain/justify why the second derivative was chosen.

    (a) *Please note that this is discussed in lines 434-441, 490-491 and illustrated in Figure 5b.*

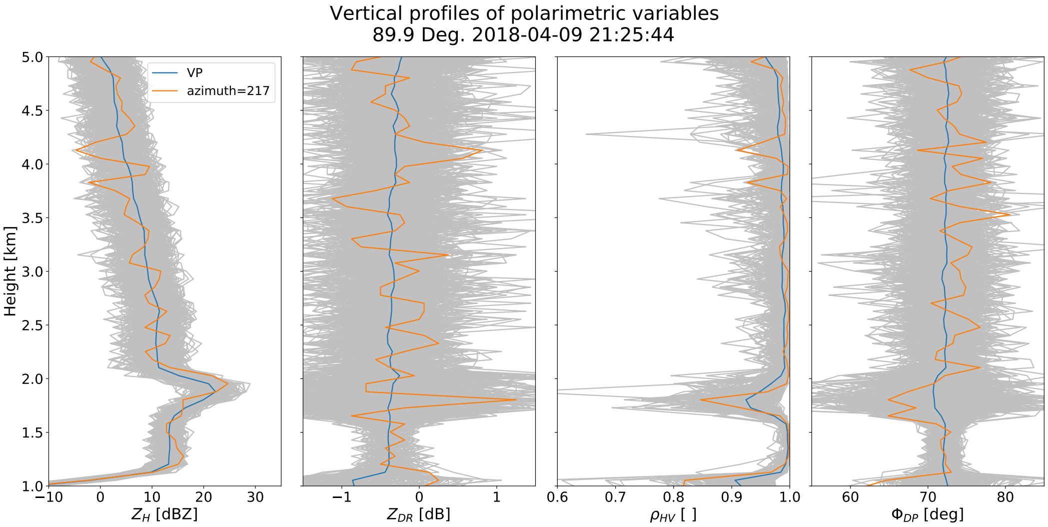
18. Line 293: "QVPs and VPs of Zh, as these variables measure similar properties of the raindrops" What does this mean?

    (a) *We added further explanation about this variable in Lines 503-505.*

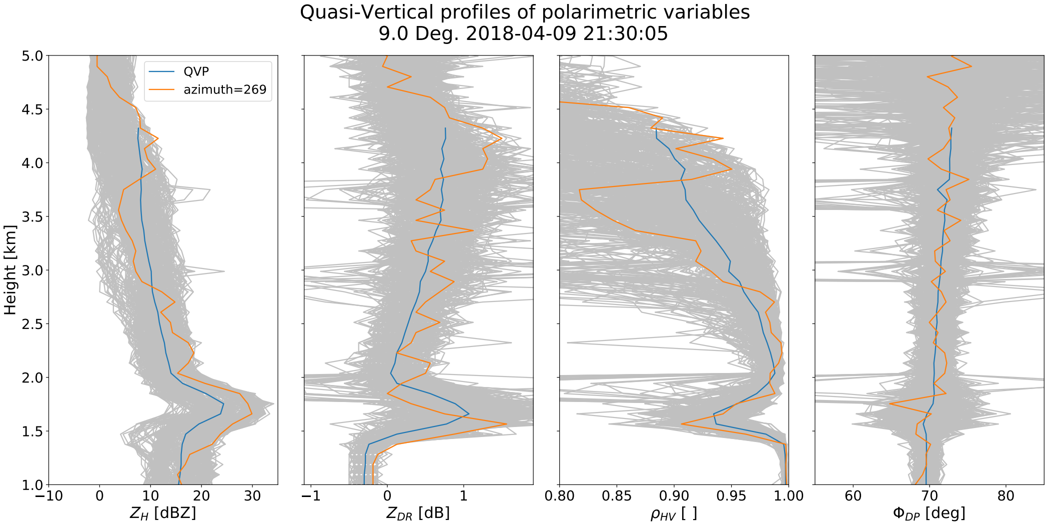
19. Line 303: What does "resides on relative low values of reflectivity" mean?

    (a) *We rewrote this statement as outlined in Lines 516-518.*

20. What is the purpose of Section 5.1 if only the Z comparisons are given? It's not clear how it is relevant to the rest of the paper.



Revision Figure 1: VPs comparison regarding peak heights.



Revision Figure 2: QVPs comparison regarding peak heights.

   (a) *This section is intended as a validation of the reflectivity QVPs. We believe it is necessary to assess the consistency between VPs and QVPs as the ML algorithm is based on the geometry of the profiles. But apart from Z_H, it is not possible to compare the figures of the other polarimetric variables due to the azimuthal averaging on the construction of the QVPs.*

21. Regarding Fig. 9: What does 'FL estimated' represent exactly, that is in relation to the radar BB (peaks in all the variables), and the 0 deg C isotherm level?

    (a) *At this point, we compared heights of the* 0 °C *Wet-Bulb isotherms and the output of the algorithm (i.e. the top boundary in the enhanced profile) described in step 2.d (lines 424-427). We modified Figures 9 and 12 to clarify the scatter plot.*

22. What about attenuation corrections needed for Zh and Zdr? Were these applied?

    (a) *We are aware that rain attenuation is an error source for radar QPE in particular when using low-elevation scans and this is why the height of the melting level is essential to implement rain attenuation correction algorithms. For most of the PPI scans used in this analysis (90-deg scans and higher elevation scans at 9-deg elevation) rain attenuation at C-band was relatively small as demonstrated by the total differential phase shift. This is because the rain region for most of the profiles was below 3km in altitude a.g.l., which is equivalent to about 20km in range when using the 9-deg elevation scans. For this reason no attempt was made to correct for attenuation. This is now clarified in the paper in Lines 198-201.*

2 Response to Anonymous Referee 2

This study proposes an approach to estimate the freezing level (FL) using vertical/quasi-vertical profiles (VP/QVP) achieved from polarimetric radar observations. The proposed approach was applied to some selected events, and the estimated FLs were evaluated using radiosonde observations. Based on the evaluation results, the authors concluded that the combinations of ZH, ρ_{HV}, and the gradient of the velocity V, and ZH, ρ_{HV}, and ZDR for each VP and QVP method are the best predictors for the FL estimation. I think that the study was well-designed, and the focus and experimental details and results of the study are clearly addressed in the manuscript. However, I have a basic question about the utility of this study for radar QPE and additional comments/suggestions for some other aspects presented in this study.

We thank the reviewer for the positive remarks and for the interesting feedback/discussion that helped to improve our work. Please note that considering the reviewers' comments, we modified the paper title to 'Detection of the melting level with polarimetric weather radar'. We modified the manuscript as outlined below, replying point by point in blue. The changes refer to the marked-up version of the manuscript.

Major comments:

1. Utility of FL height. The authors discuss the necessity of FL information for radar-based applications (e.g., QPE) in Introduction. In my opinion, what is really useful for radar applications is to provide a range of the melting layer (ML), not just a single value of FL height itself (as this study mostly devoted to find the FL height) because mixed (liquid-solid) precipitation is usually located below the FL height, and this is a significant challenge e.g. for rainfall and attenuation estimation. I am wondering what specific applications require the estimated FL height. I think that a bottom height of the ML presented in Figures 10 and 13 is much more useful than the FL height itself because the majority of scattering and propagation theories can be applied only to the region below this height (liquid precipitation or pure rain region).
    (a) *We completely agree with the reviewer on the importance of accurate detection of the bottom of the melting layer as most of the QPE algorithms can only be applied in the rain region. Unfortunately, if the output of the algorithm is the bottom of the melting layer, it would be challenging to validate it using the radiosonde datasets or some other instrument. Hence, the proposed algorithm detects both the ML and the bottom of the melting layer based on the geometry of the profiles and the ML is validated using radiosonde data. Then, the bottom of the melting layer can be determined using a fixed thickness or by using the output of the algorithm, as shown in Figures 10 and 13.*

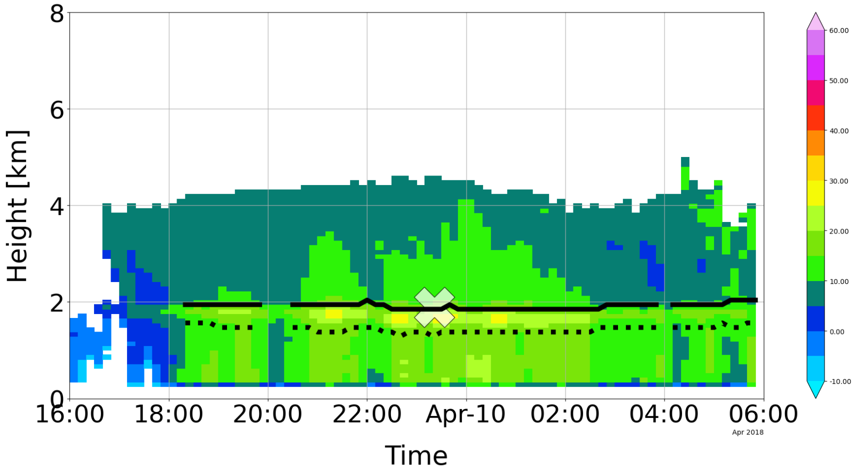
2. QVP. It is not clear if either time-averaged or instantaneous QVP is used in the proposed FL detection algorithm. I think that instantaneous QVP is not appropriate for the proposed algorithm because it could be affected by local storm structures (although it is derived from higher elevation angles) particularly for the ones near the radar. If the authors used time-averaged QVPs, they need to clarify it and define the averaging time window. It might be helpful for readers to understand the QVP method if the authors provide a brief description on the background and procedures to retrieve QVP from radar observations, rather than just referring to Ryzhkov et al. (2016).

    (a) *We are aware of the advantages of using time-averaged QVPs, and we did some tests using time-averaged QVPs. The algorithm considers this situation with the parameter k, which is helpful to deal with the smoothness caused by the time-averaging of the profiles, e.g. in Revision Figure 4, the profiles are averaged using a time-window of 30 minutes, and the parameter k is modified to allow the different values of the resulting profiles. The melting layer detected do not vary that much (see Revision Figures 3 and 4). Hence, we decided to display examples in the instantaneous QVPs format at this is the most common format of QVPs. We added further discussion about this Lines 602-609.*

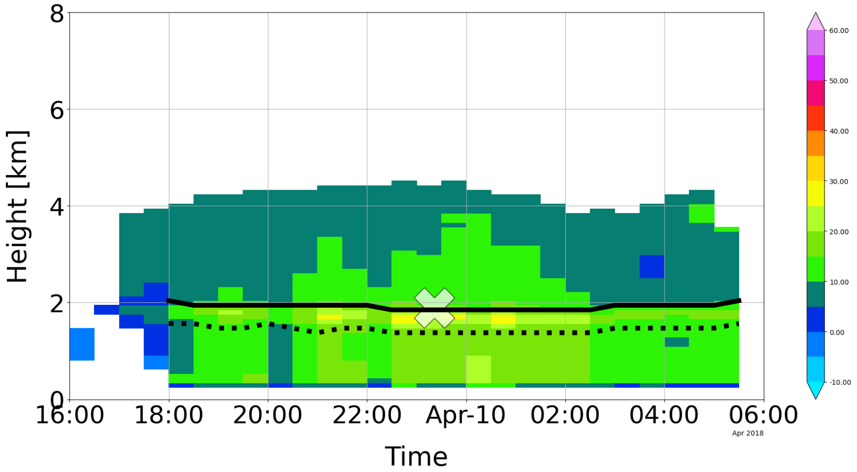
3. FL spatial variability. I think that the proposed QVP method results in the average FL over the entire radar domain while the VP method yields limited FL to the radar site (if VP was obtained from a 90 degree elevation angle). I am wondering how the spatial variability of FL over the radar domain looks like, and the authors may compare the FL information retrieved from the NWP model with the one achieved from this study. It might be helpful to discuss this spatial variability issue in the discussion section as a limitation of this approach.

    (a) *A strong motivation for this work was to avoid relying on NWP products. One of the advantages of the algorithm is that it enables the estimation of the ML based entirely on the radar data; this is really helpful to implement corrections that depend on hydrometeor discrimination. We agree with the reviewer that there is a spatial variability of the ML over the radar domain. Still, after weighing the options, we considered that for the ML accuracy required in radar corrections, a straightforward algorithm and its validation using radiosonde surpass the complexity of data retrieved from numerical models and its computationally expensive runs, as showed by Hall et al. (2015) or Mittermaier and Illingworth (2003). We discussed the ML spatial variability in Lines 609-615.*

4. Error analysis. Whereas the analyses presented in this study focused on finding the best predictors of the polarimetric radar observations, it is



Revision Figure 3: Instantaneous QVPs and detected melting layer, related to a stratiform-type rain event.



Revision Figure 4: Time-averaged QVPs and detected melting layer, related to a stratiform-type rain event.

valuable to characterize the structure of errors resulted from the proposed methods. I think that it would be useful to demonstrate error distributions of each VP and QVP method (e.g., P14 and P26), rather than just reporting "the errors in the FL estimation using either VPs or QVPs are within 250m."

   (a) *We provide an error analysis in Figure 9 and 12 along with discussion in Section 6.*

Minor comments:

1. Line 10 Maybe better to remove "extremely."
   (a) *Corrected.*
2. Line 24-26 It would be interesting to compare the FL heights computed from between this study and the NWP model.
   (a) *Please refer to the answer of major comment No. 3.*
3. Line 87 Please define "UKMO."
   (a) *Corrected, UKMO refers to the UK Met Office.*
4. Line 106 Please replace "twice daily" with "twice a day."
   (a) *Corrected.*
5. Table 1 I think that the "Location" in Table 1 represents coordinates on a certain projected coordinate system. Geographic coordinates are more common and please provide latitude and longitude of the radar site.
   (a) *Agreed and corrected. Figure 1 and Table 1 are now in geographic coordinates.*
6. Figure 2 Please use consistent height (y-axis) and color scales for the same radar observables to enable easy comparisons between left and right panels for (a)–(h). Please also define "HTI" in the figure caption.
   (a) *Figure 2 was updated with consistent y-axis and similar color scales as requested.*
7. Line 144 Please clarify if QVPs shown in Figure 3 were time-averaged before they were normalized.
   (a) *Please check the answer provided to major comment No. 2.*
8. Line 181-182 How are "type of precipitation" and "phase of the hydrometeors" different?

   (a) *We corrected this statement in Lines 279-282.*

9. Line 278 It turned out that “magnitude (k) of Ppeak” was a threshold (e.g., parameter) for peak magnitude (Line 287). Please clarify it here.

   (a) *We correct this aspect of the algorithm and clarify it throughout the manuscript.*

10. Line 291-294 Something is missing. Please rewrite.

    (a) *We rewrote this subsection to improve its readability.*

11. Line 300 Why do the authors compare VPs and QVPs? Is this comparison performed because the authors used instantaneous QVPs for FL estimation? I think that they (VP and QVP) are not necessarily consistent, and QVP should be used with timeaveraging to avoid local storm effects and capture the consistent vertical structure with VP.

    (a) *This section is somewhat intended as a validation of the construction of the QVPs. We consider it necessary to assess the consistency between both types of representations as the ML algorithm is based on the geometry of the profiles. But apart from* Z_H*, it is not possible to compare the figures of the polarimetric variables due to the azimuthal averaging on the construction of the QVPs. Please refer to Lines 602-609 for a detailed discussion on this matter.*

12. Section 5.2 This section does not describe the result of this study and should be moved to the “Methodology” section.

    (a) *Agreed and corrected.*

13. Line 352 Please replace “better” with “best.”

    (a) *Corrected.*

14. Line 358 Why P16? Both ZH and [grad V] are the elements of P26. [grad V] was used for P16–P31, not just for P16.

    (a) *The purpose of showing the performance of* P_{16} *in Figures 10c and 10d was to emphasise the value of the proposed variable* $gradV$*. We removed this analysis in the revised version of the manuscript.*

15. Line 359 Figures 10a and 10b instead of “Figures 13a and 13b?”

    (a) *We appreciate this observation, references are now correct.*

16. Line 361-362 The estimation procedure of the ML bottom was not described.

   (a) *We added further description regarding the ML bottom detection in Lines 424-427 and in Figure 5b.*

17. Line 383 Please replace "better" with "best."

    (a) *Corrected.*

18. Line 386 Why P10? P10 does not have to be mentioned here because the two factors shown in Figure 13 are also included in P14.

    (a) *We used the variable* P_{10} *to compare the different outputs of the algorithm. However, we removed this analysis in the revised version of the manuscript.*

19. Line 404-407 I think that the ZDR calibration bias is not an issue in this study because relative ZDR values (e.g., normalized) are used to construct vertical profiles. ZH also contains the calibration issue.

    (a) *We agree with the reviewer, the calibration in both,* Z_H *and* Z_{DR} *are not an issue when implementing the proposed ML identification algorithm. However, if we want to use* Z_{DR} *quantitatively, then we must ensure* Z_{DR} *is calibrated. Hence the necessity of the knowledge of the ML before the implementation of the* Z_{DR} *calibration procedure. On the other hand, please note that* Z_H *is routinely calibrated by the UK Met Office.*

3 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 were considered for the revised version of the paper. Please note that considering the reviewers' comments, we modified the paper title to 'Detection of the melting level with polarimetric weather radar'. In the following, we provide below point-by-point answers (in blue) 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 accuracy of ML estimates depends on the application. For instance, Kitchen et al. (1994) quoted 200m as the required accuracy in the ML height for VPR correction, whereas for rain attenuation correction the accuracy in the ML height could be lower (see Islam et al. (2014)). The added value of our algorithm is its capability to detect the ML height and at a certain degree, the melting layer, using data collected by operational weather radars. Previous studies require the processing of data collected/generated by other instruments, or data not available in operational radar networks. We believe that the proposed ML algorithm is helpful for radar data corrections that require the knowledge of the ML or the boundaries* *of the ML, and when running NWP models it is not a feasible option.*

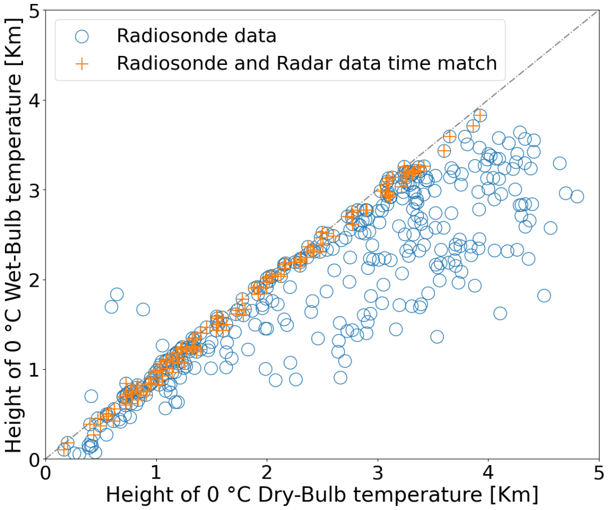
2. QVPs are spatial averages that favor widespread precipitation, homogeneous fields. QVPs are clever, convenient, but one 'practical' issue is related to their generation –e.g., this requires more statements on the tolerance for 'when' (under what conditions) these are generated, e.g., 'how frequently' 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 ML 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. In the revised version of the paper, we added discussion on this matter in Lines 602-622*

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 ML 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 ML. Our algorithm only works with QVPs that are likely to contain ML signatures if the conditions outlined in the paper are satisfied. Please refer to Lines 602-622 for a detailed discussion of the ML spatial variability. Our goal is not to produce estimates of ML heights at every azimuth angle given the variability of the precipitation and noise in the radar measurements.*

The QVPs smooth out to some extent this variability and the multiplication of the normalised profiles ensures that the ML estimation is robust as demonstrated when validating the ML estimates with radiosonde data.

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. In Revision Figure 5) we present an analysis through a year of radiosonde measurements and we found that, although the height is somewhat lower, the variation is not significant as theoretically, the Dry-Bulb and the Wet-Bulb temperatures are similar in the rain medium (as measured by the radar). Furthermore, we updated the error analysis shown in Figures 8-13 using* 0 °C *Wet-Bulb isotherms heights.*

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*



Revision Figure 5: Heights of 0 Wet-bulb/Dry-bulb temperatures.

process on the construction of the QVPs, as showed by Kumjian et al. (2016). However, we consider that the ML fingerprints are 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. Also, note that an altitude constrain was defined in the algorithm to minimise the effects of these problems, i.e. the algorithm is constrained to a height of 5 kilometres, and the QVPs are constructed from relative high elevation angles, in this case, for 9-deg scans, the height of the centre of the beam is similar to 30 km in range, which is relatively close to the radar and minimise the NBF problem.

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).

   (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 level with polarimetric weather radar" could be a more appropriate title for the paper. We added further discussion on differences between these terms in 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) *Please note that the behaviours described by the reviewer are mainly related to profiles extracted from individual slant ranges. Similar profiles can also be observed in our datasets, e.g. in Revision Figures 1 and 2 the peak in Z_H is higher in altitude than the peaks of Z_{DR} and ρ_{HV} for the azimuthal profiles (orange lines). However, due to the averaging process carried out its construction, the peaks' height of VPs and QVPs (blue lines) show behaviours that differ from previous studies based on RHI scans or theoretical profiles. We provide an explanation of these profiles in Sections 3 and 6.*

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.*

References

Griffin, E. M., Schuur, T. J., and Ryzhkov, A. V. (2018). A polarimetric analysis of ice microphysical processes in snow, using quasi-vertical profiles. *Journal of Applied Meteorology and Climatology*, 57(1):31–50.

Hall, W., Rico-Ramirez, M. A., and Krämer, S. (2015). Classification and correction of the bright band using an operational C-band polarimetric radar. *Journal of Hydrology*, 531:248–258.

Islam, T., Rico-Ramirez, M. A., Han, D., and Srivastava, P. K. (2014). Sensitivity associated with bright band/melting layer location on radar reflectivity correction for attenuation at C-band using differential propagation phase measurements. *Atmospheric Research*, 135-136:143–158.

Kaltenboeck, R. and Ryzhkov, A. (2017). A freezing rain storm explored with a C-band polarimetric weather radar using the QVP methodology. *Meteorologische Zeitschrift*, 26(2):207–222.

Kitchen, M., Brown, R., and Davies, A. G. (1994). Real-time correction of weather radar data for the effects of bright band, range and orographic growth in widespread precipitation. *Quarterly Journal of the Royal Meteorological Society*, 120(519):1231–1254.

Kumjian, M. R., Mishra, S., Giangrande, S. E., Toto, T., Ryzhkov, A. V., and Bansemer, A. (2016). Polarimetric radar and aircraft observations of saggy bright bands during mc3e. *Journal of Geophysical Research: Atmospheres*, 121(7):3584–3607.

Mittermaier, M. P. and Illingworth, A. J. (2003). Comparison of model-derived and radar-observed freezing-level heights: Implications for vertical reflectivity profile-correction schemes. *Quarterly Journal of the Royal Meteorological Society*, 129(587 PART A):83–95.

Ryzhkov, A., Zhang, P., Reeves, H., Kumjian, M., Tschallener, T., Trömel, S., and Simmer, C. (2016). Quasi-Vertical Profiles—A New Way to Look at Polarimetric Radar Data. *Journal of Atmospheric and Oceanic Technology*, 33(3):551–562.

Tian, J., Dong, X., Xi, B., Williams, C. R., and Wu, P. (2019). Estimation of liquid water path below the melting layer in stratiform precipitation systems using radar measurements during MC3E. *Atmospheric Measurement Techniques*, 12(7):3743–3759.

Williams, C. R., Ecklund, W. L., and Gage, K. S. (01 Oct. 1995). Classification of precipitating clouds in the tropics using 915-mhz wind profilers. *Journal of Atmospheric and Oceanic Technology*, 12(5):996 – 1012.

Williams, C. R., Gage, K. S., Clark, W., and Kucera, P. (01 Jul. 2005). Monitoring the reflectivity calibration of a scanning radar using a profiling radar and a disdrometer. *Journal of Atmospheric and Oceanic Technology*, 22(7):1004 – 1018.

Williams, C. R., White, A. B., Gage, K. S., and Ralph, F. M. (01 May. 2007). Vertical structure of precipitation and related microphysics observed by noaa profilers and trmm during name 2004. *Journal of Climate*, 20(9):1693 – 1712.