Authors response on ”Calibration of radar differential reflectivity using quasi-vertical profiles”

Daniel Sanchez-Rivas and Miguel Angel Rico-Ramirez

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1 Response to Anonymous Referee 1

We thank the reviewer for the insightful review of the manuscript and the interesting feedback that helped us improve our work. The comments were considered for the revised version of the paper. Please note that we made several changes to the manuscript to describe and address some flaws of the proposed method. In the following, we provide below point-by-point answers (in blue) to the comments; the changes refer to the marked-up version of the manuscript.

General Comments:

This manuscript offers a method for monitoring the ZDR offset of a dual-polarization radar using quasi-vertical profiles (QVP). The method is applied on C-band weather radars in light rain media. The authors suggest accuracy to O[0.1 dB], e.g., potentially in-line with 'bird-bath' calibration (natural media). There are two apparent justifications for this publication: **its improvement compared to previous natural media efforts**, and its QVP application towards these ideas.

1. *Please note that we corrected the statements regarding the accuracy of the proposed method throughout the revised version of the manuscript. Also, note that we are not suggesting that our approach is better or should replace the existing calibration methods based on intrinsic values of natural targets. As with any other method, ours faces different advantages and disadvantages, which are now discussed in Lines 465-583.*

The manuscript is not recommended for publication. The study is functional with elements similar to the typical AMT scope, but the reviewer finds **low value in the 'new' concept/application**. The use of intrinsic liquid properties for ZDR monitoring is well known, origins in low angles and selective ZDR averages (i.e., cell peripheries). **This manuscript adds a "QVP"-wrapper aimed now at liquid media, yet lacks the physical underpinning as to why such methods would improve performance over a boilerplate practice to 'average ZDR in light rain'. These "QVP" concepts are evaluated against a modest dataset, but reads to the reviewer as motivated by convenience and applying a poorly-matched "QVP" concept** (hammer looking for a nail?) in a less-behaved condition (light rain) to be 'novel'. Yet, it seems **a straightforward evaluation of an existing snow QVP application (as less original as that seems) may have been far less controversial.** The authors perhaps unintentionally increased their degree of difficulty (at least, to this reviewer), by leaving the reviewer questioning whether simpler, quicker, or (existing dry snow) options for targeted averaging may be equally/more effective.

2. *We consider that the main contribution of our method is that it reviews the capabilities of QVPs built from low elevation scans to compute the Z_{DR} offset. We are aware that there are other methods based on measurements taken in liquid precipitation, like the one proposed by Bechini et al. (2008) based on Z_{DR} averages on the cell peripheries or the one proposed by Gorgucci et al. (1999) based on vertical observations. Although these are great options and should be used where possible, we consider that we have demonstrated that it is possible to exploit the practicality of QVPs to detect the Z_{DR} offset.*

3. *Please note that we added more examples in the manuscript to demonstrate the efficacy of the proposed method; we consider that this address the problem in giving the impression of hand-picking the evaluation events (see Figure 11 for example).*

4. *In this revised version of the manuscript (Lines 492-509), we provide a discussion on why using dry snow as the target to detect the Z_{DR} offset in low elevation QVPs do not yield acceptable results. Furthermore, we now review the limitations of our method in the discussion section (pages 21-26).*

Moreover, a central claim for this effort seems to follow its **'relative' calibration performance (oversold), esp. for "light rain".** It is unlikely any 'natural' method can genuinely guarantee accuracy better than 0.2-0.3 dB – this has been well-argued by previous authors, including several cited; Prior efforts were rightfully cautious in their claims. Yes, some allowance can also be extended to older studies that are occasionally captives to their moment (i.e., radar technology improves with time → better ability to target lighter rain, etc.). Nevertheless, the intrinsic "light rain" variability is significant and comes in many forms (not limited to):

- Capabilities to provide 'ground truth' (e.g., disdrometers as a poor light rain reference);
- What gets defined as 'light rain' (regional / physical process variability),
- How one identifies these regions with existing radar (Z calibration, etc.), and Location, radar sensitivity/quality, other vertical profile factors (e.g., evaporation, sorting, process) that undermine accuracy claims when averaging over regions.

5. *We agree with the reviewer on the point that we performed a relative evaluation against the VPs method. This is now clearly stated in the revised version of the manuscript (see Lines 347-350 for example). However, we consider that the long-term comparison between the proposed method and the disdrometer data confirms the validity of our approach. We observed reasonable agreement between our results and the disdrometer data, considering the well-known discrepancy when comparing radar data to a fixed point location, as shown in Figures 8-11.*

For this reviewer,the authors have not demonstrated they built a better mousetrap. The reviewer understands there is an inevitable overconfidence (aka, marketing) in most manuscripts. However, "relative", not absolute calibration concepts are typically quite conservative, and it should be obvious that selective performance may be better under ideal conditions. The authors' disdrometer image (Figure 5) alludes to some inherent variability in (surface, 'instantaneous') **ZDR properties in "light rain" (aka, dynamic range of intrinsic ZDR > 0.6 dB)**. These depictions are consistent with discussions by Bechini et al., Ryzhkov et al., for what those authors expect from "light rain", or why "light rain" (generic) is less suitable than "dry snow" (see also, specific comment). Select locations (UK) experience different bulk microphysical expectations (e.g., propensity for widespread rainfall, stratocumulus), thus **performances may reflect strong local process / natural advantages (e.g., contrast with "light rain" at the peripheries of thunderstorms)**.

6. *We agree with the reviewer on the point that the inherent variability of Z_{DR} in light rain adds some uncertainty to our method. Hence, we proposed a constraint* $(0 < Z_H < 20)$ *in an effort to reduce the variability of Z_{DR}. This decision was made based on theoretical values of $Z_H - Z_{DR}$ expected in real storm events (see Figure 4(a)). This is now described in Lines 513-523. Moreover, Figures 7-8 show that this constraint produces good results compared to the traditional method based on VPs, in which the Z_{DR} variability is close to 0 dB due to the circular shape of the raindrops when the radar antenna is rotating about the vertical and averaging over full cycles of $360°$ (Gorgucci et al., 1999).*

7. *Please see the reply given in point 4 on why we discarded the use of natural targets with no Z_{DR} variability like dry snow to detect the Z_{DR} offset using low elevation QVPs.*

8. *We consider that the performance of our method is not contingent on UK local processes, but on the presence of stratiform light rain events instead. Yet, in Lines 593-596 we set the absence of stratiform light rain as a limitation of our method.*

Overall, one takeaway message is that this reviewer does not feel the authors have justified the "QVP" application as a genuine improvement over a generic "average" ZDR monitoring practice, for rain, snow or otherwise. Rather, **the reviewer claim may be that "QVPs" in light rain are arguably far worse**, given this form of averaging enables mixtures of less suitable profile properties that produce apparently viable "light rain" profiles. Why use a "QVP" process at all? Fundamentally, this is a reduction of information; Many previous studies speak to physical 'profile' issues convolved with "QVPs" and similar averaging, with even the QVP originators shifting to "CVPs" or other targeted averages – For example, ZDR should naturally evolve below the melting layer in response to processes such as sorting, evaporation, break-up, and/or other regime-averaging nuances (within event, or tropical vs midlatitude differences). This all points to why previous studies may have remained cautious in their claims on relative 'light rain' use and uncertainty, but also where QVP-ideas are suboptimal (esp. in rain, below cloud, etc.). The reviewer is questioning the need in using a QVP in these contexts if the QVP cannot be justified as out-performing any number of simpler, targeted ZDR averages of 'light rain' (if one is already thresholding regions loosely on Z, RHV regardless, you've already opened that echo classification bag once one introduced decision-tree thresholding for 'drizzle', etc).

9. *We agree that the inherent averaging process in the QVP construction may wash out some key microphysical processes within the precipitation events. However, Ryzhkov et al. (2016) demonstrated the usability of QVPs in radar meteorology, and in particular, to monitor the calibration of Z_{DR}. We consider that limiting the QVPs until a specific range/height and several other constraints enable the detection of suitable QVPs that capture the Z_{DR} offset. This is somewhat reflected in the relation of VPs/QVPs that meet the "stratiform light rain" criteria and are included in the analysis, as stated in Lines 369-375 and shown in Figures 5 and 6. As expected, the number of valid VPs is larger than the number of valid QVPs. However, we consider that such QVPs are capable of detecting the Z_{DR} offset.*

Specific Comment:

Why do the authors use "light rain" for the "QVP"? Many efforts point to why they avoid light rain (see, Ryzhkov et al, discussions). Unfortunately, **the reviewer might have been more amenable to an AMT manuscript that was simply a long-term validation for an existing 'dry snow' QVP concept.** That is because most "QVP" concepts and ZDR calibration at higher tilts focus on the properties of lower density, dry aggregate snow as a claimed better-case media. They often note that the spatiotemporal averaging/variability is still a concern, but perhaps less **in cloud and widespread stratiform selective events**. Overall, those rationale (e.g., Ryzhkov et al. and subsequent) reflect a somewhat different take on the role of higher tilts and the expected ranges for ZDR media at higher tilts. The current authors use expressions such as:

> "The intrinsic value of Z_{DR} for angles below 90° and collected in light rain is different from zero. Also, it is elevation-dependent, as demonstrated by Bringi and Chandrasekar (2001) and formulated by Ryzhkov et al. (2005) as:"
>
> $$Z_{dr}(\theta) \approx \frac{Z_{dr}(0)}{\left[Z_{dr}^{1/2}(0)\sin^2\theta + \cos^2\theta\right]^2} \quad (8)$$

10. *Please note that we explored the use of dry snow as the target to detect the Z_{DR} offset not only using QVPs but also VPs, as stated in this revised version of the manuscript (Lines 492-506). However, we concluded that issues like beam broadening or non-uniform beam filling prevent the use of such targets based on QVPs built from 9° tilt scans. This is why we set light rain as our target to detect the offset, but we also defined constraints that minimise the uncertainty of using measurements taken in liquid precipitation.*

The reason **Ryzhkov et al. give for higher tilts and dry snow** is seemingly opposite to the current authors' logic – Ryzhkov argues **dry snow has lower natural ZDR variability**, and when these media are viewed from higher tilts (e.g., the eventual multiplier on ZDR in equation (9) would be closer to 0 instead of 1), the dynamic range of potential ZDR variability is low. When the underlying media experiences a wider range of variability, aka, light rain ranges from 0.1 dB to 0.6+ dB at typical trusted Z ranges, etc., this implies added uncertainty for any 'average' reference frame. These issues are at their most problematic at grazing angles, and possibly not preferable at lower altitudes (given evaporation, other profile physical processing that evolves ZDR below cloud). Thus, it is not immediately preferable (for their concepts) to have:

$$Z_{dr}(\theta = 10°) \approx 0.968 Z_{dr}(\theta = 0°)\,[dB] \tag{9}$$

e.g., a high coefficient close to 1 is 'bad' for "light rain" in these contexts, b/c the intrinsic ZDR for $Z \sim$ 15-20 dBz remains in those ranges from 0.1 to 0.6 dBz (aka, author Figure 5); This drives the potential uncertainty against the 'reference' ZDR, and one may be correcting by > 0.3 dB quite often (perhaps this was worse in Oklahoma, where lower, unregulated use of Z caries a wider range of ZDR). High tilt intrinsic property sampling (if available, aka, 'birdbath' at its limit) acts to limit that range of possible ZDR $\rightarrow$ better chance to accurately pinpoint ZDR. Thus, the authors' statement,

> "Hence, ZDR radar measurements collected at elevation angles below 10° are similar to those collected at lower elevation angles and so they do not add additional uncertainty to the offset correction method."

... feels opposite this 'dry snow' rationale. This seems to be a question of whether the authors genuinely believe they can target low-ish variability ZDR 'drizzle' better than low-variability dry snow? This may be regional to the UK, e.g., stratocumulus w/drizzle, but may not seem as reasonable if painted with a US NEXRAD radar lens, as a separate example.

11. *We consider that the statement "Hence, Z_{DR} radar measurements collected at elevation angles below 10° are similar to those collected at lower elevation angles and so they do not add additional uncertainty to the offset correction method" was vague, so we modified it accordingly (see Lines 253-256). We hope that this correction clarifies that we are aware that dry snow has lower natural Z_{DR} variability compared to light rain when using high tilts ($>$ 40°). But this variability increases at lower elevations and the QVPs are affected by this issue, as discussed in point 10. This is why we restricted the height within the QVPs along with thresholds in ρ_{HV} in an effort to keep the variability at the minimum. Thus, we consider that if such conditions are met, our approach yields reliable estimations of the Z_{DR} offset.*

Even with light rain, these issues are likely worse than presented; For example, this effort has not fully discussed that the disdrometer (Parsivels, etc.) references are poor in light rain R ¡ 1-3 mm/hr. It is unlikely most units capture light rain properties perfectly, esp. with assumptions made for disdrometer processing (a different subset of literature on Parsivel, 2DVD and other light rain comparisons). Dry snow media, similarly, has its own issues with identification, wavelength dependency, complications to "QVP" profiles from non-uniform beam-filling (at C-band, there is potentially intrinsic negative ZDR above the ML owing to non-uniform beam filling!). There is not a quick fix, unfortunately.

12. *We performed a new procedure to estimate the Z_{DR} reference value as described Lines 264-277 and shown in Figure 4(a). We consider that this new approach reduces the uncertainty on using a reference value that may reflect UK local processes, as it was computed using a range of parameters expected in real storm events. Also, note that we provide more examples using data collected throughout one year for the validation of our method. This includes not only light rain events, but heavy rain events as well.*

2 Response to Anonymous Referee 2

We thank the reviewer for the detailed, positive remarks that helped us to improve the work. 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.

This study proposes an operational method to estimate a systematic bias of radar differential reflectivity (Zdr) using quasi-vertical profiles (QVP). The authors compared the results of the proposed QVP method with those derived from vertical profiles (VP) and disdrometer data for one year period of 2018. They concluded that the new approach is consistent with the traditional method and is operationally applicable.

I think that this study is very important for radar quantitative precipitation estimation (QPE) based on polarimetric variables. However, I see a limitation of this study for an operational application. After reading the manuscript carefully, I found that the QVP method requires a disdrometer-derived Zdr bias for light rain (e.g., 0.18 dB). This is a challenge where there is no disdrometer near radar sites. Additionally, using the disdrometer data in the QVP procedure (e.g., Zdr correction) affects an independent evaluation based on Zdr derived from the disdrometer data (e.g., Fig. 10). My detailed comments are provided below.

We thank the reviewer for raising these important points. Indeed, the method requires a disdrometer-derived Z_{DR} bias in light rain. However, this value can be computed using measured or simulated DSDs. In the paper, we used measured DSDs, but the results are the same if simulated DSDs are used (see replies to points 1 and 2 below and Figure 4 of the revised version of the manuscript). The second point is about using the same disdrometer data set to compute the Z_{DR} bias in light rain and to validate the results. It is fair to say that the validation of the method is performed not only in light rain but also in moderate and heavy rain. However, to address this issue, we have now simulated a wide range of DSDs expected in real storms in order to compute the intrinsic value of Z_{DR} in light rain, and we used the measured DSDs to validate the method (see reply to point 2 below). Finally, note that we modified the manuscript to show different precipitation events throughout one year of data. Moreover, we added a new case study (Figure 11) to illustrate the performance of the proposed method.

Major comments:

1. Title is misleading

   Just looking at the title, I started reading the manuscript with high hope to see how the QVP method can estimate a Zdr bias. However, it turns out that the method needs a reference Zdr value simulated from disdrometer measurements. This is a limitation for the operational estimation for most radar sites, particularly in the United States. I think that the author should include "disdrometer data" in the title.

   We believe there is no need to include "disdrometer data" in the title of the manuscript. The proposed method can be applied even if disdrometer observations are not available. We initially computed the Z_{DR} bias in light rain using measured DSDs, but this bias can also be calculated using simulated DSDs. The results show that the simulated value is consistent with the value obtained using measured DSDs (see reply to point 2). Therefore, the proposed Z_{DR} bias in light rain can be extrapolated to other radar sites.

2. Independent evaluation

   Part of evaluation in this study is not independent. The disdrometer data used in the QVP procedure were also used in the evaluation (e.g., Figs. 10 and 11).

   To address this issue, we simulated a wide range of DSDs using a range of parameters expected in real storm events, as described in Lines 264-277 and shown in Figure 4(a) of the revised version of the manuscript (page 12). We consider that this can be viewed now as an independent validation of the proposed method.

3. Zh- Zdr dependence
   There is no Zh- Zdr dependence demonstrated in the manuscript. I think that the authors took simple averages of Zdr values conditioned on Zh values (0-20 dBZ) at each different disdrometer location.

   The Z_H- Z_{DR} dependence is now shown in Figure 4(a) of the revised manuscript, which is also consistent with previous studies (see Bechini et al. (2008); Bringi et al. (2006); Giangrande and Ryzhkov (2005); Ryzhkov et al. (2005) for instance.). See also reply to point 2.

4. Discussion section
   The discussion section seems to be the summary of this study. Most of the paragraphs are summaries of the results presented in the figures described in the previous sections. I would like to see actual discussions e.g., regarding any challenges or limitations (or sensitive factors) that can affect the accuracy of QVP method. Additionally, there is no "outlook" in the last section.
   We modified this section and discussed several limitations of the proposed method taking into account the reviewer's suggestions (Lines 465-583).

Minor comments:

1. Line 4
   Maybe "light rain" instead of "rain?"
   Corrected in Line 5. Please note that we made slight changes to the abstract to describe the natural targets explored in this work.

2. Line 4
   Please replace "expected" with "desirable."
   We consider that "expected" fits better in this context.

3. Line 95
   Could the author specify the elevation angle of birdbath scans? Based on "averaging azimuthally," the elevation angle is not 90 degrees.

   These are scans collected by pointing the antenna vertically (elevation angle of $90°$) while at the same time the antenna rotates around its axis (from $0°$ to $360°$ in azimuth). We clarified this issue in Lines 114-115.

4. Line 169
   Why not a "solid phase?" I think that Zdr for solid phase should be reliable (for VP) if the authors avoid the melting layer (e.g., mixed phase) as seen in Fig. 2 (right).

   We discussed the advantages and disadvantages of using dry snow and the reasons of using light rain for the Z_{DR} offset detection in Lines 492-509.

5. Figure 2
   The lines indicating the ML and ML bottom are different between right and left panels.
   The individual profile shown in the right panel depicts exactly the same data as in the HTI plot. However, we agree that the lines indicating the ML in the left and right panels do not use the same thickness. We modified the figures accordingly, as shown in Figure 5. Please also note that this figure was updated as we consider that this new format enables a better comparison of the methods used to detect the Z_{DR} offset.

6. Line 217
   What are "the mean dependencies?"
   This refers to the intrinsic Z_{DR} values expected for a range of Z_H values, in this case, $0 < Z_H < 20$ dBZ. We clarified this in Lines 285-291.

7. Line 219
Is the value 0.18 dB supposed to dynamically change depending on different event cases in an operational situation? Otherwise, is this value static?
We proposed this value as the intrinsic Z_{DR} value expected in light rain at ground level on simulated DSD measurements (see reply to major comment 2 above). Thus, we do not expect this value to varying if the physical process leading to the light rain remains similar (widespread stratiform precipitation events).

8. Line 236
Please remove the negative sign in "-0.18 dB."
Fixed in Line 328.

9. Line 239
Why does Zdr offset fluctuate hourly? Is it a mechanical issue?
Previous works found hourly variations on the computed Z_{DR} offset (see Chu et al. (2019); Holleman et al. (2010), for instance). This was not the case in our datasets, where the greatest variations on the Z_{DR} offset were related to updates on the radar configuration, as shown in Figure 6.

10. Figure 4
Please insert a legend for lines with different colors.
We modified this figure accordingly and now it shows all the lines in black (no colour is necessary).

11. Figure 7
While values with VP look consistent, what is the reason of variations with the QVP method in the insets?
Due to the inherent averaging process in the construction of the QVPs, the spatial variation of rain events could lead to QVPs that do not fully represent light rain producing some variability in the estimation of the Z_{DR} offset. We discuss these limitations of our approach in Lines 354-375.

12. Line 293
Please replace "The top row of Figure 10" with "Figure 10(a)."
Fixed in Line 423.

13. Line 344
Please provide more details about "vague polarimetric signatures."
Fixed in Lines 556-557.

References

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