Review for AMT-2021-194

"Calibration of radar differential reflectivity using quasi-vertical profiles"

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This study proposes an operational method to estimate a systematic bias of radar differential reflectivity (Z_{dr}) 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 Z_{dr} 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., Z_{dr} correction) affects an independent evaluation based on Z_{dr} derived from the disdrometer data (e.g., Fig. 10). My detailed comments are provided below.

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 Z_{dr} bias. However, it turns out that the method needs a reference Z_{dr} 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.

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).

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

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.

Minor comments:

1. Line 4
Maybe "light rain" instead of "rain?"

2. Line 4
Please replace "expected" with "desirable."

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

4. Line 169
Why not a "solid phase?" I think that Z_{dr} 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).

5. Figure 2
The lines indicating the ML and ML bottom are different between right and left panels.

6. Line 217
What are "the mean dependencies?"

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?

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

9. Line 239
Why does Z_{dr} offset fluctuate hourly? Is it a mechanical issue?

10. Figure 4
Please insert a legend for lines with different colors.

11. Figure 7
While values with VP look consistent, what is the reason of variations with the QVP method in the insets?

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

13. Line 344
Please provide more details about “vague polarimetric signatures.”