Thanks to the authors for their additions and responses to my original comments. The authors have

- performed substantial investigations of the effects of uncertainties,
- applied the Maahn and Kollias post-processing to the MRR data, then recalculated retrieval results,
- removed the filtering based on reflectivity-weighted velocity,
- and added a comparison of retrieved bulk density against PIP estimates of bulk density, finding good agreement.

These modifications address most of the issues from my previous review.

I do still have a substantial concern about the method used to decompose the bulk density into an ice volume fraction ("bulk ice fraction", vi) and a liquid volume fraction ("bulk water fraction", vw). The authors do not provide a physical basis for the approach they use. Instead, they use what seems to be an ad-hoc requirement to obtain the smallest possible bulk water fraction given the Z_{HH} and the retrieved ρ_{bulk}.

It is clear from equation (2) that Z_{HH} is a function of ρ_{bulk}, so I think the part of the study related to determining ρ_{bulk} is reasonable. But it is also clear from equation (2) that Z_{HH} provides no information that could be used to distinguish bulk ice and water fractions. From equation (1), the best that can be obtained is a linear relationship between vi and vw. An error in vw could be compensated by an offsetting error in vi to give an accurate ρ_{bulk}. Thus an accurately-retrieved ρ_{bulk}does not indicate or imply that an estimated vw is correct.

This concern could be addressed if the authors can provide a rational justification for the approach they have taken. Perhaps there are reasons that they have decided to select the solutions that provide minimum vw. Why is it desirable to choose the minumum vw solution? Note my comment below for lines 164-171 of the revised article. If they have sound reasons and can elaborate on those in the methodology, that would address this concern.

If the authors are unable to do this, I think the proper approach would be for the authors to de-emphasize their claim of "retrieving" bulk water fraction and instead state that their analyses are for one possible approach to selecting the bulk water fraction.

Line-by-line comments on new revision

L 28: Do you specifically mean "liquid water content" here? Do you instead mean just "water content," since remote sensors observe both ice and liquid hydrometeors.

L 38-39: Since density is not related to the "aerodynamic process," maybe rewrite this as "... induces higher density as well as higher fall velocity by the aerodynamic process."

L 107: The word "minute" by itself in English is ofter used to mean "small" or "tiny." I suggest using "one-minute" instead, which has the desired meaning of "a sample of length one minute of time," here and at other locations in the paper.

L 111: Is the "ICEP-POP" that is used here intentional, rather than "ICE-POP"?

L 144-147: If each hydrometeor is truly "regarded as a symmetric sphere" and ice and water are assumed to be evenly distributed within the particle, canting angle is not relevant - the scattering properties will not change with respect to any rotation of the sphere. Why are canting angles considered? Were the particles not actually symmetric spheres? Please enhance this description to be clear and correct about what is being assumed for the calculation of the scattering properties.

L 164-171: This description of the methodology is the point of my most significant concern with the study. To obtain distinct vi and vw, the authors make an ad-hoc choice to pick the solution with the maximum ρ_{bulk} and vi. The justification they provide is that this is "similar to Huang et al. (2010), which assumes" the particles are only ice and air. My opinion is that this justification is not sufficient to allow a claim that vw is being retrieved.

L 172-173 and 188: Per the statements by the authors here, the validation approaches that are being used are to validate the retrieved bulk density, not the bulk water fraction.

L 184-185: Reflectivity-weighted velocity (for comparison to radar Doppler velocity) is more often seen calculated from PSDs as

$$V_Z^{\rho_{bulk}} = \frac{\sum_i \sigma_{bk}(D_i) V(D_i) N(D_i) \Delta D_i}{\sum_i \sigma_{bk}(D_i) N(D_i) \Delta D_i}$$

where $\sigma_{bk}(D_i)$ is the backscatter cross-section for particles in size bin i. It's not clear here what is meant by $Z\left(\rho_{bulk}, D\right)$. Please add some description of $Z\left(\rho_{bulk}, D\right)$, how it is calculated and whether your formula gives results that are the same as this more typical formula. If not the same, the comparisons of $V_Z^{\rho_{bulk}}$ and V_Z^{MRR} may be of concern.

L 196: Per the reference, Kim et al. (2021), equation (6) gives the volume-weighted mean diameter, not the mass-weighted mean diameter. The Kim et al. statement seems correct, since equation (6) gives the ratio of the fourth moment of the PSD to its third moment.

L 211: I think this should be "... and CPO are slightly lower."

L 220: Regarding "various measurement issues" that induce inconsistency, please be more explicit by stating what are these issues.

L 264: Please check Figure 5. I do not see a gray area.

L 291-292 and 295-296: I think these lines overstate the interpretation of Figure 12 somewhat. I agree that the distributions in Figure 12 do show changes in fall velocity-diameter relationships. It is probably OK to say that the particular changes in the relationships are consistent with increases in ρ_{bulk} which could be associated with increases in bulk water fraction resulting, for example, from melting of particles. But I believe it is an overstatement to say that "gradual increases in density, as well as the bulk water fraction" can be *found* in the $V(D)$ distributions in Figure 12 (L291-292) or that the retrieved bulk density and bulk water fraction *reveal* distinct $V(D)$ relations (L295-296).

L 303: I'd suggest "transitioned" rather than "transited".

L 304: Should this be "at other sites" instead of "as other sites"?

L 304-305: See my earlier comment regarding L 291-292. Saying that the $V(D)$ relation "is consistent with" the bulk water fraction seems more appropriate.

L 326: See my earlier comment regarding L 196 and what is actually calculated by equation (6).

L 334: Where does this relationship between D_m and D_0 come from? Is there a reference?

L 359: See my opening comments and concerns along with the related line-by-line comments regarding the ability of the retrieval to determine bulk water fraction. This statement also falls under that concern and should be addressed.

L 366: While Battaglia et al. do discuss Parsivel fallspeed errors, I don't believe they are discussed in Wood et al.

L 376: Especially since this approach of using CF is from personal communications and not from a published reference, the values of the particle size-dependent CF and the method by which its values are determined should be documented here, to allow the results to be reproduced.

L 410-411: No, I don't think it is justified to say that since the bulk densities are in agreement with those from the PIP, the bulk water fractions are confirmed. Z_{HH} is dependent on bulk density in a way that makes it not possible to discriminate the contributions of vi and vw. Since bulk density depends on both vi and vw, offsetting errors in vi and vw could still give a correct ρ_{bulk}.

L 413: Usually "mixed-phase" rather than "mixing-phase".

L 416-417: See comment regarding L 410-411.

L 433: There appears to be an incomplete sentence here: "The retrieved bulk density."

L 441: Again, see and address my overall comments regarding retrieval of bulk water fraction.

L 444: Usually "unattended" rather than "unattentively".

General comments

Does EGU have a policy on including information about where to obtain the input datasets used for the study presented in the paper? Is a data availability statement required? In the acknowledgements, I note that the source of the PIP data is not mentioned.