======= REVIEWER #1 =======

amt-2021-427: Helms et al. A Comparative Evaluation of Snowflake Particle Size and Shape Estimation Techniques used by the Precipitation Imaging Package (PIP), Multi-Angle Snowflake Camera (MASC), and Two-Dimensional Video Disdrometer (2DVD)

In short: This study compares the different algorithms behind the measurement techniques of three digital video disdrometers: the Precipitation Imaging Package (PIP), the Multi-Angle Snowflake Camera (MASC), and the Two-Dimensional Video Disdrometer (2DVD) in observing snowflakes. The focus is on defining the uncertainties in the defined area influencing the equivalent diameter, and the aspect ratio. The authors quantify the motion blurring, in the case of PIP also the image compression, the shape-fitting measurements, and in the case of 2DVD, the estimate of the bounding box measurement when particle horizontal motion needs to be adjusted with an unskewing algorithm.

The topic is interesting and relevant for surface observations and the development of retrieval methods in global monitoring of snowfall. The study has novelty in the way it examines the measurement algorithms internal to the instruments, which typically are not transparent to the end-user of the data. The theory is clearly outlined with illustrative examples, and the conclusions are well-supported and valid. The manuscript is well written and provides a clear storyline, however, at least in my opinion, it leaves the reader questioning what is the magnitude/importance of these studied uncertainties in respect to the other uncertainties e.g. particles out of focus or only a fraction of particles observed, wind effects to particle fall velocity, miss-matching of particles, partially illuminated measurement space or limited observations of particles from only one plane projection, these are referred in several publications prior to this one. I would like to see more discussion on this topic and references to other related studies. My recommendation is to publish the paper after addressing this concern and some small remarks mentioned below.

I've added a discussion section that takes a look at how the findings of this study compare to other studies (Section 6, Lines 550 – 587).

Minor comments:

Line 11: "… PIP or 2DVD which provide similar precision once the effects of the PIP image compression algorithm are taken into account." This sentence is somehow unclearly connected to the previous statement in lines 10-11 and is not clear for a reader who just reads the abstract. Please rephase.

I rewrote that bit of the abstract and this sentence was removed.

Line 41: "There are numerous examples of studies which rely heavily on either of these measures of particle size." The statement "numerous examples" follows only by two references, leaving for example relevant fields such as the snow model or satellite retrieval development unmentioned. I would like to see a broader scan of the research field, just mentioning applications and example references would be enough.

The previous paragraph gives a list of five studies which use one of the two measures of particle size. As such, I've reworded the start of the paragraph that started on Line 41 (and now starts on Line 39) with the intent of making the paragraph serve as a deeper dive into one of the studies. The paragraph now starts off with:

"In the case of Han and Braun (2021), the authors characterize the global three-dimensional distribution of precipitation mean particle sizes using satellite radar data; they used a form of the equivalent diameter as their metric of particle size, specifically the mass-weighted mean liquid-water-equivalent diameter." Lines 39 – 41

Subsequent paragraphs delve into some of the other studies listed (Pettersen et al 2020,2021; Locatelli and Hobbs 1974; Heymsfield and Westbrook 2010).

Line 64: "separate from the snowflake size," This is unclear. I don't understand how these mentioned studies are concerning only aspect ratio separate from size. Please rephrase.

My intent was to make sure the reader wasn't thinking of snowflake shape as referring to the combination of relative dimensions and overall size. In hindsight, I don't think the clarification is strictly necessary, especially if it has the potential to cause confusion. As such, I've simply opted to delete the clause. Line 61

Line 87: Altitude of the site?

Added altitude (789 m) after the latitude and longitude coordinates. Line 94

Line 89-91: I would like to see more data of this event to support the assumptions of aggregation and lump graupels, e.g., time series of temperature, PSD, and mean fall velocity.

I've added a figure showing the PSD, mean fall speed, and the air temperature (Fig. 1). I've also added the following text to the end of the paragraph:

"Although the general presence of these habits were primarily identified through visual inspection of the PIP data, further support of their presence can be found by examining the time series of the particle size distribution, particle fall speed, and air temperature, as depicted in Fig. 1. Periods when aggregates are present can be identified by the larger equivalent diameters and slower fall speeds and periods with lump graupel can be identified by the smaller equivalent diameters and faster fall speeds; the lump graupel can be discerned from liquid precipitation based on the below freezing temperatures, which extend over a deep layer according to a nearby thermodynamic sounding (not shown)." Lines 98 – 103

Line 336-8: “Even with a very fast fall speed of 4 m/s, the overestimation of the equivalent diameter for very large circular particles (diameter ~ 10 mm) is approximately two orders of magnitude or smaller than the actual equivalent diameter.” Two orders of magnitude? This is not clear to me.

A fractional bias of 0.01 would mean that the bias is two orders of magnitude smaller than the measurement. In this case, a fraction bias of 0.01 for a circular particle with a diameter of 10 mm would mean the bias is 0.1 mm. I’ve clarified this point in the text by changing the sentence to read

“For a very fast fall speed of 4 m s^{-1}, the overestimation of the equivalent diameter for very large circular particles (diameter $\gtrsim$ 10 mm) is approximately two orders of magnitude smaller than the actual equivalent diameter (i.e., a fractional bias of 0.01).” Lines 389 – 391

Line 339: “perfectly circular”. Why assumed the particle to be circular, though written in lines 68-69 “aspect ratio is frequently prescribed, often with a mean value of 0.6 assumed (e.g., Matrosov et al., 2005)” and then without quantification stated that for the oblate particles “the relative (and absolute) effects of motion blurring on the area and equivalent diameter measurements will also grow”. Please justify and elaborate.

The choice of using a circular particle comes down to simplicity: a circle is easy to mentally picture, there’s no need to worry about assuming an orientation, and the equivalent diameter is equal to the diameter so the “ground truth” is easy to convey in the plots. As for oblate particles, if you make the particle more oblate without changing the area, the number of top-edge pixels will grow, which will increase the area and equivalent diameter biases (i.e., the “absolute effects”) as they are more or less proportional to the number of top-edge pixels (as well as the particle speed of course). As we are keeping the area and equivalent diameter constant, the fractional bias (i.e., the “relative effects”) will increase as well. I don’t feel like this needs quantification to back it up, but it certainly could do with more of an explanation. The text now reads:

“As the particles under consideration become more oblate, with their horizontal dimension growing relative to their vertical dimension, both the absolute and relative effects of motion blurring on the measured area and equivalent diameter will increase relative to circular particles of the same area. An oblate particle of equal area to a circular particle will have more top-edge pixels than the circular particle. Increasing the number of top-edge pixels increases the number of pixels that the motion blur is applied to, thereby increasing the absolute effects of the motion blurring. Furthermore, because the particle area remains constant, the relative effects of the motion blurring are also increasing. For similar reasons, increasing the complexity of the particle outline such that the number of top-edge pixels increases will also result in larger biases due to motion blur.” Lines 393 - 400

Figure 4: Could you add the number of analyzed particles and a density plot would add information instead of a scatter plot.

Switched to a 2D histogram and added particle count to color bar title of Fig. 5.

Figure 6. Just to add more information about the particle habit, could the approx. fall velocity be added to the corner of the image? The colored fitted shapes, could the line be slightly thinner or the image larger, it is now hard to see the lines in respect to the shaded image, they are all on top of each other.

I've gone through and added fall speeds for each particle and played around with the line thicknesses of Fig. 7. Annoyingly, it seems that particles (c) and (d) are not listed in the PIP velocity tables, so I had to go through and manually calculate the fall speeds for these. I've also made the annotated circles for the MASC and tensor methods thinner to make the PIP annotated circles easier to see.

Lines 404-406. As PIP is only seeing a plane projection of the particle, but here the particle is referred to as an ellipsoid, it is confusing whether in this perimeter stretching factor analysis, the computations are performed in 3D with ellipsoids and is it then assumed the same axes ratio in both directions or is it performed in the 2D projection. Could you please clarify this?

It seems that this is a bit of sloppy word choice on my part. I've changed ellipsoid to ellipse. Line 464

Paragraphs 408 – 445: I understood that this section provides explanations why the ellipse-fit in PIP has an arbitrary upper threshold close to 0.6, and why with the rectangular fit in PIP, there is a gap in aspect ratio between 0.9 and 1.0. However, it was not always clear, which "gap" the authors were pointing at. I would suggest that you would refer in the text (when addressing for the first time) to the image, where the "gap" is shown. E.g. in lines 436-438, I assume here the authors are referring to Figure 5b?

I've gone through this section of the text and tried to clarify the wording and point the reader to relevant figures more frequently. Lines 464 – 507

Figure 9. Same as Figure 4. It would be nice to see the number of analyzed particles and then rather a density plot than a scatter plot.

Swapped to 2D histograms and added the number of analyzed particles to the colorbar title for Fig. 10. I've also removed the height plot based on reviewer 3's comments.

Lines 526-527: "and, as a result, the maximum dimension and aspect ratio measurements are unreliable; however, the PIP variables other than the ellipse and rectangle dimensions appear to be reliable" I assume here it is referred that the PIP-fitted maximum dimension of an ellipse is unreliable and not that the observed maximum dimension is unreliable. Please clarify.

Clarified. The text now reads:

"Unfortunately, the PIP shape-fitting routines do not perform well on precipitation particles and, as a result, the PIP-fitted ellipse and rectangle dimension (and, therefore, aspect ratio) measurements are unreliable; however, the PIP variables other than the ellipse and rectangle dimensions appear to be reliable." Lines 525 – 528

Lines 528-530: "As the present study has demonstrated, the PIP imagery can be reprocessed and reliable measurements of maximum dimension (the previous comment) and aspect ratio can be made via the application of an alternative ellipse-fitting algorithm, such as the MASC or tensor-based algorithms." In the manuscript, it was described that the AVI file contains only the first 2000 frames from the 10 - minute section, and with 380 frames per second, this translates to 5.3 seconds of data. It is unclear that can an end-user reprocess the whole data volume or just the sample frames in the AVI-files? Could this be elaborated?

The good news is that yes, the entire data volume can be reprocessed from the raw files. The bad news is that this is a massive amount of data. My own efforts ended up taking an excessive amount of time to reprocess even a day of the raw data files. I've been told that the actual PIP processing code is written such that it can be used to reprocess data. From what I've seen, it should be possible to implement the tensor-based ellipse-fitting method using the IMAQ software so perhaps the data set can be reprocessed that way at some point in the future.

All that aside, I added the following paragraph to the Method section (just after introducing the AVI files for the first time) to make it more clear that the complete data set could (theoretically) be reprocessed:

"Ideally, we would include all PIP images from the 7--8 March 2018 period in our study rather than just those images that are included in the AVI files. Since the raw PIP image files are archived and contain every frame with precipitation, this is theoretically possible. That said, due to the very high data rates involved with the PIP data, the processing code would need to be highly optimized or use specialized software packages in order to process even a couple days of the raw data in a reasonable time frame. Thankfully, the AVI files contain a sufficiently large collection of precipitation particles to enable us to perform our study." Lines 202 – 207

======= REVIEWER #2 =======

This is an important contribution to understanding how various optical imagers observe properties of snowfall. Overall, the manuscript is understandable. While I have no qualms with the included analysis, there does appear to be some low hanging fruit that would greatly improve the impact of the paper. These and other comments are highlighted below.

Specific comments:

Horizontal motion should/needs to be addressed. Sampling/flow issues aside (and this is extremely important but beyond the scope of the article), the open design of PIP will lead to more translational motion vs. the MASC. Based on the field campaign data, there should be surface wind data you could use that would help guide upper-values for analyses like Figure 3. Further, I would like to see how this relates to the PIP vs. 2DVD analysis (e.g. Figure 9).

I've added a 10 m/s particle motion to the analysis and discussion of motion blurring (as well as to Fig 3, now Fig 4). Changes to the text are scattered throughout section 5.1.

The normal caveats apply to studies based on one case during a campaign. Is there the opportunity to conduct this analysis on other cases? How many were available during ICE-POP 2018? More reasoning is needed. While this case provided lots of variety, it is important to demonstrate how varied results are for other types of cases as perhaps you could demonstrate for certain types of cases, the discrepancies between instruments is either amplified or diminished.

I would argue that, for this kind of study, each particle is a separate case, but just to be on the safe side, I processed data from a couple other cases (specifically, 9 January 2018 and 22 January 2018). Looking at the PIP vs MASC vs tensor dimension plots (similar to Fig 4, which is now Fig 5), the differences from the figure in the manuscript are minimal. As such, I've opted to stick with only using the 7-8 March 2018 event to make the narrative simpler. I also added a note mentioning this to the data section:

"Data collected during a snow event on 9 January 2018 and 22 January 2018 were also examined, but are not included here as their inclusion did not produce any notable changes in the results." Lines 103 – 105

The manuscript could be more concise by merging the Data and Instruments sections. For example, the first paragraph in section 3.1 is essentially duplicative with the material under data. I would remove this, then make the sections on the instruments as sections 2.x.

I don't see the duplication between the introductory paragraph of the instruments section (I assume this is what you were referring to) and the contents of the data section. I toyed around with merging the sections, but the result kept feeling awkward and forced so I reverted back to having them as separate sections. In the process of responding to the reviewer comments, however, I've added the following additional text to the end of the data section that I think helps separate the two instances of "This study…" that might be making the text sound repetitive:

"Although the general presence of these habits were primarily identified through visual inspection of the PIP data, further support of their presence can be found by examining the time series of the particle size distribution, particle fall speed, and air temperature, as depicted in Fig. 1. Periods when aggregates are present can be identified by the larger equivalent diameters and slower fall speeds and periods with lump graupel can be identified by the smaller equivalent diameters and faster fall speeds; the lump graupel can be discerned from liquid precipitation based on the below freezing temperatures, which extend over a deep layer according to a nearby thermodynamic sounding (not shown). Data collected during a snow event on 9 January 2018 and 22 January 2018 were also examined, but are not included here as their inclusion did not produce any notable changes in the results." Lines 98 – 105

Technical comments:

Line 200: is vs. will be performed?

Switched to 'is' Line 227

Paragraph (255-268): This paragraph could be cleaned up. Examples include multiple 'For simplicity' phrase and you could omit 'it should be noted'. I would lead off with the 2nd sentence to remind the reader, than discuss the number of factors that aren't addressed rather than revisiting the 'number of factors' phrase.

I've gone in and cleaned up the text a bit based on these suggestions. Lines 283 – 295

Line 275: Agreed, but you should probably provide a citation for this statement. The larger concern is potential horizontal motion

I added a new paragraph to explain the new statistical approach to the motion blurring calculations that I performed for the revision (Lines 296 – 307). As part of that, I included a sentence explaining our selection of particle motion speeds, including references to Locatelli and Hobbs (1974), Garrett et al. (2012), and Vazquez-Martin et al. (2021):

"The particle motion speeds are chosen as follows (e.g., Locatelli and Hobbs, 1974; Garrett et al., 2012; Vázquez-Martín et al., 2021): 1 m s^{-1} represents a relatively fast falling snow particle, 4 m s^{-1} represents an excessively fast fall speed for snow particles, and 10 m s^{-1} represents relatively fast horizontal motion (albeit below the U.S. National Weather Service wind threshold for blizzard conditions, -15 m s^{-1})." Lines 300 – 303

Line 304: Extra 'both' in this sentence.
Fixed. Line 347

Line 321: How about: Motion blur of the top (bottom)- edge pixels occurs when the particle leaves (enters) those pixels during the image exposure period.

This sentence became much simpler after correcting the error Reviewer 3 found where we were double counting the motion blur effects. The new sentence reads:

"The motion blur of these top-edge pixels occurs when the particle leaves those pixels during the image exposure period." Line 369 – 370

Figure 3: The sentence starting with 'Calculations… and Specifically…' is repetitive with the body of the text and does not describe the visual properties of the figure. I would omit for brevity or restate in text instead of the caption.

I've removed this part of the Fig. 4 caption (I also remade the figure to be statistically generated rather than the rough estimate I had been making as well as added a 10 m/s line to talk about horizontal blurring).

Figure 4: best fit lines? I would omit the last sentence in the figure caption as this is already included in the text right after Line 355.

Removed final line of the Fig. 5 caption and added "The diagonal grey lines indicate where the x value is equal to the y value."

Figure 7: Once again, some of the caption is discussed in text (sentences starting with 'This' and the first 'The'.

Removed the sentence starting with 'The' from the Fig 8 caption. I feel like "This example uses a circle with a radius of 0.5 mm to compute the base perimeter and area" is worth retaining as a brief reminder to the reader.

Line 447: 'Because the 2DVD'

Fixed. Line 509

======= REVIEWER #3 =======

The sudy is an interesting evaluation of processing algorithms to derive two characteristic dimensions, length and width, of snow particles from 2-dimensional images.

Algorithms from three instruments, PIP, MASC, and 2DVD, and their resulting dimensions are evaluated. The conclusions allow future users of these instruments to chose a suitable algorithm.

Only PIP data are used. Using emulated data for testing other algorithms, which are used with MASC or 2DVD data. This provides a fair comparison of the algorithms only. However, this method cannot compare the actual qualities of the PIP, MASC, or 2DVD measurements related to specific instrumental issues. In particular, incase of MASC and 2DVD, the method cannot evaluate if the PIP-derived emulated or the actual measurements (if available) would more accurately represent the particle's dimensions.

The study clearly describes the chosen method and recognizes its limitations.

I suggest publication of the study after some shortcomings have been addressed.

Major issues:

1) All conclusions are vaguely formulated, some are only speculative.

In general the analysis is not sufficiently quantative in comparing the resulting derived measurements from the various algorithms. Similarly, the Abstract uses many "should" and leaves doubts about the usefulness of the conclusions.

Quantify and better describe things like "spread", "agreement", "reasonable estimates", and "greatly underestimates".

Examples:
Improve discussion around Fig.4. In these scatter plots it is difficult to see the differences tated in the discussion of for example 4a) vs. 4b) or 4d) vs. 4e). As suggested by Referee 1, density plots can be more useful. In addition, some quantitative statistical measures will be useful (e.g. to better argue that "MASC-fitted and tensor-fitted ellipses tend to produce fairly similar particle dimensions", L. 394, and that "MASC-fitted ...ellipses tend to provide more reasonable estimates of particle dimensions...", L 396).

I've added mean absolute differences and mean absolute fractional differences to the figure panels (Figs. 5 and 10) and included them in the discussion of the results (Lines 423 – 491, 525 – 549, and some mentions in Section 6). I've chosen the term 'differences' rather than 'error' as, to me, 'error' implies that I'm comparing against a ground truth, which is not the case. I've also switched Figs 5 and 10 to use two-dimensional histograms rather than scatterplots as per Reviewer 1's suggestion.

L. 371-372: reformulate this sentence, using a certain aspect ratio will not increase the spread in dimensions.

I've rewritten a large chunk of this section and corrected this bit of poor wording in the process. The replacement sentence reads:

"Using the PIP-fitted rectangle as the basis of the particle dimension measurements produces a sizable improvement to the agreement between the PIP-fitted long dimension and the tensor-fitted long dimension (Fig. 5a,b) but almost no change in agreement for the short dimension (Fig. 5d,e)." Lines 422 – 424

Fig.9: a) not needed, instead argue/discuss that the two heights are the same.; b) quantify the range (and maybe distribution as histogram) of ratios between 2DVD width and PIP width (I guess they vary between 0.8 and 20 and will show two modes on a histogram); c) quantify similarly and then compare to b). That will allow for less vague descriptions than "can sometimes underestimate the bounding box width" (L. 473) or "are surprisingly accurate" (L. 477).

For Fig 10, I've removed panel a and switched to using a 2D histogram rather than a scatterplot (as per Reviewer 1 suggestion) and have added the mean absolute differences and mean absolute fractional differences to the plot panels. I took a look at the ratios and they didn't really add much, but I think the addition of the mean absolute difference has helped reduce the vagueness.

2) The ellipse-fitting algorithm (PIP, MASC, or tensor variant) could also be applied to 2DVD measurements. This can be tested within this study on the emulated 2DVD measurements, adding a valuable aspect to 2DVD measurements. Then, the conclusion about the limited usefulness of the 2DVD measurements can be revisited (L. 519-521.)

I actually did produce tensor-based ellipse fits for the emulated 2DVD images when I reprocessed the data for this study. That said, I made the decision fairly early on to not include an analysis of them in this manuscript (which has had an ongoing problem with ever expanding scope since its inception). As such, I haven't taken a particularly close look at these fits; perhaps a topic for a future paper. That said, I have added a note in the conclusions mentioning the potential application of ellipse fitting to 2DVD images:

"While not demonstrated here, it may be possible to also implement a shape-fitting algorithm for 2DVD using the reconstructed images captured by the line-scan camera, although the reliability of the resulting shape measurements from such an algorithm would need further investigation to test the impacts of the image skewing." Lines 626 – 629

3) Sect 5.1, L256-258:

"For this study, however, we will examine the theoretical accuracy and precision of the MASC, PIP, and 2DVD area and equivalent diameter measurements in terms of motion blur as determined from the pixel resolution, exposure time (i.e., shutter speed), and particle fall speed."

Motion blur is not related to precision. Overall, I find the discussion around "precison" unnecessary and not well introduced (only later on in Sect 5, L347, it is mentioned what precision or "precise measurement" refers to. The effects of this theoretically higher precision are, however, not discussed in this study. If the authors consider this to be an important aspect of their study, then I would recommend to evaluate the consequences by using the same algorithm with differing pixel resolutions. As MASC measurements are only emulated here, the actual effects of the higher precision remain unclear (and are not part of this study and instrumemnt specific and related to questions such as if the increased precision is accompanied by a corresponding better optical resolution and accuracy).

I should have said "in terms of pixel size and motion blur" as precision is tied to pixel size and accuracy is tied to motion blur. You make a good point in that I don't delve deeply into the precision and I don't make any examination of how precision of the images impacts the measurements of particle dimensions. As such, I've gone through the manuscript and removed most of the mentions of precision. I've kept a few where I felt they were particularly relevant to the discussion.

4) Sect 5.1 and Figures 2 and 3:

The discussion of motion blur and its effects on accuracy seems to be wrong. It is correct that, considering a vertical particle motion during expeosure, the blurring affects both the upper and lower edge. However, the particle extension is not increased (blurred) upwards and downwards. At the start of the exposure, the particle has an upper and a lower edge. Both these edges are moving (blurring) downwards, i.e. blurring will not add extra pixel(s) above, only below. By incorretly assuming added pixels above and below, the authors seem to overestimate blurring by a factor of two.

Thank you for catching this! After thinking about it, you're absolutely correct that we are double counting the motion blur effects. I've gone through and made corrections to the figures and text where needed.

5) The arteficial "cap" is not explained satisfactorily.

Instead, the value of the cap is translated in a certain perimeter stretching factor. However, the authors do not try to explain, why no smaller perimeter stretching factors exist. Assuming that (L. 442-443) only few particles have a smaller perimeter stretching factor seems wrong, I guess (from looking at Fig. 4.g) that there is not a single particle with smaller perimeter stretching factor.

The reason for this cap is likely to be found in effects of pixelation affecting the perimeter by artificially extending it, more noticeably for smaller particles (~0.5mm) than for larger ones.

Having said this, I need to remark that it should be discussed how the perimeter is determined.

L. 410-413: The pixelation effects should be considered.

Fig.7: Reformulating the discussion around the artificial cap may result in that Fig 7 is not needed. E.g., currently the whole discussion about it in L. 418-445 is difficult to understand and doesn't explain the cause of the cap.

Reconsider the usefulness of Fig.8.

I've added a sentence to the discussion of the perimeter stretching factor to note the sources of small increases in perimeter relative to area:

"Small increases in perimeter, such as this, can be introduced by a few very small deviations of the particle edge from a perfect circle as well as by the inability to perfectly represent a circle using square pixels (i.e., pixelation effects)." Lines 489 – 490

As for figures 7 and 8 (now 8 and 9), I think they materially contribute to the manuscript by enabling a discussion of the sensitivity of aspect ratio to small changes in perimeter length, which is the mechanism responsible for the artificial cap in PIP ellipse aspect ratios. That said, I have gone through and tried to clean up and clarify the discussion of the perimeter stretching factor. Lines 464 – 507

Finally, I added a sentence to the instrument description section for PIP that describes the perimeter calculation:

"The IMAQ Vision software package computes the particle perimeter by subsampling the boundary points to produce a smoother representation of the perimeter; for this purpose, the boundary points are located at the corners of the pixels that make up the particle perimeter." Lines 127 – 129

6) Similarily, the apparent gap between aspect ratios around 0.9 and 1.0 is not explained properly (L. 375-376). It seems to stem from the fact that there is a minimum perimeter stretching factor that is above 1 in case a rectangle(square is fit instead of an ellipse/circle. There is no gap, but all particles with smaller perimeter stretching factor are simply "piling up at the aspect ratio of 1.0.

I agree with your explanation, but I would describe such a feature in the distribution as a 'gap'; the "piling up" is simply the cause of this gap.

7) Using ellipses or rectangles that best fit the particle can be used to describe shape, they are, however, not sufficient as complete measurements of the particle's shape. The limitations of the evaluated algorithms could be highlighted better.

Added a paragraph to the introduction that touches on this point:

"As will be discussed in greater detail below, both PIP and MASC produce their measurements by fitting simple two-dimensional shapes (ellipses and rectangles, specifically) to two-dimensional projections of the three-dimensional snowflakes that the instruments are observing. Because snowflakes come in a large variety of shapes, especially when taking aggregate snowflakes into consideration, any attempt to use a simple shape, such as an ellipse or rectangle, to represent these particles suffers from the inherent limitation of under-representing the complexity of the snowflakes. Furthermore, the use of two-dimensional shapes to represent three-dimensional snowflakes adds an additional layer of limitations revolving around the degree to which the two-dimensional projection accurately represents the dimensions and orientations of the three-dimensional particle (e.g., Jiang et al., 2017). Despite these shortcomings, the measurement of snowflake dimensions based on shape fitting has proven to be a useful tool for studying snow microphysics and understanding the relative capabilities of these measurements is critical to their successful use in research and applications." Lines 70 – 79

Other minor or technical issues:

Terminology:

Inconsistent use of terminology:

E.g. "tensor method" only used twice (L179-180 "hereaftyer referred to as the tensor method" and L242 "referred to here as the tensor method"), elsewhere "tensor-fitted ellipse" or "tensor-fitted ellipse method" or "tensor-fitted ellipse measurement"

Or: Inconsistent use of "resolution", not always used correctly. L100 "resolution" refers to the size on the particle that corresponds to one pixel. This is later more adequately referred to "pixel resolution" (e.g. L.269) or "pixel size" (L. 314).

Maximum dimension is not used in this study. The term "maximum dimension" is, however, used three times in the Conclusions. The authors likely wanted to refer to an ellipse- or rectangle-fitted dimension.

I went through and consolidated the 8 or so different ways I refer to the tensor method into a much more manageable number. Specifically, I now use 'tensor method' to refer to the method in most instances after I first introduce the method in the text. In cases where I want to be more generic, I refer to it as 'a tensor-based ellipse-fitting algorithm' in order to avoid the question of "what is 'the tensor method'?" before I've had the chance to introduce it (i.e., in the abstract and first paragraph of the conclusions, under the assumption that some readers will start there).

Although, at the end of the paper, I went even more generic and used ‘tensor-based algorithms’. When talking about an ellipse constructed using the tensor method or the measurements derived from said ellipse, I use ‘tensor-fitted’ as the adjective form.

Additionally, I’ve gone through and switched ‘resolution’ to ‘pixel size’ where relevant.

Finally, I’ve switched ‘maximum dimension’ to ‘long dimension’ as I had intentionally avoided ‘maximum dimension’ up until this point. The sole exception to this is in the introduction.

Sect 3.3:

Make it clear that the viewing planes are horizontal and that they are separated vertically by 6 mm (or 7?). Discuss how the "piecing together" of the single line scans is carried out and what errors or accuracies are to be expected. Is the sentence in L. 517-519 ("highly accurate") true? Provide information on pixels and pixel resolution (as done for PIP and MASC in 3.1 and 3.2).

I’ve updated the first two paragraphs of the section to include the request information. Lines 156 – 172

L. 199-200 reformulate "made" (measurements are doen or carried out), e.g. "... before the MASC measurements are emulated by using the same ..."

Reworded sentence now reads:

“Because both the PIP and MASC ultimately perform their measurements using two-dimensional images, no special processing is performed to prepare the images before emulating the MASC measurements.” Lines 225 – 256

L.205: "a five pixel particle" is ambiguous as the PIP measured particle image and the emulated MASC image have different pixel resolutions. Use something like "a five PIP-pixel particle".

The sentence now reads:

“In the case of the emulated MASC, a five PIP-pixel particle would have an area of 0.5 mm^2 and a maximum dimension of at least 0.3 mm as the PIP pixels are calibrated to be 0.1 mm of each side.” Lines 232 – 233

L. 214: "product of the particle fall speed and the camera observation frequency" seems wrong, should it be v/f?

Not sure why I wrote ‘product’ here as the code uses v/f…. Either way, the corrected sentence now reads:

"The vertical motion is replicated by shifting the vertical coordinates of the bilinear interpolation upward by an amount equal to the particle fall speed divided by the camera observation frequency; for 2DVD, the camera observation frequency is 68200 Hz." Lines 240 – 242

Sect. 4.3: Specify that the tensor elements are mean values of the quantities (e.g. square of Delta y) for all particle pixels (or otherwise explain better eq. 1).

I've cleaned up the sentence, which now reads:

"This method works by computing the eigenvalues, e_1 and e_2, of a two-by-two mass distribution tensor matrix, which is defined as [EQUATION 1], where Δx and Δy are the distances from the particle centroid in the horizontal and vertical directions, respectively, and the overbars indicate averaging." Lines 270 – 274

L. 377-378: Repeated use of "expected" and unclear when the increase in aspect ratio (or the period of lump graupel) is.

Reworded sentence and explicitly indicated period of lump graupel:

"It should be noted, however, that the PIP-fitted rectangle aspect ratio does capture the increase in aspect ratio associated with the periods of lump graupel precipitation on 8 March around 0900 UTC and after 1400 UTC." Lines 436 – 438

L. 389-390: "lack of a warm nose" and its implications should be explained if that is relevant for the discussion.

The presence of a warm nose in the sounding would open the possibility of the circular particles being ice pellets or liquid (or mostly liquid) precip. The only bearing this has on the discussion is that it supports the classification of particles (c) and (d) being lump graupel. That said, 'warm nose' might be a bit too niche terminology for this manuscript, so I've changed the wording on the sentence. The new sentence now reads:

"Based on the relatively circular shapes of the remaining two particles, relatively high fall speeds (black line, Fig. 1), subfreezing near-surface temperatures (red line, Fig. 1), and the lack of an above freezing temperature layer in a nearby thermodynamic sounding (not shown), particles (c) and (d) are likely both examples of lump graupel." Lines 450 – 452

L. 399-401. While it seems intuitively obvious what the sentence tries to explain, it needs to be refromulated for correctness and clarity.

I don't see any issues with correctness with this sentence, but I've tried to improve the clarity. Here's the updated sentence:

“For a particle with complicated outlines, such as particle (b), the particle perimeter is far greater than the perimeter of the ellipse or rectangle of either equal area or equal dimensions.” Lines 460 – 462

L. 402-403: remove last part of sentence ("note, extending the short...") to improve clarity.

Removed.

L.473-476: reconsider the explanation, it seems that the example particle should move to the left while moving down to be compressed horizontally.

I believe my explanation is correct since the particle is scanned from the bottom upwards as it falls through the plane of the line scan camera. As a result, the position of the bottom of the particle remains unshifted while the top of the particle gets shifted in the direction of horizontal motion. This has tripped me up multiple times, so it certainly warrants clarification in the text. I’ve added a note to the end such that the new sentence reads:

“This can occur when a particle of sufficiently low aspect ratio is moving in the opposite direction of the tilt of the top of the particle (e.g., a needle crystal whose top is to the left of the particle centroid moving towards the right); this will result in the particle being compressed in the horizontal because the particle is scanned from the bottom upwards as it falls through the plane of the line scan camera.” Lines 536 – 540

L.247 correct spelling: "eingenvalues"

Fixed. Line 274

L. 304 delete duplicate "both"

Fixed. Line 347