Review of amt-2022-169 (Revised submission)

The authors addressed the issues mentioned in the first review.

In particular, the following points, in my opinion, significantly improve the quality of the manuscript.

- The proposed method undergoes a better evaluation:
  - The spectral width produced by the proposed method is compared with the one measured by a C-band radar.
  - A quantitative evaluation of the impact clutter removal and sidelobe mitigation has been performed.
  - The removal of the range sidelobe artifacts is compared with a different algorithm from the literature (Liu and Zheng, 2019) in section 3.2 and Appendix C.
- The clutter mitigation (section 3.1) is explained in more detail, with a dedicated appendix to describe the choice of the threshold |ΔS|.
- The removal of the range sidelobe artifacts is now illustrated by a clearer figure. The reasons behind the existence of these interference lines are also better explained.
- The application of the mode merging to the Ka-band is shown in an additional figure and briefly discussed in the text, clarifying one of the comments for the previous review.

These large modifications are accompanied by many smaller ones, consisting mostly of clarifications and improvements in the English language of the text.

I recommend the article for publication after addressing the following minor issues.

Specific issue

- Section 5.3.1
  As briefly mentioned in the introduction of the review, I think that the quantitative evaluation of the clutter removal is a useful addition that improves the quality of the manuscript.
  In my opinion, however, some of the terms used in the comparison should be renamed to better reflect the true nature of the evaluation presented in this subsection.
  In particular, the name "true data" for the median of the decluttered products (section 5.3.1) may be misleading.
  These measurements labeled as "true data" are not a set of independent observations of better quality (as could be, for example, the variables from another radar with better sensitivity). Instead, they are simply a combination of the best products that the proposed algorithm is able to generate. Therefore, I would suggest to re-phrase some parts of this section, to highlight that the results presented here are a measure of the impact of the decluttering in each mode, but not an estimation of how much closer the processed data are to the true meteorological signal.

Technical comments

- Lines 113-116
  The information on cross-calibration is a useful addition. For completeness, I would add the values of the reflectivity offsets computed.

  I also have a small question regarding the computation of the offsets: are these offsets computed on unprocessed data?
  If this is the case, what would the effect of the not-yet removed artifacts (clutter, side-lobes) be on this offset? Would the spectral power of the non-meteorological signal be included in the reflectivity of the unprocessed data, affecting the value of the offset, given the difference in the artifacts between the different modes undergoing the cross-calibration?
  I would expect the effect of the non-meteorological signal to be small, but you could check whether it can be truly ignored by re-computing the offset on your data after processing and then comparing this second offset with the one you computed from the unprocessed ones. I do not think that this additional check should be included in the manuscript, but it could be useful to the authors to verify the cross-calibration.

- Line 165
  The whole explanation of the choice of the threshold on $|\Delta S|$ is a great addition to the manuscript, providing an answer to one of the specific comments mentioned in the previous review.
  Regarding the whole explanation, I have only one small technical comment: the existence of "Appendix A" (detailing the analysis conducted on the $|\Delta S|$ distribution) is only mentioned in parenthesis after referring to the figure A1. Moreover, the appendix is not referred to as "Appendix A" but only as "Appendix".
  I would explicitly mention in the main text that the analysis is provided in the appendix, and I would refer to the latter as "Appendix A", to avoid confusion with the other appendices.

- Lines 259-269
  The addition of a comparison with the algorithm from Liu and Zheng (2019) illustrates the advantage of using the new method proposed by the authors.
  However, I find the last part of Section 3.2 difficult to read, due to the constant references to a figure from the Appendix.
  In my opinion, it would be better to relegate the discussion on the figure to the Appendix, mentioning only a summarized version of it in the text and referring to Appendix C for more details. Alternatively, if the authors want to keep the explanation in the main text, I would move Fig. C1 to the main text as one of the figures of Section 3.2.
  In both cases, I think that "Appendix C" also needs to be mentioned in the main text (similarly to what I wrote in the previous comment regarding Appendix A).

- Line 361
  Why the comparison is done specifically with mode 3? Is it because of its smaller blind range? In my opinion, it could be useful to add a brief explanation behind the choice of this mode in the text.
  I also noticed that other modes (e.g. mode 4, in line 368) are mentioned in the text as a target for the comparison. Why is mode 4 not mentioned alongside mode 3 at the beginning of the section?

- Line 371
  As for Appendix A and C, I would mention Appendix D explicitly in the text.

- Lines 374-375
  "Namely, no meteorological signals present in the range of [...]"
  I believe that the phrase is missing an "are", and it was supposed to be:
  "Namely, no meteorological signals <u>are</u> present in the range of [...]"

- Line 395
  What would be the results if another variable (e.g. reflectivity factor) was used for the comparison? In case you tried the comparison, would it show any improvements linked with the removal of spurious side lobes (which I would expect to be responsible for a slight overestimation of the reflectivity in the unprocessed data), or is the effect too small to be seen?

- Line 418
  Is the median here (and in line 415) performed for each range gate and time step separately? If this is the case, I am confused by the usage of the median as the metric, since it would be the median between only two values (at each gate and time step), and in that case, I think the average would be a better choice.

- Line 425
  The "1dbZ" may be a bit misleading here. For mode 2 the improvement is 0.36, for mode 3 it is 0.8 and for mode 4 it is 0.65. I would re-phrase this statement more accurately as:
  "[...] the SD for the reflectivity at Ku-band is reduced by a value between 0.36 and 0.8 dB after imposing the clutter removal algorithm."
  The difference between two reflectivity factors in dBZ is expressed in dB, so the unit for the difference should be changed.

- Lines 440 – 441
  In my opinion, Appendix B (and D, if not introduced previously) should be introduced explicitly here, with a short phrase detailing the content/objective of each of them.

- Line 467
  Since the applicability of the method in snowfall has not been explicitly shown in the manuscript, I would change the phrase stating clearly that the correct functioning of the algorithm in snowfall is expected but has not been proven yet.

- Line 470 – Appendix A
  I suggest the addition of a short text in Appendix A for explaining the context of Figure A1.

- Line 480
  Is the standard deviation here computed in a similar way as in section 5.3.1?
  In general, I would expand slightly this Appendix, explaining the procedure more extensively, and clearly stating the objective of the comparison (i.e. finding the value of alpha that minimizes the SD).

- Line 499 – Appendix C
  Same comment as for Appendix A.

- Line 513
  How is the height of the peak and sidelobe determined?
  From the figures in the manuscript I expected the sidelobe to span multiple range gates, is its height set as the average of all heights affected? I think that the procedure should be briefly explained in the appendix.

- Line 515
  The terms "main lobe peak power" and "sidelobe peak power" should be explained. Is "sidelobe peak power" the same as the term S_{peak} previously introduced?