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 $|\Delta S|$.
- 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.

The authors would like to thank the reviewer for all the comments on the manuscript, and all of them will be addressed in some manner. Please see the point-to-point response below in blue color.

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.

We thank the reviewer for the good suggestion. Given that the standard deviation was calculated relative to a result that we consider to be relatively accurate, the name “true data” has been replaced by “reference data”.

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.

  Yes, the reflectivity offsets between different modes were computed on unprocessed data. The weak and stable precipitation cases were selected and observations from mode 2 were used as a reference due to high sensitivity, then the reflectivity difference was computed. For each profile, the median of the reflectivity difference was recorded and the values of offset were determined as the median of all the recorded results.

  We did check the offset before and after the decluttering, and the clutter has very minimal impact on the calculation of the offset. This is expected, since the occurrence of non-meteorological signals is much smaller than rain.

  The values of the reflectivity offsets have been added to the revised manuscript.

  “For both radars, the reflectivity observations at mode 2 were used as the reference to calibrate radar data at other modes. The reflectivity offsets are 3.8 dB (mode 2 - mode 3) and -3.6 dB (mode 2 - mode 4) at Ka-band, respectively. For the Ku-band radar, these values are 7.5 dB (mode 2 – mode 1), -1.0 dB (mode 2 – mode 3) and -2.9 dB (mode 2 – mode 4), respectively.”

- 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 |ΔS| 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.

Appendix A has been explicitly mentioned in the revised main text.

- 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).

To make the article clear and easy to read, we have moved the analysis in the last part of Section 3.2 to Appendix C, and a summary clarification was added to the text.

"Furthermore, we have compared this algorithm with the threshold method (Liu and Zheng, 2019), all the results and analysis are included in Appendix C."

- 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?

We agree with the reviewer. The comparison with mode 4 has been added in the revised manuscript.

- Line 371

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

The text has been revised to "In addition, we have calculated statistics of the power leakage to range sidelobe, and the results for Ku-/Ka-band radars are given in Appendix D (Fig. D1)."

- 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 are present in the range of [...]"

  Corrected.

- 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?

  We did give thinking on this. However, the radar frequencies are different, and different radars suffered from different wet radar radome and rain attenuation. It is rather challenging to do the attenuation correction for the profiles of Ka and Ku band radar reflectivity observations.

- 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.

  We agree with the reviewer. It has amended to average. In addition, we have fixed a mistake in our code and updated the values in Table 3.

- 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.

  Thank you very much for this suggestion, the statement has been re-phrased to "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."

- 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.

  Appendix B was introduced in Line 245 and Appendix D was introduced in Line 387.

  The text has been revised as "Radar observations from 4.5 to 6 km are used for the assessment, and the results for the Ku-band radar are given in Appendix B (see Tab. B1 for details). Since the signals associated with sidelobe are relatively weak (Fig. D1 in Appendix D) […]"

- 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.

  The statement has been revised to "[…], the applicability of the presented framework 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.

  "The meteorological signals with a height of 2 ~ 3 km and Doppler velocity of 2 ~ 5 m s-1 were statistically analyzed to determine the appropriate |ΔS|." has been added in the revised manuscript.

- 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).

  Yes, the standard deviation was computed in a similar way as in Section 5.3.1. To make it clear, we added some descriptions in the revised manuscript.

  "A similar quantitative evaluation can be made to find the appropriate value of α to maximize the sidelobe mitigation. Since the Doppler spectra observations at mode 3 for both radars are not affected by the sidelobe effect, they are used as the "reference data" at both Ku- and Ka-band. That is,

$$X_{Ku/Ka,ref} = X_{Ku/Ka,M3}^{mitigated} \tag{B1}$$

Then, the standard deviation between spectral moments with different α and observations at mode 3 was calculated through Eq. (B1) and compared. Radar observations between 4.5 km and 6 km were evaluated, and the results for Ku- and Ka-band radars are given in Tab. B1 and B2, respectively."

- Line 499 – Appendix C

  Same comment as for Appendix A.

  All the analysis of appendix C has been moved from the main text to the appendix.

- 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.

  The height of peak spectral power refers to the height of the range gate with maximum spectral power for each velocity bin in a spectra profile, and the height of sidelobe refers to the height of the range gate that is identified contaminated by range sidelobe. The procedure has been briefly explained to the appendix.

  "This appendix shows how much power is leaking into the range sidelobes. For a given velocity bin in a spectra profile, the maximum spectral power is denoted as S_{peak}, and the corresponding height is H_{peak}. Then, the spectral power of sidelobe is denoted as $S_{sidelobe}$ and the height as $H_{sidelobe}$. The difference between S_{peak} and $S_{sidelobe}$ and the corresponding H_{peak} and $H_{sidelobe}$ were analyzed."

- 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?

  The peak sidelobe ratio measures the waveform after pulse compression, it is different from the term S_{peak}.

  These terms have been clarified in the revised manuscript.

  "The theoretical peak sidelobe ratio (the ratio of the main lobe peak power to the highest sidelobe peak power) depends on the transmitted waveform after pulse compression, and is 36 dB and 30 dB for mode 2 and mode 4, respectively."