Review for the manuscript "Improved spectral processing for a multi-mode pulse compression Ka/Ku-band cloud radar system" by Ding et al.

The manuscript describes a set of processing techniques applicable to spectral observations from cloud radars. The techniques include clutter filtering, mitigation of artefacts resulting from pulse compression, and merging of observations taken with different pulse modes.

The manuscript has many flaws, to name few:

1. The authors claim "…the results show good performance of clutter/sidelobe suppression and spectral merging", but the manuscript completely lacks any quantitative evaluation of the proposed methods.
2. The described methods contain a number of decision rules (e.g. Figs. 2 and 6). Intuitively, these rules may sound to be reasonable. I, however, would certainly expect a statistical justification of the used rules. For instance, the authors write "The selection of deltaS=3dB is a compromise between the radars' observation uncertainty and the spectral ratio between different observing modes." I do not think this is enough. First, observational uncertainties depend on the operational settings. What if I use different settings on my radar, shall I change the settings to the ones used for the manuscript in order to apply the method? Or how shall I adapt the method to use it with different settings? Second, which rule was used to get the compromise? What I, as a radar operator, would like to see is, for a given pulse modes, what is the probability not to filter clutter? What is the probability to erroneously filter out a spectral line with meteorological signal only? How these probabilities depend on the pulse modes? How would these probabilities change if different thresholds are used? There are no answers in the manuscript.
3. All methods are illustrated using rain cases. How would these methods work under other conditions? For example, would the clutter-filtering algorithm still be able to discriminate between a thin liquid layer cloud with highly variable reflectivity and non-meteorological targets? I would expect that the performance in the statistical sense changes and I want to know how, before I apply the proposed method. Also, it is hard to say how well the side-lobe mitigation algorithm would perform in solid precipitation because there will be no clear separation between the sidelobe and meteorological signal as in case of rain right above the melting layer.
4. In the case of a novel processing technique with a number of subjectively chosen parameters, I would at least expect a comparison to a reference radar which does not have artefacts in measurements (e.g. a magnetron-based cloud radar).
5. Since the authors mention that there are alternative techniques available to mitigate side-lobes. I would also expect a comparison of the proposed methods with the available one.

Based already on these points I conclude that the manuscript is not ready for a further more detailed review and I recommend rejection.