Dear Editor,

we would like to thank the two anonymous Referees for their comments, which helped us to clarify the presented analysis.

Based on the Referees' comments, more details have been included in the revised manuscript, especially to provide additional context for how our work relates to previously presented spectral noise and clutter filters (Sections 2.2 and 2.3). We have also modified Figs. 5, 8, 10, 11, 12 for better readibility and added some details to the description of our methodology (Chapter 2).

We enclose point-by-point responses to the comments from the Referees, including line numbers that indicate changes in the revised manuscript. Additions and modifications to the previous manuscript in response to the comments from the Referees are also highlighted by blue text in the marked-up .pdf version of the revised manuscript. Minor modifications that were made throughout the text to maintain consistency are not highlighted.

With best regards,

Mathias Gergely, Maximilian Schaper, Matthias Toussaint, Michael Frech

Response to Referee 1:

Referee 1:
I found the paper to be well written, presented, and makes a useful contribution to scientific progress within the scope of Atmospheric Measurement Techniques, however I believe there are several areas in which it could be improved, either to provide improved consideration of previous related work, clarity for the reader, or presentation of results. These are detailed in the attached document.

Authors:

Thank You for reviewing our manuscript. Based on Your comments and suggestions, we have now provided more context in terms of how our work relates to previously presented radar noise and clutter filters, and we have modified the presentation of some of our results for clarity. Point-by-point replies and the corresponding changes in the revised version of our manuscript are listed below.

Referee comments in the attached document:

• line 2: Are BB scans as part of a scan strategy still considered novel or a standard tool for Zdr calibration? While the addition of the spectral measurements are novel, I'm not sure that the scan itself is.

As Referee 1 notes, the attribute 'novel' was originally meant to specify how we use the birdbath scans, i.e. to analyze the corresponding Doppler spectra, and not to imply that birdbath scans themselves are a novel scan strategy that has never been explored in radar analysis.

To avoid confusion, we have replaced the specifier 'novel' simply by 'new' (because we updated the settings of our previous birdbath scans to be able to use the Doppler spectra) in the revised manuscript in line 2 and instead moved the attribute 'novel' to describe the newly developed postprocessing scheme in line 4. Additionally, we updated the rest of the text and the short summary accordingly.

• line 108: This may be the sample resolution but with a 0.4uS pulse the intrinsic range resolution is 60m. This should be clarified. If benefits are expected from the range oversampling this should be stated.

We use oversampling at 25 m, which is the finest range sampling interval available, to have the most spatial structure in the radar data available for our analysis.

Our use of oversampling is now explicitly stated in the text in line 108f and in Table 1.

• line 125f: This sentence feels as though it is dangling ... "and therefore a relatively fast/short scan speed/time is required" ?

The sentence was rewritten in line 124ff to provide more context, following the suggestion of the Referee. For better text flow, the sentences in the paragraph were also rearranged.

• line 149, Fig. 1: Consider making this clear in the caption.

Following the suggestion of the Referee, the information was also included in the caption of Fig. 1 in the revised manuscript.

• line 151: could the phrasing be clearer - stationary ground clutter?

Following the suggestion of the Referee, the 'clutter at 0 ms-1' was specified as 'stationary ground clutter at 0 ms-1' in line 153 of the revised manuscript.

• line 169: Is this not just a spectrally decomposed Zdr? Perhaps mentioning Zdr would help the reader.
Were the polarmetric spectral filters suggested by Moisseev, D., and Chandrasekar, V. 2009. Polarimetric Spectral Filter for Adaptative Clutter and Noise Suppression considered, and if so could it be stated as to why would they not be appropriate in this case, or the relative benefits given?

This is the absolute value of uncalibrated spectral Zdr. Following the suggestion of the Referee, we have added an explanation to give the reader a better idea of what the polarimetric parameters mean in line 171ff.

The paper mentioned by the Referee presents an interesting approach to achieve a similar separation of the weather signal from clutter and noise in Doppler radar observations at low elevation angles as is presented in our study for DWD's vertically pointing birdbath scans. One of the polarimetric parameters (texture of sZDR,u) is very similar. However, we do not have the parameters that are derived from the cross spectra available for our analysis. Instead, looking vertically upward lets us use sZDR,u itself (in addition to its texture) as an indicator for non-weather signals.

To provide more context for our approach, we have now also included a brief reference to M+C, 2009, and briefly discuss differences + similarities to our method in l. 174ff of the revised manuscript.

• line 174: From figure 3 the minimum feature size appears to be ~150m rather than the 75m I might have expected from this description. Is this due to the range correlations due to oversampling or is this a processing effect?

Looking at Fig.3 in detail reveals a wide range of apparent 'features' and their sizes, from vertical dimensions of 25 m up to over 200 m. Only in the near-field may there be a consistent 'minimum' feature size present across all Doppler velocities. Higher up, there is a variety of sizes present. Furthermore, features that one may subjectively discern by eyeballing extend across different range bins for different Doppler velocities (i.e. the bases or the centers of features at similar heights are usually shifted up or down with respect to each other). This high variability does not suggest a systematic effect due to oversampling. The polarimetric parameters calculated before smoothing by grayscale closing also do not suggest any systematic feature size.

In terms of processing, using different structure element sizes clearly leads to different granularity, i.e. apparent features, in the images, but does not strongly affect the sharp contrast between weather signal and non-meteorological contributions, which is the crucial characteristic for separating the two signals.

• line 204: Does the noise showing non-zero difference for "Zdr(v)" in figure 4 suggest a receiver noise difference between the H and V channels? Should we expect zero difference for well matched Rx. channels or do I misunderstand?
Were other spectral noise filtering approaches considered, such as the GMAP rank order stats. based one or is it unsuitable in this case?
The GMAP approach seems similar to the UniDip. Does this give scope for simplification?

We are showing an uncalibrated version of spectral Zdr, as described in l. 171f, (which is now explicitly mentioned in the revised manuscript). Only for a theoretical model of an idealized radar, I would expect zero difference. Nonetheless, the crucial point for applying the algorithm here is not the exact value of the difference between H and V channel but the clear separation of clusters of 'weather' and clusters of 'clutter' and 'noise' based on the 3 parameters, as described in l. 191ff, 222ff.

GMAP (or similar noise filtering algorithms) and UniDip are fundamentally different algorithms and, up to now, applied to different types of signal-processing 'problems': GMAP tries to identify noise vs. clutter vs. weather based on prescribed numerical thresholds used in the rank order spectra and under the assumption of a Gaussian clutter signal, while also presupposing a strictly unimodal Gaussian weather signal; UniDip tries to find (all statistically significant) peaks (within a noisy and potentially multimodal signal) independent of the functional form of each peak, as described in l. 269ff. UniDip is applied after clutter and background noise have already been removed from the signal.

To provide more context for our approach, we have included additional information about why we do not use other algorithms in l. 263ff (GMAP) and l. 174ff (M+C, 2009, as discussed above) of the revised manuscript.

• line 354ff: Is there any trade off to be made between the number of DFTs averaged and this? Given the high velocity resolution from the large number of points in the DFT, followed by a high degree of subsequent smoothing is further optimization possible here or is the ~1s per DFT sample time chosen to match the de-correlation time of the sample volume in the vertical?
"impact" rather than "imapact".

In general, further optimization is always possible. The question then is optimization for what type of precipitation, for what precipitation intensity, for what overall operational scanning cycle, for which Doppler moments. The main requirements and tradeoffs for our intended applications are discussed in l. 122ff, leading to a very feasible ~1s per DFT sample time as an adequate compromise. We see in our analysis that these radar settings allow us to cover a multimodal analysis from light snowfall to intense rainfall. As higher-order Doppler moments are generally more affected by atmospheric air movements, we focus on lower-order Doppler moments in our work and in this article for analyzing precipitation processes.

Currently, we are investigating the possibility to analyze hailstorms with DWD birdbath scan data. Here, a modification to the radar settings could be benefitial to avoid having to unfold Doppler spectra of hail falling faster than 13 m/s, for example. However, this is not the immediate topic of this manuscript and may be discussed in a future study.

Spelling of ‘impact’ was corrected.

• Fig. 8: This would be much clearer if the vertical scales were aligned.

Based on the comment of the Referee, the vertical scales of the subplots in Fig. 8, as well as in Figs. 10, 11, 12 are now aligned in the revised manuscript.

• line 445: Fig 11 seems more like a textbook typical stratiform case to me. In figure 10, the increase in reflectivity below the bright band seems to suggest additional processes.

While Fig. 10 also contains a clear melting layer as defining feature of stratiform precipiation, we have deleted the characterization of Fig. 10 as ‘typical stratiform precipitation’ from l. 457ff in the revised manuscript to avoid confusion. We have also deleted the word ‘stratiform’ when specifically referring to Fig. 10 throughout the Results and Discussion and the Conclusions.

• line 591f: The link to operational impact is good however it is unclear without further detail as to how the BB scan data contribute to storm tracking. In particular in reference to "coarser spatial resolution" of the tracking, where it may be thought that the BB scans are a point measurement in the context of cell tracking. Presumably this is referring to vertical resolution but then the connection to tracking is not obvious.
Is this is in relation to the use of 3D radar reflectivity in the DWD Nowcasting or severe weather detection systems or something else?

As the Referee points out, it is difficult to describe clearly in few sentences how the birdbath scans can be used for complementing current operational services. The idea here is not to help with tracking storms, but to evaluate and optimize the current operational analysis from scanning polarimetric measurements (and short-term weather forecasting), based on several case studies where the storms move directly over one of the DWD radar sites, i.e. where coincident high-resolution birdbath-scan measuremens are also available. Here, Doppler spectra birdbath scans as profiler measurements provide a better understanding and identification of the precipitation process, while polarimetric information from volume data may fail to provide an unambiguous classification and detailed quantification of the precipitation. Therefore, birdbath scans can initially be used to verify and improve nowcasting algorithms or may eventually be included as part of the algorithm in the future.

This idea is not directly related to using 3D radar reflectivity in DWD Nowcasting, but more generally applicable to test and evaluate current (or future) operational analysis methods for several test cases where the storm also moved directly over one of DWD's radar sites.

The final paragraph of the manuscript was rewritten to describe this train of thought more clearly, see line 607ff.

Response to Referee 2:

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Referee 2:
This manuscript is well written and organized. The authors present a technique to use
vertically-pointing dual-polarization data to automatically isolate the precipitation
signal in the data. This single-polarization precipitation data is the analyzed to
obtain information about the precipitation overhead, including an estimation of
riming. This paper is a good contribution to radar meteorology and should be published
in Atmospheric Measurement Techniques. There is room for improvement and several
details that should be clarified. These are detailed in the attached document.
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Authors:

Thank You for reviewing our manuscript. Based on Your comments and suggestions, we have provided additional details of our methodology and modified the presentation of some results for clarity. Point-by-point replies and the corresponding changes in the revised version of our manuscript are listed below.

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Referee comments in the attached document:
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• line 15: I assume RMF is rime mass fraction.  It should be defined.
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Following the Referee's recommendation, RMF is now explicitly defined in the abstract of the revised manuscript in line 15.

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• line 69: birdbath?
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Yes, thank you. Corrected in line 69.

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• line 115: The Blackman window should be better defined.  I refer the authors to
Harris 1978 (F. J. Harris, "On the use of windows for harmonic analysis with the
discrete Fourier transform," in Proceedings of the IEEE, vol. 66, no. 1, pp. 51-83,
Jan. 1978, doi: 10.1109/PROC.1978.10837.) for clarification.  Are you using the
Blackman window of Harris figure 22 or figure 23?  Both of these windows are excellent
windows, but they are not the same.
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Thank you for pointing us to this excellent overview of window functions used in signal processing by F. J. Harris. After comparing the numerical values of the coefficients for the window function used in our signal processor with the different flavors of Blackman windows described by Harris 1978, we find that we use Harris' 3-term Blackman-Harris window that achieves the minimum sidelobe level of -67 dB (shown in his Fig. 24).

Based on these findings, we have specified the flavor of Blackman window in the revised manuscript in line 117f, following Harris 1978, and also included a reference to this window function in his paper.

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• line 116: I suggest deleting this sentence as it doesn't add to the discussion.
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We followed the Referee's suggestion and deleted this sentences in the revised manuscript.

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• line 128: corresponding?
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Yes, corrected in line 128.

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• line 174: SciPy is a large package.  Please be more specific and cite the routine or
routines that you use.
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We use the ndimage.grey_closing() routine in SciPy. As requested by the Referee, the name of the grayscale closing function is now mentioned in the text in line 183.

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• line 175: smooth?
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The sentence was rewritten in line 184f to better express the idea that grayscale closing was chosen to essentially fill small holes, or deep minima, in the radar data.

• line 175: high?

As noted for the comment above, this sentence was rewritten in line 184f to better express the idea that grayscale closing was chosen to essentially fill small holes, or deep minima, in the radar data.

• line 225: Figure 4 would be better if it had C1, C2, and B1 labels on the lines.

Following the Referee's recommendation, we have now included the 3 different threshold identifiers as labels in Fig. 4 and also list them in the caption.

• line 228: Is the interpolation done in linear power space or log (dB) space?

The interpolation is done in dB space.
We have included this information in line 238 in the revised manuscript.

• line 234: The weather signal has been isolated very nicely. Now you analyze the part of the spectra that compose the weather signals. Do you use data from one polarization or are data from both polarizations combined? If combined, how are they combined?

We use only the data in the H polarization channel for the analysis. The V channel does not add new insights to the meteorological interpretation.
To better explain the final interpolation step for isolating the weather signal for subsequent quantitative analysis, the corresponding paragraph was rewritten in the revised manuscript (see line 236ff). In the revised manuscript, we also remind the reader in line 243f that only the filtered and interpolated data in the H polarization channel are used as the weather signal for analysis.

• line 235: weather?

Yes. Corrected in line 245.

• line 272: precipitation?

Yes. Corrected in line 285.

• line 289: I suggest replacing "a suitable" with "the UniDip". You have established this as the technique you are using for you proceesing. There may be other suitable methods but here the issue is what you are doing for the processing.

As the Referee states, being more specific here is probably better. So, we replaced 'a suitable' with 'the UniDip' in line 302 of the revised manuscript as suggested.

• Fig. 6: As a reader I find this hard to compare with Fig. 5a since the y scales are different. My recommendation would be to the y scale of Fig 6 for Figure 5a. This would remove a lot of "white" space from 5a and allow direct comparison.

The original idea was to have the same y-axis scales for Figs. 1, 3, and 5a, i.e. for all postprocessing steps applied to isolate the weather signal. But, as Referee 2 notes, it makes probably more sense to instead have consistent y-axes for Figs. 5a and 6 (and 7) to better compare the analysis results with the isolated weather signal.
Therefore, we followed the recommendation of the Referee and modified the y axis of Fig. 5a, which is now consistent with Fig. 6.

• line 313: The authors have all the data and processing. Rather guessing about the cause of the multimodality could the original and intermediate data be examined to determine the cause? This multimodality could be an artifact of the processing, or it could be caused by some physical process that might tell more about the meteorology.

For these very weak Doppler powers (compared to the main precipitation signal at lower altitudes), a definitive statement is difficult. This ambiguity in interpreting our analysis results at very low Doppler powers was originally implied by the use of the qualifier 'probable'. This conclusion is also consistent with finding generally higher uncertainties for the calculated quantitative characteristics of the spectral modes at higher altitudes in our uncertainty assessment in line 362f. Nonetheless, a detailed comparison of radar output, postprocessed data, and quantitative characteristics, as recommended by the Referee, indicates that this multimodality is indeed an artifact of the postprocessing, and of the interpolation routine in particular.

In the revised manuscript, we have addressed the Referee's comment by rephrasing the corresponding text passage accordingly (see line 325ff).

- line 316: Is there are reference to atmospheric turbulence or are you assuming atmospheric turbulence is the cause?

In this specific case, we assume atmospheric turbulence is the cause, because we are looking at the initial more convective phase of a snowstorm. The analysis of birdbath scan measurements before and after the birdbath data analyzed in the manuscript also suggests variable vertical air movements over a short time scale with various degrees of spectral broadening, characteristic of turbulence.

Since such a clear interpretation is not evident only from the one example of birdbath data and analysis presented in the manuscript, we do not include our specific interpretation as turbulence in the revised manuscript (deleted reference to turbulence in line 331).

- line 328: Is BMA bimodal amplitude? It needs a definition.

Yes. BMA is now defined in line 342 of the revised manuscript.

- line 330: relatively?

Yes. Corrected in line 345.

- Fig. 8, 10-12: This would be much easier to understand of the color scale were at the left or right of figure a so the plots were all the same size with the same y axis scales. This comment applies to Figures 10-12 also.

Based on the Referee's comment, the vertical scales of the subplots in Fig. 8, as well as in Figs. 10, 11, 12 are now aligned in the revised manuscript.

- line 422: analysis?

Yes. Corrected in line 437.

- line 564: Figure 12 appears to me to be affected by strong vertical motions, not atmospheric turbulence. Turbulence affects the width while vertical motions changes the mean fall velocity of the precipitation.

Fig. 12a alone does not show clearly that turbulence also plays a role here, because some parts of the fringes of the weather signal are cut off by the postprocessing algorithm. This loss at the fringes is the cost of rejecting most of the clutter and background noise here to isolate a relatively 'pure' weather signal. We also state explicitly in the discussion of Fig. 12 the strong impact of atmospheric air movements (causing the striking zig zag shape of the Doppler spectra profile, see line 535ff of the revised manuscript).

To not suggest that the characteristic shape of the Doppler spectra profile in Fig. 12 is mostly due to turbulence and not due to vertical air motions we have replaced 'turbulence' by 'vertical air motions' in line 580 in the revised manuscript. And for completeness, we have added a sentence about the loss of the fringes of the weather signal due to the postprocessing in the discussion of Fig. 12 in line 519ff.