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.

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• 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?
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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.

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• 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?
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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.

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• 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".
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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.

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

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• 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.
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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.

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• 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?
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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.