This study examines the relevance of hydrometeor inertia for the accurate simulation of radar Doppler spectra. The authors develop a novel approach to simulate Doppler spectra based on the equations governing the motion of hydrometeors, and compare it with the traditionally used convolution-based approach. They find that the traditional approach tends to overestimate the degree of broadening associated with inertial particles, while it correctly represents the degree of broadening for particles whose inertia is low enough for them to act as tracers. The authors then compare spectra simulated both with their novel approach, and with the traditional approach, with one observed spectrum. The comparison displays a better matching between the observed spectrum and the spectrum simulated with the author's approach.

I find the research question of the study to be significant for the cloud radar community, and the novel approach developed by the authors to be sound. The figures are polished, and the overall structure of the manuscript is well layed-out, clearly displaying the reasoning process of the authors. However, I find several sentences in the manuscript to lack in clarity or to have been poorly written, and improvements in this regard are needed. See my detailed comments below.

The only major flaw that I found in the study is in the comparison with observations. The comparison was performed only for one observed sample (one Doppler spectrum). In my opinion such a limited validation is not sufficient to prove the accuracy of the approach developed by the authors. The validation with respect to observations needs to be considerably expanded, and should be performed on a statistical basis using a large number of Doppler spectra, if possible recorded during a few separate events.

I do understand that the authors might want to include such an extensive validation in a follow-up study, but if that is the case, the comparison with observations in section 5 should be framed as a simple illustrative exercise, instead of a proper validation analysis. I included below a list of changes that I deem necessary if the authors do not intend to expand the comparison with observations in the current study. I anyhow strongly recommend that such an extensive validation is included in the current manuscript, as it would make the whole study substantially more sound.

In conclusion, I recommend this study for publication in AMT after major revisions: clarity of the text needs to be improved and I recommend either that the comparison with observations is considerably expanded or the text is adjusted so that such comparison is not presented as a complete validation.

Response: We want to thank the reviewer for the detailed edits and for the constructive suggestions. We agree with reviewer that only one comparison example is insufficient to make a robust validation statement. A solid and comprehensive evaluation of the Doppler spectrum simulator would require observation from different cloud/precipitation scenarios: cloud-drops only, drizzle, light precipitation, heavy precipitation etc. However, as what we have discussed in the revised manuscript, such validation effort would require high-quality of DSD observation and turbulence broadening estimation. Unfortunately, the observation what we have obtained from the current instruments have relatively large uncertainties and cannot meet these high standards. To this end, we prefer to present this comparison section as an illustrative example, while with more focus being placed to the discussion of the uncertainties should be considered for a careful Doppler

spectrum simulator validation effort. We also hope this discussion can promote more suitable observational datasets in future field campaigns which can be used for robust Doppler spectrum simulator validation.

Changes needed if the validation is not expanded

The sentences at lines 19-23 in the abstract (from "Doppler spectra observed …" until "...morphology") need to be removed. The comparison with observations should not be mentioned in the abstract as it doesn't have scientific significance.

Response: The related sentences have been deleted from the abstract.

The sentence at lines 84-85 ("section 5 uses … simulator") needs to reflect the fact that the comparison presented in section 5 is not a validation, but a mere illustrative exercise. Additionally the phrasing "real observed Doppler spectra" is not accurate and should read "one real observed Doppler spectrum".

Response: Changes have been made in the revised manuscript:

Line 84: "…in section 4 one observed Doppler spectrum is used as an illustrative example to compare the Doppler spectrum generated from the two simulators…"

The text in section 2 should be moved to section 5 (or an appendix), as it is not relevant for the main topic of the manuscript, which is the approach development. The text could also be condensed.

Response: Section2 and section 5 in the previous manuscript are combined to section 4 in the revised manuscript.

The title as well as the text in section 5 should reflect the fact that only one observed spectrum is used in the comparison instead of multiple spectra. The singular "spectrum" should be used instead of the plural "spectra".

Response: The title of section 4 in the revised manuscript is modified as:

Line 368: "An illustrative example of Doppler spectrum comparison between observation and simulation"

The sentences at lines 373-377 need to be rephrased in a more careful manner, as the simple analysis shown does not provide enough evidence to support these statements.

The same applies to lines 410-415, 418-419, 421-422 in the conclusions.

The conclusions need to clearly state that the new approach needs to be systematically validated against observations, and that the applications suggested at lines 422-427 may only be looked into after the accuracy of the approach is demonstrated against observations.

Responses to the previous three comments:

We have incorporated the reviewer's comments and rephrased the Doppler spectrum comparison section (section 4) in the revised manuscript. We have clarified that the comparison is not used for validation purpose but serving as an illustrative example. We have also made a thorough discussion on the uncertainties involved in this validation framework and the datasets would be required for future validation effort.

Line 419: "…The purpose of this Doppler spectrum comparison is not for a robust validation but used as an illustrative example to show the morphology of the simulated Doppler spectrum in real environment and to discuss the required measurements needed for robust Doppler spectrum simulator validation. To a certain degree, a more consistency Doppler spectrum morphology is identified between the observation and from the PBS simulator, especially for the right edge of the spectrum. However, great cautions should be taken for further interpretation as both of the simulators cannot represent the left part of the Doppler spectrum and the second notches very well. This discrepancy is mainly because the observed DSD by disdrometer may not an adequate representation of the hydrometeors that contribute the Doppler spectrum observed by WACR. Specifically, there are three critical challenging issues should be overcome before a solid and convincing Doppler spectrum simulator evaluation effort being performed: 1) the disdrometer is located at the surface, while the lowest measurement height of WACR is 460m. When the rain droplets fall, droplets may collide, breakup, and being advected from adjacent regions by the horizontal wind; Thus a large uncertainty is expected by using the surface-observed DSD to represent the hydrometeor distribution at 450m above; 2) the observed DSD from the disdrometer only measure droplets with 20 size categories, which is insufficient for the physics-based simulation to generate a smooth and complete Doppler spectrum; 3) the uncertainty of the estimated σ_t is challenging to be well constrained due to the large uncertainty of the observed DSD mentioned above. A comprehensive and solid validation of the Doppler spectrum simulator require simultaneous and well- aligned DSD and Doppler spectrum measurement; large number of the measured droplet size categories and carefully estimation of the environment…"

Further scientific questions/issues

Throughout the whole text the term "fall velocity" or "droplet velocity" is used as synonym for "still-air terminal velocity" (e.g. at lines 56, 195, 310, ...). This is incorrect and should be adjusted. Throughout the whole text the term "quiet air" is used (e.g. lines 63, 211, 243, ...). The term "still air" is far more commonly used and I recommend that this is used instead.

Response: We thank the reviewer's comments. We have adapted the term "still-air terminal velocity" where it applies in the manuscript.

Throughout the whole text the term “movement” is used as synonym for “motion” (e.g. at lines 82, 102, 105, ...). This is incorrect and should be adjusted.

Response: Corrections have been made throughout the manuscript.

Throughout the whole text the verb “resolve” is used when referring to simulated drop velocities (e.g. at lines 106, 215, 265, 270, 400, 418, ...). I find this ambiguous because the actual particle velocity is not observed in any way, but an artificial velocity value is produced and then used to calculate the Doppler spectrum. Therefore I suggest this is adjusted, e.g. by replacing “resolve” with “simulate”.

Response: We thank the reviewer’s suggestion. Corrections have been made in the manuscript where we consider it is appropriate for such replacement.

References should be given for all formulas in sections 3.1 and 4.1.

Response: References have been added to the equations.

Throughout sections 3.1 and 3.2 it is not stated in the text whether gravity was included in the simulations. I assume it is not since the corresponding term is missing in eq. (1). Its inclusion or omission should be stated explicitly. If it is included, eq. (1) should be adjusted accordingly.

Response: In the revised manuscript we have considered the gravity in the physical framework (Eq.1). The gravity is included in the Doppler spectrum simulation (section 3 and 4), but it is not included in section 2.2. We have clarified this statement in the revised manuscript:

Line 154: “…We first illustrate the inertial effect by calculating droplets motion using a constant wind velocity. For simplicity, here we assume all the droplets are moving horizontally, thus the gravity (mg) is neglected in Eq.1…”

Lines 160-162: since the calculation of the artificial wind time series is an integral part of the proposed approach, the method by Deodatis (1996) should be briefly summarized here.

Response: We have added more details of the wind generation process in the revised manuscript:

Line 172: “…In this study we adapt the approach proposed by Deodatis (1996) by using the Spectral Representation Method (SRM) to generate the turbulent wind field based on a predefined Von Karman energy spectrum. The SRM is widely used in the wind engineering community due to its high accuracy, simplicity and computational efficiency. (Shinozuka and Deodatis, 1991;Zhao et al., 2021). Here, the 1-D turbulence wind is generated with 2 Hz sampling frequency, 1000s duration and with standard deviation of 0.3 ms^{-1}, the codes being applied to generate the wind field can be accessed from Cheynet (2020)…”

The same applies to the method used for the calculation of the broadening term sigma_t by Borque et al. (2016), mentioned at lines 247-248.

Response: We have added the equation for σ_t estimation in the revised manuscript:

Line 178: "…The selection of 0.3 ms^{-1} standard deviation is based on a quantitatively estimation of cloud radar observation under a typical cloudy environment. Specifically, for the convective cloud system with eddy dissipation rate (ε) of 5 ×10-3 m^2 s^{-3} (Mages et al., 2022), the turbulence-contributed Doppler spectrum width (σ_t) from a vertical pointing radar with 30m range resolution(ΔR) and 0.3° beamwidth (θ) at 1km height is estimated to be 0.27 ms^{-1} based on the equation from (Borque et al., 2016)

$$\varepsilon \approx \frac{{\sigma_t}^3}{\sigma_z(1.35\alpha)^{3/2}}\left(\frac{11}{15}+\frac{4}{15}z^2\frac{\sigma_x^2}{\sigma_z^2}\right)^{-3/2} \quad (1)$$

Where α is the Kolmogorov constant with 0.5, $\sigma_z = 0.35 * \Delta R$, $\sigma_x = \frac{\theta}{4\sqrt{ln2}}$, θ is the one-way half-power width with unit of radian. z is height above surface…"

When citing a book (e.g. Lhermitte 2002) the exact chapter or pages should be indicated.

Response: Changes have been made in the revised manuscript.

Line 241: the radar reflectivity $d\eta(D)$ (m^2/m^3) from particles with diameter between D to $D + dD$ can be expressed as (Lhermitte, 2002, p. 228):

Line 249: "…Here, the function proposed by (Lhermitte, 2002, p.120) is used to estimate V_t as a function of droplet diameter (D)…"

Figure 3a: what values were assigned to the initial velocities of the droplets?

Response: Changes have been made in the revised manuscript.

Line 195: "…The generated air velocity is assigned to V_w(Eq. (2)) to simulate the motion of droplets with initial velocity set as 0 ms^{-1}…"

Eq. (12): the symbol S_t appears here for the first time and it should be introduced.

Response: We have rephrased the description of the simulator framework. More details can be found in Section 3.2 in the revised manuscript.

Eq. (12) and line 270: if I understand the text correctly, here V_t is the turbulence-affected drop velocity, but the symbol V_t was previously used to indicate the still-air terminal velocity. I believe a new or different symbol should be used here instead.

Response: We have rephrased the description of the simulator framework. More details can be found in Section 3.2 in the revised manuscript.

Line 271: please clarify what the term "DSD Doppler spectra" means.

Response: We have rephrased the description of the simulator framework. More details can be found in Section 3.2 in the revised manuscript.

Lines 294-297: this sentence should be split and expanded. First the concept of Mie notch should be introduced. Then the fact that the Mie notch can be used to compare the two approaches should be explained.

Response: We have modified this part in the revised manuscript:

Line 308: "…The selection of W-band radar and the use of a rain DSD is because it is well known that the W-band radar Doppler spectrum in rain has distinct feathers which allow to pinpoint the Doppler spectrum morphology. Specifically, due to the Non-Rayleigh scattering, the backscattered power for rain droplets with specific radius is identified as local minimal value, this characteristics is manifested as the "Mie notches" in the observed Doppler spectrum…"

Figures 4 and 5b: the labels to the y axes should indicate the name of the variable ("spectral reflectivity" in this case) in addition to the unit. The unit is also misindicated as "dB(10log(mm6 m-3))", it should read either "dBZ" or "10log(mm6 m-3)".

Response: Changes have been made in the revised manuscript.

Section 5: I would like more details on the processing of the observational radar data. Were the data corrected for attenuation? Was de-aliasing performed? Looking at Fig. 5b it seems that the spectrum was shifted to have its left edge at 0 m/s, is this the case? If the comparison with observations is expanded these details need to be included in the text, otherwise it is sufficient if they are only included in the authors' reply.

Response: The radar Doppler spectrum is not calibrated for attenuation. More details of the Doppler spectrum processing are added in the revised manuscript:

Line 373: "…For the WACR, the maximum unambiguous velocity is 7.8ms^{-1}, which is smaller than the still-air terminal velocity of droplets with diameter larger than 3*mm* and lead to velocity folding. Here velocity de-aliasing process is performed to reconstruct the Doppler spectrum with velocity from 0 ms^{-1} to 11 ms^{-1}. The location of the observed Doppler spectrum is further calibrated from the displacement caused by vertical air motion by pinpointing the location of first Mie notch of the Doppler spectrum to 5.83ms^{-1} …"

Line 355 reports that the data were collected between 05:44 and 05:45, but line 94 reports a temporal resolution of 4.28 seconds. Were multiple spectra observed over that one minute averaged together? If not please indicate the exact timestamps with hours, minutes, and seconds. If yes, please clearly state it in the text.

Response: We have modified the corresponding part in the revised manuscript:

Line 381: "…The temporal resolution of the WACR and the disdrometer is 4.28s, 1min respectively. To make the observation from two instruments comparable, the WACR-observed Doppler spectra are averaged over 1min to coincide with the disdrometer observational period. For this example, we use the disdrometer-measured DSD from 05:44 to 05:45 UTC to simulate the radar Doppler spectrum and compare it with the one observed of WACR in the same period…"

Stylistic/technical corrections

We appreciate the reviewer's detailed edits. All the corrections have been made in the revised manuscript.

Line 40: I believe the correct phrasing is "...remove clutter and identify hydrometeor signal".
Line 44: I find the phrase "improve the microphysical medlin process" to be unclear.
Lines 49-50: I find the grammar in this sentence to be overall incorrect, and it should be rephrased.
Lines 53-55: I believe that the phrase "… spectrum is contributed by …" is gramatically incorrect and it should be improved.
Line 58: I believe the correct phrasing is "… to reduce retrieval uncertainties…".
Line 60: I believe the correct phrasing is "Doppler spectrum simulators".
Line 61: I believe the correct phrasing is "Doppler spectrum shape".
Line 69: "… unlike small doplets".
Line 71: "… large uncertainties for retrieval products".
Line 75: "How does inertia…".
Line 97: "… identify hydrometeor signals".
Line 97: "Additionally, an impact disdrometer…".
Line 98: "The disdrometer".
Line 136: "The values used for … are …".

Line 137: I find the phrase “...as a representation of environment…” unclear. I recommend that it is rephrased.
Line 145: “...cloud droplet, drizzle, …”.
Line 160: “...a turbulent environment...”.
Lines 160-161: I find the phrasing “...are equivalently inertia-free…” unclear.
Line 190: How small? Quantify please.
Line 202: “...wind field...”.
Lines 210-212: This sentence is hard to follow and should be rewritten.
Line 221: “...is only applicable to vertical...”.
Line 232: “...Doppler spectrum density … Vt is the droplet…”.
Line 256: “...and its impact on radar Doppler…”.
Line 274: I believe this line should read “… total number of simulated timesteps...”.
Line 276: “...where T and f are ...”.
Lines 287-288: I would rephrase “the values of the intercept parameter N0 and the slope factor Gamma are chosen to be ...”.
Line 288: “… droplet diameter ranges…”.
Line 293: “...larger differences between the generated…”.
Lines 298-300: I find the term “adjusted time” unclear.
Lines 313-314: this sentence reads wrong and should be adjusted. E.g.: “… a large differences between the right edges of the spectra from the two simulators can be clearly identified.”
Line 330: “Comparing the three ...”.
Lines 353 and 371: Marshall is spelled with two l.
Line 367: “...spectral power compared to ...”.
Line 370: I believe the sentence should read “… both the simulated Doppler spectrum and the convolution-based Doppler spectrum near the second notch are not consistent ...”.
Lines 375-376: “… has shown significant improvement in correctly emulating…”.
Line 387: either “Radar Doppler spectra ...” or “The radar Doppler Spectrum...”.
Line 392: I would rephrase “...inertial effects are typically neglected…”.
Lines 397-398: “… velocity field… incapable of following … as small droplets do.”.
Line 406: “… caution should be taken when applying convolution-based approaches to represent ...”.
Line 419 “… various potential ...”.
Line 450: I believe this should read “initial draft”.
Line 456: It should read either “contribution is” or “contributions are”.

Reference

Borque, P., Luke, E., and Kollias, P.: On the unified estimation of turbulence eddy dissipation rate using Doppler cloud radars and lidars, Journal of Geophysical Research: Atmospheres, 121, 5972-5989, 2016.

Cheynet, E.: Wind field simulation (text-based input), Zenodo, Tech. Rep., 2020, doi: 10.5281/ZENODO. 3774136, 2020.

Deodatis, G.: Simulation of ergodic multivariate stochastic processes, Journal of engineering mechanics, 122, 778-787, 1996.

Mages, Z., Kollias, P., Zhu, Z., and Luke, E. P.: Surface-based observations of cold-air outbreak clouds during the COMBLE field campaign, Atmospheric Chemistry and Physics Discussions, 1-39, 2022.

Shinozuka, M., and Deodatis, G.: Simulation of stochastic processes by spectral representation, 1991.

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