Author responses

Author response 1:

General comment

I believe this paper is a very interesting contribution to highlight the value of dual-pol observations for the evaluation of microphysics scheme in convective scale models.

The addition of information related to the forward operator and to the density options in the different schemes is very welcomed in this new version.

The new analysis where all ice species are gathered enables an easier comparison from one scheme to the other.

I suggest publishing the paper as it is (just correcting for remaining typo errors):

Thank you for the positive review. Our point-by-point review is again highlighted in blue. A marked up-version showing all changes to the manuscript is provided along with the revised manuscript.

l 39:
to answer is: How much complexity => to answer is: how much complexity
Changed as suggested.

l 41: knowledge: It is known => knowledge: It is known
Changed to “knowledge: it is known”.

l 131:
model grid spacing is at 400 m => is 400 m
Changed as suggested.

l 148:
for each hydrometer type => for each hydrometeor type
Changed as suggested. Also corrected in lines 120, 276, and 470.

line 153: vertical ice ==> you should keep the Dolan (2013) terminology “vertically aligned ice” at least in the text
Changed as suggested.

line 451 :
Tt slightly smaller reflection => The slightly smaller reflection
Changed to “For slightly smaller reflectivity thresholds”.

Author response 2:

Second review of "**Influence of cloud microphysics schemes on weather model predictions of heavy precipitation**", by Kocher et al., submitted to ACP.

General comments. The paper is improved from the previous version. The authors have well addressed my main concerns with the previous version, particularly consistency of CR-SIM with the microphysics scheme assumptions and consistency of the ice analysis with how P3 represents ice particle properties. I have several additional comments and suggestions below to mainly improve the presentation; all are minor. Once these are addressed I recommend accepting the paper.

NOTE: line numbers refer to track changed version.

Overall recommendation: Minor revision

Thank you for this positive and thorough review. Our point-by-point response is highlighted in blue below. The changes made to the manuscript are highlighted in red. A manuscript version in which all changes are highlighted is provided along with the revised manuscript.

Minor science and editorial comments.

1. General (incl. abstract and main text): terminology of "model" versus "scheme". There seems to be a mix and match of this (e.g., lines 7 and 10 in abstract says "scheme", but then lines 12, 13, 14 say "model"). Usually "model" refers to the main model system or driver model, like WRF or ICON. I suggest using consistent terminology throughout the paper where "scheme" refers to the microphysics parameterization and "model" refers to the main driver model or modeling system.
Changed as suggested. With "model", we now refer exclusively to the main NWP model and with "scheme" we refer to the microphysics parameterization. Within this context, we also improved consistency regarding the use of "microphysical scheme" and "microphysics scheme". We now consistently use "microphysics scheme" throughout the manuscript.

2. Which version of P3 was used (there are multiple versions available in WRF)? Was it the two-moment, single category version (MP option 50)? It would be good to clarify this somewhere in the paper. Perhaps even give the MP option #'s for all the schemes used here, to make it clear to readers for all the schemes tested.
Yes, it was the version with the MP option 50. We added a table to the data section that shows all employed microphysics schemes, the abbreviation used throughout the manuscript, the WRF-ID and the corresponding publication.

3. Lines 86-87. (1) and (2) are not approaches per se; they are questions covering specific science problems. Thus I suggest rewording by not calling (1) and (2) "approaches" or clarifying these are approaches to address questions (1) and (2).
We were actually not referring to the two science questions, but to the two possibilities of comparing model output with radar observations: (1) by applying a radar forward operator, i.e, comparing in "radar space" and (2) by retrieving microphysical information from radar signals, i.e., comparing in "model space". This was described in lines 69-70 and called "approach" 1 and 2. We understand that is very confusing, so we changed the phrasing, avoiding the numbers and simply stating directly what was done. The paragraph now reads like this:

> In Köcher et al. (2022), cloud microphysics schemes of varying complexity are assessed by a statistical comparison of the observed radar signals with the simulated radar signals from the model output. This study builds on the study of Köcher et al. (2022) and goes one step further by additionally retrieving hydrometeor information from the polarimetric radar observations and comparing it with the simulated hydrometeors.

4. Line 110. “particle property prediction” should be “Predicted Particle Properties” (and it is typically capitalized).
Changed as suggested. Now part of the table, as of comment 2. Also changed in abstract.

5. p. 4-5, this paragraph is very long. It might help to improve the structure by starting a new paragraph beginning with the sentence “A forward radar operator (CR-SIM; Oue et al., 2020)…” Also, I’d start another new paragraph beginning with the sentence “The horizontal model grid spacing is at 400 m.” since this is covering a different topic than CR-SIM discussed in previous sentences.
Changed as suggested.

6. Lines 117-120. Here you refer to different ice types in P3 (e.g. small ice, graupel, unrimed and partially rimed ice). Do this mean the different ice types distributed across the particle size distribution (PSD) as in Fig. 1 of Morrison and Milbrandt (2015)? If so, I’d clarify this and perhaps cite Fig. 1 in Morrison and Milbrandt (2015). Is CR-SIM coupled in a consistent way with these different regions of the PSD?
Yes, this is how we understand CR-SIM. CR-SIM claims to be consistent with the supported microphysics schemes (Oue et al., 2020). For the P3 scheme, this means CR-SIM must differentiate between the regions of the PSD, as visualized in Fig. 1 of Morrison and Milbrandt (2015). We have not tested our self if this was implemented correctly. However, from inspecting their code (openly available here: ftp://ftp.radar.bnl.gov/outgoing/moue/crsim/src/crsim-3.33.tar.gz ; within src/crsim_subrs.f90), we can see that for the P3 scheme, they indeed calculate critical sizes to separate different regions of the PSD. We now clarify that we refer to the different ice types distributed across the PSD by referring to Fig. 1 of Morrison and Milbrandt (2015) in the text:

> P3 deviates from the traditional schemes regarding ice, and uses different ice types distributed across the particle size distribution (see Fig. 1 in Morrison and Milbrandt, 2015). Here, CR-SIM assumes that small ice and graupel are spherical, while unrimed and partially rimed ice is assumed to be oblate with an aspect ratio of 0.6.

7. Line 135. Suggest replacing “Both, radar and model, require...” with “Both the radar and model require...”.
Changed as suggested.

8. Line 160. Would “quantities” be better than “moments”? Are these quantities formally moments of a distribution? (I don’t think so).
Changed as suggested.

9. Line 184-185. Confusing wording here. Suggest replacing “and P3, the simulations are for the most part even able to...” with “and P3 are for the most part even able to...”.
Changed as suggested.

10. Line 190. “relevant over a larger statistic” is confusing. Maybe replace with “relevant statistically over a longer period”.
Changed as suggested.

11. Lines 195-200. Same comment as #3 above referring to (1) and (2) as approaches. Can this be reworded?
We think it is not necessary to number the approaches an it seems to confuse more than it helps. So we removed any instance where “approach #” was used.

12. Line 202. Typo: “cays” should be “days”.
Changed as suggested.

13. Line 231-237. There are several places here that report rain rates in units of $1/m^2$. I don’t understand this. Is it a typo or error? Same comment in the Fig. 3 caption as well. If the units are indeed $1/m^2$ can you explain where that comes from?
The unit is l/m^2 (liters per square meters), which equals “mm”. It is not a typo, but you are right that the l and the 1 look very similar in the manuscript. This is the journal template, so we cannot change it. However, technically it is l/m^2 in one hour – so it should be $l/(m^2h)$ or mm/h. We decided to use mm/h to avoid the confusion of liters and 1.

14. Line 238, I don’t follow this sentence. Can it be reworded or clarified?
What we meant by that sentence is that by converting the heavy rain thresholds of the DWD to reflectivity thresholds, this should help to give an orientation where “heavy rain” is in terms of reflectivity. We rephrased the sentence like this:

This gives an indication of the reflectivity thresholds that correspond to heavy rain.

15. Line 275. In my previous review I had suggested that the authors clarify that the D^3 and D^6 relations for mass and reflectivity apply to liquid drops. However, whether this requires isometric particles depends on how diameter is actually defined. For larger liquid drops which of course are not isometric (since their aspect ratio changes with size), this problem is resolved by defining the diameter as that of a volume-equivalent sphere. All this to say, I’d simply remove “(isometric)” from this sentence as that might be confusing.
Thank you for the clarification. We removed “(isometric)” as suggested.

16. Line 300. Maybe “behaviors” instead of “behavior”?
Changed as suggested.

17. Line 314. “is” should be “was” for consistency of tense.
Changed as suggested.

18. Line 333. Suggest replacing “is undergoing” with “undergoes”.
Changed as suggested.

19. Line 335. Add “rate” after “evaporation”.
Changed as suggested.

20. Line 336. Replace “in” with “at”.
Changed as suggested.

21. Line 337. Add “and” before “all schemes”.

Changed as suggested.

22. Line 340. Replace the comma after “thresholds” with a semi-colon.
Changed as suggested.

23. Line 342. “indicate” should be “indicates” for subject-verb agreement.
Changed as suggested.

24. 346-347. Suggest rewording this to “However, hail events also have damage potential and therefore are of interest.”
Changed as suggested.

25. Line 353. Suggest a small rewording to “The P3 scheme does not have a separate hail or graupel class.”
Changed as suggested.

26. Line 354. Suggest replacing “into” with “in”
Changed as suggested.

27. Line 356. “anyways” should be “anyway”.
Changed as suggested.

28. Line 360. Suggest replacing “the microphysics schemes” with “microphysics scheme”.
Changed as suggested.

29. Line 365. I’d reword this sentence. SBM not producing a single instance of ice at 35 dBZ or higher is one of two possibilities of a binary situation (either this occurs, or not), so it’s awkwardly worded in this sentence to say the same is true for the Morrison scheme but to a lesser extent.
We rephrased this part:

> The most extreme case is the SBM scheme, which hardly produces any ice events at higher reflectivities. There is not a single time step within the 30 day dataset at which the SBM scheme simulated ice grid cells of 35 dBZ or higher (Top left image in Fig. 5). However, most of the other schemes, and especially the Morrison scheme, also consistently show fewer ice events compared to the observations.

30. Line 374. Remove the first comma. Also, remove “when”.
Changed as suggested.

31. Line 376. I think you mean “too large” not “too small”.
That is correct. Changed to “too large”.

32. Lines 377-378. This may be true with the way the schemes are configured here, but the Morrison scheme in WRF does have an option to use properties of hail (with a high density of 900 kg m^{-3}) rather than graupel for the rimed ice category. This could be mentioned here (perhaps in a footnote?).
It is correct that the Morrison scheme has a switch to use hail with a high density instead. The same is true for a new version of the SBM scheme. However, the configuration applied in this study did not utilize these switches. We clarify this in a footnote now:

> With the configuration that was used in this study.

33. Lines 381-386. A newer version of P3 actually *does* include partially melted ice (Cholette et al. 2019). This is not yet implemented in WRF, but could be mentioned here.

Cholette, M., H. Morrison, J. A. Milbrandt, and J. M. Theriault, 2019: Parameterization of the bulk liquid fraction in the Predicted Particle Properties (P3) scheme: Description and idealized tests. *J. Atmos. Sci.*, 76, 561-582.

FYI there is a paper also just accepted in JAMES that applies this newer version of P3 to simulations of a squall line, and discusses the impact of wet ice on the reflectivity calculation:

Cholette, M., J. A. Milbrandt, H. Morrison, D. Paquin-Ricrad, and D. Jacques, 2022: Combining triple-moment ice with prognostic liquid fraction in the P3 microphysics scheme: Impacts on a simulated squall line. *J. Adv. Mod. Earth Sys*. (accepted)

Thank you for this information, it is much appreciated. Regarding our manuscript, we added another footnote mentioning the newer version that includes partially melted ice:

> A newer version of the P3 scheme does include partially melted ice: Cholette et al. (2019).

34. Line 393. Suggest replacing “The P3” with “P3”.
Changed as suggested.

35. Line 394. See comment #32 above regarding the graupel/hail switch in the Morrison scheme, which is relevant here as well. Also relevant to the sentence on line 399.
In line 394, we added a part stating that this is true only with the configuration that was used for this study:

> With the configuration that was used in this study, P3 is also the only scheme that allows ice particles to reach densities up to 900 kg m^{-3}, i.e., to simulate hail-like particles.

We think the sentence on line 399 can stay like it is, because at this point it should be clear that the ice density is depending on the configuration (mentioned twice already at this point) and also, this sentence refers specifically to graupel density, not to hail.

36. Line 395. For consistent tense with the rest of this paragraph, I’d replace “were” with “are”.
Changed as suggested.

37. Line 397. “reason” should be “reasons”.
Changed as suggested.

38. Line 405. Suggest adding “but” before “this time”.
Changed as suggested.

39. Line 407. Since you’ve generally referred to the spectral bin scheme as SBM in the rest of the paper, my suggest is to replace “the spectral bin simulations” with “the SBM simulations”.

Changed as suggested.

40. Line 408. Remove “the”.
Changed as suggested.

41. Line 415. Suggest replacing “is melting” with “melts”.
Changed as suggested.

42. Line 431. I would replace “indicates” with “suggests” because in principle other processes could be responsible for more rapid decrease of ice toward the surface, like changes in fallspeed leading to divergence. Although I agree that greater melting is the most likely explanation.
Changed as suggested.

43. Line 437. Suggest replacing “the Morrison and SDM” with “Morrison and SDM”.
Changed to “Morrison and SBM”

44. Line 472. Replace “summerly” with “summer”.
Changed as suggested.

45. Line 479. Add “model” before “resolution”.
Changed as suggested.

46. Line 480. Not sure what “Tt” is supposed to be. Maybe “For”?
Changed to “For slightly smaller reflectivity thresholds…”

47. Lines 500-502. This sentence is very long. You might consider breaking it into 2 sentences.
We split up the sentence into 2 sentences:

> Within a sub-project of the DFG (German Research Foundation) Priority Programme 2115 (PROM, Trömel et al., 2021), an HMC algorithm is currently being developed for this purpose. This algorithm is based on a clustering approach and an algorithm for quantifying the mixing ratio following Grazioli et al. (2015), Besic et al. (2016) and Besic et al. (2018), and aiming to calculate the mixing ratios of hydrometeor classes as well.

Other changes:

We added a sentence at the end of the abstract to paraphrase one of our conclusions that was missing in the abstract:

> More complex schemes do not generally yield better results, emphasizing the need to first improve the existing microphysical parameterizations with observational constraints that have the potential to infer microphysical parameters.

Figure improvements:

- changed ‘spectral bin’ legend entries to ‘SBM’ for consistency
- changed figure format from png to pdf
- increased font sizes (and adjusted label descriptions to fit)
- Increase x-axis limits for the ice reflectivity (Fig. 5) from 35 dBZ to 32 dBZ

References

Morrison, H. and Milbrandt, J. A.: Parameterization of Cloud Microphysics Based on the Prediction of Bulk Ice Particle Properties. Part I: Scheme Description and Idealized Tests, Journal of the Atmospheric Sciences, 72, 287–311, https://doi.org/10.1175/jas-d-14-0065.1, 2015.

Oue, M., Tatarevic, A., Kollias, P., Wang, D., Yu, K., and Vogelmann, A. M.: The Cloud-resolving model Radar SIMulator (CR-SIM) Version 3.3: description and applications of a virtual observatory, Geoscientific Model Development, 13, https://doi.org/10.5194/gmd-13-1975-2020, 2020.