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Comment on acp-2020-1326

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Referee comment on "Sensitivity of precipitation formation to secondary ice production in winter orographic mixed-phase clouds" by Zane Dedekind et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-1326-RC2>, 2021

GENERAL

This manuscript describes a numerical modeling study of the impact of including parameterizations for additional (besides Hallet-Mossop) mechanisms for secondary ice production (SIP) in a 2-moment microphysics scheme used in a 1-km mesoscale model simulation of a winter orographic precipitation event. This is a really interesting and timely study. SIP is a popular topic in ice microphysics these days, but parameterizations of different SIP mechanisms seem to be still in the exploratory stages, at least in common use in 3D mesoscale model simulations. This is probably related in part to previous limitations in microphysics schemes that limited the relevance in SIP in models; however, this is definitely changing so this topic is becoming increasingly important in atmospheric modeling. Overall, I think this manuscript is very solid. The topic is timely, the study is original, the scientific methodology is sound, and the presentation (writing quality, figures, and organization) is high quality.

I do have some concerns about the treatment of graupel in the microphysics scheme used in this study (see comments below). I do not think it is a "deal breaker", but I think the authors should address in the text (or with sensitivity tests) the implications of certain limitations in the bulk scheme used to their conclusions about parameterizing SIP in bulk microphysics schemes.

I am indicating a recommendation of "major revision", but this is only to reflect the importance for which I regard those major comments to be addressed in the paper. Overall, this is a great paper.

MAJOR COMMENTS

1. The choice of microphysics scheme used and the implications in this study need to be discussed on more detail. The risk of not doing so adequately is that readers may dismiss the results as not being sufficiently general, but only relevant to the particular scheme used. The scheme used is the Seifert-Beheng scheme, which is a detailed 2-moment, multi-ice-category, bulk scheme. This has several implications -- strengths and limitations -- with regards to studying the effects of SIP.

First, it is a bulk scheme, not a bin scheme. In my opinion, this is perfectly fine but it needs to be defended since there is a popular (and incorrect) assumption that bin schemes are inherently more accurate than bulk schemes. This is simply not the case when it comes to ice-phase microphysics, where the results with bin schemes have at least as much, if not more, variability as those with bulk schemes (see Xue et al., 2018, MWR). It will strengthen the paper if the use of a bulk scheme is defended in this regard.

Second, S-B is a 2-moment scheme. 2-moment is, of course, necessary (rather than 1-moment) as a minimum in order to properly capture the effects of changes to ICNC and other impacts. Is 3-moment necessary? Probably not for this study, but since 3-moment schemes exist, it is probably worth discussing, given that you are advocating methods to advance the modeling of ice-phase microphysics in models.

2. Following from the above point, there is a major limitation (set of limitations) in the S-B scheme which is relevant to this study. The S-B scheme, although 2-moment and detailed, can be regarded as a "traditional" bulk scheme in that it represents ice-phase hydrometeors by partitioning them into representative categories (e.g. "snow", "graupel", etc.) with fixed parameters for bulk physical properties. There is a growing viewpoint in the modeling community that this category-based approach has some inherent weaknesses (in terms of physical realism) and practical limitations (in terms of consistent simulation results). The alternative that some modelers have explored is to focus on the continuous evolution of physical properties (density, etc.) of ice (e.g. Hashino and Tripoli 2007; Morrison and Milbrandt 2015; Jensen et al. 2017). In the current study on parameterizing SIP, this has some important implications on interpreting the results. As the authors pointed out, S-B has a fairly crude parameterization of the "conversion" of snow to graupel; this is a non-physical process anyway but it has a large impact on graupel production, and thus collisional breakup. Also, the representation of graupel also has some other relevant limitations; it has a fixed density and fixed fall speed parameters. Thus, even if the parameterization of SIP by mechanical breakup were perfect, the representation of graupel is not -- therefore, the representation of the effects of some SIP mechanisms are highly dependent on the treatment of graupel.

At the very least, these aspects of the baseline microphysics scheme used need to be discussed in the paper in order to provide a broader context to interpret/evaluate the results. Of course, it is not practical to say that the authors should switch to a property-based scheme. However, the degree to which the limitations of the category-based scheme could be examined further through a small number of sensitivity tests (e.g. adjusting the rate of conversion from snow-to-graupel; changing the graupel fall speed parameters). I think this could strengthen the paper. But at the least, the aspects/limitations related to the category-based S-B scheme should be included in the discussion.

MINOR COMMENTS

- I suggest that the author make a clearer distinction in the text when referring to natural ice-phase hydrometeors vs. categories in the bulk scheme. This is a minor but important point, in my opinion. Bulk schemes do not have ice crystals; they have bulk categories with prognostic bulk quantities. Not making the clear distinction in the wording can invite readers (and researchers) to make inappropriate comparisons (e.g. between observed ice crystals and model "ice", "snow", or "graupel"). In several places in the text, the authors refer to model "ice crystals" and it is difficult to tell if the mean ice-phase hydrometeors (of any category) or specifically the "ice" category.
- The writing is very good in general. Just two suggestions: 1) In a few places it says something like "All of the simulations do not capture..." – it would sound more natural to write this as "None of the simulations capture...". [e.g. line 416, 419, and elsewhere] 2). There are a few cases of contractions (e.g. "can't") where as it should read (e.g.) "cannot". [e.g. line 234]
- Line 121: Suggest "grid spacing" instead of "resolution".
- Line 153 (and reference list), "Milbrandt and Morrison, 2015" – should be 2016.
- Line 324, "Not as obvious, .." This is not actually a sentence.
- Line 355, "... we have shown that COSMO benefits..." (and similar statement in the conclusion [line 427]). I suggest changing this to "... we have provided evidence that COSMO may benefit...". There are too many uncertainties in the model and in the S-B scheme itself (not to mention the SIP parameterizations) to make this claim definitively. Models are a system of compensating errors, so making change that results in an improved result may not be systematic and may not be due to genuinely improved representation of physics.
- Fig. 9: This is a great figure! 2 minor suggestions: 1) in the text at the top, put "Lower ICNC" first (since the larger diameters are the result, not the cause). 2). The term "sedimentation velocity" is probably not quite correct, given that this arrow refers to the vector sum of sedimentation plus horizontal advection. Perhaps "hydrometeor trajectory" would be better, and explain this point in the caption.
- Fig. 11: The units (and description) in panels a) and e) do not seem correct. Is this (a)) the column-integrated graupel mass – i.e. "graupel water path"?
- Line 430 (and a couple other places), "secondary ice parameterization": Should read "secondary ice production [or SIP] parameterization".
- Line 422: "represented" should probably be "reproduced". (Also, line 440, plus one or two other places.)