Author Response to Reviews of

Assessing the potential for simplification in global climate model cloud microphysics

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Atmospheric Chemistry and Physics, doi:10.5194/acp-2021-801

RC: ***Reviewer Comment***, AR: *Author Response*, ☐ Manuscript text

We sincerely thank the reviewers for their constructive and detailed feedback. We implemented their feedback into a newly revised version of the manuscript. Please find our answer to the reviewers' points below, followed by a marked-up manuscript verison.

1. Reviewer Comment #1

RC: ***It's clear that the authors put considerable effort into revising their paper and it is much improved. Most of my previous comments were addressed. I do have a few broader comments followed by several minor comments/suggestions and technical edits. None of these are major, and my overall recommendation is to accept pending these minor revisions. Note all line numbers referred to in this review correspond to the track-changed version.***

AR: *Thank you for your thorough feedback. Please find our respective answers diretly below your comments.*

1.1. Semi-major comments

1.1.1

RC: ***Overall I appreciate the authors main argument about implications for simplification of process formulations based on the process rate sensitivity analysis/PPE proposed here. However, an essential part in deciding whether or not a process parameterization is a candidate for simplification is the uncertainty in that process. Here the authors perturbed parameters by multiplicative factors from 0. 5 to 2 with equal probability across this region, which doesn't correspond to actual process uncertainty (though to be clear, the authors never claim that it does). The gray area in Fig. 2 is the region "of most interest" over which process sensitivities are considered as candidates for process simplification. However, I feel the authors need to emphasize that this region "of most interest" should ultimately depend on the range of process uncertainty; moreover, this uncertainty range varies from process to process (since, of course, some processes are relatively much more uncertain than others). Thus, sensitivity to process perturbations, as the authors analyze here, should go hand-in-hand with estimating the process uncertainty when considering which processes to simplify. Of course, quantifying process-level uncertainty is itself a major challenge and generally not very straightforward.***

AR: *Thank you for your suggestion, which we have gladly incorporated.*

If the process uncertainty were known, it would influence the extent of the perturbation range, which could be different for each process.

However, in deciding how drastic these simplifications should be, process uncertainty should also be considered.

1.1.2

RC: ***While I agree about the main arguments concerning simplification, I don't follow how process simplification would lead to greater process understanding as stated on line 73. One could argue that simplification could lead to a greater understanding of the effects of a process in a model, but not the process itself. Thus, I suggest rewording this discussion around line 73.***

AR: *We agree that the gained understanding first and foremost concerns the model itself. We have reworded the sentence accordingly. For the sake of explaining our intentions, what we had meant was that if processes as represented in the model have anything to do with reality, than in turn learning about the model and the processes within might allow to learn more about the processes themselves.*

In this paper, we propose a new methodology to assess where process parameterisations can be stripped of detail to aid the development of a simplified model as well as to increase ~~process understanding~~ understanding of the model.

1.1.3

RC: ***The importance of a process as gauged by the method in this paper is also conditional on other parameters/processes that were not varied, including those in parameterizations besides microphysics. I'm sure the authors know this and would agree, but I suggest this be mentioned in the paper. Perhaps around line 574, where they discuss a similar situation with the process analysis being conditional on model resolution.***

AR: *We agree and have added a clarifying remark:*

We emphasize that our findings are conditional on the design of the ECHAM-HAM model, including the implementation of other processes and parameters that were not varied in the current study.

1.1.4

RC: ***I have some additional questions about interpretation of process simplification depending on the response to the process perturbations. For example, on lines 225-230, the discussion mentioning a critical point with a slope of 0 at eta = 1 seems questionable, since to me this implies a local max/min or perhaps even***

a cusp. I think instead what you mean is that the slope around eta = 1 is small, not that the slope at eta = 1 is 0.

AR: *Your reasoning is correct: the slope does not need to be 0 exactly and we have modified the statement accordingly:*

> For example, if the model output variable (e.g. ice water path, IWP) as a function of η_i has a ~~critical point~~ slope close to zero at $\eta_i = 1$ (~~i. e. slope of zero~~green and purple lines in Fig. 2), this suggests that the process i needs to be represented only approximately and that some detail could probably be removed from its parameterisation without much of an effect on the model performance.

RC: ***This is also related to the interpretation given in the Figure 2. I think this figure is useful to include, but if we care about the region in gray, the green and purple lines practically overlay within this region. Thus, it seems questionable to argue that behavior of the purple line means this process is necessary but doesn't need to be accurate while for the green line the process is dispensable – they practically overlay in the region "of most interest". Overall, it seems that the key is the average slope of the line within the gray region of interest (the average being consistent with process uncertainty represented as a prior with constant probability across this region). In my view, a near 0 slope across this gray region would imply the process is dispensable, weak slope would be the process is necessary but with less need for accuracy, and moderate to steep slope would mean the process needs accurate representation. Do you agree? The fact that the purple line has a steep slope but it's outside the gray region of interest would seem to imply the steep slope is not relevant. This comment is related to comment #1 above about how the gray region "of most interest" should be tied back to the uncertainty in a specific process (again, varying for different processes).***

AR: *The green and purple lines purposefully overlay in the grey region. As you say, it is key that their slope is small here, suggesting that both corresponding processes may be simplified. And as you state correctly, the larger the slope the less simplification is advisable. However, the point we wish to make with the difference between the purple and green line is the following: their small slope in the grey area suggests that they can both be simplified. But because the green line is close to zero throughout the whole phase space, it may even be removed. Thus we chose to give them a similar slope to make that point more clearly. We have added text to the caption to explain our interpretation of the Figure.*

> Sketch of the envisioned interpretation. The shading indicates the area that is of most interest to judge the effect of process simplifications on the model output. If the slope in this area is small, this suggests that the process can be simplified (green and purple lines). A large slope indicates that the process needs to be represented accurately (orange lines). If no perturbations of the process in the 0.5 to 2 perturbation parameter range and the suppression of the process (perturbatin parameter of 0, not shown) have a significant influence on the model output, the process may be removed entirely (green line).

1.1.5

RC: ***Minor comment but relevant to many places in the text: I'd replace "inflicted" with "induced" throughout the text. "inflicted" usually implies some kind of malevolence (imposing something unwelcome).***

AR: *Thank you for making us aware of these implications. We have substituted all appearances of the word.*

1.2. Minor comments

1.2.1 Line 32

RC: ***Similar to a comment in my first review on the Archer-Nicholls et al. (2021) paper specifically focusing on land surface modeling, citing Morrison et al. (2020) seems questionable here since that paper specifically focuses on microphysics and this sentence is a very general statement about the Earth system. I'd either drop the citation here (a citation is not really needed here anyway, this statement is general knowledge), or add "cloud microphysical" before processes if you want this sentence to refer specifically to microphysics.***

AR: *As suggested we have dropped the citation.*

1.2.2 Line 89

RC: ***I feel a bit uncomfortable with how this is worded because these papers used synthetic observations (generated by a model) for the constraint. This is stated on line 91 for the Morales et al. (2021) paper but it's not clear to readers this is the case with the other papers as well. Perhaps add "synthetic or real" before "observations" on line 89. Also, you might replace "artificial" with "synthetic" on line 91 for consistency (I think "synthetic" is a slightly better word choice here).***

AR: *We have added a clarifying note and thank you for your suggestion of the word "synthetic", which indeed is a better choice here.*

> In a next step, parameter ranges can be constrained when comparing the PPE to observations ~~(Posselt, 2016; van Lier-Walqui et al., 2014; van Lier-Walqui et al., 2019)~~(Posselt, 2016; van Lier-Walqui et al., 2014; van Lier Morales et al. (2021) built a PPE of CMP process parameters and environmental conditions, generated using a Markov Chain Monte Carlo algorithm, in idealized simulations to then constrain the parameters with ~~artificial~~ synthetic observations.

1.2.3 Lines 205-206

RC: ***With the changes to the text this description now makes much more sense and I actually don't think Eq. (1) is needed. (unfortunately Eq. 1 also introduces a few new issues, like it only makes sense if level number decreases with height but that isn't explicitly stated anywhere). My suggestion is just to remove Eq. 1. now. Also, you refer to level i in this equation but later to process i on p. 9, which I suppose could be confusing. Maybe use k instead of i here?***

AR: *Since we had added the equation following your suggestion and you now think that the updated description is clear enough, we have removed the equation again.*

1.2.4 Lines 216

RC: ***But processes don't affect "tracer variables" (tracer by the usual definition means not impacted by sources/sinks). Would "prognostic variables" be better?***

AR: *Thank you for this suggestion, which we have incorporated.*

> In the present study, we achieve this by setting to zero the change that the process ~~inflicts on tracer~~ induces on prognostic variables.

1.2.5 Line 221

RC: ***Again, "phasing" is confusing terminology here. I'd suggest to reword this "...induced by a process, we can perturb the process using a newly defined parameter eta."***

AR: *We have incorporated this as you suggested.*

> More generally, instead of setting to zero the changes ~~inflicted~~ induced by a process, we can ~~phase these changes in and out~~ perturb the process using a newly defined parameter η.

1.2.6 Line 231

RC: ***I think the wording here is awkward. Suggest instead "...serving as a proxy for understanding sensitivity to processes, while in actuality process uncertainties would likely be variable in time and space."***

AR: *We have incorporated some of your rewording suggestions but refrain from using the complete wording you suggest as it changes the meaning of the sentence. Here we are discussing that simplifications of processes will likely have an effect that varies in space and time while you are referring to process uncertainties.*

> Note that the perturbations are constant in space and time for each PPE member, serving as a ~~reasonable proxy for~~ proxy for understanding the effect of possible simplifications, which would likely be variable in ~~space and time~~ time and space.

1.2.7 Line 239

RC: ***I think you mean the range is expanded to eta = 2 in this second step? Can that be clarified in this sentence?***

AR: *This is correct and we have added this specification as follows:*

> In addition, the range of η_i is expanded to values up to $\eta_i = 2$ to imitate an overestimation of a given process due to an inaccurate description.

1.2.8 Line 247

RC: ***Could replace "thus generated" with "LHS generated", which seems better wording.***

AR: *We agree that this is better wording and have replaced as you suggest.*

> *Each of the ~~thus~~ LHS generated input combinations was then used as input for a 1 year ECHAM-HAM model simulation, creating a perturbed parameter ensemble (PPE) with 48 members.*

1.2.9 Line 437

RC: ***"slight simplifications" is rather vague. I'd just suggest dropping "slight".***

AR: *We have dropped it as suggested.*

> Due to the small deviations in the considered variables in response to variations around $\eta_i = 1$ for riming and autoconversion (purple line in Fig. 2), there is potential for ~~slight~~ simplifications of their formulations.

1.2.10 Line 475-476

RC: ***For the sentence "Most variables had to be excluded..." could you provide a bit more information or context about this? For example, which variables were kept? What's meant more specifically by "most"?***

AR: *We have specified that only 7 variables could be retained (note that this number changed compared to the last version of the manuscript because the spherical harmonics analysis is now applied to absolute values, in response to reviewer comment 2.1.7).*

> ~~Most~~ 4 out of 11 variables had to be excluded because too many members were defaulting or because their variations were too small to be sensibly emulated.

1.2.11 Line 513

RC: ***Similar to a comment above, I think "prognostic variables" would be better as "tracers" often has a somewhat different meaning in the literature.***

AR: *We have incorporated your suggestion.*

> Of course, if numerous CMP processes and interactions with aerosols were simplified, this would allow for more drastic steps such as fewer prognostic aerosol ~~tracers~~ variables as those could become redundant.

1.2.12 Line 517

RC: ***Perhaps this is nitpicky, but it's not clear to me how process simplification leads to improved robustness, while the arguments for improved scheme compactness and interpretability are much clearer. Perhaps just deleted "robustness"?***

AR: *Following your suggestion we have deleted it.*

> However, as detailed in Sec. 1, there are numerous benefits in simplification that are independent of the associated computing cost, such as a gain in compactness ~~, robustness~~ and interpretability.

1.2.13 Line 548

RC: ***Again, "tracer" here is inconsistent with the typical use of the term in atmospheric or climate modeling. Suggest simply removing "tracer" in this sentence.***

AR: *Removing "tracer" lends itself to a broader discussion of the CMP scheme and so we welcome this suggestion.*

> For example, CMP schemes that contain only one ~~tracer~~ category for ice, e.g. the Predicted Particle Properties (P3) ice microphysics scheme (e.g. Morrison and Milbrandt (2015), Eidhammer et al. (2017), Dietlicher et al. (2018), Dietlicher et al. (2019), and Tully et al. (2021)) are more physical as well as more interpretable.

1.2.14 Line 552-554

RC: ***I disagree somewhat with the argument here. Yes, autoconversion generally is associated with the aggregation process (though not always, e.g. Harrington et al. 1995, JAS). However, so are self-collection and accretion, so one has to make some ad-hoc choice, such as a size threshold, to discriminate particle aggregation that leads to self-collection, accretion, or autoconversion. Thus, I agree that autoconversion is "difficult to constrain in observations", but would add that a fundamental challenge in constraining autoconversion is that it is not even a distinct physical process.***

AR: *In the ECHAM-HAM CMP scheme, autoconversion is associated only with the aggregation process (as depicted in Fig. 1). We think we have covered the point of the artificial size threshold in this discussion by stating that autoconversion "is a transfer mechanism between the two artificial classes". Still, we thank you for your suggestion and have now added a remark stressing that autoconversion is not a distinct physical process.*

> Still, autoconversion is difficult to constrain in observations (Morrison et al., 2020) also because it is not a distinct physical process, and so moving towards a ~~one ice category scheme seems advisable.~~ scheme with evolving instead of pre-defined ice categories seems advisable (see e.g. Milbrandt and Morrison (2016) and Jensen et al. (2017)).

1.2.15 Line 554

RC: ***I don't agree with the argument that this challenge points towards moving to a single ice category scheme, but rather suggests moving to schemes that do not use pre-defined ice categories corresponding to e.g., cloud ice, snow, graupel, whether one category or multiple categories. For instance, there is a multi-category version of the P3 scheme (Milbrandt and Morrison 2016) in which different categories have distinct physical properties, but these properties evolve in time and space and any category could evolve to any ice type depending on local conditions and growth history that the category experiences. Other multi-category"particle property" based schemes have also been developed that similarly allow multiple categories to evolve to any type of ice (e.g., ISHMAEL, Jensen et al. 2017).***

AR: *We completely agree with your point and have gladly added it and the references you point to.*

> Still, autoconversion is difficult to constrain in observations (Morrison et al., 2020) also because it is not a distinct physical process, and so moving towards a ~~one ice category scheme seems advisable.~~ scheme with evolving instead of pre-defined ice categories seems advisable (see e.g. Milbrandt and Morrison (2016) and Jensen et al. (2017)).

1.2.16 Line 577

RC: ***I would suggest replacing "in reality" with "other schemes" since processes like autoconversion don't correspond with a distinct process in nature (as argued above, self- collection, accretion, and autoconversion are all associated with the physical process of ice particle aggregation and must be separated using some ad-hoc method like a size threshold). Of course, even the same scheme but in another model, this would likely produce different results with regard to process sensitivity as well.***

AR: *We agree that the results cannot be transferred to other schemes and that in reality the processes investigated here do not exist as such. However, we would like to insist that this is a point worth making and have therefore left the remark about reality as well as adding the other schemes.*

> Thus the results as such are only applicable to this CMP scheme and cannot be transferred to the significance of the investigated processes in other schemes let alone in reality.

1.3. Editorial comments

- Line 58. Could remove "have".
- Line 301. I don't follow this sentence, maybe a grammar problem. Should "constrain" be "constraint"?
- Line 309. I think there should be "a" before "few".
- Lines 350-351. I'd remove "what we call".
- Line 497. Suggest changing "less strong or consistent" to "weaker or less consistent".
- Line 505. Suggest replacing "can be" with "may be".
- Line 567. There is an extra right parenthesis.

AR: *Thank you for these editorial remarks. We have incorporated your points as suggested.*

2. Reviewer Comment #2

RC: ***I would like to thank the authors for addressing the key points from my first review of this paper to a high standard. The PPE design now covers the parameter space considered much more evenly, leading to a more robust emulator and hence resulting analysis. This is a novel use of the emulation and sensitivity analysis approach to assess model behaviour under uncertainty. I have a few further minor comments, but once these are addressed I would recommend the publication of the manuscript in ACP.***

AR: *Thank you for your again very thoughtful feedback. Please find our answers to your comments below.*

2.1. Specific Comments

2.1.1 Line 139-144

RC: ***Here, the sections of the paper are introduced, but there is no mention of the sections relating to the seasonal and spatially resolved analysis, which is mentioned in the abstract? Perhaps add a sentence here to also point to these for easy reference?***

AR: *Thank you for pointing out these missing references, which we have now included.*

> In Sect. 3 the results from a "one-at-a-time" sensitivity study that explores the axes of the parameter space (Sect. 3.1), the emulated PPE (Sect. 3.2), and of the sensitivity study on the fully sampled parameter space (Sect. 3.3) including a scale dependency (Sect. 3.4) and seasonal analysis (Sect. 3.5) are presented and discussed. Conclusions and an outlook are given in Sect. 4.

2.1.2 Figure 2 and Section 2.2

RC: ***I like the new Figure 2 that has been added to the manuscript to help with the interpretation on how the effects of the perturbations point to potential for process simplification. However, there is no link or reference to the new Figure 2 in the text of this section, or any text description of this interpretation, so it's not clear. And, the caption just says 'Sketch of the envisioned interpretation…', but interpretation of what? Please add some informative description as to what Figure 2 shows in this section and make the caption clearer as to the interpretation it corresponds to.***

AR: *Indeed we had forgotten to link to the Figure in the text, which we have now adjusted. Following your suggestion we have also extended the Figure caption.*

In our case, the parameters aid to understand the sensitivity of the model to each process: From the response of model output to variations in η_i, we can extract information on how accurately a process i needs to be represented in the model . (see Fig. 2 for a visualisation). For example, if the model output variable (e.g. ice water path, IWP) as a function of η_i has a critical point slope close to zero at $\eta_i = 1$ (i. e. slope of zerogreen and purple lines in Fig. 2), this suggests that the process i needs to be represented only approximately and that some detail could probably be removed from its parameterisation without much of an effect on the model performance.

Sketch of the envisioned interpretation. The shading indicates the area that is of most interest to judge the effect of process simplifications on the model output. If the slope in this area is small, this suggests that the process can be simplified (green and purple lines). A large slope indicates that the process needs to be represented accurately (orange lines). If no perturbations of the process have a significant influence on the model output, the process may be removed entirely (green line).

2.1.3 Line 297-298 (and elsewhere: L289, L469)

RC: ***'. . . only 47 PPE members were used as with the 48th member the computational constraint was too tight for the emulator'. This is very ambiguous, and I don't think a general reader will understand what is meant by this and so will find this statement confusing. If you need to state that there were issues with the emulator construction, then it needs to be phrased more directly as that. . . something like: '. . . only 47 PPE members were used due to instabilities in the computations when constructing the emulator. ' I am a little surprised that this happens, with <50 data points over a 4-d parameter space. For my interest - Is it a specific run that causes this? i.e. the same run each time, so some sort of outlier in your PPE?***

AR: *We have used your suggested phrasing as indeed the problems seem to originate in numerical instabilities of the emulator construction. This is supported by the fact that it is not a specific outlier causing problems. In fact, in cases where the model cannot be trained with 48 members, training with any 47 members works.*

The difficulty in emulating the response surface for some of the variables was also apparent in computational limitations: some of the leave-one-out validation emulations were not possible to compute because the constrain of the emulator was too tight for the variability in the data. of numerical instabilities in the computations when constructing the emulator. As these were only a few cases (up to six for global means and four for seasonal means in 48 validation emulations), the validation for those variables as a whole is still deemed valid.

For the variables which passed the leave-one-out validation, the final emulator used for the sensitivity analysis was trained on all PPE members (note that in a few cases only 47 PPE members were used as with the 48th member the computational constraint was too tight for the due to numerical instabilities in the computations when constructing the emulator).

2.1.4 Line 327 (and throughout Section 3.1, and in Section 3.3, 3.6)

RC: ***'... that it's inhibition leads to ... ' I mentioned this in my previous review, and I still don't fully understand the meaning of the word 'inhibition' when describing the parameter effects. What is a parameters' inhibition? Do you mean it has very little effect? Or switching it off? – it's not clear. [When I google the meaning of this word, I don't find a relevant meaning for this context.] If you must use this word, then please define what it means before you first use it. Or alternatively, re-phrase the sentences to be clear in meaning and take it out.***

AR: *What we mean by inhibition of a process is switching it off. In search of a better word we have replaced inhibition by suppression throughout the text.*

2.1.5 Figure 7 caption

RC: ***The projections of the sampling here are 2-d, not 3-d. And the response surface is a 4-d response surface, not 5-d. The response variable (here, IWP) is not a dimension of the sampling – You only have 4 input parameters that the sampling is over, and so the dimension of the response surface has to be 4-d, as it is showing how the response variable changes over those 4-dimensions of parameter space. Each individual plot here considers 2 of those input dimensions, and therefore it is showing the response over a 2-d projection on the space. Please amend the caption.***

AR: *We have amended the caption as you suggested.*

> ~~Three~~ Two dimensional projections of the ~~sampling of~~ IWP values sampled from the ~~five~~ four dimensional ~~response surface~~ parameter space of the emulated PPE. Each perturbed process is a dimension, and the colorbar denotes the global annual mean ice water path for each input parameter combination.

2.1.6 Figure 8 caption

RC: ***Please remove the term 'correlation panel' from this caption – see previous review.***

AR: *Please excuse that we had misunderstood your previous comment. We have adapted the Figure caption as suggested.*

> Same as Fig. 7 but for the global annual mean liquid water path. ~~Correlation panels~~ Results for additional variables are presented in Fig. B1 in Appendix B.

2.1.7 Line 445-446

RC: ***Why do you need to use the difference to the control simulation for this analysis? I don't understand what that achieves ... Could it affect (reduce) the amount of signal that you see?***

AR: *Thank you for bringing up this question. Indeed, there is no need to use the difference to the control simulation in this analysis. To do so made sense for us during the development of the analysis but for consistency we have now changed it so that the absolute values are used.*

2.1.8 Figures 11 and 12

RC: ***Following a comment in my previous review – I think it would be more informative to show the first-order effects in these figures and have the total effect figures in the appendix. If the first order effect plots look similar to the total effect plots then they are more informative, as with the total effect, the reader is left to ponder/guess whether some of the effect is in fact interaction, when it's probably not? The first order effects correspond to individual parameter effects alone, and so are surely more informative and conclusive here?***

AR: *As you suggested we have exchanged the total and first order effect plots.*

2.1.9 Line 481

RC: ***I'm not sure where the value of 0.2% comes from – please clarify.***

AR: *We have added a clarifying remark.*

> They show that at most, with naively removing (the most drastic simplification) the whole cold precipitation formation routine, only about 0.2% of total computing time can be saved (since the cold precipitation formation routine makes up 4.8% of the 4.7% of computing time that the whole CMP take up, see Table 1).

2.2. Technical Corrections

- Line 289: Change '... As these were only few cases ...' to '... As these were only a few cases ...'
- Line 326: I know IAV is defined on page 10, but I got to the acronym here and couldn't remember what it meant ... maybe give the full wording here as a reminder? In fact, do you really need to use an acronym for this, given it only appears 3 times?
- Line 372: Change 'Only for LWP Lohmann and Ferrachat (2010) find...' to 'Only for LWP do Lohmann and Ferrachat (2010) find...'
- Figure 12: The plots in the figure need to be labelled to indicate which is plot a), b), c) and d).

AR: *Thank you for these corrections, which we have implemented as suggested.*

References

Dietlicher, Remo, David Neubauer, and Ulrike Lohmann (2018). "Prognostic Parameterization of Cloud Ice with a Single Category in the Aerosol-Climate Model ECHAM(v6.3.0)-HAM(v2.3)". In: *Geoscientific Model Development* 11.4, pp. 1557–1576. ISSN: 1991-9603. DOI: 10.5194/gmd-11-1557-2018.

— (2019). "Elucidating Ice Formation Pathways in the Aerosol–Climate Model ECHAM6-HAM2". In: *Atmospheric Chemistry and Physics* 19.14, pp. 9061–9080. ISSN: 1680-7324. DOI: 10.5194/acp-19-9061-2019.

Eidhammer, Trude, Hugh Morrison, David Mitchell, Andrew Gettelman, and Ehsan Erfani (2017). “Improvements in Global Climate Model Microphysics Using a Consistent Representation of Ice Particle Properties”. In: *Journal of Climate* 30.2, pp. 609–629. ISSN: 0894-8755, 1520-0442. DOI: 10.1175/JCLI-D-16-0050.1.

Jensen, Anders A., Jerry Y. Harrington, Hugh Morrison, and Jason A. Milbrandt (2017). “Predicting Ice Shape Evolution in a Bulk Microphysics Model”. In: *Journal of the Atmospheric Sciences* 74.6, pp. 2081–2104. ISSN: 0022-4928, 1520-0469. DOI: 10.1175/JAS-D-16-0350.1.

Milbrandt, J. A. and H. Morrison (2016). “Parameterization of Cloud Microphysics Based on the Prediction of Bulk Ice Particle Properties. Part III: Introduction of Multiple Free Categories”. In: *Journal of the Atmospheric Sciences* 73.3, pp. 975–995. ISSN: 0022-4928, 1520-0469. DOI: 10.1175/JAS-D-15-0204.1.

Morales, Annareli, Derek J. Posselt, and Hugh Morrison (2021). “Which Combinations of Environmental Conditions and Microphysical Parameter Values Produce a Given Orographic Precipitation Distribution?” In: *Journal of the Atmospheric Sciences* 78.2, pp. 619–638. ISSN: 0022-4928, 1520-0469. DOI: 10.1175/JAS-D-20-0142.1.

Morrison, Hugh and Jason A. Milbrandt (2015). “Parameterization of Cloud Microphysics Based on the Prediction of Bulk Ice Particle Properties. Part I: Scheme Description and Idealized Tests”. In: *Journal of the Atmospheric Sciences* 72.1, pp. 287–311. ISSN: 0022-4928, 1520-0469. DOI: 10.1175/JAS-D-14-0065.1.

Morrison, Hugh et al. (2020). “Confronting the Challenge of Modeling Cloud and Precipitation Microphysics”. In: *Journal of Advances in Modeling Earth Systems* 12.8. ISSN: 1942-2466, 1942-2466. DOI: 10.1029/2019MS001689.

Posselt, Derek J. (2016). “A Bayesian Examination of Deep Convective Squall-Line Sensitivity to Changes in Cloud Microphysical Parameters”. In: *Journal of the Atmospheric Sciences* 73.2, pp. 637–665. ISSN: 0022-4928, 1520-0469. DOI: 10.1175/JAS-D-15-0159.1.

Tully, Colin, David Neubauer, Nadja Omanovic, and Ulrike Lohmann (2021). *Cirrus Cloud Thinning Using a More Physically-Based Ice Microphysics Scheme in the ECHAM-HAM GCM*. Preprint. Clouds and Precipitation/Atmospheric Modelling/Troposphere/Physics (physical properties and processes). DOI: 10.5194/acp-2021-685.

van Lier-Walqui, Marcus, Tomislava Vukicevic, and Derek J. Posselt (2014). “Linearization of Microphysical Parameterization Uncertainty Using Multiplicative Process Perturbation Parameters”. In: *Monthly Weather Review* 142.1, pp. 401–413. ISSN: 0027-0644, 1520-0493. DOI: 10.1175/MWR-D-13-00076.1.

van Lier-Walqui, Marcus, Hugh Morrison, Matthew R. Kumjian, Karly J. Reimel, Olivier P. Prat, Spencer Lunderman, and Matthias Morzfeld (2019). “A Bayesian Approach for Statistical–Physical Bulk Parameterization of Rain Microphysics. Part II: Idealized Markov Chain Monte Carlo Experiments”. In: *Journal of the Atmospheric Sciences* 77.3, pp. 1043–1064. ISSN: 0022-4928, 1520-0469. DOI: 10.1175/JAS-D-19-0071.1.

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