Second review of "**Assessing the potential for simplification in global climate model microphysics**", by Proske et al., submitted to *ACP*.

General comments. 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**.

Semi-major comments.

1. 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.

2. 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.

3. 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.

4. 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. 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).

5. 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).

Minor comments.

Line 32. 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.

Line 89. 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).

Lines 205-206. 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?

Line 216. But processes don't affect "tracer variables" (tracer by the usual definition means not impacted by sources/sinks). Would "prognostic variables" be better?

Line 221. 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.”

Line 231. 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.”

Line 239. I think you mean the range is expanded to eta = 2 in this second step? Can that be clarified in this sentence?

Line 247. Could replace “thus generated” with “LHS generated”, which seems better wording.

Line 437. “slight simplifications” is rather vague. I’d just suggest dropping “slight”.

Lines 475-476. 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”?

Line 513. Similar to a comment above, I think “prognostic variables” would be better as “tracers” often has a somewhat different meaning in the literature.

Line 517. 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”?

Line 548. Again, “tracer” here is inconsistent with the typical use of the term in atmospheric or climate modeling. Suggest simply removing “tracer” in this sentence.

Lines 552-554. 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.

Line 554. 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).

Line 577. 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.

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.

References.

Harrington, J. Y., M. P. Meyers, R. L. Walko, and W. R. Cotton, 1995: Parameterization of ice crystal conversion process due to vapor deposition for mesoscale models using double-moment basis functions. Part I: Basic formulation and parcel model results. *J. Atmos. Sci.*, 52, 4344–4366.

Jensen, A., J. Harrington, H. Morrison, and J. Milbrandt, 2017: Predicting ice shape evolution in a bulk microphysics model. *J. Atmos. Sci.*, 74, 2081-2104.

Milbrandt, J. A., and H. Morrison, 2016: Parameterization of cloud microphysics based on the prediction of ice particle properties. Part 3: Introduction of multiple free categories. *J. Atmos. Sci*., 73, 975-995.