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Interactive comment on “Fluctuations in a quasi-stationary shallow cumulus cloud ensemble” by M. Sakradzija et al.

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General Comments

This is an excellent study, and I was impressed by the thoroughness of the investigations. I will raise a few issues below, but these are largely seeking for clarification of points rather than raising any real doubts about the work. For the most part, it is very well written, and overall a pleasure to read. Therefore, I have no hesitation in recommending publication.

The study aims to lay some necessary groundwork for a stochastic parameterization of shallow convection, with the idea of capturing the scale-dependent stochasticity associated with limited cloud-sampling that occurs for grid boxes of size less than 50km

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or so. For shallow convection, the consideration of two modes seems to be necessary to explain the full details of the variability in the simulated cloud field, as well as a consideration of the lifecycles of the clouds. It is rather tempting for the reader to speculate about those aspects that should be preserved for the parameterization to be developed, but in my view, the authors are wise not to speculate along those lines in this paper. Rather, this paper provides the parameterization developer with a detailed analysis of the consequences of various assumptions that may be practically necessary and/or useful. Importantly, as well as indicating possibly–desirable extensions to the Craig/Cohen/Plant methodology, this includes establishing that some assumptions that might appear doubtful at first glance, are in fact, relatively unimportant.

It would be easy to criticize that this is a single simulation only. That may be a fair criticism of an over-reliance on the results for a parameterization, but it would not be a fair criticism of this paper, which delivers the detailed look at the particular simulation that it sets out to do,

(Finally, I would just like to suggest that a related analysis for other heights, while clearly out of scope here, might be an interesting thing to try...)

Specific Comments

1. Sec. 2.1. It makes complete sense to analyse the RICO-140 simulation for the most part and use the RICO-GCSS only for some sensitivity tests later. However, this only becomes clear late in the paper, and it would be helpful for the authors to explain the rationale explicitly here.
2. Sec. 2.1. Can you explain *why* the RICO-GCSS case produces organization but the RICO-140 does not?
3. p1236, clarification about the definition and description of the two modes would be very helpful. Two modes are identified by means of a buoyancy threshold, and are also identified through the character of fits to the mass flux results. However,

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- so far as I am aware there is no one to one relationship between these identifications. In other words, the data for clouds identified through the buoyancy threshold as belonging to a particular mode is not then fit separately. Thus the link between the lower and upper parts of the distribution and the passive and active cloud respectively, would seem to be assumed rather than demonstrated. A very reasonable assumption, doubtless, but one to explain a little more.
4. Sec. 3. What timestep / timestepping process is used in the numerical stochastic model? In principle, it seems that it would have to be *very* small if the explicit lifecycle of the shortest-lived clouds is to be resolved, with $\Delta t \ll \tau(m)$ for small m . Of course, this will be an issue if an explicit lifecycle is intended to be included for a full stochastic shallow cumulus parameterization because it is likely that Δt of the host model is of order τ for many of the m .
 5. P1243, lines 14-17. The discussion / presentation should be expanded a little here, as the expression for lifetime-averaged cloud area does not immediately follow unless we can assume that $\overline{a\bar{w}} = \bar{a} \times \bar{w}$.
 6. p1255, lines 6-8. Having established the point that convective organization is potentially important for the statistics, this comment that it presents a challenge to model those effects reliably is perfectly true of course. However, a more basic point worth making is that this is scarcely just an issue for stochastic treatments per se. The explicit treatment of such organization within our deterministic parameterizations is missing.
 7. p1257. I would agree with the authors' comments on the subject of consistency here. However, as a reader I did have some concerns about the self-consistency of some of the model-formulation tests earlier on, and so it would have been helpful to have these remarks appear earlier in the text.
 8. A related point about consistency is that the theoretical model used for parameter

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fitting does not include an explicit lifecycle, although the parameters obtained are then applied to a model that does include a lifecycle representation. Are the authors able to speculate / comment on whether adding a lifecycle to the theoretical model might impact on the parameters?

9. Figure 8, and the explicit lifecycle. I was surprised that the authors showed simply a few examples of lifecycles given that they appear to have tracked very many cycles. A composite lifecycle would seem to provide a much better guide for the construction of the explicit formula.

Technical/Minor Corrections

1. p1233, line 11. This is a very standard and very long-standing definition of the mass flux. By all means remind the reader of it, but it seems strange to be citing Cohen and Craig (2006a) just here.
2. p1234, line 1. Other side of what?
3. p1234. line 14. Clarify what is meant by the normalization of $p(m)$.
4. p1235, line 21. “size” is not quite the right word here: no cloud sizes are shown.
5. p1236, line 1-2. I mention this only as a minor point to consider, but there has been some discussion in various contexts as to the relative roles of cloud-area (number) and cloud vertical velocity in accounting for changes in mass flux. This result that the cloud-area dominates here is (I think) worth stating explicitly.
6. p1238, lines 9-10 but there are other examples, please search globally. The phrases short-living and long-living are often used but would read more naturally as short-lived and long-lived.
7. p1242, line 4. straight.

8. Eq. (15). It would be useful to add a note here to clarify that you have assumed \bar{w} is independent of m .
9. p1261, line 15. Miller.
10. There is some repetition in the presentation of the Tables (especially Tables 1 and 3) which could be simplified/rationalized.

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