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Nonlinear Processes in Geophysics
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Interactive Comment

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

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The authors thank Dr R. S. Plant for a positive and thorough review, and insightful comments, which will certainly help in clarification of some points in the manuscript. It is true that there are many important questions and further consideration related to this study, since it is merely a first step towards developing an actual parameterization. Depending on the chosen framework in the future parameterization development, some aspects of this study could and should be modified. One example would be a distribution choice for the passive cloudiness, which is not represented using a mass flux approach in current atmospheric models. The purpose of using a mass flux consideration for all the clouds in this study was to constrain the cloud ensemble, and calculate the contribution of passive and active clouds to cloud cover and mass transport. It is

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also true that taking a single cumulus case we are over-fitting the distribution and this would mean over-reliance for a parameterization. However, an actual development of a general parameterization was not the purpose of this paper where we aim for the thorough understanding of the processes and statistics in a particular cloud ensemble.

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.

   Answer: We acknowledge this comment and the manuscript is edited accordingly in Section 2.1.

2. Sec. 2.1. Can you explain why the RICO-GCSS case produces organization but the RICO-140 does not?

   Answer: The reason lies in a higher precipitation efficiency in the case with the lower cloud droplet number density - RICO-GCSS. Higher rain rate means faster inhomogenization of the sub-cloud layer by the formation of the colder and drier areas by evaporative cooling and moist patches on the edge of these areas. Section 2.1 is edited to mention the rain effect on organization.

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, 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

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active cloud respectively, would seem to be assumed rather than demonstrated. A very reasonable assumption, doubtless, but one to explain a little more.

Answer: The two distribution modes and the link between the upper and lower part of the distribution in this case can actually be identified with passive/active cloud groups based on the tracking results (Figure 1). In that sense we can indeed separate the two modes and it would be possible to fit the distributions separately. However, we choose a mixed theoretical model instead. In the end, the mixed Weibull model is also an approximation and the shape parameter of both distribution modes is set to $k = 0.7$. Given this simplification, a one to one relation between the buoyancy threshold identification of the two modes and identification through the character of the fits of the distribution modes is not explicitly established. For the purpose of modeling the first two moments of a compound distribution of the subgrid convective states, and qualitatively evaluating the compound distribution skewness and its dependence on the grid resolution, these approximations are sufficient and acceptable.

Changes in the manuscript: Figure 3 is modified by including the PDF plot and added to Section 2.3. The two distribution modes are further explained.

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.

   Answer: The time step of all simulations in this paper is one minute. A stochastic parameterization for shallow cumulus would be the most beneficial in atmospheric models with a high horizontal resolution between 1 and 10 km, where it is anyway necessary to have short time steps. As an example, we are currently using ICON (Icosahedral non-hydrostatic) general circulation model on a kilometer scale, where the dynamics time step is 1-10 seconds approximately, and the physics time step is four times longer. This is still well beyond the shortest resolved cloud lifetime of one minute in this study. However, the stochastic module can be set to run simultaneously with the NWP model and exchange information every time step. Even if the cloud lives shorter than the model time step, its contribution to cloud cover and cloud water will be accounted for, however, in this case the variability due to the lifecycle of this cloud will not be taken into account.

Information about the time step is added at the beginning of section 3.

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 $\overline{aw} = \overline{a} \times \overline{w}$.

   Answer: That is correct, we indeed assume that $\overline{aw} = \overline{a} \times \overline{w}$, and we take a step further by setting $w = const.$ over the lifetime of a cloud and as well $\overline{w} = const.$ among different clouds. We show in the manuscript Fig. 3c that the lifetime-average cloud vertical velocity does not scale with the mass flux. A quick test was performed as well with the random sampling of the cloud vertical velocity (not shown here) which suggested that the approximation $\overline{w} = const.$ is reasonable. We write about this as well in short near the end of Section 4.3, where we examine the influence of different choices in model formulation on variability. This assumption may not be sufficient for a conventional mass flux scheme closure, but it is a reasonable assumption to model the variability in shallow convection using a stochastic model.

   Explicit statement about $\overline{w} = const.$ is added to Section 3.2.

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 scarecely just an issue for stochastic treatments

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per se. The explicit treatment of such organization within our deterministic parameterizations is missing.

Answer: We now point this remark out in the manuscript, as well as a suggestion that a stochastic approach may offer a route to parameterize organization. A comment is added near the end of Section 4.2.

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.

   Answer: We now introduce the point about the consistency at the beginning of the section 4.

8. A related point about consistency is that the theoretical model used for parameter 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?

   Answer: The variable of the distribution that we are fitting is the lifetime average, so no lifecycle shape has to be assumed at the moment of closure and fitting. The parameters of the Weibull and Poisson distribution are not affected by assuming an explicit lifecycle function. When it comes to evaluation of the variance of instantaneous compound distribution, that is where the shape of the lifecycle comes into play. However, the theoretical expression of the variance in the case with explicit lifecycles can not be derived analytically.

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.

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Answer: We did not attempt to construct an explicit formula for the lifecycle based on LES and cloud tracking, instead we have chosen a simple approximation having in mind that subtle details in lifecycle shape might not show any benefit for the current study. This is the reason to show only few examples of the lifecycles from the tracking to state that they can be very complicated and diverse. However, when it comes to the parameterization in later stages of development, especially microphysics, this assumption about the shape will have to be further tested. We decided that this would be out of the scope of this paper, and we also show in paper that for the purpose we include the lifecycles the exact shape does not play a big role, i.e. the variance is well reproduced using a simple function.

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.

   Answer: We included Arakawa and Schubert (1974) as a reference here.

2. p1234, line 1. Other side of what?

   Answer: Well, there are no sides. This is now corrected.

3. p1234. line 14. Clarify what is meant be the normalization of $p(m)$.

   Answer: A mistake in writing: “normalized probability density function“ is a pleonasm, and is not correct. The correct version would be: $g(m)$ is normalized using a normalization constant G to get a PDF $p(m)$, so that $\int p(m) = 1$.

   This is now corrected in the manuscript.

4. p1235, line 21. “size” is not quite the right word here: no cloud sizes are shown.

   Answer: This lapsus is corrected, it was referring to the “cloud area“.

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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.

   Answer: A comment is added into the manuscript.

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.

   Answer: Comment acknowledged and changes in the manuscript are made.

7. p1242, line 4. straight

   Answer: The authors thank the reviewer for noticing this spelling error. This is now corrected.

8. Eq. (15). It would be useful to add a note here to clarify that you have assumed $\overline{w}$ is independent of m.

   Answer: Comment added.

9. p1261, line 15. Miller.

   Answer: This is now corrected.

10. There is some repetition in the presentation of the Tables (especially Tables 1 and 3) which could be simplified/rationalized.

    Answer: Table 3 is deleted, since it was obsolete.

Interactive comment on Nonlin. Processes Geophys. Discuss., 1, 1223, 2014.

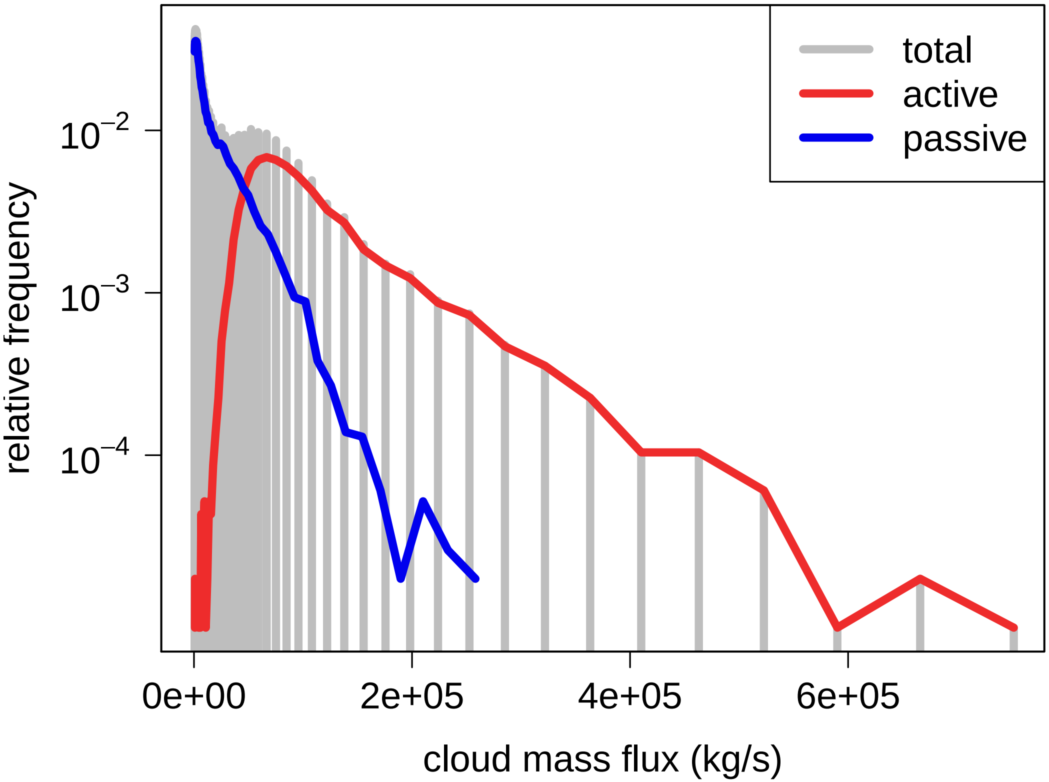

Fig. 1. Semi-logarithmic plot of the cloud rate probability density function of cloud-base mass flux with the split into passive and active mode.

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