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Nonlinear Processes in Geophysics
Discussions

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

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The authors are thankful to the reviewer for comments and suggestions. The authors agree on the remark of reviewer qualifying this study as a proof of principle, rather than as a complete parameterization.

Answer to a general comment:

In the paper we show that the cloud base mass flux does not scale with cloud base vertical velocity, which means that the mass flux is mainly controlled by the cloud base area (Figure 1). Therefore, the results can be extended and are also capable of predicting the cloud area. However, there is a condition to be fulfilled, in order to compare the results from the stochastic model to the coarse-grained LES results. Namely, in

the LES coarse-graining clouds are treated as points belonging to a grid box. This condition is fulfilled for the individual cloud mass flux in our model, where clouds are point-like and always placed withing a single grid box. When it comes to the cloud area, we had to take into account that there can be a situation where the area of a grid box is smaller than the cloud area. To avoid this from happening, we have allowed the clouds in the model to spread across the area of multiple grid boxes, so that the area can be translated to the cloud fraction. In this case the result is no longer comparable with the coarse grained LES because of the choices we made during coarse-graining when the clouds are positioned within the grids of different resolution. So, it is merely a matter of choice whether clouds are point-like or area-like at the point of coarse-graining.

Minor remarks:

- It would be good to check the use of the definite article ("the") throughout the text. I think in a few occasions it could be omitted, e.g. "To collect the information"! "To collect information" (abstract, line 4)

  Answer: We acknowledge and thank the reviewer for this suggestion.

- Abstract, line 9: The word "explaining" here is somewhat confusing. The cloud lifecy- cles have not been introduced yet.

  Answer: abstract changed

- Abstract: the active and passive cloud subtypes could also be mentioned explicitly in the abstract, this makes it more clear to the reader what is meant by subtypes.

  Answer: abstract changed

- Page 1225, line 23: Although the results of LES of shallow convection may only converge at scales of order 10 meter, LES of shallow convection is often performed with grid spacings as large as 100 meter traditionally. The largest eddies

in the boundary layer are also much larger than this order 10 meter. Could you clarify this point?

Answer: By the order of magnitude of 10m, it is meant to enclose the scales from 10 to 100 m. Traditionally, LES models are not converged at the scale of 100m, which is acceptable as long as only domain averaged quantities are used. In our study we have to use somewhat higher resolution because we have to incorporate information about individual events in the formulation of the stochastic model.

- Page 1225, line 26: indicate coarse-graining is one of the possible approaches: “can be coarse-grained”/ “an analysis of coarse-grained results...”

  Answer: ”can be“ is used instead.

- Page 1230, line 10: the use of the term “cloud resolving model” is perhaps confusing here, as CRM usually refers to coarser scale simulations.

  Answer: changed to ”LES as a cloud resolving model“.

- Page 1236, line 4: “contribute around 72%” : make explicit that this concerns the cloud number.

  Answer: This is now added in the text.

- Page 1236, line 24: are the forced clouds indeed part of the same group as the active clouds, or are they classified with the passive clouds?

  Answer: The reviewer has a point here and we reconsider our statement about forced clouds. The tracking routine does not identify the forced clouds as a special cloud type, but the clouds are classified based on the identification of cloud cores as columns where the maximum incloud θ_v excess exceeds a threshold of 0.5 K. Passive clouds contain no buoyant cloud cores, so forced clouds can fall into this category. This however, does not affect any of the results in our

study, because the two cloud-mass-flux distribution modes are distinguished by the buoyancy criterion.

The statement about forced clouds is corrected in the manuscript.

- Page 1237: I found the remarks on system memory and failure rate here may need further explanation. It is suggested that the parameter k is unequal to 1 due to memory effects, but time-dependence does not explicitly come into the analysis at this point as far as I can tell...

  Answer: It is true that in a model setup for a stationary case we do not have a long-term memory, so what affects the shape parameter is a correlation of the cloud mass flux to its lifetime only. The time dependence is introduced as a local effect of diverse cloud lifetimes. In a non-stationary case, the mass flux of individual clouds would be affected through the changes in the large-scale forcing as well, and in that case we would expect further changes in the distribution shape, i.e. it would diverge further from an exponential towards a log-normal shape.

  A short explanation added, section 2.4.

- Page 1241, section 3.1: this section explicitly tests the random properties of the distribution in time, but not in space. This is a point that the authors come back to later on when they test their approach for organized convection (fig. 11d), and the appropriateness of the spatial distribution is also tested in fig. 9, but it would be good to state this more explicitly here.

  Answer: Explained more explicitly in the text.

- Page 1242:, line 4: strait! straight

  Answer: We thank the reviewer. This is now corrected.

- Section 3.2: could you repeat that the mean vertical velocity is a closure parameter.

Answer: Comment added.

- Equation 24 and 25. Maybe the notation for expectation value and standard deviation needs to be explicitly introduced before equation 24 and 25 rather than 26 and 27.

  Answer: Acknowledged and changed.

- Page 1258, line 10: “in their contribution”. Make this more explicit: contribution to what?

  Answer: Acknowledged and changed.

- A problem that would be worth discussing in a bit more detail, is the issue of how to couple the scheme to prognostic fields with partially resolved variability. In the present framework, the large scale variables (e.g. mean mass-flux) are closed on the scale of the full domain, but this would not be the case in a typical 1-kilometer model.

  Answer: In a 1km model the closure would be applied at somewhat larger scales than the grid scale, as in Keane and Plant (2012), but on smaller scales than in their study, which applies to deep convection. *(R. J. Keane and R. S. Plant: Large-scale length and time-scales for use with stochastic convective parametrization, Q. J. R. Meteorol. Soc. 138: 1150 – 1164, July 2012 A)*

- Another point to discuss more explicitly (although clear from the title) is that the cur- rent scheme is designed for convection that is, at least on the large scale, in quasi- equilibrium.

  Answer: In the current study the quasi-equilibrium is assumed at the large scale and this is now written explicitly at the beginning of section 3.

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

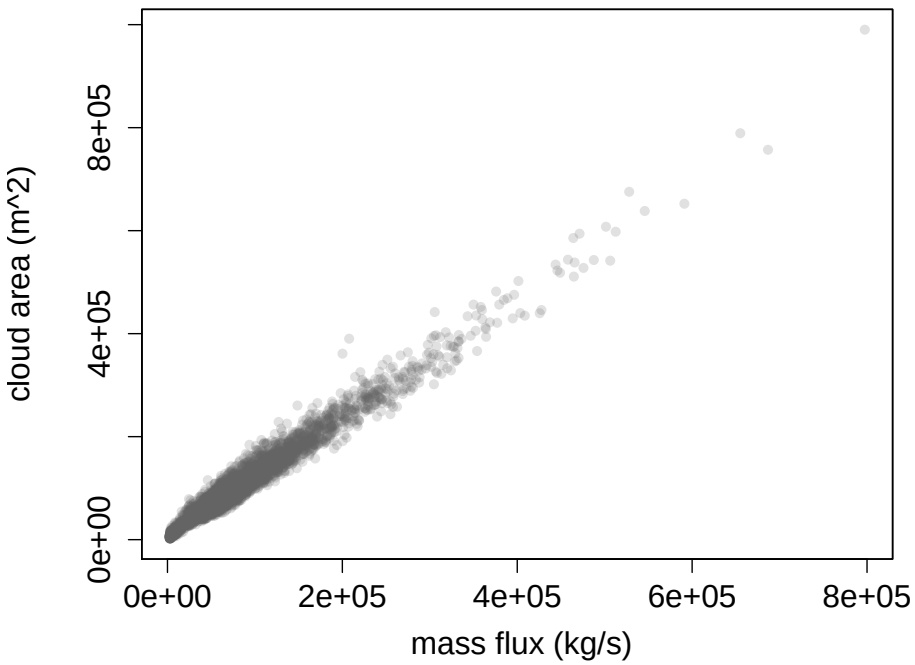

Fig. 1. Scatter plot of cloud-base mass flux and cloud area at the cloud base.