Nonlin. Processes Geophys. Discuss., 1, C573–C576, 2014
www.nonlin-processes-geophys-discuss.net/1/C573/2014/

Nonlinear Processes in Geophysics
Discussions
Open Access

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

Anonymous Referee #2

Received and published: 23 October 2014

This paper presents a new and original framework for the parameterization of shallow convection in models with grey zone grid spacing. In a similar fashion as in the Plant-Craig parameterization for deep convection, individual clouds (or cloud elements) are initialized randomly, with the corresponding mass-flux chosen from a prescribed probability distribution. The authors use Large Eddy Simulations to construct the parameterization framework, and constrain its parameters for a specific case (the GCSS RICO case with a higher droplet number concentration). Rather than a single exponential probability distribution for cloud mass-flux, two generalized Weibull distributions (for an active and a passive cloud mode) are used, as is consistent with LES results. Moreover, in order to represent convective memory, the lifecycle of shallow clouds is explicitly represented. Both of these modifications allow the framework to better reproduce the spatial variability of shallow convection on small (in the order of a few kilometers)

scales.

Although the current approach is limited to this single case, there is ample material to discuss. The paper can in this regard be considered as a proof of principle, rather than as a complete parameterization. The authors undertake a significant effort to test their assumptions, and also test to what extent model complexity can be reduced.

The paper is generally well written with clear figures, and I only have a few concerns which I think should be addressed. I certainly recommend publication: most of my comments are on smaller details. One point which I think is important, is that although the model formulation includes a description of the cloud area, the validation of the spatial and temporal variability currently focuses on the mass-flux. I was wondering to what extent result such as those plotted in figures 9-12 also hold for cloud area.

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)

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

- 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.

- 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?

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

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

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

- 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?

- 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...

- 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.

- Page 1242:, line 4: strait → straight

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

- 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.

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

- 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.

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

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