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Interactive comment on "A refined statistical cloud closure using double-Gaussian probability density functions" *by* A. K. Naumann et al.

Anonymous Referee #1

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This manuscript proposes new parametrisations of cloud fraction, liquid water, and liquid water flux for shallow clouds. The manuscript then tests the new parametrisations using large-eddy simulation (LES) data.

Strengths of the manuscript include the facts that the problem studied is important, the parametrisation approach is sound, and detailed LES of four cloud cases are used to evaluate the results.

However, the manuscript does contain two aspects that could be improved or further discussed, namely, the method of evaluation of the parametrisations, and the possible generality of the results.

I. The method of evaluation of the new parametrisation does not follow standard statis-

tical practice. In particular, the same LES dataset is (apparently) used both to tune and to evaluate the new parametrisation. Instead, in standard statistical cross-validation, the data are divided into independent training and testing parts; a parametrisation is tuned using the training data but then evaluated using the testing data. It is risky to rely on estimates of training error, such as those shown in Table 2 for the new parametrisation, because they typically underestimate the test or generalisation error, i.e. the performance of the model for other data sets.

Relatedly, the comparison between the new parametrisation and the older parametrisations is not entirely meaningful for 3 related reasons:

1. The older parametrisations (L01 and CB95) were trained on different observational or LES datasets than the LES used for evaluation in the manuscript, and hence a (higher) generalisation error is being calculated for the older parametrisations. However, because the error estimate of the new parametrisation is a (lower) training error, the two types of error listed in Table 2 are not comparable quantities.

2. The new parametrisation adds extra tunable parameters that are not present in the older parametrisations. For instance, the CB95 parametrisation for <w'ql'> has two parameters (1.4 and 1.0), whereas the new parametrisation has four parameters (a, b, sk, and 1.0). (Although skewness is readily available from LES data, it is not easy to parametrise accurately in a large-scale model.) Similar, the new parametrisation adds parameters (gamma2 and gamma3) in the prediction of cloud fraction and liquid water. The addition of extra parameters might degrade the generalisation error of the new parametrisation.

3. The parameters were tuned for the new parametrisation but not re-tuned for the older ones.

I recommend that the authors divide the LES data into training and test parts. Then tune all the parametrisations, old and new, to the training data. Finally, compute error statistics and generate plots for the test data. That is, I recommend that the evaluation use cross validation.

II. Although the PDF works well for the cases selected, it is unclear how well the PDF will generalise to practical applications of interest.

1. The new parametrisation of <w’ql’> does not work for the full range of relative humidities. The manuscript states “Because this new parametrisation is designed to fit the LES data with Q1 > -4.0, we limit the range of application for this parametrisation to Q1 > -4.0.” (p. 1099, lines 21-22) The manuscript also states “Note again that for layers with Q1 < -4.0 the parametrisations of the liquid water ïňĆux are not valid.” (p. 1101, lines 11-12). This might hinder use in weather forecast or climate models, which need to behave reasonably in all conditions. I suggest that the authors either test the new parametrisation over the full range of Q1 or else modify the parametrisation so that it does work for the full range.

2. For large values of skewness (6), the new parametrisation leads to large values of sigma1/sigma (4!). This will lead a long tail on the right side of the distribution and large values of kurtosis. This is apparently a good assumption for the PDF of s for the chosen LES of cumulus, but it is unclear how such a spiky PDF will behave when generalised to other cases. Further discussion in the manuscript is welcome.

Minor comments:

I. The computational cost of the method might be larger than expected because it involves iterating to find the relative weight (a), and at each iteration, a square root must be computed. Could the authors comment on the number of iterations used to obtain the results shown in the figures and tables?

II. p. 1094, lines 18-19: In a list of references, it is customary to list earlier references before the later ones.

III. Fig. 7, panel c: Is the blue dashed line computed from Larson et al. 2001a or Cuijpers and Bechtold 1995? There is no legend in panel c. It is unclear whether to

use the legend in panel a or panel f.

Interactive comment on Geosci. Model Dev. Discuss., 6, 1085, 2013.