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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Simulating deep convection with a shallow convection scheme" *by* C. Hohenegger and C. S. Bretherton

Anonymous Referee #1

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General Comments

There is much that is praiseworthy in this article: (i) the basic idea that a shallow scheme can be adpated to handle deep convection by taking into account a few key effects of precipitation processes is an interesting one; (ii) the use of CRM data to study such effects is entirely appropriate; (iii) single-column model results with the modified scheme are encouraging. Therefore, I would consider the article to be ultimately well worth publishing in ACP. However, in a parameterization paper particularly, details of the methods used are extremely important, and it is in specifying those details where the paper does have some weaknesses that are discussed below. I would particularly highlight points 3 and 8 below. Re point 3, is it true that the fits are strongly weighted towards KWAJEX, and very weakly weighted towards BOMEX as Fig 4 would sug-

gest? Re point 8, is the scheme iterative, and if not then is the alternative computation approach actually defensible?

I would also like to encourage the authors to address one further question, perhaps with a paragraph added to the conclusions. To what extent are their modifications generic and extendable to other shallow convection parameterizations operating with other boundary layer schemes, and to what extent are they specific to their particular model configuration? No doubt the authors hope that their paper will attract the attention of other researchers into parameterization, and clearly its impact will be much greater if at least some of their methods could be usefully taken over or adpated for other models.

Specific Comments

1. Sect 2.2. It would be useful to add a line of clarification about the role of TKE: i.e., it seems that the UW shallow scheme must be used alongside a boundary layer scheme that provides TKE as an output.

2. On various occasions the authors refer to a spatial average without specifying how that average has been performed (height integral, pressure integral, mass-weighted height integral....?). They should be specific about the averaging methods.

   - p8395, line 8. The TKE average.
   - p8395, line 23. The cloud-base is also a type of domain-average? Assuming so, is it calculated for each point and averaged, or do you construct an average profile, and then calculate the cloud-base?
   - p8395, lines 26-27 and p8398, line 6. The 200-400m means?
   - Fig 10. The 1km average?

- p8396, line 1 and p8398, line 13. At least in the latter case, please confirm that the horizontal standard deviation of q was computed (how?) directly from the SAM humidity data.

3. The article includes various scatter plots in which results from different experiments are presented together. Also, fits are sometimes constructed to the points on these plots. However, it is important to establish how many points have been taken from which experiment at which time, and so what will be weighting of the different experiments in the fits, and how independent are the data points. Thus, the authors must be explicit about their data sampling strategy for Figures 2, 4 and 7.

4. p8396, Eq. 1. Is the value of $\overline{\mathrm{TKE}_{\mathrm{dry}}}$ obtained from the fit consistent with TKE values found in SAM before the onset of precipitation?

5. p8396, lines 16-17. This sentence seems to invite a lag-correlation analysis to test the suggestion. Have the authors attempted such an analysis?

6. Sect. 3.1, last sentence. This explains why $\mathrm{RR}_{\mathrm{cb}}$ is an appropriate predictor, but not why it is appropriate to take a logarithm?

7. Sect. 3.2. The actual mass-flux closure equations are important, as well as the description of them that is given here. It would be helpful to the reader to show these, making the article more self-contained. Note for example that on p8407, line 12, the modifications to cloud base mass flux are not known to us from Eq. 1, which tells us only the modifications to TKE.

8. p8399, line 2. The first example of a generic issue with this paper. The rain rate at cloud base is used to modify various aspects of a shallow convection scheme, such as the entrainment and (here) the closure. However, the rain rate is generally thought of as an output from a convection scheme rather than an input to

it. For example, the rain rate will depend on the closure, but here we have a closure that depends on rain rate. Thus, I am led to believe that the scheme must surely be iterative. Is that so? If so, how does the iteration work? If not, do the authors perhaps use the rain rate from the previous timestep? But that would seem fraught with danger, given the on-off nature of most convection schemes. In short, it is far from obvious from the information given that the scheme is actually use-able in practice and I need to be told *how* the scheme's calculations were actually performed. (I note that on p8411, line 11 there is the phrase "prior precipitation". The word prior here seems very important, so I was somewhat perturbed to find it mentioned for the first time in the penultimate paragraph of the conlusions!)

9. Sect. 4.1, 2nd paragraph. There is an issue about causality here. For lower values of ϵ_0 (however they are produced), we would expect deeper clouds and more precipitation. Thus "covariability of ϵ_0 and precipitation is expected" as the authors state, but this point by itself does not say anything at all about a causal mechanism or boundary layer organization. I actually agree that the suggested mechanism is a very plausible one, but the language used needs to be much more careful.

10. Sect. 4.1. How was a value of the bulk entrainment coefficient ϵ_0 estimated from SAM data? Extracting entrainment rates $\epsilon = \epsilon_0 \chi_c^2$ from CRM data is a contentious issue in itself: e.g., dependent on the definition of updrafts. The authors need to make clear how they obtained ϵ_0 values.

11. p8401, Eq. 5. It is worth commenting that this uses the vertical velocity at cloud base, which is presumably predicted in the UWS scheme but would not in general be available to a mass flux scheme.

12. p8402, line 16. Please clarify whether Figs. 7a and 7c include data from the decay phase.

13. p8403, line 16. What is the reasoning behind this change?

Technical Corrections

1. p8390, line 4. as starting → as the starting
2. p8390, line 11. θ potential temperature → θ the potential temperature
3. p8390, line 15. θ_{li} equates → θ_{li} is set to
4. p8390, line 20. buoyancy → buoyancies
5. p8391, line 20. related with → related to
6. p8395, line 14. as apparent → as is apparent
7. p8395, line 19. Emanuel's scheme or the Emanuel scheme, but not the Emanuel's scheme
8. Eq. 2a. $L/c_p\sigma_q \rightarrow (L/c_p)\sigma_q$
9. p8399, line 6. increased → decreased (!)
10. p8401, line 13 formulas → formulae
11. p8402, line 4. doesn't → does not
12. p8402, line 9. corresponding Eqs. (7)-(8) → corresponding to Eqs. (7)-(8)
13. p8403, line 2. chosen proportional → chosen inversly proportional (!)
14. p8403, line 3. corresponding Eq. (5) → corresponding to Eq. (5)
15. p8405, lines 16, 18 and 20. UWSD → UWSDall

16. p8406, line 26. as sole → as the sole
17. p8407, line 25. delete the word positive (the boundary layer structure does not have a sign!)
18. p8408, line 25. amounts up to → amounts to up to
19. p8409, line 20. compared to dry → compared to that in the dry
20. p8409, line 28. mixing rate to precipitation → mixing rate on precipitation.
21. p8413, line 32. shallow-t-deep → shallow-to-deep
22. p8414, Rio et al. The page numbers in this reference cannot both be right.
23. p8420, Fig 3 caption. mass-flux binned MSE would be better written as something like mass flux as a function of MSE, similarly to the wording in the main text.
24. p8420, Fig 3. Could the green lines be made clearer?
25. p8430, Fig 14. Please put a note on the caption to explain that a 1:1 line has been added to the plot.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 8385, 2011.