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Interactive comment on "Scheme for calculation of multi-layer cloudiness and precipitation for climate models of intermediate complexity" *by* A. V. Eliseev et al.

Anonymous Referee #1

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This manuscript describes the equations and the tuning procedure of a new scheme for clouds and precipitations, to be used in models of intermediate complexity. The description is fairly complete and, besides few details listed below, it is certainly worth publishing. Still, the authors are making many modeling choices that are not always discussed or even explained. As a result, many formulas appear rather ad-hoc, in particular for readers that may not be familiar with cloud parameterizations. I would also recommend the authors to provide a few definions in the text. For instance "cloud amount" of type x (I would prefer "cloud fraction") is the fraction of the area covered by clouds of type x. Idem for cloud water path. This would help non-specialists to understand equations. Similarly, for eq (18) and following lines 10-20, it would be much more

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usefull to explain that Wtot is a simple weighted mean (assuming a certain superposition scheme), than to introduce many useless intermediate notations W(i) and W(i,j). More generally, it would have been interesting to find a bit more explanations, discussions and background in part 2 of the paper (governing equations). The discussion of the results is quite descriptive, and it would be interesting to have at least some clues on the limitations of the proposed scheme. In particular, why is the cloud water path so severely underestimated by the model? Which processes are missing ? The authors are claiming (page 3263 lines 5-10) a significant improvement in precipitations in the tropical area after calibration. This is not visible on figures 11 to 13. Either the text is a bit over-optimistic, or there is something wrong on the figures. I would therefore recommend publication with minor revisions.

page 3243 line 12: " one-layer cloud schemes are may provide ... ". Two verbs.

p3244 eq (3): C(h,s,m). m should be an index

p3245 l13-15: It seems awkward to me to discuss stratiform cloud base in equation (1) then many other terms (including some which refer to H(b,co) in line 7), then only afterwards introduce H(b,co) in equation (6). Please put Equation (6) with its equivalents in Eq (1).

Equation (7): effective vertical velocity is noted W(e) here, but W(eff) in Table 1.

Equation (7): Why are coefficients indexed by 3,4,5 ? Why not 1,2,3 ? This appears quite a strange choice... This kind of detail does not help the reader.

Equation (10): Idem: why are the indexes 1,2,5 ? Why not 1,2,3 ? Why are some many variables and parameters called C (which certainly makes things a bit confusing...).

p3247 l20: "alphaW is constant". This is also the case of rMK which is not mentioned here.

p3250 l11: is there really a factor f in this formula for precipitation, since it is afterwards multiplied by f according to eq (20) ? In other words, is the final precipitation of, say

liquid water, proportional to the square of f_drop ?

p3250 l11: Why the exponent $\frac{1}{2}$ in this formula, while it is 1 in the equivalent formula for convective precipitations (p3251 l2) ?

Equation (23): Please say that a_tau is a constant.

p3259 l25: "flawed". This seems an inappropriate word to me. The comparison might not be very relevant, but I don't know any example of a model-data comparison "without flaws".

p3264 l11: " Cloud water path is severely overestimated by the scheme". I believe the authors mean "underestimated" (see Fig,6) ? I do not understand what storm tracks have to do with this.

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