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Geoscientific Model Development Discussions

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

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The authors are grateful to the reviewer whose comments helped to clarify the paper. Below the point–to–point answer to the comments made by the reviewer are given.

General comments

- *'I would . . . recommend the authors to provide a few definitions 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.
We replaced the term 'cloud amount' by the term 'cloud fraction' which is suggested by the reviewer. In the place of the text in where this term first appears, we have added an explanation for this. Because the latter explanation refers to the total cloud fraction, we have repeated it once more before the formulae for cloud fractions for individual cloud types (Eq. (9)). The definition of cloud water path is added in the beginning of Sect. 2.3.

- *'Similarly, for eq (18) and following lines 10–20, it would be much more useful 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).'*
This rather lengthy part of the text is replaced by the sentence suggested by the reviewer.

- The main comment by the reviewer was due to *'. . . making many modeling choices that are not always discussed or even explained. . . . It would have been interesting to find a bit more explanations, discussions and background in part 2 of the paper (governing equations).*
We agree with the reviewer. The scheme presented here is an extension of a very simple respective scheme earlier implemented in Climber–2 (see (Petoukhov et al., 2000)). Some equations of the present scheme are based on the equations reported earlier by Petoukhov et al. (2000), some adapted from the book by Mazin and Khrgian (1989), and some derived heuristically. Upon revision, we add heuristic arguments used to achieve our equations.
In the beginning of Sect. 2, we added an explicit statement that the scheme is designed for use in the Earth system models of intermediate complexity. This is the reason why we tried to keep all equations as simple as possible. The latter precludes usage of more computational expensive approaches which are implemented in the state–of–the–art global circulation models.

- *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?*

  We are especially grateful to the reviewer for this comment. We agree that this version of the scheme has a number of important shortcomings. They are listed and discussed in Sect. 4. We note, however, that before implementation of relevant processes into the scheme, such a discussion is just a speculative one. More specifically:

  1) One limitation of the present scheme is the lack of stratocumulus (Sc) decks over the eastern parts of the oceans. Annual mean stratocumulus cloud fraction in these regions fractions up to 0.6 (Wood, 2011) and yields about 80–90 % of all low-level cloud fraction here. Our scheme produces low-level cloud fractions in these regions smaller than 0.2, which underestimate markedly the observed one. We believe that this underestimate is due to neglecting the impact of atmospheric inversions on cloud formation. Such inversions suppress moisture fluxes from the planetary boundary layer to free troposphere. In turn, under these conditions vertical profile of specific (and relative) humidity may deviate strongly from the respective monthly averaged profile. An implementation of this impact may be one of future improvement of our scheme. Note, however, that ERA-40 data underestimate the satellite-derived cloud fraction in these regions as well. This is an example that most contemporary cloudiness scheme in global climate models (GCMs) have problems in representing stratocumulus decks. In particular, Lauer and Hamilton (2013) reported that the latest generation of these models, the CMIP5 GCMs, underestimate amount of subtropical stratocumulus decks by 30–50 %.

  2) In the tropics, too small W_{tot} at least partly related to the above–mentioned lack of stratocumulus decks in the model. In the storm tracks, the respective underestimate is likely due to combination of the processes which are neglected

in our scheme. First, geometric thickness of stratiform clouds is likely too small in our model. In particular, typical thickness of low–level stratiform clouds h_{sl} in middle latitudes is from $150\,m$ to $300\,m$. The latter is markedly smaller than (very limited) observational data summarised by (Mazin and Khrgian, 1989) for which $h_{\mathrm{sl}} \geq 300\,m$. We note that low–level stratiform clouds are major contributors to W_{tot} in the middle latitudes. Similar is true for upper–level stratiform clouds. In our calculations, h_{sh} in middle latitudes is slightly larger than $100\,m$, while according to observations these clouds could be as thick as $1\,\mathrm{km}$ (Mazin and Khrgian, 1989). Thus, the scheme could possibly be improved by revising Eq (9). Additional source of error in W_{tot} is due to underestimated cloud fraction in the storm tracks (recall that our W_{tot} is per grid cell rather than per cloudy part of the cell). Finally, the current version of the scheme completely lacks cloud–aerosol interaction which increase cloud life time and, therefore, enhance their water content. In addition we note that our (very large) biases of the simulated cloud water path are still within the range exhibited by the state–of–the–art global climate models. The latter statement is supported by references on Jiang et al. (2012) and Lauer and Hamilton (2013).

3) One notes that the above–mentioned severe underestimate of the fraction of stratocumulus decks in the subtropics should not severely affect simulation of precipitation because these clouds are non–precipitating ones (Houze, 1994). However, because our precipitation is somewhat too large in middle latitudes, and the cloud water path is too small there, it is likely that the calibrated life times for stratiform clouds are too small, probably by a factor of two.

- *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.*

  The sentence about the large increase in precipitation in the convection–affected regions was in error. It is removed in the revised version. However, another sentence (about the respective increase of precipitation in the monsoon–affected region) still holds. The latter increase is visible in Figs. 11–13.

Minor and technical comments

- *p. 3243, line 12: " one–layer cloud schemes are may provide ... ". Two verbs.*
  The extra word 'are' is removed.
- *p. 3244, eq. (3):* $C_{\mathsf{h,s,m}}$. m *should be an index*
  Upon revision, this coefficient is renamed to $T_{\mathsf{h},0}$.
- *p. 3245, lines 13–15: It seems awkward to me to discuss stratiform cloud base in equation (1) then many other terms (including some which refer to* $H(\mathsf{b},\mathsf{co})$ *in line 7), then only afterwards introduce* $H(\mathsf{b},\mathsf{co})$ *in equation (6). Please put Equation (6) with its equivalents in Eq (1).*
  Upon revision, the equation for the height of convective cloud base (former Eq. (6))) is described before the respective equation for stratiform cloud base (former Eq. (1)). As a result, all relevant values in the latter equation are introduced in advance.
- *Eq. (7): effective vertical velocity is noted* $w(\mathrm{e})$ *here, but* $w(\mathsf{eff})$ *in Table 1.*
  The notation for effective velocity in Table 1 is corrected.
- *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*:
  The subscripts of the coefficients are changed according to the reviewer's suggestion.

- *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...)*
  We agree that this notation is difficult to read. Upon revision, some parameters are renamed based on their physical meaning. In particular, c_{h} in Eq. (2) is renamed to l_{h}, $C_{\mathsf{h,s,m}}$ in Eq. (3) is renamed to $T_{\mathsf{h},0}$, $C_{c,\mathsf{s},5}$ in Eq. (10) is $w_{\mathsf{e},0}$ now, and $c_{\mathsf{e},j}$ is $l_{\mathsf{RH},j}$. Now only the non–dimensional parameters are named C. The number of subscripts for other parameters is reduced to simplify the reading.
- *p. 3247, l. 20: p3247 l20: "*α_W *is constant". This is also the case of* r_{MK} *which is not mentioned here.*
  We have added a sentence that r_{MK} is constant.
- *p. 3250, l. 11: 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}*?* The factor f_k in this formula was typed erroneously. It is removed in the revised version of the manuscript.
- *p. 3250, l. 11: Why the exponent 2 in this formula, while it is 1 in the equivalent formula for convective precipitations (p. 3251, l. 2)?*
  We assume that precipitation falls only in the cloudy part of a grid cell. Thus, precipitation from clouds of type j is proportional to c_j. For convective clouds, life time is assumed to be independent of their extent. For stratiform clouds, however, we assume a dependence $\tau_{j,k} \propto c_j^{1/2}$ ($j \in \{sl,\ sm,\ sh\}$, $k \in \{drop,\ ice\}$). This is done in order to make the clouds, which are more horizontally extensive, exists longer than smaller (presumably broken) clouds. This part of the manuscript is clarified upon revision.
- *Equation (23): Please say that* a_τ *is a constant.*
  We have added a sentence that a_τ is a constant.

- *p. 3259, l. 25: "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".*
  We have reformulated this sentence in a more appropriate way.

- it p. 3264, l. 11: "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.
  We have replaced the word 'overestimated' by the word 'underestimated'. We note that storm tracks regions make a major contribution to this underestimation.

Interactive comment on Geosci. Model Dev. Discuss., 6, 3241, 2013.