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Interactive Comment

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 comment

Comparison with observations is too rosy. More needs to be said about shortcomings and limitations of the described scheme.

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Interactive Discussion
Discussion Paper

We agree that some biases of our scheme were not stated in the previous versions, and that some statement about the scheme's performance was in error. Similar comments were made by the other reviewer. We are grateful to these comments because they helped us to outline possible future improvements of the scheme. In particular

1) One major weak point 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 complete neglect of 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

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larger than $100\,m$, while according to observations these clouds could be as thick as $1\,\mathrm{km}$ (Mazin and Khrgian, 1989). All this indicates to a possible necessity to revise 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.

4) The sentence about the large increase in precipitation in the convection–affected regions and the respective improvement in performance 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 in Figs. 11–13.

All this items are added to the relevant places of the text and briefly mentioned in Conclusions and in the abstract. In particular, the abstract is extended by the sentence stating that the simulated cloud water path is too small, probably because the simulated vertical extent of stratiform clouds is too small. In addition, a statement on the underestimated precipitation in the tropics is added to the abstract.

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Minor and technical comments

- *Page 3244, Eq (3): indexes "s" and "m" are not specified*
  According to the suggestion made by the other reviewer, to simplify reading, this coefficient is renamed to C_{h}.
- *Page 3245, Eq (7): w_{oro} is not defined.*
  The definition of w_{oro} is added right after Eq. (7).
- *Page 3246, line 4. "is zeroed" sounds awkward.*
  This phrase is replaced by 'set to zero'.
- *Page 3248. Meaning of index "MK" should be explained here, not on page 3252*
  A sentence is added that the subscript indicates that this Equation is adapted from the book by Mazin and Khrgian.
- *Line 4: "by" is not needed here.*
  the word 'by' is removed.
- *Lines 12–13. Approximation cannot be based on Figure; it can be based on the results or dependency shown on Figure.*
  The sentence is reformulated.
- *Page 3249, line 15. It should be W(1,3) not W(1,1)*
  According to the suggestion made by the other reviewer, this part of the text is shortened. The latter has resulted in removal of the variables listed by this reviewer.
- *Page 3251, line 19. Replace "figure" with "described"*
  The suggested replacement is made.

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- *Page 3252, line 19. Replace "constructed" with "calculate".*
  The suggested replacement is made.
- *Page 3255, line 8–9. It is said in abstract that ERA–40 data for 1979 to 2001, not 2002, were used*
  We have used the ERA–40 data for 1979–2001 because year 2002 is incomplete. The respective change is made in the beginning of Sect. 3.2
- *Page 3257, lines 19–22. Six year averages hardly can be called climatological means.*
  We agree that a 6–year period is too short to be called 'climatological mean'. In the revised version this phrase is replaced by 'multi–year monthly means'
- *Page 3262. It should be mentioned (and, if possible, explained) that scheme rather poor simulate precipitation in the tropical region.*
  This bias is now explicitly mentioned in Sects. 4.2, 5, as well as in the abstract.
- *Pages 3263–3264. Once again, discussion of the scheme's shortcomings needs to be added to the conclusions.*
  A list of such shortcomings in the conclusion is slightly extended. In addition, a sentence is added that some equations entering the scheme are necessary to revise in future.

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