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Geoscientific Model Development Discussions

Interactive comment on "A dynamic probability density function treatment of cloud mass and number concentrations for low level clouds in GFDL SCM/GCM" *by* H. Guo et al.

Anonymous Referee #1

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This manuscript describes the introduction of an aerosol activation equation to a sub-grid scale parameterization for clouds for use at global scales. The parameterization ("CLUBB") is unique in that it predicts the joint distribution of temperature, water mass, and vertical velocity; the distribution of vertical velocity is then a natural link to aerosol activation. Here an equation for mean droplet number is introduced that includes transport, a source of drops (i.e. aerosol activation), and sinks. This initial implementation neglects microphysical sinks, so that only activation, transport, and evaporation are tested. The scheme is tested in three diverse regimes and performs as expected, producing larger drop numbers in the presence of more aerosol. On a tangential note, the authors demonstrate that CLUBB on a fine vertical mesh can be used effectively with

a coarse resolution host model.

The work reported here is an important preliminary step in allowing for aerosol-cloud interactions in this particular sub-grid model. But the contribution is so modest that it seem premature to publish. I suggest the authors wait until the scheme can be tested in its entirety, i.e. after allowing the changes in drop concentrations to affect the evolution of the clouds themselves, and then submit a manuscript that builds from the material presented here.

Broad comments

The present material is interesting enough if unsurprising. Things will get substantially more interesting when the predicted droplet number is allowed to influence cloud microphysics, which can in turn feed back on droplet number through precipitation. One imagines that a manuscript that included the results from section 3.2 but also showed the cloud evolution in a fully coupled system would be a contribution well worth publishing.

The more senior authors would do the first author a service by helping him or her calibrate the level of discussion to a technical journal. Readers can be introduced to the basic ideas here fairly simply: droplet activation depends non-linearly on vertical velocity at fine scales that are not resolved or treated in other parameterizations, but CLUBB has precisely the information needed to consistently diagnose this activation. This can be put into context in one or two modest paragraphs regarding low clouds in climate models, etc. These points about subgrid-scale distributions and nonlinearity only need to made once, though. Similarly, when revising a paper with more results, the authors may find it useful to try to streamline the writing.

Appendix A contains an interesting practical result, namely that CLUBB coupled to aerosol activation can be successfully run on a fine vertical grid coupled to a coarse grid in the host model. It would be useful to draw more attention to this result by foreshadowing it in the introduction, remarking on it cleanly in section 3, and highlighting the result (not the simulations attempted) in the conclusions.

Can the authors comment on the consistency between using a PDF to describe the distribution of cloud liquid water and the use of a single area-averaged value of droplet concentration (e.g. Eq. 3)? One could interpret this as implying instantaneous mixing of droplet number, but then one wonders what kind of system would support fast mixing of number but a PDF of water content.

The three cases are well-chosen to span a wide range of boundary layer cloud regimes, but there is so much discussion of the specifications and of CLUBB's performance that it's distracting. Roughly 1/4 of the manuscript addresses CLUBB performance in simulating cloud macrophysics (cloud fraction and liquid water content) but these are not at all the subject of this paper. One possible way to organize section 3 is to mention the three cases and provide Figure 1 with relatively little commentary, so that readers will know that the cloud fraction profiles that figure in droplet concentrations are not wildly out of line. Some of the details on pages 549-552 can then be moved to an appendix if the authors feel strongly about it; the paper would not suffer greatly if much of this detail was simply omitted.

Specific comments

The term "dynamics PDF" is not very informative. There are other cloud schemes that predict the moments of one or more PDFs; what's unusual about CLUBB is that the distribution of vertical velocity is also included. It's not clear why the parameterization can't simply be referred to as CLUBB.

The authors assert that using a predicted PDF of vertical velocity is a solution to the problem of a "scale gap." But the PDF doesn't have an explicit scale - it's simply assumed that the grid box contains enough realizations of the process at hand that the PDF can be treated as continuous. It would be useful to help readers understand what it means to use a PDF in this context.

As a personal opinion, the introduction of the discrete equation on page 547 seems unnecessary - one can easily make the points on line 15 in a sentence or two, and the discretization holds no other surprises.

Details

It would be useful to introduce CLUBB in a short paragraph at the beginning of section 2. A few sentences would do.

Page 542, Line 19: It is more accurate to say that low-level clouds explain the diversity among current model projections of climate sensitivity. This true uncertainty is undoubtedly larger.

Page 543, line 26: One need not cite a PhD thesis if the results are also available in more easily-accessed literature.

If the authors say (as, for example, on page 544 line 11) "One purpose of this paper…" readers will then look for a second purpose in the text. Those readers will be frustrated.

Discussion of the single-column modeling framework can be deferred to section 2, perhaps at the end as a section discussing "implementation in CLUBB and the GFDL SCM."

Page 554: I interpret the text here as implying that diagnostic predictions of N_d are in better agreement with the LES when the standard deviation of vertical velocity is specified as 2 m/s as opposed to 0.7 m/s, but that the former value is inconsistent with the observed and modeled PDFs in these cases. Here is a learning opportunity - why, precisely, does unrealistic variability provide realistic drop numbers?

Interactive comment on Geosci. Model Dev. Discuss., 3, 541, 2010.