Dear Editor and Reviewers,

We greatly appreciate the constructive comments from the anonymous reviewers, and have addressed all the reviewers' comments. The details are as follows.

Anonymous Referee #1

Received and published: 5 July 2010

This manuscript describes the introduction of an aerosol activation equation to a subgrid 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 seems 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.

We have now coupled a two-moment microphysical scheme with CLUBB. For three non-precipitating cloud cases, we will explore how clouds and precipitation respond to different aerosol loadings. In addition to these non-precipitating cloud cases, in the revised manuscript, we will further add two precipitating cases and examine the effects of aerosols on cloud properties, e.g., liquid water path, cloud water content, and precipitation rate.

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.

We will add two precipitating cases, and discuss the aerosol effects on both precipitating and non-precipitating clouds.

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.

We will re-structure the manuscript.

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.

In the revised manuscript we will focus on the high-resolution results in the main text, and discuss the low-resolution results in the Appendix.

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 dynamics-PDF approach provides a PDF of droplet concentrations, which are shown in Fig. 5. Results from LES are generally reported as area-averaged means, and area-averaged means are important measures of the behavior of the parameterization. Presenting area-average means facilitates comparison between the dynamics-PDF parameterization and reported results from LES studies in the published literature. In presenting area-averaged means, there is no implication that the physical system is uniform. Rather, the physical system is characterized by both means and variations about these means. When using the droplet concentrations with microphysics or radiative transfer parameterizations, there are various approximation strategies available, some of which can explicitly consider the PDF of droplet concentrations, e.g., sub-columns (Pincus et al., 2006, *Mon. Wea. Rev.*) and Latin hypercube sampling (Larson et al., 2005, *J. Atmos. Sci.*).

In the revised manuscript, after Eq. (3) we will note that information from the PDFs remains available after the area means are calculated and can be used with microphysics and radiative transfer as noted. Area-averages can also be used, based on the assumption that the non-linearity associated with activation is more important than those associated with other microphysical and radiative processes.

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.

We will shorten the discussion of CLUBB performance in simulating cloud macrophysics (e.g., cloud fraction and liquid water content) in the revised manuscript.

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 revised manuscript will define "dynamics PDF" more thoroughly the first time than it is used in the manuscript. In using the term "dynamics PDF" we are attempting to convey its most unique attribute, namely that it includes a PDF of motions (vertical velocities). Note that the term is *dynamics* PDF, not *multi-variate* PDF. In choosing this term, we agree with the reviewer that what is unusual about the method is that it includes the distribution of vertical velocities. The CLUBB (Cloud Layers Unified By Binormals) acronym does not immediately convey this; indeed, even if the acronym is expanded, there is no indication that the distribution of vertical velocities is included.

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.

It is correct that the PDF doesn't have an explicit scale. However, the functional form of the PDF was chosen based on large eddy simulations with domain sizes of a few to tens of kilometers. These domains are smaller than a typical grid box in a GCM and it is therefore realistic to assume that a GCM grid box would contain enough realizations. The use of a PDF is important because it provides the subgrid-scale information necessary for a realistic representation of cloud drop nucleation.

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.

We will remove the discrete equation on page 547, and add

"We use center-difference to discretize the above transport equation. In order to avoid a potential division by zero, we place a lower threshold on cloud fraction (CF_{min}) in the denominator. As long as cloud fraction is smaller than CF_{min}, N_d is set to be 0."

Details

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

At the beginning of section 2, we will add a few sentences to introduce CLUBB.

"The dynamics-PDF parameterization 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. So the dynamics-PDF parameterization has precisely the information needed to consistently diagnose the activation."

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.

On Page 542, Line 19, we will change to

"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."

We will delete the citation of the “PhD thesis” in the context and in the “Reference”.

We will change from "One purpose of this paper …” to “Main purpose of this paper …”.

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?

To provide realistic drop numbers, we need both the PDF of vertical velocity and its associated parameters are as accurately as possible. The fact that the unrealistic variability of 2 m/s provides realistic drop numbers might be due to some compensating errors, and might imply that the assumption of a single Gaussian PDF is not sufficient to represent the sub-grid variability of vertical velocity realistically. We will provide further information on this point in the revised manuscript.

Anonymous Referee #2

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This paper presents initial tests for the new joint treatment of boundary layer turbulent and cloud processes (CLUBB) in the GFDL AM3 single column model (SCM). The performance of the new scheme, or more precisely, the droplet activation part of the scheme, is demonstrated using three cases with different cloud types and cloud fractions, with an LES model serving as a benchmark. The tests include simulations with two aerosol loadings as well as CLUBB runs at low and high resolutions. While treating subgrid vertical motions is clearly necessary for any realistic SCM simulations of clouds, the goals and benefits of the specific approach needs to be identified more clearly to be useful for the modeling community. The outlined model development seems viable, but a major revision of the manuscript is needed to bring it to the publication level.

General comments:

1. The study is motivated by the need to have a droplet activation scheme driven by the sub-grid turbulent motions. Other models have use pdfs of vertical velocity to predict droplet activation. As pointed out in the manuscript, such pdf often take a form of a Gaussian distribution with a width related to some measure of turbulence intensity (e.g., TKE). The CLUBB treatment discussed here is different because it uses a pdf which is bi-modal and multi-variate. Unfortunately, neither feature is discussed in the context of droplet activation. A double Gaussian vertical velocity pdf is quite apparent in figure 4 but never mentioned in the paper. A multi-variate nature of CLUBB's pdf is mentioned and reflected in Eq. 3, but its role in treating droplet activations is not discussed. These are the two unique aspects of the new treatment, which this work should focus more instead of concentrating on a comparison with a somewhat artificially simplified parameterization with a prescribed updraft.

The bi-modal and multi-variate features of CLUBB are based on the published work on large eddy simulations and analyses of aircraft data (Larson et al., *J. Atmos. Sci.*, 2001, 2002; Golaz et al., *J. Atmos. Sci.*, 2002a, 2002b).

The double Gaussian functional form probably confers the most benefit in the case of cumulus clouds, which are highly skewed. A single Gaussian is always unskewed.

The multivariate PDF is useful because only the updrafts in the saturated regions matter for droplet activation. The multivariate PDF is most useful whenever i) there is partial cloudiness; or ii) there is a strong correlation between vertical velocity and any thermodynamic variables that influence droplet activation.

Examples of PDFs from aircraft data show that some PDFs are skewed and correlated (Larson et al., 2002). We will add some discussion in the revised manuscript.

2. The main conclusion of the paper, that the proposed implementation is promising and feasible, is rather weak. What aspects of the simulations were improved using the new scheme? What is the reason for these improvements? Does the bi-modality or the use of a joint vertical velocity – temperature – moisture pdf plays a larger role? In the introduction it is mentioned that the droplet number transport is also handled by CLUBB. Does this have any effect on the results?

The revised manuscript will show additional, precipitating cases that better illustrate links between cloud drop activation, microphysics, and cloud properties. Use of the same underlying sub-grid PDF for sub-grid scale transport, cloud properties, and activation is new. Comparisons with aircraft data and LES (Larson et. al *J. Atmos. Sci.,* 2001, 2002) have shown that using a PDF with variable skewness, such as a double Gaussian, is important in order to accurately represent shallow cumulus layers. Using a joint PDF of vertical velocity (w), liquid potential temperature (θ_l), and total water mixing ratio (q_t) allows for the coupling between the dynamics and thermodynamics. A key term leading to the production of turbulence is the buoyancy term which involves the coupling of all three variables.

As we noted on p.543 (Ramanathan et al., 2001, Fig.5) of the original manuscript, diagnostic methods based only aerosol concentration can not capture the observed range of cloud droplet number. The dynamics PDF method has the potential to do so. We will also discuss in more detail in the revised manuscript the importance of the relationship between distributions of vertical velocity and droplet number.

Cloud droplet transport term is an important term for the cloud drop number budget. New cloud drops nucleate near cloud base and are transported upward by turbulence.

3. Adopting a higher order turbulence closure parameterization obviously requires extra computations. How much does the CLUBB slow down the SCM?

CLUBB slows down the entire SCM simulations by about 14%. However, the computational costs of single column simulations are not representative of those of global simulations, since in the single column simulations, a majority of CPU time occurs during the initialization process (> 80%). Also the SCM includes microphysics but not detailed radiative transfer.

For the main dynamic and thermodynamic loops (except initialization, termination, and restart), incorporating CLUBB slows down single column simulations by a factor of ~ 2.5 on average.

4. The sensitivity of the simulations to CLUBB's vertical resolution is an interesting aspect of the study but needs to be put into context. The changes appear to be not that large – much smaller than the difference between the SCM and LES benchmark. Does this improvement worth extra computing power? Also, since one would expect the simulations to improve at higher resolution, should the high resolution CLUBB be compared with the high resolution SCM with a diagnostic sigma_w treatment?

In the main text of the revised manuscript, we will focus on the performance of high resolution CLUBB with high resolution SCM, and we will also compare the high resolution CLUBB with the high resolution SCM with a diagnostic σ_w treatment.

The performance of low resolution CLUBB with low resolution SCM will be discussed in the Appendix.

An assessment of the benefits and costs of high resolution awaits studies with a general circulation model (GCM). In practice, GCM construction entails trade-offs between accuracy and computational speed. Our purpose here is only to provide a general indication of the robustness of the simulation to reduced vertical resolution and to suggest that some reduction in resolution is possible without fundamentally altering the character of solutions.

Specific comments:

1) Consider a more specific title since the manuscript covers only one aspect of the number concentration treatment (i.e., droplet activation). Also GCM could be removed from the title; otherwise readers may expect to see results from global simulations.

We will change the title to

"A dynamics probability density function treatment of cloud droplet activation for large-scale models: Single Column Tests"

2) The meaning of "dynamic pdf" or "dynamics-pdf" in title and text is not clear. Is it the same as "multi-variate"?

They are not the same.

The term "dynamics pdf" conveys its most unique attribute, namely that it includes a PDF of motions (vertical velocities). It does use a multi-variate joint PDF of vertical velocity (w), liquid potential temperature (θ_l), and total water mixing ratio (q_t), in the interest of keeping the characterization short we have not included the adjective “multi-variate” in the description. We will define “dynamics pdf” more clearly in the revised manuscript.

3) p. 551, ln. 4: A plot of time series of cloud fraction or liquid water path could be useful to illustrate the “quisi-steady states” of the cloud fields.

We will add the time series of liquid water path to illustrate that cloud fields have reached “quasi-steady states” for the last hour for BOMEX, RF01, and ATEX.

4) p. 552, last paragraph: Aerosol activation spectrum, or, at least, a size distribution spectrum would be helpful to show in addition to providing the mass loadings.

The aerosol size spectrum basically follows what is adopted in the GFDL AM3 (Ming et al., *J. Atmos. Sci*. 2007). But some modifications have been made.

i) Sulfate is assumed to be entirely in the accumulation mode.

ii) Sulfate aerosol spectrum consists of two lognormal modes (N_1:N_2=17:3, $D_{g,1}$=0.01 μm, σ_1=1.6, $D_{g,2}$=0.07 μm, σ_2=2.0) in Ming et al., 2007. But in this study, the diameter of the second mode ($D_{g,2}$) is changed from 0.07 μm to 0.11 μm.

5) p. 553, lns. 20-25: I am not convinced that it is justified to abandon a more realistic diagnostic treatment for the sigma_w in favor of a constant sigma_w for the sake of simplicity. Is this what is used in GFDL GCM? If not, then why not use a TKE-diagnosed sigma_w?

Like many GCMs, the GFDL GCM estimates σ_w from the boundary layer eddy diffusivity coefficients and imposes a lower bound on σ_w. In the GFDL GCM, the lower bound is invoked 98% of the time. The parameterization thus essentially behaves as if σ_w was fixed. This may also be the case in other GCMs. Some GCMs directly use a constant variance which is not related to boundary layer turbulence (e.g. Chuang et. al *J. Geophys. Res*, 1997, 2002).

6) p. 555, ln. 5: Do you mean the positive skewness is indicative of turbulent structure of a convective boundary layer?

We mean the vertical velocity skewness is often indicative of turbulent structure. It can be positive, negative, or neutral (Moeng and Rotunno, 1990, *J. Atmos. Sci*.).

7) p. 556, lns. 3-5: Are there any global models that use a constant velocity for droplet activations? If so, a reference is needed here.

We will clarify the text in the revised manuscript. As mentioned above, some GCMs use a constant variance. Some also replace the subgrid w PDF with a single characteristic w related to TKE or CAPE (Lohmann et al., 1999, *J. Geophys. Res*.; Lohmann, 2002, *J. Atmos. Sci*.). Based on LES work of Jiang and Cotton (*J. Geophys. Res*, 2005), it might be difficult to find a single characteristic w applicable for a wide range of regimes.

8) Figure 3: The two dark-colored lines are hard to distinguish. Consider changing color or using markers to make these lines more easily identifiable.

We will re-plot Fig.3 using different color lines.

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