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Atmospheric Chemistry and Physics Discussions

Interactive comment on "How important is the vertical structure for the representation of aerosol impacts on the diurnal cycle of marine stratocumulus?" *by* I. Sandu et al.

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We would like to first thank the reviewer for the time he/she took on the manuscript, and for helping us refine our arguments and more clearly bring out our main ideas. Please find below our responses to the different comments and how we are going to address them in the revised version of the manuscript.

Sci Questions/Specific Comments:

Comment p 5471, l 25

R: We have indeed not clearly explained how the subsidence was prescribed. In fact,

we calculate the subsidence by assuming a fixed divergence, D within the boundary layer, and no divergence aloft. By continuity this implies that w_s = -Dz for z< z_i and w_s(z) = w_s(z_i) for z> z_i. Here z_i is the initial value of the inversion height, i.e., 600 m, which implies a constant subsidence with time. For the simulations with time-varying divergence the same procedure is used, except D is linearly interpolated between its initial value and its value after 1, 2, and 3 days. Formulating the subsidence in this way means that the cloud-top subsidence does not increase with a deepening boundary layer. We choose this approach because by fixing w_s above the initial height of the boundary layer it was easier to minimize the drift in the values of the free-tropospheric temperature. Even so some drift in the temperature profiles remains, however because this is the same in both the polluted and pristine cases we do not think it significantly impacts our findings (see Fig. 1 included here, which is the same as as Fig. 2 from the article but for pristine clouds). In retrospect our solution to this problem is somewhat artificial, specifying subsidence using a local weak temperature gradient approach above the boundary layer would have been a more elegant approach, but makes it difficult to maintain continuity in w_s at the top of the boundary layer. These issues arise from the need to separate the large from the boundary layer-scale dynamics, which is inherent in the LES approach for such flows.

In order to clarify these issues in the text, we will include the above explanation in the paragraph beginning on p. 5471, line 25, after the phrases:

Throughout the entire set of simulations, the only specified large scale forcing is the large scale-subsidence. The influence of the subsidence, w_s, on the temperature and the water content is accounted for via the source terms $w_s \: \frac{\partial \theta_l}{\partial z}$ and $w_s \: \frac{\partial q_t}{\partial z}$ in the equations for θ_l and q_t. We calculate the subsidence by assuming . . ...

Moreover, the text on p. 5472, line 9 is revised as:

For the simulations presented here, the imbalance between the subsidence warming and the net daily averaged radiative cooling within the free troposphere results after 72 h in a slight temperature drift, from 1 to 3 K depending on the boundary conditions, as illustrated in Fig. 2a for the polluted set of simulations. The pristine simulations experience the same temperature drifts to within a few tens of K (not shown). We can therefore consider that the free troposphere temperature drift does not play a significant role in the differences between the pristine and polluted sets of simulations.

Comment p 5481, l 17

R: This verification was actually done (and we will mention that in the manuscript). The graph summarizing the results was not shown in the article, but we include it here (Fig. 2). We propose to revise the sentence as:

Third, the mean radiative flux divergence of the LES is different from the flux divergence calculated with the mean LES fields (up to 5 W m-2) because radiative transfer is non linear and the LWP is heterogeneous horizontally (heterogeneous radiative bias (Barker and Davies, 1992)).

Comment p 5484 Section 5.2

R: First, we would like to mention that the MLM used here does not include precipitation, except for the two tests mentioned in sections 4.3 and 5.2 where we included precipitation at the surface (given by the empirical parameterization similar to the one proposed by Geoffroy et al. (2008) mentioned in paragraph 4.3). So for the MLM run without precipitation, mechanisms 1a and 1b are not included. Mechanism 1a is taken into account for the MLM run with surface precipitation. As by definition, the STBL is perfectly mixed the effects of condensational warming within the cloud and evaporative cooling of precipitation beneath the cloud (i.e. mechanism 1b) cannot be incorporated in the MLM. So it appears that the differences between the two clouds evolution are associated with the differences in cloud top entrainment between the two clouds.(this idea is sustained by the fact that the differences between the MLM runs with and without precipitation at the surface, i.e. fig. 8 are small).

Concerning the entrainment rate, as we stated in the article the Bretherton (2007) scheme gives a rather important bias, i.e -0.19 cm/s if a2=15 and -0.1cm/s if a2=60. We note that one probably could derive a better parameterization for entrainment if one adjusted the coefficients to an existing model, i.e., Bretherton et al. (2007), but we wanted to avoid introducing yet another parameterization for entrainment. Using the biased entrainment rates for the MLM given by Bretherton et al. (2007) original parameterization would have made in our opinion the results even harder to interpret. This explains our choice for the original Turton and Nicholls (1987) parameterization. As we mentioned in the manuscript, we consider that the good behavior of this parameterization is explained by the fact that the effects of the liquid water fluxes are captured, both through changes to the liquid water profile (which enters into the a2 term) and to the buoyancy flux which determines the w* term.

General comment 1.

R: We completely agree with this view that indeed precisely reflects ours and we understand that the message was poorly transmitted. We will make these ideas more clear throughout the paper. Thus, the last phrase of the abstract will be revised as:

It is shown that the deviations from the mixed layer model are crucial ingredients of the aerosol impacts so that a mixed layer model, by definition, misses crucial physics, hence shall not be used to predict the sign of the liquid water path changes.

The last phrase of section 3 (p 5477 l 9) will be revised as:

The most contrasting features of a precipitating and a non-precipitating cloud diurnal cycle, thus arise from the differences in the level of decoupling and its time of occurrence. So, it appears that the well mixed framework is not suited to capture the origin of the difference between the diurnal cycles of the two clouds.

Moreover the last paragraph of the conclusions will be revised as:

This exercise therefore suggests that the deviations of the vertical structure from a well mixed layer are key ingredients to the response of marine stratocumulus to changes in the aerosol loading. Such deviations should hence be properly represented by the parameterizations of cloudy boundary layers in order to correctly predict the aerosol impacts on clouds and thus to reduce the uncertainties of aerosol indirect effects in climate change predictions.

Technical Corrections:

Comment p 5467 l 2-8

R: We completely agree with this comment and we will replace the lines 2-9 on page 5467 by the following phrase:

A common starting point for descriptions of the STBL is that for a bulk layer whose entire evolution is largely dictated by the energy and the moisture fluxes through the surface and the inversion layer (see, Stevens, 2005, for a review). By further assuming that the STBL is vertically uniform or well mixed one arrives at an effectively 0-dimensional model first expounded by Lily (1968).

Comment p 5469 l 25ish

R: We addressed this comment by introducing here the phrase

The three pairs of simulations represent modified runs of the EUROCS/FIRE case \citep{Duynkerkeetal-2004}.

General comment

R: It is true that we simulated here 3 cases similar to FIRE case. For two of them we let the large scale conditions vary in time to capture the advection of the air mass towards a warmer ocean and a weaker large-scale divergence which is typical for these regions. However, even if the simulations cover thus a quite wide panel of conditions (as showed in table 2), we agree that this type of simulations should be extended to other regions and other times of the years, before trying to generalize the results.

p Comment 5469 l 25ish

R: This comment was addressed by including the following phrase after the first sentence of sect 2.2 (page 5471).

The last two pairs of simulations are thus performed in a Lagrangian framework.

Comment p 5470 l 20ish

R: The domain size is of 2.5x 2.5 x 3 km (this is mentioned in table 1). A sensitivity test performed with a 5 x 5 x 3 km domain for one pair of pristine/polluted simulations, showed that the results are not significantly modified while the horizontal size of the domain is increased.

Comment p 5471 l 1

R: Yes, the model uses now this new scheme. This formulation will be changed in the text.

Comment p 5472 l 15

R : This remark will be included in the revised version of the manuscript.

Comment p 5472 l 21

R: The title of the section 2.3.1 will be changed to Polluted clouds in the revised version of the manuscript.

Comment p 5475 l 4

R: We will rephrase this in the revised version of the manuscript as

In summary, an aerosol indirect effect has been simulated, where increased CCN enhances CDNC and inhibits drizzle precipitation in the STBL. In these simulations, droplet and drizzle sedimentation appears to be important at two levels: at cloud top where it attenuates the entrainment of free tropospheric air, and below cloud base where it modulates the degree of decoupling of the STBL and thus moderates the amplitude of the diurnal cycle and hence affect the cloud albedo.

Comment Table 4

R: Indeed, we had omitted to mention that these are the correlation coefficients/bias for the entrainment rates. This will be changed in the revised version of the manuscript.We decided to show the correlation/bias only for the parameterizations performing the best as the relative errors for Konor et al. parameterization were very high (∼200-300%), while for Bretherton et al. parameterization the correlation/bias depended on the value chosen for the a2 coefficient (as mentioned above), so we didn't want to confuse the reader.

We choose to show the bias/correlation coefficients in order to give the reader a global view of how well the different parameterizations perform, and an easy way to compare them. Given the number of parameterization tested, the alternative, i.e. a figure superposing the time series of the parameterized against the simulated entrainment rates, might have been in our opinion harder to read, and the differences between the different parameterization harder to visually quantify.

Comment p 5480 l 23:

R: In order to address this comment, the title of the subsection 4.3.1 will be changed to: Fluxes at the boundary layer top and entrainment rate, and the phrase in question will be moved to the beginning of this section.

Comment p 5479 l 27 Comment p 5482 l 5

R: The bulk properties of the EML are not just simple averages of theta_l and qt of the mean LES column over the vertical (though it is true that they are very close to these values). They are computed through an iterative method, which requires that the EML and the mean LES column should have the same total water integrated content and the same integrated air mass. The differences between the entrainment rates described in this paragraph (4.2.3) and the ones discussed previously (in sec 4.1.3) can be resumed

as follows. In order to evaluate the performances of the different parameterizations, in sec 4.1.3 we computed the parameterized the entrainment rates by using the properties of the mean LES column (for e.g. jumps at cloud top, radiative divergence, buoyancy flux at surface, etc.). Thus we could evaluate the explicit entrainment rates against the implicit (derived from the simulations) ones. In the mentioned paragraph on page 5482, we discuss the case where we computed the parameterized entrainment rates by using the properties of the EML (jumps at cloud top, radiative divergence, fluxes at the surface), which as we mentioned in the previous paragraphs are different from those of the mean LES column. This allows to evaluate the additional error on the entrainment rates due to the error on the fluxes made when using the ML framework.

We agree though that to the comment that these changes are not significant. Therefore we will revise the paragraph beginning on page 5482 l5 as:

When the entrainment rate is computed using the surface fluxes, the radiative divergence at cloud top and the bulk properties of the EML instead of those of the mean LES column, the results obtained with Turton and Nicholls 1987 parameterization somewhat degrade, as expected, but the changes are not large.

Comment p 5482 l 27

R: Yes

Comment p 5482, section 4.3

R: We used here the standard energy and water budgets to transform the errors made when using the ML framework on the theta_l and qt fluxes at the surface and at the boundary layer top (errors estimated in sect 4.2), into errors on the heating/moistening rates of the boundary layer (dtheta_l/dt and dqt/dt). Then we used the ML theory to transform these errors on the heating/moistening rates (associated to the different fluxes) into errors on the prediction of the LWP tendency. Our reasoning was that the errors on the heating/moistening rates are easier to interpret once they are expressed in terms of errors on the tendency in LWP. The assumption made here is that these errors on the tendency in LWP (expressed in gm-2 s-1) are associated with a change in the cloud base level, while the cloud top remains unchanged. The total error is indeed computed as the sum of the different errors (we will mention that in the text). Concerning the precipitation, for the computations of the errors showed in fig. 7, we considered indeed that there is no precipitation in the EML. So the errors associated with the precipitation fluxes shown here are due actually to the lack of precipitation in the EMLs (we are also going to mention this in the manuscript). The H PP and H LE are the rightmost terms in right hand side of eq 2 b and 2d. (We will include these precisions in the revised version of the manuscript). Then, in a supplementary test mentioned in the 2nd paragraph on page 5483, we use an empirical low to include the precipitation at the surface in the EMLs and we recomputed the errors on the LWP tendency. This test is not shown, but we mention and explain its results in the respective paragraph.

Comment p 5484 l 7 Comment p 5485 l 20

R: We will take these comments into account in the revised version of the manuscript.

Comment p 5484 l 7 Comment p 5485 l 20

R: We will take these comments into account in the revised version of the manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 5465, 2009.

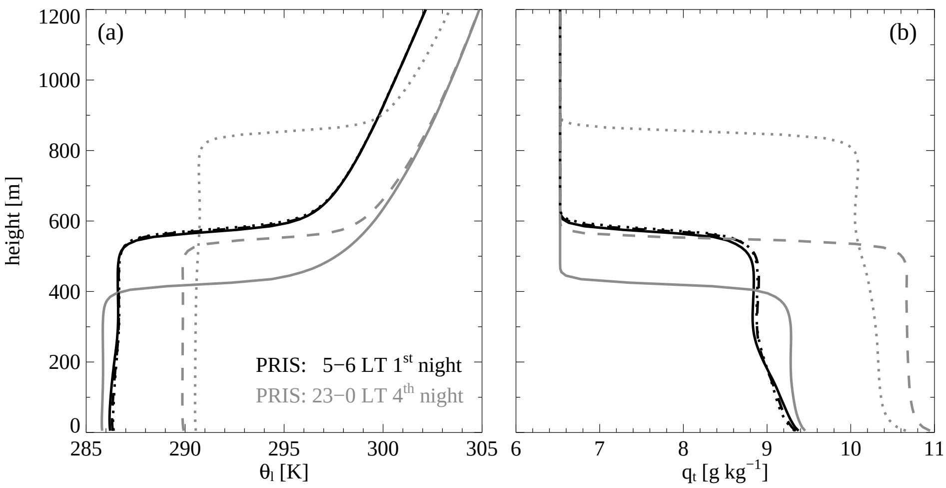

Fig. 1. As Fig. 2 from the manuscript but for pristine simulations.

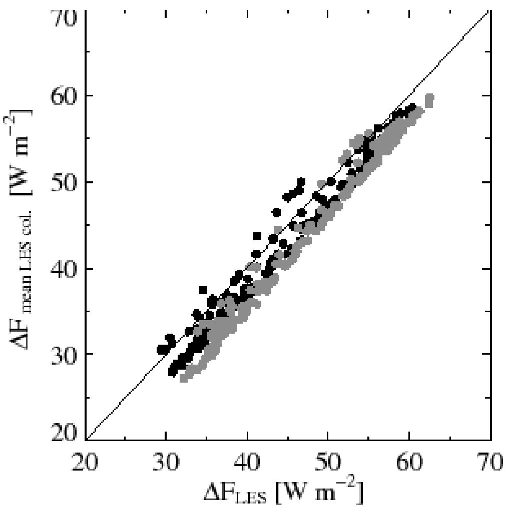

Fig. 2. Radiative divergence at cloud top computed for the mean LES column against the horizontally averaged radiative divergence at cloud top (W m−2). The black/grey dots correspond to PRIS/POL simulations.