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Interactive Comment

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.

Anonymous Referee #1

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Overall Quality/General Comments:

This study does a nice job of comparing the diurnal cycle of NE Pacific stratocumulus as represented by a large eddy simulation (LES) results against results that would be generated from a simpler mixed layer model (MLM). It is well written, and the graphics in particular are clear and illuminating.

The major insights I gleaned from this manuscript are:

1. Even during the relatively well-mixed FIRE period (or at least this LES simulation of

it), diurnal and (potentially) drizzle decoupling plays a strong role in BL dynamics. This reinforces the idea that aerosol-albedo response can't be investigated solely through nocturnal simulations (as done in most studies). Additionally, models which properly handle decoupling (ie which explicitly handle vertical structure) will be needed to properly model these responses.

2. The standard MLM - which doesn't handle decoupling - seems to do a good job when the BL is well-mixed, but isn't useful for this type of study because it can't handle decoupled periods.

3. Precipitation increases mixing and thus LWP during the day and decreases mixing and LWP at night. Because of this, it acts to damp the diurnal cycle of LWP. This point was already mentioned in Sandu et al 2008, but is interesting and should get more press.

4. The timing of BL decoupling differs for polluted and pristine cases. This was hinted at earlier in Caldwell et al (2005), but again deserves more attention.

5. Droplet sedimentation plays a crucial role in LWP response to aerosol changes and absolutely must be included in an credible study of indirect aerosol effects in stratocumulus.

I have some concern that the LES setup artifically alters the results, that the authors overstate problems related to MLM usage, and that the choice of MLM entrainment parameterization in section 5 was poor and that corresponding results are more a statement about this choice than a condemnation of MLMs. I also find the methodology of section 4.3 somewhat hard to follow. These objections are noted below.

Sci Questions/Specific Comments:

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p 5471 l 25: It seems strange to make free tropospheric subsidence change in response to the inversion height. I understand how this isn't important for qt since dqt/dz is constant in height above the BL, but won't this feedback on theta_l just above the BL? One could imagine that in this model high zi -> stronger w_s -> free-tropospheric air is pulled towards the BL quicker so it doesn't get the chance to radiatively cool as much -> theta_l just above the BL gets warmer -> entrainment doesn't penetrate the (stronger) inversion as well -> entrainment (and thus BL depth) decreases. In short, this relation could artificially stabilize simulated BL depths. The text states that free tropospheric drift is "only" 2-3 K over 72 hrs (p 5472 l 10). Unless I'm missing something, this is actually a huge (order 20%) effect. This is reflected by the fact that the free tropospheric theta_l looks radically different at the beginning and end of the simulations in Fig. 2a. I'd suggest a sensitivity study using another technique that doesn't tie higher BL depth to warmer free troposphere to make sure this isn't an issue.

p 5481 l 17: The effect of using horiz averaged fields for radiative transfer could easily be tested by running the radiative transfer code on the horizontally-averaged data (for some timestep) and comparing to the online result.

p 5484 Section 5.2: Because it ignores the underlying physics, I think this section misses the point. The main mechanisms by which increased aerosol loading affects STBL LWP are:

1.It decreases precipitation. This a. reduces the amount of water leaving the BL, increasing LWP (so long as precip reaches the surface). b. decreases cloud base stability (and thus increases mixing) by reducing condensational warming in cloud and evaporative cooling below cloud. This causes LWP to increase towards its adiabatic value. c. As noticed by Feingold et al 1996, in conditionally stable cases where precip evaporates high in the subcloud region, decreasing precipitation actually reduces instability and hence decreases mixing. This could cause LWP to decrease as moisture supply from the surface is decreased. 2.It decreases droplet sedimentation at cloud top. This results in higher LWC at cloud top and hence stronger cloud top radiative cooling and more evaporative enhancement of entrained plumes. These both act to increase entrainment, which reduces moisture transport from the surface and dries out

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the BL (reducing LWP).

Notice that the MLM chosen here only incorporates mechanisms 1a and 1b, so LWP will by design be higher in its polluted case. As noted in the introduction, however, mechanism 2 plays a critical role in the development of the LES simulation. Thus the contrasting results here are unsurprising and don't - as implied in this paper - imply a fundamental fault with the MLM methodology. This comparison would be much more meaningful if its MLM entrainment parameterization included droplet sedimentation.

There may at this point be other w_e parameterizations to choose from which include sedimentation, but the one which I'm currently aware of is Bretherton et al 2007. The authors mention that they tried this parameterization and found it to have large bias in some undisclosed quantity). Even though it is biased, it at least contains the right mechanisms for reproducing the LES results so I think it should be tested in this context. As an aside, I think there was a typo in the Bretherton paper with regard to the evaporative enhancement parameter. Perhaps using this wrong tuning is why it seemed to have such large bias?

General comment: I strongly disagree with the conclusion (expressed throughout) that the EML/MLM framework is fundamentally flawed. In particular, I disagree with the last sentence of the abstract because the MLM which gets the wrong sign is missing crucial physics. I also disagree with the last sentence of section 3 since Fig. 6 shows that the MLM does quite well within the model's domain of validity), and only fails when it should be expected to. The last paragraph of the conclusion is a bit overstated, but more reasonable.

In my opinion what this paper shows us about MLMs is that they can do a good job at reproducing STBL properties under well-mixed conditions, but that some of the most interesting cloud changes take place when the BL becomes decoupled. As a result, MLMs (while sound within their domain of validity) are of limited utility for predicting aerosol indirect effects. Further, parameterized droplet sedimentation is required to

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get LWP response to aerosol loading right.

General comment: I think the fact that simulations span the diurnal cycle has been underutilized. I liked the comment in the conclusions section saying that the polluted and pristine simulations decouple at different times of day. I think partitioning by time of day could yield interesting results.

Technical Corrections:

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p 5467 l 2-8: This explanation is confusing. I think you may want to say that the free troposphere is only marginally affected by the STBL *AND* that the STBL tends to be horizontally homogeneous. Thus, *AS A RESULT OF THESE 2 PROPERTIES*, BL evolution is largely dictated by energy *AND MOISTURE* fluxes through the surface and the inversion layer.

p 5469 l 25ish: It would be easier for the reader if it was mentioned here that the simulations are modified EUROCS/FIRE runs. That way readers familiar with those simulations won't need to spend as much time scrutinizing the methodology.

General comment: I would like to see some context for the simulations. If I remember right, the BL during FIRE is one of the shallower, better mixed that we've seen. It interests me that decoupling seems so important in the investigated simulations. Is this the case for all Sc regions? Is the LES maintaining the right level of mixing? If the answer to either of these is no, it should be made more clear that these results just hold for one simulation/region.

p 5469 l 25ish: I think mentioning that simulations are Lagrangian would make the methodology much clearer.

p 5470 l 20ish: what are the horizontal, vertical extents of the simulations? It seems likely that using an overly small domain would affect BL motions and could lead to spurious results...

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p 5471 l 1: If the model "disposes" of a PD 3rd order advection scheme, what is it using now? I think you mean it "uses" a PD... Also, "Since recently" is improper grammar.

p 5472 l 15: I prefer the term "nudging zone" to "sponge" here because I think of a sponge as damping motions, while the simulation pushes towards the initial condition.

p 5472 l 21: I'd prefer the section to be titled "polluted" since precipitation hasn't been explicitly turned off and these simulations are referred to elsewhere as "polluted" (eg in Fig 2).

p 5475 l 4: I think most people see the aerosol 2nd indirect effect as an effect *on albedo*. This paper focuses instead on LWP changes so staying with LWP terminology would be better. If you really want to say "2nd indirect effect" the LWP/albedo connection should be made explicitly.

p 5479 l 27: Don't theta_l and qt jumps use the EML value for the BL condition? If not, using the BL integrated radiative cooling is probably inappropriate.

Table 4: What is this the bias/correlation, etc of? entrainment? moisture flux at zi (suggested by p 5480 l 12)? cloudtop theta_l flux? Also, this table should include the Konor and Arakawa and Bretherton et al results... Further, it seems like a plot of the timeseries of correlation/bias would be more useful because nobody expects the MLM entrainment parameterizations to work when the BL is not well-mixed (as it appears to be frequently in this study).

p 5480 l 23: What is the point of this last paragraph? It seems out of place.

p 5482 l 5: Aren't the "bulk properties of the EML" = LES BL ave of qt and LES BL ave of theta_l? What are the differences here? Are you using EML LWP? Also, decreasing the correlation coefficient from 0.83 to 0.78 is really not a big effect. Is this paragraph necessary?

p 5482 l 27: do you mean "bigger" instead of "more important" here?

p 5482, section 4.3: I'm confused what's going on here. Aren't you using the standard energy and water budgets to compute dqt/dt and dtheta_l/dt for the MLM, then using these to compute the change in LWP? Converting dLWP/dt => dzi/dt and dzb/dt => dqt/dt and dtheta_l/dt is a subtle process and I think it would be best to at least cite a paper using the same methodology. In particular, are there any simplifications/assumptions being made? Is the total error computed as the sum of the individual errors or as the difference between the LES LWP and the EML LWP? Is the total error including the effect of horizontal averaging (which doesn't appear in the other terms)?

I'm particularly confused where it says that there is no precipitation parameterization in the MLM. Does this mean that the exact LES precipitation is being used so there is effectively no precip error in the "total" results? When the the Geoffroy parameterization is introduced, what is it used for? Does it affect the "total" error? Is it included in Fig. 7? Is it related to H PP and LH PP (which are never defined) in Fig 7? How?

p 5484 l 7: there is no section 3.2.

p 5485 l 20: no comma after "Not".

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

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