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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The sensitivity of stratocumulus-capped mixed layers to cloud droplet concentration: do LES and mixed-layer models agree?" *by* J. Uchida et al.

Anonymous Referee #1

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This study thoroughly discusses the limits of using Large Eddy Simulation and Mixed Layer Models for assessing aerosol effects on stratocumulus-capped layers. It also clearly states the conditions in which one can use a MLM for such studies. The main finding appears to be that the MLM and the LES predict similar changes in the cloud LWP in response to an increase in the droplet concentration, as long as the cloud top entrainment and the precipitation closures used in the MLM are tuned to match the LES behavior (and of course as long that the boundary layer remains well mixed). The paper is generally clear and well-written. I however find that its quality could be substantially improved by addressing some of the issues noted below. The first part highlights some of the major concerns I had when reading the paper, while the second presents some specific comments that could improve the quality of the manuscript.

General comments

1. As shown by the previous LES studies, cloud top entrainment and precipitation are two of the main factors (if not the two main factors) controlling the LWP response to an increase in the droplet concentration. Therefore, if the MLM closures are tuned to match the LES entrainment/precipitation rates and their dependency to cloud droplet concentration, it is not unexpected that the MLM response to a droplet concentration increase is similar to the one of the LES. I think that the authors should acknowledge this idea throughout the paper.

2. My other concern regards the way the authors present the differences between the current study and the one of Sandu et al. 2009 (S2009). I think the paper would benefit if instead of the “they’ve done this, we’ve done that” approach, the authors could rather highlight the different aims of the two papers. For me the two papers do not contradict, but they complete each other. More precisely, I see the current study as an elegant completion of S2009. That said, S2009 used a rather simple MLM as a proxy for emphasizing that a model which does not represent the vertical structure of the boundary layer and its modulation by precipitation cannot not reproduce the differences between the diurnal cycles of a precipitating and a non precipitating marine stratocumulus. For that purpose, from the bunch of existing entrainment parametrizations they used the one which matched the closest their LES entrainment rates, and a surface precipitation which scaled with the one of the LES. Nevertheless, they acknowledge the fact that the main reason for which their MLM gives a LWP change of opposite sign compared to the one of the LES is the fact that the entrainment parameterization does not correctly account for the dependency of entrainment on the strength of the condensed water sedimentation, and that it does not account for the redistribution of latent heat within the boundary layer by the precipitation. The current study goes one step further and shows that if one uses entrainment and precipitation parameterizations which account for this dependency in the same way the LES does, the two models agree

(as expected), at least in the sign of the LWP change. However, the authors should also acknowledge that S2009 focus on the differences between a precipitating/non-precipitating cloud, whereas the authors of this study focus on differences between rather non-precipitating clouds.

3. Finally, I find that the authors leave a too strong impression they take the observations, and more precisely the "observationally-based default" MLM parameterizations as "the truth". Yet, we know that both the entrainment rates and the rather weak precipitation typical of shallow convective clouds are rather difficult to measure. So, I think it would worth stating these observational difficulties, and re-writing the phrases which leave such an impression (for e.g.last paragraph of section 3, lines 17-18 page 25869, lines 15,20 page 25870, line 8 page 25872). Something that I do not understand related to what the authors call the "observationally-based default" parameterization for entrainment: isn't the parameter a_sed entering in this parameterization deduced only from LES performed with SAM? In this case, I wonder if one can call such a parameterization observationally-based.

Specific comments

Introduction

1.Line 7. "a more decoupled and more cumuliform character" does not make too much of a sense.

2. Line 16-15. Perhaps you can make this discussion less confusing for the readers by stating that there is no consensus concerning the stratocumulus LWP/cloud fraction response to an increase of the droplet/aerosol concentration, both an increase and a decrease having been observed/simulated. And that the key mechanisms for this response seem to be precipitation and entrainment. Then you can come to your examples. Concerning drizzle effects, I think you might also want to mention that in some cases (i.e. when it is evaporating right under the cloud base), drizzle can also have the opposite effect, that is to lead to a higher LWP (Lu and Seinfeld, 2005, Sandu et al.

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2008).

3. line 2 page 25856 Do we have enough observational evidence to say that “many boundary layers are fairly well-mixed”? Wouldn’t “sometimes” be a more appropriate word here? Besides, the term “fairly” is rather vague, especially since the MLM is applicable only in perfectly well-mixed cases.

4. Last paragraph (page 25856). Besides the remarks made in the 2nd of my general comments, I also find somewhat unusual that the results are announced in the introduction. I think it would be valuable if you could better explain to the reader your reasoning for not considering the diurnal cycle, despite the fact that the coupling between the variation of the solar forcing and the mechanisms controlling the cloud response to an increase in the aerosol loading has been shown to matter (Lu and Seinfeld 2005, Sandu et al. 2008. And I guess that your main reasoning is that you want to focus on well-mixed cases, and the boundary layer is often decoupled during daytime.

Section 2.

5. Section 2.1 You could also mention the advection scheme that you are using, and the degree to which it can be considered as a diffusive scheme. And perhaps tell the reader that details about the radiation scheme will be given further.

6. last paragraph on page 25859. As far as I understand, by imposing this minimum rate of cooling, you are taking into account the radiative cooling due to water vapor in cloud free-columns only. Shouldn’t this radiative cooling due to water vapor also be accounted for in cloudy columns? Or it is considered that its effect is negligible compared to the cloud top radiative cooling?

7. Line 1-2 page 25860. I was wondering what is the benefit of such an approach.

Section 3.

8. Lines 6-8 page 25861. I don’t think that mentioning the fact that previous simulations were of the order of a few hours brings any essential information here. Besides,

simulations of the order of a few days have already been done for e.g. by Johnson (2005), S2009. So you might want to limit yourself to saying that you are doing 5 days simulations because you want the system to reach equilibrium.

9. Lines 20-22 page 25861. It might worth mentioning that this is mainly due to entrainment, foreshadowing thus the ulterior discussion. And also, why are you saying that “will be more radiatively susceptible”?

10. Lines 10-13 page 25862. The link words (“accordingly”, “therefore”) make the respective phrases difficult to understand. Instead of “accordingly”, you might want to state that “Because it has lower LWP/CF, the simulation N=150. . ... By the word “therefore” you make reference to the fact that as the radiative divergence decreases, the entrainment rate decreases and hence the inversion level is not growing as much as in the case N=30? Why not stating it explicitly? This phrase could also gain in clarity by replacing the word “crossover” with the idea behind, i.e. that the LWP of N150 remains lower than the one of N30 for the rest of the simulations.

11. Lines 14-18 page 25862. If your intent here is to give the reader an overview of the mechanisms we consider responsible for the impacts of condensed water sedimentation on entrainment, why don’t you mention as well the “asymmetric vertical currents theory” developed in Stevens et al. 1998?

12. Line 19 page 25862. I don’t see the role of the word “specifically”

13. Last paragraph on page 25862. The discussion about the entrainment efficiency concerns a certain time period? And the given values represent the average over that period ? If so, over which period of the simulations? As some readers might have a hard time reading between lines, it might be also useful to better explain the idea behind :”the reduction in vertical velocity is accomplished by decreased LWP”, i.e. that the decreased LWP implies reduced radiative cooling, hence reduced production of turbulence, and therefore reduced vertical velocity.

14. Lines 2-3 page 25863. The phrase “In particular, . . .” could also be better phrased to highlight the idea behind, i.e. that the enhanced entrainment efficiency implies a more efficient drying and warming of the cloud via mixing with free-tropospheric air, hence a more efficient evaporation of cloud droplets, and therefore leads to a decrease in LWP.

15. Lines 13-14 page 25863. Isn’t that also due to the decoupling of the cloud from the surface layer associated with precipitation evaporation in the subcloud layer?

Section 3.2.

16. Lines 6-7 page 25864. Has the a2=25 been obtained by fitting the RF01 data? Otherwise I would say that it is not very rigorous to affirm that the agreement of MLM with observations is better because its closure has been tuned to match the observations.

17. Lines 9-10 page 25864. If you computed the MLM efficiency and compared it to the LES, but decided not to show it, this should be mentioned.

18. Second paragraph on page 25864. As the BL is by definition well-mixed in a MLM, I it find confusing to talk about the stabilization of the boundary layer in the MLM. Also, it is not easy to read between lines what to do you mean by “the drizzle feedback is even stronger and leads to a runaway decrease of entrainment and increase of drizzle and LWP”.

Section 3.3

19. Line 16 page 25865: How do you chose/find these values? Do they represent averages over the entire simulation?

20. Last paragraph page 25865: It is not obvious for me why you have plotted the r.h.s of the eq.8 as a function of c1, so the phrase “As anticipated. . ..” is not easy to understand. Why not plot r.h.s as a function of exp(-asedwsed/w*) and determine a2 as the slope of the best linear fit. It is also not clear how do you find the value a2=110? Last phrase of this paragraph: Is that surprising, given that Bretherton 2007 used the

same LES and also performed simulations for different droplet concentrations?

Section 3.5

21. Last paragraph on page 25867: Could you better explain what you mean by "We interpret this as due to cloud-fraction interaction"?

Section 4

22. Line 6 page 25869: are these average values?

23. Line 13 page 25869: Does this threshold depend on domain size?

24. Lines 21-22 page 25869. S2009 found this result only if the droplet concentration increases enough to suppress drizzle. They don't discuss differences between hardly precipitating cases.

25. Lines 20-22 page 25871: The message of the phrase :"Interestingly, ..." is not clearly conveyed.

Technical corrections

1. Why not name your cases N150, N50 etc, instead of N=150. This expression gives me all along the impression that the unit is forgotten.

2. There are quite a few places in the manuscript where it appears "an MLM" or "an LES" instead of "a MLM", "a LES"

3. 10. line 5 page 25862 I wonder if "trends" is the right word here. Wouldn't "differences" be more appropriate?

4. line 11 page 25863 "cuts" should be replaced by "diminishes"

5. line 19 page 25863 : It would be clearer if "N's" was replaced by "these simulations"

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