Atmos. Chem. Phys. Discuss., 9, C10310–C10313, 2010
www.atmos-chem-phys-discuss.net/9/C10310/2010/

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 #2

Received and published: 30 January 2010

Overview

This manuscript describes a study of the response of stratocumulus liquid water path (LWP) to aerosol, comparing a mixed-layer model (MLM) to large-eddy simulations (LES) for a single meteorological sounding, in which nocturnal simulations are run for several days. The results of the LES, which produces at best an order of magnitude less than the expected (based on observations) amount of drizzle at cloud base, are used to tune the entrainment parameterization of the MLM, which allows the MLM to qualitatively reproduce the response of the LES, in which LWP is found to decrease as droplet concentrations increases.

Specific comments

1. This manuscript extensively discusses drizzle and its effects on boundary-layer dynamics. It also compares its findings with other stratocumulus studies using MLMs (Wood 2007, Sandu et al. 2009), and is ultimately inconclusive regarding the different sign of the LWP response between this study and that of Sandu et al. 2009. Seems to me that this study would be stronger if it also considered another sounding that was more prone to drizzling, since, as noted here, there is very little drizzle in these simulations. For instance, in considering the difference in MLM response to increasing droplet concentrations, it might be relevant that the sounding used by Sandu et al. 2009 had more than three times the specific humidity above the boundary layer than that used here. Furthermore, it would be interesting to know if this MLM can qualitatively reproduce the LES result in a drizzling regime, with surface precipitation from the LES of at least a few tenths of a millimeter per day. One might get the impression from Fig. 6 that this LES is unable to produce such precipation rates, though such rates are commonly attained in nature. If it cannot, one is left to wonder what the value of this LES is for such a study. If the production of drizzle in the LES is suspect (and the authors certainly suggest that it is), it might make more sense to focus more explicitly on non-drizzling stratocumulus: just turn off the warm rain parameterization in both models, and purposely limit the study to that regime (I'd change the title, if such a path is followed).

2. It should be mentioned prominently that the effect on entrainment rates induced by changes in droplet evaporation time scales (see Hill et al. JAS 2009) is omitted here, which presumably implies that the response of entrainment to changes in droplet concentrations are underestimated in the LES results here. I'd think such an admission would be appropriate at a number of places in the manuscript. Failing that, I'd explain why such an effect is implausible or irrelevant here.

3. Symbols are used but not defined in the text. For instance, rho_a and rho_w on p. 25858, q+ and h+ on p. 25860, z_i and w_e on p. 25862. The last two are presumably

inversion height and entrainment rate, but how are they defined here?

4. Eq. 5 does not make sense. The units don't work. Also, it seems like cloud thickness must be part of the expression. Come to think of it, I don't see why eq. 5 is even included.

5. In the figures, how are cloud top, cloud base, cloud fraction, and convective velocity defined? A vertical buoyancy integral is mentioned for the latter on p. 25862, but the description is needlessly vague.

6. What is the reasoning that underlies the statement on p. 25861 that non-precipitating, thin clouds are more radiatively susceptible?

7. The simulations are run for five days, allegedly to approach an equilibrium response, but the figures give no indication of approaching equilibrium, since in nearly all cases the rate of increase of LWP is constant over the last four days. I'd expect the absence of equilibrium to merit some discussion, in light of the stated design goals.

8. It is difficult to reconcile the statement that as LWP drops below 25 g/mˆ2 that cloud fraction increases with the results shown. For instance, the N=50 case has LWP below that value for the entire second day, during which cloud fraction continually increases.

9. What is the basis for choosing the period 2-2.25 days for the plotted profile?

10. Why doesn't the LES reproduce the observed LWP at time zero? Is there some inconsistency between the initial conditions and the observations they are based on?

11. I'd spell out why the MLM is "clearly inapplicable" when the entrainment rate vanishes.

12. In the 3rd sentence of section 3.3, I'd insert "of the same case" after "prior simulations" and replace "our" with "the same" to make the historical context clearer.

13. The match between the tuned efficiency and the LES statistics looks pretty bad for N=10 and N=150. This tuning should be done in a way that all four of the LES results

are actually matched. Otherwise, it should be noted explicitly that the tuning process itself only really works for half of the LES results.

14. It would be more accurate to replace "but a different set of cases" on p. 25866 with "with the same case but using a different method to tune a_sed".

15. I'd explicitly note in the text that cloud-base drizzle in the LES never gets within an order of magnitude of the rate expected from observations.

16. It is stated in section 3.5 that Wyant et al. 2007 showed that LES microphysics can be biased compared to observations. But that study was an intercomparison of single-column models. I'd cite LES-focused studies instead, ideally including one that uses the LES employed here.

17. After asking whether a tuned MLM can \textit{quantitatively} (italics in original) reproduce the LES sensitivity, it should be answered explicitly in the same section that the answer is negative, since it's difficult to appreciate a roughly factor of two difference as a quantitative reproduction. Qualitatively, yes, of course, as the sign of the response is consistent here, unlike in the Sandu et al. 2009 study of (drizzling) stratocumulus.

18. In the sentence starting "That is, we should be wary" on p. 25869 I'd append "particularly so for LES models that do not reproduce expected drizzle rates to within an order of magnitude."

19. It is stated in the next to last paragraph of section 4 that evaporation of drizzle below cloud base decreases turbulence and entrainment in this case, but there is no mention of such a mechanism in the model description here or in Caldwell and Bretherton 2009. Further explication is needed.

20. In the second paragraph of section 5, I'd replace "to match LES simulations (sic)" with "to qualitatively reproduce the LES sensitivity of LWP to droplet concentrations".

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 25853, 2009.