

Interactive comment on “A comprehensive numerical study of aerosol-cloud-precipitation interactions in marine stratocumulus” by Y.-C. Chen et al.

Anonymous Referee #2

Received and published: 15 July 2011

The authors present a series of LES of the diurnal cycle of marine stratocumulus in which they vary both the background aerosol concentrations and aspects of the larger scale environment in which the clouds exist. The summary of previous work in this area is good and useful and most of their sensitivity tests are valuable. The conclusions are essentially to support the earlier findings, with the most important addition being that the situation in the daytime is yet more complicated due to dynamical decoupling!

In general the paper is clear and well written and most of my more detailed comments below are minor. I do, however, find the authors show repeated and fundamental misunderstanding of the relative importance of thermodynamics and microphysics for the

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evolution of stratocumulus clouds, as I explain in detail below. As a result I find that many of their arguments about what is responsible for the effects they see in their simulations need to be rewritten. However, I don't believe these will fundamentally alter their conclusions, hence I consider these revisions to be minor.

1. abstract, line 20, "the cloud thickness effect is positive for moderate/heavy drizzling clouds": only one single simulation showed a positive cloud thickness effect so how do you justify extrapolating to this conclusion? At least correct this statement to say "...for a moderate/heavy drizzling cloud".
2. abstract, line 23: I don't personally feel the concept of "cloud susceptibility" is well enough known to refer to it here without explaining what it actually is.
3. p15502: in this discussion of the possible relative impacts on cloud top and base heights, and thence on whether the cloud thins or thickens, the study of Randall (1984) is highly relevant.
4. p15503, line 15: "In order to obtain a comprehensive view of these [aerosol-cloud-precipitation] interactions, high resolution LES simulations are carried out". Strictly you could just use a Lagrangian parcel model, for example, to study aerosol-cloud-precipitation interactions! LES allows the interactions with the turbulent dynamics to be studied.
5. p15504, line 14: I assume you mean the "cloud droplet profile tends to be sub-adiabatic", otherwise I don't know what you mean?
6. p15504, Eq (2) and subsequent text, and section 2.4: this factor of $(1 - f)^{(2+m)/3}$ is introduced to represent the effects of a subadiabatic profile, which is fine, but given section 2.4 dismisses these dependencies ("this term cannot be evaluated separately ...the effect of diabaticity is intertwined with all the previous effects") there appears no need or justification for its complexity. The second part of the

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statement in line 19 also makes no sense to me. Cloud-free is not a natural opposite state to adiabatic! In what way, physically, are you thinking of approaching cloud free conditions? As stratocumulus breaks up the in-cloud profile could still be reasonably close to adiabatic and yet the cloud cover decreases. Mathematically you can think of the liquid water gradient reducing from adiabatic to zero, at which point you have approached cloud free, assuming zero cloud water at cloud-base, but that is not a physical limit. Why not simplify this whole section by introducing only a factor g , say, to (2) that equals 1 for an adiabatic layer and reduces as the degree to which the profile is sub-adiabatic increases?

7. p15505, Eq (4): this equation is clearly making significant assumptions about the dynamics in the cloud layer as it only requires knowledge of the updraught velocity at cloud base. Given the point of this paper is to include dynamical interactions, through use of the LES, these assumptions should be discussed.
8. p15506, line 14, “the dispersion forcing”:the dispersion in the droplet distribution is responding to the N_a forcing so why do you refer to the dispersion as being the forcing agent?
9. p15506, line 24, “this trend is evident in in-situ measurements”: I don’t see how you can distinguish the dispersion effect from the Twomey effect in observations where both must occur simultaneously, without running an off-line radiation code where each aspect is altered independently. In which case the effect is evident in the radiative transfer calculations, not the observations.
10. p15508, line 11: for the present study it would be good if the WRF LES showed good agreement with observations for relevant dynamical fields but I note that Wang et al (2009) showed the WRF model had w'^2 smaller than the entire range of LES in Ackerman et al (2009) where those LES already underestimated this compared to the observations. Similarly, comparing the WRF turbulence profiles in Fig.3 with the LES and observations in Fig.9 of Duynkerke et al, WRF

is clearly very poor in the daytime with both w'^2 and the buoyancy flux greatly underestimated. Even at night, what has happened to the buoyancy fluxes in the subcloud layer in WRF? The observations show roughly constant values of 1.5 to $2.0 \times 10^{-4} \text{ m}^2\text{s}^{-3}$ between cloud-base and the surface while in WRF it is 1 or less at cloud base and tends almost linearly to zero at the surface? A lack of turbulence in stratocumulus clouds would seem to me to be a serious weakness of an LES for studying cloud-turbulence interactions. This weakness should at least be discussed here.

11. p15508, line 20: what are the implications of this cutoff radius between cloud and rain drops?
12. p15509, line 13: it doesn't seem particularly realistic to hold the aerosol number concentration constant as it must surely evolve in reality with wash-out and cloud processing etc. What might be the implications of this assumption?
13. p15510, line 20: was any drizzle observed for this case? Drizzle was not represented in the Duynkerke et al study so it seems odd to have to reduce the total water mixing ratio in order to generate moderate drizzle? If the LES doesn't reproduce the observed precipitation with the observed mixing ratio it rather questions the validity of the LES for this microphysical study.
14. p15510, line 27: why was the divergence rate reduced from the Duynkerke et al study in the control?
15. p15512, line 23: what does "a cloud top predominantly defined by LW radiative cooling" mean?
16. p15512, line 25, "In the clean cloud, sedimentation causes the cloud base to lower as precipitation nears the surface": cloud base in stratocumulus is typically where the relative humidity reaches saturation and so the cloud base falls as the

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RH of the air below increases towards saturation. In the clean cloud case, cloud-top falls compared to the other cases indicating, presumably, reduced cloud-top entrainment. This reduced entrainment of warm dry air would usually increase the RH of the PBL and so lead to cloud base falling, as is observed. This has nothing to do with droplet sedimentation, though, which I suspect is a minor perturbation on what is really the result of changes in the PBL heat and moisture budgets. Similarly, as the next sentence goes on to claim, the cloud does not dissipate because the larger droplets fall out! What about the smaller droplets? Again, I suspect the cloud dissipates because of the thermodynamics of the environment to which it is intimately coupled.

17. p15513, line 18: as with the preceding point, microphysical arguments are being used to explain what appear to be simple thermodynamic budget responses typical of the stratocumulus diurnal cycle. The cloud top falls due to reduced entrainment (in turn induced by SW heating stabilizing the cloud layer and so reducing TKE), cloud base rises simply because of the dominance of SW heating in the thermal budget reducing RH more than the reduced entrainment leads to increased RH.
18. p15515, line 5: if the rise in cloud top were to be due to increased SSTs warming the PBL and so reducing the inversion strength, then that should result in a gradual acceleration of cloud-top rise - initially the inversion is the same strength as the control and so should initially show the same entrainment rate. However, Fig.5b shows cloud top diverging from the outset. This suggests to me it is rather the increased surface fluxes themselves driving stronger TKE and thence stronger entrainment.
19. p15515, line 20, “smaller cloud droplets evaporate more efficiently”: more confusion of thermodynamics with microphysics I suspect, see my points above. What are the differences in droplet sizes being referred to and what does that imply in

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- terms of difference in timescale for evaporation? Typically the difference in evaporation timescales would be tiny compared to the hours over which the cloud is dissipating, so how can they possibly be relevant as you suggest?
20. p15516, line 17: QFT1 is not the only clean case with cloud at the end of the simulation - what about SST292 in Fig.5?
 21. p15517, line 11, “entrainment is weaker in this case”: but cloud base and surface moisture fluxes are unchanged. This indicates that the PBL T and moisture profiles must be very similar and so the heat and moisture budgets must also be very similar. Hence how can the entrainment rate have changed as this is a significant term in those budgets?
 22. p15517, line 12, “during the second night the cloud grows even thicker with $LWP > 200 \text{ gm}^{-2}$ ”: not in Fig.7e it doesn't! The maximum is at most 160 gm^{-2} .
 23. p15517, line 15, “the cloud becomes thinner due to stronger “capping” from the air above”: what do you mean by “capping”? If you mean because the stronger subsidence has resulted in a lower inversion height and thence cloud-top, then just say that?
 24. p15517, final paragraph: this summary is simply confused! When D is decreased the cloud thickens and LWP increases in the short term (ie first 5 hours), as shown in Fig 7a, not the other way round.
 25. section 5.2.4: were there any changes in cloud droplet number concentration as wind speed might have some impact on aerosol activation?
 26. section 5.3: why is only the difference between N_a of 100 and 1000 shown? The effect of changing aerosol has already been shown to be non-linear (eg. for the control, Fig 4a shows the LWP generally increases between 100 to 200 and decreases from 200 to 1000) and so this figure is rather misleading.

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27. p15519, line 3, “overall LWP is found to be more sensitive to precipitation than entrainment”: what is the justification for this? Doesn't the greater number of cases with $\Delta LWP < 0$ imply that entrainment related effects (that would be expected to reduce LWP, ie c,d,e in the introduction) are dominating over the precipitation effects (that would be expected to increase it)?
28. p15523, line 11, “during daytime the ranges of values are more scattered due to the MBL decoupling”: given that the key issue is what happens to the cloud SW albedo under aerosol changes, this suggests that the role of decoupling is a leading order mechanism that needs to be investigated more thoroughly!
29. p15523, line 25: as with previous comments, why only consider changes in aerosol from 100 to 200 cm^{-3} when changes beyond that can have the opposite effect?
30. p15523, line 26: again, the indirect effects are concerned with the cloud albedo and so why look only at nighttime data (hours 4-7)?

References:

Randall, D.A. (1984) Stratocumulus cloud deepening through entrainment. *Tellus*, 36A, 446-457

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 15497, 2011.

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