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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Factors determining the effect of aerosols on cloud mass and the dependence of these factors on liquid-water path" *by* S. S. Lee and J. E. Penner

Anonymous Referee #2

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The submitted manuscript examines aerosol-cloud interactions in marine stratocumulus. Specifically, the work attempts to explain an apparent dichotomy between clouds of low and high liquid water path (LWP). Using the Goddard Cumulus Ensemble (GCE), the authors perform eight simulations generating mean LWPs ranging from 73.3 g m^{-2} down to 36.2 g m^{-2} by simply multiplying or dividing the latent heat flux from the surface by an arbitrary factor. It is noted that the clouds are considered to be "thin" when the LWP is less than about 50 g m^{-2}. A budget analysis of the production and loss terms (i.e., condensation, evaporation, autoconversion, accretion, and sedimentation) is performed to analyze the microphysical nature of each cloud. This analysis shows that condensation/evaporation is at least 1-2 orders of magnitude greater than the re-

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maining processes for all cases, thus implying that the LWP is more-or-less controlled by the condensation/evaporation rate and not the autoconversion, accretion, or sedimentation rates.

The LWP is shown to increase in all simulations when run with present day (PD) aerosol concentrations in comparison to the runs performed with pre-industrial (PI) aerosols, except for when the latent heat flux is divided by 5 (LH-D5). The focus of the paper shifts towards explaining this occurrence. The authors show that the evaporation rate is higher under PI conditions (i.e., low aerosol concentration), leading to more cooling via latent heat released immediately below cloud base. This cooling creates a more unstable environment in comparison to the PD simulation, hence providing more available water vapor for condensation within the cloud. With that said, the paper does not address why the LWP decreases from PI to PD when the latent heat flux is divided by 5, but increases when the latent heat flux is divided by 10 (and thus resulting in an even lower LWP).

Major Comments:

A) Significance of Scientific Contribution

The manuscript lacks significant scientific advancement from previous works. In fact, much of the discussion found in the manuscript can be traced back to the authors' previous work in the paper entitled Aerosol Effects on Liquid-Water Path of Thin Stratocumulus Clouds (Lee et al., 2009). The previous study used the GCE to study aerosol effects on thin stratocumulus clouds (i.e., clouds with LWP smaller than about 50 g m^{-2}) by initializing the simulations with different temperature and specific humidity profiles. The chosen profiles produce mean relative humidities at the top of the planetary boundary layer (PBL) ranging from 40% to 80% (dry to wet, respectively). Furthermore the LWP ranges from about 60 g m^{-2} for the high aerosol concentration and the wet case down to about 13 g m^{-2} for the low aerosol concentration and mid-wet run (Fig. 4, Lee et al., 2009). Moving to the study at hand, we find that the range of LWPs produced is in fact smaller than that of the previous work (i.e., 73.3 g m^{-2} down to 36.2 g m^{-2}). Moreover, except for the high aerosol, increased latent heat flux case in the present study, the LWPs shown all fall within the range of those presented in the previous study.

The manuscript also attempts to explain the effect of instability on LWP using the case in which the latent heat flux is divided by 5 (LH-D5). Table 2 in the present manuscript shows that this is the only case in which the LWP decreases with increased aerosol concentrations (from 40.9 g m^{-2} to 39.9 g m^{-2}). The explanation (as described above) for this discrepancy is that the evaporation of rain is higher for the low aerosol (PI) case in comparison with the high aerosol case (PD). The latent heat released as result of the evaporation is higher in the PI case. Hence, the sub-cloud layer is more unstable and the updrafts are invigorated (Fig. 12). However, if we turn our attention back to Fig. 4 of Lee et al. (2009) we find that the LWP in the dry case is more or less the same for the low and high aerosol runs. The text claims that the time- and domain-averaged LWPs are 29.70 and 30.21 g m^{-2} for the high and low aerosol runs, respectively. Again, we have a (slightly) higher LWP for the low aerosol scenario. This discrepancy is explained well by Fig. 11 in Lee et al. (2009), which is qualitatively the same as Fig. 12 in the present manuscript. The magnitudes of the evaporation, heating, and conversion rates may differ slightly between Fig. 11 of Lee et al. (2009) and Fig. 12 of the current work, but they are qualitatively identical and explain the exact same phenomenon, previously discussed in Feingold et al. (1996). The important factor here is that precipitation does not reach the ground for the corresponding cases in both the present manuscript and Lee et al. (2009).

B) Language and Understandability

First and foremost, entire paragraphs have been taken verbatim from Lee et al. (2009). For example, paragraph 17 of Lee et al. (2009,) is duplicated in section 3 of the current

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manuscript, paragraph 38 and the first half of paragraph 39 of Lee et al. (2009) is replicated at the beginning of section 5.3, and paragraph 41 of Lee et al. (2009) is used in section 5.3 (pg. 19326), just to name a few. If a difference exists between the two works, it merely lies in the naming convention, figure number, and/or LWP value; there is absolutely no difference in the verbiage used in the explanations. This is entirely unacceptable.

I found the manuscript very difficult to understand. The use of adjectives like increase and decrease are used in excess. Many sentences attempt to explain too much information, e.g., the first sentence of Sect. 4.

The use of “in other words” is not necessary. The clarifying statements that follow “in other words” are no clearer than the preceding statement. Moreover the article “the” is also used excessively and in places where it is not necessary, e.g., the two sentences beginning on line 6 of pg. 19320.

Eq. 1 represents the production equation for liquid integrated over the domain and the duration of the simulations. The units of $< A >$ are given as mm in Table 2. However from Eq. 1 itself, I do not see how one arrives at units of length only. Using units of kg kg^{-1} s^{-1} (or simply mass over mass times time) for Q_j where j represents one of the microphysical processes in the GCE model, $< Q_j >$ has units of mass over length squared. Whereas in the present manuscript, the values of $< Q_j >$ are given in mm (i.e., some unit of length). It appears that a density factor is missing from Eq. 1. Along the same lines, Eq. 3 and Eq. 4 are given without explaining what each variable means. Specifically, the variables D_n, u, and G(u) are not defined. Furthermore, it is not mentioned that $f_{gam}(D)$ represents a gamma distribution.

C) Basis for Analysis Using Eq. 1

Eq. 1 is used to produce the quantities reported in Table 2. Ignoring the units discrepancy for the time being (discussed above), and instead focusing on comparing the magnitude of the budget terms themselves, I find the discussion comparing the condensation rate to that of the conversion (autoconversion plus accretion) troubling. The reason for this is that the manuscript states numerous times that condensation outweighs the effects of autoconversion and accretion (by 1 to 2 orders of magnitude) in all cases. However, this may be comparing apples and oranges. Condensation is a change of phase while autoconversion and accretion are simply the conversion of liquid from one category to another. I think it would make more sense to compare net condensation (condensation minus evaporation) to that of autoconversion and accretion. Averaged over the domain, using Table 2 for guidance, one finds that the net condensation is 0.44 (LH-M5, PD), 0.63 (LH-M5, PD), 0.04 (CONTROL, PD), 0.08 (CONTROL, PI), 0.01 (LH-D5, PD), 0.01 (LH-D5, PI), 0.0 (LH-D10, PD) and 0.0 (LH-D10, PI). Then, given the values of autoconversion plus accretion, i.e., 0.38, 0.57, 0.04, 0.08, 0.006, 0.011, 0.003, and 0.004, respectively, it is clear that the net condensation is of the same order of magnitude as that of the conversion rate. This should be addressed in the manuscript.

Minor Comments

A) Why Mexico? Why July 14 and 15, 2002? Why 14:00 LST on the 14th to 14:00 LST on the 15th?

B) The aerosol concentrations given in the text (Sect. 3) do not correspond to those given in Fig. 4.

C) Why do you not multiple the latent heat flux by 10 also?

D) Section 5.1.1 mentions that the cloud fraction is larger than 0.8 except for the first and last 30 minutes of cloud evolution. Is there a figure to support this? The previous work (i.e., Lee et al., 2009) provides a figure showing the cloud fraction as a function of time.

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E) It is mentioned that the difference between MODIS retrieved LWP and model output LWP is less than 10%. What about retrieval errors? Furthermore, what is meant by “good” agreement in line 18 on pg. 19321.

F) The conversion efficiencies reported in the text on line 23 of pg. 19324 do not correspond to those reported in Table 2. The table leads the reader to believe that the efficiencies should be about 0.68% and 0.91%, not ≈1% and ≈3%.

G) Line 1 on pg. 19327 is unclear. The slope of the cumulative condensation has units of length per time while the condensation rate, according to Table 2 has units of simply length. If one uses the condensation rate before applying Eq. 1, the units are simply per time, again not matching those derived from the slope of Fig. 10.

H) The phrase “reduced increase” is used in lines 28 and 29 on pg. 19328. The sentence should be reworded so that the authors’ intention is clear.

Summary

A) There is very little in the present manuscript that is not already described in the authors’ earlier work (Lee et al., 2009).

B) A large fraction of the present manuscript has been taken verbatim from the authors’ earlier work (i.e., Lee et al., 2009). Moreover, the original portion of the text is filled with poorly explained material and bad grammar.

C) Recently, I noticed an additional paper (i.e., Lee and Penner, 2009) that follows in the footsteps of the current manuscript by again using text verbatim from the Lee et al. (2009).

D) I do not recommend that his paper be published in ACP.

References:

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