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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of solar radiation on aerosol-cloud interactions in thin stratocumulus clouds" *by* S. S. Lee and J. E. Penner

Anonymous Referee #2

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This study examines the effect of changes in aerosol concentration and solar radiation on thin, marine Stratocumulus with LWP of ∼50 g m-2 or less, i.e. non-precipitating marine Sc. The authors employ a liquid water budget analysis to demonstrate that aerosol effects on condensation in thin, non-precipitating marine Sc are more important than aerosol effects on precipitating processes in determining the overall cloud response. The overall results compare well with other studies on a similar subject, e.g Wang et. al, 2003; Bretherton et al, 2007; Hill et al, 2008, 2009. While the budget analysis demonstrates that condensation is more important than accretion or autoconversion, this paper fails to present the underlying mechanisms for the change in LWP with changes in aerosol. Previous work by a number of researchers (see below) has

proposed mechanisms by which aerosol induced changes in condensation/evaporation influence cloud dynamics of non-precipitating clouds, which in turn leads to changes in LWP; however, none of this work is discussed or tested. Thus, while the results are comparable to previous work, such results have not been put into context, so it is hard to understand how original the conclusions are. Furthermore, I question the author's rejection of the role of sedimentation in the LWP response to aerosol. This rejection seems to stem from overlooking some the aerosol-cloud-dynamic interactions discussed in previous work, interactions that would not be born out of their budget analysis.

To some extent the authors do demonstrate the importance of solar radiation; however, once again the authors fail to reference or compare their results with previous work on the same subject. Finally, other than the inclusion of solar radiation lot of this work does not seem to be particularly new as much of what is stated in this paper has been stated by the same authors in Lee et al (2009), hence the originality of this paper is questionable. For these reasons, I feel this paper should be rejected in its present form. Having said this, I feel that the topic is important and will be of interest to the community. From the description of the simulations, many of the issues that have not been tackled in this draft can be addressed with the present simulations and some extra tests. Hence, I feel the authors think about addressing the topics below and re-submitting.

Major Comments

1) This paper discusses thin marine Sc yet throughout the authors barely discuss cloud top entrainment rate, cloud top longwave cooling, cloud top evaporative cooling, which have been shown to be important processes in the evolution and diurnal variation of marine Sc. Figure 3 shows that the polluted cloud top height tends to rise in altitude earlier than the clean cloud. This indicates that there is a more efficient entrainment rate, but this is not discussed. I except that the paper is about the influence of solar radiation on aerosol-cloud interactions, with a particularly focus on an

aerosol-condensation effect; however, the authors have to show how these processes inter-relate with other processes that have been demonstrated to be important in marine Sc by numerous observational, theoretical and modelling studies, including GCSS intercomparisons. By ignoring such processes there is no way to tell how important the proposed mechanisms are relative to some of the driving forces of marine Sc.

2) I have serious doubts about section 5.2 and 5.3 for 2 reasons

- I feel this is a lifted from the Lee et al, 2009 but not referenced, so such discussion is not original

- I think there are underlying flaws in the author's conceptual model.

In Figure 4 the Control run clearly shows that the LWP is much less in the high aerosol case yet this change is not really explained. Hill et al, 2008, showed a similar response in a diurnally varying non-precipitating marine Sc. They argued this response was due to an evaporation-entrainment effect. Bretherton et al (2007) and Sandu et al (2008) showed a similar decrease in LWP and argued this resulted from a sedimentation-entrainment effect. Hill et al (2009) showed that both these processes play a role in reducing LWP in nocturnal marine Sc. Are these processes occurring in the simulations presented here? If not what is happening in the CONTROL run? It is important to establish this before sensitivity studies are used to justify a hypothesis.

From the later results (Figure 8 and description of SW-M1.5), I think the authors believe that the reduction in the LWP with increasing aerosol is the result of less cumulative condensation leading lower LWP. I am struggling with this explanation. I except that increasing CDNC leads to an increase in competition for water vapour, an increase in the condensation rate and stronger updrafts; however, this is associated with increased evaporation in the downdraft so that the net change in the latent heating in the cloud with increasing aerosol is zero. This is shown in Hill et al (2008). The differences in condensation-evaporation (CE) rate between simulations with low and high aerosol occur at the cloud top and cloud base. Changes in the CE rate at cloud top will impact

entrainment and entrainment warming of the cloud (not discussed in this manuscript), while changes at cloud base will effect decoupling (discussed in this manuscript). The bulk analysis will not pick up on these subtleties, yet I think that it is this change in the condensation-evaporation rate in the vertical that is important in determining the response of the cloud to increasing aerosol. In addition to their discussion on decoupling due to solar radiation and cloud base evaporation, the authors need to discuss cloud top entrainment and the previous work on this topic to justify why they discount the cloud drop sedimentation and cloud top evaporation as possible causes of the change in LWP with increasing aerosol. The authors need to present plots of that demonstrate the underlying mechanisms relative to those suggested in previous work. It would be useful to see vertical profiles of CE rates as this is an important variable when considering the effect of aerosol induced competition for water vapour.

3) The paper presents some sensitivity simulations in which the downwelling solar radiation is increased by half, reduced by a factor of 2 and a factor of 5. I think these tests are interesting numerical experiments but I am struggling to see the point of them as they are not well justified. As this paper revolves around an aerosol induced condensation effect, the authors have not presented a full set of results to back-up their conclusions from these sensitivity runs. By changing the downwelling solar radiation, the cloud top heating rate will change, which in turn will influence the cloud top entrainment. Nothing is presented or discussed on this issue. Furthermore, if the changes in cloud microphysics due to increasing aerosol feed into the radiation calculation, then increasing aerosol will lead to a different vertical profile of solar heating. These differences could be influencing the results of the sensitivity runs yet nothing is mentioned about this. As this paper concentrates on solar radiation it is important to discuss influence of solar radiation on cloud top as well as cloud base, particularly when changing the downwelling solar radiation.

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