Atmos. Chem. Phys. Discuss., 9, C7335–C7338, 2009
www.atmos-chem-phys-discuss.net/9/C7335/2009/

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 #1

Received and published: 20 November 2009

Summary: This paper provides nice support for current theory about the effect of aerosol, precipitation, and radiation on LWP, but suffers from a conceptual mistake early in the paper which causes problems throughout the entire analysis. It also doesn't seem that new, with most of the middle section coming almost directly from a previous paper by the same authors. For these reasons (explained in more detail below), I strongly urge that the paper be rejected. I think with more thought the existing model runs could be used in an interesting paper, even though they are not ideal (as noted below).

I quit reading in detail and only skimmed after 5.3.2 because everything seemed to depend on the flawed conceptual framework, so careful reading didn't seem worth my time. Perhaps I missed some original results by doing so.

I'm sorry I couldn't be more positive.

Major Issues:

1. I have a big problem with sections 5.2 and 5.3. First, they are almost identical to sections 5.2 and 5.3 of Lee et al, (2009; JGR) so they aren't original research. Second, I'm pretty sure their assertions are wrong.

Section 5.2 computes the domain- and time-averaged cloud liquid water budget and finds condensation and evaporation to be much larger than autoconversion and accretion. This is unsurprising in light of Fig. 2, which shows both cloud top and cloud base rising rapidly throughout all of the simulations. This graphic suggests several things. First, the difference between condensation and evaporation is probably a better measure than the individual components. Second, condensation/evaporation in these model runs is largely driven by entrainment, which determines the rate of PBL deepening. This means that even though sedimentation doesn't itself remove much liquid from the cloud, it could still be responsible for the observed LWP changes by altering the entrainment rate (and in fact, both cloud droplet and raindrop sedimentation are well known to have strong effects on entrainment). These sedimentation-induced entrainment changes are erroneously attributed to condensation in your analysis.

Section 5.3 builds off of the faulty logic in 5.2 by trying to identify why aerosols have such a strong impact on sedimentation through the use of the vapor diffusion equation. This is misguided because vapor diffusion is just a vehicle for converting supersaturated vapor to liquid and is unrelated to the underlying *source* of the supersaturation. The source of that supersaturation is aerosol-induced change in moisture fluxes, entrainment, etc. as noted above. Vapor deposition *could* be the limiting step preventing condensation from occurring, but if that was the case supersaturation would build up over the course of your simulations as supersaturation created by other processes fails

ACPD

9, C7335–C7338, 2009

to be converted into liquid. This doesn't happen because liquid drop nucleation and vapor deposition are extremely fast (almost instantaneous) processes. This is why your peak supersaturation is always less than 0.1% in Fig. 7. I am not surprised that increasing Nd decreases supersaturation, but I'm pretty sure this just constitutes a repartitioning of how supersaturation generated by other processes is converted to liquid without actually affecting the rate at which this occurs.

2. I am concerned that you are nudging moisture and temperature towards the ECMWF reanalysis at all model heights. This will tend to lock your model into having the same PBL height and structure as ECMWF. It also introduces aphysical forcing tendencies into the model. Both of these problems make it difficult to ascribe model behavior to physical mechanisms.

My feeling is that your results are still useful because all of your model runs are being nudged towards the same background state, so differences between runs can still be unambiguously attributed to aerosol/solar forcing differences. I think the results we see are probably damped/distorted by your nudging choice though. At the very least, I think you should show that the nudging tendencies in/near the PBL are always much smaller than other terms in the relevant budgets.

3. I don't like your MODIS validation. First, more explanation is needed - are we looking at MODIS data for a single overpass? What time(s) are involved? What is the spatial resolution of the MODIS data (ie are you comparing boxes of similar spatial scale)? What is the observational uncertainties? Are there any relevant known biases? Second, it's easy to get the time-averaged mean state right for the wrong reasons. In fact, perhaps your agreement with MODIS comes from nudging to the ECMWF data and doesn't reflect your model's ability to respond to forcing changes at all! I would have much more faith in your analysis if I knew that you were getting the evolution of PBL height right, since entrainment plays such a critical role in PBL structure/evolution/response to forcings. The fact that your PBL depth almost doubles over 12 hrs (Fig. 2) makes me worry that you *aren't* getting entrainment right. I would feel

much better if you could show me that your model captures the BL evolution for that day, or even that it matches the climatological diurnal cycle.

Minor Issues:

1. p. 23795 l. 2: You use CDNC in text to mean the same thing as Nd in equations. I'd suggest using Nd throughout.

2. p. 23795 l. 5: Can you defend why do you choose this location/time? It would be much easier to show that your model is behaving reasonably if you ran one of the standard cases from a Sc campaign.

3. p. 23796, l. 11: Is there added value in using aerosol from the CAM-IMPACT model, or would your conclusions be the same if you just fixed the droplet concentrations at high and low values (or if your model can't handle that, fixing the aerosol to be high and low)?

4. p 23797, l. 6: Why do you need to include the stratosphere to simulate stratocumulus?

5. I'm unclear why increasing/decreasing the solar constant is relevant. Are you trying to simulate stratus in different seasons? Or are you just using solar forcing to push the model into different regimes (precip reaching surface or not). Some explanation of your motivation would be helpful on p. 23797.

6. p. 23803 l. 20: Increased updrafts are probably what causes the condensation, not the othe way around (as noted in major pt #1).

7. Fig 2 really displays the evolution of cloud base and cloud top, but its labels make it look like a contour plot, which is confusing.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 23791, 2009.