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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A comparative study of the response of non-drizzling stratocumulus to meteorological and aerosol perturbations" *by* J. L. Petters et al.

Anonymous Referee #1

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REVIEW: J. L. Petters et al. (2012), ACPD:

"A comparative study of the response of non-drizzling stratocumulus to meteorological and aerosol perturbations"

This manuscript describes from a modeling perspective, the sensitivity of marine stratocumulus clouds to independent atmospheric and aerosol perturbations. The authors attempt to validate their simulations with the use of aircraft observations collected during VOCALS-REx. Consistent with other numerical studies, Petters et al. found that clouds are very susceptible to changes in the meteorological variables, with an aerosol effect partially offset by cloud thinning. A novel contribution of the paper is the quantitative analysis of the cloud radiative forcing as a function of changes in atmospheric variables. The paper is well written and organized, the topic is very challenging and highly relevant, and the numerical evidence generally supports their conclusions. Nevertheless, I have several comments/concerns that need to be addressed by the authors:

1. Diurnal cycle and subsidence: Although it seems the model compared well with aircraft observations, it remains an open question whether the diurnal cycle in liquid water path (LWP) or cloud top height (Z) are well reproduced by the model. The authors claim that the Z diurnal cycle is negligible, however, observational evidence does show that the Z diurnal cycle can be significant over the southeast Pacific domain (alongshore and far offshore e.g. Brunke et al., 2010; Zuidema et al. 2009). I believe this is related to a diurnal cycle in subsidence (not necessarily the subsidence wave described in Garreaud and Munoz, 2004). In contrast to Petters et al., a better treatment of the subsidence allowed Caldwell and Bretherton (2009) to reproduce a nice diurnal cycle. Without a better treatment of the subsidence in the simulations, the results are less relevant in the context of VOCALS-REx. In addition, although the aircraft observations are not suitable to investigate the daytime cycle in liquid water path, microwave climatologies (e.g. O'Dell, 2008) should provide a nice dataset to validate the simulations.

Another conflicting point is the authors' decision to simulate six hours of the 24-hour cycle. I agree with the authors that perhaps the simulation of the entire cycle is unnecessary, but at least I would expect they extend the simulations until 1400 local solar time (14+5=19 UTC), because LWP is a minimum and the solar insolation is a maximum (it would also resolve the amplitude of the LWP diurnal cycle). Moreover, a cycle between 11-19UTC would help explore potential differences between Aqua and Terra satellite retrievals, something that would further support the idea of the importance of the time of observation in aerosol indirect effect studies. From a cloud albedo perspective, simulations before 11 UTC (6 local time) are irrelevant.

2. ECMWF reanalysis It is difficult to justify the use of reanalysis to calculate the magnitude of the atmospheric perturbations. This is not only because coastal regions

are not properly represented in the reanalysis but because it is difficult to get a good magnitude of the inversion jump with a dataset that does not have the vertical resolution to resolve such inversion jump. It is likely that as a consequence, the changes in temperature and humidity used in the simulations are too small. This idea is supported by Zheng et al. (2010, ACP, Figure 11). The rather modest LWP response might be attributed to the small magnitude of the perturbations used in the model. It would be more adequate to select the perturbations based on Figure 11 of Zheng et al. (2010).

3. "6.3 Response to perturbations in radiative heating" According to the authors, the goal of this experiment is to understand the effect of sampling time (particularly from satellite instruments) in cloud-aerosol studies. Nevertheless, the satellite sampling does not have anything to do with changes in the solar forcing (unless I am missing something) and it depends only on the part of the diurnal cycle that is sampled by the instrument. In other words, the authors can explore the importance of sampling time by analyzing the simulated cycle in cloud properties from the control simulations, without section 6.3, which is unphysical.

4. Other comments

- The authors should clarify the title by adding the word modeling or LES: e.g. "A modeling study of. . ..."

- It is very surprising the large increase in LWP with a decrease in number of droplets. LWP changes up 30 gmˆ2 and seems to dominate the cloud radiative response near noon. Hill et al. (2010) found smaller changes in LWP due to a decrease in aerosols (nocturnal stratocumulus). It is conflicting the fact that the evaporation-entrainment effect in LWP (due to a decrease in aerosols) is larger than the meteorological factor.

- p27113, line 13-15: VOCALS-ACP papers (George and Wood, 2010, Painemal and Zuidema, 2010) do account for variations in meteorological context. Matsui et al., 2006 also explore the importance of the lower tropospheric stability in changes of cloud microphysics.

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- p27118 lines 9-14. I did not fully understand the sentence.

- Table 1, is the value of subsidence consistent with the one found near the coast?

- Figure 2: The simulations underestimate the fluxes below the cloud base. Do the authors think that this misrepresentation can affect their main results?

- P27124 lines 9-10. Wyant et al. only compared monthly averages, not day-to-day variability in cloud cover. Moreover, cloud fraction along the VOCALS-REx coastal region is particularly misrepresented by ECMWF (maps in Wyant et. al).

- P27128 lines 5-11: This is correct, but another factor not mentioned by the authors is that the atmospheric variables are also correlated. That is, changes in the temperature inversion are simultaneous with changes in humidity, subsidence, and temperature advection.

- P27131 lines 1-3: This idea (the importance of sampling time) was also explored in Painemal et al. (2012) with the use of geostationary satellite retrievals.

- Optical thickness is not proportional to the radiative response. In fact the two-stream cloud albedo rapidly increases with small magnitudes of optical thickness (tau) but it remains almost constant for large tau. For this reason Figure 12 is misleading, in the sense that it does not represent the cloud radiative response. From two-stream considerations only, the radiative impact of the perturbations should be larger for optically thin clouds (i.e. near noon). This idea further stresses the importance of extending the simulations beyond 15 UTC (10 local time).

- 7.3 Computed sensitivities: I wonder if delta(log(response))/delta(log(perturbation)) is constant and independent of the magnitude of the fractional perturbation. If this is not the case, then the sensitivity should be calculated for the same fractional change in Nd, humidity, and potential temperature. For this reason, it makes sense to calculate the cloud response as the simple difference between the perturbed case and the control simulation, keeping in mind that these changes are associated with broad ranges in

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variability of the perturbations.

- Figure 14: This in an interesting figure. I wonder why the authors did not show the differences for extreme cases, e.g. (base Nd –quarter Nd), (base moist- up2xmoist), (up theta- down theta). The inclusion of these cases should show the range of variability of the radiative forcing.

- P27143 lines 5-6. I did not find evidence in the paper that supports this statement.

- P27143 line 22, it should be: Garreaud and Muñoz, (2004)

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 27111, 2012.