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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.

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We thank Anonymous Referee #1 for their thorough review and numerous suggestions, as they have led to substantial improvements in our manuscript. We hope this referee finds our response and the accompanying revisions of our manuscript (included as a supplement) reasonable.

To begin our response, we find it important to mention one important criterion we used in the determination of the length of our simulations. This study was intended to address the cloud response to perturbations in aerosol and meteorology of daytime overcast non-drizzling stratocumulus only. At approximately 1430 UTC the cloud fraction

(as defined in this manuscript) for our base simulation departed from unity and decreasing to 0.92 by 1530 UTC, by which time the cloud layer can be considered broken. It is for this reason that our simulations ended at 1530 UTC.

There are a few reasons why we wished to eschew simulation of and comparison between broken cloud layers. First, our large-eddy simulations assume homogenous mixing within model grid-boxes. Hill et al. (2009) did not find this assumption to substantially influence the LWP and cloud optical depth of an overcast model cloud layer. However, this assumption may play a more important role in the simulation of a broken stratocumulus cloud layer (Morrison and Grabowski, 2008), and its effect has yet to be studied to our knowledge. Second, the radiation parameterization used here (Harrington, 1997) uses the Independent Column Approximation (ICA), under which radiation is not exchanged between model columns. Use of the ICA could lead to biases in both computations of cloud radiative heating and cloud radiative forcing (e.g. Zuidema et al. 2008).

That we did not mention this criterion in our manuscript is an error on our part, and we apologize for this oversight. We have added a description of this criterion into our manuscript, including the references above, and we will reference this criterion in the rest of this response.

We now address each one of this referee's comments/suggestions in more detail.

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.

RESPONSE - We agree with this referee that, in order to best reproduce the diurnal cycle of stratocumulus-topped boundary layer (STBL) properties for Southeast Pacific coastal stratocumulus, simulating the diurnal cycle of large-scale subsidence is important (e.g. Caldwell and Bretherton, 2009). In our manuscript, we did not argue that large-scale subsidence was negligible (see 27143, line 20). Rather, we noted that we did not account for large-scale subsidence in our simulations, and this an important caveat in interpreting our results.

As stated above, our intention was to make an observationally based daytime overcast stratocumulus case, not to simulate the diurnal cycle of stratocumulus. To this end we found it expedient to keep large-scale subsidence constant. We have made this intention more clear in our manuscript.

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 solartime (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.

RESPONSE - We agree that Aqua and Terra satellite retrievals of the diurnal cycle of liquid water path (e.g. O'Dell et al. 2008) would be useful for comparison to our

simulated liquid water path, if our intent was accurate simulation of the diurnal cycle in STBL properties. As stated above, our intention was to simulate daytime, overcast stratocumulus and examine how perturbations in aerosol and meteorology changed the cloud radiative response; for this reason our simulations ended at 1530 UTC.

We also agree with this referee that, from a cloud albedo and shortwave cloud radiative forcing perspective, simulation before 1100 UTC is not important. Indeed, it is for this reason that Figures 10 to 13 (previously Figures 11 to 14) show data from 1200 UTC to 1500 UTC. However, simulation prior to 1100 UTC was necessary to spin-up realistic boundary-layer eddies (see 27119, line 10), and some interpretation of model output prior to 1100 UTC is necessary to interpret our daytime model output.

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).

RESPONSE - From this comment, and similar comments by the other referees, we must conclude our description of the use of ECMWF reanalysis (ERA-Interim data) in our manuscript (27124 to 27125) was not clear. We have modified our description in the hopes that our use of this reanalysis output is more understandable.

As this referee noted, we did not use the ERA-Interim output to compute the magnitude of the temperature and moisture jumps. We used the ERA-Interim output to compute the variability in the temperature and moisture jumps, and thus the size of perturbations in these jump properties to use in our simulations. We agree with this referee that the ERA-Interim output is of too coarse a vertical resolution (25 mb) to resolve the inversion jump. For this reason we computed the jump properties as the maximum gradient across two 25 mb vertical levels (over 50 mb) between the 1000 and 700 mb levels. With this approach we should avoid the 25mb layer wherein the inversion is represented, and obtain the total change in potential temperature and vapor mixing ratio across the boundary layer top (i.e a representation of the jump properties). We have added a description of this computation into the manuscript.

We have also noted in our manuscript that, while the ECMWF model suite reasonably represents cloud fraction in the Southeast Pacific, the agreement between the ECMWF models and MODIS observations lessens as you approach the coast.

We have also added a comparison between the magnitude of our perturbations in potential temperature and moisture jump to those in Zheng et al. 2011, ACP, Table 2 (their variability in these jump properties). Our potential temperature jump perturbation agrees quite well with Zheng et al. 2011 (0.1 K larger than theirs), while our moisture jump perturbation is smaller by more than a factor of two. In agreement with this referee, the ERA-Interim data does not appear to capture the free tropospheric moisture variability observed from the Twin Otter during VOCALS. Our possible underestimation of the magnitude of moisture jump perturbations suggest that the associated cloud responses might also be underestimated. This underestimation lends further strength to our conclusion: perturbations in meteorology can elicit cloud responses as large or greater than cloud responses to perturbations in aerosol. We have added this statement to our conclusions section.

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.

RESPONSE - We agree with this comment, and appreciate the referee pointing out a more physically realistic approach to examining the importance of sampling time. Because other studies have examined the importance of sampling time with respect to satellite retrievals in detail (e.g. the Painemal et al. study this referee refers to below), we have decided to remove our analysis entirely in lieu of modifying it. Our primary conclusion, that meteorological context can be as important in determining stratocumulus radiative response as aerosol state, is unaffected by our choice to remove this analysis.

Instead, we have added a brief comment on the importance of sampling time into our discussion/conclusions section.

4. Other comments - The authors should clarify the title by adding the word modeling or LES: e.g. “A modeling study of: : :..”

We have modified the title to begin as ’A comparative study of the response of modeled non-drizzling stratocumulus...’ It is true that a knowledgeable reader will try to ascertain whether our study is more simulation based or observation based, or a mix of both.

- It is very surprising the large increase in LWP with a decrease in number of droplets. LWP changes up 30 gmËĘ2 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.

There are many possible reasons why we find a larger increase in LWP with a decrease in droplet concentration than Hill et al. (2009). A few might be differences in the initial thermodynamic profile and that we are modeling daytime stratocumulus instead of nocturnal stratocumulus. However, the focus of our study is not a comparison of our results to those in other studies (apart from our comparison of sensitivities in section 7.3). We find it more important that our simulation set be internally consistent, so that cloud responses to meteorological perturbations can be reasonably compared to those responses to perturbations in aerosol.

- 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.

We thank the referee for making us aware of other satellite-based studies where stratocumulus variation due to meteorology is compared to variation due to aerosol. We have added comment on these studies into our introduction.

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

These two sentences outline our reasons for choosing to use LWC as recorded by the Phase-Doppler Interferometer instead of LWC recorded by the PVM-100, another instrument on-board the Twin Otter. We have re-written the sentences in the hopes that these reasons are more clear to the referee.

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

As now better described in Table 1, we chose the large-scale subsidence value to best match the evolution of boundary layer height during observational period (i.e. no net detrainment or entrainment into the boundary layer). This value is generally larger than that observed for this region and portion of the diurnal cycle.

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

This underestimation of vertical velocity variance below cloud base suggests the model boundary layer is not as well-mixed as that observed, and perhaps our simulations are more controlled by processes at the top of the STBL than they should be. However, it

is difficult to ascertain what impact this underestimation might have on our simulations and the accompanying results.

Our simulation set is based on a base case that best matches in-situ observations, and is internally consistent in how cloud processes are treated. Of course, there are many known limitations in the ability of large-eddy simulation to represent the STBL (overestimation of entrainment efficiency, for example), and we must keep these limitations in mind, as one must when interpreting output from any numerical model.

- 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).

We have added these further caveats to our description of our choice to use ERA-Interim output, in lieu of simply stating that the ECMWF family of models 'reasonably simulates' low cloud fractions in the region.

- 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.

We agree with the referee on this point, but also note that in this study we are investigating the cloud response to only one perturbation at a time (see 27127, line 5). We had noted the importance of correlation between aerosol and meteorological context in this section, and have added that correlation between meteorological variables could also be a subject of future study.

- 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.

Again, we thank the referee for making us aware of an important study relevant to our work. We have included comment of the Painemal et al. (2012) study in our brief comment on the importance of sampling time.

- 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).

The referee's concern that Figure 12 does not represent the cloud radiative response is precisely why we compute shortwave cloud radiative forcing, as shown in Figure 14. Cloud optical depth is but one factor in the computation of cloud radiative forcing; solar insolation is also important, as we describe in section 7.1.3.

However, the referee is correct that albedo does decrease more rapidly as tau decreases towards zero, in a non-linear fashion. We have attached here a simple figure showing the relationship between tau and albedo, as computed by Bohren (1987), that supports the referee's assertion.

We again thank the referee for their insight, as we have neglected the possible impact that this non-linearity could have in our discussions shortwave cloud radiative forcing responses. We have modified our comments on the tau-albedo relationship in section 7.1.3 accordingly. Changes in solar insolation over the morning clearly play a role in the cloud radiative forcing response, but we can not neglect the tau-albedo relationship entirely.

Regarding the referee's interest in extension of our simulations, we again refer to our desire to simulate overcast stratocumulus only, as described in detail above.

- 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 variability of the perturbations.

We strongly agree with the referee that simple absolute differences between cloud properties in the perturbed and control cases are the most relevant quantities for comparison. It is for this reason that most of our discussion centers on absolute changes (see Figures 11 to 14) as opposed to our computed sensitivities. We computed these sensitivities mainly for simple comparisons of our results with other studies, to show that our results were not in complete disagreement with previous literature.

- 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.

The differences for extreme cases can be easily obtained by adding up the two perturbation values from each hour. We show the differences for each of the two perturbations to show that the cloud radiative forcing responses can depend strongly on the reference state of the cloud system (see 27139, line 16).

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

We thank the referee for bringing this to our attention, as we meant to have a similar comment in section 7.1.2. When we perturbed the temperature and moisture jumps in our base case, there we no large subsequent changes in droplet concentration. There are certainly feedbacks between changes in inversion characteristics and droplet concentration in general, but in our case, droplet concentrations remained quite steady. We have added reference to this explanation in section 7.1.2.

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

We thank the reviewer and have corrected the name in our bibliographic records.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/12/C11812/2013/acpd-12-C11812-2013-supplement.pdf

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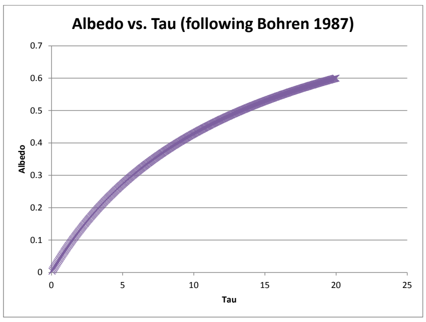

Fig. 1.