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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

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 #2 for their thorough review and numerous suggestions, as they have led to substantial improvements in both our work and our manuscript describing that work. We hope our response and the accompanying revisions of our manuscript (included as a supplement) assuage the concerns of this referee.

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

1. Since you mentioned the simulations might be resolution dependent, have you tried different resolutions to make sure the model output won't change too much when the resolution is higher than the current setting? Furthermore, if you increase the spatial

and temporal resolution, will the main results (e.g. Fig 11-12, Table 6) still be similar as the current results?

RESPONSE - We have not studied the variation of spatial and temporal resolutions on our results. Other studies, such as Cheng et al. (2010), have investigated the impacts of spatial and temporal resolution on large-eddy simulation output. Though such an investigation was not the focus of this study, we have added a statement about this important caveat into the conclusions section of our manuscript.

Although it is desirable to test the robustness of simulation results to a wide range of parameters and conditions (including choices of spatial and temporal resolution), it is not always possible due limitations in computational resources. Large-eddy simulations are in general computationally expensive, and because these simulations include the use of bin microphysics and computation of solar radiative heating they are even more expensive.

2. The paper specifies the large-scale subsidence and initial condition for best match to observations (Table 1, 3), and argues that the base case simulation without the sub-grid scheme (NODIFF in your paper) matches better to the observations (pp27123 L19-20). When you tune the initial condition and large scale forcing to match the observations, which one (DIFF or NODIFF) is the testing case? Because the shape of LWC distribution from DIFF looks closer to observation (Fig 4 a-c), is it possible that you can tune the initial condition and large scale forcing to let DIFF match better?

RESPONSE - The large-eddy simulation testing case, for which the initial thermodynamic profile and large-scale subsidence were modified to generate a best match with observations from the Twin Otter, was the NODIFF simulation.

We agree with this referee that it is possible that further tuning of the DIFF simulation could lead to a better matches in liquid water path and LWC distribution. However, we caution the referee that cloud water is only of many STBL properties of importance. Accurate simulation of boundary-layer circulation strength, exhibited in our study through

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vertical velocity variance, is also important for accurate simulation of STBL evolution.

Both simulations underestimate vertical velocity variance, especially below cloud. However, we can expect that, regardless of further modification to thermodynamic profile or large-scale subsidence, NODIFF will exhibit a better match to vertical velocity variance observations. It is this combination of STBL properties (good match in LWP and the better match of circulation strength) that leads us to choose NODIFF as our model base case (see 27123, lines 14-20).

3. According to section 6, both meteorological and aerosol perturbations impact the cloud layer through changing cloud top entrainment rate (Table 4 and section 6.4). Recent direct numerical simulations (DNS) of the cloud top interface indicated that the small eddies and molecular processes near the cloud top play a key role in regulating the entrainment rate (Mellado 2010, J. Fluid Mech.). Traditional LES, however, is unlikely able to represent such small scale processes. Therefore, the cloud top mixing processes are largely simplified in LES and cloud top entrainment might not be simply overestimated as the paper discussed (pp 27132, L 16-18). You may explain more or prove that the comparison in this study is very solid.

RESPONSE - This referee is certainly correct that entrainment processes are simplified in large-eddy simulation. It is but one of many simplifications necessary to reasonably simulate the numerous important processes in the STBL (e.g. radiative transfer, cloud microphysics, turbulence).

It was not our intention to suggest that ’overentrainment’ is the only important result of this simplification (27132, lines 15-20), though we can understand how this referee might interpret the relevant passage as such. In our description of our model (Section 2), we now more clearly state that processes at scales smaller than what we explicitly resolve at 5m are important in cloud top entrainment (e.g. Mellado, 2010).

Minor comments: Abstract: pp27112 L2-11: The key research methods could be more detailed.

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We agree with the referee, and have added a few sentences to discuss the investigative approach (e.g. LES as our modeling framework, our use of reanalysis data etc.)

pp27112 L9: “LES” should be spelled out at its first occurrence.

We agree and thank the reviewer for catching this inappropriate use of an abbreviation.

pp27112 L12-15: “those responses found due to similar changes in aerosol state.” I don’t quite understand “similar changes”. Does it mean the similar magnitudes of the changes? Or the similar relative changes in aerosol state?

This sentence could me more clear, we agree. Because our aerosol and cloud droplet concentration perturbations are meant to span a reasonable range of observed values, we have changed the offending phrase to read as "those responses found due to realistic changes in aerosol state."

Introduction: pp27115 L21-23: The sentence is a bit misleading. Section 2-4 are model configuration, observation, and comparison. You may either modify the sentence, or reorganize section 2-4 to be observation, model configuration, and comparison.

We agree that we should order the topics within the sentence in question to correspond to sections 2 through 4. Thus we have changed the sentence to read "Describing the LES model description and configuration, the observations used to create the model base case, and the comparison between LES output and observations (Sects. 2 to 4) comprise the first part of this study."

pp27115 L26 - pp27116 L4: These sentences already appear in section 7 and need to be removed.

While it can be argued that these sentences do not contain introductory material and hence do not belong in the introduction, we feel this material helps to both grab the attention of and orient the reader early in the manuscript. Thus we respectfully decline to incorporate this particular suggestion.

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Section 3 pp27118 L23-24: Can you roughly estimate how the cloud layer changed by checking the profiles taken at the beginning and the end of the flight mission?

Yes, we can estimate the rough changes in the cloud layer over the observational period, as the referee suggests. The referee's implicit suggestion, that we should quantify these changes in the observed profile in our manuscript, is indeed helpful. We have modified section 3 (Observations) accordingly.

Section 5 pp27124 L4-10: the vertical resolution of the ERA-Interim dataset is not high enough to resolve the strong inversion at the top of stratocumulus layer. Furthermore, the ERA-Interim dataset tends to underestimate the boundary layer depth near the coastal region. Both issues can affect the estimates of qt and theta jumps.

From this comment, and similar comments by the other referees, we have concluded our description of the use of ECMWF reanalysis (ERA-Interim data) in our manuscript (27124 to 27125) is not clear and have modified the description.

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.

To the reviewer's concern regarding the validity of our jump property variability: 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). We find it important to reiterate that 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

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in our simulations.

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. Thus 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.

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.

pp27126 L5-6: During VOCALS, the Twin Otter observed Nd ranged between 80 and 400 cmˆ-3(Zheng et al., 2011, ACP).

We thank the referee for this suggestion, as we should refer to the most recent observations available in the literature. We have modified the manuscript as suggested.

Section 7 pp27141 L25-26: The mean value (Table 6) in this study is much smaller than in other studies (Lu and Seinfeld, 2005; Hill et al., 2009). Can you explain what physical processes might contribute to the large value of standard deviation?

For this question we assume the referee is referring to the sensitivity of optical depth to droplet concentration in Table 6. The relative magnitudes of the mean and standard deviation, averaged over the last four hours of simulation time, can be explained by the variation in the cloud optical depth explained in section 7.1.2. As the cloud layer thins during the day, this increase in optical depth due to increases in droplet concentration is increasingly offset by decreases in liquid water path. The perturbations in droplet

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concentration lead to an increase in optical depth for the hours centered at 1200 and 1300 UTC, while for 1400 and 1500 UTC this increase is more than offset by cloud thinning. Thus, averaged over the four hours the sensitivity of optical depth is near zero while the standard deviation is not.

We have this discussion into our manuscript and thank this referee for their insightful question.

Section 8 pp27142-143: Many symbols, such as DIFF, NODIFF, tau, SW CRF, have been defined in previous chapters.

We agree with the referee, and have altered the abbreviation usage in section 8 accordingly.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/12/C11824/2013/acpd-12-C11824-2013-supplement.pdf

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

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