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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 #3 for their review and suggestions, as they have led to 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. Although the manuscript focuses on nonprecipitating stratocumulus, the authors should include Wang et al., 2010, ACP "Modeling microphysical and meteorological controls: : :" and Mechem et al., 2012, JAS "Thermodynamic and aerosol controls: :

:," who explicitly address similar sensitivities in precipitating VOCALS cloud cases and come to similar conclusions as to the relative importance of meteorological vs. aerosol factors.

We thank the referee for making us aware of important studies relevant to our work, and have incorporating comments on these studies into our manuscript (see Sections 1 and 2).

2. Page 27120, lines 13–14, "reasonable" to omit SGS fluxes. I think "necessary" might be a better term. The fact that LES modelers can just throw out the SGS terms (e.g., Stevens et al. 2005) and call it "OK" is somewhat disturbing. Isn't this just masking some other problem, perhaps excessive numerical diffusion? (just a comment: : : no need to respond.)

We have changed this word from "reasonable" to "acceptable" in order to express more chagrin in this particular choice. Use of "necessary" implies that we have little choice in the matter, but we could have chosen to include sub-grid scale fluxes in lieu of a better match with observations.

3. Page 27124, lines 4–5 and onward. The vertical resolution of the Era–Interim data is not ideal for resolving the qt and theta jumps. The authors should quantify the uncertainty in these estimates.

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 temper-

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ature 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 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 in our simulations. Towards quantifying the uncertainty in our variability estimates, We have 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. 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.

4. Page 27125–27126, lines 20–29 and 1–9. It is not clear that the aerosol variability is constrained from the VOCALS observations. And I do not understand how or whether these numbers rigorously correspond to a 1-sigma variability.

Our model aerosol variability is not constrained by the VOCALS observations. Rather, we simply multiplied the observed aerosol and cloud droplet concentrations by factors of 0.50 and 0.25, resulting in a range of concentrations that span polluted to clean aerosol conditions. We have modified our manuscript so that it is clearer that our model aerosol variability is not constrained by observations. Instead, we are comparing our model aerosol variability to that in Zheng et al.

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Because our model aerosol variability is not determined in the same method as the variability in our jump properties, there is not rigorous correspondence between the two. We attempted to state this (see 27125, lines 16-19), but we were evidently not clear enough. We have modified this passage to be more clear.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/12/C11831/2013/acpd-12-C11831-2013-supplement.pdf

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