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Interactive Comment

Interactive comment on "Wind speed response of marine non-precipitating stratocumulus clouds over a diurnal cycle in cloud-system resolving simulations" *by* J. Kazil et al.

Anonymous Referee #1

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General Comments

The motivation for the paper as stated in the abstract is the question "How might marine stratocumulus clouds and their radiative properties respond to future changes in large scale wind speed?" One way to answer this would be to perform LESs, without and with perturbed geostrophic wind speeds, that are allowed to reach a steady state. The recent study by van der Dussen et al. (2015) is an example of this approach. In that study, only the SST and the above-cloud thermodynamic profiles were changed between the control and perturbed sets of LESs. The duration of each LES was 10 days, and the horizontal domain size was 6 km by 6 km. As shown by Schubert et al.

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(1979), the adjustment time scale of the boundary layer mean thermodynamic variables to a step change in SST is fast (a few hours) while that of the boundary layer depth much slower (a few days). In light of these two studies, it seems that the authors' choices of short duration LESs (only 24 h) in a large domain (30 km by 30 km, which is 25 times larger in area than that used by van der Dussen) were perhaps not the best ones for answering the question posed.

The most significant immediate impacts of a change in the boundary layer wind speed are to the surface fluxes of sensible and latent heat. These fluxes are of course also affected in a similar way by a change in SST, so that one could presumably use the results of perturbed SST experiments for guidance. Or one could use a cloud-topped mixed-layer model like the one used by Schubert et al. (1979) and simply change the specified surface wind speed in the model.

The authors' experimental design produces complicated results that could have been made simpler and easier to interpret by a different design. The complicating design aspects include the short (24-h) duration of the LES and representing the diurnal cycle. As a result, much of the paper is devoted to explaining the complicated results that arise, and make it cumbersome.

Specific Comments

1. p 396 lines 2-4, p 398 lines 23-24, p 399 lines 9-10, p 400 line 3, p 400 lines 19-20: The simulations presented cannot answer this question (how might marine stratocumulus clouds and their radiative properties re- spond to future changes in large scale wind speed?) because they are not run to equilibrium.

2. p 397 line 15: There are many more recent papers that could be cited. There are many such papers in JAMES, for example.

ACPD

15, C10203–C10207, 2015

3. p 402 lines 18-19: Why use such a large domain size? It might be better to use a smaller domain and smaller grid sizes. The grid sizes are about 3x larger than recommended for LES of Sc

4. p 403 lines 8-9: Omitting sedimentation of cloud droplets will affect the entrainment rate (Bretherton et al. 2007).

5. section 2.1.3: Matching observations is not critical for a sensitivity study like this one. For that reason, this section could be greatly shortened, or moved to the appendixes.

6. p 412 lines 17-21: The (updraft) mass flux profile might provide a useful measure of the changes in BL circulation. See Krueger et al. 1995a,b for examples. Those studies were the first to document the deepening stage of the stratocumulus to cumulus transition. As far as the BL circulation is concerned, increasing fluxes due to SST increase or wind speed increase are no different because the BL sees only the surface fluxes, not the processes that produce it.

7. p 412 lines 23-4: One way to quantify the circulation strength is with the mass flux profile.

8. p 412 line 27 and p 413, line 1 “Circulation" is ambiguous. I don’t think it is synonymous with TKE however. I think instead of mass flux as being a measure of circulation.

9. p 412 line 5-7: Why doesn’t the enhanced entrainment counteract the surface flux increase and decrease the LWP? You should explain this.

10. p 412-3 section 3.1.3: The last two paragraphs may be too much detail. Summarize instead.

11. p 414 lines 6-7: It is the decoupling that increases conditional instability and updraft speed.

ACPD
15, C10203–C10207, 2015

12. p 414 lines 11-12: The radiative warming is distributed throughout the BL if the BL is not decoupled. Decoupling restricts the warming to the upper layer, but not because it is cloudy.

13. p 414 lines 22-24: This is a consequence or even a definition of decoupling (actually, not completely decoupled).

14. p 415 lines 7-8: Such a scenario does not occur in simulations of the transition to cumulus as the surface latent heat flux increases due to SST increase, so it should not happen due to wind speed increase.

15. p 417-419, section 3.2.2: There is a lot of detail which makes it hard to follow. Maybe a schematic diagram would be helpful.

16. p 421 lines 3-4: These are the transient responses! For climate change, need the equilibrium responses, as in van der Dussen et al. (2015).

17. p 421 lines 22-23: Can this be quantified with a generalized turbulence velocity scale like Moeng and Sullivan proposed for the CBL?

Technical Corrections

Please see the annotated manuscript for the technical corrections (highlighted in yellow; red underlines should be ignored). Note that the comments listed above are also included on the annotated manuscript.

1. Fig. 2: Please add uncertainty or std dev to observed values if possible. Are the light gray shaded areas supposed to indicate this? If so, it is not clear from the caption.

ACPD
15, C10203–C10207, 2015

2. Fig. 3: What time period do these cover? Is the forcing steady in the spin up period? Why does the wind speed vary in the nocturnal case? Why aren't the initial values the same?

3. Figures 3, 4, 6, C1, C2, C3: There are too many panels. Are they all necessary? I recommend no more than four panels per figure.

References

Bretherton, C. S., P. N. Blossey, and J. Uchida (2007), Cloud droplet sedimentation, entrainment efficiency, and subtropical stratocumulus albedo, *Geophys. Res. Lett.,* **34,** L03813, doi:10.1029/2006GL027648.

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15, C10203–C10207, 2015