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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Strongly sheared stratocumulus convection: an observationally based large-eddy simulation study" *by* S. Wang et al.

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ACP Manuscript Review

Title: Strongly sheared stratocumulus convection: an observationally based large-eddy simulation study

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Reviewer: Patrick Chuang, UC Santa Cruz

General Comments

This manuscript describes simulations performed to better understand the effects and consequences of cloud-top shear on stratocumulus-topped marine boundary layers. Comparisons with aircraft data from VOCALS are also used to help connect the simulations with a real observational case.

Overall, the manuscript addresses an interesting subject. The choice of simulations and analyses support nicely the main story. The manuscript is very well written – there are clear, in-depth physical explanations for the simulation results.

My one major comment is that, common to many studies of this type, the comparisons of observations with the LES simulations are not quantitative. Most of the comparisons are described as (I'll paraphrase here) "consistent with" or "closely matches" or "has a similar shape to" and the like. There are, obviously, many more sophisticated quantitative ways to compare two sets of values and I wonder if the manuscript would be even more appealing if some of these were applied to this study, at least in those places where the observations are most important. To me, the statement in the Summary (p. 4959, line 26-27) "In particular, the SS simulation compares the best with observations in terms of the turbulence statistics profiles" is much stronger if it's backed up with some analysis. The other statements in the same paragraph could also use some quantitative support. I'm not suggesting the authors go completely crazy with analysis of every observation, but maybe some simple statistics could be used in a few places when it's particularly important. I know I'm fighting a whole culture here – that observations are even included is (unfortunately) notable, and for which the authors should be commended, since this is more than is shown in plenty of other modeling studies.

Other than this one general comment, and the few specific comments below, I think it's more or less ready for publication.

Specific Comments

Cover page: Zheng's affiliation should probably be "RSMAS"

p. 4946, line 20 (hereafter 46, 20): above *the* MBL

47, 17: qc values off by a factor of 10

48, 3-4: I'm not sure what "directly controlled" means since a few sentences later it is stated that the buoyancy is affected by shear mixing. Is it really true that the shear production of TKE near the interface is negligible relative to the buoyancy production? Or is this not what this sentence is meant to say?

48, 10-13: this sentence should probably be moved one sentence back to connect better with the preceding sentences.

49, 7: w(w'ˆ3) looks like it's missing an important space (otherwise it looks like multiplication)

49, 11: Needs more explanation? It's not clear to me why change in the mean radiative cooling rate causes a different distribution between updrafts and downdrafts.

49, 12: "It may be also..." This is not well-explained – probably needs another sentence to help the reader understand.

49, 15: "For all three w fields..." It's not clear this is true for the SS case. To my eye, it's close to 50/50 up vs downdrafts. Quantify?

50, 22: Can you mark z_itop and z_ibase levels on the profiles in Fig. 5?

51, 5-14: Fig 6d shows around 8h, the shear in the WS cases crosses over and becomes larger than SS. No discussion of this? How does the inversion thickness at the end of the simulation remain smaller for WS than SS if the shear is stronger? There's not even much sign of inversion thickness change in WS over the entire period, where shear is increasing. If shear controls this, why do we not see this?

51, 20-11: "In fact..." This is simply the discretization of z, right? If so, I don't quite see why is this interesting.

51, 24: Fig 6e (and also the next sentence) say that shear decreases over time, so this statement seems a bit confusing.

52, 14: "three levels within..." I don't quite understand how the levels were chosen. If the exact level within the cloud-free EIL is changed, does the result change? Would the result change if you used all points within the sublayer instead?

52, 15: In my copy, there's no curve for the NS condition in Fig 7a. Does this imply that most of the values for NS are above Ri=2? It's a PDF after all... the area needs to integrate to unity! Update: only much later do you clarify that this PDF peaks at much larger values than is shown on the graph. I'm not sure what the value of showing NS is, then, in Fig 7a. It's more confusing than helpful. I'd just remove it with an explanation.

54, 20: "This is a departure..." This doesn't seem totally true to my eye at least - the same effect happened in the SS, etc. simulations but the differences among the sims was not as strong.

55, 4: Change Fig 9e to plot radiative heating like in Fig 3f – no reason for the inconsistency and heating rates are easier to interpret.

58, 1: "zero just below clouds" Doesn't this demonstrate fairly strong decoupling of the cloud and sub-cloud layers? There are clearly theta perturbations in the cloud layer for SS, but they aren't propagated into the sub-cloud layer. If so, then why is the decoupling characterized as "weak" (58, 14)?

Fig.3: needs a different title, since it's not all simulations! Why no measurements in panels a and b and i? X-axis in panel d is wˆ2 but should be w'ˆ2. Same problem for panel i.

Fig. 5: all panels are missing the "primes" on the x-axis labels.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 4941, 2012.