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Interactive comment on "Coupling a new turbulence parametrization to RegCM adds realistic stratocumulus clouds" *by* T. A. O'Brien et al.

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We would like to thank Referee 2 for insightful and thorough comments. In addition to improving clarity and readability, the comments generally serve to broaden the appeal of the manuscript. We have addressed each specific comment below, and we have quoted text that is modified in the revised manuscript. Our comments are in italics.

Introduction: The author should add more information/references about the state of the

art of Msc simulation in climate models (GCM and RCM). How problematic is this point for climate studies ?

The referee makes a good point here; we were too narrowly focused on RCMs in the introduction. We have added a paragraph (pasted below) to the introduction that gives an overview of the current state of global climate models. Additionally, we added a reference to Wyant et al (2010), who show a nice intercomparison of GCMs, RCMs, and operational forecast models in their abilities to simulate stratocumulus off the coast of south America:

"Martin et al. (2000) improved the simulation of cloud-topped boundary layers in the Hadley Centre global model by implementing the parameterization of Lock et al. (2000), which uses a separate diffusivity profile for the clear and cloudy portions of the boundary layer. K´ohler (2005) implemented a flux-decomposition approach with a special treatment of the stratocumulus to shallow cumulus transition in the European Centre for Medium-Range Weather Forecast global model; in one case, this improvement added stratocumulus clouds where they had previously been missing, and it generally increased (improved) the amount of liquid water in low clouds. Bretherton and Park (2009) implemented a new moist turbulence paramterization and a new shallow cumulus parameterization into the Community Atmosphere Model; Park and Bretherton (2009) demonstrate that this addition reduces the overall climate bias of the model relative to the original model. These improvements to the representation of low clouds in GCMs are critical for understanding how climate might change in the future, given that 'cloud feedbacks... have been confirmed as the primary source of climate sensitivity differences' in climate model simulations (Meehl et al., 2007). Lauer et al. (2010) have demonstrated that regional climate models (RCMs) can provide a valuable, additional modeling framework in which to study MSc and their sensitivity to climate."

p3443 : What does self-similar TKE profile mean ?

We added a parenthetic notation after this sentence to clarify the meaning: "(i.e. the

TKE profile maintains a consistent, geometrically similar shape as the boundary layer height changes)"

P3444: Did the author test the sensitivity of the lambda parameter, and if yes what would they recommend ?

We have not tested the sensitivity of the model to this parameter. In this study, our approach was to use parameters that Grenier and Bretherton (2001) determined were optimal for the UW model; accordingly we can only recommend that others also use these values (e.g. 0.085 times the inversion height for the lambda parameter). We decided that such sensitivity tests would be better-suited for a separate manuscript.

P3448,L 15. How does the RegCM-UW precipitation bias compare to the standard holstlag version ?

We added a sentence to this paragraph indicating that the biases are quite similar between the Holtslag and UW versions: "These biases are quite similar, in terms of magnitude and spatial pattern, to the biases from the standard version of RegCM, although the average wet season bias is slighty higher (by 0.04 mm/day), while the dry season bias is slightly lower on average (by 0.02 mm/day)."

P3449: The authors show an improvement of the simulated interannual variability of precipitation by changing the BL scheme. Is there any physical insight that could explain this result ? e.g: Is convection triggering less 'sensitive' with the UW BL scheme (convective versus non convective precip ratios).

This is a good hypothesis, and it is one that we had not considered. As the referee suggests, the ratio of convective precipitation to total precipitation over land is lower in the UW simulation (52% on average) than in the HOLT simulation (56% on average). Additionally, 19% of the interannual variance of total precipitation over land comes from convective precipitation in the UW simulation, while convective precipitation contributes 29% of the variance in the HOLT simulation; the variance of convective precipitation in

the UW simulation is almost half of what it is in the HOLT simulation.

This reduction in the magnitude and the variance of convective precipitation indeed suggests that convective precipitation trigger is less 'sensitive' as the referee suggests. It seems likely that this dampening of convective precipitation is a by-product of the relatively lower anemometer temperatures in the UW simulation (typically almost a degree cooler on average). Lower near-surface temperatures will tend to stabilize the atmosphere and reduce the convectively-available potential energy; absent any other changes, lower anemometer temperatures will raise the level of free convection and directly reduce CAPE, which is a core variable in the Grell convection parametrization.

We added text to the corresponding paragraph summarizing this finding:

"Of all the changes in interannual variability associated with adding the UW model, improvement of coastal temperatures and of inland precipitation are the most notable. We attribute the improvement of coastal temperature directly to the improved representation of the marine boundary layer (which we demonstrate in the next section). It is less clear why the UW model improves inland precipitation. Comparison of the UW and HOLT runs indicates that convective precipitation in the UW run is significantly less, in terms of magnitude and variability, than in the HOLT run. We attribute this reduction in convective activity in the UW run to a reduction in the near-surface temperatures (typically about 1 K), which should directly reduce the amount of convectively available potential energy, since the level of free convection should be higher on average."

P3452: L 10: Both parameterizations do not capture the observed inversion jump. Could that also be related to the noted underestimation of CLWP and likely cloud liquid water content impacting the radiative budget at the cloud top? Are there any radiative observations which could also be compared to model outputs?

This is a good point, and it is certainly something worth investigating in the future, especially since underestimation of CLWP is a systematic problem for most models (e.g. Wyant et al. 2010). At the moment, we don't have access to direct radiative flux *measurements from these field campaigns, and it is outside the scope of this study to apply a radiative transfer model to the in-situ observations to estimate the radiation budget.*

P3452: L20 -...: underestimation of CLWP: Are the authors comparing grid-level or in-cloud CLWP to in cloud observations? What is the cloud fraction in this case?

We are comparing in-cloud liquid water paths, so that cloud fraction is essentially irrelevant to this discussion; we have made a note of this in the text.

P3452: L20 -...: Can SST or sea-air flux parametrization play a role here in explaining discrepancies of simulated vs observed CLWP .(e.g by compensating the effect of dry air entrainment)?

The air-sea flux parameterization are a potential issue, however we do not think this is the case. Figures 6–8 show that the BL water vapor mixing ratios are very close to those observed, and the SSTs are likely close to the actual SSTs, since we force the RegCM simulations with observed (monthly-mean) SSTs. Since the evaporation rate is essentially controlled by the difference between the BL mixing ratio and the saturation mixing ratio above the sea surface, this implies that the evaporation rate in RegCM is in accord with the actual evaporation rate.

P3452: L20 -...: As seen from section 6.2 and 6.3 the model seems not to be able to simulate CLWP above 30 to 45 g/m2. Beside cloud evaporation tuning, is it possible that simulated cloud water is precipitated out too efficiently which could prevent reaching high CLWP as observed (Does the model simulate any precipitation for the different field experiments?)? What could be the role of cloud to rain microphysics parameterization here?

This is a point that we should have noted in the text; the referee makes a good point that excessive drizzle, which RegCM-UW does produce (typically 1 mm/day in the modeled stratocumulus deck) potentially causes the low CLWP bias. A future study

might investigate this by looking at average drizzle rates, in observations and in the model, sorted by CLWP.

We added a statement noting this possibility (in pp 3455 p 1): "It is also possible that systematically excessive precipitation (drizzle) may cause the the low LWP bias. Figure 4 indicates that precipitation rates over the ocean are within the range of observations, however this does not rule out the possibility that precipitation rates are too low for low-LWP clouds and too high for high-LWP clouds. If this were the case, then the mean precipitation rate might be correct, but precipitation would act as an excessive moisture sink when LWP begins to become large, thus limiting the growth of LWP. A future study might investigate this by comparing LWP-sorted drizzle rates between model and observations."

P3453: Vertical resolution: Sc are affected but what about clear sky boundary layer properties ? Could the author recommend an optimum (or critical) vertical grid step in th BL to keep a good consistency in Sc simulation ?

Good point. From comparing a few clear-sky areas over land in the 23-level and 30-level simulations, it is clear that there are significant differences even in the clear-sky profiles. We broadened the language in this sentence accordingly: "A sensitivity test indicated that the model's vertical resolution has a significant impact on the simulation of boundary layer profiles"

P3462; what could be the influence of vertical resolution in simulating the decoupling process ?

This is an interesting question, but it is one that we don't know how to answer. We are not aware of any studies looking at the sensitivity of the decoupling process to model resolution, and the experiments presented in this manuscript were not designed in a way to examine this.

P3463: Do the authors have the feeling that, beside the refinement of the UW scheme, high horizontal resolution can also bring an added value in term of simulating essential dynamical feature for the simulation of Msc (e.g moutain breeze like circulation near the coast) compared to GCM ?

Absolutely. This is a major theme in a follow-up manuscript: O'Brien et al. "Multi-decadal simulation of coastal fog with a regional climate model", Climate Dynamics (2012; in revision). We added horizontal resolution to the list of sensitivities that should be investigate in future studies.

P3463. Discussion: Most of the discussion concerns and shows the added value of the UW approach for the marine boundary layer. Over land, the authors mention a cold and precipitation bias. How does this bias compare with the standard scheme bias?

This is a good point. As we note in the response to the comment about P3448, the UW parameterization hardly changes the precipitation biases. We also added a note about the difference between the temperature biases in the HOLT and UW runs to pp 3447, p1: "The temperature biases for the HOLT run are similar in spatial pattern to those shown in Figure 3, though they are smaller on average: a 1.0 K cold bias for the wet season, and a 0.02 K cold bias for the dry season."

Interactive comment on Geosci. Model Dev. Discuss., 4, 3437, 2011.