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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.

Anonymous Referee #2

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General comments:

The paper deals with updating the boundary layer parameterization in a regional climate model (RegCM). It demonstrates the benefit of basing turbulent diffusivity calculation on local turbulent kinetic energy rather than surface conditions which translates into a more realistic boundary layer representation, as well as the possibility of simulating stable marine stratocumulous,which is extremely relevant for climate modeling. The topic is in-line with GMDD, and the paper of interest to climate modelling community. A rather detailed analysis of the old vs newly coupled scheme is proposed based on different observations campaigns and is quite convincing in showing the added-value

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of the new scheme in simulating realistically (for a climate model) marine boundary layer and stratocumulus. A multi-year regional scale evaluation of the performance of the model compared to satellite data is also proposed, showing that the model can capture reasonably well the mean, diurnal, seasonal and interannual variability of Msc deck features. This is particularly relevant for forthcoming regional climate application. I would recommend this paper for publication after addressing the following minor comments and suggestions.

Specific comments:

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 ?

p3443 : What does self-similar TKE profile mean ?

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

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

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).

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?

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?

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) ?

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?

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 ?

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

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 ?

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?

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