Review of “Process-Level Diagnostics of Marine Stratocumulus in TaiESM1: Insights into Parameterization Successes and Deficiencies” by Yi-Hsuan Chen and Chein-Jung Shiu

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The authors present a detailed study into the representation of stratocumulus in the TaiESM1 GCM. Combining the analysis of SCM experiments from the DYCOMS-II field campaign with initialised global forecasts for the same region and period has great potential. They highlight a peculiar and unphysical feature of their model, whereby the increments from the turbulence and macrophysics (cloud) schemes oppose each other in the sub-cloud layer. They conclude, correctly in my opinion too, that this behaviour needs to be corrected.

However, I find their explanation of this behaviour and much of the analysis of the model increments to be wrong or significantly lacking. To me, what they have shown looks like a turbulence scheme acting to homogenise the water vapour and liquid water profiles through the PBL (hence the drying of vapour and increase of liquid in the subcloud layer, which the cloud scheme then evaporates to moisten and cool). In other words these are, to me, the characteristics of a turbulence scheme that is mixing water vapour and cloud liquid separately. Yet, they state that TaiESM1 uses the Bretherton and Park moist turbulence scheme that acts on moist conserved variables, so total water (vapour plus liquid). While Bretherton and Park don’t show increments, their Fig 13f shows the total water flux profile from their SCM of the DYCOMS2-RF01 case used here and that scheme generates more or less uniform moistening throughout the PBL (the flux vertical gradient is constant and negative). Can this really be the same turbulence parametrization? It if is, how do they explain such different behaviour?

More detail, together with other more minor comments, is given below.

1. Line 69, “identification of its successes and deficiencies” should be rephrased. There is no “truth” provided to allow this identification (as could in principle be done from by using the increments from a data assimilation process). Rather you have used this analysis to identify unexpected and apparently unphysical behaviour.
2. Line 156: I realise this is pretty standard vertical resolution for CAM (from which TaiESM1 is derived) but it is inadequate to resolve stratocumulus clouds, as the authors go on to demonstrate. Is it really not possible to increase the number of levels?
3. Line 171, “the initial cloud liquid water mixing ratio is set to zero, and supersaturation is not allowed in the initial state”: I don’t understand why the GTS cloud scheme imposes this restriction. Why can you not use the initial liquid water profile from the intercomparison and let the cloud scheme adjust it? As it stands, are you not losing significant water (vapour plus liquid) from the atmosphere by doing this? Yes, you go on to show that the SCM spins up some cloud towards the end of the 6 hour simulation but the mean profiles have drifted significantly in that time.
4. Line 243, “TaiESM1 can realistically reproduce SWCRE”: one key diagnostic that is missing from your analysis is the cloud fraction from the model. Adding that to your time series in Fig 2, in particular, could help explain how TaiESM1 SWCRE is much larger than ERA given fairly similar LWP.
5. Line 274, “From July 9 to 11, advective cooling and drying tendencies appear in the mixed layer, likely due to horizontal advection”. To me it looks like these are still strongest in the grid level with the strongest vertical gradient to the level above and so still from vertical advection.

6. Line 307, “At ql_tend+, the horizontal advective cooling nearly offsets the subsidence warming at the mixed-layer top”: similar to my point (5), you need to do more to justify this statement. For example, have you looked at the vertical velocity profile and does it remain reasonably constant and subsiding through the inversion during this period?
7. Line 313, “the turbulence scheme… tends to cool the cloud layer and above because of entrainment”: this can’t be true because entrainment would mix warm air down into the boundary layer from the free troposphere above, so must surely warm the cloud layer?
8. Line 322, “Turbulence can be so strong that it dries the lower mixed layer”: I don't understand these turbulence increments. Do you have an explanation for why the turb moisture increments are so variable between these 3 times, while turb temperature increments always warm? I would expect sea surface fluxes surely to both warm and moisten? I also don’t understand what you mean by "so strong"? Strong turbulence could drive large surface moisture fluxes that would then moisten strongly.
9. Line 337, “stratocumulus maintenance depends on …(3) cloud liquid diffusion by the turbulence scheme”: but this is acting to deplete the stratocumulus cloud, not maintain it
10. Line 343, “This change reduces the …LWP…mainly due to weaker cloud liquid tendencies from …the turbulence …scheme”: given turb removes liquid water then weaker tendencies would surely increase LWP!
11. Line 432, “The specific humidity biases may be due to spin-up, during which moisture is not efficiently transported by turbulence”: but Fig 7e shows turb is drying the subcloud layer so I don’t see how this moist bias can be due to inefficient turbulent transport? Unless the turbulence scheme initially moistens the sub-cloud layer (which is what I would expect it to do over an ocean surface), in which case why does it stop by hour 6, as shown?