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Interactive comment on “Near-surface diurnal warming simulations: validation with high resolution profile measurements” by B. Scanlon et al.

Anonymous Referee #2

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This paper describes a comparison of very high resolution temperature profile time series with 5 different versions of an ocean mixing model. The model is the Kantha-Clayson the closure model. The original model and various combinations of modified turbulent and radiation absorption profile parameterizations. The observations are truly high resolution (cm scales) in the upper 5 m of the ocean; time resolution is roughly 10 per profiles per hr.

The paper is reasonably well-written. The introduction is good and the model and observation descriptions are mercifully brief but sufficient. The authors have chosen three cases to illustrate three different regimes: mid-day heating, early morning heating, and

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night-time cooling. I find the analysis somewhat cosmetic – basically a description of the graphs with little physical insight into what is going on. The approach to the graphics could likely be improved to aid the reader. I am not sure the analysis approach is well-conceived. It would be more informative to me to see an entire 24 hr cycle in one simulation for several different forcing situations. However, I presume this was not observationally practical. For example, the 3rd case is classified as ‘day-night transition’, but the models start off too cold by 0.5 C. Presumably this could be due to not producing enough warming during the day? Also, I don’t understand how the models can warm 1 C past the data while the sun is down. Since there is not other source of heat, the warming can only be caused by mixing or advective components not accounted for by the model – yes? The authors incorrectly state this warming occurs after sunrise but it looks to me (a little hard to tell from the poor graphics) the warm occurred well before sunrise. Even if the data are more time limited, it might be amusing to see the entire 24-hr model run.

On balance, though, I think this is an interesting paper and valuable look at model sensitivities with some unique observations. I recommend publication with some work to improve the graphics.

Here are some specific points.

*P 3854 line 8. It is important to note that this study evaluates the ability of the models to exactly reproduce a specific realization of observations. It does not address a more traditional statistical evaluation of model uncertainty. *P 3856 description of the two temperature profilers. The authors could remove some of the un-needed detail such as the sleeve is made of neoprene or the tether is high breaking strain. Suggest reading the section and paring out stuff the reader is not likely to need to know. *Page 3857 line 4. The text refers to ‘both cruise’ but I thought there were 3 cruises.. *Page 3857 line 4. I question the use of the Paulson and Simpson solar flux formula. Wick published a paper previously that suggested it contains highly absorbed bands that do not make it through a humid atmosphere. * Page 3859 line 29. The observations used to initialize

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the models – are they CTD? Do they get close to the surface? * Page 3857 line 4. I don't understand this part about 'shifting to the left'. *Fig. 2. Is quite interesting but hard to puzzle out. It would be useful to see the solar envelop on this same figure. Also, would be nice to see the actual model outputs, perhaps over a longer period. Perhaps make this 3 separate figures with 2 columns each? *Figs3,4, 5. I am confused by the dot on these graphs representing the surface value? Is this the upper layer of the model minus a cool-skin corrections? Does the AERI value play are part in this? *Fig. 6. The caption says this profile is 'normalized' to the mean SSTSkin value. Is that from AERI? Also, I think of normalized as divided by; perhaps 'referenced to' is better? *Fig. 7. What are these profiles referenced to?

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