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Interactive comment on “A statistical model for sea surface diurnal warming driven by numerical weather prediction fluxes and winds” by M. J. Filipiak et al.

Anonymous Referee #3

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This paper describes a model of the statistics of satellite SST diurnal warming in terms of NWP outputs of net surface cooling, solar heating, and wind speed. Two reviews of the paper have been submitted. I have read the paper and the reviews and my comments follow.

Regarding the reviews. My own opinion is in complete agreement with Reviewer 1 who felt the paper needed much more work. I also found the paper very hard to follow. The authors seem to prefer a thousand words when one equation would suffice. I even found the title misleading. It does not appear to be statistical model of the diurnal warm layer; rather, a model of the statistics of the layer. This appears to have been

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misunderstood by reviewer 2 who (his point 4) suggested explicit comparisons against several days of observations. However, buried on p1513 the authors state 'We do not compare the model pt-by-pt to observations: it is a statistical model only'. It looks like some innovative thinking has gone into deriving the distributions, so I think the paper is worth publishing.

A few other specific comments. *Reviewer 1 suggests the motivation section relies on too many unpublished references. I think this section is adequate but suggest that the authors might consider doing a good, thorough job. They also claim they will review the state of art in other models. I found this review to be cursory and not well thought out. One model is distinct from another because it has a 'more complex approach ...'. My opinion is the review should be dropped if I have to look up the references anyway. *I think (but cannot verify because I am on an airplane) that SEVRI is a GOES and AMSR-E is a polar orbiter. Perhaps a sentence or two of background would help the non-satellite people. *Eq 1 is dropped out of the sky with the justification that they did a lot of experimenting and like how it fits. I note Fairall et al use physical arguments to show that

Depth proportional to $Q^{3/2}/\int W^2 dt$

Which differs substantially from (1). Some reassurance that the authors' expression works better than this would be helpful. Preferably with some plausible arguments. *Section 3.4 is completely inadequate and almost incomprehensible. A few equations showing the poor reader precisely what was done is essential. Why should I have to guess or derive it myself? *The discussion of Fig 8 glosses over a major problem: the obs give minimal warming in the stratus/subtropical bands in the southern hemisphere but the model gives a strong max. Please explain. *Not sure the authors want to claim the POSH or ZB models under or over-estimate the heating because they don't agree with their model in an idealized test. This would be a much more useful test if some 'truth' were available.

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Interactive comment on Ocean Sci. Discuss., 7, 1497, 2010.

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