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Ocean Science Discussions

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Interactive Comment

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.

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Reply to comments of Anonymous Referee #3 Ocean Sci. Discuss., 7, C496–C498, 2010

> 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 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.

The model does give a value for the warming as a function the the wind and heat flux, but, as we have pointed out, it is only possible to compare the statistics of the model predictions with the statistics of the observations. We will change the title to (probably) 'An empirical model for the statistics of sea surface diurnal warming' and make its limitations clearer in the introduction.

> 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 *Reviewer 1 suggests the motivation section relies on too many unpublished references.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.

Our intention was not the review the whole field but highlight models of similar application. We will reduce this sections and refer to the recent, thorough review of Kawai and Wada (Kawai, Y. and Wada, A.: Diurnal sea surface temperature variation and its impact on the atmosphere and ocean: a review, Journal of Oceanography, 63,721-744, 2007).

> *I think (but cannot verify because I am on an airplane) that SEVRI is a GOES and

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AMSR-E is a polar orbiter. Perhaps a sentence or two of background would help the non-satellite people.

Yes, more information will be provided. SEVIRI is an infra-red imager on the Meteosat Second Generastion satellite in geostationary orbit over approximately 0W. Images are recorded every hour over the Atlantic and Mediterranean (50S-50S, 50W-50E) and SST is derived from the 11 and 12 micron channel radiance observations. AMSR-E is a microwave imager on the NASA Aqua satellite in polar orbit. It observes most of the globe twice per day at approximately 0130 and 1330 local time.

> *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 Qˆ3/2/Integral (Wˆ2) dt which differs substantially from (1). Some reassurance that the authors' expression works better than this would be helpful. Preferably with some plausible arguments.

We decided to put the model formulation near the beginning of the paper with the detailed derivation afterwards (in an early draft it was in an appendix!). The reasoning that led to the chosen functional form is scattered throughout the paper and will be brought together with Equation 1. But essentially it is as you say: a lot of experimenting with fitting of the data. We only to the basic assumption that the warming D would vary directly with some function the heat flux Q and inversely with some function of the wind speed W.

We could use the Fairall model, or the POSH model which is a further development of it, to derive the probability distribution of warming from the NWP winds and fluxes and then compare those with the probability distribution of observed warming. Instead we have found an empirical model (Eqs 1 and 2) which will reproduce the observed distributon, and compared the results of this empirical model with other models for an idealised test case (Section 5). And if (say) POSH's results differ from the empirical model results in the test case, then the probability distribution of warming derived using

POSH is not likely to be the same as the probability distribution of observed warming.

(You mean, *Warming* proportional to Qˆ3/2/Integral (Wˆ2) dt)

> *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?

Section 3.4 is the 'Methods' section and had to be detailed enough to allow others to reproduce the results. Evidently this has made the section difficult to understand and it will be revised in the re-submitted manuscript

> *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.

We have no ready explanation. We don't expect a clear sky bias (which would bias the model results high) in the model since it has a net heat flux dependence. There may be systematic errors in the fluxes or winds but they should cancel out since we use the same NWP model to derive the model and to drive the model for this AMSR-E comparison. And note that the northern hemisphere sub-tropics do not show this effect.

> *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.

Our contention is that the statistical model reproduces the statistics of the observed warming and so can be a surrogate for actual observations and under idealised conditions would also give the correct statistics. So it can be used to directly test other models in idealised test cases. However, since the staistical model is still a model itself, we should change the phrasing in this section to '... ZB model has similar overall magnitude to the statistical model, but *it predicts larger warming* at moderate wind

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speeds ...', etc

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