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Ocean Science Discussions

Interactive comment on "Arctic surface temperatures from Metop AVHRR compared to in situ ocean and land data" *by* G. Dybkjær et al.

Anonymous Referee #2

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General Comments

This paper addresses an important area of development, namely ice surface temperature (IST). I agree with the authors on the potential significance of a IST product viewed as usable as a boundary condition in NWP, much as SST presently is. I would have like to have had more discussion of this nature, about the context and possible uses in NWP in this paper. The authors state there is "a potential for improving model predictions", but how? It is probably harder to achieve than for SST, because I suspect the radiometric temperature of ice is more closely coupled to air temperature, and so prescribing IST from observations (as is done for SST) could have negative impact. So, I think this aspect of the introduction could be strengthened – perhaps give more context from the Stammer reference.

IST has been determined from AVHRRs before (by Comiso, for example) and for MODIS (part of the LST product), so what is the particular contribution of this paper? There is some new validation data collected and matched to Metop AVHRR, which is nice since there are >20000 points (although possibly only ~2000 independent points if I understand correctly that multiple satellite points go with each in situ). But the algorithm is not a new formalism (it is a conventional split window) and (although I find this hard to believe) doesn't use coefficients designed for Metop but for NOAA12. To me, this makes the paper premature. To make this work, there needs to be a way successfully define the retrieval coefficients for different sensors. The authors do define new coefficients by regression to the in situ, but then "validate" these against the same in situ. Of course, this does not prove that successful coefficients can be defined from match ups since by design the results improve when applied back to the data from which they arrive. The good results found are not convincing evidence that such results can be representative of the true errors.

I encourage more work on this topic, for sure. But a significant advance would be to report more than the collection of new data points and application of an old algorithm. Here are some proposals to make the paper of real significance: 1. define Metop-specific coefficients by regression against routinely available in situ data that could be routinely applied without special field campaigns, and then test this against the new data matched in this study to give an independent test of the retrieval performance, OR (and probably better) 2. define Metop coefficients by radiative transfer modelling and show the degree to which this works, OR (best) 3. move on to cutting edge algorithms like optimal estimation. In any case, something different than using coefficients from the wrong sensor.

Specific comments

Is ECMWF 2 m the right comparison to the IST? There is an ECMWF skin temperature field that is probably more like-for-like? Also, show the difference field rather than side by side. I had to look carefully to see there are actually very big differences.

The scatter plot figures would be much easier to view if plotted, first of all, square, and second, with the same axis range on the horizontal and vertical.

Your bias was -3 K. Was the Hall bias mentioned +2 K, as written, or is it also negative, which it seems to read like? Although an agreement in bias would be a coincidence if using coefficients from a different sensor.

Interactive comment on Ocean Sci. Discuss., 9, 1009, 2012.