

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

Anonymous Referee #1

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Review Dybkjaer et al (os-2012-30) “Arctic surface temperatures from METOP/AVHRR compared to in situ ocean and land data”

General comments

The authors have defined an operational method to derive Ice Surface Temperature (IST) from METOP/AVHRR over the Arctic region. They have gathered validation data from buoys and ship measurements and from two sites: in inland Greenland (air temperature measurements) and in a fjord in NE Greenland. This is an interesting paper, addressing a challenging task and which should be published after the authors have accounted for the remarks below. My main concern is the IST algorithm they use which has been developed for NOAA12 and is certainly not optimal for their purpose. I suggest that in a future work they use NWP model profiles, both for improving the retrieved

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IST accuracy and their cloud mask.

Specific comments

P.2, line 27. The authors should be more precise in their description of the existing products: are they not available in real time? or available in real time but not reliable? or available in limited historical time series, etc.. A quick review of the methods used in the MODIS or the AVHRR case would be also quite useful.

P.5, line 10. Thresholding T11 temperatures to determine ice free, marginal ice zone and ice covered areas is only a gross rule that shows many exceptions. Ice areas in summer can be observed in summer with T11 around 0 C, due for instance to air temperature inversion. Did the authors envisage more efficient methods (Bayesian approach?).

P.5, line 17. The IST algorithm is formally identical to a classical split window equation, could the authors briefly recall how the coefficients have been determined? The use of a IST algorithm defined for NOAA12/AVHRR and used for METOP/AVHRR is a very questionable solution. A better approach could have been to redefine an algorithm and its coefficients by using simulated brightness temperatures applied to arctic atmospheric profiles, as this is currently done for SST algorithms (see below). I understand the authors are pioneering a validation experiment, but adopting NOAA12 coefficients is clearly taking a risk.

P.5, line 26: The reference for the EUMETSAT OSI SAF AVHRR SST algorithm should be: EUMETSAT (2010) Low Earth Orbiter Sea Surface Temperature Product User Manual, SAF/OSI/CDOP/M-F/TEC/MA/127 (<http://www.osi-saf.org>).

P. 6, line 12: Since the authors have access to NWP ice temperature (NWP_{surface}) as explained in section 4.2, why did they choose to compare MAST results with NWP 2m air temperature (NWP_{2mT})? For non Arctic specialists, they should explain here (rather than at the end of the paper) the ice versus air temperature relationship?

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P.8, line 24: The adjustment of coefficients on 4 days of data is very far from ideal and will lead to a very locally adapted algorithm.

P.10, line 6: The acronym table should be introduced earlier in section 1.

P.10, line 8: I do not understand what represents the number of cases (20000 cases). The authors use a 4x4 pixel box and in this box each of this pixel is accounted for individually. Are those individual pixel values compared independently to the in situ measurements? Did the authors try any mean or median value in the box?

P.10, line 18: The MU summit (air temperature?) data are used in the validation experiment: Using air temperature is surprising and should be justified (already mentioned above).

P.10, line 22: The disappointing results of the re-calibrated algorithm is due to the fact that it is narrowly specialized for the location and the time period of the ISAR experiment.

P.10, line 29: An improvement of the agreements with the MUisar datasets is no surprise, for the same reason.

P.11, line 10: The figures obtained with ISAR measurement are quite interesting. Do the authors think they are representative of what could be obtained with an adequate algorithm, a good cloud mask and a reliable in situ. In other words, is this the potential accuracy of a TIR based IST method?

P.20, line 21: The Diurnal cycle shown in figure 3 and 6 is impressive and surprising, at least for non arctic specialists. Can the authors give an indication of the amplitude of this diurnal warming, since it is not easy to infer from the figures.

P.11, line 26. It is difficult to understand how the same split window algorithm can provide atmospheric correction at sea surface level and at 3200m altitude, where the atmospheric absorption should be much lower, could the authors comment on that?

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P12, line 3: The comparison of OBSsummit and NWP2mT is not quite clear: the bias is small but the standard deviation large; how do this fit with the author's explanation of OBSsummit being assimilated in the model?

P13, line 27: There must be errors also linked to the algorithm itself, even though I agree the error trend with satellite zenith angle is encouraging. This error is illustrated by the fact that the original algorithm showed a negative bias of -1.81 K against ISAR measurements according to table 2. This algorithm linked negative bias probably contributes the negative biases recorded in table 3.

P14, line 1: This discussion should have been introduced earlier (in section 4.1 for instance)

P.14, line 14 and p.16 line 16: I am surprised that the authors envisage to "recalibrate" their algorithm with in situ measurements, knowing how scarce good matchups conditions are in the Antarctic. In my opinion this is clearly a weakness in the authors' approach. My recommendation would be to use a NWP model based approach to determine an optimal retrieval algorithm. To do that the authors could use either radiosonde profiles or NWP model profiles, and build up a simulated BTs by using a fast radiative transfer model such as RTTOV and realistic surface temperatures (see Francois et al. RSE 2002 or Merchant and LeBorgne JAOT 2004) . Similarly they could introduce model profiles in their operational retrieval scheme; this would guarantee a better adaptation of the atmospheric correction to actual atmospheric conditions (including altitude effects). they could either adopt a full Optimal Estimation technique (OE, Merchant et al. RSE 2008), or a Bias Correction (BC) method (Le Borgne et al RSE 2011, Petrenko et al. RSE 2011). These methods are now currently used in SST retrieval and I do not see why they cannot be adapted to IST retrievals, providing a correct ice emissivity model is available. Since improving the cloud mask is an other challenge, comparing the true IR and the simulated IR values gives a good indication on how cloud contaminated is the pixel (if not using a full Bayesian method)

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