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The Cryosphere
Discussions

Interactive comment on "Near-surface thermal stratification during summer at Summit, Greenland, and its relation to MODIS-derived surface temperatures" *by* Alden C. Adolph et al.

Anonymous Referee #2

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General Comments:

The authors present results from a new field campaign near Summit Station, Greenland conducted between 8th June and 18th July 2015. This campaign data is used to investigate near-surface temperature inversions at the site and validate MODIS MOD/MYD11 Collection 6 products. Overall this is an interesting paper which presents original results from a short field campaign as well as from validation of MODIS collection 6 products over Greenland. The manuscript is generally well-written and the methods used are appropriate.

However, I feel that there are issues which need to be addressed by the authors in

redrafting this manuscript. There are also sections of this manuscript which would benefit from a tightening of prose and improvement of structure. Both are elaborated on in the specific comments.

Specific Comments:

It was hard to ascertain the new and original contributions that this manuscript provides to current scientific understanding at first. Outlining these in the introduction or similar would aid reader understanding.

Line 14: suggest "can be assessed" or similar rather than "are assessed" as satellite derived temperatures over land are not in common use for this yet.

Line 53-54: The section starting "however, satellite remote sensing" should mention that the focus of this manuscript is on thermal infrared remote sensing, rather than microwave remote sensing, of surface temperatures. It should also be mentioned that thermal infrared remote sensing observations are affected by cloud cover. The sentence currently gives the impression that the satellite derived surface temperatures are spatially and temporally complete.

Line 60-62: "inversions... which may cause a disparity between the 'surface' temperature at 2m and the actual skin temperature of the snow surface". This sentence currently gives the impression that there is uncertainty about whether inversions cause a disparity between skin and 2 m air temperatures over snow and ice. Yet, the references cited in Section 2.1 note both the presence of inversions in these areas and their effect on temperature stratification. In a related issue, the manuscript could be read as suggesting that the issue of inversions (or other causes of disparity between 2m and skin temperatures) in validation of surface temperatures from thermal infrared remote sensing has not previously been considered. However, various previous studies have noted the issue of the difference between skin and 2 m air temperature over snow and ice surfaces in relation to remote sensing of surface temperatures and their validation. The effect of inversions on the difference between skin and 2 m air temperature over snow and ice surfaces has been noted by e.g. Comiso et al, 2003; Yu et al, 1995. There are also other reasons why differences may be seen between these two temperatures over cryospheric surfaces under clear sky conditions e.g. ice crystal precipitation (Yu et al, 1995) and latent heat effects (Wiese et al, 2015). As a result it is recommended that validation of satellite surface temperatures over land areas is done with situ surface temperatures (ideally from ground-based radiance measurements) if possible (Guillevic et al, 2017). In light of these previous studies and recommendations, this sentence at least should be rewritten and references included in the manuscript to give credit to previous work looking at the issue of the difference between skin and 2 m air temperature over snow and ice surfaces in relation to validation of remotely sensed surface temperatures.

Lines 66-73: Does question b) refer to the specific in situ sensors you use, or to the use of in situ skin temperature data for validation in preference to air temperatures? I could not find these questions referred back to in the results or conclusions. Would suggest removing these or referring back to them later in the manuscript.

Section 2: The content of the background section was informative and interesting. However, in combination with the introduction and methods I found the structure of the manuscript a little hard to follow here. This section would benefit from a rewrite, or a restructuring. Some suggestions follow. The content of the surface temperature inversion section would provide a nice introduction to the issues raised in lines 55-65. The Remote Sensing of Surface Temperatures section mostly focuses on MODIS rather than remote sensing of surface temperatures more generally. The content relating to MODIS products could be moved to Section 3.2 or the section could be renamed.

Line 215 to 220: these metrics are in very common use so the equations do not need stating unless there are notable differences from how they are commonly applied.

Line 241: There is very little discussion relating to figure 3. Reconsider including this figure or provide more discussion of the results shown.

Section 4.1: I found this section quite hard to read and understand. Please rewrite or restructure. Also, please include some values for the differences (bias, RMSE, etc.) noted between the in situ sensor surface temperatures to provide context to previous study comparisons (lines 304-329).

Lines 246-260: Given the issues noted with the thermochrons, please reconsider including these or provide some comment on whether these sensors are suitable for measuring surface temperature. If the authors decide to retain the thermochron analysis, in Figure 4 the difference between the thermochrons and the other sensors increases noticeably after day of year 167 but before they are buried by snow. Please provide some comment on this.

Lines 266-267: “The measurements show”. Ambiguous as to whether this refers to the measurements affected most by solar heating or other observations.

Lines 304-318: Did Hall et al. 2008 use the same infrared radiometer as in the study detailed in the manuscript? Table 1 suggests this study only looked at 2 m air temperature.

Line 317-318: Sentence is ambiguous as to whether the need for future studies refers to the results of your study specifically, or in general.

Lines 323-324: Is there publicly available snow depth information at these sites to address this question if it is not included in the paper?

Lines 324-329: Why was the thermocouple data not compared to MODIS? If there is a reason this should be stated.

Lines 353-359: Do the authors have any thoughts on what else could be causing the remaining differences? If so please include this.

Lines 366-368: I think the authors say “improving the cloud mask” when they mean increasing cloud masking strictness which may improve the product but also overflag cloud so that there is loss of un-cloud contaminated data (“reduce the amount of measurements available"). Data loss due to cloud masking, assuming that the pixels removed are genuinely cloud contaminated and the cloud masking is therefore accurate, is not a problem as these pixels will not contain sensible infrared surface temperature estimates. The issue is when there is significant over-flagging of cloudy pixels, leading to loss of non-cloud contaminated data, due to an increase in cloud masking strictness. If so these sentences (and lines 395-396) should be re-written.

Line 384: Do not use the word "correct" here as this suggests that MODIS is perfectly accurate, when actually MODIS data (and indeed any observation) will not measure the true (generally unknown) value of the surface temperature. There are always biases and uncertainties when measuring.

Lines 390-391: Sentence unclear in meaning. Are the authors suggesting using MODIS data and in situ 2 m air temperature to study inversions? If so, are the biases between the MODIS and in situ skin temperatures understood adequately to allow such a study? Lines 353-359 suggest not.

Table 1: This table could do with a little restructuring and/or reduction of text as it is currently a little difficult to read and understand. Also, if this is for studies over land ice only please include this information in the caption.

Technical Corrections:

Line 47: "for understanding ice sheet…" rather than "of understanding ice sheet…"?

Line 53: remove extra space in "Fausto et al., 2012) ; however"

Line 534: remove curly brackets

Figure 1: The location dot is a little small. The north arrow is also a little difficult to see.

Table 1: missing "n=" on last row.

References:

Comiso, J. C. (2003). Warming trends in the Arctic from clear sky satellite observations. Journal of Climate, 16(21), 3498-3510.

Guillevic, P., Göttsche, F., Nickeson, J., Hulley, G., Ghent, D., Yu, Y., Trigo, I., Hook, S., Sobrino, J.A., Remedios, J., Román, M. & Camacho, F. (2017). Land Surface Temperature Product Validation Best Practice Protocol. Version 1.0. In P. Guillevic, F. Göttsche, J. Nickeson & M. Román (Eds.), Best Practice for Satellite-Derived Land Product Validation (p. 60): Land Product Validation Subgroup (WGCV/CEOS), doi:10.5067/doc/ceoswgcv/lpv/lst.001. https://lpvs.gsfc.nasa.gov/PDF/CEOS_LST_PROTOCOL_Oct2017_v1.0.0.pdf

Overland, J. E., & Guest, P. S. (1991). The Arctic snow and air temperature budget over sea ice during winter. Journal of Geophysical Research: Oceans (1978–2012), 96(C3), 4651-4662.

Wiese, M., Griewank, P., & Notz, D. (2015). On the thermodynamics of melting sea ice versus melting freshwater ice. Annals of Glaciology, 56(69), 191.

Yu, Y., Rothrock, D. A., & Lindsay, R. W. (1995). Accuracy of sea ice temperature derived from the advanced very high resolution radiometer. Journal of Geophysical Research: Oceans (1978–2012), 100(C3), 4525-4532.

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