

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 #1

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General comments:

The authors present 40 days of near-surface air and surface skin temperature measurements at Summit station Greenland. They analyse the datasets with focus on identifying the timing and magnitude of near-surface thermal inversion. They compare their best measurements of surface skin temperature, from IR upward radiation, to the latest MODIS Land Surface Temperature (LST) and find smaller bias than in other studies validating that product.

The skin temperature is a major variable in the surface energy balance of snow and ice surfaces and has drawn little attention so far. Product such as MODIS LST is a

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valuable tool for quantifying that variable but needs ground validation before it is used for the whole Greenland ice sheet. The authors therefor address an important question in a well written study using adequate tools and methods.

However, the study suffers at the moment from the lack of temporal and spatial coverage. Indeed one can ask if these 40 days of observations are representative of the surface conditions on the ice sheet and adequate to validate the MODIS LST as a whole. Especially, it is unfair to compare a 40-days comparison to previous studies that all use multiyear or spatially distributed datasets. As far as I know, NOAA can provide suitable data (IR skin temperature) for a longer period at Summit and PROMICE stations for sites at lower elevation.

Additionally the study could also benefit from more in-depth definition and discussion of all the measurements (f.e. how is calculated MODIS LST, effect of surface emissivity and of the air below the sensor for IR radiation measurements . . .) and concepts (what is skin temperature, how is defined thermal inversion, which indicators and metrics are used to describe it, which are its drivers. . .) that are being used.

Eventually the use of unshielded thermochrons and thermocouples is highly sensitive to radiation (even more during midnight sun period) and should not be used without appropriate segregation of the erroneous periods or correction of radiative heating. At the end of the study, the authors even discard these measurements and only compare MODIS to IR surface radiation. I would recommend greatly minimizing the description of that data and any conclusion that derives from it (f.e. statement regarding inversion in the 5 cm above the surface).

Technical corrections:

l. 19: Define NOAA. Define again in main text.

l.84: Please only mention the conclusions from Good (2016) relative to snowy surfaces.

l. 100: “at but” word missing?

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I. 105: It is said” Berkelhammer et al. (2016) discuss the impacts of the surface-based temperature inversions (with 2 m air temperature as the base)”. I can see in Berkelhammer et al. (2016) that they have measurements down to 20 cm. Please adjust or justify.

I. 114: “There are”, there is

I. 119: The equation points at the various variables that need to be calculated before the surface temperature can be determined. Please give a brief description of how it is being done for MODIS LST and how accurate is that calculation.

I. 126: “its products are chosen as the remote sensing product” rephrase

I. 130: “A number . . .” here validation studies are described. It seems to be the same topic as the next paragraph so consider merging them. Also you discuss validation of MODIS products before you actually describe what these products are and how LST is being calculated. Consider rearranging paragraphs in this section.

I. 134: You mention the recurrent confusion between near-surface, 2 m air temperature and surface skin temperature. Please define your vocabulary at the beginning of the manuscript and then be specific every time temperature is mentioned. Here what does “ground surface temperature” refer to? Check other cases throughout the manuscript.

I. 140: You mention a cold bias in Østby et al. (2014) but it is not reported in Table 1.

I. 144: “A bias in the data can obscure or alter trends within a dataset.” Very general statement, consider removing.

I. 145: “Furthermore, it is possible that. . .” at this point, you have not exposed any theoretical (potentially from how MODIS LST is being calculated) or empirical reasons to think that one of the 2 m or skin temperature would match better than the other to the MODIS product. Either add a justification or reference for this hypothesis or move it to what is being interpreted from your validation.

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I. 148: What do you mean by “standard”?

I.154: “Both MOD/MYD11 and the preliminary version of the MOD29 special product were compared to our in situ data”. If both have been compared and validated then both should be presented. The conclusion that a LST product developed for land performs better than a LST product developed for ice on the Greenland ice sheet is a very important conclusion. Quantifying and localizing the errors in MOD29 could be important for future studies. You can also validate only MOD/MYD11 but then don't mention that you validated MOD29 but not show the result.

I.156: In this paragraph, the authors should provide a clear and concise description on how LST (and all necessary variables such as water vapour, air temperature, emissivity, and cloud cover) are being calculated. It is important so that the reader can be reminded of the assumptions and uncertainties linked to the calculation and to which level of accuracy can be expected from the product.

I. 160: “feature” not clear what it refers to.

I. 161: “Over snow and ice, this. . .” Not very clear, please rephrase.

I. 165: In this paragraph, please quantify the difference between C5 and C6. More especially, how much of the cold bias seen in validation studies can be explained by the defects of C5 products?

I. 171: “decrease in measured brightness temperatures” define brightness temperature or use vocabulary previously defined. Does this sentence imply that the corrections in C6 would lead to even colder bias if compared to the previously mentioned validation studies?

I. 210: “the pixel that has the minimum distance” a pixel is an area so the station should be located within one at all time. Provide pixel size at some point in the manuscript.

I. 213: “the nonsynchronicity may introduce some error to the comparison” It seems that the IR radiation comes is recorded at 5 min interval and 2 m air temperature at 1

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min interval so this nonsynchronicity error could be removed by taking the measurements that are within few minutes of the MODIS acquisition time. Even if it is random noise, removing that error could potentially show better match of MODIS LST with ground measurements.

I. 246 “Several different” redundant

I. 247 “in order to compare this study to previous [...] studies” I assume the main goal was to validate MODIS, comparing to other validation studies comes after. Maybe rephrase.

I. 255 “In Koenig and Hall (2010) ...” the following sentences should be in the methodology where the measurements and their expected limitations are being presented. Additionally Hall et al. (2015) seem to conclude that unshielded thermochrons are subject to measurement error. So why using/presenting that data at all? The use of unshielded thermocouple is subject to the same issue.

I. 270: “differences are much higher at lower wind speeds” Unfortunately, conditions favourable to inversions are also the one enhancing radiative heating of both thermochrons and thermocouple: low wind speeds at 2m imply even lower wind speeds at the surface and will hamper ventilation of the sensors leading to sustained radiation absorption even with low sunlight. Better information should be given to ascertain that this difference is due to inversion.

I. 274 “similar” quantify

I.275 “larger” quantify

I. 280 “increased discrepancy” seems redundant to what is said in the previous sentences. Also quantify here.

I. 281 “most frequently” quantify.

I.289 The following paragraph brings in discussion about MODIS surface temperature

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when we are still in the ground measurement section. Consider moving to the next section.

I. 294 “Previous studies acknowledge that near-surface stratification may be part of the cause of the discrepancy” this is very important and has not been mentioned clearly in the introduction. How did these studies arrive to these conclusions?

I. 311 “In the summer. . .” and following. Can you be more quantitative, f.e. binning observation into night and day time and showing that the mean differences in each group are statistically different. Maybe also find a cut off value in radiation below which you have significantly stronger inversion.

I. 319 You are just repeating what is said about Good (2016) in the introduction.

I. 322 not clear what “median difference” with a +/- sign refers to.

I. 332 Can you specify what is the average solar radiation and zenith angle during that time.

I. 335 Since you aim at validating MODIS LST, better or worse match with it is not a good reason for discarding ground measurements. Consider quantifying the error of the thermochrons using IR temperature instead. If these measurements were not considered reliable enough to validate MODIS LST, why are they being used to quantify the thermal inversion earlier in the study?

I. 335 please give mean bias along with RMSE

I. 350 please give mean bias along with RMSE

I. 356 please state at which viewing angle the error is maximal and quantify that maximum error

I. 360 Why not comparing MODIS to the thermocouple data that you also presented earlier? If, as for the thermochrons it is considered as erroneous measurements it should also be stated and in that case it shouldn't be used for quantifying near surface

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inversion earlier in the study. Also consider comparing MODIS with the 2 m temperature over the study period and see if you find same bias as previous validation studies using 2 m temperature.

I.362 Along with the present discussion, it would be important to analyse the performance of MOD35 using the MMCR (false positive, false negative) to draw more broad conclusions (also using findings from Østby et al. (2014)) about when and where MODIS LST might be wrong because of cloud cover.

I. 381 “5 cm nearest to the...” this conclusion comes from potentially heated thermocouple. Needs to be mentioned.

I. 387 “the lower RMSE is likely ...” here you make a hypothesis when you could actually show it. Is your RMSE greater when comparing MODIS to 2 m temperature? Please discuss mean bias along with RMSE.

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