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The Cryosphere Discussions

Interactive comment on "Comparison of MODIS-derived land surface temperatures with near-surface soil and air temperature measurements in continuous permafrost terrain" *by* S. Hachem et al.

Anonymous Referee #2

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The paper by Hachem et al. compares MODIS LST with in-situ air and soil temperature measurements in the continuous permafrost domain in Alaska and Quebec. In the beginning, the authors state that the paper does not intend to deliver a validation of MODIS LST, as in-situ measurements of radiometrically measured surface temperatures are not presented. While the authors have compiled an extensive data base, the paper lacks focus and the authors fail to answer the questions what the scientific motivation for the performed comparisons is and what can be learned from their results with regards especially to permafrost science (the paper is part of a special issue on permafrost). Furthermore, the paper is rather lengthy and features a number of cumbersome passages, which further obscure the focus. I challenge the authors to come up with a revised version that is shortened by at least a few pages (in the TCD version).

General Comments:

1. Comparison of MODIS LST to soil temperatures: The authors must explain why this comparison is performed at all for snow-covered ground. The insulating effect of snow on the ground with much warmer soil temperatures than surface temperatures has been described extensively in literature (e.g. Goodrich 1982). If the authors wish to examine the hypothesis that skin temperature is a good representation for soil temperature at 3 to 5cm depth in the yearly average, the clear answer from the data shown and the MDs of several Kelvin is that this hypothesis is false. But this is not surprising and does not justify a publication. On the other hand, the comparison of summer temperatures (snow-free ground) makes sense. I would suggest to rename soil temperature to “ground surface temperature (GST)”, and have an introductory statement, that near-surface soil temperatures are a good representation of GST, at least as good as it mostly gets in practice. Then the connection to PF models, where GST plays a role is much more evident. When comparing MODIS LST to GST, I would not call a MD of several Kelvin for a specific site a “good agreement”.

2. Comparison to air temperatures: A number of studies from polar areas (e.g. Hall et al. 2004, Scambos et al. 2006) have mentioned that air temperature is somewhat different from skin surface temperature. This literature should be acknowledged. Nevertheless, air temperatures are used for validation of LST products (Hall et al. 2004), but the authors state that such a validation is not the purpose

of the paper.
In permafrost (PF) science, air temperatures have long been used to drive both simple and more sophisticated permafrost models, but mainly because air temperatures are often the only available data sets. The authors mention themselves that surface temperatures would be better suited as input for PF modeling , so it is not clear why a comparison of MODIS LST to air temperatures is helpful. The relevant question in the context of PF modeling is whether an accurate average surface temperature (or even better GST) can be inferred from MODIS LST, and this can not be answered by comparing to air temperatures (since the air temperature is different from the surface temperature, which again could be different from MODIS LST due to subpixel variability, measurement errors, clouds).
To summarize, the authors should present a more concise picture, what the scientific purpose(s) of the comparison to air temperature in this paper is.

3. I suggest to use the data to compute the freezing and thawing indices (FI and TI), which are employed in simple permafrost models (e.g. in Hachem et al 2009). This would essentially mean reworking the analysis of seasonal averages, but the connection to PF would be much more obvious. For most of the stations, FI and TI could be calculated from GST, air temperature and MODIS LST. Furthermore, the authors could calculate n-factors for MODIS LST for freezing and thawing (i.e. FI(measured GST)/FI(MODIS LST)), and e.g. examine whether they are constant over several years. While n-factors are certainly not the most sophisticated apprach in PF modeling, they are easily calculated and well suited for large-scale applications. Using the air temperatures, the authors could compare the n-factors for air temperatures and the n-factors for MODIS LST. This way the relatively large data basis on n-factors (which are generally based on air temperatures) might become useful for PF modeling based on MODIS LST.

4. The authors could scan their data set for strongly different LST values (from air

temperatures), which would quantify the amount of MODIS LST measurements where the MODIS cloud detection fails. This is a potentially important error source which the authors mention several times without actually quantifying it. I think the authors could deliver a stronger paper if they attempt to quantify this error source.

Detailed comments:

p. 1584

The abstract should be shortened, particularly the first very general part.

l. 23: The paper does not deliver a proof that surface heterogeneity is the reason for the deviation. In the results section, surface heterogeneity is rarely mentioned, and an analysis of the impact of surface heterogeity on LST is not performed.

p. 1585

l. 2: cite the IPCC report

l. 8-12: provide citations for all points

l. 13-14: delete the explanation in the bracket, it is not relevant here and clear for most of the readers of The Cryosphere

l. 17-19: very general statement, which I generally agree with, but which requires backing up by literature.

l. 23: The GIPL 1.x model as presented in Sazonova and Romanovsky (2003) does not use surface temperature, but air temperature as input. While it is possible to use surface temperature in the model (or actually even preferable) in place of air temperature, the reference must be changed here.

p. 1586

l. 7: better “spatial” instead of “horizontal”

p. 1587

l. 12: awkward sentence, rephrase

l. 24: cite e.g. Wan and Dozier (1996) for split-window method here

p. 1587-1590

Most of the information given in paragraphs 2.1.1 and 2.1.2 is not needed for the paper. The paper makes use of established and documented MODIS LST products, and the technicalities of the LST calculation are described elsewhere. What I find important: operation period, temporal and spatial resolution, reported accuracy of LST. The split-window method can be mentioned, but does not need to be explained in detail. It says in the title, that MODIS is used, so the lengthy justification in 2.1.2, why MODIS and not AVHRR is used, comes rather unexpected. I guess that one or two sentences about the the main advantages of the "present-generation" sensor MODIS over the "previous-generation" sensor AVHRR could be interesting for many readers, but in the present form, this discussion is excessive.

p. 1590

l. 7: there are generally more than four actual measurements (or overpasses) per day, depending on the latitude of the site, as stored in the MODIS level 2 products. From these measurements, the day and night LST values given on the level 3 product for both Aqua and Terra are selected.

p. 1591

2.2.2 At least Franklin Bluffs and Sagwon (and probably also Betty Pingo) are located near the Sagavonirktuk River, which is not part of the Kuparuk watershed, but a separate, parallel drainage. Please correct!

2.2.2 The other used sites (West Dock, etc.) are not described at all, although their characteristics are at least partly distinctly different from the sites along the Sagavonirktuk River. West Dock is on a small barrier island in the Beaufort Sea!

p. 1592

l. 5-12: This paragraph does not fit to the headline of 2.3. It should be moved to 2.1.

2.3: The description given in this paragraph implicitely assumes, that the LST values given in the MODIS level 3 products correspond to measurements of the 1km-pixel-footprint. This is not the case. The contributing area of the actual measurements is larger than 1km2 and furthermore depends on the scan angle (Nishihama et al., 1997). From these actual measurements (given in the MODIS level 2 products), the level 3 products are calculated by interpolation from selected measurements, but it is no longer possible to explicitely control the footprint here. I do not suggest to use the level 2 products and perfom a detailed footprint analysis (such as done in Langer et al. 2010 and Westermann et al. 2011), as using the level 3 products is far more practical. However, the paragraph should reflect the fact, that this is not an exact footprint analysis and that measurements with e.g. strongly different water fractions (than those given) can be and most likely are contained in the data set, thus probably explaining some of the observed deviations.

p. 1595

l. 20. Delete “first”

p. 1596:

l. 3: delete “whereas”

l. 6-10: Awkwardly phrased paragraph. Better “The overpass times were rounded to a full hour to facilitate comparison with in-situ measurements” or similar.

l.12-20: Move to the corresponding setcion in 2. Data and Methods, this does not fit to Results.

l. 19-24: “There were no near-surface soil temperature measurements available at West Dock, Salluit Airport (SalA), and Kuujjuaq (Table 1).” “MODIS retrieved LSTs over Salluit and the Kuparuk River Basin are well correlated with Tsoil within. . .” Are there soil temperature measurements in Salliut or not?

l. 22: What is meant by “well corellated . . . for either the ascending or descending mode”? Is it the overpass time or the temperature? If it is the latter, the reference to Table 4 is wrong.

3.1.1: I would not call a mean difference of up to 8K for some of the stations a “good agreement”. This is a severe offset and the seemingly good corellation coefficient is simply a consequence that MODIS can represent the seasonal characteristics of LST at a site.

p. 1597:

l. 17: How can R’s and MD’s be calculated for mean LST-daytime, etc.? I though these measures are employed to characterize the relation between satellite and in-situ measurements.

p. 1598:

l. 2: What is meant by “Here, it can be established that the relation between Tsoil and LST with mean LST-Day/Night increases only weakly the relation with R values ranging from 0.86 to 0.96.”?

p. 1599:

l. 3: Please rephrase the sentence “In these graphs,. . .”

l. 4-8: This is a long paragraph to state: “In summer, MODIS LSTs show a diurnal course, while in winter (mostly polar night or low solar radiation) they don’t.”

p. 1600:

l. 19: Here, erroneous cloud detection is mentioned for the first time. This should either become a discussion point or it should be mentioned earlier, when individual measurements are discussed. Tthe authors could also attempt to quantify this error source (see above).

p. 1601

l. 13: rephrase or delete this sentence. It is well known that skin temperature and soil temperature are not equal.

4.1.1 This paragraph describes the well-known fact that soil temperatures form as a result of heat conduction, freezing of water, etc. The insulating effect of the snow cover is e.g. detailed in Goodrich (1982). The authors compare two different physical quantities, namely the soil temperature and the skin temperature, where little can be learned from a 1:1 comparison, at least if a snow cover is present.

p. 1602

l. 24: reference to figure missing

l. 24: not sure what is meant by “heat transfer (radiative cooling) from the ground surface to the air above at night”. I guess the reason for the colder surface temperatures are simply stable atmospheric stratifications and near-surface inversions. The authors should better use “radiative cooling of the ground and formation of an inversion layer”

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Interactive Comment

or similar.
p. 1603:
l. 12: Why give the example of Fairbanks and not for one of the employed stations? Maybe give the the numbers for the northern- and southernmost stations used.
l. 15: What is meant by “according to the shape of the distributions in the plots of Figs. 6–7”? I don’t understand this statement by looking at the figures.
l. 16ff: I don’t see the causality between the two sentences implied by the word “thus”.
l. 23: The formation of stagnant water depends on the hydrology of the site, largely on the surface runoff. The statement is too general. If stagnant water has been observed at the sites, the authors should state where and (roughly) when, and relate it to the observations of LST and air temperature.
l. 24: I don’t understand the reasoning “The presence of the stagnant water modifies the heat exchanges between the soil surface and the atmosphere at the soil interface, which the 2-m height air temperatures do not measure.”
l. 28: 0.66 instead of .66
l. 28: From the figures, it appears as if surface and air temperatures match reasonably well during snow melt (at least better than during winter). The authors should provide the MD’s to back up the statement “During snowmelt the LST better represents the near-surface (melting snow) temperature than Tair.”
p. 1604
l. 17: Why “soil temperature” in a paragraph about air temperature?
p. 1605
l. 10: This is not a conclusion of this study, since no in-situ measurements of the surface (skin) temperature have been presented.
l. 17: That very much depends on the definition of “close”. In my view, MD’s of a few Kelvin are not really close.
l. 21: Although I agree with the statement, I disagree that this conclusion can be drawn from this study. I) monitoring: it is clear that the skin temperature is not a good measure for soil temperature, especially when the ground is snow-covered (compare

Fig. 3). II) modeling: Skin temperature as input for a heat transfer model is a promising approach (as detailed later), but this study does not follow or evaluate this approach, nor does it it investigate whether accurate time series of skin temperatures can be inferred from MODIS LST.

p. 1621 The small numbers “Terra n”, etc. are confusing and make the figure less understandable

p.1624 In both figure captions should state that it is a comparison to measured air temperatures.

References: Goodrich, L., 1982. The influence of snow cover on the ground thermal regime. Canadian Geotechnical Journal 19 (4), 421–432.

Hall, D., Key, J., Casey, K., Riggs, G., Cavalieri, D., 2004. Sea ice surface temperature product from MODIS. Geoscience and Remote Sensing, IEEE Transactions on 42 (5), 1076–1087.

Nishihama, M., Wolfe, R., Solomon, D., Patt, F., Blanchette, J., Fleig, A., Masuoka, E., 1997. MODIS level 1A Earth location: Algorithm theoretical basis document version 3.0. SDST-092, MODIS Science Data Support Team.

Scambos, T., Haran, T., Massom, R., 2006. Validation of AVHRR and MODIS ice surface temperature products using in situ radiometers. Annals of Glaciology 44 (1), 345–351. Wan, Z., Dozier, J., 1996. A generalized split-window algorithm for retrieving land-surface temperature from space. IEEE Transactions on Geoscience and Remote Sensing 34 (4), 892–905.

Interactive comment on The Cryosphere Discuss., 5, 1583, 2011.