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The Cryosphere Discussions

Interactive comment on "Thermal remote sensing of ice-debris landforms using ASTER" *by* A. Brenning et al.

Anonymous Referee #1

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The authors provide a statistical evaluation of the potential to use apparent thermal inertia derived from a pair or satellite based estimates of nighttime and daytime land-surface temperatures. They use statistical analyses of the correspondence of satellite-derived variables with point-based ground measurements and with spatial patterns of geomorphic features. Personally, I have found this manuscript very inspiring and read it with great interest. Nevertheless, I recommend either to ask for major revisions or to reject it because in its present state its value for other researchers is quite limited.

It is the declared aim to explore the utility of satellite-derived products for mapping certain geomorphic features. This is done empirically on a rather small test area. Even if the outcome were more encouraging, the transfer to other areas would remain uncertain given the many potential confounding factors and the diversity of mountain

landscapes. A rigorous discussion of the processes and possible sources of error involved both with respect to the remote sensing and the ground thermal regime is necessary for judging the merit of the empirical analysis presented. What limitations are we to expect? What are the sensitive parameters? How do we know that the variables taken into account (e.g., albeo) are really the important ones? Two important factors not adequately discussed are the influence of (a) sub-pixel angular effects and their changes with topography and (b) the temporal characteristics of the heating and cooling previous to the satellite overpasses.

Angular effects in reflective bands are described by the bidirectional reflectance distribution function (BRDF). In mountain topography, this can lead to strongly differing apparent reflectance for one and the same surface material, even after atmospheric and topographic effects have been removed. This is because the direction of the surface normal vector and, therefore, its relative angles to the sun and the satellite change with pixel slope and aspect. In the visible wavelengths this is only relevant for the derivation of albedo that may not be too important, here. But more important is the potential directional effect in measured at-sensor long-wave radiances. Looking from the direction of the sun onto a recently illuminated rough surface will show higher temperatures than looking from the other side, where one mostly sees the shaded faces of surface asperities. It is however the surface integral that determines the energy balance and thus the heat conducted into the ground that will then influence the slow release of heat during nighttime. The difference between the surface integral and the measured temperature is likely to exhibit spatial patterns due to an angular component that additionally depends on surface characteristics.

The ATI is here defined is the ratio of the forcing energy flux (or, a proxy of it) over the resulting change in LST (Page 2904, line 15). The change in LST is fixed to the local solar times of 14:44 and 3:31. The forcing energy flux however is approximated with the daily integral of potential radiation before acquisition. This could be a valid approximation in flat terrain, but in mountains, the timing of radiation input during the day will be different for differing slopes. As an example, the exact same material and same PISR on an East-exposed slope will have a lower LSTday as it is already cooling after its irradiation peak in the early morning, than a West-exposed slope that has its highest input just before the satellite scene was taken. This would then lead to a difference in the deduced ATI.

These analyses would be much stronger if a more rigorous background and evaluation of sources of uncertainty were included. Order-of-magnitude estimates of these uncertainties could be propagated into the products interpreted.

OTHER COMMENTS Page 2903, line 14: I believe that FLAASH only computes atmospheric correction. If this is true, then the computed irradiance is not suitable for mountain areas and hence the resulting reflectance has systematic errors. Check the software package ATCOR for a comparison.

Page 2900, line 1-3: Why is the sensible heat flux spatially invariant? Reference? One would expect this to strongly depend on wind speed that may be altered by convexity and sheltering when forced by wind in the free atmosphere and influenced by thermal winds both during day and night.

Page 2897, line 6: Check this sentence, it seems mangled.

Page 2897, line 24: This wave-band definition of TIR is not entirely correct as it is more motivated by atmospheric windows than by the emission spectrum (plot a Planck curve for 273K. . ..).

Page 2898, line 1: It is the SURFACE temperature, not the internal temperature that matters.

Page 2898, line 11: the relationship may not be trivial by why not exact?

Page 2898, lines 25-28: Why are four references given for considering depth-invariant properties but none for heat conduction and energy balance? It is not clear what is meant here. If only the equation of heat conduction is solved, why is not LST used as

the upper boundary condition? This would then bypass the difficulties to estimate the surface energy balance.

Page 2900, line 23: . . .debris-covered glaciers has. . .

Page 2907, line 7: check hours per day

Page 2908, line 28: What is the reason for these unacceptable values?

Page 2910, line23: Delete “as controlled by kinetic temperature only”. This is incorrect or misleading.

Page 2916, line 25-28: For coarse blocks, one would imagine the ATI to be dependent on the temporal scale and the mean block size: For short-term heating, it will approach the behavior of the rock material, for longer time periods it will be more similar to a macroscopic description of the thermal properties of the whole block layer.

Interactive comment on The Cryosphere Discuss., 5, 2895, 2011.