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Interactive Comment

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

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Anonymous Referee #1

AC: Both referees find the proposed approach novel and interesting, but have some critical comments concerning the transferability of the presented results to other areas and the uncertainties present in our analysis and discussion. To emphasize the exemplary character of this study and its limited spatial scope – which is, however, supported by a substantial sample size in terms of number of ice-debris landforms and in-situ measurement sites – we decided to indicate the study region in the title of the revised article. In addition, we have made amendments to the article that will put stronger emphasis on the discussion of uncertainties, and physical aspects of the present research such as links between apparent thermal inertia and surface energy balance are now discussed in more detail.

We would like to thank the anonymous referees for their thoughtful comments, which helped to improve the manuscript.

Point-by-point reply to the referee's comments

RC: 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.

RC: 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.

AC: From an empirical point of view, we would like to argue that it is not necessarily the size, but the replication that matters in the first place. There are over 50 ice-debris landforms of strongly varying size and characteristics in our study area (see Fig. 1 of the discussion article), and field measurements were made at 37 locations. Comparison with state-of-the-art publications in this field reveals that replication is typically rather limited (e.g., Taschner and Ranzi, 2002: one glacier; Shukla et al., 2010: one glacier; Bhambri et al., 2011: three glaciers; Casey et al., 2011: comparison of satellite-derived LST with in-situ measurements at three locations).

This being said, we agree that while the methods and issues being studied in our contribution are of more general importance, the numerical results would differ in other mountain areas, which is why we decided to indicate the exemplary nature of this study by changing the title to "Thermal remote sensing of ice-debris landforms using ASTER: an example from the Chilean Andes".

RC: 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., albedo) 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.

AC: We will divide our response to this comment into three parts:

RC: What limitations are we to expect? What are the sensitive parameters? How do we know that the variables taken into account (e.g., albedo) are really the important ones?

AC: The surface energy budget at the air / debris interface is characterized by the balance between net radiation components from one side and flux components from the other side. Net radiation and flux components are the sensitive parameters one should expect to influence thermal inertia mapping, additional details can be found in Reid et al. (2010) (see also revised Sect. 2.2, which includes the surface energy balance). We have controlled the net radiation component by using (PISR) to account for incoming short wave radiation, albedo to account for reflected short wave radiation, incoming long wave radiation is expected to be minimum and constant given clear sky conditions, while corrected thermal images characterize the outgoing long radiation. With respect to the flux components, latent heat and precipitation flux could be assumed to equal zero given the dry conditions of the study area. Therefore the energy at the air–sediment interface is divided between ground and sensible heat flux. Ground flux is controlled by the debris temperature, our target variable, and the thermophysical properties of the debris layer which are accounted for using the 'COARSE' variable and ice-debris landform types. Sensible heat flux under stable conditions is related to air temperature which varies inversely with altitude, which is controlled, in our study, by the elevation variable. We did not control directly the variation in sensible heat flux under unstable condition (turbulent air flow) in our study, which leaves us with one potential confounding factor that could interfere with the thermal inertia signals. Differences in lithological characteristics are an additional possible confounder that had not been mentioned in the discussion paper; information on lithology has been added to Sect. 3.1 of the revised manuscript (see reply to comment of referee #2).

On the other hand, from a statistical perspective the presence of this and potential other confounders in the matched-sample analysis is controlled through randomization, replication and matching. While we can never completely exclude the possibility of being fooled by confounding factors that cannot be controlled, we do think we have made a reasonable effort to control these issues.

RC: Influence of sub-pixel angular effects

AC: This is discussed later in response to a more general comment on directional reflection.

RC: Temporal characteristics of heating and cooling previous to the satellite overpasses

AC: Meteorological conditions before and during the days of the satellite overpasses are described in much detail in Sect. 3.4 and 3.5. Cloud-free conditions prevailed throughout and are typical of this time of the year in a summer-dry subtropical climate. Near-surface ground thermal regimes during and before the reference period are displayed in Fig. 3 for ice-debris landforms and other locations separately. The curves are mostly synchronous (the number of exceptions being roughly equal between ice-debris landforms and other sites), and Sect. 4.1 mentions that the choice of readings at fixed times (3 a.m. versus 3 p.m.) versus the actual daytime maxima and nighttime minima affected neither the LST-NSGT correlations nor the following statistical analyses in any substantial way. Furthermore, possible uncertainties arising from the temporal mismatch between the overpass period and in-situ reference period were examined (p. 2913, l. 5-6 and especially l. 16-19). We therefore argue that sufficient information on temporal characteristics is provided, and that possible influences of spatial variation in timing have been discussed. No changes made.

RC: 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.

AC: From our perspective any attempts to correct the anisotropic reflection of the surfaces of interest without detailed data about the angular distribution of the energy radiated by the surface at the required spatial scale may introduce an unknown error rather than an improvement of data (e.g., Gruber et al., 2003; Li et al., 2011). The collection of such data was beyond the scope of this study (in fact, modeling the BRDF for the study area would likely be a study by itself). Consequently, while being aware of this effect we decided to perform our analysis based on surface apparent reflectances, which we consider sufficiently reliable for the purposes of this study. However, in the revised version of the manuscript we mention explicitly that such corrections were not applied to the reflectances (Sect. 3.2).

Since anisotropic reflection effects depend largely on slope aspect (slope angles are already controlled by matching), we decided to perform a post-hoc statistical analysis by re-fitting the mixed models with sine and cosine of aspect as additional explanatory variables. While there are overall no big surprises, the LSTday and ATI effects of debris-covered glaciers are larger under these models, which makes physical sense as this would be expected due to the thinner debris layer. These additional results are explicitly reported as post-hoc results in the methods, results and discussion (Sect. 3.6, 4.2, 5.2).

RC: 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.

AC: The reviewer expresses a valid concern, which we have been aware of during our analysis. The decision to use PISR until the acquisition time was a pragmatic one because we felt that it would be difficult to provide a good justification for any particular different time interval such as the radiation received since noon, or during the one or two hours prior to image acquisition. However, this choice is not decisive for the results of our study. Repeating the statistical analysis with PISR from noon until acquisition time only results in changes in estimated model coefficients that are well within the confidence intervals reported for our models. Furthermore, the matching step controls most relevant influences on and proxies of absorbed solar radiation (matching by slope, PISR on acquisition date, albedo, elevation).

We therefore decided to keep the analysis unchanged and to add the following sen-

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tence at the end of the results (Sect. 4.2), where the sensitivity of the model to several data selection choices is presented:

"Similarly, the use of a PISR correction term in Eq. (5) covering only the time span between noon and acquisition time has no appreciable influence on the results of our statistical analyses considering the estimated standard errors of model coefficients."

RC: 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.

AC: The question of the effects of measurement errors in the independent variables is indeed a legitimate concern. We addressed several sources of error and uncertainty in our discussion article (e.g., scale effects: p. 2914, l. 11-13 of discussion paper; possible unobserved confounders: p. 2914, l. 7-10; insensitivity to the choice of the reference period: p. 2913, l. 16-19), and more so in the revised manuscript (e.g., plausibility of remotely-sensed albedo values: Sect. 3.3 and new Table 1; insensitivity to an alternative PISR adjustment of ATI: Sect. 4.2; accuracy of temperature data loggers: Sect. 3.5). But how strongly do these sources of uncertainty affect the interpretation of our results? We use statistical methods precisely to account for uncertainties, which are partly controlled through our sampling designs, and otherwise reflected by the estimated standard errors, random effects and error terms, which also represent non-systematic measurement errors of the remotely-sensed and in-situ response variables. The “products” that we are aiming at in this analysis are in the first place the estimated model coefficients, which we interpret in accordance with physical and geomorphological processes and their confounders, and subject to the uncertainties expressed by standard errors and hypothesis testing.

We therefore believe that sources of uncertainty are well documented, especially in the revised manuscript thanks to the referee’s comments, and that they have been taken into account in the choice of a statistical methodology and the discussion of results.

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RC: 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.

AC: FLAASH output data are surface apparent reflectances, which means that FLAASH takes into account the absorption and scattering affecting the irradiance that enters into the atmosphere. In other words, FLAASH assumes that the exoatmospheric irradiance or top-of-atmosphere irradiance is unsuitable and that a model of the atmosphere is needed to calculate the irradiance that really strikes on the surface. The performance of FLAASH and ATCOR software is quite similar, since both are based on MODTRAN atmosphere models. We routinely use both correction algorithms (available in ENVI and PCI Geomatics, respectively) and have noted that results are quite similar (see also, e.g., San and Suzen, 2010).

Also, differences between atmospheric correction methods would likely have little effect on statistical results because matching by slope, PISR and elevation would ensure that introduced errors in ATI and LST are much the same for samples at ice-debris locations and at other locations.

Based on these considerations we have decided to leave the atmospheric correction unchanged.

RC: 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.

AC: We appreciate the reviewer's comment regarding topographic effects on wind speed and turbulent heat flux. We address this possible confounder in the revised manuscript (Sect. 5.1; see also response to one of the comments above). However, we also found that our results for the in-situ data do not depend on the particular choice

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of the reference period, which suggests that this might not be a decisive factor (p. 2913, l. 16-19 of the discussion paper).

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

AC: Sentence structure corrected and simplified in revised version.

RC: 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. . ..).

AC: The revised version of the manuscript uses the definition of the wide thermal infrared range (from 3 to 14 micrometers).

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

AC: Our interpretation is that the emission depends on the kinetic temperature and the emissivity of the material, and surface temperature is the result of the emission (see Lillesand et al. 2004, pp 347-353).

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

AC: We did not intend to talk about precision and therefore changed the wording from “although this relationship is not exact” to “although there are other relationships as well” in order to express the complexity of the relationship.

RC: 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.

AC: We have responded to the reviewer comment by omitting references that are related to depth-invariant properties and discussed in detail the surface energy balance supporting it with recent literature (e.g.,Reid et al., 2010). Regarding the use of LST as an upper boundary condition, we changed “and solving the heat conduction equation with the surface energy balance estimated from LST as an upper boundary condition” to “and solving the heat conduction equation with LST as an upper boundary condition”

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

AC: Changed “shown” to “found”.

RC: Page 2907, line 7: check hours per day

AC: “80 h on one day” should read “8.0 h on one day”; this error was introduced at the typesetting stage (manuscript was correct), we will double-check this value in the proofs of the final article.

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

AC: This comment refers to the removal of “areas with albedo a<0.05 or a>0.20 or ATI>0.60”. It should be noted that (after removal of ineligible areas such as low elevations, exposed bedrock, glaciers and snow patches), only 2% of the area of interest have albedo < 0.05, 1% have albedo > 0.20, and another 1% of the pixels have ATI > 0.60; we clearly should have mentioned this in the discussion paper and therefore included this information in the revised paper.

The small percentage of albedo values >0.20 can be attributed to the presence of dark volcanic rocks in this area (see new Table 1 with albedo values) and the lack of vegetation. Mixed pixels that partially include small, possibly dirty snow patches or small water bodies (melt-out ponds) on the debris-covered glacier would clearly have distinct thermophysical characteristics; in order to avoid possible confounding with such influences, we decided to remove these samples from our study using the criteria described in the text.

The following changes have been made in response to the referee’s comments: The presence of (dark) volcanic rock types in most parts of the study area is mentioned

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in the revised Sect. 3.1 (see also comment of referee #2 requesting information on lithology). The text was modified as follows: “Areas with albedo a < 0.05 or a > 0.20 or ATI > 0.60 were removed in order to avoid artifacts resulting from mixed pixels involving fractional snow cover, melt-out ponds or other local phenomena with thermophysical characteristics that may bias the coefficient estimates. The rather low albedo cut-off value of 0.20 is due to the prevailing low albedo of dark volcanic rocks (Table 1). Only 3.6% of the samples obtained after the previous filtering steps were removed based on their extreme albedo or ATI values.”

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

AC: Changed to "as controlled by kinetic temperature, surface emissivity and incoming long wave radiation".

RC: 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.

AC: This is certainly an interesting aspect under circumstances where the periodicity of heating differs (e.g., different planets, or polar versus low latitudes) or can be controlled experimentally. We disregard temporal scale effects in our discussion as only 24-hour heating cycles are relevant in our study and in possible similar studies in extra-polar mountain areas on Earth.

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