Review of the study by Sasaki et al.

General comments

The study by Sasaki et al. applies a method to classify debris-covered glaciers as developed in earlier studies to a large part of the global glacier inventory as available from the RGI (Randolph Glacier Inventory). In contrast to numerous other studies, the aim here is not to classify the debris-covered parts itself, but to distinguish between clean ice and debris-covered ice within given glacier boundaries. The resulting dataset is going beyond a simple debris yes/no map and instead provides distributed thermal resistance of the glacier-covered area. This information is most welcome for a large number of applications looking at the energy balance for calculations of mass balance and future glacier development. I am also fine with the simplified approach suggested here, as achieving near-complete global coverage within a reasonable computational effort must have drawbacks somewhere. For these reasons I find the study timely and an important contribution.

On the other hand, I find some major shortcomings in this study that are partly based on problems of already published earlier studies, including some that are not cited here. In part and indirectly, the other two reviewers have mentioned the problem as well. The main issue is the never clearly defined and increasingly misleading use of the term debris-covered glacier. The term is used widely and independent of the spatial completeness of the debris coverage or its thickness. To my knowledge neither a glacier with a medial moraine nor with some more debris near the terminus should be called a debris-covered glacier. This requires that larger parts (tbd) of the ablation area have a near-complete (tbd) coverage with optically thick pebbles, stones or rock. Millimetre-sized particles (sand, silt, clay, BC, pollutants) that are often creating the rough microstructure of the ice do not fall into this category, as they never cover the ice completely. A side issue of this problem is that the aggregation of often highly variable thermal information at the level of a 90 m thermal pixel is not addressed in most studies, e.g. by looking at least at the co-registered spectral information at 15 and 30 m resolution (speaking of ASTER). In consequence, an increasing amount of clean ice (towards higher elevations) within a 90 m pixel is often confused with apparently decreasing thickness of debris cover (although a glacier is never sorting particle sizes).

I exemplify the problem here for Hailuogou Glacier (HG), as it has been used for the methodological development that is forming the base of this study (Zhang et al. 2011). HG might be considered as a debris-covered glacier, but in fact the (optically thick) debris is only covering its lowermost part. For the largest part of the ablation area the heavily crevassed surface is *dirty* but not really debris covered. On a micro-scale, the dirty ice is not covering the ice completely and a variable amount of zero-degree bare ice is contributing to the thermal signal when aggregated at 90 m pixels. For HG this issue is strongly enhanced due to the highly crevassed surface that adds further zero-degree zones to the 90 m pixel. In fact, the 15 m ASTER VNIR image shown in Fig. 3a by Zhang et al. (2011) also clearly reveal (despite the resolution limits) that HG is not really debris covered but the ice is dirty at best (see also tourist photos in Google Earth). Accordingly, the thermal information leads to a very low thermal resistance and high melt rates (Fig. 6a/b). My key question here is: How could the thermal resistance derived from a dirty/crevassed glacier serve as a base for characterizing resistance for really debris-covered glaciers (let alone to determine debris thickness)?

When looking at Fig. 6 in Zhang et al. (2011) there is another severe issue in their analysis: A large portion (maybe not all) of the pixels indicating a high thermal resistance (the red-orange band) are actually located outside the glacier boundary and cover the lateral moraine (and regions above) that are especially warm due to their south-easterly exposition and steep slope, i.e. the sun might hit these surfaces under a zero-degree incidence angle. There is definitely no ice underneath this “debris” that cools it. In consequence, the derived equations / regressions make no sense and could not be applied. This mistake highlights another problem with the (coarse) 90 m pixel of the thermal band. Not a single 90 m pixel with parts outside the glacier boundary should have been used to determine thermal information, as even a very small part of this warm rock impacts considerably on the mean value for the 90 m pixel (the same applies in the other direction when some bare ice is included). When the determination of debris thickness on a glacier is derived from rocks in the lateral moraine and an unconsidered bare ice part in the 90 m pixels, I do not wonder about funny results. At least a scientific base is missing.

I agree that this might not be the correct place to criticise the former study by Zhang et al. (2011) or note that the peer-review system fails from time to time. But when such studies are used as a base for other studies so that their misleading results are multiplied, there should be a possibility to stop it. I am aware that the above might have consequences for several other already published studies but I do not ask here to withdraw them. However, I would highly appreciate if all scientists working on the thermal identification of debris-covered glaciers (debris extent, thickness, reflectance or whatever) would do it more carefully in the future and consider all effects playing a role. This includes ice cliffs, melt ponds, albedo variations, shadow, crevasses, clean ice between thick debris (in regions of incomplete coverage) and the accurate distinction between dirty ice and really debris-covered ice. All these features can be present within the limits of a single 90 m ASTER pixel.

My other main objection is the rather poor validation performed here. I fully appreciate that several examples are given to illustrate the performance of the method (Figs. 3-6), including those where the method did not work. But the pure visual comparison gives a very unreliable base for a proper assessment. For example Baltoro Glacier in Fig. 3a: When I compare the thermal resistance map with a map of the pattern of clean ice / debris cover (nicely arranged in parallel medial moraines), I do not see any similarities. Upwards of the confluence area (Concordia) everything is completely blue despite several thick medial moraines, whereas rather clean ice (or again regions outside the glacier?) has a high thermal resistance. In the upper right it seems the glacier mask includes a larger rock outcrop (please note that the region in the red circle on Fig. 5 is also a rock outcrop; the glacier in this region melted away). To me it seems as if a satellite scene with a high amount of seasonal snow has been used for the classification, largely underestimating the real extent of debris cover. I am pretty sure this problem is prevalent also in many other regions.

Which brings me to a final major point, the selection of satellite scenes. I doubt that (a) the method applied here to select the most suitable scenes has always found the scenes with a minimum of snow cover and (b) I think for many regions suitable scenes simply do not exist (e.g. Fig. 6b). The selection of the scene with the largest amount of debris (P8, L21) is certainly a good idea but it does not imply that all debris is exposed. I think the uncertainty in this regard is not realistically estimated. I certainly miss a pixel-by-pixel comparison (omission and commission errors) based on a couple of manually created debris extents. This can be easily achieved by subtracting a clean ice classification (using a simple red/SWIR band ratio) from the RGI glacier extents (please use RGI 5.0!) converted to a grid. Such a clean ice mask would also help to determine the amount of clean ice within each 90 m thermal pixel and correct the details of the thermal resistance classification accordingly.

Overall, I think the study was worth a try as it ultimately creates a dataset of high interest and demand. Given that the methodology is further improved, better validated and creating more realistic results, I am pretty sure that the study can be published. I would thus like to encourage the authors to check if they can improve their study along the suggested lines and resubmit it at a later stage. In the following I add some further points requiring consideration in the case the authors intend to resubmit the study.

Specific comments

For future submissions, please use a continuous line numbering scheme and apply it to all lines rather than each 5th. This facilitates the work of reviewers greatly as they do not have to waste time with counting lines and page numbers.

P1

L17-19: As a more general comment: Please note that the use of the simplified terminology 'thin and thick debris' is misleading as it does not consider grain size and degree of coverage. The 'thin debris' that is enhancing surface melt is often just dirty ice (sand, clay, silt) with lots of clean ice in-between (i.e. incomplete spatial coverage). Moreover, for thin plates of rock (<2 cm) the albedo of the material is increasingly important. Please also note that surface melt is not the only factor contributing to volume/mass loss of glaciers and thus the amount of melt water. Several studies using DEM differencing have shown that specific mass loss of heavily debris-covered glaciers is often as high as for clean glaciers. Ice cliffs, melt ponds and so far completely unconsidered en-glacial melt (internal collapse of conduits) might play a major role for this. In short, the impact of debris cover on the amount of melt water from glaciers is not fully clear.

L24: Please just write 'glaciers'. Those contributing to sea level are certainly not 'small' but very large.

L28: How does melt water from glaciers cause rock slides?

P2

L4: Please note that glacier retreat (change in length) is something different than volume loss caused by the melting rate. Glaciers can lose mass without a change in length and advance without a change in mass. Global glacier models have a good hand on melt rates but difficulties with geometric changes, as this requires precise knowledge of the glacier bed (ice thickness distribution).

L7: As mentioned above, I would not call the material that is enhancing melt 'thin debris' but dirty ice, as grain size must be small, albedo low and its distribution disperse.

L13: Please use glacier instead of glacial when reference is made to contemporary glaciers (or here The melting of ice beneath debris …).

L31: Please see my comments above on the study by Zhang et al. (2011). I think it contains major systematic and methodological errors and cannot be used as a reference.

P3

L4: As the thermal resistance map is obviously not able to distinguish clean ice from dirty ice and debris-covered ice, what about starting with a simple debris cover yes-no map?

L9: Please use RGI 5.0.

P5

L14/15: The threshold values of thermal resistance for debris yes/no and thick/thin should be provided in the methods section.

P6

L4: Can ‘close agreement’ be described in more detail? I did not find a very good agreement between the thermal resistance map and the distribution of clean ice and debris for Baltoro glacier (see General comments).

P7

L1: This is not a debris-covered tributary but bare rock. That this region has been mapped as debris-covered ice might be a consequence of the calibration over bare rock rather than debris-covered ice.

L11: Please note, debris cover should only appear on the surface below the ELA (or as a proxy: mean elevation) due to emergent flow. Apart from the higher amount of snow cover in the study by Lambrecht et al., I assume that your method has simply mapped dirty ice as being debris covered. Dirty ice is rather common in this region below steep (and ice free) rock walls.

L22: by reducing albedo? I do not think that albedo reduction is the major process here. The key point is that the material on the surface can get warmer than zero degrees and when the material is thin enough, conductivity can get the base of the material also above zero degrees so that it can melt into the ice.

L23ff: This sounds like methods rather than discussion.

P9

L5: Please note that also the RGI outlines are not perfect and please avoid including any thermal pixels that reach beyond given outlines. Only 100% inside pixels should be used for the calculation.

P17, Fig. 3: I suggest adding also here optical images for comparison (those with minimum snow amount).