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Interactive Comment

Interactive comment on "A comparison of glacier melt on debris-covered glaciers in the northern and southern Caucasus" *by* A. Lambrecht et al.

Anonymous Referee #1

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This study presents first results of glaciological investigations in two regions of the Caucasus. The authors put a focus on supra-glacial debris cover and its reducing effect on glacier melt. A number of field data including ice ablation surveys in several seasons as well as meteorological data have been collected. Ablation rates over debris-covered ice are compared to other mountain ranges and are interpreted using a simple modelling approach.

I highly appreciate the efforts of the authors to measure glacier mass balance in a region that is not easy to access and is characterized by a general lack of glaciological data. Therefore, the publication of these data will be a valuable contribution to glaciological literature. However, the presentation of the data and the conclusions drawn from the observations require some additional work.

In general, I have the impression that the authors could get more out of their data and provide some more useful conclusions for advancing glaciological research. For example, there is no explanation for the reasons of the different thermal resistances of supra-glacial debris in comparison with other mountain ranges. The modelling approaches presented are not new, nor do they allow addressing important problems in modelling such as the future expansion of the debris-covered area, as well as the likely debris thickness increase. The comparison of ablation rates alone (see title of the paper) does not yield fundamentally new insights and process understanding.

Important points that should be addressed by the authors are listed below:

1. The structure of the paper needs to be enhanced. Currently, data, models, results, and discussions are mixed up following the work flow. I suggest to clearly separate the description of data, of the evaluation techniques, and of the model.

2. In particular regarding the topic of supra-glacial debris, I have the impression that the authors could provide a better review of current literature. Several important studies of the effect of debris coverage on ice melt and the modelling of the related processes are missing as much as I can judge.

3. I miss the link between the energy balance (its terms are obviously measured in detail at the meteorological stations) and the degree-day model that is proposed. The authors also provide interpretations (that seem to be based on the model) relating to energy balance terms (e.g. reduced melt due to enhanced cloudiness, see page 440, line 2). But all evaluations of differences in ice ablation are only given as degree-day factors, although ice ablation below the debris-coverage is determined by the entire energy balance. Is it possible to compare DDFs between different regions / mountain ranges with differing meteorological conditions and, consequently, different energy fluxes at the ice/debris surface? This point certainly requires additional discussion and maybe some more data analysis.

4. One main focus of the paper is the comparison of ablation rates on the debris-covered tongue of two glaciers. The comparability of these ablation rates is in my opinion not given a priori: As stated in the paper, ablation below the debris cover can vary because of many local factors. Furthermore, the elevation of the glacier tongue depends – besides the climatic forcing – also strongly on the size of the accumulation area / the glacier geometry. This factor is not yet adequately discussed in the paper.

5. Some estimates of the uncertainty in the main results of the paper should be provided. How accurate are, for example, the calculated thermal resistance values? Or the modelling results? What is the impact of the various assumptions (e.g. constant debris thickness distribution over the glacier tongue) on the results?

6. The modelling procedure is not yet explained in sufficient detail. It is not clear enough how the degree day functions for the debris coverage were derived. Does the model include any accumulation term? If not, can it really be used to reasonably simulate the melt reduction by debris (summer snow fall events)?

7. The authors evaluate the thermal resistance of the debris coverage and find significantly higher resistances in the Caucasus in comparison to other mountain ranges. What can we learn from this observation? In my opinion, such a finding is only useful if there is a plausible explanation. This would be helpful for *understanding* variations in ice ablation below debris in different regions of the world, and for model development. If there is no process-based reason for the differences, they could also be related to different measurement and evaluation techniques. If differences in the thermal resistance of the debris are mainly driven by the geological conditions (rock type) this could be easily included in the evaluation. Or is it due to differences in the texture of the debris (fine/coarse)?

8. Based on the modelling the authors provide percentage numbers of the melt reduction due to the debris coverage (e.g. page 432, line 22). I assume that the

authors here refer to the reduction in bare ice melt (which should be stated!). However, these numbers are unrelated to the total glacier mass balance and are, thus, difficult to interpret or to transfer. I strongly suggest that the impact on glacier-wide mass balance of the entire glacier is evaluated. This allows comparing the effect of supraglacial debris for glaciers with differing sizes and debris-cover percentages and in different climatological regimes.

9. It would be very useful to provide a map with the location of the ablation measurements.

Detailed comments are provided below:

- page 432, line 25: A general introduction into the topic of glacier melt over debris-covered glaciers and the related impacts would be useful before starting directly with the study site description.
- page 435, line 13: What is sigma? Do the authors mean the correlation coefficient? In that case the correlation (0.75) would not be 'very high'.
- page 435, line 26: Is this the squared correlation coefficient r^2, or r? The number provided is almost the same as for the Djankuat Glacier (see last comment). Clarify.
- page 437, line 1: What was the criterion to decide whether there is debris or not? Often, the transition between debris-covered and bare ice is fuzzy.
- page 437, line 4: Here, and also in Figures 4 and 5, the drainage basin size is discussed. What defines the drainage basin? As there are no hydrological consequences discussed or hydrological models presented, I cannot see the motivation for including a drainage basin into these evaluations. I suggest to remove the

drainage basin from the text and from the figures in order to provide a better focus on the main topic of this paper, which is unrelated to discharge totals. Discharge modelling would be another topic.

- page 437, line 13: The *relative* changes in debris-covered area are quite similar (+43% versus +35%). This seems to be in contradiction to what is stated in the paper (strong versus minor *absolute* changes in debris-covered area). Maybe this could also be commented.
- page 438, line 16: 35 years rather than 25?
- page 438, line 18: The analysis of glacier inventory data always shows that the relative area changes are strongly different for size classes of small and large glaciers. Thus, I wonder whether the relative changes obtained for the Alps can be directly compared between mountain ranges that probably show different glacier size distributions.
- page 441, line 4: Provide units for DDFs.
- page 441, line 22: Is there an explanation for the increase in melt with debris thicknesses larger than 10 cm (see Fig. 7)?
- page 442, line 7: As the authors have shown and discussed that DDFs for debris-covered ice vary greatly over time (page 440, line 12), they should also provide some measure of the uncertainty in the calculated resistance values that will also be biased by the above-mentioned variations.
- page 442, line 14: Is this assumption justified? On page 437, line 15, the authors state (probably according to field observations) that composition of the debris cover strongly depends on slope etc. What is meant with 'characteristic' debris composition?

- page 444, line 14: Here, a degree factor function in dependence of debris thickness is introduced. The authors should show how this function was derived (maybe using a figure) and state the statistical confidence level of this function.

- page 444, line 22: What is the basis for the assumption that the debris 'thickness/elevation' distribution on neighbouring glaciers is similar? I assume that different glacier geometries and morphological settings also cause different sediment input rates and thus varying debris thickness. If this assumption is made, it should at least be supported by some evidence from published literature.

- page 444, line 22: What is the motivation for including more glaciers into the calculations at this stage? The authors should better introduce their strategy at the beginning of the paper. I actually only realized now that the calculations are performed for unmeasured glaciers as well, which probably significantly increases the uncertainty. As no integrated results (e.g. discharge) for the catchments are presented, I wonder why this extrapolation is performed or required.

- page 445, line 15: As much I as I understand, the used degree-day model does not include the effects of cloudiness. So, this conclusion cannot be drawn based on this model approach (but based on the meteorological data that have been collected).

- page 445, line 18: The elevation of the glacier terminus is mainly determined by the size of the accumulation basin / the elevation range of the glacier and the general glacier geometry. The argumentation of the authors is relative to the equilibrium line altitude.

Interactive comment on The Cryosphere Discuss., 5, 431, 2011.