Response to Anonymous Referee #1

We acknowledge the constructive comments of both reviewers and have responded to each comment point by point. Our responses are italicized.

Anonymous Referee #1 Comments

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.

The main objective of the paper is the comparison of two basins in the Caucasus with rather different glaciological conditions in respect to the melt regime. Both basins are, in our opinion, a good choice as representatives for the conditions to the North and to the South of the main divide of the Caucasus. Therefore the linkage between the measured melt rates and the prevailing meteorological conditions will provide important insight into the general situation of glacierised basins in the Caucasus. This paper was never meant to contribute to specific process investigations. Also it is not the aim of the study to provide a model for debris cover evolution which requires a rather different approach. The main goal is to conclude on specific differences for the two regions based on a rather limited data set, typical for the situation in many mountain regions. However, we agree that the quality of the paper can be improved and we are grateful for the constructive suggestions provided by the reviewer.

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.

We agree that the structure could be improved. The final revised paper will be restructured accordingly.

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.

We will improve the presentation of earlier work in respect to debris covered ice ablation by including a short review in the introduction. In respect to our results we also will provide better reference, relating our findings to work done in other mountain ranges.

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.

Sub-debris ice ablation is in fact determined by the entire energy balance. One major problem which usually inhibits the application of more sophisticated energy balance models for debris covered glaciers is the unknown local surface energy balance. Even with available data from a meteorological station in the vicinity, the spatially varying surface characteristics on debris covered glaciers make it very difficult to determine the local fluxes. There are attempts of using the debris surface temperature instead of the air temperature in order to obtain a better idea about energy fluxes at the debris surface (Reid et al, 2010). But this approach requires the knowledge of debris surface temperatures across the entire area of the glacier, or a parameterisation which depends on continuous measurements of required meteorological parameters (radiation components, temperature, wind, humidity, pressure) on the debris covered part of the glacier. Such data are not available, not on the benchmark glaciers and not at all on the other glaciers in the basins. Our approach aims on providing a good and valid estimate of the melt amounts in the basins using a minimal set of input data, typical for remote basins in many Asian mountain ranges. We could probably perform such an exercise described above for Djankuat glacier with its good data situation, but not for Zopkhito glacier.

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.

It is true that glacier ablation is not easily compared between glaciers as long as the specific conditions are not taken into account. At least the glacier tongues of Zopkhito glacier and Djankuat glacier are in very similar elevation ranges. But still the differences depend on a number of factors, which is actually one motivation for this paper. We will improve the discussion on the potential reasons for the observed differences. In particular the meteorological conditions and the debris characteristics have the strongest influence on sub-debris ablation. A more detailed discussion will give a better insight into the specific differences. On the other hand, in our opinion glacier dynamics are not important in respect to glacier ablation (in contrast to mass balance) as long as the same altitude bands are compared.

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?

We agree that error margins would improve the value of the results presented. Given the simple input information and the rather generalised approach, it is rather difficult to provide good error estimates. However, we will perform some additional calculations, for exploring the parameter space of individual parameters. This will allow a more in depth discussion of uncertainties in 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)?

The modelling procedure is discussed in more detail in the accompanying paper on glacier ablation in the Altay (Mayer and others, TCD, 2011). However, an improved description will be included in the revised manuscript. Especially the way how parameters are defined and derived from our *measurements will be included. According to our observations, summer snow fall events are not a critical issue for the glacier tongues in the basins investigated. At both glaciers, summer snow fall is very rare on the glacier tongues, so that it has no significant effect on the ablation.*

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)?

The measurement strategy and data evaluation is the same for the regions compared in this study. Therefore we are sure that the differences observed are due to differences in local conditions. We now provide a comparison with known thermal properties of different rock types and discuss the influence of debris composition (grain size, stratification, water content…). In our opinion the differences can be attributed to the local conditions of the debris cover. An evaluation is difficult without specially designed and elaborate experiments. But a qualitative assessment can be provided which provides reasons for the observed differences.

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 debriscover percentages and in different climatological regimes.

We relate the reduction in melt to the bare ice case. This is now also stated clearly in the text. This is the most significant result of our investigations and values are comparable for different glaciers and different regions, as they are given in dependence of altitude and for the entire ablation area. An investigation of the impact on mass balance is not possible, because Djankuat glacier is the only glacier for which mass balance measurements exist. Thus the only possibility of a sensible comparison is to provide the relative influence of the debris cover in respect to clean ice conditions at individual elevations. In order to provide an idea about the relative impact on mass balance the case of Djankuat glacier is now discussed for the year of observations.

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

A map with the location of the measurements for Djankuat and Zopkhito glacier will be added in the revised paper.

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.

A general introduction to the topic "debris cover on glaciers" and the existing research work will be added.

• 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'.

We exchange „very high“ with „reasonably high“. Still, such a correlation is significant and our approach of transferring temperatures from the distant long term meteorological observations is justified. In both cases the correlation was calculated as r².

• page 435, line 26: Is this the squared correlation coefficient r2, or r? The number provided is almost the same as for the Djankuat Glacier (see last comment). Clarify.

See above.

• 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.

Indeed the partitioning between bare ice and debris covered ice sometimes is difficult, because this is sometimes even difficult to define directly in the field. In some cases a thin dust cover on the glacier strongly alters the glacier surface albedo. If such surfaces already can be classified as debris covered is not clear, however, melt rates might be decidedly higher than on a clean ice surface. Also the ice itself might exhibit rather large variations in surface colour/albedo, depending on the ablation history (radiation crust, predominantly sublimation, strong melt induced superficial water drainage). Classification of debris cover on remote sensing imagery is therefore not straightforward.
Our approach was to map clean ice surfaces first. In dependence of the pixel size, it usually is impossible to resolve sub-pixel clean ice patches in otherwise debris covered areas and vice versa. The next step will be to map obvious glacier margins, which in most cases is possible by analyzing changes in slope, general surface roughness and obvious breaklines on the images. With this approach very dark ice and thin dust films on clean ice might be included in the debris cover class. An additional visual quality check will improve the result, but there is no guarantee for a unique and correct solution, because as mentioned above such definitions cannot even be unique during assessments in the field. According to our observations in the field, the transition between clean ice and a clearly defined debris cover is usually not very large on the sampled glaciers and wrong classifications will be in the order of very few pixels. An additional quality check is the comparison of debris cover maps (as they exist e.g. for Djankuat glacier (Popovnin and Rozova, 2002 and newer mapping results) with our results from the analysis of remote sensing analysis. Stokes et al., 2007 reported a similar approach where results were satisfactory for the Djankuat glacier.
Regarding the temporal variability of supra-glacial debris cover, the most important issue is coherent image analysis for all data used, so that no variations in debris cover will be due to changes in the classification scheme.

• 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.

The delineation of basin boundaries is mainly because they define basins used in discharge modeling. Our results from the melt simulations will be used for the investigation of the discharge variability in these basins. Especially for the comparison of the entire basins it is necessary to define boundaries (e.g. for the hypsometry). Therefore we would prefer to keep the basin definitions in the manuscript.

• 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.

We do not understand this remark. Relative to the total glacier area the debris cover increased from 16% to 23% for the glaciers in the northern sample region. In the South the change was from 6.2% to 8.1% during the same time period. The change in area in respect to the existing glacier area is *therefore decidedly higher in the North. But also the relative increase (43% versus 31%) is significantly higher.*

• page 438, line 16: 35 years rather than 25?

This typo will be corrected.

• 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.

We will include a more detailed analysis by comparing the size classes.
The observed area change for the Adylsu glaciers is 14.9%, while the mean glacier area is 3.7 km². The change of the glaciers in the Zopkhito basin with a similar size class as in the Adylsu basin (2.58 km²) is 13.3%. In this size class there are only Zopkhito glacier and Laboda glacier in the Zopkhito basin. All glaciers in this basin show a mean area reduction of 12%. Compared to the situation in the Alps: in the Ötztal, the glacier class 1-5 km² last 11.4% between 1969 and 1997 and 19.1% in the period 1969 – 2007(Lambrecht and Kuhn, 2007, Abermann et al., 2010). For Swiss glaciers the glaciers in the size class 1-5 km² lost 17.9% between 1973 and 1999 (Paul et al., 2004).Glaciers with a comparable size thus lost less area in the Caucsus compared to glaciers in the Alps. This will be described in detail in the revised version.

• page 441, line 4: Provide units for DDFs.

*mm/(d*K) will be added.*

• page 441, line 22: Is there an explanation for the increase in melt with debris thicknesses larger than 10 cm (see Fig. 7)?

We present all measurements carried out in Fig. 7. As can be seen, the measurements for the short periods show only a rather small variation and follow the general theory about decreasing melt rates with increasing debris thickness. The measurements covering the entire seasons show a much larger deviation from the theoretical melt-debris thickness relation. There are several reasons for this: First, debris thickness might not be constant over the entire ablation season. Especially at Zopkhito glacier the thicker debris usually occurs on exposed ridges which are prone to stronger changes over time. Strong precipitation events which are frequently observed in the Zopkhito basin (summer thunderstorms) might also add to variations in the debris thickness. We measured debris thickness only after installation of the ablation stakes in order not to disturb the natural stratigraphy of the debris cover. But this implies an uncertainty about the temporal stability of the debris cover. In addition the measurements later in the season were performed by other colleagues who might have carried out the measurements in a slightly different way. For completeness all measurements are provided in the manuscript. However, we used only data guaranteed quality for the extraction of the ablation function. This will be explained thoroughly in the revised manuscript.

• 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.

We are rather concerned with the quality of the field measurements and the derived parameters. In the manuscript we do not report about temporal variations of the degree day factors. As long as the composition and structure of the debris cover remains stable there should be no significant changes in DDF. Degradation of the debris cover and stratification due to precipitation events however will influence this temporal stability. This is especially true for small supra-glacial debris patches where redistribution of debris is more simple than for extensive debris covered glacier areas. The *determination of the thermal resistance is somewhat more difficult, because the measurement of the vertical temperature gradient in the debris column is prone to higher uncertainties (Nicholson and Benn, 2006). Especially the daily cycle of the temperature gradient and the correct determination of the debris surface temperature leave some room for variations in the parameter calculation. This requires a more detailed discussion. It can be shown that the variation in the thermal resistance for a given test site can be described within a certain error width. This will be shown in the revised text.*

• 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?

This assumption is justified if a thorough mapping of the debris cover is taken into account. It is crucial that a mean debris composition is chosen for the elevation band of the glacier, depending on the natural variation observed across the glacier. For a single small area conditions can vary strongly during the season. For the considered elevation bands of 50 m it can be assumed that the mean conditions will not change to a large degree. In our opinion: the relative effect of the debris cover within the entire altitude band is best represented with the mixing formula we provide in the paper.

• 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.

The function was derived based on the measured ablation for different debris thicknesses as shown in Fig. 7. We will show the function in a figure together with the used input data. Also the possible band width for the function will be discussed. In general the function is less well defined for very small debris thicknesses, because there debris thickness varies over time more easily and the measurements are more difficult. For larger debris thicknesses the function is usually rather well constrained.

• 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.

This criticism is in fact crucial for the applicability of our method to larger areas. In our manuscript we apply this model for rather limited glacier groups, where we have at least some information on neighbouring glaciers. For the Adylsu basin we know from neighbouring glaciers that the debris cover zonation is similar. In the Zopkhito basin there are basically only two glaciers with a significant debris cover. On Zopkhito glacier the debris cover was measured, Laboda glacier has a rather similar hypsography and debris composition. We agree that this assumption might be weak for larger areas, but we are confident that for our areas of investigation it holds.

• 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.

The main motivation for the inclusion of the neighbouring glaciers is also the main motivation for the paper: comparing glacier ablation in two basins North and South of the divide. In doing so, we want to demonstrate the characteristic differences (therefore also the discussion about the catchment wide debris distribution) and not only restrict the analysis on two glaciers where conditions might be rather specific. We suggest to include also the model results for the two measured glaciers. This will enhance the understanding of the relation between the single glaciers and all glaciers in the catchment.

• 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).

Cloudiness is not directly implemented in the model. However, cloudiness has a strong effect on air temperature. We use air temperature as the main driver in the model and thus we discuss possible reasons for the modeled differences. A higher degree of cloud cover was observed during the periods of field work and this could be one plausible reason for the differences observed in the model results. We will address this problem more explicitly in the text.

• 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.

We agree with this statement. The glacier terminus elevation is dependent on the glacier geometry (size of accumulation basin, hypsometry, width of the glacier tongue, mean surface slope), but also on the meteorological conditions: total accumulation and the ablation conditions. The ablation conditions are themselves dependent on the meteorological conditions and therefore on radiation as well. Cloudiness therefore is a determining factor for the position of the glacier tongue.

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