## Response to Anonymous Referee #2

*We appreciate the efforts of both reviewers to critically evaluate the manuscripts and provide constructive suggestions for improvements. We have responded to each comment in detail below. Our responses are formatted in italics.*

**Anonymous Referee #2 Comments**

Summary of the paper:
The authors present observations of melt rates on Maliy Aktru glacier in the Russian Altai and analyse them with respect to the influence of debris cover. They set up a degree day model applicable to both debris covered and debris free glacier surfaces. The model is calibrated with the observations from the tongue of Maliy Aktru and then applied to calculate ice melt on two neighbouring glaciers and finally for a larger set of glaciers in the Northern Chuya ridge. The authors conclude that their model is able to realistically simulate ice melt on debris covered glaciers.

General Statement:
The present manuscript reports interesting observations of glacial melt from a region where such data are probably not available yet. I do appreciate the effort to provide data from regions that have so far received little attention. However, I do have serious concerns about the comprehensibility of the method, the scientific quality of the modelling approach and the way the results are presented. I would like to encourage the authors to substantially revise the paper. Please find below a list of my major concerns followed by more detailed comments:

*We acknowledge the efforts of the reviewer to provide substantial suggestions for improving the quality of the manuscript. We also recognise that some parts of the paper are not well constrained, explanations misleading and that the data basis is far from optimal. Still we are convinced that the paper provides insights into the glaciological conditions of a region not covered by ablation studies so far. We tried to use the available data for the application of a melt model including the debris covered tongues of the glaciers. Observations of glacier change and debris cover variation over time are a valuable contribution to the discussion of the future glacier reaction. The chosen approach for characterising glacier melt is simple, but physically sound. We will improve the model description so that this can be judged by the reader. Based on the scarce data and the quality of some available data, the application of the model is difficult. Nevertheless we are convinced that the conclusions are valid and we will improve the manuscript in order to present a critical evaluation of our results and their consequences.*

1. To my opinion, the major findings of this study are not particularly new. There has already been a number of studies published focusing on the influence of debris cover on glacial melt. While the observations from the Altai would make a valuable contribution to e.g. a paper comparing the influence of debris cover in different mountain areas, they are a rather narrow base for an independent paper.

*The key issue of this manuscript is the characterisation of the influence of debris cover on the melt regime in the Altai region. Field data are used as base line for the calibration of a melt model which is developed especially for situations with a rather poor data situation, as it is the case in the Altai. As there exists no information so far about the status of the Altai-glaciers in respect to their debris cover and their temporal evolution, we think that this manuscript provides interesting and scientific valuable results for a so far not very well known region. Because Maliy Aktru glacier is a benchmark glacier of the WGMS and some long term data are available, it was obvious to choose this region for the first application of our methods.*

2. The paper does not solely focus on the observations from Maliy Aktru but an attempt is made to set up a model using these observations and to apply this model for calculating ice melt for the entire Northern Chuya ridge. However, this attempt is seriously hampered by limited data availability. I do have doubts that the conditions on Maliy Aktru can be simply applied to all other glaciers. Assuming that the snow cover is distributed evenly over all glaciers is a critical simplification. I do understand that the data base for studies like the present one is sparse in the Altai. To my opinion this must be at least addressed through an uncertainty assessment.

*We agree that the conclusions drawn from our investigations are based on many simplifications. Still, the major findings are valid and also significant for the characterisation of the general melt conditions in the region. The investigations at Maliy Aktru mainly provided information about the thermal conditions of the debris layer, the general debris cover composition and the melt parameterization. However, the general setting of the glaciers in the Chuya Range, at least for our sample, is rather homogeneous in respect to altitude range and mean glacier size. There are no major variations in geology, therefore the general glacier appearance (e.g. accumulation basin elevation, tongue elevation), as well as the debris composition are similar. Therefore we are convinced that the extrapolation of the situation at Maliy Aktru to the other glaciers is justified. In order to address the reviewers concerns and to justify our approach we will add a paragraph about the comparability of the glaciers and their setting in the region and we will also provide a critical assessment of our results in respect to the uncertainties of the input information.*

3. Uncertainties in input data, model parameterizations as well as in the data used for model validation are not thoroughly discussed. To my opinion stating that the model results are roughly similar to previous studies is insufficient. This applies even more because similar studies have not yet been performed in the Altai and consequently might not be directly comparable.

*We agree that a more thorough description of the quality of input data and the uncertainties in the model parameterizations are needed. As the reviewer points out, such studies have not been performed before in the Altai. This makes it worthwhile in our opinion to present our results to the scientific community. But we accept that we have to perform a stricter quality assessment of our results. Unfortunately the data basis for validation is very weak. We tried to obtain additional data from Russian colleagues which will improve our data basis. In any case we will discuss the constraints of the model parameterization in more detail and provide appropriate information about the model validation.*

4. The study lacks a clear model validation. For the reasons explained in the detailed comments below, the comparison to old runoff measurements is regarded as of limited significance.

*As explained above we try to obtain additional data (mass balance and meteorological data coincident with the runoff data) which will allow us to perform a better model validation. If these data are not accessible, we will at least provide a thorough error analysis of the model results in relation to the existing model validation.*

5. The model description is difficult to follow and the way short wave radiation balance was incorporated is unclear to me. Furthermore statements in the model description raise the concern that there are basic misunderstandings about the glacier surface energy balance. It is essential that the relevant equations are shown and that all model parameters are listed together with their respective values.

*We will provide a restructured model description in the revised version of the manuscript, where all parameters are discussed and listed. Some probably misleading statements in the model description have been identified and we will provide a better explanation. Also the parameterisation of the potential solar radiation will be improved. We do not use measured short wave radiation, as this parameter is not available for longer time spans. But we use the potential solar radiation as a measure of the shading effect, similar to the approach of Hock (1999). This will now be described in more detail.*

6. The structure of the paper must be made clearer. A comprehensive listing of all data sets is required. At the moment there are a certain observations mentioned which seem not to be used in model calibration/validation, while other observational data sets are only introduced in later sections.

*The paper structure follows a logical path beginning with the analysis of debris cover, the field measurements, the model description and the model results. However, it seems that this*

*structure is misleading and produces confusion. Therefore we will restructure the paper according to the suggestions made by the reviewers.*

7. The authors express many quantities as a percentage. This makes it very difficult to follow and interpret the actual results. In several cases it is unclear to me what the reference quantity is. Expressing a temperature change in °C as a percentage is not possible! To my opinion, quantities should be given as their absolute value and only if sensible as a percentage.

*We will consider this in the revised version of the paper. In our opinion a relative increase or decrease of a parameter often explains more of the significance than the absolute value. But we will provide the absolute values as well and reconsider where it is sensible to use relative values.*

8. The graphical quality of most figures is poor. In conclusion, I recommend to the authors to base their calculations on a sounder theoretical base and on an improved observational data set. A PDDF model can be an appropriate model, but it nevertheless requires a theoretically sound background as well as a detailed and comprehensible description of model calibration and validation. Currently the available observations might not be sufficient for an independent publication. The data, however, could be incorporated into a broader study involving data from other regions. As I've seen, this was already done in the study by Lambrecht et al. (2011).

*We do not agree that the graphical quality of the figures is poor. This might be a personal view point, and there is always room for improvement. But in general the figures provide the necessary information and are clear. In our opinion the data base of available observations is worthwhile to be used and published, even though we would like to have a more sound basis for the analysis. The remote sensing analysis provides important information about the evolution of the glaciers and the field data combined with existing mass balance data and climate data are suitable to perform an interesting model experiment. The degree day approach is suitable to investigate areas with poor data coverage and this is the reason we have chosen this strategy. Results from this study are used in the Lambrecht et al., 2011 submission for a comparison of different regions. But this is not the focus of this study. Here we want to present the evolution of glacier ablation in a so far not well known region, which is one of the major glacierised regions in Russia. This study could provide a valuable baseline for future more focussed studies in the region.*

Detailed suggestions and corrections:

1. Page 402 Line 15: Which weather station is meant with AWS?

*The weather station in front of Maliy Aktru glacier is meant. This will be added to the text.*

2. Page 404 Section 2: On one hand certain data (thermistors) are mentioned in this section while it is unclear if they were used in the model setup or not. On the other hand the mass balance observations from Maliy Aktru (WGMS, 2007), the runoff data and also the observations from Praviy and Leviy Aktru are not mentioned. Personally I would prefer if all data used are briefly listed in this section. Data not used should not be mentioned at all.

*As mentioned above the manuscript will be restructured, so that all data are described in one section and the use of the data in the study will be described in detail.*

3. Page 405 Lines 3-5: Have these measurements actually been used in this study? They are mentioned again in section 4 but it remains unclear if they have been involved in the model setup. If not, I would not mention them.

*These observations were used to determine the thermal properties of the debris cover. This will be added in the text, including a description about the procedure to derive the thermal conductivity.*

4. Page 407 Lines 2-4: How was 4-5 % calculated? Please specify in more detail.

*The error margin was determined by delineating the glacier with +/- 1 pixel, calculating the areas for maximum and minimum extent and deriving the relative area variation.*

5. Page 407 Line 8: Please specify if mean ELA approx. 3300 m a.s.l. is your own observation or if this is from WGMS or another source.

*The mean ELA is the result of long term observations in connection with the mass balance measurements in the Aktru basin by Y. Narozhny*

6. Page 407 Line 16: I would recommend citing this source as WGMS (2007) instead of Haeberli et al. (2007).

*Will be done*

7. Page 409 Lines 2: Lambrecht et al. is once cited as "submitted" and once as "2011" (Page 414 Line 23).

*Lambrecht et al. is submitted, will be corrected.*

8. Page 410 Eq. 1: What stands "i" for?

*"i" indicates ice, but we will remove it, because at the moment only a constant degree day factor for ice is used.*

9. Page 410 Lines 6-9: How was this done? Was the result a map of spatial distribution of debris cover thickness or a map without spatial thickness information? To my opinion these two questions are of major importance for the given study and must be made clearer.

*Debris cover thickness was measured in detail on Maliy Aktru glacier and at some locations also on the other Aktru glaciers. As a result we retrieved a map of debris cover thickness for the Aktru basin. This was the basic information for estimating debris cover thickness also on the other glaciers. We will improve the description of the method and also include a section about the potential errors in the model results by the uncertainties of debris cover thickness.*

10. Page 410 Line 9: I am not sure if covering a large altitudinal range and different aspects is sufficient for the three glaciers to be representative for an entire mountain range. Furthermore, to my opinion you actually extrapolate from one glacier (Maliy Aktru) to the entire Northern Chuya ridge: I understand that you use only mass balance measurements (WGMS data and own observations) from Maliy Aktru. I do not see that first extrapolating to Leviy and Praviy Aktru and then to the entire ridge enhances representativity of Maliy Aktru unless you do have observations from Leviy and Praviy Aktru. You mention on Line 8 that there are some observations. However, these observations are not mentioned in the "Study area and data compilation" chapter and it remains rather unclear to what kind of observations you refer.

*The observations from the other glaciers will be included in the new "data" section. It is not the intention of extrapolating first from Maliy Aktru to the Aktru glaciers and then extend the study to the Northern Chuya range. We apply our method to three cases, where the uncertainties increase with the uncertainties, by going up to the next size of glacier ensembles. This means we obtain results which are rather precise for Maliy Aktru, but still provide a reasonable accuracy for the entire range, due to less precise input data. The question if the Aktru glaciers are representative for the entire range is different: The glaciers cover most of the area range of the glaciers in the sample. They also extend over the altitude range observed in this mountain range, they provide the major aspects and the range is rather small, so that geology and meteorological conditions will not change considerably across the region. We recognise that some additional information should be given in the manuscript about the general setting of the area and the local conditions, in order to demonstrate the validity of our assumptions.*

11. Page 410 Lines 15-26: To my opinion, this is a critical simplification which at least requires an uncertainty assessment (e.g. based on a sensitivity study where the influence of different assumptions on snow distribution is evaluated). To my own experience, snow and ice melt are not trivial to separate because of snow disapearing at

different points in time over the glacier area. What PDDF for snow do you exactly use? It is stated that the PDDF are similar to an earlier study. Please state the exact values together with the reference.

*We agree that this is a major simplification of the modelling study. However, as we already discuss in the manuscript, the most critical issue is the onset of ice melt due to a non-even distribution of the snow cover. Because there are no data on snow distribution available this is the most obvious approach to be taken. But in fact the uncertainties should be noted and discussed. Therefore we will perform a number of sensitivity studies for different elevation dependent gradients of the snow cover, in order to demonstrate the relative influence of potential snow cover variability on the ice melt conditions. The degree day factor for snow was taken from the given publication (Singh et al., 2000). But these authors give a range of degree day factors for snow (5.8 – 6.4 mm/°Cday) in dependence of albedo. We used the lowest value for clean snow.*

12. Page 412 – 413, Section 6.2: Unfortunately the first two paragraphs in this section are very much unclear to me. The description of the influence of enhanced solar radiation on sub-debris melt is puzzling. Debris cover thickness and heat conduction must play a large role here. How is this addressed? The statement about the limited ability of ice to exploit solar radiation seems physically wrong, or I assume it was formulated in a way that makes it easy to misunderstand - please clarify. I gain the impression that your interpretation of the influence of solar radiation is solely based on assumptions? There are a number of physically based models available that would allow calculating the influence of solar radiation on glacier melt. Using them would at least strongly improve accuracy of the assumptions made in this paragraph. 13. Page 412 – 413, Section 6.2: After reading through Section 6.2 I do not understand if solar radiation was considered in the model or if this is just a discussion on its potential influence if it would have been considered. If it was considered, please provide a clear and comprehensible model description showing the applied equations, the parameters and their respective values. If it was not considered, then move these paragraphs into the discussion.

*The description of the relative influence of the solar radiation seems to be misleading and thus we will rewrite the entire paragraph in order to provide a clear description of the approach we used. In principle we included the relative radiation budget from potential solar radiation to include the effect of slope and aspect in the calculation of ice melt. This is even more important for debris covered glaciers, as the debris surface can be heated by radiation well above the ambient air temperature. The main way of energy transport through the debris cover is by conduction, but this process is already incorporated in the application of thermal conductivity in the model. The wording of "limited ability of ice to exploit solar radiation" is misleading and will be removed. The role of supra-glacial debris for heat conduction to the ice surface will be described in a better way. We will use the scarce radiation data to calculate the potential influence, in order to compare this with our assumptions on the role of radiation in the sub-debris melt conditions. As discussed above, our approach includes the potential solar radiation in order to account for shading effects, but measured solar radiation is not available for longer periods.*

13. Page 412 – 413, Section 6.2: Please specify absolute values: The information that solar radiation is 90% larger on one glacier than on the other is of limited evidence when there are no absolute values given. Absolute values cannot be extracted from Figure 7 since values can only be determined with low accuracy.

*Absolute values for the potential solar radiation depend on the specific location on the glacier and the date. Therefore, in our opinion the relative difference in radiation between averaged areas on different glaciers are easier to understand. The relative difference of course can only be quantified for given absolute values. We can provide mean radiation values for the given example of 15. July, separated into ablation and accumulation area of the different glaciers. This might provide a basis for evaluating the radiation differences on the individual glaciers.*

14. Page 413 Line 11: How are these more favourable accumulation conditions addressed? To my interpretation this statement shows once again that assuming a uniform accumulation distribution is a critical simplification.

*We agree that this conclusion might not be supported by the data and the relation to accumulation conditions might be speculative. Therefore we will remove this paragraph of the interpretation.*

15. Page 413 Lines 25 to Page 414 Line 12: This paragraph is unclear to me. If 6.6 million $m^3$ are 67% of 9.8 million $m^3$, how can 10.8 million $m^3$ correspond to 70% then? I assume that the theoretical ice melt without debris would be 7.6 million $m^3$ which corresponds to 70% of 10.8 million $m^3$. The glacier area of the three Aktru glaciers is 13.6 million $m^2$. Dividing 6.6 by 13.6 results in a mean summer balance of ~ -0.5 m w.e. However, according to Table 3 the summer balance on Maliy Aktru is -0.8 to -0.9 m w.e. and there could be a significant amount of snow melt which is not considered? How can ice ablation and runoff be smaller in reality than the 6.6 mio. $m^3$ calculated? I understand they already refer to 2000 - 2002 where glacier debris cover was already increased and glacier area had decreased. I agree that snowfall during summer might not have a large influence nowadays, but it might have had a significant influence back in the 60s and 70s. On the contrary the influence of rain on runoff might be very large: the total area of the catchment is approx. 31 $km^2$ and already a conservative assumption of 0.3 m summer precipitation results in over 9 million $m^3$ of rain. Together with the modelled ice melt of 6.6 million $m^2$, potential runoff adds up to a minimum of 16 million $m^3$ and snow melt is not yet considered. How do you explain the large difference to the existing measurements? Given all the uncertainties in the runoff measurements, the differing time frame, the change in climate, the fact that only ice melt was modelled and that model calibration relies only on one glacier, I do have serious doubts that the model results can be validated this way.

*This might be written in a misleading way in fact. The 10.8 million $m^3$ are calculated for the case with no debris cover assuming the same precipitation and snow melt conditions as in the original case. Therefore the relative contribution of the ice melt to total run-off would increase to 70%. The mean summer balance for ice melt is about -0.5 m w. eq. which is only about 60% of the measured summer balance. But there is no discrepancy in these values, as the missing 40% of the summer balance are attributed to snow melt mainly in the accumulation area. Still there remain uncertainties in such an evaluation with discharge data from a different time period. Therefore we will rewrite this paragraph in order to make clear that this comparison is only a weak validation, but the best what could be done without additional data available. Summer snowfall has no significant influence on the mass balance of these glaciers and a comparison with the climate data of the two periods that was also not the case during the time of the discharge data.*
*For the years with discharge data, the mean summer precipitation at Aktru station was about 230 mm. Given that there is an increase of precipitation with altitude the total amount of summer precipitation will be in the same order of magnitude as the total measured run-off. We agree with the reviewer that the quality of the discharge data will be very problematic for validation of the model. Therefore we will concentrate on finding another solution for model validation. One possibility could be to use the single years of mass balance measurements on other glaciers to validate the model for another specific glacier application.*

16. Page 414, Section 6.3: Did you do spatial modelling (=elevation bands) using a map of debris cover or is this a rough assessment of the influence of debris cover? What is the debris thickness you assumed or derived from remote sensing? This is unclear to me. I do not agree that these values can be compared to the studies of Nicholson and Benn (2006) and Reid and Brock (2010): To my understanding, the value of 20% refers to a mean reduction of ice melt for the entire glacier area. Both aforementioned studies were devoted to the description and validation of a glacier surface mass balance model including debris cover. The analysis, however, was restricted to the influence of different debris coverage at the point scale. You state that you calculate only ice melt but then you write " . . . the contributions of the glaciers . . ." I assume that there is a considerable amount of snow melt during summer.

*The model works with "spatially distributed" input data, averaged over elevation bands. Thus we can use maps of debris cover for the relative influence of the debris cover on melt. The thickness distribution was derived from field measurements on the glacier tongues in the Aktru basin. We will remove the studies of Nicholson and Benn (2006) and Reid and Brock (2010) in this context and compare our findings to other studies (e.g. Rana et al., 1997) which consider sub-debris melt over larger areas.*

*The formulation "the contribution of the glaciers" will be changed, as summer snow melt cannot be calculated with the desired accuracy.*

17. Page 414 Line 14: It is not possible to express a temperature change in °C as a percentage. Simply state that the temperature has risen by 1.5°C

*We will do so.*

18. Table 3, 4 and Figures 5,6,8: Do these values in mm (cm) refer to ice equivalent or to water equivalent?

*All values refer to mm w.eq. and it will be included in the captions.*

19. Figure 1: Please improve the quality of the map. Hillshading and/or contours on the glacier surfaces would ease interpretation

*We tried several options for improving the legibility of the figure. We will provide the best result (probably additional contours across the glaciers) in the revised version.*

20. Figure 4: I suggest making the bars for "debris covered area" more narrow because the other bars are difficult to read where covered by them.

*We will produce a revised figure accordingly.*

21. Figures 6 and 8: I would suggest exchanging x and y-axis as this is more intuitive and common praxis in displaying mass balance profiles. Furthermore the y-axis is already used for elevation in Figure 4 and comparability of the figures would be improved.

*Will be done.*

22. Figure 7: Please improve the quality of the map. There is no need to show an ELA outside the glaciated area. Add contour-lines to ease interpretation. I would recommend changing the units to $Wm^{-2}$: the Information is the same but $Wm^{-2}$ is widely used and thus findings from this study can more easily be compared to other studies.

*Adding contour lines will make it difficult to see the differences in radiation. But we will remove the ELA outside the glaciers and display the clear sky radiation in W m-2.*

References:

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