

## ***Interactive comment on “Glacial debris cover and melt water production for glaciers in the Altay, Russia” by C. Mayer et al.***

**Anonymous Referee #2**

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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.

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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:

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.
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.
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

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even more because similar studies have not yet been performed in the Altai and consequently might not be directly comparable.

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.
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.
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.
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.
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.

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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).

Detailed suggestions and corrections:

1. Page 402 Line 15: Which weather station is meant with AWS?
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 Leviiy 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.
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.
4. Page 407 Lines 2-4: How was 4-5 % calculated? Please specify in more detail.
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.
6. Page 407 Line 16: I would recommend citing this source as WGMS (2007) instead of Haeberli et al. (2007).
7. Page 409 Lines 2: Lambrecht et al. is once cited as "submitted" and once as "2011" (Page 414 Line 23).
8. Page 410 Eq. 1: What stands "i" for?

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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.
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.
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 disappearing 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.
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,

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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.

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.
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.
15. Page 413 Lines 25 to Page 414 Line 12: This paragraph is unclear to me. If 6.6 million m<sup>3</sup> are 67% of 9.8 million m<sup>3</sup>, how can 10.8 million m<sup>3</sup> correspond to 70% then? I assume that the theoretical ice melt without debris would be 7.6 million m<sup>3</sup> which corresponds to 70% of 10.8 million m<sup>3</sup>. The glacier area of the three Aktru glaciers is 13.6 million m<sup>2</sup>. Dividing 6.6 by 13.6 results in a mean summer balance of  $\approx -0.5$  m w.e. However, according to Table 3 the summer balance on Maliy Aktru is  $\approx -0.8$  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

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reality than the 6.6 mio. m<sup>3</sup> 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<sup>2</sup> and already a conservative assumption of 0.3 m summer precipitation results in over 9 million m<sup>3</sup> of rain. Together with the modelled ice melt of 6.6 million m<sup>2</sup>, potential runoff adds up to a minimum of 16 million m<sup>3</sup> 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.

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.
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.

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18. Table 3, 4 and Figures 5,6,8: Do these values in mm (cm) refer to ice equivalent or to water equivalent?
19. Figure 1: Please improve the quality of the map. Hillshading and/or contours on the glacier surfaces would ease interpretation
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.
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.
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<sup>-2</sup>: the information is the same but Wm<sup>-2</sup> is widely used and thus findings from this study can more easily be compared to other studies.

#### References:

Lambrecht, A., Mayer, C., Hagg, W., Popovnin, V., Rezepkin, A., Lomidze, N. and Svanadze, D.: A comparison of glacier melt on debris-covered glaciers in the northern and southern Caucasus, *The Cryosphere Discuss.*, 5, 431–459, doi:10.5194/tcd-5-431-2011, 2011.

Nicholson, L., Benn, D.I.: Calculating ice melt beneath a debris layer using meteorological data, *J. Glaciol.*, 52 (178), 463-470, 2006.

Reid, T. D. and Brock, B. W.: An energy-balance model for debris-covered glaciers including heat conduction through the debris layer, *J. Glaciol.*, 56(199), 903–915, 2010.

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Interactive comment on The Cryosphere Discuss., 5, 401, 2011.