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Discussions

# Interactive comment on "Impact of varying debris cover thickness on catchment scale ablation: a case study for Koxkar glacier in the Tien Shan" by M. Juen et al.

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general

In this paper the authors present ablation measurements on a debris-covered glacier in the Tien Shan mountain range. These measurements are used to derive degree day factors for debris covered ice, depending on the thickness of the debris layer. Secondly the debris layer thickness is calculated from surface temperatures observed with a satellite image. The thickness calculations are done with three types of fitting functions and, in addition, a distinction is made for supra glacial lakes, ice cliffs and 'normal' debris covered glacier area. The combination of the calculated debris thickness and

the degree day factors for debris covered ice are then used to calculate the ablation on the glacier during one ablation season. The authors suggest that this simple approach provides promising application in large-scale modelling of run-off in catchments with debris-covered glaciers.

Both the measurements and the attempt to arrive at simplifications that make it possible to include debris covered glaciers in large-scale models, like hydrological models, are interesting. Also the result that the ablation at ice cliffs has a smaller contribution to the ablation of debris covered glaciers than was found in previous studies is interesting. Nevertheless, I think the paper could be improved significantly.

First of all, I think the measurements deserve a more prominent place and an extensive description in this paper (or, if published elsewhere, a clear summary and reference). More than 20 stake measurements were done on the debris covered part of the glacier and in addition a debris thickness profile and multiple weather stations are mentioned in section 3. These measurements deserve to be elaborated more: indicate at which altitudes and locations the stakes were placed (i.e. in Figure 1), what the weather pattern during the measurement period was, what the typical difficulties were with the measurements of ice loss and debris thickness and what the associated uncertainties in these measurements are, how often were they measured – is there a timeseries of melt that could be combined with meteorological data?–, and possibly other relevant information. Measurements are interesting by themselves and they are essential to the results presented in the rest of the paper.

Secondly, the degree-day method should be explained further. Typically, the daily ablation is expressed in the degree-day factor times the difference between daily mean temperature and a threshold temperature. However, there exist also papers that use monthly or hourly average temperature, and the threshold temperature can vary as well. At present, the method isn't specified in the paper. You need to state exactly which relation between ablation and temperature is used, and thus how the degree-day factor is defined. Moreover, I miss an explanation on physical grounds why we would

expect a degree-day approach to work in describing the ablation of debris-covered ice. Especially since one of the conclusions of this paper is that the influence of temperature is of minor importance for the ablation, you should first argue, starting from the energy budget of debris-covered ice, why an expression for the ablation that depends solely on air temperature gives a reliable approximation.

The transfer of surface temperature of the debris into debris thickness is not really convincing. Looking at Figure 4, the scatter in the data points is so large that the fits are not well constrained. In principle, it could be anything. This is illustrated by the fact that for the debris thickness at the glacier tongue the differences between the different fits (Fig. 5b,c) are of the same magnitude as the results themselves (Fig 5a). Apparently, there are no measurements on the glacier tongue to decide for a specific fit. The calculated ablation, which is the main result of the paper, is strongly dependent on the debris thickness. I recommend to increase the reliability of the calculated spatially distributed debris layer thickness by, e.g., using the combined results of multiple ASTER images and/or by using a more advanced method such as described in Foster et al (2012) A physically based method for estimating supraglacial debris thickness from thermal band remote-sensing data, J of Glaciology 58, 201, p. 677. Hopefully, that helps to better constrain the calculated debris layer thickness.

I think the structure of the paper could be improved to enhance the readability. A major improvement would be to explain the DDF method that you intend to use in the introduction or at least before you come to the data that are needed as input. Then you can avoid to use phrases like "required temperature record for the model." (p 5311 ln 5-6) before you have explained the model. Also at smaller scales the text could be re-organized, e.g. on page 5313 you discuss the mapping of cliffs (ln 10), then switch to lakes (ln 15) to come back to the cliffs again (l 18).

specific points:

p 5307: title mentions catchment scale ablation, but you never discuss catchment scale

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ablation in the paper. Only in the outlook the developed method is recommended for larger scale.

p 5308 l 8: by "ground truth" you mean in-situ point measurements?

p 5308 l 24-26: here sources and means of transport of transport are all listed as source

p 5309 l 12-18: to what extent do simple models reduce the input needed to calculate the MB? In principle, many of the parameters in the physical models can be estimated, taken constant or otherwise approximated such that the same number of input variables is reached as in the simple models.

p 5309 l 17: have been proven to produce realistic results in other, earlier, studies? Then references are needed!

p 5309 l 19: natural? Ice cliffs are also natural.

p 5309 l 28: "complex physical" is an unexpected end of the introduction. It seems to contradict the "robust conceptual" of l 17. Mention the DDF method you intend to use.

p 5310 l 25: "during installation" is that once, when the stakes were placed, or a time series?

p 5311 l 1: replace "moraine" with "debris" here and all other occurrences where moraine is used for the debris on the glacier surface.

p 5311 l 6: the model has to be introduced before you can state "the required temperature record"

p 5311 l 6: what other quantities were measured with the AWS?

p 5311 l 12-22: were DEM and ASTER images co-registered?

p 5312 l 1: introduce the ablation model (see general comments)

p 5312 l 1-15: The lapse rates appear to be a tricky part in the story. These AWSs

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measure near surface temperature and if one is on a debris/land surface and a second one on bare ice, you can expect the measured temperature of the second to be lower (during daytime) due to the cooling by the melting ice surface. This partly explains the larger gradient. But this is not just a temperature effect as the heating of the debris surface is largely a radiation effect. If you need detailed measured near-surface temperatures in your method, it will be difficult to apply it to other locations. And this application to other, and larger, areas was the purpose of the simplified approach. Wouldn't it be a good idea to use reanalysis data to force the model, and to test the results with the in-situ observations? In addition, how short is the period of measurement in 2003 and 2004, and is this period long enough to get representative data?

p 5312 l 9: with "determined empirically" you mean that you use the measured ablation and debris thickness and the calculated temperatures?

p 5312 l 10: is this linear part supported by your field observations? How many data-points are included in this part of the fit?

p 5312 l 22: How is the comparison between measured and calculated ablation for these ice cliffs? How many data points are involved? It would be a could idea to include graphs like Fig 2 for the ice cliffs as well.

p 5312 l 24: New paragraph, new subject (geometry of cliffs instead of degree day factors)

p 5313 l 1-9: move this part, such that it follows up on the DDF of ice cliffs.

p 5313 l 3-5: But what about the larger heat capacity, lower albedo and higher rates of heat transfer of water? The lakes increase melting because of these properties (larger absorption of incoming solar radiation which energy is effectively used for ice ablation), but you restrict the ablation by lakes keeping the surface temperature at 4 degrees and using the same DDF.

p 5313 l 10-15: should be one paragraph. And I do not understand this (but I'm not a remote sensing expert)

p 5313 l 18-23: move to line 10

p 5313 l 22: what is a fundamental DEM?

p 5314 l 8: the surface temperature should go to zero in the limit of zero debris thickness, also when the exponential function is used.

p 5314 l 13: How was this resampling done, and to what extent can we expect the resampled temperature fields, and therefore calculated debris thickness, to represent features at sub-grid (90x90 m) scales?

p 5314 l 16-19: delete repeated sentence "The resulting mean ... in Fig. 5"; move sentence "The patterns ... can be assumed." to the end of the previous paragraph.

p 5314 l 19: "can be assumed" you mean "can be found" or "can be expected"?

p 5314 l 21: please rephrase first sentence, and do these three sentences require a subsection?

p 5315 l 5: please rephrase sentence, it is hard to understand.

p 5315 l 7-8: a) How is the degree-day factor for debris-free ice determined? That is not mentioned earlier in the manuscript. b) And you probably mean ice ablation beneath supraglacial lakes instead of supraglacial debris. Otherwise, I do not understand this. c) "are the same".

p 5315 l 22: How then does this explain the difference between your results and Sakai (1998)? Please explain further.

p 5316 l 5: already in steady state one would expect a low flow velocity for debris covered glaciers as the mass turnover is small due to the limited melt. Low mass turnover requires small flux and thus a low velocity. A small velocity requires a small

surface slope. Mass loss is not really needed to explain this characteristic.

p 5316 l 7: please rephrase "imaginary debris-free glacier" into something like "the ablation if no debris were present on the glacier surface".

p 5316 l 8-12: This line of reasoning false: difference in ablation in a particular climatic setting does not imply that the response to a change in the climatic setting is different. I.e. the result of 1 degree warming could be that for both the debris-covered and the debris-free ice the ablation increases with 0.5 m w.e. a-1. Or that in both cases it leads to an increase in ablation of 10%. Thus different ablation pattern does not necessarily imply different climate sensitivity!

p 5316 l 17-20: I would formulate this result as: At lower elevations, the increasing debris thickness compensates the higher temperature. But this is a tricky result as in your model setup the ablation is completely defined by temperature and debris thickness. Therefore this finding is a direct consequence of your parameter choice, rather than an independent result.

p 5317 l 6: I think Kääb et al 2012 had the same confusion regarding differences in a certain climate and the impact of climatic change as you. see comment above.

p 5318 l 25: I do not really see how this method can be applied to other glaciers without measurements. Do you claim that the DDF and the fit of surface temperature to debris thickness that you found are applicable to other glaciers without further tuning? And what temperature forcing will you use, as for other glaciers there are no AWSs near the tongue, nor measurements of the varying temperature lapse rate. If you apply the model to other glaciers, how would you calculate/estimate the uncertainty in the calculated ablation?

Figures, general: have a look at the layout. Fig 2 has a very large font for the axis labels, while the legend and labels of Figs 3,5,6,8 are so small they are very difficult to read.

Fig 1: include stake positions

Fig 3: colours of cliffs west/east and north are hard to distinguish

Fig 4: why are the limits of the axes so large? Data points range from 0 - 8 C and 0 - 120 cm but the axes run from 0-22 and 0-300. Why?

Fig 5: The differences in debris layer derived from the different fits are near the glacier tongue almost as large as the debris layer itself. Are there no measurements or estimates of the actual thickness at all? You probably passed this area multiple times during your expedition.

Fig 6: Distribution of calculated total ...

Fig 8: pixel is not a very useful unit, give meters instead (in a larger font). Indicate zoomed area in the mean figures and put a box around the inset. Also mention the insets in the figure caption.

Fig 9: blue, grey and black are not colours that are easy to distinguish. Why not black, red and green (for instance)?