Response to Reviewer: 1

We thank the reviewer for consideration of the manuscript and the useful comments.

General comments:

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.

The location of the stakes as well as the debris cover thickness measurements are now included in Fig. 1 as supposed. It's very complicated to publish chinese climate data. We are allowed to use the temperature record to run our model but not allowed to publish the time series of the temperature data. Stakes were measured several times, but not on a daily basis so we could combine it with meteorological data (that we can't publish anyway)

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 degree day method we use is explained further now in the ablation model design section of the manuscript.The explanation on physical ground why a degree day approach works can be found in a number of publications (e.g. Ohmura, A., 2001. Physical basis for the temperature-based meltindex method. J. Appl. Meteorol. 40, 753–761.), I don't think this has to be stressed in every publication using temperature index methods and it is common knowledge that there is a high correlation of air temperature with several energy balance components. The degree day – debris cover thickness relation is basically the same idea and has been used to model ablation successfully before (as describesd in the manuscript, see Hagg et al. 2007).

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 agree that the LST – DCT relation is not convincing and that is the reason why we are using three different regressions. But there are measurements of DCT in the tongue are. I pointed out that the exponential regression seems to be the most realistic when compared with the GPR measurements from Wu and Liu 2012.As Doug Benn suggested I added a Figure that shows the spatial distribution of DCT from Wu and Liu 2012. The method Foster et al. are using is very interesting but not that easy to apply in central Asia, where reanalysis data are very hard to downscale, especially in the central Tien Shan.

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).
The DDF method is now better explained in the ablation model design section of the manuscript.The phrase" required temperature record for the model" is deleted and the section where the mapping of cliffs and lakes is introduced has been re-organized to improve readability.

Specific comments:

p 5307: title mentions catchment scale ablation, but you never discuss catchment scale ablation in the paper. Only in the outlook the developed method is recommended for larger scale.
Deleted catchment scale

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

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

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.
All we have is a temperature record. If I have to estimate everything else, I will not get more realistic results. Besides that it is very difficult to estimate all the parameters needed for energy balance spatially distributed.

p 5309 l 17: have been proven to produce realistic results in other, earlier, studies? Then references are needed!
Added references (Braun 2000 and Hagg 2007).

p 5309 l 19: natural? Ice cliffs are also natural.
Deleted 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.
Deleted "complex physical", the DDF method is mentioned in the methods

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

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

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

p 5311 l 6: what other quantities were measured with the AWS?
Other quantities are measured but are not available.

p 5311 l 12-22: were DEM and ASTER images co-registered?
Yes. Added : "Ikonos and ASTER images were co-registered."

p 5312 l 1: introduce the ablation model (see general comments)
Added description of the DDF method that is used.

p 5312 l 1-15: The lapse rates appear to be a tricky part in the story. These AWSs 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 nearsurface 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?
The measurements are described in detail in the publication from Han Haidong (Near Surface Meteorological Characteristics on the Koxkar Baxi Glacier, Tianshan). I don't claim that these laps rates can be used for modelling ablation in other catchments.
Deleted the term "short term". Temperature was measured for more than 1 year.
Rephrased to: "These lapse rates have been determined for the ablation season (May to September)."

p 5312 l 9: with "determined empirically" you mean that you use the measured ablation and debris thickness and the calculated temperatures?
Added:" To obtain an equation that represents the connection from debris cover thickness to degree day factor, measured ablation, debris thickness and extrapolated temperatures are taken into account."

p 5312 l 10: is this linear part supported by your field observations? How many datapoints are included in this part of the fit?
This is not supported by our field observations.The power law regression would yield to unrealistic high degree day factors for the very thin layers and as we know that ablation is increased for those layers I used this linear equation
Changed to:" To take account of the fact that the degree day factor for very thin layers of debris cover is enhanced compared to bare ice, a~linear equation is used for debris thicknesses smaller than 0.014 m, where the power law regression would produce unrealistic high degree day factors.".

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.
Added information about the ice cliffs: "On 12 ice cliffs ablation was measured perpendicular to the surface. Mean degree day factors for north facing (0.0043 m d^{-1}°C^{-1}), south facing (0.0054 m d^{-1}°C^{-1}) and east/west facing cliffs (0.0052 m d^{-1}°C^{-1}) were calculated and applied within the model."

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

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

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.

The ice cliffs around the ponds are included in the model, but the lateral melting beneath the water surface is omitted (stated in the new manuscript). The melting on the lake ground is calculated using the water temperature (not surface temperature!) above the lake ground (as measured by Xin et al. 2012). I know this is a rough estimation, but it is the best approximation we can use for this model.

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

p 5313 l 18-23: move to line 10
Moved

p 5313 l 22: what is a fundamental DEM?
Changed to:" …the DEM derived from IKONOS data"

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.
Changed the exponential regression, so the LST is zero for zero debris thickness

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?
The resampling was done using a bilinear interpolation.

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.
Changed to:" The total debris cover volume can now be calculated by accumulating the pixel values of the entire debris covered area of the corresponding map (Fig. 5)."
Moved sentence

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

p 5314 l 21: please rephrase first sentence, and do these three sentences require a subsection?
Changed to:"The areal distribution of the features that are relevant for the ablation model are summarized in Table 2."

p 5315 l 5: please rephrase sentence, it is hard to understand.
Changed to:"The comparison of total ice melt of the different debris thickness regressions is shown in Fig. 7."

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".
a)The degree day factor for bare ice is now explained in the ablation model design section:" To compare ablation rates of several locations and time spans, mean degree day factors were calculated using the measured melt and the sum of positive degree days for the bare ice stake and the debris covered ice stakes.

b) deleted subdebris ice ablation
c)changed to "are the same"

p 5315 l 22: How then does this explain the difference between your results and Sakai (1998)? Please explain further.
Added sentence:" However, variations may also arise due to climatic differences between the sites and the fact that many of the ice cliffs on Lirung glacier are beside supraglacial ponds, where cliff retreat rates are higher."

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.
But the downwasting (surface lowering) is related to mass loss.

p 5316 l 7: please rephrase "imaginary debris-free glacier" into something like "the ablation if no debris were present on the glacier surface".
Rephrased this term. Also changed it in the figure caption.

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!
Changed the sentence to:" It becomes quite clear why debris covered glaciers respond differently in a particular climate setting." – Also changed that phrase in the abstract and the conclusions.

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.
Rephrased :" The shielding effect of the debris cover dominates over the vertical temperature gradient" to :" At lower elevations, the increasing debris thickness compensates the higher temperature."

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.
Changed the sentence to:" It becomes quite clear why debris covered glaciers respond differently in a particular climate setting."

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?

I do not claim that the DDF fit or the LST - DCT relation can be used for other glaciers! New field measurements of melt rates and debris cover thickness are needed.

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
Included positions

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

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?
We don't have direct measurements of DCTs exceeding 120cm but the DCT map from Wu and Liu 2012 shows that in the tongue area DCT is that high (around 300cm). Therefore I would like to show the entire regression that I use.

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.
Added new Figure from Wu and Liu 2012. They measured DCT with a ground penetrating radar.

Fig 6: Distribution of calculated total ...
Changed caption

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.
Deleted pixel units. Zoomed area is indicated in the Figure and mentioned in the caption.

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

Changed colours

Sincerely Yours,

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