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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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This is a useful paper, presenting interesting results of a distributed melt model for a debris-covered glacier. The methods are for the most part appropriate, although some minor modifications are recommended. The results mostly confirm what is already known about debris covered glaciers, as the model exhibits familiar features such as the inversion of the ablation gradient and ablation ‘hot spots’ at bare ice faces. The writing style is clear and succinct, and with a few minor exceptions the English is correct. My detailed comments are as follows:

Abstract, line 4: ‘set’ is better than ‘bundle’

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5309, line 19: change 'also ice cliffs and supraglacial lakes are important' to 'ice cliffs and supraglacial lakes are also important'

5309, line 24 ff: Sakai et al speculated about these processes. Direct evidence for internal ablation due to water flowing englacially from lakes was provided by Gulley and Benn (2007: J Glac. 53, 399-412) and Benn et al. (2012).

5309, line 26: change 'Caused by the collapse of water channels new ice cliffs...' to 'Following the collapse of englacial channels, new ice cliffs...'

5313, line 5 ff: More explanation is needed for the sub-lake melting. First, what debris thickness was assumed for the debris below the lake? Second, for what period was a constant lake temperature of 4° applied? The ablation season? If so, what months? Or the whole year? Please clearly state and justify all of the assumptions. I note from 5314, line 25 that lakes occupy only 0.36% of the glacier area - perhaps this can be used to justify weak parameterization of lake growth processes.

5313, line 9: how serious is this omission? Benn et al (2001: J Glac. 47, 626-638) found cliff retreat lakes around lakes of several tens of metres per year, and up to 100 m per year in some places. So this process can be very significant.

5313, line 29: it would be better to use all of the raw data points for the regression, without prior averaging.

5314, line 1: from Figure 4, there seems to be very little basis at all for extrapolating regression curves beyond the data - all of the curves are equally fictitious. The text references data from Wu and Liu (2012) as the basis for preferring the exponential regression - and then the nature of these data is only clarified on p. 5317, line 10. It would be far better to include the radar data of Wu and Liu in your analysis to constrain the relationship for thicker debris.

5315, lines 7-11. There seems to be a contradiction between the statement that '...the sub-debris ice ablation beneath supraglacial debris is the same for all three regres-

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sions' and 'the only difference in total ice ablation arises from debris cover thickness that has an effect on the sub debris ice ablation'. rephrase these sentences to make the meaning consistent and clear.

5313, line 21: is all the discrepancy attributable to weaknesses in the methods of Sakai et al? Could some of the variation arise due to climatic differences between the sites? Also, many of the ice cliffs on Lirung glacier are beside supraglacial ponds, where cliff retreat rates are higher. Could this also explain some of the difference?

5316, line 14: 'a slightly inferior ablation' - this is very unclear, please rephrase.

Interactive comment on The Cryosphere Discuss., 7, 5307, 2013.

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