Authors response to ****Review comments on "Thinning of debris-covered and debris-free glaciers in a warming climate" by A. Banerjee', Anonymous Referee #1****

I thank the Anonymous referee #1 for a critical review of the discussion paper. In particular, I am glad that the reviewer agrees with the result of the paper. Below I have outlined my arguments against his conclusions regarding inadequacy of the model and lack of significance of the results . The red lines are from the referee's review and corresponding replies are in black.

It is difficult to find the significance of the study. Glacier thinning occurs by a combination of the surface mass balance and the emergence velocity. Initial change in ice thickness is controlled by surface mass balance, and then affected by changes in glacier dynamics later. Response time of a debris-covered glacier is generally longer than that of debris-free glaciers. All these were frequently argued and well demonstrated in previous studies.Therefore, it is not surprising to see the results shown in Figure 2.

Undoubtedly glacier thinning has to be controlled by conservation of mass, a slow dynamics of ice and a fast changing mass-balance forcing. I do not claim to have introduced this ideas here in this paper for the first time.
However, to the best of my knowledge, these basic principles were not applied so far in interpreting the recent large scale thinning data from debris-covered and debris-free glaciers in the Himalaya (Kääb et al , 2012; Gardelle et al , 2012; Nuimura et al , 2012; Gardelle et al , 2013, Vincent et al, 2016), leading to the apparent puzzle that has been outlined in detail in the introduction section (from page1, line 22 to page2, line 19). Even with this long list of well-known papers that have dealt with this issue, Vincent et al (2016) has stated: “This question of area-averaged melting rates over debris-covered or clean glacier ablation areas remains unanswered”. This is contradictory to the reviewer's claim and shows that a clear understanding of this effect has been lacking in the present literature so far.
This paper attempts provide a very simple solution to this specific issue from first principles.
If the effect has already been clearly explained in some reference that is not known to me, I am ready to accept that the present contribution is redundant.

Moreover, the model and experimental conditions are very simple (1D flow line model, simple ice dynamics and mass balance). Among others, this study neglects important aspects of a debris-covered glacier, which are listed in the introduction of the paper (line 19-20); time-evolution of the debris extent, variability of debris thickness, and highly dynamic supraglacial ponds and ice cliffs.

I apologise to the reviewer and the readers for not providing a detailed justification of the simple model used in this paper. I thank the reviewer for pointing this weakness out. Such a discussion would surely be included in possible revised version of this article.

The basic point here is that the relatively fast spatio-temporal variations of melt-rate due to the advecting ephemeral thermokarst features (ponds and cliffs) on the glacier surface and an inhomogeneous debris layer, in combination with a slow response of debris-covered glaciers, imply that avarage melt-rate is a rather well-defined quantity and that is what that controls the thinning dynamics at any given point, *x*, over deacal scale. Moreover, as pointed out in the article, the present data suggest, the theromokarst feature play a relatively weaker role in terms of the total melt - at the level of 10-20% (Sakai et al, (2000); Reid and Brock, (2014)).

In addition, since the quoted thinning data are from a large ensemble of glaciers, another level of averaging over such a large ensemble would get rid of the effects of specific details of the mass balance the individual glaciers.

Therefore, it is justified to use a simple (and thus tractable) average mass balance curve to investigate the question of large scale thinning rates in glaciers in the Himalaya. The specific melt-curve used here is motivated by data from Himalayan glaciers (Chhota Shigri, Hamtah, Dokriani and Chora Bari glaciers; eg Banerjee and Azam, 2015). A more complicated representative melt-curve would not change our basic results.

There is a possibility that climatic forcing may increase the average melt rate or may lead to higher abundance of ponds/cliffs (discussed later in the reply), and thus changing the mean melt-rates near the tongue. Given the lack of long term data, this effect is hard to quantify at present. The fact that there are number of debris-covered glaciers with large stagnant tongues in the Himalaya (Scherler et al , 2011), may be a pointer that this increase is not very significant in terms of its magnitude. And the idealised mass balance used here, captures the formation of the stagnant tongue quite well.

Notably, the uppper elevation range of the thickly debris-covered region has been assumed to increase in our idealised debris-covered glacier model by the same amount as the ELA, to take care of the possible increase of debris covered area in a simple way.

In any case, the paper is too short to report complex behavior of debris-covered glaciers.

As explained above, the aim here is to investigate the specific question of decadal scale data of thinning rates of a large collection of debris covered vs debris free glaciers. I do not intend to explain all aspects of the complex behaviour of debris covered glaciers. I believe that the model/paper is adequate for this specific purpose. The complexities alluded to above, would only be relevant in answering more detailed questions like the pattern of thinning in a given glacier and therefore can be safely ignored in the present context.

The existing detailed models, in fact, may not be scalable to simulate a large ensemble of glaciers. Most of these models require high resolution baseline glacier and climate data, which may not be available.

I list below specific comments on the manuscript.
page 1, line19-20: These are very important aspects, but completely neglected in the study.

I have already explained my view on this issue in the response detailed above. These arguments/discussion would be incorporated in possible revised version of the article.

page 2, line 28: "vertical ablation" is odd. Do you mean "surface ablation"?

It would be corrected.

page 3, line 3-4: "mass balance shape remains the same" » This is a very crude assumption because the debris layer thickens and lakes are formed.

A thickening debris layer would affect the mass balance values for sure. However, in the thickly debris covered parts of the glacier this effect would be relatively unimportant. This is evident from the known variation of melt rate under a debris layer (Ostrem curve) that shows smaller decrease in melt-rate in the thick debris limit (more than about ~10 cm). (eg Juen et al, The Cryosphere, 8, 377–386, 2014). And, the possible increase in debris-extent is included in an empirical manner by moving up the saturated portion of the melt-rate curve as ELA goes up (expected in case of melt-out debris).

On the other hand, supra glacial lakes, as pointed out before, only contribute ~10-20% of the total melt (Sakai et al, (2000); Reid and Brock, (2014)) for specific glaciers studied. Also large-scale studies (eg Gardelle et al, Global and Planetary Change, Elsevier, 2011, 75 (1-2), pp.47-55) reveal that the *supraglacial* lake area is typically only a fraction of a percent of the total glacierised area in the region, and that the total supraglacial lake area is growing at a rate of a few 10's of percent or less per decade (with large uncertainties in the estimates). So the net effective lowering of melt rates due these possibly increasing supraglacial lakes can be ignored in the first approximation.

These discussions would be incorporated in the revised draft.

page 3, line 13-14: The result is not "interesting" if "this is an artifact".
I agree with the reviewer and appropriate changes would made.

page 3, line 25: What is the unit of the mass balance gradient?
Units would be specified.

page 3, line 3: Why 30 m (not 50 m)?
A change of 50m at the rate of 1m every five year, requires a total of 250 years, stretching the time axis of the figure 2 – that is why we had truncated it at 150 years ie a total change of 30 years. This would be discussed in possible revised draft.

page 4, line 4-10: These results are easily expected before the experiments. The results are like that, simply because of the assumptions given to the mass balance.
In above replies I have hopefully justified why such a simple mass balance function is enough to investigate some specific questions related to the recent thinning rates in Himalayan glaciers with and without a supraglacial debris-cover.