

Interactive comment on “Heterogeneous glacier thinning patterns over the last 40 years in Langtang Himal” by S. Ragetti et al.

Anonymous Referee #2

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General comments:

This paper presents glacier surface elevation change in Langtang Himal from 1974 to 2015 based on DEMs generated from satellite images. The authors analyzed temporal and spatial patterns of glacier thinning over the studied seven glaciers. Focuses of the discussion are spatial heterogeneity in the thinning rate, comparison of debris-covered and debris-free glaciers, changes in the thinning rate after 2006. The data are also used to quantify the impact of the earthquake in 2015.

Despite the increasing importance and interests on the Himalayan glaciers, long-term data on glacier changes are few in the region. Considering intensive research activities in the Langtang region in the past and recent periods, the presented data set is valuable. Nevertheless, uncertainty is rather large particularly in higher elevation areas.

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This is very common in photogrammetric elevation analysis because snow covered surface loses surface features required for this method. Judging from the unrealistic thickening and thinning patterns in Figure 6a, it is questionable whether the DEM analysis is applicable in the accumulation areas. Moreover, estimated uncertainties are based on very complex outlier rejection criteria, which sometimes appear to be subjective and unconvincing. These problems result in limited reliability in the conclusions. Overall impression on the manuscript is that conclusions are too conclusive as compared to what are shown by the data.

I encourage the authors to thoroughly revise the manuscript (1) by using only reliable data, (2) with well focused objectives, (3) to draw only convincing conclusions. For example, omitting data from the accumulation reduces total uncertainties in Figure 7, which leads to more reliable discussion on recent increase in the thinning rate. Among others, elevation change over the debris-covered regions and impact of the earthquake are promising subjects.

Major concerns:

1. Reliability of the DEM in the accumulation area Figure 6a shows unusually large thickening and thinning patterns in the accumulation areas. The regions of the suspicious elevation change agree with the frequently snow covered regions shown in Figure 1. Most likely, photogrammetric analysis is hampered by featureless snow surfaces. Because such data from the accumulation areas are used for the mean thinning rate over each glacier, conclusions on the recent thinning acceleration and comparison between debris-covered and debris-free glaciers are unreliable.

2. Data and Method section The authors spend more than 1/3 of the manuscript for Data and Method section. This section is suffered from too much detailed explanations on how to reject outliers and estimate uncertainty. All details are given, but hard to understand the reasoning of each process. First, I suggest the author to move these details to the supplement, and describe in the main text only essence of the techniques

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in an understandable way. Second, the structure of the section should be reconsidered. It can be something like, 3.1. Satellite data, 3.2. DEM (generation, differencing, processing, uncertainty), 3.3. delineation, 3.4. velocity.

3. Influence of the earthquake It is interesting and important to evaluate the impact of the earthquake on the glacier surface elevation. However, the elevation change due to the earthquake in 2015 is essentially different from those occurred from 1974 to 2014. Accordingly, elevation change from 1974 to 2015 (e.g. Table 5 and Figure 6b) is not suitable to discuss recent glacier changes in general. Therefore, I suggest the author to separate the elevation change after the earthquake from the rest of the study period.

4. Text I understand that the author tried to be careful and accurate in the text. However, the manuscript is lengthy, redundant and diffuse at many places. This hinders reader's understanding of the methodology, important results and conclusions. Please consider to shorten and simplifies sentences throughout the manuscript.

Specific comments:

page 1, line 15: we present volume and mass changes of ... (omit "glacier")

page 1, line 22: "mass balance trends" sounds to me "surface mass balance trends". What about "mass loss trends" or "thinning trends"?

page 1, line 22: "highly non-linear" to what? elevation? time?

page 3, line 4: What do you mean by "downslope condition"?

page 3, line 8: ... present-day "surface" lowering rates...

page 3, line 16: What is "melt due to glacier emergence velocity"?

page 4, line 26: ..., Kimoshung Glaciers...

page 4, line 31-32: Please consider to shorten this kind of sentences. It should be OK to write "... are exceeded most part of the debris-covered area (Ragette et al., 2015).

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Relatively thin debris layer appears only near the equilibrium line."

page 5, line 6: a.s.l.

page 7, line 9: ALOS PRISM

page 8, line 5: What is "correlation score"?

page 8, line 9: Either of "older" or "earlier acquisition date" is fine.

page 9, line 12-13: I understand that these parameters are useful to measure spatial non-uniformity in the melt rate. However, I do not understand why you use both of them. Particularly, the second one needs a reason why you take 50% and 10%. Moreover, why not using the information on cliffs and lakes delineated from the satellite image (Figure 2b)?

page 10, line 16: I wonder why "higher accuracy" can be the reason to apply the higher threshold.

page 11, line 11: Why do you use the thinning rate from 2006 to 2015 as a threshold?

page 11, line 18-page 12, line 18: It is hard to understand the concept and the procedure to obtain U_{adj} . If this is a commonly used parameter, please provide a good reference. I recommend the author to describe this kind of details in supplement.

page 13, line 12-13: I wonder how these thresholds were chosen and why they "effectively minimize the uncertainty".

page 14, line 5: Using three characters as a symbol is not common. By the way, do you need to define this symbol "unc"?

page 14, line 14: Do you use the same density in the accumulation area?

page 15, line 12: Should be "92 maps were removed because they FULFIL outlier criteria"?

page 15, line 22: Define the acronym "RPC".

page 17, line 5: Please be consistent with the unit, m/a or m a-1.

page 17, line 29-30: "but a majority of values suggest that . . ." » This is not very sure from the data. It appears to me that the thinning rate is decreasing recently

page 18, line 1: What do you mean by "ensemble of values"?

page 18, line 30: What about simplifies the sentence to "The most negative elevation change for 1974-2006 was observed at Shalbachum . . .".

page 19: line 2-3: It makes more sense to compare 1974-2006 and 2006-2014 to eliminate the influence of the earthquake.

page 19, line 1: m a-1 » You need a space between m and a-1.

page 19, line 9: "The most important differences in mean dh/dt values. . ." » "The greatest increase in thinning rate . . ."?

page 19, line 15-21: I find this paragraph is not necessary here. Because Figure 8 clearly shows the thinning patters, you do not need to give questionable comment on Figure 6.

page 19, line 32-page 20, line 3: This sentence is very hard to read. Please consider to rewrite it.

page 21, line 30: "dh/dt_1974-06-dh/dt_2006-15<-0.2 m/a" » Is this correct? Isn't the left side positive if the thinning is accelerated?

page 22, 5.3. Impacts of the April 2014 earthquake: This is an interesting analysis. I suggest the author to use the DEM after the earthquake only for this purpose. In other words, elevation change from 1974 to 2014 should be used for the rest part of the discussion.

page 22, line 2-8: This should be explained in the introduction section.

page 22, line8-21: This should be explained in the method section.

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page 23, line 5: "compensated by about 50%" » What density do you assume for the avalanche debris deposition?

page 23, line 12: "Elevation changes in the debris-covered area are primarily independent of elevation (Figures 8 and 10c) as previously identified in Langtang catchment (Pellicciotti et al., 2015) and elsewhere"

page 23, line 16: "downward-" » downglacier?

page 24, line 1-2: Not clear where and how water pressure is elevated.

page 24, line 5-6: Do you mean that thinning accelerated where ice motion is active because cliffs and lakes develop? It contradicts to my experience to observe cliffs and lakes formation on debris-covered stagnant ice.

page 24, line 20: "glacier uplift" » do you mean "ice thickening due to compressive flow regime"?

page 23, line 27-page 25, line 11: The goal of this section is not clear. It appears that this section discusses the mechanism of surface elevation change on debris-covered ice. However, the thinning rate is highly variable in space and time, and there is no general trend in the observed glaciers. What kind of results does the author try to explain here? Many processes related to surface elevation change of debris-covered glaciers are described, but none of them are connected to reliable interpretation of the data. Describe first an observational fact that you want to discuss, and interpret the observation in a logical manner.

page 25, line 16-17: What do you mean by "correlate with"? Which data show this?

page 25, line 13-page 26, line 9: The first part of this section 7.1.1. explains that thinning accelerated at Yala Glacier, whereas it appears to be at a similar level at Kimoshung Glacier. This kind of explanation should be completed in Result section. Interpretation on the difference begins at

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page 25, line 28, but not convincing because there is no qualitative discussion. For example, hypsometry is not shown for Kimoshung Glacier, and no information about the 0 degree C isotherm altitude.

page 26, line 11-page 27, line 11: This section has the same problem as section 7.1.1. The first paragraph describes several different observations. These details should be explained in Result section, and here the focus of the discussion should be stated briefly. In the second paragraph, speculative conclusions are given without detailed/quantitative comparison with the modeling work.

page 27, line 18-20: Not clear what are compared in Figure 6b.

page 27, line 24-25: Not clear why you compare the elevation of Yala terminus and that of maximum thinning on Langtang.

page 27, line 26-28: It is not clear which part of the elevation range is compared here. If you discuss elevation change of debris-covered and debris-free glaciers at same elevation range, why not preparing a plot for this purpose?

page 28, line 3-15: The point of the discussion is unclear.

page 28, line 16-17: Not clear what you mean. Do you mean that your result support the studies by Kaab, Nuimura and Gardelle?

page 30, line 17-page 32, line 14: Only a few data appear in Conclusion section, which results in very qualitative descriptions. This represents the weakness of the paper. Please draw your conclusions which are supported by data.

page 52, Figure 11: This velocity map is not much used for the study. Judging from the vectors on the plot, it is not sure how much this analysis is reliable.

[Interactive comment on The Cryosphere Discuss.](#), doi:10.5194/tc-2016-25, 2016.

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