**Dear Editor,**

**Thank you for handling and quick review on our manuscript. We reply to your comments point by point. Comments and replies are shown in *italic* and bold, respectively. Note that the line number is of the firstly submitted version so that it is different from the discussion paper. Replies to two anonymous reviewers are addressed latter.**

*This paper investigates the mass changes of Kanchenjunga Glacier in Eastern Nepal using ALOS and KH9 data and related the mass changes to debris cover, glacier flow and the existence of glacial lakes. This is an important topic which has not often been investigates so far. In addition, there are only very few studies which investigate mass changes for the time before ~2000.*

*The analysis seems to be overall sound and the results reliable. However, the descriptions of the methods lack detail so that it is hard to finally judge the quality.*
*One major weakness is the analysis of the uncertainty. If I understood correctly, the authors selected one smaller polygon and use the standard deviation of this area as an estimate. This is not the best method to estimate the uncertainty especially also as it is not clear whether the regions is representative or not. Showing the dh map off glacier would be important.*
**[reply] We made the TIN editing only over the TWO polygons provided in the manuscript so that the *dh* map over non-edited area does not have same quality as the edited area. We cannot extend the editing area in the same quality because of unavailability of human and calculation resources. But we revise the description of methodology and uncertainty estimation in the revised manuscript.**

*The large data voids are a further problematic issue. The authors work with scenarios and two different ways to full these voids. This is a valid approach but more information and better justification is needed.*
**[reply] It is difficult to justify the assumptions. Extrapolation of data from the same elevation band (Gardelle et al., 2013) is not possible for this study because of limitation of analyzed area, which is also not possible to extend the area as we replied above. We change the alternative assumption for "the upper bound" as mentioned below.**

*In addition, the authors miss to cite and refer to some relevant literature with respect to using KH-9 data for DEM generation and mass balance calculation and the impact of glacial lakes on the surface lowering. In this regard I recommend to compare the results not only to Khumbu glacier but also to the glaciers in Langtang Himalaya where much information is available.*

**[reply] We added description about Langtang though the reviewer #2 suggests removing it. We add further discussion in the revised manuscript.**

*Showing mass changes of a glacier since the 1970s in eastern Nepal Himalaya is important and will impact the climatic interpretation and glacier modelling. The study contributes also to the understanding of the reaction of debris-covered glaciers to climate. However, the investigation is only limited to one glacier and it is not clear if this glacier is representative or not. This restriction is a pity because the KH-9 data covers much larger area. In this regard, a map showing the coverage of the stereo imagery would be beneficial.*

**[reply] As replied to the comment on uncertainty, we conducted the TIN editing only over the polygons shown in the manuscript. Therefore, we did not discuss representativeness of the mass balance analyzed in this study with respect to climate change, and thus did simply comparison with an iconic Khumbu Glacier. The first author, D. Lamsal, spent six months for the TIN editing of ALOS and Hexagon images, and he is not able to do further analysis because of private reason. So, honestly speaking, it is practically impossible for us to extend domain.**

*The paper is in general well written. The figures are of good quality. However, I suggest some restructuring and the methods need to be described in more detail. The conclusions are rather long and some content fits better into the discussion section.*

**[reply] We revise the structure according to comments from both reviewers.**

*Editor technical corrections/suggestions*

*L14: Include the months.*

**[reply] We do not think that months are necessary in abstract. This is addressed in Data Section (Sect. 2.1) and Table 1. We do not change here.**

*L24: The results are from different dates. I suggest to compare only to a similar period.*

**[reply] We remove the latter number.**

*L34: Salerno et al. 2015 did not study volumetric changes of glacier in Khumbu region according to my knowledge.*

**[reply] We think so too about Salerno et al. (2015) but here we want to mention not only studies dealing with volumetric change of glacier but also studies targeting the Khumbu region which is most investigated. To avoid misunderstanding, we delete "volumetric" just before this sentence.**

*L48: Refer in this section to at least two further references which also used similar data for DEM generation and mass balance assessments.*

**[reply] We cite Pieczonka and Bolch (2015) and Ragettli et al. (2016).**

*L57/58: I am missing the reference to Ragettli et al. 2016. It now published in TC but was available in TCD during performing your study and your study follows a similar methodology.*

**[reply] We did not cite it because it was a TCD paper when we submitted our study. Anyhow we cite it.**

*Section 2.1 Quite short and fits better into the Introduction section.*

**[reply] We move this into the last part of introduction.**

*Section 2.2: Refer to studies which used similar data.*

**[reply] We cite Surazakov and Aizen (2010) and Pieczonka and Bolch (2015).**

*Section 2.7: Be clear how the final uncertainty was calculated. A formula would e.g. help.*

**[reply] We separate the sentence describing the standard deviation and density scenarios as uncertainty. We change alternative assumption for elevation change at the unmeasured upper accumulation zone. We calculate the uncertainty of this analysis by adding 1) error from DEM creation, which is estimated over the off-glacier terrain, 2) uncertainty from density assumption, and 3) uncertainty from assumption of elevation change for the unmeasured area. About 1 and 2, we described them in the discussion paper though we need to revise the description for more clarity. About 3, we replace the second assumption in the discussion paper by a fitting curve for the upper accumulation area (see reply to the comment on subsection 2.7). We do not use standard error because we do not understand the physical meaning of it rather**

**than use of standard deviation.**

*Section 2.8.: Uncertainty assessment?*

**[reply] We merge this subsection to the next, and change the title as "Estimates of mass balance and uncertainty", and add description for recalculation of the uncertainty.**

*Section 2.9: Clearly mention which data was used for the delineation if the ponds.*

**[reply] We changed nothing here when we re-submitted to TCD but responding to the reviewers' comments, we add description in the revised manuscript.**

*L204/205 and 215-221: Fit better in methods section.*

**[reply] For L204-205, we cannot find appropriate place to move this sentence because we did not have section for the difference between two DEMs in the method chapter. We change nothing here.**

**We move L215-221 and the description for ELA (L153-157) to the end of Sect. 2.5 (former 2.6). We also change order of Figs. 3 and 4.**

*L213f: Can you exclude that the debris-free and debris-covered parts are dynamically coupled?*

**[reply] They should be coupled each other. But this kind of comparison is performed by some previous studies. We add description discussing this.**

*L222ff: It is not clear how the final results were calculated.*

**[reply] Responding to reviewers' comments, we merge two subsections of the method section.**

*L238: Is the lifetime of ponds really "totally" unknown"? There are some relevant publications, e.g. from Doug Benn (e.g. Benn et al. 2001which provide some information.*

**[reply] Benn et al. (2001) dealt with water balance of supraglacial pond but no study exists for monitoring the pond lifetime in this context. I remove "totally".**

*Section 4.1.: This section presents new and interesting result I would rather move the presented results to the results section.*

**[reply] Although we remained this section in the TCD paper but we move this to results in the revised manuscript.**

*L263ff: Refer to references which show similar results in their profiles of surface lowering.*

**[reply] We cite Nuimura et al. (2012), Gardelle et al. (2013) and Ragettli et al. (2016).**

*L286: While I agree that debris-covered glaciers do also significantly loos mass I would be very careful with this statement. The reasons differ and the debris-covered tongues are typically located at lower elevations. Hence, several would not exist there without debris cover.*

**[reply] We add some discussions for this issue.**

*L293: Should it be Fig. 5?*

**[reply] I revise three parts where Fig. 6x was. Thanks.**

*Section 4.4: I suggest also a comparison to the Langtang Glaciers.*

**[reply] We added it in submitted TCD paper but by responding to reviewers' comments, we add further discussion in the revised manuscript.**

*L318f: The periods differ!*

**[reply] We do not catch the point. That's why we described "in different periods" in L317.**

*5. Conclusions: Draw here the main conclusions of your study and do not discuss new aspects. I would also avoid refereeing in the conclusions unless absolutely necessary*

**[reply] We shorten the conclusion.**

*L345: This statement about the relative accuracy needs much better proof. The methods to estimate the uncertainty are rather different.*

**[reply] We remove the statement though we believe that our error based on standard deviation is much better than standard errors in previous studies.**

*Table 2: Avoid too long captions and provide relevant information in the main text. All numbers need an uncertainty not only the final one.*

**[reply] We shorten the caption and add the uncertainty for each estimate.**

*References:*

*The rate of self citations of the authors is rather. While I acknowledge that the authors are quite active in the field and published many relevant studies there are others which could be better acknowledged.*

**[reply] We cite some of your recommendations. Thanks.**

**Dear Reviewers,**

**Thank you for your comments on our manuscript. I reply to your comments point by point. Comments and replies are shown in *italic* and bold, respectively.**

Reviewer #1

*I believe that this is an important paper and makes a significant contribution to our knowledge of glaciology in the Himayalas. Most decadal scale change studies have focused on the Everest Region, the Indian Himalaya, or the Tian Shian, leaving a huge area of High Mountain Asia where we have relatively no idea of how the glaciers have responding to climatic perturbations.*

*General comments*

*The English of the paper is very good although a few small typos exist. I have highlighted a couple but suggest a thorough read through. The paper is laid out well and the results are interesting, however I feel that before it is published there are some necessary changes.*

*I have suggested these below, but I think additionally there are two problems:*

*I think the justification and clarrification of how you could create a geodetic mass balance from 62% of the glacier is needed. The interpolations and such that were necessary since so much of the accumulation areas were missing. It could be I have misunderstood this, in that case clarification is needed to make it more simple to understand.*

**[reply] This is described in the section "2.7 Unmeasured upper accumulation area". We assume two scenarios for the elevation change on the unmeasured area. To convert the elevation change to mass balance, we assume further two scenarios for the density, which is described in the next section 2.8. We think that the assumptions**

**follow some previous studies. We have no other idea to "justify" our assumptions. Cluttered descriptions might lead the misunderstanding about conversion of elevation change to mass balance. In addition, title of subsection 2.8 "Uncertainty estimates of the DEMs" may be inappropriate. We revise the present section 2.8 into "Estimates of mass balance and uncertainty" and add descriptions about conversion of elevation change to mass balance with two density assumptions, and then describe the estimate of uncertainty. Section numbering was incorrect in the discussion paper so that we correct it.**

*My second concern is the figures are rather cluttered, hard to interpret and not used to their potential in the rest of the text. There is a body of information in the figures that should be interpreted, and the conclusions that the viewer should draw from each should be stated in the text so the reader can judge for themselves.*
**[reply] We prepared these figures with considering their size in the finally published paper. We want to revise the manuscript according to the comments if they are given specifically. Looking at comments for figures below, however, the comments are just "hard to interpret". We do not know how we revise the figures with these comments.**

*I recommend these small changes before publication.*
*L1 - "have been losing mass" and not "were losing mass" Take away the sentence ", and there are now several modern satellite datasets" and instead note the importance of the declassified datasets*
**[reply] We revise the descriptions.**

*L30 The mapping camera Section 2.4 - I know this is not a paper on glacier mapping, but I feel you should briefly mention how you decided where to map the glacier boundary (what features did you look out for) since this is important in the final results*
**[reply] We adopted conventional method of glacier mapping by following our previous works. We add the following descriptions.**
**"The glacier outlines for 1975 and 2010 are manually delineated using 3-D stereo-views of ortho-rectified images, which are the same stereo-models utilized for generating/editing DEMs. With helps of high spatial resolution, cloud-free and minimal seasonal snow cover in the chosen images, and the 3-D viewing, the identification and delineation of glacier boundary are feasible. In area of poor contrast, shade and steep snow-covered slope, topographic features (i.e., slopes and contour lines) and geomorphic features (i.e., surface roughness and crevasses) are**

**carefully checked for the mapping by following our experiences in the high mountain Asia including Himalayan regions (Nuimura et al. 2015; Nagai et al. 2016; Ojha et al. 2016)."**

*Section 2.5 - I would have liked a little more on how the DEMs were cleaned - how much cleaning was necessary? How was it decided which points to remove?*

**[reply] Methodological development and aspect used in this study are described in published literatures of Lamsal et al. (2011) and Sawagaki et al. (2012). Unfortunately we did not record number of deleted/added mass points through the editing so that we cannot provide specific information asked in the comment. But for more clarity, we add the following description in the method section as (shown with italic):**

**"With the aid of RPCs data and the Leica Photogrammetric Suite (LPS) Workstation, ALOS stereo-models *and triangulated irregular network (TIN) model* are generated."**

**"Editing of mass points, which are sets of points *with XYZ* coordinate values defining the terrain surface, is therefore an important post-process for obtaining precise terrain representation. *Stereo MirrorTM/3D Monitor and Leica 3D TopoMouse enabled the tasks of GCP collection and terrain editing. Number of mass points required to represent a terrain or a particular feature on a terrain surface depends on several factors including regularity or uniformity and size of the feature, and desired accuracy. The TIN model is extensively edited upon viewing the 3D images until terrain representation with mass points is satisfactorily achieved.* The major editing tasks performed include densification and thinning of mass points, removing false spikes (mass points in the air) and depressions (mass points under the ground), and thus placing adequate and representative mass points exactly on the terrain *and glacial surfaces including supraglacial ponds and moraine ridges.* The LPS Terrain Editor was used to correct and minimize errors contained in the automatically generated DEMs. Finally, the extensively edited TIN model was gridded into a DEM at a spatial resolution of 5-m (hereafter ALOS-DEM). *Detailed procedures are described in Lamsal et al. (2011) and Sawagaki et al. (2012).*"**

*2.6 - You should again focus more on DEM generation as many people will be interested in your methods. How was the affine transformation applied? Was it during the image matching to create the DEM? Instead of saying "validating the affine transformation" I suggest you rephrase to checking the elevation bias? I guess that is what was done? To compare the Z values for these 5 points? This way it is clearer for people who want to replicate your methods*

**[reply] This is caused by miscommunication between first and corresponding authors. We replace this sentence by the following descriptions.**

**"Five of the 21 GCPs are randomly selected as check points, which are then used to independently verify the quality of an aerial triangulation (GCPv in Fig. 1). Unlike GCPs, XYZ coordinates of the five GCPv are not used for the aerial triangulation while the image coordinates are used to compute new XYZ coordinates. The new coordinates of the five GCPv are then subtracted from the original values. The differences in coordinate values are residuals of aerial triangulation, which are 7.7 m in horizontal and 4.4 m in vertical, respectively."**

*2.7 - Am I to understand that these 9 points in the upper accumulation area were thickening less than the ice at the ELA? Is this due to the emergence velocity? I also think you need to further explain how you estimated the elevation change in the 38% of the glacier where there was no image matching, as these assumptions could wildly influence the geodetic mass balance derived. What interpolation method? Are these assumptions taken into account in the error assessment? Looking at the figures it seems that the debris-free areas where image matching was possible are rather steep and flow into the debriscovered area, are these areas really representative of the accumulation area?*

**[reply] The TIN edited DEMs are the core of this study, which differentiates this study from others. So, we do not apply methods using automatically generated DEMs to the upper accumulation area. Recently published studies using Hexagon KH-9 images encountered the same issue resulting in large voids at the upper accumulation zone, and these studies assumed two scenarios such as "no change at void" or "using value at the same elevation where DEMs are generated" (Pieczonka and Bolch, 2015; Maurer et al., 2016; King et al., 2016). The nine points in our study correspond to the "no change assumption" in the previous studies. We think that the "measured" nine points provide better evidence than the no change assumption. About the alternative assumption, on the other hand, we have no appropriate data for applying this approach. Although we used a value averaged above an assumed ELA in the discussion paper, we change this assumption by the responding to reviewers' comments. Adopting 6 elevations above 5800 m a.s.l. showing zero to positive elevation changes for debris-free glacier surface, we create a fitting curve with the maximum value of 0.3 m $a^{-1}$, and then calculate the entire mass balance. The maximum value of elevation change is collected from similar previous studies in the Nepal and Bhutan Himalayas (Nuimura et al., 2012; King, Maurer et al., 2016; King et**

**al., 2016). The second assumption corresponds to that of Gardelle et al. (2013), and results in the upper bound of estimated geodetic mass balance. With the two density scenarios, mass balance for the entire glacier is finally calculated to be –0.18 m w.e. $a^{-1}$.**

*2.8 - How was this 0.16 m/a uncertainity derived? Was it based on the standard deviation? Or on the standard error as other papers have used (Bolch et al 2011, TC, King et al 2016, TCD)*

**[reply] This is based on the standard deviation with four scenarios. We use standard deviation because we do not understand the physical meaning of standard error. The description in the discussion paper might be unclear so that we revise the manuscript by merging two method subsections.**

*2.7 Glacier Velocity - there is a problem with the numbering here, it goes from 2.8 down to 2.7 Also my understanding of the Heid and Kaab paper was they compared different types of feature tracking methods, so the sentence that feature tracking is one of the most efficient methods is erroneous. The parameters you talk about - robustness and masks should at least be outlines*

**[reply] We remove the description ", which is one of the most efficient methods used to derive glacier surface velocity or surface displacements".**

*3.1 - I understand why you give the values in change per year, but two data points over a 35 year period is perhaps a little risky when it comes to estimating annual change. I suggest you acknowledge this somewhere in the text*

**[reply] We suppose that this refers to the section 3.2. We do not catch what "a little risky" means. Does this mean that annual variability in mass balance is generally "quite" larger than long-term mean? Or that any acceleration/deceleration of mass balance may be embedded? We cannot understand why this procedure is not allowed for us though similar studies do. We do not change about this procedure.**

*3.2 - There has been some discussion about downwasting versus elevation lowering. I am not 100% sure which is appropriate in this context, but to some downwasting implies stagnant ice that is not flowing and only melting. Again, it could be wise to mention this in the text Again, how certain are you in the glacier wide mass balance when you are missing data for 38% of the glacier?*

**[reply] We remove down-wasting. About uncertainty from unmeasured area, we have**

**no good idea to evaluate error range.**

*4.1 - Is an r2 of 3.5 a strong correlation?*

**[reply] We do not catch what point the reviewer #1 addresses but we imagine the correlation for the entire relationship between elevation change and elevation ($r = 0.35$, $p < 0.001$). We do not assert any "strong" correlation here but, as we show p-value, it is still significant because of enough sample number. We do not change here.**

*4.5 - This is an important section but seems unfocused. The last sentence seems to contradict the study. If you need to understand regional changes then what is the justification for this study? Maybe a slightly more detailed comparison with High Mountain Asia glaciers could be useful to give the reader an idea of how your study fits into the larger picture*

**[reply] We move subsections for topographic and pond effects to results section. We merge discussion subsections for Khumbu and Langtang (this comment) as revised discussion section. We intended to describe a limitation of this study with the description about measurement of the single glacier. We try to add some descriptions about future direction.**

*Other comments*

*I think that table 3 is very interesting and should be highlighted more*

**[reply] We add descriptions of this table for ponds.**

*On figure 1, I do not see the glacier from 1975. Additionally this map could be cleaned up considerably. It's hard to even see the glacier on the image*

**[reply] Near mid-low yellow cross just above T6, we can see two red (very-short) lines. We add description to identify the location as "On the figure, we can identify the disconnected portion near the yellow cross just above T6.". We have no idea to clean up the figure.**

*Figure 2 - How did you assess the accuracy of the velocity measurements? It could be nice to see some off glacier displacements to give confidence. Additionally the vector arrows are very hard to see*

**[reply] We will provide value and add description for the velocity uncertainty calculated over the off-glacier area though we need some more calculation to retrieve**

**the data. Vector arrows are colored.**

*Figure 5 - What would you like the reader to gain from this? Additionally, B is not straightforward - larger ponds controll less mass loss? Is that because they are fewer? Is C also showing how the pond elevation has changed? Or am I reading it incorrectly?*
**[reply] This understanding is incorrect (less mass loss / fewer). In our opinion, larger size ponds control "more" mass loss because they absorb more heat. Figure 5c shows glacier surface including pond significantly lowered. Not the pond level. We believe that these are adequately described but we merge two subsections for this pond effect in results (3.4) and discussion (4.3).**

*Figure 7 is very hard to interpret, again what conclusions do you draw from this? I think you need to use the figures a lot more*
**[reply] We will add some descriptions. Without specific comments, however, we are unsure whether the revised figure satisfy the reviewer.**

*Figure 9 is again hard to interpret.*
**[reply] We will add some descriptions. Without specific comments, however, we are unsure whether the revised figure satisfy the reviewer. Although it is not determined yet, this figure may be removed by following to the reviewer #2's comment.**

*Reviewer #2*
*The paper by Lamsal et al. presents an estimate of glacier mass balance and surface velocities of the Kangchenjunga glacier in the E. Himalaya from 1975 to 2010, using Hexagon KH-9 and ALOS imagery. The topic is relevant to the Cryosphere, and this type of study is much needed for the eastern part of the Himalaya, where glacier mass balance is poorly studied. While I appreciate the authors' efforts for creating the DEMs needed for this study, I think the paper needs much improvement before it can be published. Some of the more technical comments on the DEM generation, glacier mapping etc were already addressed by reviewer 1, and I agree with those suggestions. In addition to those technical details which I also believe should be addressed, my major concerns are:*

*1. Extent of study area / representativeness: The main concern here is the focus on one glacier only seems insufficient for this study, especially since the study is based on*

*remote sensing data. I strongly advice extending this to a larger extent, which would allow to better examine spatial tendencies of elevation changes across a larger region Given that the authors already have the two sets of images (ortho- rectified and referenced), this would require minimal effort. The choice of one glacier only would be justified if the authors had field-based mass balance observations to compare the geodetic mass balance estimates with, but this is not the case.*

**[reply] We admit that one glacier is insufficient to discuss regional trend of glacier fluctuation. However, it requires huge effort than the reviewer #2 thought (NOT "minimal effort" at all). The first author, D. Lamsal, spent six months for the TIN editing of ALOS and Hexagon images. So, honestly speaking, it is practically impossible for us to extend the area with the same quality further. In addition, requirement of this comment is rather irrational because, though we admit that one glacier is not sufficient, there is no standard how many glaciers are sufficient. If someone performed a regional analysis covering a mountain massif with a fully automated method, for instance, does the reviewer #2 require to extend the domain further because of the automated method (so it would require minimal effort, just download the data!)? Anyhow we cannot extend the study area.**

*2. Focus on mass balance: This is the second major concern. I find it problematic to expand db/dz from the ablation area only to infer mass balance over the entire surface of the glacie, especially since the DEMs derived are sparse and do not cover the entire glacier surface. Also, the 4 scenarios for estimating mass balance from surface lowering are not very clear- and averaging all these scenarios lead to too much uncertainty. Furthermore, db/dz cannot be directly interpreted as mass balance changes unless the full glacier dynamics component is evaluated.*

**[reply] We are confused with the term "db/dz" in this comment because we do not use this term throughout the manuscript. The former part of this comment seems related to conversion of dh/dt to mass balance db/dt and the latter part seems comment on elevation profile of dh/dz. In all parts, we do not use db/dz, which we imagine "elevation profile of mass balance". Anyhow we try to reply to the comments.**

**First, we believe that the high quality DEM produced by the TIN editing is the core of this study. About the lack of measurement in the accumulation area, we do not want to mix up different methods so that we chose this approach, by which we estimate dh/dt from selected nine points at which the TIN editing was possible. This "lower bound" is equivalent to "no change assumption" in many previous studies. As replied to the reviewer #1's comment above, we propose alternative "upper bound"**

**for the unmeasured area. About the four scenarios to estimate mass balance from dh/dt, we restructure the method sections, which are cluttered in the discussion paper as the reviewer #1 commented. About the last comment on db/dz, we also recognized it so that we carefully avoided use of term "mass balance profile (db/dz)" in the discussion section 4.1. In this section, we describe effect of emergence velocity on the elevation change though the emergence velocity is impossible to estimate because of no data of ice thickness. So, unless db/dz implies mass balance profile, the comments addressed here are pointless.**

*3. Objective of the paper: Given these concerns above, I suggest revising the objective of the paper, for example focusing on glacier thickness changes based on DEMs as in other studies (Kääb et al. 2012, Gardelle et al. 2012a) over a larger area, and compare with trends reported in these studies. Then potentially refer to mass balance changes, but with a lot of caution because of lack of validation. However, I suggest that these be a secondary (and less emphasized aspect). I believe this would provide a much more valuable contribution.*

**[reply] We agree that extending study area would be more valuable. But as we replied above, it is practically impossible.**

*4. Role of supra glacier lakes/ice cliffs: This section is somewhat vague. The authors map the lakes based on the DEM (this should be revised as well to include spectral signal), and hint at the role of the lakes, but it is not clear whether the elevation changes reported over the surface of the lakes takes into account any shifting of the lakes, ie have the lakes stayed in the same place and just lowered (seems not likely so)?*

**[reply] Because of both panchromatic images of Hexagon KH-9 and ALOS PRISM, no spectral signal is available for mapping ponds. But ponds are delineated on 3D images with a help of TIN network (not on the DEM). We add some descriptions to delineate the ponds in the method section. About latter part of the comments, we clearly addressed that the ponds do not stay in the same place (Page 6, Line 18) and how such short-lived ponds could enhance the melting of debris-covered surface (Page 8, Line 1). We did not change description about this though we merge two subsections in results and discussion.**

*5. Figures and tables: the paper seems quite cluttered with a lot of graphs, some of which are unnecessary (see detailed comments below). It lacks in some illustrations of the imagery, also a figure showing the extent of the images used.*

**[reply] We try to follow suggestions if they are given specifically and reasonable.**

*6. English language: Some improvement in the English form is necessary- some paragraphs are hard to follow. Please see detailed comments. For example use of past tense vs. present perfect, and other nuances underlined below.*

**[reply] We apologize for our poor English, and appreciate your detailed corrections if they are given.**

*7. General structure of the paper: This also needs improvement. Most of the sections are not consistent enough on their own; also there is some mix up between study area, results and discussion. Again, comments below.*

**[reply] We try to follow suggestions if they are given specifically and reasonable.**

*I think the paper has potential and the dataset developed seems appropriate at least for en estimate of glacier thickness changes (with perhaps hints at geodetic mass balance), but it would require major revisions. I suggest the authors extend the estimate of glacier elevation change at larger scale, given the lack of field-based measurements on Kangchejunga, and address the concern above before they can resubmit the paper.*

**[reply] The suggestion to extend study area is not acceptable as we replied above.**

*Specific comments:*

*P1 l 23: " In recent decades, glaciers in the Himalayas, once one of the most glacierized mountainous areas in the world, were widely losing mass, though they exhibited a high degree of spatial heterogeneity" Please rephrase; past tense not appreciate here*

**[reply] We correct here according to the comment from the reviewer #1.**

*P1 l 27: For instance, glaciers in the Khumbu region of Nepal have been frequently studied (e.g. Nakawo et al., 1999; Bolch et al., 2011; Nuimura et al., 2011, 2012; Salerno et al., 2015). What have these studies found? What's the gap?*

**[reply] We want to address a spatial gap (some regions are intensively studied but others are not). To avoid misunderstanding and to reply to the editor's comment, we delete "volumetric" just before this sentence.**

*P1 l 27 – 33: The authors could focus on the area of interest, and the missing gaps, rather than focusing on the Khumbu and the studies conducted there.*

**[reply] We think that it is meaningful to address spatial gaps as Khumbu and**

**Langtang are intensively investigated but others are not. We add descriptions for well investigated Langtang and for few research in Kanchenjunga.**

*P1 l 33: It would is good to mention here that this study has the potential to complement the study by Racoviteanu et al. (which, in the initial version of the paper did present elevation changed but from a different dataset, with topo maps). However, the study would need to be extended to a larger area (see my previous comments).*

**[reply] We recognize that Racoviteanu et al. (2015) presented elevation change of Kanchenjunga Glacier in their discussion paper at first and that their spatial pattern is quite different from ours. But they withdrew their result after the discussion. We discussed how we dealt with this among us before we submit our study. In our understanding, it is not a fair manner to refer discussion version (same as conference proceedings paper) after its final version was published. We replied to the issue about extending study area above.**

*P2 l11: ".. insulation effect of the debris mantle (Scherler et al., 2011)" This statement is not fully accurate; debris cover does not always have an insulating effect (only when thickness > critical thickness). Other references (Mihalcea et al. 2008, Foster et al. 2012) and others are more appropriate than Scherler et al.*

**[reply] We replace Scherler et al. (2011) by Mihalcea et al. (2008) and Foster et al. (2012).**

*P2 l0- 13: this phrase is vague, please rephrase. Also, do you mean here "variability" in debris thickness, and not variation? Variation implies change.*

**[reply] This should be variability. We divide this sentence into two.**

*P2 l7 – 19: the discussion of debris covered glacier tongue, with some hints at SRTM/ASTER DEMS is vague, please revise. What is the main point here? Lack of adequate mass balance estimates of debris covered glaciers at large scale? If so, then the current study should propose to fill this gap by estimates at larger spatial scales.*

**[reply] This sentence follows description for ice cliff and pond on debris-covered area. We think that "In addition,, ,," and "these small features" describe the context adequately. We do not change here.**

*P2 l 20: The study cannot "document" mass balance (there are no field based measurements)- only "estimate". Pleas rephrase.*

**[reply] Thank you for the suggestion. We replace it by estimate here and by described (P5L6).**

*P2 l 23 satellite data are rarely refered to as "historical". I suggest replacing with "multi-temporal satellite imagery"*

**[reply] We correct here accordingly.**

*P2 l 24-25: "It is also aims for a better understanding of the effects of supraglacial ponds and other factors on ongoing changes in debris-covered glaciers under the recent climatic change." Again this second objective is vague- what kind of other factors? What kind of changes? The role of supra glacier lakes is not fully quantified- so, this objective is rather a speculation, or an item to include in a discussion, but it does not seem to me to be justified as an objective.*

**[reply] We change here as "supraglacial ponds, flow velocity and topographic factors". We describe how each factor affects change in surface elevation of debris-covered glacier in the following method sections.**

*P 3 l 36: this is not clear: images were dated 2008 for lakes, 2010 for velocity...do you mean multi- temporal images were used? Why did the authors not use the same images for lake, velocity and DEM generation?*

**[reply] The 2008 image is used for pond identification of Khumbu Glacier, which is surely written (P3L1). We do not change here.**

*P3 l 5 – 10: the section on glacier mapping is very brief, not much detail is given here. Were there varuous iterations? What about problematic areas such as frozen lakes, shadows that are often encountered, how were those dealt with? Was clean ice delineated separately from debris cover? And what about glacier lakes? The statement about ASTER DEM to extract hypsometry is confusing. If the goal was to derive the surface in the 1970s, then the 1970's derived DEM should be used, not the ASTER GDEM. Please address.*

**[reply] According to the comment from reviewer #1, we add descriptions about glacier mapping. ASTER GDEM2 is used to get "hypsometry", not for "elevation change". It is clearly addressed so that we do not change description about ASTER GDEM2.**

*P 3 l 11 "DEM generation from ALOS PRISM imagery" The authors use both DTM and*

*DEM. These terms are usually interchangeable and refer to the same thing. Please revise this throughout-*

**[reply] We use DEMs throughout the manuscript and add descriptions about the TIN editing.**

*P 3 l 22 "The LPS Terrain Editor was used to correct and minimize errors contained in the automatically generated DTMs." Please explain what does this mean in LPS- what kind of algorithm was used? What kind of corrections were applied?*

**[reply] We describe everything questioned here in the manuscript (P3L16-). We add descriptions about the TIN editing.**

*P3 l 26: distortions in declassified images do not come only from storage and processing, but also from the camera. Hexagon have considerably less distractions than Corona for example. Please include some references here about distortions present in Hexagon.*

**[reply] We are sorry that we do not understand what "distractions" means. Is this a mere typo of distortion? We have no confidence to judge this because of our poor English capability. About camera distortion, anyhow, we do not think that this is main issue of this study but was solved in Surazakov and Aizen (2010) from which we adopt a method for preprocess correction. We do not add reference for this comment.**

*P3 l 27: what is meant by "reseau grid"?*

**[reply] It means crosshairs. We change the term.**

*P3 l 29 by "ALOS stereo model" you mean the ASTER DEM? Please use the same terms throughout.*

**[reply] This is not DEM but 3D stereo image on the LPS stereo monitors. It is described in the section 2.3 with this term. We do not change the term.**

*P3 l 31 "off-the-glacier area, at which unchanged terrain is expected" Awkward phrasing, please revise English language*

**[reply] We apologize for our poor English. But, without specific comment, we do not understand what point is awkward at all. We change here as "off-the-glacier area, at which unchanged surface elevation is expected" though we have no confidence whether this correction is appropriate.**

*P 3 l 33- 34 again, DTM and DEM terms are used*

**[reply] We use DEMs throughout the manuscript.**

*P4 l 1 please revise title of the sub-section "Unmeasured accumulation area" – what about it? This seems to me it pertains to one of the previous sub-section, not a section by itself.*

**[reply] This is not a part of previous sub-section for DEM generation from Hexagon KH-9. But we merge this with the latter, and rename as "Estimates of mass balance and uncertainty".**

*P4 l 4 what do you mean by "continuous DEM"? please revise. Also l4-5 contain results- and these have not been reported yet.*

**[reply] We remove "continuous". We move calculation of uncertainty to the result section.**

*P4 l5 "Owing to" is awkward. Suggestion "due to.." but to not start the phrase with this. First state the issue and expose a potential explanation.*

**[reply] We change here as "Patchy DEM creation is attainable at nine locations in the upper area due to better local image contrast (hereafter point sites, Fig. 2a).". Thank you for the specific suggestion.**

*P4 l 8 the "average" of what?*

**[reply] We add "of rate of elevation change".**

*P4 l 10: "Because the ELA of the glacier is unmeasured, it is assumed to be around 5800-5850 m, a rough estimate based on the geodetic elevation change, at which no change of glacier surface elevation is found for the studied period of 1975–2010"*

*This is confusing: the ELA is unmeasured, and the geodetic elevation change is not yet estimated- so how can the ELA be assumed then? This seems circular. The entire section here should be revised, there seem to be too many assumptions with no measurements to back up these statements. Furthermore, the authors state to "compute" an AAR- again, this is an estimate based on other estimates.*

**[reply] We move calculation of gELA and gAAR here to the result section.**

*P4 l 14: What do you mean by "relative accuracy"? Is there an absolute accuracy?*

**[reply] We remove "relative" though previous studies used this term (e.g., Maurer et al., 2016; King et al., 2016).**

*P 4 section 2.8: Check numbering, Should be 2.9*

*this is better suited for a discussion item, not for the methodology, especially since no mass changes have been reported yet. It should be moved to the discussion section after reporting the results. Also, I do not see it fit to average the two scenarios of density. Please see Berthier at al 2007 for a similar approach, using some field based measurements.*

**[reply] We merge the two subsections and change the title to "Estimates of mass balance and uncertainty" but keep it in the method section because many previous studies did so. In discussion section, we add descriptions how these assumptions affect the estimated mass balance and uncertainty. Thanks for suggesting Berthier et al. (2007).**

*P4 l 29 "...effects of certain dynamics on changes in glaciers"*

*This is vague. What do you mean by "certain dynamics?" again, what kind of changes? Area? Elevation? Authors need to be more precise throughout the manuscript.*

**[reply] We change here as "topography and dynamics on changes in glacier elevation".**

*P 5 l 7 "The velocity distribution of Kanchenjunga Glacier is consistent with that of the produced DEMs.." I am not sure what you mean here by velocity being consistent with a DEM*

**[reply] We mean here that area where velocity was calculated is same as the area where DEMs were calculated. We remove this sentence.**

*P 5 section 2.8: again check numbering, should be 2.10 here*

*This section should be merged with glacier mapping- it belongs to the mapping process.*

**[reply] The pond delineation is possible after DEM generation so that there is no good room in the glacier mapping section located before the DEM generation section. We do not change here.**

*P5 l10 "glacier degradation"? please check term, this is not standardly used. In this study the authors talk about glacier surface change, nor degradation (= disintegration?)*

**[reply] We change here as "glacier surface lowering".**

*P6 l16 this is confusing- why delineate ponds in Khumbu? Aren't there already*

*estimates of % cover lakes on Khumbu in the literature? And why a comparison with Khumbu? It would be better to stay focused on the study area and extend the spatial coverage. The authors do not study Khumbu, so I do not see necessary to process images from Khumbu-*

**[reply] This is because Khumbu Glacier is an iconic Himalayan glacier investigated intensively. There is no data to compare with Kanchenjunga Glacier in terms of ponds. We can remove this sentence but we clearly address that "comparison performed later".**

*P5 Results: sections 3.1 and 3.2 should be combined into glacier area and elevation change. There is not enough material in 3.1*

*Also, p5 l 22 why is the average of the change used to derive elevation changes?*

**[reply] We merge them. About average, we do not know why the reviewer rises this question. Do you mean that we should use one area of 2010 (or 1975) to calculate mass balance? This does not affect the result actually because change is little. We think that, however, if area change is large, which is generally observed for debris-free glaciers, average of two areas in different times should be used. We do not change the description and calculation.**

*P5 l 24 "less significant area loss of the glacier"*

*The area change to start with does not appear to be significant (it is very small). In any case, the authors did not do any statistical tests, to the term "significant " should not be used.*

**[reply] We change here to "very small area loss".**

*P5 l 24 " Considering the uncertainty in area delineation.."*

*Should mention here again the uncertainty, this was estimated at 1 pixel*

**[reply] Although it is addressed just in the beginning of this subsection, we add the value as "area delineation (0.5-1.5 km$^2$) and ..".**

*P5 l 26: "changes in glacier mass" – I suggest the authors report first glacier elevation changes before over interpreting this as mass changes. I suggest removing "mass"*

**[reply] We do not catch what the reviewer #2 intended to mean. We surely did as comment here, we first address elevation change, and then describe mass change. We do not change the subsection title "Changes in elevation and mass of the glacier".**

*P5 l 28 "minus and plus signs denote glacier surface lowering (down-wasting) and thickening (Fig.2a)" Not needed here, this is evident and otherwise can be included in a caption.*

**[reply] We remove this.**

*P5 l 1 31 "extensive debris cover"*

*The authors seem to confound extensive (ie area) with thickness (related to insulating effect). No data are available here to support the statement on insulating effect. Please revise, the phrase is vague,.*

**[reply] We remove "extensive supraglacial".**

*P 5 33-34: this can be summarized in a caption, along with the figure, the pattern here is hard to follow.*

**[reply] We do not catch which description the reviewer #2 is pointing out. But we believe (and have been commented in many reviewed experiences) that description for features found in figure should be addressed in the main text, not in the figure caption. We do not change here.**

*P 6 l 7: see my general comments. Can the geodetic mass balance be estimated given a) incomplete DEM coverage b) lack of bn mesaurements and c) uncertainties in type of material used? I suggest the authos report only the surface lowering/thickening without further extrapolations.*

**[reply] We are surprised with such an important comment embedded here. In addition, this comment is opposite to another comments "extending area". We do not agree this suggestion.**

*P6 l 8: section 3.3 on surface velocity does not seem consistent enough. Perhaps can be included in a section called "glacier area, elevation and velocity changes"*

**[reply] We can merge these subsection but we do not deal with "velocity change". By following the comment from editor, we move subsection 4.1 (description for the topography) to results section. So this velocity subsection will be merged into it.**

*P6 l 10 "surface velocities show a general pattern"*

*A "general pattern" does not say much- why would it be a general pattern? Do all glaciers of this type show the same pattern? Please revise.*

**[reply] We change here simply as "surface velocities are almost ..".**

*P 6 l 12 : the authors speculate on "remakrlable" diffenreces in velocity which may be due to slope etc.. First of all what does "remarkable mean"? need to wuantify this. ¥ Second: the authors have derived DEMs for this glacier- with extended analysis they can explore these hypothesis (ie correlate velocity variations with slope, terrain etc..), but this has not been done in the paper. This is another area where the paper needs improvement.*

**[reply] We remove "remarkable". We move subsection 4.1 in discussion section to result section, in which we discuss relations between elevation change and topographic features including flow velocity.**

*P6 section 4.4: again: consider merging all these into a single results section (to my comment above authors could add "Changes in glacier area, elevation, velocity and supra-glacial lakes", for ex... The paper is too fragmented, and each sub sections does not have enough material by itself.*

**[reply] We merge 3.1, 3.2 and 4.2 subsections (change of glacier), 3.3 and 4.1 subsections (relations with velocity and topography), 3.4 and 4.3 subsections (pond effect).**

*P6 l 18: there is a contradiction here. The supra glacier ponds are estimate to last a few years, so it is not a surprise that the same ponds will not be found in 4 decades' time. Please revise.*

**[reply] We change here as "Lifetime of a supraglacial pond is expected to be a few to several years though it is not fully investigated. Therefore, ponds in 1975 disappeared either by deformation due to glacier dynamics or by drainage through englacial channels (Sakai et al., 2000).".**

*P6 l19 it is not the number of the lakes that necessarily leads to the increase in the average size of a lake. There can be more lakes, with small surface hence no change in average size. This is a speculative statement.*

**[reply] Increase of average size is the fact. We remove the description of total area.**

*P6 l 24. I do not agree with the statement here. Topographic profiles only show tendencies, they do not explain statistically the influence of topographic variables on elevation changes. The authors should mention the technique used – correlations?*

*Regression analysis?*

**[reply] We simplify the statement "In order to compare the elevation change with topography and flow velocity of Kanchenjunga Glacier,,,".**

*P6 l 31- 32 this belongs to methodology- and the calculation of these variables is not explained in methodology.*

**[reply] Unexplained variable is only slope. We find no appropriate room for this in the method section. We do not change here.**

*P6 l 30 – 33 this is a circular statamenent, please rephrase. Also l 33- 34: this should be included in the methodology, and also in the abjectives if it is an important focus of the paper.*

**[reply] We do not catch which sentence is circular statement. L33-34 just address that we do not know glacier ice thickness, so that it is not appropriate to be addressed in method section. Is this same as above comment? We do not change here.**

*p6 l 33 please replace "unobtainable" also references needed here for studies which did attempt to estimate thickness of debris from RS (Mihalcea et al. 2008, Mihalcea et al. 2008, Foster et al. 2012)*

**[reply] No. This is description for ICE thickness, NOT for DEBRIS thickness. We do not change here.**

*p7 l 2: this is an extrapolation, and the pattern is not valid for all glaciers. Please remove/revise.*

**[reply] We do not think that this description is too speculative to be removed.**

*P7 l 3 -6 : this is background information, and it is quite vague (no critical thickness values given, no melt rates available yet). Remove/ move later, i.e .focus on interpreting the results (l 8-9 ) and only then presenting general patters of debris cover behavior.*

**[reply] We do not think that this current structure, first describing general background and then addressing feature of the data, is wrong or odd to be corrected. We do not change here.**

*P7 l 8: "relationships with elevation look more straightforward" This is qualitative. Is this assesses statistically / what is meant by "straightforward"?*

**[reply] We add gradients of regression line and change the description as "gradients**

**against elevation look similar (1.862 ± 0.564 × $10^{-3}$ $a^{-1}$)".**

*P7 l9 "rates of elevation change increase negatively in a similar trend from higher to lower elevations" Awkward phrasing throughout manuscript, simply rephrase to something like "glacier thickness decreases towards the glacier terminus, at a higher rate than in the middle of the glacier" for example.*

**[reply] Thank you for the suggestion. We change here accordingly.**

*P7 l 10 "Along the main tributary (T3) below 5400 m a.s.l. or down-glacier from 18 km" Please interpret these separately- as the controls may be different*

**[reply] We remove "or down-glacier from 18 km".**

*P 7 l 12: not clear, please restate what the statistical test shows (and what test as performed)*

**[reply] This correlation is for the entire domain where the data available. We replace "all tributaries" by "the entire domain where the data available". But we do not provide "Pearson's test" because few study declare it.**

*P 7 l 18: awkward prhasing (less gradient, more negative lowering). This seems odd- If I read this correctly, would expect less slope lower flow (glacier more stagnant) and therefore glacier thickening. Lowering already implies a negative dh/dz – do you mean that less slope implies higher lowering rates? Please explain/justify here.*

**[reply] This is just a mistake. We remove "negative".**

*P 7 19 -22 statement is vague, and circular. Please rephrase. What is the conclusion here?*

**[reply] We intend to mean that surface lowering varies in the debris-covered area with slow flow velocity. This variety of lowering rate could be due to variety of melting rate because emergence velocity is expected to be negligible. The melting rate on the debris-covered area should be controlled by thickness of debris-mantle.**

**We change here as "On the other hand, large variability of the rate of elevation change at slow speed surfaces (Fig. 7c) should be affected by large variability of surface mass balance because the emergence velocity is expected to be negligible. The area with slow flow speed (< 10 m $a^{-1}$) extends up to 19 km from the terminus intermittently (Fig. 6d) while the rate of elevation change negatively increases oppositely with elevation (Figs. 6a and 6b). These suggest that distribution of debris**

**thickness more likely results in the observed distribution of surface lowering of the glacier.".**

*P 7 section 4.2- this should be presented before 4.1 and merged with that section. Not enough material for a stand alone section.*
**[reply] We merge this to results section.**

*P7 l 23 -25 remove "where two surfaces co-exist" and quantify the statement – what does "similar" mean? Or "slightly stronger"? please quantify. Gardelle et al 2013, should also be mentioned here.*
**[reply] We change the description "which is consistent with the results from the other Himalayan regions (Kääb et al., 2012; Nuimura et al., 2012; Gardelle et al., 2013).".**

*P7 l 25 "exposed ice surface" please revise to "clean glacier tongues "*
**[reply] This description is not limited to glacier terminus. In addition, we think that "clean glacier" is not a general term. We revise here "debris-free ice surface".**

*p 7 l 27 – 28 "These findings support the proposition that the role of debris mantles in glacier mass balance and glacio-hydrological models needs to be reassessed or treated like debris-free glaciers (e.g., Kääb et al., 2012).*
*I do not agree with this statement, ie the debris covered glaciers cannot be treated in the same way as clean glaciers in the hydrologic model- the parametrization (foe ex ablation gradients etc. need to be different than clean ice, for example non-linear vs linear). Please revise/remove. This does not improve the discussion here.*
**[reply] We remove the description of "or treated like debris-free glaciers" and change the description as "This finding suggests that the role of debris mantles in glacier mass balance and glacio-hydrological models needs to be reassessed.".**

*P7 l 30: use of assive voice, please revise. Mention what test was used.*
**[reply] We imagine that "assive" means "passive" and that the reviewer #2 requires active voice, so we change here as "We find a significant negative correlation..". We used Pearson's test but we do not think that it should be declared specifically because many studies do not provide "Pearson's test" for this kind of simple linear correlation.**

*P7 section 4.3 there is a missing word, perhaps "Role of supra glacier lakes..."?*

**[reply] We merge this subsection to that of results section.**

*P7 l 30 -33 methodology not clear, was elevation change extracted pixel by pixel and then a correlation was performed? Or glacier-wide? I do not see how the lake area was treated here while these surfaces are highly dynamic, and the lake in 1970s will not be in the same place in 2010. Please revise the methodology here.*
*I suggest assessing the rate of elevation change as a function of % lake cover- but this again will yield more results when a larger area will be taken into account.*
**[reply] We use map of supraglacial ponds in 2010 (35 ponds) and map for rate of elevation change (1975-2010) to compute the statistics. Using Zonal Statistics tool of ArcGIS, rate of elevation change at individual pond (polygon-based) mapped in 2010 was extracted. In this sense, elevation change extraction was pond-wide. Afterwards, a correlation of these two variables is calculated. As aforesaid, we only consider ponds mapped in 2010, but not that of 1975, to extract elevation change and perform correlation or similar elevational change analysis. We agree with the reviewer's comment that pond surface on debris-covered glacier surface are dynamic and ponds may not be in the same place for long, and thus we address this here (P8L2) as well as in the section 3.4 (P6L18). Our main point here is that, even if supraglacial pond exists for short period such as a few years, site including supraglacial pond absorbs heat intensively and thus results in greater rate of elevation lowering. We revise the manuscript as follow:**
**In the method section 2.8, we add the following description: "Using the polygon-based map of supraglacial ponds in 2010, we extract rate of elevation change of the glacier surface including pond pixel by pixel, and then average for individual pond."**

*P7 l 34 "the rate of elevation change for supraglacial ponds (–1.25 ± 0.34 m a–1 ) is significantly more negative" again rephrase, easier to interpret "supra glacier ponds show more lowering.."*
**[reply] We revise it.**

*P7 l 35 ' [lakes and cliffs] are the principal spots of extensive heat absorption" This is not fully argumented here and it needs to be developed. May be true for lakes (but what about edges of lakes vs lake surface. And what about clean ice cliffs, which do not have a low albedo?" Statement is speculative.*
**[reply] We do not agree with "Statement is speculative." because here we introduce**

**previous studies which dealt with ponds and cliffs as hot spots on debris-covered glacier. So we do not change here. FYI, ice cliff tends to get dirty by sand and silt supplied with meltwater from the top edge of the cliff. This is not described explicitly but we can find "observed" lower albedo of ice cliff than debris-free ice in the related studies. In addition, even if ice cliff has high albedo, debris-covered area is generally located at low elevation and thus exposed ice tends to melt faster than that at high elevation, which was tested in Fujita and Sakai (2014).**

*P8 l1 2 "The distribution of significant lowering of supraglacial ponds.." Awkward, rephrase*

**[reply] We rephrase here as "The distribution of significant lowering of glacier surface where supraglacial ponds exist".**

*P8 l 6 – 15 this whole paragraph on the role of supra glacier lakes reads quite speculative and should be marked so. Also the discussion is not very conclusing, ie why would a gentler slope be expected with the lowering of the surface? Regarding Ragletti et al 2016 it is not clear whether the pond density correlated positively or negatively with a stagnant glacier tongue. Please completely revise and clarify this section. While I do agree that supra glacial ponds play a role in surface evolution, thee statements are not well supported with the current data, and they should be marked as speculative.*

**[reply] We agree with that this paragraph is speculative. We have no other evidence to support the effect of ponds on elevation lowering of debris-covered area. However, we do not show just a correlation but provide plausible mechanism by citing previous studies dealt with this issue. We merge this subsection to others addressing pond, and mark that this is speculative discussion.**

*P8 section 4.4 This is a different study area, and it does not fit easily in the discussion, If the authors want to compare and contrast 2 areas, then the same analysis should be applied to both. This entire paragraph can be conderesed into one concise phrase.*

**[reply] We think that the same analysis is preferable but that it is not an absolute requirement. If this condition was strictly applied, many studies could not exist. Although we do not imagine what "conderesed" means (we are really sorry for our poor English capability), revise phrase in the section.**

*P8 l 22-24: this is beyond the scope of the paper to understand the mass balance of Khumbu glacier. Revise/remove. Also, what is gELA? gAAR? This is too much focus here*

*for a comparison with the main study area.*

**[reply] We define the two terms in the method section. But by following the comment, we rephrase this paragraph without using gELA and gAAR.**

*P8 l 35-37 "This high pond density should have caused slightly negative rate of elevation change around 5100 m a.s.l. (Fig. 9a) and thus likely contributed to the more negative geodetic mass balance of Khumbu Glacier." Statement not conclkusive .."should have.." what does this mean? Are the authros implying that the high % of supra glacier ponds explain the higher rates of lowering of Khumbu compared to Kangchenjunga? Please revise/clarify- the reader is left unclear here.*

**[reply] This understanding is correct. We assert that the high pond density would explain "possibly" the higher rates of lowering of Khumbu Glacier than that of Kanchenjunga. We change here to "would cause" to weaken our assertion.**

*P 8 section 4.5 Here the authors introduce yet another area, the Langtang. Please revise this reasoning- the authors could simply stay focused on the study area and then compare and contrast where appropriate with results from other areas without having a subsection for the other study areas. Again, stetemtnds in section 4.5 are not conclusive. Please revise carefully statements such as " On the other hand, the geodetic mass balances in the Khumbu region are similar each other.." English language is not correct in many of these cases.*

**[reply] We remove this section and address contrast and similarity among glaciers in other sections.**

*P 10 l 17 : pleasre remove overstatements such as ".. even though it is widely expected that the insulating effect of debris suppresses the ice melting underneath."*
*There is no debris cover thickness available to be able to have such a hypothesis to start with.*

**[reply] This comment may refer to P9. We remove this statement.**

*P 10 l 18 again instead of "largely negative elevation change" one could simply say "lowering"*

**[reply] This comment may refer to P9. We change here accordingly.**

*P 10 l 30 : It is not the purpose of the study to demonstrate the effectiveness od DEM to derive geodetic mass balance. This was already demonstrated by Berthier et al 2007,*

*and many others. Furthermore the authors do not have validation data to support this statement. The study simply derived elevation thickness based on multiple DEMs.*
*The conclusion section is quite decretive and conclusions are not very clear. Please rework this section. What are the shortcomings? Future directions? What about uncertainty? And there is no discussion to why the larger area was not considered?*
**[reply] This comment may refer to P9. We remove the description about use of high resolution DEMs, and address the shortcomings of this study (area limitation and single glacier) though we are very sorry to be force to trivialize our study.**

*References*
*The reference list seems incomplete. Suggested references to incude below, fuethermore work of (Takeuchi et al. 2000, Yukari et al. 2000, Suzuki et al. 2007, Wang et al. 2011, Foster et al. 2012) for mass balance.*
**[reply] Takeuchi et al. (2000) is a field experiment to understand/parameterize debris thickness and ice melting under the debris, which is not the purpose of our study. Yukari is the first name of Y. Takeuchi, and these are the same paper (see the titles and the journals). The other three papers recommended here are studies estimating distributions of debris thickness (or proxy of thickness) on debris-covered glaciers. These do not relate to glacier mass balance dealt in our study. We do not understand why these studies should be cited in the context of our study.**

*Figures and tables*
*Table 1 "resolution" should be "spatial resolution". Also see my previous comment, it is unclear why the ASRER GDEM was used of authirs derived their own DEMs, and how compatible this is with the current DEMs.*
**[reply] We add "spatial". It is clearly addressed in the main text that ASTER GDEM2 is used for orthorectification and hypsometry.**

*Table 2 see previous comment about averaging vs reporting the two ranges. Also, 3 decimals seems too much given the uncertainties, 1 decimal would suffice. I suggest only 2 scenarios with 2 densities without extrapolating to unmeasured area.*
*"Averaged mass balance" should be "averages mass balance change"*
**[reply] We show 3 decimals here to distinguish two values of case 1 otherwise it looks same. But we do not agree that 1 decimal suffices. We express the values in 2 decimals. We will recalculate values for case 2.**
**We believe that it is meaningful to provide geodetic mass balance for the entire**

**glacier for succeeding research. We do not think that method for estimating value and uncertainty used in this study is too incorrect to be denied. Although the descriptions may be unclear (we revise them), assumptions are similar to previous studies facing same issue. Unless method and/or concept is substantially wrong, worth of information should be judged by succeeding studies.**
**"Mass balance change" is not appropriate wording because unit of this is that of mass balance (m w.e. $a^{-1}$). Suggested "Mass balance change" sounds a term expressing some acceleration/deceleration of mass balance (m w.e. $a^{-2}$). But we do not calculate such variable. We do not change it.**

*Table 3: debris covered area is not an attribute of ponds. Either rename the table or move this variable. Dp (km-1) – how can pond density be in km-1, and not in % of glacier area? Please address.*
**[reply] We replace "attributes" by "statistics". Unit of pond density is mistaken. We revise it as ($km^{-2}$).**

*Figure 1: Is this the entire extent of the images (mostl likely not)? If not, show the entire extent. It does seem from this figure that there is enough data to extend the analysis to a larger area (see general comments).*
*I do not see needed to put the GCPs here. Also, I note that there are no GCPs towards the accumulation area- it may affect the quality of the orthorecification.*
**[reply] This is not the entire extent as you imagine. We think that it is too large to show. As we replied above, we are not going to extend the analysis area so that we do not think that it is necessary to show the entire image. We admit that high elevation GCPs should be preferable. We do not understand why the GCP on the figure is unnecessary.**

*Fig 2 it is confusing to show on glacier and off glacier with outlines. I suggest removing the outline and only showing the glacier extent as in fig 2b. "Spatial distribution" in the caption is not really needed.*
**[reply] All similar studies show glacier outlines. We do not understand why we are forced to remove the outlines. We remove "spatial distribution" from the caption.**

*Fig 2a green crosses are not visible, it is enough just mentioning in text*
**[reply] We change green to black. If we did not show the crosses, you would require to show them.**

*Fug 2b velocity vectors not visible. The map seems to contain a sontinuous surface made by extrapolating the vectors? Not clear. I suggest redoing only with vectors perhaps in black, or red to blue with magnitude on the pan image.*

**[reply] We redraw the vectors according to this suggestion. Thank you.**

*Fig 3 caption "Red and blue denote debris-covered and debris-free surfaces, respectively" remove this, it is already in the legend. Fig caption describes the symbol but does not explain the figure. On both 3 a and 3 b, it is not clear how you can have both debris free and debris covered at the same evelation- different branches? This should be explained/shown on a figure.*

**[reply] We remove this sentence. We think the explanation/interpretation of figure should be addressed in the text.**

*Fig, 4 does not bring much, I suggest removing.*

**[reply] We try to embed it into somewhere if possible. But all similar studies showed this kind of histogram. We do not understand why we are not allowed to show it.**

*Fig 5 "Pond area" – would be clearer to say pond size (because this can be interpreted as surface area covered by ponds" Fig 5 a and b I am not sure they bring much to the discussion, it would be clearer to show this on a figure (PAN +lakes 1075 and 2010 in different colors). I suggest removing these and focusing on fig 5c.*

**[reply] We use "pond size". We do not agree that Fig. 5b can be removed because relationship between pond size and surface lowering may be useful to parameterize some kinds of model (e.g., Ragettli et al., 2015). We remove Fig. 5a.**

*Fig 5c "distance from terminus" – specify "up the glacier". Plese comment on the variability (spikes) in the caption. Some peaks correspond with lakes location, others don't- so the hypothesis of increasing number of lakes thinning is not fully supported with the figure as it is.*

*Also, while the error bars are good to have, for the lakes this is pretty noisy. I suggest doing this without error bars for the lakes and just mentioning the area uncertainty in the text.*

**[reply] We change notation of x-axis. We think that discussion/interpretation of figure should be addressed in the text, not in the caption. We do not understand why the error bar should be removed. Error bar is necessary to address that the difference of**

**surface lowering is "statistically significant".**

*Fig 6 the profiles should be shown on a figure*
*Also, the figures are hard to understand- signal seems noisy –*
**[reply] We do not catch what the first sentence means. We think that these "noisy" figures are necessary to show background for Fig. 7.**

*Fig 7 There is some interesting stuff her ebut it is so busy that it is hard to see any patterns, let alone match colors with the previous figures. Not sure it is useful as is.*
*Is it really such a steep gradient in dh/dt at elevations > 5500? Or is this due to error? Please comment.*
**[reply] We do not catch what the former part of this comment about color means. We use the same color in Figs. 6 and 7. Otherwise we cannot describe the difference among tributaries. About the latter comment, this is not error. Why does the reviewer think that this is due to error? We briefly check elevation profiles of surface lowering in Maurer et al. (2016, Bhutan) and King et al. (2016, Khumbu). Although we cannot show specific values, gradient in Khumbu is similar to ours (~2 m $a^{-1}$ $m^{-1}$ = $a^{-1}$) but that in Bhutan (~1 $a^{-1}$) seems half of ours. We add some descriptions about this gradient by adding more studies if profiles are available.**

*Fig 8 is not needed, it is enough to talk about other studies in the text.*
**[reply] We can remove this but we leave it to the editor's decision.**

*Fig 9 dh/dt vs elevation was already presented in previosu figures in a different form. I do not see it fit to bring in the Khumbu data,since the analysis was not oerformed in this study; again a discussion in the text would be enough.*
**[reply] We can remove this but we leave it to the editor's decision.**

**References mentioned in the reply**

**Berthier, E., Arnaud, Y., Kumar, R., Ahmad, S., Wagnon, P., and Chevallier, P.: Remote sensing estimates of glacier mass balances in the Himachal Pradesh (Western Himalaya, India), Remote Sens. Environ., 108, 327–338, doi:10.1016/j.rse.2006.11.017, 2007.**

**Gardelle, J., Berthier, E., Arnaud, Y., and Kääb, A.: Region-wide glacier mass balances over the Pamir-Karakoram-Himalaya during 1999–2011, The Cryosphere, 7, 1263–1286, doi:10.5194/tc-7-1263-2013, 2013.**

**Kääb, A., Berthier, E., Nuth, C., Gardelle, J., and Arnaud, Y.: Contrasting patterns of early twenty-first-century glacier mass change in the Himalayas, Nature, 488, 495–498, doi:10.1038/nature11324, 2012.**

**King, O., Quincey, D. J., Carrivick, J. L., and Rowan, A. V.: Spatial variability in mass change of glaciers in the Everest region, central Himalaya, between 2000 and 2015, The Cryosphere Discuss., doi:10.5194/tc-2016-99, in review, 2016.**

**Maurer, J. M., Rupper, S. B., and Schaefer, J. M.: Quantifying ice loss in the eastern Himalayas since 1974 using declassified spy satellite imagery, The Cryosphere, 10, 2203-2215, doi:10.5194/tc-10-2203-2016, 2016.**

**Nuimura, T., Fujita, K., Yamaguchi, S., and Sharma, R. R.: Elevation changes of glaciers revealed by multitemporal digital elevation models calibrated by GPS survey in the Khumbu region, Nepal Himalaya, 1992–2008, J. Glaciol., 58, 648–656, doi:10.3189/2012JoG11J061, 2012.**

**Pieczonka, T. and Bolch, T.: Region-wide glacier mass budgets and area changes for the Central Tien Shan between 1975 and 1999 using Hexagon KH-9 imagery, Glob. Planet. Change, 128, 1–13, doi:10.1016/j.gloplacha.2014.11.014, 2015.**

**Ragettli, S., Pellicciotti, F., Immerzeel, W. W., Miles, E., Petersen, L., Heynen, M., Shea, J.M., Stumm, D., Joshi, S., Shrestha, A.: Unraveling the hydrology of a Himalayan catchment through integration of high resolution in situ data and remote sensing with an advanced simulation model, Adv. Water Resour., 78, 94–111, doi:10.1016/j.advwatres.2015.01.013, 2015.**

**Ragettli, S., Bolch, T., and Pellicciotti, F.: Heterogeneous glacier thinning patterns over the last 40 years in Langtang Himal, Nepal, The Cryosphere, 10, 2075-2097, doi:10.5194/tc-10-2075-2016, 2016.**