# *Reply to Interactive comments on* "Tracing glacier changes since the 1960s on the south slope of Mt. Everest (central southern Himalaya) using optical satellite imagery" by S. Thakuri et al.

We would like to thank Eric Larour (editor) for obtaining the reviews, and we would like to thank to Mauri Pelto, Thomas Mölg and 2 anonymous referees for providing their careful and very constructive comments on our manuscript. We think to be able to address all their suggestions and without doubt their contributions have resulted to significant improvement of the manuscript.

**Response to Short Comments by M. Pelto**

General comment

Thakuri et al (2013) provide a data set on glacier changes in the basins on the south side of Mount Everest, Nepal. The combined examination of surface area change, debris cover change, snowline altitude change and terminus change have the potential to provide a data set of enough breadth for valuable interpretation. The paper at present does not realize that potential. I come away with more questions than answers. Some key references are not cited for comparison, for example Bajracharya and Mool (2009). Key assessments are not explored, for example, the highest rate of thinning being in the transition from clean ice to debris cover (Bolch et al, 2011). A key parameter needs to be better defined that these glaciers are the highest in altitude, is this based on mean, median or top elevation? Most comments below can be addressed with simply a better presentation. There is also an opportunity to greatly strengthen the SLA record without too much additional effort, at present this data set is not robust.

**Response:** All the suggestions provided by reviewers has been followed, thus we think we have significantly improved quality of the revised manuscript. Specific answers to your general comments are reported below.

Specific comments

5391-121: Reword "We have decided to contribute to the international debate on glacier changes in the Himalaya by focusing our attention" to This contribution examines glacier changes on the south side of Mount Everest as part of an effort to better define glacier changes in the Himalaya.
**Response:** The suggestion has been followed by changing to: "This contribution examines glacier changes on the south side of Mt. Everest as part of an effort to better define glacier changes in the Himalaya."
5393-12: Many, is vague-quantify this. Bajracharya and Mool (2009), indicate there are 278 glaciers in the Dudh Koshi with 40 accounting for most of the area and all of these being valley-type.

**Response:** The suggestion has been followed by inserting: “Bajracharya and Mool (2009) indicate that there are 278 glaciers in the Dudh Koshi basin, with 40 glaciers accounting for most of the glacierized area (70%) and all of these being valley-type.”

5393-2: This period also coincides with maximum ablation low on the glacier.
**Response:** The suggestion has been followed by inserting this sentence in the manuscript.

5396-10: lateral and frontal moraines often are not in contact with retreating glacier. How was this dealt with? Were surface streams issuing from beneath glacier used?
**Response:** The glaciers are always delineated at inner bound (toward the glacier) of the lateral moraine. When the moraines are not in contact with the retreating glacier we used the surface streams issuing from beneath glacier. The suggestion has been followed by inserting this sentence in the manuscript. “The surface streams issuing from beneath glacier were used as indication of the glacier boundary when the lateral and frontal moraines were not in contact with the glacier.”

5396-22: Was the SLA only identified on glaciers or along non-glaciated terrain as well?
**Response:** The suggestion of clarification has been followed by specifying in the revised manuscript “….on the glaciers”.

5397-4: Switch time to season.
**Response:** The suggestion has been followed by replacing “time” by “season”.

5400-9: generally continuous and constant.
**Response:** The suggestion has been followed by replacing “generally continuous as well as constant” with “generally continuous and constant”.

5401-13: area loss instead of shrinkage, be careful using shrinkage in place of surface area loss, particularly in figure captions.
**Response:** The suggestion has been followed in the revised manuscript.

5403-21: Debris cover increases during periods of high ablation as more englacially stored debris is exposed. Bolch et al (2008: 2010) note the increase in debris cover and loss of clean ice area during the same period as a result.
**Response:** The suggestion has been followed and these observations reported in the revised manuscript.

5404-18-25: Confusing presentation need to present results in a table.
**Response:** The suggestion has been followed and a table added.

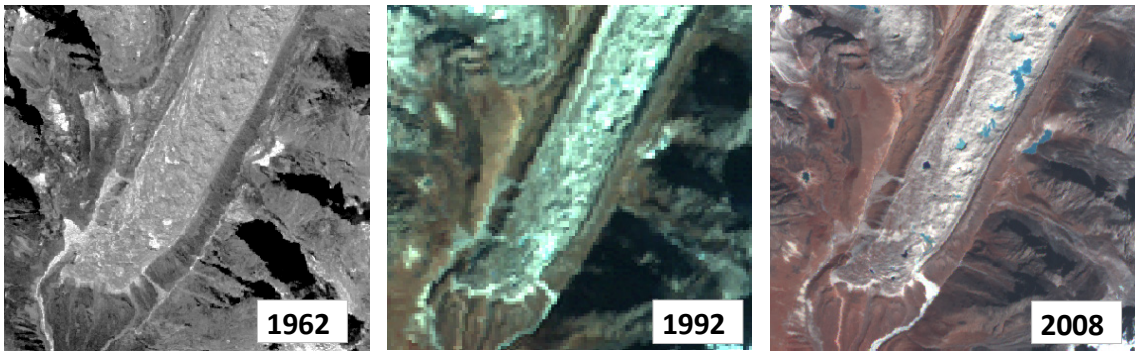
5404-28: This is expected given the lag in response time and that downwasting leads to more volume loss then retreat or area loss.
**Response:** The suggestion has been followed adding the proposed clarification into the manuscript.

5405-4: Remove if this contention is not better supported.
**Response:** According to Referee 1, we compared with the same set of 10 glaciers in the overlapping time period with mass balance data of Gardelle et al. (2013). We removed existing inconsistency as suggested and finally, we revised texts in the manuscript accordingly.

5405-27 and 5406-5: Compare to Bajracharya and Mool (2009), also to Ren et al (2006). The terminus rates reported by these papers even for the same periods and same glaciers do not match those in the supplement. Why not? Bajracharya and Mool (2009) Table 1 can be compared to the supplement, and for glaciers like Ama Dablam, Imja, Khumbu etc, the retreat rates are generally lower in Thakuri et al (2013). The supplement itself is poorly presented, unreadable without effort. This is the key data, it must be well displayed, not microscopic. Ren et al (2006) is in the references but not in paper.

**Response:** Bajracharya and Mool (2009) based their analysis on the comparison of recent imagery with old topographic maps. As already discussed by Bolch et al., 2011 and also in our manuscript) maps of this area are valuable information, but they should only be the second choice if no suitable imagery is available. Bajracharya and Mool (2009) found a high terminus glacier retreat (mean 19 m $a^{-1}$ in 1960s to 2007) while for the same set of glaciers we found 9 m $a^{-1}$ in 1962 to 2008. We think that their delineation of the glaciers is partly speculative.

We used high resolution satellite imagery to delineate the glaciers. We present as an example (already reported in Fig. 5 in Bolch et al., 2011) the terminus evolution of region of Khumbu glacier. Our images indicate that the Khumbu Glacier has undergone no change in recent decades. The following images show the terminus of Khumbu Glacier of the year 1962, 1992 and 2008.




It can be seen that there is no retreat of the distal part of the terminus. The situation did not change significantly since the 1960s as the imagery shows. Hence, we do not agree with the delineation of Bajracharya and Mool (2009) as it is highly speculative (20 m $a^{-1}$ in 1960s-2007 for the Khumbu glacier). In addition, dead ice which is in contact with the glacier ice would be included into the glacier from a glaciological point of view (Kadota et al., 2000).

We included a short statement on this issue in the discussion in the revised manuscript.

**Response:** The suggestion has been followed by changing the structure of supplement material. We separated the data into different tables and presented in a better way.

**Response:** We had compared our result with Ren et al. (2006) and this paper was cited in Figure 5.

5406-20: These are a few of the available studies looking at different glacier over various time periods. Expressing this in text alone and without citing more extensive surveys is not valuable.

**Response:** Please see Figure 5 in our manuscript and related discussion in which we considered all papers referring changes in termini and surfaces in the region (versus the relevant observation period).

5407-16: indisputable not appropriate; highly likely okay.
**Response:** We modified to “highly likely”.

5407-12: Bolch et al (2008) noted the highest downwasting near the transition between clean and debris covered glacier surface. This elevation is then important and should be mentioned. Bolch et al (2011) noted that “most glaciers show maximum lowering in their mid-ablation zones, with a negligible change near their termini”, during the 1970-2007 period. Do your results confirm this? Does this not reveal importance of debris cover?

**Response:** The suggestion has been followed by inserting the observation of Bolch et al (2008) “the highest downwasting near the transition between clean and debris covered glacier surface” in the revised manuscript. Moreover, we addressed the issue of elevation change due to downwasting in detail behind to our response to Referee #1.

We did not confirm the result of Bolch et al. (2011) because we did not carry out the analysis of mass balance variation near the SLA. We used just a single DEM to obtain the ΔSLA in 1962-2011 period that limited us the possibility to confirm their observation.

5408-6: Should refer to the local mass balances for recent years from Wagnon et al. (2013).
**Response:** The suggestion has been followed by referring the local mass balances from Wagnon et al. (2013) to the revised final manuscript.

5408-17: Changes in area and terminus position can be accurately assessed from individual satellite images every few years as both respond to long term trends. SLA varies daily and annually. Hence we can have only low confidence in the few scattered observations. It is okay that it is not the definitive ELA, just need more dates for validation note annual observations from Mernild et al (2013) and Pelto et al (2013). There is good imagery every year back to 2000 that can identify the SLA. The SLA need not be determined on all glaciers in all years just choose a couple that have a reasonable elevation gradient such as Ngojumba, Imja and Langmuche and use these for validation.

Below are dates when I found suitable Landsat images.
Year Julian Date: 2000 288 304: 2001 290: 2002 295: 2003 312: 2004 299 317: 2005 301 317: 2006 320 336: 2007 291 323: 2008 294 342: 2009 296 312: 2010 295 315: 2011 286 302:
**Response:** The suggestion will be followed. We decided to use additional images from 2000 to 2011 for the validation of SLA results. We will choose some glaciers as sample and incorporate this analysis in the final revised version of manuscript (if too long as Supplementary material).

5408-21: This paragraph on temperature can be greatly shortened and some data put into tabular form. Since this paper does not present new climate data, the topic deserves less coverage.
**Response:** The suggestion has been followed and the paragraph has been shortened.

5409-19: Poor long sentence. If the temperature increases in summer this raises the threshold for rain-snow transition during the summer monsoon. This is important because, for summer accumulation type glacier's, peak ablation and peak accumulation happen during the same period.

**Response:** The suggestion has been followed by rewriting this sentence and emphasizing the suggested point in the revised manuscript.

5411-23: The termini of smaller glaciers are indicated as retreating less as a percentage, is this at all related to the lower terminus elevation of large glaciers?

**Response:** We did not find any significant correlation between the termini retreat and the elevation of glaciers, i.e. in terms of minimum glacier elevation (terminus elevation), mean elevation of ablation zone, the lowest elevation of accumulation zone, mean elevation of accumulation zone, and thus also in general with the mean elevation of whole glacier.
We found that the termini retreat, double for larger glacier (2.3 %, 4.3 %, respectively for the 1962-2011 period), is strongly related to the increased debris coverage of each glacier during the same period ($r= 0.87$, $p<0.001$ for ΔTerm/Length of ablation zone *vs* ΔDebrisCov*), that means higher increases of debris coverage prevented the termini retreatment. Moreover we found that the termini retreatment is related to the ΔSLA of each glacier during the same period ($r= 0.67$, $p<0.01$ for ΔTerm/Length of ablation zone *vs* ΔSLA*), that means more negative glacier mass balances induce an increasing of debris coverage ($r= 0.79$, $p<0.001$ for ΔSLA *vs* ΔDebrisCov*) (Chiarle et al., 2007; Rickenmann and Zimmermann, 1993) and a lower glacier retreatment.

* Data are log-transformed to assure the normality of residuals.

5412-5: Does the higher area losses of small glaciers suggest it is the lowest elevation accumulation zones most impacted by climate change?

**Response:** The size of glaciers is significantly correlated with the mean elevation of the accumulation zone ($r=0.61$, $p<0.01$*), the minimum elevation of accumulation zone (SLA) ($r=0.54$, $p<0.01$*), while it is not significantly correlated with the mean elevation of the ablation zone ($p=0.15$) as well as with the minimum glacier elevation ($p=0.80$). Consequently, the size of glaciers is correlated with the mean elevation of the entire glacier more weakly ($r=0.45$, $p<0.05$*). Therefore it is more precise to say that the smaller glaciers present mainly accumulation zones at lower elevations.
Concerning the loss of glacier surface area, we observed that these losses impacted more the small glaciers ($r=0.73$, $p<0.01$*). Moreover, we find that ΔSurf is well correlated with the minimum elevation of accumulation zone (SLA) ($r=0.64$, $p<0.01$*), while the elevation related to the other glacier zones are not significant.

5412-23: The larger glaciers have both a higher accumulation zones and lower elevation termini, cannot just stipulate they are at the highest elevation.

**Response:** As discussed above we will specify that larger glaciers have higher accumulation zones.

5413-9: Again this statement is too simply made that the higher altitude glaciers are impacted differently than lower altitude glaciers. What is the altitude that you go by?

The larger glaciers in the Khumbu have both high accumulation zones and low terminus elevations. They are being impacted, but is it the same process as for smaller glaciers?

**Response**: We will follow the suggestion, please refer to above responses.

5413-11: Have they been preserved more than other areas? This case cannot be made that the Everest glaciers are responding less without very careful review of glacier change from the Himachal Pradesh to Sikkim. I am not suggesting this be attempted here. For example Basnett et al (2013) noted a 3.3 % area loss of Sikkim glaciers from 1990-2010.

**Response:** We modified the statement (page 5413: L8-11) by replacing "the highest glaciers in the world" with "among the highest glaciers in the world".

We had cited all the recent papers published on the glacier variations (Fig. 5) from other parts of the Himalaya, except the papers dealing with single glaciers or very small sample-sized in comparison with our study. In general, the comparison with all these papers indicated lower glaciers change except very few cases.
We were not aware of the paper of Basnett et al. (2013) by the time we submitted the manuscript to "The Cryosphere" because the paper was published just around the time of our manuscript submission and so this paper was not included in our comparison. The glacier area loss reported by Basnett et al (2013) is lower (3.3%) than we observed in our study during the same period (8.6 %) despite the terminus retreat reported by Raina et al. (2009, 13.0 m $a^{-1}$) from Sikkim Himalaya is higher than our study (8.2 m $a^{-1}$, Figure 5a). Sikkim Himalaya is located in the eastern southern Himalaya, but on the east national boundary of Nepal (central southern Himalaya), not so far from our case study site (~120 km of longitudinal distance).
The low glacier loss also for those glaciers could be due to their mean high elevation (5780 m, even if it is not clear in the text if it is weighted mean on surface or not). In our case the weighted mean elevation is 5720 m. Now, we inserted this reference in the revised manuscript and discussed accordingly.

**Response to Short Comments by T. Mölg**

General comments

I have only read the abstract of the paper, but would like to mention one thing that is possibly relevant for the whole paper. Giving the rate of areal shrinkage in percents, and deducing from the resultant time series "an acceleration of the surface area shrinkage" can be misleading. Any glacier will increase its relative shrinkage rate even if the actual area change per year is constant. For example, a glacier with initially 10 $km^2$ area, which loses 0.5 km2 every year over a period of ten years, will show an initial relative shrinkage of 5%/year (0.5/10) and a final relative shrinkage of 9.1%/year (0.5/5.5).

In my opinion giving areal shrinkage in absolute numbers is the better way, especially if a trend in areal shrinkage shall be interpreted in terms of climatic forcing. Using percentages can be useful for comparing shrinkage rates between different regions/glaciers (but over a common period). I hope my comment is helpful for preparing the final version of this manuscript.

**Response:** The suggestion has been followed. We included the absolute values and also the change rate (expressed in % $a^{-1}$), but dividing the observed difference between two analysed years by always the same reference year: the initial one, 1962. In this way the change rates can be compared, the acceleration can be evaluated correctly and other studies can directly use the change rate calculated in this work.

**Response to Anonymous Referee #1**

General comment

I have a mixed opinion on this paper. This is not a very exciting paper. Glacier area/length changes have been published for the Everest area before and the results presented here do not really differ from what has been found previously, in particular in a paper that recently appeared in press in Annals of Glaciology (Shangguan et al., in press). However, the analysis of the remote sensing data is of a high quality, the statistical treatment of the result is very serious, the temporal resolution of the analysis is high and the observation of the multi-decadal rise in snow line altitude, although not straightforward to interpret, is a clearly novel observation for this region. Currently the paper is too long (especially the discussion) so a reduction in length would be welcome. So maybe the authors simply did not manage to highlight the added value of their study in comparison to published work to make it more exciting to the reader? This should not be so difficult to fix.

**Response:** We have substantially improved the manuscript according to the suggestions and comments received from all the reviewers. We think the revised manuscript addresses all these concern raised. Specific answers to the general comments are reported below.

Substantial remarks

1) The authors need to better state what is different compared to their 2008 Journal of Glaciology article. I can clearly see an added value in the present contribution but it needs to be emphasized. They need to highlight differences in data sources, differences in results, addition of the change in SLA... Salerno et al. concluded to an area loss of 4.9% between the late 1950s and the early 1990s whereas the present study find a shrinkage of 13% between 1962 and 2011. Is the difference entirely due to the addition of the 90s and 2000s, period of rapid glacier shrinkage? Probably yes, as Table 2 show a cumulative area loss of “only” 4.8% between 1962 and 1992. This is the sort of discussion that the reader expect.

**Response:** The suggestion has been followed by inserting suggested discussion in the revised manuscript. In fact this paper is continuation of previous paper of Salerno et al. (2008) on glacier. But we should not only compare current paper solely as the update of Salerno et al. (2008). This paper has an added value and is different from the paper of 2008 in many aspects (also highlighted by the referee) like, 1) variables considered (in this paper, we are using 4 variables of change: surface area, terminus position, SLA and debris-coverage instead of only one variable, surface area in the paper of 2008), 2) data set used (all available maps and satellite images in high temporal resolution and longer period (1960s-2011) while two maps has been analysed in the paper of 2008 for the period of 1950s -1992. These differences in the variables considered and datasets used have allowed in analysing and producing results for higher temporal resolution. Despite this the surface loss in the two studies are similar in the period 1960s-1992 (4.8%). This paper provides a complete set of data for 4 variables that are not produced by any paper published yet.

2) The rise of the SLA over the last 50 years is interpreted in light of a model that was apparently developed by Kuhn in the Alps. To what extent this model can be transferred

to glaciers in the Everest area whose seasonal behaviour is dramatically different (summer accumulation type and even with ablation in winter according to [*Wagnon et al.*, 2013])? Previous authors [*Iturrizaga*, 2011] have challenged the fact that the SLA (or ELA) can be used as an indicator of glacier health for debris covered glaciers of Central Asia so this need to be discussed, at least. Also Thakuri et al. used a single mass balance gradient of 5 m w.e. yr-1 / 1000 m. But Wagnon et al. 2013, found varying mass balance gradient on two branches of the same glacier (Mera) and much more negative mass balance gradient on Pokalde glaciers. So there are limitations in using a single value for the MB gradient that the authors need to discuss. They also need to discuss the danger of picking the snowline from a limited number of images/years. If one examines Figure 3 in [*Rabatel et al.*, 2013] (http://www.thecryosphere.net/7/1455/2013/tc-7-1455-2013.pdf), he can figure out that by picking the snowline in 1998 and 2010 only he would conclude to a lowering trend in the SLA, which is not the case when all years are considered. This is the danger of having a limited temporal sampling of a variable know to present a high inter-annual variability. Thakuri et al. needs to at least acknowledge this or, better, improve the temporal sampling of their SLA analysis.

**Response:** The suggestion has been followed by inserting the suggested discussion in the revised manuscript. Moreover, we are validating the SLA records using additional images from 2000-2011, as we responded in the earlier comment by M. Pelto. Despite the simplicity in using the model, it involves certain sources of uncertainties while using this model like all the other quantitative models do. Despite these uncertainties, the model is pretty useful in appraising the possible contribution of temperature, precipitation and solar radiation. This model has previously been also used in this part of the Himalaya by Kayastha and Harrison (2008).

3) The authors must make a fair comparison to mass balance measurements in this area. In particular their claim that [*Gardelle et al.*, 2013] “seems to underestimate the mass loss in the last decade” is not justified. First, the region surveyed by Gardelle et al. is much larger than surveyed by Thakuri et al. (this paper) but also by [*Bolch et al.*, 2011] and [*Nuimura et al.*, 2012]. So quoting their region wide mass balance of -0.26 m w.e./yr and claiming that their mass loss is too low compared to other studies is not appropriate. Rather, when the same set of 10 glaciers and overlapping time periods are considered, it was found that there is a reasonable agreement between Gardelle et al. and Nuimura et al. whereas the mass balance measured by Bolch et al. are more negative. See Figure 12 in Gardelle et al.

**Response:** The suggestion has been followed. In the revised manuscript, we compared with the same set of 10 glaciers in the overlapping time period with mass balance data of Gardelle et al. (2013). We removed existing inconsistency as suggested.

4) A paper by Shangguan et al. recently appeared in press in issue 55(66) of *Annals of Glaciology* (see http://www.igsoc.org/annals/55/66/accepted.html) and deals with the same region and similar analysis. Thakuri et al. were probably not aware of this paper when they submitted their MS to TC but they need to compare their results with this other study that found a slightly larger rate of glacier area loss (19%) for a larger ice-covered area of >3000 km². Ideally, if the outlines from Shangguan et al. are available, it would be great if Thakuri et al. could compare the results of the two studies for the same group of glaciers, within the Sagarmatha National Park.

**Response:** Following the suggestion, we compared our surface shrinkage rate with the rates provided by Shangguan et al. (2014). The rates of glacier surface area loss provided by Shangguan et al. (2014) are really huge compared with all other previous studies in the Himalaya. They provide 0.63 % $a^{-1}$ in South and 0.59 % $a^{-1}$ in North of the central Himalaya. In general following the figure 5 of our paper, their observations of glacier surface loss rate are the highest in all previous studies. Moreover, giving a look at the Fig. 2b in Yao et al. (2012), Shangguan et al. (2014) estimated the highest rate of glacier surface loss compared to all the Himalaya, Tibetan Plateau, East Pamir and Karakorum.

We cannot agree with their numbers due to some important considerations. We suspect about their interpretation of glacier boundary and consider that they overestimated the glaciers in 1976. As we indicated in our paper the MSS imagery has lower spectral resolution and thus, problematic in especially for the small glaciers. We sustained our observation from the 1975 Landsat MSS image comparing with the high resolution Corona 1970s image. If we go through Fig. 3 in their paper, there are many problems with the outlines, especially, the rock outcrop of the glacier in the centre might have been a bit smaller than today, but it was for sure exiting and is counted as glacier area in 1976. Hence, the glacier area of 1976 is definitely too large. Using the same datasets in 1976 corresponding to the same geographic area, Yao et al. (2012) provided lower rate for both side of the central Himalaya compared to Shangguan et al. (2014) and confirmed more than 3 times higher surface area loss in the North (Mt. Qomolangma National Nature Preserve -0.504 % $a^{-1}$ in 1976-2006), compared to South (the Koshi basin -0.151% $a^{-1}$ in 1976-2000). Moreover, there are some other previous studies (Bolch et al., 2008; Salerno et al., 2008) on glacier surface changes for the southern part of the Koshi basin as we compared in our paper. These previous studies were important observations for the same region which Shangguan et al. (2014) avoided to compare with their result.

Specific comments

**P5390**
The total glacier area analysed in the study (~400 km²) needs to appear in the abstract
**Response:** The suggestion has been followed by inserting this point in the abstract.

values and its uncertainty.
**Response:** The suggestion has been followed.

L11. Indicate the time period.
**Response:** The suggestion has been followed.

L15. “largest”. Give the size class.
**Response:** The suggestion has been followed.

L16. South-oriented is a sense not a direction. This statement, as is, is not very clear.
**Response:** The suggestion has been followed by changing as: “South-North direction of the monsoon.”

**P5391**
L21. “we have decided to contribute to the international debate”. Strange wording to present a scientific study.

**Response:** We addressed this point in our response to M. Pelto.

**P5392**
L25. It is recommended to acknowledge the funding agencies when using GDEM with something like "GDEM is a product of METI and NASA".
**Response:** The suggestion has been followed.

**P5393**
L18. "Above 4000 m a.s.l, the precipitation starts…". And below? Do they increase?
**Response:** The suggestion has been followed by inserting in the text that "….precipitation increases with altitude by +0.053 mm[month]/m until 4000 m a.s.l. afterwards it starts decreasing (-0.017 mm [month] $m^{-1}$)".

**P5395**
L19. "obtained from the image". From which image? Did the authors extract the GCPs from a reference master image or from field measurements? Which software did they use for the processing of the Corona data?
**Response:** We extracted the GCPs from ALOS AVNIR-2 (2008) as reference image. The ERDAS IMAGINE® software was used for processing the Corona image. We included these information in the manuscript.

L26. Why did the authors use nearest neighbour resampling? It is well-known as a poor data interpolator.
**Response:** The nearest neighbor algorithm is the simplest and most common method (Citterio et al., 2009; Thompson et al., 2011; Brahmbhatt et al., 2012) that uses the value of the closest pixel to assign to the output pixel value and transfers the original data value without modifying it (Keys, 1981).

**P5397**
L25. In the error assessment of the SLA and its temporal variation, the authors also need to take into account the errors of the GDEM and the fact that they ignored temporal variations of surface elevation [*Rabatel et al.*, 2013]. Given that there is a generally tendency for glaciers in the Everest area to thin close to their ELA (e.g., Bolch et al., 2011), the authors' value for the rise of the snowline might be slightly underestimated. Something to include in the discussion together with the need to underline the caveat of the sparse temporal sampling for a variable that is known to experience high inter-annual variability.

**Response:** The suggestion has been followed by accounting the suggested errors in analysis of the SLA and modifying the manuscript accordingly. The vertical error for ASTER GDEM, as we presented in the data section 3.1, is ~ 20 m. The overall error will be calculated as the root sum of square of the vertical error (20 m) and the resolution error (Pelto, 2011) which is 9 m in the 1962-2011 period. Therefore for this overall analysed period (1962-2011) the estimated uncertainty is 22 m. Our estimation of ΔSLA in the overall period is 182±22 m therefore the magnitude of the uncertainty does not affect significantly our findings. Also, in case of ΔSLA acceleration analysis, the rates of change of SLA are 2.2 m $a^{-1}$ in 1962-1992 and 6.1 m $a^{-1}$ in 1992-2011. If we associate the calculated uncertainty to these rates, the final results 2.2±0.8 m $a^{-1}$ (1962-1992) and 6.1±1.4 m $a^{-1}$ (1992-2011) are not affected.

Bolch et al. (2011) reported for our study area an elevation decrease of 0.39 m $a^{-1}$ (from 1970 to 2007) due to downwasting in the ablation part of glaciers. This rate provides an estimate of ~ -19 m of elevation change in the 1962-2011 period (assuming the rate constant). In general we could explicit in the paper that the observed ΔSLA of 182±22 m (1962-2011) is probably underestimated of ~19 m. Moreover all ΔSLA estimations provided in the paper are affected by their specific small quote of underestimation. However this does not influence our discussions on the observed ΔSLAs (acceleration, differences vs glacier sizes) because the mass loss produces an elevation change always downwards directed. Differently, evaluating the role of climatic drivers in the ΔSLA with the simple ELA-Climate model by Kuhn (1981), we estimated that for the observed 182 m upward shift of SLA in the 1962–2011 period, e.g., a temperature increase of 1.1°C is required. If we consider more realistic an overall ΔSLA of 182+19=201m, a temperature increase of 1.2 °C is required.

**P5398**
L25. Unit of the mass balance gradient should be mm w.e. $yr^{-1}$ / m
**Response:** The suggestion has been followed.

**P5401**
L15. “has increased to 0.76%/a”. During what period?
**Response:** The suggestion has been followed by inserting the period (2008-2011).

**P5402**
L2. “robust” is probably more appropriate than “sustained” here.
**Response:** The suggestion has been followed.

L16. “the distribution of the annual rate of the SLA shift” reads better.
**Response:** The suggestion has been followed.

L23. “recent years” is not clear enough. Give the exact time span.
**Response:** The suggestion has been followed.

**P5403**
L13. is the % of debris-covered glaciers in agreement with previous authors? % of debris coverage were I think published in Scherler et al., 2011; Gardelle et al., 2013, Nuimura et al., 2012 and [*Racoviteanu et al.*, 2013]
**Response:** Yes, the percentage of debris-covered glaciers is in agreement with previous studies. For instance, we observed 32% debris-covered area in the Mt. Everest (Sagarmatha) National Park in 2011. In the same area, Nuimura et al (2012) reported 34.8% of debris-covered area in 2003-2004 and by Scherler et al. (2011) presented 36 % debris-covered area of glaciers in the central southern Himalaya in 2008. We are inserting these numbers and some others from previous authors in the manuscript.

L24. “debris cover rate”. Do the authors mean the “rate of debris-covered area change”?
**Response:** The suggestion has been followed by inserting replacing “debris cover rate” with “rate of debris-covered area change”.

**P5404**
L2. “relevant”! to be retained only if the authors compare the same set of glaciers. Right now this is rather irrelevant! See my general comment.

**Response:** The suggestion has been followed. We compared with the same set of glaciers in the revised manuscript as responded in the "Substantial remarks".

L7. Bolch et al., 2012 is not the appropriate reference for this statement. [*Oerlemans*, 2001] or other classical text books.
**Response:** The suggestion has been followed.

**P5405**
L11. "session". "section"?
**Response:** The suggestion has been followed.

L13-15. Not really clear. What is the size of the sample? Cannot the authors extract the same set of glaciers to really compare the two studies?
**Response:** The sample size of glaciers in Bolch et al. (2008) was of 92.26 $km^2$ (in 1962) covering the Khumbu, Nuptse and Lhotse glaciers. The suggestion has been followed by comparing the same set of glaciers and presenting the discussion accordingly in the revised manuscript.

L17-L22. The authors need to better link this statement from Yao et al. to their own results. Right now this reads more like a review of the literature rather than a discussion.
**Response:** The suggestion has been followed.

L23-L26. The rational behind this is not clear. More generally, there is a large room for improvement for section 5.2. For example here, cannot the authors extract the same set of glaciers as Yao et al. 2012, compare fairly the results and check whether those 3 glaciers behave like others in the area?
**Response:** The suggestion has been followed by removing these sentences.
Unfortunately, those 3 glaciers reported by Yao et al. (2012) are located outside our study area.
We think that there is much improvement in section 5.2 after addressing suggestions from all reviewers.

**P5408**
L11. "suggested" seems more appropriate than "confirmed" because the acceleration of the specific mass loss in those previous studies is not statically significant.
**Response:** The suggestion has been followed.

**P5409**
L2. Why should the rise in temperature in the Everest area "agree with" the rise in the Northern hemisphere? There is no reason given the well-known heterogeneous pattern of temperature change over the globe. This is the sort of weak statement that is useless and make the paper too long.
**Response:** The suggestion has been followed in the revised manuscript.
L20. "the temperature should be increased". Why "should be"?
**Response:** The suggestion has been followed by changing to: "is increased" in the revised manuscript.

**P5410**
L3. add "in Fig. S18 of their paper"
**Response:** The suggestion has been followed.

L29. “the increase of nearly 15%”. Make it clear this is the change needed according to the SLA model and not the change observed.
**Response:** The suggestion has been followed by adding “according to the SLA model” in the revised manuscript.

**P5411**
This whole first part of section 5.4 is more result than the discussion. It will also help to make the discussion shorter.
**Response**: We will try to change place of this part according to the integrity of the discussion.

**P5414**
L2. The conclusion about the temperature driving the glacier response is firm but this is only true if the Kuhn model can be applied as is in the Himalaya which I doubt. This need to be discussed and more nuances provided in the discussion/conclusion. Right now, the authors’ conclusion on the climate drivers are more speculation than a real demonstration.
**Response:** As we responded in the “substantial remarks” section, we will discuss the possible uncertainty in the use of model in the revised manuscript.

L15-L25. Lengthening the paper with future research plans is not really useful and make it longer.
**Response:** The suggestion has been followed by removing the future research plans.

**Figure 1**. I was surprised by the spelling “Ngojumpa” Glacier. I am more familiar with Ngozumpa. Both exist? The same for Lotse ‘no “h”). I think this one is typo.
**Response:** The suggestion has been followed by using “Ngozumpa” which is most common in use, though “Ngojumba” also appear in use for the same glacier.
We corrected the error in Lotse by replacing it with “Lhotse”.

**Figure 3**. Why are all the arrow upward? It is counter-intuitive for a retreating/area loss glacier (not really good for communicating to a broad audience).
3B. there is a strong North/south gradient in area loss. Is it mainly an effect of the altitudinal North/south gradient? May be something to discuss more?
**Response:** The suggestion has been followed. We changed the arrow in the downward direction for the terminus retreatment and surface area loss.

**Figure 4**. See my general comment. Only comparing the same set of glaciers make sense here. It seems that, for Gardelle et al., the authors used their regional value of -0.26 m w.e./yr rather than their 10 glacier values. Replace mass down-wasting by mass balance for the title of the Y-axis.
**Response:** The suggestion has been followed by comparison of the same set of 10 glaciers.
We replaced “mass down-wasting” by “mass balance” for the title of the Y-axis as suggested.
**Figure 5**. caption: “impact studies” does not describe what is shown in the graph.
**Response:** The suggestion has been followed by modifying the caption as: “Recent studies on the variations of Himalayan glaciers concerning both the ….”

**Response to Anonymous Referee #2**

General comments

In this paper, authors addressed to evaluate temporal change of glacier area (whole and debris-covered part) and SLA by remotely sensing data and historical maps. This kind of study using many kind of multi-temporal data sets valuable for evaluating glacier variation in long time period. However, the result has not yet reach publishing quality.
As authors noted in manuscript, the evaluation of glacier variation through area/length change has shortage due to delayed response to climate change. Hence, you need to explain the reason why you evaluate glacier variation through area/length change.

**Response:** Many recent extensive studies in the Himalaya and surroundings (eg, Bolch et al., 2012; Yao et al., 2012; Kulkarni et al., 2011; Scherler et al., 2011) are using glacier surface and terminus as indicator variables. Those studies have discussed the glacier changes linking to the climate and have depicted the potentiality of these variables to interpret impacts of climate change instead of using mass balance approach which requires much resources. Therefore we used surface and termini to make possible the comparison with a larger number of studies and in the extensive areas, however we compared our findings in term of Surface, Terminus and SLA with the mass balance data of such glaciers from previous studies (Bolch et al., 2011, Nuimura et al., 2012; Gardelle et al., 2013) discussing in the paper as: "This comparison shows that, for this region, the glacier surface shrinkage and the shift of snow-line altitude (i.e., the shift of late summer snow line) can be considered suitable indicators for a broad description of glacial response to the recent climate change." Of course, the mass balance data are better, but the field based measurements are difficult to use in extensive area, and the geodetic approach of mass balance analysis has also some shortcomings (Bolch et al., 2008 & 2011).

I also have same question with Mölg that the unit of area shrinkage rate is not appropriate. Absolute value should be used.
**Response:** The suggestion has been followed answering to Mölg's comment.

Authors mentioned recent increase of debris-covered part. However it might be caused by decrease of debris-free part.
I recommend to include distribution map of glacier (with boundary of debris-covered/-free part) in each year.
**Response:** The debris covered area increase is real and it is occurred reducing the debris free area. In fact we observed that the debris covered area increased by decreasing the debris-free part as already discussed by Bolch et al. (2008, 2011).
To avoid the possibility of misunderstanding, we are adding the following sentence: "……due to decrease of debris-free area" in the revised manuscript.
If we put all the boundary layers of debris-covered and debris-free parts in a single map, it will be really heavy and visually not promising for interpretation. To make clear visualization of change in debris-covered and debris-free parts, we purpose to present a comparison of 1962 and 2011 in a map. We keep the map as a Supplementary material because the paper is already lengthy and other reviewers suggest reducing it.

Specific comments

5390-13: Are The SLA upward velocity numbers (2.2 and 6.1) calculated from Table 2?

**Response:** The SLA upward velocity numbers (2.2 and 6.1) was calculated from the data presented in the "Table S1" in supplementary material. These numbers are described in the Results section 4.3 and is indicated the "Table S1" supplementary material in the first paragraph of "Results" sections. We are modifying the "Table S1" in supplement for better display by following the suggestion from M. Pelto.

5390-18: Please include specific number of upward shifts.
**Response:** The suggestion has been followed.

5390-20: To assert this, you also need to evaluate other region using same method.
**Response:** This assertion was made based on the comparison of our results with a complete and exhaustive review of previous studies in the region. All papers have used satellite images, maps, thus methods are similar.

5395-19: How much is root mean square error of GCPs?
**Response:** The root mean square error of GCPs was 8.2 m. We include this data in the revised manuscript.

5400-17: I recommend to perform detail investigation of each glacier about acceleration. The number of glacier (29) is not so much, hence, evaluation of acceleration of individual glacier would be better than statistical test of all glaciers.
**Response:** We start analyzing glaciers individually, but to discuss the general behavior of glaciers population we use statistic tests providing the relevant levels of significance in relation to the number of samples.

5400-28: It is interesting that why TISmap-63 overestimates _Term. Is there any characteristics about the distribution of overestimated glacier?
**Response:** Probably because TISmap-63 does not well represent the glacier boundary. Bolch et al. (2010) and Bhambri and Bolch (2009) reported that the glaciers are often over cited in topographic maps.

5403-13: Does it really mean increase of debris-covered area? Discussion based on area ratio is confusing. It might be caused by decrease of debris-free area (same with general comment).
**Response:** As we responded in the general comment, we are adding the following sentence to avoid the possibility of misunderstanding: "…..due to decrease of debris-free area" in the revised manuscript.

5404-17: In comparison of selected 10 glaciers among previous (Bolch et al., Nuimura et al., ) and your study, individual comparison of all 10 glaciers would be better than bulk comparison.
**Response:** We started comparing (correlation analysis) individually (it means for each of 10 glaciers) our findings in term of ΔSLA, ΔSurf and ΔTerm with mass balance data of Bolch et al. (2011), Nuimura et al. (2012), and Gardelle et al. (2013), without finding any significant result, but we observed that no significant result is found even comparing

among them the mass balance data of the three authors. So we decided to compare them in a group to represent the overall behavior of the glaciers.

5405-1: Glacier surface shrinkage and mass-balance change would have time lag due to response time. This assertion that "can be considered suitable indicators ..." is difficult from your result.
**Response:** We observed only slightly higher rate of terminus retreat in 2000-2011, compared to 1992-2000 (Fig. 4). As indicated by M. Pelto in his comment, such behavior is expected given the response time because downwasting leads to more volume loss then retreat or area loss. We are inserting this statement in the revised manuscript. Despite this fact, the surface shrinkage and ΔSLA are consistent with the overall mass downwasting rates and thus, we asserted that such change variables can be considered suitable indicators. We also responded in detail about the change variables that we used in the preceding “General comment”.

5412-26: How did you evaluate this relationship size/aspect? And you also make figure about the relation. The relationship size/mean glacier elevation too.
**Response:** We used the glacier size, aspect and elevation data extracted from the satellite images and the GDEM for evaluating the relationship size/aspect and size/mean glacier elevation. The complete dataset is presented in the “Table S1” in Supplementary material.

Please show me 1 or 2 sample image of separation of Lateral/frontal moraine and debris covered glacier.
**Response:** An example of the glacier outline delineation we presented in the figure 2, where lateral and frontal moraines are also visible.

Table 1: You considered KHmap-50s as glacier map in late 1950s. However, the map originated from photographic survey in 1921, terrestrial photogrammetric survey in 1935 and 1939. Why you did not consider the map date as 1920–30s but 1950s?
If there is reasonable reason, please include the explanation in the manuscript.
**Response:** We included additional citation (Salerno et al 2008), Byers (1997) and inserted “field survey & terrestrial photogrammetry in 1955-1963” in the revised final paper. Salerno et al (2008) have discussed in detail about the KHmap-50s (Khumbu Himal map) and the reason to consider the map as of 1950s. Byers (1997) presented that between 1955 and 1963 a field survey including the terrestrial photogrammetry was completed by Schneider in the Khumbu (Mt. Everest) region.

Figure 4: Figure about variation of individual glacier among the studies would be better for evaluating relation between glacier size and variation. Visualization of the figure might be difficult due to much information, hence, plotting figures by grouping glaciers depend on size might be better (ex. Figure 4a: <2.5, Figure 4b: 2.5-10, Figure 4c: >10).
**Response:** Considering that the total number of glaciers considered is small (10 glaciers), we think that it is not appropriate to individualize dimensional classes.

Technical corrections:

5403-18: In Fig. 3a2 mistake of 3d2?
**Response:** Corrected as 3d2**.**

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