Review of "Tracing glacier changes since the 1960s on the south slope of Mt. Everest (central Southern Himalaya) using optical satellite imagery" submitted by Thakuri et al. for publication in The Cryosphere

Summary
Thakuri et al. used remotely-sensed images and topographic maps to measure changes in terminus position, area and snow line altitude (SLA) for glaciers in the Sagarmatha National Park, Mt Everest area. The change in % of debris-covered areas are also measured. They revisit a previous study published in 2008 on the same topic but at that time it was purely based on topographic maps. The time span is also longer, the temporal sampling improved.

General Evaluation
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.

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.

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.the-cryosphere.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.

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.

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.

Specific comments

P5390
The total glacier area analysed in the study (~400 km²) needs to appear in the abstract
L6, L8 (and elsewhere?). Use the same number of significant digits after coma for the values and its uncertainty.
L11. Indicate the time period.
L15. "largest". Give the size class.
L16. South-oriented is a sense not a direction. This statement, as is, is not very clear.

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

P5392
L25. It is recommended to acknowledge the funding agencies when using GDEM with something like "GDEM is a product of METI and NASA".

P5393
L18. "Above 4000 m a.s.l, the precipitation starts…". And below? Do they increase?

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?
L26. Why did the authors use nearest neighbour resampling? It is well-known as a poor data interpolator.

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.

P5398
L25. Unit of the mass balance gradient should be mm w.e. yr^{-1} / m

P5401
L15. "has increased to 0.76%/a". During what period?

P5402
L2. "robust" is probably more appropriate than "sustained" here.
L16. "the distribution of the annual rate of the SLA shift" reads better.
L23. "recent years" is not clear enough. Give the exact time span.

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]
L24. "debris cover rate". Do the authors mean the "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.
L7. Bolch et al., 2012 is not the appropriate reference for this statement. [*Oerlemans*, 2001] or other classical text books.

P5405
L11. "session". "section"?
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?
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.
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?

P5408
L11. "suggested" seems more appropriate than "confirmed" because the acceleration of the specific mass loss in those previous studies is not statically significant.

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.
L20. "the temperature should be increased". Why "should be"?

P5410
L3. add "in Fig. S18 of their paper"

L29. “the increase of nearly 15%”. Make it clear this is the change needed according to the SLA model and not the change observed.

P5411
This whole first part of section 5.4 is more result than the discussion. It will also help to make the discussion shorter.

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.
L15-L25. Lengthening the paper with future research plans is not really useful and make it longer.

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.

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? Maybe something to discuss more?

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.

Figure 5. caption : “impact studies” does not describe what is shown in the graph.

Bolch, T., T. Pieczonka, and D. I. Benn (2011), Multi-decadal mass loss of glaciers in the Everest area (Nepal Himalaya) derived from stereo imagery, *The Cryosphere*, *5*(2), 349–358, doi:10.5194/tc-5-349-2011.

Gardelle, J., E. Berthier, Y. Arnaud, and A. Kääb (2013), 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.

Iturrizaga, L. (2011), Trends in 20th century and recent glacier fluctuations in the Karakoram Mountains, *Z. Für Geomorphol.*, *55*(3), 205–231.

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Rabatel, A., A. Letréguilly, J.-P. Dedieu, and N. Eckert (2013), Changes in glacier equilibrium-line altitude in the western Alps from 1984 to 2010: evaluation by remote sensing and modeling of the morpho-topographic and climate controls, *The Cryosphere*, *7*(5), 1455–1471, doi:10.5194/tc-7-1455-2013.

Racoviteanu, A. E., R. Armstrong, and M. W. Williams (2013), Evaluation of an ice ablation model to estimate the contribution of melting glacier ice to annual discharge in the Nepal Himalaya, *Water Resour. Res.*, *49*(9), 5117–5133, doi:10.1002/wrcr.20370.

Wagnon, P. et al. (2013), Seasonal and annual mass balances of Mera and Pokalde glaciers (Nepal Himalaya) since 2007, *The Cryosphere*, *7*(6), 1769–1786, doi:10.5194/tc-7-1769-2013.