Reply to the comments by the anonymous referee on the manuscript entitled

"**Longest time series of glacier mass changes in the Himalaya based on stereo imagery**"
by T. Bolch, T. Pieczonka and D.I. Benn

(The reviewer's comments are given in *italic* and our response in **bold**)

General comments

I suggest publication of the paper in The Cryosphere after minor revision. My main comment concerns the poorly explained error calculation (see P2597 L15 below).
We carefully revised the error calculation and considered the valuable suggestions.

Regarding the previous comment by M. Pelto about the stable terminus of most glaciers. Bolch et al. will probably discuss themselves this issue. However, M. Pelto and other readers must have in mind that (Bajracharya and Mool, 2009) compared middle resolution satellite (Landsat) imagery from year 2000 to 1960s topographic maps (without discussing their relative planimetric adjustment, the precision of the map, etc…) whereas Bolch et al. have a comprehensive, geometrically homogeneous (because orthorectified with the same set of ground control points) series of high resolution satellite imagery.
We agree. See our reply to the comment of M. Pelto.

SPECIFIC & TECHNICAL COMMENTS
TITLE
The title is not wrong but too vague. You need to indicate that the paper deals with the Everest area (Nepal, Himalaya). A suggestion: "Multi-decadal mass loss of glaciers in the Everest area (Nepal, Himalaya) derived from stereo imagery"
We changed the title as suggested. Thank you for the suggestion.

ABSTRACT

It would be good to have somewhere in the abstract the total ice-covered area
We include the information that we "…calculate a time series of mass changes for ten glaciers covering an area of about 50 km²."

P2594 L1: Strictly speaking if glaciers in the Himalayan are currently loosing mass, their contribution to river runoff does not decline but (of course temporary) increase.
We agree and it is well known that glacier mass loss results first in an increase of the overall run-off. However, we write "declining water resources" and not run-off.

P2594 L11: "an increasing rate since at least 1970". Strange statement given the poor confidence in the 1962-1970 volume change, restricted to a limited area. Did you mean "since at least 2002"? Rephrase.
We agree that this sentence is not clear. We write now: "We reveal that the glaciers are significantly losing mass since at least ~1970, despite thick debris cover." And include later the information about the possible increasing rate in recent years.

INTRODUCTION
P2594 L19. Be more cautious. It is indeed more relevant to compare mass balances than length or area changes in different glaciarized areas of the globe but still, keep it mind (or remind to the readers) that the glacier mass balance is highly dependent on the hypsometry of each glacier and, thus, can differ a lot

even for glaciers experiencing a similar climate change (e.g., Vincent et al., 2004) or (Paul and Haeberli, 2008, Figure 3 and 4).
We agree and write now: Glacier mass balance is the variable which can be most directly linked to climate and that can be compared to other regions. The topographic setting and the glacier hypsometry may alter the climatic signal but can calculated using a DTM (Paul and Haeberli, 2008).

P2595 L8. Why "mean" for the 2002 ASTER DEM time stamp?
We write now: "…and an ASTER DTM generated based on 2001, 2002 and 2003 data" for clarification.

P2595 L9. "mass balance estimates for a larger sample"
Corrected

P2595 L11. "the proglacial lake which formed in the 1960s and rapidly grown since"
Improved as suggested

P2595 L16. Indicate the total area of those 9 studied glaciers
We include the information about the debris covered area. The total area of these glaciers are indicated in table 3.

P2595 L20. Meaning of "Cwn"?
The area of Khumbu Glacier above the ice fall is called "Western Cwm". Cwm is an old British word meaning 'cirque'. We include now this term also in Figure 1.

DATA & METHODOLOGY
P2596 L8. The justification for not using SRTM is not very convincing given that (L28 of the same page) this is the altimetric reference you used to verify that the 2007 Cartosat-1 DTM is the most precise DTM and can thus be chosen as a reference... Large data gaps in SRTM (are those gaps affecting your glaciers of interest?) is more credible reason to exclude it.
We agree and write now: "We did not consider the DTM data from the Shuttle Radar Topography Mission (SRTM) due to large data gaps especially at the clean ice area of Khumbu Glacier and the coarser spatial resolution (~90m) in comparison the ASTER DTM (30m). In addition, using only stereo optical data results in a methodologically consistent data set. The SRTM DTM data in contrast are based on radar beams whose penetration into snow can cause higher uncertainties in the snow covered accumulation areas."

P2596 L10. Regarding the use of Corona data you could also cite (here or, probably better, later at the beginning of the discussion) two recent papers that used those spy imagery in the glaciological context (although the data were not used yet to measure ice elevation changes as you did) (Schmidt and Nüsser, 2009; Surazkov and Aizen, 2010)
We agree and refer especially to Narama et al. (2010), Global and Planetary Change, who used Corona data for a larger area and discussed Surazakov and Aizen (2010) especially regarding the methodology as also suggested by the reviewer later.

P2596 L10. Could you overlay on one of the figures the location of the 14 GPS points? Are they well distributed (spatially and vertically)? Do you have an estimate of their horizontal and vertical accuracy?
We do not want to include too much information in the figures. However, we agree that this information should be provided. We included now the following sentence: "The GPS points were mostly measured along the main trekking routes within a height range between 3900 m and 5600 m.

Their horizontal accuracy is about 7.9 m and their vertical accuracy in comparison to topographic map height points is about 20.6 m."

P2596 L16. Sensor models (one model for each sensor)
Corrected

P2596 L19. I think "measure" would be best than "address"
We agree. Corrected as suggested.

P2596 L22. How many points to estimate those trend surfaces? Why did not you use all the ice free terrain pixels and fit a surface to those?
Indeed, the trend surface was finally calculated based on all the non-glacierised pixels. We corrected the text accordingly.

P2596 L27. RMSE is a combination of mean and standard deviation. As you provide the mean, I think it would be best to provide the standard deviation about the mean and not the RMSE. It is then easier for the reader to "visualize" the distribution of the errors (see also my similar comment about Table 1).
We agree and provide now the value of the standard deviation instead.

P2597 L7. How did you select the 200 points? Randomly? Are they representative of the topography (slope, aspect, altitude range, etc.). Again, why did not you evaluate the DTMs on the whole ice free terrain?
We selected the more than 200 points using the height points of the topographic map. Their distribution is almost random in accordance with the unsystematic distribution of significant peaks and other geomorphological forms marked as height points. In addition, these points represent different slope angles and aspects well. However, we agree that it would be better to evaluate the relative accuracy of the DTMs based on the whole ice free terrain. We use the ~200 points for the master DTM and evaluate the others based on the whole ice free terrain of the master DTM.

P2597 L15. Provide the formula for the standard error (SE). Your estimate of the error is surprising because the mean elevation difference (MED) should already be included in the SE (or not?); More generally, it is not very clear how you get your error estimate and you should give all the information so that the reader can reproduce your calculation. Currently this is not the case. Are you certain you can readily transpose the 1 out of 20 pixels (to estimate the effective sample size) used by Koblet et al. for aerial photographs? Furthermore, if I read Koblet et al., 2010: "Under the assumption that the auto-correlation of pixels with 100m (or 20 pixels) distance is negligible". So their 20 pixels decorrelation length is in fact an unjustified "first guess". If you use it readily in your paper, it is going to propagate in the literature as a reference/unique value although this length should be estimated in each new case study... Your error bars are reasonable and this is a good thing and I do not challenge them, but I simply suggest that more details are given to the readers.

We clarified now our approach to estimate the error and include the definition of the Standard Error ($SE = \frac{\mathrm{STDVnoglac}}{\sqrt{\mathrm{n}}}$ **). We again include the mean elevation difference (MED) in the estimation of the uncertainty as the SE is most cases lower than the MED. Hence, the uncertainty would probably be underestimated due to the averaging of the SE and we wanted to provide a reasonable estimate. All the values which are needed to reproduce the uncertainty calculation and the characteristics of the uncertainty are provided in table 2. We refer now to this table also in this paragraph.**

We agree that the length of decorrelation should not be taken from another reference without reflection. Hence, we write now: "We choose a decorrelation length of 600 m for the ASTER DTMs with an effective spatial resolution of 30m and a length of 400 m for all other higher resolution DTMs to minimize the effect of auto-correlation. These numbers are slightly more conservative than the average value of 500 m utilized by Berthier at el. (2010) for DTMs with coarser resolution (mostly 40m). Koblet et al. (2010) suggested a decorrelation length of 100 m for DTMs based on aerial images of a spatial resolution of 5m. "

P2598 L21. "detailed" is too vague. Do you mean "multi-temporal"?
We changed the subheading to "Detailed multi-temporal investigations…"

VOLUME CHANGE & MASS LOSSES
P2598 L20. It would be good to show a high resolution (2007?) ortho-image of Khumbu glacier in an additional figure to really locate section A to D. It will give the reader a sense of how the glacier looks like and visually illustrate its different sections.
We agree and include now a figure as suggested.

P2599 L4. I agree that you give good indications of accelerated loss but from the purely statistical point of view the errors bars still overlap.
We agree and write now "…shows almost a statistically increase…"

DISCUSSION
P2599 L10. Do you think a better set of GCPs would permit to limit the distortions in the Corona DTMs? This point needs to be discussed and will help to guide others that would like to use Corona for measuring elevation changes in the future. Compare your results to (Surazakov and Aizen, 2010)
We improved the discussion and write now: "The accuracy and even distribution of the ground control points used for the rectification of the imagery are crucial for the resultant accuracy of the DTM. Hence, it could be expected that an even better accuracy than ours could be achieved if more precise GCPs are available. Other reconnaissance images such as Hexagon KH-9 from the 1970s and early 1980s are also suitable for this task if coverage with no clouds and little snow cover are available (Surazakov and Aizen 2010). The horizontal accuracy for mountainous terrain is with ±~20 m within the same range as our results using KH-4B data despite lower resolution of the KH-9 data. Reseau marks on the KH-9 images facilitate minimizing the distortion."

P2599 L15. I do not understand your "supraglacial lake story" and the associated timing. Given the observed thinning between 1970-1984, I would expect the lake to drain during this period (resulting in surface lowering) and then the depression to fill again either by water or ice inflow, leading to the 1984-2002 thickening. Clarify or correct.
We agree that this issue needs clarification, and have altered the text to:
"The quality of the DTMs is supported by local detail, such as the area on Khumbu Glacier arrowed on Fig. 4. A large thinning occurred in this area between 1970 and 1984, which can be attributed to the growth of a lake basin visible on the 1984 aerial photograph. Melting and calving around the margins of lakes is well known to produce locally high ablation rates (Sakai et al., 2000, Benn et al., 2001). Thickening occurred in this region between 1884 and 2002, attributable to drainage of the lake and ice inflow from upglacier." See also figure below:

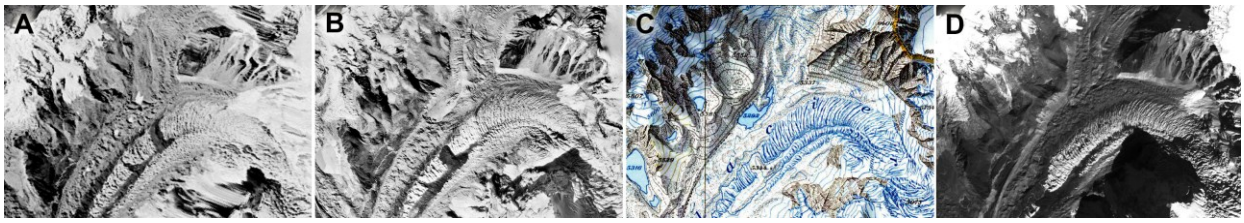

Figure: Upper part of Khumbu Glacier; A: 1970 Corona image, B: 1984 aerial image, C: 1988 topographic map based on the 1984 aerial images, D: 2007 Cartosat-1 image.

P2599 L19. Remind us the value measured by Bolch et al., 2008.
We included now these values

P2600 L10. Indicate that the possible thinning of the accumulation area is observed for the period 1970-2007.
Information included

P2600 L18. "Low velocity". What matters in term of elevation changes is not the absolute ice flux but its change with time. If you want to explain the strong lowering of Imja glacier tongue from the ice dynamic point of view you need to demonstrate first that ice surface velocities (and thus the ice fluxes toward the tongue) have decreased in the last 2-3 decades.
We agree. A decreasing velocity is likely as e.g. Khumbu Glacier shows is the case. But we do not have any data to prove this estimation. Hence, we omitted the statement regarding the low velocity.

P2600 L26. Why "only partly". I think "as it is not statistically significant yet" would be more reasonable given your error bars. The 2002-2007 time period is short and thus it is very challenging and courageous to measure an elevation change of less than 5-m, in particular using the ASTER DTM (see also my comment about Table 3).
We agree and write now "The recent trend of more negative mass balances since 2002, however, needs further investigation, as it is not statistically significant."

CONCLUSION
P2601 L18. "careful relative adjustments of the DTMs" seems a better formulation
Improved as suggested

TABLE
Table 1. I would replace the RMSEz by the standard deviation. For example the RMSEz for the 1962 DTM does not mean a lot because it mainly reflects the (really!) large mean elevation difference. Are those statistics performed on the whole ice free terrain or on the 200 selected points? Could you indicate this info in the caption?
We present the values of the standard deviation and include the information that these statistics are based on the whole ice free terrain.

Table 3. Nuptse glacier seems to behave very differently than other glaciers with thickening for the most recent period (2002-2007). Do you trust this thickening? If yes, any explanation for it?
This was only an error with the algebraic sign. We had the correct minus sign for the average elevation difference but forgot it for the specific mass balance. We corrected it now.

More generally, the large scatter of the mass balance for the 2002-2007 period is surprising and worrying (whereas the little scatter for 1970-2007 really gives some confidences in your estimates). The 2002-2007 error bars may be underestimated. I thus suggest revising the error estimate for this period. You will be more "comfortable".

We revised the error estimate and took a larger decorrelation value of 600 m for the ASTER DTM (see the respective section). In addition, we discussed now the issue with the large scatter in the 2002-2007.

FIGURE

The current color scale makes it difficult to identify region of no (or little) elevation changes. The [-10:10] elevation changes could be shown in white instead of grey to better visualize them.

We think, the area is visible. The area appears light grey as we show a shaded relief in the background.

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