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The Cryosphere Discussions

Interactive comment on "Mass gain of glaciers in Lahaul and Spiti region (North India) during the nineties revealed by in-situ and satellite geodetic measurements" *by* C. Vincent et al.

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CS = Chhota Shigri

Response to Referee #2 (Tobias Bolch)

1. General: The topic of the paper is very relevant as it is an important contribution to the ongoing discussion on glacier changes in the Himalaya where mass balance measurements are rare and, if existing, short-term. This is especially true as information about the possible mass change for the 1990s is presented, a period for which almost no mass balance data is available. In addition, geodetically derived mass changes for 1999-2010 and a reassessment of the 1999 - 2004 data published earlier (Berthier

et al. 2007) are presented. This manuscript was already submitted elsewhere and has clearly improved but unfortunately some of my earlier major concerns were not addressed satisfactorily

We thank T. Bolch for stressing that no mass balance data are available in the 1990s and for his constructive review that helped us to improve our MS. In the revised manuscript, we hope that the comments of reviewers have been addressed satisfactorily. We brought new data to assess the representativeness of CS glacier in the region and we weakened the conclusions about "the mass gain "during the nineties.

2. The authors back up the "mass gain" based on geodetic mass budget estimates for one glacier only measured for one time period (1988-2010) which has an overall negative budget of -0.17 _ 0.08 m w.e. a-1. The "mass gain" in the 1990s is estimated based on a comparison with geodetic measurements for 1999-2010, and is not significant given the high uncertainty (+0.09 _ 0.23 m w.e. a-1). Hence, the authors should be very careful with such a prominent statement. By the way: I appreciate the thorough estimation and discussion of the uncertainty.

We agree and made the appropriate changes in the revised MS (see response #1 to Rev#1).

3. One important shortcoming of the measurements at Chhota Shigri Glacier is that the authors do not consider glacier flow which can have an important impact on the point measurements. This needs to be considered if possible or at least discussed.

We disagree. We measured the thickness variations to obtain the volumetric mass balance. Consequently, glacier flow does not need to be considered here, because the glacier is studied in its whole, like in any remote sensing study dealing with volume change obtained from elevation differences (i.e. Cuffey and Paterson, 2010). The only glacier flow impact in our study comes from the area shrinkage, over the 22 years. But as already mentioned in our manuscript, the area shrinkage is small and do not have any significant impact on the results. This was already clearly mentioned in the first version of the MS.

Cuffey, K. M. and Paterson, W. S. B.: The physics of glaciers, Fourth ed., Academic Press Inc, Amsterdam, 2010

4. The authors claim that the results for Chhota Shigri Glacier are representative for a larger region and back up their statement of the similar mass loss of the glaciers compared to the whole region for the 1999-2010 period. This is a hint but may also be random. Further evidence for the representativeness needs to be presented, e.g. dhcurves, evidence from other glaciers in the same period, length and area changes. In addition, the glacier has only little debris cover while several others have large amounts of debris on their tongues.

This question has been thoroughly studied. To our knowledge and given the scarcity of available mass balance series, there is no mean in the Himalaya to test the regional representativeness of a glacier for time periods where no measurements are available. Recognizing that we do not have further evidences that other glaciers in the region may have had a stable mass budget, we downplayed on our original statement that the whole glacier region experienced mass gain during the 90s. In the revised paper, the title/abstract/discussion focuses on the balanced or slightly positive mass budget of CS Glacier only. The existence of regional mass stability in the 90s is proposed as an hypothesis that needs further work. To improve the discussion of the representativeness, we have made and presented some further analysis using dataset from the Alps (data courtesy of M. Huss). The corresponding section of the discussion reads as follow: "Our remote sensing analysis suggests similar mass balances for Chhota Shigri Glacier and for 2110 km^2 of surrounding glaciers in the Lahaul and Spiti region during 1999-2011. A crucial question is to determine whether the MB of Chhota Shigri Glacier remains similar to the MB of the whole region for other periods. If it is the case, it would mean that the whole region had a stable or slightly positive mass budget during the 1990s. This question relative to the representativeness of a single glacier has not been examined yet in the Himalaya but has been thoroughly studied in other

regions with numerous mass balance measurements. This hypothesis is supported by a growing body of literature suggesting similar temporal variability in glacier MB within a given mountain range (e.g. Huss et al., 2010; Vincent et al., 2004; Rasmussen, 2004; Soruco et al., 2009). Rasmussen (2004) found a strong correlation between the mass balance of 12 Scandinavian glaciers and concluded that measurements on one well-chosen glacier (Hardangerjøkulen) provides a good estimate of the average mass balance of other glaciers. Using fifty years of annual mass balance data for several glaciers in the Alps, Vincent et al. (2004) showed that mass balance fluctuations are very similar. The European Alps has a similar glaciarized area as the Lahaul and Spiti region ($\sim$2100 km^2) and is unique by its high density of mass balance measurements. Huss (2012) took advantage of this high field data concentration (i) to extrapolate observed mass balances to the whole Alps and (ii) to discuss the representativeness of existing long-term monitoring programs. He concluded that two glaciers, Vernagtgletscher and Sonnblickgletscher (both in Austria) appear to be suitable index glaciers for the Alpine mass balance, with a >50 years mean mass balance only 0.05 m w.e. less negative than the region-wide mass balance. Using the data from Huss (2012), we computed the differences between the decadal mass balance for Vernagtgletscher and Sonnblickgletscher and the whole Alpine mass balance. The standard deviation of the difference is $\pm$0.20 m w.e. for Vernagtgletscher (N=5 decades) and $\pm$0.09 m w.e. for Sonnblickgletscher (N=6 decades). This simple analysis in a well-surveyed mountain range provides a first-order indication of the error that one would commit by assuming a single glacier to be representative of a whole region for a specific decade where no regional measurements are available. In conclusion, given that the MB of Chhota Shigri glacier is only 0.05 m w.e. yr-1 less negative than the regional MB during 1999-2011 (Table 4), we propose that the mass balance for Spiti and Lahaul did not deviate by more than $\pm$0.25 m w.e. (sum of 0.20 m w.e. and 0.05 m w.e.) from the one of Chhota Shigri Glacier and thus, was also close to 0 during the 1990s. "

Even if the regional stability of glaciers in the Lahaul and Spiti during the 90s cannot be firmly demonstrated, we believe that the inferred geodetic mass balances of CS Glacier between 1988 and 1999 should be used to represent, together with Dokriani Glacier, the western Himalaya in regional and global compilation of glacier mass balances because it fills a gap in mass balance measurements in this part of the Himalaya during the 90s. It is surely a much better approach than using MB records only available in the eastern and central Himalaya

5. The paper lacks a more in depth discussion, e.g. no real (e.g. climatic) hypothesis about the causes of the mass gain is presented. The authors should also compare their results with other available mass balance data from the 1990s in the Himalaya and possibly also with area and length changes (considering the response time).

To provide a more in-depth discussion, the section 3.4 Comparison with other western Himalayan glaciers where our results have been compared to other available mass balance data from western Himalaya, has been moved to the discussion section, which has been rewritten. Still, we chose not to compare to other parts of Himalaya (central and eastern) or Karakoram, because in such a large mountain range, climatic conditions are very different from one side to the other (i.e. Bookhagen and Burbank, Geophys. Res. Lett., 33(8), L08405 (doi: 10.1029/2006GL026037), 2006) making glacier behaviors very different from one side to the other (e.g., Kääb and others, 2012). Therefore, we do not think it is meaningful to compare our results with glaciers from Karakoram or the SE part of the Himalayas (Indian monsoon influence). We have now removed the comparaison to the Karakoram anomaly. Concerning the climatic interpretation, the discussion is lacking climatic analysis to explain the near-zero balance in the 1990s, because as mentioned in the original MS, it is beyond the scope of the present paper. Actually, providing a detailed climatic interpretation needs to present and discuss the datasets (which are not easily accessible in India), and to do some modeling to relate mass balance to climatic variables. A preliminary analysis in our research group (PhD thesis of Julie Gardelle, defended 19 December 2012) has highlighted some large differences between the different large scale gridded climate (T,P) dataset (NCEP, GPCP, ERA, CRU, ...) so it is probably not a good idea to take one

of these coarse dataset and draw some firm glaciological conclusions without in-depth analysis. Consequently, we believe that this work is an entire study which is beyond the scope of this paper. It will require a further publication.

6- Hence, the overall conclusion is weak, is not fully supported by the data and needs to be extended. One of the real interesting results of the study is the reassessment and new data of the mass change since 1999 for a larger region, but this information is a bit hidden in the supplement. I suggest that the authors include most parts of the supplementary information in the main text as there is no length limitation for The Cryosphere.

As explained elsewhere in the present letter, we have weakened our statement regarding the representativeness of CS Glacier and thus of regional mass equilibrium in the 90s and focus more on CS Glacier, including in the conclusion that has been rewritten. Following the suggestion of the referee and the editor, the supplementary information is now included entirely in the main text.

7- Specific comments The title needs to be changed as it does not reflect the content of the study well. The mass gain for the glaciers in Lahaul and Spiti is only a hypothesis but not confirmed. In addition, the performed measurements confirm an overall mass loss between 1988 and 2010. Maybe something like “Investigations on Chhota Shigri Glacier 1988 to 2010 (: : :) indicate the possibility of slightly positive mass budgets (: : :)”

Agree. The title has been changed; see response #1 to ref 1.

8- Abstract The abstract needs to be improved after the revision of the paper. I would suggest to include also the rate per year which makes comparisons to other studies easier. In addition, the absolute ice loss should be presented.

Agree. Rates are included. However, we did not provide the absolute mass loss. In fact, we do not see a real added value from this unit. Mass loss from 2000 km^2 of glaciers will not contribute to sea level rise (if this is what the reviewer has in mind). Units of mass balance (m w.e. yr-1) are the most useful so that our new mass balances can be compared to other studies or regional/global compilation.

9- L.11. Results of the study provide important additional information about one glacier in particular in the Himalaya but the statement "This contrasts to the most recent compilation of MB data in the Himalayan range that indicates ice wastage since 1975, accelerating after 1990." is exaggerated. Bolch et al. (2012) write in their recent review: "These measurements suggest that the mass budget over large parts of the Himalaya has been negative over the past five decades, that the rate of loss increased after roughly 1995 but also that the spatiotemporal variability is high." There is e.g. no evidence provided in the study that there was a real mass gain until 2000 for Chhota Shigri Glacier and all of Lahaul and Spiti. It could e.g. also well be that there was slight mass gain from 1988 until roughly 1995 and a mass loss thereafter. In addition, this study on Chhota Shigri and Lahaul/Spiti confirms a mass loss on average since 1988 with an acceleration later. Hence, from my point of view the results of the study are generally in line with Bolch et al. (2012) but provide important further details which help to refine our knowledge about the glaciers in the Himalaya. Please revise this statement. You may for example write "A positive mass balance in the 1990s would contrast the most recent compilation of MB data in the Himalayan range that indicates ice wastage since 1975. However, we confirm an acceleration towards more negative balance since perhaps the late 1990s."

We agree that the statement was exaggerated, and we used a slightly modified version of suggested sentence, with the verb conjugated in the present instead of in the conditional and positive mass balance changed to "positive or near-zero mass balance". Indeed, the near-zero or slightly positive MB of CS Glacier between 1988 and 1999 is a solid finding based on actual measurements, not just an assumption.

10 Introduction P. 3735, L. 24: ICESat data is available until 2009. However the cited study by Kääb et al. (2012) uses only data until 2008. Please revise.

Done

11- L. 26ff: Please show the boundary of "Western Himalaya" in figure 1. Usually the ridge north of the Indus River is not included in the Karakoram but in the Western Himalaya. Also the eastern boundary can be a political boundary but a topographical one as suggested by Bolch et al. (2012) based on Shroder (2011) and discussions with ICIMOD. Bolch et al. (2012) present a glacier coverage of _8950 km2 for Western Himalaya based on recent inventory data (Fig. S1, TableS2)

Figure 1 has been changed (see below).

12 Site description, data and methodology P. 36, L. 20: Provide more details about the nature of the debris cover (thickness, form (medial moraines?)).

Done, and the reader is invited to read Dobhal at al. Current Sci., 68(9), 936–944, 1995 for additional information

13 L. 21: Please provide some more details about the amount of seasonal precipitation in this region. Is the glacier more of summer-accumulation or winter-accumulation type? This information would then also help later in the discussion about the possible causes of a slight mass gain/less negative budgets in the 1990s.

Done. We agree that it is relevant to present the seasonality of the precipitations. However, we do not think that it helps to discuss the origin of the glacier loss/gain. A striking example is provided by the study of Kääb et al. (2012) in which, Karakoram excluded, mass loss was reported throughout the mountain range from Bhutan to Hindu Kush without any obvious relationship with precipitation seasonality. Another example: Karakoram and Hindu Kush glaciers are winter-accumulating but have distinctly different behavior in term of mass balance (at least for the last decade). Kääb, A., Berthier, E., Nuth, C., Gardelle, J. and Arnaud, Y.: Contrasting patterns of early 21st century glacier mass change in the Himalaya, Nature, 488(7412), 495-498, 10.1038/nature11324, 2012.

14- P. 37, L. 1: Who is “we”? All authors? Maybe it is better to name institutions.

Institutions are named

15- P. 37, 1st paragr.: This information is not needed in this detail for this paper. The authors may consider shortening and mainly refer to the references for further reading.

Agree. We have removed three sentences in section 2.2

16- L 20ff.: At the lower part of the tongue the measurements are concentrated along the centerline. However, it was shown that using only the centerline is not representative for the entire width of the glaciers (Berthier et al. 2010). This should be at least discussed shortly.

Thanks for this comment that offers us the opportunity to clarify what was referred as a “centerline sampling bias”. The two figures below aim at clarifying how this centerline bias occurred in the laser altimetry extrapolation. Rather than a centerline bias it should be referred as “a bias when the centerline of the main tributary only of a glacier complex is surveyed and the area change is not accounted for”. Indeed, the first figure below for Columbia Glacier show that different branches of the glacier do not experience the same elevation changes. In this particular case, Univ of Alaska Fairbanks laser altimetry only sampled branch 1 (the main tributary) of the glacier for which, at a given altitude, the thinning was twice larger than other branches during 1957-2007. This is a first contribution to the “centreline” bias because UAF had to assume (by necessity) that all tributaries experienced the same amount of thinning. The second part of the bias is due to the fact that, at the time of their study, Arendt et al. (Science-2002) only had a single inventory, corresponding to the USGS 1957 map. If a transverse profile is considered (see second figure below), Arendt et al. had to assume (by necessity again) that the loss occurred for a rectangular section whose dimensions are the width of the glacier in 1957 multiplied by the 1957-2007 elevation change at the centreline (in this case two measurement points close to the centreline). Thus, their extrapolation did not take into account the reduction of the thinning at the newly-deglaciated glacier

margins (shown in yellow) which is implicitly measured by the DEM differencing technique. If Arendt et al. had a second inventory synchronous to the time of the laser flight, they could have replaced the rectangular section by a trapezoïdal section (leading to reduced volume loss). Note that this effect is clear for a rapidly retreating and thinning glacier such as Columbia Glacier but it is not obvious for other ice bodies (see Table S4 from Berthier et al., Nat Geo, 2010).

In the case of CS, the close-up view on the map of elevation difference between 1999 and 2011 does not reveal any such pattern, in part because the glacier area change was very small during this period (which also hold for the 1988-1999 time period). We also investigated whether the tributaries experienced different rates of elevation changes (see response to comment#3 by reviewer#1)

Supplementary Figure S3 from Berthier et al., Nat Geo, 2010 , see below

Unpublished figure (see below) showing a tranverse profile of surface elevation in 1957 and 2007 for Columbia Glacier (Alaska) and illustrating the reduction of thinning at the newly deglaciated glacier margins. This reduction is implicitly measured by differential DEMs but not by centerline altimetry profiles.

17- P. 38, L. 7: Using 900 kg m-3 is fine and well established. However, the correct density of ice is 917 kg m-3.

The density of pure glacier ice is usually taken as 917 kgm−3 , a value that strictly applies only at temperatures near 0 âŮęC and at the low conïňĄning pressures characteristic of small mountain glaciers. However, the glacier ice is usually not free of bubbles and the glacier ice ranges between 830 and 923 kgm−3. (Cuffey and Paterson, 2010, p.12) In addition, given the other uncertainties, taking density of ice to 917 kg m-3 to improve the calculations accuracy would be illusive. Consequently, we maintain 900 kg m-3 and mention “glacier ice” in the manuscript.

Cuffey, K. M. and Paterson, W. S. B.: The physics of glaciers, Fourth ed., Academic

Press Inc, Amsterdam, 2010.

18- L. 12f: Please write here clearly that you adjusted the DEMs following the method suggested by Gardelle et al. (2012a).

Slightly change to make it clear that "the methods we used here" are from Gardelle et al. (2012a and b).

19- L. 23: I appreciate that the authors consider a correction. This correction might be even a bit too high. More information in the beginning about the precipitation and accumulation regime would be also important for this estimation.

We do not really understand why the reviewer indicates that the correction may be a bit too high. Unfortunately, we do not have any information about the winter mass balance of CS Glacier. In the manuscript, we added the information we have from the nearest meteorological station : Âń It lies in a region alternatively influenced by the Indian Monsoon in summer and the mid-latitude westerlies in winter (Bookhagen and Burbank, 2010) with precipitations distributed equally between summer (May-October) and winter (November-April) months as recorded since 1969 at Bhuntar airport meteorological station (1050 m a.s.l.), 31 km South-West of Chhota Shigri Glacier. At 5550 m a.s.l., between 2002 and 2010, annual accumulation varied between 1.0 and 2.2 m water equivalent (w.e.)."

20- 3.1 Changes in Chhota Shigri Glacier thickness. . . General: It would be very helpful if figures 2 and 3 could be improved. The lower dots on the glacier are hardly visible in Fig. 2 and information about the debris cover should be included here. My suggestion is to show a suitable remote sensing image in the background and show only each second contour line in a grey colour. Fig. 3: Please include the information about which measurements are affected by debriscover. I have also concerns about the polynomial interpolation. The curve is strongly influenced by the measured point at _4090 m asl. (which I by the way cannot identify in fig. 2.). Please include the used values for the 50m intervals mentioned on page 38, L. 4. Provide also evidence that the

surface elevation change of _+20m in the ablation region is real true. The significant thickening does affect the overall volume change. How did you deal with this thickening in terms of glacier mass balance?

Agree. Figures 2 and 3 have been revised according to reviewer'suggestions. See below. Thickness changes interpolations have been explained in details in the manuscript.

21 - 3.3 Mass gain. . . Please revise heading and the statement L. 23. Given the high uncertainty it is not clear whether there was a slight mass gain or mass loss.

Agree. The heading and statement have been revised.

22- Please include all numbers here to understand your calculation. I suggest to include table A2 here. The authors need also to be a bit more careful with the date 1999.

The Supplementary material has been now included in the main text. A new Table 6 has been included to explain the calculations (how geodetic mass balance and glaciological mass balance are combined)

23- Currently one has the impression that the mass balance was positive until 1999 and negative thereafter. But this year might have been also earlier or later. It is just that there are measurements (or a DEM) available.

Agree. Now specified everywhere in the text

24 - Is there any other evidence which can also support the positive value in the 1990s? e.g. repeat photographs or remote sensing imagery? Is there evidence of a short glacier advance caused by this positive MB period or could it be expected in the future due to the delayed response?

Unfortunately, data are scarce. There is not evidence that the glacier advanced over this period. However, as mentioned in the manuscript, ice fluxes calculated in 2003/2004 revealed values much larger than the balance fluxes calculated from 2002/2010 averaged surface mass balance (Azam et al., 2012). These results suggest that this glacier experienced a period with near-zero mass balance in the nineties. Additionally, the ice flow velocities measured in 1987/1988 were similar to those measured in 2003/2004 and suggested also that the dynamic behavior of this glacier may not have changed much between 1988 and 1999. Consequently, our results are in agreement with these (rare) data. This comparison between results from Azam et al. (2012) and our new data is now better explained in the revised manuscript.

25 - 3.2. Comparisons with other western Himalayan glaciers This section as it is currently written can be almost entirely omitted as this information is presented in table 1 and figure 5. I suggest to move this section to the discussion. The focus should be on the comparison of the obtained data with the existing one including MB values. Any further evidence from other measurements (length, area taking the possible response time into account) besides the important remarks based on Azam et al. (2012) which might support a mass gain in the 1990s for the larger region should also be discussed.

Agree. This section has been moved to the discussion section, making the discussion more consistent.

26 - 4. Discussion General: Please include in the discussion also some climatic evidence which may support the hypothesis of a slight mass gain or little mass loss during the 1990s.

This discussion relative to climatic causes would require a thorough analysis which is far beyond the scope of this paper. This analysis will be done in a further paper. See our response to general comments

27 - P. 42, L. 21. The authors should not directly compare length changes with mass changes for the same period without considering the response time.

This comparison has been done in Azam et al. (2012) study. Here we refer to the

conclusions of this previous study. Obviously, the response time is not known and is a complicated issue, but it certainly does not exceed 2 decades (1988-2011). We do not think it is useful to discuss more about this comparison given it has been discussed previously in Azam et al. (2012) study.

28 - L. 22. “slightly positive”: : : see my comments above.

Agree. Change has been done

29 - P. 43, L. 15f. Please revise this sentence. The statement in Bolch et al. (2012) is different. See my comment on the abstract. However, I agree with the authors that the information about the 1990s is very limited and more information is needed

Done

30 - L. 25: The geodetic estimate for Hamtah is very valuable because the undocumented glaciological measurements published for this glacier are quite negative and might be an outlier as mentioned. Please show therefore also a zoom of this glacier (see my comment on Figure 4).

A detail view of the elevation changes on Chhota Shigri and Hamtah glaciers is shown in Figure 5.See below

31 - P. 44, L. 6ff. I fully agree with these points. But this heterogeneity is also true for the Lahaul and Spiti and also provides a rationale for why the authors should be careful with generalizing the results of one glacier to the larger region.

That is the reason why we conducted a remote sensing study to compare CS Glacier to the entire Lahaul and Spiti Region. In a previous version of the paper, this remote sensing regional assessment was missing, and we agree that the question of the representativeness of CS glacier was unsolved. The Lahaul and Spiti glaciers studied here cover a region that has roughly the size of the European Alps (~2000 km^2 of glaciers). This is a scale at which temporal variations of glacier mass balances have been shown to be correlated (Vincent et al., 2004; Huss et al., 2010, Huss, 2012).

Cogley and Adams (1999) analyzed the correlation between mass balance series as a scale length of 600 km. At a scale of 200 km (typical of our study area), the correlation is higher than 0.65 (see their Figure 6). We have elaborated on this to discuss the representativeness of CS Glacier (see our detailed response to the general comment "The authors claim that the results for CS Glacier are representative for a larger region…"). Also, the regional mass stability is now presented as an hypothesis.

32 - L. 13. It is true that thick debris insulates the glacier ice. The authors need to consider that several debris-covered glaciers are significantly losing mass despite thick debris cover (Bolch et al. 2011, Nuimura at al. 2012, Kääb et al. 2012). This can be explained by enhanced melting at ice cliffs and supra-glacial ponds (Sakai et al. 2000) while the debris cover favours the developments of these lakes.

Thanks for the information. We have now added a reference to Bolch et al. (2011) and Nuimura et al. (2012) in the list of studies showing high thinning rate on debris.

33 - 5. Conclusions Please revise the conclusions according to the revised manuscript. P. 45, L. 9ff: I don't see a reason why mass balance results from the 1990s should support findings from a later period. Please revise.

Done. See also the response #10 to reviewer#1

34 - Table 1: Please refer to the original reference and not to Dyurgerov and Meier (2005) to the degree possible. Dyurgerov and Meier (2005) also collected the data from elsewhere but unfortunately no reference is given there. You will find some information about the original references in Bolch et al. (2012, Supplement Table S6).

Table 1 has been changed.

35 - Figure 1: Please include the boundary of "Western Himalaya" and the coverage of the geodeticestimate (Fig. 4) and a scale bar. Please also include the information that the political boundaries are tentative only (or similar) as these are not fixed and such a map could unfortunately cause problems. What is the source for the glacier outlines?

Figure 1 has been changed. The boundary of western Himalaya has been added, following Bolch et al. (2012). Caption is now mentioning that political boundaries are only tentative. (see below)

36 - Figure 2 and 3: See my suggestions above.

It has been done.See below

37 - Figure 4: I would be pleased if the authors could show the DEM differencing for the entire region and not only for the glaciers (Maybe in the supplement and enlarged a bit in case glacier changes would not be visible anymore in addition to the figure in the main text). Please also indicate the area where there are data gaps. In this way the reader can better justify the quality. Please show a zoom of Chhota Shigri and Hamtah Glacier, the glaciers which are directly mentioned in the text. Show also the DEM difference for the non-glaciated area and not only for the glaciers. What are the sources for the outlines?

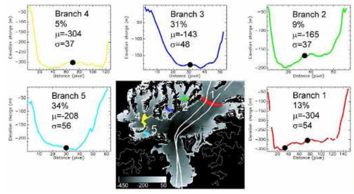
Glacier outlines are from the PhD thesis of Julie Gardelle. More details are now given on the Landsat image and the methods that were used to derive those outlines. See below

38 - Supplement Please include the units in the table (They are missing for MB). As stated above this information is of high interest and I would include the most important information (or even all) in the main text. E.g. the problems with calculating the geodetic mass balance for small glaciers and Table A2 and Figure A1 should be in the main text from my point of view.

This section is now included in the main text. The units for MB were given in the table captions, but they are now included in the tables themselves.

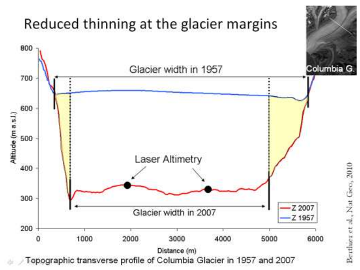
Interactive comment on The Cryosphere Discuss., 6, 3733, 2012.

Supplementary Figure S3: Elevation changes during 1957-2007 for five different branches of Columbia Glacier. The central panel is the elevation change map with the [671 m, 701 m] altitude band (from the USGS DEM) coloured for five branches (labelled from 1 to 5); the thick white lines locate the laser profiles[2]. The five other panels show the transverse profiles of elevation changes along the 686 m contour. On each panel, a black dot shows the elevation change that would have been sampled by an airborne laser following the glacier branch centreline (except for branch 1 where we plot the actual location of the two laser profiles used in Arendt *et al.*[2]). For each branch, the relative difference (in percent) between the centreline and the mean transverse elevation change is given. We also indicate on each panel the mean (μ) and standard deviation (σ) of the elevation differences (in meters) for the [671 m, 701 m] altitude band.



Supplementary Figure S3 from Berthier et al., Nat Geo, 2010

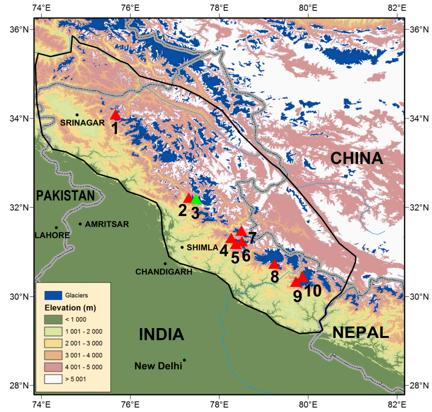
Fig. 1.



Topographic transverse profile of Columbia Glacier in 1957 and 2007

Unpublished figure showing a tranverse profile of surface elevation in 1957 and 2007 for Columbia Glacier (Alaska) and illustrating the reduction of thinning at the newly deglaciated glacier margins. This reduction is implicitly measured by differential DEMs but not by centerline altimetry profiles.

Fig. 2.


74°E
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CHINA
PAKISTAN
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LAHORE
SHIMLA
CHANDIGARH
INDIA
NEPAL
New Delhi
1
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3
4
5
6
7
8
9
10
Glaciers
Elevation (m)
< 1 000
1 001 - 2 000
2 001 - 3 000
3 001 - 4 000
4 001 - 5 000
> 5 001

Fig. 3.

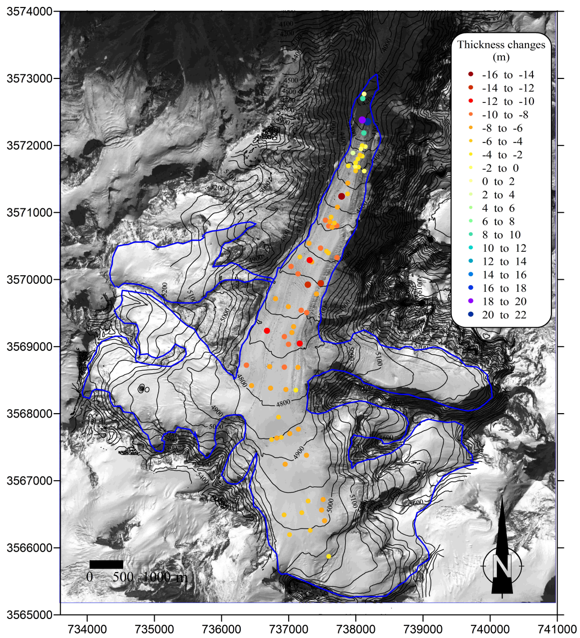
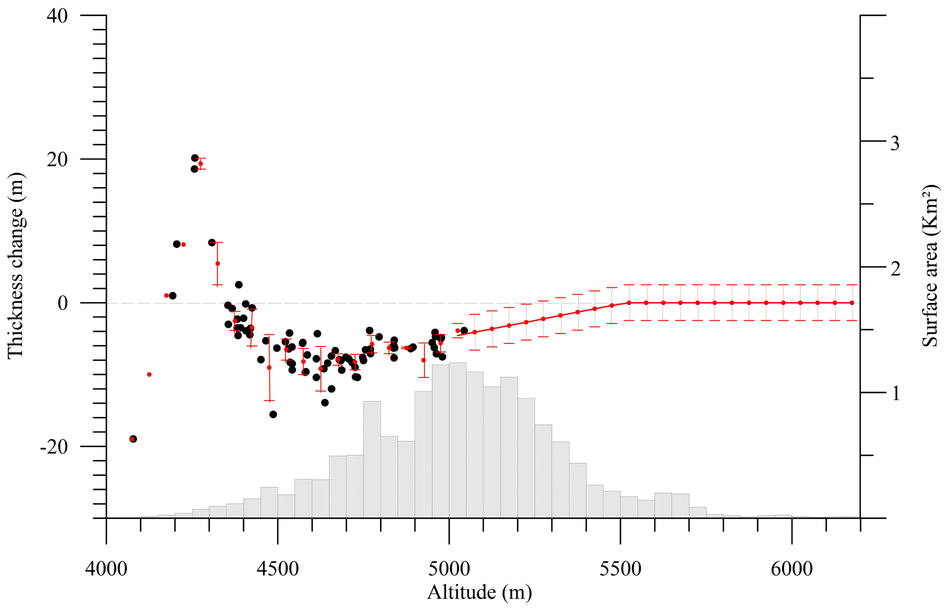
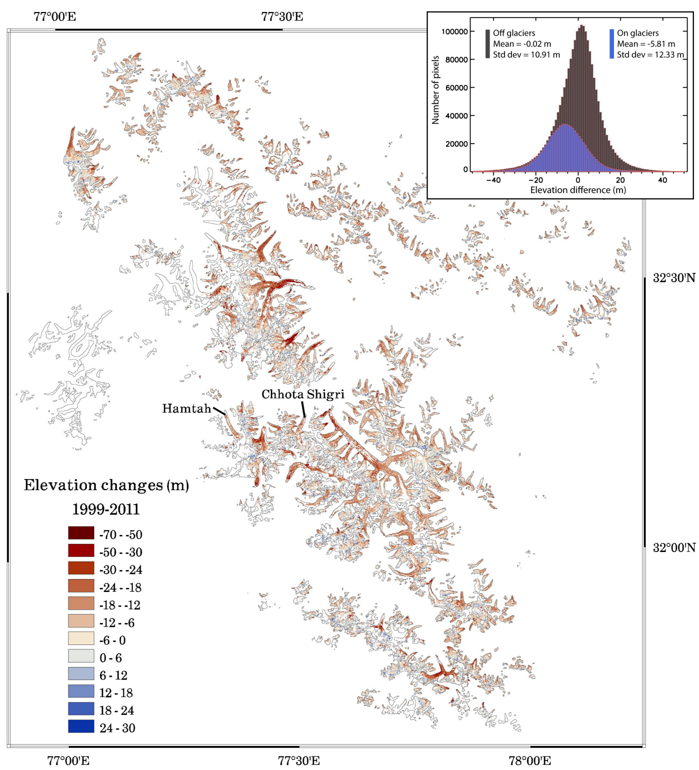

3574000
3573000
3572000
3571000
3570000
3569000
3568000
3567000
3566000
3565000
734000
735000
736000
737000
738000
739000
740000
741000
Thickness changes
(m)
-16 to -14
-14 to -12
-12 to -10
-10 to -8
-8 to -6
-6 to -4
-4 to -2
-2 to 0
0 to 2
2 to 4
4 to 6
6 to 8
8 to 10
10 to 12
12 to 14
14 to 16
16 to 18
18 to 20
20 to 22
0 500 1000 m
N

Fig. 4.



Thickness change (m)
40
20
0
-20
Surface area (Km²)
3
2
1
4000
4500
5000
5500
6000
Altitude (m)

Fig. 5.



77°00'E
77°30'E
78°00'E
32°30'N
32°00'N
Off glaciers
Mean = -0.02 m
Std dev = 10.91 m
On glaciers
Mean = -5.81 m
Std dev = 12.33 m
Number of pixels
Elevation difference (m)
Hamtah
Chhota Shigri
Elevation changes (m)
1999-2011
-70 - -50
-50 - -30
-30 - -24
-24 - -18
-18 - -12
-12 - -6
-6 - 0
0 - 6
6 - 12
12 - 18
18 - 24
24 - 30

Fig. 6.