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The Cryosphere Discussions

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Interactive Comment

Interactive comment on "Region-wide glacier mass balances over the Pamir-Karakoram-Himalaya during 1999–2011" *by* J. Gardelle et al.

Anonymous Referee #2

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This paper presents an impressive and consistent new set of elevation change data for a significant amount of glaciers in the Himalaya-Karakoram-Pamir region based on a comparison of SRTM and SPOT digital elevation models. Thus, the authors provide a sound estimate of glacier mass balances (including their spatial variability) for a highly debated region. I fully agree with the first reviewer that this study is well performed, the methods and results are clearly described and that the article deserves to be rapidly published. Nevertheless, I have some more substantive comments that might require some additional discussion in a second version of the paper. These are not meant to criticize the presented results but might help the authors to refine some of their conclusions.

Substantive comments:

Radar penetration correction:
The estimation of radar penetration depth by comparison of the different frequency bands is reasonable. However, it would be helpful to already provide the order of magnitude of this correction on page 985 (i.e. in the method description) to allow a judgment of its importance. This would be better than just referring to Gardelle et al. (2012b). Furthermore, I am not sure if it is given than the X-band (9.7 GHz) has no penetration depth at all. Obviously it is less than for the 5.7 GHz band, but the total effect is likely to be rather under- than overestimated with the correction. Wouldn't it be possible to dig deeper into this issue using GPR theory?

Seasonality correction:
The quantification of the winter accumulation rates used for the seasonality correction is weak. One single value for the Karakoram is available from the 1980s. For all other glaciers the mean of surveyed glaciers in the Northern Hemisphere is used, i.e. winter accumulations in the Himalaya-Pamir region are quantified by including e.g. maritime glaciers in Norway... A better estimate could probably be achieved using almost every method (analysis of precipitation data, isolated data on accumulation rates from ice cores, etc). For glaciers with a summer-accumulation type (Himalaya), the winter accumulation rates are probably overestimated. The authors cover the large uncertainties in this correction with their error bars. Nevertheless, I suggest to try and get a more reasonable estimate that takes into account local characteristics.

Off-glacier elevation changes:
Obviously, the off-glacier elevation changes between the SRTM and SPOT DEMs are an excellent mean to quantify the uncertainties. Numbers are provided by the authors (page 988, line 16) but are not further discussed. It would be highly beneficial to go into some more details here: Do the off-glacier elevation changes show an elevation dependence? Are there some significant differences between the study regions that might indicate regional biases? Are the off-glacier elevation changes equally dis-

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tributed within one scene, i.e. are they consistent between the center and the edges of the scene? This discussion might be valuable to judge the spatial representativeness of the error bars.

Comparison of discharge to Kaser et al. (2010):
The authors calculate runoff contributions due to glacier imbalance and compare these numbers to observed runoff in the main streams draining the study region. They acknowledge that only annual contributions can be quantified, and refer to Kaser et al. (2010) for seasonal contributions. I am troubled by this comparison and do not think that it is possible: Kaser et al. (2010) have based their analysis on global climate data sets but do neither include direct data on glacier mass balance nor runoff. The approach – and also the results – are thus inconsistent with the percentage contributions presented here. This probably explains the somewhat strange numbers given in Table 6: How can the seasonal glacier contribution (most probably the authors refer to the melt season here, although it is not stated) be smaller (!) than the annual mean contribution? This would require a better discussion of the results by Kaser et al. and more details on their approach, but I would just suggest to remove the comparison here as the methodologies are different and the numbers are rather worrying than helpful.

Thinning over debris-covered ice:
The authors convincingly show that the thinning of debris-covered ice is not smaller compared to clean ice. This would be expected from the well-documented melt reduction below supraglacial debris. The authors interpret this observation with differences in ice dynamics. I have the impression that the comparison of elevation change rates over debris-covered and clean ice surfaces might be biased (explanations see below). Based on my comments the authors might consider adding some more discussion on this important issue. Surface elevation change rates at given altitudes within individual regions are performed. The approach of comparing identical altitudes only is sound and removes a possible elevation bias. However, do the authors also consider *glacier size / elevation range* in their evaluation? In my opinion, a direct comparison of eleva-

tion changes over debris-covered and debris-free surfaces is only feasible for glaciers that exhibit the same elevation range, and thus comparable ice flow dynamics. I would speculate that clean ice surfaces at low elevations (e.g. below 4000 m a.s.l.) are just found on smaller glaciers, i.e. glaciers with relatively high accumulation rates and low ELAs, and that almost all glaciers with a large elevation range have debris-covered tongues. This might lead to completely different dynamic responses of the two glacier types to climatic changes that make an immediate comparison of the dH/dt impossible. Furthermore, the statistical representativeness would also need to be discussed: How many data points for clean ice are available at low elevation in comparison to debris-covered pixels?

Consideration of year-to-year mass balance variability:
The study provides an extensive validation of calculated mass changes against previous studies. However, I miss a comparison to direct glaciological time series. I am aware that very little is available for the region and that the uncertainties are high. Nevertheless, annual mass balance time series (such as from Chhota Shigri Glacier, Azam et al., 2012) might provide some valuable information about year-to-year variability. Strictly speaking the validation of the period mean mass balances with other studies (covering slightly different periods) is only possible after removing artefacts coming from year-to-year variability. It is impossible to provide a sound correction based on the available in-situ mass balance data sets but it would be interesting to see a short discussion about the mass balance variability within the considered 11-year period and whether this variability might explain some of the disagreement with other studies. The present results mostly give smaller mass losses (Fig. 5, Table A1). Could this observation simply be explained by above average mass balance in the last years (i.e. after about 2008) that are covered by this, but not by the other studies (Bolch et al., 2011; Nuimura et al., 2012; Kaaeb et al., 2012; Berthier et al., 2007)?

Specific comments:

- page 976, line 15-17: This sentence is difficult to understand in the abstract. Following my substantive comment above, I recommend omitting it or replacing it with another important conclusion.
- page 977, line 15: A short definition of the glacier imbalance in the present context would be helpful.
- page 984, line 3: The ELA digitized from Landsat images corresponds to this one given year and might show a considerable variability. This might need to be acknowledged in a sentence and/or some references could be provided to back up the assumption of a constant ELA.
- page 986, line 13-15: The seasonality correction would only be lower by 1-2 orders of magnitude than the cumulative signal if the mass balances are significantly different from zero. With the balanced conditions in the Karakoram and the Pamir the uncertainty in this correction might well make the difference between a positive and a negative mass budget.
- page 987, line 16: The density assumption might require some more discussion as it linearly influences the final results. Will the density of volume change be the same for all regions although they exhibit strongly different mass balances?
- page 990, line 6: Elevation changes averaged over the ablation area might be mistaken as mass balances / melt rates. I see the benefit of discussing these data here but I would suggest to clearly state the meaning of ablation area elevation changes and their limitations.
- page 991, line 25: Are there any explanations for these strong differences in the mass balance of neighbouring glaciers? Whereas the authors discuss mass balance differences between the regions in detail (in connection with climatic patterns) the glacier-to-glacier variation in mass balance (which can obviously be significant) is not addressed.

- page 993, line 22: Interesting. Can these numbers be put into physical context? i.e. do they correspond to a penetration into the winter snow coverage only, or also into the uppermost firn layers?
- page 997, line 8: Here and elsewhere: unit: m yr-1 w.e. Wouldn't it be more logical to write it as m w.e. yr-1?
- page 999, line 8-22: Although interesting I was not quite sure if this paragraph is actually necessary for the results / conclusions of this paper. The topic is only loosely related and it could be removed.
- page 1001, line 4: I think, most importantly high-elevation precipitation measurements would be needed. And weather stations in these environments probably have troubles in accurately determining precipitation. So, a sentence might be added that direct measurements of accumulation on High Mountain Asia glaciers would (also) be required to understand to ongoing processes.

Interactive comment on The Cryosphere Discuss., 7, 975, 2013.