

## ***Interactive comment on “Brief Communication: Contending estimates of early 21st century glacier mass balance over the Pamir-Karakoram-Himalaya” by A. Kääb et al.***

**Anonymous Referee #1**

Received and published: 16 December 2014

### — SUMMARY —

In this brief communication, Kääb and coauthors present a well written, extremely condensed series of results concerning the glacier mass changes in the Pamir-Karakoram-Himalaya region. The results are based upon analyses of ICESat data, and thus refer to the period 2003–2008. Important insights are gained in the spatial pattern of the glacier mass changes, differences with respect to earlier estimates are discussed, and some simple quantification of the glacier contribution to regional runoff is performed. Whilst there is no question about the scientific merit of the publication, I wonder whether the format of the contribution complies with “The Cryosphere”: The main text contains minimal methodological explanation, and the reader has basically to “discover” himself

C2648

that some additional, still not exhaustive information can be found in an online supplement. In my opinion, deciding whether this kind of format wants to be promoted by “The Cryosphere” is a task of the editorial board.

### — GENERAL COMMENTS —

The only general comment I have concerns the way methodological information is given. Basically, the main article contains only brief “hints” on the methodology, mostly concentrated in the current “introduction” section and the first lines of section 3. The problem is somehow “aggravated” by the fact that the information about some additional information being available in an online supplementary is not well communicated either: The reader has to “discover” it either through the standard sentence appended to the manuscript (“The Supplement related to this article is available online at...”) or through the caption of Figure 2 (“For details on the gauging stations used and the uncertainty of the contributions see Supplement.”). Having this information mentioned earlier would already help. I’m not sure whether the decision of not including more detailed methodological information was guided by space restrictions due to the “Brief communication”-format or because the manuscript was originally intended for another journal. As a reader, however, I would appreciate if the most important information about the methodology could be included in the main manuscript. By “most important” I mean, for example, the information necessary to understand why the authors conclude that the magnitude and variability of the SRTM C-band penetration is “of larger magnitude than previously assumed” (Abstract and conclusions). This is an important statement, but from the main text it is unclear what analyses have led to this conclusion. As said in the “summary” of this review: The question is not about the scientific merit of the publication, but rather about the format with which it is presented.

### — SPECIFIC COMMENTS —

P. 5859 L.14-16: The comparison as such still does not ensure the absence of a sampling bias, does it? Some information about the histogram-matching step that (I believe)

C2649

was performed would be useful.

P. 5861, L. 22-25: I have difficulties in understanding the sentence. From what I understood from P.5862 L 5-6, the basic statement is that by repeating the analysis using the Randolph Glacier Inventory instead of the outlines actually used in the analyses, one would get more negative elevation changes, correct? I suggest rewording the sentence.

P. 5862, L 5-6: Do you actually mean “too negative”? According to the numbers given in Table 1, the results by Gardner et al. are less negative than the one presented here. Why would they be “too negative” then?

P. 5862, L. 19-20: The references are all right, but here is definitively a point where some more information on how the penetration depth was estimated would be of great benefit.

P. 5863, L. 24-26: Again, I don't understand the sentence. Can it be splitted in two separate statements? I would think that for clarity, even an equation would be helpful.

P. 5864, L. 5-9: The description is “minimalistic” again. Taking the text literally, one could even arise the doubt whether the units have been treated adequately...

P. 5865, L. 22-23: In the text, the description of what data led to Fig. 2 is insufficient. Either include more information or (at least) point directly at the supplementary material.

P. 5866, L. 15-19: Where is this result coming from exactly? There were no sampling problems mentioned in the text, were there? Has it to do with the procedure mentioned at P. 5863, L. 24-26?

P. 5866, L. 20-22: Again: It is not clear what exactly led to this conclusion. In particular: Why is the penetration depth now estimated to be higher than what estimated in Kääh et al. (2012)?

C2650

P. 5867, L. 11: Mentioning the actual stream gauges the given numbers refer to would facilitate comparability to other studies. The generic information “where they [the mentioned rivers] leave the mountains” is too vague for being useful.

P. 5867, L.18-19: Maybe repeat what could explain the discrepancy found for the Himalaya and the East Nyainqentanglha Shan.

— MINOR COMMENTS, AND SUGGESTIONS FOR TEXT RE-ARRANGEMENTS AND FORMULATIONS —

Title: The study addresses the period 2003-2008 only. I would therefore replace the rather vague “early 21st century” with the more specific information “2003-2008”.

P. 5858 L.10-12: Since these sentences are more a methodological aspect than a proper result, I would suggest moving them directly at L.2 (i.e. after the first sentence).

P. 5858 L.13: As now, the section should probably be called “introduction and methods”

P. 5858 L.14-18: It would be good having some references backing up this claims.

P. 5858 L.19: A detail, but since ICESat was operative until 2009, you may want to include a hint already here for why your period of analysis spans to 2008 only.

P. 5858 L.14: “by” should be moved before “(i)”

P. 5859 L.5-6: Since geographic names are mentioned, it would be good being pointed at Fig. 1 here already. . P. 5859 L.18-20: More a question than an analysis I would suggest to actually include in the manuscript: Wouldn't the High Asia Reanalysis (HAR) dataset be suitable for gaining some additional insights in these processes?

P. 5860 L.2: I suggest replacing “is” with “seems” since there is no proper evidence for the claim (is there?).

P. 5860 L. 5-7: I do not understand this sentence. My guess is that you mean something like “Combined, the results by Gardner et al. (2013), Neckel et al. (2014), and the

C2651

glacier elevation change pattern of Fig. 1 suggest...". Please reformulate the sentence.

P. 5864, L. 27: I suggest reformulating the sentence into something like "Note that Gardner et al. (2013) offer a second, more negative..." (the many commas in the current formulation are distracting).

P. 5865, L. 10: Is the number "0.06 +/- 0.01 mm/yr" a results of this study or from the study by Gardner et al.? The sentence is not clear in this respect . P. 5865, L. 15: I suggest adding "(positive discharge equivalent, DE)" after "2003-2008" (as far as I understand, you don't want to mention the value given in Tab. 2 here, correct?).

P. 5865, L. 18: What does "at the glaciers" actually mean?

---

Interactive comment on The Cryosphere Discuss., 8, 5857, 2014.