The Cryosphere Discuss.,
doi:10.5194/tc-2016-165-RC2, 2016

The Cryosphere Discussions

Interactive comment on "Slight glacier reduction over the northwestern Tibetan Plateau despite significant recent warming" *by* Y. Wang et al.

Anonymous Referee #2

Received and published: 26 July 2016

This paper deals with an interesting topic and the results can be very important to complete the picture on glacier recent changes in the high mountain chains of Asia. Unfortunately, this first draft of the manuscript is not acceptable to be published on the TC and I strongly recommend to reject it. The authors need to improve the analysis of their data and in particular the evaluation of accuracy and errors affecting their results. Only after this, they can re-write the paper (smoothing the English language by a mother tongue) and they can re submit it to TCD. More precisely the most fragile part of the paper are the glacier outlines they compared to evaluate glacier changes. In the paper they refer the older data (CGI1) derive from the digitalization of a topographic map featuring a very poor scale (1:100,000) and that such data were improved and crosschecked by analyzing aerial photos. The authors need to describe the quality of the aerial photos they used (mean scale, black and white or color photos, stereo

pairs to be analyzed with a stereoscopic device or orthophotos or single photos without stereoscopic view. . .), moreover it is also important the acquisition time (winter or summer season) of such photos (to evaluate the snow coverage and then the suitability of these photos to be used to assess glacier limits). Analogously a more detailed description of the sat imageries used to derive the most recent glacier outlines (CGI2) is needed, including the cloud coverage affecting each image and the acquisition time (always to evaluate the suitability of the image to detect the actual glacier limits). I suggest to the authors to prepare a table of the input data providing additional information and details concerning the editors of the source data (for the topographic maps), acquisition time and cloud cover of each sat imagines and the analyzed bands. Once these details will be available, the authors could state the actual accuracy of their source data (the glacier outlines) and correctly evaluate the errors affecting their comparisons (and then their results). Moreover the authors need to discuss the presence of supraglacial debris (I hope they had considered this feature which can really make difficult to map debris boundaries!) and I suggest to report in the new version of the paper the number of actual debris covered glaciers they found in the CGI1 and CGI2 data base and the percentage of glacier surface covered by debris (thus affecting glacier measurements and mapping). In the case this feature should be neglected the authors need to discuss the effect of this on their results. Moreover the authors need to re-calculate the errors of glacier area changes since in the present version they adopted an incorrect method to evaluate them. In fact they state that they buffered each glaciers to evaluate the error BUT the actual error affecting the changes is the RSS and not the simple buffer values. In the paper by Minora et al (2016) is reported a valuable example of the correct method to be adopted to assess glacier area errors and glacier area change errors.

Once the authors have applied this method I think probably the errors affecting their results should result larger than the 3

Also regarding the elevation changes the paper needs a strong improvement, in fact:

1) The authors derived a first DEM from old topographic maps (scale 1:100,000), at a very poor scale to obtain a vertical resolution able to capture glacier changes. Moreover old maps are known to be erroneous in accumulation areas of glaciers and the authors should demonstrate that in their study area the topographic source are valid to describe glacier surface. 2) It is not clear the SRTM DEM version they used for the comparison (and again if they used the version featuring a 90 m resolution it seems too poor to derive glacier changes. . .) 3) more details are needed on the method applied to adjust and compare the older and new DEMs 4) A deeper and more complete discussion of the results obtained is needed in particular Fig. 2

Considering these major improvements the paper has to be re written and re-submitted

Referenced cited in my review

1) Minora U. et al., (2016), Glacier area stability in the Central Karakoram National Park (Pakistan) in 2001–2010: The “Karakoram Anomaly” in the spotlight, Progress in Physical Geography May 25, 2016 0309133316643926, DOI: 10.1177/0309133316643926

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3) Citterio M, et al. (2007) The fluctuations of Italian glaciers during the last century: a contribution to knowledge about Alpine glacier changes. Geografiska Annaler Series A: Physical Geography 89(3):164–182.

4) O’Gorman L (1996) Subpixel precision of straight-edged shapes for registration and measurement. IEEE. Transactions on Pattern Analysis and Machine Intelligence 18(7): 746–751.

5) Paul F, et al. (2009) Recommendations for the compilation of glacier inventory data from digital sources. Annals of Glaciology 50(53): 119–126.

Please also note the supplement to this comment:

http://www.the-cryosphere-discuss.net/tc-2016-165/tc-2016-165-RC2-supplement.pdf

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