Review of Wang et al., The Cryosphere, July 2016

In their paper, Wang and co-authors report on area and elevation changes for the glaciers of the Western Kunlun Mountain in the northwestern Tibetan Plateau. The rather moderate area and volume changes are interpreted in view of climate trend in the regions.

Potential added value of this study to justify publication in TC: would be, to my knowledge, the first study to provide direct evidence for close to 0 mass balance in the West Kunlun during a 3 to 4 decades period. Right now measurements showing glacier equilibrium are restricted to the 21st century only.

However, the paper is not ready for publication, and thus I will not provide line by line comments. There are some major issues that need to be tackled first by the authors before the reviewers should spend their time into the details. In view of the methodological deficiencies and the amount of work needed to raise this paper to an acceptable level, I can only recommend to the editor that she/he rejects the paper.

I/ Issues with area changes

Authors did not produce their own inventory from satellite data. They collected existing glacier outlines from global or regional database. This is not a problem in itself. But before concluding on area changes, the authors need to verify/correct the quality of these outlines specifically over their study area by overlying them on satellite images (e.g. Landsat). CGI1 for example is know to be problematic in many regions so it can not be used "as is". It was unclear to me if the authors did this necessary quality check for CGI (P2 L19 is unclear). If they did so, the authors need to report in detail on the changes between the original / revised CGI1 and illustrate the changes by a least a figure (old/revised outline). Here the authors should refer to a recent study specifically for the Western Kunlun (Ke et al., 2015). The latter paper (and reference therein such as (Bao et al., 2015)) need to be referenced and used to improve the data analysis. Omission of these key relevant references is one major problem of the submitted manuscript. In particuler Ke et al. state that

> *" We compared glacier outlines provided in RGI v2, GLIMS (updated in Feb 2012) and CGI with our new mapped glacier inventory. The CGI was completed in 2002 based on topographical maps and aerial photographs acquired from the 1950s to the 1980s (Shi et al., 2010), and was then incorporated into the world glacier inventory including GLIMS and RGI. Although the latter two databases provided significant updates to the glacier outlines in some regions of the QTP, glacier outlines in the three data sets remain similar in the WKM region,* ***and they show quality issues such as location shift, shape distortion and error delineation of inner boundaries, as illustrated in Fig. 8. Therefore, the glacier outlines from the three datasets are not suitable for comparing glacier changes*** *and were used as reference for discriminating glaciers from seasonal snow."*

Further, it is dangerous to report area changes from different sources (CGI, Gandam, etc...) because the scientists that produced the inventories may have different interpretation of what is a glacier (upper limit for example) and thus most differences in area may actually originate from the glacier definition itself (Paul et al., 2013). The authors thus need to examine each inventory and "homogenized" them so that the outlines become comparable.

Also the different inventories are covering time span of several years. Did the authors used the exact date of the corresponding images for each glacier? Again unclear.

II/ Issues with elevation changes

The DEM differencing is probably the most problematic aspect of the paper because :
1/ topographic maps are known to be erroneous in accumulation areas of glaciers due to lack of texture in the old aerial photographs). Authors should show some evidences that this is not the case in their study area and convince the readers that these old maps are a valid source for measuring elevation changes.
2/ unclear which version of the SRTM DEM was used. C-Band or X-Band? I hope the authors did not merge them.
3/ it is now well-known/admitted that in the dry/cold firn the SRTM is mapping a surface which is below the real glacier surface because of penetration of the radar signal into snow and firn. This penetration depth can reach as much as 10 m (see among others (Barundun et al., 2015; Fischer et al., 2015; Kääb et al., 2015)). A geodetic mass balance without any correction for this penetration will likely be severely biased in the dry cold climate of the western Kunlun mountains.
4/ there is a complete lack of technical details on how the DEM relative adjustment were performed.
5/ the resulting map of elevation differences (Figure 2) seems very noisy and unrealistic. If we trust Figure 2, glaciers in the west of the study regions experienced a strong mass gain (of over 0.5 m/yr during the last 40 years). Suspicious. Authors did not discuss at all the pattern of this figure. There are some evidence of glacier surges for example.
6/ The description of the source of uncertainties and how the errors were calculated is almost lacking. 2 lines only! Not acceptable.

III/ The referencing is poor

Some important references (included some dealing with exactly the same topic, see above) are missing. Some examples below of missing/inappropriate/badly cited papers.
P3L30. Did the authors make themselves this area change measurement for Tien Shan? If not, they need to provide a reference. The same apply to all other values of area change provided in the text.
P2L5. Do the authors need 11 references in total (6 +5 at the end of the sentence!) to tell that the geodetic method exists and is based on differencing of two DEMs? One (max two) reference is sufficient.
P1 L30. Example of a reference that has been misread. Kapnick et al. (2014) did not conclude to a difference in climate change. They highlighted the differing sensitivity of Karakoram glaciers to a given climate change compared to other Himalayan glaciers. Said differently, with the same change in climate (for example a 1° warming), glaciers will respond very differently depending on their climate sensitivity.

IV/ We do not learn much in the discussion

Currently, it is mainly a review of existing papers on the topic of glacier changes around the Tibetan Plateau. This is not what a discussion should be. The discussion should be based on the NEW results from the study and should discussed them in view of past studies and should help to answer this question: "What did we learn from this study that we did not know before"?

Along the same lines, the new climate analysis does not bring any new knowledge neither.

Referenced cited in my review

Bao, W., Liu, S., Wei, J. and Guo, W.: Glacier changes during the past 40 years in the West Kunlun Shan, Journal of Mountain Science, 12(2), 344–357, doi:10.1007/s11629-014-3220-0, 2015.

Barundun, M., Huss, M., Sold, L., Farinotti, D., Azisov, E., Salzmann, N., Usubaliev, R., Merkushkin, A. and Hoelzle, M.: Re-analysis of seasonal mass balance at Abramov glacier 1968–2014, Journal of Glaciology, 61(230), 1103–1117, doi:10.3189/2015JoG14J239, 2015.

Fischer, M., Huss, M. and Hoelzle, M.: Surface elevation and mass changes of all Swiss glaciers 1980–2010, The Cryosphere, 9(2), 525–540, doi:10.5194/tc-9-525-2015, 2015.

Kääb, A., Treichler, D., Nuth, C. and Berthier, E.: Brief Communication: Contending estimates of 2003–2008 glacier mass balance over the Pamir–Karakoram–Himalaya, The Cryosphere, 9(2), 557–564, doi:10.5194/tc-9-557-2015, 2015.

Ke, L., Ding, X. and Song, C.: Heterogeneous changes of glaciers over the western Kunlun Mountains based on ICESat and Landsat-8 derived glacier inventory, Remote Sensing of Environment, 168, 13–23, doi:10.1016/j.rse.2015.06.019, 2015.