Comments on "2000-2012 mass budget of Purogangri Ice Cap" by Neckel et al.

Neckel and co-authors compared January 2012 Tandem-X and February 2000 SRTM-X data to assess the volume change and mass budget of the Purogangri Ice Cap in Tibet. They conclude to a mass budget which is close to balanced.

The strength of the approach developed here is that it compares two remotely-sensed dataset acquired using the same wavelength (Band-X) so that the elevation change should not be affected by differential penetration into snow/ice. This is an important study given that, in the coming years and with the availability of more Tandem-X DEM, one can expect that those types of mass budget assessment will be quite frequent.

However, I concur with Reviewer#1 that the paper really lacks a more detailed description of the methodology and, also, the data source. More details/explanations are needed for those glaciologists who are not specialist in SAR interferometry.

GENERAL COMMENTS

1/ Nothing is said about the generation of the 2012 Tandem-X DEM. Built by the authors? With what tool/software? Or did they receive it from DLR?

2/ Can the authors confirm that they provided (P1127) the cumulative mass budgets over 12 years and not annual mass budgets (I was a bit unsure)? I think it would be best to provide annual mass budget all along the paper because this is a more common practice and the values can readily be compared to other estimates on the Tibetan Plateau (e.g., Yao et al., 2012) or elsewhere in High Mountain Asia. If the authors gave the cumulative mass budgets then the differences between the two methods and two density scenarios are rather small (if not, they are large).

3/ Do the authors really trust more the DEM difference method than the INSAR approach as suggested by its smaller uncertainty? Need to be discussed.

4/ Given that the ice cap has already been split into individual glaciers (according to Figure 1), the analysis could be strengthened/deepened by examining the variability of the mass budget among the glaciers.

5/ It is not clear whether the authors corrected for a vertical offset between the DEMs off glaciers. If they indeed corrected a vertical offset, then what value did they use in eq. (4) for the mean of non-glacier elevation differences?

SPECIFIC COMMENTS

Title. I think "ice cap" should be capitalized given that this is here associated to a geographic name.

Abstract. The range of possible annual mass budgets using different methods and density assumptions should be quoted in the abstract.

P1120. L15. “exceptional fast advance”, somewhat in contradiction with the “long time period of constant glacier advance” quoted P1129 L7…

P1122. L1. Can the authors provide the % of the glacier surface covered by the data?

P1122. L3. Is not the original SRTM Band-X DEM provided in Lat/Lon with a grid spacing of 1 arc second? This is different from a 25 m by 25 m on a cartographic grid. Did the authors reproject the DEM? If yes, how (resampling filter) and using what projection system?

P1122. L4. Can the authors provide the mean bias? The number of points? Any outliers excluded to reach this impressively low standard deviation? Which ICESat data did the authors use? From all campaigns? Did the authors exclude cloudy footprints if any? Can the authors also make it clear in the text that there are no ICESat footprints on the ice cap itself (if this is really the case as suggested by figure 1)? See also my comment below about Figure 5: those very low standard deviations against ICESat suggest that the ICESat sample may not be representative of the rest of the terrain.

P1122. L19. Same as comment just above. Sample size? Mean bias?

P1123. L1. It is not straightforward to estimate a horizontal shift between a DEM and an image. How what it done? Visually?

P1123. L22. Authors could note here that there error estimate will also include one year of glacier change so is rather conservative.

P1124. L11. Why a footnote here? Papers could be cited directly in the main text. The more recent review paper on the topic by [*Rott*, 2009] may also be cited.

P1124. L23. Are GCPs distributed in the whole scene? Or close to the glaciers? A bit more details would be welcome.

P1125. L3. “achieve”?

P1125. Eq (3). The number of non-glacier grid cells (n) can be very large if a very long strip of SAR data and all non-glacier terrain (even far away from the ice cap) are used. What value of “n” was used? Should not “n” be restricted to a reasonable number of grid cells close to the glacier?

P1126. L21. I do not think correlated is the right word here. “restricted” or simply “found” seem more appropriate.

P1127. L25. A bit counter-intuitive to have 1-sigma value for INSAR twice smaller than for DEM differencing and then mass budget uncertainties 5-10 times smaller for DEM differencing. It is explained in term of systematic error component but no value is given in the text for the latter error (I think). The reader is left a bit confused (see also my general comment #3)

P1128. L3. “pattern” of what?

Figure 2. I suggest a larger histogram to improve readability by enlarging the insets to their lower right and increasing the font size. It is not a problem if the labels of the geographic coordinates are masked. Authors should explain in the legend what is the dark solid line crossing 89°15' and 34°N (= refer to figure 5).

Figure 3. What is the solid line through the upper panel? A polynomial fit? If yes, at what order? Also explain in the legend what is the dot (=altitude of 0 elevation change, although I expect this dot will be removed in the revised version because the 0 elevation change is generally not the ELA as pointed out by rev#1). Can the authors indicate the slope of the line fitted through the non glacier elevation change? To illustrate numerically that "elevation dependant bias" is not significant. Can they also add the glacier hypsometry as an additional central panel to see how the glacier area is distributed with altitude?

Figure 4. Legend. "in glacier tongue" is vague. Is not there a name for the glacier? Or maybe his code in the Chinese glacier inventory? Or in the GLIMS database? The color scale should include the value for the central tick to confirm that the color scale is linear and, also, not centred on 0 (and thus different from Figure 2).

Figure 5. Provide the mean and standard deviation (SD) of the elevation difference for those two profiles so that the two methods can be compared numerically and not only visually (on the plot or in the legend). On this profile, it seems to me that the SD of the "DEM diff. " is higher than the SD of the individual DEM evaluated against ICESat (SRTM Band-X, SD = 2.67 on P1122, L8 ; Tandem-X, SD = 1.0 m on P1122, L19) summed in quadrature: square(2.7^2+1.0^2)=2.9 m. Can the authors check that their comparison with ICESat does not sample a flat/smooth terrain where the DEM will have a higher accuracy than on the rougher terrain close to the glacier? This presumption seems to be confirmed P1127 L25 where you quote an off-glacier SD of the elevation difference of 7.3 m, nearly three time larger than the ICESat-derived SD.

References

Rott, H. (2009), Advances in interferometric synthetic aperture radar (InSAR) in earth system science, *Progress in Physical Geography*, *33*(6), 769–791.