Response to Reviewer #1 on "Co-registration and residual correction of digital elevation models: A comparative study"

Comment received: 31 May 2023

Key:
Reviewer comment (black)
Response (blue)

They are cases where the authors do the minimum to reply to reviews and this is frustrating for the reviewers. And there are others cases where they take into account all reviewer's comments, reply to them in details and do even more than what was requested. We fall here in the latter situation and I would like to command the authors for that. It makes the life of a referee much easier. By processing numerous DEMs from different sources and in different contexts, authors managed to demonstrate that the alternative DEM coregistration method by Rosenholm and Torlegard should be now seriously considered by the glaciological community.

Thank you for your detailed review and constructive comments.

My only major criticism concerns the discussion which is very short, weak and include an analysis of the bias with altitude that is not well connected to the rest of the article. Maybe such a result could fit into the discussion but currently it lacks a clear connection to the rest of the paper. Authors need to explain that such a correction is sometime applied in the literature after the two others corrections (refs needed) but without strong physical basis. Then they can discuss to what extent this is a difficult correction to apply as in most cases the calibration sample (stable terrain) is so different from the target sample (glaciers) in term of altitude.

Some changes have been made according to your suggestions.

Reliability is the common topic for all the contents in the discussion section, while the experimental section focuses on accuracy only.

Authors followed my suggestion to examine DEMs separated by just a few days. They describe the remaining bias on glaciers (histogram in Figure S2) but there is NO reason to compute it on a specific area of the image only. Residuals on the entire image need to be computed. The histogram is useful but authors could also provide some metrics like the mean and the standard deviation for elevation changes on glaciers for both methods. The mean is important here because this is ultimately what we want to measure (the glacier mean elevation changes).

Figure R2a shows the elevation-change histogram of the glaciers from the entire image in Figure R1. The results of the L23 method are sufficiently negatively biased. The reason for this is that almost all of the glaciers locate on the north side (cf.

Figure R1). However, if glacier-covered pixels were uniformly distributed on the image, the mean of elevation changes on the glaciers would be nearly zero, like the statistical result on stable regions (Figure R2b). Though the performance difference between L23 and L57 methods can also be observed from the standard deviations, similar information has been previously provided by MedAD values in Figure R1.

Figure R2c demonstrates that how large biases can be caused by uncorrected attitude errors, which is why only a specific area of the image was used for the calculation in our manuscript. It can be seen that a bias of approximately 10 m exists in a local area, while the MedAD difference on the entire image is just about 1 m (6.126 m vs. 5.063 m in Figure R1).

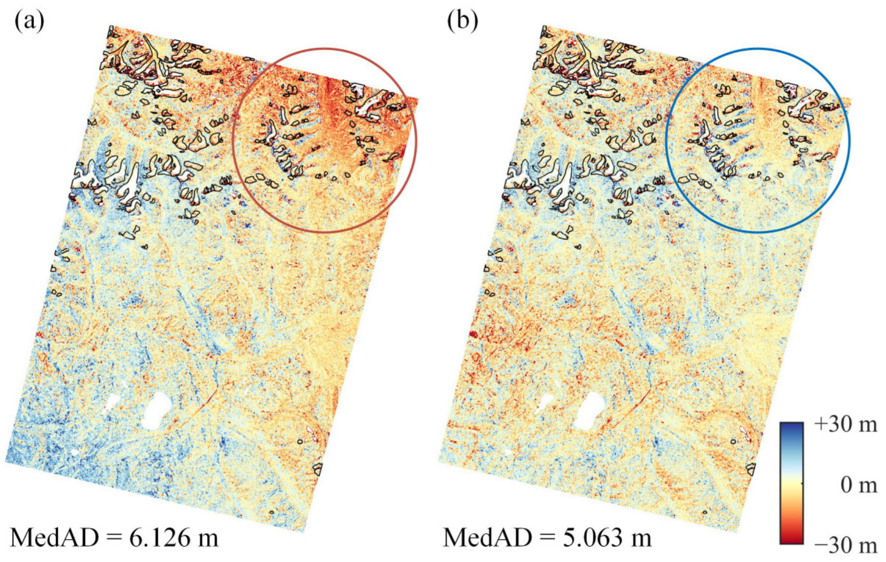

Figure R1. Co-registration results of DEM pair HMA-6: L23 (a), L57 (b)

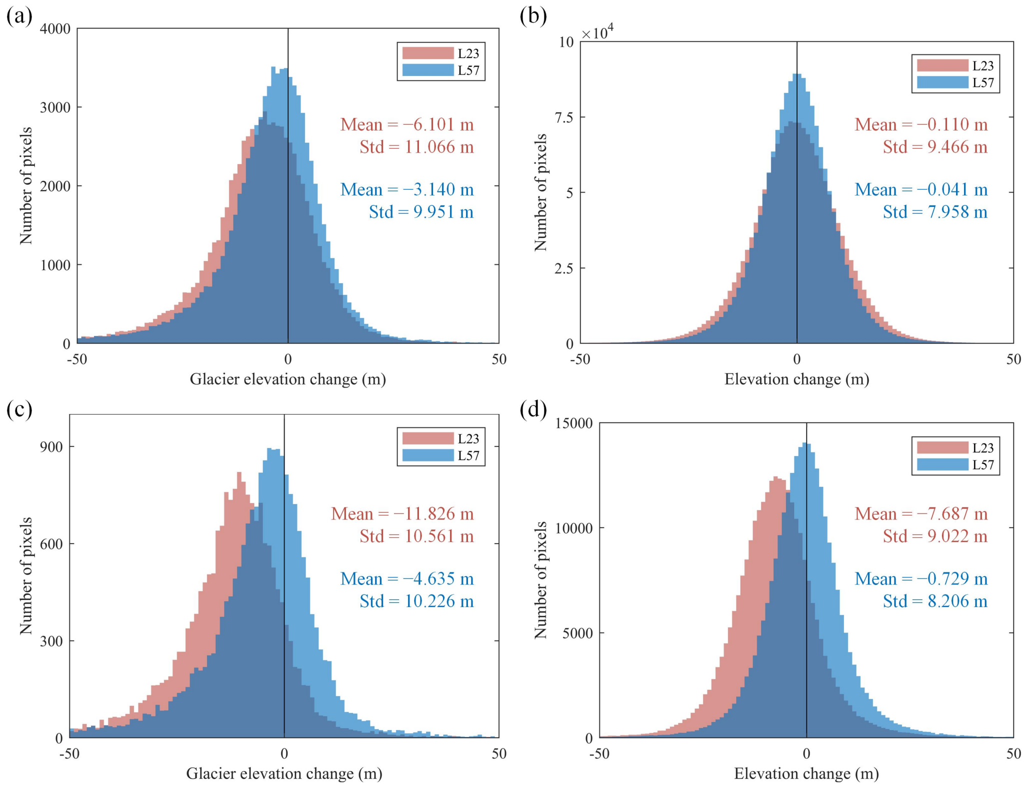

Figure R2. Histograms of elevation differences in Figure R1. Top: the entire image. Bottom: the specific area within the circle. Left: Glacier-covered regions. Right: Stable regions.

Authors need to tell how, practically, readers could access their tools to apply the Rosenholm and Torlegard coregistration methods. Do they have an implementation of the algorithm open to all? In which language?

The codes are written in MATLAB, currently available at https://github.com/shenapm/DemCoReg, and archived at https://zenodo.org/record/8098337.

Specific comments

Line numbers refers to the track change version of the MS uploaded by the authors *-ATC2.pdf

Abstract : The 83.3 % improvement is true but this an outlier. It can be quoted in the main text but it would be important to quote the mean improvement in the abstract. To avoid overselling.

This sentence has been revised to “an average of 4.6% and 13.7% for the test datasets from Greenland Ice Sheet and High Mountain Asia, respectively”.

Figure 1. I had no problem visualizing and understanding the NK-2011 figure but struggle here to visualize this one even in 2D.

The 2D graph (Fig. 1b) of our manuscript is compatible with the Fig. 2 of NK-2011 (Figure R3b vs. Figure R4a). In our drawing, the elevation difference is greatly exaggerated, which aims to make it readily interpreted in a 3D graph (Figure R3a).

For ease of understanding, Figure R3b can be divided into two parts: a DEM is first shifted horizontally (Figure R5a) and then shifted vertically (Figure R5b). The induced elevation difference is given by

$$\begin{aligned} dH &= dH_{XY} + dH_Z \\ &= a \cdot \tan(\theta) + c \end{aligned} \tag{R1}$$

The above equation is valid only for the special case when $b = \psi$, while the correct equation for the general case (i.e., the Equation 2 of NK-2011 and the Eq. 2 in our manuscript) should be

$$dH = a \cdot \cos(b - \psi)\tan(\theta) + c \tag{R2}$$

The multiplier term $\cos(b-\psi)$ in Eq. (R2) can only be illustrated by a 3-D graph (i.e., Figure R3a). In the general case, the shift vector (red arrow) and the DEM gradient vector (blue arrow) located at different vertical planes (OE'G' vs. OEF). In other words, their aspect angles are different with each other, i.e., $b \neq \psi$. It can be seen from Figure R3a that the horizontal shift vector OE' is composed of EE' and OE. On the one hand, EE' does not cause any elevation change, because it is perpendicular to the vertical plane OEF defined by the gradient vector and the terrain aspect direction. On the other hand, the elevation change induced by the vector OE is given by

$$dH_{XY} = \mathrm{EF} = \mathrm{OE} \cdot \tan(\theta) = \mathrm{OE}' \cdot \cos(b-\psi)\tan(\theta) = a \cdot \cos(b-\psi)\tan(\theta) \tag{R3}$$

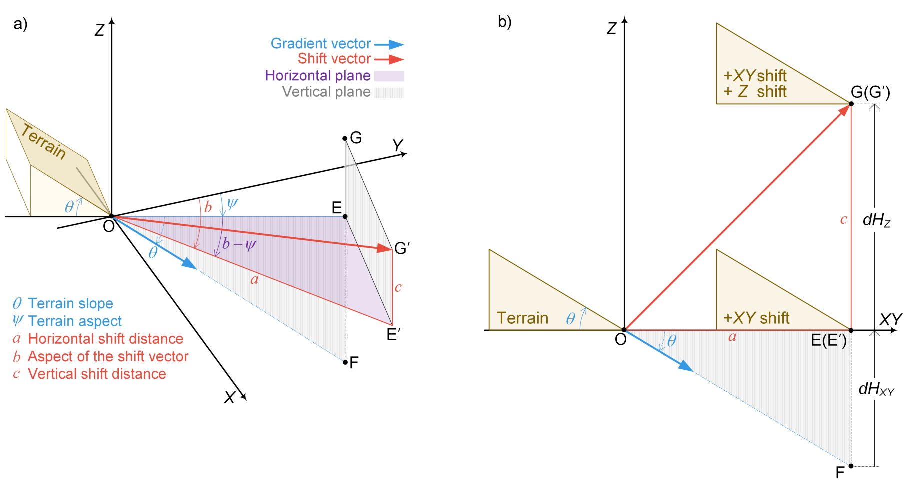

Figure R3. The Fig. 1 of our manuscript. Elevation differences induced by DEM shift. (a) 3-D view when $b \neq \psi$. (b) 2-D view when $b = \psi$.

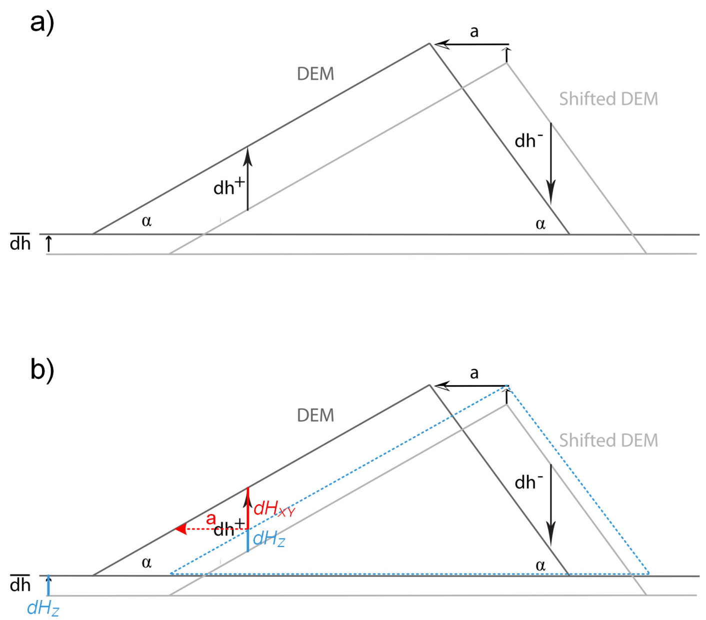

Figure R4. The Fig. 2 of Nuth and Kääb (2011). The original version (a) and a revised version with some auxiliary information (b).

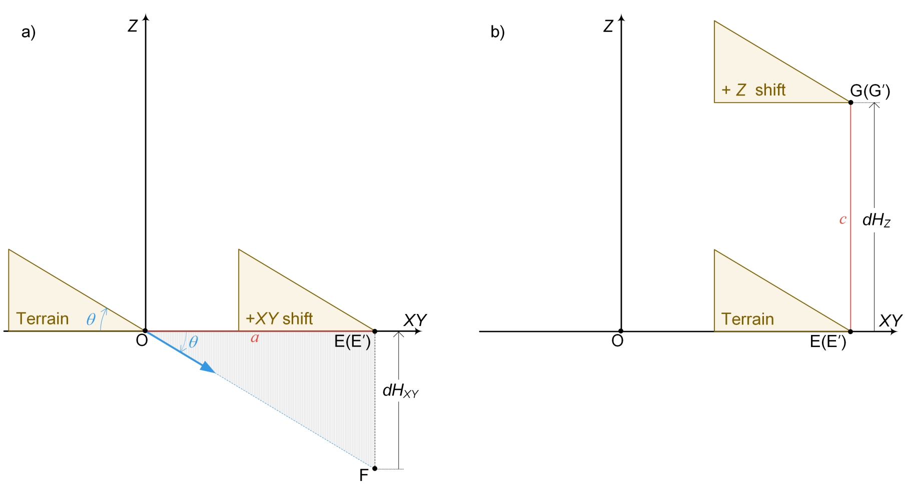

Figure R5. Decomposition of Figure R3b. Elevation differences induced by a horizontal shift (a) and a vertical shift (b) of a DEM.

Table 1. It would be easier for the reader to label Nuth and Kaab using NK and Rosenholm and Torlegard using RT all along the article.

Modified as suggested.

L183. Should Leprince et al., 2007 be retained here and elsewhere? They do not deal with DEM (but velocity fields). This ref could be quoted of course but authors need to explain why it is relevant to the present study then.

Thanks for pointing it out. This reference has been removed.

L236. The classical reference for the NMAD is Holhe and Hohle (cited a lot in the crysophere community, Höhle, J. and Höhle, M.: Accuracy assessment of digital elevation models by means of robust statistical methods, ISPRS Journal of Photogrammetry and Remote Sensing, 64, 398–406, https://doi.org/10.1016/j.isprsjprs.2009.02.003, 2009) that defined it using the 1.4826 scaling factor. Maybe make it 100% clear that this factor is not used.

There are several variants of the MedAD (a.k.a. MAD) in literature, e.g.,

1) $1.4826 \cdot \underset{i=1,\ldots,n}{\mathrm{median}}\left(\left|x_i - \underset{j=1,\ldots,n}{\mathrm{median}}(x_j)\right|\right)$

2) $\underset{i=1,\ldots,n}{\mathrm{median}}\left(\left|x_i - \underset{j=1,\ldots,n}{\mathrm{median}}(x_j)\right|\right)$

3) $\underset{i=1,\ldots,n}{\mathrm{median}}\left(\left|x_i\right|\right)$

In the first two variants, the MedAD is calculated around the median. It is a measure of scale and can be seen as a robust version of the standard deviation. The constant 1.4826 is a correction factor which makes the MedAD consistent with the standard deviation at Gaussian distributions.

The last variant was used in our experiments. It is calculated around zero and can be used as a robust alternative to the Root-Mean-Square Deviation (RMSD). This version of MedAD is a combined measure of location and scale, and, accordingly, the 1.4826 factor should not be introduced.

L274. I do not think accuracy is the appropriate term here. The authors measured the improvement as a decrease in the dispersion of the residuals which is a proxy for the precision.

This sentence has been revised to “with removing 11.8% more errors”.

L356. Fig 5d. A typo here? Authors also need here to tell (L355) that 83% improvement is an extreme case. Otherwise, it oversells the result of the study.

L358. I do not think image resolution has something to do here. It is a more modern satellite platform with better known orbital parameters. It is likely why the initial offsets are smaller than with ASTER.

Some statistical results of the other ZY-3 DEMs in the supplement have been added to the main text according to your suggestion.

Many studies have shown that ZY-3 (a.k.a. ZiYuan-3) satellite suffers from large attitude errors (Cao et al., 2017; Shen et al., 2017; Ye et al., 2019). In this study, more attitude-induced residuals can be overserved in ZY-3 DEMs, e.g., 4.680 m (Fig. 10c) → 0.780 m (Fig. 10d), when comparing to those in ASTER DEMs, e.g., 6.963 m (Fig. 5c) → 6.140 m (Fig. 5d).

When the RT method is used, the co-registration residuals of ZY-3 DEMs are much smaller than those of ASTER DEMs, e.g., 0.780 m (Fig. 10d) vs. 6.140 m (Fig. 5d). The reason for it may be a higher resolution of ZY-3 raw images, rather than the attitude measurement accuracy.

Reference:

Cao, J. S., J. H. Fu, X. X. Yuan and J. Y. Gong (2017). "Nonlinear bias compensation of ZiYuan-3 satellite imagery with cubic splines." ISPRS Journal of Photogrammetry and Remote Sensing 133: 174-185.

Shen, X., B. Liu and Qing-Quan (2017). "Correcting bias in the rational polynomial coefficients of satellite imagery using thin-plate smoothing splines." ISPRS Journal of Photogrammetry and Remote Sensing 125: 125-131.

Ye, Z., Y. S. Xu, X. H. Tong, S. Z. Zheng, H. Zhang, H. Xie and U. Stilla (2019). "Estimation and analysis of along-track attitude jitter of ZiYuan-3 satellite based on relative residuals of tri-band multispectral imagery." ISPRS Journal of Photogrammetry and Remote Sensing 158: 188-200.

Figure 11. For the sake of simplicity I would simply show panel b, e, g. It will make the life of your readers much easier. In fact I do not really see the point of introducing the coregistration to SRTM here. But authors need to compute the glacier elevation changes statistics (mean and standard deviation) for the entire set of glaciers in the scene and write these numbers close to the histograms.

This figure has been modified based on your suggestion. The reason that only a corner area of the image was used for the calculation has been explained in the reply to your earlier comment.

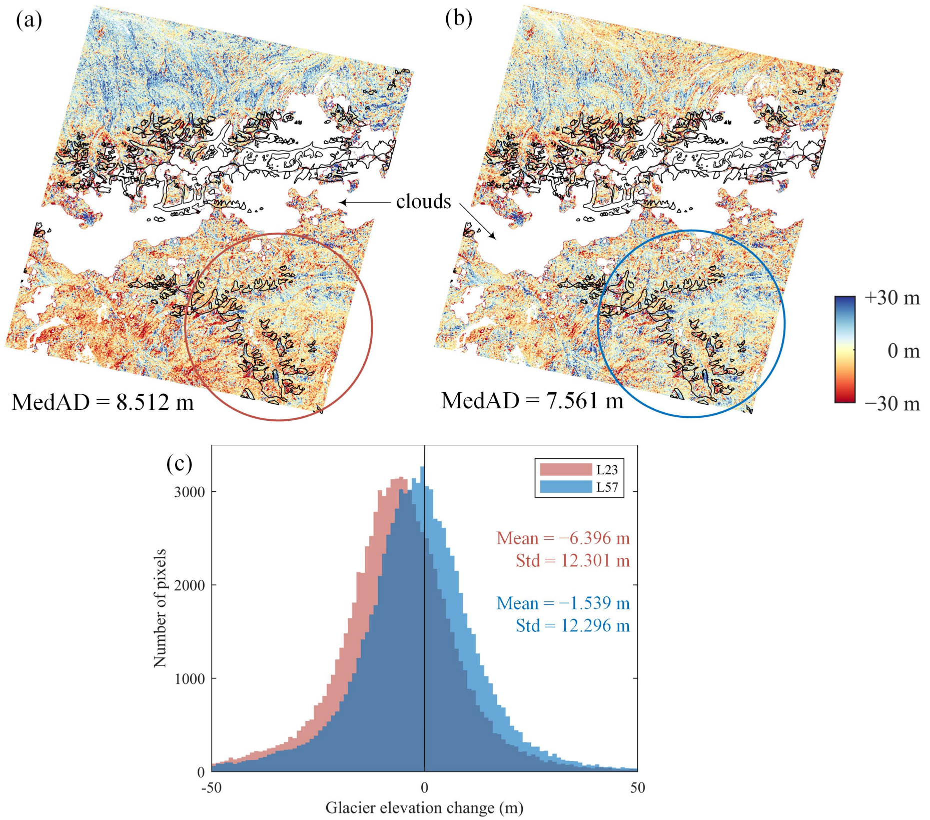

Figure R6. Co-registration results of DEM pairs HMA-2 based on linear versions of the NK method (a) and the RT method (b). (c) The histogram of elevation change for glaciers within the circle.

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Figure 12. Authors need to add a third panel to show how one of the image looks like so that the reader can understand what is bare terrain and where the ice sheet is.

Modified as suggested.

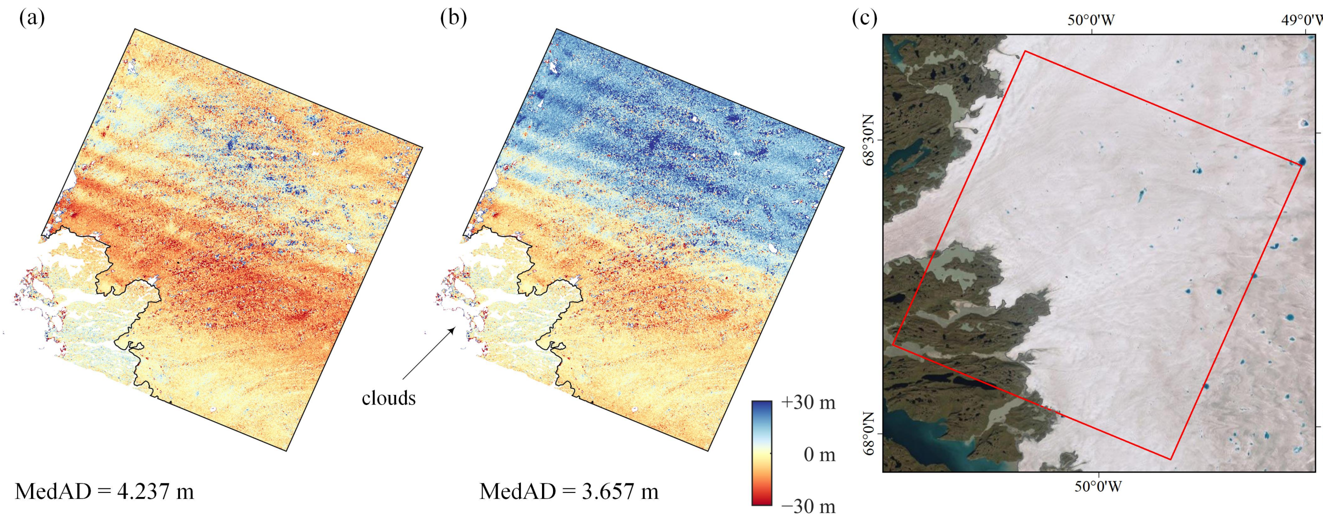

Figure R7. Co-registration results of ASTER DEM 20190725 (Scene ID: AST14DEM.003:2344943025) and ASTER DEM 20190826 (Scene ID: AST14DEM.003:2346334895). (a) The NK linear version. (b) The RT method. (c) The location of DEM pairs, overlaid on an optical satellite imagery.

L424. This does not fit well with the rest of the article as it was clearly explain that correction of DEM with altitude would not be performed. I agree with the results but I think they do not fit in well here. So basically, after removal of this part, the discussion would be limited to just some new results to show the limitation of the coregistration. This makes a very thin discussion. I think the analysis of DEMs with reduced time difference to confirm the relevance of the RT methods for glacier change study could nicely fit in the discussion.

In Sections 4 and 5, we focus on two different topics, i.e., accuracy and reliability, respectively. There was no discussion section in our initial manuscript. The editor suggested that it might be better for us to add a discussion section, so we decided to include some reliability-related stuff, considering that almost all the contents related to algorithm accuracy have been already discussed in the experimental section.

The reliability issue caused by a lack of sufficient geometric constraints occurs in various regression problems, including DEM co-registration as well as residual correction along terrain altitudes and planimetric coordinates. We agree with the reviewer that these discussions are not strongly related to other parts of the paper. If the reviewer still thinks that this part is too weak, we could remove the whole section and change the title of Section 4 to "Experimental results and discussions".

Supplement. Define abbreviation (case study sites) to make the supplement self consistent.

Modified as suggested.