# 2nd Referee report on "Heterogeneous spatial and temporal pattern of surface elevation change and mass balance of the Patagonian icefields between 2000 and 2016"

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The Cryosphere Discuss., https://doi.org/10.5194/tc-2018-258

Referee comments are shown in **black**, cited aurhor's responses in **blue**

General Response:

Thank you for providing these informative comments and the clearly improved manuscript. The general structure has improved through clarifications of sources of work / data and of the application of certain methodological steps. Also, the rearrangement of the methods section gives the opportunity to quickly trace the workflow and the error estimation. This is a great feature for a scientific publication. Yet, not all discussed issues have been resolved.

Generally, previous review comments were perceived more technically oriented than intended. The idea was to get more transparency to methods, where possible. ITP processing will not be re-trackable for most readers – o.k. But when it comes to SECR / VCR /geodetic mass balance, the community will be able and willing to re-track how the results have been produced and evaluated. Therefore, the reviewer's intention was to point out passages, where methods can be described in a more accessible way to scientific readers from related disciplines. There were also misconceptions from my side about specific points, that have been clarified and solved by now. Thank you for correcting the mistaken statements and revising the corresponding passages in the text. A few comments on methods remain for this second round.

Thus, I want to explain: I do accept the scope of the paper and do not ask to inflate the manuscript with a lot of method details. I would rather ask to substitute some phrasings with more precise ones. Even the extensive error estimation terms do not necessarily need to be in the manuscript as it now resulted from the first review. Rather they could be listed in the supplement – to enable traceability without emphasizing methodological parts of the study too much. Please consider this option for each of the subsequent detailed comments as well.

Another major concern of this and the first review was to make the paper more easily comprehensible without having to read the thesis Abdel Jaber (2016) in parallel. With respect to this, comments reappear.

Apart from the systematic error estimation, the rest is to be considered minor revisions. The paper has improved to an almost publishable version. But the reappearing concern about an appropriate coregistration validation should be seriously addressed.

General comment 3.1.2 Coregistration of DEMs and following error contribution in 3.3.3

I summarize: Coregistration is done by manual APO updates through ITP processing with regard to ref. DEM (TDX 0.4 arcsec). Further efforts are claimed to not be necessary for TanDEM products. NASADEM was vertically adjusted, horizontal adjustment was checked to be needless. I suggest support the precision of this process, which is of key importance to geodetic method, with statistical values more adequate than the provided ones.

Therefore, I want to emphasize again: If you assess the error on the same CRs, where you corrected the offset, you get the residual error of the values you calibrated your model with. Not the error of the values you claim it to be valid for which is all the rest of each DEM. It for sure is possible to find some VRs (validation regions) with similar properties as the CRs. Show the spread of height diff to reference DEM there. Including slopes up to 20° in this validation process would solve two problems: 1) the dependence of the elevation difference to slope could be addressed. 2) the claim that horizontally no shifts remain, could be padded, since they appear more significantly with slope. Values would point to a positive /negative distribution center corresponding to the spread of aspects in the corresponding scene if residual hor. shifts remain.

Specific comments: 3.1.2 Coregistration of DEMs and following error contribution in 3.3.3

revise the systematic error contribution of $\varepsilon_{reg}$:

Do not use error values on CRs. Statement above.

Do not apply IQR: The process does not cut outliers but reduces the systematic error component to its lower half. You responded: 'The IQR was chosen instead of standard deviation because the distribution is not Gaussian' → If DEMs are hor. and vert. coregistered, the residual height error should be normally distributed.

General comment: 3.2 Impact of radar penetration

The section should concentrate on /emphasize a conclusion on the extensively described distribution of σ0. In the end, a mask with σ0 thresholds as a function of elevation, look angle incidence angle, date/time etc. would be precious. It would enable applicability of this new method for future studies. At the moment the procedure leaves the reader with the barrier 'done by expert knowledge' and no chance to use or reproduce the method. If changing that is out of scope, at least the numeric outcome of the method should be supported by more than one resulting number which up to now is limited to the final $\varepsilon_{pen}$ in the supplement.
So far, the end of 3.2.1 reads (p11ll7): 'The offsets are based on empirical observations of the relationship between σ0 and height offset performed on multiseasonal TDM Raw DEMs of NPI, showing a mean penetration bias of 4 m for an increase of σ0 by 10 dB from wet to dry snow (Abdel Jaber, 2016).'

Specific comment: 3.2 Impact of radar penetration

Please, add more precision to the cited paragraph: what exactly was applied then? A 4 m mean penetration depth assumed for all masked areas? Or if not, how is it calculated/modelled? Moreover, the description in p13ll15 stays vague, except for 6 m as a maximum.

Provide a conclusion about applied thresholds (σ0 etc.) in the text at the end of 3.2.2 similar to the cited one.

Provide numeric values for the assumed (mean?) penetration depth at each location in Fig. S5 referring to the manually outlined areas they are applied to.

Re: Specific Comment: P 7 ll30

> What kind of filtering was applied? It would be interesting to see the original dataset and a Δh map outside the icefields.
>
> Response: Since the Summer 2011/2012 daily SECR is used only for the seasonal correction, we applied the following procedure for eliminating outliers: (i) conservative masking on glaciated terrain of regions with high backscattering and peaks in the daily SECR values followed by (ii) 2-step filtering with sliding 15window: (a) median filters with kernel size 9 and (b) smoothing with kernel size 9. The raster posting is 0.4 arcsec. This way the localized seasonal changes or outliers were eliminated and thus the SECR map can be used for the purpose of compensating the temporal gap in 2015/2016.

→ A shortened description would complement the caption of Fig8. Please consider reproducibility, not only answering to me.

Re: Specific Comment: P7 ll13 (update p7 ll3)

> How is the absence of horizontal shifts checked? The detection is slope dependent (cf.Nuth and Kääb (2011)), thus cannot be efficiently performed on an area without slope as the CRs (avr. slope below 4)
>
> Response: The horizontal shifts (in our case possibly acting in the ground range direction) were not checked analytically directly on our datasets, but relying on visual analysis of all available off-glacier terrain. Analytical checks using the method of Nuth and Kääb (2011) was done for the TDM-SRTM SEC datasets during the preparation of the thesis (Abdel Jaber et al., 2016) corroborating the validity of this calibration procedure. Because the same method was applied for this paper as for the thesis, the conclusions regarding this procedure can be adopted for this work.

→ Concerning the last two sentences, the thesis reads:

> 'The horizontal coregistration with respect to the SRTM appears to be achieved as well: the difference images do not display a hillshade effect (see Figure 7.9) and the method of Nuth & Kääb (2011) confirmed the validity of the correction. The quality of the coregistration was furthermore confirmed by comparing on stable terrain (ice-free) the corrected TanDEM-X Raw DEMs and the SRTM C-band DEM to several ICESat GLAS altimetry tracks acquired between 2003 and 2009. Nevertheless a certain amount of residual horizontal misregistration can be expected, given the uncertainty linked to the height offset estimation. Its contribution will be accounted in the mass balance error budget'(Abdel Jaber 2016, p 91)

There is no statistical / numerical evidence referenced, but a Figure to check the absence of aspect dependent slope offsets visually. It is due to such a response, that I must insist in you taking the suggestion seriously that I repeat here: Put more effort to divide the thesis and the paper, to make latter a scientific stand-alone document. For this specific case: Even if the method is applicable, please show this, by validating your data independently as suggested in >Comments on 3.1.2 Coregistration of DEMs and following error contribution in 3.3.3<.

Re: Comment: P9 ll8

> Can you please add more information to increase reproducibility when data gets available: what threshold on SEC values? What morphological operators?

Response: We did not include these details because we do not think that this is an interesting point and would inflate an already very long paper. For each of the 4 SECR maps we produced a raster starting from the flag mask (FLM) layer that resulted from the processing with ITP which provides roughly the regions affected by layover and shadow. Thresholds h/ t < -10 m/a and > +6 m/a were applied. A morphological operator of closing followed by a 5 x 5 median filter was applied on the mask raster in order to "clean" the mask, avoiding noise due to thresholding.

→ Eliminating the phrase from the manuscript does not support transparency.

Please integrate answer in manuscript /supplement. Especially the asymmetrical thresholding is interesting. Please provide reasoning for that. A symmetrical cut-off for outlier elimination would be methodically sounder. Executed like provided, statistically appearing (therefore normally distributed) residual noise error resulting in higher/lower rates gets dragged to more negative rates.

Re: comment: P13 ll1

According to this paragraph: for interpolated seasonal correction, the last epsilon term should dominate the quadrature sum and thus the total SECR error, if I understand correctly. What does 'increase by a factor of three' mean in this context? Times 3 (*3) ? I compared SECR uncertainty value for extrapolated glaciers (e.g. Jorge Montt, Bernardo, Tempano) in Tab. 3 with values for not extrapolated glaciers. First ones are not near triple of latter. And they should even be higher than triple, following this paragraph: scaling by year (divided by 0.27. for 99 days for example) is performed as well as a *1.5 increase for the timespan difference. Please explain where I've gone wrong and/or revise the explanations in this paragraph.

Response: Thanks for pointing this out. It seems there is some misunderstanding regarding the seasonal correction and its impact for the retrieval of SECR. The term seasonal correction refers to the difference between mean annual SECR over epochs spanning 12 years (2000 to 2012) and 4 years (2012 to 2016) without accounting for seasonal differences in SEC of the missing days vs. the mean annual SECR taking seasonal differences into account. For the extrapolated glaciers 53 to 103 summer days are missing in order to cover the full 4 year period (1461 days). This means that the missing days to be substituted correspond to 3.6 % to 7.0 % of the 4 year period for which the mean SECR is computed (and not 27 % which would refer to a single year). The impact of missing days to be substituted for the 12 year period I still much smaller. This is now made clear in the revised section 3.1.3 and in Supplement S4.

→ Thank you for correcting my wrong assumption. It really helped understanding the seasonal correction – as well as the revision of the respective section. Yet, it also emphasized the relatively small impact of the correction to the results. After reviewing the paragraph of the related error estimation again, I now understood correctly what has been done and am sorry about misconceptions. Still I suggest a little step to improve the manuscript.

The paragraph p13ll26 reads:

To compute the systematic error linked to the seasonal correction (Sect. 3.1.3), the previous three systematic error components (ε reg , ε pen and ε add ) were estimated separately for the summer 2011/2012 SECR. Here ε add was increased by a factor of 1.5 to account for the different temporal coverage. All three components were summed in quadrature and conservatively further increased by

a factor of 3.0 on extrapolated regions (north of SPI and NPI). A pixelwise scaling by the number of corrected days and by the appropriate Δt in years was applied, leading to a fourth systematic error

The calculation in the cited paragraph is:

(1) $\varepsilon seas\ (x, y) = sqrt(\varepsilon reg^2 + \varepsilon pen^2 + (1.5*\varepsilon add)^2\ ) *days)/\Delta t$
(2) $\varepsilon seas\ (x, y) = sqrt(\varepsilon reg^2 + \varepsilon pen^2 + (1.5*\varepsilon add)^2\ )*3 *days)/\Delta t$ (for extrapolated regions)

I assume. since you involved this error contribution from a seasonal yearly change rate scaled to the dh error, in this case the Δt=365days. This practice is fine, and I misunderstood it in the first place. That might be, due to the fact, that the phrasing in the last cited sentence indicates, that here a division by entire epoch would appear (Δt=4*356 d or 12*365 d respectively). Latter would be questionable, not representing what has been done, since you subsequently quadrature sum the epsilon components as

(3) $\varepsilon = sqrt(\varepsilon reg^2 + \varepsilon pen^2 + \varepsilon add^2 + \varepsilon seas^2)/\Delta t$

where the division by entire epoch appears as Δt to give an appropriate error budget for the SECR.

→ Please correct 'and by the appropriate Δt in years' to a phrasing that clarifies Δt to be 365d in this case.

Formal comments:

P5 l16: 'attitude adjustment'