

# Interactive comment 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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## Authors' response to Anonymous Referee #2

Referee comments are shown in **black**, our response in **blue**. Line numbers refer to the manuscript version (pdf) of 23 November 2018.

### General comments:

**Comment:** The study presented by Wael Abdel Jaber and co-authors is an overview of surface elevation change rate (SECR) and geodetic mass balance (MB) values for the Southern Patagonia Icefield (SPI) and Northern Patagonia Icefield (NPI) for the two epochs 2000-2012 and 2012-2016. The results are calculated on the entire icefield as well as on glacier basis, mean SECR and volume change rates (VCR) are listed in a table including observed area and error budget. For most important glaciers the hypsometric distribution of those variables is depicted in graphs. The study provides a detailed description of the error analysis and several steps to correct for biases and penetration and ablation uncertainties. The language is correct and understandable. The subject is of high interest to the community, the method and study areas are not completely novel. In the last years, there have been publications covering the study area with the same topic (Foresta, Dussailant, Malz, Abdel Jaber himself), but partly using different approaches. This new study cites and discusses those adequately. I recommend to add the recent work of Braun et al. (2019) which also includes SPI and NPI, but only covers the first observation period (2000-2011/15). The authors point out two aspect as main progress to previous studies: 1) The comprehensive and simultaneous observation of both icefields at two epochs. 2) The variety of corrections and assumptions made to guarantee a precise observation of SECR and following products. The line of argumentation is clear as far as (1) is concerned and thus I support publication in TC. Nevertheless, concerning (2), revisions should be performed to significantly improve the traceability of results and assure the validity of some of the applied steps described in the method section before publication.

**Response:** We thank the anonymous referee for the detailed review and the appreciation of our work. Although similar studies were published already we want to point out the main novelties of our manuscript. We provide the first geodetic mass balance for NPI and SPI also for a recent epoch (2012-2016) by TanDEM-X DEM differencing. Besides the entire icefields we give average SECR and VCR (incl. error) for individual glacier basins (up to 9km<sup>2</sup> on SPI and 2km<sup>2</sup> on NPI) and hypsometric plots of main glaciers (incl. error bars). We used the same method as for the preceding epoch (2000-2012) and this

allows the comparison of individual glacier and icefield behavior in the two epochs. Also we present an up to now unique analysis of the backscatter coefficients of all SAR acquisitions (SRTM and TanDEM-X) to assess the error due to signal penetration, a known issue when using InSAR based DEMs. Abdel Jaber et al. (2016) is a doctoral thesis. It has not been published in any scientific journal, neither in its entirety nor any part of it, but it is available online to everybody. The thesis, reporting many details on the methods used for SECR and VCR, provides also the basis for the technical approach applied in this paper. This review asks for many details on techniques for TanDEM-X DEM differencing and retrieval of SECR which may be relevant for a technical paper on DEM differencing, but is not the scope of our paper. We want to point out that the methods section on the current version of the paper exceeds in terms of information content previous papers on SPI and NPI retrievals of volume change. We provide in this response information on specific technical issues raised by the referee.

Thanks for pointing to the recently published paper by Braun et al. (2019). We will make reference to this paper. Regarding DEM differencing SRTM-TanDEM-X, for SPI, the numbers seem to be based on Malz et al. (2018). The reported number for NPI 2000 - 2011/2015 lacks traceability regarding the TanDEM-X data used and processing methods so that in depth comparison with our results is not possible.

**Comment:** Methods: The utilization of several thresholds or distinct values is not always transparently explained. At some decisive points, it remains vague if the method or decision follows own reasoning, own previous work or an external reference (cf. specific comments)

**Response:** We thank the referee for pointing out this aspect. We provide relevant information in the response to specific comments.

**Comment:** The correction for the observation date in epoch 2, for not being at the end of ablation period, is an unprecedented venture. However, it forms also a weak point of the study. In the reviewer's opinion, the error induced to the SECR (Epoch 2 – Epoch1) by this step is not adequately represented by the mapped datasets nor is it transparently addressed as error contribution in the text. Moreover, an interpolation of missing areas based on only two weather stations and adjusted to sparse hypsometrical patterns has to be regarded rather experimental compared to the robust methodology used for the rest of the study. It is hard to judge the validity of the seasonal correction. A  $\Delta h$  map outside the icefields and the unfiltered dataset  $\Delta h$  could help justifying, at least for the observed parts(cf. specific comments)

**Response:** We are aware that the correction applied for the missing days in the ablation season to complete the 4 years period of epoch 2 has some limitations. On the other hand performing such a correction for the short period is fundamental for obtaining reliable annual SECR for comparisons with other results. This reduces a possible bias compared to the case without seasonal correction. For the details of the procedure please refer to our response to referee #1, (AC#1, pages 3-5). The description in Section 3.1.3 of the paper will be improved by reformulating the text and adding the requested  $\Delta h$  maps in the Supplement if deemed to be necessary.

**Comment:** The error indicated for SECR is spectacularly low in this paper. Although there is a section explaining the calculation it is not totally clear, why a DEM comparison could come up with such low elevation error budget. It appears, the systematic error budget, as the main contributor, is calculated partly in favor of a small total error. Some steps along this path should be under discussion or described in more detail for traceability (cf. specific comments).

**Response:** The errors of SECR and of VCR (Table 1) are comparable to most of the recent results obtained by other authors based on elevation change approach (see Table S10). Therefore we do not understand the reason for the reviewer's statements "spectacularly low" error and "the systematic error is calculated in favor of a small total error". Furthermore, the agreement between our volume change rate 2000 to 2012 over NPI and the results of Dussaillant et al. (2018) is well within the combined error bound. This supports the validity of our error estimate, as the results of Dussaillant et al. are based on completely different data sets and methods. As suggested, we will provide further clarification on the error estimate in the revised manuscript and Supplement.

**Comment:** Structure: The work is based on the PhD thesis of Abdel Jaber (2016). However, since it is sometimes difficult to follow what is actually new in contrast to what was already in place, that presents the reader with challenges. A clear line between parts that were newly implemented and those that were adopted needs to be drawn by the authors. I recommend that the authors revise the methods and result section with regard to this aspect to make the paper a full stand-alone document. This also concerns the length of some descriptions that could be kept more concise for this paper, with reference to the thesis (or other original source).

**Response:** The referee #1 had a similar concern which we answered in our response (AC#1 page 5). We will revise the two sections of the manuscript in this respect.

#### **Specific comments:**

**Comment:** P 6 l27 ...(in order of impact, the latter being negligible in our Raw DEMs)." This and further statements could be corroborated by a similar Figure as Fig S 2 for SRTMTDM, displaying same  $\Delta h$  for outside the icefields for SRTM-TDM(Ep1) and TDM(Ep1)- TDM(Ep2).

**Response:** Off glacier SECR close to some termini were kept in the detailed maps shown in Figures 2 - 7 for this purpose. We do not see any urgent need for adding these 4 figures (on a marginal topic) as it would inflate the already extensive size of the paper. Even so, we can add these figures in the Supplement if deemed to be necessary.

**Comment:** P 7 ll3 -13 The weighted averaging of the offset values leaves the question if a spatial pattern was analysed and fitted by an offset function. A simple averaging could lead to regional maladjustment, if the sign / magnitude of the offset is a function of geographic position (tilted dataset, described in this manuscript p6 l20). For the precision of the applied method a mapped  $\Delta h$  (cf. comment to P6 ll26) could be convincing.

**Response:** As mentioned above we can include the 4 figures in the Supplement if deemed necessary.

**Comment:** P7 ll13 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.

**Comment:** p7 ll23 Please provide reference

**Response:** Rivera (2004) Fig. 6.3 shows in January clearly higher density in the upper metres of snowpits in the accumulation area of Chico glacier, compared to density in September and October. We will add the reference:

Rivera, A.: Mass balance investigations at Glaciar Chico, Southern Patagonia Icefield, Chile, Ph.D. thesis, Univ. of Bristol, UK, 2004.

**Comment:** P 7 ll30 What kind of filtering was applied? It would be interesting to see the original dataset and a  $\Delta h$  map outside the icefields.

**Response:** Since the Summer 2011/2012 daily SECR is not the main result of this study and it is used only for the seasonal correction, we applied: (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 window: (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.

**Comment:** P8 ll10 What does similar mean here?  $\pm 0.1^\circ\text{C}$ ? Please add a number for consistency.

**Response:** The 2 stations data we used are confirmed by ERA Interim temperature data (see also response AC#1 page 3) which provide even higher agreement between the summer epochs. According to ERA Interim the average temperatures of summer 2011/2012 and summer 2015/2016 agree within  $0.1^\circ\text{C}$ . This means that the SECR maps of summer 2011/2012 (scaled to the length of the missing period) can well be used as substitute for the missing days in summer 2015/2016. We will add this info in this paragraph.

**Comment:** P7 ll32 -p8 37 A comprehensive series of comments concerning the temperature variability and spatially variable ablation patterns resulting in a rather speculative adjustment in the seasonal correction section is given by referee #1. I agree on those.

**Response:** Please see the detailed response to referee #1 in AC#1 pages 3 - 5

**Comment:** P8 ll28-32 Please explain the justification of 20% reduction in correlation to a temperature value. Based on what assumption does it translate into a percentage?

**Response:** For epoch 1, although the daily SECR is from the same year (ablation season 2011/2012) and was obtained from December to March, the days which have to be compensated are in late summer and therefore we reduced the estimate for ablation by 20 % compared to the summer average. As we mentioned in the manuscript, this scaling factor is based on a time series of daily air temperature

measurements from 1995 to 2003 near the front of Perito Moreno Glacier (Stuefer et al., 2007). Furthermore, we want to point out that this correction factor applied on the hypsometric curve in Fig S3 affects only a very small area.

**Comment:** P9 II8 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  $\Delta h/\Delta t < -10$  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.

**Comment:** P9 II16-19 Where are the 17 kg m<sup>-3</sup> uncertainty taken from? Citation of Cogley et al. (2009) is misleading here, because reader would expect a reference for the density uncertainty. I found it to be mentioned in Abdel Jaber (2016), but it seems to be taken from Gardner et al. (2012) – this is not referenced here. Anyway, why using this value when recent large area studies like Brun et al. (2017), Dussaillant et al. (2018), Malz et al. (2018) use 60 kg m<sup>-3</sup>? Choosing that latter value would lead to comparable error budget.

**Response:** Yes, the uncertainty  $\pm 17$  kg m<sup>-3</sup> comes from (Gardner et al, 2012). Regarding this issue, we want to mention again the statement in response to Referee 1, that the main scope of this study is to provide volume change rate estimates at basin scale (Tables 2, 3, S8). These can then be converted to mass change rates using a constant density assumption (which provides full traceability). Furthermore we wanted to provide a reference mass change rate at an icefield level; these are the only four numbers in the paper reporting mass change estimates (Table 1). We decided to use the common scenario of glacier-wide density of 900 kg m<sup>-3</sup> facilitating the comparison of results with other studies. We agree that the assigned error of 17 kg m<sup>-3</sup> is a small one (1.8%) for firn areas. However, the main mass losses on the Patagonian icefields refer to ice areas. For the accumulation areas assumptions on changes of the vertical profiles of snow/ice density would be speculative. The changes in SEC on the firn plateau are small, suggesting that an error value of 60 kg km<sup>-3</sup> is probably an overestimation. In case, we can use different bulk estimates for uncertainty of density in the firn and ice areas in order to revise the error estimates for the icefield wide mass changes in Table 1.

**Comment:** P9 II 31 Please provide reference

**Response:** Following reference will be added:

DLR-CAF. 2013 (October). TerraSAR-X Ground Segment Basic Product Specification Document. 1.9 edn. German Aerospace Center (DLR) - Cluster Applied Remote Sensing (CAF). TX-GS-DD-3302.

**Comment:** p10 II21-24 and P11 II8 Why manual outlines? What is the decision to delimit these areas based on? If that information can be found in Abdel Jaber (2016) it should be indicated (or the original study it refers to).

**Response:** The outlining was performed manually based on the backscatter coefficient ( $\sigma^0$ ) and taking also into account the corresponding elevation to ensure that areas of high backscattering are on the smooth firm plateau and not in the ice areas. A fixed thresholding of the  $\sigma^0$  layers would have added noise because this would include regions of rough ice. The penetration height offsets assigned to each region are based on the relation between difference in  $\sigma^0$  for dry and wet snow and  $\Delta h$  (Figure 8.12 of Abdel Jaber (2016), Sect. 8.4).

**Comment:** P11 ll3-7 Is any of the values mentioned in these paragraphs used for determining the outlines? What is the interpretation of the sigma0 ranges based on? Abdel Jaber (2016) / other? Please reference it.

**Response:** The  $\sigma^0$ -values and related interpretation are based on multi-year experimental and theoretical work on X-band and C-band radar signal interaction with snow and ice by two of the co-authors, including several field campaigns on Alpine glaciers related to ERS-1/ERS-2, ASAR of Envisat, Shuttle Radar SIR-C/ X-SAR SRL-1 and -2, and TerraSAR-X. See e.g. Nagler and Rott (2000); Floricioiu and Rott (2001). Concerning the SRTM data (Sect.3.2.2), to which this comment refers, such an analysis was already performed in (Abdel Jaber, 2016) and the processing was not repeated for his study. Only the analysis of TDM backscatter (Sect. 3.2.1) is new because the data used in this study have not been used in Abdel Jaber (2016).

Floricioiu D. and Rott, H.: Seasonal and short-term variability of multifrequency, polarimetric radar backscatter of alpine terrain from SIR-C/X-SAR and AIRSAR data. IEEE Trans. Geosc. Rem. Sens., Vol.39(12), 2634 –2648, 2001.

Nagler T. and Rott, H.: Retrieval of wet snow by means of multitemporal SAR data. IEEE Trans. Geosc. Rem. Sens., Vol 38(2), 754-76, 2000.

**Comment:** P11ll 17 First sentence would be well supported by a formula. Is the HEM for the TDM elevations calculated by the phase difference to the interferometric phase of 12 m TDM products? Is it always TDM 12m as a reference (also in 3.3.3 (1))? It is mentioned once briefly in 2.1, but I think I should be emphasised there, that it is especially used as reference for elevation error assessment.

**Response:** We can add the formula, even if trivial and therefore in our opinion not necessary. The HEM does not depend on the reference DEM (the global TDM DEM) but it is processed by ITP for each TDM RAW DEM. The HEM is only the interferometric error and reflects point per point the actual error. This is the alternative of computing it over ice free terrain, as other authors did. This error is used to compute the random error of each sample. When averaging on an elevation bin this error becomes anyway negligible compared to the systematic components.

In 2.1 (p 4 lines 13-14) we state only that the global TDM DEM is used as reference for the processing, the details of how this is used are given in 3.1.1. It is also used for the DEM coregistration (see sect. 3.1.2 p 6 lines 24-25). We never mention that we used the global TDM DEM in error calculation.

**Comment:** P11 ll25: How was it included? Add some mathematical explanation of the error propagation through seasonal correction. Is it  $\sqrt{\sigma_1^2 + \sigma_2^2 + \sigma_{\text{seas}}^2}$  for each pixel?

**Response:** We can add the formula, even if trivial. However, in our view we should rather not increase the paper length by adding such material.

**Comment:** P12 ll14 Enhance precise and illustrative explanation to this whole section 3.3.3. The reader is interested how exactly the systematic error is calculated, for it is key to the low elevation error budget presented. Please provide formulas to enhance comprehensibility. That could spare some explanatory text passages, that are less illustrative.

**Response:** We will revise this section accordingly without inflating it too much.

**Comment:** p 12 ll14 Is the IQR of the areas that were adjusted (CR, calibration) addressed as the measure of error (validation) on each DEM? I do not agree with this method from a scientific perspective. On top, choosing the IQR reduces or eliminate slope dependent effects (avr. Slope below 4°, IQR slope?). But on glacier these are present for sure, so they are a source of systematic error to be addressed in the budget. It would be more reliable if validation is performed on the entire DEM (glaciers excluded), but assessed with regard to absolute elevation and slope.

**Response:** Assuming the comment concerns p 12 ll17-20.

The IQR is chosen to characterize the spread of the elevation difference between the TDM Raw DEM and the global TDM DEM within each scene over calibration regions covered by that scene. Large difference means larger uncertainty of the DEM calibration. The IQR was chosen instead of standard deviation because the distribution is not Gaussian. A tilt of a specific Raw DEM was excluded through the comparison to the reference global TDM DEM. If still present it would lead to an increase of the spread, since the calibration regions (visible in Figure S1) are relatively well distributed geographically.

Slope dependent elevation offsets are caused by horizontal shifts between the two DEMs, which we assumed to be negligible compared to vertical ones. The glaciers are rather flat, except on the small regions of the mountain ranges sticking out of the plateau. Therefore, the combination of small shifts and small slopes would make these effects negligible. We focus on vertical offsets. Evaluating on the entire off-glacier surface would have been another possibility. But this would have been biased by the higher slopes of the off glacier mountains of Patagonia and would lead to a larger error than what we found on the glaciers which have low and moderate slopes. Since this error is linked to the calibration procedure we compute it on the calibration regions themselves.

**Comment:** P12 ll21 Why 1 – 6 m? Reference, calculation or explanation for decision should be provided. Where does that assumption 1000 m.a.s.l come from? Please provide reference.

**Response:** The penetration height offsets of 1 to 6 m were assigned based on explanations given in Sections 3.2.1 and 3.2.2 and in (Abdel Jaber, 2016) and are referenced at line P12 L22. See response above. We assign the mentioned systematic error only to altitudes above 1000 m to compensate for undetected regions on the plateau and not on the rough ice of the glacier termini.

Below 1000 m a.s.l. are ice areas where the C-and X-band radar signal does not penetrate. For the SRTM data set (C-band) penetration is no issue because the snow and ice surfaces were wet during the SRTM



mission. Regarding the TanDEM-X data, only a quite small percentage of the total data set exhibited partly frozen or dry snow. As mentioned in the response to P11, line 8, the penetration height offsets for completely dry snow and firn are based on the relation between  $\sigma^\circ$  for dry and wet snow and  $\Delta h$  in Figure 8.12 of (Abdel Jaber, 2016) showing for dry snow a mean offset of 4 m and a maximum offset of 6 m. This is in agreement with the number on X-band one-way signal penetration for dry snow reported by Mätzler (1987) if converted into two-way penetration and accounting for oblique view. The good agreement between our volume change rates 2000 to 2012 over NPI and the results of Dussaillant et al. (2018) based on optical data confirms the validity of our approach regarding signal penetration.

Mätzler, C.; Applications of the interaction of microwaves with the natural snow cover, *Remote Sensing Review*, 2, 259-387, 1987.

**Comment:** P13 II1 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 is still much smaller. Our response to the general comments of Referee 1 includes detailed explanations on the seasonal correction. We will revise section 3.1.3 (seasonal correction) accordingly in order to provide better explanations. We will check the uncertainty estimates for the individual glaciers in order to be sure that the number of missing days has been properly taken into account.

**Comment:** P13 II9 A formula containing the total SECR error would be helpful for traceability. Is it  $\delta\text{SECR} = \sqrt{(\epsilon b^2 + \text{SE}^2)}$ . Just to make sure I got the method correctly and the comment above (II1) is justified.

**Response:** We can add the formula, even if trivial.

**Comment:** P13 II15 I would assume a factor of 3 to be very low for the icefields concerning a factor 5 was applied e.g. by Brun et al. 2017 in High Mountain Asia, whereas the variability of SECR patterns in the icefields (especially SPI) is rather high.

**Response:** We provided information on this issue in the response to Referee 1 (referring to his comment on page 13), and detailed information on the estimation of the mass balance gradient and its seasonal



ratio in the Appendix which is included in that response. The correction for the (few) glaciers with gaps during summer accounts for the seasonal difference of the mass balance gradient (ratio summer versus full year). The material presented in the Appendix shows that possible differences of this ratio between individual glaciers are rather small so that an increase of the error by a factor of three is a rather conservative estimate. We also want to point out that this factor does not have a large impact on the VCR rates because the extrapolation refers to a limited subset of the total data sets in respect to area and time span.

**Comment:** P13 ll19 A formula for the complete error propagation throughout mass balance computation would be appropriate.

**Response:** We think that a complete error formula is redundant. If deemed necessary, we can include a formula for the various error components in the Supplement.

**Comment:** P15 ll19 The processes described should be perfectly correct. However, I doubt the values found through the seasonal correction analysis are able to significantly support this interpretation. As mentioned, I assume this daily SECR as a study design feature hard to accept. Also, a precise description of the method that smoothed the SECR field in Fig 8 would be of interest– or even better a display of the original data (SECR field). If it is clearly shown, that the process introduces more precision to the data, than it introduces measurement/ interpolation uncertainty (also regarding comments to 3.3.3) I am willing to accept it. So far, I find it difficult to support it.

**Response:** This comment is related to the issue addressed above (referring to page 13, line 15). As mentioned in the response there, further details on the seasonal correction are included in the response to Referee 1. We will provide further information on the correction procedure in the revised manuscript and/or in the Supplement.

**Comment:** P15 ll28 For the subaqueous loss Abdel Jaber (2016) is referenced. But for the basal cross-sections an original source should be cited.

**Response:** The references for the bathymetric data on the four mentioned glaciers (Upsala, Jorge Montt, Tyndall and Ameghino) will be added. The calving cross sections of these glaciers are deduced from bathymetric data in front of the glaciers and the freeboard. References to the bathymetric data:

Ameghino Glacier: Stuefer, M: Investigations on mass balance and dynamics of Moreno Glacier based on field measurements and satellite imagery, PhD Thesis, Univ. Innsbruck, Austria, 1999. The thesis reports also on two field campaigns on Ameghino Glacier, including pre-frontal bathymetric measurements.

Jorge Montt Glacier: Rivera, A., Koppes, M., Bravo, C. and Aravena, J.C.: Little Ice Age advance and retreat of Glaciar Jorge Montt, Chilean Patagonia. *Clim. Past*, 8, 403–414, 2012.

Tyndall Glacier: Raymond, C., Neumann, T., Rignot, E., Echelmeyer, K., Rivera A., and Casassa, G.: Retreat of Tyndall Glacier, Patagonia, over the last half century, *J. Glaciol.*, 51(173), 239-247, 2005.

Upsala Glacier: Sugiyama, S., Minowa, M., Sakakibara, D., Skvarca, P. et al.: Thermal structure of proglacial lakes in Patagonia, J. Geophys. Res. Earth Surf., 121, 2270 – 2286, doi:10.1002/2016JF00408, 2016.

**Comment:** P18 ll 9 It is unclear here if that paragraph refers to previous work (Abdel Jaber 2016) or a different publication. Any citation would help. Also I would suggest a reference to the Figures displaying those datasets (provided in the supplement if it is own work)

**Response:** A similar remark was made by Referee #1. The analysis refers to ice flow velocity results external to this study and based on TerraSAR-X. We will reformulate this. Within the ESA project SAMBA (mentioned in the Acknowledgements of the paper) we generated digital maps of ice velocities of SPI and SPI derived from TerraSAR-X and Sentinel-1 data, including time series. Parts of this data set are available to the public at <http://cryoportale.enevo.at>, but we did not yet have time to write this up for a publication. In Section 5.1 we report some numbers out of these results, in support remarks of the discussion. In principle we can add one or two figures on velocities, preferably in the main paper because these are original results.

#### **Technical Corrections:**

**Comment:** p4 ll 1 'Method and error estimation'

**Response:** Not clear what is wrong here.

**Comment:** P7 ll 1 Check formula. This way it says  $\delta_{\text{hoff}}$  is equal  $\delta_{\text{hoff}}$  times the factor.

**Response:** Assuming p7 ll 11: "." is not a multiplication but a punctuation mark. We will avoid starting the sentence with  $\delta_{\text{hoff}}$ .

**Comment:** Also the distinction, when formulas are a) formatted as objects to be numbered b) written as part of continuous text c) omitted, but have a text description instead is not clearly structured. This should be reconsidered thoroughly.

**Response:** We will improve the manuscript on this issue.

**Comment:** P14 ll 'Figs' Fig. /Figure consistency Check throughout the text, also Table /Tab.

**Response:** We will check the Fig/Figs vs Figure. "Tab." does not occur.