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Interactive comment on "Heterogeneous spatial and temporal pattern of surface elevation change and mass balance of the Patagonian icefields between 2000 and 2016" *by* Wael Abdel Jaber et al.

Anonymous Referee #2

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General Comments: 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, Dussaillant, 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. 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) 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 Δh map outside the icefields and the unfiltered dataset Δh could help justifying, at least for the observed parts(cf. specific comments) 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). 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).

Specific comments:

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 SRTM-TDM, displaying same Δh for outside the icefields for SRTM-TDM(Ep1) and TDM(Ep1)-TDM(Ep2).

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 Δh (cf. comment to P6 ll26) could be convincing.

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°).

p7 ll23 Please provide reference

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.

P8 ll10 What does similar mean here? +-0°C? Please add a number for consistency.

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.

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?

P9 ll8 Can you please add more information to increase reproducibility when data gets available: what threshold on SEC values? What morphological operators?

P9 ll16-19 Where are the 17 kg m-^{3} 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-3? Choosing that latter value would lead to comparable error budget.

P9 ll 31 Please provide reference

p10 ll21-24 and P11 ll8 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 referes to).

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.

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.

P11 ll25: How was it included? Add some mathematical explanation of the error propagation through seasonal correction. Is it sqrt(σt1 ^{2} + σt2 ^{2} +σseas ^{2}) for each pixel?

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.

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.

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.

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.

P13 ll9 A formula containing the total SECR error would be helpful for traceability. Is it δSECR = sqrt(εb^2 + SE^2). Just to make sure I got the method correctly and the

comment above (ll1) is justified.

P13 ll15 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.

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

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.

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

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)

Technical Corrections:

p4 l1 'Method and error estimation'

P7 l1 Check formula. This way it says δhoff is equal δhoff times the factor.

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.

P14 l 'Figs' Fig. /Figure consistency Check throughout the text, also Table /Tab.

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