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Discussions

Interactive comment on "Four decades of surface elevation change of the Antarctic Ice Sheet from multi-mission satellite altimetry" *by* Ludwig Schröder et al.

Anonymous Referee #1

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Summary The topic of this paper, surface elevation change in Antarctica, is extremely interesting given that altimetry is the only dataset available that can provide a continuous, continent wide record of long term ice sheet change. While elevation change measurements from individual satellites such as CryoSat-2, ICESat and Envisat already exist in the published literature, the authors present new results from previously unexplored data acquired by Seasat and Geosat in the 70's and 80's which should provide an exiting new insight. This paper fails to deliver on all counts. The conclusions state that this paper presents a method for combining multiple satellite datasets. However, the method employed is not well described either in terms of the retracking, the elevation change processing chain or the post processing data filtering, extrapolation

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Discussion paper

and smoothing. On top of this, the authors haven't taken the time to calculate an error estimate for the altimetry data, and consequently all plots and maps are presented without this essential component. The elevation change dataset is presented without any serious attempt to validate any of the results. While I acknowledge that it will be difficult to find auxiliary data to validate the earliest two satellites, there is a wealth of airborne data acquired in Antarctica over recent decades that could, but hasn't been used. Instead the authors have chosen to validate a flat, unchanging region of east Antarctica with little more than one track of data. This region is not representative of the regions experiencing the most rapid change in Antarctica, therefore don't provide any confidence in the results presented in arguably the most important regions. I have concerns about the weak attribution of the cause of change throughout the paper, and the overly simplistic nature of the analysis and discussion on this topic. The paper presents only elevation and volume change, but these signals are then attributed to ice dynamics and precipitation anomalies with no supporting evidence for the former, and largely no quantitative evidence about the relationship for the later. No previously unknown dynamic or precipitation induced elevation change signal is reported on, so we do not learn anything new from this paper about why changes in ice thickness have occurred in Antarctica. On top of these technical issues, the manuscript text is lacking numbers to back up most of the statements made. For example, the abstract and conclusions contain no key numbers on elevation change in Antarctica, despite this being the title of the paper. In addition to this quite a lot of the text is poorly written, so the text will be significantly improved if thoroughly re-edited. My criticisms of the methods and results presented in the paper are major, and will be time consuming to properly address. As it stands, ambiguity about the methods, absence of an error estimate, and lack of proper validation of the result make it entirely possible that the magnitude of the elevation change signal may not be correctly reported in this paper. As the final closing statement of the abstract and the conclusions state, the single new achievement of this paper is that it has demonstrated that Seasat and Geosat 'can provide reliable information'. This is not a scientific conclusion on how elevation change in Antarctica has changed since the 70's as the papers title implies. If the authors can rewrite the paper to provide geophysical results and quantitative conclusions on this topic, then this forms the basis of an interesting paper, but as it stands this manuscript feels like an unfinished piece of work. The following specific edits should be addressed by the author if the paper is to be published. Specific Edits Abstract – The abstract is quite long, but not well written. About half of the text is spent discussing the percent coverage for the 25 year and 40 year epochs, however these numbers aren't key scientific results so would be better placed in the data or methods section. Additionally, the coverage stats are poorly defined here, for example is this the percent coverage that of the raw data, plane fit output at whatever grid resolution used, or the extrapolated interpolated result. Without being specific the coverage stats are open to misinterpretation. Abstract - The title of the paper states that this paper is about surface elevation change, but there are no key surface elevation change numbers stated in the abstract. Why not, if this is the main purpose of the paper? Abstract – The second paragraph of the abstract lacks any quantitative facts. For example, Pg1 L14 states that surface elevation change shows 'high coincidence' with precipitation anomalies and gravimetry; Pg1 L16 states that there is a 'high level' of agreement; and Pg1 L18 states that 'Geosat coincides very well with...'. The authors should replace these generic adjectives with quantitative statistics to back up their statements. Abstract – Pg1 L15 – 'Satellite gravimetry' is the technique, but the authors have presumably compared their elevation results against a derived product, such as mass loss. Edit wording to be precise. Abstract – Pg1 L18 – The Seasat and Geosat altimeters operated at radar frequencies and will consequently penetrate some depth into the snowpack, its therefore not a given that the elevation trend from these satellites should correlate with precipitation anomalies in snowfall. The penetration depth is spatially and temporally variable, influenced by snow density and moisture content. As the radar return originates from a scattering horizon within the snowpack, correlation between precipitation anomalies and rates of elevation change can't prove that the elevation change trends are 'reliable', so the satellite based elevation trends must be verified with a comparable elevation change dataset. P1 L21 – Sentence wording not correct English, edit required. The wording throughout this paragraph is poor. P1 L22 – Does the author mean sequentially rather than concurrently? P2 L6 – Edit wording to say precisely what is meant. A long time series would not have prevented Wingham observing negative elevation rates in Dronning Maud Land compared with Flaments positive result for the same area, because as stated the observational time period is different. 'Help reduce the influence of such events' is factually incorrect. P2 L10 – Edit wording. Mission calibration doesn't become 'more important', it is maybe more challenging though. P2, Fig1 – Add separate colorbar for map of spatial coverage as currently hard to interpret. Just the circle outline that corresponds to bar color, but really hard to see in pole hole. P3 L1 – Edit wording to be more precise. As the raw data from both satellites was acquired in the same pulse limited imaging mode, the use of the word 'mode' to describe a processing choice could be misinterpreted. Additionally I think the two modes authors are talking about are ocean and ice retrackers, however there are actually 3 retrackers available for these missions (ice-1 and ice-2 are separate). P3 L3 – Paolo et al presents results over flat ice shelves with zero slope, whereas this paper presents results over an ice sheet, where the most rapidly changing regions are found in the most steeply sloping terrain. The logic that Paolo used to justify including data from different imaging modes therefore may not apply to this paper. The authors should use data from a single retracker which has been shown to be more reliable, or quantitatively justify why including less reliable data improves the quality of the end elevation change result. E.g. via coverage, temporal extent, or reduced error maybe. It follows that a separate error estimate should be provided for the elevation change result derived from different quality input datasets. P3 L4 – Specify the size of the bias between both modes, for both satellites. P3 L16 – The Helm et al DEM is the ice surface during the first 3 years of CryoSat, however this surface has changed significantly in many regions throughout the 40-year study period. The ice surface of the DEM should evolve temporally to reflect the known elevation change, otherwise the slope correction will not be correct. This effect will be significant in regions such as WAIS which have shown to thin at a maximum rates of up to 9 m/yr. Have the authors done this, or if not what is the error on the slope correction that will result from not temporally evolving the ice surface for 40 years? P3 L29 – Can the authors quantitatively state how much less sensitive to noise the OCOG retracker is compared to other options, and does this affect all satellites the same way given that the spatial resolution and imaging modes are different. Some retrackers will perform better over different terrain types (sloping or flat), therefore it would be helpful for more details to be provided about the region, time period, and satellites for which this analysis was performed. P4 L3 – If the CryoSat retracking doesn't exactly replicate the methods in Helm et al, the full details should be detailed in this paper. P4 L13 – It is well known (as the authors later state) that anisotropy exists between ascending and descending tracks of radar altimetry data, and indeed many elevation change papers include a term for this in the plane fit solution to exclude any bias from this. When calculating data precision from ascending and descending tracks have the authors performed such a correction, and if so could further detail be provided on its size per mission. P5 L5 – Edit text to state quantitative statistics about the absolute or percent improvement following their slope correction. Frustrating that 'superior performance' and 'similar improvements' used when a number would be more persuasive. P5 L17 – Figure S1 does show a reduction in the anisotropy effect, but the signal is clearly still present in the data. Additionally, smoothing a result with an artefact in doesn't remove the affected data, so this error will clearly have an effect on the end result. If as the authors state recent previously published studies have designed and successfully implemented an anisotropy correction, this paper should add this step to avoid unnecessary error. If the authors chose not to do this, I would ask that the quantify what the affect of not applying the correction is to prove its not discernable. P6 L25 – If there are differences in the processing methods used for different missions as stated, this should be fully specified in the supplementary material. Use of full parameter names as found in the mission meta data will ensure that the methods and results presented are repeatable. P8 L1 – Spelling P8 L1 – Specify the thresholds and variables against which data is filtered out during the elevation change iterative processing. Are the same values used for all missions? P8 L7 – Provide some detail on how the backscatter penetration correction is calculated and applied. E.g. over what epoch? P8 L18 – State threshold used to determine outliers. P9 Fig4 – Red colors in this plot do not print well so can't easily differentiate missions. Change color scale used. P9 L3 – The authors are simultaneously arguing that for all of the more recent missions a spatially variable offset correction must be applied as the offset is spatially variable, while stating that for Seaseat and Geosat the offset correction must be a constant because some of the difference could may be real elevation change. If the later is true, why does this not also hold for the more recent missions? The fact that a spatially variable correction can't be or hasn't been calculated doesn't remove the justification for why its needed. P10 L15 – The extrapolation and interpolation steps are not sufficient. It doesn't account for the spatially variable pattern of thinning, which increases towards the coast and is larger on fast flowing ice streams, and the method is not fully described. What is the maximum distance over which gaps are filled? In areas such as the ice sheet edge or on the Antarctic peninsula, which receive exceptionally poor coverage in earlier missions, how are these larger gaps filled? Equally, in order to state EAIS, WAIS, and continent wide thinning rates, the pole hole needs filling. P10 L31 – The signal in the 1978 to 1992 map is extremely noisy with lots of variation over short spatial scales. Although the authors assert that 'coherent signal' can be obtained from these missions, to me it looks like the differences are as great if not greater than the similarities between the later data. Generate a difference map, or statistics, to quantitatively demonstrate that the results form the early missions are 'coherent', or similar to those from later missions. P10 L32 – The authors attribute elevation change across the ice sheet to ice dynamics without providing evidence in support of this. Elevation change can be caused by dynamic ice thinning, a snowfall anomaly, change in the scattering horizon, or measurement error, so all of these factors will influence the result not just dynamic thinning alone. Which specific regions are attributed to dynamic change? If based on previous publications, please provide relevant citations. The authors should quantify how much of the elevation change is dynamic, vs all of these other factors, in order to demonstrate that it's the largest contributor. P10 L33 – State what's classed as a short time scale, annual/ sub-decadal/ other? P11 L1 – Again maps of elevation change are not evidence of change in dynamic thinning without additional supporting data. The authors need to quantify and rule out the influence of snowfall variations, change in scattering horizon, and error, and a corresponding change in ice speed should also be observed. Without this elevation change due to long term, decadal fluctuations in snowfall, may be mischaracterized as dynamic change. The authors should also state which regions they are referring to. P11 Fig5a – Add distance markers to the flow line on one of the maps, hard to tie 5b to locations along it. For example, what distance is the limit of Seasat and Geoset data? P11 Fig5b – Add error bars to this plot. P12 Fig6 – Provide a spatially variable error map for each of these epochs. So far the results have been presented without any error method described, or measurements included in the plots. P12 L4 – Edit text. The discussions aren't controversial, there is just are just different approaches each of which have advantages and limitations. P13 Fig7 – Add error bars to all lines on this plot. P13 Fig7 – Clarify how the % coverage has been calculated, and add coverage labeling to an axis. For Antarctica and the LPZ, I don't see how it can be 100% during the 1990's when the pole hole is not observed. Additionally, in the methods its stated that the raw data was originally gridded at 1km resolution, which will result in data gaps of several kilometers between tracks before the CryoSat's precessing orbit comes online. So again I struggle to see how such complete observational coverage is achieved, unless extrapolated and interpolated data is classed as an observation, which of course it isn't. Could the authors clarify? P14 Fig8 - Add error bars to all lines on this plot. P14 Fig8 - Add % coverage axis label to plot. Same comment applies about how the coverage calculation is done. P14 Fig8 – Add a table in the SOM with the areas of the drainage basin sub-regions used to generate this plot. P15 Fig9 – I don't understand the figure caption, please rewrite more clearly. P16 L1 to 14 – These results sections are very poorly written as no actual results are described! The authors just state what some of the figures show, and leave the reader to do all the hard work of reading off numbers and key statistics, comparing this with numbers they have read in previous studies. Re write the results section to present some actual results. I don't think a single elevation change number has been presented in the text yet, despite that being the title of the paper! P16 L9 to 13 – The authors have described what this plot is, but haven't explained why it matters or what the key scientific result is. Either remove figure 9 or explain why its an important addition. P16 F10 – Label y axis of b, presumably count. Edit figure caption to state time period data validated over. P16 L15 – The validation performed in this paper is completely insufficient, and I would argue it leaves the result presented essentially unvalidated. Use of only 19 GNSS profiles, in a region of no known change, over a limited time period and spatial extent, and on unchallenging flat terrain, does not inform the reader about the validity of these results. At a minimum the authors must use a more comprehensive independent dataset, e.g. ice bridge. P17 L2 – This 'validation' cannot be interpreted as an error. A formal error budget based on the altimetry data itself must be documented and added to the plots in this paper. Validation and error estimation are separate things. P17 L14 – The authors don't need to limit their validation data to in situ measurements, much more spatially and temporally extensive airborne data is available and this should be used. P17 L21 – State the number, don't leave the reader to guess how much elevation change you have measured! Presumably it is different for the peninsula and east Antarctica, so again please present your result. P17 L22 – Add figure number. P17 L23 – Add figure number, or label somewhere. Location of ice streams mentioned hasn't been identified on any plots in this paper. State quantitatively how your numbers compare with this thinning rates presented by Rignot (2006), the time period is different so there should be something new to say. P18 L2 to 6 – Use statistics to show the agreement, or disagreement between the elevation change and precipitation anomaly. State with numbers what 'significant difference' is that allows ice dynamics to be determined. State how far inland the thinning was in earlier decades vs how far inland it reaches now. P18 L14 – State the threshold used to determine a strong snowfall anomaly. It looks like it varies just as much at different times around other regions in

Antarctica. P18 L15 – what distance away from the grounding line were Seasat and Geosat typically able to observe. P18 L20 – The 12 m/yr thinning suggested by Li et al was due to grounding line retreat between ’96 and 2013, however the 12 m thinning present in this paper is for 1985 – 2010. Given that the measurements presented in this paper start approximately a decade earlier, if as this paper sats the glacier was already thinning in the 80’s then the magnitude and rate are not in agreement with Li et al. Please clarify. P18 L 23 – The authors need to take more care before attributing elevation change to dynamic ice mass loss. There are many signals present in their continent wide maps that may well not be attributed to dynamic ice loss. There is not consensus in the published literature that all Antarctic peninsula elevation change is dominated by ice dynamics, and the authors themselves later attribute a different elevation change signal on the peninsula to precipitation anomalies without providing any more or less evidence that a different process could be responsible (P18 L30). The authors must present quantitative evidence to support their claims either way. P18 L30 – GRACE data cannot disentangle whether elevation change is caused by snowfall anomaly or ice dynamics, as ice mass is lost in both instances. Only velocity data can demonstrate whether ice was exported from the catchment at an increased rate, proving ice dynamics. Both dynamic thinning and snowfall anomalies result in mass loss, but gravimetry mass loss measurements don’t show which of these two different processes might be the cause. P18 L35 – Quantify very well. P19 L10 – In Fig S7c I can’t see any 2002 step in the precipitation time series so its not clear to me that there is good agreement. Again please provide quantitative stats to back this up, rather than just making unsupported qualitative statements. Cite Lenaerts et al 2013 with respect to the 2009 and 2011 precipitation anomaly results as not a new result from this paper. P19 L16 – While this appears to be true for 2008/10, there is a more significant accumulation gain in the 1990’s that is not visible in the elevation change result at all. This is in part because the authors are comparing different things, snow mass anomaly, vs elevation change. Direct comparison not possible unless elevation change converted to mass change. P20 Table2 – There is negligible data coverage outside of East Antarctica prior to 1992, so not valid to include an Antarctic wide number for the '78 to 2017 period in row 3. Remove this number as misleading. P21 Conclusions – There are no key results from this paper presented in the conclusions. Add a few key numbers. SOM A.1 – State threshold used to determine if noise is too high. SOM A.1 – State the start and end date for each satellite dataset used. SOM A.2 – State which retracker the elevation measurements were derived from. SOM A.2 – State the specific name of the metadata flag used to filter out data, and if a threshold was used, state the number that this was set at. SOM A.2 – Adjust Figure 1 to reflect the actual time period of ERS-1 data used. (same applies for all missions) SOM A.3 – State which retracker the elevation measurements were derived from. SOM A.3 – State specifically which measurement confidence flags were used, and again if a threshold was used, state the number that this was set at. SOM A.4 – State specifically which measurement confidence flags were used, and again if a threshold was used, state the number that this was set at. SOM A.5 – State which LRM retracker the elevation measurements were derived from. SOM A.5 – State specifically which measurement confidence flags were used to filter data, and again if a threshold was used, state the number that this was set at. SOM B – Edit title and section text to be more specific as its unclear specifically what the authors have reprocessed? Is it that the elevation measurements have been retracked? Read as a stand alone section I don't know what SOM E S6 and S7 - Add error bars to all lines on this plot. Add % coverage axis label to plot. Same comment applies about how the coverage calculation is done.

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