Review:
Elevation Change of the Greenland Ice Sheet and its Peripheral Glaciers: 1992–2023

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The paper provides a new data set of elevation change of Greenland and its peripheral glaciers based on analysis of multi-mission satellite altimetry for the observational time period 1992 - 2023.

In addition, the authors provide numbers of ice mass loss using a firn compaction model to convert from volume to mass.
Main findings, next to a complete monthly time series of elevation change are very strong decadal changes in ice mass loss with a total loss of 6076 Gt within the period of 1992-2022.

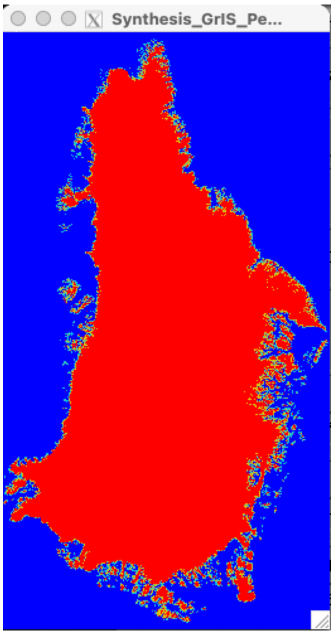
The data set is accessible via the given link and the netcdf file is easy to read and fulfills the standards of ESSD.
The article supports the data set.
The data set itself is accessible, unique, complete, usable in its current format and useful for a wide community.
The paper is well written and structured, figures are of good quality. Some methodical explanations I did not fully understand. This is listed below.

In general, I would like to see the data set published as it provides a long term elevation change product for Greenland and peripheral glaciers, which is very important, but I still would like to see some improvements or clarifications – see below.

The authors put in a massive amount of work to provide this comprehensive dataset. Thanks to the authors.

However, I still have some questions and concerns which are listed below and hope that the authors can address the given points as I think that such a data set is of high interest for a wider community.

When opening the netcdf file using ncview all variables seem to be flipped in the y-direction. This is confusing, although the y-coordinates are in line with the flipped data fields.
I think this should be improved.



As this is a companion paper to Nilsson (2022) most of the methods are already explained in detail. Improvements to the processing chain are addressed in this paper.

Improved input data products were used for CryoSat (Basline-E). For Envisat ENVISAT GDR 3.0. was used including data after the ENVISAT orbit shift. The source of ERS1/2 is not mentioned. Are you using the REAPER product?
I have to mention, that the most updated data products for ERS1/2 and ENVISAT are not used in this processing. If an update is planned, please consider to use FDR4ALT reprocessed data. https://www.fdr4alt.org/ . At this stage I don't consider this as a major request, it's more a recommendation.

In section 3.6 the radar penetration and the adjustment of seasonal amplitudes is explained. I still have some concerns if an artificial correction of seasonal amplitudes using the characteristics of another mission (here CryoSat is used as reference mission) is a good way to handle this problem. To my opinion, one should leave the data as they are after the correlation with backscatter, tail and LEW and not trying to reduce the seasonal amplitude by another mission. Especially the use of CryoSat-2 as reference I see critical, as the two modes (LRM and SARIn) are behaving different in terms of penetration. If such an additional amplitude suppression should be applied, then I would recommend to use ICESat2 instead. Please also include the reference to Helm et. al. 2024 who developed a new CNN based retracking approach to tackle the penetration issue directly within the retracking.

In section 3.8 the interpolation using velocity and hypsometry is used and an update to 2022 paper is explained. I'm not really understand why such an 1/v^2 weighting is used and why you are not using the ALOG10(v) as in the 2022 paper. I thinks the risk is high to overestimate

elevation change when using the velocity directly as the gradients are very high. I also have concerns, that a correlation which is found for velocities of 300 to 500 m/a can be used for other velocities. I personally think that the correlation is spatially highly variable and one should not transform a certain behavior which works in a specific region to another region. But maybe I misunderstand something how you applied parameters of the muli parameter regression method.
I also don't understand why a biquadratic surface is fitted to the monthly elevation change for bin sizes of 100km. A biquadratic surface was already fitted to the point cloud to remove topography, which makes sense, but I don't see why an elevation change should behave like an biquadratic surface in such a large area. Is this fit only used for outlier rejection or are the fit parameters used for the interpolation?
Would it be possible to provide two correlation maps of Greenland to demonstrate the similarity of the linear behavior of hypsometry/Velocity against elevation change, to see the spatial variability? Why is a binning in 100m bands necessary and does it make sense in the interior of Greenland, or should a smaller bin size be used?
How does the background model looks like and does it change with time? As I understand, the background model is estimated for each bin at each monthly time step? If this is correct, then the background model is varying from month to month, which I think is questionable. Personally, I think that monthly anomalies are not correlated with hypsometry and velocity as those are more driven by changes in SMB and not dynamics.
So, I would recommend to remove a long term trend for each mission and only use this velocity/hypsometrie approach on elevation change trends as they represent a 'long term' dynamic behavior, which correlates with velocity and/or hypsometry.
The monthly anomalies or residuals should be interpolated by IDW, ordinary kriging or median interpolation and can then be added back to the interpolated trend. This would speed up the processing as well and minimize the risk of introducing high values in sparsely sampled areas at the margins for early missions, due to the high velocity gradient.

Validation, section 4:
Would it be possible to provide Altimetry – ATM differences of elevation change on decadal basis: 1992-2002, 2002-2012, 2012-2023. This would help to understand the large discrepancies to other studies especially for the ENVISAT/ICESat time period 2003-2012. Liang et. al. published a table and comparing volume changes derived by different authors.

An elevation change dataset in Greenland ice sheet from 2003 to 2020 using satellite altimetry data
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Table 4 of 4
Table 4. Comparison of total volume change rates for the GrIS derived by different studies.

Source	Data	Time span	dV/dt (km^3/yr)
Felikson et al. (2017)	ICESat	2003–2009	−269 ± 37
This Study	**ICESat**	**2003/02–2009/10**	**−210 ± 18**
Simonsen et al. (2017)	CryoSat-2	2010–2014	−271 ± 32
Smith et al. (2020)	ICESat&2	2003/10–2019/02	−235 ± 3
Li et al. (2022)	ICESat&2	2003/02–2019/09	−192 ± 7
This Study	**CryoSat-2**	**2010/08–2018/10**	**−335 ± 24**
This Study	**ICESat-2**	**2018/10–2020/12**	**−363 ± 20**

In addition to this table Wouters (2008) and Velicogna (2009) published values derived from GRACE of 179 Gt/yr, 2003 Gt/yr respectively. Sörensen (2011), published values for the period 2003-2008 of -180 to -230 km3/yr. Ravinder (2024) publised values of -196km3/yr for the period 2011-2022.

I used your data set and estimated the volume change in the periods from 2003-2008, 2007-2011, 2011-2018, 2011-2022 and found for Greenland ice sheet a volume change of -337, -540, -170 and -165 km3/yr respectively.

Your data product exceeds literature values for the 2002-2011 time period by roughly a factor 2 but is showing slightly smaller values for the CryoSat2/ICESat2 time period. Especially the discrepancies in the ENVISAT/ICESat is in my opinion a major concern, which needs more careful evaluation. 540 km3/yr on average for 2007-2011 is even exceeding the extrem melt years 2012 and 2019. Where is this coming from?

In table 2 only the mass changes are given.
Can you please include a similar table with volume changes, as this would reflect the data product and also Figure 5 where volume changes are presented.

It would also be useful to add the volume change to each of the sub panels in Figure 4.

As you mention in the text and your results section mass change, it is necessary to provide exact information of how you derive mass change from volume change. How you apply the FAC and it would be useful to also provide the FAC data set in parallel with the elevation change product.

In the results section you explicitly highlight the background model approach using hypsometry and velocity. Hurkmanns (2012) already compared ordinary kriging with spatio-temporal kriging including external drift by using a velocity field to improve interpolation of sparse sampled area at Jacobshavn Isbrae. They found a 10-20% increase of volume loss and argued, that for the whole Greenland ice sheet less differences should be expected as not all outlet glaciers show such a high thinning rate and are much smaller. However, here we see an increase of up to >100% when compared with other studies. For the CryoSat2 / ICESat2 time periods where spatial sampling is much better at the margins most studies agree quite good with the values presented here. Therefore, I have some concerns about the results of the presented method in sparse sampled areas. The authors mention an orbit shift of ENVISAT 2010. Maybe this results in less coverage and therefore the method fails and provides unrealistic high values at the margins of the ice sheet. When looking at Figure 5, nearly in all Basins a very high loss can be observed between 2009 to 2011 and also between 2002 and 2004. The first period includes the orbit shift the second the combination of ENVISAT and ICESat. Maybe also the handling of velocity in the regression model not as log10 as you used for Antarctica is a parameter to look at.

Here I derived trends from your product and calculated the difference. Especially at the eastern margins where topography is complex and sampling in early missions is low you get extrem melt rates of sometimes 20 to 30m/yr. In the end this accumulates to those very high ice volume loss, which I think is not correct. One can also see patterns of flipping colors, were some negative/positive values seem to dominate the interpolation.