

Interactive comment on “Decadal changes from a multi-temporal glacier inventory of Svalbard” by C. Nuth et al.

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The manuscript by Nuth et al presents a complete and comprehensive inventory of the glaciers of the Svalbard Archipelago using the satellite record of the 2000s and basing its structure on the previous benchmark inventory from Hagen et al. (1993). The new inventory is nicely supplemented with the compilation and digitisation of older records to form two older inventories with which important change detection can be undertaken. This work represents an important step in the long term monitoring of the archipelago's ice and is the first such record of its kind in the region. It is of course important that these inventories be published and made available to the community as they will form a key future reference. The text and figures of the manuscript are of good quality and generally I believe the manuscript to be of publishable quality for The

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Cryosphere. However, there are a number of issues that must be addressed which I outline below, first as general comments and then more specific comments.

General comments:

- A main concern is the handling of the data accuracy issue. The accuracy section that is raised at the end of the results needs to be raised in the methods so this is understood by the reader before being presented with the results. These errors need to be better incorporated into the results section especially with respect to the insignificance of any length changes below 14 m a^{-1} . It is difficult to interpret the results as presented without this information at hand and incorporated into figures where appropriate. Also, the exclusion of ice cored morains is significant. Is there a way to quantify potential error introduced in excluding (the very likely) ice cored moraines? - The inclusion of a surge vs non-surge type designation in the inventory. This seems like a relatively simple thing to include based on your observations and previous publications (i.e. Jiskoot et al., 2000). It seems that during the manual editing of the glacier outlines for GI00, each glacier was 'visited' and signs of surges may have been identifiable. It's difficult to interpret change patterns in the absence of this information. If this is not within the scope of this project than the fact that this was not completed should at least be acknowledged.

- It would make this inventory and paper significantly more impactful (to the high TC standard) if the recorded parameters for each GI were listed in a table along with those of the global inventories. That would make it easier for the reader to know at a glance what was available in each, how they compare and how they can best be utilised collectively and independently.

- Clarification is needed of how the annual change rates were calculated in terms of the multiple dates used in the different inventories. A paragraph needs to be dedicated to this issue somewhere. Dividing the total area/length changes by the averaged time span is questionable. The range of time spans is presumably large given H93 ranges

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from 1960 to 1980, GIold ranges from 1936 to 1977 and GI00 ranges from 2000 to 2010 but there is no mention of this. Were the average times spans calculated on a per glacier basis or overall? Does it make more sense to measure the total change at each location, divide it by the actual time span and average the change rates? This at least needs to be clarified.

- The authors discuss the present glaciation of the archipelago through topographic and glaciological inventory parameters but much of this information is not particularly interesting on its own. There are several examples. We know the islands to the east have more of an ice cap geometry. We know what parts of Svalbard are dominated by tidewater glaciers and small glaciers etc. This general description information has not really changed since Hagen et al. (1993) and I don't think it's necessary to rehash it. The interesting things are of course the changes in the inventories and I think the manuscript results and discussion need to focus more on that and especially on how to manage the highly varying time spans.

Specific Comments

Page 2490

Line 2 – Is this 100% of Svalbard's ice cover which covers 57% the total land area? This needs to be made clearer.

L5 – it's not really terminus width you have measured here though is it? Not in its conventional sense. Maybe summed average terminus width?

L12 – Is 'temporal' necessary here? Maybe 'annual retreat rates' if just 'retreat rates' is not enough.

L25 – are 'outlines' or 'polygons' better terms than 'borders'?

Page 2491

L4-5 – are these citations examples? If so they should be preceded by e.g.

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L22 – Kohler et al (2007) only demonstrated volume losses in a relatively small part of the west. James et al. (2013) measured increasing volume loss at various locations in the archipelago including the higher rates in the west. Isn't this a more appropriate citation?

P2492

L5 – estimated

L6 – consider moving 'derived for two epochs' before the list so it's clear you are not just doing this for the 3rd parameter.

L10 – consider using analogue or analytical photogrammetry. The term manual photogrammetry is a bit meaningless since even with the most modern digital photogrammetry, some components are manual.

P2493

L13 – Not sure what you mean by coherent here? Can you rephrase to be more specific?

L17-19 – Can it be made clearer to the reader that these DEMs are produced by SPIRIT rather than in house?

L19-20 – These are the same product but for different epochs... not five separate products.

L25 – ...required to complete coverage of the archipelago.

P2494

L9-10 – Likewise my experience with GDEM is that it is of fairly low quality in the poor contrast areas of glaciers and ice caps. My impression is that the 'bumpy' texture describe is the result of failures of the image correlation algorithm in areas with little image information. These areas would be bumpy without merging with other DEMs

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and I would expect that merging DEMs would smooth these out somewhat even if the DEMs were from different times. You discuss later in the manuscript the importance of DEM errors on delineating glacier outlines/drainage basins but what is the magnitude of the errors you are filtering out? This is discussed in some detail in the appendix but I think in the methods itself a statement about the magnitude and sign of the errors that are being removed is necessary.

L17-20 – Do you compare these two drainage basin data sets to assess quality? If so how do they compare?

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L1 – ‘satellite instrument pointing’? Do you mean accuracy of the orbital parameters? The term ‘spatially coherent’ is vague. Can you be more specific? Does this mean there are positional errors relative to some ground truth?

L2-3 – it’s not clear here or from the citations you provide how you co-register a DEM in XYZ using such a low spatial resolution data set like ICESAT. Can you either explain this here or explicitly refer to the publication that explains this. I’ve not read Nuth and Kääb (2011) in great detail but it’s not obviously explained there.

L16 – Can you specify where the automatically generated hydrological basins and topographic contours come from? The former from the ASTER GDEM and the latter from the S100?

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L17 – why were these adjusted?

L23-24 – what descriptive, glacier and topographic attributes are available for Glold and GI90?

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L1-2 – glacier hypsometries come from what topographic data base? S100? The

C1260

ASTER GDEM? If the former, wouldn’t you expect significant changes since the 1990s as suggested in James et al. (2013)? If the ASTER GDEM, how does this compare to S100 and what are the effects of the ‘bumpy’ errors?

L8 – intersected with the glacier outlines for each GI?

L9 – I think ‘measured’ is more appropriate than ‘estimated’

L9-10 – why did you choose the lowermost 10% of 10 the centerline for GI00s? Is this arbitrary or based on something more substantial? Please state which/why.

L21 – maybe specify percentage change rates?

L27-2 – what are the known benefits of this approached as found in the citations you provide? Is the tongue width used here that which was previously calculated using the lower 10% of the centreline? Clarify.

P2498

L7 – Not clear what you mean by distributions of glacier numbers.

L10 – how are you defining glaciers, glacierets and snowpatches?

L17 – with a calving front

L19 – tidewater terminating

L21-22 – I don’t understand the last sentence of this paragraph (and do you mean 11 and 5?). Please clarify.

L23 – you’re not really summing actual front widths here. You have averaged the widths over an area. Here and elsewhere in the manuscript it reads as if the number you provide are actually length of the calving front which is not true.

L25 – ‘perimeter’ implies a closed polygon. Maybe length, width or extent?

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L3 – Worth a mention here that your fronts are shorter due to the averaging of width of the bottom 10%?. Worth stating here? It makes slightly less sense to sum the fronts in the way Blaszczyk et al., (2009) and others have but still an interesting comparison I guess.

L8 – Does 'truncated' suggest data has been cut off. Steep tails?

P2500

L5 – "...to control coherence between the glacier upper boundaries" Not sure what this means or if it is necessary. Delete or be more explicit.

L7 - 60% of the archipelago's land area

L17-24 – this is really for the methods. You elude to the difference in the treatment of snow patches this in the methods but it's not clear to the reader until this section is read.

L27 – What is the range of time spans that are used? I'm not sure the average time span works here for calculating annual change rates.

L27 – so this 7% is 100% change and no error?

L28-2 – should this also be specified in the methods?

P2501

L4-6 – This statement is pretty obvious. For meaningful changes it really has to be absolute area loss. I've never been sure of the significance of relative changes in this context.

L12 – Is this 30% for all of Svalbard or just southern and western Spitsbergen?

L15-16 – again, the calculation of these annual changes needs to be made clearer in the methods, in particular how the varying time spans were handled.

L17-18 – how did you choose this threshold and how do the interpretation errors you

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describe affect the quality of your results?

L21-24 – It should be mentioned in the methods when you introduce the area/width parameter what the benefits of this approach are over centreline changes alone. It's hard to know how to interpret this and what it is telling us that the centreline width does not.

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Sec 4.4 – This is very late in the paper to be addressing the accuracy issue. This should be raised in the methods and the results of the assessment given here.

L10-20 – all this needs to be raised in the methods.

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L26-29 – something is wrong with this sentence.

P2506

L13 – I think it's redundant to use the term 'temporal' with 'rates'. You use this combination elsewhere in the manuscript. I think just area/width retreat rates is sufficient.

P2508

L10-12 – again. . . it would be useful if the relationship between these data bases and those presented here were made clear in a table.

L19 – for a sample of ~400 glaciers in the south/west Spitsbergen

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L13-14 – If you don't know then I think the following statement is not necessary "...which may result from a smaller matching template in the original parallax determination of the DEM generation."

L21-22 – as previously, I'm not convinced this is due to the merging of DEM tiles but

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more due to failures of the image correlation in areas of low image texture.

L24-25 – how do you do this given the low spatial resolution of ICESat? I presume you are using all the ICESAT tracks? Explain or provide citation.

P2519

Table 2 – First time we've seen the term 'glacieret'. Can this be defined in the text? It is not clear the difference between 'glaciated area' and 'comparable area'. I think you mean There are a few issues raised here in the differences between H93 and GI00 that should be discussed. If my understanding is correct, the Total Area should be the same in most cases (unless area of ice has been lost and replaced by the sea), so some of these differences are errors. What causes the discrepancies in Glaciated Area other than glacier shrinkage which is presumably represented in 'Comparable glacier area'? Is this area that is shown as Percent Area Change? I guess what I'm saying is that it's important to state what's change, what's error and what's omission in these differences.

P2520

Fig 1. – I like this figure; very informative. A few suggestions: (i) width of the 1990's bars are wider than the other epochs suggesting a different x axis scale which should not be the case. This shows that most images are from 1990 and some from 1991? (ii) on page 2491 you say that the GIold is composed of images from 1936, 1960, 1961, 1966, 1969, 1970, 1971 but this figure suggests that only three years are used. I assume the contribution of the years not shown are small? Is there a way to include these in the figure? A compressed y axis scale? Maybe an accompanying Table? (iii) the previous might be helped if the figure could be made larger. The smaller areas on the maps are basically invisible (i.e. areas of GI00s from the early part of 2000s). Kvitøya could go in an inset to save space; Maybe a simpler and narrower outline of Svalbard would make these areas less obscured. (iv) specify in the caption that grey is no data.

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Fig 2. – Kvitøya is not included in the hypsometry figures but the % sums to 100%. Source and date of the hypsometry are necessary.

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Fig. 3 – This figure is very useful for describing the classification system. The inset is in the way a bit. Is it necessary? Can the boxes be shown in another figure or just add lat-long to the figure. Maybe it can be moved to the left corner. Can the elevation scale be stretched?

P2523

Fig.4 – The exact meaning of the panels in this figure are not clear. For example, in a) initially I thought this plot was telling me that 40% of the glaciers have an area of 1 km² and at the same time that 40% of the glaciers have an area of <100 km². After some thought that in the latter case you mean 40% of the total area is made up of glaciers that have an area of <100 km². Maybe two axes; one with glacier numbers the other with area would be more obvious? Also the y axis scale labels are misaligned. In b) and d) do you mean the percent of the total area that is (tidewater) glaciated? Make all three of these more clear in the caption and in the text on Page 2498.

P2524

Fig. 5 – Is the aspect degrees from north clockwise? I think this would tell a different story if it was by area rather than by number of glaciers. The area of north facing glaciers seems quite small in comparison to the area of south facing glaciers. Units should be added to the x axis of the histograms.

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Fig. 7b) – hard to get anything out of this figure. Maybe making the grey dots red would help? They are hard to see. Here you show change in relative units but in the text in

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Section 4.3 you discuss length changes in $m \cdot a^{-1}$ which is more intuitive.

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Fig. 8 – This is another example where it's not clear how the varying time intervals were handled. In a), the dashed line divides between a longer and shorter time interval. Were two separate time intervals used to calculate the annual changes? Nice highlighting of the likely surge-type glaciers here.

P2531-2

Fig. 11 – Move the letters for the sub figures before the text to be consistent with the other figures. x and y axis of e) and f) need labels.

References

Hagen, J. O., O. Liestøl, et al. (1993). Glacier atlas of Svalbard and Jan Mayen. Oslo, Norsk Polarinstitut.

James, T. D., T. Murray, et al. (2012). "Observations of enhanced thinning in the upper reaches of Svalbard glaciers." *The Cryosphere* 6: 1369-1381.

Jiskoot, H., T. Murray, et al. (2000). "Controls on the distribution of surge-type glaciers in Svalbard." *Journal of Glaciology* 46(154): 412-422.

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