Review Hannesdóttir et al.: Area, volume and mass changes of southeast Vatnajökull ice cap....

The Cryosphere Discussions

This manuscript present a novel multi-temporal analysis of length, area and volume changes of a region of non-surge-type Icelandic glaciers over more than a century. The data are unique and there are some interesting findings in terms of different retreat rates, different glacier types, and different periods of potential climate forcing. Although the results seem substantial, it is hard to judge how well they stand due to a lack of proper error analysis, both in the construction of the glacier data and the analysis. Although the methodology appears extensive, much of the needed information to assess the quality of the data collection and error analysis are missing.

MAIN ISSUES:

A. The paper is too long and lacks focus. Some of the major results are not stressed (e.g. that this research generated a novel multi-temporal glacier inventory) and other sections are not justifiable with the generalisations and/or the small sample of data (e.g. the volume-area scaling; mass loss).
B. The methodology is defective and poorly structured. Descriptions are mainly about what is done and not how it is done. A table of data types, sources, and errors for each of the DEMs, as well as the snowline MODIS imagery would be useful. The DEM of subglacial topography is unclear: what is the horizontal resolution, where were the transects taken and what was the interpolation technique? At what scale or zoom factor were the glacier outlines digitized and what was the human and digitizing error?
C. Some of the methodology is questionable, in part due to the lack of information (A). In particular:
    1) The mass change calculations are based on very rough generalisations, and should only be used to give an overall estimate in geodetic mass balance change, rather than calculate changes over time, or between regions.
    2) The different maps and DEMs should have been co-registered to perform a change analysis. If this was not done, the errors will be much larger than reported.
    3) I derive from Fig 4 that the snowline elevations have similar or larger seasonal variability than interannual variability. Additionally, it is always necessary to give the exact dates of the MODIS images used for the snowline measurements, and to indicate how close this is to the end of the melt season. It is unclear how the snowline pixels were derived (e.g. by image classification, or thresholding?) and how their elevations were extracted (see e.g. Jiskoot et al., 2009 for two common methods giving quite different results).
D. The error analysis is weak, and the total errors not calculated properly.
E. The paper is too long for its findings, and poorly structured. Rewrite and remove all repetitions, and remove some of the non-essential self-references. Move the volume area scaling methodology and results from the discussion section to the results section (if it is concluded that this section should stay in the paper).

F. The discussion is unfocussed and shallow, and it seems like the authors felt the need to discuss all the results. Pick the most important findings and focus the discussion around those.
G. Several figures and tables could be combined to strengthen the interpretation of these, and to focus the results and discussion.
H. Think critically about the usefulness of comparing relative area changes (in percentages of starting area) for different periods, given that the overall class sizes have changed over the reported years, and other regions have different glacier sizes. This difference (with often the smaller class size have the largest loss in relative area) is in part a scaling issue, rather than a result of climate forcing/response. Many glacier change studies (including my own) have really emphasized this relative (%) area change, but is it really that useful?
I. The use of English overall is quite good, but the use of verb tenses is confusing throughout the paper. In the manuscript the authors use the present perfect tense (has been) and past tense (was) interchangeably. I suggest using the past tense throughout, as the present perfect tense implies it still goes on. Wherever the past perfect tense (had been) is used it should imply that something was done (by other researchers) before the present study. Correct throughout and have a native English speaker check the verb tenses.

SPECIFIC COMMENTS:

Title is too long and detailed for the confidence in the data. Change to "Area and volume change of southeast Vatnajökull, Iceland, between the Little Ice Age maximum and 2010".

P4682

Abstract:

Rewrite after the paper is updated, and tone down the second part where the wording is too strong given some of the uncertainties in the results. The 'dynamic response' of glaciers is usually separate from the mass balance response, and the term 'indirect response' may be more appropriate here. Apart from the retreat in proglacial lakes, the differences in response described in this paper are more related to the reaction time and response time, rather than dynamic factors. Rewrite and tone down the causal certainty that the changes are related to hypsometry, bedrock topo and proglacial lakes.

P4683

L4 ....sea level rise and water resources.

L16-19: repetitive wording. Delete "is one of the most sensitive ice caps in the world" and reword accordingly.

L21-24: Don't the glaciers and ice caps in the Canadian Arctic contribute too?

L27-7(next page): this section is repetitive within the intro. Move to section 2 (study area).

P4684

L3: delete 'even'

P4685

L2-5: How accurately is the bedrock topo known?

L5: what is 'alpine-like'?

L13: use the term ablations stakes, rather than survey stakes.

L23-24: remove some references.

L 24: ..ELA was approximately...

L26-29: Regular monitoring of annual frontal variations..... Then remove 'providing annual records of the advance and retreat of the glacier'.

P4686

L15-18: shorten this to one sentence: T and P were extended back to the end of the 19th century, following the methodology of A et al (2011).

P4687

The subsections are too short to warrant subheaders, and the glacier geometry in sections 3.2-3.2.2 is poorly explaind. What was the resolution and scale of the datasets, were they georeferenced to each other, what scale was the outline digitized, etc.

L20: do you mean nunataks rather than erratics?

P4688

L6-14: express as the percentage of total area not mapped.

L28: Need an error analysis of the influence of snow cover on the accuracy of the mapping of glacier outlines from the nunataks.

P4689:

L1: explain ehf.

3.2.4 Explain the resolution and sampling of the RES: was this along flowlines or other transects or a grid? What was the horizontal spacing of the bedrock topo, and how much of that was through interpolation techniques?

Section 4.1: Separate the methodology clearly into DEM reconstruction and DEM differencing. Need a much more precise explanation of the georectification (any co-registration?), GCP orthorectification, and error analysis and quantify the errors better. Rewrite entire section into the past tense.

L19-24: move to results

P4690:

L11: which kriging methods?

L20: ice divide shift importance is relative to the glacier size: both ST glaciers are large, but did it do something to the smaller outlets: express as a function of area.

P4691:

L6: is the 1890 DEM explained in H et al, 2014?

L6: delete 'shape of the'

L12: how was this adjusted?

P4692:

L6: Explain how it was 'reassessed'.

L20: need to know the dates of the Landsat images, and the potential errors associated with these.

L23-26: What date was the lidar and what error? Not sure if I understand the methods here.

P4693:

L2: explain what error analysis you used. For each pairwise comparison (e.e. DEMa and DEMb) of the error should be calculated as $E = \sqrt{{E_a}^2 + {E_b}^2}$.

L7: and glacier dynamics (e.g. surging: see Jiskoot et el., 2001).

L8-9: irrelevant: remove sentence and Lliboutry reference.

L10: were these normalized curves?

L15-17: Long before ‘recent’ the ELA or snowline was estimated from aerial photography: see World Glacier Inventory (wgms.org). This was common practice in appr. 1950s-1980s.

L21: Give years and resolution for the MODIS used for the snowline. Any problem detecting the snowline different dates? What method did you use: manual, supervised classification, thresholding? See also Jiskoot et al., 2009: Shea et al. 2013.

P4694:

Reverse order of equation 1 and 2.

L9: Remove ‘Sorge’s Law’. Also, 900 kg m^3 is a very rough estimation of the average density of ice. See Cuffey and Paterson (2010) for a better range for the Iceland glaciers, and calculate associated errors in volume.

L23: bring to methods.

P4695:

L1-14: confusion between the term snowline and ELA

L15-28: Poor phrasing throughout.

L24: debris cover will introduce and additional error in the ice extent delineation. Was this the only glacier with some debris cover, and can you give an estimate of the associate error?

L25: Be more specific than ‘in the following few decades’

P4696:

L6: is this rate for ‘relative’ (%) or absolute area loss?

L10: But the lack of downwasting seems a function of you forcing this above a certain elevation. If you first force it not to change and then conclude it did not change then there is no real rocess interpretation possible. Also, the shape of advancing and retreating glaciers has been extensively discussed and is in part due to the interplay between dynamics and ablation (see Schwitter and Raymond, 1993: JGlac 39 (133)). Use this in the discussion.

L15-21: Need to know the topography of the nunataks (steep or shallow slopes) and the variability of the snow around it. See comments in methodology too.

L23 and further: Be careful concluding too much from comparing rates for different length of periods.

P4697:

L4-5: delete: this is obvious

L7-25: Simplify and perhaps only use a rough estimate for the entire region, due to large errors assuming that the density is a constant for the different glaciers and at the different elevations. Also, use a good error estimation, where the error is a function of the error in the elevation, in the area, as well as in the ice density.

P4698:

L1-14: Use the proper and accepted terminology of top-heavy, equidimensional and bottom-heavy. What are the exact boundaries of the classes, or was this just done visually? De Angelis et al (2014) use the Hypsometric Index classification proposed by Jiskoot et al. (2000 and 2009). Also, the top-heavy class (B) is typical for ice caps, so it is not a surprise that the ice caps of iceland mostly fall into that category.

L14: why 'in its greatest extent?'

L17-22: A discussion of general response time is missing.

P4699:

L12: The geodetic mass balance of - xx m. w.e. (specify)

L17-29: Be clear about what you discuss: this material can be deleted, as it is not new for iceland or the world.

P4700:

L1: from the 'non-surging' outlets

L3-8: The interesting comparison here would be a difference in maritime glacier mass balance curves and the response, relative to other more continental regions. Additionally, iceland glaciers may have wider tongues and flatter topography due to the lack of (or very little) constraining surrounding topography, relative to higher alpine regions (I include Svalbard and East Greenland). Additionally, the reverse bed slope and overdeepening give rise to a larger marginal region of recent proglacial lakes, and if neighbours surge the both water and mass piracy may occur. The combination of these factors is rather unique for Iceland and should be included in any discussion.

L9-29: very repetitive.

L17-21: So here is a hint why the Iceland glacier retreat faster? Elaborate and include if it is mb curve or in part dynamic.

L22-29: delete.

P4701-03:

6.2 is a particularly general, disorganised and weak discussion. The authors have the opportunity to focus on overdeepening, proglacial lakes, and differences in flow velocity here, but do not apart from a short mention in the last sentence (P4703, L8-10). I suggest to focus and elaborate on those factors which make Icelandic glaciers unusual, and from which we can learn about process-response. The discussion about the influence of hypsometry (and Fig 13) should include a scatterplot figure that groups the glaciers with similar hypsometries, and contrasts their length or area change, so that it becomes clear if this is a strong patters or merely a suggestion.

L11-12: these class B glaciers are the largewr ones, so the % loss is size related, and thus not particularly informing.

L19-24: These glaciers have different size and slope (which is not mentioned) thus the different response is not surprising and can be explained with response time theory.

P4703-05:

6.3 I suggest removing this entire section from the paper.Volume-area scaling should be in the methodology and results, and be properly presented and discussed. The number of glaciers may be too small (as you state later), and scaling laws are generally used to estimate volume from a population of glaciers, not individual glaciers.. Additionally, most of your glaciers are ice caps or outlets and should have different parameters (see e.g. Hagen et al., 1993: Glacier Atlas of Svalbard and Jan Mayen). Also, some recent effort have not surpassed this general scaling (e.g. Adhikari and marshall; Farinotti and Huss several papers).

P4705:

7 Conclusions: be much more specific and gve a summary of the major findings. Really focus on your own results.

P4707: References: Quite a plethora of references, and many are only used once or in conjuction with other similar references. Thin out a bit. Why are the page numbers after the references?

TABLES

Table 1: List what year(s) are these data based on? The ELA description is vague, and should have some standard deviation.

Tables 2 and 3: I think it would be more effective to have the numbers in a figure, such as for length (Fig 7). Perhaps make Fig 7 underneath each other a (length), b (area), and c (volume). With error bars.

Table 4: Delete. I doubt if the method warrants the detail per glacier. Just give the overall geodetic mass balance for the entire region as a fogure in the text. Additionaly, the average T does not say much if the time periods are for different lengths (not taking into account the NAO and AMO).

Table 5: delete, or go into much more detail and group per hypsometric class.

FIGURES

Fig 2 and 7 should be combined or at least underneath each other with the same scale so that patterns can more easily be discerned. Indicate starting year on x-axis.

Fig 3: Add a scale-bar, and overlay and align the four panelsbetter. This image suggests that the snow cover may cause a larger error in the glacier outline than suggested in the text. Also, the snowline delineation appears to be problematic.

Fig 4: This figure suggests that interseasonal variability is higher than interannual variability and trend. Use that in the text to calculate a netter error estimate.

Figs 5 and 6: nice and clear figures

Fig 7: I wonder if there are any significant volcanic eruptions than can be indicated in this figure, which may have affected the glacier mb for a year or two. The figure caption is quite wordy. An ‘unbroken line’ is a solid line. The Lambatungnajökull dotted line should at least have msymbols on the line for the years for which the remote sensing or mapped data is available,

Fig 8: This figure nicely show the scale and extent of overdeepenings in Iceland. Try to explain some of the retreat patterns in Fig 7 from the margin position in the overdeepenings and the formation of proglacial lakes.

Fig 9: caption: ‘in geographical order’: do you mean ‘from west to east’?

Fig 12: As stated before, the assumptions for the mass calculations are so general that this figure is perhaps useful when the average for each zone is given, rather than each individual glacier.