GENERAL COMMENTS

This paper and the release of the underlying glacier inventory are an important step in the completion of the world glacier inventory. The paper presents, for the first time, a reliable measurement of the glacierized area outside the Greenland ice sheet, based on established semi-automated glacier outline digitization from mainly orthorectified Landsat 7 scenes. The resulting outlines and areas of the ice masses are a baseline for ice extent peripheral to the Greenland ice sheet around the year 2000. Additionally, DEMs were used to extract ice divides and separate the ice masses into individual glacier units, and extract a range of topographic attributes. The paper presents the used datasets, the methods and the results in a clear way. The authors also attempted to classify levels of connectivity of the local glaciers to the ice sheet. Although this is commendable, I explain below that this aspect may need more extensive work before it is presented in such a prominent and conclusive way.

Overall, the paper is of good scientific quality and clear. The work is novel and of generally high standard. I have some major concerns with parts of this paper (e.g. error analysis, connectivity classification) and feel that some many other parts need minor improvement and strengthening. The language and writing style will need to be improved, and many of the figures need adjustments. More details of my suggestions for major changes, and minor corrections, are given below. I have tried to minimize repetition of Graham Cogley's referee comments. Overall, I feel this paper presents important results and a diverse set of glacier data, and that it is appropriate for publication in TC, once reviewers' comments are addressed.

MAJOR COMMENTS

TITLE: The title is misleading, as the inventory excludes the Greenland ice sheet and directly connected ice masses. Rename to e.g.: "The first complete glacier inventory of ice masses peripheral to the Greenland ice sheet."

Thank you for the comment. Actually, the term "glacier" means that we talk about glaciers rather than the ice sheet, so we think it is clear. But in response to the review of G. Cogley we have argued that we prefer (for consistency reasons) to stick to the GCOS terminology glaciers and ice caps (GIC). We have now changed the title to "The first complete inventory of the local glaciers and icecaps on Greenland". We think this also clarifies that we do not address the ice sheet.

LANGUAGE AND WRITING STYLE: The use of English is poor at times. The writing style is convoluted and contains errors, including erroneous direct translations from German. Avoid the use of acronyms and abbreviations if not necessary (in the abstract), be consistent in verb tense within a section, and use commonly accepted terminology (e.g. not 'entities' but 'glacier units', change 'glaciers and ice caps (GIC)' to 'ice masses' (Paul et al., 2009)). Some major errors are indicated in my comments below, but I also recommend the authors to spell-check (e.g. 2400-20: arround) and ask a colleague who is proficient in scholarly English to correct for 'sentence flow'. Some place names should be checked directly with GEUS (Anker Weidick or Michele Citterio) or Higgins (2010). Some of Weidick's (1995) spelling may no longer be official (see also http://www.oqaasileriffik.gl/en/resources/greenlandiccity_andsettlementnames).

We apologize that the language and grammar was poor at times and consistency in the terminology used was partly lacking. We have considered all stylistic comments made by G. Cogley and will pass the final ms to a native speaker before resubmission. For reasons of consistency with earlier works and the terminology used by GCOS for the essential climate variable, we prefer to stick to "glaciers and ice caps (GIC)" for the ice masses we have investigated.
Thank you for the comment on the place names. We will take your suggestions into account and change it appropriately.

QUANTIFICATION OF ERRORS: The ice mass outline error needs to be better quantified.

a) Ice margins are from summer images. Since the extent of calving margins has annual fluctuations of > 1 km for larger outlet glaciers, with a most retreated position in summer (Howat et al., 2010; Bevan et al., 2012), the glacierized area given in this paper thus a minimum extent. Mention this, and, if possible, estimate the seasonal fluctuation (even if just regionally).

You are fully correct; there are strong seasonal fluctuations of the terminus position of some larger outlet glaciers. But these mainly emerge from the ice sheet rather than the local GIC and do thus not influence the results of our inventory and the related error quantification. We therefore tried to select images at the end of the ablation season to keep this error as small as possible. Compared to other factors influencing product accuracy (e.g. seasonal snow hiding the perimeter) we think that the error in glacier area due to seasonal fluctuations of the terminus is very small.

b) A figure with digitized glacier margins overlain onto Landsat 7/Landsat TM of 1-2 land-terminating glaciers with/without dirty ice or moraine, 1-2 tidewater-terminating glaciers in a fjord with sea ice as well as a glacier margin with an adjacent ice shelf, will demonstrate the digitizing accuracy and problems better.

We fully agree that such images would highlight the problematic issues better and will add further overlay images for illustration.

c) Though manual correction of seasonal snow was applied, there may be regions where this remains problematic (especially in ice divide regions, e.g. Fig 3). Say something more about this potential error.

Yes, definitely! We discuss this point in more detail and include also images illustrating the problematic issues (see point b),

d) Mosaicking errors: Landsat images were not mosaicked before digitization: were there no connection shift problems detected when the outlines were mosaicked?

Partly, yes, but in general they were within the geolocation accuracy of the Landsat L1T product (+/- 1 pixel). Whenever possible, we have performed operator controlled mosaicing, i.e. GIC were split along natural divides (or not at all) rather than along scene boundaries.

e) Reprojecting: The magnitude of area errors in UTM depend on latitude. Mention a % range for the Greenland latitude range. However, reprojection also introduces errors, which should be given as a potential % error for the latitude range as well.

This is correct but, in our opinion, a minor factor in regard to other errors. Nevertheless we can investigate this issue and insert it, if it is worth to mention in the paper.

f) The cumulative error calculation needs to be clear (e.g. Jiskoot et al., 2012: p 37).

A cumulative error can only be calculated when change assessment (from different sensors) is investigated. But we have only used the USGS L1T product and a wrong geolocation has very little impact on the resulting glacier area.

g) It would be helpful to add a table with the total glacierized area based on the UTM projection as well as the Greenland Lambert Azimuthal Equal Area projection, as well as areas of the largest glaciers (e.g. in Fig 1 plus 1-2 large glaciers in the far south and west) based on both projections. This is important for the comparison of areas given in other publications, which have sometimes used UTM projections.

The area change due to reprojection or mosaicing of up to three UTM zones is very small (<1%) compared to other errors. We include now an information about this issue and the difference in the overall area.

In the same table areas of the overlapping largest glaciers from other regional inventories can be listed (e.g. Weidick et al., 1992; Citterio et al.,2010; Palmer et al., 2010; Jiskoot et al., 2012), and a full discussion of accuracy and subjectivity of glacier delineation in complex glacier systems can be discussed.

We fully agree that there is some subjectivity in the interpretation of glacier margins, but think that they are difficult to overcome in general. For the location of a drainage divide and the best thing one can do is to provide the outlines to the community, apply simple rules strictly, and work with as little manual intervention as possible. In this regard there is full traceability and rigor, and modifications can be applied to the digital dataset if required. In other words, a direct comparison of individual glacier units from various studies can only be performed when drainage divides are digitally available for all of them (which is actually not the case). We included this information in the discussion.

CONNECTIVITY: It is an admirable and novel to try and assign a level of connectivity to the Greenland ice sheet, but I have problems with way this is defined (especially CL1 and CL2). I feel it needs to be further explored and justified before releasing the dataset with that attribute, and before separating glacier areas (Table 1) on the basis of the connectivity levels and discussing regions (Fig 1 and e.g. 2409: 22-27). The now prominent result of the connectivity classification could be mentioned as a first step, but physically meaningful ways of connecting need to be explored, which may ultimately lead to a separate paper.

We fully agree with you on the last point. We have asked several colleagues on various conferences/meetings/workshops to give us a feedback on our proposed connectivity levels with the general result that they need to see it before they can comment on it. Thus this assignment has to be seen, as a feasible starting point rather than to consider everything that could be calculated in the GIS (which is indeed much more). So there was some pressure to set this up without knowing who requires what. We thus decided to keep it as simple and transparent as possible and keep the assignment away from personal judgment (as you will get 10 different suggestions from ten persons) and to apply it automatically. We still have the struggle between hydrologists (and GIC modelers) that also want to have CL2 as belonging to the GIC class and glaciologist (ice sheet modelers) that want to have CL2 being a part of the ice sheet (which was finally used for the IPCC AR5 assessments)

My major concerns with the used connectivity classification are as follows:

a) The edge of the Greenland ice sheet is not an exactly known boundary (see also Cogley's comments).

Yes, indeed. But this will be the case for the decades to come and we think it was important to start and provide a clear suggestion rather than further discussing it.

b) For a connection between ice masses to be meaningful (at least for hydrological and glaciological modelling) it should be based on physical processes: these could include shared ice dynamics (e.g. confluence), shared accumulation (divergence), or a connected hydrological system (e.g. subglacial lakes). Physically, it also matters whether units are confluent or divergent, even though when either separate both configurations may change from fully- connected to unconnected. The connectivity levels used in this paper are based on line- connectivity, not on basin configuration or physical connectivity.

This is fully correct, but our approach is (at least to our knowledge) the only way and also the standard way to do it. In an ideal world, drainage divides would be based on subglacial topography rather than changes in surface flow direction derived from a DEM.

c) On the annotated Fig 3 (Supplement), I demonstrate that it is illogical to have glacier units connected to other units that have been assigned CL1 (or CL2) automatically adopt the same class. In the top blue circle the number of ice divides between the ice sheet outlet are written in each glacier. Most of these ice divides are not shared accumulation areas, but arêtes or cirque headwalls which may have seasonal snow. Glaciers marked with '0' are tributaries of the ice sheet outlet, and clearly must have a different connectivity than 1-10. Additionally, the southerly flowing 1-10 don't discharge into the same fjord as the ice sheet outlet. The lumped CL2 connectivity level has thus neither a glaciological nor a hydrological meaning. I would argue that glaciers marked 10 have at least the same level of unconnectedness to the ice sheet as the glaciers labelled CL0. In the lower circle I have indicated missing ice divides (and arrows for flow directions). Here, it is not clear to me why the yellow and red glaciers marked '1' have two different connectivity levels.

Thank you for this thoughtful suggestions and comments. First of all, we have to repeat that the assignment was done fully automatically, i.e. it is well possible that there are inconsistencies (e.g. due to geolocation issues). On the other hand, we have shown here only one example region. If we adjust the rules here, they will likely not work well in other regions (e.g. where the central red region is an ice cap with various outlet glaciers). This as a general background. Then to the heritage rule:

As you certainly can imagine, we had long discussions if we should apply the rule in the way illustrated in Fig. 3. The key argument was finally to keep it simple. Of course, we 100% agree with you that you can additionally subdivide CL1 into a class that is directly connected (but separable) to the ice sheet or a CL2 glacier (marked 0 in the annotated Fig. 3) and another class that is separated by 1, 2, 3, 4, or even more divides and has in this regard little in common with the ice sheet or CL2 glaciers. But here personal taste might differ even stronger than for the other rules and we have thus decided not to touch it. The good thing is, when such a further division is required for any application, anybody can reselect the CL1 glaciers and reclassify them according to personal rules. For our purpose it was only required to propose what should be part of the ice sheet and what not. For this paper we decided to count CL1, CL0 and CL2 as local GIC. But as explained above, some modelers prefer to have also CL2 or even CL1 as part of the ice sheet (in particular when they are large ice caps). So it's a matter of personal choice.

The CL2 glaciers you see in this image do all either have an uncertain divide to the ice sheet in their accumulation area or contribute to the flow of an ice sheet outlet glacier in the ablation region (the yellow glacier with the 1 and the flow arrows). The nearby red glaciers with a 1 are clearly separable in the accumulation area (e.g. by rock outcrops) and are not connected to the yellow glacier, i.e. they have CL1. So this is at least glaciologically consistent. We agree that an ice divide is missing for the region with the arrows in the circle (though this would not change its CL2 assignment).

d) Geikie Plateau (for example) is a separate accumulation zone with radial flow into complicated outlet glacier systems, of which some are confluent with outlets from Watkins Bjerge accumulation area, but not with those of the Greenland ice sheet. Watkins Bjerge in the paper's connectivity should have a CL1, but Geikie Plateau and outlets CL2

We agree that the GIC in the Watkins Bjerge region might also get CL1, but there are two reasons why they also have CL2: First of all: the location of the drainage divide is highly uncertain as the topography is very flat in this region (our perspective), and secondly the entire region is traditionally seen as a part of the ice sheet by modelers (user perspective). So for the time being (IPCC AR5 calculations) we prefer to have this region in CL2 as well. Nevertheless we will carefully check again the smaller glaciers surrounding the major streams and if necessary refine it.

COMPARISON OF DEMs (3.3): Stauning Alper, is probably one of the most inappropriate regions for the comparison of DEMS that are not exactly taken in the same years. Firstly, the region has a high percentage of surge-type glaciers (Jiskoot et al., 2003), which deplete rapidly during quiescence, and during a surge thin and thicken suddenly, and may change ice divide position. Secondly, the ASTER GDEM has been found inaccurate in high steep terrain (Frey and Paul, 2012), such as the Stauning Alper. This known inaccuracy should be mentioned and referenced. I suggest another region, devoid of surge-type glaciers, is used to demonstrate differences in DEMs.

We agree that glaciers do not provide stable ground in this region and any changes that might be visible of glacier surfaces might be due to real surface changes rather than DEM differences. However, as we do not determine surface elevation changes but topographic parameters these changes (if recognizable at all) have very little influence on the parameters (min, max and mean elevation). The key point for selecting this region was to visualize the artifacts of the GDEM in the accumulation region as visible in the hillshades.

DISCUSSION: In the minor comments I give suggestions for strengthening parts of the discussion that are too vague or incomplete.

MINOR COMMENTS

Abstract: State in a new sentence how many of the ice masses are ice caps. Delete CL0, CL1, CL2 here, as the explanations are sufficient.

We have not assigned and counted ice caps separately. One reason is that there is no clear distinction possible between an ice cap and a valley glacier with a dome-shaped accumulation area, another one is that we have separated some of the ice caps into glacier entities so that they are no longer ice caps from this point of view. Both points are related to the specific interpretation of the analyst and the number itself has thus little scientific merit (e.g. how small can an ice cap be?)

Introduction: don't skip from what you have done to past inventories and back to your work: reorder into a more logical sequence.

OK, we are going to do that.

2400-4: and FOR the past, potential and future.

Done

2400-14: larger THAN

Done

2400-16: Did you mean former instead of latter? Even for the former this is incorrect: Some estimates of local ice cover on Greenland are in the order of 70 000–100 000 km2, with extremes up to 163,200 km2 (Thomsen and Weidick, 1992; Yde, 2011). Many of the estimates were originally considered as mimimum estimates, as they were based on large-scale mapping which excluded the smaller glacier units (Yde, 2011). Delete 'according to all', and adjust the percentage.

We have only included here more recent studies that had access to high quality cartographic material. We would argue the extreme numbers reported in Sharp (1956) and based on the estimate of Hess (1933) do not belong into this class. However, we will adjust the ‘all’ and better explain this in the revised ms.

2400-17: smaller THAN

Done

2400-20: around

Done

2400-20-23: not sure what the authors are trying to say here about aspect because of the sentence structure. Not sure if this really belongs in the abstract. The median elevation is strongly dependent on the underlying topography, which generally increases with distance from the ocean.

We agree that also the topography (elevation range) increases inland from the coast, but a related higher potential elevation range of the glaciers has very little influence on the median or mean elevation itself. This value is strongly controlled by the climatic regime (i.e. precipitation amounts) as numerous studies have shown (e.g. Ohmura 1992). The aspect dependence of mean or median elevation has been shown by Evans (2006) and confirmed in numerous further studies analyzing topographic glacier inventory data. So we think these are two interesting points deserving a short note in the abstract.

2401-5 incorrect sentence after the comma, replace with ‘but inventory information is incomplete’.

Done

2401-6: Delete ‘and differently used’

Done

2401-8: database is one word

Done

2401-15-23: Delete this section, as it contains methods, discussion and results and should be in the summary but not intro. If information given here is missing from the methods section includes it there instead.

We agree that this section is a little bit out of place and have moved it to the end of the introduction. Actually, the problem of separating the local GIC from the ice sheet was a key motivation for our study and a most important part of it. We would thus prefer to have the ‘What is the problem?’ issue already mentioned in the introduction.

2401-25: Replace “Geikie glacier inventory (Jiskoot et al., 2012)” with ‘the Geikie Plateau and Scoresby Sund regions (Jiskoot et al., 2003; Jiskoot et al., 2012).’ The first paper is an inventory that covers both regions in 1995-1996 and is downloadable from GLIMS, but there were problems with the projection. The second paper is an updated inventory of the Geikie Plateau region only, based on 2000-2005 outlines, and will be uploaded to GLIMS soon.

Ok, thank you for the clarification.

2402-5: Kargel et al. (2012) not 2011

Done

2402-11: They range up to 1000000 km2 (Thomsen and Weidick, 2012).
Can you please provide us this reference? We cannot find it.

2402-12-14: poor phrasing after the comma: direct translation from German.

Done

2403-6: Gunnbjørn Fjeld (correct spelling: Higgins, 2011).

Done

2403-8: Weidick (1995): not et al: see references. Correct this throughout the paper.

Done

2403-9: Sector division: If I compare your Fig 1 with Weidick (1995) Fig 1 I don't see a further section in the south. His sections are just labeled southeast, southwest and southern west. Be more specific if you did anything different. What is the basis of your sector division lines: do you exactly follow Weidick's major divisions? With the present knowledge of flow directions and slopes I suggest using ice divides on the basis of Rignot and Mouginot (2012) Fig 1F. Here you can see it does not make much sense to label a section South, as the flow direction is clearly southeast or northwest (see your Fig 7). Why do you have SE, S, SW, W, NW, etc in your table A4, but don't indicate these divides in Fig 1?

The idea to divide our study region into these four sectors was basically to allow for more regional assessments that are related to major climatic differences. This division refers only to the local GIC and not to the ice sheet (or its flow directions). The divides are just on top of the ice sheet for graphical reasons. We fully agree that the division can be done differently leading to different results in the statistical analysis. For consistency with earlier studies we are now going to introduce new sectors following partly Rignot and Mouginot. The South sector will be separated in two and the North Sector will be separated in three (East, North, West) parts.
The eight cardinal directions listed in Table 4 refers to the mean aspect sector of the glaciers rather than the topo-climatic differentiation in 4 zones.

2404-3: Replace the sentence with" ...., for instance in the Stauning Alper and Geikie Plateau regions (Jiskoot et al., 2003 and 2012; Weidick, 1988) and the Disko–Nuussuaq region (Yde and Knudsen, 2005).
If you want to keep to Weidick's spelling it is Disko–Nûgssuaq. Remove Jiskoot et al. (2001) from the reference list and replace with Jiskoot et al. (2003).

Done

2404-12: These TM scenes cover a long period: at least indicate on the footprint map which areas were in need of this filling of data gaps.

This can unfortunately not be visualized in the map (too small), but all data are provided in Table A1 for reference. After May 2003 the Landsat ETM+ scenes have the scan line error and were often mosaiced.

2404-22: Hans Tausen Iskape: in Fig 1 this is called: Hans Tausen ice cap. Be consistent. Weidick (1995) spells it as Iskappe.

2404-23-24: What does 'partly not consider' mean? Also, you exclude glaciers smaller than 0.05

km2 so this should not be a problem.

Done

2404-26: ‘stick to the’ is non-scholarly language.

Done

2405-1-2: sentence needs a reference.

Done

2405-7: ASTER GDEMII: give a range of years of ASTER images on which the was based and a reference (or website).

We have added a reference for the GDEM2. The years of the ASTER images used to create the GDEM2 were unavailable and actually also not required in this case (e.g. we have not calculated elevation changes).

2405-9-10: This sentence needs to be at the end of the Introduction, or near the beginning of the Study Region.

Done

2405-13: ….INTO three steps. There are 3 major steps and then within those sections more steps. This is confusing. Use different terms for the two levels.

We have added the section numbers after each step to clarify where these points are discussed.

2406-13: This is an important step, but is not the second (but the third within overall step a: very confusing), and these are in fact two steps. Mosaicking and Reprojecting.

See comment above.

2406-23: GREATER than 30 %

Done

2406-25: ‘for clean ice’: what was the accuracy for snow, dirty ice, etc? The margins of land-based glaciers are often not clean. Also, what is the precision of calving margins? Did you have problems with fast-ice or pack-ice? How were ice shelves removed (2404-22) and what is the estimated error?

The accuracy assessment is indeed an important point, but requires some differentiation (we have more clearly described this in the revised paper):
(1) Clean ice is well mapped by the automated method and does not require manual corrections. The accuracy given refers to its potential uncertainty of +/- 1/2 pixel along the entire perimeter.
(2) Areas with debris covered ice, and shadow were partly not mapped (omission errors) or water, ice bergs and sea ice were wrongly mapped (commission errors). These regions were corrected manually with the original satellite image in the background (contrast enhanced false colour composites)
(3) Snow is correctly mapped by the algorithm, but we don’t want to have seasonal snow outside of glaciers in the inventory. Most snow patches were removed by applying a size threshold (> 0.05 km2) or deselecting the related polygons manually (either directly or with the drainage divides enclosing the glacier)

(4) The terminus of calving glaciers was in nearly all cases clearly visible as we only used late summer images with mostly no sea ice. In the case of ice bergs or frozen water close to the terminus this was more difficult to do, but in most cases straight forward as well. The largest uncertainty here is related to the western outlet of Flade Isblink, which was really poorly constrained by optical contrast.

2407-6: remove ‘that are explained in the following.’ This phrasing is unnecessary when the explanation follows directly.

Done

2408-1-16: Two other methods are through (visual) glaciological interpretation of surface flow (Racoviteanu et al., 2009), or through ice flow direction from gravitational driving stress (for which you need a surface and bed DEM, smoothed (Bevan et al., 2012). Both can be more accurate than just relying on the surface slope from DEMs. Did the authors assign an uncertainty to their ice divides (e.g. Paul et al., 2009)?

We are aware of these possibilities but have not yet seen how they work in practice. Moreover, we do not have the flow fields (we would need them for all GIC) or bed topography. So we prefer to work with the widely applied standard here (see also comments above). The ice divides are sharp in regard to the DEM used, i.e. we have not analyzed the impact on using another DEM on the location to determine uncertainty (basically we do not have another suitable DEM). Consequently, uncertainties were not assigned to the ice divides.

2408-16: Delete ‘and can be discussed’ (is implicit in ‘subjective’).

Done

2410-11: Wrong calculation: Jiskoot et al (2012) state that the entire glacierized area is 41 591 km^2 of which 90% is tidewater terminating = 37432 km^2. Minus Kong Christian IV (11 079km2) is 26 352 km^2. This is quite close to the 24 494km^2 (+/- 750 km^2), given the error margins in both inventories. This is actually quite encouraging, given the two different projections (we used UTM) and the complexity of drainage basins in this region!

Thank you for this comment and please apologize our wrong calculation. We have corrected it accordingly and agree that the difference is now rather encouraging.

2411-7: peak elevation?

We have changed this to maximum elevation.

2411-7-26: The discussion of the cause of the elevation differences belong in the discussion section. The authors should give data on measured MAAT and accumulation rates (from papers using model output in combination with meteorological stations: Weidick (1995, p C19) and papers by Box and/or van den Broeke will be useful): both factors influence the glaciation level and equilibrium line. This discussion should be expanded and improved.

We agree that parts of this section have more a discussion character and have moved this to section 5.3. But the reminder is closely related to the derived data and more a description rather than a discussion (i.e. discussing our results in regard to other studies) and thus should be here.

2412-17: doms should be domes

Done

2412-26: Also compare the assumed area of the Greenland ice sheet with the range of estimates given in Kargel et al. (2012: TC)

Thank you for this remark, we have now added this comparison.

2413- 5-15: A range of 70 000–100 000 km2 is given by Thomsen and Weidick (1992) and (Yde, 2011), so this discussion will need adjustment.

We do not find Thomsen and Weidick (1992) Can you please provide it?

2413-17-25: this discussion is too vague.

We fully agree that this part of the discussion sounds vague, but actually it covers some key points that are most important to stress: the number of glaciers (though often reported) has little scientific value as it depends on the rules applied. This has also consequences for the aspect distribution. And - rather different from other regions - mean elevation does here not depend on aspect. We think these are all relevant and important points relating our results to results of previous studies.

2414-2: The continentality effect. That it is shown 'for the first time from the topographic glacier parameters in Greenland' is not entirely true as e.g. Jiskoot et al. (2003 and 2012) indicate differences in snowline along the coast and inland in East Greenland. See also Weidick (1995: C19-22). Many models also show this distribution, which should be brought into this discussion.

Thank you for this comment. We will change it accordingly in the revised ms.

2414-18: 'values calculated here' is vague: do you mean the ice divides, hence glacier outlines and areas, or the elevation/aspect data or both?

In principle both as they influence each other, but the latter are not very sensitive to it as transitions in this region are rather smooth. So it is mainly glacier area we refer to here. We have rewritten this section.

2415-1: Straightforward is one word

Done

2415-19-25: this may need to be adjusted: see earlier comments.

OK, we can do that.

2415-27: 'might not yet be fully consistent': do you mean you still have to do some error checking?

In principle, yes as due to geolocation glitches some glacier outlines might be connected to others although they are not (see also the discussion above). But this should only have a small impact on the numbers in each class, as we have checked all larger glacier complexes carefully. We are going to clarify this.

2516-11: correct: ....largely due to differences in terrain topography, continentality and mass balance (precipitation rates and temperature).

Done

2516-113-15: rather than 'hints' it confirms the effect of continentality on the precipitation rate.

Done

FIGS:
For all figs that have satellite images as background give the type, scene, bands, and date, in the fig caption. Many figs are missing location, and scale, and some could use glacier names.

Thank you for this comment. We agree that the figures should be improved in this regard and have added the required information.

FIG 1: All the numbers for the largest glaciers are in the wrong location! See e.g. Weidick (1995), Jiskoot et al. (2012) or Rignot & Mouginot (2012) for correct locations. 'Blackicebank' is not a glacier: do you mean Sortebrae??
Geikie (spelling!) Plateau should be turned 90° counterclockwise, and not cover Gåseland with its letters 'Ge'. It is only the NE part inland of Blosseville Kyst, not the entire region (see Jiskoot et al., 2012: Fig 1, and Higgins, 2010)

OK, we can do that.

FIG 2: Replace 'Glacier outlines' with glacierized area delineation (delineation of ice masses). Change 'glacier entities' to 'glacier units'.

We prefer to stick to the topological name here to make clear that it is one polygon with one ID. Glacier unit is somewhat arbitrary and can be confused with a unit for measuring glaciers (e.g. size in km2).

FIG 3: Give lat-lon and a scale bar, or a location box in FIG 1. Adding some glacier/peninsula/island names will also clarify the location. See referee Supplement figure and comments above, for concerns with the connectivity.

OK, we can do that.

FIG 4: This fig does not really show the difference between the ice cap with topographic structure and without. Topographic structure may be missing at the scale of the used DEM, but this is not visible on the image. Superimpose DEM shading or contour lines to clarify. The lat-lon tick marks need to be in a larger font.

OK, we can do that.

FIGA2: Why use triangle locations that are not at the tidewater margin? Instead, give two shades to the glacier outlines, with the darker indicating the TW terminating and lighter the land terminating glaciers (e.g. Jiskoot et al., 2012: Fig 1). The glaciers area drained by TW margins can then be easily assessed visually.

OK, we can do that.

REFERENCE LIST:
Weidick (1995) is the sole author: Williams and Ferrigno are editors. See below for the correct reference.

ADDITIONAL REFERENCES: Thanks a lot.

Bevan, S. L., Luckman, A. J., Murray, T., 2012. Glacier dynamics over the last quarter of a century at Helheim, Kangerdlugssuaq and 14 other major Greenland outlet glaciers, The Cryosphere, 6, 923-937.

Frey, H, Paul, F., 2012. On the suitability of the SRTMDEM and ASTER GDEM for the compilation of to-pographic parameters in glacier inventories, International Journal of Applied Earth Observation and

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