The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2017-234-AC1, 2018
© Author(s) 2018. This work is distributed under
the Creative Commons Attribution 4.0 License.

The Cryosphere
Discussions

Interactive comment on "Reconstruction of the Greenland Ice Sheet surface mass balance and the spatiotemporal distribution of freshwater runoff from Greenland to surrounding seas" *by* Sebastian H. Mernild et al.

Sebastian H. Mernild et al.

smernild@gmail.com

Received and published: 10 April 2018

To the Editor of The Cryosphere April 10, 2018

Dear Editor,

First of all, thank you for your mail by 4 February 2017 and for the valuable and thorough comments from the reviewers. We apologize for the delay of our reply.

Included below are our comments for the paper: "Reconstruction of the Greenland Ice

Sheet surface mass balance and the spatiotemporal distribution of freshwater runoff from Greenland to surrounding seas" by S. H. Mernild, G. E. Liston, A. P. Beckerman, and J. C. Yde based on the comments from the reviewers.

On the next pages, the reviewers' comments are shown with blue color and our responses to the reviewers' comments are shown with black color.

After the submission of this manuscript to TC (autumn 2017), a study by Mernild et al. was published (https://www.tandfonline.com/doi/full/10.1080/15230430.2017.1415856). This study included a detailed evaluation of SnowModel/HydroFlow simulated GrIS air temperature, SMB, ELA, and catchment outlet river runoff for the Kangerlussuaq sub-catchment in west Greenland. The same model setup, forcing and DEM were used in both studies, but on different domains. The reviewers requested a SnowModel GrIS evaluation, and this is now available (see more below).

Thank you for your help. If we can be of any further assistance, please feel free to contact me.

Best regards Sebastian H. Mernild and co-authors

—

Anonymous Referee #1

Review of: "Reconstruction of the Greenland Ice Sheet surface mass balance and spatiotemporal distribution of freshwater runoff from Greenland to surrounding seas", by S. H. Mernild et al., submitted to The Cryosphere.

General comments Using the updated version of SnowModel/HydroFlow, the authors simulate the surface mass balance (SMB) and components, i.e. runoff, melt and retention, of the Greenland ice sheet (GrIS) at 5 km resolution for the period 1979-2014. Precipitation is downscaled from ERA-Interim re-analysis for the same period. The model includes a snow module accounting for meltwater retention in snow, an energy balance scheme and a meltwater runoff routing module. This routing module allows to quantify the runoff contribution of 6 GrIS sectors, further refined to over 3,000 individual catchments. The authors first discuss the modeled contemporary (1979-2014) GrIS SMB and recent trends (2005-2014) in these 6 sectors. The analysis is further extended to the numerous individual catchments to show that about 80% of these have experienced increasing meltwater runoff since 1979. Then, the authors correlate this recent runoff increase with the natural variability of the Atlantic Multidecadal Oscillation (AMO) and North Atlantic Oscillation (NAO).

The manuscript is overall well written, but the results presented seem inaccurate and raise questions and concerns on the model ability to reproduce the contemporary GrIS SMB, potentially altering the conclusions drawn in this paper. These concerns are summarized in the Substantive Comments. In brief, I suspect that the current version of SnowModel/HydroFlow has several issues resulting in inaccurate SMB estimates over the GrIS. Compared to other studies, runoff is significantly overestimated likely due to inaccurate representation of meltwater retention in firn. This study suggests that only 25% of melt is retained in the firn pack while most recent efforts demonstrated that it is closer to $\sim$45%. Such a retention underestimation has severe implications on runoff calculation and SMB. For instance, the authors present negative modeled GrIS-integrated SMB for the period 2005-2014 as opposed to recently published GrIS mass balance and GRACE studies. This makes these results potentially unreliable and the conclusions drawn inconsistent. Authors: We are grateful to the reviewer for pointing out the overestimation in runoff caused by underestimation of meltwater retention in the firn pack. The model simulations were tested and evaluated by Mernild et al. (2018) based on long-term observations from the GrIS. Due to the GrIS surface water balance components (using the hydrological method, the continuity equation): P – (Su + E) – R + ΔS = 0 $\pm$ ðİIJĆ, a change in one parameter effects ΔS, also referred to as SMB. The evaluation against runoff observations from the GrIS have caused adjustments to SMB and retention/refreezing. Therefore, the text (Results and discussion section), Figures and Tables have been updated since the last version of this manuscript. For

the GrIS, we find that the 35-year mean refreezing and retention was estimated to be 49 % (269.9 $\pm$ 77.4 Gt yr-1), and it was 45 % (318.0 $\pm$ 62.8 Gt yr-1) for the period 2005–2014. The Substantive Comments have been addressed below.

For these reasons, I judge that the manuscript cannot be published in the current form and needs major revisions, unless the authors prove that their estimates of precipitation, runoff, melt, refreezing and SMB are reasonably accurate. To achieve this, the authors must perform a thorough model evaluation against in situ SMB measurements and compare their results to remote sensing mass change records, and mass balance estimates compiled in previously published studies. This would highlight potential issues in their snow model and provide some insight on how to solve them. If these evaluations/comparisons and validation of the model results can be successfully achieved, and if the suggested corrections listed hereunder are applied, I would be happy to reassess this manuscript.

Substantive Comments 1 Throughout the manuscript, the authors discuss changes in SMB components and recent trends without providing a thorough evaluation of their modeled SMB estimates. Nowadays, comprehensive in situ ablation (Machguth et al. [2016]) and accumulation (Bales et al. [2001, 2009]) data sets are available over Greenland to evaluate modeled SMB in time and space. Such evaluation must be performed and discussed in the manuscript to provide some insight on how well SMB components, i.e. notably precipitation and runoff, are represented in the SnowModel/HydroFlow model. Such evaluation is now systematically performed in Greenland mass balance publications, i.e. Fettweis et al. (2017), Noel et al. (2016), Niwano et al. (2017) or Langen et al. (2017). Authors: We thank the reviewer for bringing this to our attention. SnowModel/HydroFlow evaluations have been conducted against independent long-term observations from the Kangerlussuaq GrIS catchment, e.g. against air temperature observed at AWS located on the GrIS (at the K-transect for AWS: S5, S6, and S9 to illustrate the elevation variability in MAAT; van den Broeke et al. 2008a, 2008b; van de Wal et al. 2005), GrIS SMB observations to illustrate the elevation variability

(K-transect for AWS: S4–S9 and SHR; van den Broeke et al. 2008a, 2008b; van de Wal et al. 2005), GrIS ELA estimations (van de Wal et al. (2012); van As et al. (2017), and observed GrIS catchment outlet river runoff (Hasholt et al. (2013); van As et al. (2017)). The Kangerlussuaq GrIS catchment is the best observed catchment in Greenland – and therefore, it was used as a test site. These evaluations have been published in Mernild et al. (2018, doi.org/10.1080/15230430.2017.1415856). The Mernild et al. (2018) study uses the exact same model settings, forcing, and DEM as this study. The Mernild et al. (2018) study can be seen as a Part 1, where this study can be seen as Part 2 (similar Part 1 and 2 studies were seen in Liston and Mernild (2012, JCLI) and Mernild and Liston (2012, JCLI), where Part 1 was an evaluation of the model and Part 2 an application of the model). In this TC manuscript, we follow the advice from the reviewer by evaluation SnowModel/HydroFlow against GrIS observations from Mernild et al. 2018. We used observed GrIS independent values for validation – we did not use other model simulations for evaluation. However, in the Result and Discussion section we discussed our simulated results of SMB and retention/refreezing against outputs from other model studies, see Section 4.1.

2 GrIS-integrated SMB components presented in this study, i.e. notably runoff, meltwater retention and SMB (see Table 1), do not generally agree with recent GrIS SMB studies, e.g. Fettweis et al. [2017], van den Broeke et al. [2016], Noel et al. [2017], Mottram et al. [2017] or Vizcaino et al. (2013), suggesting potential issues in SnowModel/HydroFlow. For instance, Table 1 shows that only 25% of meltwater is retained in snow while other recent studies suggest 45%, e.g. van Angelen et al. (2013), Noel et al. (2017), Steger et al. (2017a). Steger et al. (2017b) performed a similar basin analysis of SMB components (8 sectors) of the GrIS using another state-of-the-art firn model (SNOWPACK); the authors must compare their results to that study and discuss the differences. Authors: We agree with the reviewer. Therefore, simulated runoff was evaluated against observations from the Kangerlussuaq catchment and adjusted. In the study by Mernild et al. (2018) simulated runoff was on average overestimated by 31 % (2007-2014) compared to observations. This was likely because of missing mul-

tiyear firn processes, such as nonlinear meltwater retention, percolation blocked by ice layers, and refreezing in SnowModel, more specifically in the submodel SnowPack-ML. Therefore, based on the findings from the Kangerlussuaq catchment, we have now adjusted runoff due to the missing firn routines, and subsequently retention/refreezing and SMB were changed. After the GrIS runoff, retention/refreezing, and SMB adjustments, the 35-year (1979-2014) runoff, retention/refreezing and SMB values seems in line with earlier studies, although SMB was in the low end compared to other model SMB simulations (see Section 4.1).

3 Figure 2f strikes me as the ablation zones in north Greenland, i.e. notably in northwest and northeast Greenland are by far larger than in other studies (Fettweis et al., 2017; Noel et al., 2016; Mottram et al., 2017; Vizcaino et al., 2013; Cullather et al., 2014). In addition, Table 1 suggests that these northern basins contribute equally or more runoff than southern basins, e.g. NW and NE contribute 70 Gt/yr and 63 Gt/yr on average (1979-2014). The study also suggests that these northern basins showed a negative SMB on average (1979-2014), meaning that these have been losing mass for more than 30 years. This is not supported by other studies, e.g. Mouginot et al. (2015) and Steger et al. (2017b). Authors: After adjustments and recalculations of GrIS runoff, retention/refreezing and SMB (Figures and Tables were updated), our values were compared to earlier studies and discussed in Section 4.1. Mean refreezing and retention was estimated to be 49 % (1979-2014), and 45 % (2005-2014), and not around 25% as in our earlier version of this manuscript. These values (49 and 45 %) are more in line with previous studies. A comparison and discussion of SnowModel simulated GrIS SMB values were further done against other SMB studies (section 4.1).

4 The authors state that they use an updated version of the SnowModel/HydroFlow model but they never discuss the relevant changes implemented, or their impact on the modeled data. The authors should at least list and discuss relevant model updates, compare results from their previous and current model version, and explain where and why changes occur. This would highlight the novelty of the presented data set. To me,

it is not clear whether this new data set is a general improvement on previous versions. Authors: We agree with the reviewer that the implications of using the improved model should be better clarified. The text was rewritten to make this clearer. The novelty of this work can be summarized by the following bullet points: 1) never before we have simulated GrIS SMB for present day conditions using the ERA-Interim (ERA-I) reanalysis atmospheric forcing (in earlier simulations, only observed meteorological data from AWS were used); 2) we implimented retention/refreezing conditions in more detail than in earlier SnowModel GrIS studies; 3) we simulated spatial runoff in more detailed than earlier; and 4) we improved the understanding of the spatiotemporal distribution of freshwater river runoff from Greenland to the ocean using the statistical EOF method. The spatiotemporal distribution of runoff is important because the GrIS and Greenland plays an essential role in the Arctic hydrological cycle and for the individual catchment budgets, where river runoff is the hydrological link between snowmelt and ice melt and hydrographic and circulation conditions in fjords and the adjacent seas.

5 Tables and figures are sometimes very difficult to read and interpret, especially Figure 5 and Table 1. These may potentially confuse the reader with too much information. Suggestions to improve the text, figures and tables are provided below. Authors: Figure 5 and Table 1 were updated and changed. We hope that this has improved the readability. Please see further points below.

Point Comments L59-61: In Wilton et al. (2016), Fig. 2 only shows SMB as low as ∼100 Gt/yr in years 2010 and 2012, the same applies for runoff. This should be clarified in the manuscript. Authors: The text was rewritten following the advice from the reviewer.

L65-66: I would reformulate as follows: "(Chen et al. 2017), and up to 43% for the GrIS and peripheral glaciers and ice caps in 2010-2012 (Noel et al. 2017)". Authors: The text was rewritten following the advice from the reviewer.

L70: I think the authors mean "melt season duration" instead of "surface ablation duration", see Tedesco et al. (2016). Authors: Yes, thanks. This was fixed.

L100-102: I think this sentence is somewhat misleading as Enderlin et al. (2014) compiled estimates of solid ice discharge from ∼178 marine-terminating glaciers around Greenland. Could the authors reformulate to clarify this? Authors: The text was rewritten.

L121: In the abstract and at L134, the authors state that they use ERA-I reanalysis to force their model, while automatic weather stations are mentioned here. Could the authors clarify and further elaborate on how their model was forced? It would also be useful to learn more about how the snowpack was initialized at the beginning of the simulation. Authors: Here, we talk about how the study by Mernild and Liston (2012) was forced with data from AWS. The present study was forced with ERA-I. The text was rewritten to clarify that Mernild and Liston (2012) was forced with data from AWS. The snowpack was initialized in September 1 each year.

Section 2.1: Here, the authors state that they use an updated version of the SnowModel/ HydroFlow model but do not discuss the changes implemented in this new version, nor the impact on the modeled results. See also our Substantive Comment 4. Authors: The text was rewritten to clarify this.

L169: Could the authors clarify what they mean by "water-equivalent evolutions"? I assume this is related to meltwater retention/refreezing in the firn pack and runoff production. Please, elaborate. Authors: The text is rewritten to make this clearer.

L171: Could the authors explain what they mean by "hypothetical gridded topography and oceanmask datasets"? Authors: The word 'hypothetical' was erased. The text is rewritten to highlight that the runoff configuration was based on gridded topography.

L184: Could the authors briefly explain how the 6 or 12-hourly forcing fields were downscaled to 3-hourly data in MicroMet? Replace "and" by "on" before "a 5-km". Could the precipitation underestimation suggested at L338-343 be the result of this downscaling? Authors: The downscaling to 3-hours is now explained, and the word 'and' was changed to 'on'. No, the downscaling to 3-hours has nothing to do with the underestimating mentioned at L338-343.

L188: Could the authors mention the original resolution of the DEM presented in Levinsen et al. (2015)? Authors: The original resolution was 4 kmˆ2; 2 $\times$ 2 km. This information was added to the text.

L213-215: Why do the authors obtain more catchments? Is this a result of the DEM and/or model updates? Please, clarify. Authors: Yes, this is a result of the new DEM.

L216-223: Here, the authors describe MicroMet and then resume their discussion on HydroFlow. I would therefore suggest moving these sentences to L201 on page 9. Authors: Is done.

L221-222: Could the authors provide a reference that corroborates this assumption on blowing snow? Authors: Reference is added.

L244: Could the authors provide a reference for the 10 hydrometric monitoring stations? Authors: Reference is added.

L252: For model evaluation, the authors refer to a paper that is not published yet. As the authors analyze average SMB components and recent trends, I feel that a proper evaluation, as suggested in the Substantial Comment 1, of the modeled SMB data set must be added here. This would provide some insight on the model performance across the GrIS ablation and accumulation zone, i.e. how well the model simulates runoff and precipitation, respectively. Without model evaluation, the discussion on SMB components and recent trends in Section 4 is somewhat insubstantial. Authors: The mentioned paper is now published: Mernild et al. (2018), doi.org/10.1080/15230430.2017.1415856. In this paper, an evaluation is done for the only GrIS catchment where long-term observed AWS data, GrIS SMB data, and catchment runoff data are available. The model evaluation has been mentioned in Section

2.3.

L308-317: These lines are somewhat descriptive, additional insight on the model performance could be gained by performing the SMB evaluation against in situ measurements in the accumulation zone using Bales et al. (2001, 2009). Authors: Sure, we did additional SMB comparisons using e.g., Bales et al. (2001, 2009), see Section 4.1.

L338-351: Here the authors discuss uncertainties and underestimation of precipitation in the ERA-I forcing field based on, e.g. Fettweis et al. (2017). Again, a proper evaluation of SMB in the accumulation zone would be more convincing to evaluate precipitation and quantify a potential bias that could be further corrected. See also Subtantial Comment 1. Authors: An evaluation of SMB was shown and discussed in the study by Mernild et al. (2018) using observed SMB data from AWS from the K-transect, covering the period from 1990-2014. This evaluation was done for the K-transect stations (S4, S5, SHR, S6, S7, S8, S9, and S10) located in elevation from c. 340 m a.s.l. to c. 1,850 m a.s.l., where AWS S9 likely are located in the accumulation zone, since ELA is within the elevation range of 1,610-1,800 m a.s.l. (van de Wal et al. 2012; van As et al. 2017). Further, an evaluation of SnowModel simulated ELA was done (the location of ELA is based on both accumulation and ablation processes), showing 35-year mean ELA simulated values (1,760 $\pm$ 260 m a.s.l.) to be within the range of ELA values from other studies 1,610-1,800 m a.s.l. (van de Wal et al. 2012; van As et al. 2017) within the Kangerlussuaq catchment, SW Greenland.

L366-371: I don't think that discussing "ablation" adds relevant knowledge to the paper. The authors should better discuss and evaluate melt, runoff and refreezing in more detail. I would also replace ablation by runoff fields in Figs. 2, 4 and Table 1. The authors could consider including these information about "ablation" in a Supplementary Material. Authors: Ablation as well as accumulation are important processes to understand the GrIS SMB conditions. Therefore, we keep the discussion of ablation in the paper. We have added runoff time series to Figure 4 and Table 1, as runoff is an important part of the ablation, but also to show all the water balance components together (Equation

1, Section 2.4).

L375: The authors probably mean "573.7 +- 119.8 Gt yr-1", which is relatively high compared to other estimates. See also the next comment and Substantial Comment 2. Authors: The reviewer is correct. The manuscript has been updated after Mernild et al. (2018) was published, after the evaluation and adjustment. Further, see comments after the Substantial Comment #2.

L389-392: The ablation zone in north Greenland, especially in northeast and northwest, are relatively large compared to e.g. RCMs estimate from Fettweis et al. (2017, Fig. 6a), Mottram et al. (2017, Fig. 5) and Noel et al. (2016, Fig. 1), or GCMs estimate from Cullather et al. (2014, Fig. 9) or Vizcaino et al. (2013, Fig. 7), suggesting ablation overestimate in SnowModel/ HydroFlow. Could the authors elaborate on this? Again, it would be very useful to perform a proper SMB evaluation in the ablation zone of the GrIS. This would allow for estimating SMB (runoff) uncertainty and make the following regional SMB (runoff) analysis more robust. See also Substantive Comment 1. Authors: Absolutely, and that is why SMB was compared to observed SMB values from the K-transect, also covering elevations ranging into the ablation zone, below around 1,600 m.asl. (AWS S4-S9 and SHR). In the Result and Discussion section, we compare SnowModel simulated SMB against other studies. Further, see comments after the Substantial Comment #1.

L404-406: I'm not sure to understand these lines, could the authors reformulate? Authors: These lines were erased to avoid any confusion.

L407-419: The authors obtain a refreezing-retention fraction of 25% for the period 1979-2014, which is by far lower than other estimates of ~45%, e.g. Steger et al. (2017a) or Noel et al. (2017). This could likely explain why runoff is so high compared to other studies, e.g. Van den Broeke et al. (2016). The authors should stress this as these inaccuracies may strongly impact the discussion of contemporary runoff production and recent trends discussed in the paper. In addition, the recent study of Steger et

al. (2017a) also integrated refreezing (Gt/yr) and fraction (%) from two state-of-the-art firn models (IMAU-FDM and SNOWPACK) over the same 6 GrIS sections discussed here. For all these sections, the refreezing fraction is lower by almost a factor of 2 compared to Steger et al. (2017a). See also Substantive Comment 2. Authors: Sure, we are aware about this, but after the evaluation and adjustment in Mernild et al. (2018) we end up with a refreezing-retention fraction of 49% (1979-2014) and 45% (2005-2014). These values are more in line with other studies. This is discussed in the text.

L415: The authors should refer to Steger et al. (2017a) rather than Ettema et al. (2009). Authors: Sure, is fixed.

L438-439: Here the authors state that their runoff and SMB product is improved, but no comparison with a previous version or with observations has been conducted. Could the authors elaborate on how they draw these conclusions. Authors: These lines were erased. Please see our response to substantive comment 4.

L442-443: Given the potential underestimated precipitation, and overestimated runoff, the obtained SMB product is unrealistic. Van den Broeke et al. (2016, Fig. 9) show a reconstruction of GrIS mass balance, solid ice discharge and SMB, clearly refuting an average SMB of ~120 Gt/yr for 1979-2014. This is also supported by other studies: e.g. Fettweis et al. (2017, Fig. 8) and Mottram et al. (2017, Fig. 3). In addition, they obtain a negative SMB after 2005, which is again not supported by other studies. Authors: This sentence was reformulated due to the updated (evaluated/adjusted) SnowModel/HydroFlow simulations and calculations, based on the Mernild et al. (2018) study.

L459: It is misleading to say that Wilton et al. (2016) obtained a GrIS SMB of ~100 Gt/yr in the late-2000s, as this is only true for years 2010 and 2012. Authors: Thanks for making this mistake clear to us. The text is corrected and the years 2010 and 2012 are used instead.

L480 and 482: The authors probably refer to Figures 5a and 5b. Authors: Yes, the reviewer is right.

L498-500: I agree that many catchments show this out-of-phase pattern but there are still quite some that don't. Could the authors quantify this, e.g. as a percentage of the number of catchments in southeast Greenland. Authors: This is now done, and it is around 20 %.

L509-514: While I agree that some correlation exists between EOF1 and AMO, it is not so clear for NAO (see Fig. 9b, r =0.4 or r2 =0.16). I would suggest some more caution when drawing firm conclusions on these teleconnections, as at e.g. L44-45, L519 or L544-548. Authors: We erased the word 'strong' related to the correlation, to be more caution when drawing conclusions (related to L509-514). Also, other places where mentioned by the reviewer (L44-45, L519 or L544-548) the text was carefully looked through to be more caution when drawing conclusions on these teleconnections.

L538-540: This sentence is misleading, to my knowledge no other studies show average SMB of $\sim$120 Gt/yr for 1979-2014, nor a negative SMB in the period 2005-2014. I think the authors should reformulate to stress this. Authors: This sentence was reformulated due to the updated (evaluated/adjusted) SnowModel/HydroFlow simulations and calculations, based on the Mernild et al. (2018) study.

L545: I would replace "indicates" by "suggests" as the correlation obtained for AMO and notably NAO are relatively low. I suggest: "This suggests that runoff variations are related to large-scale natural variability of AMO and NAO in Greenland." Authors: Is fixed.

L549: My main concern on using the data set presented in this study is that the modeled runoff and SMB are by far overestimated and underestimated, respectively, when compared to other studies. It is therefore questionable whether this data set accurately reproduces the contemporary SMB of the GrIS, if it can be used to force ocean models or to quantify mass changes over Greenland. Authors: We see the concern of the reviewer. In the catchment-scale study by Mernild et al. (2018,

doi.org/10.1080/15230430.2017.1415856) (see also other places in this reply letter) GrIS surface conditions and Greenland runoff conditions were tested against independent observations, from where simulations were evaluated and subsequent adjusted. Simulated runoff was adjusted against observations and subsequent calculations were redone.

Stylistic comments L33: I would suggest 'resolution' instead of 'increments'. This holds for the whole manuscript. Authors: Is fixed.

L34: I suggest: 'Compared to previous studies, simulated SMB is low whereas the GrIS surface conditions remain similar.' In addition, the authors should use the present tense here and at L34-40. Using the past tense is confusing as it suggests that the authors discuss previously published model results. Authors: The first part of the comment was erased from the abstract. Present tense was used in the abstract.

L49: Present tense should also be used in the introduction and following sections when referring to the data discussed in this study. Authors: This is an interesting question raised by the reviewer. Basically, the rule of thumb is when refereeing to studies which had happened (already been published) the text should be written in past tense. If one is describing a figure or a table, then the text should be in present tense.

L71-72: I would suggest: "[. . .] because meltwater may be retained or refrozen in the porous [. . .]". Authors: Is fixed.

L89: "particularly common". Authors: Is fixed.

L92: "[. . .] understanding is used to explain [. . .]". Authors: Is fixed.

L104: Remove the "of" before "catchments". Authors: Is fixed.

L109: Remove "the" before "link". Authors: Is fixed.

L110: I would suggest: "This has further implications [. . .]" as the "unaddressed knowledge gap" is already mentioned in the previous sentence. Authors: Is fixed partly.

L127: Maybe replace “land area” by “tundra region”. Authors: Is fixed.

L140: Remove “conditions”. Authors: Is fixed.

L141: Remove “(the last decade [. . .])”. Authors: Is fixed.

L150: I would suggest: “spatiotemporal patterns of runoff”. Authors: Is fixed.

L155: I would replace “verification” by “evaluation”. Authors: Is fixed.

L161: I would suggest: “interpolation scheme. Interpolation fields were adjusted [. . .]” Authors: Is fixed.

L172: Remove “from” before “catchment outlets”. Authors: Is fixed.

L173: Replace “tested” by “evaluated”. Authors: Is fixed.

L189: I would suggest “resolution” instead of “increment”. Authors: Is fixed.

L201: Replace “of” by “with” before “glacier ice”. Authors: Is fixed.

L230: Remove “, which include a part of the GrIS”. Authors: Is fixed.

L231: I would suggest: “[. . .], feedbacks from a thinning ice [. . .] will not influence the catchment [. . .]”. Authors: Is fixed.

L235-236: Remove “, not by the glacial drainage system.” as this is already mentioned in the previous sentence. L236: Maybe “obtained” instead of “gained”. Authors: Is fixed.

L240: “Evaluation” instead of “Verification”. I would also suggest this throughout the whole section (L249, 251). Authors: Is fixed.

L253: “SMB” instead of “surface mass balance”. Authors: Is fixed.

L285: I would suggest: “The latter analysis enables to link changes in, for example, NAO or AMO with GrIS outlet catchments mass loss and runoff.” Authors: Is fixed.

L299 and 301: Remove “balance” before “loss”. Authors: Is fixed.

L319: Refer to Fig. 1b after “six sections”. Authors: Is fixed.

L323-324: I would suggest: “[. . .] towards the steep slopes of the southern coast of Greenland, generating orographic enhancement [. . .]”. Authors: Is fixed.

L341: “between 642.0-747.0 Gt yr-1”. Authors: Is fixed.

L349: Maybe: “This highlights the importance of accurately representing precipitation for estimating the energy [. . .]”. Authors: Is fixed.

L352-354: I would suggest: ”Besides precipitation, melt (including extent, intensity and duration) and ablation are other [. . .] and understanding GrIS SMB. Surface melt can influence albedo, as wet snow absorbs [. . .]”. Authors: Is fixed.

L357: “[. . .] affect total runoff, but also ice dynamics [. . .]”. Authors: Is fixed.

L366: The authors certainly mean “northern and southwestern sections”. Authors: Is fixed.

L374: “in southwest Greenland” and for clarity add “over the GrIS” after “period”. Authors: Is fixed.

L394: Maybe “Therefore, in that region the snowpack persists longer compared to [. . .]”. Authors: Is fixed.

L402: Maybe “within the range of our previous study (Mernild et al., 2008)”. Authors: Is fixed.

L448: This sentence could be removed. Authors: Is erased.

L455: “24.7 Gt decade-1”. Authors: Is fixed.

L484: Maybe “variance” instead of “variation”. Authors: Is fixed.

L489-490: Replace “goes down/up” by “decreases and increases”. Authors: Is fixed.

L515: This sentence could be removed. Authors: Is fixed.

Figures and Tables Figure 2c and e: The authors should display regions showing surface melt = 0 in white. This would highlight the dry snow zone of Greenland. The authors should also show runoff instead of ablation using a color scale similar to the one used for melt, i.e. runoff = 0 in white. Authors: Figure 2c: Since this is 35-year mean spatial surface melt the grid values will never be equal to zero if just melt occurred a single day. That is the reason why we used the purple color for mean annual melt values below 0.0625 m w.e. Figure 2e: we don't see the issues here – it could be ablation (R+Su+E) and/or runoff (R) alone displayed. Since Su+E is relatively low, ablation and runoff would be rather insignificant difference. However, in Figure 4, the runoff time series (shown in red color) are shown together with retention/refreezing time series.

Figure 3: The scale of SMB components is too small, and numbers are difficult to read. I would also suggest showing values ≤ 0 in white. Authors: The numbers were made bigger in size. It seems not proper to show refreezing/retention values ≤ 0 as white color, since the ocean around Greenland already is white. It can provide misunderstandings.

Figure 4: The authors should better show time series of runoff instead of ablation. Authors: Now runoff time series are shown together with ablation time series.

Figure 5: This figure is rather overwhelming and confusing. It is very difficult to interpret the data or identify any spatial pattern. In addition, the representation of individual catchment in color is somewhat redundant as it is already shown on Fig. 1c. Therefore, I would suggest to display runoff, variance and trends for each catchment using a color scale instead of circles. For trends, a blue-to-red scale, centered on 0, could be used to distinguish negative from positive values. Authors: The representation of individual catchments in colors are erased due to avoid redundancy. For each catchment, a circle is shown where the size depends on the mean runoff volume. The suggestion

by the reviewer of making runoff, variance, and trends following a color scale bar was tested, but it seems even more confusing (it is very difficult to differentiating the different colors) and this gives even less overview of the differences between catchments (when plotting it is even more difficult to get an overview of the runoff variability from 3000+ catchments). After careful considerations, we have therefore decided to keep the runoff volume illustrated as circles. Here, positive trends are shown in red color, and negative trends in blue. The negative trends are placed on top of the positive trends to better highlight the differences between increasing and decreasing trends.

Figure 6: EOF2 and EOF3 are not significant and the associated figures are not discussed in the paper. Therefore, these could be moved to a Supplementary Material. A new Figure 6 could consist of 3 subpanels combining Figs. 6a, 7 and 8a, all referring to EOF1. Authors: Sure, a new Figure 6 was established (combining the 'old' figures 6a, 7, and 8a).

Figure 8b and c: These figures could be shown in a Supplementary Material as they are not discussed in the main manuscript. Authors: All figures related to EOF2 and EOF3 was placed in a supplementary material.

Figure 9: The x-axis of the lower Fig. 9a and b should read "years". Authors: Is fixed.

Table 1: This Table is rather overwhelming and shows too much information. I think that ablation and "E + Su" could be removed as they are not discussed in detail. In addition, the description of refreezing and retention could be included in the figure caption instead of within the Table itself. Authors: To understand the water balance components, we think that it is important to still keep 'E+Su' in the Table. Otherwise, a part of the water balance is not shown. We followed the advice and removed the description of refreezing and retention. The description is now in the caption.

Additional references and DOI âĂć Steger et al. (2017a): https://doi.org/10.3389/feart.2017.00003 âĂć Steger et al. (2017b): https://doi.org/10.5194/tc-11-2507-2017 âĂć Langen et al. (2017):

https://doi.org/10.3389/feart.2016.00110 âĂć Mottram et al. (2017): 10.14943/lowtem-sci.75.105 âĂć Noel et al. (2016): https://doi.org/10.5194/tc-10-2361-2016 âĂć Niwano et al. (2017): https://doi.org/10.5194/tc-2017-115 âĂć Van den Broeke et al. (2016): https://doi.org/10.5194/tc-10-1933-2016 âĂć Vizcaino et al. (2013): 10.1175/JCLI-D-12-00615.1 âĂć Cullather et al. (2014): 10.1175/JCLI-D-13-00635.1 âĂć Machguth et al. (2016): 10.1017/jog.2016.75 âĂć Bales et al. (2001): 10.1029/2001JD900153 âĂć Bales et al. (2009): 10.1029/2008JD011208 âĂć Mouginot et al. (2015): 10.1126/science.aac7111 Authors: Some of these published studies are referred to in the text, where we found it relevant.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2017-234, 2017.

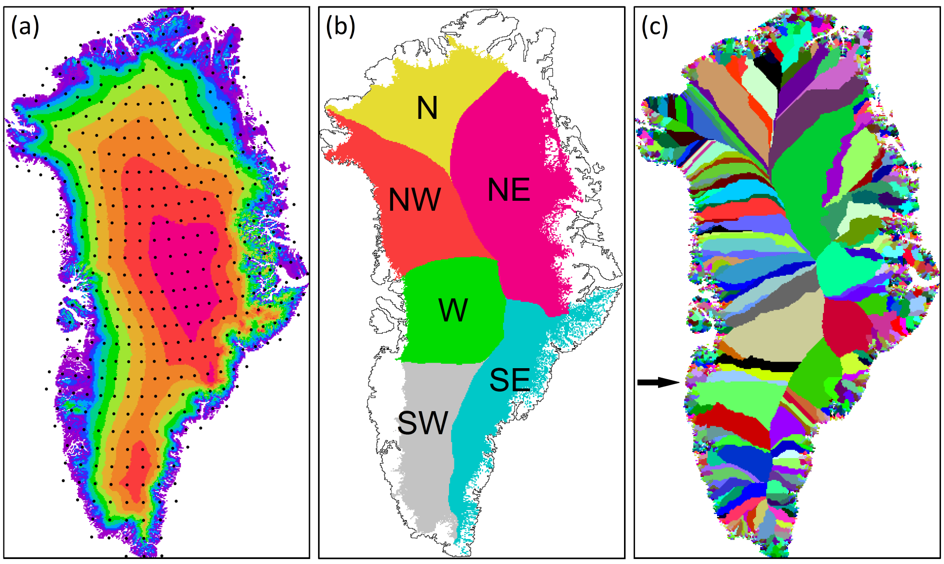

Fig. 1. Figure 1

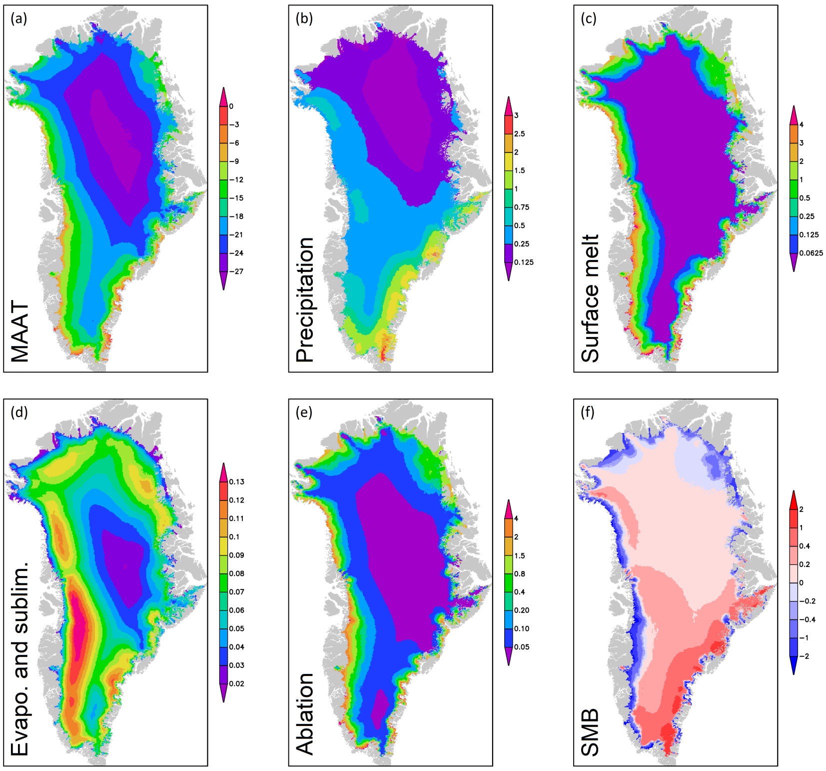

Fig. 2. Figure 2

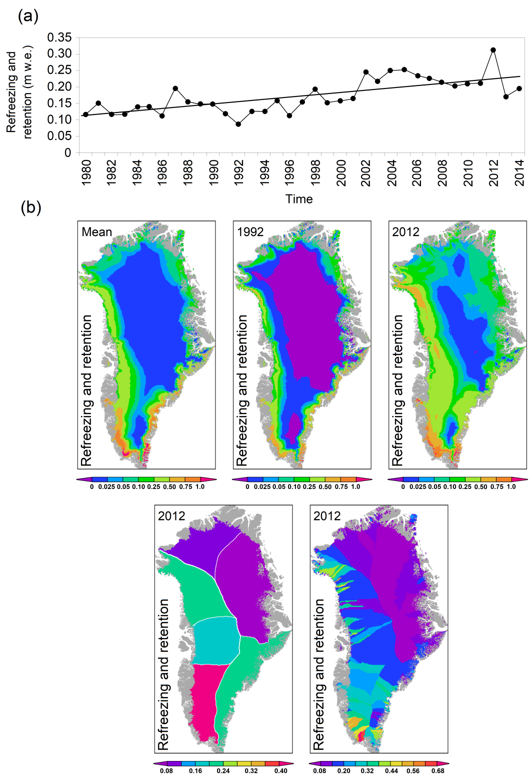

Fig. 3. Figure 3

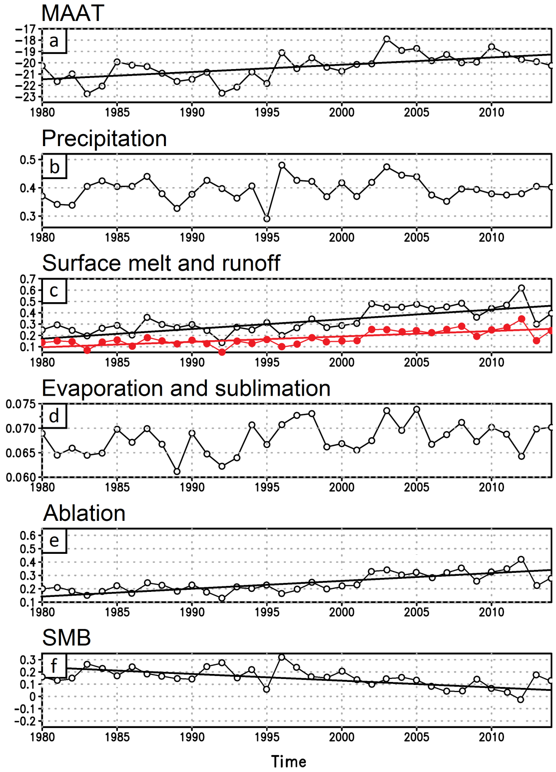

Fig. 4. Figure 4

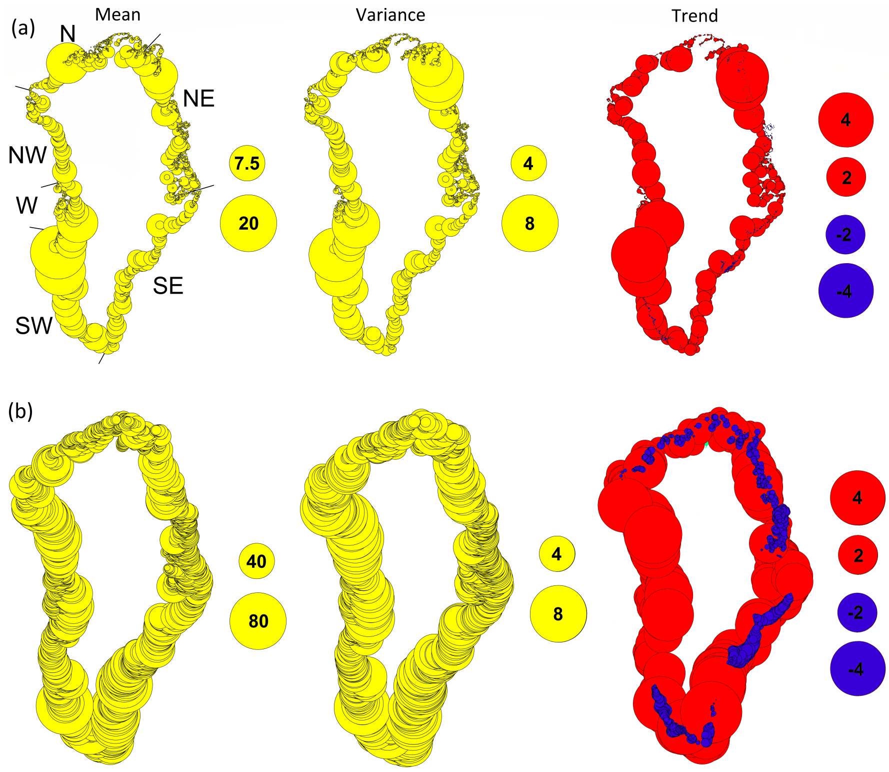

Fig. 5. Figure 5

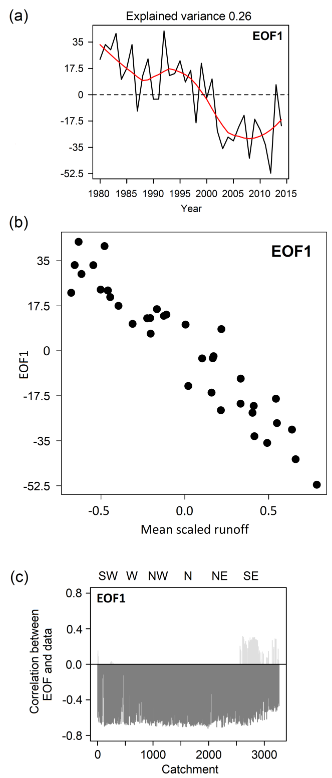

Fig. 6. Figure 6

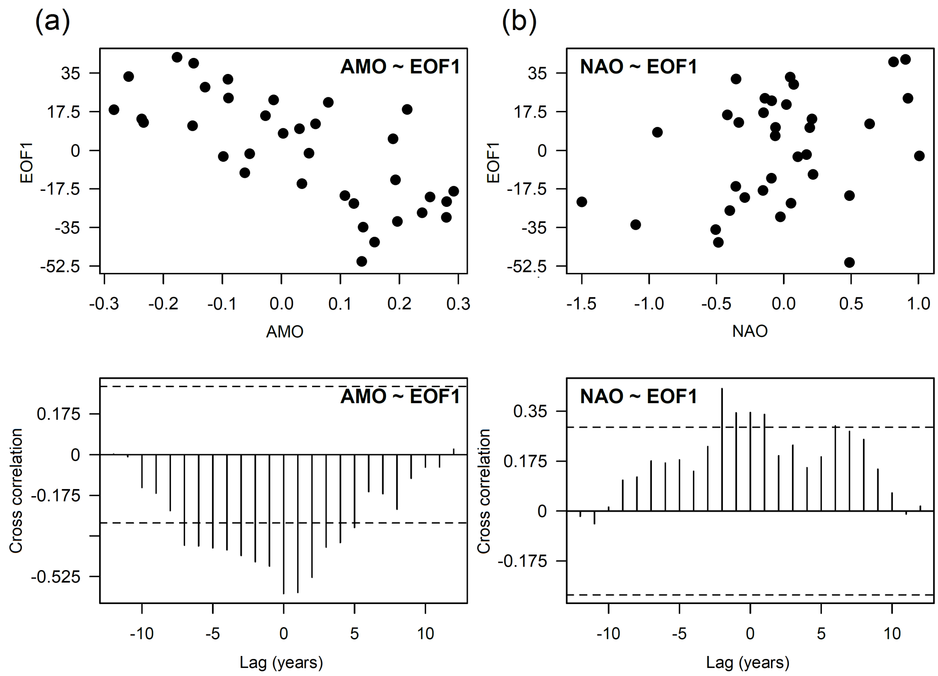

Fig. 7. Figure 7

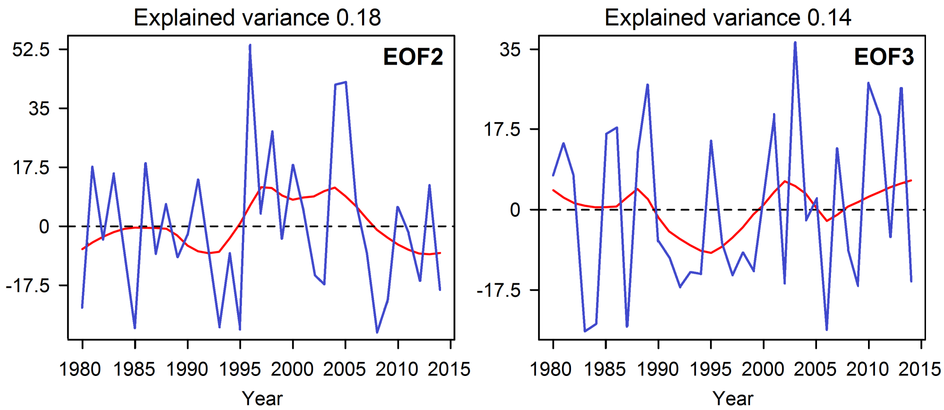

Fig. 8. Figure S1

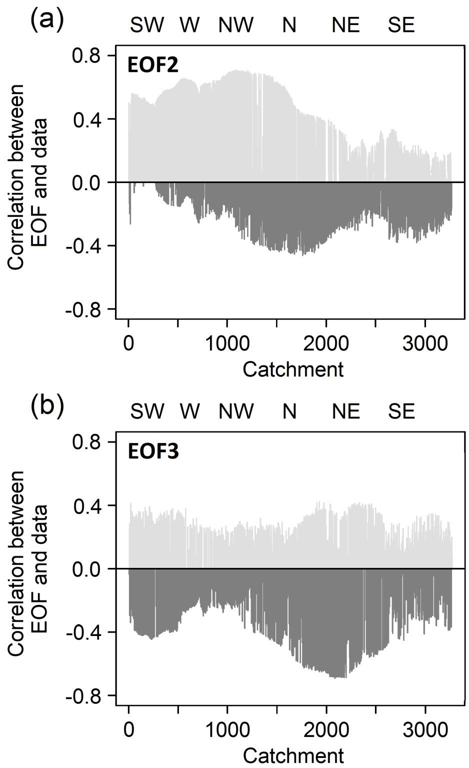

Fig. 9. Figure S2