The Cryosphere Discuss., 6, C1156–C1166, 2012
www.the-cryosphere-discuss.net/6/C1156/2012/

The Cryosphere Discussions

Interactive comment on "Recession, thinning, and slowdown of Greenland's Mittivakkat Gletscher" *by* S. H. Mernild et al.

Anonymous Referee #2

Received and published: 11 August 2012

General Statement

In this paper the volume changes of a mid-sized glacier on Greenland are estimated and compared with changes in observed surface velocity. The authors conclude that the observed decrease in ice velocity is an effect of reduced ice deformation due to the thinning. Furthermore it is stated that thinning and deceleration are likely to be also present on other glaciers in the region. The study is generally well written, but I do have a number of serious concerns with respect to the scientific content and statements made in the paper. To my opinion this study cannot be published in its current form but requires very substantial revisions. Please find below first a list of major concerns followed by detailed suggestions.

Major Remarks

1. Reading through the text and comparing the results to previously published materials from the same first-author, one becomes aware that a considerable amount of the here presented results has been published in a similar form before. Please find a detailed listing of these sections under *"Detailed Suggestions"* below. To my opinion it is essential that these statements are either completely removed or listed in a *"Background"* or *"Study Site"* section.

2. To my understanding the present study does use only surface topography and thickness (Knudsen and Hasholt, 1999) from one point in time and extrapolates using the mass balance observations. The authors base their entire analysis on these extrapolated ice thickness changes that are not measured directly. It is a well known fact that mass balance observations based solely on the direct glaciological method bear a large risk of systematic measurements errors leading to significant over or underestimations of longer-term mean mass balances (e.g. Andreassen, 1999; Cox and March, 2004; Huss et al., 2009; Cogley, 2009). Thus it should be considered a standard that measurements carried out with the direct glaciological method are compared against geodetic mass balance observations. This might be especially the case for Mittivakkat where according to the data presented in this study the stake network does not cover the entire glacier (Figure 1a). Winter mass balance seems to be measured over an even more restricted perimeter (Figure 3a). To my opinion it is a prerequisite to the analysis performed in the present study that the mass balance observations AND at least two DEMs from two different points in time are used. As long as this is not done, the foundation of the entire study must be seen as weak.

3. I am seriously concerned with the validity of the linear extrapolation of mass change from 1994 back to 1986. On the one hand the authors do not justify their extrapolation apart from the statement: *"This is a simple approximation of*

the 1986 mean ice thickness, but we have confidence in the method, since the trends in air temperature and precipitation for the region during 1995–2011 are consistent with trends for 1986–1995 (Mernild et al., 2012b)". Firstly the authors do not show that their extrapolation has any statistical reliability and significance. Secondly, from studying Figure 2a and 2b in Mernild et al. (2012b) I get the impression that the trend from 1986 to 2008 is clearly not linear. In addition, the inter annual variability is shown to be very large for both precipitation and temperature. I doubt that it is sufficient for the two rather short time spans (1986–1995 and 1995–2011) to look at trends alone and not discuss inter annual variability. On the other hand the calculated mass loss is at least partly contradicted by the comparison of a digitized 1981 1:20000 map and the surface topography derived during the 1994 radio-echo sounding campaign (Knudsen and Hasholt, 1999). The comparison is summarized by Knudsen and Hasholt (1999) in the following statement: *"The map of surface change {. . . } shows changes within +10 m except at the margin where ice losses up to about 30 to 40 m are seen. This probably indicates that much of the glacier was rather unchanged during the period 1981–1994."* It is unclear to me why this 1981 map (further detailed maps from e.g. 1972 are also available, cf. Knudsen and Hasholt (1999)) is not used nor mentioned in the study. Figure 4 would look much different with a horizontal line (volume) drawn from 1981–1994. I would strongly suggest to completely remove the linear interpolation and to work with the available surface topography from the maps. This would also help solving the issues mentioned under point 2 above.

4. I do have problems reproducing the calculation of thickness changes. I understand that the methodology is described in Section 3.2 and the main result is shown in Figures 5c and 5d. However, I found it puzzling to understand which GPS data (single frequency and/or dual frequency) where used for what purposes in the calculation of thickness changes. In addition to that I am afraid that the input data to the equations (1), (2) and (3) have considerable uncertainties

and those are not discussed at all. Furthermore Figures 5c and 5d strike me because after the consideration of w_e there is almost no change in surface elevation above 400 m a.s.l. Considering the very negative mass balances, how is this possible? Of course the thickness loss due to negative mass balance in the ablation area is replaced by ice flow from the accumulation area. However, on Mittivakkat the ELA was by average (1995–2010) at 730 m a.s.l. (Mernild et al., 2011) with a mean annual mass balance close to -1 m w.e. and it is very difficult to imagine where this mass replacing the negative mass balance between 400 and 730 m a.s.l. should come from. Please either revise the entire calculation or make clear why the presented changes in thickness are correct. This would involve to present figures that allow for the reader to follow and comprehend the argumentations in the text: The reader does not know which stake is located where, and to color code a large amount of stake numbers instead of labeling them (cf. Figure 1a) is simply an unfeasible solution. Thus it becomes almost impossible for the reader to understand where on the glacier the profile shown in Figure 5d starts and ends. In addition to that, the labeling in Figure 5 is mostly unreadable as pointed out in the following.

5. Figure 3 and 5 (and to a lesser extent 1a and 7) do feature fonts that can not be read at the resolution provided. I am not able to assess their quality nor read any data from Figures 3 and 5. To my opinion figures carry much of the story of an article and if provided at this level of quality, it become difficult to assess the paper as a whole. It looks like all figures included are JPG graphics and although this seems to be a minor comment, it results in various small fonts becoming unreadable. I would encourage the authors to submit their figures as vector graphics whenever possible. Figure 3 and 5 must be enlarged in any case, also when submitted as e.g. EPS file. I would like to add that I am not entirely sure whether the low quality of the figures is also a problem of the Journal. However, checking through a few recent papers in TC/TCD I found mostly excellent vector

graphics (also maps!). At this occasion I would also like to ask the editorial team at The Cryosphere to do a quick check of the figure quality prior to publication to avoid such issues.

Detailed Suggestions:

1. Page 2006, line 13: *"Satellite observations show area losses for most other glaciers in the region"* Strictly speaking, this is not a result of this study but is a repetition from Mernild et al. (2012a).
2. Page 2006, line 15: Where is it shown that other glacier than Mittivakkat are also slowing down? Although likely, this is to my opinion a speculative statement that I would not mention in the abstract.
3. Page 2006, line 22: In the meantime the first complete glacier inventory for Greenland has been published, showing that these numbers were far too low. According to Rastner et al. (2012) local glaciers and ice caps around Greenland cover approximately 90'000 km^2. I would suggest replacing the out-dated numbers.
4. Page 2008, line 8: Please rewrite the units to avoid confusion with "per millimeter". How is the surface slope calculated and based on which data? The variable is important in the equations used later to calculate thinning. To my opinion it is essential that the authors provide more details including an uncertainty assessment.
5. Page 2008, line 13: What exactly is meant with *"updated"*. Is this the mean AAR for 1995-now? Please specify.
6. Page 2009, lines 9–11: Tachikawa et al. (2011) clearly state that the deviation of GDEM v2 and ICESat over Greenland is on average 235 m with a standard

deviation of 535 m (and reaching extreme values of as much as $\pm$4000 m)! The same authors also state that only over ice free areas and/or provided a rather high scene-count the standard deviation falls to 12 m. How did you deal with the fact that the GDEM is calculated using scenes from various years? Please clearly show that GDEM v2 is a valid choice for Mittivakkat and your purposes.

7. Page 2009, line 29: *"(this omission is not likely to bias the results)"* I appreciate that the lack of observations for a larger part of the glacier is clearly indicated in the text and the figures. However, I am not sure whether measuring only half of the accumulation area does not bear the risk of systematic errors. Please explain why this is not a problem and how the mass balance is extrapolated to the unmeasured part of the glacier.
8. Page 2010, line 1: *"(This movement has an insignificant impact on estimates of the mean annual surface velocity.)"*. This could indeed be true. However, I believe it is still essential to provide evidence that this is the case. For instance short-distance mass balance variability could be investigated with respect to the stake movement.
9. Page 2010, line 8: The volume can be either estimated or observed, in this case it is estimated.
10. Page 2010, lines 10–12: Unfortunately this statement is unclear to me. How did you deal with the area change while subtracting *"cumulative observed net ablation"* from the observed mean 1994 thickness? Strictly speaking, *"cumulative observed net ablation"* does exclude accumulation. I suppose you mean "cumulative observed mass balance"?
11. Page 2010, line 23: While the horizontal accuracy of a hand held Garmin 12 XL GPS receiver can be $\pm$2 m under good satellite coverage, my own experience has shown that the vertical accuracy of this device is definitely worse. This is

especially the case for measurements of stake locations that are carried out once a year and cannot be repeated at the same location again. Is there an impact of inaccuracies in vertical positions on the calculations of thinning according to equations (1), (2) and (3)? Please comment on that.

12. Pages 2010–2011, Section 3.2: Please clearly specify what was used for input to calculate dh/dt, discuss the uncertainties therein and propagate them through the calculation.

13. Page 2012, lines 11–24: Most of the mass balance data have been published previously in various studies (e.g. Knudsen and Hasholt, 2008; Mernild et al., 2011). Although there is one additional mass balance year compared to Mernild et al. (2011) I would strongly suggest to move this paragraph into the sections "Study site" or "Background".

14. Page 2013, line 3: *"Since 1995/96 the mean annual accumulation has decreased (Fig. 2)"*. Is this statistically significant? Most of the more recent years are lacking measurements of winter balance.

15. Page 2013, lines 11–13: *"The inhomogeneous annual change in winter accumulation can therefore likely be linked to increasing wind speed and snow redistribution."* This statement seems somewhat speculative to me.

16. Page 2013, line 15: *slight* instead of *slightly*.

17. Page 2013, line 17 to page 2014, line 6: Most of the here presented statements have already been made in earlier studies (e.g. Knudsen and Hasholt, 2008; Mernild et al., 2011). To my opinion the fact that compared to Mernild et al. (2011) one more mass balance year is included, does not justify showing these findings as results of this study.

18. Page 2014, line 8: The fact that glacier area is relatively easy to observe is known for decades. I would suggest either remove the citation or cite one of the first studies that have dealt with the problem to measure the different geometric properties of glaciers.

19. Page 2014. lines 8–12: These statements and also Figure 1b are already made in almost identical form in Mernild et al. (2012a). Please do not list these statements as results of the present study.

20. Page 2014, line 14: The change in ice thickness is not observed but estimated as correctly stated two lines above.

21. Page 2014, line 20 and 21: *"This shows that if area changes are not included, volume changes will be underestimated"*. This is unclear to me. If one uses a given thickness change and calculates volume loss over a fixed perimeter that represents the initial glacier extent, then this volume loss must be higher compared to the same calculation but with a decreasing perimeter. Please make this paragraph clearer.

22. Page 2015, lines 1–3: This is not a result of this study but has been shown before. Please remove from the Results section.

23. page 2015, lines 19–20: Firstly I am not convinced that the agreement can be called good because the trends shown in Figure 4 do clearly differ. Secondly I do not understand how a reasonable agreement on one glacier can be a valid proof or considered a suggestion that the method will also work on other glaciers?

24. Page 2016. line 5–6: Please reword and maybe also indicate where you see the center line on Mittivakkat.

25. Section 4.3: As already pointed out above under *Major Remarks*, this section raises a lot of questions. Once again the listing of surface elevations changes

in the lower and upper part of the glacier indicates that calculated emergence velocity (w_e) reduces mass loss everywhere. But where does this mass come from? Based on the given information and with the very low quality of the figures it is almost impossible to understand how you divide the glacier into upper and lower section. Finally I am not convinced with the major conclusion that thinning of the glacier has resulted in reduced velocity: On the one hand the calculation of the thinning must be explained more thoroughly as explained above and under *Major Remarks*. Secondly the fact that strain rates in thinner ice are smaller is well known physics and known for decades.

26. Page 2017, line 15: Why is this stake representative? Please explain.

27. Page 2018, line 25: Why is sliding negligible during winter? Please show evidence or appropriate citations from the literature.

28. Page 2019. lines 13–14: Long-term records of velocity are not that rare. The issue, however, with the present study is that there is no thickness record. Instead an estimated thickness change is calculated in a way that, to my opinion, cannot be reproduced: (1) Input parameters and their uncertainties are not clearly listed and discussed, (2) it is not discussed whether the applied equations are appropriate to Mittivakkat glacier who is clearly out of balance (e.g. what is the effect of applying shallow ice approximation instead of less simplified flow-models?), and (3) figures are of a very poor quality and it becomes impossible to comprehend changes on Mittivakkat and the results of the calculations. I would strongly suggest to the authors to revise this study by using at least two different quality checked DEMs together with the mass balance record.

29. Figures: As already mentioned the quality of all figures must be improved. In addition I suggest using a Landsat Mosaic in Figure 1a with no or less obvious scan-line errors. Thinner lines in Figure 1b would make it easier for the reader to assess changes (also replace *Sebtember 9* with *September 9*).

References

L. M. Andreassen. Comparing traditional mass balance measurements with long-term volume change extracted from topographical maps: A case study of Storbreen Glacier in Jotunheimen, Norway, for the period 1940–1997. *Geografiska Annaler*, 81A:467–476, 1999.

J. G. Cogley. Geodetic and direct mass-balance measurements: comparison and joint analysis. *Annals of Glaciology*, 50(50):96–100, 2009.

L. H. Cox and R. S. March. Comparison of geodetic and glaciological mass-balance techniques, Gulkana Glacier, Alaska, U.S.A. *Journal of Glaciology*, 50(170), 2004.

M. Huss, A. Bauder, and M. Funk. Homogenization of long-term mass balance time series. *Annals of Glaciology*, 50(50):198–206, 2009.

N. T. Knudsen and B. Hasholt. Radio-echo sounding at the Mittivakkat gletscher, southeast Greenland. *Arct. Antarct. Alp. Res.*, 31:321–328, 1999.

N. T. Knudsen and B. Hasholt. Mass balance observations at Mittivakkat glacier, Ammassalik island, southeast Greenland 1995–2006. *Geografisk Tidsskrift–Danish Journal of Geography*, 108(1):111–120, 2008.

S. H. Mernild, N. T. Knudsen, W. H. Lipscomb, J. C. Yde, J. K. Malmros, B. Hasholt, and B. H. Jakobsen. Increasing mass loss from Greenland's Mittivakkat Gletscher. 5:341–348, 2011. doi: 10.5194/tc-5-341-2011.

S. H. Mernild, J. K. Malmros, J. C. Yde, and N. T. Knudsen. Multi-decadal marine- and land-terminating glacier recession in the Ammassalik region, southeast Greenland. 6:625–639, 2012a. doi: 10.5194/tc-6-625-2012.

S. H. Mernild, M.-S. Seidenkrantz, P. Chylek, G. E. Liston, and B. Hasholt. Climate-driven fluctuations in freshwater flux to Sermilik Fjord, East Greenland, during the last 4000 years. *The Holocene*, 22(2):155–164, 2012b.

P. Rastner, T. Bolch, N. Mölg, H. Machguth, and F. Paul. The first complete glacier inventory for the whole of Greenland. *The Cryosphere Discuss.*, 6:2399–2436, 2012. doi: 10.5194/tcd-6-2399-2012. http://www.the-cryosphere-discuss.net/6/2399/2012/tcd-6-2399-2012.html.

T. Tachikawa, M. Kaku, A. Iwasaki, D. Gesch, M. Oimoen, Z. Zhang, J. Danielson, T. Krieger, B. Curtis, J. Haase, M. Abrams, R. Crippen, and C. Carabajal. ASTER Global Digital Elevation Model Version 2 – summary of validation results, , available at: . Technical report, NASA Land Processes Distributed Active Archive Center and the Joint Japan-US ASTER Science Team, 2011.

Interactive comment on The Cryosphere Discuss., 6, 2005, 2012.