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The Cryosphere
Discussions

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

**S. H. Mernild et al.**

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Below you will find reply to the two Anonymous Referees. Thanks...

Anonymous Referee #1

Received and published: 28 July 2012

Overall comments: This is a well written paper about a well-studied glacier in southeast Greenland. The relatively long observational record allows for interesting statistics on mass loss and velocity changes. The paper focuses on two separate but related topics, being mass/volume change of Mittivakkat, and the change in its velocity field. The mass budget is highly relevant for sea level rise estimates, since little is known about the health of independent glaciers and ice caps in Greenland. Looking into the changes



of the velocity field is interesting, and may be able to tell us something about changes in glacier thickness, towards which this paper may be a first step. AUTHORS: Thanks a lot.

However, I have problems with both topics in the paper. The calculations of ice thickness for 1986, 1999 and 2011 are based on only one direct measurement of ice thickness in 1994. The 1999 and 2011 estimates may have large errors due to inaccurate annual stake measurements and lack of spatial coverage in the accumulation zone, which are not discussed in detail. AUTHORS: The inaccuracy related to the Mittivakkat observation program and the direct stake observations have earlier been discussed in Knudsen and Hasholt (1998, 2004, 2008), and in Mernild et al (2011a). The vertical inaccuracy in observed annual stake measurements has been estimated to be less than 5 cm; however, when extrapolating the stake values the uncertainty is expected to be $\pm$15%. To minimize the overlap between this paper and earlier papers, the uncertainties will not be discussed again in detail, but presumably are at least mentioned here. The lack of stake observations in the heavily crevassed zone, in the accumulation zone, is obvious (due to safety reasons); however, surface mass balance (SMB) simulations by Mernild et al. (2006, 2008) indicates, that the missing observations (from the crevasse area) is unlikely to bias the results.

Besides, one may wonder what this paper adds to Mernild et al, 2011a. AUTHORS: Mernild et al (2011a) discuss observed time series of mean annual net mass balance related to the "out of balance" conditions. The present paper, however, is much more detailed because- besides the mean annual net mass balance time series - it also discusses: 1) the spatial variability in winter, summer and net annual mass balance; 2) area and volume changes; and 3) links between the surface mass balance and hydrological conditions and the spatial surface velocity and time series of velocity on both the mean annual and daily timescales. These two Mittivakkat papers are quite different in many ways. Text has been added to clarify the differences between this paper and Mernild et al (2011a). Figures 1b and 1c have been erased to avoid overlap

The 1986 ice thickness estimate is even based on a crude linear extrapolation of the 1995-2011 SMB trends, which absolutely requires validation by direct ice thickness observations – which do not exist. AUTHORS: Although no ice-thickness observations exist for 1986, we estimated the 1986 mean ice thickness by adding the net ablation during 1986–1994 to the 1994 mean thickness, based on a linear extrapolation of the observed 1995/96 to 2010/11 net mass balance. We know that this is an approximation of the 1986 mean ice thickness, but as we said, we have confidence in the method and in the calculated 1986 volume, since the trends in air temperature and precipitation (the atmospheric forcing) for the region during 1995–2011 are consistent with trends for 1986–1995 (Mernild et al., 2012b).

The discussion of velocity changes is new, as this has not been done before for an independent glacier in Greenland. It is interesting to see that the glacier is slowing down, while many authors these days discuss glacier speed-up in Greenland. AUTHORS: It is important to separate velocity observations from local glaciers and Greenland Ice Sheet outlet glaciers, and as stated by the reviewer, a decreasing velocity for local glaciers in Greenland is new.

The discussion of this result is too long, since the outcome is not very surprising. The glacier thins a relatively large amount and there is no doubt that this should lead to a deceleration. (It is good to have it seen proven though.) The statistics that are used to show that sliding is not important are weak, since the thinning component is not taken out of the equation. Finally, the discussion of how the velocity responds to meltwater generation is speculative, and unjustified comparison to processes on the ice sheet is made. Authors: The discussion part has been shortened, and the argument that "sliding is not important" has been strengthened. The section about the velocity response to meltwater has been rewritten as well, and additional references have been added and other references removed.

In all, I fear that this manuscript may not have the impact nor scientific quality that The Cryosphere is after. AUTHORS: We disagree, since this paper analyses and discusses



for the first time a unique velocity time series from a local glacier in Greenland. Furthermore, the paper provides the scientific community with information on glacier mass budget matters, which are highly relevant for estimates of future sea-level change, since little is known about the health of independent glaciers and ice caps in Greenland.

In any case I'd suggest removal of pre-1994 results, a more thorough attempt at ice thickness validation, a thorough discussion of uncertainties, an improved interpretation of velocity data and removal of speculations on the relationship between velocity and meltwater production. AUTHORS: The issues have been discussed: The pre-1994 part was changed, so instead of using net mass balance data estimated from a linear extrapolation, we used simulated net mass balance data from Mernild et al. (2008). The 2011 calculated MG ice thickness has been validated against 2011 radio-echo sounding ice thickness observations, uncertainties are added to mean values, and parts of the velocity sections have been rewritten to fulfill the requirement from the reviewer.

Specific comments Page 2006 3-4: Mittivakkat does not have a long-term mass balance record – though it is long by comparison. It does not cover "decadal time scales" (line 10 and other places in the paper). Besides, there are longer records for Russell glacier in west Greenland, and at least three other glaciers in Greenland have records spanning a decade or more. AUTHORS: By the standards of Greenlandic independent local glacier conditions the Mittivakkat Gletscher has a long-term mass balance record (and are the longest-observed independent glacier in Greenland), since most other independent local glacier mass balance records from Greenland are around 5-8 years or less. The Mittivakkat observational program has been running since 1995, and contains a mass balance series of 17 years (including observations from 2012). The reviewer mentions the Russell Glacier, but it is important to separate mass balance observations at the margin of the Greenland Ice Sheet (GrIS) from independent local glaciers. Since Mittivakkat is an independent glacier, and disconnected to the GrIS, we have clearly stated, that concerning independent glaciers in Greenland, the Mittivakkat Gletscher is the only such glacier having a "relatively" long-term mass balance record.

Remove 'only', or specify that you do not take into account outlet glaciers of the ice sheet. AUTHORS: We specified that we did not take into account GrIS outlet glaciers, but only local glaciers. The text is rewritten.

6-7: There is no mention of surface albedo in the paper, even though this has a large and likely potential contribution to glacier change. Please discuss in the paper. AUTHORS: We did not mention changes in surface albedo in detail, since the purpose of this paper is not to discuss (in detail): 1) the Mittivakkat Glacier surface energy balance, and 2) the link of changes in albedo and surface energy balance with mass balance changes. In earlier Mittivakkat Gletscher model studies changes/variations in surface albedo between snow and bare ice have been discussed related to mass balance changes. We have, however, added a sentence to the manuscript about mass balance changes related to changes in surface albedo and surface conditions (see end of section 4.1).

Page 2007 22-26: This should be in the discussion, not in the introduction. AUTHORS: This part is moved from the introduction.

Page 2008 16: Add reference for ELA rise. AUTHORS: Reference is added.

17-19: As long as the AAR is non-zero, statements on significant glacier imbalance should be substantiated. AUTHORS: The imbalance is in detail substantiated in the mentioned references.

Page 2009 9-10: Is this random error or offset? AUTHORS: This is offsets related to the use of Landsat images. See also Mernild et al. 2012, where fx errors and offsets are discussed in detail. The Mernild et al. 2012a reference is added to the manuscript.

19 and onwards: Mauri Pelto mentions that the methods section can be shortened because there is overlap with a previous paper – I do not agree. The reader should



be capable of understanding the paper without having to read one or more previous papers. AUTHORS: The overlap has been erased to avoid duplicity between papers, and the reader is still able to follow the used method and uncertainties.

28: How do you know that the omission of a large part of the glacier is not likely to bias the results? The region is crevassed and therefore unlike the rest of the glacier. Your results are heavily based on changes in the mean thickness of the glacier. How do you know the mean thickness if you do not have full spatial coverage? Same goes for your stake measurements – they do not cover the entire glacier (Fig. 3 and 5), as they do not provide information in large parts of the accumulation zone. This must cause a large uncertainty in your accumulation estimate, which is already a difficult to measure parameter to begin with due to its spatial heterogeneity. How can you make statements about the entire glacier then? I think you can't. This should be discussed in great detail and added to the error estimate. AUTHORS: The lack of stake observations in the heavy crevassed zone, in the accumulation zone of Mittivakkat Gletscher, is obvious, due to safety issues. Therefore, surface mass balance (SMB) simulations were conducted indicating (Mernild et al. 2006, 2008, and Liston and Mernild 2012), that the lack of observations in the heavy crevassed zone is likely not to bias the results, since e.g., the winter, summer, and net mass balance are in the same order as the results observed in accumulation area. References are added to the text.

Page 2010 8-11: I do not agree with this approach (calculating ice thickness from a 1994 survey and annual surface height change from stakes), since errors in annual SMB estimates, which can be 15% (line 6) will accumulate during the study period. This error propagation is not discussed in the rest of the manuscript, though it may have a large impact on the results and conclusions. On top of this, the lack of full spatial coverage for ice thickness and stake measurements as discussed above will add question marks. You need a second measurement campaign to see whether your thickness calculations are anywhere near the truth. Without this, you have no validation for both your SMB and ice thickness estimates, and a study like this loses credibility.

At the very least you should have a detailed discussion of uncertainties in the results section.

AUTHORS: Uncertainties (15%) are now added and discussed in the manuscript on request from the reviewer. Also, as stated earlier, the missing stake observations from the heavy crevasse zone are likely not to bias the results. Further, 95% of the ice volume was observed in 1996 (the only part missing is the SE corner of MG, therefore we don't see any problems in using this observed data set. Two observed MG cross section ice thickness profiles from 2011 (based on monopulse radio-echo sounding) were added to the text (Figure 3) to confirm our method, and to confirm that our ice thickness calculations were near the radio-echo observed.

12-17: Estimating the 1986 mean ice thickness by linearly extrapolating the SMB for 1995-2011 is impossible to justify. There is nothing linear about the SMB of a glacier, so you can't build confidence on similar trends in temperature and precipitation (uncorrected and measured at quite a distance in a mountainous terrain). Besides, it is my impression that warming in Greenland was larger for 1995-2011 then for 1986-1995. You have no information on length of the melt season, surface albedo, solid versus liquid precipitation, snow erosion by wind, etc. You can't make such large assumptions without validating the result, for which you'd need ice thickness measurements in 1986. Since these measurements do not exist, you can't present reliable nor convincing estimates for ice thickness in 1986. AUTHORS: The linear extrapolation from 1986–1995 is erased from the manuscript, and calculated MG net mass balance data are used instead (Mernild et al. 2008b, Figure 7b; here MG annual net mass balance was simulated back to 1898 based on standard synoptic meteorological data from Tasiilaq), to calculate the SMB change (1986–1995), and the 1986 volume. In Mernild et al. (2008b) simulated annual mass balance was compared against observed for a control period (1995/96 to 2003/04), indicating an r2 value of 0.71 ($p < 0.01$) and a difference less than 0.01 m between observed and calculated net glacier mass balance. The manuscript is adjusted due to the use of calculated MG SMB data, and figures are



updated. Based on the linear extrapolation the mean 1986 MG depth was calculated to be 122 m, and based on simulations in Mernild et al. (2008b) the mean thickness was 115 m, overall indicating volume of 3.90 km3 and 3.65 km3, respectively. The difference between the methods of 6%.

23: Uncertainty = standard deviation? AUTHORS: Yes, standard deviation.

Page 2012 19: Give rËĘ2 and p for winter and summer balances. AUTHORS: rËĘ2 and p are added to the manuscript.

Page 2013 12: Precipitation is highly spatially variable. Why speculate by stating that there is a connection with wind speed? AUTHORS: Wind speed in this area is relatively strong and earlier studies by e.g., Hasholt et al. (2004) state that strong snow redistribution occurs in the region, which likely explains the glacier locations and also the regional runoff differences. The Hasholt et al. (2003) reference has been added to the manuscript. The strong winds and storms in the area can justify why we added a connection between winter mass balance and wind speed.

13-14: This depends on a lot of factors – I would add some nuance to this statement or remove it. If you wish to keep this you should state at which height above the surface this is valid for (wind speed at 2 or 10 m). AUTHORS: We rewrote the lines, to include the concern from the reviewer. We added the wind speed elevation, which is at 2 m.

24-25: Rewrite. As ice makes place for rock the reflected solar radiation should decrease. Longwave emissions increase and more heat is advected. AUTHORS: Is done.

Page 2014 13: Give uncertainty based on the earlier reported +-15%. AUTHORS: Uncertainties are added.

21-22: Of course it is important to take area changes into account. There is no need to mention the scenario of a thinner glacier with an identical surface area. AUTHORS: The example from Knudsen and Hasholt (2004) is erased from the manuscript.

25: Give statistical significance for trends in temperature and precipitation. AUTHORS: Is added to the manuscript.

Page 2015 1-3: Area change is consistent with glaciers in the region: This is not remarkable when there is such a large range in area reductions (27% +- 24%). AUTHORS: The text is rewritten.

16-17: These values do not match with earlier reported values, and the calculations of volume loss with the power law are incorrect. These values should be 23 and 35%, respectively. This means that your 'calculated' observational values are 50% larger that those calculated by the power law. This is not a "good agreement" (line 21). In my opinion the power law is not applicable to Mittivakkat, in which case you should remove this section. Actually, the entire section (line 4 until the end) does not fit into the paper well and could be left out without a problem. Why use a poorly functioning power law to give a spatial perspective, never to refer to this again in the remainder of the paper? AUTHORS: The entire section is erased (line 4 until the end) on request from the reviewer.

Page 2016 9-12: The theory that velocity is proportional to HËĘ4 is not proven by simply interpreting figure 5. Either remove the sentence or prove the statement. AUTHORS: This sentence is removed.

27: How did you arrive at 50%? To me it seems to be over 60%. AUTHORS: The vertical stain was calculated for both the lower part and the upper part, and was able to compensate 62% at the lower part and 25% at the upper part, on average around 50% for the longitudinal profile. The text has been rewritten, and the values are added. Page 2017 On this page you explore whether the slowdown is related to deformation or basal sliding. You unsurprisingly find that deformation is likely to be the main cause, even thought the shallow ice approximation does not seem to produce velocity values that match the measurements (50% off in table one). I don't see the relevance to the aims of the paper, nor do I agree with the quick and dirty approach. This section has



been rewritten and the calculations have been extended to a profile across the ablation zone rather than relying on a single point. We consider this calculation important for explaining the cause of the change in velocity. Though we expect a thinning to glacier to move slower, the results here demonstrate that the entire change in velocity is consistent with the thinning that occurred and that other factors (e.g. changes to hydrology) are not necessary to explain the changes in velocity observed.

21-23: The velocity should be better correlated with HËĘ4. I'm surprised you find high significance and good correlation comparing v to H, especially for such few data points. AUTHORS: The correlation was recalculated.

Page 2018 First paragraph: Mittivakkat is nothing like the ice sheet. Please compare to other small glaciers. AUTHORS: References to the ice sheet have been removed and comparison is made with other small glaciers.

10: Why is calculating the dynamic effect of meltwater beyond the scope of the paper? A few paragraphs above you calculate the dynamic effect of thinning and flattening. Why does this fit the scope of the paper? AUTHORS: This statement has been removed.

14-17: I am unimpressed by the low correlation values here, 'showing' that SMB and summer temperature are correlated to velocity. "Thus higher melt : : : cannot explain the decreasing mean annual velocity" is a false statement, as above you showed that the glacier got thinner quite a bit. You need to filter out this effect before you can make statements about sliding, otherwise it is no surprise that lower velocities occurred at the end of the observational period, when temperature and melt were consistently high and thus the glacier thinner. AUTHORS: This conclusion has been reworded to emphasize that changes in melt are not important for changes in velocity relative to the changes in thickness that occurred.

25: "If we assume that sliding is negligible during winter: : :" is quite an assumption. Refer to other literature. What if almost all movement is due to sliding? This would change your quick calculation here a lot. There must be a large uncertainty in this sliding estimate – state it. AUTHORS: This entire calculation has been removed.

Page 2019 2: As mentioned above, the negative correlation does not prove anything. Please remove. AUTHORS: This sentence has been removed.

4: There is not strong evidence. AUTHORS: The word strong is erased from the text.

8-9: “The rarity of long-term thickness records” is misleading, since the record is not long (just relatively long), and the thickness record is calculated, not observed. I can calculate thickness change records that are much longer, but without direct repeat measurements of thickness they won’t carry much weight. AUTHORS: The text has been rewritten to fulfill the requirement of the reviewer.

23-24: I find it very hard to believe that the “summer” correlation is statistically significant given the scatter of data points. Furthermore, it seems that you forced the two linear fits through the 0 C and 0.04 m/day intersect, which you chose as the winter velocity. This would not be suitable in statistical analysis. Find the fits for positive and negative temperatures without locking them to a coordinate pair. I suspect that you find no correlation worth reporting. AUTHORS: Without forcing, no significant correlation for both winter ($r2=0.06$) and summer ($r2=0.07$) occurred, Figure 8 was therefore erased from the manuscript.

Page 2020 10: Do not compare to the ice sheet; the different processes and scales make comparison unreliable. Mittivakkat is a small undynamic glacier. As opposed to the ice sheet, meltwater channels may not deform much, and may still be ready for transport in the next melt season. Your calculations of correlations between discharge and velocity also indicate that you approach this the wrong way. AUTHORS: References for small glaciers are added.

There is not supposed to be much of a relationship between melt at the surface and glacier velocity. Glaciers speed up when englacial water pressure is high, which hap-



pens when the drainage system cannot cope with the delivery of meltwater. For the ice sheet, this is when melt increases, mostly in spring, when subglacial channels have collapsed/deformed. So if you don’t see a relationship between meltwater INCREASE and velocity, this to me indicates that the glacier has an efficient drainage system. Or that meltwater runs off over the surface of the glacier, and not underneath. AUTHORS: This section has been rewritten to more clearly describe the similarity of MG to other temperate mountain glaciers. We agree that there is not supposed to be a clear relationship between melt and velocity. The text has been clarified that such a relationship only occurs during spring speedup.

In the following paragraph you discuss this in a better way, trying to explain the double peaking of velocity. I do have objections here too, though, since this paragraph is speculative. First of all, I’m not convinced that we should read anything into the possibly accidental occurrence of two velocity peaks in only 3 melt seasons. You may as well try to find a reason for the few days with lower velocities separating these peaks. Second, the “substantially smaller peak” is not substantially smaller except in one year. Third, explaining the second peak by higher temperatures/melt does not agree with the temperatures, which are not higher during the second peak, except in one year. Fourth, the observed discharge record is too short to be of use. How do we know that discharge is high in the observational period if there are no measurements in the rest of the year? The graph only SUGGESTS it, since the discharge is scaled to match the temperature record. In all, this last paragraph should be taken out on grounds of being speculative and unsubstantiated. AUTHORS: The observation of a double-peaking of velocity each summer has been eliminated. This paragraph has been removed.

Page 2021 6: Replace warmer by higher. AUTHORS: Is done.

Page 2022 1-2: Where did you get this from? If a glacier thins a relatively large amount, then yes, its surface will slow down. But if it’s a thick glacier, then increased melt/ablation will not impact the dynamics though deformation much, and even may speed them up due to enhanced basal sliding. You need information on glacier thickness before you can make such a statement. AUTHORS: Parts of the conclusion have been rewritten and erased accordingly.

Figures Your captions are generally quite long and include text that does not purely describe the figures. Shorten the captions so that it is a figure description only, not also a quality assessment, for instance. AUTHORS: The captions have been shortened such that they are now only figure description.

Fig. 8: Are these daily average velocity and air temperature? What is the difference between black and grey markers? AUTHORS: It is mentioned in the caption that is it mean daily air temperature. Thereare no gray diamonds, only black on this figure. This figure has been deleted.

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. AUTHORS: Are removed



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. AUTHORS: Radar observations have been used from 2011 illustration two profiles – these profiles were used for comparison between mean calculated and radar observed MG ice thickness. For further see review comments under reviewer #1.

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 40m 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. AUTHORS: See comments above under review #1. Also, the 1986–1994 calculation method was extended back to 1981, where the mean calculated surface elevation was compared to the 1981 map (1:20000) digitized mean surface topography.

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 we there is almost no change in surface elevation above 400m 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 730m a.s.l. (Mernild et al., 2011) with a mean annual mass balance close to -1m w.e. and it is very difficult to



imagine where this mass replacing the negative mass balance between 400 and 730m 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. AUTHORS: Is done.

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. AUTHORS: Is done.

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). AUTHORS: Erased.

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. AUTHORS: Erased.

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 90'000km2. I would suggest replacing the out-dated numbers. AUTHORS: New reference is added, and values are updated.

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. AUTHORS: Re-written and slope calculations are explained.

5. Page 2008, line 13: What exactly is meant with "updated". Is this the mean AAR for 1995-now? Please specify. AUTHORS: Is fixed.

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 235m with a standard deviation of 535m (and reaching extreme values of as much as _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. AUTHORS: The area part is erased from the text.

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. AUTHORS: Is done by adding references of Mernild sim studies.

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. AUTHORS: Is calculated.

9. Page 2010, line 8: The volume can be either estimated or observed, in this case it is estimated. AUTHORS: Is fixed.

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"? AUTHORS: Is fixed.

11. Page 2010, line 23: While the horizontal accuracy of a hand held Garmin 12 XL GPS receiver can be _2m 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. AUTHORS: The uncertainty is 2 m, as stated (standard deviation).

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. AUTHORS: Is explained.

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". AUTHORS: Is done.

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. AUTHORS: Is fixed.

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. AUTHORS: Is fixed.

16. Page 2013, line 15: slight instead of slightly. AUTHORS: Is fixed.

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. AUTHORS: No spatial distribution has been shown in earlier studies, therefore this is new. Only mean MG values have been published so far.

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. AUTHORS: Is fixed.

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. AUTHORS: Is fixed.

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

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. AUTHORS: Is erased from the text.

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. AUTHORS: Is erased.

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? AUTHORS: Is erased from the manuscript.

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

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 (we) 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. AUTHORS: The two sections, the upper and lower, are just different examples of the glacier behavior.

26. Page 2017, line 15: Why is this stake representative? Please explain. AUTHORS: The calculation at a single stake has been replaced with a calculation across a longitudinal profile through the ablation zone.

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

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. AUTHORS: Is discussed and done.

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). AUTHORS: Is done.

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