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Discussions

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

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Anonymous Referee #2 Received and published: 11 December 2012

Please find below first a list of major concerns followed by detailed suggestions. Major Remarks 1. The paper lacks a clear focus: various results are presented (spatial variability of surface mass balance, discussion of wind speed changes and its potential influence on accumulation distribution, glacier volume changes, the influence of emergence velocity, the influence of subglacial hydrology, surface velocity changes, seasonal velocity variations) but either they have been presented similarly before (the fact

that a spatially distributed mass balance of Mittivakkat is presented makes not a very big change to the previously reported mass balance profiles), they lack any validation (a recent high precision GPS profile along the transect stake 31–140 would be required to validate the calculated influence of vertical strain) or they are not really novel: the discussion about the potential influence of sub-glacial drainage system development on seasonal glacier flow velocities is merely a repetition of the cited literature and the observation that thinning is accompanied by a slow-down, is not surprising. The lack of a clear focus results in various aspects remaining unclear. I want to give just one example: AUTHORS: The spatial distribution of winter, summer, and net mass-balances have never been published before by the first author, neither have volume and velocity observations from MG: a detailed screening of the literature shows this. The discussion of wind speed changes and their potential influence on accumulation distribution is removed from the manuscript. The literature section has been shortened. Observations of MG surface lowering (decrease in elevation) have been observed at different nunataks by spraying a line where the glacier margin was located. A photo is added to the manuscript showing MG surface changes from 1994 to 2011: in total at this specific nunatak the surface lowered 24 m in elevation since 1994 to 2011.

It is stated that 19 stakes were measured continuously but it is never shown how these 19 stakes are distributed over the glacier. I believe that giving the paper a clear direction by addressing one topic in a detailed and thorough manner could clearly improve the scientific content of the presented work. AUTHORS: These 19 stakes are highlighted in bold in Fig 1.

2. The structure needs to be improved as portions of the applied methodology are explained in the "Results" section. I hereby refer in particular to the shallow ice approximation which might better be explained in the "Methods" section. AUTHORS: The section about the shallow ice approximation has been moved to the Methods section.

3. I still do not understand how it is possible that vertical strain exhibits such a large influence on surface elevation change (I raised this point already in my first review).

The authors state that over the entire profile (stake 31 to stake 140) vertical strain reduces surface elevation loss by 50%. I want to repeat my question from the previous review: where does this mass come from? I see that the profile does not cover the entire glacier, but is the mass gain in the very small remaining accumulation area (AAR = 0.15) sufficient to supply so much mass to the large ablation area? I am not convinced by the result presented here (please find a simple mathematical explanation in the following section) and ask the authors to provide evindece and explanations that their results are valid. AUTHORS: The mass supplied by vertical strain to the ablation zone where the profile is located is likely to be supplied from a thinning accumulation zone. We agree that the accumulation rates in the accumulation zone are much too small to maintain a steady-state mass of the glacier through mass flux into the ablation zone. However, if the accumulation zone is also thinning (through a flux of ice to the ablation zone that is larger than the accumulation rate), it is possible to have the large rates of vertical strain throughout the ablation zone that we observe. We are unable to document the thinning rate occurring in the accumulation zone, but casual field observations over the last decades do indicate thinner ice there, supported by the new photograph that we have added showing thinning in the upper ablation zone (Figure 6). We have added estimates of the uncertainty in our calculation of SMB, dh/dt, and thickening due to vertical strain to Figure 5, and found that the positive vertical strain appears to be statistically significant throughout the profile (except for the lowest point where errors are large).

I also want to repeat my concern about the quality of the input data used. I asked in my previous review whether the vertical accuracy of a handheld Garmin 12XL GPS is suitable for measuring surface elevation changes and the authors replied, that the accuracy is ± 2 m. However, this contradicts all of my personal experience, I've never seen handheld single frequency GPS devices being so accurate on the vertical axis. One could average measurements over a longer time period to improve accuracy, but was this done at every stake? To my opinion, the calculated 2011 surface elevation (Figure 5) is of very limited value as long as there is no validation (e.g. a recent high

precision GPS survey along the entire profile). AUTHORS: Here, we are talking about horizontal ± 2 m accuracy. This value is based on long-time repeated fixed station measurements with the same instrument during several years.

Detailed Suggestions: 1. Page 4388, lines 2–3: I would suggest omitting the text in brackets. I would simply call it "glacier" or otherwise use "local glacier". To my opinion it is clear enough that you do not refer to the ice sheet. AUTHORS: This has been changed.

2. Page 4394, lines 10–25: I would remove the discussion of a possible impact of wind velocities on mass balance distribution. The part is speculative and adds to the unclear focus of the paper. AUTHORS: The discussion about the impact of wind velocity on the winter balance distribution has been erased from the manuscript.

3. Section 3.2 and Page 4395 (line 26) to Page 4396 (line 9): Please clearly specify what was used for input to calculate dh=dt, discuss the uncertainties therein and propagate them through the calculation. AUTHORS: Uncertainties are now discussed and propagated through the paper.

I have made this comment already in the first version of the paper and I still have great doubts in the reliability of the calculated surface elevation changes presented in Figure 5. Looking at Figure 6c it becomes clear that the profile presented covers most of the elevation range of the glacier. I simply do not understand how it is possible that over the entire profile 50 % of the mass loss from SMB is replaced by vertical strain. Where does this mass come from? The remaining accumulation areas (given the mean ELA of 750 m a.s.l. and the AAR of 0.15) are tiny. If this mass were provided by the remaining accumulation areas, then accumulation there must be very high (by a factor of 0.15/0.85 times larger than the average ice emergence over the 85% of ablation area). A very rough calculation should illustrate this: annual we according to Figure 5a is 0.75 m w.e. and hence annual accumulation in the accumulation area must be about 4.3 m w.e. This seems very unlikely, also since Knudsen and Hasholt (2008) show that

Mittivakkat never experiences abundantely positive mass balance in its accumulation area (mean annual mass balance above 750ma.s.l. seems to be in the range of 0 to 0.5 m w.e.). AUTHORS: See above. This point raised is more or less similar to one of this reviewer's points raised earlier.

You calculated surface elevation changes according to equations 1 to 3. How were changes in firn density dealt with? This remain entirely unclear. I assume densitiy changes in the surface layers must be significant due to the strongly decreasing mass balance. AUTHORS: Density changes in the snow column (from snow to firn) was dealt with in the SMB (b) component in Equation (1), since changes in firn density is an integral part of the SMB calculations: SMB was calculated based on changes in snow depth and density, from snow-pit density samples.

In conclusion: calculating the impact of emergence velocity might be an interesting experiment, but without any validation (i.e. a recent high precision GPS survey along the entire transect) and a thorough discussion of the various sources of uncertainties involved in the calculation, it is of a very limited scientific value. AUTHORS: See above. This point raised is more or less similar to an above point raised by this reviewer.

4. Page 4397, line 23: Remove "that". AUTHORS: Is fixed.

5. Page 4401, line 2: I would suggest using simply "glacier" or "local glacier" instead of the less common term "independent glacier". AUTHORS:Done.

6. Figures are now supplied at better resolution which is appreciated. However, fonts and some of the figures are still so tiny that they can only be read when highly magnified in a PDF viewer. I still do not believe that this is reasonable and I see no particular reason making the figures (1, 3 and 6) so small. AUTHORS: The font has been enlargened in all figures.

7. Figure 5, caption: Why is it "longitudinal mean surface elevation"? I suppose the profile shows the elevation of the different stakes? Or is elevation somehow averaged

over the width of the glacier? AUTHORS: This has been changed, and the word "mean" is erased.

Interactive comment on The Cryosphere Discuss., 6, 4387, 2012.

Thinning and slowdown of Greenland's Mittivakkat Gletscher

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Fig. 1.