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Interactive comment on “Thinning and slowdown of Greenland’s Mittivakkat Gletscher” by S. H. Mernild et al.

Anonymous Referee #2

Received and published: 11 December 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. I have already reviewed the first version of this paper and can see that the authors have addressed some of the points raised in my previous review. However, I am still not convinced by the scientific quality of the presented work for the following reasons: (i) The major finding of a glacier-slowdown resulting from strong thinning is not a novel result, (ii) the calculated influence of vertical strain appears highly questionable to me and lacks validation (iii) the paper lacks a clear focus. Please find below

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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: 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.
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.
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

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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 evidence and explanations that their results are valid. 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).

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.
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.
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

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propagate them through the calculation. 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 w_e 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 abundantly positive mass balance in its accumulation area (mean annual mass balance above 750 m a.s.l. seems to be in the range of 0 to 0.5 m w.e.). 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. 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.

4. Page 4397, line 23: Remove "that".
5. Page 4401, line 2: I would suggest using simply "glacier" or "local glacier" instead of the less common term "independent glacier".

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.
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?

References

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.

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

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