The Cryosphere Discuss., 6, C1094–C1102, 2012
www.the-cryosphere-discuss.net/6/C1094/2012/

The Cryosphere Discussions

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

Anonymous Referee #1

Received and published: 28 July 2012

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.

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. Besides, one may wonder what this paper adds to Mernild et al, 2011a. 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.

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. 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.

In all, I fear that this manuscript may not have the impact nor scientific quality that The Cryosphere is after. 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.

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. Remove 'only', or specify that you do not take into account outlet glaciers of the ice

sheet.

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.

Page 2007

22-26: This should be in the discussion, not in the introduction.

Page 2008

16: Add reference for ELA rise.

17-19: As long as the AAR is non-zero, statements on significant glacier imbalance should be substantiated.

Page 2009

9-10: Is this random error or offset?

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.

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.

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.

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.

23: Uncertainty = standard deviation?

Page 2012

19: Give r^2 and p for winter and summer balances.

Page 2013

12: Precipitation is highly spatially variable. Why speculate by stating that there is a

connection with 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).

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

Page 2014

13: Give uncertainty based on the earlier reported +-15%.

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.

25: Give statistical significance for trends in temperature and precipitation.

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%).

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?

Page 2016

9-12: The theory that velocity is proportional to Hˆ4 is not proven by simply interpreting

figure 5. Either remove the sentence or prove the statement.

27: How did you arrive at 50%? To me it seems to be over 60%.

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.

21-23: The velocity should be better correlated with Hˆ4. I'm surprised you find high significance and good correlation comparing v to H, especially for such few data points.

Page 2018

First paragraph: Mittivakkat is nothing like the ice sheet. Please compare to 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?

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.

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.

Page 2019

2: As mentioned above, the negative correlation does not prove anything. Please remove.

4: There is not strong evidence.

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.

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.

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. 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 happens 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. 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.

Page 2021

6: Replace warmer by higher.

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.

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.

Fig. 8: Are these daily average velocity and air temperature? What is the difference between black and grey markers?

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