Final Response: Reply to the reviewers' comments, paper TC-2010-90 Petermann Glacier, North Greenland: massive calving in 2010 and the past half century

O.M. Johannessen, M. Babiker, M.W. Miles

Dear Reviewers,

Please find below our point-to-point response (blue text) to your reviews. They were all highly appreciated as they were very constructive and helped to improve the paper considerably. We have indicated the points for which we have revised the manuscript, or otherwise explain the reasons for having the manuscript unchanged.

In general, the reviewers agreed that the topic is important, and that providing perspective for the 2010 calving event is needed. There were no major objections to the data analysis that was done. The reviewers wanted however want more analysis and more material to be written into the paper. The general criticism is that the paper is cursory. We acknowledge that the paper should be more comprehensive, and that the brief paper as submitted reflected to our original impetus, which was to provide a timely response to the remarkable 2010 calving event, which called for temporal perspective.

In the revised paper, we have added substantially to the material and discussion as requested, including (a) new material on ice velocity – including 20-years of velocities derived here, (b) more background material on Petermann glacier, (c) more material on each of the observed major calving events from Petermann glacier, (d) consideration of Petermann calving in the context of other major marine-terminating glaciers in northern Greenland, and (e) more assessment of the evidence for a possible change in Petermann glacier over the long record.

Sincerely,

O. M. Johannessen, M. Babiker, M. W. Miles

REFEREE #1

The authors present an excellent dataset in regards to Petermann Glacier terminus position. This research, however, feels like step one in a longer project rather than a complete analysis that is ready for publication. This paper is missing much of the important background information for Petermann Glacier, such as a discussion of thickness and velocity, and imagery showing grounding line as compared to the terminus region of interest.

Agreed, in the revised manuscript, we include thorough background information on Petermann Glacier – both ice-thickness and ice-velocities, the latter including not only published records, but also a new set of velocities derived here for a 20-year period, 1991–2010. Together these substantial enhance the background material provided for Petermann glacier.

Regarding the grounding line, we have revised Fig. 1 to include the grounding line for reference.

While the authors are focusing on Petermann Glacier, they fail to put the changes at Petermann into perspective in respect to other northern Greenland glaciers. This includes mention of terminus changes on other glaciers (e.g., as mentioned by Moon and Joughin, 2008, for C.H. Ostenfeld Gletscherand Zachariae Isstrom) or discussion of the type of calving events expected from this sort of glacier (large tabular icebergs) versus the more commonly studied western and eastern Greenland tidewater glaciers (small icebergs and bergy bits).

We agree that it is of interest to place the Petermann glacier calving into perspective with respect to other glaciers in the region. In the revised manuscript, we describe and discuss changes in the calving front in the other pertinent marine-terminating glaciers in northern Greenland, based on published material, and make comparison to the Petermann.

The authors have overstated the conclusions of the Rignot and Steffen (2008) paper, which did not show a recent change in basal melt, but rather documented the significant basal melt on Petermann, which may or may not be changing. With no additional research in the submitted paper, it is appropriate for the authors to speculate that the channelized basal melt influenced the 2010 calving event but not to provide this as a more conclusive mechanism for the event. I find the discussion seriously lacking as well as a dearth of additional data considered, even though much more information can be readily found. In the final sentence in the Discussion, the authors state that calving was ultimately triggered by strong winds, but they provide no data or analysis for the reader to view, nor do they provide a reference if this was not work completed by the authors. How does this strong wind event compare to other wind events or mean winds?
Was this truly an anomaly of the magnitude that one can conclude it was the ultimate cause of calving?
Similar questions could be asked for the other points raised in the final sentence of the Discussion – Sea ice, ocean water temperature, surface and subsurface melt. Each of these points should be separated and discussed individually.

Agreed that we appeared to overstated the Rignot and Steffen (2008) conclusions, although it is plausible that changes in ocean-ice regime are underway there. The reasons for the extraordinary 2010 calving event are certainly speculative at present – a point that Falkner et al. also note in their newsletter report – although there many candidate factors (e.g, sea ice, ocean water temperature, surface and subsurface melt). These factors can be separated and to varying degree that are some commensurate data – although not extending back enough in time to ascertain if the 2010 (pre)conditions are exceptional in the long term. This is clearly a subject for further analyses; here in the revised manuscript we have separated these various aspects and discuss them individually, as recommended.

In “Future Research” the authors mention interest in looking at Petermann Glacier velocities. There is data available on the topic, yet the authors make no use of it in this paper, even though it could greatly aid their analysis. The authors should consider combining the work presented in this paper with the work they state will be published in a subsequent paper, as the work presented here simply does not provide a thorough analysis.

Agreed, as mentioned earlier, velocities from our own analysis covering each year a 20-year period 1991–2010 are now included, together with published velocities from several years within the same period, and importantly those for years in previous decades. In the revised manuscript, we present these data and results from statistical tests.

The authors also mention, in Future Research, the need for more glaciological, meteorological, and oceanographic data. They have, however, failed to use additional data that is already available. This is a major weakness of the paper.

As stated earlier in our general response and referee #2 response, this reflects our original goal to provide timely perspective context for the 2010 event (which has since also been done in an AGU newsletter, EOS, Falkner and others). We agree that more substantial data analysis and discussion is needed, and we now provide this material, including ice velocity (produced both here and in previous research) and ice thickness (from previous research). The revised paper nonetheless does not go through a complete new oceanographic and meteorological data analysis – that does remain a topic for another research paper.

REFEREE #2

General comments:
This paper presents some interesting results from calving records spanning 1959-2010. While the results are intriguing, the paper lacks any real analysis and reads more like a report of observations than a scientific paper.

Agreed. As stated earlier in our general response and referee #1 response, this reflects our original goal to provide timely perspective context for the 2010 event (which has since also been done in an AGU newsletter, EOS, Falkner and others). We agree that more substantial data analysis and discussion is needed, and we now provide this material.

Specific Comments:
The abstract concludes by posing an interesting line of study: “...this event supports the contention that the ice shelf recently has become vulnerable due to extensive fracturing and channelized basal melt.” I was interested in reading more about this result, but didn't really see it mentioned again.

We do now address this in the paper itself, in the Discussion.

Can the authors look at the distribution of rifts and flow speed estimates to see if advection and/or growth of rifts matches the quasi-regular calving chronology?

Interesting idea. We have not done a systematic study of the rifts through time, although for some recent periods these could be tracked (e.g., J. Box / I. Howat, BPRC / OSU). For the longer-term, the sporadic aerial and satellite data coverage is not so conducive to such an analysis. In the revised paper, we do however mention this.

The introduction states that the “glacier velocity and terminus position have been considered to be relatively stable”, but then you outline a pretty cyclic behavior of calving (punctuated by the 2010 event). It's likely that the cyclic nature of calving is 'stable'.

Agreed that the recurrent irregular (though not really cyclic) major calving on roughly decadal time scales should be considered stable – and indeed, that is one of the major messages here. In the revised manuscript we better convey this message. In the Introduction however, we stand by the contention in the “glacier velocity and terminus position have been considered to be relatively stable”, as this is the case for ice velocity, and for terminus position it has also been stated as such in previous studies, e.g., Zhou and Jezek (2003).

Typological comments:
Abstract: I'm not sure the results presented here are entirely new, since others have written about Petermann calving variability

The fact that Petermann sporadically calves large tabular icebergs is of course not new. However, our results are new because previously published studies have been based either on snapshots, a limited set of snapshots, or a limited period of observations (e.g., the decadal record of Box and Decker, 2011). There have not been any journal papers having comprehensively long-term analyses of variability, nor have any analyses used data to delimit the calving front for each year over a 20-year period, as we have done.

As we noted in the last paragraph of the Results (section 3), studies that have intermittent or otherwise limited temporal sampling can miss the bigger picture:

> “Previous analyses based on limited temporal sampling missed most of these major calving events, thereby suggesting the Petermann Glacier front position to be nearly constant. First, an analysis of satellite observations from 1962, 1963 and 1992 – by 25 chance just after the major calvings in 1959–1961 and 1991 – concluded the variability to be negligible except for “local, kilometre-scale variations” (Zhou and Jezek, 2003). Second, an analysis of ice-front position changes from 1992–2007 happened to miss the two major calvings in 1991 and 2008 (Moon and Joughin, 2008). Third, a historical retrospective survey published by Higgins in July 1991 just missed the massive calving event in August–September 1991..”

line 3: glacier (not glacial) ice

Agreed – changed as suggested.

line 4: longest (not long) ice tongue (not shelf)

Agreed – changed as suggested.

lines 6-7: delete retrospective – it's redundant

Agreed – changed as suggested.

line 8: write >100 km2 instead of 100+ km2.

Agreed – changed here and throughout.

line 9: Give the months during which the calving event occurred. Surely you know it retreated by comparing a few image pairs, so let the reader know too.

Agreed, in the revised manuscript, we give the months for each of the major calving events listed.

line 11: delete “has” (the terminus position in 2010 retreated...)

Corrected

line 11: This sentence is a bit confusing. How about: “The terminus position in 2010 retreat ~15 km behind the extent of previous observations.”

Agreed – changed as suggested.

line 12: “massive” is a bit subjective. How about: “Whether the large calving event in 2010...”

Agreed – changed as suggested, although we will retain the “massive” in the title.

line 13: is “global” really different from “ocean” warming? I think you mean atmospheric vs. ocean warming.

Agreed – clarified as suggested.

line 14: Has “fracturing” increased, or are these events just episodic?

An excellent question, and it remains an open one. We do expand upon this in the Discussion in the revised manuscript.

Introduction
lines 16-17: The first sentence is indirect and hard to follow.

Agreed – sharpened up the sentence and clarified the different ways that mass is lost.

line 19: This sentence is a bit muddled. We usually consider the extent of the glacier (and its 'mass') as ending at the grounding line. Saying that mass loss of outlet glaciers occurs primarily from basal melting of floating glacier ice is confusing.

Agreed – have sharpened up the sentence and clarified the different melting from different sorts of marine-terminating glaciers, with appropriate references for each.

line 23: delete the “+” after century (if you want to say that the timescales are often longer than a century, just write it out).

Agreed – changed as suggested.

Line 1 (page 2): delete “in the melting” (redundant)

Agreed – changed as suggested.

line 4 (page 2): what does “previously” refer to?

By “previously”, we simply meant that it was 70 km long before the massive calving event in 2010. In the revised manuscript, we state this explicitly.

Line 10: missing period after references

Corrected.

Data and methodology
line 22: the first sentence is awkward. Consider deleting “repetitive”

Agreed – corrected as suggested.

line 24: “intermittent” might be a more suitable word than “sporadic”

We prefer to keep it as “sporadic” (“occurring at irregular intervals; having no pattern or order in time)”. Although “sporadic” is nearly synonymous with “intermittent” (“occurring occasionally or at regular or irregular intervals; periodic”), “sporadic” seems more appropriate here, as the timing of earlier observations is completely irregular.

line 14 (page 3): the images were consistently “co-registered” not geo-referenced

Agreed, co-registered is technically correct – changed accordingly.

Results
line 24: no value listed “~km”????

Corrected to include “ ~15 km”

line 25: extent (not envelope)

Agreed, changed as suggested.

line 26: Again, if referring to a number greater than 100, write >100 km2, not 100+ km2

Agreed, changed as suggested, here and elsewhere.

lines 1-6 (page 4): combine these two paragraphs and reword one of the sentences so that they are not so similar.

In the revised manuscript, we have re-written the opening sentences for each paragraph. We have however maintained the separate paragraphs, expanding each. Each of the paragraphs is meant to report results for two different decades, 2000s and 1990s, respectively, corresponding to different panels of Fig. 2.

line 15: incorporate the plot results in to the text, don't just insert something that looks like a figure caption (“in order to visualize...here we plot...”)

Agreed, re-written.

Discussion
line 3 (page 5): This sentence (especially “business as usual”) is a bit too informal/casual

Agreed, have changed to “...such behaviour is normal and stable…”

line 4: “gigantic” is subjective

Agreed, have changed to “relatively large”

The discussion needs a lot more analysis!

Agreed, as described earlier and in later parts of our Final Response, the revised manuscript – in the Discussion and elsewhere – includes a more substantial consideration of Petermann glacier calving behaviour beyond simply reporting the major calving events. In the Discussion, paragraph 1 has been expanding, including but not limited to a discussion of the results for ice velocity. Paragraph 2 of the Discussion (which in the revised manuscript is paragraph 3) is also expanded, although it remains necessarily speculative regarding the reasons for the unprecedentedly large calving in 2010.

References
Add Rignot (Jglac, 1998): Hinge-line migration of Petermann Gletscher, north Greenland, detected using satellite-radar interferometry.

Added the Rignot (1998) paper in *J. Glaciol*.

REFEREE #3

General comments:
This paper presents a valuable data set of Petermann Glacier terminus position since 1950s. The documented results are very interesting, but not enough. The paper lacks some scientific analysis of the glacier behavior or a comparison study with other similar outlet glaciers in North of Greenland.

Agreed – please see our summary general response.

Specific comments:
They mention that perhaps the absence of sea ice and warmer fjord temperature or strong wind might have triggered the calving event, but they don't provide any data to support this statement. How was the sea ice coverage in previous years? Has the basal melt increased during the last decades? To make any conclusion they need to show additional available climate and oceanic data. Furthermore, to put the changes of Petermann Glacier into perspective in respect to other outlet glaciers in Greenland, it is necessarily to present some data on glacier velocity, shelf thickness and ice discharge from the grounding line.

a. Regarding climate and ocean data, we include a more thorough discussion – and available data – without overstating the case.

b. Regarding glacier data: Agreed. Present velocity from our own and previous work, as well as more information on thickness and ice discharge.

Calving front position in figure 3 represents the furthest point of the glacier front. I found it a bit confusing since in figure 3, it looks like that the glacier has been advancing between 2007 and 2008 but in reality the shelf is getting smaller (figure 2). It is better to show the average shelf length at each year instead of the furthest shelf front.

Good point. Agreed that we should revise the estimates to represent the average front position rather than the furthest front position. This makes the 2007–08 area loss (when some km^2 were calved from the sides) more apparent in the revised Fig. 3, and also reconciles to some degree the differences between the numbers for ice front retreat for the 2010 event, with those mentioned in, e.g., Box and Decker (2011).

I agree with the comments made by the two other reviewers (RC C39, RC C83) and M. Pelto and will not repeat them here again.

REFEREE #4
T. Scambos

The paper is an appropriate topic and potentially interesting. The several reviews already submitted offer many ideas to build upon.

M. Pelto lays out an approach to expanding and improving the paper that focuses on a mass-balance perspective. This is completely valid, but another approach that would lead to a worthwhile study would be one that aims to understand the timing and cause of the calvings. What this primarily requires are more images, and a homing in on the timeing and process of the major calvings.

As to finding more images: using the EarthExplorer search tool at USGS, I was able to see 3 or 4 clear-sky MSS images spanning the period 1976-1977, and I believe they showed an advanced ice front (Path 49, Row 1, 18 April 1976, 6 May 1976, 1 May 1977). http://edcsns17.cr.usgs.gov/NewEarthExplorer/
SPOT Sirius search tool also provided several images, on the order of 10 or 12, spanning 1991-1997 http://www.spot.com/web/SICORP/1249-sicorp-sirius-spotimage-online-catalogue.php

What I recommend is to isolate a few of the calving events and see if you can bracket the dates of calving better. Also examine climate/weather characteristics of the period surrounding the calving events. Are they always late summer (as the 2010 calving was?) Are they always following some particular climate pattern, e.g. negative AO, or unusual sea-ice-free conditions?

It is possible to do a lot more, and at least rounding up more images appears to be easy; you now have several ideas of how to proceed. I would say that a climate cause or calving approach would be fruitful, but Mauri's ideas outlined earlier should also be incorporated.

Thanks for the suggestions on avenues for enhancing the paper. In the revised paper, we have added substantially to the material and discussion, as mentioned in our general response and in the responses to the other reviewers. This includes, among others (1) more material on each of the observed major calving events from Petermann, including constraining and evaluating the timing of such events, as suggested, and (2) new material on ice velocity – including 20-years of velocities derived here, together with published velocities from several years within the same period, and from previous decades. In the revised manuscript, we present these data and results from statistical tests for changes through time.

Taken together, we believe that the revised paper provides a better understanding of major calving events and an assessment of the evidence for a possible change in Petermann glacier over the long record. The point about the possible role of the absence of sea ice in fjord is intriguing, and while a complete systematic analysis of ocean and sea ice conditions is "beyond the scope of the present analysis", it is considered in the revised manuscript.

M. Pelto

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Johannessen et al (2010) examine the calving history of Petermann Glacier. This history if thorough in its analysis, including providing regional context would be of importance.

At present the paper is cursory in its documentation of the terminus change analysis and of the basic characteristics of Petermann Glacier. Petermann Glacier is significantly different in its dynamics from the marine terminating outlet glaciers of west and southeast Greenland that have received the majority of our attention. Nowhere in this paper is the thickness of the glacier mentioned either at the calving front or at the grounding line. The thin calving front 70 m is similar to that of Ryder and Steensby Glacier and has a different calving behavior as a result from thicker, faster moving outlet glaciers further south in Greenland. Again no context is given with respect to these or other Northern Greenland glaciers with a large thin floating terminus.

As mentioned in paragraph 2 of our general response and in our response to reviewer 1, we now describe and discuss changes in the calving front for Ryder and the other pertinent marine-terminating glaciers in northern Greenland, based on published material, and provide a comparison to Petermann.

Nowhere is the volume flux mentioned. The actual velocity of the glacier, usually the key parameter reported in a calving analysis is not reported. The authors note that the velocity and terminus position have has been relatively constant until 2010. The average annual velocity would then provide a good first estimate of the area of ice that would have to calved each year for the terminus to maintain its position. Contrasting this with the actual calving record provided in Figure 3 is essential.

Interesting point. As mentioned earlier, we now present extensive material on ice velocity during the 50-year period. In the revised manuscript, we present these data and make an assessment of the relationship to ice area calved through time, as suggested.

170-21: The volume flux past the grounding line and at the terminus should be noted, using the published record. Johnson et al (2011) note that nearly 2/3 of the volume loss occurs due to the high basal melt in the first 20 km below the grounding line, the glacier thickness declines from 600 m to 200 m in this region. The volume flux at the calving front for Petermann Glacier is _0.6 km3 (Higgins, 1990), whereas Jakobshavns yields close to _40 km3. This is a vast difference that should be discussed at least briefly.

Good point. In the revised paper, we present and discuss the volume flux in the context of losses from calving and basal melt, using estimates from the published record, including the new Johnson et al. (2011) paper in *J. Geophys. Res.* In the revised paper, we also place this in context of Jakobshavn and some other major outlook glaciers.

171-4: It is critical to examine the published velocity record of the Petermann Glacier. The record does span a significant time period and has illustrated relative consistency within +10%. The velocity has been reported in the 900 m/a range near the calving front by Higgins (1991), Johnson et al. (2011), Rignot and Steffen (2008) and Joughin

et al (2010). Given the 53 year period reported on and the observed velocity of _900 m/a, the 16 km width of the fjord near the calving front would suggest that to maintain its calving position the glacier would have needed to produce on the order of _800 km2 during the interval. The sum of the major calving events noted in this paper up to the 2010 event that led to terminus retreat provides _50% of this total. Does this suggest many minor events or a missing major event? This is the quantitative analysis that must be completed in an examination of the calving history of Petermann Glacier. This area that was lost must be accounted for.

Interesting point. As mentioned earlier, we now present extensive material on ice velocity and consider it in the context suggested above.

171-8: Patrick Lockerby was the first to report the iceberg discharge from the Petermann Glacier in 2010 and should be mentioned here as well.(http://www.science20.com/chatter_box/arctic_newsflash_petermann_ice_tongue_loses_huge_chunk)

Fair enough. In the revised manuscript, we credit Lockerby as suggested, although retain the other citations that are equally valid.

171-13: At some point in the paper the type of icebergs produced should be mentioned. The large calving events reported are tabular icebergs. The thin nature of the floating terminus tends to produce this type of iceberg.

Agreed, this is presented in the Background of the revised paper.

173-5: Must define how this massive 1991 calving events was identified. This is the main new piece of information to the recent calving history of this glacier that the paper offers.

Agreed. In the revised manuscript, we now present more details on the 1991 event, as well as for each of the major calving events.

174-11: The unique nature of the melange of ice particularly along the northeast margin of Petermann Glacier is worth mentioning. Such a melange is also a part of the terminus of Ryder Glacier.
174-17: Johnson et al. (2011) provide estimates of the basal melting and should be cited here also.

Agreed on both points – these are taken into the revised manuscript.

J. Box

It is valuable to have a paper like this, in which a survey of available data is made. The conclusions points 1 and 2 are entirely valid and very interesting.

Thanks.

p3 line 10: “The calving removed 28 km of the 70 km ice shelf” seems to be in error. Box and Decker (accepted) measure 17 km effective retreat, that is, area change (-

275km) divided by width (16 km). I don’t see how you can get 28 km.

The retreat was estimated from the furthest extension of the front, which does not represent the average position of the front, nor does it represent the full 16km width. This 28 km estimate is close to that since reported in the Falkner et al. Eos newsletter report, their Fig. 1b. This clarified in the revised version, which also (as mentioned in the last response to referee #3) includes estimates that represent the average front position rather than the furthest front position.

minor comments p2 line 4: "longest floating" instead of "long floating"

Agreed. Revised as recommended.

p3 line 6: "between 3 and 5 August 2010" instead of "5 August 2010"

Agreed. Revised as recommended.

p4 line 24: missing number for
effective length change. The accurate number is 17 km according to Box and Decker.

Missing number is now inserted; our estimate is 15 km. Here this is the difference between the 2010 post-event position and the previous most-retracted front position after the 1991 event.

p5 line 19: “annual” instead of “interannual” p6 line 9: remove “northwards” p8 line 13:

Agreed. Revised as recommended.

title of Box and Decker is: “Box, J.E. and Decker, D. T.: Greenland marine-terminating glacier area changes, 2000–2010, Ann. Glaciol., in press, 2011.”
References Box, J.E. and D.T. Decker, accepted: Greenland marine-terminating glacier area changes: 2000-2010, Annals of Glaciology, submitted August 2010, accepted February 2011 preprint

Reference updated.