The Cryosphere Discuss., 5, C2046–C2079, 2012
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The Cryosphere Discussions

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Interactive Comment

Interactive comment on “Seasonal speed-up of two outlet glaciers of Austfonna, Svalbard, inferred from continuous GPS measurements” *by* T. Dunse et al.

T. Dunse et al.

thorben.dunse@geo.uio.no

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We like to thank editor I.M. Howat and reviewers M. King, G.S. Hamilton, P. Christoffersen and one anonymous reviewer for thorough and encouraging reviews. The comments regarding technical aspects of the GPS data and processing encouraged us to extent previous filter testing and investigate further the resolving power of our smoothed velocity time series with respect to speed-up events. We have reorganized the discussion and included numerous references, among those very recent studies of Greenland outlets, to put our observations from Austfonna in a wider context.

We will first address the reviewer's general criticism, organized by topic, before addressing each reviewer's list of minor, editorial issues.

The Reviewer's comments are typeset in bold font.

Our response is typeset in normal font.

Abbreviations include P = Page(s), L = Line(s) and GC = General comment(s)

1 General comments (GC)

1.1 Filtering and resolving power of GPS data

M. King suggest that for producing smoothed velocity timeseries, a gaussian averaging kernel may perform better than the chosen box(square) kernel.

This comment may relate to the discussion by den Ouden et al. (2010) who used identical GPS receivers on Nordenskjøldbreen on central Spitsbergen. They noted apparently spurious waves (∼15 days period) in their smoothed velocity time series and attribute these to a filter effect given hourly sampled data. They chose to apply a Welsh-filter on their data, which they found to work best with respect to minimizing such spurious waves.

We have treated the filtering slightly differently from den Ouden et al. (2010). As a first measure to improve the signal-to-noise ratio (at the sake of temporal resolution) we have grouped the hourly record into daily means before applying moving averaging to further smooth the timeseries. We have experimented extensively with different filter settings, both applied to the recorded data (hourly record and daily means), as well as synthetic data. For the latter, we have assumed a constant pre-summer velocity

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(100 m a^{-1}) and a step change to a summer-plateau (200 m a^{-1}). Random noise of up to 2 m is superimposed on the hourly positions. Using a gaussian kernel does not eliminate potential spurious waves. In contrast, their amplitude seems to be even larger than with the square filter applied (see Figure 1 provided along with this response).

The center point within a gaussian kernel receives the highest weight and consequently, the smoothing effect is less than with a square filter at the same window length. The square filter, smears out velocity anomalies more drastically than the the gaussian, and may therefore entail stronger limitations on the resolving power of velocity anomalies in the time domain. Performing 100 runs of synthetic data generation (with random noise) reveals that using a box-shaped kernel, the detected onset date precedes the step change by 2.3 days, while using the Gaussian kernel, the detected onset date follows the step change with 1.3 days delay. Extending the window length of the gaussian kernel to 9 days, or applying it directly to the hourly raw data, i.e. skipping the step of daily grouping, did not change the result significantly.

To conclude, we do not find any significant improvement in signal-to-noise ratio, using a gaussian kernel. Similar to den Ouden et al. (2010), our smoothed velocity time series may contain spurious waves. These waves have a potential amplitude of $\lesssim$30 m a^{-1}. Fortunately, our absolute velocity values and magnitude of seasonal variations are one order of magnitude larger then the amplitudes of the potential spurious waves, ensuring that our results and discussed features are meaningful, i.e. we do not discuss small-scale fluctuations.

We added a comment on potential spurious waves to the error description in the methods:

"'Given considerable noise level on short timescales (1 day to a few weeks), we cannot exclude the presence of spurious waves, similar to those described by den Ouden et al. (2010). Consequently, we do not interpret periodic fluctuations of smaller amplitude ($\lesssim$30 m a^{-1})."

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Reviewer M. King and reviewer 4 (GC2) point out that 7-day averaging limits precise on-the-day determination of onset date of summer speed-ups. M. King suggest analyzing in artificial event superimposed onto the recorded winter signal, in order to investigate the resolving power of our onset-detection algorithm and to provide a measure of uncertainty.

The above experiments suggest that an uncertainty estimate concerning the resolving power is indeed required, in order to identify significant differences in timing of events at specific sites. We followed the suggestion of M. King and insert an artificial event into the "winter background". Specifically, we modified the recorded data of Basin-3 #2 (see Figure 2 provided along with this response; This data was chosen earlier to present the filtering effects (Fig. 3 in paper; note that the presented data was inadvertently associated with Basin-3 #3, instead of #2). The stake moves approximately eastwards. We create an artificial event by adding displacement in easterly direction over a certain event-length period. The event starts on 15-04-2009 at 1200 hours. Acceleration and deceleration take place over a random period ranging from 2 to 7 (integer) days, each. Both acceleration and deceleration are assumed to take an exponential form $\exp^x$ with $x \in [-3\ 1]$ projected onto the event's acceleration period, and mirrored onto its deceleration period. The maximum additional velocity corresponds to a random value between 0.5 and 2 $\mathrm{m\,d^{-1}}$. Mean and standard deviation during March 2009 provide reference for the onset criterion.

We performed 100 runs in order to generate a range of events with differing length and amplitude and applied filtering as described in the paper, i.e. using a box-shaped kernel. A 7-day averaging filter reduces the magnitude of the speed-up event and smears out the onset and fading of the event in the time domain. The choice of a threshold value (larger than the winter-background's mean and 3 times its standard deviation) partly compensates for this smearing out, because the threshold is only reached further up along the slope (of acceleration). The detected onset depends both on the raw data filtering and the selected onset criterion/threshold. 98% of the

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detected onset dates are within 2 days from the inserted event onset (3 days offset for the remaining 2%). We regard an uncertainty of ± 3 days, corresponding to the approximate halfwidth of the applied kernel, as a conservative estimate of uncertainty in the detection of the onset.

If the velocity peak is symmetric, the position of the peak will be unaltered by the filtering. Asymmetry and random noise may, however, cause some offset. In 70% of our 100 runs, the maximum is detected on the correct day, while for 30% it is 1 day off. We do not know the exact shape of the summer speed-up. If the velocity maximum extends over several days (plateau-like), the exact peak location will be influenced by the present noise. Since the velocity maximum is more symmetric than the onset of the speed-up, and the detection of the maximum velocity does not require formulation of a specific threshold, we regard the associated uncertainty smaller or equal to that of the onset. Again, we select a conservative estimate of ± 3 days.

The revised paper contains these uncertainty estimates. Consequently, we only discuss differences in the timing of events when those are significant, i.e. the difference is larger or equal to 3 days.

In particular, we have added three sentences to Sec. 4.2, presenting the results of the summer speed up:

"We define the onset of the summer speed-up at a particular location as the day when velocities exceed three standard deviations from the pre-summer mean. This threshold determines velocities significantly above the winter background. We have tested the resolving power of our smoothed velocity time series by inserting an artificial speed-up event in the winter background of our recorded GPS data, assuming exponential acceleration and deceleration. Based on 100 realizations with randomly chosen event-lengths (2–7 days) and amplitudes (0.5–2 m a^{-1}), we assign a conservative timing error of ± 3 days to both the onset dates and timing of velocity maxima."

Reviwer4 (GC2) criticizes the criterion for the onset-detection algorithm and re-

marks that "eyeballing" [we suppose that means manual identification of the break of slope, just preceding the summer maximum] does not support the reported increased delay in the speed-up of upglacier sites.

We have defined the reported onset as the date at which velocities rise significantly above the winter-background/pre-summer minimum (now including an uncertainty of ± 3 days). The onset should not be misunderstood as a precise timing of the minute velocity anomaly that develops into a major summer speed up. This is hardly achievable given the accuracy of our system on short time scales (hours to ~ 3 days) and the present background noise (potential spurious waves; see comment above). We regard the chosen criterion as an objective and uniform measure that can be compared at the different sites.

1.2 Interpretation & discussion of deduced velocity results

R4 GC1 suggested to consider the mean velocities over the data gap at Basin-3 in summer 2009, if possible.

The positions before/after start and end of data gaps are known. Daily blocked positions form the basis for our velocity computations. The data gap during summer 2009 does not effect the annual velocities over the period May 2009 to May 2010, inferred from the begin-end method. It does, however, effect the velocities inferred by the daily mean method, and this is discussed in the method-section. Nevertheless, the 2009–2010 mean annual velocity inferred by the mean daily velocities is similar to that inferred by the begin-end method, indicating that the mean velocity over the gap is (coincidentally) similar to the annual mean. We do not provide explicit values for the mean velocities over the gap(s), because we are not interested in the mean velocity over these arbitrary time period(s). Unfortunately, the amplitude and timing of the 2009 summer speed-ups at Basin-3 ,our primary objective , cannot be recovered. However, our stated annual velocities, our secondary objective, inferred by the begin-end method

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are not influenced by data gaps within the period.

R4 GC3 notes that the second speed-up of Basin-3 and Duvebreen are not concurrent but offset by ∼7 days and that this may represent the limited resolving power of velocity time series or that different forcing mechanisms operate on the two outlets.

Several factors may influence the asynchrony of the second velocity peak on the two outlets in 2008 (the second speed-up of Basin-3 actually appears to consist of two merged peaks, one of larger amplitude, followed by another one of smaller amplitude): the fact that surface melt is based on the temperature record of one single AWS, that local rain may influence the availability of water. Furthermore the state of the basal hydraulic drainage system may be different for the two outlets, and hence the response (basal sliding) to meltwater input. In the revised manuscript we acknowledge the asynchrony in the second peak and provide an explanation for it, i.e. limitations in the representativeness of local weather conditions in the temperature record from a single AWS at high elevations. We further relativize the statement by providing an estimate of the resolving power of the smoothed velocity timeseries (see above).

R4 GC4 notes that longitudinal coupling is an instant process and cannot explain delayed speed-up at upglacier sites.

We do not agree with the statement that longitudinal coupling is strictly instantaneous. Ice has viscous properties, and a glacier may hence respond to local application of a force by either (dynamic) thinning or thickening. Spatially differentiated changes in velocities are evident in variable longitudinal strain rates, that may vary in space (e.g. along the flowline) and time (e.g. seasonal variations). Dynamic thinning is associated with positive strain rates (in flow direction). On short time scales, ice may also response to application of a force by brittle fracture, i.e. crevasse formation. This mechanism may also delay the response of upglacier sites to velocity changes farther downstream.

Nevertheless, we have relativized the statement concerning longitudinal coupling in the discussion section:

"Reduced meltwater production at higher elevation may explain the dampened amplitude of the velocity peaks at stakes farther upglacier (Andersen et al., 2010). Flow at these higher locations may also be influenced by longitudinal stress coupling to lower portions of the glacier (Thomas, 2004; Nick et al., 2009), that may be more sensitive to melt-induced acceleration."

R4 GC6 notes that if the observed velocity changes are to be attributed to a certain phase in a surge cycle, this could reduce the relevance of the results in a mass-balance perspective

We do not understand this comment. The implication of an observed increase in ice flux does not depend on its driver. We show that velocities over 2008–2009 were significantly higher than in the mid-1990s and this implies larger mass loss through calving in the more recent years.

R4 GC6 and PC GC2 question our quantitative comparison of continuous GPS observations with the Jan–Feb snapshot of the mid 1990s.

We clearly point out the limitation of the velocity snapshots in providing representative annual mean velocities. In the discussion we state: "Dowdeswell et al. (1999, 2008) considered their annual ice flux estimates to represent a lower limit, because flow rates during the winter months December to February, corresponding to the acquisition period of the SAR data used, are usually lower than the annual average. However, our two-year record indicates that velocities in December to February are approximately equal or, in case of Basin-3 over the period 2009–2010, even slightly larger than the corresponding annual average (Fig. 4). Mean-annual velocities 2009/2010 of ... indicate a more than four-fold velocity increase compared to those used to derive previous ice flux estimates."

Our observations reveal Jan–Feb velocities on Duvebreen ranging from 35–200 m a^{-1}, compared to 20–80 m a^{-1}, reported by Dowdeswell et al. (1999), an increase by a factor 2; on Basin-3 the Jan–Feb velocities are up to $\sim$530 m a^{-1}, compared to $\lesssim$140 m a^{-1}, an increase by factor 4. The acceleration factors of $\sim$2 and $\sim$4 given in the abstract and the discussions are thus justified.

1.3 Extrapolation of temperature record

G. Hamilton points out that a single high-elevation weather station might have missed coastal temperature inversions. He suggest to at least acknowledge the limitation of extrapolating a single melt record to all sites.

We are aware of the limitations when using the temperature record of a single AWS (placed on the glacier surface) to explain local weather situations on the particular flow lines. We have no detailed information of whether or when temperature inversions take place. A strong temperature inversion may indeed restrict local melt at low elevation sites. The temperature record at higher elevations may still be a useful indicator for potential basal lubrication as not only local melt contributes to basal lubrication at a specific site, but potentially all melt occurring farther upstream.

In the the description of the temperature record, we have pointed out that we do not apply elevation-specific corrections of the temperature record. The revised paper, acknowledges this limitation further by attributing the asynchrony of the second velocity peak of 2008 to spatial deviations in local temperature/weather conditions.

Changed "The 2008 summer speed-up was characterized by 3 distinct peaks, concurrent at all locations." to

"The 2008 summer speed-up was characterized by 3 distinct peaks, concurrent at all locations, except for the second speed-up event that followed $\sim$2 weeks after the primary summer maxima in case of Basin-3 and $\sim$3 weeks in case of Duvebreen. The third and last speed-up event during summer 2008 followed ∼7 weeks after the primary summer maximum (Fig. 5a, b)"

In the discussion of the 2008 events, we have added "The asynchrony in the second speed-up event on Basin-3 and Duvebreen may be explained by different local weather situations on the northern and southern side of the ice cap, which may not be equally well represented in a single temperature record. In addition, spatially variable rainfall may have contributed to differently enhance the water input and hence basal motion, at the two sites (Fudge et al., 2009)."

1.4 Consideration of recent publications from Greenland

Most reviewers would like to see the Austfonna results discussed in relation to recent work from Greenland; new references should be included in the introduction and these should be referred to later in the discussion.

We have added numerous references to the discussion that put our observations of Austfonna into a wider context. This required some reorganization and rewording of the discussion section.

R4 GC5 notes that the iceberg calving flux references limited to Dowdeswell et al. (1999, 2008) and that none of the numerous calving papers from Greenland are considered here.

We observed longterm changes in ice-surface velocities on two outlet glaciers of Austfonna and are primarily interested in the specific calving flux associated with this outlet glaciers. The publication of Dowdeswell represent the only previous estimate of the calving flux from Austfonna and therefore the only study to which we can relate our inferred ice flux for 2008–2009.

1.5 Consideration of oceanic forcing mechanism

Reviewer4 (GC4–6)criticizes that oceanic forcing mechanisms are not considered in explaining the observed velocity variations and points at extensive studies of Greenland outlet glaciers, that found significant correlations between changing flow dynamics and changing oceanographic conditions.

Many of the Greenland outlets, reviewer 4 may refer to, have extensive floating tongues. In contrast, the marine portions of Austfonna are well grounded. Consequently, the contact surface between the glacier and the ocean is essentially limited to the submerged part of the calving front. Oceanic conditions may influence the frequency and magnitude of calving events at the front, while significant changes in sea ice cover may impose changes in backstress at the marine terminus. We acknowledged in the introduction, that these factors may influence flow velocities. However, because of the fact that the glacier is well grounded, we believe that changes in basal conditions provide the primary control on both seasonal and decadal variations in fast-flow. Our dataset does not include observations directly at the calving front, nor information on oceanographic conditions, i.e. we have no basis for a discussion on potential oceanic forcing. Here, we report on observed velocity fluctuations along the flowlines of two outlet glaciers and show that the observed fluctuations can be explained, at least to first order, by surface melt.

An additional thought of the variability in oceanic conditions: the West-Spitsbergen current (WSC; the final branch of the Gulf Stream) flows along the western shelf break of Svalbard. The WSC represents the main contributor of oceanic heat and salt to the Arctic Ocean (Aagaard and Greisman, 1975). Temporal variations of WSC overflow onto the shelf may cause significant variations in heat budget of the fjords along the west coast. On the east side of Svalbard, however, no such warm current exist and fluctuations of oceanic conditions are probably much smaller.

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2 Editorial corrections and minor issues

2.1 Reviewer 1; Matt King (MK)

MK: P3425 L28: "and basal lubrication does not occur" - I didn't understand this phrase in the context of the sentence and suggest a reword
OK; reworded and split into 2 sentences: "Meltwater refreezes in the snowpack until the cold-content of the snow (and firn) is diminished, thereby causing delayed runoff. Basal lubrication does not occur before a connection between the supraglacial and englacial/subglacial drainage system is established (Copland et al., 2003)."

MK: P3426 L8: the work of Bartholomew et al, Joughin et al, and Sole et al should be considered here
added references Joughin et al. (2008); Andersen et al. (2010); Bartholomew et al. (2010); Sole et al. (2011)

MK: P3426 L24: "Also" didn't quite flow from the previous sentences; L25: "rely $->$ relies"
reworded to " A previously published calving estimate of 2.5 ± 0.5 Gt a^{-1} (Dowdeswell et al., 2008) relies on the mid 1990s surface-velocity snapshots."

MK: Figure 1: it would be really nice to have the Dowdeswell et al. InSAR velocities plotted on here - it would save some text. Hopefully this has been, or could be, made available
OK, modified Figure 1, which now includes Figure 1b, the InSAR ice-surface velocities, modified from Dowdeswell et al. (2008).

MK: P3430 L19: "of" $->$ "to"
OK

MK: P3431: this analysis of error (in taking standard deviations) ignores tempo-

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ral correlations in the time series. that is, when you assume white noise (uncorrelated) std. dev. assumes too many uncorrelated obs and hence is too optimistic. Is there evidence of temporal correlation? If not in the raw (the hourly sampling may decorrelate it) the moving average obviously does introduce temporal correlations. recall the 2*sigma_xy value when propagating variances (the correlation that is normally ignored). I have often found a Gaussian averaging kernel does a better job than a boxcar
See general response to filtering and note that we now mention that our smoothed velocity timeseries may contain spurious waves, similar to those discussed by den Ouden et al. (2010).

MK: P3433 L10: express as a %?; L11: ♯1 does not go higher than in June 2008; Figure 4 & 5: the period over which these velocities were computed is not clear.
OK; "Monthly mean velocities in June 2009 were up to 23% faster than those observed in June 2008, except for the lowermost site, where velocities did not change significantly."

MK: P3433 L15: it is ambiguous if it is meant that the speed increases over the entire period, or just between May 2009 and May 2010. Table 2 seems to contain text which may belong here.
Reformulated "Annual mean velocities during May 2008 to May 2009 ranged from ca. 120 m a^{-1} at B3 #5 to 400 m a^{-1} at B3 #1 (Table 2). Over the period May 2009−2010, velocities increased by 21−41%." to

Over the time period May 2008 to May 2009, annual mean velocities inferred by the begin-end method ranged from ∼120 m a^{-1} at B3 #5 to ∼400 m a^{-1} at B3 #1 (Table 2). Over the subsequent period May 2009−2010, annual mean velocities increased by 21−41%.

MK: P3434 L5: "speed-ups were"
OK

MK: P3434 L12: significance of 0.1m/yr quoted precision is no doubt questionable?
OK; also reduced level of detail in Tables (see response to reviewer 4)

MK: P3434 L14: sentence beginning "At Duvebreen" seems to belong to the next paragraph
Split up in three paragraphs (previously two) and reworded the second :

1. description 2008 summer speed-up of Basin-3
2. description 2008 and 2009 summer speed-up of Duvebreen
   "At Duvebreen, the 2008 summer speed-up was less pronounced (16–43% acceleration compared to the pre-summer minimum) and only detected at the three lowermost sites along the flowline (Duve #1−3; Fig. 5b). The 2009 summer speed-up at these locations was of higher amplitude (up to 59% at Duve #2; Table 3) and also detected farther upglacier than in 2008."
3. discussion summer speed-up and air temperature (PDD)

MK: P3437 L19: "considered their"
OK

MK: P3440 L9: do the authors care to widen their discussion and conclusions out to consider the other glaciers draining the ice cap?
No, because we have no in-situ observations from the other outlet glaciers and regard it not justified to extrapolate the results from the two observed outlets on all outlets.

2.2 Reviewer 2; Gordon Hamilton (GH)

GH: P3424(2) L15: not entirely clear what you mean by "the principle melt was of high amplitude". Do you mean the major melt event of the season?

Changed “principle” to “major” (the main melt event, once the melt season has started, and which is followed by subsequent melt events upon termination of the summer melt season)

GH: P3424(1) L20: “that” –> “those”
OK

GH: P3424(2) L25: “strong” – > “important”
OK

GH: P3424(2) L26: delete “the” in “the ice is exposed...”
OK

GH: P3425(3) L24: the Greenland references could be updated to include more recent work (e.g., Bartholomew et al., 2010; Andersen et al., 2010, 2011; etc.)
“Excessive input of meltwater into the subglacial drainage system early in the summer melt season ... promoting high velocities in excess of those during winter (Iken and Bindschadler, 1986).”; added references Nienow et al. (1998); Mair et al. (2001) - actually not a Greenland reference, nevertheless a relevant addition

“...enhanced ice-surface velocities following surface melt were also reported for the Greenland ice sheet (Zwally et al., 2002)...”; added references Joughin et al. (2008); Andersen et al. (2010); Bartholomew et al. (2010); Sole et al. (2011)

“Sustained input of large volumes of meltwater may retard rather than enhance glacier motion... (van de Wal et al., 2008; Sundal et al., 2011).”; added references Bartholomew et al. (2011)

GH: P3425(3) L28: delete comma after “occur”
OK

GH: P3426(4) L5: “hamper” – > “retard”
OK

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GH: P3426(4) L25: it would be more useful to report calving fluxes as a mass (Gt/yr) instead of a volume. See elsewhere as well.
OK; Dowdeswell et al. (2008) reported his calving estimate in $\mathrm{km^3\,a^{-1}}$ w.e., which is identical with $\mathrm{Gt\,a^{-1}}$. We now provide all calving flux estimates in $\mathrm{Gt\,a^{-1}}$.

In particular: P3426 L24–25: Changed "Also the calving estimate of $2.5 \pm 0.5\,\mathrm{km^3\,a^{-1}}$ w.e. published by Dowdeswell et al. (2008) rely on mid 1990s surface-velocity snapshots." to "A previously published calving estimate of $2.5 \pm 0.5\,\mathrm{Gt\,a^{-1}}$(Dowdeswell et al., 2008) relies on the mid 1990s surface-velocity snapshots."

P3428 L8–9: Changed " Yet, the net mass balance of Austfonna over the period 2002–2008 is negative, $-1.3 \pm 0.5\,\mathrm{Gt\,a^{-1}}$(Moholdt et al., 2010), due to calving and retreat of the marine ice margin at rates of several tens of meters per year during the past few decades (Dowdeswell et al., 2008)."; Moholdt et al. (2010) result was actually given in $\mathrm{km^3}$ w.e. $\mathrm{a^{-1}}$!

P3437–3438 at several occasions: changed $\mathrm{km^3}$ w.e. $\mathrm{a^{-1}}$ to $\mathrm{Gt\,a^{-1}}$

GH: P3426(4) L27: no hyphen necessary in "GPS-observations"
OK

GH: P3426(4) L28: delete "presented"
OK

GH: P3427(5) L8: delete "large"
OK

GH: P3427(5) L26: "...also suggests that..."
OK

GH: P3428(6) L2: be careful not to confuse surface elevation change with "thickening". Fix by deleting "with interior thickening at rates" and replace with "of up to +0.5 m/a"
We do not understand the comment. We are careful not to confuse elevation change

with mass change, with regards to e.g. firn densification. Post-glacial rebound rates are two orders of magnitude smaller than the observed positive elevation changes and can be neglected given the short timescales, discussed here. Positive elevation changes are thus related to glacier thickening, in terms of added snow, firn and/or ice.

GH: P3428(6) L6: delete “mass”
Surface mass balance is a common term, that we chose to keep. However, we rewrote and corrected the particular section:

“Geodetically derived mass balance for the time period 2002$-$08 suggest a surface mass balance close to zero with For 2008, mass balance stakes indicate an unusual low ELA (Moholdt et al., 2010). For 2008, mass balance stakes indicate an unusual low ELA. Yet, the net mass balance of Austfonna is negative, -1.3 ± 0.5 Gt a^{-1}, due to calving and retreat of the marine ice margin at rates of several tens of meters per year during the past few decades (Dowdeswell et al., 2008).” to

“In-situ mass balance measurements over the time period 2004$-$08 indicate a surface mass balance close to zero (Moholdt et al., 2010). The mean equilibrium-line altitude (ELA) equaled about 450 m a.s.l. for the northwestern and 300 m a.s.l. for the southeastern basins, including unusual low values in 2008. Yet, the geodetic net mass balance of Austfonna over the period 2002–2008 is negative, -1.3 ± 0.5 Gt a^{-1}, due to calving and retreat of the marine ice margin at rates of several tens of meters per year during the past few decades (Dowdeswell et al., 2008; Moholdt et al., 2010).”

GH: P3428(6) L8: I think you have confused ELA with snowline. The ELA is the long-term average annual snowline position.
ELA is correct and inferred from in-situ mass balance measurements, i.e. a network of mass balance stakes.

GH: P3428(6) L13: “have been” $->$ “were”
OK

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GH: P3428(6) L19: "...and the geometries along their central flowlines."
OK

GH: P3428(6) L25: "down to" $->$ "at"
OK: "grounded at a maximum depths of ∼150 m"

GH: P3428(6) L27: delete "has"
GH: P3428(6) L28: ditto.
GH: P3429(7) L1: "since" what? Part of a sentence missing here?
OK: "Basin-3 has surged around 1850–1870... Subsequently, the terminus retreated..."

GH: P3429(7) L2: "for" $->$ "of"
OK

GH: P3429(7) L7: state the distance over which the elevation increase occurs.
OK: Changed to "...with a mean surface slope of 0.84° over 16 km"

GH: P3429(7) L14: in what way did Landsat imagery suggest inactivity?
OK: Changed to "The absence of significant crevasse fields in Landsat imagery from 1973−1991 suggest that the flow unit was fairly inactive over that time period and fast flow initiated after 1991."

GH: P3429(7) L23: delete "the" so, "...drains into narrow Duvefjorden"
OK

GH: P3430(8) L1: awkward phrasing, change to "...ranges from x300 m to >400 m from the lower to upper ends, with a mean of..."
OK

GH: P3430(8) L3: "further" $->$ "farther" because you are talking about distance. Also needs changed throughout the manuscript.
OK

GH: P3430(8) L4: delete "down to sea level and beyond"

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OK

GH: P3430(8) L10: why write "may" when a quick check of satellite imagery would tell if the supraglacial lake forms at that location?
OK: after checking SPOT-5 imagery with colleague C.Nuth at the Department of Geosciences (UiO), we changed sentence to: "Field inspections in spring 2008 and SPOT-5 satellite imagery from Aug 14 2008 (Korona et al., 2009) suggest that a supra-glacial lake forms within the depression during the summer melt season."

GH: P3430(8) L14: "recording units" $->$ "receivers"
OK

GH: P3430(8) L15: rephrase, "...atop stakes drilled..."
OK

GH: P3430(8) L18: "units" $->$ "receivers"
OK

GH: P3430(8) L20: Insufficient power implies the receivers did not have solar charging capability. Is that really the case? No solar??
Yes, no solar! Include info on power supply in instrument description (see below).

GH: P3430(8) L23: by single-frequency, do you mean L1 only?
Changed "We utilized single-frequency GPS receivers that operate unmaintained over time periods of..." to

"We utilized single-frequency (L1 band) GPS receivers. Powered by a single 3.6 V lithium battery, the receivers operate unmaintained over time periods of..."

GH: P3430(8) L25: unclear what you mean by allowing the system to "stabilize"
Changed "...allow the system to stabilize." to

"...allow stabilization of the received satellite signals."

GH: P3431(9) L5: slightly unfocused discussion of errors in this section. You

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start by mentioning the accuracy of single position estimate, then discuss filtering of the data, and then describe the uncertainty estimates. It seems like this flow of ideas could be made more intuitive.
Moved discussion of the accuracy of the single position estimate (P3431 L5–11) downwards to just before the description of the uncertainty estimates. Split text into three paragraphs:

1. description of instruments and data recording
2. description of raw data and processing
3. discussion of the accuracy of raw data and uncertainty of final products

GH: P3431(9) L10: awkward phrasing, change to "A larger error is expected in year with higher..."
OK

GH: P3431(9) L14: what do you mean by an "individual dataset entry"? Do you mean an epoch?
changed to an "hourly position"

GH: P3431(9) L15: "energy" $->$ "power"
OK

GH: P3431(9) L17: "e.g., due to riming...of the satellite signal" $->$ "multipath"
Added a sentence concerning multi-path errors to the instrument description: " Multipath errors are reduced by employing an antenna design that minimizes signal reception from below."

GH: P3431(9) L19: "measurements" $->$ "estimates"
OK

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GH: P3431(9) L27: "...are calculated trigonometrically from the averaged positions." delete "utilizing..."
OK

GH: P3432(10) L18: write out AWS on first use.
OK; Abbreviation AWS was previously introduced in the last paragraph of the introduction, where it is now only written out.

GH: P3432(10) L19: was the AWS on ice or rock? It was at quite a high elevation (510 m), so presumably all the GPS receivers were at lower elevations. Granted, your analysis only examined the timing of melt events and not their absolute magnitudes, but there is a strong likelihood that a single high-elevation weather station might have missed coastal temperature inversions. In other words, you infer melting occurs everywhere based on the AWS record from 510 m elevation, but there might have been instances when low-elevation GPS sites were experiencing much cooler (non-melting) temperatures during an inversion. Is there any way to address this issue? Do you have access to any weather station data from sites closer to sea level? If not, you ought to at least acknowledge the limitation of extrapolating a single melt record to all sites.
See general comment.

GH: P3433(11) L8: "...their summer maxima but maintained relatively high speeds..."
OK

GH: P3433(11) L10: "above" $->$ "faster"
OK; "faster than"

GH: P3433(11) L11: not clear what you mean by "not captured". Do you mean there is missing data, or do you mean the event did not occur?
Changed "Unfortunately, the summer speed-up of Basin-3 was not captured in 2009,..." to

"Due to missing data, the 2009 summer speed-up of Basin-3 was not captured,..."

GH: P3433(11) L17: define what you mean by "short-lived". Do you mean a few hours? Several weeks?
Modified this section to "At Duvebreen, ice-surface velocities maintained a relatively steady level over the two-year period. Short-lived summer speed-ups were only noticeable at the lower locations (Fig. 5b), i.e. within a few weeks after the summer maximum, velocities returned to just above pre-summer values."

GH: P3433(11) L22: "unit" $->$ "receiver"
OK

GH: P3434(12) L1: the phrase "quasi-stationary pre-summer low" is unclear.
Changed sentence to "During June, the velocities fluctuated around a pre-summer low and apparent trends were absent."

GH: P3434(12) L4: "when" $->$ "where" since the object is a time ("the day"), not a place ("a particular location").
OK; "where" $->$ "when"

GH: P3434(12) L4: "are in excess of" $->$ "exceed"
OK

GH: P3434(12) L6: "between" $->$ "at"
OK

GH: P3434(12) L10: "lowermost site"
OK

GH: P3434(12) L25: "where" $->$ "when"
OK

GH: P3435(13) L12: "provoke" $->$ "promote"
OK

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GH: P3435(13) L21: "For both flowlines..."
OK

GH: P3435(13) L27: "This indicates that basal water pressures remained high and only..."
OK

GH: P3436(14) L12: define "short-lived"
explained above

GH: P3436(14) L19: "possess" $->$ "impart"
"cause"

GH: P3436(14) L21: "where" $->$ "when"
OK

GH: P3436(14) L24: delete "initiated at the calving front."
OK

GH: P3437(15) L5: delete "negative in the case of retreat"
OK

P3437(15) L9: use consistent units (m or km, not both)
OK; $5 \times 0.24\,\mathrm{km}^2$

GH: P3437(15) L19: "his" $->$ "these"?
"their"

GH: P3438(16) L1: "...usually lower than the annual average."
OK

GH: P3438(16) L1: "indicate" $->$ "indicates"
OK

GH: P3438(16) L10: no apostrophe in 1990s

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OK

GH: P3438(16) L20: "...factor of four..."
OK

GH: P3438(16) L20: "showed also" $->$ **"also showed"**
OK

GH: P3438(16) L24: put 550 m/a in parentheses.
"...550 m a^{-1} (1.5 m d^{-1}; Lefauconnier et al., 1994)" and

"...recent summer maxima of 900 m a^{-1} (2.5 m d^{-1})..." and changed

"...as high as those reported from Kronebreen..." to "...almost as high..."

GH: P3439(17) L5: in the introduction, you raised the issue of sustained meltwater inputs retarding glacier flow, as shown by the observational work of Van de Wal et al. (2008) and Sundal et al. (2011) in Greenland, and modeling work by Schoof (2010). Maybe you can include some remarks about how your Austfonna results support (or do not) this idea. It would help put your work in a wider context.
OK- see general comment.

GH: P3439(17) L19: "Navigation data were..."
OK

GH: P3440(18) L7: "legs" $->$ **"segments"??**
OK

2.3 Reviewer 3; Poul Christoffersen (PC)

PC: 1. A comment on size and dimensions. The size and maximum height of Austfonna is almost always referred to as 8000 km2 and 800 m, respectively.

Given the detained work of recent years, I think it would be helpful to add more precise characteristics. Please be precise.
Previous publications providing a figure on the areal extent of Austfonna were not always clear of whether or not Vegafonna, a 300 km^2 ice cap connected to SW Austfonna, is included in the figure. The most recent figure for Austfonna (excluding Vegafonna) is 7800 km^2 with a maximum elevation of 800 m a.s.l. (Moholdt and Kääb, in press). We changed "Austfonna is a $\sim$8000 km^2 ice cap..." to "Austfonna is a 7800 km^2 ice cap (Moholdt and Kääb, in press)..."

PC: 2. A comment on velocity. The GPS velocity data is compared against the InSAR velocities from the early 1990, as reported in Dowdeswell et al. (1999). I suggest mentioning also the InSAR velocities derived by others, e.g. Bevan et al. (Ann. Glaciol., 2007) and Moholdt and Kaap (2011, + Thesis?). Make sure the inferred change between the early 1990s and 2008-10 is real, i.e. that the comparison of InSAR snapshots and continuous GPS records is valid.
We specifically focus on Dowdeswell et al. (1999), because they provide explicit velocities along the flowlines of both Duvebreen and Basin-3. The velocities published by Bevan et al. (2007) and Dowdeswell et al. (2008) are also based on mid 1990s InSAR data and yield similar results. Dowdeswell et al. (2008) employed dual-azimuth differential SAR interferometry (DInSAR) and intensity tracking methods and we have chosen to present the resulting velocity structure (Fig. 4 in their paper) in modified form as a Figure 1 b in our paper. Moholdt and Kääb (in press), nor Moholdt's PhD thesis, report velocities. Moreover, also Moholdt and Kääb (in press) uses InSAR data from the mid-1990s, but combines those with more recent ICESAT laser altimetry to derive a new DEM of Austfonna.

Changed "InSAR data acquired in January 1994 revealed that a large portion of the ice cap is moving at low velocities $< 10\,\mathrm{m\,a^{-1}}$, interrupted by spatially limited flow units characterized by enhanced ice surface velocities in the range of about $50-250\,\mathrm{m\,a^{-1}}$ and coincident with subglacial valleys or troughs (Dowdeswell et al., 1999).” to

“InSAR data acquired in the winter months of the mid 1990’s revealed that a large portion of the ice cap is moving at low velocities $< 10\,\mathrm{m\,a^{-1}}$, interrupted by spatially limited flow units characterized by ice surface velocities in the range of about $50-250\,\mathrm{m\,a^{-1}}$. The location of these flow units coincides with subglacial valleys or troughs (Dowdeswell et al., 1999, 2008; Bevan et al., 2007, Fig. 1 b).”

PC: 3. A comment on outlet glacier behavior. The discussion on water system vs. deformation of basal sediment is very interesting. It is clearly relevant due to the different behavior recorded in the two drainage basins. I wonder whether a more explicit distinction could be made, i.e. whether Duvebreen could be interpreted to behave similar to mountain glaciers controlled by drainage system evolution, while Basin-3 is influenced more directly by basal slipperiness controlled by sediment yield strength? Similar observations are made in Greenland (see e.g. Howat et al., JGlac, 2010) - a link worth mentioning at least briefly.
We have no direct information on basal conditions and any discussion remains speculative. We are therefore careful to not over-interpret our observations and regard cumulative PDD, derived from the temperature record as a first-order control on the observed velocity fluctuations (see also response to general criticism, i.e. consideration of publications from Greenland). See also general comments.

PC: Title: Why use ’inferred’ when the speed-up is measured?
Strictly speaking, both position and velocity are computed variables, based on the GPS receiver’s measurement of the traveltime of broadcasted satellite signals. Thus, we decided to keep the original title.

PC: 3425/18: Add a sentence on the delayed effect from diffusion of elevation change.
Do not really understand this comment. The corresponding paragraph discusses factors that may influence sliding velocities on short time scales (hours to weeks) on which

changes in glacier geometry are negligible.

PC: Section 2.2: The surge record cited seem anecdotal? The out-of-balance velocity described in paragraph 2 may be the more convincing argument for cyclical variability. Consider merging the two paragraphs. Add newer velocity data if possible.

We refer here Lefauconnier and Hagen (1991) who gathered evidence of Basin-3's previous surge. What may have made it sound anecdotal is the fact that the exact timing of the surge cannot be pinpointed. Swedish explorer Nordenskjöld, undertook the first expedition across Austfonna in 1873 and reports a heavily crevassed 'ice-valley' in the area(Leslie, 1879; Nature 20, 1879). To make it sound less anecdotal and consistent with previous notion of the surge in Sec. 2.1 we changed the sentence:"Basin-3 has surged some years prior to 1873 (Lefauconnier and Hagen, 1991)." to "Basin-3 has surged around 1850–1870 (Lefauconnier and Hagen, 1991), coincident with the Little Ice Age, when many glaciers on Svalbard reached their Holocene maximum extent (Svendsen and Mangerud, 1997)."

PC: Section 2.2: Add newer velocity data if possible.

No other velocity data, apart from the mid-1990s InSAR data has been published for the survey area. So, therein lies the motivation of this paper. We are, however, aware of unpublished velocities for 2008, derived from ALOS PALSAR data (personal communication T. Strozzi, GAMMA Remote Sensing AG). Analysis of longterm, basin-wide velocity, elevation and terminus-position changes using multiple ground-based and spaceborne observations are subject of an ongoing study at the Department of Geosciences, University of Oslo, and will be published in a later paper.

PC: Section 3.2: The PDD results should be described in the Results section, not here.

OK; moved the PDD results to the last, reworded paragraph of the result, where they are first used; the section on the summer speed-ups. A separate result section would be somewhat immoderate.

PC: 3435/13: New paragraph?
OK

PC: 3435/19: After '... ice-bed coupling.', you could add a sentence saying that the lack of response to hydrological inputs could be caused by full saturation of the basal sediment, i.e. that the added extra water has no or only little effect (which is different from assuming that the hydrological system accommodates the water). This extra comment would be relevant given the discussion in the following paragraph.
We point out the potential influence of sediment deformation at several occasions within the discussion. At the same time we are careful to not overinterpret our data, as we have no direct information about the basal conditions.

PC: 3435/23: Add 'delayed' between 'hence' and 'local'
OK

PC: 3437/19: replace 'his' with 'their'.
OK

PC: 3438/10-11: Is this difference influenced by the different techniques used? Make sure the comparison of InSAR snapshots and continuous GPS records is valid.
OK

2.4 Reviewer 4; anonymous (R4)

R4: P3424, L23 spatially limited FAST-flow units. . ..
OK

R4: P3424, L26 implications FOR glacier. . .
OK

R4: P3425, L2 and the CHANGE IN POSITION of the. . .
OK

R4: P3425, L10 delete "or near" - basal sliding can occur in cold ice but I don't know any work that suggests fast glacier flow can occur without basal temperature at PMP.
The formulation "at or near PMP" allows for a case in which sliding over a certain area takes place or is initiated, even if a small fraction of the basal area is slightly below PMP.

R4: P3425, L18 the Price reference is not really appropriate here as it was looking at land terminating ice margins. It should however come in later in the discussion when you are talking about longitudinal coupling and coupling lengths.
We reformulated the sentence "In addition, calving events (Thomas, 2004) or buoyancy perturbations due to ocean tides (O'Neel et al., 2003) may influence the stability of marine termini, and cause velocity fluctuations to propagate up-glacier by means of longitudinal stress coupling (Price et al., 2008; Nick et al., 2009)." to "In case of tidewater glaciers, velocity fluctuations may also be driven by the reduction of back-stress at their termini (Joughin et al., 2008; Nick et al., 2009), such as caused by calving events (Thomas, 2004) or buoyancy perturbations due to ocean tides (O'Neel et al., 2003). Velocity fluctuations may propagate up-glacier by means of longitudinal stress coupling (Price et al., 2008)."

R4: P3425, L19 not clear what you mean by excessive "charge" of the subglacial drainage system
Reworded "Excessive charge of the subglacial drainage system**...**" to "Excessive input of meltwater into the subglacial drainage system**...**"

R4: P3426, L2 start new paragraph at sentence beginning "The relationship. . .**."**
OK

R4: P3426, L10 can be DERIVED FROM satellite. . .

OK

R4: P3427, L2 in parallel WITH the
OK

R4: P3427, L4 and flow dynamics. WE ALSO compare the...
OK

R4: P3427, L17 the suggestion that this ice-surface velocity pattern "is typical for a slow moving Arctic ice cap" needs a reference to support it.
That statement is supported by observations from other Arctic ice caps, e.g. the Devon Island Ice Cap and the Academy of Sciences Ice Cap. Reworded “The general ice-surface velocity pattern derived from InSAR data acquired in January 1994 is typical for a slow moving Arctic ice cap. The bulk of the ice is moving at low velocities $< 10\,\mathrm{m\,a^{-1}}$, interrupted by spatially limited flow units characterized by enhanced ice surface velocities in the range of about $50-250\,\mathrm{m\,a^{-1}}$ and coincident with subglacial valleys or troughs (Dowdeswell et al., 1999).” to “InSAR data acquired in the winter months of the mid 1990’s revealed that a large portion of the ice cap is moving at low velocities $< 10\,\mathrm{m\,a^{-1}}$, interrupted by spatially limited flow units characterized by ice surface velocities in the range of about $50-250\,\mathrm{m\,a^{-1}}$. The location of these flow units coincides with subglacial valleys or troughs (Dowdeswell et al., 1999, 2008; Bevan et al., 2007, Fig. ?? b). This velocity pattern appears to be typical for many Arctic ice caps, e.g. the Devon Island Ice Cap in Arctic Canada (Dowdeswell et al., 2004) or the Academy of Sciences Ice Cap in the Russian Arctic (Dowdeswell et al., 2002).”

R4: P3427, L23 "in the past; Etonbreen and ...."
OK

R4: P3428, L1-4 The observed interior thickening and marginal thinning are explained with reference to timing within a surge cycle. However, could this behaviour be indicative of a warming climate signal with enhanced accumulation inland and enhanced melting at the margins? (along the same argument as re-

cent observations from Greenland e.g. Pritchard et al, 2009).
Added: “Bamber et al. (2004) suggested increased moisture flux from the adjacent Barents Sea, and hence increased accumulation, as the cause for the positive elevation changes over the accumulation area. However, this hypothesis is not supported by investigations of shallow ice cores, indicating unchanged longterm net accumulation rates over the time period 1963–1986 and 1986–1998, respectively (Pinglot et al., 2001).”

R4: P3428, L25 delete "down"
Changed “ A large area of the basin is marine grounded down to depths of ∼150 m below sea level...” to “ A large area of the basin is marine grounded at a maximum depths of ∼150 m below sea level...”

R4: P3428, L27 earlier you say that Basin-3 surged between 1850-70 so this reference to some years prior to 1873 is rather odd.
Changed “Basin-3 has surged some years prior to 1873 (Lefauconnier and Hagen, 1991).” to “Basin-3 has surged around 1850–1870 (Lefauconnier and Hagen, 1991), coincident with the Little Ice Age, when many glaciers on Svalbard reached their Holocene maximum extent (Svendsen and Mangerud, 1997).”

R4: P3428, L28 does this need to say "from its maximum SURGE or LIA extent"?
see just above

R4: P3429, L1 delete "since"
OK; also added at beginning of sentence: “Subsequently, the terminus retreated...”

R4: P3430, L6-7 should really refer to the work of Gudmundsson here in terms of the effect of bed topography on the surface expression of ice.
Added reference to Gudmundsson (2003)

R4: P3433, L17-18 at Duve 1 and 2, velocities appear to stay considerably above the pre-summer values for several months, not just a few weeks as stated i.e. at

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Duve 1 pre value is ∼175m, post value is ∼200m for 4 months and at Duve 2, pre value is ∼135m and post value is ∼150m for ∼6 months.
Changed "...velocities returned to pre-summer values." to "...velocities returned to just above pre-summer values."

R4: P3434, L1 "quasi-stationary" is the wrong term, approximately constant or invariant perhaps better.
Changed "During June, the velocities were characterized by a quasi-stationary pre-summer low." to "During June, the velocities fluctuated around a low, stable level."

R4: Tables - Many of the columns in the tables have too many decimal places - either this level of detail is not needed (e.g. azimuth) or not appropriate given the associated errors (e.g ice thickness/bed elevation) so space could be saved here.
We have skipped some of the decimals, less for the matter of space, but the appropriate level of detail;

Tab. 1: Number of given decimals in Latitude corresponds to ∼1 centimeters, which is the same order of magnitude as the error. For the same reason, the level of detail for surface altitude is now reduced to decimeters and for both ice thickness and bedrock elevation to meters. We thus drop the ±x m for thickness and bed rock elevation and provide the error in the last row. The table caption is complemented by the sentence "Values marked by an asterisk are interpolated values (see Sect. A)."

Tab. 2: Decimals are omitted from the azimuth of the flow direction and the means and standard deviations of the velocities.

Tab. 3: Decimals are omitted from means and standard deviations of June and summer-maximum velocities.

R4: Similarly, using '08 and '09 instead of 2008 and 2009 would save space.
We prefer the cleaner look of e.g. 2008 compared to '08 and believe the minimal

additional use of space is justified.

R4: Fig 2. Add distance to the calving front terminus to the caption for both glaciers.

Added sentence to figure caption: “The lowermost stakes on both flowlines are located ∼5 km upstream from the calving front.”

R4: Fig 3b. The x axis cannot be interpreted. Can you make it easier to make sense of with better labelling and clearer hash marks for the named months.

Changed ticks of x axis to extend outside of frame and plotted corresponding grid lines slightly thicker and in black, instead of grey. Note that Fig 3 a and b show the position and velocity of stake Basin-3 #2 NOT 3. Changed the caption accordingly.

Note: only references not included in the revised manuscript are provided.

References

Aagaard, K. and Greisman, P.: Toward New Mass and Heat Budgets for the Arctic Ocean, J. Geophys. Res., 80, 3821–3827, http://dx.doi.org/10.1029/JC080i027p03821, 1975.

Leslie, E., ed.: The Arctic voyages of Adolf Erik Nordenskjöld, Macmillan, London, 1879.

Nature 20: Nordenskjöld’s Arctic Voyages, Nature, 20, 631–637, doi:10.1038/020631a0, 1879.