The referee's comments are printed in bold (partly re-organized and in summarized form). Our response to each item follows in normal font. We first summarize and address the key issues raised by both reviewers, then respond to each individual comment. Finally, the key changes to the revised manuscript are listed.

SUMMARY OF COMMENTS for both reviewers

1. The study only focuses on the hydro-thermodynamic feedback to surface melt; the proposed mechanism needs to be separated from the general surge mechanics

The main focus of this paper is indeed the role of surface melt in promoting the surge of Basin-3. Our continuous GPS timeseries reveals details of a surge that were, to our best knowledge, never recorded before. The active role of external forcing for glacier surges contrasts the general notion that surges are purely controlled by internal mechanisms.

It was not our aim to exclusively focus on the proposed hydro-thermodynamic feedback to summer melt. We state that Basin-3 was pre-conditioned during a quiescent phase, i.e. it experienced a geometric built-up of a reservoir area and the associated changes in thermal regime and driving stress. This pre-conditioning initiated spatially confined fast flow (phase 1) and can be explained by thermally controlled soft-bed surge mechanisms, suggested earlier in the context of Svalbard glacier surges (Hamilton and Dowdeswell, 1996; Jiskoot et al., 1998; Murray et al., 2000). The hydro-thermodynamic feedback amplifies and spreads the dynamic changes (phase 2 and 3). We consider the hydro-thermodynamic mechanism as an integral part of the surge mechanics, not a separated process. Previous observations of glacier surges relied on archived satellite data. Isolated snapshots of surface motion were derived mainly for the winter season, as surface melt degrades the quality of the derived fields (Murray et al., 2003a; Murray et al., 2003b). Changes during the summer melt season have therefore not been captured. Our continuous GPS timeseries reveals that the multiannual surge initiation of Basin-3 was not gradual, but occurred in discrete steps, coincident with the summer melt period. Based on our observations of surface velocities it is not possible to isolate the effect of surface melt. A numerical model of glacier surges, incorporating both internal mechanisms and surface melt, would allow quantification of the contribution of surface melt to glacier surges. To our best knowledge, such a model does not exist. Its development would be an interesting and challenging follow-up study, however, far beyond the scope of this paper. In the revised manuscript, we tried to clarify the above views better.

2. The paper could be better structured with a stronger focus on Svalbard glacier surges against which to compare the observations of Basin-3

This paper is not meant to represent a review paper on Svalbard glacier surges. Nevertheless, the introduction paragraph on glacier surges has been extended to include theoretical considerations of thermally-controlled soft bed surge mechanism, as well as previous observations of Svalbard glacier surges, adding relevant key publications. Our observations are than discussed in the context to these studies. The information on ice sheet instabilities are removed from the introduction and moved to the discussion, where we consider the implications of our observations from Basin-3 in a wider context.

3. Conclusions of a surging Arctic ice cap cannot be applied to the "non-surging" ice sheets

We believe analogies between glacier surges and partial ice sheet collapses are widespread throughout the glaciological literature (e.g. Clarke et al., 1984; Jiskoot et al., 2000) and, for instance, this year's AGU fall meeting features a session on "Ice Sheet Surging, Meltwater Pathways & Abrupt Climate Change"). We do not expect different physical laws to apply for glaciers, ice caps or ice sheets, while the importance of individual processes and the involved timescales certainly differ. In case of ice sheets, climatic boundary conditions may not be stable over a sufficient time period in order to generate a cyclic behaviour as observed for smaller surge-type glaciers, or the timescales involved in ice sheet instabilities may lay outside scientific records.

4. Some of the GPS data have been published earlier

Dunse et al. (2012) presents the first 2 out of 5 years of the GPS timeseries and discuss the data in the context of seasonal velocity variations. The most important processes became visible only after the initial 2 years, i.e. unstable ice flow and its drivers reported on in the present paper were not yet conceivable (see figures below).

5. The manuscript and its conclusions are too speculative

Our observations are limited to surface processes. Stepwise acceleration, coincident with the annual melt period strongly suggest summer melt as an important driver of the observed changes. Linking ice-surface dynamics to processes at the glacier base remains to some degree speculative by nature. We clearly distinguish between observations (results) and interpretation of the observations (discussion). We think that the discussion of a specific phenomenon in a wider context is important for the scientific progress. This includes in particular the implications of a hydro-thermodynamic feedback to summer melt for the ice sheets in a changing climate, i.e. increasing significance of surface melt, especially also in the background of very recent publications on Antarctica (Mengel and Levermann, 2014) and Greenland (Khan et al., 2014; Lüthi et al., 2014).

6. The proposed hydro-thermodynamic feedback is not well described, and it is not clear how it compares to (thermally-controlled) surge mechanism described earlier, and whether it is a positive or negative feedback.

Glacier surges are generally regarded as internal instabilities, and not driven by external factors (Meier and Post, 1969). The climate is, however, thought to set the boundary conditions, as it influences the glaciers thermal regime, as well as the time period required for building up the reservoir area/depleting the receiving area (Dowdeswell et al., 1995). Our unique continuous in-situ observations of ice surface velocity challenge this common understanding, because our data reveal the importance of summer melt for a stepwise surge initiation. Previous observations of glacier surges were reconstructed from archived satellite radar data, only after a surge was noticed. Snapshot velocities were derived preferably for the winter season, i.e. when surface melt is absent, as liquid water deteriorates the quality of the derived motion maps. Changes during the summer melt season have therefore not been captured and the acceleration was thought to be gradual and uniform (Murray et al., 2003a; Murray et al., 2003b).

On Svalbard, glacier surges are thought to be controlled by a thermally-controlled soft bed surge mechanism (Hamilton and Dowdeswell, 1996; Jiskoot et al., 2000; Murray et al., 2000; Murray et al., 2003b). This theory concerns the slow adjustment of the basal thermal regime and driving stresses in response to geometric changes during the quiescent phase, ultimately leading to a thermal switch from cold to temperate basal conditions (Clarke, 1976; Robin, 1955). Basal melting sets in, subglacial till, if present, is thawed and its shear strength reduced by rising pore water pressure, destabilizing the glacier and initiating the surge (Clarke et al., 1984). Dynamic thinning and reduction in surface slope reduce the driving stress and eventually bring the surge to a halt (Clarke et al., 1984).

The novel aspect of the hydro-thermodynamic feedback proposed here, is that the processes at the glacier base that lead to the surge are driven, at least strongly amplified, by surface melt that reaches the bed within an initiation zone (described in the paper as phase 1). Those processes include cryo-hydrologic warming, which facilitates the thermal switch from cold to temperate basal conditions; basal lubrication of areas where the bed previously was frozen to the bed, and the emerging drainage system is thus not well developed; thawing of underlying frozen till, if present, and its enhanced deformation due to rising pore-water pressure. These processes all result in an enhancement of ice flow and longitudinal extension opens new surface crevasses in regions of the glacier that previously were characterized by supra-glacial drainage. The newly formed crevasse fields provide pathways for meltwater in subsequent summers, subjecting a larger region of the bed to surface melt. This closes the feedback loop.

As with any feedback, whether it is positive or negative depends on both timescales and timing. If a glacier is properly pre-conditioned, i.e. it has built up during a long quiescent phase, the feedback is positive as long as it mobilizes previously stagnant ice regions and as long as the driving stress remains sufficiently high. Once the entire basin is moving by fast basal motion, i.e. the entire base is at pressure melting, the reservoir area has been depleted, the glacier dynamically thinned and its surface flatted out, then, the hydro-thermodynamic feedback is no longer operating.

In the revised manuscript, we tried to clarify the above views by extending the information about glacier surge mechanisms in the introduction and by discussing our observations in the context of these previous studies.

7. The hydro-thermodynamic feedback should be demonstrated on other basins as well, e.g. on Duvebreen which was studied by Dunse et al. (2012). This would help to extract the non-surge contribution of the process.

Glacier surges require a certain pre-conditioning (see above). Preconditioning is different for the individual basins and surge-type behaviour therefore not synchronous. Furthermore, basins with fast flowing outlets are already characterized by temperate beds. In other words, there exists no cold-based marginal ice plug that restricts ice flow (quiescent phase) and that could potentially be weakened by cryo-hydrologic warming. This is the case for Duvebreen. We admit that we do not understand the fixation of the reviewer on “non-surge” contribution. This is a surge, but the details of our interpretation have never been recorded before. We see no reason to believe that the observed stepwise acceleration and the proposed mechanism should not be part of the surge, i.e. an independent overlay over another ice-accelerating process.

Anonymous Referee #1

MAJOR CONCERNS

The contributions from surge dynamics and the proposed hydro-thermodynamic feedback to the observed dynamic behaviour of Basin-3 should be clearly separated. In order to address the surge component, the existing literature on glacier surges in Svalbard should be reviewed. The dynamics of Basin-3 first need to be put in a proper context, before considering the relevance of extending the suggested mechanism to non-surging ice sheets with other properties. While surges arise from an internal imbalance, also involving an excess of mass in the reservoir area, external factors may have a different outcome once a surge is in progress, than on glaciers purely subjected to increased melt water input. The study only focuses on surface melt-driven processes as an explanation to the behavior. It would nevertheless be appropriate to refer some basic literature on the (surface) meltwater influence on dynamics.

In the discussion section we describe three phases of changing glacier dynamcis; P2694 L25 ff: "(1) activation of a spatially confined ice stream in the early 1990's (Dowdeswell et al., 1999); (2) multi-annual acceleration from < 2008 to 2012, along with an expansion of the ice stream; (3) active surge phase following the destabilization of the entire terminus in autumn 2012." Phase 1 acknowledges that the basin was pre-conditioned prior to our observations (i.e. 2008 onwards), and that these dynamic changes could be explained by surge mechanisms proposed in earlier studies, i.e. changes in glacier thermal regime and driving stress, associated with longterm geometric changes during the quiescent phase. This also addresses the "excess of mass in the reservoir area" as pointed out by the reviewer. The current understanding of Svalbard glacier surges is summarized in the term "thermally-controlled softbed surge mechansim", which was mentioned in the introduction. In the discussion we point to previous model experiments of Austfonna that support the concept of a thermally-controlled softbed surge mechansim (Dunse et al., 2011). The proposed hydro-thermodynamic feedback becomes active during phases 2 and 3.

In the revised manuscript we have firstly extended the introduction of glacier-surges and observations of glacier surges on Svalbard, and secondly, we discuss more thoroughly how our observations compare to previous theory of thermally-controlled surges. Further, we have included a schematic illustration of the feedback mechanism and its role within the surge-cycle of Basin-3. We like to stress that the focus of the present paper is the role of surface-generated meltwater and the associated hydro-thermodynamic feedback in promoting the surge, and not on providing a review of Svalbard glacier surges in general. This focus is motivated by the unique GPS timeseries we present. It clearly shows the effect of the surface melt during summer, resulting in a stepwise acceleration. Based on our observations, it is not possible to separate the proposed feedback from the "general surge mechanics". It appears that the hydro-thermal feedback itself is an integral part of the thermally-controlled surge dynamics in the observed case. A numerical model of glacier surges, incorporating both internal mechanisms and surface melt, would allow quantification of the contribution of surface melt to glacier surges. To our best knowledge, such a model does not exist. Its development would be an interesting and challenging follow-up study, however, far beyond the scope of this paper.

The term “thermally-controlled surge mechanism” points at a switch from cold to temperate basal thermal regime that along with sufficient driving stress (sufficient ice thickness and surface slope) activates fast basal motion in regions previously characterized by (slow) ice deformation only. A number of processes determine the temperature distribution within the glacier and at its base, such as heat conduction through the ice, ice advection, strain deformation/heating and frictional heating. The theory on thermally regulated surges focuses on the above mentioned physical processes with the geothermal heat flux and the annual air temperature (plus firn warming) as lower and upper boundary conditions, respectively. Thicker ice leads to a warmer bed and increases the likelihood for a temperate base. Thermal effects by water flow within the glacier have previously been mentioned and considered important (e.g. Clarke, 1976). However, Clarke (1976) neglected cryo-hydrologic warming in thermal models of glacier surges, justified by the assumption that water flow within cold ice is limited. Only recently, has “cryo-hydrologic warming (CHW)” put back in focus (Phillips et al., 2013; Phillips et al., 2010). Phillips et al. (2010) simulates the effect of CHW on ice rheology and hence, ice deformation. Phillips et al. (2013) linked observation of increased velocities in the wet snow zone of Greenland to CHW, also discussing the potential of CHW for changing the basal temperature regime on short timescales that may lead to temperate basal conditions, permitting fast basal motion. We cannot explain our observations using standard theory of glacier surges alone, but incorporating the effect of CHW over an expanding area of the glacier bed provides a good explanation, we believe. The hydro-thermodynamic feedback we propose for the mobilization of the reservoir area of Basin-3, corresponds well to the explanation of enhanced velocities in the wet snow zone of western Greenland, following an increase in ELA (Phillips et al., 2013). Here, we expand the application of CHW to the cold-based marginal ice plug of Basin-3 and suggest that weakening/elimination of the ice plug by CHW initiated the basin-wide surge in autumn 2012. Mobilization of the reservoir area in the prelude of the basin-wide surge is evident from crevasse formation and supported by multi-annual acceleration, indicating increased ice discharge from the reservoir area. The hydro-thermodynamic feedback described here is however not limited to CHW/the thermal component, but also includes the additional hydraulic lubrication effects. Ice regions undergoing a switch from cold to temperate basal conditions were previously characterized by frozen conditions, i.e. absence of water. In the initial stage of basal-hydraulic drainage system development, water input is likely to raise water pressure and enhance basal motion, in line with the established theory on basal lubrication, e.g described by (Schoof, 2010). The revised paper discusses more thoroughly the effects of surface melt on rising pore-water pressure within subglacial sediments. Unconsolidated sediments do likely underlay the marine-grounded ice regions of Austfonna.

In case of Austfonna, or Svalbard in general, surface melt has been a widespread phenomenon throughout most of the Holocene. Therefore, a summer-melt driven hydro-thermal feedback could also have played a role in previous Svalbard glacier surges. The current surge should not be mistaken as a response to recent Arctic warming. The role of surface melt in glacier surging has not been described earlier, to our best knowledge, perhaps simply because of the lack of continuous in-situ data prior and during the surge, such as our GPS timeseries from Basin-3. In the context of continued global warming, surfaces melt and hence, the hydro-thermodynamic feedback may gain importance in destabilizing ice regions outside of Svalbard, with similar pre-conditioning as Basin-3 – including the ice sheets. One such example described in the discussion is the Wilkes basin of East Antarctica. The basin is characterized by sufficient driving stresses and a cold-based marginal ice plug (P 2696 26ff) and has undergone a partial collapse during the Pliocence (Cook et al., 2013). A very recent model study confirms the potential dynamic instability of the Wilkes basin, following the removal of a cold-ice plug (Mengel and Levermann, 2014). In their study, a retreat of the grounding line is forced by oceanic warming, thereby eliminating the cold-ice plug.

Some of the GPS data have been published previously, but it is not clear how the application in this manuscript substantially expands the previous results.

The GPS data published in (Dunse et al., 2012) only covered the time period May 2008 to May 2010 (Fig. 1). The timeseries was discussed mainly in the context of seasonal velocity variations. The multi-annual, stepwise acceleration and the strong acceleration since autumn 2012 (Fig. 2) marking the surge of the entire Basin-3 was not yet conceivable.

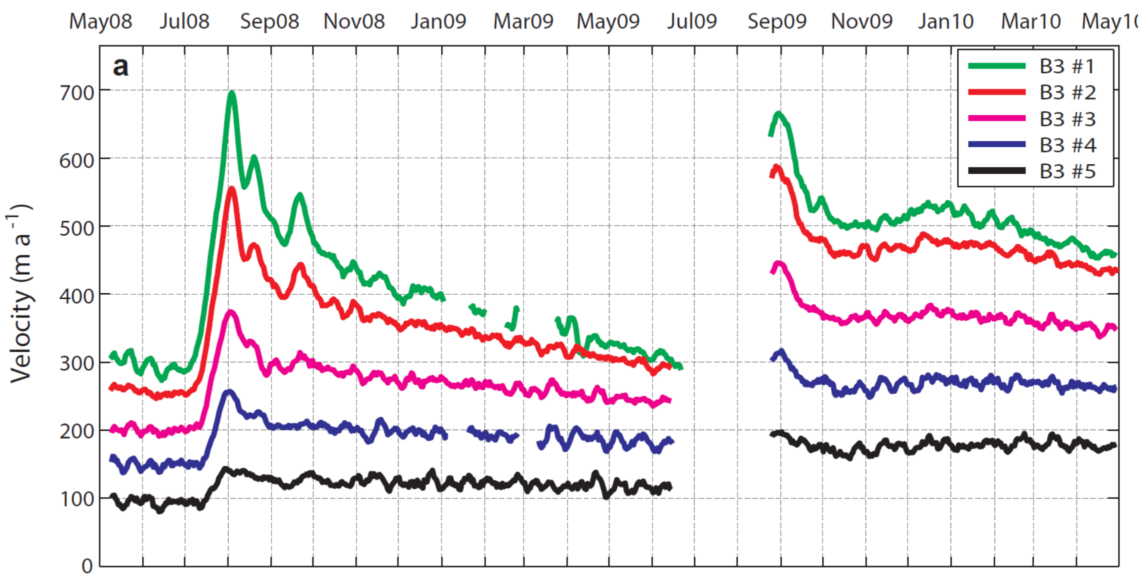

Figure 1. GPS timeseries as published in Dunse et al., 2012

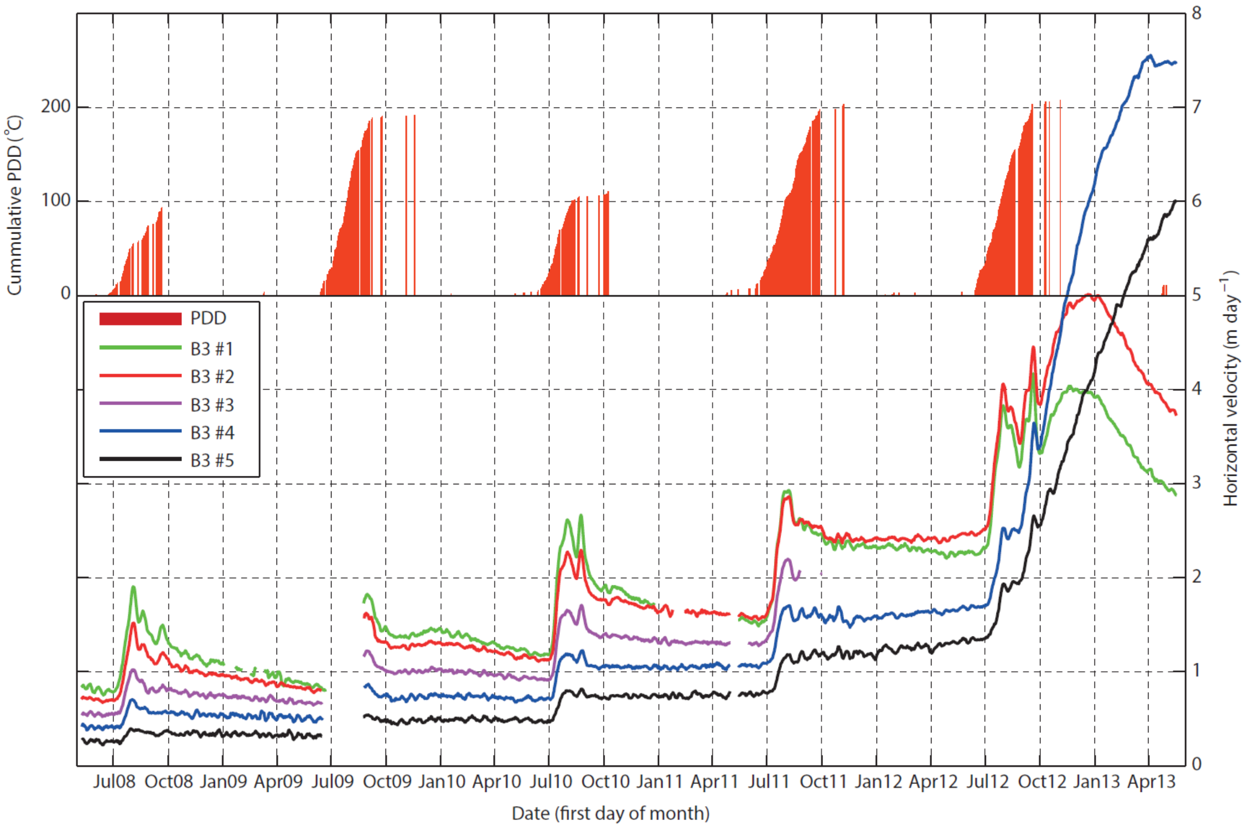

Figure 2. GPS timeseries presented in the present paper

I do not find the hypothesis sufficiently supported and the discussion does not justify the conclusions drawn.

Above we have discussed the integral role of the hydro-thermodynamic feedback in the surge mechanics of Basin-3. The feedback is a contributing factor to thermally controlled glacier surges and does thus not oppose the current/previous understanding of Svalbard glacier surges. In the revised paper we now provide a more detailed background on glacier surges in Svalbard against which we discuss our observations. Our observations are limited to surface processes. Consequently, linking ice dynamics to processes at the glacier base remains to some degree speculative as for most studies on ice dynamics. We have gone through the individual statements, taking care of that interpretations of observations are identified as such. This includes the discussion of possible implications for the ice sheets in a changing climate. We think that the discussion of a specific phenomenon in a wider potential context is important for the scientific progress.

I found the manuscript a bit fragmented and suggest it to be re-organized and aim and objectives of the study to be better defined in the introduction. The manuscript appears a bit fragmented and could do with a better link between the line of thoughts in the introduction and the discussion. Parts of the introduction are not picked up in the discussion. Parts of the results section belongs in the discussion and vice versa.

The background chapter of the revised paper now focuses on glacier surges, as suggested by the reviewers. The aspect of past ice sheet instabilities and abrupt sea-level rise, evident from the geological record, is moved over to the discussion section in which the potential implications of the hydro-thermodynamic feedback for the dynamic stability of the ice sheets are discussed.

The data used in this study have a high temporal resolution for the last six years, however they cover only about half way of the full length of the glacier basin. This should be taken into account. The authors also show that crevasses were formed in the upper part some years before the detailed study of the changes downglacier. Adding the fact that Svalbard surges are known to be long lasting (cf. Dowdeswell et al., 1991 and Sund et al.,2009), an investigating the dynamics at the higher elevations would be appropriate in order to distinguish between the dynamics possibly resulting from processes other than increases in surface melt water.

Are there any possibilities of supplying with data between 1990 and 2008?

Our study employs the GPS timeseries starting in May 2008, and the TerraSAR-X scenes acquired since April 2012. In order to assess the changes prior to 2008 (phase 1) and changes in the upper part of Basin-3, we investigated and presented other available in-situ data, such as annual repeated ground-penetrating radar surveys since 2004 (revealing temporal and spatial crevasse formation; Appendix C) and annual position/displacement of a mass balance stake (P2695 L23-25). Furthermore, we consider previous observations by Dowdeswell et al. (1999) that indicate quiescence of entire Basin-3 until development of a spatially confined fast-flow region in the early 1990s. We distinguish three distinct phases (see Discussion from Page 2694, L24). Phase 1 describes the development of a spatially confined fast-flow region, explained by long-term (quiescent phase) changes of glacier geometry, in line with the current understanding of Svalbard glacier surges. The temporal length of our GPS timeseries misses phase 1. Nevertheless it captures significant changes in the prelude of the surge (phase 2), starting with a strong summer-speed up 2008 and a slow, but gradual approach of the pre-summer values during winter 2008/2009. 60-95% of the velocity increase during summer is reversible, as displayed in the supplementary figure 8, while during the following years an increasing fraction of the summer speedup is of lasting nature, i.e. increased summer velocities are sustained throughout the winter. The fast-flowing region reflects the mobilization of the reservoir area, however, the lateral dimensions of the outlet restricts the outflow from the reservoir area. Increased mobilization of the reservoir area is indicated by the intense crevasse formation observed by repeated radar surveys since 2004 (P2695 L18ff). Multi-annual acceleration of the ice stream further down, as observed with GPS since 2008, provides evidence of increased ice discharge, supporting the hypothesis of an increasingly mobilized reservoir area. Since the unequivocal summer speedup has a lasting effect on background velocities, we conclude that the hydro-thermodynamic feedback had an important contribution to the observed changes, both with respect to the mobilization of the reservoir area and the lateral expansion and speedup of the outlet observed in the lower half of Basin-3. Finally, the GPS timeseries capture the autumn/winter acceleration associated with the start of the basin-wide surge in autumn 2012 (phase 3). Furthermore, we investigated the spatial velocity pattern during the transition from phase 2 to phase 3 by TerraSAR-X.

How does the successive destabilization differ from or resemble the surge development described in previous studies (cf. Murray et al., 2000; 2003 and more recent Sun et al. 2014)? The authors show that the lower parts of a basin of Austfonna experiences multiannual velocity accelerations, but this finding is not particularly surprising given that the basin is also found to be surging. Multiannual velocity accelerations are consistent with previous studies of other surging glaciers in Svalbard (cf. Murray et al., 2003, Sund et al., 2014, and in other areas Burgess et al. 2012). Taking these into account might help to better distinguish the surge contribution of the dynamics from the suggested hydro-thermal feedback.

The hydro-thermodynamic feedback proposed here represents an integral part of thermally and hydrological controlled glacier surges, as explained above. It does not oppose the theory on thermally-controlled (soft-bed) glacier surges, but adds the aspect of a growing area of the glacier bed subjected to surface meltwater and the associated amplification of surge mechanisms, i.e. warming/thermal switch and water pressure/lubrication as well as pore water pressure and sediment deformation, if subglacial sediments are present. Observations of multi-annual acceleration during the active surge phase have indeed been reported earlier, as pointed out by the reviewer. However, studies such as Murray et al. (2003a) and Murray et al. (2003b) rely on snapshots velocities derived from satellite radar and were generally limited to the winter season. Changes in ice dynamics during the summer melt period remained therefor unnoticed. The supplements to our paper contain an analysis of background velocities. The multiannual development of background velocities of Basin-3 alone are in accord with the schematic surge phases as described by Murray et al. (2003b): uniform acceleration over several years in the early surge phase, followed by a strong acceleration over several months. The novel aspect of our study is that we have continuous in-situ observation of ice-surface velocities throughout the development of a surge that helps to pinpoint relevant processes. The multiannual acceleration of Basin-3 did not occur uniformly or gradually, but in discrete steps, each of which coincident with consecutive summer melt periods. Only this temporal continuity of our in-situ data allows us to identify the marked and lasting impact of the summer melt season on the flow of Basin-3.

Following the reviewers advice, the introduction of the revised paper now includes more information on thermally controlled soft-bed surge mechanism and provides a summary of some of the key studies for Svalbard. In the discussion we then compared our observations with those previous studies and point out where our observations can be explained by previous theory and where not.

The work of Solheim (1991) shows a good match between the estimated surge cycle period for Basin-3 and the current surge. This could have been mentioned.

This is added…

Phillips et al. (2013) suggested CHW might facilitate temperature change in the Greenland Ice Sheet due to upward migration of the snow zones, is this case at Basin-3 as well? P2690 L13 states: Over 2002–2008, the climatic mass balance of Austfonna was close to zero (Moholdt et al., 2010a). In addition the fact that Solheim's surge cycle period estimate among other based on total net accumulation matched well, does not seem to point towards a substantial upglacier shift of the ELA.

No, we have no indication of a systematic shift of the ELA. In case of Basin-3, CHW gains significance not because of an expansion of the surface area exposed to melt processes, but because of the first occurrence of surface crevasses, providing pathways for the surface meltwater to enter the glacier and develop moulins that eventually may connect to the glacier bed. First occurrence of crevasses and development of spatially confined fast flow started in the early 1990s as described by Dowdeswell et al. (1999) - we considered this as phase 1 in our description of surge initiation and attribute the dynamic changes to longterm changes in geometry and associated changes in thermal regime and driving stress. Phase 1 entailed an ice flux out of the accumulation area in excess of the balance flux (Dowdeswell et al., 1999) and hence extension and draw down of the reservoir area. Since 2004, we have mapped the first and cumulative occurrence of crevasses along two transects across the reservoir area, areas outside the boundaries of the crevassed and fast-flowing ice stream observed by (Dowdeswell et al., 1999). The estimated surge period of 140-150 years by Solheim (1991) fits surprisingly well with the timing of the renewed surge activity of Basin-3. The figure represents a rough estimate of the time required for the reservoir area to accumulate the mass that corresponds to the surge-lobe volume. The good estimate by Solheim (1991) does in any case not disqualify the proposed hydro-thermal feedback as relevant for the surge initiation, as surface melt on Svalbard is not a phenomenon of recent Arctic warming, but was likely a widespread feature throughout the entire Holocene, and could have played a role for Svalbard glacier surges as much in the past as in the present.

Furthermore, if there is a rise of ELA at basin-3 causing CHW, this would possibly also occur in the other basins as well? It would be nice if the study reflected on why some areas a more affected than others by the proposed feedback. A previous paper covering an additional basin of Austfonna was partly using the same seasonal speed-up data. Why not demonstrate the suggested mechanism on both basins here as well? This possibly makes it easier to extract the non-surge affected contribution to the process.

Glacier surges require a certain pre-conditioning. Basins with fast flowing outlets are already characterized by temperate beds. In other words, there exists no cold-based marginal ice plug that could potentially be weakened by CHW. This is also the case for Duvebreen, the basin referred to by the reviewer. Furthermore, englacial connection must exist to provide pathways for surface meltwater to reach the glacier bed. We admit we do not understand the fixation of the reviewer on “non-surge” contribution. This is a surge, but the details of our interpretation have never been recorded before! We see no reason to believe that the observed stepwise acceleration/ the proposed mechanism should not be part of the surge.

I found the title a bit misleading since it appears there were changes to ice stream behavior prior to the suggested hydro-thermodynamic feedback; hence the basin was already “triggered”. Also the term ”destabilization” is a bit vague and strange as the authors state there is a surge, which is defined to be short term and with cyclic reoccurrence and accordingly the fast flow is expected “to slow down or come to a halt within a few years”. Finally, what is treated here is only a part of the ice cap.

What changes in ice dynamics represent the trigger point of the surge in time is debatable. Is it the development of a fast flow unit in the early 1990s (phase 1), the annual acceleration by mobilization of the reservoir area (phase 2) or the failure of the marginal ice plug in October 2012 (phase3). We argue for that the hydro-thermodynamic feedback process had a major contribution to both phase 2 and 3, while it is not strictly necessary nor can we confirm (or have stated) its contribution for the initiation of phase 1.

Anyway, we have changed the title from

“Destabilisation of an Arctic ice cap triggered by a hydro-thermodynamic feedback to summer-melt”

to

“Glacier surge mechanisms promoted by a hydro-thermodynamic feedback to summer melt”.

Figures

“a” and “b” and so on, could be indicated on each figure, not just in the caption. Fig.1b. The outline of Basin-3 could be made slightly more visible. The fonts of the current figures could be possibly be slightly enlarged for better readability, but this depends on the final size of the figures.

ok

Specific comments

Specific comments are implemented in the revised manuscript, if not stated otherwise.

P2686 L 5. “Basin-3” or “parts of the“ could be inserted before “Austfonna ice cap” as the data does not cover the entire ice cap.

P2686 L9. I'm a bit confused over this sentence, "By autumn 2012, successive destabilization of the marine terminus escalated in a surge", and I'm not sure if "escalated" is the right word here, considering the long surge development in Svalbard.

We think "escalate" is a good word to describe the continued acceleration after the end of the melt period. The two upper GPS show a 3-fold increase in velocity over the winter months. Nevertheless, we are grateful for suggestions of suitable wording.

P2688 L25. "We propose that cryo-hydrological warming may have a drastic effect on glacier dynamics..." Please consider using another wording than "drastic". This is used several times.

We consider the term "drastic" as appropriate in describing the observed changes. But we are open for suggestions of alternative terms.

P2689 L3. For surge duration it would be more adequate to reference estimates for Svalbard which are years to more than a decade, rather than months, since the surges in Svalbard (cf. Dowdeswell et al., 1991).

Here, we describe surge-type behaviour in general and provide relevant timescales of surge durations both for temperate glaciers (months) and polythermal/Svalbard glaciers (years).

P2689 L7. Strictly speaking I think Hamilton and Dowdeswell, 1991 suggested a deforming bed surge mechanism for Svalbard, while Murray et al., 2000 added the thermal aspect for Svalbard.

More references are included

P2689 L12. MacAyeal, 1993 is not the proper reference here.

(Meier and Post, 1969)

P2689 L13 "provoke" change to "promote".

P2689 L14-17. Please add reference.

(Dowdeswell et al., 1995)

P2690 L2. ice thickness of up to 600m – referred to Lefauconnier and Hagen, 1991, is this right reference? By the way, they suggested that the previous surge might have been larger than the Brasvellbreen surge in 1937-38.

The right reference concerning maximum ice thickness is Dowdeswell (1986)

P2690 L16. It would be useful to get an indication of the approximate length of the

basin, especially as the locations of GPS'es are mentioned with distance from calving front (2692 L 16).

About 60 km

P2692 L20. "High sensitivity and short response time (days) of glacier dynamics to melt periods clearly suggest surface-melt triggered acceleration." Belong in the discussion.

P2694 5 Discussion This section needs a more thorough discussion and comparison with previous findings on Svalbard and elsewhere (cf. Solheim, 1991; Murray 2000; 2003; Sund 2009; 2014; Burgess 2012; Tangborn, 2013). I suggest first discussing the elements caused by surge dynamics. Then explain how additional factors and mechanisms such as CHW can be found, extracted and separated from the surge dynamics, and finally how these constitutes a possible hydro-thermodynamic feedback.

P2694 L6. How long time is considered to be within "prior to"? It is referred to Fig. 4b. This only shows data from April 2012.

C981

P2694 L18. The last part of this paragraph belongs in the discussion section. While showing the large increases due to surge during a short period, it would also be appropriate to mention the possible influence during the long quiescent phase.

P2694 L25. Dowdeswell et al., 1999 attribute the increase in flow to be a surge or mini-surge and the following three phases outlined appears to have similarities with those outlined by Sund et al., 2009. This should be considered in the discussion.

P2695 L7. Parts here resemble other studies on Svalbard surges.

P2695 L8. Please be more specific about what you mean by "the current understanding", to make it easier for the reader to follow.

P2695 L10. Fig 8 does not exist.

P2695 L1.9 "Ground-penetrating radar (GPR) surveys reveal first occurrence of surface crevasses from 2004 onwards (Appendix C; Fig. 7)." This belongs in the results section.

2695 L24. This stake is not mentioned before, should be in the result section.

2696 L4. Maybe add a reference for “sticky spots”?

(Alley, 1993)

2696 L4. Please cf. Murray et al., 2000 to cover further aspects.

2696 L19. Other references on surge termination could be preferentially be added.

(Clarke et al., 1984)

2696 L24. Add “in Svalbard” after “drainage basins”. There are surge-type glaciers in other areas that are temperate.

2696 L21 and onward. The text jumps forth and back between surge-type glaciers and ice sheets with no observed surge history. If the authors believe the situations can be compared, they need to explain why the surge context can be ignored.

P2697 L20. Please consider another phrasing than “enormous”, or simply skip. This sentence doesn’t really bring any new information.

References

C982

G.S. Hamilton (Referee #2)

Received and published: 28 August 2014

This manuscript describes a recent surge-like event of the southern margin of the Austfonna ice cap, Svalbard, which the authors apparently link to a feedback mechanism involving increased surface meltwater production leading to enhanced basal motion over an ever-expanding area. The GPS observations of ice motion have been described in an earlier paper (Dunse et al., 2012) (updated here) but the velocity maps from satellite radar images are new, as is the description of the 2012-2013 surge event affecting Basin-3. There is certainly enough new information to warrant publication in The Cryosphere but, as currently written, I find much of the manuscript to be a bit too speculative and I’m not sure the conclusions are really supported by the observations. I encourage the authors to rethink a lot of the interpretation and discussion, and come back with a suitably revised manuscript.

One of my main concerns is the proposed feedback mechanism. The authors don’t really describe the mechanism in detail so it difficult to tell how it differs from (or is similar to) the various thermal mechanisms proposed for glacier surges, or to the cryo-hydrologic and other melt-induced mechanisms proposed for Greenland.

Dunse et al. (2012) presented the first two out of five years of the GPS timeseries presented here. Multi-annual acceleration culminating in a surge of Basin-3 was not yet conceivable (see response to reviewer 1). Previous studies of surge initiation mainly relied on achieved satellite radar data from which snapshots of ice-flow velocity were derived. The required phase coherence for satellite radar interferometry is generally only achieved in the absence of summer melt, and ice-flow maps were mainly derived for the winter months (e.g. Murray et al., 2003a; Murray et al., 2003b). Winter snapshots do not reveal changes associated or coincident with the summer melt-period. For the first time, to our best knowledge, we have obtained continuous in-situ velocity observations during glacier surge initiation. This enabled us to identify the lasting effect of consecutive summer-melt seasons on the multi-acceleration of Basin-3. Changes in basal thermal regime and basal hydraulic drainage system are widely recognized as controls of glacier surges. The novelty of our study lies in identification of the role of surface-melt driven processes in promoting glacier surges, which are generally regarded as arising from internal instabilities, with only their surge period being determined by climate (mass balance and build-up of a critical geometry). The hydro-thermodynamic feedback to surface melt, proposed here, affects (surge) dynamics as a consequence of changes in basal thermal regime and hydrology. CHW has been discussed in the context of englacial ice temperatures and ice rheology (Phillips et al., 2010), as well as its importance for basal sliding, if CHW would spread inland, following the upward migration of the ELA in the wet snow zone of western Greenland (Phillips et al., 2013). Here, we point at its role in eliminating a cold-based marginal ice plug that prevents inland ice to quickly drain into the ocean.

Two missing pieces, in particular, stand out. One is the exact nature of the feedback, i.e., is it positive or negative. It seems to me that the mechanism as outlined would tend to be self-limiting (negative). Increased ice flux from the interior would gradually thin the ice column and promote refreezing of the bed (I think this type of feedback is similar to thermal mechanisms of surging).

As with any feedback, it is a matter of both timing of the underlying processes and the timescales these processes operate on. With regards to the surge initiation of Basin-3, the feedback is clearly positive, i.e. surface meltwater is routed to the bed and enhances ice flow; extensional flow opens surface crevasses in regions previously characterized by surface run-off, subjecting an increasing area of the bed to surface melt; further flow enhancement and spread of the active zone promotes the surge. We have stated that we expect the surge to be of temporary nature, i.e. that it will likely come to a halt within a few years, after dynamic thinning has occurred, the driving stress is greatly reduced and temperate conditions at the base can no longer be maintained.

Alternatively, and this leads to the second missing piece, the subglacial drainage system gradually evolves to a more efficient state under continuing high meltwater inputs and surface velocities actually drop. This type of negative feedback has been inferred quite convincingly for the western margin of the Greenland Ice Sheet, yet the authors do not really explore the reasons why their observations are seemingly inconsistent with the Greenland story.

Our conclusions do not contradict the current understanding of the positive and negative effects of meltwater input into the basal hydraulic drainage system (Schoof, 2010). In line with Phillips et al. (2013) we point at the potential of CHW and the thermo-hydrological feedback in mobilizing more inland ice regions and thereby increase ice discharge through existing outlet glaciers. The most obvious difference between Basin-3 and the outlet glaciers of western Greenland is the presence of a cold-based ice plug that, until autumn 2012, restricted drainage from the reservoir area. In case of fast-flowing Greenland outlet glaciers, the thermo-dynamic feedback would lead to a gradual acceleration of lasting nature as long as the ELA continues migrating upglacier, rather than leading to surge-type behaviour.

Some reorganization of the paper would definitely help. For example, much of the introduction strays into material that is not directly relevant to the present study (e.g., oceanic triggers for outlet glacier changes, ice shelf (in)stability in West Antarctica, or post-LGM Heinrich events). A better way to introduce the paper would be to summarize current state-of-the-art in thermal mechanisms for glacier surging and maybe meltwater-induced speed-ups of the West Greenland ice margin. Reviewing this background material would allow the authors to frame their current study in a more meaningful way, and might also help the authors refine the details of their proposed feedback mechanism in a way that builds on existing ideas and is supported by their observations.

OK - the introduction of the revised paper will have more focus on glacier surging and less on ice sheet instabilities. In the discussions, we then compare our results with previous studies of (Svalbard) glacier surges. The paragraph on ice sheet disintegration is moved from the introduction to the discussion chapter, where we discuss the implication of our results for the ice sheets (see also response to reviewer 1).

I have a few additional comments that I hope are useful to the authors.

Specific comments are implemented in the revised manuscript, if not stated otherwise.

P2685 L3: Delete the hyphen in "summer-melt" in the title.

C1684

P2686 L6: no hyphen in "summer-melt"

P2686 L16: change "glacier wastage" to "mass loss"

P2686 L22: change "not only the...but also the ice sheets" to "both glaciers and ice caps (Kaser et al., 2006) and ice sheets"

P2686 L26: delete the sentence "Recently, the West Antarctic..."...not sure of its relevance to a paper about Svalbard.

P2687 L4: too much disparate information in one sentence...separate the ideas about basal lubrication and oceanic forcing. As written, there is an implication that the two mechanisms are linked.

P2688 L17: start a new paragraph at "The effect of surface..."

P2689 L1: rewrite slightly "...variations in glacier flow, and is..."

P2690 L7: change “ground” to “bed”

P2690 L8: change “intersperse” to “embedded within”

P2690 L12: change “to some extent” to “partially”

P2690 L21: write out acronyms on first usage (“SAR”)

P2690 L25: “lineations” plural

P2691 L1: change “years” to “time”

P2691 L6: need a short paragraph here to set the stage for the work that follows. So far you’ve provided a general introduction, but now you need to describe the motivation for the present study.

P2691 L6: delete “GPS”

P2691 L6: the section on GPS analysis needs to be expanded a bit for clarity. I know the most of the details were written up in an earlier paper (Dunse et al., 2012) but readers will benefit from a brief overview here. For example, you say “single-frequency code receivers (L1 band)” which could mean two things – you used the C/A code modulated on the L1 frequency, or you use the L1 phase measurements. Position qualities will be different for either approach. I suspect you used C/A code because you go on to mention “geographical positions were logged”, implying onboard processing without ephemeris information. But please clarify to reduce reader confusion.

We use the term “CODE receiver” to indicate that the receiver uses the code information of the L1 band and not the phase. To be clearer, we now use “single-frequency code receivers (L1 band, C/A code only)”

P2692 L2: where does the ice thickness information come from???

P2692 L6: change “was” to “has”

P2692 L15: delete sentence “In spring 2008...” which is an exact repeat from section 3.1

P2692 L21: change “periods” to “events”

P2692 L25: not clear what you mean by “progressively irreversible”. Is that even possible??

Increasing degree of irreversibility

P2693 L9: change “further” to “farther” because you are talking about distance.

P2693 L10: unclear what you mean by the sentence “Consequently, ...”

P2693 L12: change order of words, "also displayed"

P2693 L15: instead of inferring the lack of basal motion, you could actually estimate how much of the observed motion is due to internal deformation (using glacier geometry)

P2693 L24: re-order the sentence, "Fast flow of Basin-3 (Table 2) continued at least until the end of 2013."

P2694 L1: calving flux estimates require information on ice thickness. Do you know that? Where do the data come from??

The appendix describes in detail how the calving flux was calculated.

P2694 L10: change "as opposed to stable front position" to "compared to the stable

C1686

Key changes to the revised manuscript

As part of the major revision, we have edited the text throughout the entire manuscript. Only the key changes are listed here:

1. Title

We have changed the title from "Destabilisation of an Arctic ice cap triggered by a hydro-thermodynamic feedback to summer-melt" to "Glacier surge mechanisms promoted by a hydro-thermodynamic feedback to summer melt".

2. Introduction

We have extended the background on theory and observation of glacier surges, especially Svalbard glacier surges, adding numerous key references.

3. Discussion

The hydro-thermodynamic feedback is now better described and put in context with previous studies of Svalbard glacier surges to point out similarities and differences with the established theory. To this end we have also added a schematic illustration of the feedback (new Fig. 5).

4. Structure

We have moved content from the results to the discussion, and vice versa, in order to better distinguish between observed facts and our interpretation of the observations. Furthermore, comments on ice-sheet instabilities are moved from the introduction to the discussion.

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