Anonymous reviewer 1

Black original review, *red/italic our response*

This paper presents some really nice, comprehensive data sets showing seasonal speedup over an extended period. Although there have been several recent papers showing such speedup, the results presented here are accompanied by borehole pressure observations. Although boreholes can provide vastly different results, this one seems to have been well connected to the basal hydrological system.

We thank the reviewer for his kind words and agree that the extended record and combination with water pressure data is the strength of the paper.

General Points In several places the word feedback is used incorrectly, or at least ambiguously. Its fine on 4620L21-24, where melt speeds up flow, which moves ice to lower elevations, causing more melt. Elsewhere it simple refers to summer melt speeding flow. In this case text such as "suggesting a positive feedback between melt and velocity" on 4620L26 that simply refer to melt induced seasonal speedup should replace "feedback" with a better work like "relation" or correlation", including the abstract.

We agree with the reviewer and critically evaluated the use of the word feedback and changed it a two places in the text

Most of the analysis is sound, except the part about the acceleration by a factor of 2 in the last sentence of the abstract and the three paragraphs starting at 4629L26. The fact that that the speed increases from 50 to 100 m/yr from S10 to S9 is no evidence of any speedup related to warming because a) this area is above the ELA, so the flow should be extending and some speedup expected b) the ice overall is thinning as elevation drops so the ice should speedup further c) speed can vary substantially over short distances. The S9 and S10 speeds up have not changed substantially relative to each other over the period of observation, indicating no overall transition. If cryo-hydrologic warming had occurred over the period prior to this, there should be a substantial thinning signal between these two sites if the flux at S9 had increased substantially (50% or more) relative to S10. There is no such evidence of such thinning (e.g., Pritchard). Furthermore this is not evidence the bed is wet in one location and frozen in the other. Several studies suggest the bed is melted at both sites, though sliding could be more at S9 since the faster speed would lead to more basal melt production. Thus, while there could be some small amount of cyro-logic warming in this region, the data and analysis presented here don't support any finding of such warming.

We clearly have put the reviewer on the wrong track here. We are not intending to claim that the annual mean velocity already accelerated with a factor two simply by the fact that the velocity is at present twice as high at S9 as the velocity at S10. What we intended to do is to discuss/ the importance of a potential extension of the sliding area for the velocities in the region. We do note that the acceleration at S9 during summer is exceptionally large in 2012. This shows that even with limited melt and a thick ice cover including potentially some firn layers this location is vulnerable for melt-induced accelerations even this far from the margin. This may well be more important than the temporary variation at lower elevations, which eventually do not affect the annual mean velocity. Hence the potential extension of the sliding area to higher elevations is more important. Throughout the text we have tried to clarify this point, which we consider as the most important improvement of the revised version.

Its important to note that prior evidence of cryologic warming in this region (Phillips et al., 2013) was an incorrect interpretation of processing artifacts in the data they used, which is obvious from a quick glance at the figure showing speedup in that paper. The data presented in van de Wal Figure 2 shows little or no trend in winter velocity over the last 7 years, which is completely and correctly at odds with the findings of Phillips et al. Essentially this paper would be greatly improved by simply removing these 3 paragraphs and the reference to them in the abstract.

This point is hard to reply to as we are not aware of published data suggesting that the Philips et al. interpretation is wrong neither are we pointed to this literature by the reviewer. Moreover we do not refer to Philips et al. 2013 and finally we are as said above not arguing that cryologic warming already occurred, we merely want to say that it may be more important to study the area above the ELA since temporarily speed-ups below the ELA do not affect the annual mean velocities. So as said in the previous point we have clarified what we mean to say with the S9 S10 differences as this seems to be misinterpreted by the reviewer.

While the data have good temporal resolution, they provide sparse spatial sampling and even along a flow line the response to melt can vary significantly. To put this in context, it would be worth adding a couple of sentences and references to Palmer et al. 2011 and Joughin et al., 2013, both of which show the high degree of spatial variability. This type of variability explains why, for example, the response of S8 is less than S7 below it and S9 above it.

We fully agree and added this suggestion

Specific points.
Page 4621L25 Change 168 h to "weekly (168 h)"
Page 462L26 Change "168 h spaced" to "weekly"
Page 4621L27 The velocity data in Figure 2 appear not be "hourly" but "weekly"
Page 4622L14 "argue" I am not sure you are arguing so much as observing. Such "observe" rather than "argue"
Figure 5 caption "infers" should be "implies" Figure 9 caption "divided" should be "divide

We fully agree with the specific points and changed them accordingly

P. Nienow and A. Sole

This paper presents an extremely rich data set of ice velocity, meteorological and subglacial water pressure observations from the western margin of the Greenland ice sheet (GrIS). The data extend from the ice sheet margin to above the equilibrium line altitude (ELA) and cover in detail the period 2005 to 2012. The data come from an area of the ice sheet that has been a focus of considerable research concerning interactions between the hydrology and dynamics of a land-terminating sector of the ice sheet and the data presented in this paper make a valuable addition to this literature, in particular because of the length of the velocity-melt time series.

No further comments

Nevertheless, while the observations presented are extremely valuable, it is important that the findings and scope of the paper are placed in the appropriate context relative to previous work. In particular, the citations to some of the existing studies incorrectly limit the spatial and temporal coverage of the earlier observations. For example, the authors state that observations by Tedstone et al. (2013) "only covered the lower ablation zone" (p 4622, l 13) and that Sole et al. (2013) "showed that in the lower ablation zone" (p4628, l6). This is incorrect; both studies presented observations from the ice sheet margin to approximately >115 km inland to an elevation of >1700 m. Thus observations extended across the whole of the ablation zone to above the ELA. (see Sole et al. (2013), Figures 1 and 2).

Yes we agree that this formulation can be misinterpreted we do not aim to claim that there are no measurements by Tedstone and Sole in the higher areas, but as far we understand no anomalous peak was detected by Tedstone at their higher site. We apologize for the confusion and have rephrased this.

Furthermore, the results presented in the current manuscript regarding "self-regulation" of the ice-sheet were made strongly in the papers by both Sole et al. (2013) and Tedstone et al. (2013) which, between them, include four years of ice motion data (2009-2012) across the whole ablation area, incorporate the exceptional

2010 and 2012 melt years and state unequivocally in their abstracts that strong summer melt (and associated ice motion) is followed by a compensating winter slowdown (i.e. self-regulation), e.g. “despite record summer melting, subsequent reduced winter ice motion resulted in 6% less net annual ice motion in 2012 than in 2009” (from Tedstone et al.’s 2013 abstract). Thus the current paper valuably extends and supports findings that have already been stated and this should be made clearer.

We agree with the reviewers and mentioned this already once in the original manuscript. To put more emphasis on this we have now mentioned it twice.

Many statements in the current paper are made about rapid and significant velocity responses in 2012 at S9 (e.g. p 4630, l 15), but the effect of these on annual velocities is not shown clearly. The statement “S9 accelerated to over double its previous velocity maximum” needs additional context regarding the duration of this acceleration to clarify its overall effect on net ice motion.

We have added once more that the peak in S9 is visible in figure 2 and stress that it is a summer peak not a strong signal in the annual velocities

The closing statement from the abstract therefore appears unfounded; “During the extreme melt in 2012 a large velocity response near the equilibrium line was observed, highlighting the possibility of rapidly changing bed conditions in this part of the ice sheet that may lead to a doubling of the annual ice velocity.” This may refer to the afore-mentioned acceleration of S9 to over double its previous maximum velocity or the doubling of mean annual velocity between S10 and S9.

The closing statement of the abstract was ambiguous and we have rephrased this.

There is however no convincing evidence presented in the current paper to support the notion that such a transient velocity increase could lead to a doubling of annual velocity. Indeed, Tedstone et al. (2013) also observed pronounced speed-ups in 2012 at their S6 (at 1482m elevation compared with S9 at _1520m) but no net annual speed-up in 2012 compared with the ‘normal’ melt-year of 2009. The fact that mean annual icevelocity doubles from _50 to _100 m/yr between S10 and S9 before actually dropping again to _75m/yr at S8 also suggests that local driving stresses (e.g. surface gradient) are far more likely to be the key difference between the velocities along this section of the transect rather than “rapidly changing bed conditions”. Overall, a table displaying the mean annual ice velocities and annual mass balance at each site for the 7 years of detailed GPS data would be extremely illuminating and help the reader assess the importance of the velocity variations referred to.

We don’t claim that the annual velocities already increased, we only want to point out that it may well be that the extension of the sliding area is a more important fact here, than the temporal increased speed up in the lower ablation area.

In order to help the reader we will provide not just the yearly data in a table, but the hourly data as supplementary data so that the key part of the data set is available for everybody.

Regarding ‘spring-events’, the paper uses very convincing data to state (p4622, l23) that “The delay of the spring event the higher on the ice sheet is illustrated in Fig. 4 and confirms the idea that the spring event is related to the onset of the melt season”. It would be helpful to add references to the papers (from the wider Alpine and Greenland literature) which these new observations are confirming.
The availability of the borehole water pressure data is a very valuable component of the paper as reported in section 4. It would however greatly strengthen this section if some of the copious literature from the Alps on borehole water pressure (e.g. Iken and Bindschadler, 1986; Hubbard et al, 1995 etc.), drainage system evolution and ice motion was used to aid your interpretation here.

We have added reference to the work by Iken and Bindschadler. The work by B Hubbard does to our *understanding not make a clear link to velocities and therefore does not seem to be an appropriate reference for this manuscript.*

Anonymous reviewer 2

In this paper the authors present an on an impressive dataset of glacier velocity, bore hole pressure and surface mass balance for the K-transect, South-West Greenland. Such datasets are critical to improving knowledge of glacier response to changes in water supply to the bed, an area of intense debate. The authors do a commendable job at synthesizing the data into a coherent story. The main weakness of the paper lies in the overall strength of the conclusions that are drawn from the data and the poor acknowledgement to previous work that has come to similar conclusions for the same region, as is well detailed in Peter Nienow's comments. The authors also over-generalize their conclusions beyond the K-transect which are not justified by the localized data. I agree with the review by "Anonymous Reviewer #1" and the comments of P. Nienow so will refrain from repeating their concerns with the interpretation of the data. I have little to add in addition to their comments.

See comments to the previous reviewers

P4621L23: Not being a field person I'm not familiar with what "commercial L1 measurements" means. . . are all commercial L1 measurements the same?
P4622L5-7: I found this sentence difficult to follow
P4622L15: relative acceleration. . . do you mean seasonal amplitude?
P5623: The discussion of PDD and sonic elevations is irrelevant for the type of analysis that is being done (melt magnitude correlated with velocity). All approaches would likely yield similar results.

Fully accepted

P4623L4-5: retrieving accurate net radiation from an unmanned weather station is non-trivial as has been documented by the co-authors.. 5% uncertainty seems low

Yes this may seem low, but we have Sonic height ranger data for validation of the energy flux calculations and there is barely any summer accumulation so the uncertainty is indeed very low See also the paper by van den Broeke et al. 2004 indicating a far better performance for daily mean from a CNR1 radiometer of net radiation measurements (<2%) than specified by the manufacturer (10%) (comparison was done at a BSRN site).

Van den Broeke, M. R., D. van As, C. H. Reijmer and R. S. W. van de Wal, 2004: Surface radiation balance in Antarctica as measured with Automatic Weather Stations, *Journal of Geophysical Research* 109, D09103, doi:10.1029/2003JD004394.

P4623L18: Why do you need to specify a density for ice?

Because energy fluxes and stake measurements are compared

P4623L22-24: where do these errors come from?

Sigma in daily totals as stated

P4623L25: Not accounting for refreeze has HUGE implications with a migrating ELA . . . this is clear in your Figure 9 which has little meaning if refreeze is not accounted for.

It is an interesting point raised by the reviewer here. First of all it maybe noted that there is not a clear trend in the ELA over time. Hence the question focuses to whether the ELA can be estimated in an area where refreezing takes place. If the ELA varies and does not change one transfer mass from one year to another year by the fact that one neglects the density change which may take place below the level of the measurement being the surface in case of a stake measurement. So the long term average is ok, but not necessarily the individual years. We tried to estimate for this reason by another method the equilibrium line altitude being a linear extrapolation of the surface mass balance measurement in the lower ablation area. This requires the assumption of a linear gradient. Results of this estimate of the ELA are again different on yearly basis. So we decided to remove figure 9 from the text. This can be done without any consequence as the only importance of the figure is to point to the exceptional conditions in summer 2012. The latter can simply be shown by the extreme melt at high elevation.

P4624: the use of the word "instantaneous" throughout the documents is a bit strange.

Changed at one place

P4624L17: I am not familiar with the term "over-pressuring"
P4625L7-10: I found this sentence difficult to follow

Both sentences are rephrased

P4625L24-29: Why would "absolute" water pressure not dive variations in velocity?
This seems important. Maybe I'm missing something.

Because of the transient drainage capacity as explained in the next sentence. Figure 5 also clearly shows that absolute water pressure is not driving velocity changes. Likely velocity variations will be driven by the average pressure change over a larger region, but that is not captured by a single borehole.

P4627L6: What is a "velocity surplus"
P4628L2: "percent" is one word

Both are rephrased

P4629: You can not calculate the ELA without accounting for refreeze.
We decided to remove figure 9