Reviewer 1

The author makes the claim that speedup is concurrent with the period of lake drainage. I have had a really difficult time understanding Figures 3 and 4 because it is unclear to me what it means when a lake is coloured in (see notes re. Fig. 3 below).

> ***We forgot to explain the significance of the solid fill, which we now have added to the caption for Figure 3. Other than that, the caption clearly explains the significance and the figures already included a corresponding legend. The caption now reads. "Prior to drainage, lakes are shown with black outlines. A solid fill indicates the period when a lake drained rapidly (green) or began draining slowly (magenta). For the periods following initial drainge, magenta outlines indicate continuing overflow to a nearby moulin. For fast draining lakes, the green outlines indicate the post-drainage period when water inflowing to the lake basin drains directly to the lake's moulin."***

As such, it's difficult to know what the periodicity of lake drainage is. However, I don't think that there is enough data presented to definitively link lake drainage to the observed speedup for several reasons: 1) there are locations where lakes drain and no velocity response is observed (e.g. the very northern tip of the data set between June 12-June 23, 2009) and other locations where it appears that lakes drain after the speedup occurs.

> ***We focus more on the general pattern and timing rather than specific lake drainages since we do not know the details of each drainage. For example, in the example cited the actual lake volume may be quite small and produced little speedup relative to some of the larger lakes upstream. In fact, that lake does show some very modest speedup associated with the drainage. We are somewhat limited in our temporal resolution. So a lake could have drained on the last day of an 11-day interval, so that most of the speedup would have been captured in the next day interval. While the complexity of the system may produce a few outliers, overall the pattern is robust.***

2) speedup is shown as a snapshot in time on one particular day with no prior knowledge of the melt conditions on the previous days. Since speedup is known to be a function of variability in melt water input (Schoof, 2010), it is equally possible that melt had peaked prior to the observed speedup.

> ***The reviewer here has clearly misinterpreted the results as being 1-day snapshots, when in fact we have an unbroken, back-to-back series of 11 (occasionally 22) day intervals covering the full motion from first to the last panel in each figure. We went back through the text to see if we could modify the text so that other readers would not be confused, but it clearly states "Except for two instances when missed 2010 acquisitions yielded images separated by 22 days, these data yield velocity estimates averaged over 11-day intervals,..."***

To clarify even further, we have added to the caption for Figure 3

"The date for each panel corresponds to the central date for each 11 or 22 day interval over which velocity is determined."

Since there are numerous moulins in your study area that are connected via streams to lakes it is equally likely that meltwater flowing into moulins 'primes' the system thus allowing melt to be concurrent with speedup, without needing the water to be previously stored in lakes.

Clearly melt flowing into moulins will speed flow. But in the lakes-dominated regions, most moulins do not open until there is a sufficient reservoir of lake water to open the moulins. When this happens, there is both the daily melt plus the additional lake volume.

Melt is well correlated with speedup on multiple time scales as seen in many GPS-based observations (e.g. Hoffman and others, 2011; Bartholomew and others, 2010 and 2011b). The authors discuss how their observed spatial variability makes point-based GPS observations difficult to interpret, but the downside of the approach used here is the temporal sampling used. How do the authors reconcile this?

We fully agree that both have their limitations and we are not suggesting that such InSAR observations provide a complete solution. Nonetheless, we believe that it is enlightening to consider the spatially varying pattern. For example, we show that moving a GPS a few km in some cases could yield a substantially different signal for the same regional melt rate. Both types of data are clearly important.

I suggest that the authors show sub-daily or daily melt estimates along side their GPS data and the analysis presented for a more thoughtful interpretation of their data in light of the GPS-based observations that have occurred previously.

These data are currently being integrated into another manuscript that examines the individual lakes adjacent to the GPS in far more detail. We have elected here to focus more on the regional pattern of speedup, which represents a complement to the large number of GPS based studies that have recently been published.

I think the strength of this manuscript really lies in what is discussed on 1109 and 1110 and not the correlation of lakes and velocity change. The conclusions spend little time on lakes and more on the ideas developed in the latter part of the discussion. I think changing the title to reflect that the manuscript is about more than just lake drainage would be appropriate.

> *We have changed the title to "***Influence of ice-sheet geometry and supraglacial lakes on seasonal ice-flow variability***" (i.e., swapped the order of lakes and geometry).*

My other (more minor) concern is that the authors need to do a better job of referencing previous work. There is work on crevassing and moulin development by other authors as well as a large body of literature on overdeepenings and GPS applied to this problem that should be cited properly though the manuscript.

> *While there is substantial basal topography, nowhere do we even mention the word overdeepenings, so there seems no need to reference them. We mention 4 references to hydrofracturing, 1 observational, and 3 theoretical (we also added one additional, bringing the total to 5). We feel this is sufficient. There are at least 9 references in the manuscript to papers using GPS to examine such processes.*

Specific issues:

1103, 17: Zwally and others, 2002 is a bit outdated - especially after many numerous more recent publications on seasonal meltwater-induced acceleration.

> *Zwally is the first published reference for Greenland and we provide 4 more recent references in this paragraph to more recent papers (and several more throughout the text). Thus, we fully justified using this reference.*

1105, 6: meltwater drains into crevasses - aren't these then moulins also? or are you saying that meltwater fills crevasses and does not drain?

> *The point is that the surface of the ice sheet is covered with a network of streams that channels the water into a relatively sparse set of moulins. An exception is crevassed areas where there are no apparent streams (i.e., its hard to run a stream across a crevasse field), so the water presumably drains into crevasses. To speculate whether the water drains to the bed or refreezes in the crevasses would be beyond the scope of this paper. We did amend with the underlined text to clarify "***With the exception of these high-strain-areas where melt water likely drains into crevasses <u>rather than via surface streams</u>, the overall pattern of sparsely distributed moulins fed by extensive stream networks…***"*

Similarly on 1105, 29: what is the difference between large hydro-fractures that drain lakes and moulins?

> *Fractures provide and initial route to the bed, they then close along most of their length, except where viscous dissipation keeps moulins open at sites where there is stong stream flow. This is clearly stated in the text.*
>
> *One of the original references and a newly added reference (Doyle et al) provide observational evidence and we have visually observed this many times. We have*

changed the text to "...__often__ do so through large hydro-fractures..." adding "often" so as to allow the possibility of other styles of drainage.

Finally, what is the evidence that water is continually supplied to the bed for these fast-draining lakes?

These lakes are lie in closed depressions fed by an extensive network of streams. That fact that the lakes do not refill indicates the water is draining to the bed. There is observational evidence in the citations and we have certainly seen this first hand for many lake basins. The stream networks terminating at moulins in the middle of the lake basins provide additional evidence (where else might this water go?).

1106, 16: This is confusing: did N. Lake drain on June 12 or between June 12 and Jun 23? How does the author know that the lake drainage was responsible for the observed speed up? I think the speedup is showing the difference between wintertime speeds and speeds on June 12 - so the speedup could have occurred at any time previously, correct?

We have clarified the date issue with "(e.g., near North Lake __during the 11-day interval centered on__ 12 June __shown in Fig. 3__)"

The second part of this comment seems to be related to the reviewer's confusion with respect to 1-day snap shots, which we have addressed above.

1107, 10: when/how is the peak lake drainage identified?

We have expanded the text to cover how we identify and classify the lake drainages. Since there are different periods when many lakes drain, instead of mentioning a peak we now say "...periods when several lakes begin draining."

1107, 23: "The observed relationship between surface melt production and ice-flow speed: : :" - from this manuscript? Where? From another study? Please cite. There is no data presented here indicating surface melt production.

The reviewer appears to have missed the average melt values that accompany each panel in Figures 3 and 4. We checked to see if we could clarify further, but the original text already says

"These figures also show average melt, m_{avg}, for each time period as determined using the Regional Atmospheric Climate MOdel v.2 (RACMO2; van Angelen et al., 2012) daily meltwater runoff estimates."

And the Figure 3 caption says (Figure 4 has an abbreviated version)

Mean daily surface melt estimates, m_{avg}, determined using the RACMO2 (van Angelen et al., 2012) are shown beside the corresponding date for each map.

Fig. 3: The caption mentions green and magenta outlines but you show those plus lakes that are infilled in green/magenta but with black outlines. This is confusing. Why are some (but not all) lakes coloured in?

> ***Although it was included in the legend, we forgot to clarify the solid infill in the caption and we have fixed that (see response to similar comment above)***

Also, the author points out that the maximum increase in surface speed occurs downstream of two major surface streams - however, other major surface streams see no similar acceleration. Is this worth pointing out?

> ***There already is substantial discussion as to why these streams seem to be offset and why others would not.***

Reviewer 2

This paper investigates the role of supraglacial lakes and ice sheet geometry in influencing the seasonal variability in ice motion, across a large area of the GreenlandIce Sheet (nearly 2000 km2). The majority of the results in the paper are not new in themselves; that surface inputs of meltwater, often sourced through supraglacial lake drainage, cause considerable dynamic variability over the course of a melt-season. This behaviour has been reported in a number of publications over the last decade. The paper is nevertheless important because of the temporal and spatial resolution in the datasets. The paper demonstrates that there is considerable spatial structure and variability in the seasonal ice dynamics at a far better spatial resolution than ground based GPS surveys (e.g. Zwally et al, 2002; de Wal et al, 2008) and at far better temporal resolution than other papers using remotely sensed data products (e.g. Palmer et al, 2011; Sundal et al, 2011 and additional papers by Joughin et al). As such, it reveals the spatial and temporal complexity in seasonal ice motion at considerably improved resolution. The paper also makes use of excellent surface and bed-DEMs and the results suggesting that areas with transverse flowing supraglacial streams are located just upglacier of areas of seasonally enhanced ice flow is particularly interesting.

> ***Thanks.***

The paper is limited in places because an enormous wealth of data has been presented that has not always been analysed in the detail that it merits. Furthermore, the amazing complexity in much of the data is not always apparent because of the scale at which the figures are produced. These and other issues are discussed below in more detail.

> ***We regret the lack of resolution, but we feel it is important to put each summer's data in one figure to best show the seasonal evolution. We will provide a high-resolution version of the figure in the supplement that should allow people to zoom in.***

1) The Figures. Several of the figures are simply too small to do justice to the remarkable richness in the data. In particular, the subplots in Figures 2, 3 and 4 are simply too small. I found that only by looking at these figures at _200% (Fig 2) and _300% (Figs 3 and 4) could I see properly the structure in the data. This devalues the impact of these figures and I would much prefer to see them enlarged. Without doing so, the links between ice-sheet geometry, moulin and lake distribution and the associated dynamic variability are lost completely.

> ***Addressed via comment above.***

2) Results – more details are needed in a number of areas. P1104, L13. Digitization of stream subsets. Given the quality of the Worldview data (_0.6m), were the 'subset' of digitized streams characterized by prescribing a critical width of e.g. >5m when entering each lake? As written, it's not easy to understand what a "sufficient number of stream channels" means.

> ***A resolution of 0.6 meters does not limit us to identifying distinct, linear features with widths well below the resolution (for example, radar sounders image layers well below the resolution of the radar). What can't be done at this resolution is determine the width of such features or to separate multiple features (i.e. to discriminate 0.1 m wide streams, 0.3 m apart). To address this, we have added "With this imagery, it is possible to identify distinct, quasi-linear features such as streams and crevasses with widths well below the image resolution (i.e., a few 10s of cm)."***
>
> ***We also reworded to " Instead of digitizing every stream, we digitized a subset (<~20%) of streams with a density sufficient to qualitatively reveal the spatial extents of the catchments feeding each lake"***

P1104, L17. Digitization of moulins. Can you be more specific than we digitized "most" moulins. Was this e.g. _60%, 90% and was this scale dependent? i.e. did you digitize all > certain diameter, recognizing that you'd be missing smaller ones, or did you not look at all areas in the same detail?

> ***In the upper region, we feel like we found most moulins (over the years we have flown around extensively and what we find with the images is consistent with our field observations). Thus, we feel that "most" is an appropriate description (it could easily be 90%, but it could also be higher or lower). Similarly, "we could not identify a substantial fraction of the moulins at lower elevations" seems appropriate and any attempt to quantify the uncertainty would be more uncertain than what we originally trying to quantify. Our goal is largely to give a qualitative idea as to what the surface hydrology is like and since we do not make quantitative inferences from the these data, we feel this is sufficient.***

P1104, L24. Lake aerial extent. Is there any reason why the individual lake extents may not have also been substantially larger than indicated by the 2001-06 data?

The way that the algorithm takes the max overall years with the crude resolution of MODIS, probably not. We don't recall any instances where the lakes exceed these bounds, and even if this were the case, it would not substantially change our conclusions. The opposite is far more likely to be the case, and in many cases the lakes did not fill to their maximum bounds before draining, which is why we have clearly indicated this in the text.

P1105, L2. It would be good to report the variability in moulin density more quantitatively than "more widely spaced (several km)". Can you not give an estimate of changing moulin density, with elevation, based on the data that you have? This will likely have important implications for subglacial water pressure perturbations and potential channel spacing and would be useful to report, especially for the modelling community.

Although this is a big area with respect to say a few isolated field sites, its still comparatively small area on the scale of an ice sheet. Thus, for any given elevation range we have a relatively small sample. Furthermore, moulins are not uniformly distributed. So we don't feel it would be appropriate to put a number that might be misinterpreted for a modeling study. We do show the locations and provide a scale bar, so one could examine our figure and come up with some realistic values for an idealized model run, perhaps tailored to the type of region one is trying to simulate.

P1105, L21 TerraSAR-X velocity errors. As written at the moment, it is not clear to me what the errors are of the SAR data relative to the GPS data – is the <10% quoted the error between different SAR velocity estimates or between the SAR and GPS measurements?

We changed to "(<10% <u>of nominal winter speed</u>)".

P1105, L24 Lake drainage timing and rates. I don't understand how lake drainage events are defined as "rapid vs. slow" when you say earlier that you determine "timing of lake drainage with 11 day resolution: : :." P1104, L25. How does such 11 day resolution resolve rapid lake drainage that can occur over <1 day (e.g. Das et al, 2008 and Doyle et al, 2013). Furthermore, you go on to say "Lakes that drain rapidly (within hours..)" P1105, L28 and "within days" P1106, L6 – I cannot understand what data is being used here to generate your "fast" and "slow" characterization?

To clarify, we have added the following text (earlier in the results section).

"Once lake drainage commences, those lakes that require at least one additional interval between image acquisitions to drain fully, we classify as slowly

draining lakes. Those lakes that fully drain over a single 11-day interval, we classify as rapidly draining. While past observations suggest most lakes defined here as rapidly draining likely drain within hours, we can we cannot exclude the possibility that some may have drained over several days (< 11 days)."

P1106, L25. The correlation between lake drainage and speed-up. The suggestion that "the general pattern is a regional speedup concurrent with the period when lakes drain" currently has to be taken on face value. So does the statement that "much of the excess seasonal motion in our study area occurs during the period of peak lake drainage". I'm sure the authors are correct here but it is not obvious from the way data is presented as there is no analysis to quantify or confirm these claims (and the figures are too small to help support them either).

We agree. To some extent it will take more data to more firmly establish. With respect to the second part ("peak drainage"), we have softened this language as described above.

P1106, L21. From the data that you present, you cannot say that speeds reach "annual" minima in the periods represented by Figs 3h and 4h as you do not resolve the full 11 day time series over the whole year. Also, in figs 3h and 4h, at least _50% (2009) and 10% (2010) of the area are going faster than your "nominal winter speed" (and the northern end of the region is experiencing fast velocities approaching 100% above 'winter'). Furthermore, your RACMO2 melt estimates suggest that surface melting has not yet ended.

To be more accurate, we have reworded to

In both years speeds reach <u>seasonal</u> minima <u>over much of the area</u> around the time when surface melting <u>is in strong decline</u>.

Wider significance of the summer speed-up results The line suggesting that "the observed relationship between surface melt production and ice-flow speed indicates behavior broadly consistent with the conceptual model described above" is true but somewhat neglects the fact that the data presented is also consistent with field data that has observed exactly the same behaviour and placed it in the same 'conceptual' context. Thus the paper should really report that the data confirm what has been observed by others, not just postulated theoretically, in terms of seasonal evolution in hydrology and dynamics (e.g. Andersen et al, 2010; Bartholomew et al, 2010 and 2011; Hoffman et al, 2011; Sundal et al, 2011).

We agree and have added '..., which also has been demonstrated with several other observations (e.g., Bartholomew et al., 2010;Bartholomew et al., 2011b;Hoffman et al., 2011)."

The suggestion that "weeks of abundant meltwater supply" are needed to generate an efficient drainage system are not borne out by other more detailed data that report

on the effect of lake drainage on ice motion. Subhourly GPS data indicate that ice-flow close to rapidly draining lakes exhibit only a short-lived (_24 hr) spike in velocity before returning to _pre-drainage velocities (e.g. Das et al, 2008, Bartholomew et al, 2011, Doyle et al, 2013). These data suggest that an efficient subglacial channel (which must be necessary to evacuate such large volumes of meltwater) can develop very quickly driven by the large volumes of water input into the system.

> ***One could argue what efficient means, but here and as we describe in text, we take it to mean a low pressure channelized system. The events mentioned above are cases where there are pressures approaching or in excess of flotation (e.g., with accompanying uplift), likely leading to drainage via sheet flow as we have discussed in the text, which differs from the definition of efficient just given. Thus, we don't feel our statement represents anything that contradicts our "weeks … " statement. The existing manuscript has a whole paragraph devoted to this type of transient drainage.***

Minor grammatical suggestions

P1104, line 6. Better to say we "used" rather than "requested" as presumably you both requested and were provided with the data.

> ***No we mean "requested" in the sense that we developed a coverage plan and requested these specific acquisitions (i.e., we didn't just order them from a catalog).***

P1104, l11 – say how large the area is that you map with TerraSAR-X – it's _35 x _60 km but this is important as the large spatial resolution of the data is a key advance on much previous work.

> ***We inserted the following a little bit earlier in the text (at the end of the intro) over an approximately 30-km-by-50-km area.***

P1108, L17. Units re moulin distribution is odd – surely kmˆ2 or at least amplify sentence to clarify meaning.

> ***We changed to "…(e.g., kilometers apart)…" to convey what we mean by sparse and our point was that water has to travel several km to fully lubricate the bed.***

Figure 1. The velocity arrows need a scale.

> ***While arrow length gives speed, we have included them largely to show direction. The length is conveyed with far more resolution for each arrow by the underlying map of speed, making a scale arrow unnecessary.***

Figure 2. In the caption, "speedup exceeded 100%" of what? Presumably the "nominal winter speed" but you need to make this clear.

This text in the Figure 2 caption explicitly references Figures 3&4, where the speedup is clearly defined.

Figure 3. The triangles are almost impossible to see.

We acknowledge that there is a bit of a "where's Waldo" aspect to these symbols. We did experiment with making them more visible, but that made them either distracting or obscured other features more important to the paper. We have included them only for the sake of completeness, so we feel its fine if those really interested have to invest a few seconds to locate them (they don't jump off the page, but they can be found).

Maurie Pelto

The comments below are suggestions for adding value to what will be a fine paper. Joughin et al (2013) provide a spatially important data set on glacier velocity during two years on a Greenland outlet glacier. The data set provides a unique perspective compared to the temporally rich, spatially poor data sets. The data sets value is in its ability to address two key questions from this perspective. 1) Sundal et al. (2011) posed a central question is melt-induced speed-up of Greenland ice sheet offset by efficient subglacial drainage? 2) How does the seasonal progression of velocity vary with co-alignment or non-alignment of bed and surface gradients? The lack of attention to the first question is a significant issue that needs to be addressed. The authors focus more attention on the enhanced flow, versus the late melt season slowdown.

With only 2 years of data we elected not take up the first issue as one cannot derive a firm relation from 2 points. There is no shortage of papers tackling this issue.

The significant late summer flow reduction that has been observed by Sundal et al. (2011), Bartholomew et al (2010), Sole et al (2011), Ahlstrom et al (2013).

Several papers before these do also, including but not limited to, Zwally et al 2002 and Joughin et al, 2008.

This data set provides an opportunity to compare the net velocity effect of each. The analysis here avoids contrasting the magnitude of the enhanced speed up and the following slow do. To what degree do they offset? Sole et al (2011) note that after GIS outlet glacierice motion increased above background for up to 2 months, that ice flow at all sites decreased to below background.

Our data are not sufficient to do this since we don't have continuous data through the full cycle (i.e., a fast summer, may lead to a slow winter in response to an overdeveloped drainage). Again, this issue has and is being tackled by several groups elsewhere and is well beyond the scope of this paper.

The second question is relatively well addressed. This paper suffers from organizational confusion with the results and methods mixed together as well as the results and discussion. The first page (1104) of results is all methods for example, whereas the last paragraph that begins on 1105 is all results.

> *It is custom rather than a requirement, to do ...methods, results... We don't have particularly detailed methods, so we have merged them results to improve the flow of the paper.*

1103-17: Equally as notable as enhancements is the sharp flow reduction that has been observed Sundal et al. (2010), Bartholomew et al (2010), Sole et al (2011), Ahlstrom et al (2013).

> *Many of these papers are cited elsewhere throughout the paper and we wanted to keep the introduction brief.*

1105-27: The drainage speed coloration is hard to see in Figure 2. Should provide better quantification of the number of lakes that drain quickly versus those that do not. How many drained fast both in 2009 and 2010?

> *We assume this is meant to apply to Figure 3. Although perhaps less than ideal, the color table is the result of quite a bit of experimentation and we feel we have reached the point of diminishing returns. As mentioned in Response to reviewer 1, we will put high-res versions of the figures in the supplement.*
>
> *It was outside the scope of this paper to provide such lake drainage stats (especially since we are somewhat limited by the 11-day temporal resolution of our imagery). Such stats have been collated elsewhere in papers by Sundal et a and by Selmes et al.*

Figure 2: Contains an impressive amount of data on lake size, the number of streams that end in moulins versus crevasses etc. A table that quantify pond area, duration and type would be informative.

> *We don't feel such stats would be informative here and as we have noted there papers out there that do a really nice job of presenting such data.*

1106-22: This annual minimum is what percent below the peak flow?

> *See response to reviewer 2, where we have modified this text so this comment no longer is applicable.*

1107-10: A graph of the change in velocity with time at specific points such as GPS North and South would be useful to better illustrate the temporal changes, such as the series of figures in Ahlstrøm et al. (2013) for GPS locations on specific glaciers.

The paper focuses on the fine spatial and somewhat coarse temporal variability. The GPS data will be presented in another paper.

1107-29: In comparing the enhanced flow early melt season flow to the later melt season reductions, to what extent do they offset?

The length of the time series is not really appropriate to this calculation (i.e., you really want the full year time series, and our data have somewhat arbitrary start and ends).

1108-19: Is this explanation of more widespread impact more pronounced where the surface and bedrock gradient are not aligned?

See paragraph starting at 1109-22

1109-1: This is data descriptive and should be in results.

No it's a lead in to the discussion and belongs where it is.

1110-5: Does any of the data allow quantification of this difference of maintaining higher basal water pressures, such as a reduced rate of velocity reduction after the peak?

Not at this stage.

1110-13: Though locations where surface water is routed transverse to ice flow can be seen in Figure 2, a figure focusing on identifying such regions would be of value.

This is the purpose of the arrows pointing out such regions in Figures 3 and 4.

1110-16: Where bedrock and surface gradients are aligned is the period of enhanced flow prolonged or does it end earlier? Is the period of reduced flow different? This is a key observation of this paper that is unique. The varied impact of velocity response with the different relationship of surface and bedrock gradient relationship and should be explored in more quantitative detail.

Developing this quantitative relationship is a subject for future research.