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Interactive Comment

Interactive comment on "Influence of supraglacial lakes and ice-sheet geometry on seasonal ice-flow variability" *by* I. Joughin et al.

Anonymous Referee #2

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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 Greenland Ice 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

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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.

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.

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.

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.

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?

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?

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.

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?

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?

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).

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

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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.

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).

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.

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.

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.

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

Figure 1. The velocity arrows need a scale.

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

Figure 3. The triangles are almost impossible to see.

Interactive comment on The Cryosphere Discuss., 7, 1101, 2013.