I. Joughin – comments

General Comments

This paper presents results for a time-dependent model simulation of Jakobshavn.

The best model out of 50 runs is presented and the results are claimed to match the observations reasonably well.

In fact, the model does rather a poor job of matching the velocity data.

Examination of velocity fields in Figure 2 indicates a wide (~10 km) region of fast flow, which falls off with distance inland (within about 5km). Comparing 2010/2012 observations in Figure 6 shows that the region of fast flow extends much deeper into the ice sheet. Furthermore, maps of velocity from other papers (see refs in this paper) show the fast flow is also much narrower (~5km). Thus, it is really surprising that model does so well with things like the terminus retreat given that it should not be correctly reproducing the thinning well since it has the strain rates all wrong.

Authors: Our velocity field is wider but its magnitude falls more rapidly inland. The observed flow for JI after 2005 may be narrow but the fast flow area within that narrow flow strip is longer (see Fig. 5 from http://www.mdpi.com/2072-4292/7/7/9371/htm). Therefore, from a modelling perspective it is more relevant to make an overall comparison of the observed and model distribution of surface speeds for the drainage basin area rather than simply refer to a particular small area or an ensemble of grid cells that do not match. Furthermore, Fig. 3 (top) which shows the points S1 to S6 positioned over a distance of more than 20 km (see Fig. 1) show a fairly good match with time series of observed velocities.

It seems relevant to remind that the model was tuned such that it matches the observed mass loss and mass change estimates for the period 1997-2014. This is stated in SI, page 2 as being one of the 2 conditions, beside the terminus positions, for selecting the best simulation out of the 50 performed. Therefore, the match is not surprising but expected and one should see the velocities as an "output" rather than an "input" to the model.

Furthermore, a perfect match in terms of the shape of the flow can only be obtained when inverse methods to infer the basal friction parameter from observed surface velocities are used. Considering that we are doing forward simulation runs from 1990 to 2014, 24 years during which the shape and the speed of the glacier have changed significantly, would further translate in significant changes in the basal friction parameter.

Figure 6a is somewhat unclear. Is the glacier really grounded as far out as they show (what happened to the floating ice tongue.) Is this just poor plotting that fails to distinguish between grounded and floating ice – though if that is the case why do some areas show a small floating ice tongue.

Authors: The glacier is grounded as far as shown. Floating ice shelves with thickness smaller than 50 m are not shown. For consistency and clarity we have created a new Fig. 6 which includes both modelled surface elevations and velocities in the same graph.

Where are the bed elevations coming from where the tongue used to be?

Authors: The bed elevations used are the ones from Bamber et al. (2013).

The 2012 observations plotted in Figure 6 should extend to within about 1-km of the terminus. Why then, if the model is doing such a good job with the terminus, are the modeled 2012 velocities 5- km forward of the observations (this seems to indicate the terminus position is off by 5-km).

Authors: You cannot judge a retreat that spreads over 25 years where you have several terminus positions that match observations based on one single position that does not match. The modeled 2012 velocities are around 3-5 km forward because the terminus is 3-5 km forward.

However, unfortunately the profile shown in the original version of the paper was misplaces (a bit too close to the margin of the southern tributary). We have updated the bed profile.

More likely it maybe the seasonal timing. It seems like winter positions are shown in Figure 2, not the much more retreated summer positions, which also drive the fluctuation in speed. Does the model capture the seasonal variation (the winter positions are not particularly informative because often the terminus in the winter is the front of a weak ice shelf that doesn't really provide much buttressing).

Authors: Yes. The model captures the seasonal variation (or to be more precise, some sub-annual fluctuations).

Please explain the strong swings in speed at sub-annual time scales (Figure 3). In any given year from about 2000 onward, there are a several swings in speed by more than 2000 m/yr (some as large as 10 km/yr). What is causing these fluctuations?

Authors: In the eigen calving model, calving is related to the product of the eigen strain rates. Each calving event changes the stress distribution near the terminus. With our grid resolution of 2km, the smallest modeled calving event calves an area of >= 4km^2 which is quite large compared to the size of the floating tongue. We think that the spikes in velocity magnitude are a consequence of the stress redistribution. Therefore, we would expect that by increasing the grid resolution such that it becomes smaller than, e.g. the average calving event observed at JI, the terminus advance/retreat could be modelled more accurately and the magnitude of these peaks and the large fluctuations in the modelled velocities could be reduced.

The paper claims that the 2012 observations must be melt because the model does not reproduce the observed speeds. Yet there are peaks in many years comparable to the 2012 peak, and the modeled velocities at S1 are quite close to the peak velocities in 2012.

Authors: We agree. Updated in the new version of the manuscript.

Why are there multiple (2-4) peaks in a given year, which is inconsistent with the single seasonal peak shown by the observations. What is driving this variability? Because it doesn't seem to line up with the melt season, I doubt it can be explained by whatever seasonal forcing the model uses. If it has to do with the terminus position and the thickness at the terminus, then I would argue that it would confirm (not nullify) the hypothesis in the Joughin et al papers because it demonstrates the kind of sensitivity they describe.

Authors: The multiple peaks in a given year are mostly created by the advance and retreat of the front (See SI, sect. 1.4 and Fig. S7, S8, S14). Increasing the grid resolution and including other potential seasonal forcings (e.g. ice mélange variability, seasonal ocean temperature variability) will probably help to minimize some of the sub-annual noise. E.g. The ice mélange can prevent the ice at the calving front from breaking off and therefore could reduce the calving rates. The discussion includes now the points raised here.

Comparing a simulation with monthly climatic forcing vs. a simulation with constant climatic forcing (see SI, Fig. S12-A vs. B) we see that the climatic forcing does influence (although the influence is relatively small) the velocity peaks at a sub-annual scale.

What is driving this variability? Because it doesn't seem to line up with the melt season, I doubt it can be explained by whatever seasonal forcing the model uses. If it has to do with the terminus position and the thickness at the terminus, then I would argue that it would confirm (not nullify) the hypothesis in the Joughin et al papers because it demonstrates the kind of sensitivity they describe.

Authors: As stated above, the multiple peaks are mostly created by the advance and retreat of the front and by the thickness at the terminus and indeed this confirm the hypothesis in the Joughin et al papers (see Fig. 6).

The differences could then be explained by the fact that there is no overdeepening in the bed in the Bamber model at the location. In fact at the 2012 terminus location, the Bamber DEM is about 700 meters deep while the CRESIS DEM shows a sharp dip of 1300 meters. While one can argue which DEM is better, we do note that the 2012 peak corresponds well with the time when the terminus was at the overdeepeening and the subsequent lesser peaks (2013-2015) correspond to when the terminus was at the upstream high (see also comments in next paragraph). Furthermore, our analysis in the 2012 paper shows the seasonal variability can be explained by retreat and advance over the slope downstream of the overdeepening. You can choose to believe one bed model over the other, but you need to clearly articulate why the differences exist (not merely dismiss the earlier results based on the absence of a peak in your model, which as described below it would be rather surprising if the skill was such that it could predict the peak).

Authors: This paper do not nullifies but rather sustains the hypothesis in the Joughin et al papers. Unfortunately, the profile shown in the original version of the paper was misplaces. We have updated the bed profile and it is now fully consistent with the profile shown in e.g Joughin et al., 2014, Nick et al., 2013 (nature).

It is interesting that Figure 4 nails the mass loss so well, yet the modeled speeds (S1/S2 Figure 3) are consistently biased high (eyeball average by 2 to 4 km/yr – 20 to 30%). If they are indeed getting the mass flux correct, with an ice stream that is too wide as well, then this would indicate that the bed is too shallow. If the bed is not right, then model will have a difficult time reproducing the observed behavior (i.e., getting the 2012 peak right).

Authors: We disagree. The bed is not too shallow. Please see comments to reviewers.

In summary, at a minimum this paper should do the following if it is to make a contribution .

• Better explain the ocean forcing (not clear what the ocean is doing – how much meltback of the terminus is occurring once the shelf is lost).

• Better explain what is driving the swings of in speed of 2 to 10 km/yr near the terminus over time scales of 3 to 4 months, which does not seem to indicate a seasonal behavior based on the timing.

Authors: Done.

• Better justify or remove the statements about the 2012 melt event as cause for the 2012 peak, because a) the model clearly can produce swings that large in other years and there is no reason to expect the model has the skill necessary to nail the timing of the 2012 peak precisely, particularly since the bed is not conducive to doing so (remarkably the model is not that far off in 2012);

Authors: Done.

b) the model doesn't do a very good job of reproducing the velocity field and by extension it can't be doing a good job of reproducing the thinning patterns;

Authors: We disagree, as the results prove otherwise. Please see Fig. 3 again (top and bottom). Regarding the shape we have given our explanation above.

c) simply because a model does not reproduce observed behavior, it cannot be assumed to be the melt event by default as tempting as that may be given the melt that year, when there is little evidence for melt producing such changes on this glacier (see comments below) – and certainly not the point of saying that "it is proved" (again especially when there is so much unexplained variability in the modeled speed record); and d) the reference to Doyle does not provide good support for the reasons described below.

Authors: We do not imply that the melting alone triggered the summer acceleration observed in 2012 but rather that it may contributed to it. Several changes have been made throughout the manuscript to address this.

Specific Comments

4866:L7-10, "We identify two major accelerations. The first occurs in 1998, and is triggered by moderate thinning prior to 1998. The second acceleration, which starts in 2003 and peaks in summer 2004" This makes is sound as though this work is the first to identify these accelerations. What the model does is find accelerations consistent with observations – this distinction should be made clear.

Authors: Done.

4866: L10-12: "As opposed to other regions on the Greenland Ice Sheet (GrIS), where dynamically induced mass loss has slowed down over recent years,…" This statement is really not supported by facts. It could be argued that the rate of increase in loss has declined. Although there is some interannual variability, losses were at their highest in 2011 (2012 was a close second and the two years were not significantly different) [Enderlin et al, 2014].

Authors: Done.

4866: L15-17 "Our analysis suggests that the 2012 acceleration of JI is likely the result of an exceptionally long melt season dominated by extreme melt events." This statement needs some revision in light of discussion below.

Authors: Removed in the new version of the manuscript.

4866: L24:25 "Jakobshavn Isbræ is the largest outlet glacier in terms of drainage area as it drains ∼ 6 % of the GrIS." This should say "one of the largest" not the "largest". For example, Rignot has 79N at 103,314 km^2, while he has Jakobshavn at 92,080 km^2. Making statements like this just tend to introduce inaccuracies into the literature that then get repeated over and over.

Authors: Done.

4867: L2coincides with thinning of up to 15 m a−1 between 2006 and 2012 near the glacier front (Nielsen et al., 2013) as observed from airborne laser altimeter surveys. The period over which the thinning rate was 15 m/yr is longer than this and there are several other papers that have cited such a rate prior to the 2013 reference, including the Krabill et al 2003, and the Joughin et al, 2008, which with the Nielsen paper indicate thinning from 2003 to 2012 (I would say "from 2003 at least through 2012", since the rates did not decline as of 2012 – if anything they have increased).

Authors: Done.

4868: L9 "with observations available only during 1992, 1995 and between 2000 and 2003." If you check the Luckman paper you will find measurements bridging this gap.

Authors: Done. The statement has been removed in the new version of the manuscript.

4870:L1-2 "The bed elevation dataset for all of Greenland has a 1 km spatial resolution" The bed elevation model is "posted" at 1-km, the actual resolution is far poorer for most of Greenland as it was interpolated from flightlines many kilometers apart.

Authors: The statement is corrected in the new version of the manuscript. However, note that in the outlet of Jakobshavn the bed from Bamber et al. 2013 has been improved based on CReSIS DEMS produced at a resolution of 125 m (see SI, sect. 1.3.2).

4870:L25-28 "Along the ice shelf calving front, we apply a physically based calving (eigencalving) parametrization (Winkelmann et al., 2011; Levermann et al., 2012) and an ice thickness condition (Albrecht et al., 2011)." Should comment on how appropriate this law is for calving from a grounded terminus (potential several meters above flotation), which is a very different style than an a floating Antarctic ice shelf. And for that matter, is the terminus allowed to go above flotation? What is the dynamic boundary condition applied at the terminus (does it assume flotation)?

Authors: Additional information regarding the calving law and the conditions at the boundaries has been included in the new version of the manuscript. Please see to sect. 2.1.2 and 2.1.3 for more details.

4872:L11:15 "In order to match the observed front positions a sub-shelf melting parameter (Fmelt) with a value of 0.198 m s−1 (see Eq. 5 from Martin et al., 2011) is used in our simulations and results in basal

melt rates slightly larger than those obtained by Motyka et al. (2011)." While I appreciate there is a reference cited, since a specific parameter is given it would help to have a little more detail (I assume the melt rate is not 365*86400*.198 m/s). Given a range for what the modeled melt rates are. Also how is the melt rate applied to the vertical calving front once the ice shelf breaks up (i.e., how is the ocean coupled to the model once the shelf if gone).

Authors: The basal melt rates were added in SI. Please refer to SI, sect. 1.2.8.

4872:L16 "Smith et al", should be "Joughin et al" (Smith was a coauthor).

Authors: Done.

4872:L23-end of page, "A previous analysis by Joughin et al. (2014) attributes the acceleration and the summer peak of 2012 to the retreat of the JI terminus to the bottom of an over deep- ened basin (see Fig. 3 from Joughin et al., 2014). This retreat, which started in 2009 (Joughin et al., 2014), should have triggered an acceleration of JI as soon as the terminus started to retreat in 2009 over the slope of the over deepened basin. However, there is no evidence of such acceleration either in the observational record" Please see Joughin et al., 2012, where we do an analysis that shows that all of the seasonal speedup can be attributed to the movement of terminus seasonally back and forth along the reverse slope. Note also the discussion there that as the terminus thins, this effect is lessened with time (i.e., see Figure 7, Joughin et al, 2012 for how thinning with time changes the overall response).

Authors: Deleted in the new version of the manuscript.

4873:L10 "which likely triggered the rapid acceleration in the observed ice surface speed in the summer of 2012." I don't doubt some of these processes could have contributed to the timing of the calving events, but nothing presented thus far supports the idea that it was triggered by the extreme melt. In particular, most of the speedup occurred during the June and early July, before the melt events that are cited.

Authors: Note that the highest melt event was on July 10 (see Fig. 3 and 4 below). Anyway, the discussion has been completely changed.

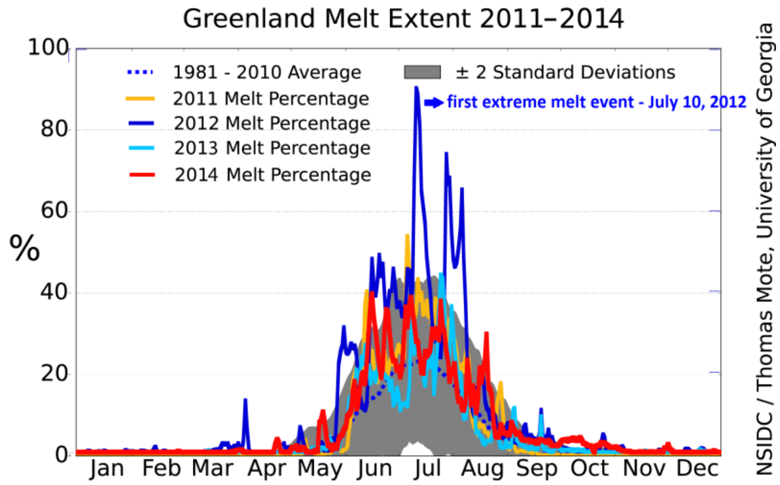

Figure 3. Melt extents for 2011-2014. The figure is as taken from NSIDC (http://nsidc.org/greenland-today/2015/01/2014-melt-season-in-review/)

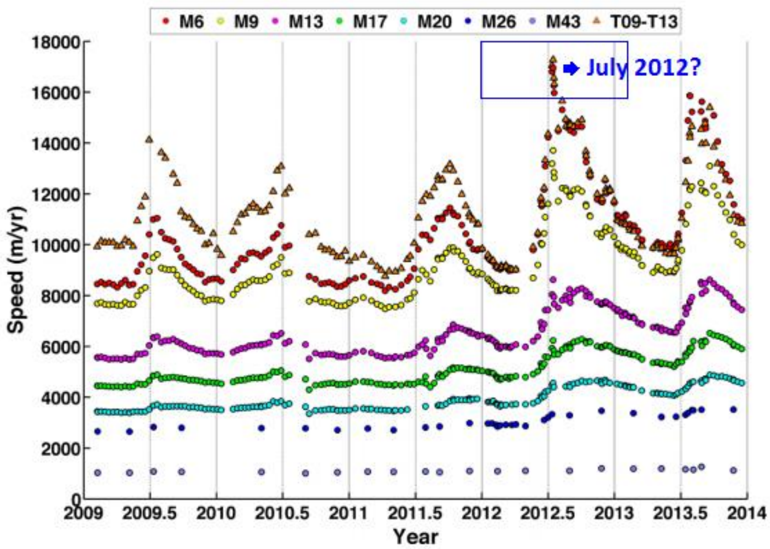

Figure 4. Figure representing speeds as taken from Joughin et al. (2014) (Fig. 2)

4873:L15 "Furthermore, a study by Doyle et al. (2015) found that during an amplified melt event (caused by late-summer cyclonic rainfall that occurred between 23 August and 3 September 2011) JIs speed increased by 10 % (see Fig. S3 in Doyle et al., 2015)." If you examine the 2011 event referenced by Doyle, you see that the there as a summer long build up in speed, with a late summer peak roughly corresponding to the period examined by Doyle et al. There is not a sharp signal or anything anomalous that corresponds that or any other particular melt event. All of the events Doyle examined show a decline following the melt event, generally late summer annual minimums by Sept 15 (see small speedup on Store, followed by below average). By contrast, Jakobshavn speeds in 2011 didn't decline to a seasonal minimum until the following spring. While Jakobshavn does have summer peaks, there is no real correspondence to the melt season. Some years the peak is in June, sometimes late summer (early

September). Generally the speedup commences well before there is melt and extends to well after when there is melt. The timing of the peak tends to correspond well with changes in the position of the terminus. 2013 was the second strongest peak, yet it was an extremely low melt year.

Authors: The discussion has been changed and the above is not included in the new version of the manuscript.

4873:L17:18 "Therefore, the influence of enhanced surface melting in JIs dynamics has been proven." Based on some handwaving, weak correlation, and the fact the model can't reproduce the observations by no means constitutes a proof. I would say Joughin et al, 2012 and 2014 make a good case for the influence of the trough geometry, but I would not be so bold though as to say that it is proven (though there is much better evidence at this stage than for melt).

Authors: Done. The statement has been deleted.

4873:L19-24 "Under normal circumstances (i.e. average melt years), the winter slow-down is usually able to compensate for the summer acceleration events. The summer of 2012 was however preceded by a series of warm summers (2007, 2008, 2010 and 2011) (Hanna et al., 2014), which may have created the conditions under which the winter slowdowns can no longer compensate for the summer accelerations leading to an increase in the mean annual flow." What exactly is meant by this statement (note 2013 was a below average year for SMB, yet its peak was higher than 2007, 2008, 2010 and 2011). The mean annual speed is such that Jakobshavn has been well out of balance for more than the last decade (well prior to 2007). Even the winter speeds are much faster (>2x) than the speeds prior to speedup. It is true the lower trunk thickens over the winter, but only at the expense of the inner catchment so the basin thins as a whole. Moreover, the net thinning in this region has been ~15 m/yr for more than the last decade. So what is meant by compensate????

Authors: We do agree it sounds confusing. Therefore, it has been deleted and is not included in the new version of the manuscript.

4875:L1 Fix Smith et al., 2010 (see comment above);

Authors: Done.

4875:L24-25; You should also cite the Luckman paper here as they present the best record for this period.

Authors: Done.

4877: L12-15 The period 2004–2014 is characterized in our simulation by relatively uniform velocities with strong seasonal variations (Fig. 3). During this period, the terminus remains close to the grounding line position with no episodes of significant retreat. I would hardly describe the speeds in this time as uniform. Moreover, are the fluctuations seasonal or merely sub-annualy (they certainly seem more frequent than annual and I can't tell at the resolution of the plot whether there is a strong seasonal correspondence. Certainly the frequency of variation is contrary to observations). The late summer grounded terminus over this period retreated by more than 6-km – does the model not reproduce this behavior – if so how is it claimed that the model well reproduces the observations?

Authors: Uniform in the sense that we do not observe any significant acceleration, e.g similar with 1998 or 2000-2003 and the summer peaks are relatively "uniform". We have added:

"The period 2004-2014 is characterized in our simulation by relatively uniform velocity peaks"

Furthermore, we do not agree that the frequency of variation is contrary to observations (see Fig.3 and SI, sect. 1.4). Also, keep in mind that this is a model and one cannot expect that the timing at a sub-annual scale will fit perfectly with observations. See also the answers above regarding the sub-annual variation (e.g. grid constraints (calving) and other potential seasonal forcings).

4878:L3-6 "The acceleration that characterizes JI during 2012 is likely the result of an exceptionally long melting season dominated by extreme melt events (Nghiem et al., 2012; Tedesco et al., 2013; Hanna et al., 2014) and may not be caused by a retreat of the JI terminus to the bottom of an overdeepened basin" The fact that model did not reproduce the summer speedup is not any kind of evidence that it must be due to the strong melt event. The Joughin 2012 paper uses quite a different bed model. While its arguable which model is better, the model they used can explain the seasonal variation. As mentioned above, the paper does not currently explain what is driving the large swings in speed in the model.

Authors: The statement was removed in the new version of the manuscript.

4878:L15-16 "Our model results provide evidence for two distinct flow accelerations in 1998 and 2003, respectively." This is written as though it's a new discovery. But work by Luckman (for the 1998 speedup) and Joughin (for the 2003 speedup) basically show this. So it should say something more like "our model results are consistent with observations…"

Authors: Done.

4878:L25:26 "Furthermore, despite the slow-down of glacier speed in other drainage basins of the GrIS over recent years (Bevan et al., 2012; Enderlin et al., 2014), our modelled and observed results suggest that JI has been losing mass at an accelerated rate, and it continued to accelerate through 2014 when other glaciers slowed" This statement makes it seem like Jakobshavn is the only glacier speeding up. While there is a lot of variability, Enderlin et al (2014) and Moon et al (2012) demonstrate that on average, Greenland's glaciers are continuing to speedup.

Authors: Done. Not included in the new version of the manuscript.