Response to Anonymous Referee #2

We thank the anonymous referee for his/her comments on the manuscript. Especially the clarifications of the applied methods and the closer look at our approximation of the basal yield stress will improve the manuscript greatly. Our responses are marked in blue.

The paper describes an inversion for basal slipperiness of Jakobshavn Isbrae performed using data from several different years. This is one of very few such studies performed to date. I found the paper very interesting and I recommend it for publication subject to some revisions.

I could not see any description of the actual minimisation procedure used. The only information is that the 'Toolkit for Advanced Optimisation' was used. I am guessing that some sort of gradient-based minimisation method was used. How was the gradient of the cost function obtained?

We will add: "We discretize the functional I(\tau_c,\alpha) by representing \tau_c via a finite-element approximation, and by computing a finite element solution for u(\tau_c). The gradient of this discretized functional can be computed exactly, and a minimum can be sought by any one of a number of gradient-based minimization algorithms. We use a limited-memory, variable-metric method from the Toolkit for Advanced Optimization (TAO) (Munson, 2012) to seek an exact minimum of the discretized cost function, I(\tau_c,\alpha). Assuming that there is a unique minimum (which is true at the very least when \alpha is small), an exactly computed minimum of the discretized functional will be independent of the numerical method used to find it."

Was tau_c enforced to be positive, and if so how was that done? I would like to see some further technical details of the inversion procedure.

Will add: "The positivity of \tau_c is enforced by solving for \zeta in \tau_c = \tau_{c,scale}\exp(\zeta) where \tau_{c,scale} is a scale parameter to keep \zeta of order 1 for typical values of \tau_c."

I could also not see any statements about the spatial resolution of the numerical model.

The beginning of the data section mentions the 500 by 500 m grid, but I will add this information in the model section as well. And add this: "We chose a grid resolution of 500 by 500 m. A finer resolution is not warranted by the data and tests with coarser grids show convergence. A finer grid might be desirable in the area of the deep trough, where basal topography changes rapidly."

What were the boundary conditions applied to the lower limits of the numerical model?

Will add: "In regions where H is zero, the product of effective viscosity and thickness is regularized with a constant (set to 1x10^13 Pa s m), for details see http://www.pism-docs.org."

Did the model extend towards the calving front? I could not see this information anywhere in the paper. Figure 1 is a bit confusing in this respect.

Will add: “The SSA is solved over the entire model domain, but only velocity data within the misfit area is used to adjust the basal yield stress. Results are only interpreted within the misfit area, which is taken to be the same for all years. Areas outside the misfit area are shaded or excluded in all figures.” to section 2.2.4.

Is the model domain the whole area shown in the Figure?

Will change caption of Fig. 1 to: “Model domain (whole area shown) with MODIS image…”

I would like to see a better description of the boundary condition applied at the lower boundary.

Will add: “PISM treats the SSA as if it applies to the entire grid domain, even in ice-free locations. No additional boundary conditions are applied to the terminus of the glacier, instead the ice thickness simply decreases to zero from one grid point to the next.”

Is it possible that the changes in velocity might be due to decrease in buttressing at the grounding line?

Will add: “We solely concentrate on snapshots of ice geometry and do not investigate causes of the change in geometry, such as increased melt or decreased buttressing at the ice front.”

The reference to tau_c as basal yield stress is confusing. tau_c is defined in equation (1). As far as I can see equation can also be written as \tau_b= C^{-1/m} |u|^{1/m-1} u with m=1/q and C^{-1/m}=tau_c/u^q_{threshold} So is this not just the standard (viscous) Weertman sliding law? Why talk about a yield stress in this context? It appears that the inversion effectively solves for basal stickiness (inverse of basal slipperiness). Since u^q_{threshold} is fixed at 100 m/a one can always calculate C directly from tau_c. The value q=0.25 corresponds to Weertman stress exponent m=4.

The sliding law stated in our paper entails the standard (viscous) Weertman sliding law, but it also allows for a perfectly plastic sliding law. Our choice of q=0.25 is meant to approximate the plastic case. We will adjust the text in the following way to clarify this:

“… The purely plastic case is achieved by setting q = 0, whereas q = 1 leads to the common treatment of basal till as a linearly viscous material: \tau_{b,x} = \gamma u and \tau_{b,y} = \gamma v, where \gamma > 0 is a scalar function of position, called the basal stickiness. When setting q = 1 the basal stickiness, \gamma, and the basal yield stress, \tau_c, are related through \gamma = \tau_c / u_{threshold}. Here, instead of setting q = 1 and solving for \gamma we solve for \tau_c, which has units of stress and is the basal yield stress if q=0. Despite approximating the perfectly plastic case by setting q=0.25 for this study, we call \tau_c the basal yield stress. Test inversions with q=0.1 and q=0.001 for the 1985 and

2006 data sets resulted in different \tau_c values, but the pattern and amplitude of changes in \tau_c remain and the main conclusions of this paper are unchanged."

As mentioned in the text the bed is not known in complete detail. How can this be expected to affect the inversion? Will errors in bed geometry affect the estimate for tau_c? Was an inversion performed for some other possible bed geometry to test the effect of errors in bed topography on tau_c estimates?

Will add: "We investigate the influence of bed topography on the inversion results in Habermann (in print) and we suspect that errors in bed topography lead to residuals that are larger than the residuals due to errors in velocity observations. This large expected error is consistent over all inversions performed here and we do not expect a significant influence on the changes in basal yield stress." to sec. 2.2.2, where the bed topography data set is discussed. 'Habermann (in print)' refers to Habermann, M. (in print). *Basal shear strength inversions for ice sheets with an application to Jakobshavn Isbrae, Greenland*. Ph.D. Thesis, University of Alaska, Fairbanks.

I found the reference to the Mohr-Coulomb puzzling. After all tau_c is not a basal yield stress. However, at the same time I found it useful to see that the variation in tau_c could not be explained from the difference between ice overburden pressure and ocean pressure (\rho g H - \rho_w).

The tau_c that we infer is an approximation to the basal yield stress and as such, a rough comparison to the Mohr-Coulomb law is deemed useful. To investigate the influence of this approximation a bit more, we conducted additional experiments with q=0.1 and q=0.001 and we will add in the discussion: "In this study we use an approximation to a perfectly plastic sliding law, therefore, \tau_c is only an approximation to a basal yield stress. We test additional smaller values of q (q=0.1 and q=0.001) in Equation \ref{eq:tau_b} to see if a closer approximation to a plastic till affects our findings. The actual value of \tau_c increases by up to 2x10^5 Pa, but the lowering of \tau_c in the first 7 km during the time of acceleration is a robust result. Comparing \tau_c to effective pressure leads to slightly higher values of till friction angle (\phi ~3), but these values are still low compared to measured values mentioned above."

Fig. 6 gives a nice overview of the results for different years. But it is very difficult to see the spatial pattern of tau_c in the figure. Spatial scale of x and y axis is missing in both Fig 4 and Fig 6.b I suggest producing at least one figure showing tau_c/u_{\threshold} in greater detail.

Will add a figure showing a close-up of the basal yield stress result for each year. Will add spatial scales to Fig. 4 and Fig 12. There is no Fig. 6b.

Minor comments:

-p 3, l 1:Not sure what is meant by 'dynamic evolution'?

Will change to "rapid evolution".

-p3, l 2: the term 'stable' is used in a few places where presumably 'steady-state' or 'stationary' would be a more accurate term to use.

Will change 'stable' to 'stationary'

-Why should one expect tau_c and tau_d to be similar? Is that because the surface velocities are about 100m/a?

We don't expect them to be similar, the figure illustrates that despite only small changes in driving stress the basal shear stress changes significantly.

Will add: "Despite minimal changes in driving stress from 1985 to 2006 the basal shear stress changes significantly over this time period."