Reply to S. Cornford's Review on "Thermal structure and basal sliding parametrisation at Pine Island Glacier - a 3D full-Stokes model study"

First we would like to thank the reviewer for his constructive and helpful comments. Below you find a point-by-point response to them.

1 General comments

Reviewer1: The paper investigates basal traction laws based on measured bed roughness for PIG, studying them using a rather sophisticated new Stokes model. A typical PIG flow experiment finds the basal traction $\boldsymbol{\tau}$b through an inverse problem and then assumes a physical law of the form C $\boldsymbol{\tau}$b = u^{m-1} $\mathbf{u}$ for use in prognostic simulations, holding C(x, y) constant in time. The authors compare this conventional kind of approach with two roughness based laws, with the roughness derived from measurements (measured along flight lines and extended to the whole region). The resulting C would still be constant in time (unless some roughness evolution model was added), but its time independence would have some more physical justification.

If the model had reproduced all the features of the flow using these measurements, this would support the conventional approach, but perhaps supplant it. However, the model does not reproduce the observations as well as it does with an inferred C (even when the inversion, as in this paper, is not as sophisticated as elsewhere) and so the authors find that C is not just a function of roughness but other fields that are not observed – which could include roughness at the scales below those measured, or other physics, including physics that could evolve on short – such as basal hydrology. Nonetheless, the paper shows that many of the flow features can be accounted for by measured roughness, so that the proposed laws could form part of more complex laws, and I suggest could be directly useful in, for example, paleo ice stream simulations where the ice has gone but bed roughness data might be available.

The friction laws themselves are presented are in the results section, but I think they are more important to this paper than the description of the Stokes model, so should be described earlier. I also think that the sections describing the laws need some revision, in particular the manuscript needs to summarize the way in which the one-parameter roughness was measured (inferred from airborne radar, in Rippin 2011, estimated to be sensitive to roughness wavelengths > 500 m), and explain the calculation of the two-parameter roughness in more detail.

Answer: We agree that the roughness data and the sliding laws need more attention and also need to be described earlier in the manuscript. We therefore added another Section titled **Methods: roughness data and sliding laws**, which is included right after the model description. There in detail the derivation of the roughness data is explained. It is also emphasised that the single-parameter roughness was already presented in Rippin et al. 2011, while the two-parameter roughness measure was especially calculated for this study. The sliding laws are also described in more detail and a better overview about the connections of the different formulations and parameters is given.

R1: At the same time, a temperature distribution is estimated. This is a useful result on its own.

2 Specific comments

R1: L10: Some inverse problems seek effective viscosity (or some equivalent) as well as or instead of a basal friction coefficient in some inverse problems. Especially in the ice shelf, but to some extent in shear margins, effective viscosity determines the flow. This is mentioned in the discussion.

Answer: We focussed in this study on the parameterization of basal sliding and therefore did not mention in detail the inversion for effective viscosity. One advantage of our model is, that we managed to solve for the temperature also in the ice shelf. Therefore the need to invert for effective viscosity there is not as crucial. Nonetheless we do recognise the effective viscosity as being especially important in the shear margins, and address some of the deviations in surface velocity to this shortcoming.

Changes: Changed P4914, L8-10 to: Inversion methods are commonly applied to reproduce the complex surface flow structure at Pine Island Glacier, which use information of the observed surface velocity field, to constrain, *among other things*, basal sliding.
Changed P4915, L16-18 to: These methods use the measured surface velocity field to invert for basal properties *or effective viscosity* and to adjust basal sliding parameters.

R1: P4916 : 'Changes in basal conditions, by for example grounding line migration (Park et al., 2013), subglacial erosion (Smith et al., 2012; Rippin et al., 2014) or dynamic hydraulic systems, can not be considered with this approach. '

I agree that subglacial erosion or evolving hydrology defeat a inverted C that is constant in time, but I don't think that is true for grounding line retreat – the evolution of C (becoming zero in newly grounded regions) is straightforward in that case.

Answer: We agree that grounding line retreat could be considered nonetheless.

Changes: Changed P4916, L4-6 to: Changes in basal conditions, by for example subglacial erosion (Smith et al., 2012; Rippin et al., 2014) or dynamic hydraulic systems, can not be considered with this approach.

R1: 2.3.2.
Temperature transport includes strong advection, but you don't say whether this affects your numerical scheme (you just say that you use linear elements). Do

you ensure the local Peclet number is always low by choosing a mesh, or add artificial diffusion, or use DG methods, or something else?

Answer: We do use stabilization methods provided by COMSOL, which we forgot to mention. The details are described below.

Changes: We added a paragraph in Sect. 2.3.2: To avoid numerical instabilities due to strong temperature advection, and thus to ensure that the element Péclet number is always < 1, we use consistent stabilization methods provided by COMSOL Multiphysics©. Equation (7) is solved using a Galerkin Least Square (GLS) formulation (Codina, 1998) in streamline direction and crosswind diffusion (Hauke and Hughes, 1998) orthogonal to the streamline direction. The chosen stabilization methods add less numerical diffusion the closer the numerical solution comes to the exact solution (COMSOL, 2012).

R1: You say that all Dirichlet conditions are implemented as weak constraints. Does this mean you are adding a source term S to the equations on the basal face along the lines of S=a(T0-T), so that as a →∞ the solution approaches T0=T. If so, you are actually implementing a Robin condition (which is fine).

Answer: Weak constraints satisfy a condition in a weak sense, i.e. in integral sense over the entire boundary of the element. They are different to Robin conditions.
Pointwise constraints: $u(x) = f(x)$ for x on Γ (boundary)
Weak constraints: $\int\Gamma\, u\, \varphi\, \delta x = \int\Gamma\, f\, \varphi\, \delta x$ with φ being the test function.

Changes: Changed P4923, L10-11 (where „Weak constraints" are first mentioned) to: The kinematic condition at the ice base is implemented as a weak constraint, for stability reasons. Weak constraints apply boundary conditions in an integral sense and are therefore not as strict. They stand in contrast to pointwise constraints, which force the nodal value to the constraint and can thus lead to numerical instability (COMSOL, 2012).

R1: Your heat flux condition looks like a softening of the step change around Tb;max, did you have problems with a sharp step?

Answer: Yes, it is a softening of the step change. A sharp step led to numerical instability and the model did not converge.

Changes: Added a sentence at the end of Sect. 2.3.2: The smoothing of the step function ensures numerical stability, which was not found with a sharp step.

R1: 2.3.3
I don't think that the aspect ratio requires an unstructured mesh (and indeed, several models use structured or block-structured meshes). Even the need for fine horizontal mesh resolution near the grounding line / shear margins is a little contentious, for example you might use high order elements instead. The figure looks as though you have extruded a 2D mesh of triangles vertically to get prisms with a vertical extent that varies only with thickness, (in which case you have

structure in the vertical direction). That could be because we can't see into the mesh - maybe you have finer vertical resolution in the regions with finer horizontal resolution. If so, is it possible to make a cut into the mesh figure to show that?

Answer: The original first sentence in Section 2.3.3 ("*The small aspect ratio of PIG (ratio of vertical to horizontal extent $\varepsilon = HL^{-1} \approx 10^{-3}$) requires an unstructured finite element mesh, to maximise the resolution while minimising the amount of elements."*) was not intended to say, that due to the small aspect ration the unstructured mesh is mandatory, but it is very useful to maximise the resolution in regions of interest, while minimising the amount of elements. The higher mesh resolution around the grounding line is, as you say, not the only way to manage the steep gradients there, but it is a solution for this study that works well. Using high order elements – by the way – is not a very good solution in the case of sharp changes, since they expose Gibbs phenomena and would require additional filtering in order to control those.

It is true, that the mesh is structured in the vertical direction. We have 12 vertical layers everywhere, where the thickness varies only with ice thickness, which results in sigma layers.

Changes: We changed the paragraph to: To maximise the resolution while minimising the amount of elements, we use an unstructured finite element mesh, shown in Fig.2. The upper surface z_s is meshed first with triangles. The horizontal edge lengths are 5–500m at the grounding line and the calving front, 50–1000m at the inflow area and 100–2000m at the rest of the outer boundary. The resulting 2D surface mesh is extruded through the glacier geometry with a total of 12 vertical layers everywhere. The thickness of the vertical layers varies only with ice thickness. The spacing between the layers is refined towards the base. The ratio of the lowest to the upper most layer thickness is 0.01, leading to a thickness of the lowest layer of about 5m for a total ice thickness of 3000m. The final mesh consists of $\sim 3.5\times 10^5$ prism elements, which results in $\sim 5 \times 10^6$ degrees of freedom (DOF), when solved for all variables.

R1: 2.3.4
Which direct linear solver: MUMPS? UMFPack ? A citation might be in order. I think this paragraph could do with some attention, although the meaning is clear to me, the grammar is a bit awkward. Especially, you say that you use a directed segregated solver which solves iteratively, which sounds self-refuting. I think that what you do is a kind of quasi-Newton iteration to solve a non-linear problem in u;p;T where you
1.Choose initial u*;p*;T*,
2. Use u*;p*;T* to define a linear system which is solved directly to get u;p
3. Use u;p;T* to define a linear system which is solved directly for T
4. Set u*;p*;T* ← u;p;T and repeat 2-3 until your error estimate is small.

Answer: We used the direct solver Pardiso and added citations. And yes, what you write about the quasi-Newton iteration is correct. We rewrote Sect. 2.3.4 to make it clearer (see below).

Changes: We changed Sect. 2.3.4 to: For solving the nonlinear system, a direct segregated solver is used, which conducts a quasi-Newton iteration. It solves consecutively: first for the velocity u and the pressure p, and thereafter for the temperature T (Comsol, 2012). This allows for reduced working memory usage. For the remaining linear systems of equations the direct solver Pardiso (COMSOL (2012) and http://www.pardiso-project.org/, last access: 9 December 2014) is applied. While uncommon for such large numbers of DOF's, it proved to be computationally viable and robust, since all available iterative solvers exposed instabilities on this problem.

R1: 3.3.2
Figure 5 could be replaced (or complemented) with a map of differences. With fig 5 as it is, you have to point out that the differences are exaggerated at low speeds and suppressed at high velocities, given the choice of log axes, which you need because so much of the glacier is slow. At the same time, there are maps to compare your results with in e.g Morlighem 2010. The same goes for figures 8 and 11.

Answer: A map of differences can be found in Wilkens, 2014 (Fig. 4.26). We choose to not show it here, as it would require elaborate description for the structure of the differences, which are partly related to the rheological treatment of shear margins and the inversion technique. We use the reference simulation to compare it to the roughness-related results. So the rheological treatment of the shear margins is consistent in all simulations.
Our focus of this study is not essentially to reproduce the surface flow field as precise as possible, but to understand basal sliding mechanisms and connect them to basal parameters.
Figure 5 shows in our opinion the difference between the simulated and measured velocities well. We will add some explanation in the figure caption for the logarithmic axes chosen. It is already mentioned in the main text (P4928, L23-24): *"The spread around the diagonal for lower velocities appears bigger, which is mainly due to the logarithmic axes chosen."*
We decided to not compare our surface velocity field to the results from Morlighem et al., 2010, as our deviations to the measured field are much higher, would have to be explained in detail and is mainly due to the more sophisticated inversion technique they use. Here again it should be stated that the focus of this study was not to reproduce the field perfectly.
Comparing the results in Fig. 8 and 11 to the results of Morlighem et al., 2010 would give no further understanding of the findings. The differences in magnitude would be very large, which is why we compare the results in a qualitative manner.

Changes: We changed the caption of Figure 5 to: Observed surface velocity field $|\mathbf{u}_{obs}|$ vs. reference surface velocity field $|\mathbf{u}_{s,ref}|$. The logarithmic scales exaggerate the spread around the low speeds. The angle offset $\Delta\alpha$ between the vectors of the surface velocity field $\mathbf{u}_{obs}$ and the reference surface velocity field $\mathbf{u}_{s,ref}$ is shown as the colour code.

R1: Technical corrections

L8: Dependend → Depending

2.2.2 Stokes → Stress, or velocity

p4923, L18 :The temperature is *solved* for with linear elements : → discretized

p4925, L9 : 'The basis data A. Le Brocq used are for the surface elevation from Bamber et al. (2009), which combines satellite radar and laser measurements. The ice thickness data is from Vaughan et al. (2006).' could be something like 'The Le Brocq data are based on the the surface elevation data of Bamber et al. (2009) and the ice thickness data of Vaughan et al. (2006)'.

Answer: Thanks for reading the manuscript carefully.

Changes: All technical corrections are included in the revised manuscript.