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Interactive comment on "Thermal structure and basal sliding parametrisation at Pine Island Glacier – a 3-D full-Stokes model study" *by* N. Wilkens et al.

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

Reviewer2: In the paper the authors perform a surface-to-bed inversion for basal slipperiness using a numerical (full Stokes) flow model. They then compare the inverted (spatially variable) basal sliding parameter with two estimates of basal roughness.

Answer: This is not entirely correct. We do not simply compare the inverted sliding parameter with the basal roughness. This would mean we are comparing the fields $\beta 2$ with for example the single-parameter roughness measure ξ. This would result in

just comparing spatially varying fields. Instead we incorporate two different data sets of measured basal roughness into formulations for basal sliding. Since the model is thermo-mechanically coupled, the system evolves into a new equilibrium with sliding and non-sliding regions (at least for the first approach with the single-parameter roughness). Additionally the effect of basal water pressure is also included in the sliding law. Since the system consists of many unknowns, the outcome cannot be known afore.

R2: The main objective of the paper is, in the words of the authors: to connect measured basal properties to the parameterisation of basal sliding and therefore constrain basal sliding with physically justified assumptions.

Two estimates of basal roughness are used. One is a measure of basal roughness suggested by Li et al, 2010. If I understood correctly this measure is calculated directly by the authors.

Answer: This is correct, the two-parameter roughness measure was calculated by David Rippin especially fort his study. It is based on the same data as the single-parameter roughness measure presented in Rippin et al. 2011.

R2: The other measure of roughness is based on Rippin et al, 2011. My understanding is that here previously published roughness estimates were used.

Answer: This is correct. We included now a paragraph which explicitly describes the roughness data, to make it less confusing. Additionally we changed the lines below.

Changes: Changed P4916, L20-24 to: The first method matches a single-parameter basal roughness measure for PIG, as presented in Rippin et al. 2011, onto a basal sliding parameter. The second method is based on ideas from Li et al. 2010, where we use a two-parameter basal roughness measure, especially calculated for this study, to connect basal roughness to basal sliding.

R2: If I've understood correctly, the roughness estimates are derived from the same bed-topography data set as the one used in the numerical model.

Answer: This is correct; they are both based on the data from Vaughan et al. 2006. Still the roughness measure and the bed-topography enter the model in different ways. The model geometry is built based on gridded data with 1 km spacing. The roughness measure is calculated based on along track sample spacing of the order of 30 m (cf. Rippin et al. 2011). Therefore higher resolution information is included in the roughness measure, even though it is also eventually gridded onto a 1 km grid. Since the roughness data and the use of it was not formulated in a comprehensible way previously, we included a whole new section describing the data and the sliding laws, included just after the model description and titled Methods: roughness data and sliding laws. In this section a paragraph states the differences (see below).

Changes: Included is a new section, including the paragraph: For PIG the single parameter roughness measure ξ was calculated by Rippin et al. 2011 from a RES data set generated in austral summer 2004/05 (Vaughan et al. 2006). It is the same data set the model geometry is based on (Sect. 2.3.5), still the roughness measure includes higher resolution information, as the derivation is based on along track sample spacing of the order of 30 m (cf. Rippin et al. 2011). Both data sets are then gridded with 1 km spacing.

R2: And this brings me to the main issue I have with the paper: If the bed of the numerical model is based on the same data set at these estimates of 'roughness', and since the numerical model calculates the effect of this bed on the flow, has the effect of the 'roughness' on the flow not already been modelled?

Answer: Although the model geometry and the roughness measure are based on the same data, they influence the flow behaviour on very different scales and with different mechanisms. A main key is here the different resolution, as mentioned by yourself. Maybe considering Weertman's original ideas can bring some clarification. He formulated a sliding law by combining the size and spacing of the basal obstacles into a roughness parameter, and relating it to the sliding velocity and basal shear stress. The small-scale roughness is thus included in a formulation influencing the large scale

sliding of the glacier. In his theoretical derivation he assumed a bed with evenly distributed obstacles. If we assume now, that the size and spacing of the obstacles is not uniformly distributed in the entire model region, a locally varying description of the roughness can be useful.

R2: The effects of the bed topography on the flow are calculated by the model. To fit surface data the basal slipperiness distribution is then optimized. This optimized basal slipperiness distribution turns out to be spatially non-uniform because bed topography alone does not produce the observed spatial variations in surface velocity. The inverted basal slipperiness, needed by the numerical model for it to reproduce measured surface data, is therefore not due to some variations in modelled bed topography. The slipperiness distribution is related to processes that are NOT accounted for by the modelled basal topography.

Answer: Yes, this is correct. Therefore again the interpretation of the basal roughness as a representation for a certain bed type is important. And the influences of the temperature and the basal water pressure have to be considered, which is done in the first approach.

R2: Since the 'roughness' estimates are based on the same topography data already included in the numerical model, then why would we expect these roughness estimates to give us added insight into the retrieved basal slipperiness distribution? This retrieved basal slipperiness distribution reflects aspects of the bed other than the geometry needed to fit the data (other than the geometry because it is already included). What these other aspects of the bed are is an open question (my guess is that they reflect spatial variations in till properties, basal water pressure, etc. etc.), but the point is that model does not need the spatial variations in basal slipperiness to mimic the effects of flow over its own bed geometry.

Answer: This we don't understand. The model results clearly show, that other factors are important, other than the glacier geometry, and thus bed shape, itself, to reproduce the surface flow structure. The question we are aiming to address is therefore what these other factors could be, how important they are and how they could be included in a formulation for basal sliding.

R2: I therefore don't fully understand why the authors try to relate inverted slipperiness with a roughness estimate of the bed they are already using in their model.

Answer: The roughness measure ξ we use does not directly represent the roughness as formulated in the original approach by Weertman. To use the roughness information anyhow, it is therefore necessary to translate the basal sliding parameter Cb to a meaningful range. For this the initial inversion step is needed.

R2: Now I'm open to the possibility that I may not have understood the paper correctly. If, for example, the basal roughness is estimated from a very high resolution (less than a fraction of ice thickness) area measurements of bed geometry, or if the resolution of the numerical model is not high enough to capture the known variations in basal topography on which the roughness estimates are based, then my criticism is invalid. But I do not know of any such high resolution measurements (measuring roughness along flight lines not aligned with flow and then interpolating between flight lines kilometres apart as done by Rippin et al is a futile exercise), and the resolution of the FE-mesh is clearly high enough to capture all spatial variations in existing compilations of PIG bed. I suggest giving the authors the chance to clarify the thinking behind their work and explain why comparing retrieved basal slipperiness with estimates of the 'roughness' of the bed, that are based on the same (or similar) data set as they are using in their numerical model, is an interesting and important scientific question.

Answer: Estimates of basal roughness help us to try and say something about bed conditions and bed variability, and how these influence flow. Additionally the formulation of basal sliding in glacier modelling is still one of the most difficult questions to address; therefore inversion for basal parameters is the most commonly chosen approach. This approach, on the other hand, does not lead to a better understanding of the processes

at the base. We therefore aim to combine information about the bed in a formulation of basal sliding.

We aim to reproduce the flow behaviour observed on a "real world" glacier and therefore understand the system better and make guesses about the underlying processes. This is different to assuming purely theoretical conditions and investigating specific processes, which can be clearly separated, like for example estimating the effect of roughness onto basal sliding. We believe that aiming to understand a "real world" glacier behaviour is an important scientific question to address.

R2: We should not forget that a sliding law is (to use an old phrase by Andrew Fowler) a matching condition between the inner and the outer flow. As such the sliding law represent processes not directly included or resolved by the model. For example processes happening on a spatial scale much smaller than those that can be resolved, or processes not included (regelation, cavitation, till deformation, etc. etc.). So for example in the old works by Nye, Kamb, Weertman, the focus was on how processes on small scales affect the bulk flow of ice. One of the questions was, for example, how one could replace a sinusoidal bed geometry with a flat one by changing the boundary conditions accordingly. Hence, the 'roughness' of the bed translates into a sliding law over another less rough bed. Comparing (inverted) sliding law parameters over a given bed with the roughness of the bed itself appears in this context questionable.

Answer: As the stress conditions in underlying substrate (hard bedrock or till) are not known, a jump condition for the momentum cannot be formulated. Instead a sliding relation often called a sliding law is required. We agree with the reviewer, that sliding relations that are applicable for this type of modelling study here, are approximating the sliding processes for a number of different reasons. First, these processes occur on a spatial scale much smaller that the model resolution. Second, not all the relevant processes themselves are implemented (e.g. heat transfer through the small obstacles required for regelation as in Weertman's analysis of sliding). In addition, very little is known about the bed conditions for most areas in Antarctica and Greenland, e.g. if there exists sediment or hard bed rock, how thick the sediment layer is, how much water there is in the sediment or in the space between the ground and the ice or how mobile/resident the water is? Due to the pioneering work of Nye, Kamb, Weertman (mentioned by the reviewer, and we would like to add Lliboutry), we have now a better understanding of processes that are or might be important for basal motion (sliding + bed deformation). But their work is based on theoretical analyses for processes on a spatial scale that is up to now not accessible for observations. One aim of our study is to bridge the gap between the processes on the sub-metre scale, the observed 'roughness' on the several-meter scale and the kilometre scale used in the numerical flow model. We think that every information available from observations should be used to constrain model parameters, if the processes must be parameterised for the stated reasons.

R2: p.s. There is an additional point I would like to make that is just a general statement and does not directly relate to the submitted work but might be worthwhile to consider. The roughness used in Bingham and Siegert 2009, Rippin et al. 2011 appears very different from the one used by Nye and others in the late 60s and early 70s. It is unclear to me what the mathematical relationship between basal roughness, as defined by Siegert and others, and sliding over smooth bed really is. Has it been proved that sliding velocity increases monotonically with increasing roughness? And if the 'roughness' increases by, for example, a factor of 2, how does that affect sliding velocity? Will it increase or decrease?

Answer: We do not think that Bingham, Siegert and Rippin aim at answering this question. They do not try to derive a mathematical relationship between the roughness parameter they derive and velocity. Additionally it seems to be acceptable to use the term 'roughness' to describe bed variability, as this is what they are investigating. They use it more as a qualification of bed type and make guesses about the long-term structure of glacier flow. However, this should be answered by the according authors.

However, the reviewer points to a number of interesting questions, like the one if the

sliding velocity increases with roughness. Results of the ISMIP-HOM experiment A, which simulates sliding over a bumpy bed (Pattyn et al., 2008) showed that the surface velocity decreases significantly with increased roughness - at least in the isothermal case. Our model results presented here agree with this finding. So far we cannot say how important strain-heating could be for a very rough bed and if this additional heat source could lead to a significant softer material at the base.

R2: I know that the expectation is that sliding velocity will decrease with increasing roughness, but that assumes roughness has been defined in a sensible way. I can't see anything in the Rippin or Bingham and Siegert papers to support this.

This may be a bit surprising statement on my behalf but even if one calls something roughness it does not mean that it is a useful or even a meaningful definition of roughness in terms of glacier motion. I suggest re-reading these papers on 'roughness' and while doing so replacing the word 'roughness' with some non-descriptive and less suggestive term. For example by replacing 'roughness' with 'hohu' (just some made up non-descriptive word). ' The question then becomes if and how 'hohu' affects basal sliding velocities. For 'hohu' to be a useful quantity this needs to be not only proven but quantified in detail as was done in the old works by Nye, Kamp, etc. (using a different definition of roughness) and then extended using various numerical and analytical methods by Fowler, Meyssonnier, Gudmundsson, Schoof, and Gagliardini, to name only a few. Unless this is done, there is no reason to expect the 'roughness' (or the hohu) as defined by Bingham and Siegert, and others, to be of any particular relevance to glacier flow.

Answer: The reviewer is right in expressing that the definition of roughness in the original sliding law by Weertman and the roughness we can observe today underneath ice sheets is on completely different horizontal and vertical scales. However, even decades after the first introduction of the sliding law and decades after technological development, there is no means to survey the roughness required for the sliding law on an adequate scale.

Last but not least, one has to keep in mind, that ice modelling requires a basal boundary condition for the momentum balance equation. As there is no information available on the stress inside the lithosphere and hence the jump condition does not lead any further, the sliding law serves as a dynamical boundary condition – in all ice sheet models, not only in ours. Using a sliding law is thus not only a pragmatic, but the only approach we can do today, although we hope that the next decade will lead to new insights of how to treat the basal boundary condition more realistic.

Therefore, we think that our treatment of the basal sliding law is an enhancement to many previous modelling studies, where often the parameters of the sliding law are simply tuned to match surface velocities. Wether we call the parameter roughness, local geometry variation or hohu doesn't really matter – although we think that the link between ξ and the term roughness is not too bad. To deal with the terminology roughness adequately, one would need to treat it scale dependent.

Article (ISMIPHOM_2008) Pattyn, F.; Perichon, L.; Aschwanden, A.; Breuer, B.; de Smedt, B.; Gagliardini, O.; Gudmundsson, G. H.; Hindmarsh, R. C. A.; Hubbard, A.; Johnson, J. V.; Kleiner, T.; Konovalov, Y.; Martin, C.; Payne, A. J.; Pollard, D.; Price, S.; Rückamp, M.; Saito, F.; Souček, O.; Sugiyama, S. & Zwinger, T. Benchmark experiments for higher-order and full-Stokes ice sheet models (ISMIP–HOM) The Cryosphere, 2008, 2, 95-108

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