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

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.

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?

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.

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.

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.

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.

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.

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? 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.