## *Interactive comment on* "Basal sliding of temperate basal ice on a rough, hard bed: pressure melting, creep mechanisms and implications for ice streaming" *by* M. Krabbendam

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This is a very interesting paper on a topic of great importance: sliding is a dominant form of ice motion in many parts of glaciers and ice sheets but its parameterization remains poorly constrained which leads to lots of uncertainties in prognostic models, probably the largest uncertainty in glacier flow models. Having just read the review by Maurine Montagnat I will not comment on the rheology of basal ice. All was said there.

First let me state that I feel this article is extremely relevant and important because sliding continues to be understudied in glaciology despite everyone agreeing that it is a major source of uncertainty. Linked to sliding, there are also clear evidences that basal ice is physically different from bulk ice, displaying different mechanical behavior. Yet this

difference is not included in numerical models of glaciers and ice sheets. The problem with sliding, I think, is two-fold: (1) there is a lack of data making it hard to have a comprehensive view of sliding processes and their relative importance. This also makes it difficult to build a coherent verifiable mathematical model; (2) ice-sheet modelers usually perform diagnostic models of present-day ice sheets/glaciers (what the author calls "near steady-state situations") using inverse models to obtain the slip coefficient (or the "friction parameter") of the sliding law neglecting the physics of sliding and the dependence of that coefficient on basal conditions (water, debris, basal topography, etc). Attention has thus shifted to a fitting problem rather than to the physics. I fully agree with the author that inverse models are not appropriate for long-term diagnostic simulations and for palaeo-glacier modeling. To perform such simulations, one needs a better understanding of basal processes and a better mathematical description of sliding based on physics.

The paper is constructed around the sliding theory of Weertman of 1957 arguing that Weerman's model of sliding is invalid because it wrongly scales pressure melting with obstacle length instead of obstacle height, and it uses power-law creep with an exponent of 3 instead of grain boundary sliding with a smaller exponent and with much higher strain rates. The author then argues that his modified conceptual model of sliding could explain the fast sliding observed in parts of bedrock-dominated ice sheets (order 100 m/year). First I am not convinced by this approach which tries to deconstruct Weertman's theory. Lots of new data have been acquired since 1957 and one cannot blame Weerman's for not knowing them. Weertman was a precursor of sliding theory and his first model clearly represents an extreme simplification of the process. Yet it captured some very essential components. In my opinion some parts of the text come out too strongly as a critic of Weertman's work. Second, I am also not convinced by one of the main point of the discussion, that pressure melting, even an enhanced form, has any significant impact on ice-sheet sliding. Weertman's calculations indicate a speed of about 0.01 m/year or less for pressure melting for a 1 meter obstacle. Even using an enhancement factor of 100 this mode of sliding is negligible (see below).

Rheology, as discussed in the paper, can have a much greater impact on sliding and this is clear from the data. Also as discussed in the paper, the basal thermal regime and the temperate ice layer, including the effects of basal meltwater and friction, water fluxes, surface water influx, are key elements to properly quantify sliding, and, as correctly pointed out, are not often taken into account. I find Figure 6 very useful as a conceptual model of what is going on at the bed.

Regarding the pressure melting mechanism, using the author's numbers in Section 2 and the appendix, I come up with a pressure-melting velocity Vpm, Equation 4, of 1.5 cm/year (Equation 4 should read as Q/(rho H)). This is completely negligible when talking about ice sheets that move ca 100 m/year. As stated by Weertman in 1957, pressure melting is unimportant for "large" bumps. One meter here is already a large bump. Given these numbers, I am not sure of the relevance of pressure melting for sliding. Whether the scaling is length or height (Section 5) the numbers will remain extremely small. I agree, however, that height is probably a better scaling than length.

Again in Section 6 I am not sure how pressure melting can explain the increase in sliding due to surface melt water input given its negligible effect (cm per year). Even increasing stoss side pressure by a factor of 10 would not help explaining the high sliding speeds observed underneath ice streams. I think surface water is an important issue and has an impact on sliding. I just don't see how the arguments presented here for enhanced pressure melting can explain the observed effects. May be I missed something in which case quantifying this effect in this section would be helpful. The same argument goes for Section 8.1 regarding the rate-controlling mechanisms: numbers are necessary to illustrate the importance of stoss side pressure melting. In my opinion, for bumps of the size of 1 m or greater, this has no effect. Other thermal effects discussed in the paper, like debris-bed friction, basal melt, viscous heating, surface meltwater influx and drainage, are more important and control, in some way, the sliding speed and the temperate ice layer.

Overall I think the paper brings a new and interesting point of view of glacier sliding. It

clearly explains what the problems are with sliding as it is used in glacier models today. The paper is overly focused on Weertman's theory and it lacks first-order estimation of some of the basic controlling mechanisms.

Specific comments

- Page 1 last line: Probably "near steady-state situations" should be replaced with something like "near-instantaneous situations". Clearly most of the ice sheets/glaciers where this inverse model is being used are not in a steady state.

- Page 2 Line 10. I think the author meant to refer to Cohen et al 2005 and not Cohen et al 2006. That reference is actually missing from the bibliography. Cohen et al 2000 in the bibliography is not cited in the text. The 2005 reference is: Cohen, D., N. R. Iverson, T. S. Hooyer, U. H. Fischer, M. Jackson, and P. L. Moore (2005), Debris-bed friction of hard-bedded glaciers, J. Geophys. Res., 110, F02007, doi:10.1029/2004JF000228.

- Page 2 Line 24. I would insert the word "either" before "pose problems"

- Page 3 Line 20. I think the mechanisms of sliding are clear: ice at the melting temperature contains water and water between ice and bedrock forms a thin lubricating layer with near zero shear resistance that allows ice in contact with the bedrock to have non-zero velocities. The question is how to quantify sliding and what glaciological parameters control it.

- Page 4 Line 20 Equation 4. There is an typo/error. It should be: $Vpm = Q\_obs/(H\_ice * rho\_ice)$.

- Page 4 Line 24. Strictly speaking in regelation ice does not flow around the obstacle. That's the viscous part of motion. In regelation ice melts on one side, the water flows to the other side and refreezes there. May be change wording.

-Page 5 Line 2. The vertical stress could even be higher than the effective pressure since, due to melting, there is a component of ice flow towards the bed that creates a vertical downward force on the debris. This force could be significant and further

increase basal friction. See Hallet 1979, 1981, and Cohen et al 2005. Hallet, B. (1979), A theoretical model of glacial abrasion, J. Glaciol., 23, 39 – 50. Hallet, B. (1981), Glacial abrasion and sliding: Their dependence on the debris concentration in basal ice, Ann. Glaciol., 2, 23 – 28.

- Page 6 Line 11 (ii). Strictly speaking this is not true. There will be differences in temperature in temperate ice due to differences in stresses (if only with depth). These temperature differences will cause thermal gradients and heat fluxes (arguably small). These gradients will only serve to melt ice or freeze water. See Lliboutry 1993.

- Page 7 Line 10. The use of the words 'cold patch' is confusing. The ice is at the melting temperature so it's not cold. I think the term cold patch should be restricted to cold ice not ice at the melting temperature that is colder because under a higher pressure. See also Figure 1.