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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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I am sympathetic to the motivation for this paper: Weertman sliding is indeed flawed in important ways, and heat transferred by subglacial water flow and produced by debris-bed friction may affect sliding physics. More broadly, the problem of how glaciers slide rapidly over hard, rough beds is an important one. This paper, however, has serious deficiencies:

1) Most importantly, it contains little new analysis or data that help shed light on sliding physics. For example, the calculation of p. 4 yields conclusions that could have been reached without the calculation (see below, comments on p. 4, 1-21) and is used inappropriately to assert that the Weertman model is "illogical" (p. 4, 23-26). Too much of the paper consists of inferences not supported by data or relevant formal analysis.

2) Misconceptions/errors indicate a muddled understanding of relevant physics related to sliding (e.g., p. 7, 5-7; p. 7, 8-11; p. 8, 26; p. 10, 26-27; p. 11, 19) and ice rheology (e.g., p. 9, 8-15). The attempt to assess the extent to which temperate ice obeys a power-law flow rule by considering the dependence of strain rate on temperature, rather than on stress, is a particularly major error.

3) References are used inappropriately to support conclusions (p. 9, 8-15; p. 9, 8-23).

4) Inadequate justification is provided for some of the paper's assumptions (p. 5, 2; p.7, 5-7; p. 8, 1-4; p. 8, 5)

5) The introduction would benefit from substantial revision. The attempt to motivate the subject of this paper (i.e., sliding mechanics) with bullets 1-5 is not successful (see my comments below on p. 2-3).

Specific comments keyed to page and line numbers:

p. 1, 14-15. "Thermal equilibrium" is vague. There is "thermal equilibrium" in Weertman's model. The intended meaning needs to be clarified.

p. 1, 18. Power Law Creep should not be capitalized.

p. 2, 13-15. "The essence of Weertman's sliding model is that basal ice movement past an obstacle is controlled either by stoss-side pressure melting around the obstacle or by ductile flow enhanced by stress concentrations near the obstacle, whichever is the fastest." This wording is a bit misleading. It suggests that either pressure-melting or ductile flow occurs, when regardless of bump size, there will always be components of both, albeit with one more important than the other (except for the transition bump size for which they equally important).

p. 2. 25-29. It is not clear why fast ductile flow and soft ice somehow contradict Glen-type power law flow (i.e., the nonlinear dependence of strain rate on stress).

p. 2-3. 30-4. Although these are valid observations, the author needs to be more explicit about how they contradict pressure melting or a power-law ice rheology.

p. 3 12-18. These comments on basal thermal regime and basal hydrology in Greenland come as a surprise because there is no allusion to Greenland earlier in the introduction. Such an allusion is necessary because temperate ice of thicknesses much larger than bump size and water access to the glacier bed are, of course, normal for temperature glaciers. If sliding OF THE GREENLAND ICE SHEET, is how the author wants to motivate this paper, then that should be made clearer at the beginning of the introduction.

p. 3. Omit "worked" before "example" here and elsewhere.

p. 4. 13. "was seen" indicates that this was actually observed. Express this differently here and where used elsewhere.

p. 4 "is then", earlier "was seen". Here and elsewhere the author tends to change tense in midstream.

p. 4. 1-21. The reason for this calculation is unclear. The points made after it–that cavity formation can impede heat transport and that regelation speed decreases linearly with bump length–could be made without presenting the calculation. Even if there were better motivation for presenting this calculation, the source of numerical values, such as those for ice thickness and bed shear stress, is unclear (not in the text or appendix but presumably from somewhere in Greenland) and the choice of bump size and spacing is seemingly arbitrary.

p. 4, 23-26. The author concludes at the end of the calculation: "This implies that ice flowing around an obstacle that is, say, four times longer than another obstacle (Fig. 1b), would be four times slower, even though this obstacle is more streamlined (having a longer aspect ratio). This result is illogical, contradicts most observed geomorphology (Stokes and Clark, 1999; Bradwell et al., 2008), and is a major weakness of the Weertman model." This decrease in speed with elongation is a major weakness of

the Weertman model ONLY if one neglects viscous flow in the Weertman model, as the author does here. And how can the author consider only pressure melting and refreezing–most relevant to bumps less than 0.5 m in wavelength–and assume that the calculation has relevance to the much larger landforms considered by geographers like Stokes and Clark? The Weertman model is indeed flawed, but this calculation adds nothing new to the subject, and the verbiage toward the end of the paragraph is misleading.

p. 5, 2. Explain why this approach is valid. In Hallet's abrasion model (1979; 1981) for example, debris-bed friction is independent of normal stress (and effective normal stress) and instead depends on the rate of ice convergence with the bed, so the assumption made here requires justification. Even for a flat bed, ice will converge with it due to basal melting, and that process exerts a downward drag on clasts, increasing friction between them and the bed. I think equation 5 can, in fact, be justified, but the necessary justification is not provided here.

p. 6. 11. A temperate ice layer, in fact, has a thermal gradient–one that reflects the decrease in melting temperature with pressure, as pictured in Figure 1 of this paper. Rewrite.

p. 6. This page-long digression ("Intermeezzo") distracts from the theme of the paper and will leave readers wondering what this paper is supposed to be about.

p. 6 34. This is a melt rate rather than a flux, which begs the question why melt rate was not expressed in Equation 6 with these units, as it normally is, either by defining the heat of fusion volumetrically or including density.

p. 7. 5-7. The assertion here that heat flow thorugh advection by flowing water is "more efficient" than heat flow due to thermal gradients in rock may be correct but is not demonstrated here or later in this paragraph. Also, if some of the frictional and geothermal heat is advected by water, why doesn't equation 6 reflect that? It should have a heat sink term in it associated with advection by flowing water.

p. 7. 8-11. The physics here is muddled. The ice temperature will be pinned everywhere along the bump surface at values set by the distribution of ice pressure (unless the temperature of the water in the film that divides ice from rock is not in equilibrium with the ice temperature, which seems unlikely). Although the water flow will cause some extra melting, if lee-side cavities do not form, the thermal gradient will be set by the pressure deviation from hydrostatic on both the stoss and lee sides of the bump. For a reason I don't understand, the author is assuming pressure is only important on the upstream side (see Figure 1c also). Also the word "cold patch", as used in the classic paper by Robin (1976) and by subsequent textbook writers (e.g. Hooke), describes ice below the pressure melting temperature (PMT). To use it in this context, where all ice is at the PMT, is thus confusing.

p. 8, 1-4. Debris-bed friction can be affected by water flux to the bed only if water can gain entry to zones where there may be small cavities beneath debris particles. This requires moving water from the channel at the point of entry to the bed out through the thin film that divides debris-bearing ice from bedrock. This propagation of pressure will be diffusive and slow, so it is unclear how much a sudden increase in warm meltwater will really decrease effective pressure and thereby reduce frictional drag.

p. 8, 5. The thermal gradient towards stoss surfaces depends not only on the temperature of the incoming warm water but on the distance between it and the stoss surfaces of bumps. Because the distances between channels carrying water and stoss surfaces is poorly known and could be far larger than the distance across a bedrock bump, the importance of this effect for stoss-side melting is uncertain, contrary to the certitude of the statement made here. Perhaps the author is assuming that all of the warm water in a Das-like event moves in the thin film that divides ice from rock. If so, that is a dubious assumption.

p. 8, 26. "Weertman (1957) assumed that the creep component of ice flowing around a hard obstacle worked with a rheology" according to 'Glen's Flow law', albeit enhanced by stress concentration on the stoss side." Again here, the author makes the error

of assuming deviatoric stresses are concentrated only on the stoss sides of bumps in Weertman's theory. Rather, deviatoric stresses are symmetric across bumps in the theory, with lee-side deviatoric stresses equal to but opposite in sign of those on stoss surfaces.

p. 8, 27. "strain-rate" rather than "strain".

p. 8, 28. Power Law should not be capitalized, here and elsewhere.

p. 9, 3. "comparisons" "suggests" Correct subject-verb correspondence.

p. 9, 9. In fig. 3 strain rate is plotted, not strain as reported here.

p. 9, 8-15. Here the author asserts that if the log of strain rate plots as a straight line against the reciprocal of temperature, then power law creep is indicated. He needs to be aware that power-law creep is defined on the basis of the relationship between stress and strain rate, rather than between stress and temperature. Note that Morgan (1991) (the source of the data reproduced here) never commented on whether his data conformed to power law creep rules because all of his tests were done at a single stress (0.1 MPa).

p. 9, 8-23. This point of the paragraph–that temperate ice obeys a near Newtonian flow rule–could conceivably be correct, given the relative lack of work on warm ice, but is not convincingly argued here. Other authors cited, such as Byers et al (2012) and Chandler et al (2008), did indeed suggest values of n near 1.0 but were careful to attribute those low values to low deviatoric stresses, for which there is some micromechanical justification for low n. For ice near glacier beds, however, and particularly near bumps, deviatoric stresses will tend to be high, and thus justification for low values of n is weak.

p. 9, 31. Why does the abbreviation, GBPS, not coincide to the first letters of "grain boundary pressure melting"? And why choose a new term here when there is ample discussion of this sort of deformation in the literature, much of which is not cited here? Overall, I am left with the impression that the author does not have sufficient familiarity with the ice rheology literature.

p. 10, 26-27. “In summary, ice flow around a bedrock obstacle in temperate ice is constrained either by stoss-side pressure melting or by enhanced creep.” Again this is misleading. Any obstacle is accommodated by both mechanisms, although one can dominate the other depending on bump size.

p. 10, 29-32 & p. 11, 1-4. None of these bullets, or the following assertion, has been demonstrated in this paper.

p. 10. 5-9. The conclusion here that temperature ice does not obey a power-law rheology has not been demonstrated in this paper.

p. 11, 19. The idea that no sliding occurs at subfreezing temps is not strictly correct. See Shreve 1984 J Glaciol.; Cuffey et al 1999, GRL.

Section 8.2. This is an interesting story but no aspect of it has been demonstrated in this paper.

p. 12, 8-9. “The corollary of the processes described herein is that if a thick temperate layer is present, basal motion over a hard bed with bedrock humps provides less drag than previously thought.” This is misleading. The fact that traditional sliding theories, including Weertman’s, under-predict rates of glacier sliding and over-predict drag has been discussed for many decades, and is well described in the leading reference book by Cuffey and Paterson (2010), for example. Under-emphasized in this paper is the role of cavity formation in reducing basal drag (e.g., Schoof, 2005).

p. 12, 11. “weaker bulk rheology”? I can see how ice can be “weak”, but I don’t understand how the relationship between stress and strain rate (rheology) can be “weak”.

Section 8.3. The problem of fast flow on a hard bed is indeed important and may certainly involve soft ice and the mechanical and thermal effects of water flow under glaciers. The problem is that this paper does not provide new analyses (or data) that convincingly bear on the issue, so these comments on ice streaming come off as spec-

ulative and poorly motivated.

Conclusions. Not convincingly supported.

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