Reply to interactive comment by Anonymous Referee #3

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

Reviewer comments in black, reply in red.

Dear anonymous reviewer.
Thank you for the review.
Please note: below my detailed reply is a:

- Revised introduction (draft)

- Revised section on the "Creep Component in Temperate ice" (draft)

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 debrisbed friction may affect sliding physics. More broadly, the problem of how glaciers slide rapidly over hard, rough beds is an important one.

Thank you. I wish to stress that this paper is conceptual in character, and arguably poses more questions and hypotheses than it answers, but these are questions that need be answered and hypotheses to be tested if the science is to move forward. This is now emphasised in the revised introduction.

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).

MK: I beg to differ. It contains an important element of consideration concerning frictional heat production, which is widely ignored; it reviews what little is known about temperate ice, and questions, with good reason, the use of Glen's flow law for such ice; it proposes a different way of heat transfer for pressure melting, and hence suggests that ice sheet modelling need to take into account 3 rather than 2 thermo-mechanical modes of sliding. As the reviewer does not state that this has been done before, these analyses (conceptual and qualitative as they are) are new.

and is used inappropriately to assert that the Weertman model is "illogical" (p. 4, 23-26).
MK: Fair point: I will rephrase this as: "This result contradicts most observed geomorphology (Stokes and Clark, 1999; Bradwell et al., 2008) and supports the notion that pressure melting is not important for large obstacles".

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).
MK: some poor phrasing on my part, but I reject the term 'muddled understanding of relevant physics':

- p. 7, 5-7; p. 7, 8-11. No, this is entirely logical . The question is whether water flow is sufficient – and that needs observations to confirm – see below.
- p. 8, 26. Merely poor phrasing on my part. Will change to: "albeit enhanced by stress concentration near the obstacle".
- p. 10, 26-27; Merely poor phrasing on my part, see below. :
- p. 11, 19: Can't think of what is muddled here, unless the reviewer refers to the negligible (negligible!!!) contribution of cold-based sliding – see further below. The remainder is mainstream, accepted glaciology.
- p. 9, 8-15 I will change this section, as per comments to M Montagnat (reviewer)

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.

MK: Not it is not. The reviewer needs to realise that the Arrhenius relation is part and parcel of a power law in most if not all materials, and certainly ice (Glen 1955). Nevertheless, I've probably phrased this too strongly too early on, but the reviewer also misses the point that later the stress dependency is dealt with, with good evidence in some circumstances for n~1. I've now restated that the Arrhenius temperature relationship does not work anymore, but also provided more examples of experiments/observations that show that n < 3, i.e. a departure from the 'normally assumed' n=3. The main point is that temperate ice behaves fundamentally different from cold ice. With the sudden strain rate increase at -0.2°C AND evidence for n~1 behaviour, I feel this is justified. It is unreasonable to change the Activation energy by some 'fudge factor' if there's a fundamental and significant change in rheological behaviour near the melting temperature. Thus, temperate ice does not follow a standard power-law creep behaviour, and it would be irresponsible to claim that it does if there's evidence to the contrary. See new section on 'Creep component' at the end of this reply.

3) References are used inappropriately to support conclusions (p. 9, 8-15; p. 9, 8-23).
MK: p. 9, 8-15 I have strengthened this, and provided more references that show that the stress dependency in many cases is close to 1. I do not see how this is inappropriate. See new section on 'Creep component' at the end of this reply.
p. 9, 8-23: I do not understand what the reviewer refers to, but if so probably covered in the new section on 'Creep component' at the end of this reply..

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)
MK: p. 5, 2 . "The normal vertical stress σnv can be taken as the effective pressure" Explained in detail below

MK: p.7,5-7; "If sufficient water is flowing through the system, heat advection by flowing water will be much more efficient than heat conduction through rock or ice": As such this is true, but the question is whether water flow is sufficient – fair point, will explain better. I will rephrase – see more below.

MK p. 8, 1-4; *" Increase of basal water pressure Pw, resulting in a drop in effective pressure Pe , lowering the friction on flat surfaces. Frictional heating and drag on the flats will drop, as long as Pw remains high. Because there is less drag on the flat surfaces, the normal stress σn stoss onto the stoss side of obstacles, however, increases (also temporarily), enhancing stoss-side melting and basal melting"*. These are simple physic/mechanical principles, frankly. But I will change the last part into: "… enhancing both stoss-side melting as well as enhanced creep".
MK: p. 8, 5. Explained in detail below

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).
MK: Fair point, in a sense the paper explores conceptually the potential effects of a temperate layer of significant thickness below cold ice in fast flowing ice, rather than just focussing on 'Weertman Sliding' : then the bullet points are appropriate as they show both the existence of that layer and the relevance to real world glaciological and geomorphological problems.
A draft of e new a revised Introduction is at the end of this reply.

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.
MK: Oh dear. Weertman's model relies on a thermal gradient between the stoss and the lee-side, so there is no thermal equilibrium. Muddled thermodynamics, I'm afraid. Thermal equilibrium has a very clear meaning, i.e. having a uniform temperature distribution. (I do of course state later on there is thermal gradient near the stoss-side only, but this is the abstract, so 'near-thermal equilibrium' is a good enough). No reason to change.

MAIN TEXT
p. 1, 18. Power Law Creep should not be capitalized.   MK. Fair point. It may need a hyphen: 'power-law creep'.

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).
MK: To be replaced by: "The essence of Weertman's sliding model is that basal ice movement past an obstacle occurs by stoss-side pressure melting around the obstacle and by ductile flow enhanced by stress concentrations near the obstacle, with ductile flow being more important with larger obstacle size."

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).

MK fair point, although in the end it is true (because temperate ice does not behave according to classic Glen's flow law, with both a breakdown of the Arrhenius relationship AND a departure from n=3). , but this is explained – see also 2 points below. Arguably, the paper is more about the effects of a temperate ice layer on sliding and deformation in fast flowing ice on hard, rough beds (e.g.hard-bedded ice streams), which includes the effects on the sliding mechanism. I will change the Introduction accordingly – see Revised Introduction at the end of the reply.

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.
MK see above point, I will rephrase the introduction. Revised Introduction at the end of the reply

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.
MK see above point, I will rephrase the introduction. Revised Introduction at the end of the reply

p. 3. Omit "worked" before "example" here and elsewhere.
MK: OK.

p. 4. 13. "was seen" indicates that this was actually observed. Express this differently here and where used elsewhere.   MK: changed to 'was regarded'

p. 4 "is then", earlier "was seen". Here and elsewhere the author tends to change tense in midstream.
MK: bearing in mind my limited knowledge of English grammar, I feel that in this case it is justified: X was regarded to be a function of Y; this means that A, B. But I'll check the tenses.

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.
MK: I see your point. The main point is to show that the thermal gradient set up through the obstacle is very small, so that other heat transport mechanisms are likely to be more efficient and hence more important. But I will explain this better.

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.

MK Fair point. Will rephrase as follows: "This result contradicts most observed geomorphology (Stokes and Clark, 1999; Bradwell et al., 2008) and supports the notion that pressure melting is not dominant for large obstacles".

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.

MK. What I've taken are the bulk friction coefficients from experiments (lab and subglacial) and which represent friction coefficients averaged over an area (eg. Budd et al, 1979; Zoet et al. 2013, Cohen et al. 2005) and then applied standard Coulomb friction, rather than the theoretical approach of Hallet, which depends on the contact friction coefficient at the (sparse) debris/bed contact points. Hallet's model is somewhat different, as it looks at abrasion by clast-bed wear rather than friction (pure ice over bedrock gives friction but negligible abrasion of the bedrock). Therefore, Hallet focussed on the clast-bed contact forces, and these are indeed in his model dependent on ice convergence to the bed rather than the normal stress. Note that the friction coefficient used in the Hallet model is the rock-rock friction coefficient (in the order of mu = 0.5-0.6) at the debris-bed contact spot (which is likely to be quite small), rather than the bulk ice / bed friction coefficient that results from the experiments

Note further that Budd et al. (1979) experiments were for clean ice, so a standard Coulomb friction law is applicable. Only Budd et al. (1979) varied the normal stress, so the other experimental work cannot be used to validate/invalidate the notion that normal stress has no effect. Budd et al. (1979) reported a c. 6 x increase in wear at constant sliding velocity associated with a doubling of the normal stress, suggesting that normal stress *does* play a role.   Possibly, the disparity between Hallet's theory and the empirical observations arises because if debris is present, some of it will be in contact with the bed regardless of the convergence rate (if only due to negative buoyancy forces) combined with the effect that in fast moving ice, the sliding rate is orders of magnitude greater than the convergence rate. A further possibility is that the contact force of a clast is a combination of the Hallet model (independent of normal stress) and the Boulton (1974) model (proportional to normal stress).

I will add a couple of sentences summarising the above, to better justify the standard Coulomb friction model.

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.

MK: rewritten to: "a temperate ice layer has no bulk thermal gradient (it has arguably a very small negative gradient, but this is ignored here), so no heat can be conducted through it; it forms a near-ideal thermal barrier (e.g. Aschwanden and Blatter, 2005)". Key here is that the normal increase in temperature cannot be sustained by conduction alone, the slope of the gradient (compared to the geothermal gradient below and the effective gradient (sustained by advection) above) is miniscule and opposite. This should now be obvious. (For information: the temperature gradient is c. 0.0007 °C/m or 0.7 °C/km)

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

MK: Fair point. I will move this forward, to explain more about how a thick temperate layer can grow and be maintained. This is a serious problem, but by dealing with this first, it is not an intermezzo anymore. I plan to reorganise the revised MS as follows (see also my reply to D Cohen's comments:
1 Introduction
2 Basal meltwater production by frictional sliding
3 Growing and maintaining a temperate ice layer
4 The creep component in temperate ice (this will be expanded to take care of M Montagnat's comments)
5 The pressure melting component
6 Stoss-side pressure melting in temperate ice
7 Effect of surface water input on temperate ice on a rough bed
8 Critical obstacle size
9 Discussion
10 Conclusions

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.

MK: 1) No, it is not the melt rate, although the melt rate puts an upper bound on to the flux. However, it is possible that some/much of the meltwater produced by frictional heating at the base is NOT percolated through the temperate layer towards the CTB, but flows towards the terminus and (eventually) escapes through a subglacial drainage system. So there's a difference between meltwater production (melt rate) and water flux through the temperate layer
2) the unit point is valid, and I will rejig equation (6) in SI units.

p. 7. 5-7. **(A)** The assertion here that heat flow through 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. **(B)** 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.

> (A) Fair point, see reply to next point.
> (B) Equation (6) describes the melting rate of over a very large area, whereas the advection here merely transports (potentially) heat from one spot to another (from bump to bump) within the larger overall system. Similar small thermal gradient will also for instance occur at the high-friction clast/bed contacts, which may locally and possibly short-lived have high temperatures above the melting point on the very small (mm^2) contact areas….

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).

MK: Mmmm, the key here is a) we're in a net-melting environment, with excess water and hence high Pw; b) cavities may occur; c) therefore no net-negative deviatoric stress can be set up on the lee-side and the pressure there will be controlled by Pw, rather than the deviatoric stress. In that case (deviatoric) stress (not pressure!) is indeed only important at the stoss-side. I will reword to:

"In our conceptual model the temperate layer is thicker than the height of the obstacle. Water is continually produced by frictional heating, there is a net-melting environment with an excess of water, and water pressure on the ice-bed contact will be high. Water likely flows in a film and/or small gaps between bumps and obstacles, and cavities filled with water are likely to form. If sufficient water is flowing through the system, heat advection by flowing water can be as efficient if not more so than heat conduction through rock or ice. Consequently, no significant thermal gradients can build up and the entire basal system (temperate ice, water, and top rock) is held at *Tm*. "

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.

MK: Not at all: *"The only exception (to PMT) is the stoss-side of a bedrock obstacle, where the melting temperature is continually depressed as a result of the concentrated deviatoric stress acting onto it"* this is the same as the use of Robin (1976), and is perfectly clear and not confusing at all. The other reviewer suggested to think of a different term, as cold patches can be taken to mean frozen patches in a polythermal situation

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.

MK: This is the increase in Pw that occurs locally near to the point to where supraglacial lake drainage events occur. It has been observed that this can lead to minor but measurable local uplift (das et al. 2008; Hoffman et al. 2011), suggesting that, despite what the reviewer asserts, Pw increases do occur and quite rapidly so, although they maybe localised and short-lived.

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. (B) 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.
MK: I do not assume this (all of the warm water in a Das-like event moves in the thin film), but the reviewer assumes that all water in a 'Das-like' event will flow through channels, which is even more unlikely: it is clear from the scale of these events, and the measured vertical uplift that the subglacial channel system cannot carry all the water away, and thus some will develop a film.   I agree that the effect and importance are poorly known, and will put a caveat in.

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.
MK: will rephrase as: " …. albeit enhanced by stress concentration around the obstacles"

p. 8, 27. "strain-rate" rather than "strain".
MK: It reads: "strain rate". I'm not sure if these needs a hyphen, it is not the case that two nouns modify a third. No need to change.

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

p. 9, 3. "comparisons" "suggests" Correct subject-verb correspondence.
MK: good point, corrected.

p. 9, 9. In fig. 3 strain rate is plotted, not strain as reported here.
MK: It reads: "strain rate", not clear what the reviewer refers to.

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).

MK: I will reword this into: "Thus, at constant stress, ice above c. -0.2°C shows a sudden weakening of a factor 5 to 10, and the Arrhenius relation of equation (7) clearly does not describe this, potentially implying that Power Law Creep is not dominant close to the melting temperature (see also Barnes et al. 1971; Colbeck and Evans, 1973; Morgan, 1991). "

The reviewer needs to be aware that the Arrhenius relation is part & parcel of the power law (certainly for ice) , so that if the Arrhenius relation breaks down suddenly it is only correct to start questioning power law. Clearly something is happening…

I then have provided more evidence from the literature that the stress dependency is NOT 3, but in many cases is close to n = 1. Thus, power-law creep 'breaks down', but I've phrased this differently (although any metallurgist would agree with me..).

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.

MK: I have rewritten this, and added more evidence from the literature. The reviewer has a point with the low-stress / high stress, and this is now described. However, there is ample evidence that the stress dependency in both experiments and in nature is NOT the 'standard' n=3 that is usually assumed. Note that Chandler's work IS on a real glacier. The main point is that glacial modellers should not assume n = 3 in temperate ice, if there's no evidence. See revised section on the "Creep Component in Temperate ice"

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.

MK: I have rewritten this, with more emphasis on dislocation creep & recrystallisation enhanced by the presence of melt, discussed the potential role of Grain Boundary Sliding, and used the more traditional term 'grain boundary melting' / 'internal pressure melting'. There is again much evidence for partial melting in deforming temperate ice, and I've strengthened this. As it is unclear as to which mechanism is dominant (I admit I was pushing too hard for a single mechanism), I use the term 'melt-assisted creep' as a bucket term. The problem is that, to my knowledge, no-one has studied the stress-strain behaviour AND the microstructures, which can tell us about the deformation mechanisms. See revised section on the "Creep Component in Temperate ice"

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.

MK: not so much misleading, as poorly phrased. " Ice flow around a bedrock obstacle in temperate ice is accommodated by stoss-side pressure melting or by enhanced creep, with creep being more important for larger obstacles. In temperate ice, the creep component is….[}. And the pressure melting is [….]".."

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

MK: Note: " it is _proposed_ here that stoss side pressure melting is constrained by ….". Given the preceding matter, these is a very reasonable propositions to make, that is potentially testable. It is clear from the English that I oppose a hypothesis here, rather than demonstrate or assert.

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

MK: It is now convincingly shown that temperate ice cannot be responsibly modelled according to a standard power-law rheology with the standard n=3. What it should be modelled as maybe unclear, but part of the point of this paper is to point out where and why temperate ice is important, but that there are large gaps in the knowledge of that material. It appears to me that the reviewer thinks that temperate ice behaves the same as cold ice, and can be modelled in the same way. This is clearly not true.   Power law cannot be regarded as operating in temperate ice.

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.

MK: that maybe true, especially near cold/warm boundaries. However, both these papers admit that compared to normal, let alone fast warm-based sliding velocities, the sliding is 'negligible', and therefore as an overall approximation, the scale of which is clear here, it is correct.

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

MK: The first part is mainstream glaciology, the parts of 3) and 4) follow logically from the paper. A dismissive comment, that is not very useful for an author to act upon. However, I will rephrase the introductory line as: " In such a model, three thermomechanical basal regimes may thus occur, with a potential fourth operating seasonally".

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).

MK: I assume that the reviewer refers to the phrase 'previously thought'; if so, fair point. Rephrased as: "The corollary of the processes described herein is that basal motion of temperate ice over a hard bed with bedrock humps provides considerably less drag than if the ice is modelled with cold ice properties".

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".

MK: Rheology is the general study of plastic/viscous deformation, so has a rather wider meaning than merely the stress / strain rate relation – it includes such important things as temperature for instance. In some disciplines, this is then (possibly erroneously) used in term of 'rheological behaviour' and hence the 'rheology of material X'. I will rephrase this as: "Instead they suggest a different, weaker bulk rheological behaviour"

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.

MK: exactly, that's part of the point of the paper

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 speculative and poorly motivated.

MK: I stated right in the introduction that this paper is conceptual in character. It does provide new concepts that do bear on the issue, because, to my knowledge, no-one has invoked a thick temperate layer as an explanation for the NEGIS, nor an explanation for how palaeo-ice streams with poor topographic steering operated during the Pleistocene. As I understand, the fast flow of the NEGIS is explained either by very high geothermal heat flow that is geologically unreasonable, and for which the presented geophysical evidence (gravity, aeromag) is unconvincing, or by the presence of soft sediment, which is at odds with the rough nature of the bed, as well as with the observations that hard gneiss in deglaciated areas is normally free of till where significant ice sliding has occurred during Pleistocene (see also my Reply to O Eisen). Thus, there is currently no satisfactory explanation for the observed fast ice flow in the NEGIS. Yes, the existence of a thick temperate layer is speculative, but so are all previous explanations put forward. Science cannot move forward if hypotheses cannot be proposed: as long as they are testable. This hypothesis is eminently testable (by drilling or doing radar echo sounding with an appropriate frequency): whether it is correct, time will tell. I feel it is justified to suggest the possibility of the presence of a thick layer of temperate ice at the base of the NEGIS. Speculative: yes, but in a responsible way of posing a testable hypothesis; poorly motivated: no, quite the opposite.

Conclusions. Not convincingly supported.

MK: How interesting that the two other reviewers did not pick this up. I cannot escape the impression that the anonymous reviewer is a bit dismissive of alternative approaches and just 'does not like it. It would be a bit more helpful if the reviewer had indicated in a bit more detail which of the conclusions are not convincingly supported and why not. Yes, this paper arguably poses more questions than it answers, but I believe these are pertinent questions that need to be put forward, rather than buried under a blanket..

Draft rewrite of the Introduction and the Creep are below:

1 Introduction (new version)

The manner in which ice deforms within an ice sheet and moves or slides over its base are critical to accurately model the dynamic past, present and future behaviour of such ice bodies (e.g., Marshall, 2005). The internal deformation of cold ice is fairly well understood, and predictions made on the basis of physical laws (e.g., Glen's flow law) are broadly confirmed by observations (e.g., Dahl-Jensen and Gundestrup, 1987; Paterson, 1994; Ryser et al., 2014, but see Paterson (1991) for problems with dusty ice, and Hooke (1981) for a general critique).   This is not the case for basal sliding, for which many parameters are poorly constrained. Instead, many models of modern ice sheets use an empirical drag factor or slip coefficient, derived from observed ice velocity and estimated shear stresses (e.g., MacAyeal et al., 1995; Gudmundsson and Raymond, 2008; Ryser et al., 2014). Using an empirical slip coefficient is reasonable to describe and understand present-day near-instantaneous ice sheet behaviour, but cannot reliably predict or reconstruct ice velocities if parameters such as ice thickness, driving forces and meltwater production change significantly.

This problem is particularly acute for ice streams with poor topographic steering. For such ice streams it is commonly assumed that the necessary low drag can be explained by the presence of soft sediment or deformable till (e.g., Alley et al., 1987; Hindmarsh, 1997; Winsborow et al., 2010), which has indeed been shown to occur below some ice streams in West Antarctica (e.g. Alley et al. 1986; King et al. 2009) and also in the geomorphological record (e.g. Margold et al. 2010). However, there is increasing geomorphological evidence for palaeo-ice streaming on rough, hard bedrock-dominated beds without clear topographic steering. Hard, rough beds are widespread on the beds of the former Pleistocene ice sheets and also likely beneath the present-day Greenland and Antarctic ice sheets (e.g., Kleman et al., 2008; Eyles, 2012; Rippin, 2014; Krabbendam and Bradwell, 2014; Krabbendam et al., 2016). Evidence for paleo-ice streaming has been reported from the former Pleistocene Laurentide and British ice sheets and deglaciated parts of West Greenland (Smith, 1948; Stokes and Clark, 2003; Roberts and Long, 2005; Bradwell et al., 2008; Eyles, 2012; Bradwell, 2013; Eyles and Putkinen, 2014; Krabbendam et al., 2016). In these areas, the deforming-bed models cannot apply because little or no soft-sediment is present. These palaeo-ice stream zones are surrounded by areas also subjected to less intense warm-based ice erosion suggesting intermediate ice velocities (e.g., Bradwell, 2013), consistent with ice velocity analysis and borehole observations from the Greenland Ice Sheet that show significant warm-based sliding (10-100 m yr-1) outside ice-streams (Lüthi et al., 2002; Ryser et al., 2014, Joughin et al., 2010). Thus, fast ice flow appears to be possible on hard, rough beds and cannot be explained by a simple cold/warm thermal boundary (cf. Payne and Dongelmans, 1997). In Greenland, the massive Northeast Greenland Ice Stream remains difficult to explain, as current explanations invoke geologically unreasonably high geothermal heat flows (e.g., Fahnenstock et al., 2001) and a deformable bed with an unknown till source (Christianson et al. 2014).

A solution may be presented by the occurrence of a basal layer of temperate ice (ice at the melting temperature), below cold ice that makes up the remainder of the ice sheet. Drilling in Greenland Ice Sheet adjacent to the Jakobshavn Isbrae has documented a c. 30 m thick basal layer of temperate ice below cold ice (Lüthi et al., 2002), and has been modelled to occur beneath other parts of the Greenland Ice Sheet (e.g., Dahl-Jensen, 1989; Calov and Hutter, 1996; Greve, 1997). Two pertinent questions follow from these observations:

1) How does such a temperate layer develop and how is it maintained, given that it is overlain by cold ice? In-situ measurements at a glacier base and experiments have shown that warm-based basal sliding occurs under significant friction, caused by basal-debris / bedrock contacts (Iverson et al., 2003; Cohen et al., 2005; Zoet et al., 2013), generating significant frictional heat at the base, which is important for the development of a temperate ice layer.

2) How does basal sliding work in temperate ice, and how does this differ from classic sliding models? The essence of the classic Weertman (1957) sliding model is that basal ice movement past an obstacle occurs by stoss-side pressure melting around the obstacle and by ductile flow according to Glen's flow law but enhanced by stress concentrations near the obstacle, with ductile flow being more important with larger obstacle size. Sparse experimental evidence suggests that temperate ice is considerably weaker than cold ice, and that creep may not be modelled reliably according to the standard Glen's flow law (e.g. Colbeck and Evans, 1973; Duval, 1977; Morgan, 1991). Secondly, in a temperate ice layer, the thermal gradients required for the pressure melting (e.g. heat flow *through* the obstacle) to proceed may have different controls than in the classic model.

This paper deals with four issues:

- The problem of how a temperate layer can develop below cold ice, including the role of frictional heating;
- How the basic assumptions of classic Weertman sliding (enhanced ductile flow controlled by Glen's flow law and stoss-side pressure melting controlled by heat flow through an obstacle) may not be applied to temperate ice, and alternative controlling mechanisms are proposed;
- The potential thermo-mechanical role of temperate ice below cold ice in an ice sheet;
- The role of a temperate basal ice layer on the occurrence of ice streaming on rough, hard beds, such as seen on deglaciated terrains and possibly relevant to the Northeast Greenland Ice Stream.
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The paper takes a rather conceptual approach, focussing the primary thermodynamic and rheological controls, so to achieve an improved conceptual model of basal ice motion on a rough, hard bed, rather than the exact quantification of geometries and stress distributions around bedrock obstacles.

4 The creep component in temperate ice

Weertman (1957) assumed that the creep component of ice flowing around a hard obstacle worked with a rheology according to 'Glen's Flow law', enhanced by stress concentration oaround the obstacle. This law concerns the general relation between imposed deviatoric stress and resulting strain. The temperature dependence follows the Arrhenius relation, so that the relationship between strain rate $\dot{\varepsilon}$, deviatoric stress σ and temperature is typically described in one dimension as:

$$(7)\ \dot{\varepsilon} = A\sigma^n e^{(-\frac{Q_a}{RT})}$$

where A is a constant, R the gas constant, n the stress component and Q_a the activation energy (Glen, 1955; Paterson, 1994; Alley, 1992). This general power law is applicable at appropriate conditions to many crystalline materials such as quartz, olivine and metals (e.g., Poirier, 1985). For ice, experiments suggest that $n \approx 3$ and $Q_a \approx$ 80-120 kJ mol^{-1} for T >-10 °C (e.g., Barnes et al., 1971; Duval et al., 1983; Alley, 1992; Paterson, 1994). Comparisons with borehole tilt deformation studies suggest that this describes the rheology of clean ice reasonably well (e.g., Dahl-Jensen and Gundestrup, 1987; Lüthi et al., 2001). Note that strain rate $\dot{\varepsilon}$ increases exponentially with temperature. The rate-controlling deformation mechanism of power-law creep in ice and other crystalline materials such as quartz and olivine is normally regarded to be intracrystalline creep, mainly by dislocation glide along basal planes (e.g., Duval et al., 1983; Poirier, 1985; Alley, 1992). The question is whether temperate ice behaves according to the same power law and has the same rate-controlling deformation mechanism (e.g., Hooke, 1981; Parizek and Alley, 2004) and is thus applicable to temperate ice flow around hard obstacles.

Experimental data compiled by Morgan (1991), all performed under constant stress (1 bar = 10^5 Pa), are plotted in Fig. 4 to illustrate the effect of temperature on the strain rate. The natural logarithmic of strain is plotted against the reciprocal of temperature, so that a straight line would confirm the Arrhenius relations within the power law. For temperatures between -5 and -0.5 °C, the data plot on a straight line, the gradient of which equals ($-Q_a/R$), confirming the Arrhenius relation in equation (7) and thus power law behaviour over this temperature interval. However, at about -0.02 °C there is a sharp nick in the trend, with strain rates increasing by up to a factor of ten as the melting temperature is approached. Thus, at constant stress, ice above c. -0.2°C shows a sudden weakening of a factor 5 to 10, and the Arrhenius relation of equation (7) clearly does not describe this, potentially implying that power-law creep is not dominant close to the melting temperature (see also Barnes et al. 1971; Colbeck and Evans, 1973; Morgan, 1991).

The stress-strain rate dependency for temperate ice is ambiguous. Experiments at constant temperature but varying stress (T ≈ -0.01 °C, σ = 10-100kPa) by Colbeck and Evans (1973) suggest $n = 1.3$; experiments of creep at the melting temperature past a sphere by Byers et al. (2012) also

suggest $n < 1.5$. By analysing the bulk stress with borehole tilt measurements to constrain strain in a 3D borehole network in the temperate Worthington Glacier, Marshall et al. (2005) noted n ~ 1 at low stresses, but, interestingly, a change to n ~ 4 at higher stresses (>1.8 kPa). De La Chapelle et al. (1999) noted a similar change from n ≈ 1.8 at low stresses to n ~ 3 at high stress (>250 kPa) in experiments of pure ice in the presence of a brine (i.e. not temperate ice sensu stricto). Analysis of bulk stress and strain rate of the temperate Glacier de Tsanfleuron, however, suggest $n \sim 1$ (Chandler et al., 2008). At the base of the same glacier, Tison and Hubbard (2000) documented large grain sizes (5-20 mm) and a well-developed crystallographic fabric in basal deforming ice.

It thus appears that deforming temperate ice behaves fundamentally different from deforming cold ice and cannot be realibly modelled with a standard poer law or Glen's flow law. The sharp transition just below the melting temperature suggests that this difference is largely caused by the presence of water. Duval (1977) noted in temperate ice experiments that a rise in water content up to c. 0.8% lead to a 5-8 times strain rate increase. Temperate ice in Alpine glaciers can contain 1-2 % water (Vallon, 1976), and has been observed in experiments to gather along grain triple junctions (Barnes and Tabor, 1967; Wilson et al. 1996), so that it is likely that a vein network along triple junctions exists (Nye and Frank, 1973; Mader, 1992).   Partial melting of a deforming temperate layer is furthermore suggested by the formation of bubble-free ice, both in experiments (Barnes and Tabor, 1966) as well as in Alpine and surging Svalbard glaciers (Tison and Hubbard, 2000, Lovell et al. 2015).   The dominant deformation mechanism for temperate ice, however, is uncertain and it is possible that different deformation mechanisms operate simultaneously. Possible deformation mechanisms and their potential enhancement by the presence of water include:

1) Diffusion creep is enhanced by the presence of liquid along grain boundaries, since that liquid functions like a fast diffusion path (Ashby and Pharr, 1983; Raj, 1982; Goldsby and Kohlstedt, 2001).

2) Dislocation creep is also enhanced by liquid (Duval, 1977; De la Chapelle et al. 1995; 1999). Water along grain boundaries decreases the surface area of grain-to-grain contacts and cause an increase in grain-to-grain contact stresses; this will enhance dislocation creep (De La Chapelle et al., 1999) but also other deformation mechanisms. Liquid may also suppress strain hardening and enhance easy intracrystalline basal slip (Duval, 1977; De La Chapelle et al. 1999).

3) Dynamic recrystallization and grain growth is rapid in deforming temperate ice (e.g., Duval et al. 1983; Wilson, 1986). Dynamic recrystallization aids dislocation creep as it grows crystals with orientations favourable for easy basal slip and suppresses strain hardening (e.g.

Duval et al. 1983). Dynamic recrystallization results in a coarse grain size and should aid development of a crystal fabric.

4) Grain boundary sliding is commonly invoked to explain weakening in ductilly deforming materials (superplasticity). Superplasticity has been experimentally achieved in ice at very low temperatures (-30 to -80°) and very small (3-40µm) grain sizes (Goldsby and Kohlstedt, 1997, 2001; Goldsby and Swainson, 2005), rather different than the temperate ice under discussion here. Whether grain boundary sliding in ice leads to the formation or destruction of a crystallographic fabric appears debatable (Goldsby and Kohlstedt, 2001; 2002; Duval and Montagnat, 2003; Goldsby and Swainson, 2005).

5) Grain boundary melting (or 'internal pressure melting') has been observed in ice deformation experiments with indentors by Barnes and Tabor (1966), Barnes et al. (1971) and Wilson et al. (1996). The principle is that ice melts at highly stressed grain boundaries, liquid is transported to lesser stressed grain boundaries where it refreezes – or water may escape if the intergranular vein network is efficient. Either way, this leads to strain. For ice (in contrast to almost all other materials), it is important to emphasise that grain boundary melting involves a negative volume change upon melting ($\Delta V = -9\%$), which makes grain boundary melting under elevated stresses a thermodynamically favourable mechanism. The distance of heat transport to the stressed grain boundaries necessary to sustain grain boundary melting is half the grain size (Fig. 4), some three orders of magnitude smaller than the size of most bedrock obstacles. It further requires that ice and water are in thermal equilibrium, and might thus not be observed in experiments where the liquid is a brine (cf. De La Chapelle, 1995; 1999). Grain boundary melting is supported by the formation of bubble-poor ice at the base of temperate glaciers: both Tison and Hubbard (2000) and Lovell et al. (2015) show that such ice not formed by direct freeze-on (regelation ice), but by a metamorphic process involving partial melting. Grain boundary melting is loosely analogous to pressure solution (solution-precipitation creep) observed in salts and limestone, in that material changes from solid to liquid or vice-versa along grain boundaries in different stress states (Ashby and Pharr, 1983 , McClay, 1977; Rutter, 1983), but differs in that grain boundary melting creates more liquid.

Which of these deformation mechanisms is dominant or rate-controlling is difficult to establish. Given the very different grainsizes and temperatures at which grain boundary sliding has been shown to occur (Goldsby and Kohlstedt, 1997, 2001), as well as the development of a crystallographic fabric (Tison and Hubbard, 2000), suggests grain boundary sliding is probably not significant in temperate ice. Diffusion creep, grain boundary sliding and grain boundary melting all work on grain boundaries and are grain-size sensitive: they are favoured by a small grain size and the presence of a liquid; these mechanisms normally result in n < 3, and thus could explain the n ~ 1 behaviour seen in some

experiments and natural glaciers. Considering the near-unique pressure-melting behaviour of H_2O, grain boundary melting is worthy of further study. However, all grain-size sensitive mechanisms are at odds with the large grain sizes observed and can, on their own, not explain well-developed fabrics. Conversely, well-developed fabrics potentially attests to dislocation creep, but this at odds with the n ~ 1 behaviour commonly observed. Altogether there is no clear evidence of a single dominant deformation mechanism, and all deformation mechanisms mentioned above may contribute. The change in the stress-dependency as observed in the temperate Worthington Glacier (Marshall et al. 2005) as well as in some experiments (De La Chapelle et al. 1999) suggests that the dominant deformation mechanism in temperate ice depends on the magnitude of stress. For the moment the rather non-generic term '*melt-assisted creep*' is used herein, while stressing that regardless of the actual mechanism, all experiments show that temperate ice with high water content is significantly weaker than cold ice.

A strong crystallographic fabric and concentrations of dust or silt particles are known to significantly weaken cold ice in simple shear (e.g., Lile, 1978; Paterson, 1984, 1991; Dahl-Jensen and Gundestrup, 1987; Azuma, 1994), but whether this leads to further weakening of temperate ice is not known. There is still much unknown about creep in temperate ice; all that can be said is that temperate ice is significantly weaker than, and behaves very differently from, cold ice.