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Interactive comment

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 paper focuses on the basal ice motion in ice sheet conditions, by re-analysing the classical Weertman sliding model, and by suggesting new processes to take into account in order to better model and understand the physical mechanisms. In particular, the paper concentrates on the role of frictional heating at the base of ice sheets and glaciers, and on the impact of a specific rheology of temperate ice compared to cold ice.

Considering my skills, my comments will concentrate on what concerns the rheological model suggested for taking into account the role of temperate ice. The approach suggested to take into account frictional heating as a way to modify the amount of

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Discussion paper

temperate ice that can be encountered at the base of glaciers and ice sheets sounds very coherent with previous works on frictional heating in ice (and other materials), as far as I know. The author insists therefore on the fact that the ice could be, at least in some areas, mostly temperate, and that an appropriate rheology must be considered. Temperate ice is indeed known to deform at much faster rates than does cold ice. Several studies where made to show that, some are cited in this work (Morgan 1991), and some could be much deeper studied and used in the same direction, such as De La Chapelle et al. 1999 (GRL), and 1995 (Scripta Mat.), in order to infer the basic mechanisms that could explain the strain-rate increase in temperate ice. Temperate ice can be considered as ice with a liquid intergranular phase. It is therefore very similar to some geological materials, and some metals deformed at high temperature. Although classical rheological mechanisms such a dislocation creep are strongly enhanced at temperature very close (or at) the melting point, grain boundary sliding (GBS) could also be very efficient in such conditions. I therefore do not understand why this mechanism is not discussed in the present paper? De La Chapelle et al. (1995 and 1999) show that, when deforming ice with a liquid intergranular phase, they obtain a mechanical response with a very similar power law that in cold ice, except that the minimum strain rate reached can be more that 6 times higher (similar to what Morgan 1991 found). Nevertheless, their curves keep showing a power-law exponent of 3 for stress higher than ~ 0.4MPa, and close to 2 for lower values of stress. The value of the transition stress depends on the water content. These observations are therefore coherent with the fact that dislocation creep keeps dominating the deformation behavior of ice with an intergranular liquid phase, and that GBS, that should occur, is not dominating (would induce a stress exponent of 2, whatever the level of stress, and a grain size dependance, that was not tested in De La Chapelle's work), nor should dominate diffusional or solution-deposition creep (and therefore pressure-solution creep as evoked here?). The increase in strain rate is attributed to the fact that the liquid phase decreases the internal stress field (coming from strong strain heterogeneities between grains), and facilitates basal dislocation glide, and therefore deformation. Grain boundary migration could also be facilitated in such a condition (or at T close to Tm), and would also relax the stress heterogeneities, and facilitates deformation (such as during dynamic recrystallization).

Instead of using such observations (although coming from saline ice, and not "exactly" temperate ice), the author evokes a mechanism called "grain boundary pressure melting". This mechanisms, which is supposed to be supported by the work of Wilson et al. 1996, assumes that some grain boundaries (GB) would be submitted to higher stress, depending on their orientation compared to the maximum deviatoric stress. Such an assumption would not take into account the strong stress redistribution in polycrystalline ice, that lead to strong strain heterogeneities, with very weak relation between the level of deformation and grain orientation (Grennerat et al. 2012, acta Mater, for instance). And, indeed, Wilson et al. 1996 results tend to show these strong heterogeneities of the strain distribution, and therefore of melt at GB. To sum up, taking into account the fact that temperate ice rheology can be different than that of cold ice, especially the fact that activation energy and strain rates are much higher, is very important in the situation described in this paper. The relation between frictional heating and temperate ice is also well described and makes a lot of sense to me. I just do not see the interest of trying to find "another type" of rheology for temperate ice, especially since the one suggested here (GBPS) is very unlikely, in order to support the main assumption made in this work. Although few experiments exist on temperate ice, the work of Morgan, 1991, and De La Chapelle et al. (1995, 1998) can be efficiently used to enhance the main assumption.

Some specific comments:

- p 9, 2d paragraph: I don't think that results on temperate ice show that power-law creep does not adequately describe ice creep above -0.2°C. With increase in temperature, the activation energy can change, and therefore, we do not expect a linear relation between strain rate and temperature. To get rid of a power-law creep relation, one needs to plot minimum strain-rate as a function of stress... and show that this is not linear. Which is not provided in Morgan 1991. Results by De La Chapelle et al tend to show that power-law creep remains even when a liquid phase exist at GB, which is mostly what happens when ice is temperate (Wilson et al. 1996)?

- p9, last paragraph: to my point of view, there are not enough information to be able to suggest a mechanism such as the one suggested here (GBPS), that has never been observed in ice, and that appears more than unlikely regarding knowledge about ice deformation behavior, with or without liquid layers... A discussion would nevertheless be required concerning the possibility of grain boundary sliding at such high temperatures.

- p10, 1st paragraph: As far as I know, pressure solution requires different phases to be present, in order for some to be dissolved under local pressure, and migrate is some fractures together with the fluid phase, and re-precipitate further away (see J-P. Gratier, D. K. Dysthe and F. Renard. The role of pressure solution creep in the ductility of the Earth's upper crust. Advances in Geophysics, vol. 54, 2013). I do not see at all how this can occur in the temperate ice layer at the bottom of glaciers and ice sheets...

- p11, 1st paragraph: Once again, there are not enough proof or information to assess so directly the occurrence of some grain boundary pressure melting... Power-law creep can also be fast, if accommodation mechanisms are efficient (dynamic recrystallization, GBS, liquid intergranular phase)... so it can not be ruled out so easily.

- p12, 1sr paragraph: same comment about power-law creep being ruled out...

- Conclusions: To my point of view, they need to be rewritten by being much more precise about the terms used. What is basal creep?? is it creep due to basal dislocations glide? Creep at the base of the glaciers? Therefore, it can be 10 times faster only because the presence of a liquid layer and high temperature enhancing dislocation creep... Dynamic recrystallization is evoked briefly, without being much explained before. It needs a lot of dislocations to be activated, and therefore is in favour of dislocation creep... It is more efficient at high temperature. Its occurrence should however be reduced by the presence of a liquid phase at GB, since this liquid phase will most probably be very efficient to relax the local stress field, and absorb dislocations in excess, that are responsible for dynamic recrystallization mechanisms (nucleation and grain boundary migration). GBS would be much more likely to occur than GBPM in order to be associated with dislocation creep.

As a summary, it seems quite important that the hypotheses performed here about temperate ice rheology be clarified, and cleaned of non or too lightly justified hypotheses, that, as far as I understood, are not strictly required to provide the main message of this work.

Sincerely

M. Montagnat April 28th

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