

Interactive comment on “The role of grain-size evolution on the rheology of ice: Implications for reconciling laboratory creep data and the Glen flow law” by Mark D. Behn et al.

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I already commented briefly in my unsolicited comment on the interesting and provocative manuscript of Behn et al. that proposes a novel (at least in glaciology, I believe) way to address the question of grain size in glaciers and ice sheets and its relationship with the rheology and stress exponent for power-law creep of ice. I was fortunate that by the time I was asked for a review, one thorough review was already published. I concur with the anonymous reviewer and need not repeat her/his comments.

I hope the manuscript by Behn et al. will be published in TC as it gives the community valuable food for thought. However, I would suggest to first address a few issues: 1)

C1

Does the paradox on which the paper is based really exist? 2) Grain-growth parameters may be over-simplified. 3) The merits of alternative explanations for the grain size - stress relationship could be discussed more.

These issues are discussed in more detail below.

In the section starting at line 40, a crucial aspect is missing. Glen and some other authors made it very clear that their stress exponent was determined for the minimum strain rate/maximum stress and not for steady state. Comparing the low n (≈ 3) at very low strain (about 1-3%!) with high-strain steady-state flow may be like comparing apples and oranges.

The manuscript is based on the "paradox" mentioned in line 59. Simply put it is postulated that experiments indicate a stress exponent n of either ca. 1.8 (low stress) or ca. 4 (high stress), while natural flow is closer to $n=3$, the value generally (and uncritically!) used in flow modelling. The question is whether this paradox really exists. In lines 28-34 it is argued that natural flow is consistent with $n\approx 3$. Although several studies indeed come to this conclusion, others do not. For example Bons et al. (2018) deduced $n\approx 4$ for a large area of the Greenland Ice Sheet (excluding the divides, ice-sheet margins and ice streams), while Pettit & Waddington (2003) find $n\approx 1$ at divides. Glen (1955) himself wrote "... it is noteworthy that practically observable long-time creep rates, as in a glacier, would probably depend on a higher power of the stress than the 3.2 found here", although he did not actually determine this in natural ice. Cuffey and Kavanaugh (2011) write "we conclude that the effective n must be between 2.6 and 5.1 (99% confidence). The best match occurs with $n \approx 3.5$ ". However, in the conclusions they also write "... supports the nearly universal practice of treating ice as an $n = 3$ nonlinear fluid in analyses of glacier flow". This may be symptomatic: despite evidence or indications to the contrary, some authors appear to (want to) stick to $n=3$, even if the data are inconclusive or allow alternatives. Another example is fig. 14 in Budd & Jacka (1989). They plot surface velocity/height against driving stress and find a best fit with a slope between $n=3$ and $n=4$. However, assuming $n=3$, they interpret the range in data in

C2

terms of temperature differences. Close (re-) assessment of the literature shows that there is quite abundant evidence for n unequal to 3 for natural ice flow, even though the literature unfortunately does not always fairly acknowledge this. I suggest the authors: (1) qualify their basic starting assumption that natural ice follows $n \approx 3$ (2) and include in their following analysis what the consequences would be if n for natural flow is not 3, but perhaps indeed 4 as some claim to have measured in nature. Would this, for example, mean no contribution of GBS? Would the wattmeter work and give reasonable results?

In my unsolicited comment I already briefly addressed the grain-growth "constant" K and the grain-growth exponent p . The authors use $p \approx 6$, based on natural grain sizes in drill core and experiments with bubbly ice. There are a number of issues that I would ask the authors to consider.

(1) The exponent p reflects the scaling of the governing process(es). If grain growth is driven by unrestricted reduction of grain-boundary curvature and grain-boundary velocity is linearly proportional to the driving force (curvature), p should be 2. Restricted grain-boundary movement due to pinning or drag leads to a slow-down of growth, which gives a growth curve that may be fitted with a power law, but which is not a power law. The exponent p is "effective" or "apparent", but has little physical meaning and cannot be regarded as a universal material property. Growth then just does not follow a power law. If bubbles hinder growth, the effective p will depend on bubble size and distribution, relative to grain size (Arena et al., 1997; Roessiger et al. 2014). The main factor is probably the fraction of boundaries that is hindered in their movement by bubbles. If that fraction is small at the equilibrium grain size, the exponent p is expected to be close to 2, as most boundaries simply "don't know that they are in bubbly ice". In a grain-growth experiment that runs for long enough, one inevitably comes in the range where a significant number of boundaries interact with bubbles, which slows down the growth. The effective mobility of grain boundaries goes down, which raises the apparent p . This apparent p may not be relevant to the wattmeter if grain sizes are below this interaction

C3

range. It should be noted that in the numerical simulations of Roessiger et al. (2014) p is always 2, just because of the scaling of the numerical simulations and governing equations. However, the growth curves would give a wide variety of $p > 2$ values, if one would erroneously assume a power law.

(2) K is also not a universal constant, because it depends on the microstructure. This was actually one outcome of my very first paper: Bons & Urai (1992; I was so proud that I sent reprints to my whole family!). Static grain growth typically leads to a particular microstructure (grain shape and size distribution): a foam texture as in a soap froth. Changing the microstructure means changing K . Growth experiments are probably often hampered by this effect: it takes some growth to establish the steady-state growth rate. Measurements of K and p should only start after this is reached. Roessiger et al. (2014) therefore suggest a grain size increase of at least about 4-5 times. The resulting K is for static grain growth and does not apply to a dynamic grain-size equilibrium under consideration in the manuscript, where the microstructure is expected to be quite different. The distribution of bubbles may also be different during deformation compared to static experiments (Steinbach et al. 2016). It is not clear if a different, but constant K applies, or that K is a function of stress and/or strain rate.

The bottom line is that one should not consider a single, constant p and K . It is very well possible that $p=2$, but K varies depending on a variety of factors. How would this affect the analysis?

Line 83: " However, the piezometer does not account for the physical processes that control ice grain size - namely the competition between grain growth and grain-size reduction via recrystallization (e.g., Alley, 1992)." I suggest qualifying this rather sweeping sentence. There is a huge body of literature in materials science, metallurgy, geology, etc. on the physical processes that determine the piezometer. These models cannot be dismissed as "simple", nor do all say that grain size is the inverse of stress. The authors cite Jacka and Jun (1994). The authors of the paper are T.H. Jacka and Li Jun. The header of the original printed paper reads: "Jacka and Li: Steady-state crys-

C4

tal size of deforming ice". I therefore assume that the surname is "Li", not "Jun" and the Chinese convention of surname first was used. They do not find that grain size is inversely proportional to stress, but by an exponent of about -1.5. I do appreciate that the Jacka & Li piezometer is plotted in fig 3. It plots pretty much exactly on the boundary between the two deformation mechanisms as is acknowledged in the manuscript. So far, the data of Jacka & Li appear the only experimental grain size - stress data published in the literature and they would at first sight strongly support the de Bresser model. The slope of the piezometer is actually quite in line with that found for several other minerals, as pointed out by Jacka & Li and de Bresser et al (2001). Considering that natural flow of ice appears to be faster than experiments predict (compare the $n=4$ rates in Bons et al. (2018) with those used in the manuscript), the difference between grain size predicted by experiments and natural ice may be due to the infamous and "ad-hoc" enhancement factor. Line 284 is of interest: "Overall the piezometer [of Jacka & Li, 1994] results in smaller strain-rates throughout most of the column and a significantly higher effective stress exponent ($n_{eff} \sim 3.9$), similar to the experimental value for dislocation creep." This $n \approx 4$ is exactly what is proposed by some authors for natural flow, which would fit very well with the piezometer. I suggest not to be too dismissive of the de Bresser model and the data of Jacka & Li (nor assume that natural flow has $n=3$).

Line 86: Typo in Roessiger Line 376: typo in Kipfstuhl

I hope these comments are not perceived as overly critical. The matter of the stress exponent is crucially important and far from trivial. All the more reason to be extremely careful. Only then may the community gain confidence in the rheological parameters it uses.

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C5

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C6