Review of: Moreno et al: "Microstructure and ice dynamics - Integrating Grain Properties, Fabric, and Borehole Data in the NEEM Ice Core"
Review by Dave Prior, University of Otago.

This is an excellent scientific idea, but the data presentation and analysis are superficial and incomplete and the writing and particularly the use of citations is imprecise and unclear. It needs a lot of work to make this paper publishable. I encourage the authors to do this, as this research is important for our understanding of ice rheology.

In my review I outline first why the article is important then deal with the issues of superficiality/incompleteness. I make a general statement about writing/citation precision followed by specific issues in manuscript order. I hope my comments are useful. I was time-limited so stopped before making all the comments I could have. I have posted an annotated copy of the manuscript that may be useful: it has more than this document but may be a bit unstructured..

Importance

The overall premise of this paper, to use the different strain rates of different layers in NEEM to say something about rheology, is sound. I agree with the authors that the equivalent fabric of the two layers means that other differences, specifically grain size and/or impurity content, must be the control on strain rates. What is important from a rheological perspective is that the NEEM ice represents high-strain natural ice deforming in steady-state (~tertiary creep). We have good experimental data on grain size sensitivity (Goldsby, 2006; Goldsby and Kohlstedt, 1997, 2001; Qi and Goldsby, 2021) of ice deformation, but this is all from low strain (secondary creep/ peak stress) data in isotropic ice. Ice experimental data from steady-state (tertiary creep) do not show grain size sensitivity explicitly (Durham et al., 1983; Fan et al., 2025). This is primarily because grain size obtains a steady-state mean value that relates to the stress magnitude (A piezometer relationship (Jacka and LI, 1994; Platt, 2023)) and because of this, grain size does not need to be explicitly included in the flow law, even if GSS mechanisms are active (Fan et al., 2025). Rheologies for rocks other than ice (polycrystalline quartz, olivine, calcite, halite, multiphase rocks etc) are invariably defined at steady-state (Hirth, 2002; Renner and Evans, 2002; Tullis, 2002); the idea of a secondary creep/ peak stress flow law is non-existent in the rock mechanics community. Grain size sensitivity at steady-state cannot readily be measured in pure, single-phase systems because of the piezometer effect (grain size adjusts to the stress magnitude). So, to parameterise grain size sensitivity at steady-state, "pinning" particles are added to restrict grain growth during high-strain experiments: for example graphite is added to calcite (Walker et al., 1990), melt or orthopyroxene added to olivine (Hirth and Kohlstedt, 1995, 2003). Equivalent experiments, with sparse pinning particles in ice have not yet been used to parameterise grain size sensitivity at steady-state, so your work provides the first good insight into this topic. My reading of your story is that the impurities in the stadial ice restrict grain growth, maintaining a grain size smaller than expected for the stress (from a piezometer relationship) and this then has an impact on rheology, making the ice weaker than it would be without the impurity pinning. This is awesome and needs to be published.

Superficiality / Incompleteness:
The paper says little more than - we have data that shows finer layers with more impurities deform faster under the same stress and temperature. There is no detailed description or analysis of either the grain size data or the shear strain rate data. Furthermore, the investigation of the relationship of these two parameters to potential flow laws is superficial/ non -existant.

i. *Grain size data*

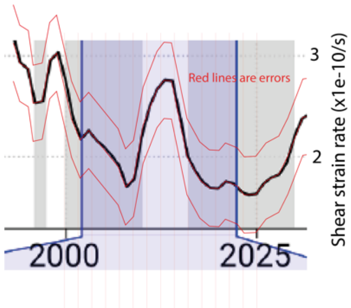
How are grain sizes measured? The units are mm^2 so I guess that these are grain areas. Are they the mean (or median?) of many individual grain area measurements or are they a measured area divided by the number of grains identified. The section around line 105 provides the most detail about grain segregation (I think you have done this ?) and this level of detail is inadequate, the reader needs to know what you have done. Line 196. alludes to grain size distributions, but there are no grain size distributions presented or described.

Given the power of the xLASM method I would expect example grain size distributions (e.g corresponding to the two images in Fig 3) to be presented and discussed and summary statistics for all samples included on fig 4. Presentation should include mean, median (or other different ways to represent an average) and quartile (or standard deviation) data. Examples exist from naturally sheared ice (e.g. fig 8 in (Thomas et al., 2021)) and experimentally deformed ice (e.g. Figs 4,5,6 and 11 in (Fan et al., 2020)). (Lopez-Sanchez and Llana-Funez, 2015) is a really good review of some important approaches.

Another important parameter is how irregular the grain boundaries are. Various published approaches are useful (Fan et al., 2021b; Johnson et al., 2025; Takahashi et al., 1998). Grain size distributions and grain boundary shape are very useful data to help infer operating deformation mechanism and the absence of data from this paper is a significant weakness.

ii. *Shear strain rate data*

Shear strain rate date are presented with no discussion of error or limitations. There is also a related issue of finding detail on the method and, in particular, the raw data used to derive the shear strain data. I went back to the literature cited and it is not straightforward to find the details of the borehole logging. (Gundestrup et al., 1993) refers back to (Gundestrup and Hansen, 1984) for details of instrumentation and neither (as far as I can tell) present directly an estimate of inclination error. If I follow up the specs on the currently available versions of the inclinometers used (https://jewellinstruments.com/products/inertial-tilt-sensors/inclinometer/forced-balance-i/lsrp-single-axis-analog-inclinometer/), this suggests that the inclination error is ~ 1 degree. If I reverse engineer inclination change from the shear strain rate (see worksheet in the spreadsheet “Calculations related to Moreno et al.xlsx” and apply this error over a 9-year period I get an approximate error pattern shown below.



Now I may be way off here (reverse engineering always tricky), but my point is the errors and a justification of the errors are not presented – and I cannot find this anywhere in public literature.

The strain rate ratios of the stadial vs interstadial ice is crucial to this paper and the reader needs to know how robust it is. There are likely features of the raw data that may demonstrate robustness, for example inclination changing towards the same azimuth with time. The minimum I would expect is a plot of inclinations and azimuths for each of the five logging runs together with the calculated strain rates (over different time increments) and errors. If this is already publicly available (I can't find it) then cite it. If not, these data (for the 16m of interest in particular) need to be presented here.

There is no clear description of the shear kinematics. As the shear is calculated from borehole inclination, I presume the shear is along horizontal shear planes in a horizontal direction. This needs to be stated explicitly. Note describe this in full, as the shorthand of two key research communities is different. Structural geologists would describe this as horizontal shear (horizontal shear planes) whereas glaciologists/ ice sheet modellers would describe this as vertical shear (shear relationships apparent in the vertical plane). Do the borehole data constrain the shear as simple shear? (needed vertical motion data as well as inclination). Maybe that comes from the inference of the fabrics (Montagnat et al., 2014) alone? You need to be clear about this.

iii. *link the results to published flow laws*

You can and should go much further than the statement on line 219: "Constitutive equations thus need to include a term to explain changes in ice grain properties, alongside terms related to ice temperature and ice grain orientation…".

You have the potential to test and expand on existing relationships. This is the best data set I have seen to do this. As I said in the section of importance, we do not have a grain size sensitivity parameter for steady-state (~tertiary) creep. A simple assumption is that the grain size sensitivity will be the same as at secondary creep/ peak stress (low strain). Your data can test this.

A simple first step is to estimate a grain size exponent based on the data, assuming a single component GSS flow law e.g. (Fan et al., 2025) eqn (3)

$$\dot{\varepsilon} = A\sigma^n d^{-p} \exp\left(-\frac{Q}{RT}\right).$$

So if environmental parameters T and σ and mechanism/material parameters n, Q and A are constant a grain size exponent p can be calculated; as done in the "GS Exponent simple" worksheet of spreadsheet "Calculations related to Moreno et al.xlsx".

$$\log(\dot{\varepsilon}_{coarse}) - \log(\dot{\varepsilon}_{fine}) = p(\log(d_{fine}) - \log(d_{ccoarse}))$$

If I use 2.8E-10 vs 1.8E-10 as strain rates for grain sizes (d) of 1mm vs 1.7mm respecyively, I get a value of p = 0.8 - which is the same value that (Fan et al., 2025) get when fitting all low strain (secondary creep) experimental data to a single component GSS equation (Table 1 in Fan et al). This will be a minimum p value as a single component GSS equation is unlikely to represent the processes in the ice. The assumption that mechanism/material parameters n, Q and A are constant breaks down if you use a multicomponent flow law.

Our best understanding is that ice deforms by more than one mechanism and a multicomponent flow law is needed (Fan et al., 2025; Goldsby and Kohlstedt, 1997, 2001). Multicomponent flow laws with a GSS component can be tested against your data. The way to test is to iteratively calculate the strain rates for given stresses and find the stress that gives the ratio of strain rates observed in your data for the measured grain sizes. i.e. you are using the assumption that stress is the same. I have done this in the "Composite Rheology Calcs" worksheet of spreadsheet "Calculations related to Moreno et al.xlsx" spreadsheet using (all data from table 1 of Fan et al, 2025):

1. the Goldsby and Kohlstedt two component flow law ((Goldsby and Kohlstedt, 2001) with A values for octahedral stresses and strain rates (Fan et al., 2025).
2. the Fan et al three component flow law with A values for octahedral stresses and strain rates (Fan et al., 2025).

When the "easy?" parameter (orange):

R	0.008314			
T	-18	Areas	D	
D	1.00	1	1.00	
D2	1.73	3	1.73	
easy?	0.00	if zero no enhancement for easy slip		
Easy_enhance	1.00	enhancement factor for easy slip (add to log)		

is set to zero the flow laws use the parameters in table 1 of (Fan et al., 2025). Both flow laws give a strain rate ratio of ~ 1.75 for stresses of ~160-170kPa:

20	Stress (Mpa)		G+K_GBS_cold	G+K_Dis_cold	G&K_tot_cold		G+K_GBS_cold	G+K_Dis_cold	G&K_tot_cold	ratio G&K	difference	GSI/GSS Fine	GSI/GSS Coarse
51	0.155		7.6E-10	3.2E-10	1.1E-09		3.5E-10	3.2E-10	6.7E-10	1.61	0.04	0.41	0.89
52	0.160		8.1E-10	3.6E-10	1.2E-09		3.7E-10	3.6E-10	7.3E-10	1.59	0.02	0.44	0.96
53	0.165		8.6E-10	4.1E-10	1.3E-09		4.0E-10	4.1E-10	8.0E-10	1.57	0.00	0.48	1.03
54	0.170		9.0E-10	4.6E-10	1.4E-09		4.2E-10	4.6E-10	8.8E-10	1.55	0.02	0.51	1.10
55	0.175		9.5E-10	5.2E-10	1.5E-09		4.4E-10	5.2E-10	9.6E-10	1.53	0.04	0.54	1.17

This stress is high, when compared to shallow ice approximations: fig 6 from (Kuiper et al., 2020b). BUT the flow law is from isotropic ice. There is no allowance for the weakening effect of fabric. The weakening effect of the fabric is likely to be ~ one order of magnitude of strain rate (Fan et al., 2021a; Treverrow et al., 2012) for a given stress. If I apply an artificial enhancement to the dislocation creep (grain size insensitive) components of the flow laws ("easy?" parameter non zero adds 1 ["Easy_enhance"] to the A value for GSI):

D10 =IF(easy?=0,6.85,6.85+Easy_enhance)

A	B	C	D	E	F	G	H
R	0.008314						
T	-18	Areas (mm2	D (mm)				Note a values u:
D	1.00	1	1.00				Kuiper has 0.07
D2	1.73	3	1.73				0.05
easy?	1.00	if zero no enhancement for easy slip					
Easy_enhance	1.00	enhancement factor for easy slip (add to log)					
		G+K_GBS_c old	G+K_Dis_c old			G+K_GBS_c old	G+K_Dis_c old
n		1.8	4			1.8	4
Q		70	64			70	64
logA		2.48	7.85			2.48	7.85

Then the stress that gives a strain rate ratio of ~ 1.75 is reduced to values of 55-60kPa for G&K and 40kPa for Fan et al. These are stress values much more in keeping with the shallow ice approximations (fig 6 from (Kuiper et al., 2020b)).

I think there is a great opportunity of demonstrating grain size sensitivity in high strain, steady-state ice and provide a starting basis for flow laws that incorporate that. If there are aspects of my calculations that do not make sense feel free to reach out and ask.

Writing/citation precision

Writing is imprecise and often unclear and ambiguous. A lot of this relates to very poor use of citations. In using citations please be very specific about why you are using a citation. If you are using data from a citation, outline specifically the data you refer to. If you are using an interpretation from a citation, explain briefly the observational basis in the paper cited and the interpretation. A common poor practice is (not just you, but this mistake is everywhere in your paper):

"make a relatively complicated statement with lots of points followed by a list of citations".

The reader then has:

1. No idea in detail of what the support is for specific bits of the statement
2. No idea why an individual citation is there.

Line 30 -34 is a really good example. *"laboratory experiments and detailed ice core analyses significantly advanced the understanding of ice deformation by highlighting the influence of microstructural parameters such as grain size, impurity concentration, and grain fabric variations on ice viscosity and deformation rates (Dahl-Jensen et al., 1997; Durand et al., 2006a; Obbard and Baker, 2007; Baker et al., 2003; Barnes and Wolff, 2004; Iliescu and*

Baker, 2008; Weikusat et al., 2009; Rhodes et al., 2011; Hammonds and Baker, 2016; Stoll et al., 2021)."

You are making the reader do the work of linking what citations are experiments/ ice core analyses and which relate to grain size, impurities, fabric etc. This is a lazy approach and the sentence is useless to the reader. It is also misleading as the statement says " …. *on ice viscosity and deformation rates"*; not all of the cited papers have a direct link to this. The statement needs to be broken up: for example a statement on the lab stuff followed by a statement of the ice core stuff. The lab statement could be *"Laboratory experiments show that grain size (Goldsby and Kohlstedt, 2001), impurities (DahlJensen et al., 1997; Hammonds and Baker, 2016) and fabric (Azuma, 1995; Azuma and Higashi, 1984; DahlJensen et al., 1997) affect ice viscosity and deformation rates".* The equivalent ice core statement would need to be clear where there are data that correlate rates to microstructure (as your work does) as opposed to an inference in the paper that this is the case. I've not tried to write this as I don't know this literature so well and have no time to research robustly.

Issues as they arise in the order of the paper: *please also see annotated comments (there is overlap) as I am running out of time to transcribe everything here.*

L34-35. The citations do not support the statements you make. (Jacka, 1984) states: "All sets of tests demonstrated little or no dependence of the minimum ice strain rate on crystal size. In tertiary creep the strain rates were in closer agreement". This is the opposite of what your writing used Jacka for. The first part of the Jacka statement has been the basis for many researchers to ignore/ not believe grain size sensitivity. We now know that there is a complicated sample size/ grain size relationship that masks grain size sensitivity in some experiments (paper by Sheng Fan in prep explains Jacka's data). The latter statement by Jacka is the initial basis of a dependence of grain size on stress (Jacka and LI, 1994) but not grain size control on rheology. (Pimienta et al., 1988) states "As a result the high impurity content in glacial ice does not seem to influence the mechanical behavior of the Vostok ice core!! This is the opposite of what you attribute to Pimienta et al. (Duval et al., 1983) do show a deformation mechanism map (from (Frost and Ashby, 1982)) but the grain size dependent part of this has no experimental basis and is entirely superseded by Goldsby's work (Goldsby, 2006; Goldsby and Kohlstedt, 1997, 2001). Use Goldsby group work ad the main experimental basis for GSS creep.

L39 poor unspecific citations.

Fig 1. Units mixed up. Keep to the same units.

L62 Weikusat is in the authors list so why does this need a perss comm? Deal with this in the section where you say who did what.

L88-93 ?? A mess. See comment annotated.

Fig 3. Good examples of image data. Two things are clearly needed. One is annotation of all the features in the image (cloudy bands, areas of different size, bubbles). Second is the processed line diagram of grain boundaries and grains as used in analysis, as an example. This could be where you show examples of grain size distributions etc.

Line 115 needs citations for eigenvector approach (Vollmer, 1990; Woodcock, 1977)

L124-128 Poor writing. Very unclear. (DahlJensen et al., 1998) has nothing to do with borehole inclination and cannot be used as a citation here

L150 – 155 The data on fig 4 say nothing about fabric orientation, only about fabric intensity. Add the max eigenvector inclination on to fig 4 and you have fabric orientation.

L169 Grain size reduction is an evolution term. You should not use this unless you think (and evidence) that the grains in the fine layers were originally coarser and have become finer. On line 173 this is further confused: Zener pinning cannot be responsible for grain size reduction. It can limit grain growth. (Durand et al., 2006) talk about the inhibiting effect on grain growth, grain size reduction is not mentioned once in their paper. I think your use of "grain size reduction" is simply an English thing. You mean some layers have finer grains than others. Do not use language that implies an evolution unless you have evidence for that evolution.

L176 Comment from annotation "This is a bit mixed up here. There are two different grain size stress relationships. One is a "piezometer" that reflects the observation that dynamic recrystallisation generates mean grain sizes that correlate to stress magnitude (Platt, 2023). This is mentioned in Duval and Castlenau, citing Jacka and Li 1994. The second is smaller grains facilitating GBS (or more generally a grain size sensitive mechanism). This is not mentioned in Duval and Castelnau. Behn et al is a modelling paper that uses this relationship (from Goldsby & Kohlstedt), but is not a key reference."

Key GBS refs for ice include inferences from GSS behaviour in lab experiments (Fan et al., 2025; Goldsby, 2006; Goldsby and Kohlstedt, 1997, 2001; Qi and Goldsby, 2021) and inferences from ice microstructures (Craw et al., 2018; Fan et al., 2020). (Kuiper et al., 2020b) is not an original source reference for a composite flow law - he corrects a silly error in (Goldsby and Kohlstedt, 2001) and in (Kuiper et al., 2020a) he modifies the g&K composite flow law so that there is no viscosity change at the temperature where the cold and warm flow laws intersect. Unfortunately conclusions from (Kuiper et al., 2020b) regarding mechanisms in NEEM have to be re-evaluated, as they are in part based on a comparison of an axial composite flow law with an octahedral "Glen" flow law (see supp info for (Fan et al., 2025). (Fan et al., 2025) models NEEM with a variety of flow laws (all octahedral).

L184 Simple shear is a kinematic term - it has nothing to do with stress.

L186-192 Throw this away. Not useful

L196-197 Unintelligible

L209 There is no direct evidence that impurities facilitate GSS creep. There is evidence that impurities inhibit grain growth and this has an indirect effect on GSS creep.

L209 Copied from annotations "Very misleading statement. "Grain orientations same during event." says something about time, evolution, for which you do not have direct evidence. A simpler statement "CPO is the same in fine and coarse layers.." is much better. Grain orientations is a term that suggests individual orientations. Fabric CPO, COF are better terms as they infer that data are from many grains (statistical)."

Conclusions are terrible? Have another go.

Fig 4. Nice figure.
Label the stadial- interstadial or say which is which in legend/ caption. Not everyone is familiar with this (I am not)
Add inclination of eigenvector 3 to show orientation
` Add more to grain size (see section earlier: mean, quartiles etc)

Fig 5 One per grain or one per pixel data?

Fig 7. What are the grain size units. If these are pixels, change them for something useful.

Fig 8. Most of this figure is not needed (simplify it) and this approach is not well explained in the figure nor in the text. Please expand the description of what has been done and why important. In particular you need to make a clear link to yield surfaces and mechanical behaviour. One question I have is whether the results are dependent on the artificially imposed and unrealistic a-axis distributions (compare to a-axis data in (Journaux et al., 2019; Monz et al., 2021; Qi et al., 2019; Thomas et al., 2021). I think the VPFFT uses the full slip system so the a-axes could have an effect. It would be way simpler to just calculate sample basal plane Schmidt factors or to model using (Azuma, 1995) as done in (Fan et al., 2021a). Whatever way you do it it needs to be explained so the reader can understand what you have done and why it is important. If Llorens an author, she does not need citing. Do that in the outline of who did what.

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