

Author(s): Chauve and others

General comments

I find the paper is mostly interesting, glaciologically relevant, and well-written. Conversely, I believe its core purpose is unclear, I believe the main interpretation (of velocity-and-shear events) should be improved substantially, and the figures are far from well produced. The last of these is disappointing, particularly so considering the paper has a substantial list of co-authors.

I have a few comments.

- (i) I find the focus of the first part of the paper—borehole-based material characterization—to be interesting and to progress logically. I have a few specific suggestions, notably on some OPTV interpretation, stereo plotting, and the inferred liquid water content of the basal-zone ice (more below)—but it's fundamentally solid. However, I find the second part—on the characterization and interpretation of velocity/shear events—to be light on argument and rather unconvincing. This is probably partly due to the difficulties posed by multiple data sets that don't converge conveniently to support a single straightforward interpretation. Here, I find specific explanations to be selected without full and fair consideration of alternatives. Just one example is that Stoneley wave velocity in particular is sensitive to any number of physical controls, with the interstitial water content of the ice being just one. If the analysis was in relatively 'uniform' stratified ice then it might be a decent approximation, but it is applied in the debris-rich (and variable) basal ice where both the ice and presumably the borehole x-section change substantially. A 3-arm caliper may well not necessarily pick up on the latter.
- (ii) Related to (i), a fair bit of the initial englacial physical characterization—which I enjoyed reading and found to be interesting—is barely relevant to the paper's stated aim. The tangential elements include the OPTV-derived indication of primary stratification/foliation and open crevasses (which I agree with) and closed crevasses (which I don't; more below), in the glacier's uppermost ~60 m. The aim of the paper, and most of the argument/interpretation, focuses on deformation during two episodes of meltwater release and increased surface velocity; deformation that appears to be localized within or adjacent to the debris-rich basal ice. In this light, the elements noted above are given too much prominence in the paper. I would either amend the aim (but then it becomes a two-part paper with the risk of diluting both) or (better) reduce this section substantially—removing all OPTV other than the basal zone as well as perhaps Figs 2, 5 and 6—to focus more on the basal zone. I would expand the analysis of this OPTV image—presumably available at mm-resolution (is resolution stated in Methods?)—to approximate variability in debris concentration through the basal ice (see Miles and others on reconstructing englacial debris concentration in Khumbu Glacier). This would be of interest to basal ice characterization and to the mechanism of deformation/localized strain. It may be possible, since P-wave velocity scales with debris concentration, to discretize the smoothed sonic signal—guided by reconstructed variations in debris concentration—to a vertical resolution closer to that of the OPTV (at least to 10s of cm and perhaps to cm). That may be too much work for this paper though, but the analysis could go some of the way. My own inference from the data presented is that the shear is indeed localized in a way not hitherto appreciated, and that this focusing is largely, or perhaps entirely, related to structural weakness associated with the layering by debris concentration/texture within the basal ice. I'd pursue this angle more rigorously, based on a far closer examination of the OPTV image of the basal ice (which is really neat and almost deserves publishing on its own).
- (iii) Is it possible that the ice-bed interface (this is stated as bedrock in several places, but I don't believe that has been demonstrated) is at ~95 m depth, and the hole below this is drilled into subglacial sediment with or without ice matrix? I imagine the hot water drilling at this depth was challenging (given the debris that must have fallen into the borehole from the basal ice alone). Linked to this, the acoustic log stopping at 94.75 m suggests it may not have been lowerable any further. I know it's a longer probe, but did the base of the acoustic probe reach the inferred ice-bedrock interface? How consistent was this depth across different approximations? If the ice-bed interface is at 95 m then the shear events are closer to the interface and the lack of shear below this may be explained more readily.

- (iv) Where do the water pressure data come from? These are not noted in the Methods. Did the borehole into which the probe was installed drain to indicate a clear connection to the basal water system? Is it possible that the Pw sensor is effectively isolated from the basal drainage system after ~Oct 12?
- (v) The interpretation advanced for shear localization events within the basal zone should hold more generally and fairly frequently. Can a longer term record of shear and Pw be shown for this to be ascertained and for more general background fluctuations to be evaluated?
- (vi) I think the poles to plane dots on the stereo plots in Figure 3 (which I think should be Panels e – h for clarity) are the wrong way around—plotted as upper hemisphere plots rather than a lower hemisphere plots. It could be that they are upper hemisphere plots but that would contrast with structural glaciological convention, and it is anyway not stated. The method should be described more fully in Methods, and not restricted to the caption of Fig 3.
- (vii) I do not see evidence of dust layering (indeed, of dust at all) in the stratified/foliated ice in the OPTV logs (Fig 3a etc). I do see vertical variability in bubble concentration typical of such ice and I agree with the interpretation—just not the dust part.
- (viii) I do not see evidence of healed crevasses in the OPTV sections used to illustrate this (Fig 3c). Where I have encountered these (the paper is cited), these are very distinctive, with sharp boundaries that usually cut across existing foliation and even other crevasse traces. Healed crevasses may well be there, but the illustration does not convince me. Given the comments in (i) and (ii) above, I don't believe the paper will suffer from omitting this particular interpretation.
- (ix) An inferred interstitial/vein water concentration of 8%—although made with disclaimers—seems highly unlikely. That ice near the bed deforms more readily and includes more vein water—quite possibly pressurized up from the bed—is not at issue (DOI: 10.3189/S0022143000004068, DOI: 10.3189/172756403781815474), but substantial uncertainty in the approach (both the initial relationship and the field-derived softness/fluidity) do not justify this conclusion. Duval's relationship may well not even be applicable in such a setting. Finally, I'm unsure but 8% may be impossibly high (see work by Nye and in particular Mader: 10.3189/S0022143000002227, 10.3189/S0022143000002240).
- (x) Finally, the general conclusion that models/glaciologists need to account for glacier heterogeneity is a little simplistic; I'm sure modelers are aware of this approximation, and also that a general constitutive relation actually does a pretty good job at the time and space scales at which such models are generally applied. Field glaciologists, are also aware of heterogeneity at the scale revealed in this paper. However, associating heterogeneity within the basal ice with discrete shearing would allow this statement to be recast more specifically and usefully.

Specific comments

Line/Location	Comment/Suggestion
17-20	In fairness, studies are aware of this and, where relevant, introduce an additional E or similar to represent enhancement, however approached in terms of physical properties.
70	Suggest Methods
72	If it is polythermal then that may well be relevant to the interpretation etc. Is it at the study site—and what is the evidence? The rest of the paper generally seems to assume it is temperate...
76	Note borehole length and whether it reached the bed and any other potentially relevant aspects of the drilling. Drilling is under the 'fieldsite' heading; perhaps a separate section on drilling.
88	'long-section'?
Fig 1b	I think elevation rather than altitude
Fig 1c	Yes, the forefield is rock, but there is also debris on the glacier and substantial basal ice (as revealed by your OPTV images) so what is the evidence that the bed is not mixed or patchy?
S221	Something on stereo plotting here.
97-8	That it is granite is an inference and not a Method (I don't doubt it is, but the lithology is really a result of this study)
100	Suggest settling rather than decantation. Great image.

105	I think knowing the borehole diameter exactly (or as well as 3 arms can do) improves the calculation of layer dip, not orientation.
Fig 2	I don't believe this figure is needed and a citation to a geophysics text will suffice, as done for OPTV.
117-9	Would it not be appropriate to give the full velocity range here: from minimum (single CPO orientated perpendicular), through no CPO/isotropic, to maximum (single CPO orientated parallel)? And it may be worth noting that this assumes a single max, not a girdle or multiple modes.
179 and elsewhere	Here, in contrast to elsewhere a x is used to indicate multiplication. Generally this is just a space—as used mostly throughout. However, in many places, and in figures in particular, a . is also used. If this is not intentional to denote some difference, then I'd delete those too.
Fig 3	I'd label the stereo plots Panels e-h
	Here and in many other places, the axis label text capitalization is inconsistent ('depth' should be 'Depth'; 'width' should be 'Width' etc). With 12 authors to check, one might be tasked with this before re-review...
	I'd call the x axis on the OPTV plots 'Perimeter' or preferably 'Circumference' rather than 'Width'.
	I think the points on the stereo plots are the wrong way around (or they are, unconventionally, upper hemisphere)
Fig 3a and caption	This is almost certainly not 'Dust layering'; even the rest of the manuscript interprets it as stratified or foliated ice, which I believe it is.
Fig 3c and caption	I see no evidence for a healed crevasse here
Fig 4a	Needs an x axis label
Fig 4b	'Time' and not 'time' (there is a lot of this and I'll not mention again—please check throughout)
Fig 4c	Orientation should be 'W' and not 'O', and I'd put these labels at the top for consistency and convention. Also, Fig 3 includes 'width' but not here. I do actually think it is useful for the reader to appreciate the feature scale.
233	These layers are highly unlikely to be 'dust foliation planes'. Whether they are (primary) stratification or derived/transposed foliation is probably for another study—Hambrey has written a lot on that, but I'd be happy with 'highly deformed and rotated primary stratification' for now...
249	Is the increase significant? If so, give P. I'd quantify the increase anyway since the data have been recorded.
256 and elsewhere	Use of . to indicate multiplication in the unit.
261-2	Yes, lower than uniform, but is it lower than single max CPO orientated orthogonal to the borehole? (I know such a crystal configuration is unlikely, but in theory is the velocity lower?)
Fig 6	I cannot see evidence of a healed crevasse in Panel d.
273-4	Pw needs to be explained fully in methods.
297->	I think some of this is Method and not Result
Fig 9	Panel labels d - f in caption are given after the text inconsistently
	Panels c - f y axis uses solidus inconsistently
	Panel b has a x axis label (incorrect in several ways), but the base of the figure does not. It should have the label Time (date, 2024).
	Panels b, d, e and f have labels 'RBT_H' etc. that I believe serve no purpose
	Panel b; interesting, but with such a velocity event, it seems highly unlikely that there was zero signal in the subglacial water pressure. Even if not the cause, the ice velocity itself would surely cause a subglacial Pw response. This is what makes me think the probe is isolated at this time.
Fig 10	Panel labels too small and inconsistently at the base
	Panel d; Paterson (not Patterson)
352	Not sure foliation planes are associated with significant impurities, or at least have not seen evidence of this.

355	I see no evidence of dust in Fig 3a. It is there in stratified ice since it gets incorporated during firnification—but at such low and fairly steady concentrations that it can only be seen in OPTV images where concentrated, for example by some specific event. These are rare and cannot really be associated with the physical properties of stratified glacier ice.
360	Citation to Hambrey needed here; they have specifically written about the transposition of stratified ice to (long-) foliated ice
363->	Given I see no clear evidence of healed crevasses/crevasse traces in Fig 3c and I believe the stereo plots are plotted incorrectly, I would look at this section again. Indeed, this structural glaciology doesn't really address the stated aim at all does it? Is the paper OPTV report, structural glaciology or focused basal shearing? I suggest paring the paper down to address (only) its stated aim—the focused basal shearing.
364	Incidentally, I think the open crevasses do in fact have a preferred orientation.
Fig 11	What is that right hand y axis scale: all y axis value lines are +10? Axis labels not capitalized
	The major tilt excursion at 95 m is particularly interesting in that almost all of the initial (reverse) shear is recovered immediately after. Normally, I'd look at a method-related explanation for this—but I suppose it could be some kind of elastic recovery. It could be off-plane too. I'd look at this in more detail to see if anything can be teased out.
369-70	I think this is over-speculative given the data.
374-5	Here is the velocity for a single max aligned perpendicular, which I argue above should really be presented earlier.
391-2	But we know that debris does influence P-wave velocity... This apparent absence of correspondence cannot simply be used to claim 'little influence'. Indeed, the paper's own data—shown in B3—indicates an association between debris and P-wave vel; in this case reducing velocity likely due to the fractured/unconsolidated nature of the layer (as opposed to bedrock).
415	I think the Stoneley-wave velocity signal is over-interpreted here, or at least insufficient clear and progressive argument is dedicated to isolating other influences. Water is not either interstitial or macroporous in this zone; it is almost certainly both and the discussion should be nuanced to this.
418	I'd focus more on this relationship between debris concentration (and grain-size/texture), meltwater, and net material softness or shear/yield strength.
433	The analysis needs to address why Pw does not change with this event. This section and analysis is interesting but I'd like it to progress in a more structured—and therefore understandable—way. Maybe sub-headings for each event and a brief summary of the properties associated with each before a coherent and progressive interpretation is argued.
462	'less pronounced structural heterogeneity' is unnecessarily vague; can the difference be stated (I'd say 'focused within englacial ice located * m above the upper surface of the debris-rich basal ice')
457	I don't really like use of fluidity for the ice, and this is particularly notable here where the basal ice 'exhibits enhanced fluidity' when also addressing water content and flow. Is there a good reason not to use the term 'softness' rather than 'fluidity' for the ice? I also would urge use of 'roughness' rather than 'rugosity' for bed roughness. The latter isn't incorrect, but it is rarely used.
Fig 12	I think this conceptual model should be the focus of a shorter and tighter paper. Incidentally, didn't Hilmar Gudmundsson run a finite element model recreating circular flow in such hollows (scale independent I think) and Rob Law/Poul Christoffersen 2023? Bedrock (not Bed Rock)
	I'd say the background colours are too bold for the text (and the eye) here (also Fig 2, but to a lesser degree)
482-4	This disclaimer is true and should really be stated earlier to underpin the nature of the interpretation