Response to reviews on *Fibre-optic strain sensing on an alpine glacier* (egusphere-2026-3329)

October 2026

We thank the editor and both reviewers for their time and comments. Reviewer comments are reproduced in boxes, numbered by reviewer, with our responses following each comment.

Before our response to individual comments we highlight a change to the component of this paper that links theory to observations. This was prompted by our own curiosity and that of reviewer 2 regarding compression towards the top of the column.

In our initial model we assumed that the borehole is perpendicular to the bed. In the more accurate case for a steep alpine glacier where it is in fact tilted upslope relative to the bed normal, the expected behaviour fundamentally changes from extensional to compressional.

The maths gets a little complicated, hence the delay in posting this response. We produce equations that describe a translation from fibre-recorded strain to tilt and $\frac{\mathrm{d}d}{\mathrm{d}z}$ profiles (Eqs. 3 and 4 in this response).

We also find that the steeper the slope, the lower the relative error when obtaining parameters for Glen's flow law – resulting from both a borehole installed away from the vertical, and from longitudinal/latitudinal strain complicating the assumptions of plane strain. And, that early re-measurement (within months, rather than after a year) should facilitate more accurate estimates of flow parameters.

In the case of assessing uncertainty from longitudinal strain – reviewer 2 suggests the use of a full-Stokes model but we find our approach, outlined in Section E, to be much more flexible. We agree that longitudinal strain influences flow-law parameter estimates. An issue in previous papers, which we will address in our revision through the inclusion of Section E, is that this uncertainty is not quantified in the general sense.

In sum, we anticipate that these additions – description of expected compressional behaviour, equations to obtain $\frac{\mathrm{d}d}{\mathrm{d}z}$ under this framework, analysis supporting site selection and measurement timing to minimise uncertainty, and longitudinal uncertainty quantification – will be of great use to those planning future fibre-optic strain sensing installations.

Beyond this, we can be even more confident that the extension towards the bottom of the fibre is a result of slip at the ice-bed interface. We therefore disregard any rheological interpretations based on the uncoupled regions of the fibre. However, using the new framework we can actually reject $n = 1$ for the upper column (Fig. 11). We will report this in the revised version, but not make it central to the story.

Below, we highlight the main aspects of this new approach. We intend to implement this comprehensively in the revised manuscript. The updated equations link back directly into the quantities described by Eqs. 7, 8, and 9 of the original submission meaning that the sections describing the DSS installation and fitting of n and A function as before.

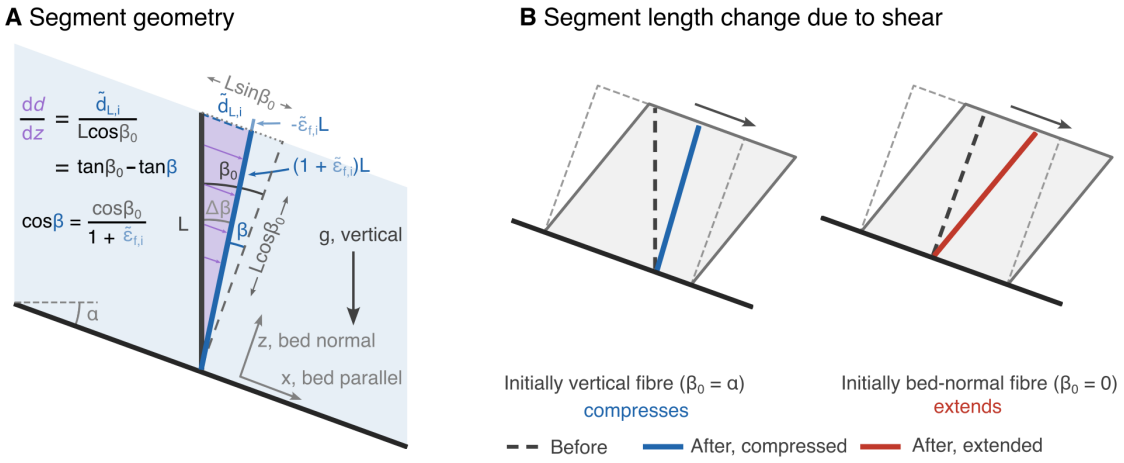


Figure 1: **Fibre-segment geometry and length change due to shear**. **A** A segment i of length L in a vertical borehole on a bed sloping at angle α (here 20°) and the same segment after bed-parallel shear. At installation the segment is at angle $\beta_0 = \alpha$ from the bed-normal and moves to angle β following shear ($\Delta\beta$ relative to vertical). **B** Bed-parallel shear acting on initially vertical and bed-normal segments, showing compression and extension, respectively. These are for the lowermost (bed-adjacent) segments for illustration but the mechanics are the same for segments in the mid-column.

Overall, we hope that we have successfully outlined all of the challenges one may face when using this technology, but also provided a clear methodology toward gathering transformative information regarding ice rheology in the future.

The sections below are labelled by letter for reference within the reviews.

A: Calculating tilt and horizontal displacement from a vertically installed borehole

Slightly counter-intuitively, for a borehole installed vertical, and therefore tilted by an angle β_0 from the bed-normal, the installed fibre segment will initially compress rather than extend (Fig. 1A). Only after the segment has rotated past the bed-normal position will extension occur (Fig. 1B). For the purpose of obtaining $\frac{\mathrm{d}d}{\mathrm{d}z}$ from observations along the fibre – which can then be compared to a theoretical displacement profile (Section 2.4.1 in the original manuscript, Section titled Fitting for n and A below) – we wish to obtain expressions for $\frac{\mathrm{d}d}{\mathrm{d}z}$ in terms of $\tilde{\epsilon}_{f,i}$, the observed extension or compression of each fibre segment. These are Eqs. 3 and 4, with derivations outlined below.

To obtain these equations, we describe the orientation of a fibre segment by its angle, β, from the bed-normal (positive when its upper end lies upslope) with β_0 its value at installation ($\beta_0 = \alpha$ for a vertical borehole) and $\Delta\beta = \beta_0 - \beta$ its rotation away from vertical (Fig. 1A). For a segment of length L at angle β from the bed-normal (Fig. 1), with bed-normal and bed-parallel extents $L\cos\beta$ and $L\sin\beta$, the upward-pointing unit vector along the segment is $(-\sin\beta, \cos\beta)$ in (x, z), with x pointing along flow parallel to the bed and z normal to it. This approach follows the shallow ice approximation (SIA)

$$u_\mathrm{d} = \frac{2A}{n+1}\left(\rho_\mathrm{i} g \sin\alpha\right)^n h^{n+1} \tag{1}$$

where u_d is surface velocity due to deformation, A and n are the rate factor and flow exponent in Glen's flow law, respectively, ρ_i is ice density, and g is acceleration due to gravity. The upper

Table 1: Default parameter values. The rate factor is the default value for ice at 0°C from Cuffey and Paterson (2010).

Parameter	Symbol	Value	Unit
Initial borehole tilt from vertical	θ_0	0	°
Glen exponent	n	3	–
Rate factor	A	2.4×10^{-24}	$\mathrm{Pa}^{-3}\ \mathrm{s}^{-1}$
Ice density	ρ_i	910	$\mathrm{kg\ m}^{-3}$
Gravitational acceleration	g	9.81	$\mathrm{m\ s}^{-2}$
Surface and bed slope	α	20	°
Installed fibre length	L_0	40	m
Bed-normal ice thickness, $L_0 \cos\alpha$	h	37.6	m
Initial segment angle from the bed-normal, $\alpha - \theta_0$	β_0	20	°
Modelled surface velocity from ice deformation using the SIA	u_d	2.15	$\mathrm{m\ yr}^{-1}$

end of the segment then moves downslope relative to the lower end at

$$\Delta u = \frac{\mathrm{d}u}{\mathrm{d}z} L \cos\beta, \tag{2}$$

where u is velocity in the x direction. Integrated over time t from installation, this gives the displacement d in the x direction as $\frac{\mathrm{d}d}{\mathrm{d}z} = \frac{\mathrm{d}u}{\mathrm{d}z} t$.

Considering the measured extension or compression of a fibre segment, $\tilde{\epsilon}_{f,i}$, its angle from the bed normal, β, is obtained from the constraint that the bed-normal height of the segment ends will not change. The segment initially has length L at angle β_0 from the bed-normal and a bed-normal extent $L\cos\beta_0$. Following deformation its length is $(1+\tilde{\epsilon}_{f,i})L$ at angle β from the bed-normal with a bed-normal extent of $(1+\tilde{\epsilon}_{f,i})L\cos\beta$. Equating $L\cos\beta_0 = (1+\tilde{\epsilon}_{f,i})L\cos\beta$ and dividing by $(1+\tilde{\epsilon}_{f,i})L$ gives

$$\cos\beta = \frac{\cos\beta_0}{1+\tilde{\epsilon}_{f,i}}. \tag{3}$$

With β known, the corresponding bed-parallel displacement of the upper end of the segment relative to its lower end is the reduction in its bed-parallel extent, $\tilde{d}_{L,i} = L\left[\sin\beta_0 - (1+\tilde{\epsilon}_{f,i})\sin\beta\right]$ (Fig. 1A). Dividing through by the bed-normal height $L\cos\beta_0$ then gives

$$\frac{\mathrm{d}d}{\mathrm{d}z} = \frac{\tilde{d}_{L,i}}{L\cos\beta_0} = \tan\beta_0 - \tan\beta \tag{4}$$

where Eq. 3 is used to substitute $1+\tilde{\epsilon}_{f,i} = \cos\beta_0/\cos\beta$. Eqs. 3 and 4 can then be used to fit strain results for n and A (Section 2.4.1 of the original manuscript, and Section B below).

The whole-fibre length change, ΔL_total, is

$$\Delta L_\mathrm{total} = \int_0^{L_0} \epsilon_f(s)\,\mathrm{d}s \approx \sum_i \epsilon_{f,i}\,L, \tag{5}$$

where s is distance along the fibre at installation and L_0 is the length of fibre at installation ($L_0 = H$ in the case where $\beta_0 = \alpha$, where H is the vertical ice thickness). This demonstrates – for default parameters reflecting our borehole (Table 1) – first a compression and then an extension of the fibre (Fig. 2).

B: Fitting for n and A

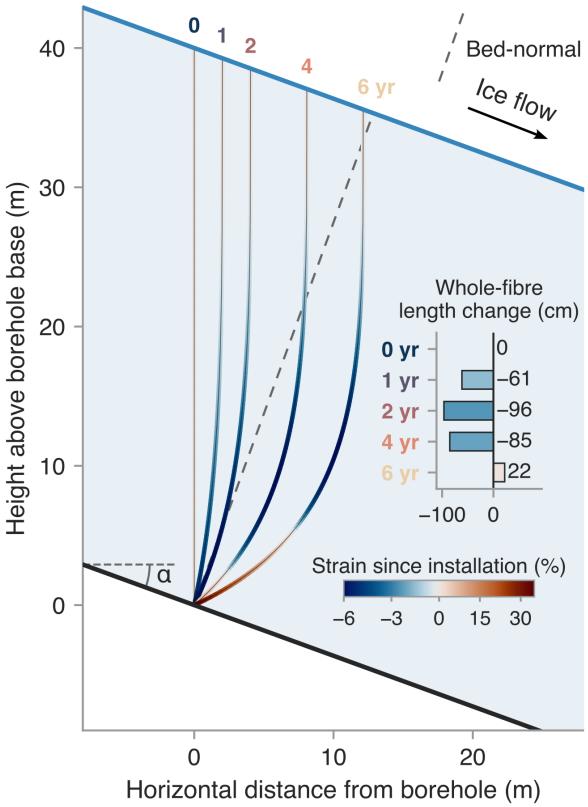


Figure 2: **Deformation of an initially vertical fibre in a shearing slab**. Shallow ice approximation flow with $L_0 = 40$ m and default parameters from Table 1. Colours show the strain of the fibre since installation, calculated with Eq. 6 from the SIA displacement profile (Eq. 6 of the original manuscript) at each time. The inset shows change in whole-fibre length relative to the installation.

Note that this Section has only minor alterations in light of the above. We explain these changes below rather than re-printing in full. The changes will be fully integrated in the revised manuscript.

Eq. 9 for $\epsilon_f(z;n)$, the expected cable strain as a function of n from the original manuscript becomes

$$\epsilon_f(z;n) = \sqrt{1 - \frac{\mathrm{d}d(z;n)}{\mathrm{d}z}\sin 2\beta_0 + \left(\frac{\mathrm{d}d(z;n)}{\mathrm{d}z}\right)^2 \cos^2\beta_0} - 1 \tag{6}$$

in order to account for the fibre being installed vertically rather than bed-normal. The original expression, $\sqrt{1 + \left(\frac{\mathrm{d}d(z;n)}{\mathrm{d}z}\right)^2} - 1$, assuming a bed-normal installation is reproduced from Eq. 6 when $\beta_0 = 0$.

Eq. 6 is then used in the original manuscript Eq. 8 rather than a simpler $\mathrm{d}d/\mathrm{d}z$ term to give

$$\Delta L_{\text{total}} = \frac{1}{\cos\beta_0}\int_0^h \epsilon_f(z;n)\ \mathrm{d}z\,, \tag{7}$$

where we also change δ_{total} to ΔL_{total}. Beyond this, Section 2.4.1 of the original manuscript will be unchanged.

Eq. 6 gives strain since installation. For strain measured between two surveys at t_1 and t_2 with lengths L_{t_1} and L_{t_2} the measured strain is

$$\tilde{\epsilon}_{f,i} = \frac{L_{t_2} - L_{t_1}}{L_{t_1}} = \frac{1+\epsilon_f(t_2)}{1+\epsilon_f(t_1)} - 1. \tag{8}$$

Similarly, β_0 in Eqs. 3 and 4 is then the segment angle at t_1.

We also note that section 2.5 of the original manuscript, Rate factor A rescaled to $n = 3$, does not need adjusting relative to the new theory.

C: Relative rates of change in fibre segment orientation and strain

Going further (and required for assessing uncertainty from longitudinal strain, Section E), we can derive expressions for the rate of change of tilt and length change as the fibre passes from upslope to downslope relative to the bed-normal.

Only part of the bed-parallel motion in Eq. 2 acts along a segment tiled by β from the bed-normal. The part along the segment, $-\Delta u \sin\beta$, changes its length. The remainder, $\Delta u \cos\beta$, acts across the segment and rotates it downslope without changing its length. Substituting $\Delta u = \frac{\mathrm{d}u}{\mathrm{d}z} L\cos\beta$ from Eq. 2 into each component gives a strain rate $\frac{1}{L}\frac{\mathrm{d}L}{\mathrm{d}t} = -\frac{\Delta u}{L}\sin\beta = -\frac{\mathrm{d}u}{\mathrm{d}z}\sin\beta\cos\beta$ and rotation $\dot{\beta} = -\frac{\Delta u}{L}\cos\beta = -\frac{\mathrm{d}u}{\mathrm{d}z}\cos^2\beta$. Writing the velocity gradient in terms of the bed-parallel shear strain rate, $\dot{\epsilon}_{xz} = \frac{1}{2}\frac{\mathrm{d}u}{\mathrm{d}z}$, gives the along-fibre strain rate and the rotation rate of the segment,

$$\dot{\epsilon}_f = \frac{1}{L}\frac{\mathrm{d}L}{\mathrm{d}t} = -\frac{\mathrm{d}u}{\mathrm{d}z}\sin\beta\cos\beta = -\sin 2\beta\ \dot{\epsilon}_{xz}, \tag{9}$$

$$\dot{\beta} = -\frac{\mathrm{d}u}{\mathrm{d}z}\cos^2\beta = -2\cos^2\beta\ \dot{\epsilon}_{xz}. \tag{10}$$

D: Uncertainty from borehole tilt

Errors will arise if the borehole installation is not strictly vertical but the inversion assumes that it is. We quantify this through an angular displacement of θ_0 along flow (positive when a segment's upper end lies downslope of its lower end, so that $\beta_0 = \alpha - \theta_0$). We take the borehole to be straight, so that θ_0 is the same at all depths.

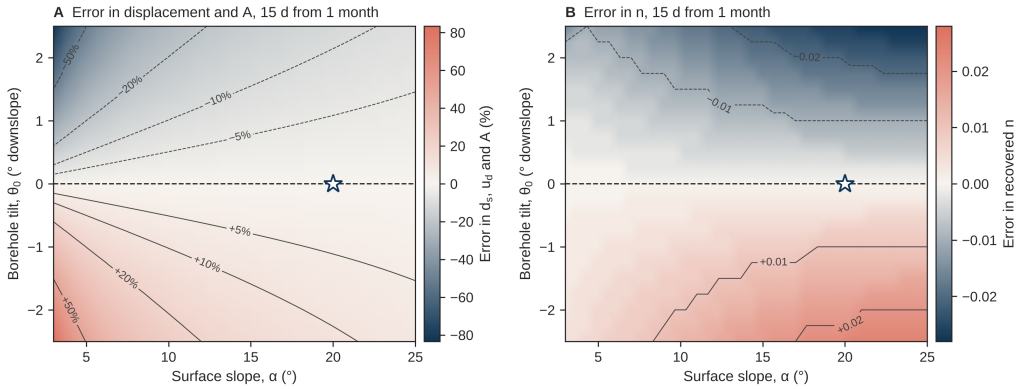


Figure 3: **Error from offset borehole tilt** for a borehole tilted along flow by θ_0 but assumed vertical ($\beta_0 = \alpha$). **A** Relative error in u_d and A, and **B** error in n for a 15-day interval starting one month after installation. In both panels the dashed line marks a vertical borehole and the star marks CJ1G. Ice thickness and flow parameters as in Table 1.

To quantify the error resulting from a deviation away from a vertical borehole we generate a synthetic profile of the strain measured over a survey interval of $t_\mathrm{m} = 15$ days for each combination of slope, α, from 3 to 25° and tilt, θ_0, from −2.5 to +2.5° as Fig. 3, with the interval beginning at a time t_1 of one month after installation.

Each profile is calculated as the fibre strain, $\tilde{\epsilon}_{f,i}$, over the interval, t_m, from the change in length of each segment between the start and end of the interval using the strain since installation, $\epsilon_f(z; n)$, from Eq. 6 evaluated at both times, using the SIA displacement profile (Eq. 6 of the original manuscript) and parameters from Table 1.

We then treat each generated profile as real data and fit it as in Section B (Section 2.4.1 of the original manuscript) for a vertical borehole, with the modelled strain over the interval calculated as above. For each trial n, d_s is set so that the modelled total extension (Eq. 7) matches that of the profile, and the fitted n is the one that minimises the strain RMSE (Eq. 10 of the original manuscript). The errors shown are the fitted d_s relative to the true displacement over the interval, and the difference between the fitted n and 3.

Shortly after installation the tilt mainly rescales the calculated displacement. One month after installation, a tilt of 2.5° gives an error for d_s of 12 % at $\alpha = 20°$ but 50 % at 5° (Fig. 3A). Tiltmeters would mean these errors could be effectively eradicated. As the error for displacement is close to a uniform rescaling, it has little effect on the shape of the recovered profile, and hence on n. In the interval starting one month after installation the fitted n at CJ1G is within 0.03 of the true value for the range of θ_0 shown (Fig. 3B). Measurements made soon after installation are therefore suitable for obtaining flow parameters.

E: Uncertainty from longitudinal strain

The inversion and fitting above assume the SIA, in which shear is the only non-zero strain rate. This situation is complicated in reality by bed-normal strain arising from longitudinal compression or extension and latitudinal convergence or divergence.

We can explore this through Eq. 9, with the addition of a bed-normal strain rate, $\dot{\epsilon}_{zz}$, to give

$$\dot{\epsilon}_f = -\sin 2\beta\ \dot{\epsilon}_{xz} + \cos 2\beta\ \dot{\epsilon}_{zz}, \tag{11}$$

where $\dot{\epsilon}_{zz} > 0$ is longitudinal compression. Note that in 3D $\dot{\epsilon}_{xx} + \dot{\epsilon}_{yy} + \dot{\epsilon}_{zz} = 0$ such that the $\dot{\epsilon}_{zz}$ term in 2D can act as a proxy for latitudinal strain also.

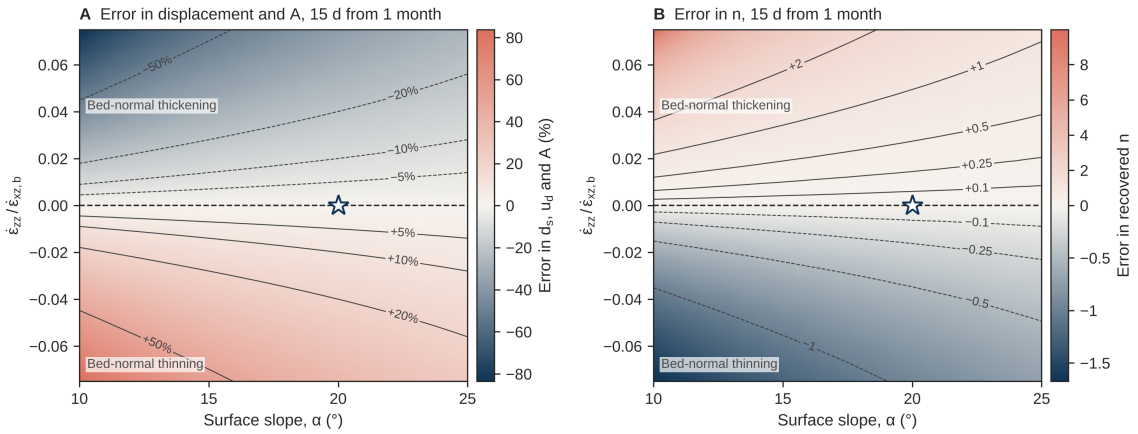


Figure 4: **Error from unmodelled bed-normal thinning and thickening.** Strain over a 15-day interval is computed for SIA shear plus a uniform bed-normal strain rate and fitted as in Section B, assuming the SIA, for a vertical borehole. **A** Relative error in the recovered surface displacement, and hence in u_d, and A, and **B** error in the recovered n, for an interval starting one month after installation. Ice thickness and flow parameters as in Table 1.

The SIA inversion attributes all of the measured $\dot{\epsilon}_f$ to the $\dot{\epsilon}_x x$ component, giving an inferred shear strain rate denoted $\tilde{\dot{\epsilon}}_{xz}$ such that $\dot{\epsilon}_f = -\sin 2\beta\ \tilde{\dot{\epsilon}}_{xz}$ (Eq. 9). Equating this with Eq. 11 and dividing by $-\sin 2\beta$ gives

$$\tilde{\dot{\epsilon}}_{xz} = \dot{\epsilon}_{xz} - \frac{\tan 2\beta}{\dot{\epsilon}_{zz}}\ . \tag{12}$$

We take a constant $\dot{\epsilon}_{zz}$ for the purposes of this analysis, which means that the relative error will increase upwards through the column as $\dot{\epsilon}_{zz}$ decreases.

We display this in Fig. 4, constructed as for Fig. 3 but for a vertical borehole and uniform $\dot{\epsilon}_{zz}$ alongside the derived $\dot{\epsilon}_{xz}$ for each slope, normalised relative to the basal shear strain rate, $\dot{\epsilon}_{xz,\mathrm{b}}$. Unmodelled longitudinal compression therefore underestimates d_s, u_d and A and overestimates n, and longitudinal extension does the opposite (Fig. 4). A realistic range for $\dot{\epsilon}_{zz}$ through the column is best obtained from a full-Stokes model of the glacier (e.g. Gagliardini2013) constrained by the observed surface velocities. However, this analysis provides a flexible way for assessing these uncertainties in the first instance, and in the case of site selection. As in the case of uncertainty from borehole tilt, this analysis also shows that repeat measurements made shortly after installation are suitable for obtaining flow parameters, granted that the borehole is suitably coupled to the ice.

F: Application to alpine glaciers

For a vertical borehole the deformation the cable must survive following Section A is set by compression rather than extension. At CJ1G the basal segment passes -1 % within two months and the segment at 30 m depth within four, and the basal segment reaches its maximum compression after 1.7 yr (Fig. 5. This implies that earlier measurement of the data, ideally within the same season as installation, is preferable.

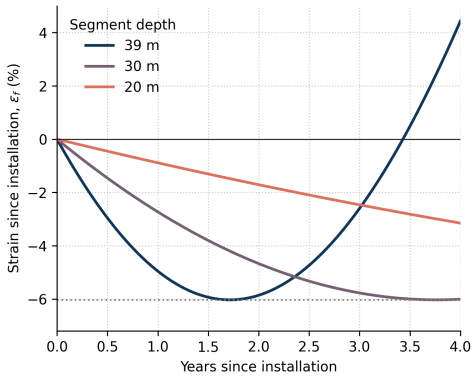


Figure 5: **Strain since installation at three segments** 39, 30 and 20 m below the surface in a 40 m vertical borehole, for default parameters (Table 1).

Reviewer 1

General comments

1.1 This is an interesting and well written study demonstrating a variety of fiber-based measurements techniques in temperate ice. The authors have done a very good job of both reviewing the techniques but also making the most out of a challenging measuring environment (a weakly freezing borehole!). The paper is a sound contribution.

Response: Thank you!

1.2 As a general comment, I would suggest that the authors expand their comparison of resolution of these techniques to other vertically resolvable measurements, such as APReS, etc.

Response: We will compare the resolution of DSS to APReS, GPR and active-source seismic. APReS also provides a useful constraint on ice extension/compression, and we will add this to the discussion regarding reviewer 2's points, as it potentially offers a route towards separating vertical strain and plane strain in future efforts.

Specific comments

1.3 Line 74 While Raman backscatter can be used independently, the backscatter return can be significantly altered by local strain on the fiber, requiring more sophisticated fiber geometries (double ended) to account for non-uniform attenuation. I would suggest noting this here that even Raman sensing is not independent of strain and strain distribution. A small point but helps demonstrate the linkage between strain and temperature in all of these techniques.

Response: Thanks, we agree. We will include the connection between Raman DTS and strain (distribution) and why double-ended configurations are important in this regard. I (RL) exploited this in an earlier DTS paper (Law et al., 2021) which we will mention.

1.4 Line 163 An overstuff of 1% appears large by some industry standards. Can you please double check this?

Response: We think this is standard (and is standard for FO cables more generally, https://www.rdm.com/blog do-fiber-optic-cables-shrink/) but we could not find this information in the data sheets for our cables. We will replace this with 'while a loose-tube cable features a fibre excess length to minimise strain transfer'.

We will also replace the term 'loose buffered' with 'loose tube'.

1.5 Line 185 I do not see a reference for the borehole diameter; can you give the reader some sense of the expected thermal equilibration time following freeze in? Some further discussion here on the borehole thermal equilibration would be helpful. While this is discussed further in the manuscript, I believe pointing out that freeze in in a temperate setting such as this is quite challenging to give the reader a foreshadowing of some of the issues that will arise later on.

Response: The borehole freezing model I (RL) have used deals only with cold ice (Greenler et al., 2014, https://doi.org/10.3189/2014AoG68A033) and we are not aware of similar models adopting an enthalpy framework. The clearest information on this problem comes from the work of Harrison (1972, https://doi.org/10.3189/S0022143000022450), during the golden age of temperate ice research, which we cite in the discussion.

Following comment 1.5 we will introduce this problem earlier, in the Instrumentation section, as well as discussing it in the Discussion. On rechecking Harrison (1972) we find this value to be 1.7 mm week^{-1} of diameter reduction, rather than 1 mm week^{-1}, which we will update.

1.6 Line 316 This is the first appearance (I may have missed it earlier) of an assumption of fiber failure at 1% strain. This may be a bit confusing to some readers when compared with the 1% overstuff reported in the loose buffered cable for temperature. I would suggest devoting a greater discussion to the maximum strain that can be measured by typical tight buffered (or bare fiber that matter). This would be helpful for other future uses. For example, does your tight buffered fiber contain any strength elements such as Kevlar? If so, how would be impact the potential for viscoelastic slip along the fiber. It may be just as easy to use bare fiber in this case, to ensure that there is not strain resistance due to other components of the tight buffering.

Response: We will introduce cable overstuff, cable failure, and the construction of the tight-buffered DSS cable within the Instrumentation section (and retain it within the Discussion, too). We will then add to the Discussion to include commentary about how strength elements in the tight-buffered cable may interact with strain readings and how this is a balance between cable strength and cable deformation – essentially the cable should be strong enough to survive handling and installation, but weak enough that it does not influence the movement of the ice itself.

See also our response to 1.4, but we do not have data for the exact amount of overstuffing. We will update the text to remove the 1% overstuff statement. We will keep the reference to failure at around 1%, but also cite Law et al. (2021) who did an independent lab test for fibre failure to illustrate that there is usually a mismatch between manufacturer reporting and the actual failure point.

Following Section A of this document, the larger concern now is actually compression. This is something less clear from manufacturers. Failure through compression would also mean that the cable does not buckle laterally. In an incompressible mass of ice we do not think this should be the case but we will flag this in the revised manuscript for future practitioners of DSS. We also recommend fairly fast repeat measurements, as the rate of strain in the revised method (Section A of this document) is much higher than the original methodology.

Last, while the bare fibre may be 'safe' once frozen in, a larger problem is that the very delicate fibre is severed during surface operations, snowfall settling, etc.

There is a 'multi-layer buffer and strain transfer layer with interlocking system' within the strain-sensing fibre. We do not have information on the exact material but we will include the information we do have from the datasheet in the revised manuscript.

We conducted a return visit in July 2026. This revealed severed DTS and DSS fibres due to snow movement over winter, despite the strengthening (see Fig. 6, below). A further return trip in August 2026 to repair the cable through a fusion splice was called off as the path to the field site had collapsed (Fig. 7). We will also include these details in the revised manuscript.



Figure 6: Severed DSS fibre, Chessjen 10 July 2026



Figure 7: Collapsed path to Chessjen field site, 26 August 2026

1.7 Radial strain during freeze-in And while on the subject, readers may ask if there are radial strain issues on the fiber during freeze in that could impact longitudinal strain measurements. Tyler et al. (https://doi.org/10.3390/photonics11070630) demonstrated very negligible impacts to single mode fiber under high stress conditions suggesting that freeze in is unlikely to impact at least Raman backscatter.

Response: Thank you for this reference. We will include it in reference to DTS. We have in fact also found a couple of relevant references for Rayleigh and Brillouin pressure sensitivity (Palmieri et al., 2022, https://doi.org/10.3390/s22186811 and Gu et al., 2012, https://doi.org/10.3788/COL201210.100604 respectively). This could result in an influence of 5 – 20 $\mu\epsilon$ MPa^{-1} (roughly 100 m of ice), so around 2 – 7 $\mu\epsilon$ at the base of our borehole ($\sim$0.3 MPa). That would be enough to be observable (just). However, both DSS techniques measure relative strain, so unless pressure changes significantly this is unlikely to be observable in practice. We will add this to the discussion, and also hint at the possibilities of the technology for extra-terrestrial missions as we believe that is an interesting use case.

Reviewer 2

Overall assessment

2.1 This manuscript presents the first application of fibre-optic Distributed Strain Sensing (DSS), combined with the also innovative application of fibre-optic Distributed Temperature Sensing (DTS), to the measurement of deformation within a glacier borehole. The authors compare Brillouin- and Rayleigh-based measurements and combine these observations with distributed and discrete temperature measurements. The study is technically innovative, and the overall agreement between the different strain measurements suggests that DSS has considerable potential for resolving along-cable strain at spatial scales inaccessible to conventional borehole instruments.

Response: Thanks, we are happy to receive this positive feedback regarding the foremost focus of this paper.

2.2 However, the mansucript's central message goes beyond the technical applicaiton of DSS/DTS, concluding that the measurements support a near-linear rheology for near-temperate ice (outlined in the abstract and throughout). This conclusion is of primary importance since such change in rheology compared to the classically used Glen's flow law with $n = 3$ has a strong impact on glacier modelling strategies and outcomes. In my view, however, this finding is not supported by the authors analysis. The three most prominent problems I have are the following ones. First, as acknowledged by the authors, the cable in the bottom section from 23 to 35 m deep expected to moslty constrain the rheological law is not coupled, causing the inferred deformation to be linear, consistant with a linear rheological law best fitting this artefact. Second, the interpretation of the DSS measurements using the plane-strain approximation is highly questionnable. One evidence of this assumption being violated here is that the displacement predicted at the surface from integrating the strain over depth is four times higher than the independent GNSS observation. Finally, the authors appear to be using the shallow ice approximation to estimate shear stress with depth, which has recently been demonstrated to not hold at least on an Alpine glacier configuration, such that borehole deformation measurements may provide little constraints on the Glen's flow law exponent (Roldan Blasco et al., 2025).

Response: This is Reviewer 2's most significant comment, and, following Section A we agree that our record does not support a near-linear rheology. The near-bed extension recorded is then more straightforwardly interpreted as being a result of the fibre being dragged at the ice-bed interface.

We will therefore remove the suggestion of $n = 1$ and fitting of n and A for our site (though we are able to provide some information, still, see response to 2.10).

Below we outline why we nonetheless retain a simple analytical treatment rather than a full-Stokes model, and then our intended revisions.

There are, in our view, broadly three approaches to collecting and analysing field data.

(**1**), the data is collected and presented more or less *as is*. Discussion of the data may be included, but there is no further in-depth analysis/modelling of the data itself. Examples of this first type are Sharp et al. (1953, https://doi.org/10.3189/S0022143000025685), Booth et al. (2020, https://doi.org/10.1029/2020GL088148), and Prior-Jones et al. (2021, https://doi.org/10.1017/jog.2021.16).

(**2**), following data collection some simple – usually analytical – modelling is conducted that provides insight into some process or parameter. For example, Hooke and Hansen (1986, https://doi.org/10.1016/01

232X(86)90039-X), Law et al. (2021, https://doi.org/10.1126/sciadv.abe7136). In some cases, this allows more in-depth modelling in future work (c.f. Ryser et al., 2014a, 10.3189/2014JoG13J196; Ryser et al., 2014b; and Law et al., 2021; Law et al., 2023, https://www.science.org/doi/10.1126/sciadv.abq5180)

(**3**), in depth 3-dimensional modelling accompanies the data report itself. This applies to Roldan Blasco et al. (2025) and Gimbert et al. (2026, https://doi.org/10.5194/egusphere-2026-4058) among others.

Importantly, examples of technologies being used for the first time that follow the pattern of (**3**) are scarce. Applying a new technology requires high investments in terms of time and resources. Bringing the new technology to maturity is likely to be beset by at least a few pitfalls. For example, Prior-Jones et al. (2021) report on successful aspects of early Cryoegg deployments but also malfunctions related to drill fluid ingress and Cryoeggs ending up in places they are not 'supposed' to be. It took some years more before the proven technology was used purely for investigation (Ing et al., 2026, https://doi.org/10.1029/2026AV002403).

The problem then as we see it, is that *at least some* analytical modelling is required in our case to even reveal if there are indeed problems with the underlying data (demonstrated nicely by Section A). If we adhered to type (**1**) we simply would not know and it would be impossible to make assessments about the drawbacks or benefits of DSS, or to asses the type of record we would expect. On the other hand, type (**3**) is quite clearly beyond the scope of this study, necessitating a complex model (as suggested by reviewer 2) and a 'full house' approach to geophysical data collection including full GPR bed survey and several continuous GNSS stations (the former implicitly and latter explicitly suggested by reviewer 2). And, even if this approach were to be taken uncertainties will remain in the model (which is not a perfect replication of reality even in the 3D full-Stokes case) and in the data (which, as we describe already has issues related to coupling and).

This is especially the case given the new theory introduced in this response document (Section A-E). It would clearly be overcomplicated to introduce this for the first time through the use of a 3D model (that would then be site-specific, rather than easily adaptable to other sites).

In that sense, we defend our use of the shallow ice approximation as opposed to a full-Stokes model for approximations of ice flow. We do not think in this case that the outlay on increased geophysical survey and complicated numerical modelling would be at all commensurate with the returns, and nor would it satisfy the issue of incomplete freeze in. Similarly for the use of plane strain; excluding the coupled use of fibre-optics and tiltmeters, which would have been beneficial but was not achievable in the current installation, there are no real suitable alternatives. We feel that Section D is now an appropriate manner to quantify these issues for future studies.

Furthermore, we emphasise that our paper is primarily a fields methods paper. If we wished to report $n = 1$ for temperate ice as the major finding (if we had greater confidence in it), we would have placed this in the title and not included a clear caveat statement in the abstract.

What do we propose to do?

We will be more upfront in outlining the limitations of both plane strain and shallow ice approximation throughout the paper, including in the abstract.

We will remove all mention of a finding for n based on the theory at the start of this review, showing instead that we should see compression throughout the profile. Rather than stating 'Fitting observed strain profiles to a plane-strain deformation model supports a near-linear ice rheology for temperatures within 1 deg C of the pressure melting point' we will instead emphasise the development of a method that allows for n to be constrained if the above mentioned issues relating to plane-strain, freeze in, etc. can be resolved (which we expect they can).

Within the manuscript, we will add a section looking at how longitudinal and latitudinal strains can introduce errors into a plane-strain assumptions (see Section E of this document). To our knowledge this has not been carried out in a clear way before and allows us to see how much these changes can influence estimations of n and A much beyond the simpler conclusions of previous papers that plane strain is not suitable (without a clear quantification of by how much).

However, for the reasons outlined above, we believe that removing the analysis altogether would be counter-productive, as it would not facilitate discussion of potential pitfalls of DSS nor how to remedy them.

Since the initial submission we have also become aware of shape-detecting DSS installations that make use of three coupled fibres to determine deformation shape. This technology is still largely in the lab development phase, but we will include it in the discussion as an option for getting around the limitations of plane strain in the future. As covered in comment 1.2, we will also discuss the possibility of using APReS simultaneously to recover vertical extension/compression independently which would be very powerful when combined with the new framework outlined above.

2.3 I also have concerns on the overall presentation of the study and results. I don't find the authors provide sufficient quantitative information on both the field setup and their theoretical considerations for the reader to judge the robustness of the assumptions and results. On the observational side, several aspects of the measurement strategy are insufficiently documented, particularly in Sections 2.3 and 2.4 (lines 160–224): the GNSS surface velocity measurements over the period of acquisition are not shown, additional information about the local glacier geometry and dynamics including expectations of spatial velocity gradients, possible basal motion and bed geometry are not presented. On the theoretical side, the inferred glaciological quantities depend on several insufficiently justified assumptions. There are also numerous vague statements with lacking proper justification/quantification/references (e.g. l 56–58, l 187–189, l 221–224, l 231–234, l 253–254).

Response:

GNSS measurements were taken annually, not continuously. We agree that a continuous record would be preferable, but this is not available. We have also corrected a small error in the initial submission. The GNSS was taken at site CJ2TT, roughly 6 m east and 15 m north of CJ1G (the site where the fibre optics were installed). We have updated Fig. 1 in the manuscript to reflect this (see Fig. 8).

The GNSS measurements are available in the now published Beer et al. (2026, https://doi.org/10.5194/tc-20-5453-2026). Formal uncertainties are not provided in that paper. Error associated with the devices used (Leica RTK in 2024, Emlid RTK in 2025) is negligible (on order of 2 cm). Measurement was always taken over the borehole mouth. The main error source comes from the borehole not being perfectly vertical as the surface melts down which may mean the borehole mouth shifts sideways. As a result we will include error bounds of ± 0.15 m/a.

Repeat measurements were obtained on 10 July 2026. Unfortunately however, the Emlid did not get an RTK correction for this measurement so the 2026 positions are only accurate to about 1 m. We therefore do not use this later data in the main manuscript. In addition, further GNSS measurements are available for sites roughly 30 m above and below the main installation.

We will include the discussion above (regarding GNSS precision, possibility of non-vertical borehole) in our Results section.

We will include all GNSS data points (including those without RTK correction) in a supplementary table alongside a reproduction of the Chessjen figure from Beer et al. (2026), both of which

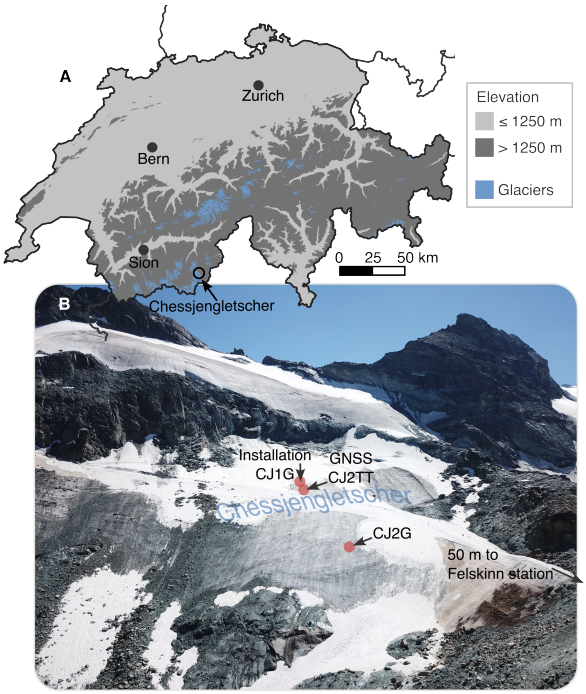


Figure 8: Updated Fig. 1 showing location of GNSS station at CJ2TT (Chessjen TinyTag chain).

are displayed below as Fig. 9 and Table 2. We will use this information to discuss how surface displacement varies between CJ2TT and CJ1TT (a slowdown).

In response to '*additional information about the local glacier geometry and dynamics including expectations of spatial velocity gradients, possible basal motion and bed geometry are not presented*', please also see our response to 2.2 for why we do not think a full-Stokes modelling approach including a GPR survey is appropriate in this instance.

Line comments

l56 We will remove the statement that 'glaciers sharing this general geometry typically do not have complicated flow fields' and instead state that the flow field of Chessjen is unconstrained. Full-Stokes modelling has been applied to Tête Rousse (Gagliardini et al., 2011, http://dx.doi.org/10.1029/2011GL047536), made possible by GPR and a dense stake network motivated by the hazard presented by the glacier. We will cite this paper by way of the closest parallel to our site (though highlighting that the differences are still large!).

l187 This is an issue related to citations for the plane-strain assumption. We will add the following citations at this point: Gerrard et al. (1952); Nye (1953, https://doi.org/10.1098/rspa.1953.0161); Paterson (1983, https://doi.org/10.1016/0165-232X(83)90007-1); Blatter (1995, https://doi.org/10.3189/S00221 Keller and Blatter (2012, https://doi.org/10.3189/2012JoG11J234); and Ryser et al. (2014, https://doi.org/10.3189/2014JoG13J196) all of which use plane strain or simple shear approximations. For the sake of a rounded argument, we will also cite Harper et al. (1998, https://doi.org/10.1126/science.2 and Roldán-Blasco et al. (2025) to highlight that some studies oppose the use of this approximation.

l221 This is an issue related to the use of 1 for the shape factor (f) in Eq. 11 of the original manuscript. In that case, the value of f has very little bearing on the value of A (for example, changing f to 0.8 from 1 led to changes of the order 1.12e-13 to 1.11e-13). However, as we no longer fit parameters this will be removed in any case.

f cannot influence n because it doesn't enter the n fitting procedure. We will make that clear

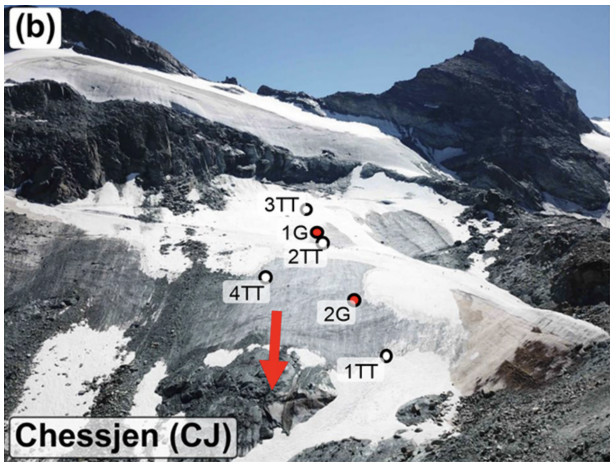


Figure 9: Full site location details following Beer et al. (2026). TT refers to TinyTag thermistor chains, while G refers to Geoprecision chains. Red arrow indicates flow direction.

Table 2: Borehole-mouth positions on Chessjengletscher (Swiss LV95 coordinates, m) and the surface velocities derived from them. (a) RTK surveys on 9 August 2024 (Leica) and 8 August 2025 (Emlid); velocity uncertainty is ±0.15 m a^{-1}. (b) Survey on 10 July 2026 (Emlid, no RTK correction); positions are accurate to about 1 m, so 2025–26 velocities are indicative only (±∼1 m a^{-1}). Dashes indicate no measurement.

(a)

Borehole	2024		2025		Velocity 2024–25 (m a^{-1})
	x	y	x	y	
CJ1TT	2636956.77	1101858.08	2636956.76	1101858.36	0.28
CJ2TT	2636976.59	1101744.48	2636976.64	1101745.91	1.44
CJ1G	–	–	2636970.56	1101731.07	–
CJ2G	–	–	2636963.22	1101802.26	–
CJ3TT	–	–	2636969.90	1101687.21	–
CJ4TT	–	–	2637011.63	1101794.98	–

(b)

Borehole	2026		Velocity 2025–26 (m a^{-1})
	x	y	
CJ1TT	2636957.4	1101858.8	0.9
CJ2TT	2636977.4	1101746.4	1.0
CJ4TT	2637012.8	1101796.7	2.3

in the revised manuscript.

l231 We believe we present sufficient information and justification here. We use a DEM to calculate the average slope, finding it matches the slope at the study site. A full GPR survey would be required to go beyond this which is beyond the scope of the study.

l253 Please see our response to 2.18.

2.4 I expose my criticims more in details below. Addressing all these concerns would require more than a conventional major revision because they affect the central framing, analysis, and conclusions of the paper.

Response: The changes outlined in this document will result in a pretty broad re-framing of the study.

Major comments

2.5 1. Linear deformation in the bottom layer The cable is known to be mechanically uncoupled from the surrounding ice at several locations along its depth. This uncoupling results in apparently linear deformation profiles, as is particularly evident within the lowermost ∼10 m thick layer. Such linear deformation is a measurement artefact and therefore does not represent the local deformation of the ice.
To my knowledge, previous observations based on tiltmeters consistently show increasing non-linearity in ice deformation with depth (e.g. Maier et al., 2019; Roldán Blasco et al., 2025). The present study appears to be an exception, but this difference can be explained by an instrumental artefact that is both acknowledged by the authors and straightforward to understand. Consequently, the affected sections of the cable and, most critical for constraining the power law exponent, the deeper ice deformation, provide very limited information with which to constrain the rheological law.
I suspect that the resulting best-fitting rheological law being linear arises primarily from this measurement artefact. One way to reduce this bias would be to exclude the linear sections of the observed deformation profile from the power-law fit. Since these sections effectively represent interpolation across mechanically uncoupled portions of the cable rather than direct measurements of local ice deformation, allowing them to contribute to the fit artificially favours a more linear rheological response.

Response:

We appreciate the concerns of the reviewer here. Given that the new Section A indicates that we would expect compression throughout the record we will not fit n and A within this paper. We include the methodology for this as the fact that compression is anticipated is slightly counter-intuitive and we expect this to be of great use for future studies.

We also clarify one point. We read the non-linearity in Maier et al. (2019) and Roldán-Blasco et al. (2025) as referring to the curvature of the deformation profile rather than to the flow exponent. Roldán-Blasco et al. attribute this curvature to A increasing with depth and note that their profile has limited sensitivity to n, and Maier et al. (2019) attribute departures from a Glen's-law prediction to bed obstacles rather than to varying n.

This trade-off between n and A is known, but as with the influence of longitudinal strain not really clearly quantified in the literature. A single strain profile will only constrain the local slope of strain rate against stress, which combines n with the depth variation of A. This makes it very challenging to distinguish a larger n with uniform A from a smaller n with A increasing

towards the bed. We got a bit carried away and perform an analysis for a 40 m borehole at a slope of 20 deg below which shows this can be quite a big problem.

We demonstrate this in Fig. 10. Taking a synthetic 15-day strain profile for our setting (Table 1) with $n = 3$ and uniform A, we fit it with $\dot{\epsilon}_{xz} = A(z)\,\tau^n$ for trial values of n from 1 to 6, where $A(z) = A_s\,C^{(h-z)/h}$ varies exponentially with depth, reflecting the dependence of A on a linear temperature profile. Here A_s is the value of A at the surface and C is the contrast in A between the bed and the surface, so that $A = CA_s$ at the bed and $C > 1$ means softer ice at the bed. For each n we scan C, fit A_s by least squares, and take the root-mean-square misfit over all segments relative to a 20 $\mu\epsilon$ segment noise.

The $n = 3$ profile is matched to within noise by $n = 2$ with A increasing towards the bed by a factor of 3.7, or by $n = 4$ with A decreasing by a factor of 3.6. A will usually increase towards the bed as ice gets warmer and wetter, so the lower-n case is more likely, and a uniform-A fit will then tend to overestimate n. This shows that an n recovered from a single profile is conditional on the assumed $A(z)$ or, conversely, that a recovered A is conditional on the assumed n. Separating the two requires prescribing $A(z)$ independently. Temperature is well suited to distributed temperature sensing, whereas water content is more complicated to measure in situ, as is its relationship with A.

We appreciate that Fig. 10 may not be suitable for the main body of the current manuscript. We intend to include this in an appendix.

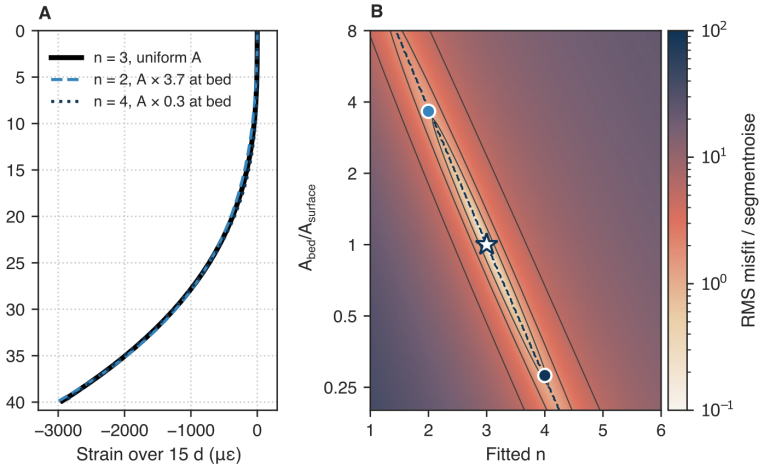


Figure 10: **Difficulty fitting n and A simultaneously. A** strain over a 15-day interval for default parameters (Table 1), for $n = 3$ with uniform A and for $n = 2$ and 4 with A varying exponentially with depth, fitted to the $n = 3$ strain profile (obtained with equations in Section A of this document). **B** Root-mean-square misfit, relative to a 20 $\mu\epsilon$ segment noise, of the $n = 3$ profile (star) fitted with each n and contrast in A between bed and surface. Circles mark the profiles in A. The contours are at x1, x2, and x5.

2.6 2. Assumption of negligible vertical extension or compression A central assumption made by the authors is that all the deformation of the cable is induced by vertical shearing (the plane strain approximation, lines 187–194). The justification currently provided – the relatively simple geometry of the glacier and the use of similar assumptions in borehole inclinometry studies – first strongly lacks precision and references, but most importantly is not robust in the present case. Regarding the effect of geometry, the contribution of vertical extension/compression from longitudinal stress gradients should be evaluated using

more elaborated numerical models. Regarding the justification that borehole inclinometry studies also do this hypothesis, I agree this is mostly true (references should be given), but I expect such assumptions to much more likely hold for inclinometry studies than for DSS studies. In the inclinometry case, tilt is by essence primarily sensitive to shear: a given shear deformation gives a large tilt change, while a vertical extension or compression of similar magnitude gives a small tilt change. For a fibre cable, it is the opposite, because the cable is vertical and by essence mesures elongation, such that vertical deformation has a disproportionate effect on the DSS measurements compared to the tilt measurements. The authors should acknowledge this difficulty and address the subsequent challenge of disentangling both contributions, which is a major objective if we want to use DSS as a robust technique to quantify ice rheology under shear.
An independant observation suggesting this source of bias is likely a major one is the finding that surface displacement obtained from integrating the DSS measurements assuming plane strain gives surface velocities four times larger than observed from GNSS. The statement at lines 284–285 that displacement integrated above 23 m matches the total GNSS displacement implies that the measured strain above 23 m already accounts for nearly all observed surface motion, leaving little or no contribution from deformation below 23 m or from basal sliding, where it is expected to actually be the largest.
The authors mention this bias in the discussion, among with others (lines 288–291). In my view, this is not a secondary caveat but one of the most plausible explanations for the unexpectedly large inferred displacement. I think this last point could be the main emphacis of the present paper.

Response:

Tiltmeters will not be free from contamination by lat/longitudinal strain, either.

We disagree that more elaborate numerical models are needed for this evaluation. We think Sections A-D are the most appropriate way to evaluate this influence, especially for the general case and for the first time this behaviour has been described. See also our response to 2.2 for why we do not use a full-Stokes model here.

As outlined in our response to 2.2 we will include numerous references supporting the use of plane strain. We also recognise that methodologies may naturally progress to become more complex (i.e. from plane strain to full-Stokes), but for reasons outlined in response to 2.2 we do not think it is appropriate here to use a full-Stokes model

It is also very clear that even full-Stokes models do not accurately capture all effects, for example Roldan Blasco et al. (2025) do not capture the near-base strain pattern because the quality of the bed topography map and model resolution still present limits on resolvable features.

We will however add to the discussion that APReS provides one potential way of better disentangling vertical extension/compression from the plane strain components (see also response to 1.2).

w.r.t the paragraph concerned with GNSS, this section will not be included in the revised manuscript following the theory developed in Sections A-F.

w.r.t. lines 288-291, this paragraph, and much of the discussion, will be re-written to take into account the new theory in Sections A-F of this document.

2.7 3. Use of the shallow ice approximation to infer the shear stress profile Although the authors do not state it explicitely, it appears to me that they use

the shallow ice approximation to express shear stress as a function of depth, and use this expression to confront shear stress with the assumed plane-shear deformation. However, previous study shows based on numerical modelling (Roldán Blasco et al. 2025, https://doi.org/10.5194/tc-19-267-2025) that, due to 3D geometry complexity, internal ice stresses depart significantly from the shallow ice configuration, such that adjustement of the Glen's power law exponent, which heavily relies on the stress description with depth, is underconstrained. A similar comment applies to the choice of $f = 1$. The authors should evaluate the impact of their description of shear stress with depth on their finding of n, and evaluate the degree to which this exponent is actually constrained or not given all the uncertainties following my comments 1- to 3-.

Response:

We will make it clearer that Eq. 7 of the original manuscript is in effect the SIA.

We cover some issues in the degeneracy between n and A, which also relates to Roldan-Blasco et al. (2025) in our response to 2.5. We will include a full analysis of the importance of longitudinal stress in the revised manuscript (see Section E of this document) that is applicable to all settings (not a single setting where a full-Stokes model is available). Reviewer 2 is correct that some existing studies (Harper et al., 1998, Roldan-Blasco et al., 2025) suggest that plane strain is not suitable, but these do not fully quantify the issue making it difficult to say exactly how important it is.

2.8 4. Lacking information on the measurement strategy Section 2.3 provides useful information about the installation and instruments (lines 160–185), but it does not describe the acquisition and processing strategy in sufficient detail for the reader to understand exactly how the reported strain profiles were obtained. For each DSS instrument, the authors should specify the gauge length, the integration time used for each measurement, the timescale over which differenciation is made, if there are any averaging performed over consecutive measurements and over the entire period, etc... This would allow assess the uncertainty associated with individual measurements and how these are mitigated when averaging or taking a long time period for differencing. There is also no information on any calibration procedure. It seems that the strain reported in the manuscript represents the difference between the measurements made on 19 August and 3 September 2025, is it? If so, why the 8 August reference measurement was not used in the principal analysis and how this choice relates to the progression of cable freeze-in. We would also want to see a timeseries of GNSS velocities or some better constraints on how velocities may vary trhough the year at this site.

Response:

We will also include the timescales over which measurements were obtained. DTS measurements were averaged, DSS measurements were not – we will include this information.

DTS measurements were averaged over 20 minutes. We later found that doubling this reduced measurement noise, though the record we obtained is more than sufficient for the purpose of this study.

Gauge length only applies to the Rayleigh measurement. We tried with 2 cm and 5 cm. In the manuscript we use sampling window to describe this, which is an appropriate and widely used term.

DSS Rayleight was measured over 10 minutes, and Brillouin over 5 minutes. We will make this clearer in the revised manuscript.

Discerning between gauge, sample, resolution, etc. is complicated as these sometimes have overlapping definitions. We hope that the original manuscript does an appropriate job of this already, but we will update the wording to make this clearer.

Yes, strain is the difference between measurements on 19 Aug and 3 Sept. We will include this information. We did not include 8 August as a reference as the cable had just been installed and no refreezing had occurred. We will make this reasoning clearer in the Instrumentation Section of the manuscript.

Please see our response to 2.3 for a full description of the GNSS record.

2.9 5. Imprecise/vague statements At numerous instances the language is imprecise/vague. Section 2.1 states that glaciers with a similar geometry "typically do not have complicated flow fields" (lines 50–58), but based on which evidence this statement is supported, and what specifically the authors consider a complicated flow field? For example, it is known that glaciers undergo vertical compression and extension from longitudinal stress changes, which would directly convert into strong along cable deformation measured by DSS, which the authors do not account for. A GNSS measurement of changes in vertical elevation through time would have been quite key to address this problem. Similarly, the description of the glacier geometry as "relatively planar" is used to justify a shape factor of 1 (lines 221–224), while the choice of representative slope and thickness is simply described as "reasonable" (lines 231–234), without sufficient geometric evidence or sensitivity analysis. In the Results, the authors state that the profiles "broadly conform" to several possible rheological behaviours (lines 247–249) and that the different measurement records are in "good agreement" (lines 244–246), but neither assertion is supported by quantitative criteria. Also surprising is the attribution of the DTS noise to the age of the interrogator and asserting that it has no effect on the results (lines 253–254), which appears as quite speculative. The authors also state that values of A fall very close to the standard ones but do not give numbers allowing the reader to judge (lines 260–261).

Response: See Section E of this document which outlines the influence of longitudinal strain on fitting.

We will remove the statement about the glacier having simple geometry.

We note that f does not influence how n is fitted. f will have a small influence on A which we will discuss (See response to 2.3). However, we no longer report n and A values and will update the manuscript accordingly. Reviewer 2's final line comment (260-261) is therefore no longer applicable.

Agreed. We calculate mean absolute differences between records (Table 3), with Rayleigh averaged over 1 m to match the Brillouin spatial resolution. Over the whole column the Rayleigh records differ from Brillouin by 10–18 $\mu\epsilon$, comparable to the 10 $\mu\epsilon$ (2σ) Brillouin repeatability. We will add this table to the Appendix and replace 'good agreement' with these values. 'Broadly conform' will be removed (see response to 2.16).

The term 'reasonable' is very common in scientific literature when referring to an approximation. In our case, this reasonableness was quantified using high resolution DEM data (contained within the same parentheses). We do not think this needs changing.

2.10 6. Interpretation of positive and negative strain Lines 241–244 report both positive and slightly negative strain over different parts of the profile. Lines 249–251 then

Table 3: Mean absolute difference ($\mu\epsilon$) between strain records, 19 August – 3 September 2025, by depth below the 3 September surface. Rayleigh records are averaged over 1 m for comparison with Brillouin and with each other, and normal range is averaged over 5 cm for comparison with extended range.

Records	0.5 m–base	0.5–14 m	23 m–base
Rayleigh a normal vs Brillouin a	9.9	11.4	9.9
Rayleigh b normal vs Brillouin a	18.3	11.4	29.3
Rayleigh a extended vs Brillouin a	15.9	19.2	10.7
Rayleigh b vs a, normal	13.7	4.9	24.6
Rayleigh a normal vs extended	31.3	47.6	7.2

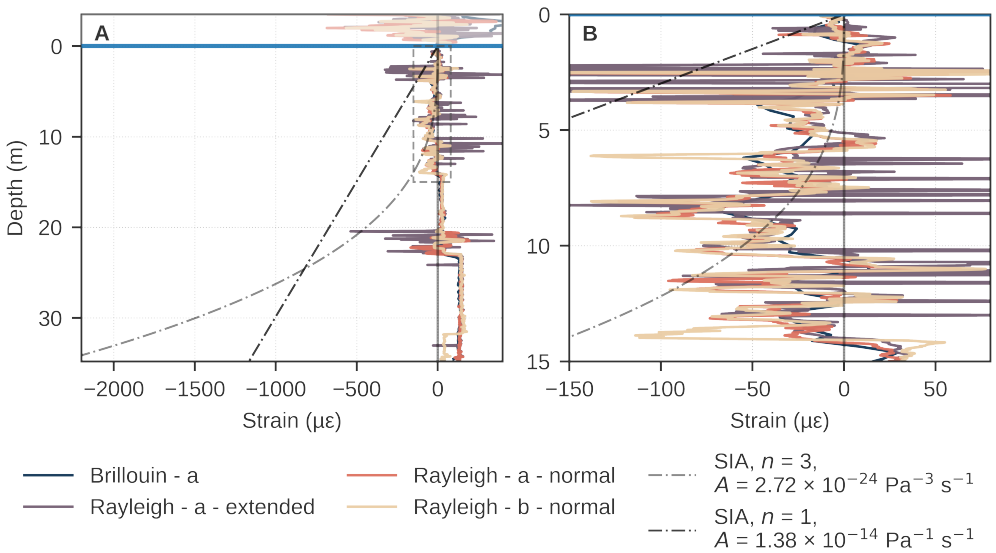


Figure 11: **Observed strain against the vertical-borehole prediction**, 19 August – 3 September 2025. Blue horizontal line at depth 0 m is the ice surface position on 3 September. Dot-dash lines are SIA predictions for $n = 3$ and $n = 1$, with A (Eq. 11 of the original manuscript) giving a deformation surface velocity equal to the GNSS velocity (1.43 m a^{-1}). **A** Whole column; the dashed box marks panel B. **B** Upper 15 m.

argue that the negative values do not result from temperature-change interference because the temperature had stabilised. Would be nice if the authors could explain further the physical meaning of these negative values. It should also be clarified how positive and negative strains can occur at similar depths or differ between the two branches of the looped cable.

Response: Section A of this document explains the sign of the negative strain: shear compresses a vertically installed fibre, whereas the bed-normal model of the original manuscript cannot produce compression from shear at all.

Fig. 11 compares this prediction with our record. In the upper 14 m all four records show compression of around 20–40 $\mu\epsilon$. We plot the SIA profiles for $n = 1$ and $n = 3$ using an A that matches surface displacement. This actually indicates that $n = 1$ is not a good fit. Given that this only uses the uppermost section of the column undergoing the lowest deformation rates, we will not emphasise this as a key finding but will report it in the results and discussion.

Minor comments

2.11 Figure 2A The schematic is not particularly informative in its present form. It could be improved by showing the borehole, cable geometry, coupled and potentially uncoupled sections, basal turnaround, the positions of the GNSS, the two boreholes, etc. . .

Response: We will remove this part of the figure and replace it with a simpler schematic focussing on the borehole column itself. The updated Fig. 1 contains further information about borehole positions.

2.12 Lines 59–60 The surface displacement of 1.43 m should be given in the results section, and accompanied by the measurement interval.

Response: We disagree. We think this is the appropriate place to put this information as it is very standard methodology and more useful as background for the reader than in the results. We will however include information about the measurement interval here, as it falls closer to methodology.

2.13 Lines 170–172 Clarify why the GNSS displacement was measured in 2024 while the DSS experiment was conducted in 2025. Please provide the processing uncertainty and evidence that the 2024 velocity is representative of the 2025 DSS interval.

Response: See Table 2, which will be included in the updated manuscript, for full information about GNSS. Glacier velocity is subject to variation at annual and sub annual timescales. Our recommendation aligns with yours that continuous GNSS records are preferable. As that is not available we will outline these caveats in the updated manuscript. We also correct the GNSS survey dates given at line 171 ('August 8, 2024–September 9, 2024'), which should read 9 August 2024 – 8 August 2025.

2.14 Lines 181–182 Correct "The results presented here are between based on the two follow-up measurements." State the analysed dates explicitly.

Response: We will state the analysed dates specifically at this point.

2.15 Lines 183–185 Distinguish between sampling interval, spatial resolution, gauge length, and strain resolution for both instruments.

Response: Please see our response to 2.8.

Each measurement type only requires one sample, but we will also state this clearly.

2.16 Lines 247–249 Replace the statement that the profiles "conform" to anticipated displacement patterns with more cautious language reflecting the assumptions imposed in calculating those profiles.

Response: Given the change in methodology outlined at the opening of this document our initial statement on lines 247-249 is no longer accurate. We will adjust this, and all other relevant parts of the results, to account for the updated methodology.

2.17 Lines 249–251 Explain the physical interpretation of negative strain rather than only excluding temperature change as its origin.

Response: Section A of this document now outlines why this occurs, and our response to 2.10 compares it with the observed profile. We will update the Results and Discussion to account for this.

2.18 Lines 253–254 The suggestion that DTS noise results from the age or unknown service history of the interrogator is speculative. Either provide supporting evidence or remove this explanation. The statement that the noise has "no bearing" on the results should also be supported quantitatively.

Response: The noise is indeed very small and well within the error range of the thermistors (<0.1 K). The thermo-optic effects for Brillouin and Rayleigh are 44 $\mu\epsilon$ K^{-1} and 31 $\mu\epsilon$ K^{-1} respectively, so the error from this response will be no greater than 4.4 $\mu\epsilon$ (and aside from one kink at 4 thermistors up from the bed much less). We will state this clearly in the results in support of our statement.

Following further use with this particular DTS unit we isolated the issue to much reduced noise with much longer (>40 minutes) integration times. We will update the text to reflect this.

2.19 Lines 258–260 Correct "plane-strain profile profiles."

Response: We will correct this repetition.

2.20 Figure 5 Show uncertainty or confidence intervals for the fitted value of , not only the RMSE curves. The figure or caption should identify the depths included in the fit.

Response: We assume reviewer 2 is referring to fitted values of n here but we are not certain. Figs. 5, 6 from the original manuscript have been removed. We will include analysis of n and A variation with difference in borehole installation angle and longitudinal/latitudinal strain (Figs. 3, 4).

2.21 Lines 282–285 The values of 6 and 1.43 m should be fully documented before they are interpreted in the Discussion.

Response: As covered in previous comments, we will outline GNSS measurements from both years in the Instrumentation section. We will introduce the 6 m a^{-1} value in the results.

2.22 Lines 285–286 The two-week duration of the DSS measurement should be stated prominently in the Methods, Results, and Figure 4 caption.

Response: We will update this.

2.23 Lines 305–307 Correct "in line with with surface displacement."

Response: We will remove the duplicative 'with'.

2.24 Lines 307–309 The description of the site as a "surprisingly robust field laboratory" seems inconsistent with the unresolved coupling and displacement issues, and with the lack of a 3D model applied on the area which would largely helps interpreting these measurements. Please use more cautious wording.

Response: We will alter the wording of this sentence to be more cautious.

2.25 Line 338 Remove the additional closing parenthesis after "Law et al., 2021)."

Response: We will remove this, and check carefully for any additional typos.

2.26 Lines 332–343 The broader claims that DSS can constrain ice rheology from grain to ice-sheet scales should be moderated or clearly presented as potential future applications. The present experiment highlights cable-strain measurement is likely high resolution in certain places of the borehole, but not an unambiguous recovery of local rheological properties.

Response: We will moderate this statement in line with revisions made throughout the paper.