

Response to Reviewer 1

We thank the reviewer for their careful reading of the manuscript and their constructive comments. Reviewer comments are shown in grey boxes and numbered for cross-referencing (R1.1, R1.2, ...). Our replies follow each comment, and the changes we propose to make to the manuscript are listed in green boxes at the end of each reply.

The stress state of the model is set out in Sect. 3.2, but our discussion of the unconfined compression test in Sect. 3.1, which precedes it, gave the impression that the model assumes unconfined compression. This underlies comments R1.2 and R1.8, and in part R1.16. This is not the case. The unconfined compression test is used only to define the viscous Poisson ratio as a material property. The model itself prescribes the observed horizontal strain rates and reduces to confined compression, as the reviewer recommends, in the absence of horizontal strain. We explain this in detail under comment R1.2 and propose revisions to remove the ambiguity.

The main revisions we propose are: (i) clarifying the stress state of the model (Sects. 3.1–3.2); (ii) stating and testing the assumptions of steady flow and depth-uniform horizontal strain rates; (iii) testing the sensitivity of the inferred parameters at high density to density biases, and quantifying the contribution of the measured crystal fabric (Appendix E); (iv) testing the inferred coefficient functions at six other Greenland sites; and (v) stating more clearly that the inferred parameters are effective quantities, and over which density range they are well constrained.

Review egusphere-2026-3464 “Firn Rheology Revealed by Contrasting Shear Regimes” by Aslak Grinsted et al.

General comments

R1.1

This article presents a method for deriving the two rheological functions of the firn law, originally proposed by Gagliardini and Meyssonier (1997), based on two density profiles measured in northeast Greenland. Noting correctly that one-dimensional model does not accurately reproduce the strain-rate and stress history experienced by the snow, the authors developed a strain-rate and stress model based on the observations. My first major criticism is that, due to the ice dynamics at the sites, the assumptions made to infer strain-rate and stress history might strongly influence the results.

Reply:

We agree that the inferred rheology is conditional on the reconstructed strain-rate and accumulation histories. Treating this history explicitly is, however, a central motivation of the study: most firn models neglect horizontal strain and upstream history altogether, whereas our flowline approach tracks each firn parcel from deposition along its back-trajectory. This requires assuming that the present-day flow field has been steady during the formation of the firn column (approximately 700 years for the upper 100 m at EastGRIP), and we agree that this assumption should be stated and tested.

The accumulation history provides a test. The accumulation rates derived from the EastGRIP ice core, corrected for layer thinning by the horizontal divergence accumulated along the back-trajectory, reproduce the main multi-centennial variations in the present-day radar-derived accumulation pattern sampled along the same back-trajectory back to about 1100 CE (Fig. 2c), i.e. over the full age range of the EastGRIP firn column. Because the radar-derived field is scaled to match the ice-core mean (Appendix A), the test lies in these variations rather than in the mean level. Errors in the reconstructed trajectory or the integrated divergence would be expected to grow with age and degrade this agreement. At S5-1, the strong shear occurs only in roughly the last century of the trajectory (Fig. 2a), its most recent and best-observed

part.

The steady-state and flowline inversions treat the strain history very differently: the former assumes that the present-day strain rates have acted throughout, the latter uses the reconstructed history. They yield nearly identical r (Fig. 3b), showing that the combined parameter, which controls densification in the absence of horizontal strain, is robust to how the history is treated. The partitioning between a and b , which depends on the strain history, is where the two inversions differ, and we regard this difference as a measure of the uncertainty that the history introduces (see comment R1.16).

The vertical stress is obtained from the overburden of the observed density profile and does not depend on the flow reconstruction. The depth-uniformity of horizontal strain rates is addressed under comment R1.4.

PROPOSED REVISIONS

- State explicitly in Sect. 2 that the present-day flow field is assumed steady during the formation of the firn column, including the approximate age span this covers.
- Expand the discussion of this assumption, presenting the agreement between the temporal variations in ice-core-derived and radar-derived accumulation along the EastGRIP back-trajectory as an independent test, and noting that the ice-core-derived mean is used to scale the radar-derived field.
- State that r is insensitive to the treatment of strain history, whereas the difference between the two inversions in a , b and ν_v reflects the uncertainty introduced by it.

R1.2

Furthermore, the assumed state of stress, unconfined compression, is not appropriate for a sample of firn confined within an ice-sheet. Confined compression, assuming no lateral deformation, would be more appropriate (a standard assumption for one-dimensional vertical model). To what extent are these hypotheses responsible of the differences obtained with the rheological functions of Zwinger and others (2007)? I develop a bit more these two points in the list of comments below.

Reply:

Our model does not assume that firn within the ice sheet is subject to unconfined compression, but we recognise that the discussion of the unconfined compression test in Sect. 3.1, preceding the model formulation in Sect. 3.2, gave this impression.

The unconfined test is used only to parameterise the rheology. Within the GM97 rheology, the viscous Poisson ratio ν_v depends only on a and b (Eq. 6), not on the stress state: it is defined by the response in an unconfined test, just as the elastic Poisson ratio is defined from a uniaxial stress test but applies to any state of stress. We use $(\log a, \nu_v)$ as a one-to-one reparameterisation of (a, b) because it allows the physical bound $\nu_v \in [0, \frac{1}{2}]$ to be imposed.

The stress state of the model is described in Sect. 3.2: the horizontal strain rates are prescribed from observations and the vertical stress is the overburden load, while the vertical strain rate and horizontal stresses follow from the inverse rheology. This is the confined compression recommended by the reviewer, generalised to include the horizontal deformation imposed by ice flow, and it reduces exactly to confined (uniaxial strain) compression when $\dot{\epsilon}_1 = \dot{\epsilon}_2 = 0$ (Eq. 15).

The differences from the rheological functions of Zwinger et al. (2007) (hereafter Z07) therefore cannot be attributed to the stress state. The comparison of r in Fig. 3b is made precisely in the confined-compression limit, where r alone controls densification for a given overburden. There, Z07 implies considerably lower densification rates at intermediate and high densities

than both our inversions and the independent Herron and Langway Jr (1980) model. Nor can the differences be attributed to the reconstructed strain history: they are present in the steady-state inversion, which does not use it, and at the low-strain EastGRIP site (see comments R1.1 and R1.16).

PROPOSED REVISIONS

- Restructure Sects. 3–4 so that the stress state of the model (Sect. 3.2) is presented first, and ν_v is introduced in the Methods alongside the $(\log a, \nu_v)$ parameterisation used in the inversion.
- State explicitly that the unconfined compression test serves only to define ν_v and η_E , and does not represent the stress state assumed in the model.
- State explicitly in Sect. 3.2 that the model reduces to confined compression in the limit of vanishing horizontal strain.

Specific comments

R1.3

I have compiled a list of comments below that I hope will help improve the current manuscript (listed in order of appearance).

- lines 39: you could list some of the papers that have used this law (at the end there is quite a number of applications using this law, but not in the context of interpreting ice-sheet density profile).

Reply:

We thank the reviewer for this suggestion. As the rheology of Gagliardini and Meyssonier (1997) (hereafter GM97) is the formulation on which our study builds, we agree that its wider use should be acknowledged. Applications include the dating of ice cores and modelling of heat flow at high-Alpine glaciers (Lüthi and Funk, 2000, 2001; Licciulli et al., 2020), the flow of a crater glacier (Zwinger et al., 2007), the long-term evolution of a snow cave at Dome C (Brondex et al., 2020), and, more recently, the collapse of snow caves and enhanced densification in ice-stream shear margins (Arrizabalaga-Iriarte et al., 2025). As noted by Oraschewski and Grinsted (2022), the law has rarely been applied to the polar ice sheets, and, as the reviewer points out, mostly not to interpret ice-sheet density profiles. Where it has been calibrated against density profiles, each site was fitted individually within a prescribed functional form, which fixes the ratio of a to b . GM97 used the Site 2 profile (see comment R1.11), and Arrizabalaga-Iriarte et al. (2025) fitted the single free parameter of Z07 to six Greenland sites, finding that no single value fits all of them. Our study instead combines density profiles from two sites with contrasting strain regimes to constrain a and b separately. We propose to add a brief statement on these applications, while keeping the focus of the introduction on firn densification.

PROPOSED REVISIONS

- Add a sentence at line 39 citing representative applications of the GM97 rheology (Lüthi and Funk, 2000, 2001; Zwinger et al., 2007; Brondex et al., 2020; Licciulli et al., 2020; Arrizabalaga-Iriarte et al., 2025), noting that it has rarely been applied to the polar ice sheets, and that previous calibrations against density profiles fitted each site individually within a prescribed functional form.
- Rephrase line 277, which currently states that our finding of strong near-surface enhancement is in agreement with Arrizabalaga-Iriarte et al. (2025), to: “Arrizabalaga-Iriarte et al. (2025) found that the near-surface enhancement required by the GM97 rheology varies between sites and experiments, and that no single calibration fits all

Greenland sites.”

R1.4

- Caption Fig. 1: You should mention that it is surface effective strain rates. Strain-rate at depth will be different because of the complex dynamics at these two sites. This should be discussed later in the model assumption. Same for the back-trajectory, they are reconstructed from the present day flow from the observed surface velocity ? It then does not account of the real trajectory of the snow/ice particle.

Reply:

We agree that the caption should state that these are surface effective strain rates, and that the assumption that they apply throughout the firn column should be discussed.

We expect the difference between surface and firn strain rates to be negligible. The firn column considered here (upper 100 m) is less than 4 % of the ~ 2.6 km ice thickness, and vertical shear is concentrated near the bed, where shear stress is highest. For an isothermal shallow-ice velocity profile with $n = 3$, the decrease in horizontal velocity from the surface to depth d is a fraction $(d/H)^{n+1}$ of the total deformational velocity, i.e. $\sim 3 \times 10^{-6}$ at the base of the firn. This is an upper bound: warmer ice near the bed concentrates deformation even closer to the base, and basal sliding, which is significant in NEGIS, makes the deformational velocity a smaller part of the total, bringing the profile even closer to plug flow. The observed surface strain rates are therefore representative of the whole firn column. The same assumption of depth-invariant horizontal strain rates was made in the two-dimensional modelling of the NEGIS shear margins by Arrizabalaga-Iriarte et al. (2025). In the shear margin, the strongly developed fabric at ~ 24 m depth inferred from radar birefringence (Appendix E) is consistent with strong horizontal shear acting on the firn.

The same argument applies to the back-trajectories: with depth-uniform horizontal velocity, a firn parcel of age t was located at the point on the surface back-trajectory t years earlier, while its depth follows from burial in the Lagrangian model (see comment R1.5). The assumption that past flow resembles present flow is discussed under comment R1.1.

PROPOSED REVISIONS

- Specify in the caption of Fig. 1 that the strain rates are surface effective strain rates.
- Justify the assumption of depth-independent horizontal strain rates in Sect. 3.2, including the scaling argument, and note that the same assumption was made by Arrizabalaga-Iriarte et al. (2025).
- Clarify how firn parcels at depth relate to points on the surface back-trajectory.

R1.5

- Caption Fig. 2: from the text one understands that it is along the core. How do you link a given depth in the ice core with a given point on the back-trajectory? How do you account for the vertical velocity? This should be clearly stated in the text.

Reply:

We agree that this should be stated more clearly. Fig. 2 shows quantities sampled along the surface back-trajectory as a function of time (year CE), not depth. In the flowline model (Appendix D), the firn column travels along this trajectory towards the core site. Each year a new layer is added at the surface with the local accumulation rate, and all layers are subjected to the horizontal strain rates at the column’s current position, since the horizontal velocity is effectively uniform through the firn (see comment R1.4). Layer depths are diagnostic, given by the cumulative thickness of the overlying layers, which evolves through vertical compaction

and horizontal divergence. The vertical velocity is thus not an input but follows from mass conservation, and the link between depth and position on the trajectory is given by the modelled depth–age relation. In the steady-state approach, the vertical velocity is given explicitly by Eq. (17).

PROPOSED REVISIONS

- Clarify in the caption of Fig. 2 that the quantities are shown as a function of time (year CE) along the surface back-trajectory, i.e. the position of the firn column at that time.
- Describe in Sect. 4.2 how the firn column travels along the back-trajectory, and state explicitly that depth and vertical velocity are not prescribed but emerge from the model.

R1.6

- line 94: a uniform temperature? What is important is how evolve the temperature with depth. If it is not uniform with depth, which I suspect is the case, then the evolution with depth should not be neglected as it will change the ice viscosity with depth and can explain, as the evolving anisotropy discussed later, the mismatch of the rheological a and b functions.

Reply:

The temperature is not uniform with depth, but the variation is small. Measured profiles at both sites show a very similar structure: below the depth of seasonal influence, temperatures decrease by approximately 1 K between 15 and 95 m, consistent with recent surface warming, and the two sites differ by less than 0.3 K (lines 92–94).

We retain a constant temperature because representing this structure properly would require solving the heat equation along the flowline with an assumed surface temperature history, which is not warranted by the size of the effect. With $Q = 60 \text{ kJ mol}^{-1}$ at $T \approx 242 \text{ K}$, $d \ln A / dT = Q / (RT^2) \approx 0.12 \text{ K}^{-1}$, so a 1 K variation changes A by about 13 %. For $n = 3$, a change in A is equivalent to a uniform rescaling of a and b by $A^{-1/2}$ (see comment R1.10), i.e. by about 6 %, leaving ν_v unchanged. This is negligible compared with the deviations of a and b from Z07, which exceed an order of magnitude at low density (Fig. 3a). Moreover, because the temperature structure is nearly identical at both sites, it cannot explain the contrast in densification between them, on which the inversion relies.

PROPOSED REVISIONS

- Describe near line 94 the common vertical temperature structure at both sites ($\sim 1 \text{ K}$ decrease between 15 and 95 m), and note that the corresponding variation in the rate factor ($\sim 13 \%$) is equivalent to a rescaling of a and b by only $\sim 6 \%$, common to both sites.

R1.7

- line 103: Glen-Nye rheology for ice

Reply:

We agree.

PROPOSED REVISIONS

- Replace “Glen rheology” with “Glen–Nye rheology” at line 103 and wherever else the flow law is named (e.g. line 158), citing Nye (1953).

R1.8

• line 124: Is an elementary volume of snow within the ice sheet subject to unconfined compression? I believe it is actually confined compression, since the medium is infinitely large and the assumption is that lateral deformation is therefore zero. Unconfined compression would apply to a snow sample with free lateral sides under a compression apparatus. Since the solutions for unconfined and confined are very different, this choice strongly influences the derived rheological functions a and b . In reality, an elementary volume of snow/ice is subject to neither an unconfined stress nor a confined state of stress. Only a three-dimensional model would adequately resolve the history of stress and strain-rate history experienced by the snow/ice as it flows within the ice-sheet. This strong assumption should be at least discussed. Furthermore, since it acts as a bound, the solution for the opposite bound (confined compression) could also be derived.

Reply:

As explained under comment R1.2, our model does not assume unconfined compression. The unconfined test only defines ν_v , which is a one-to-one reparameterisation of (a, b) , so this choice does not influence the inferred a and b .

We agree that firn within an ice sheet is subject to neither an unconfined nor a confined state of stress. This is precisely why our model prescribes the observed horizontal strain rates rather than assuming either limiting case. The full three-dimensional strain-rate tensor enters the rheology at each depth (Eq. 9); the only neglected components are the vertical shear strain rates, which are negligible in the firn column (see comment R1.4). The strain-rate history is resolved by tracking each firn parcel along its trajectory (comment R1.5). The confined-compression solution suggested by the reviewer is contained in our formulation as the limit of vanishing horizontal strain (Eq. 15), and the comparison of r in Fig. 3b is made in precisely this limit.

PROPOSED REVISIONS

- See the revisions proposed under comments R1.2 and R1.9.

R1.9

• line 139: this is a strong hypothesis? Especially regarding all the critics to the 1D model in the introduction and the choice of the site for which the horizontal velocity is not vertically uniform because of basal friction.

Reply:

We agree that the horizontal velocity is not uniform over the full ice thickness. What matters here, however, is whether it is uniform over the firn column, the upper ~ 100 m of a ~ 2.6 km thick ice column. Vertical shear ($\partial u / \partial z$) is concentrated near the bed, and basal sliding brings the upper profile even closer to plug flow. The resulting variation in horizontal velocity across the firn column is of order 10^{-6} of the deformational velocity, so the firn strain rates are effectively those observed at the surface (see comment R1.4).

Our criticism of 1D models in the introduction concerns their neglect of horizontal strain, including lateral shear, not of vertical shear. We include the former while continuing to neglect the latter, which is well justified for the firn of a thick ice sheet.

PROPOSED REVISIONS

- See comment R1.4 (justification of depth-independent horizontal strain rates in Sect. 3.2).
- Specify at line 136 that the neglected components are the vertical shear strain rates $(\dot{\epsilon}_{xz}, \dot{\epsilon}_{yz})$.

R1.10

- lines 163-164: why not using the more classical Cuffey and Paterson 2010 formulation? How much the results are sensitive to the choice of the parameters in the Glen-Nye flow law? Also, I understand that the temperature was constant from what is mentioned above (line 94)?

Reply:

We use the rate factor of Greve and Blatter (2009), based on Paterson (1994), because it is identical to the Glen rate factor underlying Z07 (prior to their enhancement factor $E = 1/3$), so that our comparison with Z07 is not affected by the choice of rate factor. Below -10°C it uses the same activation energy ($Q = 60\text{ kJ mol}^{-1}$) as Cuffey and Paterson (2010) and differs only in the prefactor, which is about 1.4 times larger than that of Cuffey and Paterson (2010). For $n = 3$, a constant change in the prefactor is exactly compensated by a uniform rescaling of a and b . Adopting Cuffey and Paterson (2010) would therefore increase the inferred a and b by a factor of $\sqrt{1.4} \approx 1.2$, and leave the modelled density profiles and ν_v unchanged.

More generally, the choice of flow law matters more than the choice of prefactor. In a recent Bayesian re-analysis of 70 years of laboratory experiments, Fan et al. (2025) found that several alternative flow laws fit the laboratory data similarly well (their Fig. 2a), yet diverge when extrapolated to low temperatures (their Fig. 2d). Near -31°C , they differ by about a factor of four at 0.3 MPa, typical of the overburden in the deeper firn, but by up to a factor of about twenty at 0.05 MPa, typical of the upper $\sim 10\text{ m}$. For $n = 3$, these correspond to factors of about two and four in the inferred a and b , respectively. The larger spread at low stress reflects differences in how the flow laws represent grain-size-sensitive creep, which becomes increasingly important at low stress.

Temperature is indeed constant in both inversions (-31°C at both sites). temperature cycle is accounted for only in the steady-state forward model (Sect. 3.4 and Appendix B). There, including it changes the modelled density by at most $\sim X\text{ kg m}^{-3}$ (a depth offset of at most $\sim 0.5\text{ m}$), comparable to the measurement uncertainty and confined to the upper $\sim 20\text{ m}$. The seasonal signal is attenuated within the upper few metres, and the slower near-surface densification without it is largely compensated further down: firn that remains less dense is also less viscous, and therefore densifies faster (cf. the negative feedback described in Appendix C). Using the rate factor at the mean temperature in the inversions keeps them strictly local in density, i.e. the inferred parameters at a given density do not depend on those at other densities. Below the depth of seasonal influence, the measured temperature profiles vary by only about 1 K, corresponding to a change in the rate factor of approximately 13 % (see comment R1.6).

PROPOSED REVISIONS

- State in Sect. 3.3 that the rate factor is that underlying Z07, that it differs from Cuffey and Paterson (2010) only by a constant prefactor equivalent to a uniform rescaling of a and b , and discuss the uncertainty in the flow law, including its stress dependence, using Fan et al. (2025).
- Clarify in Sect. 2 that the inversions use a constant temperature, and that the seasonal cycle is accounted for only in the steady-state forward model (Appendix B).
- State at the start of Appendix B that the effect of the seasonal cycle on the modelled profiles at our sites is small (at most $\sim X\text{ kg m}^{-3}$, confined to the upper $\sim 20\text{ m}$), and that the appendix is included because the method is generally applicable to steady-state and annual-time-step firn models.

R1.11

- line 175: in the original paper by Gagliardini and Meyssonier (1997), the parameters a and b have were not calibrated solely using E_{zz} at single sites, but also based on mechanical tests for which the two parameters could be derived simultaneously. The various formulations are more on certain relationships that the parameters a and b had to satisfy and that had not been taken into account in the initial work by Gagliardini and Meyssonier (1997).

Reply:

We agree that our statement does not do justice to GM97 and subsequent work. GM97 calibrated the coefficient functions against both the densification observed at Site 2, Greenland, and the laboratory creep tests of Landauer (1957). These tests include uniaxial, confined uniaxial and isotropic compression, and therefore constrain a and b separately, but cover only relative densities of about 0.4–0.7. The parameterisation implemented in Elmer/Ice, which corresponds to Z07, was obtained by fitting both datasets (Gagliardini, 2012).

The in-situ part of this calibration also shows why a single density profile is not sufficient on its own. As noted by Gagliardini (2012), inferring a and b from a single profile is ill-posed, because only the combination $4/(3a) + 1/b$ is constrained, and they suggest resolving this by fixing the ratio a/b to that of the theoretical functions of Duva and Crow (1994). In our notation, where b is larger by a factor of 3, this combination is exactly $2r$, consistent with our finding that a single profile constrains r but not its decomposition. Our approach replaces the assumed ratio with an observational constraint from two sites with contrasting strain regimes.

We also agree that later formulations introduced relationships that a and b must satisfy. In particular, the condition $3a > 2b$ (Gagliardini, 2012), which ensures that the cross-section of a sample under uniaxial compression does not decrease, is equivalent in our convention to $\nu_v \geq 0$, which our inversion imposes. The solid-ice limits are discussed under comment R1.15.

PROPOSED REVISIONS

- Rephrase lines 173–176 to state that GM97 and Z07 calibrated the coefficient functions against both laboratory creep tests (Landauer, 1957), covering relative densities of about 0.4–0.7, and the Site 2 density profile, and that inferring both functions from a density profile alone requires fixing the ratio a/b .
- Note that the combination constrained by a single profile (Gagliardini, 2012) corresponds to r , and that $\nu_v \geq 0$ corresponds to $3a > 2b$.
- Correct the description of Arrizabalaga-Iriarte et al. (2025) at line 174: they calibrated the single free parameter of Z07 against six Greenland density profiles, finding that no single value fits all sites.

R1.12

- line 193: the density profile is not a flow line as far as there is horizontal velocity.

Reply:

We agree that, in general, a vertical profile at a fixed location is not a flow line: firn at a given depth was deposited upstream. The steady-state equation (Eq. 19) neglects horizontal advection of density and thus assumes a horizontally uniform density field, which holds when the forcing is uniform along the trajectory, as assumed in the steady approach. Under this assumption (Sorge’s law; Bader, 1954), the vertical profile is equivalent to the densification history of a parcel. The same assumption underlies the inference of a and b from the Site 2 profile in GM97 (Gagliardini, 2012, Eq. 26), and is made explicitly by Z07.

The flowline approach (Sect. 4.2) relaxes this assumption: each layer is tracked from its point of deposition along the back-trajectory, which is precisely the situation the reviewer describes. The two approaches yield nearly identical r , while their differences in a , b and ν_v reflect the different treatment of the deformation history (see comment R1.1).

PROPOSED REVISIONS

- State explicitly in Sect. 3.4 that the steady-state formulation assumes a horizontally uniform density field (Sorge’s law), which is relaxed in the flowline approach.

R1.13

- line 215: history experienced by a firn volume along a flow line.

Reply:

We agree. The observed profile consists of firn volumes deposited at different points along the trajectory, each with its own history (see comment R1.12).

PROPOSED REVISIONS

- Rephrase lines 214–215 to: “When applying the model along flowlines, the density at each depth reflects the integrated deformation and deposition history experienced by a firn volume along its flow line.”

R1.14

- line 248: because the model is forced to fit this profile, this is then not a validation in itself.

Reply:

We agree: the close fit is a consistency check, not a validation. For the steady-state inversion, the fit is in fact exact by construction, since at each density two parameters are determined from the two observed vertical strain rates. For the flowline inversion, the regularisation strength was chosen to give a reduced χ^2 near unity, so the fit alone does not show that the inferred rheology is correct.

The results do, however, contain an independent check: the inferred r agrees closely with that implied by the Herron and Langway Jr (1980) model (Fig. 3b), which is calibrated on other sites and not used in the inversion. In addition, the steady-state and flowline inversions, which treat the deformation history very differently, yield nearly identical r , showing that r is robust to this treatment (see comment R1.1).

As a further independent test, we will apply the steady-state model with the inferred coefficient functions to the six Greenland sites considered by Arrizabalaga-Iriarte et al. (2025), which span

a wide range of temperatures and accumulation rates, and report the results irrespective of their outcome. Because horizontal strain is small at these sites, this primarily tests whether the inferred $r(\rho)$, and the temperature and accumulation dependence implied by the model, transfer to other climatic conditions.

PROPOSED REVISIONS

- Rephrase line 248 to present the fit as a consistency check, and state in Sect. 4.1 that the steady-state inversion fits both profiles exactly by construction, except where the bounds on ν_v are active.
- Present the agreement with Herron–Langway as an independent check, and the agreement in r between the two inversions as a test of robustness to the treatment of the deformation history.
- Add a test of the inferred coefficient functions against the density profiles at the six Greenland sites of Arrizabalaga-Iriarte et al. (2025).

R1.15

- Figure 3: There is an issue here. As stated in the text, the limite of the parameter a for ice should be 1. Here you obtain 0.1!

Reply:

We agree that $a \rightarrow 1$ is expected for isotropic ice, and that our inferred values at high density are well below this limit. We do not consider the lowest inferred values to be robust. Near ρ_{ice} , densification is controlled by the small remaining porosity: at $\rho = 900 \text{ kg m}^{-3}$, $\rho_{\text{ice}} - \rho \approx 20 \text{ kg m}^{-3}$, so a density bias of a few kg m^{-3} , or the choice of ρ_{ice} itself (values between 910 and 921 kg m^{-3} are used in the literature), translates into large changes in the inferred parameters. This is reflected in the divergence between the two inversions near pore close-off (Fig. 3a), and is discussed further under comment R1.18.

Part of the deviation is nevertheless physical. Applying the CAFFE anisotropic rheology (Placidi et al., 2010; Rathmann et al., 2021) to the c -axis fabric measured on thin sections from the S5 core at 68 m (Weikusat et al., 2022), previously used by Gerber et al. (2023), gives a vertical enhancement factor of $E_{zz} \approx 0.17\text{--}0.18$, corresponding to $a \approx 0.4$ (see comment R1.18). Effective hardening relative to the reference rheology also has precedent: Z07 applied an enhancement factor $E = 1/3$ throughout the firn and ice to match observed temperature and age profiles, equivalent to $a \approx 0.58$ in the solid-ice limit (see comment R1.18). Uncertainty in the rate factor, by contrast, can account for only a small part of the deviation (see comment R1.10).

This illustrates why we emphasise that $a(\rho)$ and $b(\rho)$ are effective parameters: within an isotropic, single-mechanism framework, they absorb processes not explicitly represented, such as fabric, and may not transfer directly to regions with a different deformation history. We agree that this should be stated more clearly.

PROPOSED REVISIONS

- Expand the discussion of the sensitivity of the inversion near ρ_{ice} , including a test of the effect of small density biases and of the choice of ρ_{ice} (see comment R1.18).
- Quantify the effect of the observed fabric on vertical compaction (see comment R1.18).
- State more clearly in the Results, Discussion and Conclusions that the inferred parameters are effective quantities, specific to the deformation regime of the study region, and that the low values of a at high density reflect both the loss of constraint

near ρ_{ice} and, in part, fabric, which is not represented in the isotropic reference rheology.

- Note in the caption of Fig. 3 that the isotropic limit $a \rightarrow 1$ is not imposed in the inversion.

R1.16

- lines 265-271: all the discussion suffers from the strong assumptions made before. The nonmonotonic variation with density of the viscous Poisson ratio is difficult to understand. What would in the microstructure evolution explains that? Can it be that the difference with Z07 on the functions a and b is not a result of the the applied strain-rate and stress history?

Reply:

The assumptions are addressed under comments R1.2 and R1.8 (stress state), R1.4 and R1.9 (depth-uniform strain rates), and R1.1 (steady flow).

ν_v is the least well-constrained inferred quantity. The two inversions agree on its first-order behaviour but differ in detail (lines 321–322): the maximum lies near $500\text{--}600\text{ kg m}^{-3}$ in both, but the subsequent minimum lies near 750 kg m^{-3} in the steady-state inversion and near 800 kg m^{-3} in the flowline inversion (Fig. 3c). We regard this difference as a more realistic measure of the uncertainty than formal statistical errors, which reflect only measurement noise. With this caveat, the structure is suggestive of established transitions in densification mechanism summarised by GM97: from grain rearrangement to dislocation creep at grain contacts at a relative density of ~ 0.6 ($\approx 550\text{ kg m}^{-3}$), and to the closed, spherical-pore regime of Duva and Crow (1994) at ~ 0.785 ($\approx 720\text{ kg m}^{-3}$). As each regime partitions deformation differently between volume reduction and shape change, ν_v need not vary smoothly across them. Fabric development may contribute further.

We do not believe the differences from Z07 result from the applied history. The comparison of r (Fig. 3b) is made in the no-horizontal-strain limit, and the deviation in r is the same in the steady-state inversion, which does not use the reconstructed history. Z07 also underestimates densification at the low-strain EastGRIP site (Fig. 3e). Finally, Arrizabalaga-Iriarte et al. (2025) found that the Z07 coefficient functions underestimate densification at high density at six low-strain Greenland sites, regardless of the value of their free parameter.

PROPOSED REVISIONS

- Expand the discussion of the non-monotonic ν_v with reference to the densification regimes summarised by GM97, revising lines 314–320 accordingly, and state that its detailed structure is weakly constrained, using the difference between the two inversions as an indication of uncertainty.
- Add that the deviation from Z07 is present in the steady-state inversion, in the no-horizontal-strain limit, at the low-strain EastGRIP site, and at the six low-strain sites of Arrizabalaga-Iriarte et al. (2025).

R1.17

- line 274: Z07 was already defined above

Reply:

We agree.

PROPOSED REVISIONS

- Remove the repeated definition of Z07 at line 274 and refer to Z07 directly.

R1.18

- lines 294-301: Do you really expect a factor of 10 due to the anisotropy at 24 m depth? At least this should be quantified but the anisotropy at 24m depth is not so marked? Physically, the inferred parameters a and b are then of no use for an other sites as parameter a does not fulfill $a > 1$ when the density approaches that of solid ice. Then it does not make any sense to compare it to Z07 which, by construction, fulfill this limit.

Reply:

We agree that this should be quantified. In short, we do not expect fabric to account for a factor of 10, and we do not consider the lowest inferred values of a to be robust. Near ρ_{ice} , densification is controlled by the small remaining porosity: at $\rho = 900 \text{ kg m}^{-3}$, $\rho_{\text{ice}} - \rho \approx 20 \text{ kg m}^{-3}$. A density bias of a few kg m^{-3} , or the choice of ρ_{ice} itself, therefore translates into large changes in the inferred parameters, as reflected in the divergence between the two inversions near pore close-off (Fig. 3a). Moreover, because the partitioning between a and b is constrained by the contrast between the two sites, whose profiles were measured with different methods, a small systematic offset between the methods maps directly into a at high density.

We also note that an effective hardening relative to the reference solid-ice rheology is not unique to our study. Z07 applied an enhancement factor $E = 1/3$ to the rate factor throughout the firn and ice, which they found necessary because the low-density firn otherwise compacted too readily to match the observed temperature and age profiles. For $n = 3$, this is equivalent to scaling a and b by $E^{1/2} \approx 0.58$, so that, as applied by Z07, the effective a tends to ≈ 0.58 rather than 1 in the solid-ice limit. That the Z07 coefficient functions required slower densification at Gorshkov crater glacier, but underestimate densification at our sites and at the Greenland sites of Arrizabalaga-Iriarte et al. (2025), further illustrates that a and b act as effective, site-dependent parameters.

Crystal fabric is a physical effect acting in the same direction, and we have quantified it using measured fabric. The inferred a falls below unity only above $\rho \approx 750 \text{ kg m}^{-3}$ (below $\sim 33 \text{ m}$ at S5-1), and reaches its lowest values near 900 kg m^{-3} ($\sim 60\text{--}70 \text{ m}$); at 24 m ($\rho \approx 640 \text{ kg m}^{-3}$) it is still well above unity. We applied the CAFFE anisotropic rheology (Placidi et al., 2010; Rathmann et al., 2021) to the crystal-orientation fabric measured on thin sections from the S5 core at 68 m depth ($\rho \approx 905 \text{ kg m}^{-3}$; Weikusat et al., 2022), which gives a vertical enhancement factor $E_{zz} \approx 0.17\text{--}0.18$. For $n = 3$ in the incompressible limit, a directional enhancement factor E is equivalent to multiplying a by $E^{1/2}$ (Eq. 3), so the measured fabric implies $a \approx 0.4$. Scaling the fabric linearly with the radar-derived eigenvalue difference $\Delta\lambda$ (Appendix E) gives $E_{zz} \approx 0.39$ in the upper $\sim 24 \text{ m}$, i.e. $a \approx 0.6$. The reviewer is right that the fabric is weaker there, but it is not negligible. CAFFE describes the ice matrix rather than porous firn, but this caveat matters least near 900 kg m^{-3} .

Fabric therefore plausibly accounts for a reduction of a to ≈ 0.4 at high density. We attribute the lower inferred values ($a \approx 0.1\text{--}0.3$ at $800\text{--}900 \text{ kg m}^{-3}$) primarily to the loss of constraint near ρ_{ice} described above, although stronger or differently oriented fabrics can produce harder

responses (Castelnau et al., 1998). Since $a(\rho)$ is shared between the two sites, we note that fabric hardening is not confined to the shear margin: Gerber et al. (2023) found parts of the ice stream to be more than an order of magnitude harder than isotropic ice for along-flow extension and compression.

We agree that the inferred parameters at high density are effective quantities, specific to the deformation regime of the study region, and should not be transferred without care. We will recommend imposing $a \rightarrow 1$ at high density when applying them elsewhere, and state the density range over which they are well constrained. Our main conclusions do not depend on the behaviour of a near ρ_{ice} . The combined parameter r , which controls densification in the absence of horizontal strain, is tightly constrained and agrees with the independent Herron and Langway Jr (1980) model (Fig. 3b), and the largest discrepancy between our r and that of Z07 occurs at intermediate densities (500–800 kg m⁻³), where the limit $a \rightarrow 1$ plays little role. We will also test the transferability of r at six other Greenland sites (see comment R1.14).

We therefore consider the comparison with Z07 informative: Z07 is a widely used parameterisation, and the comparison tests whether it reproduces the observed density profiles, which it does not (Fig. 3d,e). Satisfying $a \rightarrow 1$ does not by itself ensure realistic densification at high density: Arrizabalaga-Iriarte et al. (2025) found that Z07 underestimates densification above $\hat{\rho} \approx 0.8$ at six low-strain Greenland sites. For transparency, we note that we evaluate the Z07 coefficient functions as implemented in Elmer/Ice, i.e. without the enhancement factor $E = 1/3$, and with $\rho_{\text{ice}} = 921$ rather than 910 kg m⁻³; both choices increase the Z07 densification rates relative to the original formulation.

PROPOSED REVISIONS

- Test the sensitivity of the inferred a at high density to plausible density biases (a few kg m⁻³) and to the choice of ρ_{ice} , and indicate in Fig. 3a the density range above which the parameters are weakly constrained.
- Extend Appendix E with the fabric-to- E_{zz} calculation: the measured S5 fabric at 68 m (Weikusat et al., 2022), the linear scaling of fabric strength with $\Delta\lambda$ used to estimate the fabric in the upper ~ 24 m, and the resulting CAFFE estimates of E_{zz} , including their limitations.
- Revise the discussion of $a < 1$ (lines 290–301) to present the loss of constraint near ρ_{ice} as the main reason for the lowest inferred values, with fabric as a physical contribution quantified via $a = E^{1/2}$, and note the precedent of the enhancement factor $E = 1/3$ in Z07.
- Clarify the magnitude of $\Delta\lambda$ in Sect. 5 and Appendix E.
- State the density range over which the inferred parameters are well constrained and potentially transferable, and recommend imposing $a \rightarrow 1$ at high density when applying them elsewhere.
- Clarify that the comparison with Z07 is a forward test of a widely used parameterisation, that the main discrepancy in r occurs at intermediate densities, and that Z07 also underestimates high-density densification at other Greenland sites (Arrizabalaga-Iriarte et al., 2025).
- State in Sect. 4.3, where Z07 is introduced, that the Z07 coefficient functions are evaluated as implemented in Elmer/Ice, without the enhancement factor $E = 1/3$, and with $\rho_{\text{ice}} = 921$ kg m⁻³.

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