Dear Dr. Védrine,

We thank you very much for your comments to continue to improve the manuscript. Please find your comments below in red text and our responses in black text.
The manuscript now contains the revisions described below and highlighted in italics.

Thank you for your response. I appreciate that the authors have now clearly stated their assumptions regarding the constitutive law, in particular the use of constant values for B and n, as well as the use of the concept of effective stress. I also thank the authors for highlighting the inconsistency in the retrieved values of n, which is indeed difficult to determine at low temperatures, as steady state is reached very slowly at such densities. Restating these assumptions is crucial because they strongly influence the interpretation of the data.

After this substantial revision, I believe the manuscript can be accepted. I only have a few minor comments and suggestions:

Line 567: Why do the authors rely solely on the value previously measured by the first author in 2022? I also regret that the requested sensitivity analysis of the activation energy to the chosen value of n was not carried out, given the uncertainties associated with determining n.

In many experimental contexts, a sensitivity analysis would be essential. However, we would like to respectfully clarify that a sensitivity analysis of Q_c on n was not performed because, within the specific framework of our experimental derivation, these two parameters are mathematically decoupled. The value of n does not influence the calculated value of Q_c as addressed to the reviewer in our previous response letter. This decoupling arises from how the activation energy is derived from the Arrhenius relation. As the reviewer recalls, the constitutive equation is:

$$\dot{\varepsilon} = B\sigma^n \exp(-Q_c/RT).$$

In our study, the activation energy Q_c was determined by measuring the strain rate ($\dot{\varepsilon}$) at a constant applied stress (σ) across a range of temperatures (T). For a constant stress, we can simplify the equation. Taking the natural logarithm, we get:

$$\ln(\dot{\varepsilon}) = \ln(B) + n\ln(\sigma) - (Q_c/R)*(1/T).$$

Since the stress (σ) and the stress exponent (n) are held constant for a given experiment, the term $n\ln(\sigma)$ is a constant. The equation therefore simplifies to a linear form:

$$\ln(\dot{\varepsilon}) = C - (Q_c/R)*(1/T)$$

where $C = \ln(B) + n\ln(\sigma)$ is a constant.

When we plot $\ln(\dot{\varepsilon})$ against $1/T$, the slope of the resulting line is - Q_c/R. The value of the stress exponent n is contained within the constant C, which determines the y-intercept of the Arrhenius plot, but it has no effect on the slope. Therefore, the derived activation energy Q_c is entirely independent of the chosen value of n.

This fundamental principle is precisely why we relied on the value of n previously measured for the same firn core samples in Li and Baker (2022a). Our goal was to ensure methodological consistency. Using an n value derived from a different material or experimental condition would simply shift the entire Arrhenius plot up or down (affecting the pre-exponential factor B), but it would not change the slope from which Q_c is calculated. Using an incorrect or arbitrary n would be physically meaningless for this specific core, as it would break the self-consistency of the material's constitutive law, even while leaving Q_c unchanged.

In summary, while we deeply appreciate the reviewer's vigilance on this matter, the requested sensitivity analysis is not applicable here because the value of n does not influence the calculation of Q_c under constant-stress experimental conditions. The derivation of Q_c is solely dependent on the temperature dependence of the strain rate.

To prevent similar misunderstandings for future readers, we are happy to revise the manuscript to include a brief note in the text explicitly stating this point:
"It is noted that under constant-stress conditions, the value of the stress exponent n influences the pre-exponential factor B but does not affect the slope of the Arrhenius plot and therefore the derived activation energy Q_c*."*
Please see Lines 457–459.

Please carefully check the bibliographic references cited in lines 113–122. In addition, you refer to "snow" for densities up to 830 kg/m^3. Why not mention the experiments by Scapozza at different temperatures?

We clarify that our discussion of activation energies, as introduced on Line 112, incorporates both snow and ice.
And we cited the work of Scapozza and Bartelt (2003, Lines 117-118), stating that *"…69 ± 5 kJ mol*$^{-1}$ *for a mean snow density of 423 ± 8 kg m*$^{-3}$ *at –19°C to –11°C (Scapozza and Bartelt, 2003);…".*

Figure 6. The expression "ln Strain rate minimum" mixes mathematical notation and text, which may not be immediately clear to the reader. Would "ln $\dot{\varepsilon}$ (minimum strain rate) (s^{-1})" be clearer? Alternatively, you could use a logarithmic scale on the axis to make the representation explicit and fully consistent with Figure 6.

Figure 6 illustrates the evolution of strain rate with strain to precisely locate the minimum strain rate. We note that the logarithmic scale of the y-axis across all subfigures is used to better visualize the data range and should not be misinterpreted as plotting the minimum value itself.

Line 441: You may read the work (Védrine et al., 2025), which explicitly demonstrates what you are suggesting. It shows that in porous polycrystals, increasing porosity enhances basal activity, likely due to reduced crystalline frustration—a point you revisit in lines 546–547.

Thank you. We have now included this citation.

Sincerely,

Yuan Li, Kaitlin Keegan, Ian Baker