November 10, 2025

To the Editors/Authors,

I have reviewed the manuscript *"Revisiting snow settlement with microstructural knowledge"* submitted by Louis Védrine and Pascal Hagenmuller to *The Cryosphere*. Overall, I find this to be a very strong and well-crafted study that makes a meaningful contribution to our understanding of the viscoelastic and microstructural controls on snow settlement. The work combines microstructure-based crystal plasticity modeling with a thoughtful analysis of previous creep data, and the synthesis presented here brings valuable physical clarity to a problem that has long been described in more empirical terms.

The manuscript fits well within the aims and scope of *The Cryosphere*. It meets the journal's criteria for originality, scientific rigor, and clarity, and it provides new insights of practical importance to snowpack and firn modeling communities.

General Evaluation

The study is carefully designed and well executed. The modeling approach is technically sound, the connection to experiments is well argued, and the presentation is clear. The results convincingly show why seemingly linear settlement laws appear to work under typical Alpine conditions, while also explaining where and why these relationships may fail.

I have only a few points that I believe should be clarified before publication. These are primarily matters of explanation rather than scientific substance.

Specific Comments

1. On the 1% strain and the strain-rate minimum

Around line 175, the authors use 1% strain to determine the steady (minimum) creep rate. This is a reasonable and widely accepted choice: for many materials, including polycrystalline ice, the minimum creep rate occurs at about 1% strain. Jacka (1984) reported the minimum at roughly 0.6%, and Treverrow et al. (2012) found steady conditions near 1%. I suggest the authors briefly note this and include one or two references to show that their cutoff corresponds to the transition from transient to steady-state creep.

2. On the stress exponent and grain boundary sliding

Lines 307–308 state that the observed stress exponent ($n \approx 2$) does not require invoking grain boundary sliding (GBS). This conclusion seems plausible given the microstructure-based modeling. However, previous studies—such as Alley (1987) and Goldsby & Kohlstedt (1997, 2001)—have associated $n \approx 2$ with GBS in fine-grained ice. It would be helpful to acknowledge that this similarity exists and to explain that the present work offers an alternative physical explanation (based on constrained basal glide in a porous polycrystal) for the same effective exponent. Doing so would better situate the results in the context of existing literature.

3. On crystal orientation and slip systems

The model includes basal, prismatic, and pyramidal slip systems. Because basal slip dominates and is rotationally symmetric about the c-axis, the specific a-axis orientation typically has little effect on mechanical behavior at these stress levels. Still, it would be worth clarifying whether the simulations explicitly included full crystal orientations, or if c-axis randomization alone was used. A short note referencing Schulson & Duval (2009) or Weikusat et al. (2017) would be helpful to reassure readers that this simplification is appropriate.

4. On the word "frustration"

The term "frustration" appears several times and is used correctly in the mechanical sense—referring to geometric or kinematic incompatibility between neighboring grains that limits basal glide. For readers unfamiliar with materials-science terminology, I suggest adding a brief clarification at its first occurrence (e.g., "Here, 'frustration' refers to mechanical incompatibility between neighboring grains that constrains easy slip."). Beyond that, no change is needed.

Other Comments

The discussion connecting the microstructural simulations to settlement parameterizations in snowpack models is one of the most valuable aspects of this paper. The authors also do a good job highlighting the role of missing microstructural descriptors such as bond size (Hagenmuller et al., 2014a), connectivity (Schleef et al., 2014b; Schöttner et al., 2025), and inter-crystalline surface area. It might be helpful to indicate briefly which of these factors could most readily be incorporated in future work.

Recommendation

This is an excellent paper that represents a meaningful step forward in understanding snow rheology and densification from a microstructural perspective. I recommend **minor revision** to address the small clarifications noted above. Once these are implemented, I would be happy to see the paper accepted for publication in *The Cryosphere*.