Review for EGUSPHERE-2025-764

The central idea of this paper is clever and thoughtfully executed. The authors use satellite-derived surface velocities to invert for basal shear stress and sliding speed, applying a suite of commonly used basal sliding laws that either implicitly or explicitly depend on effective pressure. While the inversion of satellite data using various slip laws is not itself novel, having been employed in numerous prior studies, this paper introduces a direct, testable link between effective pressure predictions and independent constraints derived from seismic acoustic impedance data. By leveraging Buckingham's Viscous Grain-Shearing (VGS) theory, informed by marine sediment analogs and supported by site-specific porosity and grain size priors, the authors generate effective pressure predictions from seismic observations that can be compared directly to the inversions. While the acoustic model carries inherent uncertainties, the authors mitigate these through a Bayesian model selection framework, which allows for robust comparison of competing sliding laws while accounting for parameter uncertainty.

I find this to be a well-written and conceptually significant paper that advances our ability to constrain basal conditions of Antarctic ice streams. The methodology sets a precedent for integrating seismic and glaciological observations into a coherent, probabilistic framework—providing new potential for mapping the spatial (and eventually temporal) distribution of basal effective pressure. Importantly, the results add further weight to the relevance of Coulomb-style or regularized-Coulomb sliding laws, which continue to be debated within the modeling community.

My primary critiques do not detract from the paper's novelty or value, but they do point to natural directions for future works. I do believe they should be briefly addressed in the text.

- The VGS theory, while well-motivated, is adapted from oceanographic contexts and relies on assumptions about pressure dependence that have not been directly tested under glacial conditions.
- The use of independent prior distributions may oversimplify the relationships among subglacial sediment properties, particularly where physical coupling through compaction or consolidation is expected.
- The study draws on seismic data from only five sites, which limits the spatial resolution and generalizability of the inferred effective pressure fields. It is not surprising, at least to me, that PIG exhibits Coulomb-like behavior as I'm not aware of any studies that contradict this. As a point of curiosity, I am interested to see how this methodology performs in other environments where basal conditions are debated, such as the interior of the Greenland Ice Sheet or alpine glaciers (obviously outside the scope of this study!)

I believe the authors work transparently with the available data and generally acknowledge the limitations. I view this paper as an important step forward in linking geophysical observations to ice sheet model parameterizations and recommend it for publication following minor revisions that clarify the scope of applicability and emphasize caveats in interpreting the effective pressure maps. *I have a few minor comments which I detail below:*

Comments related to the prior distributions:

I would like to see more detail on the logic behind the formulation of the custom prior distributions. While I appreciate the justification for the C_{max} prior shown in the Supplement, the distributions for porosity and grain size are less clear. Since these appear to be new compilations from the literature, it would be helpful to include the underlying data (in the Supplement would be sufficient) and to show how those priors were constructed from the compiled observations. Additionally, in cases where porosity was estimated from active seismic data (e.g., Blankenship et al., 1987), it's worth noting that those estimates assumed no dependence on effective stress. This could introduce some circularity when those values are used to constrain priors in a model that explicitly incorporates effective stress. Clarifying these points would strengthen the study.

Secondly, grain size, porosity, and effective stress are not independent in natural systems, but are physically coupled through compaction, consolidation, and sediment mechanics. If I understand the methodology correctly, parameter sets were sampled independently from their prior distributions, grain size and porosity, for example, and then used to calculate effective stress via Buckingham's VGS theory. However, relationships between these variables have been described in the sediment mechanics literature and impose constraints on what combinations are physically reasonable. I am concerned that treating them as statistically independent in the prior sampling may lead to internally inconsistent sediment states. While the Bayesian framework helps downweight poor-fitting combinations, would explicitly incorporating physically based constraints or coupled priors could improve the robustness of the analysis in a meaningful way?

Regarding u_t, I respect the uncertainty that leads the authors to use a log-uniform prior, but as I recall, the Zoet-Iverson slip law includes a prediction for u_t based on sediment properties (most notably grain size) which already has a relatively narrow range in this study. Given that, it doesn't seem reasonable to expect u_t values near 10^4 m/yr as equally likely as, say 10^2? There are also at least two other studies I can recall that provide calculated values of u_t in different configurations: Helanow et al. (2020; DOI: 10.1126/sciadv.abe7798) for sliding over rough, rigid beds and Hansen et al. (2024; DOI/10.1029/2023GL107681) for frozen sediments over till. Some discussion of this would be helpful, as it's not clear whether the wide prior range used here is physically justified.

It would be helpful to emphasize more clearly in the introduction or discussion that the method presented here is primarily applicable to soft-bedded glacier systems, since the acoustic impedance contrast relies on wave propagation through a granular medium. This is an important distinction, especially considering that some of the tested sliding laws were originally formulated for rigid or mixed bed topographies. I think an open question remains in glaciology regarding how these different sliding laws apply across regions with spatially heterogeneous basal conditions (e.g., Maier et al., 2021, https://doi.org/10.5194/tc-15-1435-2021). The result that a fast-flowing, soft-bedded glacier like Pine Island Glacier exhibits Coulomb-style sliding is not surprising to me, given the preponderance of experimental and field evidence in the literature. But in light of continued and recent discussion in the literature (e.g., Law et al., 2024, https://doi.org/10.48550/arXiv.2407.13577) it would be worth emphasizing the both the utility and the limitation of the geophysical datasets to constrain the slip law.

Excellent work overall!