Author's response to Anonymous Referee 2 Comment 1

July 14, 2025

General comments

We thank the referee for their constructive comments. A point-by-point reply is reported below, with referee comments in orange and our replies in black. We agree with the specific referee comments not listed here and will revise the manuscript accordingly. Specific comments that merely repeat points already addressed in the referee's general comments are also not listed here.

[Bayesian approaches are generally used to determine the posterior probability density function (PDF) of model parameters, given prior information and constraining observations. For each sliding law, you thus obtain a posterior PDF as a function of the three chosen varying parameters. To obtain a probability for a sliding law, you integrate the posterior PDF over the three-dimensional parameter space (if I understand correctly). This last step is not justified at all in the manuscript while it is critical as all conclusion are based on this. It is not clear to me that a higher integrated probability over the whole parameter space makes a sliding law more likely than another. For example, a model with high but localized maximum PDF can have a lower score than smaller maximum PDF spread on a larger domain of the parameter space. The way the sliding law probability is calculated clearly needs theoretical background. This is critical for the paper as the data does not bring significant difference in misfit and thus data-based likelihood.]

The referee is correct to point out that Bayesian approaches are generally used to determine the posterior probability density function (PDF) of model parameters, given prior information and constraining observations. The situation that we consider here is slightly different, however, and is more akin to Bayesian model selection than the routine application of Bayes' rule for a single model. The main difference for the model selection framework is that the probability space is extended to cover multiple models, each of which has its own parameter space. Apart from that distinction, standard manipulations of probability are used, including Bayes' rule, marginalisation, and normalisation [e.g., Jaynes, 2003].

It is true that we do not justify these standard manipulations, but we will emphasise in the revised manuscript that nothing unusual is happening beyond a straightforward extension of the probability space to acknowledge the possibility of multiple different models.

The statement $\int_{\Theta_i} P(\Theta_i|M_i)\, d\Theta_i = 1$ says that once a model has been chosen, the parameters of that model must lie somewhere in its parameter space with certainty. This is self-evident. By performing this normalisation for each model, we take advantage of the well-known capacity of Bayesian model selection to automatically apply Occam's Razor. Overly flexible models with a large range or dimension of parameter space are penalised relative to simpler, less flexible models with fewer parameters or tighter bounds upon parameters.

The referee questions the marginalisation over the model parameters Θ_i (as expressed by equation 16), but this is standard because we wish to compare the posterior probabilies of models $P(M_i|D)$, not the joint posterior probability of models and parameters $P(\Theta_i, M_i|D)$. Note that $P(\Theta_i|M_i)$ is a conditional probability like any other and can be manipulated using standard rules of probability (e.g., Equation 16).

Unlike the more standard application of Bayes' rule, each model in the Bayesian model selection framework has its own particular parameter space, and this parameter space can be of any dimension. The two fixed effective pressure endmember scenarios have a 2D parameter space (grain size and porosity).

All other sliding laws have a 3D or 4D parameter space (one or two additional sliding law parameters). The number of values examined for these additional sliding law parameters varies across different sliding laws. Having an additional dimension or a larger number of values examined effectively increases the chance of obtaining a good fit to the data, and this is compensated for appropriately in the Bayesian approach. The key idea is that a balance between goodness of fit and model flexibility is desirable, but we emphasise that no special manipulations are required to enforce this balance in the Bayesian approach, as it emerges quite naturally. We will add these further details about the theoretical background of Occam's razor to the revised manuscript.

[I do not understand why you are limiting your parameter space to three varying parameters. I suspect this is because you do an exhaustive grid search to build the posterior PDF. There are simple methods such as the Monte Carlo algorithm, that can be used to efficiently calculate the posterior PDF in cases where the parameter space is large. This would be easy to implement in your case where the forward model is fast to compute. This limitation forces you to calculate two different probabilities for some sliding laws (Schoof and Zoet-Iverson), where you arbitrarily fix one of the sliding parameters. This makes no sense to me, especially when you assume $\mu = C_{\max} = 0.5$ without justification (when varying ut or C_s). The PDF should be built with varying all relevant parameters together.]

The referee is correct that we limited the parameter space to three dimensions due to the computational cost of the grid search. Therefore, our previous results more precisely identified which of the models with three or fewer dimensions best represent the measured acoustic impedance data. We have since expanded the parameter space to four dimensions for all sliding laws with previously two different three-dimensional representations (Tsai-Budd, Schoof, and Zoet-Iverson) and will discuss these results in the revised manuscript (see Fig. 1 for a subset of the examined sliding laws; not all 4D results are available at the time of upload of this response). We agree that methods to simultaneously explore even more parameters, e.g. different exponents, should be explored in future studies, and we will comment on this limiting factor in the revised manuscript.

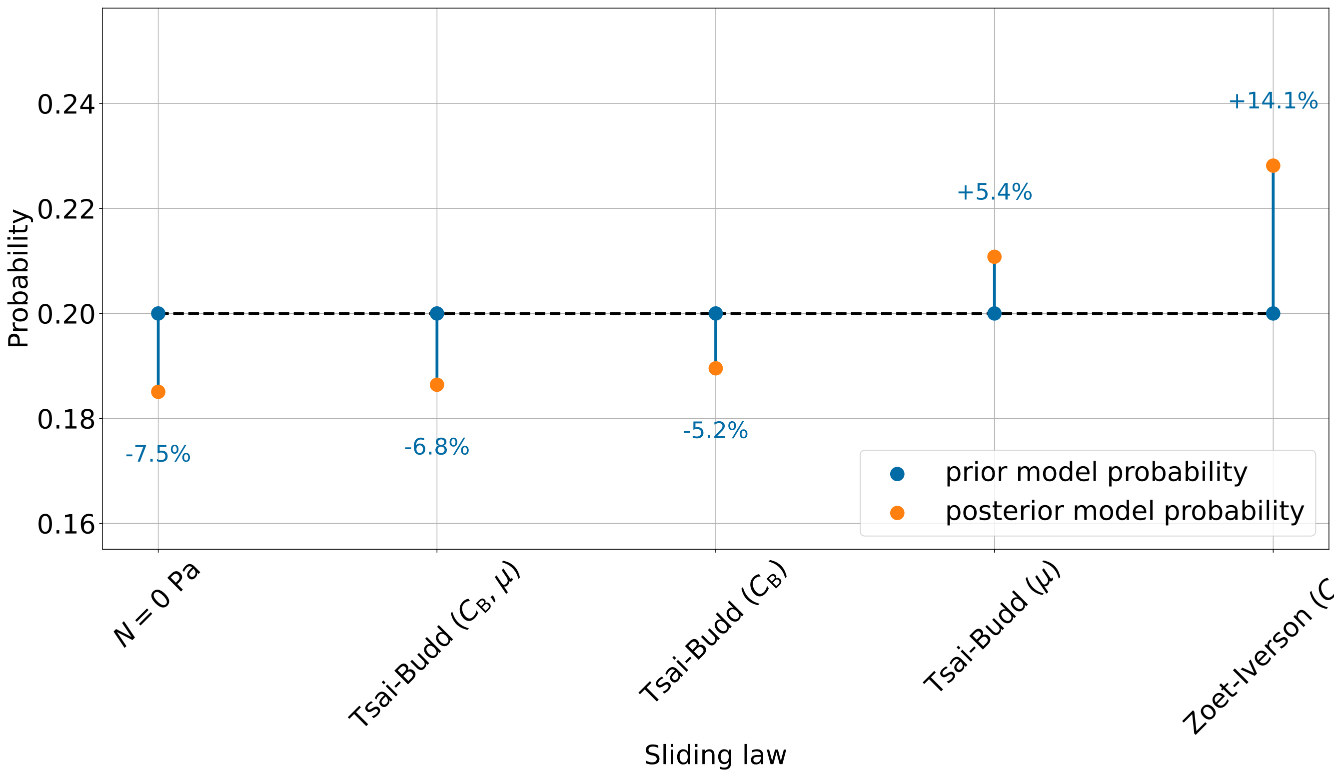

Figure 1: Normalized probabilities of a subset of the examined sliding laws, including the Tsai-Budd sliding law when simultaneously varying C_{B} and μ. Note that not all 4D results are available at the time of upload of this response.

[I do not agree with the claim you are testing the Weertman law. You are simply testing the hypothesis of uniform effective pressure which as nothing to do with the Weertman law. If you want to say that the Weertman law is not appropriate you should show that the inverted τ_b as a function of u_b does not match a power law. A figure showing the inverted τ_b as a function of u_b is missing in the manuscript in any case.]
It is true that we can not directly test the Weertman sliding law or any sliding law, for that matter, that has no effective pressure dependence. To clarify this, we will refrain from using the term *Weertman-type endmember scenarios* and instead refer to these experiments as *fixed effective pressure endmember scenarios*. However, the effective pressure is only uniform in the $N = 0$ Pa case, as all other fractions of the ice overburden pressure vary spatially due to the dependence on ice thickness.

The relation between the inverted τ_b and u_b is the same for all sliding laws and, therefore, provides by itself no information on which sliding law is most appropriate. Therefore, we refrain from adding a figure showing the inverted τ_b as a function of u_b.

[I do not think you are able to distinguish which of the stress bounded sliding law perform better when the result is so dependent of the design of the Bayesian approach. You also hide that the Schoof law is almost the exact same law as the Zoet-Iverson law. You can indeed write the equation (7) of the manuscript in this form:]

$$\tau_b = C_{\max} N \left(\frac{u_b}{u_b + \left(\frac{C_{\max}}{C_s} N\right)^{\frac{1}{m}}} \right)^m \tag{1}$$

This is very similar to Zoet and Iverson with $p = 1/m$, $\mu = C_{\max}$ and $u_t = (C_{\max}/C_s N)^{1/m}$. The only difference is that u_t is a function of $N^{1/m}$ in the Schoof formulation and a function of N in Zoet-Iverson.
We agree that it is difficult to select a single-best sliding law due to the small differences in posterior probabilities between some of the sliding laws incorporating a Coulomb friction term (Coulomb, Tsai-Budd, Schoof, Zoet-Iverson). For this reason, we focus on the distinction between the Coulomb-type and non-Coulomb-type sliding laws (fixed N endmember scenarios and Budd). We will state this more clearly in the revised manuscript.

Generally speaking, whenever we are using a sliding law originally formulated for hard beds (e.g., Budd, Schoof), we assume a granular, relatively undeformable material that cannot support tangential friction at its interface with the ice (here referred to as *rigid bed*). The formation of cavities, for example, is most appropriate for undeformable bed protrusions, but larger rock fragments embedded in granular sediment or even fine-grained deformable sediment might play a similar role [Schoof, 2007a,b, Fowler, 2009, Schoof, 2012]. The basal drag for rigid beds is dominated by the deformation of ice around bed obstacles (form drag). In contrast, basal conditions dominated by skin drag are covered by the soft bed (deformable sediment) sliding laws (e.g., Coulomb, Zoet-Iverson).

While the form of the Schoof and Zoet-Iverson sliding law is indeed very similar, the physical reasoning and interpretation differ. As described above, the Schoof sliding law is most applicable for ice sliding over a rigid bed (granular but relatively undeformable material). It allows for the formation of cavities and incorporates Iken's bound [$C_{\max} = \tan\beta$; Iken, 1981, Schoof, 2005]. β is the maximum up-slope angle of the bed in flow direction. In contrast, the Zoet-Iverson sliding law aims to describe ice sliding over a water-saturated till bed (deformable). $\mu = \tan(\Phi)$ is the Coulomb friction coefficient and Φ the till friction angle. Thus, the two sliding laws represent different basal conditions, and μ and $C_{\max}$ describe different physical properties. We will add a brief discussion of the similar mathematical form but different physical reasoning and interpretation to the revised manuscript.

[Posterior PDF are not shown, it would be usefull to have them in some figures to discuss the influence of prior PDF.]
We examine the influence of the prior distribution by applying (log-)uniform priors to all parameters (Fig. 6 vs. S22). As showing 2D planes of the 3D or 4D posterior PDF might be misleading, and to keep the manuscript concise (7 additional plots would be required), we refrain from adding additional map plots.

[Given the resolution of Bedmap-2, the estimation of $C_{\max}$ based on basal topography observation does not make any sense. Even if the inversion is performed at the kilometer scale, the relevant scale at which to estimate $C_{\max}$ is the meter scale, as this is the scale at which shear resistance is built. Also, the impedance model is based on the assumption of a sediment layer, which is inconsistent with the estimation of $C_{\max}$ based on the hard-bed theory. I do not see why μ and $C_{\max}$ should have different priors, given that they play the same role in the friction law. Doing so favours one sliding law based on unjustified choices.]
While we agree that shear resistance is most likely built at scales smaller than the resolution of Bedmap-2, the bed roughness and therefore the actual relevant scale are less clear and likely vary spatially. However, these smaller scales will not be explicitly represented by the basal drag derived from the inversion. Therefore, the $C_{\max}$ prior is determined by a combination of the Bedmap-2 bed angles and autonomous underwater vehicle data (2 m resolution), taking smaller resolutions into consideration. To clarify this, we will provide further details on the determination of the $C_{\max}$ prior in the main manuscript (previously primarily described in supplement section S4).

The referee is correct that, strictly speaking, the Viscous Grain-Shearing theory only applies to granular material. However, as outlined in detail above, this is consistent with our definition of rigid beds (granular but relatively undeformable material). Furthermore, glacier beds, e.g. the bed beneath Thwaites glacier, often do not support the clear differentiation between rigid beds and soft sediments assumed in the derivation of sliding laws. Instead, the bed might consist of a thin, deformable sediment layer draped over a rigid bed or alternating patches of sediment and rigid bed. Ultimately, the goal of this study is to find the basal sliding parameterisation that best captures the basal conditions identified by the acoustic impedance measurements. As the referee pointed out, by assuming $C_{\max} = \mu$, we would effectively be testing two very similar sliding laws, which undermines this objective. Following this logic, and since μ and $C_{\max}$ describe different physical properties, there is no reason why the two parameters should have the same prior.

[The title is a too strong statement compared to what you are actually able to infer. Furthermore you focus only on Pine Island glacier, not all Antarctica. I would propose instead: "Evidence of stress bounded friction law at Pine Island Glacier (Antarctica) inferred from seismic observations."]
We agree that the title is misleading and we will adjust it in the revised manuscript. However, while we only infer the sliding law for Pine Island Glacier, the methodology developed here can be applied to basically any acoustic impedance measurement collected on the Antarctic Ice Sheet.

Specific comments

[L123 - By doing this you are not testing the Tsai law anymore .... This is the Budd part which make Tsai less likely in your result. I would remove the Tsai law as you cannot really test it.]
It is correct that we examine the Tsai-Budd instead of the Tsai sliding law itself and we will clarify this in the revised manuscript. However, due to its unique concept and mathematical form, the Tsai-Budd sliding law provides valuable insights, and we, therefore, prefer to keep it as part of the analysis.

[L125 - why this value ?]
and
[L137 - you should mention that you fixed Cmax=0.5 when varying Cs and explain why]
and
[Fig. 5 - why 0.5 ?? The best μ is 0.23. I expect you would use the best value found when varying u_t]
$\mu = 0.5$ is the Coulomb friction coefficient with the highest prior probability. Following this logic, we initially set $C_{\max} = 0.2$ (the value with the highest prior probability). However, this value led to a high percentage of incompatible $u_{\mathrm{b}}-\tau_{\mathrm{b}}$ pairs and we, therefore, increased it to $C_{\max} = 0.5$.

Using $\mu = 0.23$ would favour the Zoet-Iverson sliding law compared to the other sliding laws, as this information only becomes available through running the experiments. For the Tsai-Budd sliding law when varying μ, we previously relied on the referee's suggested approach as our prior knowledge about C_{B} is limited (log-uniform prior). However, all of the above is no longer an issue, since we now vary all four parameters simultaneously.

[L150 - you could call this parameter differently as it is not a speed anymore...something like "transition speed coefficient" and write it $1/C_{\mathrm{zi}}$. this would be more consistent with the Schoof law where a similar coefficient is equal to $(C_{\max}/C_s)$. So you would have in the schoof law: $u_t = (C_{\max}/C_s * N)^{1/m}$ and in the Zoet-Iverson law: $u_t = (1/C_{\mathrm{zi}})N$.]
We agree that "transition speed coefficient" is a better description of $u_{\mathrm{t,noN}}$ and will revise the manuscript as follows: $u_{\mathrm{t,noN}} = C_{\mathrm{ZI}} = u_t/N$.

[L217 - Based on what the prior values are chosen?]
Following the suggestion of the first referee, a table containing detailed information on the porosity and grain size data as well as further details for the priors of the specific sliding law parameters (e.g., $C_{\max}$) will be added to the revised manuscript.

[Fig. 6 - why Schoof(C_s) is not here ?]
Schoof(C_s) was not included here because of the large number of incompatible u_b–τ_b pairs. We will add further information regarding this issue to the revised manuscript.

[L266 - you should give the MAP parameters]
The MAP parameters are listed in Fig. S21 and S23. We will add a reference to these figures here.

[L299 - this comes from the friction law, not directly the modeled impedance. It should be clear.]
The effective pressure is calculated using the friction law, but the friction law parameter used in this calculation is inferred from the acoustic impedance misfit. We will clarify this in the revised manuscript.

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