Review of "Viscoelastic mechanics of tidally induced lake drainage in the Amery grounding zone" by Zhang et al.

I thank the authors for their response to my comments and for performing a new set of simulations with the value of the ice stiffness parameter, which is more relevant for the Amery Ice Shelf conditions. From the revised text, it appears that the conclusions of the study have not changed, despite quite substantial changes in the results (e.g., the Maxwell time). Although the magnitudes of the values have been updated, the text describing them have not. As a result, many statements appear to be at odds with themselves. For instance, sentences in lines 61-62 state that the semi-diurnal period of 12 hrs is close to the Maxwell time. With the original value of 9 hrs that was a reasonable statement. With the new estimate of 120 hr, the two values differ by an order of magnitude. Typically such a difference is viewed quite large and the two timescales are considered as distinct. In the context of rheology, an order of magnitude difference between the high-frequency forcing (12 hr) and slow viscous relaxation (120 hrs) implies purely elastic response to the tidally induced flexure. Below, I make suggestions on how the manuscript could be fairly straightforwardly modified to avoid such issues.

Taking another close look at fig. 1, it is not clear to me how strain rates shown in panels (c) and (d) have been computed for the portion of Amery Ice Shelf where the lake is located. The text in lines 90-94 states that the strain rates were projected on the flowlines, however, the flowlines, including those that pass through the lake meander and do not follow straight lines. For instance, fig. 1(a) shows that at the lake location, on the ice-shelf side the direction of ice flows almost perpendicular to the ice flow upstream of the grounding line. However fig. 1(b) does not seem to reflect that, as such a convolving flow would result in a large shear at the lake location. It seems that computations of the along-the-flow and transverse strain rates did not account for changes in the direction of the flowlines themselves. From fig.1(a), it appears that ice is under compression (the size of arrows indicating the velocity magnitudes upstream of the grounding lines get smaller towards the lake), however, figs.1(c-d) show that strain rates are extensional. Though the authors state that they follow Wearing (2017), which is a PhD thesis (typically they are peer-reviewed), the procedure of computing the strain rates is not clear. A standard approach would be to compute principal strain rate components, including at the lake location. Fig 1(b) shows a field labeled 'principal strain', not 'principal strain rate'. I suspect it is a typo, and the panel shows principal strain rates. Still, it is very difficult to make any inferences whether they are extensional (positive) or compressional (negative) at the lake location.

Since no changes to the text have been made, it appears that there are continuing confusions about the leading-order momentum balance of ice flow in particular geometric settings. The authors simulate ice flow along a flowline of a narrow outlet glacier. The current understanding of such glaciers and their leading-order momentum balance is based on studies by Raymond (1996), van der Veen & Whillans (1996) and many others from that decade and earlier. The results of these studies, widely supported by observations, show that the along the flow velocity u has a strong dependence on the width of the glacier. For instance, van der Veen & Whillans (1996) approximated it as

$$u \propto W \left(1 - \left(\frac{y}{W}\right)^4\right), \tag{1}$$

where y is the distance from the centerline in the direction transverse to the main direction of ice flow, and W is a halfwidth of a glacier. As apparent from this expression, despite the fact that shear at the centerline is zero, the ice flow along it, as well as through the glacier width, is strongly affected by the lateral shear at its sides, and the narrower the width, the larger its effects are.

The authors argue that they disregard the effects of the lateral shear based on the estimates of the transverse strain rate shown in fig.1(d). However, the momentum balance eqn. (4) is between the **divergence** of stress and the gravity force. The gradient of the extensional strain rate of ice flow in a narrow channel upstream of the lake is large in the direction transverse to the ice flow (fig. 1c). There are numerous studies of laterally confined glaciers that use a flowline geometry, similar to one used in this study. In such studies, the effect of lateral shear is parameterized, and is accounted for by an additional term in the momentum balance of ice flow upstream of the grounding line

$$\tau_w \propto \frac{|u|^{1/n-1}}{W^{1/n+1}}. \tag{2}$$

A discussion of the flowline formulations of the momentum balance and the effect of this term could be found in Schoof et al. (2017, p. 2285, third paragraph).

The tidal response of narrow outlet glaciers is also strongly affected by their lateral confinement. It is the ice flexural response to the tidal signal that is used to determine the location of the grounding line, either in altimetry or interferometry observations. The grounding zones - a span of the grounding line positions during high and low tides - are smaller in the case of narrow outlet glaciers (a few hundred meters, e.g. Antropova et al.; 2024) compared to laterally unconfined or very wide glaciers (order of 6-7 km, e. g., Rignot et al.; 2024). Consequently, the model used in this study is not suitable for investigations of the stress regime at this particular location of the grounding line of a very narrow outlet glacier.

However, this study and its results as they are, can be applied to many other locations where ice flow upstream of the grounding line is laterally unconfined. Without claims that the results explain this particular lake drainage, the manuscript is a solid and very useful contribution to the literature. By moving away from this specific location, the authors can use a much wider range of parameters and consider circumstances, for which Maxwell rheology is not only appropriate but also necessary. Their sensitivity analysis to the Maxwell time becomes more valuable as they can consider many locations with broader range of the ice viscosity values. Being tight to one specific location, such an analysis is not informative. The Maxwell time is a ratio of viscosity to Young's modulus. Although the exact value of Young's modulus is not well constrained for ice, the results of laboratory experiments show that it does depend on other parameters, such as temperature, stress, etc., and could be treated as a constant. In contrast, the ice viscosity strongly depends on the strain rates and ice temperature. Essentially, a sensitivity to the Maxwell time is a sensitivity to the ice viscosity, which can vary significantly for different glaciers, as well as over the same glacier. Its results would be useful either in the context of various locations or various climate conditions or both.

In summary, I would be glad to recommend the manuscript to publication if the authors shift its focus from the drainage of the lake on Amery Ice Shelf to a more general question of tidally induced stresses at the grounding line in the presence of supraglacial water. This can be achieved with very minor modifications of the text (mostly removing parts related to Amery Ice Shelf). With its current focus, the assumptions of the study are at odds with the current understanding of the leading-order momentum balance of outlet glaciers; the use of the Maxwell rheology is not well justified due to a very specific value of the ice rigidity parameter that the authors have to use for this particular location. As a consequence, the conclusions feel somewhat far-fetched.

References

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