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

The authors use a two-dimensional model (the vertical cross-section) to simulate ice flexure at the grounding zone caused by diurnal tides with the goal to explain drainage of a supraglacial lake on the Amery Ice Shelf observed by Trusel et al. (2022). The authors begin with estimates of stress regime of the outlet glacier and the grounding zone where the draining lake was observed and then proceed to describe their model and its results.

I have two fundamental problems with this study. The first one is application of the results of a highly idealized study to the observed lake drainage. Although I commend the authors for trying to estimate various stresses from remote sensing observations, the estimates seem to have been done after the fact, as an attempt to justify the modeling assumptions. These estimates, or rather presented or describe data suggest that various assumptions made in this study are violated. This suggests to me that conclusions drawn from the model results are not applicable to the stress regimes and conditions at the outlet glacier and the grounding zone of the Amery Ice Shelf. Firstly, the authors argue that because the background stresses are small, they "focus on the the streamline and adopt a 2-D flow line model" (as a side comment, it would be highly desirable for the manuscript to have lines numbered). The figure below is fig. 1a of the manuscript. It shows

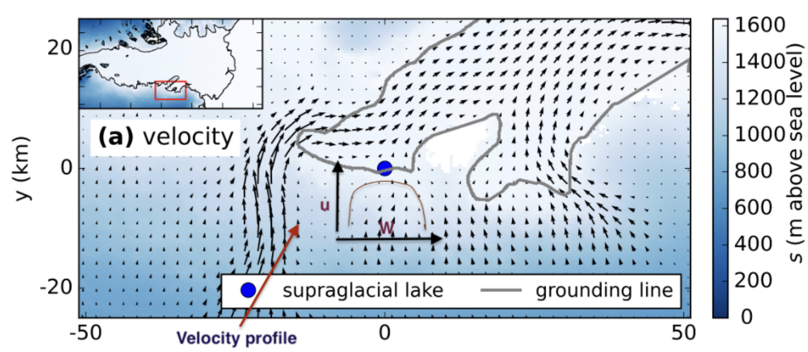

Figure 1: Panel (a) of fig.1 of the manuscript. The thin red line depicts how ice velocity u varies along the width of the outlet glacier W,

that the flow of the outlet glacier is strongly affected by the lateral shear.

The size of the arrows depicting the ice velocity are much lower outside of the centreline. My hand-drawn thin red line meant to illustrate how the along the flow ice-velocity component changes across the outlet glacier. This is a typical velocity profile of ice flow strongly affected by the lateral shear caused by the presence of the lateral confinement (*e.g. Raymond, 1996*). Its effects cannot be ignored. Consequently, they need to be accounted for either by having a three-dimensional model that includes the second horizontal dimension transverse to the ice flow and imposing the relevant conditions on the lateral boundaries, or by parameterizing them in the momentum balance eqn. (3). These effects of the lateral shear will substantially alter the model results.

Secondly, the quoted magnitude of the observed velocity imposed at the inflow boundary is low, 9 m/yr; so are the magnitudes of velocity shown in fig. 1f (a minor comment: it is unclear whether this velocity profile is computed or observed). Using parameters listed in table 1 and the Shallow Ice Approximation one could estimate the ice surface velocity resulted from the internal deformation only, assuming no-slip at the ice bed interface. That value is ∼20 m/yr, which is larger than the observed surface velocity by a factor of two. This suggests that (a) either the chosen parameters are off (specifically the ice stiffness parameter A_0, which I will come back to) or (b) the ice flow is dominated, or strongly influenced, by the vertical shear, and the focus on the longitudinal stress τ_{xx} is unwaranted, or both.

Thirdly, the chosen value of A_0 is very high. The ice-stiffness parameter is a function of the temperature of ice through its column. The chosen value would correspond to ice temperatures of the range from -5°C to -7°C, which is very warm. Although summer temperatures can exceed freezing point from time-to-time, as indicated by the supraglacial lakes, the annual mean surface temperature is around -20°C (*e.g.*, Kittel et al., 2021). With ice flow primarily driven by the internal deformation, the ice temperature through the most of the ice column is not substantially warmer; it is only in the fairly narrow band near the bed it is warmer due to the geothermal heat flux. The very high chosen value of the ice stiffness parameter leads to a very low ice viscosity, of the order of 10^{13} Pa·s, which is at least an order, or more likely two orders of magnitude lower than the typical values of ice viscosity.

This brings me to the second problem with the study — the choice of the ice rheology. The authors have estimate it 9 hrs (the penultimate line on page 2) and 40 hrs (the penultimate line of section 2.3 page 6). For more realistic values of ice viscosity it is of the order 5-15 days, which is substantially longer than the period of diurnal tides that cause the ice flexure. This fairly unambiguously indicates that ice responds to diurnal tides as elastic medium. Two questions that immediately comes to mind — is it worth the effort the authors have gone through and complexity of the viscoelastic rheology? Can't one simulate it with much simpler elastic rheology?

Considering all issues with the study, applications of its results to the lake drainage on the Amery Ice Shelf and the drawn conclusions are questionable. It is entirely plausible that tides might have played a role in it. However, one cannot make any relevant statements based on the presented results.

References

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Trusel, L. D., Pan, Z., & Moussavi, M. (2022). Repeated tidally induced hydrofracture of a supraglacial lake at the Amery Ice Shelf grounding zone. *Geophysical Research Letters*, 49, e2021GL095661. https://doi.org/10.1029/2021GL095661