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The Cryosphere
Discussions

Interactive comment on "Tidal bending of ice shelves as a mechanism for large-scale temporal variations in ice flow" *by* Sebastian H. R. Rosier and G. Hilmar Gudmundsson

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In their manuscript, the authors describe an alternative mechanism for producing tidal modulation of ice stream motion that is dominated by fortnightly periods. Their new mechanism relies on the viscous softening caused by high deviatoric stresses induced by flexure, and has its forcing in the ice shelf rather than within grounded ice, which many other mechanisms would suggest. While the basic result is clearly described and interesting, there are a number of significant issues with the manuscript, the foremost being (1) it is not clear that the right order of magnitude of stresses can be produced if realistic bending, without an assumed fulcrum/fixed boundary condition at the grounding line, is used since allowing grounding line migration significantly lowers the bending stresses; and (2) there needs to be discussion of and comparison with the recent paper of Robel et al. (Annals of Glaciology, 2017) which suggests some of the same points but has a different basic mechanism responsible. Given that the recent Robel et al. paper appears to adequately explain the observations and that the newly proposed model may overestimate its own relevance, even for a confined ice stream like Rutford, the authors need to significantly revise their manuscript to address these major issues before it will be a useful contribution. Related to this, a more direct comparison with observations seems necessary; the authors should discuss why their proposed mechanism fits observations better than other theories, which aspects of the observations point specifically to the proposed mechanism, and how general their mechanism is.

Major Comments:

1. In reality, the grounding line does not act like a fulcrum and is not fixed. Although the authors have discussed the possibility of grounding line migration somewhat, they have not discussed whether the bending stresses simulated near the grounding line might be overestimated because of the lack of migration (which alleviates the need of the grounding line to bend somewhat). Because the grounding line is assumed to be pinned ("clamped"), they cannot evaluate the possibility that asymmetries in grounding line migration may produce a strongly nonlinear ice shelf flow response (as in Robel et al. 2017, see later comment). This fixed nature of the assumed grounding line therefore seems to be a very important difference between the simulation result with reality, and must be discussed. At a minimum, the authors should describe why they expect their modeling framework to still be useful despite the simplifications.

2. It is a basic mathematical fact that a nonlinear process forced at more than one frequency will produce a response at harmonics and beats of those frequencies. The authors claim later in the paper that the flexure mechanism is the only way to produce the M4 response, but they have not proven that other nonlinear processes could not produce such a response. Indeed, Robel et al. 2017 makes this exact point in their

equations 11-13. Which brings up the next point. . .

3. There needs to be much more engagement throughout this paper with the arguments put forward by Robel et al. 2017. While we recognize that this paper was published near the time of submission of the current manuscript, the fact that the article discusses so many of the same issues, including many of the main points of the present manuscript, while also proposing a different basic mechanism related to asymmetries in contact stress from asymmetric grounding line migration, obliges the authors to discuss the Robel et al. paper and contrast their work with that work. For example, at a number of points, it is claimed that the tidal flexure mechanism is the only way to produce an increasing Msf signal in the shelf, which is also what Robel et al. 2017 claims, and the authors also claim that previous models do not reproduce observations in floating ice shelves (which is not true anymore due to the Robel work). Lines 25, 36, 155-160, 295-300, and all of the discussion and conclusions therefore need modification to be accurate and to appropriately cite the present literature.

4. (Lines 357-361 and elsewhere) What about Msf signals generated in the grounding line and then propagated downstream throughout the shelf? Wasn't this the previous explanation for the ice shelf Msf signal? Something that is not remarked upon in this paper in the temporal phasing of signals, which is important given than the Msf signal appears first in the ice shelf.

5. I agree that the elastic response can only ever yield a linear response. However, the elastic response can potentially produce a large signal at the primary tidal frequencies. The authors should at least provide an argument (in the analytic section) as to why the elastic deformation is small and so can be neglected in the analytic section.

6. It is clear from the difference between n3xyz and n3xy experiments that confinement plays an important role in producing the Msf signal at an amplitude comparable to that observed at the RIS shelf. What about unconfined shelves? Does this indicate that such shelves should have much less Msf response? What about Bindschadler and the

other FRIS ice streams? For example, does this imply that the proposed mechanism does not explain the observations of a significant Msf response at Bindschadler.

Also, it would be good to state, early on, that Rutford Ice Stream goes afloat in a trough and remains in that trough, for perhaps $\sim$100 km downstream of the grounding line. A map of ice velocities (like Figure 1b of Minchew 2016) would help put this in context.

7. One aspect of the Minchew 2016 observations that are not explained by this model is the along-flow variation in strain rate in the ice shelf. That study invokes a possible pinning point to produce such heterogeneity. Perhaps this should at least be remarked upon.

Minor Comments:

Line 16-17: Please rewrite for clarity: "the primary ice flow response is at a different frequency than the highest amplitude frequency of tidal forcing"

Line 26-27: Awkward sentence phrasing

Line 30: rheological behavior and the response to external forcing

Line 79-81: Would be useful if this sentence came much earlier to direct focus to the figure

Line 122: There are not separate viscous and elastic stresses. There is simply one stress which causes both viscous and elastic deformation. This sentence therefore should be rewritten to clarify. Equation 10: This is clearly only do-able in this way for n=3. Could you use a perturbation approach (or expand about \tau_xy) to solve for general n?

Line 176: How large are the linear elastic and damming stresses in comparison to the nonlinear viscous changes that you are simulating?

Line 231: It needs to be explained more clearly why the elastic modulus in the analytic calculation has to be so different from the numerical calculation. (Why does this produce such high bending stresses in the analytic case?)

Line 240: What is an elastic foundation? Perhaps use a clearer term.

Line 245-250: Should say why you can be sure that the nonlinear sliding law isn't producing the Msf response. (I can see that it isn't because all the numerical experiments use the same sliding law.) Also, the authors should comment about what this implies about the results in Gudmundsson 2007, 2011 and other studies. . .

Figure 4: Msf amplitude in what quantity? (same comments for line 298)

Line 303-308: Should state outright here that the amplitude of Msf response is an order of magnitude less than in the n3xyz experiment.

Line 327: units of w_a?

Line 367: Would the response at M2 and S2 frequencies be larger if the elastic modulus was different? The relative difference between linear elastic and nonlinear viscous responses will be a strong function of the relative size of viscosity and elastic modulus.

Line 408: Remove "to this day"

-Victor Tsai

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