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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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We thank Victor Tsai for his comments on our manuscript. Included below is our response (in italics) to each of his major concerns (in bold).

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.

This point raises two separate issues: overestimating the magnitude of bending stresses and the potential role of GL asymmetry to generate an Msf signal, and so we address each in turn. Firstly, regarding bending stresses, although it is not mentioned in the manuscript we tested whether allowing the GL to migrate (for a steep bed slope) had a major impact on the magnitude of bending stresses and this was not the case, we will add a discussion on this issue in the revised manuscript. On the second point, as we mention in the manuscript, we intentionally do not allow the GL to migrate in order to isolate the nonlinear rheological mechanism that we are proposing from this alternative mechanism. Since there is currently no strong evidence of GL migration in this area and bed slopes around the GL are not known, it would be difficult to properly model this effect anyway. Also, GL asymmetry was first proposed (and modelled in some detail) as a mechanism a few years previously (see section 3.2 of Rosier et al., 2014).

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. . .

We absolutely agree with the point made here, certainly any nonlinear process will produce other frequencies as we discuss in the paper. This needs to be made clearer in our manuscript and we are not trying to claim that this mechanism is the only one capable of producing these high frequencies. Our argument is that there should be a large *difference in the amplitude of the response in ice velocity at these higher frequencies that could help diagnose which mechanism is at play. In the Robel (2017) mechanism, the primary response over one tidal cycle is to increase velocity at high tide and decrease velocity at low tide. As the reviewer points out, other frequencies will be in the velocity waveform because the response is nonlinear. However, in the mechanism we put forward, the primary response over one tidal cycle will be to increase velocity twice during one tidal cycle (i.e. precisely at the higher frequencies), and so the high frequencies can be expected to be of much larger amplitude. In this way, observations of a strong velocity response at these frequencies would be evidence that this mechanism is playing an important role. The main point we are raising, therefore, is that while all non-linear processes can give rise to M4, S4, Msf etc. the ratios of those amplitudes will, in general, be different. We do realize that in our original manuscript this was not particularly well articulated, and we will make changes accordingly.*

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.

We will add discussions of the Robel 2017 paper throughout as is appropriate since we are investigating the same problem but come to very different conclusions (incidentally,

we did not see a copy of the Robel 2017 paper before submission, as can be easily verified by comparing the dates of submission/publication).

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.

The assumption previously (before the Minchew 2016 satellite observations) has been that the Msf signal was generated upstream of the grounding line (due to a nonlinear sliding law and/or subglacial drainage processes). The Minchew 2016 observations show the phase leading on the ice shelf, and this is replicated in our model. We can add a remark on this point in the revised manuscript.

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.

We do not understand the point being made here. We all agree that the elastic part of the Maxwell model can only yield a linear response and we explain in the same paragraph that we are concentrating on the nonlinear response because this is the only thing that can explain the observed Msf signal. We are hence not neglecting the elastic deformation but simply using the fact that the linear response cannot generate any Msf signal and does therefore not need to be considered in this particular case.

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 signif-

icant 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 âĹij100 km downstream of the grounding line. A map of ice velocities (like Figure 1b of Minchew 2016) would help put this in context.

In an unconfined ice shelf (such as an ice tongue) our proposed mechanism would still produce an Msf signal at the main GL, and since there would be no sidewall friction the amplitude of this signal would not decay downstream, unlike in our n3xy simulation. Certainly this mechanism will be strongest for very confined ice shelves of which the outlet of Rutford is a good example but many others exist, for example Evans and Foundation Ice Streams. The Msf response at Bindschadler is far smaller than is observed on the FRIS ice streams (because the semidiurnal tides are of low amplitude) and it seems that this could be easily produced by bending stresses at the GL but it is possible that the pinning point downstream plays a role. Determining this would require more observations, together with accurate measurements of bed slopes and/or migration distances.

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.

Our mechanism could in fact produce heterogeneity in any number of ways, through variations in ice properties. The ice rheology in our model is kept intentionally homogeneous to avoid complicating the interpretation and although we use a very simplified Rutford geometry our goal is not to reproduce these observations or indeed discuss them to any great length. That being said, the heterogeneity is interesting and we will add a discussion on this.

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