We are extremely grateful to the anonymous reviewer for going through the manuscript in detail and providing very helpful comments. In the text below we have addressed (in red italics) all the main points that were raised (in bold). All minor points/corrections will be implemented into a revised version of the manuscript if we are invited to submit one.

Sebastian Rosier and Hilmar Gudmundsson

## Anonymous Referee #2

In this study, the authors use a sophisticated 3D viscoelastic model of ice shelf flow to calculate the horizontal flow response of the Filchner-Ronne Ice Shelf (FRIS) to tidal forcing. The study is meant to be a comprehensive examination of the many theories that have been put forth to explain the large amplitude response of ice flow at FRIS at both M2 and Msf tidal frequencies, all within a single modeling framework. In this goal, I think the authors have generally been successful, and this is an impressive achievement which brings together many ideas and observations spanning the last decade or so. What is perhaps most interesting, is how many theories (including their own) the authors are able to rule out for explaining the conundrum of the large amplitude Msf signal at FRIS. This study provides what I think is a valuable "refocusing" of the conversation around low frequency tidal signals in ice shelf flow and how to explain/model them. Though I am generally happy with the scientific content of this study, in what follows, I mainly point out some issues regarding information that is missing, information that could be condensed, and general textual edits that could be made to the manuscript.

Major points:

**1. The manuscript has a narrative quality that takes the reader through the process of trial-and-error followed by the authors in eliminating possible explanations for the observations and coming to a final conclusion. This is fine and I think actually helpful to bring negative results into the literature in a way that is rarely done nowadays. That being said, the discussion in section 3.2 is often a bit cumbersome and overly detailed and will sometimes talk about the results of certain experiments (more on this in comments below) before they have been described. It would be worth it to condense the discussion on pages 11-13 a bit and spend more time talking about the details of the successful GLmigration experiments (which are currently a bit light on detail). I have a similar critique of the discussion and conclusion sections which re-hash many of the details right after they are discussed in section 3.2, these could also be streamlined to focus on what you learned from the successful simulations.**

*The paper was somewhat difficult to write due to large number of different model experiments. We would argue the manuscript is more ponderous than cumbersome but we agree that there is room for improvement! We will take on board this advice and streamline the later sections of the paper to hopefully condense our discussion without missing important results.*

**2. In all the figures (3-5) comparing modeled amplitudes to observed amplitudes, the reader must guess the correspondence by eye. This is not really feasible and doesn't permit an easy identification of spatial vs. constant-offset mismatches. What would enable this comparison better is a scatter plot of the modeled amplitude vs. observed amplitude at the GPS sites for all simulations. This could be done by adding a single panel to each figure with markers for each experiment distinguished by color and/or marker type.**

*Both reviewers have suggested improvements in how we could present our model results and we are grateful for these suggestions. We will implement a combination of these suggestions that works well and produce improved figures in the revised manuscript.*

**3. Overall, you do a nice job of constructing clean experiments that test the role of individual mechanisms in generating the tidal signal of interest. I think it is fair to say that you find the only mechanism which can really generate anything near the observed Msf amplitudes for reasonable parameters is GL migration. But then you go off on this exploration of how you could have slightly less GL migration if you also increase the Glen's law nonlinearity. This is true, but you could also slightly boost the amplitude using many of the other mechanisms that you tried as well. The point isn't necessarily that the other processes (including a high value of n) aren't happening, its just that they likely aren't playing much of a role in generating the Msf signal. I think the n = 5 experiment in Figure 5 muddies this point by selectively picking one of the "failed" mechanisms to bring back into the conversation. It would be better to leave it out (or to a supplement) I think.**

*We can see the point that the reviewer makes here but we would still argue that exploring how the grounding line migration mechanism is affected by changing the Glen's law exponent is a valuable part of the manuscript. Certainly other mechanisms that were tested could also have reduced the amount of grounding line migration needed to match the signal but choosing a value of n=5 had the largest impact (by a considerable margin) on Msf amplitude after the grounding line migration mechanism. Given this, and that the value for n is almost universally and somewhat arbitrarily fixed at 3 despite being poorly constrained, we felt that including this additional experiment would be of greatest interest to the community. With more observations and modelling it might become possible to use this approach to obtain additional constrains on the value of n. We hope that by including a better explanation for our choice in the manuscript that the reviewer will agree that this is worth keeping.*

**4. When you describe the model in section 2, you specify certain choices (e.g., boundary conditions) which you later loosen or change. It would be helpful if you said up front, we set X for the default simulation, but we will loosen this assumption in later simulations. Just so that the reader is prepared to know what choices are constant across all experiments, and which are changed.**

*We will ensure that the revised manuscript makes it clearer which modelling choices are fixed and which are changed for our various experiments.*

**5. You do a nice job of reviewing the literature on proposed mechanisms for the FRIS Msf signal early in the manuscript. However, later when you discuss the experiments you try and which are successful there is less attribution of ideas to previous studies. In particular, the idea that asymmetries in grounding line migration can produce a large Msf signal through changing basal contact and pinning points was originally put forward in Robel et al. (2017). Yet there is no discussion of what that study gets right and wrong in any of sections 3.2, 4 or 5. It would be helpful to discuss and evaluate these previous studies in more detail, as you have done for some of the other mechanisms discussed.**

*On reflection we agree with the reviewer that the later sections of our paper would be greatly improved by adding more discussion regarding previous studies and we will expand on this in the revised manuscript.*

Minor points:

P. 1 Line 6: entire Filchner-Ronne

P. 1 Line 11: tidal ice flow response

P. 1 Line 5-11: this is a bit detailed for the abstract, perhaps compress this discussion somewhat

P. 1 Line 19: flows, on average, 21%

P. 1 Line 21: floating ice shelves

P. 1 Line 23: who is the "us" here? did you do this work?

P. 1 Line 24-25: Ice shelves are not thought to modify upstream flow, and also to have a fundamental effect on the. . ..

P. 2 Line 4: delete sentence: "Ice shelves. . .viscous processes" - more confusing than illuminating P. 2 Line 4-5: increasing quantity of GPS and InSAR observations

P. 2 Line 8: delete "while. . .as such"

P. 2 Line 12: ones -> frequencies

P. 2 Line 12: delete "long-periodic"

P. 2 Line 14: several times in the paper you state that a nonlinear mechanism is needed to transfer tidal energy from short to long periods. Why? Readers not versed in this literature (or the idea of frequency mixing) will not find this to be an obvious statement. Could be helped by a brief explanation here of heterodyning/freq mixing.

P. 2 Line 16: models that only include elastic flexure

P. 2 Line 17: delete "with exact numbers dependent on location"

P. 2 Line 23-24: explain "the mechanical coupling between vertical ocean tides and ice flow occurs in the grounding zone"

P. 2 Line 24: our modelling and observational efforts

P. 2 Line 27: using a viscous

P. 2 Line 31: will be to replicate

P. 2 Line 34: suggested in previous studies as responsible

Figure 1: there's about 5 different types of information on this map? Are all of them strictly necessary (i.e. is elevation)? Would be helpful if you simplified.

P. 4 Line 7: constituents have the largest amplitude in this region

P. 4 Line 12: motion, but not

P. 4 Line 14: constituents, these do not rotate around an amphidromic point, and instead

P. 4 Line 22: signal and the (horizontal) ice shelf flow response

P. 4 Line 23: in the Weddell Sea, the semidiurnal

P. 4 Line 29: tidal cycle, periodically causing direction

P. 4 Line 33: diurnal

P. 5 Line 22: also these studies have shown that the Msf signal occurs in the ice shelf first (using phase information)

P. 6 Line 4: processes using a X, Y, Z model

P. 6 Line 5: focus on modelling the two strongest responses observed in horizontal

P. 6 Line 13: angular momentum (respectively):

P. 7 Line 8: what does upper-convected mean in this context

P. 7 Line 5: explain the i, j indices

P. 7 Line 14: from what observation is the rate factor inverted? using what inversion approach? I know the detail is in appendix, but summarize in one sentence here

P. 7 Line 15: what are the other parameters(a table would be helpful) and how are they set. You get at these details somewhat (regarding E) later, but would be more useful here.

P. 7 Line 19: At the ice surface, a stress-free boundary

P. 8 Line 1: are uobs and vobs constant in time?

P. 8 Line 3: here is one place where it would be helpful to explicitly state that vertical clamping at grounding effectively prevents GL migration in the default setup, but this assumption will be loosened later on to allow grounding line migration to occur

P. 8 Line 15: interpolation is used to

P. 8 Line 5: the other benefit of only including principal diurnal and semidiurnal constituents is that you ensure that the presence of any other constituents in the ice shelf response has to do with processes in the flow and not the forcing itself

P. 8 Line 24: how do you generate the mesh?

P. 8 Line 30: this implies that the mechanisms which produce M2 and Msf flow variability are not the same. Perhaps it is worth saying this explicitly?

P. 9: Please provide more details on what you mean by "processing surface model displacements using the Tide MATLAB package"

Figure 3 and elsewhere: you often say things like "ice shelf motion" - this is really vague. Is this horizontal displacement? is it detrended? please be more specific on this

P. 10 Line 14: you refer to RFstreams here before even describing what it is. Either do that here, or remove this reference

P. 10 Line 18: its it's

P. 11 Line 6-10: why was no M2 signal generated in the grounding zone

P. 11 Line 13-15: delete "we start by. . .fig 4" sentence - or move down, it doesn't make sense here

P. 11 Line 20: rheology which is the source of nonlinearity in this mechanism

P. 11 Line 24-25: how can we tell the origin from the plots in figure 4? Are you relying on phase information that isn't plotted? It may be useful to say something about phasing.

Figure 4: the colorbar of observed amplitudes is inconsistent with the modeled colorbar (and pretty much every other figure). I can see that there will be a large discrepancy, but that's the point, isn't it? Also, plot observed filled circles on all panels.

P. 13 Line 4: model assumes hydrostatic

P. 13 Line 30-35: you introduce the gammas, but don't explain how they are calculated so we have no context for what these numbers mean, or that they come from Tsai and Gudmundsson (I know its in the appendix, but since this mechanism is important for this study, its worth explaining in the main text)

P. 16 Line 2: exploring the processes and parameters that play a role in generating Msf signal

P. 16 Line 11: found that the amplitude of the Msf signal in observations

P. 16 Line 14: difference

P. 18 Line 5: tidal modulation