We are extremely grateful to Julia Christmann 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 #1

The paper "Exploring mechanisms responsible for tidal modulation in flow of the Filchner Ronne Ice Shelf" by Sebastian H. R. Rosier and G. Hilmar Gudmundsson presents an interesting comparison of GPS measurements and simulation results considering the influence of ocean tidal forcing. The authors explore the impact of different mechanisms on tidal modulation in horizontal flow. First, the modelled semi-diurnal tidal constituent M2 for the whole Filchner-Ronne Ice Shelf is compared to results of GPS measurements and afterwards the same is done for the long-periodic tidal constituent Msf. The authors claim that the relatively high amplitude for Msf can only be obtained if the model allows grounding line migration.

General comments:

I have a number of major concerns with this paper, which are detailed below. In addition, some more specific comments and technical corrections are listed at the end of this review.

- **The authors show in Table 1 an overview of the various model versions. Can you also include a Table where all parameters of the default setup are included, please? Then it is directly clear to the reader of the paper which parameter change is done for a different model setup and all parameter values are defined. For example, a value for Poisson's ratio is completely missing in the paper.**
  We will add a table of the model parameters for the default setup to the revised manuscript, together with a description of the Poisson's ratio which is currently missing.
- **For most model setups (except RF_streams), the velocity directly at the grounding line is set to an observed value but assumed to be constant – i.e. independent of the tidal forcing. In reality, the tides will have a big influence on the velocities near the grounding line, which is also verified by GPS measurements the authors show in this paper (shortly noticed p.10, l.13-15). The major simplification of a constant horizontal velocity is hardly discussed in the paper. Would the results be different if the horizontal velocity at the GL depends on tidal forcing? This could be checked by adding a sinusoidal behaviour onto the prescribed velocities at the boundary, with amplitudes and frequency derived from measurements. In Figure 4c (RF_streams), several ice streams are added but the results are only slightly better. However, how long are the added ice streams? At their boundary, the authors also prescribe constant velocities. So maybe it is impossible to model the influence of the tides by this extended geometry as the length of the grounded ice parts is too short to get independent horizontal velocities (of the inflow boundary condition) at the GL. Why can the authors do this major simplification and then state by the process of elimination that the strong long-periodic Msf component is only obtained by grounding line migration?**
  We agree with the reviewer and this is why we included the 'RF_streams' experiment, we believe that it addresses these concerns. As the reviewer states, tides are known to influence velocities at and upstream of ice stream grounding lines. The 'RF_streams' experiment includes all major ice streams flowing into the domain. All ice streams in this experiment (apart from Moller which is

wide and slow flowing) extend over 100km upstream of the grounding line which is the maximum distance at which tidal perturbations in ice flow have been observed. Their lateral extent is determined by the shear margins, such that the lateral side walls of the ice streams are located where ice flow speed is approximately zero. As such, the fact that the ice stream boundary condition forces a constant velocity does not affect our results (for this experiment). We actually began our modelling work treating this setup as the 'default' but found that removing the ice streams had a negligible effect on the results (as we show in the paper). This is both interesting in of itself and distils the problem to a simpler one thus reducing the number of potential parameters. Forcing the model at its boundary with an Msf signal would cause the signal to be generated at the boundary but all the evidence points to this being a process generated within the ice shelf. This would not therefore shed any light on what causes the observed Msf signal.

- **Figure 2 (c): Why are there such large excursions in the geometry of the Filchner Ronne Ice Shelf? For example directly at the ice shelf front of Filchner Ice Shelf or on the left and right side of Ronne Ice Shelf. Also at other places irregularities are noticeable.**
  In the caption, the authors forgot to include (panel c).
  Depending on which specific features the reviewer is referring to these are ice rises and small outlet glaciers feeding into the shelf whose bathymetry is vertically exaggerated. We will add a reference to panel c in the revised manuscript.
- **To better understand the effects of the various model setups shown in this paper, it would be nice to have relative deviations between observed and modelled results. For instance, in Fig. 4 the reader can imagine the influence of the different cases, but cannot really detect how a certain change in the setup reduces or increaes the difference between simulation and observation. The Filchner-Ronne Ice Shelf covers a large area and the GPS measurements are only points increased in size to be visible.**
- *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.*
- **Did the authors do a study considering the influence of the applied mesh to the results? From my experience, a Maxwell model as every viscoelastic material model needs a very very fine mesh resolution (I agree that it is not possible to get such a fine mesh resolution for the whole Filchner Ronne Ice Shelf) to generate reasonable results that are nearly mesh-independent. Is the mesh resolution the authors take not too coarse for a viscoelastic material model? Are the results reliable? An additional question about the mesh that remains unclear for the reader: How many layers are used for the vertical model coordinate?**
  We tested mesh resolution by doubling the number of elements both horizontally and vertically and found that these did not significantly affect our results. We will add a brief description of these tests in the revised manuscript.
- **p.15, l.20-29: Why is Young's Modulus not a material constant? In all tested setups in your paper Young's modulus is constant using E=2.4 GPa except in the damage setup where Young's modulus is spatially changed but not in time. Or did I understand this incorrectly? Why should Young's modulus be a function of loading frequency for a viscoelastic material? I also wonder why are the viscoelastic properties of ice shelves better provided by your simulation? And the last point I do not really understand is what do you mean by cumulative elastic strain, if you fit Young's modulus to GPS observations?**
  Our wording here could be improved as we can see how this leads to confusion. The Young's Modulus as defined in the Maxwell model is a constant in most of our simulations and fixed at 2.4GPa. Our point stands however, that for a viscoelastic material subject to a periodic forcing, the concept of a constant elastic modulus breaks down and 'E' becomes a complex dynamic

modulus that is a function of forcing frequency. This is an important point to make since it has been overlooked in many previous studies of tidal behaviour.

Regarding the second point, as we show in the paper the horizontal motion at ice the front at semidiurnal frequencies is generated as an elastic response to the ice shelf tilting as the tides rotate around the Weddell Sea. Hence, the M2 signal we see in the model is not locally generated and is a result of cumulative elastic strain over the entire ice shelf. Thus, it does not provide a local estimate of elastic rheology (as is the case for all other previous experiments) but an integrated estimate over the entire ice shelf. We will try and make this point clearer in the revised manuscript.

- **p.16, l.6-9: In which setup did the authors test periodic narrowing and widening of the ice shelf and reduced buttressing from ice rises/rumples?**
  Adding grounding line migration to the model implicitly includes these two processes, since as the grounding line retreats/advances the ice shelf widens/narrows and ice rises within our domain unground/reground reducing/increasing buttressing.
- **In my opinion, the conclusion would benefit from a reduction to clearly stated important take-home messages. Maybe the authors can move some of the statements from the conclusions into the discussion?**
  The conclusion is admittedly rather long but there are quite a few results in the paper that need highlighting. The last paragraph of the conclusions is our best attempt to wrap everything up into the main take home messages. We will look to reduce the length of the conclusions where we can in a revised version of the manuscript.

Specific comments and questions:

- **p.2, l.15,16: Give a reference. What is the expectation based on simple models of elastic flexure?**
  We will add a reference to cover this in the revised manuscript.
  Are the numbers in the next line results the authors show later on?
  Yes, this will be rephrased for clarity.
- **p.4, l.32: How long are the measurements of the additional GPS sites near the outlet glaciers?**
  A large number of GPS sites are described in the cited paper and the measurement period varies from weeks to over a year.
- **Table 1: Include an additional table that contains the material parameters for the default setup. Then it is clear which changes between the various model versions are done. What is the value of Poisson's ratio?**
  This will be added to the revised manuscript. We use a Poisson's ratio of 0.41 in line with previous studies.
- **What are the unknows of the model (velocities or displacements), i.e. do the authors use a velocity or displacement formulation for the viscoelastic material model? For the boundary conditions, the authors use Dirichlet conditions for the velocities (p.7, l.25) and also show the resulting ice velocity field in Fig. 2c, but for the element discretization the authors write (p.8, l.22) "triquadratic interpolation shape functions are used for displacements". Is an arbitrary Lagrangian–Eulerian moving grid included in the model or how are the surface nodal displacements (p.9, l.2) determined in the model?**
  The unknowns are displacements, the Dirichlet boundary condition is implemented as a displacement divided by the time step.

- **p.7, l.18: For the water pressure applied at the ice front one needs the freeboard also depending on the time-varying local sea level (see the if condition “z<0”). Is this only a typo or how do the authors implement this?**
  The reviewer is correct, this should be written such that z is also a function of time and that is how it is implemented in the model, we will correct this in the revised manuscript.
- **p.8, l.2-12: The reader cannot see the increase of the M2 amplitude towards the grounding zone. A second figure with a magnification near a chosen grounding line would be helpful.**
  This will be added to the revised manuscript
- **p.11, l.4-6: When the vertical boundary condition is removed, the grounding line has to move and in my view, bending will always occur near the real position of the grounding line but maybe not at the position where the grounding line was before. What do the authors mean with “removing the effect of bending in the grounding zone”?**
  The point of this experiment is to remove the effect of bending stresses generated in the grounding zone. It is not a ‘realistic’ simulation but serves to shed light on what mechanisms are responsible for each part of the observed ice shelf response. The ice shelf does not bend in any meaningful way once this boundary condition is removed, perhaps the reviewer is referring to the tilting of the ice shelf?
- **p.13, l.35: What happens for a positive or negative tidal motion of 4 m, which fits better to the tidal range given in Fig. 1 for the grounding line region?**
  The migration distance increases linearly with tidal amplitude, and thus the Msf amplitude will also increase. However the 8m tidal range is only present in a limited part of the domain and it is highly unlikely that the steep sidewalls in this region allow such large migration distances so we don’t see any benefit in including results for many different tidal ranges.
- **p.21, l.7: Why did the authors put square brackets around K?**
  These will be removed
- **p.22, Fig. C1: The damage factor could reach a value of 0.8 and below in interesting regions, for example at the boundary of inflow regions to the ice shelf. In the text, the authors stated values of E between 1 and 9 GPa, but 0.2*2.4 GPa = 0.48 GPa. Are these realistic (meaning physically useful) values? In my opinion, E has to be a material constant.**
  Firstly, these refer to different experiments. We tested a Young’s Modulus of between 1 and 9 GPa to match the observed M2 signal on the ice shelf. We chose this range because most studies of elastic properties of glacial ice find E to lie within this range (although there are some considerably outside of it). In the damage experiment, the reviewer is correct that in some regions the high damage will lead to a very low ‘effective young’s modulus’ – but that is in the nature of the continuum damage mechanics modelling approach. The aim is not to derive realistic values for E, the aim is to attempt to model fractured ice as a continuum by representing the effects of damage on the material stiffness.
- **p.23, Fig. D1: The strain over time plots are only correct for a constant viscosity eta. These plots are confusing because in the paper the viscosity is always the nonlinear Glen-type viscosity. Furthermore, the authors should delete the dashed lines (as they are also confusing) and maybe give its explanation on the sides near the already existing arrows.**
  We will redraw this figure to make it clearer and address these concerns.
- **p.23, l.7,8: The Kelvin model does also not capture the long-term viscous behavior and is a representation of a solid and not like the Maxwell material which represents a viscoelastic fluid. Rephrase it for a better understanding.**
  We do not state anywhere that the Kelvin model captures long-term viscous behaviour and do not understand the reviewers comment.

- **p.24, l.14: The correlation between the bulk modulus K and the two elastic material parameters for an isotropic material, namely Young's modulus and Poisson's ratio, is missing. The same thing is missing for the shear modulus G. Is the viscosity the authors take for the Kelvin model not too small to see any influence? The order of the Maxwell viscosity from Glen's flow law is two orders of magnitude higher or is this not the case?**
  We define the shear modulus G by E and nu in equation 5 and we are not sure what is to gain from defining K since we only show the deviatoric viscoelastic equation which does not include K. We would prefer to keep the details of the viscoelastic rheology to a minimum since is likely to confuse readers who may be unfamiliar with these relations. We define the viscosity of the Kelvin element in line with previous studies although we tested various viscosities and did not find a value within a sensible range that lead to an improved fit with observations.

Technical corrections:

• p.1, l.1: "shows" (an extensive network is singular)

• p.1, l.10: "Filchner-Ronne Ice Shelf"

• p.1, l.11: delete the in "We evaluate all relevant mechanisms…"

• p.2, l.8,9: delete as such and in the following line a: "…in response to tidal forcing."

• p.2, l.27: a instead an "….using a viscous model."

• p.2, l. 11 and p.2, l.32 adjust notation of semi-diurnal in the whole manuscript

• Figure1: state the unit of tidal range in the caption or directly in the figure (dashed contour lines)

• Table 1: "Uses an exponent of 4…." instead of and

• p.6, l.7,11: abbreviation of three-dimensional is missing

• p.7, l.5 f.: (comma) definition of (.)…,… is missing

• p.7, l.8: definition of deviatoric stress and strain is missing

• p.7, l.8: a rheological model is the graphic depiction of spring and dashpot (cf. figure D1); delete rheological in "…an upper-convected Maxwell model…"

• p.7, l.11: definition or reference for the upper-convected time derivative is missing

• p.7, l.18: definition of the water density rhow is missing

• p.7, l.21: bold face of sigma as sigma is the stress tensor. Until now the authors only define its components

• p.8, l.8: CATS2008a instead of "Cats2008a", revise this in the whole paper

• p.10, l.12: outlet instead of "oulet"

• p.15, l.11: "is symmetric."

• p.20, l.14: unphysical without hyphen

• p.22, l.9: Did the authors mean T_B for the basal temperature instead of T_S?

• p.23, l.2: viscoelastic instead of "viscoelsatic"

- p.24, l.15: modulus instead of “Modulus”
- p.24, l.26: the bracket should end two words later
- p.25, l.20: i.e.
- p.26, caption Fig. E1: a verb is missing in the last sentence
- p.26, l.5: Why is U_T boldface? It is just a number