We are very grateful to Julia Christmann for going through the manuscript again and providing her feedback. The document is colour coded as follows: black bullet points are the original reviewer comments, red text is our original reply, green text is the most recent reviewer comments and blue italicised text is our reply to these latest comments.

Sebastian Rosier and Hilmar Gudmundsson

Specific comments and questions:

• 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.

We have reworded this paragraph so that both of these points are hopefully much clearer.

I agree with the authors that for a viscoelastic material 'E' becomes a complex dynamic modulus for a periodic forcing with a sufficiently large frequency. But the frequency of the tidal forcing in ice is very low (<1e-4 1/s) and the response of the viscoelastic system will reduce to the one for a static load. Therefore, in my view, Young's modulus should be constant also for studies that model tidal behaviour.

The tidal frequency falls within the range for which both elastic and viscous effects are important which is why a viscoelastic model is essential. As a result, the system does not reduce to that of a static load and the Young's modulus is altered to reflect this. If the tidal frequency was significantly higher then we agree with the reviewer and a 'true' instantaneous value of the Young's modulus as measured by laboratory measurements (i.e. approximately 9GPa) would be appropriate, but in this case the Young's modulus must be altered to account for viscous effects.

• What are the unknowns 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.

Is the time step constant? How did the authors choose it? How long is it?

The time step changes adaptively based on a target number of iterations to reach certain specified convergence criteria. Furthermore, the nonlinear rheology is solved implicitly such that time stepping is much less of an issue than it would normally be.

• 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?

The sentence has been reworded slightly

The authors are totally right to test the role of all mechanisms represented in the model. Nevertheless, I suggest to mention somewhere that not all simulations are designed to faithfully reproduce processes that occur in the real world. For instance, the experiment with n=5 is a (useful) sensitivity study rather than a 'realistic' simulation. Maybe the authors can shortly discuss which of the experiments are more realistic than others.

We have altered the text to make it clear that both experiments described in this paragraph are purely to test the model and not designed to replicate reality. The results from both of these 'unrealistic' experiments are not discussed anywhere else or shown in any figures for this reason. With regards to changing the flow law exponent we do not agree with the reviewer that this is necessarily unrealistic; this value is a major source of uncertainty in ice sheet simulations and its appropriate value remains an open question.

• 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.

But the tidal range of 8m occurs very near the grounding line (Fig.1). I would expect that the migration of the grounding line is mainly influenced by the tidal amplitudes nearby. Is this not the case?

Indeed the tidal amplitude at the grounding line is the important thing when considering grounding line migration. Possibly there is a misunderstanding about what we mean in this sentence: it is not that our parameters are specifically chosen to replicate a tidal motion of 3m.

This sentence is only to give some meaning to the parameters in terms of distance and amplitude which are more easily understood than slopes. The tidal amplitude in these experiments is not fixed to 3m, we use the actual tidal amplitude to force our model which will include the highest amplitudes of 4m near some of the grounding lines. We have re-worded this sentence to hopefully make this clearer.

• 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.

Yes, sure the authors are right that a small effective Young's modulus can model highly fractured ice. But my question to the damage experiment is: Is the FilchnerRonne Ice Shelf in the regions where the authors get high damage factors highly crevassed/damaged? See for example Filchner Ice Shelf where the mean flow velocities are pretty small. In these regions, the resolution of the mesh is additionally very coarse.

Overall our aim with the damage experiment is not to replicate the details of damage across the entire ice shelf but to introduce a plausible damage field that can at least serve to indicate whether or not including damage could explain the discrepancy between our model and observations. As such, many areas with damage do not necessarily correspond to crevassed regions of the ice shelf. In general, however, and particularly where mesh resolution is higher, damage is high in shear margins which is where crevasses would be expected to form on the ice shelf. The Mesh resolution is coarse in regions where mean flow velocities are small because the Msf signal is not generated in these areas. Thus whether or not the damage field is correct in these slow flowing regions should not affect our results.

Technical corrections:

• p.16, l.17: leads to an increase of the Msf amplitude

Done

• p.29, Figure E1: \delta S+ instead of \delta h

Done