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 $M_2$ 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 $M_{sf}$. The authors claim that the relatively high amplitude for $M_{sf}$ 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.
- 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 $M_{sf}$ component is only obtained by grounding line migration?
- 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).
- 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.
- 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?
- 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?
- 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?
- 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?

Specific comments and questions:
- p.2, l.15,16: Give a reference. What is the expectation based on simple models of elastic flexure? Are the numbers in the next line results the authors show later on?
- p.4, l.32: How long are the measurements of the additional GPS sites near the outlet glaciers?
- 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?
- 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?
- 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?
- p.8, l.2-12: The reader cannot see the increase of the $M_2$ amplitude towards the grounding zone. A second figure with a magnification near a chosen grounding line would be helpful.
- 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"?
- 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?
- p.21, l.7: Why did the authors put square brackets around K?
- 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.
- 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.
- 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.
- 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 to 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?

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 $rho_w$ 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.

I enjoyed reading the paper. Best regards
Julia Christmann