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The Cryosphere Discussions

Interactive comment on "Ice shelf flexures modeled with a 2-D elastic flow line model" *by* Y. V. Konovalov

Anonymous Referee #1

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This manuscript presents a numerical model simulating ice-shelf flexural response to effects of ocean waves. Although the model is novel in its formulation, its description and more importantly its results require significant revisions to warrant the manuscript publication. The novelty of the model is extension to include the grounded part of ice. However, practical aspects of that treatment is unclear, because none of the presented plots show what happens upstream of the grounding line.

Major comments

I have numerous issues with the model descriptions and a lot of suspicions about it results. My major concern with the results is that the amplitudes of the calculated shelf deflections are larger than the amplitude of waves forcing these deflections. These results are physically inconsistent due to a vary simple argument: to the zero's order

the results of this model should be similar to results of the shelf treatment as a thin beam (e.g. Vaughan, 1995). Exact solution of the beam equation with periodic forcing (eqn (3) in Vaughan, 1995) shows that the deflection amplitude cannot exceed the amplitude of the forcing amplitude. Of course, the shelf deflection could be amplified due to resonance, however, it is difficult to believe that it was the case in all scenarios considered in this study. Most likely, there is a sign error in the implementation of the boundary condition on the shelf base.

With regards to the model formulation, it is difficult to follow its description due to mixed presentation of the governing equations and numerical implementation. It will be beneficial to present the governing equations in their strong form without referencing to the numerical implementation. If the author finds useful to include a description of numerics, it should be done separately. It seems that there are typos in the right hand sides of all boundary conditions (apart from eqn (5)), they include the right hand side of the momentum equations (1). Also, there is a missing boundary condition at the left, upstream boundary.

From physical aspects of the presented model, it is unclear why the acceleration terms are necessary. For instance, Sergienko (2010) argues that they could disregarded due to large speed of the sound waves in ice. The author indicates that the response frequency is the same as the forcing frequency in section 3.2, however, does not make a connection to the (un)importance of the acceleration terms.

Figure 10 that shows eigen frequencies and corresponding shelf deflections casts doubts on correctness of the model due to unrealistically large deflection amplitudes. Most likely, there are errors in the model and/or bugs in the code.

Minor comments

The title is ambiguous because there is no flow *per se* in this model, it is rather a centerline geometry formulation. In many places the author refers to infragavity waves as high frequency waves. Comparing to tides this true, however, infragravity waves are very lower frequency waves in the **wave** spectrum.

In summary, the manuscript cannot be published in the present form. It requires major revision in both model formulation and implementation.

References

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Vaughan, D. G. (1995), Tidal flexure at ice shelf margins, J. Geophys. Res., 100(B4), 6213-6224.

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