Author's interactive comments

Dears referees, editors.

Thanks for your attentive manuscript analysis and for the comments to the manuscript. Author's interactive comments are below.

Referee#1: "*The citations are incomplete and sometimes appear rather random. Why all these citation on page 6065 page 4 to glaciological work when referring to the sigma coordinates? This is a standard numerical method, and not invented by glaciologists. There is no need for a citation here, and if, then to some standard textbook in numeric. And why are the citations to applications of beam theory to completely different type of a problem (line 14, page 6061)? It seems more logical to refer to work done on the impact on tides on ice shelves. There is no reference to any measurements and no attempt to try to verify the model against observations.*"

Author: I agree with the referee, that the list of references should be reviewed. Some references can be omitted, and more works investigating the impact of tides (ocean waves) on ice shelves, should be referenced to in the manuscript. I believe that measurements in the conditions of the resonant impact is the challenge. Moreover, as the two models comparison shows (see the supplement), the accurate measurements of the ice shelf geometry and the cavity geometry are essential for good validation the full model. However, the models comparison (I mean the full model and the Holdsworth and Glynn model) can be thoroughly carried out and the results can be added to the manuscript.

Referee#1: ***"****I was hoping the paper would answer if high freq. wave induced by the ocean have the potential to set up stresses large enough to cause fracture. The possibility of this is mentioned in the abstract, but this is then not revisited in the conclusions."*

Author: In the supplement I've added the example (Fig. 8A, page 15 in the supplement), which shows that the long-term resonant (nearly resonant) high freq. forcing can cause fracture of the ice-shelf. In this example (ice shelf at a constant thickness is considered) the shear stress reaches the maximum beside the grounding line and in this maximum the threshold value ($10^6\ Pa$) is achieved as the result the high freq. forcing.

Referee#1: *"It is unclear why one needs to use the full set of the momentum equations in this situation. The deflections are clearly small compared to ice thickness and the wavelengths are long. So it would be very puzzling if beam theory could not be used."*

Author: As I understand, the eigenvalues that we obtain in the thin-plate theory/model, are the first terms of the power series expansion for these eigenvalues. Moreover, we don't know theoretical expressions for the eigenvalues in the thin-plate approach, because the mathematical formulation of the problem includes the coupling of different equations (I mean the momentum equations for the plate and the wave equation for the water layer). Thus, we can specify the eigenvalues employing the full model, and in this is one of the purposes of a full model development.

Referee#1: "*I found it difficult to understand the spacing of the spectrum peaks (Fig. 2 and 6). Are these somehow related to the cavity geometry? Or are the independent of the cavity geometry and only function of ice thickness and the elastic parameters?"*

Author: I agree that the results in Fig. 2 look unintelligible, disseminate attention of a reader and due to the their diversity, and they don't give the answers to your questions. The spectrums, which are shown in Fig. 1A, a and in Fig. 1A, b (see the supplement, page 7), more evidently justify that the *eigenvalues depend on the cavity geometry* (the supplement, pages 5-6, Tab.1). Relying on your comments, I am going to add these results to the manuscript. Moreover, the spectrums (Fig. 2 and Fig. 3 in the supplement) contain less resonant peaks (due to smaller lengths of the plates) and thus facilitate visualization of the plots.

Referee#2: "*The description of the model should be presented better. As is, the author presents a straightforward momentum balance (Eq1) and boundary conditions (Eq3), and a result of Holdsworth (Eq 2), but then jumps to an unintelligible mess of 9 lines in Eq 4, which is completely not helpful. If the equations are the same as in Konovalov 2014, none of this discussion of the model needs to be discussed, and it can simply be referenced. (In this case, none of Eq1-4 or the related text are necessary.) If they are not the same, then the author needs a lengthier discussion of how Eq 4 comes about."*

Author: Eq (4) are the three momentum equations that differ from the initial momentum equations (1) so that they include the standard (typical) well-known boundary conditions (3). I've described the procedure of this inclusion in the supplement (see pages 1-2). In Konovalov, (2012, 2014) I considered the first-order numerical approximations in the boundary conditions with intent to minimize the routine that is required to perform these approximations. While in this manuscript I employ the second-order numerical approximations that enhances the routine, but improves the implementation of the model.

Referee#2: *"Since the main conclusion of the work seems to be that the results are qualitatively similar to those of Holdsworth, but quantitatively different, the results of Holdsworth should be*

quantitatively compared in the figures and there should be an explanation of the major differences. For example, I agree with Referee #1 that the spacing of the spectral peaks should be explained, as it should be relatable to the geometry/length, particularly since it is implied there is a perfect analog in the thin-plate Holdsworth case."

Author: I agree that the full model requires confirmation by the experimental data and validation by the comparison with the results obtained by other models. I've performed the quantitative comparison of the full model with the Houldsworth & Glynn model and the results are presented in the supplement (see pages 5-14). I will insert this comparison into the final version of the manuscript.

Referee#2: "*As noted by Referee #1, the implications/impacts of the paper are not clear and must be clarified before the paper should be published. The abstract, results and summary all need to be clearer about how this work adds to the existing body of knowledge, particularly what importance it has beyond the work of Holdsworth. For example, do any of the differences in predictions (e.g., increasing shear stress?) result in different physical implications than what would be concluded from the Holdsworth work?"*

Author: Relying on the referees' comments, the motivation of the manuscript is the following. First, to introduce the method, which provides the stability of the numerical solution in the full model, which implies the coupling of the fundamental momentum equations with the wave equation for the water layer. Second, to compare the results obtained by the two models with intent to reveal the principal distinctions, if they exist, and specifics of the full model.

Referee#1: "*The English is poor and will have to be corrected by a professional copy editor. TC does offer this service, but at a cost. Anyhow, as it stands now someone must go through the manuscript and improve the English."*

Author: I agree to improve English in the manuscript using the help of the professional editors of Copernicus Publisher.

Thanks and all the best,

Yuri V. K.