Author's final comments

Dears referees, editors,

Thanks for careful manuscript revisions and for the comments to the manuscript. I also would like to thank E. Bueler for comprehensive text revision. In general, I agree with your comments and my answers are follows.

1. **E. Bueler:** *"A large fraction of this paper is devoted to continuum formulas for a 3D model, with the stated purpose of implementing a finite difference approximation. The resulting numerical model, about which we know very little, is then barely used, much less effectively exploited. In particular, the actual 3D geometry in the experiments consists only of rectangular box (Experiments A & B) and wedge (Exp. C) geometries for the ice shelf, and material properties are assumed constant. This reviewer is left wondering if an exact solution of the 3D model was attainable by analytical means, which would short-circuit the whole numerical enterprise."*

   The 3D model was developed so that the model allows the numerical experiments with ice tongues bounded an arbitrary continuously differentiable functions $h_b(x,y)$, $h_s(x,y)$, $y_1(x)$, $y_2(x)$. The Holdsworth and Glynn model is considered here in the basic formulation, i.e. for a rectangular ice tongue. Respectively, the comparison was implemented for the rectangular ice tongues. However, experiments with non-rectangular ice tongues were also carried out for validation of the model. For instance, the amplitude spectra in Fig. 1A were obtained for tapering in transverse direction (y-direction) ice tongues. The spectra reveal shifts of the peaks toward smaller periodicities if the average ice width decreases.
   Nevertheless, one of the main aims of the study were decleared as to reveal the principal distinctions, if any, and features of the full model by comparing with the Holdsworth and Glynn model. Given examples haven't revealed sufficient distinctions in the spectra. However, as you supposed, the distinctions grow for the smaller values of the $\gamma = \frac{\sqrt{H\,d_0}}{L}$, and the distinctions are significant already for $\gamma = 0.01$. The experiments reveal not only different eigenvalues, but the distinctions also occur in the number of the resonance peaks. And this difference will appear as for rectangular ice tongues as for non-rectangular ice tongues, depending mainly on the γ.

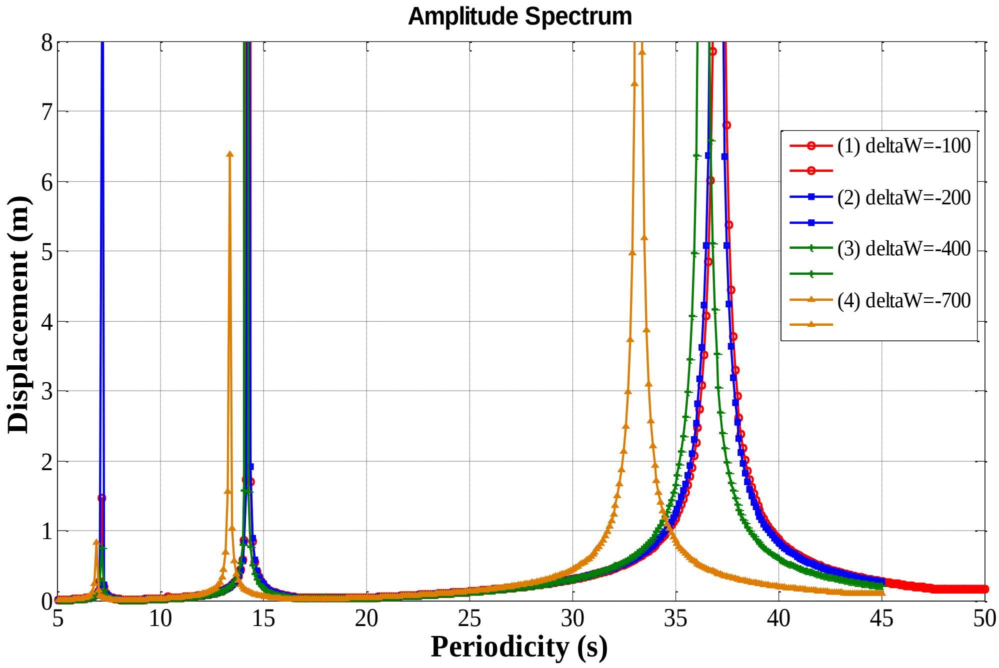

Fig. 1A. Amplitude spectra obtained for linearly tapering in transverse direction (y-direction) ice tongue. **(1)** Ice width linearly decreases from *800m* to *700m*; **(2)** Ice width linearly decreases from *800m* to *600m*; **(3)** Ice width linearly decreases from *800m* to *400m*; **(4)** Ice width linearly decreases from *800m* to *100m*.

2. **E. Bueler:** *"This is not "arbitrary ice shelf geometry", and the assertion to that effect (page 1609) is distressing. Practical ice shelf models, whether for elastic properties or flow or etc., already work with shelves that are neither simply-connected nor specifed by fixed-length logical rectangles as here; compare the various geometries in [1]. The assumed geometry in the paper under review might be acceptable for the limited purposes of this study, but in that case the results should be correspondingly precise and powerful, if this is to be a worthwhile effort. No luck."*

   Probably, the referee implies that the term "arbitrary" means a more general case, when the ice-shelf domain cannot be considered as the domain, which bounded by continuously differentiable functions $h_b(x,y)$, $h_s(x,y)$, $y_1(x)$, $y_2(x)$ (or $h_b(x,y)$, $h_s(x,y)$, $x_1(y)$, $x_2(y)$). So that, we should previously split the domain into a set of simple areas that satisfy to the described conditions (i.e. that the simpler domains are bounded by continuously differentiable functions $h_b(x,y)$, $h_s(x,y)$, $y_1(x)$, $y_2(x)$). From this point of view the word "arbitrary" can lead to ambiguous treatment of the mathematical formulation and should be removed\replaced from the text (I have removed the "arbitrary").

3. **E. Bueler:** *"Only by carefully reading the formulas in Appendix A, and carefully comparing to Holdsworth & Glynn (1978) which is quite clear on this matter, did I finally see that this is not an "ice shelf" paper as asserted in the title. It is an ice tongue paper, though the author never mentions the distinction. That is, the lateral boundary conditions are the same as the terminus conditions, namely "free" in the thin-plate or beam interpretation. Thus the entire enterprise is worthless for the vast majority of ice*

shelves and floating tongues, which have vibrations dominated as much by side buttressing as by their grounding lines. Only true "ice tongues" like Erebus and Drygalski and Mertz Glacier tongues are modelled here."
S. L. Cornford: *"(2) buttressing : the geometries considered are grounded at x = 0, and free edges on the other lateral boundaries. This resembles e.g the Drygalski ice tongue, but a more common configuration is for the ice shelf to have only one free edge."*

The developed 3D model allows to apply different types of the boundary conditions: both stress-free\forced boundaries or fixed\deformed boundaries can be used in the model. Respectively, stress-free boundaries can easily be transformed to the fixed boundaries in the model/program code. Moreover, it seems that the application of the second type of the boundary conditions (stress-free\forced boundaries) is more worthwhile for validation of the model. I agree that the term "tongue" exactly defines the considered geometries with the stress-free lateral edges.

4. **E. Bueler:** *"A comparison between a 3D and a thin plate model is only interesting in the ice shelf context if the dependence of the eigenvalue differences on aspect ratio is included in the analysis. This is finally mentioned in the last paragraph of the Summary (= conclusion) section, where $\gamma = \frac{\sqrt{H\, d_0}}{L}$ is the small parameter. At that latepoint we finally see that all numerical experiments are performed at fixed aspect ratio $\gamma = \frac{1}{20}$. The fact that there is a difference in eigenvalue (= resonance frequency) between 3D and thin-plate models is then not surprising in the slightest. At this value of γ, an ice shelf might as well be an ice cube. Major ice shelves in Antarctica have aspect ratios substantially thinner than this (i.e. they have $\frac{1}{2000} < \gamma < \frac{1}{100}$). This fact is not mentioned but it is highly-relevant to the utility of the very modest results produced in this work. On the other hand, that the numerical results from the 3D model must converge to those of the thin-plate model in the $\gamma \to 0$ limit is also never mentioned, much less exploited for testing the quality of the 3D model."*
S.L. Cornford: *"(1) aspect ratio: The ice geometries considered have rather a high aspect ratio (4 km long, 200 m thick), but real ice shelves tend to be much longer and only a bit thicker."*

In addition to the considered experiments, where the aspect ratio was taken equal to 0.035, I have obtained the spectra for the rectangular ice tongues that have the following geometric parameters: ice thickness is equal to 100 m, sub-ice water depth is equal to 100 m, ice width is equal 300 m, and ice length is equal to 10 km and to 15 km, respectively. The aspect ratio is equal to 0.01 and to about 0.007, respectively. The Fig. 2A shows the spectrum for $\gamma = 0.01$. In the short-period part of the spectrum (Fig. 2A,a) the comparison reveals relatively good agreement between the two considered models. Although, more careful observation reveals that the models provide a different spacing beatween the resonance peaks. However, in the long-period part (Fig. 2A,b) the significant difference is observed. The Holdsworth & Glynn model provides only two resonance peaks in the range 50..300 s at $T_1 \approx 220\ s$ and at $T_2 \approx 82\ s$, while the full model yields four resonance peaks at $T_1 \approx 302\ s;\ T_2 \approx 166\ s;\ T_3 \approx 100\ s;\ T_4 \approx 66\ s$.

The full model requires a high spatial resolution. For instance, Fig. 3A shows the amplitude spectra obtained at different spatial resolutions. Thus, for instance, the aspect ratio 0.001 implies that the high-performance computational system is used for the simulation. I used usual PC with dual Intel Xeon CPU E5-2609. Nevertheless, the aspect ratio 0.01 already reveals the essential difference in the spectra. It seems that an intercomparison project like, for instance, the "*Benchmark experiments for higher-order and full Stokes ice sheet models (ISMIP-HOM)"* will be useful for the eigenvalue problem. Practical experiments with rectangular plates in the laboratory conditions also can be useful as they can provide the benchmark data for the modeling.

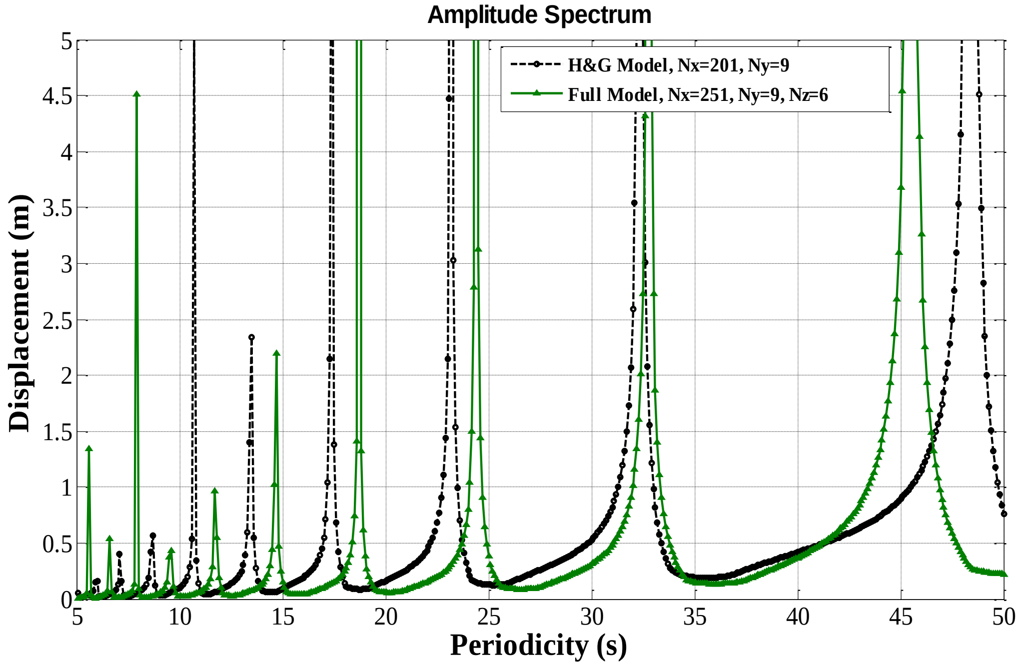

Fig. 2A,a. The amplitude spectra, maximal ice-shelf deflection versus ocean wave periodicity, obtained for ice tongue, which has 10 km ice length, 100 m ice thickness and 300 m ice width. Sub-ice water depth is equal to 100 m. *Aspect ratio* $\gamma = 0.01$.

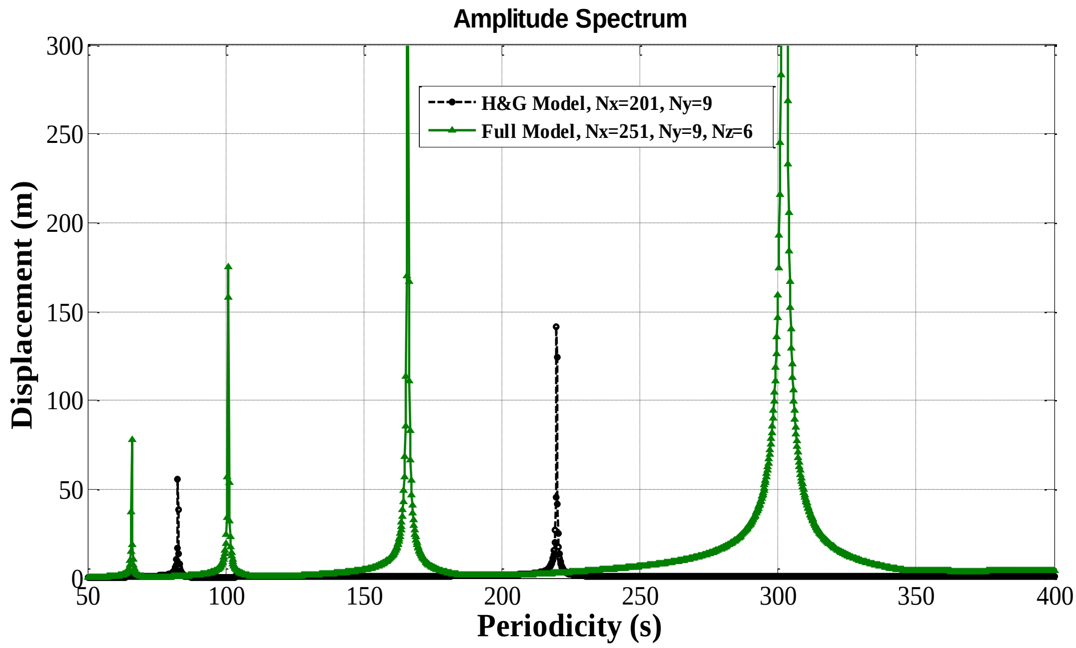

Fig. 2A,b. The amplitude spectra obtained for ice tongue, which has 10 km ice length, 100 m ice thickness and 300 m ice width. Sub-ice water depth is equal to 100 m. *Aspect ratio* $\gamma = 0.01$.

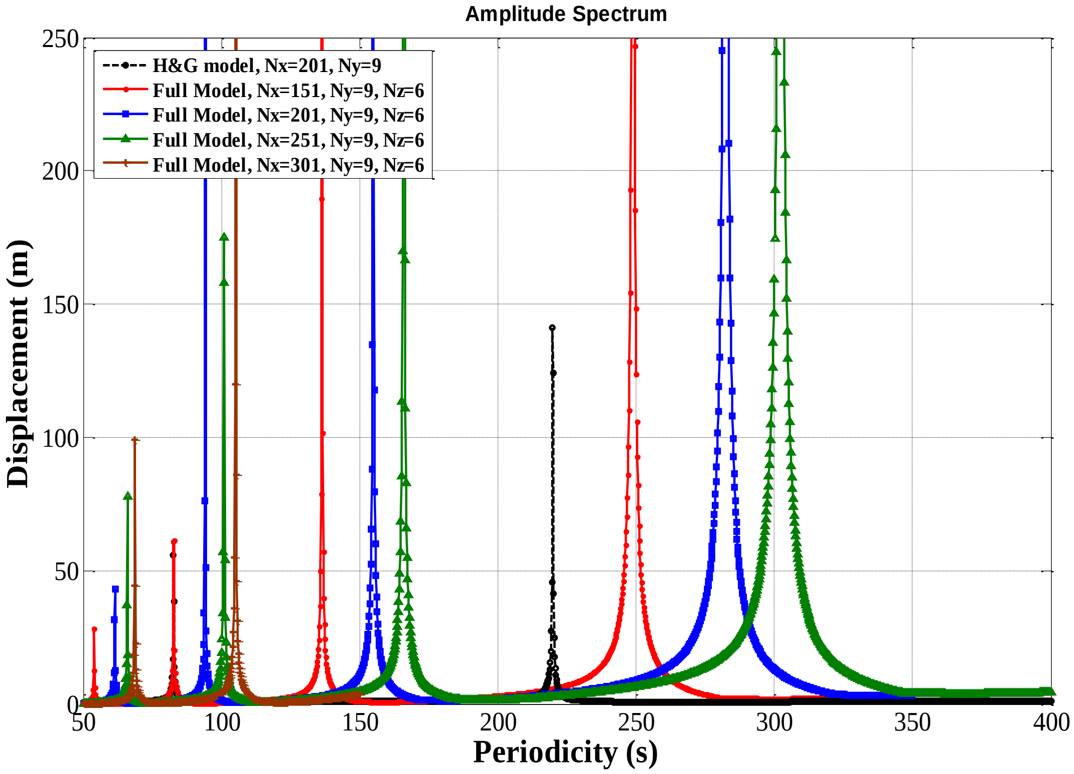

Fig. 3A. The amplitude spectra obtained at different spatial resolutions. Ice tongue has 10 km ice length, 100 m thickness and 300 m width. Sub-ice water depth is equal to 100 m. *Aspect ratio* $\gamma = 0.01$.

5. **E. Bueler:** "*My personal feeling is that results based on numerical models are only publishable if the model code is open source, but, in this case, such purity is probably too much to ask. However, no reproduction of the experiments reported here is possible because no clarity on methods is even attempted. Certainly we have no idea of model resolution, numerical method choices (e.g. for eigenvalue computations), or convergence rate of the numerical results under grid refinement.*"… "*I cannot imagine anyone using these formulas from this source, but they do serve to document the work. They should be put in a supplement or in the documentation associated to an open-source version of the unmentioned code on which this work is based.*"

   I agree with the referee and maintain the idea. I have prepared the supplementary file which contains the equations together with the program code. If the referees agree with the final manuscript, the supplementary file will be published with the final manuscript. Likewise, the program code publication is useful, because it has been written by the one human and, therefore, it can still contain errors. Thus, the open access publication can help to find them, if any.

6. **E. Bueler:** *“page 1605, the title: The title is inaccurate and unnecessarily long. Perhaps: “Ice-tongue vibrations in 3-D and thin-plate models".”*

   I’ve changed the title to “The eigenvalue problem for ice-tongue vibrations in 3-D and thin-plate models”,saving the key word “eigenvalue” in the title.

7. **E. Bueler:** *“The phrase “in shear stress" should be made morevprecise in an efficient way. For example, “in shear stress in planes parallel to the ice shelf base" if that is correct.”*

   I agree with the referee. However, I have decided to save in the manuscript the results that concern only the eigenvalue problem. Considering the last results, the eigenvalue problem, the question about agreement/disagreement of the eigenvalues derived by different models, becomes more important. The stress distribution we can obtain by a full model.

8. **E. Bueler:** “*page 1607, lines 11-12 : The “several" are unneeded. I suggest starting this sentence simply as “Models of ice-shelf bends and vibrations have been proposed by Holdsworth (1977), ... "*”

   Done.

9. **E. Bueler:** “*page 1608, lines 8-16 : This very long run-on sentence should be its own paragraph and should be split into sentences. Thus: “The main objectives of the study were as follows: Firstly, to introduce ... layer. Secondly, to compare ... , if any." (See next comment.)*”
   “*page 1608, lines 15-16 : I don't know what the phrase “and specifcations of the full model" means. It should be deleted or totally rewritten.*”

   Done.

10. **E. Bueler:** “*page 1608, line 21 (last part of equation (1)): The specifcation of the ice shelf domain is not merely the fourth part of a multi-part equation. Instead make a separate statement: “The ice shelf is of length L and flows in the positive x-direction.The geometry of the ice shelf is assumed to be given by lateral boundary functions $y_{1,2}(x)$ and functions for the surface and base elevation, $h_{s,b}(x,y)$. Thus the domain on which equations (1) are solved is $\Omega = \{0 < x < L,\ y_1(x) < y < y_2(x),\ h_b(x,y) < z < h_s(x,y)\}$ "*
    Use of the math-style symbol Ω for a domain is perfectly acceptable here. For instance, later in the paper there is a transformation to a standard rectangular box denoted Π.”

    Done.

11. **E. Bueler:** *"page 1609, lines 1-5 : While "(xyz)" (line 1) is acceptable notation for listing dimensions, it should not be used for function arguments (lines 3-5). And it is not needed: "...density;* h_b*and* h_s *are ... and* y_1 *and* y_2 *are the lateral edges." "*

    Done.

12. **E. Bueler:** *"page 1609, lines 5-7 : This is not "arbitrary ice shelf geometry". This sentence should be deleted."*

    Done.

13. **E. Bueler:** "*page 1609, line 8 : Replace: "non-viscous" → "inviscid" "*.

    Done.

14. **E. Bueler:** *"page 1609, line 9: Replace: "gradually horizontally" → "gradually in the horizontal" "* .

    Done.

15. **E. Bueler:** *"page 1609, lines 14-16: Please do not write e.g. "*W_b*(xyt)" without commas between independent variables."*

    Done.

16. **E. Bueler:** *"page 1609, lines 15-16 : Suggest more clarity and precision: "... and* W_b*(x, y, t) =W(x, y,* h_b*(x, y), t); and P'(x, y, t) is the deviation of the sub-ice water pressure from the hydrostatic value."*

    Done.

17. **E. Bueler:** *"page 1609, lines 17-22 : Given the word "eigenvalue" in the title, and given that the reader may either be inexperienced or unable to read the author's mind, this paragraph needs to be expanded and clarified. First, subsection 2.4 should come first so that the reader knows that equations (1), (2), and (4) are 2nd-order PDEs for the strain components* $u_{ij}(x,y,z,t)$ *and the displacement* $W_b(x,y,t)$*. Then the point is that special time-dependent solutions of the form*

$$u_{ij}(x,y,z,t) = A\, e^{i\omega t} U_{ij}(x,y,z)$$

are considered, and this form of separation-of-variables yields a time-independent eigenvalue problem for the modes Uij , namely something like

$$-\omega^2 U_{ij} = \mathcal{L}\, U_{ij}$$

where L is a linear partial differential operator. All of this is fully-understood by the author, of course. These basic facts are all apparent to any reader who could do the work themselves, but not to a broader readership.
Now, L is described by (1),(2),(4) and various formulas in the Appendices, for example. It is numerically-approximated by finite differences to yield a large square matrix - this should be stated. The eigenvalues of this matrix are then approximated numerically; how this is done, and limitations of size and resolution, should at least be mentioned! As it is, about this approximation, we know nothing because the author reports nothing. The software that does it is apparently not open, and its verification is not addressed."

Revised.

18. **E. Bueler:** "*page 1610, line 5 : Suggest replacing: "where P is pressure* ($P = \rho g H + P'$, *H is ice-shelf thickness)" by "where P is pressure. Note* $P = \rho g H + P'$, *with* $H = h_s - h_b$ *the ice-shelf thickness."*

    Done.

19. **E. Bueler:** *"page 1610, line 6 :Replace "this developed model," → "this model" (sans comma)."*

    Done.

20. **E. Bueler:** *"page 1610, line 19 : Replace "transformation transfigures an arbitrary ice domain into ... parallelepiped" → "transformation maps the ice domain into ... parallelpiped" (correct spelling error)."*

    Done.

21. **E. Bueler:** *"page 1610, lines 22-23 : This paragraph needs rewriting. What is "the method"? What is meant by "initial boundary conditions"? (Note "initial conditions" and"boundary conditions" are standard phrases.)"*

    Revised.

22. **E. Bueler:** "*page 1611, line 13 : The words "eigenvalue", "frequency", "amplitude spectra", "resonance peaks", and "eigenfrequencies" are all used in this paper in undefined and closely-related ways. There is no need for inging buzzwords around here! Better, for the beginning reader especially, to precisely say what number (e.g. ω) is the "eigen-value", what the "amplitude spectrum" is, what a "resonance peak" is, and then stick to a direct, simple vocabulary. My main point is: define your terms instead of randomly choosing them."*

    The term "eigenvalue" means eigen-frequency (ω_n) of the system ice-water or corresponding periodicity ($T_n = \frac{2\pi}{\omega_n}$). That is, the term means the eigenvalues described in the Steklov theorem.

Eigenvalues are denoted by the letters ω_n or T_n with the subscript n (or other), which is integer, because the array of the eigenvalues is a countable set.

Letters ω or T without the subscript denote the current values of frequency or periodicity of the system ice-water and they are defined by the frequency/periodicity of the incident wave (of the forcing). The set of frequencies/periodicities is the continuum.

"Amplitude spectrum" means the dependence of the deflection amplitude (the maximum value across the ice-plate is considered) on the frequency (of the incident wave/forcing). "Resonance peaks" in the spectra imply that the amplitude abruptly increases at a resonance frequency/periodicity.

The eigenvalues correspond to the eigenvalues of the matrix, which results from the discretization of the model. However, it should be noted that not all equations of the full system, which also includes boundary conditions, have the form $\mathcal{L}\,\varsigma = -\omega^2\varsigma$. On the other hand, the matrix eigenvalues satisfy the equation $|A - \lambda I| = 0$, which supposes that the term $\lambda\,\delta_{ij}$ is subtracted form each string of the matrix A. Thus, in general, it seems that not all matrix eigenvalues λ_i correspond to the eigen-frequencies ω_i, so that ω_i can be defined as $\sqrt{-\lambda_i}$. Most likely, the key to explanation of the distinctions in the spectra (see item 4; Fig 2A,b) is in the boundary conditions. In the 3D model a part of the boundary conditions is rewritten in the form $\mathcal{L}\,\varsigma = -\omega^2\varsigma$ while in the Holdsworth & Glynn model the boundary conditions is applied in their usual form.

I have written some comments in the item 2.5 in the revised manuscript.

23. **E. Bueler:** *"page 1611, line 17 : Replace "an impact" → "the impact"."*

    Done.

24. **E. Bueler:** *"page 1612, line 2 : What does "should" mean? Suggest remove it to write: "… the cavity geometry change alters the eigenvalues …" "*

    Done.

25. **E. Bueler:** *"page 1613, lines 18-19 : Simplify: " … for coinciding (corresponding) eigenvalues the deformations in the modes are …" → "… for corresponding eigenvalues the deformations are …"*

    Done.

26. **E. Bueler:** *"page 1614, line 6 : Replace "The ice-shelf … can be performed by the …" → "In this paper, ice-shelf … is performed by a …"."*
    "page 1614, line 7 : The phrase "… although the volume of the routine sufficiently increases in comparison …" is almost unintelligible. Probably: "… although the computational cost of the routine is large in comparison …" "

Revised.

27. **E. Bueler:** *"page 1614, lines 25-27 : Presumably "realistic finite motion" at the resonance peaks can also be recovered by adding more complete viscoelastic effects to the physical model, exactly as is accomplished in a large fraction of the literature. This omission of physics (i.e. omission of energy dissipation) is a big deal. Unmentioned, but big! Its absence damages the whole concept of the work, though the elastic-only modelingmay be acceptable if the 3-D-versus-thin-plate contrast is sufficiently interesting. It deserves more than a self-citation about finite ingoing water suckage."*
S.L. Cornford: *"I think that the manuscript relies a bit too much on readers checking earlier work (by the same author), for example, I was puzzled by reference to resonant modes in a problem with no dissipation, and then noticed a paragraph referring to a 2014 paper in the summary - not the method section, where it is needed - (p1614,lines 24 onward), which talks about modifications to the boundary conditions at the base. A concise explanation within the manuscript would be helpful."*

    I agree that the problem of the "finite motion" can be recovered by adding the viscoelastic effects. Moreover, it seems that in a general case (of a side incident wave) the ingoing water flux cannot be defined explicitly along the whole opened boundary. Thus, viscoelastic model can be more preferable in that case.

28. **S.L. Cornford:** *"Nature of the grounding line boundary: how much does the stress concentration, which from fig 7 is greatest toward the vertical center, depend on the rigid pinning at x=0 and all z? If some deformation was permitted across the GL , how does thischange?"*

    Thanks for the question. I agree that the stress distribution beside the GL depends on the boundary conditions, i.e. whether is the boundary fixed or not? More correctly to consider the system which is composed of ice-shelf/tongue, sub-ice water and glacier. Then, the glacier will diminish the stress concentration beside the GL. And the 3D model allows to consider the three-component system, because the thin-plate approximation is not used in the model.
    I have decided in the revised manuscript to concentrate the attention on the eigenvalue problem. Because there are many questions about the model and the problem. And the stress distribution is a subject of a new manuscript.

Thanks and all the best,

Yuri V. K.