

REVIEW OF
THE EIGENVALUE PROBLEM FOR ICE-SHELF VIBRATIONS:
COMPARISON ... BY Y. V. KONOVALOV

Summary. The abstract and introduction of this paper are promising, and I approached the content optimistically. Unfortunately there is very little reported here that is either surprising or useful. Replacement of the Holdsworth & Glynn (1978) thin-plate model by a 3D model would, in practical ice-shelf simulations, of course impose a large computational burden. Replacement would be worthwhile in some cases, however, if interesting results arose from the 3D model, but I am not convinced from what appears here. Analysis of the 3D results, relative to the thin-plate model, is attempted, but the presentation is technically narrow and conceptually impoverished. Most analytical statements provided here seem obvious, after applying some effort to understand what is said. Comparison of this paper to old literature—e.g. Holdsworth & Glynn (1978), Lingle et al (1981), Reeh et al (2003), all of which exploit simplified models and observations to understand real ice shelves—suggests how little insight is gained with this model and these experiments. As a developer of both 3-D and reduced-dimension models of ice sheets and shelves myself, I would want to see, in a 2015 paper, more demonstrated utility from such numerical models, and also better presentation of their theory and verification.

Major concerns.

- I. 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.
- II. 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 specified 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.

- III. 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.
- IV. 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 $\gamma = \sqrt{d_0 H}/L$ is the small parameter. At that late point we finally see that all numerical experiments are *performed at fixed aspect ratio* $\gamma = 1/20$. 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 $1/2000 < \gamma < 1/100$). 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 $\gamma \rightarrow 0$ limit is also never mentioned, much less exploited for testing the quality of the 3D model.
- V. Little thought has gone into the design of the Figures, apparently. I suggest:
- Given the simplicity of the Figures, they should be in monochrome. (Much of the world still works with B/W printers. On a B/W printer, the blue parts of Figures 1–4 are essentially lost.)
 - As noted below, a new cartoon Figure showing the domain of the computation *would* be useful.
 - Figures 2–4 in effect communicate only a shift in resonance location, so they could be simplified to one or zero Figures. The precise locations of peak resonance, supposing these are important, are already adequately reported in the text.
 - Figures 5 and 6 seem to show y -independent modes only, so there is no reason whatsoever to use color or 3D views. Indeed the 3D views totally obscures the purpose of these Figures which is, putatively, to show the difference between 3D and thin-plate results.
 - Line widths and markers should be made larger.
- VI. 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.

Detailed suggestions/comments/questions.

- *page 1605, the title*: The title is inaccurate and unnecessarily long. Perhaps: “**Ice-tongue vibrations in 3-D and thin-plate models**”.
- *page 1606, abstract line 18*: The phrase “in shear stress” should be made more precise in an efficient way. For example, “in shear stress in planes parallel to the ice shelf base” *if that is correct*.
- *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), ...”
- *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 specifications of the full model” means. It should be deleted or totally rewritten.
- *page 1608, line 21 (last part of equation (1))*: The specification 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 ”.

- *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.”
- *page 1609, lines 5–7*: This is *not* “arbitrary ice shelf geometry”. This sentence should be deleted.
- *page 1609, line 8*: Replace: “non-viscous” \rightarrow “inviscid”.
- *page 1609, line 9*: Replace: “gradually horizontally” \rightarrow “gradually in the horizontal”.
- *page 1609, lines 14–16*: Please do not write e.g. “ $W_b(xyt)$ ” without commas between independent variables.

- *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.”
- *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

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

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

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

where \mathcal{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, \mathcal{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.

- *page 1610, line 5*: Suggest replacing: “where P is pressure ($P = \rho gH + P'$, H is ice-shelf thickness)” by “where P is pressure. Note $P = \rho gH + P'$ with $H = h_s - h_b$ the ice-shelf thickness.”
- *page 1610, line 6*: Replace “this developed model,” → “this model” (sans comma).
- *page 1610, line 19*: Replace “transformation transfigures an arbitrary ice domain into . . . parallelepiped” → “transformation maps the ice domain into . . . parallelepiped” (correct spelling error).
- *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.)
- *page 1611, lines 1–2*: In no sense does forming a finite difference approximation “additionally provide numerical stability to the solution”. This nonsensical statement should be removed and the idea rethought.
- *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 flinging buzzwords around here! Better, for

the beginning reader especially, to precisely say what number (e.g. ω) is the “eigenvalue”, 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.

- *page 1611, line 17*: Replace “an impact” \rightarrow “the impact”.
- *page 1612, line 2*: What does “should” mean? Suggest remove it to write: “...the cavity geometry change alters the eigenvalues ...”
- *page 1613, lines 18–19*: Simplify: “...for coinciding (corresponding) eigenvalues the deformations in the modes are ...” \rightarrow “...for corresponding eigenvalues the deformations are ...”
- *page 1614, lines 2–4*: Stating the sentence “Moreover ... any ice-shelf geometry and for any resonance peak to estimate risks ...” is more than optimism. It is not at all convincing based on the substance of this paper. The idea should be removed.
- *page 1614, line 6*: Replace “The ice-shelf ... can be performed by the ...” \rightarrow “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 ...”
- *page 1614, lines 18–20*: I have no idea what the phrase “...evidently maintain the fact that shear stress should reinforce dislocations in the nodes (of the mode)” might mean! This possibly useful idea should be carefully rewritten.
- *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 modeling may 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.
- *page 1615, lines 8–9*: The first “the assessment” makes sense. The second “the assessment” is not helpful. Is (5) an asymptotic approximation or an exact formula for the thin-plate eigenvalue? Say.
- *page 1615, line 12*: Use equality when defining a variable name: “ $\gamma = \sqrt{\frac{d_0 H}{L^2}}$ ”.
- *page 1618–end, Appendix B*: 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. They are a waste of ESD space.

REFERENCES

- [1] T. ALBRECHT AND A. LEVERMANN, *Fracture-induced softening for large-scale ice dynamics*, The Cryosphere, 8 (2014), pp. 587–605.