Review Comments Regarding the Manuscript

OhBemn (version 0.2): A Simple Boundary Element Method Model for Ocean Surface Waves

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1 Summary

The manuscript details a BEM numerical code that solves the Helmholtz equation in the interior of simply connected domains. Dirichlet, Neumann, and Robin boundary conditions are imposed to model the various types of boundaries. The premise itself is not novel and has been extensively studied in the acoustics community and elsewhere, but its application to ocean surface waves provides sufficient merit for the manuscript to be considered for publication in GMD.

That being said, my assessment is that in its current form, the manuscript does not present the qualities necessary for publication and warrants major revisions.

My main point of criticism is that the manuscript does not properly demonstrate the distinctive characteristics of the presented package with respect to other open-source options that solve similar equations. These need not be substantial differences, but at least some comparison must be made. It is not clear what distinctive role this BEM formulation is intended to play for such problems, or why it is more appealing than, say, Finite Element approaches. The same applies to the implementation. The use of Rust is mentioned without discussing its comparative advantages or whether its use for such problems is common or advisable.

Lastly, the manuscript itself seems to contain disjointed and erroneous statements regarding the mathematics and numerics, and does not properly showcase the code's features. The manuscript should at least adequately describe the code so that the reader/user can utilize it properly.

Below, you will find specific comments regarding the manuscript.

2 General comments

Comment 2.1.

The literature review in §1 is incomplete. It should showcase existing BEM packages, applications of BEM to waves, and, in general, other options for wave modelling.

Comment 2.2.

The portion of the manuscript that details the methodology in §2 is lacking and must be enhanced in order to be considered adequate.

First of all, the equation that is actually solved (Helmholtz) is not cleanly declared in §2, since it is preceded by the Laplace equation, thus confusing the reader (see also Comment 3.2). Furthermore, in §2.1, details concerning 3D boundary conditions are presented without the manuscript indicating how these are incorporated into the model (I suspect that they are not explicitly used in the formulation).

From what I gather, the core of the model is a typical Helmholtz equation with the classic Dirichlet/Neumann/Robin boundary conditions modelling the various types of possible boundaries. Since all these are standard, I think that the manuscript should provide clarity of notation alongside short, descriptive statements of the various options so that the reader can identify the package's capabilities. Thus, I would avoid general, theory-like equations like eq.(15) and gather all the relevant boundary conditions, such as eqs.(16) and (22), in a compactly written section.

Also, in the results section, contours of the solution in the interior of the domain are shown without detailing the equation (either in its discretized or analytic form) used to compute it at those points. I gather that it must be eq.(14), but with the factor 1 instead of $\frac{1}{2}$. If so, it should be stated clearly in the manuscript.

Comment 2.3.

Given that the manuscript details a numerical code, §2.2.1 is far too terse. It should be enhanced with some equations detailing the numerics and at least some details concerning the linear system assembly and solver. Does the assembly entail full all-to-all element calculations? If yes, this results in a steep $\mathcal{O}(N^2_{\text{elements}})$ computational cost that should at least be noted. If no, what kind of technique (e.g. fast multipole or other) is used? What order of Gauss-Legendre quadrature is used? Is the method sensitive to the quadrature's accuracy? Is the resulting linear system well-behaved? What linear solver is implemented in the Python/Rust package?

Furthermore, the manuscript should go into some of the details and features of the implementation. First, which packages are used, for example, for numerical integration? Even if the implementation uses only the standard mix of NumPy/SciPy, those dependencies should be noted in such a technical paper. Also, what are the benefits of using Rust instead of standard C? What acceleration could be used? I believe that since the setup is quite standard (Helmholtz + standard BC), the manuscript could place an emphasis on discussing such aspects.

Comment 2.4.

The results section contains various test cases without clear motivation behind them. Usually, when introducing a numerical scheme/code, one should include a mix of simple benchmark/validation cases and more complex ones to showcase the code's ability to handle relevant scenarios.

There is little to no explanation of why these particular test cases were chosen to validate the method, and, aside from Figure 6, the manuscript lacks any direct quantitative comparison of the method's results against analytical solutions or other models. The revised manuscript should include such validation. This need not be elaborate; simple error curves benchmarked against exact solutions of the Helmholtz equation, which are widely available in the literature, would suffice.

Lastly, the results do not provide discretization details, numerical sensitivities, or sample runtimes. Those should be added so that a reader can assess the code's capabilities.

Comment 2.5.

Most figures have axes without units, and colormaps are also absent from most contours. To improve reproducibility, those should be added.

Comment 2.6.

The conclusion section reads mostly as a summary of the implementation and its immediate use cases. It would be more useful to the community if it drew clearer lessons from the presented examples, discussed the code's limitations in a way that helps readers judge where the method is appropriate, and identified open questions or future research directions that could spark further discussion.

3 Major specific notes

Comment 3.1.

The introduction of BEM in lines 24–39 seems quite vague and misleading.

To start with, Figure 1 does not provide the BC or anything BEM-related. Furthermore, lines 27–31 seem to describe BEM as a method for calculating wave fields. This is not the case, as BEM can be utilized for time-dependent equations and, in principle, any equation that can be transformed into a boundary integral equation, as indicated by its wide use in transient external aerodynamics [Katz and Plotkin, 2001, Moran, 2003].

Also, the statement in lines 30–31 that the solution can be reused is not a feature of BEM, but rather of the frequency-domain approach in general, which can be treated numerically with other methods (Finite Elements, Finite Volumes, Fourier Transform, etc.).

Lines 34–35 contain citations to 3D frequency-domain BEM (Belibassakis et al., 2020; Kurnia and Ducrozet, 2023; Liu, 2008), 2D wave-related BEM Helmholtz solvers (Viswanathan et al., 2021), a FEM solver (Hecht, 2026), and what I gather is a reference to the repository of OpenFAST, a wind farm simulation code (!). Grouping all these together, coupled with the vague description of BEM as a method for calculating waves, induces confusion and does not provide valuable information for placing the showcased method in the broader BEM literature.

By contrast, the succeeding lines 36–39 are left unsupported and without references. Also, the statement they contain is vague, and I do not understand its meaning. What is meant by "discrete domains with constant parameters"? And if there is such a problem, how is it solved by changing the numerical method to Finite Elements? This passage needs considerable revision.

Comment 3.2.

I believe that Section 2 conflates two different things: 3D potential flow and 2D linear wave equation dynamics.

Potential flow relies on the irrotationality assumption $\nabla \times \vec{u} = \vec{0}$ for the fluid velocity that ensures the existence of ϕ such that $\vec{u} = \nabla\phi$. Thus, through the incompressibility condition, we get: $\nabla^2\phi = \nabla \cdot \vec{u} = 0$.

Thus, potential theory results in the Laplace equation and not the Helmholtz equation that is solved herein. This incompatibility can also be observed in the fact that if eq.(1) holds, then $\nabla \cdot \vec{u} = \nabla^2 \phi = \frac{1}{c^2}\frac{\partial^2 \phi}{\partial t^2} \neq 0$, which breaks the incompressibility constraint. This is also illustrated by the inconsistency between eq.(1) and eq.(4).

To obtain the 2D Helmholtz equation from the 3D potential field, one must introduce a dimensional splitting, which is what I believe is done in the manuscript. If, in addition, we work in the frequency domain, one can write:

$$\phi(x, y, z, t) = \mathfrak{Re}\left[\phi_{2d}(x, y)N(z)e^{i\omega t}\right] \tag{1}$$

with $\phi \in \mathbb{C}$ and $N \in \mathbb{R}$. Then, through the Laplace equation, we obtain:

$$\frac{1}{\phi_{2d}}\nabla^2_{2d}\phi_{2d} = -\frac{N''}{N} = -k^2 = \text{const} \tag{2}$$

where $\nabla^2_{2d} \overset{\text{def}}{=} \frac{\partial^2}{\partial x^2} + \frac{\partial^2}{\partial y^2}$ is the horizontal, 2D Laplacian operator. The constant k can be determined by the boundary conditions in z through standard eigenmode arguments. A derivation along these lines would clarify Section 2 considerably.

Comment 3.3.

The notation must be gradually introduced and explicitly defined.

The domain and boundary are denoted by D and S, respectively, in eqs.(11) and (14), but by I, E, and ∂S in Fig. 1 and elsewhere. Also, the notation ∂S usually denotes the topological boundary of S, so a more appropriate notation would be either I, S, E or $D, \partial D, D^c$.

Furthermore, are these domains defined in two-dimensional $x - y$ space? If so, it would be helpful to state it plainly.

Also, the paper does not sufficiently differentiate 2D/3D and real/complex fields (see Comment above). A great example of this is §2.1, where eqs.(6)–(9) imply that the potential is a 3D real-valued field $\phi(x, y, z, t) \in \mathbb{R}$, while eq.(10) implies that it is a 2D complex field $\phi(x, y) \in \mathbb{C}$.

Such instances should be amended throughout the manuscript in order to restore consistency.

Comment 3.4.

In eqs.(26)–(28), the Neumann conditions for plane waves are defined. I don't understand the connection with eqs.(29)–(31), which describe the dispersion relation for 3D fields. Either explain their relation to the BC imposition or remove them.

Comment 3.5.

Lines 249–256 seem redundant and conflict with §2.3. The Neumann BC should not be defined in the Results section and belongs in §2.3, where it is already hinted at in line 118. Also, if the incident wave is defined through a Dirichlet BC, why is the directional derivative mentioned in §2.4?

4 Minor specific notes

Comment 4.1.

Lines 15–23: A citation could be helpful to illustrate some of these numerical wave models.

Comment 4.2.

Lines 30–31: This is incorrectly attributed to the use of BEM. This is true for BEM when the assembled matrix does not change. It is not true for domains with boundaries that change with time or in other cases.

Comment 4.3.

Equation 2: What does the subscript k signify? There should also be a clear definition of $H_0^{(1)}$.

Comment 4.4.

Equation 14: The factor $\frac{1}{2}$ is valid only for smooth regions of the boundary. Also, solutions of elliptic problems in domains with corners yield singularities that are addressed in the mathematics literature. That being said, an investigation of the subject lies beyond the scope of GMD, and just a short statement such as "for boundaries sufficiently smooth" should be sufficient.

Comment 4.5.

Equation 11: The LHS should be $\iint_D \left[\phi\nabla^2\psi - \psi\nabla^2\phi\right] = \ldots$

Comment 4.6.

Figure 1: The figure does not seem to contain pertinent information and merely defines the domain notation, which could be done in text. I propose either removing it or enhancing it with definitions of the normal directions, the p and q points, etc.

Comment 4.7.

Line 111: Collocation does not mean ϕ is constant per segment. It refers to the strong imposition of the Boundary Integral Equation (BIE) at a discrete set of points. This differentiates this approach from the weak enforcement of the BIE in a projection/Galerkin manner (see also [Atkinson and Han, 2005, §12.1]). This statement needs rephrasing to properly address the concept of collocation alongside the collocation points used (usually the midpoints of the boundary segments).

References

[Atkinson and Han, 2005] Atkinson, K. and Han, W. (2005). *Theoretical numerical analysis: a functional analysis framework*. Springer.

[Katz and Plotkin, 2001] Katz, J. and Plotkin, A. (2001). *Low-Speed Aerodynamics: From Wing Theory to Panel Methods*. Cambridge University Press, Cambridge, UK, 2nd edition.

[Moran, 2003] Moran, J. (2003). *An Introduction to Theoretical and Computational Aerodynamics*. Dover Publications, Mineola, NY. Reprint of the 1984 Wiley edition.