Dear reviewers,

Thank you for taking the time to review this manuscript and providing detailed and thoughtful comments. They will make the manuscript better and more understandable, and they have improved our understanding of the method and mathemathical framework.

Below we respond to each comment invidividually. Some of the issues raised overlap between the reviewers, and the response to each may refer to each other.

Response to referee comment 1

1. The review of the linear wave equation and the Helmholz-equation is confusing.

This is a good point. We have re-organized the order of equations, starting with the conservation of momentum and mass equations and stating the assumptions clearly, arriving at the Laplace equation. The Helmholtz equation then follows from depth separation of the Laplace equation, with the wavenumber k determined by the dispersion relation for the given frequency and depth. The confusion of 2D and 3D gradient operator is also resolved: the Laplace equation is 3D, and depth separation gives the 2D Helmholtz equation for the surface wave field. Since this manuscript describes the implementation of a boundary element method, the section has still been kept brief with references to standard texts for detailed derivations.

In this model we solve for a single frequency at the time, within each domain the depth is constant and the wavenumber (magnitude only) is fixed by the dispersion relation $\omega^2 = gk\tanh(kd)$ for that frequency and depth. No shallow-water approximation is made. The Helmholtz equation therefore holds for one wavenumber at the time, with k determined from the full dispersion relation (Holthuijsen 2007). Since we solve for each frequency independently, the phase speed is constant within each domain and there is no refraction due to changing depth. If needed, the wave field is calculated for many frequencies and summed, effectively performing an inverse Fourier transform. The examples here use a plane-wave source (single wavenumber vector), although point sources are also available in the implementation (they are more relevant in acoustics).

2. The left-hand side seems to contain a typo mixing up the ψ and φ.

Thank you very much for pointing this out. It has been fixed.

3. The 1/2 factor assumes a locally smooth boundary.

The $\frac{1}{2}$ factor holds for boundaries that are sufficiently smooth (no corner) at $\mathbf{p}$. In this implementation the collocation point on each segment is placed at its midpoint, so it always lies on a straight, and therefore locally smooth, part of the boundary, and the $\frac{1}{2}$ factor applies exactly at every collocation point. See answer to referee comment 2 below (points 4.4/4.7).

In the revised manuscript, this is now also visible directly from the equation itself (Eq. 17), see point 4 below, which gives the interior, exterior, and boundary cases explicitly, following Kirkup (2019, Eqs. 14 and 30).

4. The steps skipped for eq. 14 (now eq. 17) are useful for readers.

Eq. 17 has been expanded from a single boundary-only expression into the full bracketed definition covering the interior, exterior and boundary cases, i.e.

$$\begin{cases} -\varphi(\mathbf{p}), & \mathbf{p} \in D \\ 0, & \mathbf{p} \in E \\ -\frac{1}{2}\varphi(\mathbf{p}), & \mathbf{p} \in \partial S, \end{cases}$$

for the interior problem (with D and E swapped and the right-hand side sign reversed for the exterior problem. The left-hand side, the integral operators, is unchanged). This shows how, once φ is solved for on the boundary, the same equation gives the solution at any point in the interior (or exterior) by substituting that point for $\mathbf{p}$: solving for the interior sets the exterior term to zero, and vice versa.

5. Does the singularity in the boundary integral need numerical special treatment?

Yes. Each of the four kernel integrals have a special branch for when the collocation point lies on its own element. This occurs at the diagonals in the matrices. They are either calculated or approximated analytically, or they become zero (due to the colocation method with straight segments) and are set directly to zero at the diagonal.

When choosing which points to solve for in the interior or exterior, the point may therefore not be chosen to be exactly at the boundary, since these are not guarded against the singularity. The solution at the boundary points is rather available through the boundary solution.

This is now noted in the revised manuscript (Sect. 2.3.1).

The solver uses a combined (Burton-Miller) formulation to avoid non-uniqueness at characteristic wavenumbers (Burton and Miller 1971; Kirkup 1998, Sect. 5.3.2). It adds the normal derivative of the integral equation, weighted by a coupling parameter (default value $i/(k+1)$, following Kirkup (1998)). This is now described in the revised manuscript (Sect. 2.3.1).

6. (28) Should be the definition of $\nabla\varphi$.

Yes, correct, the gradient is dotted with $\mathbf{n}$ to get the normal component. Manuscript has been updated. Also updated the sign of the derivative to stay consistent with the convention used elsewhere in the manuscript.

7. Some listings showing code in the paper could be omitted.

We have moved the long listings to an appendix, hopefully improving the flow of the manuscript. In our opinion they are short enough that they fit in the appendix, and are useful for illustrating how the boundary condituions are assigned in the model and how it should be run.

8. Section 3.2 is not useful.

True, the content of this has been incorporated into the introduction of Section 3, and Section 3.2 has otherwise been removed.

Response to referee comment 2

> 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.

We have attempted to address this main point by adding a sentence to the introduction stating explicitly what distinguishes this implementation: the combination of a simple, generic BEM formulation for ocean waves with a fast, easy-to-install Python/Rust package, suited for quick, exploratory investigation of reflection, scattering and diffraction in simple domains. A matching closing paragraph has also been added to the conclusion (see the answer to Comment 2.6 below). The specific points on Rust, FEM, and the literature review are answered in the comments below.

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.

The literature review has been improved and clarified. So that the BEM-method and its advantages and disadvantages compared to other models are clearer. A complete overview of the currently relevant wave models is outside the scope of this paper, so examples are given.

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 state- ments 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 1 . If so, it should be stated clearly in the manuscript.

Section 2 has been rewritten and re-organized so that boundary conditions and the development of the linear equation and eventually the Helmholtz equation should be much clearer. We believe the issues in this comment have been solved by restructuring the text and more clearly state in which plane and dimensionailty the boundary conditions and wave equations apply. Still the section is kept compact, since these concepts are not developed in this manuscript, but are useful for understanding the method that is implemented. We refer to standard textbooks for more details. Also see the answer to comment 1 (reviewer 1) above.

On the factor used to compute the interior/exterior contours shown in the Results section: this is no longer implicit, since the boundary integral equation (Eq. 17 in the revised manuscript) is now written with the interior, exterior, and boundary cases given explicitly as a bracketed expression, with factor 1 for a point in the interior D, 0 in the exterior E, and $\frac{1}{2}$ only at the boundary ∂S itself. The contour plots in the Results section evaluate this equation at interior (or exterior) points, so the factor 1 (not $\frac{1}{2}$) applies there. See the answer to Comment 4.4/4.7 below, where the relation between this factor and the collocation method is discussed in full.

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?

The assembly is indeed a full all-to-all element calculation, giving an $\mathcal{O}(N^2_{\text{segments}})$ assembly cost and an $\mathcal{O}(N^3_{\text{segments}})$ direct-solve cost (see below); no fast multipole method or other acceleration technique is used. This is now stated explicitly in the revised manuscript (Sect. 3), along with a note that this would need to be addressed (e.g. with a fast multipole method or an iterative solver with a suitable preconditioner) to scale to much finer discretizations than shown here.

> What order of Gauss-Legendre quadrature is used? Is the method sensitive to the quadrature's accuracy?

The regular (non-singular) element integrals are evaluated with an 8-point Gauss-Legendre quadrature rule, following Kirkup (1998). This is noted in the revised manuscript (Sect. 2.3.1 and Sect. 3). We have not performed a systematic sensitivity study of the quadrature order; 8 points follows directly from the reference implementation in Kirkup (1998) and has been sufficient for all cases shown here.

> 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.

This is entirely implemented in Rust (there is no NumPy/SciPy dependency; NumPy arrays are only used at the Python-Rust boundary, to pass data in and out of the Rust core). The dense linear system is solved directly (LU decomposition) using *ndarray-linalg* (https://github.com/rust-ndarray/ndarray-linalg), which wraps the reference LAPACK routines (Anderson et al. 1992). We have not encountered ill-conditioning in the cases presented here, but have not undertaken a systematic study of the conditioning of the resulting linear system as a function of geometry or discretization. The Hankel functions needed for Green's function (Eq. 22) are evaluated using *complex-bessel-rs* (https://crates.io/crates/complex-bessel-rs), and the Python bindings are generated with *PyO3* (https://github.com/PyO3/pyo3) and *rust-numpy* (https://github.com/PyO3/rust-numpy).

Regarding Rust versus C: Rust gives the numerical performance of C++ or Fortran, while being a higher-level and more expressive language, with bounds- and type-checking enforced at compile time, and with vectorized (SIMD) code expressible through ordinary, safe abstractions rather than compiler intrinsics. The per-segment element calculations (matrix assembly) and the per-point sample evaluations are embarrassingly parallel and are parallelized across CPU cores using *ndarray*'s (https://github.com/rust-ndarray/ndarray) parallel iterators, backed by *rayon* (https://github.com/rayon-rs/rayon). This is now described in the revised manuscript (Sect. 3).

Comment 2.4

We have added a further quantitative validation to the existing breakwater diffraction test case (Sect. 4, "Water-solid structure: Breakwater"): a new comparison against the closed-form Fresnel single-slit diffraction formula, alongside the existing Rayleigh-Sommerfeld/*diffractio* comparison. This shows that misalignment of boundary elements with the opening edges introduces a discretization error, and that a small residual peak-amplitude discrepancy remains even with aligned elements, attributed to the finite computational domain and the approximate radiating boundary condition. See the new text and figure in Sect. 4.

Comment 2.5

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

We have added colorbars and units to all figures.

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.

The conclusion states the method's limitations (no wave growth, no nonlinear interactions, no wave breaking except as parameterized attenuation, constant depth per domain) and positions it relative to phase-resolving models and a practical use case (Norwegian Standard NS 9415). We have added a short closing paragraph on the ease of installing and using the Python/Rust package, and its usefulness as a simple 2D horizontal model for domains with clear, even if complicated, boundaries.

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. [...] 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 [...] By contrast, the succeeding lines 36–39 are left unsupported and without references. [...] 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?

The introduction has been revised. The "solution reuse" statement is now attributed to frequency-domain methods in general, not specifically to ray-tracing or BEM (see also the answer to Comment 4.2 below). We have kept Fig. 1 unchanged, since its purpose is only to establish the interior/exterior/boundary notation used throughout Sect. 2, and the boundary-condition options are introduced later, in Sect. 2.4, once the necessary notation is in place. The citation grouping has been reworked: the introduction now separates standalone 3D panel/BEM codes for wave-structure interaction from multi-physics tools that couple such a method internally (e.g. OpenFAST's HydroDyn module, which does solve a boundary-integral diffraction/radiation problem for floating structures), rather than listing them together with a Finite Element tool (FreeFEM) without distinction. The "discrete domains with constant parameters" statement has also been clarified: it refers to situations where the domain cannot be split into pieces of constant depth (wave speed), so that the BEM boundary-condition matching used here no longer applies; in that case the method is sometimes coupled with, or replaced by, a Finite Element Method, which discretizes the whole domain rather than only its boundary and can therefore represent continuously varying properties (e.g. depth) directly.

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 $\nabla \times \mathbf{u} = 0$ for the fluid velocity that ensures the existence of ϕ such that $\mathbf{u} = \nabla\phi$. Thus, through the incompressibility condition, we get: $\nabla^2\phi = \nabla \cdot \mathbf{u} = 0$. Thus, potential theory results in the Laplace equation and not the Helmholtz equation that is solved herein. [...] To obtain the 2D Helmholtz equation from the 3D potential field, one must introduce a dimensional splitting [...] A derivation along these lines would clarify Section 2 considerably.

Section 2 has been rewritten to follow exactly this structure. We now state the continuity equation and the linearized momentum equation, introduce the velocity potential φ through the irrotationality assumption ($\mathbf{u} = \nabla\varphi$), and substitute into continuity to obtain the three-dimensional Laplace equation $\nabla^2\varphi = 0$, consistent with the reviewer's derivation above. The horizontal, two-dimensional Helmholtz equation is then obtained explicitly through dimensional splitting: writing $\varphi = \hat{\varphi}(x, y)\cosh[k(z+d)]e^{-i\omega t}$ (separating both the depth and time dependence substitution into the 3D Laplace equation gives the 2D Helmholtz equation for the horizontal

envelope $\hat{\varphi}(x, y)$, with k fixed by the dispersion relation (obtained from the bottom and free-surface boundary conditions, by the same eigenvalue argument the reviewer outlines). This removes the previous inconsistency between the (now 3D) Laplace equation and the free-surface/bottom boundary conditions, and makes explicit at which step the equation changes from 3D/Laplace to 2D/Helmholtz. See also the answer to Comment 2.2 and Comment 1 (reviewer 1).

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 paper does not sufficiently differentiate 2D/3D and real/complex fields.

Notation has been made consistent throughout the revised manuscript: the interior is denoted D, the exterior E, and the boundary S (or ∂S), matching Fig. 1, and these are used consistently from their first introduction through the boundary integral equation and the results. The 2D/3D and real/complex distinction raised here is resolved by the same change described in the answer to Comment 3.2 above: the momentum and continuity equations and the Laplace equation are now explicitly 3D and real-valued ($\varphi(x, y, z, t) \in \mathbb{R}$), and the complex-valued, 2D $\hat{\varphi}(x, y) \in \mathbb{C}$ envelope only appears once the depth-and-time separation into the Helmholtz equation has been introduced, so the two are no longer used interchangeably before that point.

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.

This has been resolved by the reorganization of Section 2 described above: the dispersion relation is now derived once, in Sect. 2.1, directly from the free-surface and bottom boundary conditions, and is not repeated near the plane-wave source and its Neumann (normal-derivative) boundary condition, which is now presented separately in Sect. 2.5. The two no longer appear adjacent to one another, removing the confusion about their relation.

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?

The general Dirichlet/Neumann/Robin boundary condition framework is now defined once, compactly, in Sect. 2.4 (see also answer to Comment 2.2). The statement $\partial\varphi/\partial n = 0$ that appears in the Results section (Water-solid structure: Breakwater) is not a re-derivation of this general framework, but a case-specific instantiation of it, needed there to describe which boundary condition is used for that particular example (a rigid wall). We believe this is not redundant in the same sense as before, since the general theory and the specific example are now clearly separated. On the directional derivative in the (now) plane-wave source section: this is needed regardless of whether the incident field itself is imposed through a Dirichlet or a Neumann condition, since a Neumann (or Robin) condition elsewhere on the same boundary requires the normal derivative of the incident field to be evaluated there as well.

Comment 4.1

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

Citations to WAVEWATCH III and SWAN have been added as examples of the operational numerical wave models referred to in this passage.

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.

Agreed, and this has been corrected (see also answer to Comment 3.1 above). The sentence now attributes the ability to reuse a computation across different input directions and periods to frequency-domain methods in general, rather than singling out ray tracing and BEM.

Comment 4.3

> Equation 2: What does the subscript k signify? There should also be a clear definition of H0^(1).

The subscript k on Green's function G_k denotes that it is defined for a single wavenumber (equivalently, a single frequency, through the dispersion relation) at a time, consistent with the single-wavenumber formulation used throughout the manuscript. This is now stated explicitly where G_k is introduced. $H_0^{(1)}$ is now also explicitly identified as the Hankel function of the first kind, of order zero.

Comment 4.5

> Equation 11: The LHS should be $\iiint_D (\varphi\nabla^2\psi - \psi\nabla^2\varphi) = \dots$

This is the same typo raised by Reviewer 1 (Comment 2); see our answer there. It has been fixed in the revised manuscript.

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.

We have kept Figure 1 as is, showing only the continuous and discretized domain and the $D/E/\partial S$ notation. The normal direction and the points $\mathbf{p}$ and $\mathbf{q}$ are introduced directly in the text, alongside the equations where they are first used (Sect. 2.2), which we think keeps the figure simple while still defining all the notation needed.

4.4/4.7 The $\frac{1}{2}$ factor and the meaning of collocation.

These two comments are related and are addressed together.

On the $\frac{1}{2}$ factor (Eq. 14, Comment 4.4): as noted in the response to referee comment 3 above, the manuscript states the general condition under which the $\frac{1}{2}$ factor holds, independently of any particular discretization, namely that the boundary is sufficiently smooth (has no corner) at $\mathbf{p}$. The manuscript now notes, as a separate, discretization-specific point, that since the collocation point on each segment is placed at its midpoint, it is always on a straight, and therefore locally smooth, part of the boundary, so the general condition is satisfied exactly at every collocation point in this implementation.

On collocation (Sect. 2.3.1, Comment 4.7): we agree the previous wording conflated two separate things, the choice of basis function (the wave field taken constant along each segment) and the choice of how the boundary integral equation is enforced (collocation). Collocation refers to imposing the boundary integral equation strongly, at a discrete set of points (here, the segment midpoints), as opposed to enforcing it weakly in a by using e.g. the Galerkin method. The manuscript has been reworded to state this explicitly and to keep the two choices distinct.

Sincerely,
Dr. Gaute Hope

References

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