[Note: Referees' and editor's comments are in italic, my replies are in Roman, and texts taken from the manuscript are "quoted". In "Author's response" and "Author's changes in manuscript", figure, equation, and line numbers generally correspond to those in the marked revised manuscript.]

-------------------- Additional author's notes to the handling editor --------------------

Re: Revision of the manuscript "Process-based modelling of nonharmonic internal tides using adjoint, statistical, and stochastic approaches. Part II: adjoint frequency response analysis, stochastic models, and synthesis" (egusphere-2024-4193)

Dear Dr. Julian Mak,

Thank you for your comments and handling the manuscript. I have revised the manuscript carefully following your and referees' comments. Please see my point-by-point replies to individual comments listed below.

I am looking forward to hearing the outcome of this peer-review process.

Kind regards,
Kenji Shimizu

-------------------- Author's response to Editor comments --------------------

<Editor comments>

Line 100: there feels like a word missing in "which is used in various parts of this paper including [WORD] hydrodynamic modelling"? I might suggest removing everything after "including" (then the missing word comment becomes redundant).

<Author's response>

Thank you for your comment. I removed words after "including".

<Author's changes in manuscript>

l.97: "Appendix B provides the description of internal-tide dynamics in terms of vertical-mode amplitudes, which is used in various parts of this paper ~~including hydrodynamic modelling~~."

<Editor comments>

Fig 2 and elsewhere: I am not quite sure how to make of the "complex-valued amplitude". It wants to be a magnitude (?) but the complex plane C is not an ordered field (while the real line R as a field is ordered). A clarification here would be useful (or don't use "amplitude"?)

<Author's response>

Thank you for your comment. I think the use of complex-valued amplitude is common in studies that deal with waves or oscillations of exp(i*phase) form. It can be viewed as one of the coefficients of a complex Fourier series. I added a clarification as follows.

<Author's changes in manuscript>

- Caption of Fig. 2: "$x_j + \mathrm{i}\, y_j$ is (total) complex-valued amplitude (i.e., its magnitude represents wave amplitude and its angle represents wave phase as the coefficients of complex Fourier series), ..."
- l.130: "Since harmonic analysis determines harmonic amplitudes and phase lags using the method of least squares, the complex-valued amplitudes (i.e., their magnitudes represent wave amplitudes and their angles represent wave phases as the coefficients of a complex Fourier series) are further decomposed into the expected values and deviations from them:"

<Editor comments>

Eq (12) and line 221: "Normally" (!?) Hermitian transpose are denoted with daggers? (This may be a physics convention.) Present notation is ok I guess.

<Author's response>

I think it is a matter of preference, because both superscript *H* and dagger are commonly used for conjugate transpose. For example, some common resources for the *H* notation includes Abramowitz and Stegun (1972) and its online version (https://dlmf.nist.gov/), and Wolfram MathWorld (https://mathworld.wolfram.com/). Although the *H* notation may be more common in mathematics, I personally prefer the *H* notation because the use of the superscript + is common in fluid mechanics (but not in physical oceanography), and it appears rather similar to dagger (especially in non-Serif fonts). I kept the *H* notation.

<Author's changes in manuscript>
No change was made to the manuscript based on this comment.

<Editor comments>
Line 254: the "adjoint" is not the "inverse" necessarily (or vice-versa), and it's really a "dual". Would suggest "The problem dual to this...", "A related / converse problem to this..." or similar.

<Author's response>
Thank you for your comment. I think the problem was "The" in front of "problem", which was my grammatical error. As stressed in Preface of Wunsch's textbook (1996), there is no unique "inverse problem" to a particular forward problem, and I use "inverse" in this sense elsewhere in the manuscript. I revised the text following your suggestion.

<Author's changes in manuscript>
l.219: "~~The~~ A problem ~~inverse~~ converse to this yields ..."

<Editor comments>
Eq (16): so I am with one of the referees in that (16) is not an SDE, since "equation" would need some equals sign to "equate" things (not sure if that's what the referee had in mind). As written this could be a suggestive relation that could be made into an SDE (maybe). Please tighten this by either writing out the proportionality constants or missing terms, or change the text to "relation (16) implies an SDE" or similar (for clarity the former is more desirable).

<Author's response>
I am sorry that the meaning of ~ symbol was unclear. It means "asymptotically equal to" in the perturbation theory (e.g., Van Dyke 1975, p.21), but I realized I should not use it in an oceanography journal. As my reply to the referee's comment, I have changed ~ to = in the revised manuscript.

<Author's changes in manuscript>
Eq. (16): The symbol ~ was replaced by =.

<Editor comments>
Line 455: "It" here refers to what?

<Author's response>

I am sorry that it was unclear. The text was revised as follows.

<Author's changes in manuscript>

l.351: "~~It~~ The frequency response function was calculated for complex-valued VM1 isopycnal-displacement amplitude ..."

<Editor comments>

Line 570: need some units on the bottom drag coefficient value

<Author's response>

A quadratic drag coefficient is dimensionless, because (bottom stress) = ρ * (drag coefficient) * (u^2+v^2). Please let me know if I need to specify that it is dimensionless.

<Author's changes in manuscript>

No change was made to the manuscript based on this comment.

<Editor comments>

Methodology (e.g. sec 4.2): some others disagree with me on this but I think most of this should be in present tense. The reasoning being that the model details (and/or results) are still true, although the choices and creation were made in the past. Past tense implies to me "the model details used to be true", which is not the intention presumably.

<Author's response>

Thank you for your comments. I revised Methods section (especially the model descriptions) to give preference to present tense, although I kept past tense when I stress what I did and what I chose.

<Author's changes in manuscript>

I revised Methods section (especially the model descriptions) to give preference to present tense.

<Editor comments>

Line 534 + line 600: "arbitrarily" is not overly scientific and also presumably not true (because there should have been a reason for choosing it, if only e.g. to give reasonable results). Consider "empirically" or similar, and possibly e.g. "empirically based on WHAT" or similar (WHAT = resulting solutions?)

<Author's response>

Thank you for your comment. I revised the relevant sentences as follows.

<Author's changes in manuscript>

- l.400: "We ~~arbitrarily~~ chose the middle of these likely upper and lower limits, α_C=0.7, as a reference value."
- l.415: "We ~~arbitrarily~~ chose the middle-ground value of α_L=2 as a reference value."
- l.454: "We ~~arbitrarily~~ chose the middle of these likely upper and lower limits, α_r=3, as a reference value."

<Editor comments>
Table 1 caption: consider reminding the reader what the LOC, NWS etc. acronyms are here to avoid a back-and-forth jump for the reader.

<Author's response>
Thank you for your comment. I revised the caption as follows.

<Author's changes in manuscript>
Caption of Table 1: "~~Regions~~ Abbreviations for the regions are LOC: local region near PIL200 location shallower than 1500 m, NWS: Australian North West Shelf region excluding LOC region, LAS: region around Lombok and Alas straits, SS: region around Sape Strait, and IND: the rest of the model domain, mostly deep Indian Ocean. These regions are shown in Fig. 10."

<Editor comments>
Line 1155 (in appendix D): is "0" (zero) rather than "o" (oh; not sure what this is) meant? If former, please use 0, otherwise clarify.

<Author's response>
Thank you for your comment. The o was replaced by 0.

<Author's changes in manuscript>
l.941: "This may appear inconsistent with Eq. (D4), but can be obtained by considering the Fourier integral of Eq. (D4a), and applying integration by parts to the left-hand-side and the "initial" condition Eq. (D4b), assuming $\boldsymbol{\lambda}$= ~~o~~ 0 for $t>t_j$."

-------------------- Author's response to RC1 --------------------

<Referee comments>

Continue the equation in line 332 and (27).

In the equation in line 332, is a monochromatic wave assumed? According to the dispersion relation of inertia-gravity wave, a displacement in location $d\xi$ is linked to a modification in wavenumber (or wavenumber is a function of location).

(27) is obtained perturbatively from the equation in line 332. (1) I do not know what the ~ symbol means, and why not = is used? (2) If I understand ~ as =, I cannot see why one equation, the equation in line 332, leads to two equations.

<Author's response>

Thank you for clarifying your earlier comment. Yes, a monochromatic wave in frequency (constant ω) is assumed (but the wavenumber k can change with phase speed). I believe wave dispersion does not appear in Eq. (27) (which is Eq. (16) in the revised manuscript) even for dispersive waves, because it is the phase relationship d(phase) = $\omega\, dt$ - $k\, d\xi$, but written using the phase speed $c = \omega/k$. The relationship between k and ξ along a ray path is obtained through the dispersion relationship, i.e. $d\omega/dt = (d\omega/\mathrm{d}k)(dk/dt) + (d\omega/d\xi)(d\xi/dt)$, after the introduction of the phase relationship. Since wavenumber changes along the propagation path, Eq. (27) is written in terms of angular frequency, which remains constant at least under the slowly varying assumption (Lighthill, 1978, chap 4.5).

I am sorry that the meaning of ~ symbol was unclear. It means "asymptotically equal to" in the perturbation theory (e.g., Van Dyke 1975, p.21), but I realized I should not use it in an oceanography journal. I have changed ~ to =.

Regarding the two equations, only one equation is presented in the revised manuscript. But my answer to your question is as follows. Using the phase speed $c = \omega/k$ and assuming $c = \bar{c} + c'$ and $|c'| << c$, the deviation of total wave phase is

d(total phase) = $\omega\,(dt - (1 - c'\bar{c}^{-1})\,\bar{c}^{-1}d\xi) - d(\varphi + \theta)$.

Since c' and θ are stochastic variables with zero mean and φ is assumed to be constant, the mean phase is constant for $d\xi = \bar{c}dt$. Then, following the constant mean phase, the deviation of (total phase) due to c' or θ is given by $c'\bar{c}^{-2}\omega\, d\xi = c'\bar{c}^{-1}\omega\, dt$ and $-d\theta$, respectively. Since θ is assumed to be equivalent to random phase deviation caused by phase speed deviation c' in this study, this yields two equations in Eq. (27) in the original manuscript. I added some explanation about this derivation before Eq. (16) in the revised manuscript.

(During the revision, I noticed that the signs of Eq. (16) and Eq. (A13b) were opposite, although they should be the same. The sign of Eq. (16) was changed based on the above derivation. This changed the signs in the covariance equations, Eqs. (18), (20), and (E3a), but the results used later, $P_{\theta\theta}$ and $P_{\Delta\theta\Delta\theta}$, remain the same. So, it does not affect any results, conclusions, and figures presented in the manuscript.)

<Author's changes in manuscript>

- l.250: "To develop stochastic models of phase statistics, we consider waves with a constant frequency that arrive at an observation location after travelling through regions of random phase-speed variability. Following Zaron and Egbert (2014) and the analysis in Appendix A, the random phase deviation along ~~a~~ the wave propagation path between a source located at $\vec{x}_j$ and the observation location (say, j^{th} path) can be calculated considering the variation of the total wave phase ~~$(\varphi_j + \theta_j)$~~ $d(\text{phase}) = \omega\,(dt - c_j^{-1}\, d\xi_j) - d(\varphi_j + \theta_j)$ and that of the phase speed c_j ~~in the phase relationship $d(\varphi_j + \theta_j) = \omega\,(dt - c_j^{-1}\, d\xi_j)$~~, where ξ_j is the coordinate along the path."
- l.258: "Assuming $|c_j'| << \bar{c}_j$~~, the relationship between θ_j and c_j' from the phase relationship is~~ and following the constant mean phase ($d\xi_j = \bar{c}_j\, dt$), the deviation of total phase due to c_j' or θ_j is given by $c'\bar{c}^{-2}\omega\, d\xi$ or $-d\theta$, respectively. This yields (see Appendix A for alternative derivation)"
- Eq. (16): The symbol ~ was replaced by =.
- Eqs. (16), (18), (20), and (E3a): The signs of the some of the terms were wrong and corrected.

-------------------- Author's response to RC2 --------------------

Review of revised manuscript, egusphere-2024-4193, "Process-based modelling of nonharmonic internal tides using adjoint, statistical, and stochastic approaches. Part II: adjoint frequency response analysis, stochastic models, and synthesis", by Kenji Shimizu.

---------- General Comments ----------

<Referee comments>

The author has thoroughly revised the paper in response to my previous comments and those of another reviewer. I only read the first 20 pages of the 34-page response to the reviewers, but I can attest to the thoughtfulness and care put into the revisions. My context window cannot encompass the scale of the reply, so I have proceeded to re-read the manuscript afresh.

My reaction to this paper remains very positive; although, I acknowledge that it is long and difficult and will be of interest to a limited readership. It addresses the difficult question of explaining the properties of the nonharmonic tides observed at a fixed location. It identifies and then describes the salient oceanic properties: the distribution of the sources, the randomness of the propagation medium, and the topographic coupling/conversion among vertical modes. The ideas are developed in the context of an idealized linear model for internal wave propagation in a particular region of the globe, and it is acknowledged that it is, in fact, a preliminary exercise or feasibility study for more extensive analyses.

I highly recommend publication of this article. In spite of its length and challenges of the material, the work is salient to a wide range of other work focused on internal waves and tides. I think the paper provides many nuggets which will need follow-up in subsequent publications in order to extend it to more regions and situations. For example, one could imagine dedicated papers concerned solely with modeling the covariance of the propagation speed, or case studies with different configurations of source regions, or more extensive intercomparisons of observational datasets/regions. This is original and innovative work and I believe the Ocean Science journal is an appropriate venue for disseminating it.

<Author's response>

Thank you for your very positive comments. I admit that the manuscript is longer and uses more tools than I would like in one article. However, I thought it would be important to show how different components can be combined to obtain useful results before investigating the details of individual components, which have rooms for improvement in the future. Also, my work situation unfortunately did not allow me to split this manuscript into more parts.

<Author's changes in manuscript>

No change was made to the manuscript based on this comment.

---------- Detailed Comments ----------

<Referee comments>

l6: I don't understand the usage of "provides" in this sentence. Does the adjoint sensitivity and Fourier theory "provides distributed determiniatic sources" in the sense that there are other, more general, kinds of sources which are approximately represented by "distributed deterministic" ones? Or, does the adjoint sensitivity and Fourier theory "provide" the sources by identifying them (which would otherwise be unknown)?

<Author's response>

I am sorry that it was unclear. I tried to reduce the number of words, but it made the sentence ambiguous. I revised the sentence to mention what was done, as follows.

<Author's changes in manuscript>

l.6: "A combination of adoint sensitivity modelling and the frequency response analysis from Fourier theory ~~provides~~ is used to calculate distributed deterministic sources of internal tides observed at a fixed location, which enables assignment of different degrees of randomness to waves from different sources."

<Referee comments>

l55: I wonder if "What determines the variance?" should be quoted. Consider rephrasing this sentence.

<Author's response>

Thank you for your comment. I revised the sentence as you suggested.

<Author's changes in manuscript>

l.54: "~~When nonharmonic internal tides are~~ Because the (co)variance controls the PDFs (and the associated higher-order statistics) in the "many source" limit, this suggests that one of the most important questions ~~is what~~ is: 'What determines the ~~variance, which controls the PDFs (and the associated higher-order statistics).~~ variance?'"

<Referee comments>

l109: The paper has taken us through a good overview and survey. I think I understand what is coming, and can mentally organize it better than in the original version of the manuscript. Based on the reply to reviewers, I understand the author's rationale for presenting this material in a single paper, but it is still a lot to digest.

<Author's response>

Thank you for your understanding.

<Author's changes in manuscript>

No change was made to the manuscript based on this comment.

<Referee comments>

Fig2: When I learned math, the usual convention was to measure positive angles with counter-clockwise rotation, but this diagram appears to do the opposite. If it is trivial to rework the graphics so that positive φ_j is shown counterclockwise, aiming r_j into the 1st (upper-right) quadrant, I think it would be good.

<Author's response>

Thank you for your comment. The reason why the angle is clockwise is that harmonic analysis conventionally uses phase lag (instead of phase lead), and I do not wish to challenge the convention. Considering that there are also other examples in which mathematical and practical conventions are opposite (e.g., wind and current directions), I left the figure as it was but added text why the angle is positive clockwise.

<Author's changes in manuscript>

Caption of Fig.2: "x_j+i y_j is (total) complex-valued amplitude (i.e., its magnitude represents wave amplitude and its angle represents wave phase as the coefficients of complex Fourier series), ~~and~~ x_j'+i y_j' is that with zero mean~~.~~, and angles are positive clockwise because harmonic analysis conventionally uses phase lags."

<Referee comments>

l134: Typo? Should "φ'" be just "φ" here?

<Author's response>

Thank you for pointing out. It is typo.

<Author's changes in manuscript>

l.137 and 173: φ' was replaced by φ.

<Referee comments>

l204-l207: Can this be omitted? I am unclear on exactly what "process-based modelling of nonharmonic internal tides" means. When we are "modelling" something, I expect to see some equations with dependent variables on the left-hand-side of an equation, and the model inputs (pre-modulation amplitudes, phase variance, and horizontal phase difference) sitting on the right-hand-side. Although the equations describing the situation may have this form, it is not obvious.

<Author's response>

Thank you for your comment. I deleted the sentences.

<Author's changes in manuscript>

l.204-207 in the original manuscript: "~~The above statistical model suggests that process-based modelling of nonharmonic internal tides is possible if process-based models of the deterministic internal-tide sources (or~~

~~pre-modulation amplitudes) $s_j = a_j e^{-i\varphi_j}$, the phase variance σ_j^2, and the variance of horizontal phase difference $E(\Delta\Theta^2)$ are available. These three models are developed in turn in the following sections.~~"

<Referee comments>
*l219: add "the" before "*Fourier"

<Author's response>
Revised as suggested.

<Author's changes in manuscript>
l.222: "These methods can be extended to sinusoidal internal tides using the Fourier transform."

<Referee comments>
l248: Since you previously used j to index spatial location, I think you should use a different variable to index the path. Although, later, do you use just the single mean path for each source? If so, then I guess it would make sense to use the same index for both.

<Author's response>
I am sorry that my intention was unclear. I considered the path between a source (s_j) located at x_j and the observation location. This seems to be the most straightforward way to consider a propagation path at that stage of the manuscript, although I use a different method later to reduce the computational cost (finding the path between each grid point and the observation location requires path search). Rather than changing indicies, I revised the text as follows. Also, I changed indices in Fig. 4 to be consistent with the text.

<Author's changes in manuscript>
- l.251: " Following Zaron and Egbert (2014) and the analysis in Appendix A, the random phase deviation along ~~a~~ the wave propagation path between a source located at $\vec{x}_j$ and the observation location (say, j^{th} path) can be calculated considering the variation of the wave phase ..."
- Fig. 4: x_1, x_2, ξ_1, and ξ_2 were replaced by x_i, x_j, ξ_i, and ξ_j.

<Referee comments>
l257: "as" -> "is"

<Author's response>
Revised as suggested.

<Author's changes in manuscript>
l.263: "Stochastic differential equations are commonly forced by white Gaussian noise, but it is undesirable to assume c_j' ~~as~~ is white noise because c_j' certainly has spatial correlation."

<Referee comments>
up to l489: There have been several assumptions introduced along the way about the covariance structures. Although it is a lot of detail, I suppose it is all needed if anyone else attempts to build on this work.

<Author's response>
I tried to remove nonessential details, and I think what are left are necessary to reproduce the results and build upon this manuscript.

<Author's changes in manuscript>
No change was made to the manuscript based on this comment.

<Referee comments>
l537: Thus, 300e-7 m^2 variance is equivalent to 0.3 cm^2 SSH variance. Assuming these results are typical, it seems to show that much of the nonharmonic SSH variance is due to a few strong distance sources, rather than an isotropic sea of random waves. This lends some hope to the idea of predicting part of the nonharmonic signal by explicitly modeling the eddies.

<Author's response>
Thank you for your comment. I speculate that there are regions affected by a few to many significant sources over the world ocean, although this requires further investigation in the future. But I agree with you in the following points: (i) the results suggest that better representation of mesoscale variability provides at least some skill in predicting nonharmonic internal tides, and (ii) the methodology proposed in this study can be useful to assess where such an improvement is expected.

I would also point out one detail that the number of significant sources appears a few in Fig. 10, but there are at least more than 30 (equivalently independent) sources statistically (which was included in the original manuscript but deleted during the revision). From a theoretical point of view, this is important because it allows us to assume the "many source" limit.

Based on your comment, I added the following sentence in Discussion.

<Author's changes in manuscript>
l.679: "For investigating the predictability of nonharmonic internal tides, the locations and quantitative contributions of internal-tide sources, such as Fig. 10, would provide useful baseline information."

<Referee comments>
l710: Why so many references to Colosi, 2016, for wave propagation in random media? What other texts have you found useful?

<Author's response>

Thank you for your comment. I cited Colosi (2016) because the textbook summarizes different ideas and theoretical tools used in wave propagation in random media, and because it is more relevant to this study due to the fact that underwater acoustics almost always deal with curved rays (compared to optics or communications). I added references to Ishimaru (1997) as a general textbook of wave propagation in random media, and Born and Wolf (2019) as a textbook of optics.

Also, I noticed that the related sentence in Introduction was inconsistent with the potential application of ideas and methods from wave propagation in random media, mentioned in Discussion. The sentence in Introduction was revised.

<Author's changes in manuscript>

- l.70: "In addition, although comprehensive literature survey is difficult, the ~~methodologies developed~~ results for wave propagation in random media in other fields of physics and engineering do not appear to be directly applicable to distributed sources, because they usually consider a signal from a small number of ~~point~~ sources (e.g., ~~Colosi, 2016, for underwater acoustics~~ Ishimaru, 1997; Colosi, 2016, Born and Wolf, 2019)."
- l.644: “However, studies on wave propagation in random media in other fields~~, such as acoustics,~~ (e.g., Ishimaru, 1997; Colosi, 2016) suggest that ray tracing may have wider applicability than it seems."
- l.720: "For example, turbulence and short stochastic internal waves (approximately represented by the well-known Garrett-Munk spectrum) are nonlinear, but ~~an acoustic~~ a signal modulated by these processes can be modelled well by linear methods (e.g., Ishimaru, 1997; Colosi, 2016)."

<Referee comments>

l772: good

<Author's response>

Thank you.

<Author's changes in manuscript>

No change was made to the manuscript based on this comment.

<Referee comments>

l781: "does not appear to be available" is cryptic; could you say it is "small"?

<Author's response>

Thank you. The sentence was revised as follows.

<Author's changes in manuscript>

l.791: "This is because (i) nonlinear wave excitation can also occur at near-resonant conditions, (ii) the inclusion of vertical mode two (VM2) does not change the following order-of-magnitude argument for the PIL200 location, and (iii) it appears that there is no previous study that investigated nonharmonic internal-tide variance ~~does not appear to be available~~ for higher modes in the deep water within the model domain."

-------------------- Additional references --------------------

Born, M. and Wolf, E.: Principles of Optics, 7th edition, Cambridge University Press, 2019.

Ishimaru, A.: Wave propagation and scattering in random media, IEEE Press and Oxford University Press, 1997

Van Dyke, M.: Perturbation methods in fluid mechanics, Parabolic Press, 1975.

Wunsch, C.: The ocean circulation inverse problem, Cambridge University Press, 1996.