Reply to Comments

Reviewer 1

General assessment

The manuscript presents a modelling workflow: coupling a 3D MITgcm background state, a 1D Price–Weller–Pinkel (PWP) mixed-layer model, and a Garrett–Munk (GM) internal-wave field, to reconstruct local sub-daily density and sound-speed variability where real-time profiles are unavailable. The multi-station, multi-season validation and the practical (acoustic/naval) motivation are appreciated. However, the central methodological claim, which MITgcm+PWP+GM can reconstruct a credible diurnal density field at locations without observations is never tested against the appropriate control, and several results appear affected by an amplitude artifact. Specifically: (i) the "diurnal" framing is inconsistent with the GM formulation and with the validation metric; (ii) the internal tide is likely double-counted in the integrated configuration; (iii) reported isopycnal-displacement (ID) amplitudes are physically implausible, exceed the plotted colorbars, and are consistent with an ill-conditioned definition (Eq. 9); (iv) the coupling is purely kinematic, so the "IW-driven" mechanism has no dynamical realization in the model; and (v) key parameters and the core 2D methodology are not documented I recommend major revision.

Ans: We sincerely thank the reviewer for the careful and constructive assessment of our manuscript. We greatly appreciate these suggestions and feedback, which have helped us clarify the methodology, improve the presentation of the results, and strengthen the scientific interpretation. The major comments raised by the reviewer are addressed point-by-point below.

(i) In this manuscript, the term **"diurnal variability"** refers specifically to the daily (24-hour) variability in the vertical density structure and does not refer to the diurnal tidal frequency band or to the K1/O1 tidal constituents. Our objective is to reconstruct the within-day evolution of the vertical density structure and quantify its 24-hour variability. The modelling framework adopted here is similar to that of Hareesh Kumar et al. (2010) and Yadidya et al. (2021), in which the coupled model is initialized using observed T–S profiles at 00:00 UTC and subsequently evolved at hourly intervals using hourly surface forcing to resolve the temporal evolution of the vertical density structure. In our study, the hourly T–S fields over each 24-hour period are used to quantify the daily variability of the density structure, which is then evaluated against the corresponding variability observed in the in-situ measurements. Thus, throughout the manuscript, **"diurnal variability"** denotes the within-day/24-hour variability resolved from hourly model output, rather than variability specifically associated with the diurnal tidal frequency band. To avoid any ambiguity, we have clarified this definition at the beginning of the manuscript and revised the terminology and description of the methodology accordingly (Lines 138-140).

(ii) We clarify that the MITgcm and GM components are not combined as two independent representations of the same internal-tide signal. In the integrated configuration, the MITgcm T–S profile at 00:00 UTC is used to initialize the coupled PWP+GM model for each day. The PWP+GM model then evolved hourly using the corresponding hourly surface forcing to obtain the T–S profiles for the subsequent 24-hour period. At 00:00 UTC of the following day, the new MITgcm T–S profile is again used to reinitialize the PWP+GM model, and this procedure is repeated throughout the simulation period.

Thus, the MITgcm fields provide the daily initial hydrographic state, while the GM component represents the internal-wave-induced vertical displacement within the 24-hour evolution of that state. The GM contribution is therefore not an additional internal-tide signal added to the MITgcm output at the same time. Furthermore, the MITgcm hydrographic fields have been validated against available *in-situ* observations, supporting their use as the background state (Makar et al., 2025). Therefore, we do not consider the procedure to constitute a direct double counting of the internal tide. We have clarified the distinction between the resolved MITgcm background variability and the unresolved internal wave (IW) contribution in the revised manuscript (Lines 373–378).

(iii) We clarify that the values referred to as the maximum amplitudes are from the single-hour snapshot shown in the figure. The apparent discrepancy with the colourbar arises because the figures used extended colourbar limits for consistent visual comparison among the different stations and months; hence, the labelled limits did not represent the absolute minimum and maximum values of each subplot. The reported maximum ID at AD09 values refer to the values at the analysed depths, including 200 m, for which *in-situ* observations are available at 100, 200, and 500 m. In the revised manuscript, we have retained the extended colorbar primarily to provide a clear visual representation of the spatial variability in isopycnal displacement. The actual maximum displacement values are explicitly reported in the manuscript text, while the colorbar is intended mainly for effective visualization of the spatial patterns.

(iv) We agree that the present mechanism has no dynamical realization in the model. The purpose of the coupling is instead to incorporate the unresolved IW variability into the hydrographic structure and thereby quantify its contribution to sub-daily density variability. In this sense, “IW-driven” refers to the IW contribution to the reconstructed density variability, rather than to a fully dynamically coupled IW–mixing feedback. We have revised the wording throughout the manuscript to make this scope and limitation explicit and to avoid overstating the dynamical interpretation

(v) We have now expanded the methodological description to provide the key parameters, assumptions, governing formulation, spectral representation, and details of the 2-D GM implementation. These details have been added in the revised manuscript (Lines 249–285).

Major comments

M1 The "diurnal" framing is inconsistent with the GM spectrum and with the validation metric.

The title and abstract characterize the reconstructed field as "diurnal variability of internal waves" (e.g., L27), but the GM spectrum is a broadband background continuum (f to N) with no diurnal periodicity. Genuine diurnal signals arise only from (i) PWP solar forcing and (ii) diurnal internal-tide lines (K1/O1), which enter through the MITgcm, but not GM. The validation metric, daily standard deviation (L318), mixes the diurnal band with all higher frequency GM variance and cannot isolate a diurnal component. Could authors provide diurnal band harmonic analysis or day composited diagnostics, and define explicitly which internal wave classes the framework represents. As currently formulated the framework represents a linear, broadband, random-phase background continuum, excluding nonlinear solitons and not explicitly carrying internal tide phase, so the "diurnal" terminology should be reconsidered.

Ans: We thank the reviewer for raising this important point. In this study, "diurnal variability" refers to daily (24-hour) temporal variability and does not imply variability at the diurnal tidal frequencies (e.g., K1/O1). The GM spectrum represents the broadband unresolved internal-wave (IW) field and is not intended to reproduce a specific diurnal tidal constituent.

The coupled PWP–GM framework is implemented on a daily basis. At 00:00 UTC each day, the MITgcm T–S profile is used to initialize the coupled model, which is then evolved at hourly intervals using hourly surface forcing for the subsequent 24 hours. The model is reinitialized with the next day's MITgcm T–S profile, following the approach of Hareesh Kumar et al. (2010) and Yadidya et al. (2021). Thus, the daily standard deviation in Figure 2-4 represents the magnitude of variability within each 24-hour period, allowing direct comparison with the corresponding in-situ observations

The MITgcm provides the daily hydrographic state, including the effects of prescribed tidal forcing, while the PWP–GM framework resolves the hourly evolution and represents the unresolved broadband IW contribution through the GM spectrum. We have clarified this terminology and the daily initialization procedure in the revised manuscript (Lines 138–140).

M2 Probable double counting of internal tides in the integrated configuration.

The MITgcm background already resolves the internal tide. Superimposing a GM displacement field adds IW variance in the diurnal/semidiurnal bands on top of variance already present. Authors should justify why an external GM field is needed once the internal tide is explicitly resolved and demonstrate that IW variance is not counted twice.

Ans: We thank the reviewer for raising this important concern. We clarify that the MITgcm internal-tide signal is not directly superimposed on the GM displacement field in the integrated configuration. The coupling is performed offline daily: at 00:00 UTC each day, only the MITgcm T–S profile at the study location is extracted and used to initialize the coupled model. The coupled model then evolved hourly over the following 24 hours using the prescribed hourly surface forcing. Thus, the hourly T–S evolution during this period is generated by the coupled model and does not use the time-varying MITgcm internal-tide fields.

At 00:00 UTC of the following day, the coupled model is reinitialized using the corresponding MITgcm T–S profile, and the same procedure is repeated. Although the MITgcm simulation is tide-forced and its dynamically evolved fields contain internal-tide variability, only the instantaneous 00:00 UTC T–S state is transferred to the coupled model each day. The subsequent hourly MITgcm tidal variability is not incorporated into the coupled-model evolution. Therefore, the procedure does not directly add the resolved MITgcm internal-tide variance to the GM internal-wave variance at each time step.

We have revised the description of the coupling procedure to explicitly state this daily initialization and hourly coupled-model evolution, thereby clarifying why the resolved MITgcm internal-tide signal is not directly double-counted by the GM component (Lines 373–378).

M3 The isopycnal-displacement definition (Eq. 9) is ill-conditioned below the pycnocline, and the reported amplitudes exceed the plotted colorbars.

The displacement is diagnosed as $\eta = \rho'/(d\bar{\rho}/dz)$ (Eq. 9). This definition is ill-conditioned wherever the background stratification is weak. Below the pycnocline $d\bar{\rho}/dz$ close to 0, so the small denominator amplifies any residual density anomaly, including numerical noise, into large apparent displacements. Moreover, in the present construction the density anomaly itself is generated as $\rho' \approx \zeta \cdot (\partial \rho\_p/\partial z)$ (Eqs. 6–7), which also vanishes as the gradient vanishes; η therefore approaches an indeterminate 0/0 form in the weakly stratified interior, where the result is controlled by noise rather than by a physical displacement.

This conditioning problem is consistent with the reported pattern. The maximum ID at AD09 in January (~34 m) occurs precisely at 200 m (L443), i.e. below the pycnocline in the weakly stratified layer where $d\bar{\rho}/dz$ is smallest, exactly where Eq. (9) is most unstable, and where a genuine internal wave displacement would be expected to decay rather than peak. Such amplitudes are also large for background (GM-type) internal waves in the open ocean; while large displacements can occur at internal-tide generation sites, a ~34 m daily displacement in the deep interior warrants independentphysical support (local N, GM energy level, or comparison with observed IT amplitudes) rather than being reported at face value.

Separately, and independent of the conditioning issue, the quoted amplitudes are inconsistent with the figures: values of 27–34 m (L443, L448, L454) and ~18 m (L554, L557, L600) exceed the Fig. 5 (±17.5 m) and Fig. 8 (±10 m) colorbars, so these regions are saturated and the reported values cannot be read from the panels.

Ans: We thank the reviewer for this detailed comment. We agree that the formulation in Eq. (9) $\eta(r,z) = \frac{\rho'(r,z)}{d\bar{\rho}/dz}$, can become sensitive in regions where the background density gradient is very small. We have therefore clarified this limitation in the revised manuscript. In our formulation, however, the density perturbation is generated by applying the IW-induced vertical displacement to the PWP-derived hydrographic profile. Thus, the resulting density anomaly and the density gradient used in Eq. (9) are physically related to the same displaced hydrographic structure. The ID obtained from Eq. (9) therefore represents the equivalent vertical displacement associated with the IW-induced density perturbation, following the approach adopted in previous studies including Yadidya et al. (2021). Nevertheless, we agree that the quantitative interpretation of ID becomes less robust in weakly stratified regions, particularly below the main pycnocline, and we have explicitly acknowledged this limitation in the revised manuscript (Lines 329–330).

Regarding the reported maximum ID values and the color scales, we clarify that the reported values are the actual ID values obtained from the corresponding model output. The reported maximum ID values are the actual values obtained from the model output. The common color scale was adopted primarily for visualization and consistent comparison among stations and months; hence, its extended ends do not represent the exact maximum/minimum values of each case. To avoid ambiguity, the actual maximum displacement values have been explicitly reported in the manuscript.

M4 Purely kinematic coupling: the "IW-driven" mechanism has no dynamical realization.

The coupling is one way and offline: GM displacements are kinematically superimposed on PWP profiles (Eqs. 6–7) with no feedback of IW-induced shear or mixing onto the mixed-layer dynamics. Consequently, the framework structurally cannot produce internal-wave-driven mixing, but the title claims an "IW-driven" process. In addition, Eqs. (6)–(7) linearize the displacement (first-order Taylor, $T = T_p + \zeta\, \partial T_p/\partial z$), while for ζ of 20–34 m across a sharp pycnocline this is invalid. Please (a) justify the pure kinematic superposition or discuss its limitations for stratification and acoustics, and (b) apply the finite displacement directly ($T(z) = T_p(z - \zeta)$) or justify the linearization.

Ans: We agree that the present framework represents an offline, one-way kinematic coupling and does not explicitly provide any feedback of IW-induced shear or mixing onto the mixed-layer dynamics, which is the limitation of this study. The title of the manuscript is appropriately modified.

In our framework, the MITgcm T–S profile at 00:00 IST is used to initialize the PWP model. The PWP model subsequently evolves the upper 500-m T–S structure at hourly intervals using the prescribed surface forcing. These profiles are then extended below 500 m using daily ORAS5 T–S fields to obtain the complete local water-column hydrographic structure. The IW model is subsequently used to introduce the IW-induced vertical displacement into these hydrographic profiles. Thus, the purpose of this coupling is to quantify the kinematic effect of IW displacement on the hydrographic and acoustic structure, and not the dynamical feedback of IW-induced mixing on the PWP evolution.

The kinematic displacement formulation adopted follows the approach used in previous coupled PWP–IW studies, including Yadidya et al. (2021). In this formulation, the IW displacement is represented through the first-order perturbation of the background hydrographic field,

$$T(r,z,t) = T_p(z) + \zeta(r,z,t)\frac{\partial T_p}{\partial z}$$

$$S(r,z,t) = S_p(z) + \zeta(r,z,t)\frac{\partial S_p}{\partial z}$$

We acknowledge that the validity of the first-order approximation depends on the displacement amplitude relative to the vertical scale of the background hydrographic gradientsIn our simulations, the largest displacement occurs in January, reaching approximately 34 m at the 200 m observation depth. The other seasonal maxima are comparatively smaller. The displacement values are reported at the observation depth of 100, 200, and 500 m. Specifically, the maximum positive displacements are approximately 17.5 m in April, 10 m in July, and 21.8 m in October, respectively. Thus, the 34-m value represents an isolated maximum rather than the characteristic displacement amplitude throughout the simulations.

We therefore agree that the first-order approximation should be interpreted with caution for the largest displacement values, particularly where the displacement is comparable to the local vertical scale of strong hydrographic gradients. We have explicitly acknowledged this limitation in the revised manuscript and have moderated the interpretation of the largest displacement values accordingly. Nevertheless, we retain the first-order formulation because it provides consistency with the established PWP–IW framework of previous studies and is intended here as a kinematic perturbation formulation, rather than as a fully nonlinear description of wave-induced deformation.

Importantly, the resulting perturbed T–S fields are subsequently used to calculate density and sound speed. Thus, the framework can quantify the effect of the IW displacement on the hydrographic and sound speed variability, while processes involving nonlinear wave deformation, wave–wave interaction, shear instability, turbulent dissipation, and feedback on the mixed-layer dynamics are beyond the scope of the present formulation. These limitations are now explicitly stated in the revised manuscript (Lines 355–356).

M5 Missing GM parameters and undocumented 2D methodology.

The GM reference energy E0, which sets the amplitude of all displacements and variances, and is therefore central to M3 and M7, is never specified, making the results non-reproducible. The shallow-water slope is left as "p = 3 or 4" (L219) without stating the value used. More critically, the stated novelty, a "revised approach for solving the IW boundary-value problem" enabling ID in time and space (L138–139) is not described: how horizontal wavenumbers enter, how random phases are assigned, and how the 2D field ζ(r,z,t) is synthesized are given only by citation (Yadidya et al. 2021; Evans 2000). If the 2D spatial extension is the main contribution, it must be presented explicitly, otherwise the study reads as an incremental application of Yadidya et al. (2021) to a new region. Please also report the mixing coefficients, sponge strength, and bottom-drag coefficient (only the 120 s time step is currently given).

Ans: We agree that these details are important for reproducibility and have now expanded the description of the GM and 2-D IW methodology in the revised manuscript (Lines 243–285).

M6 No absolute-density comparison; deterministic claims from a stochastic field.

Only the daily standard deviation is compared, no absolute density/temperature profile, and no single snapshot, is compared against observations. This is not incidental: in the integrated configuration the mean state of any reconstructed profile is essentially the MITgcm/ORAS5 field, and the GM term is a zero-range-mean perturbation, so an absolute comparison would test background-state accuracy, which the std metric removes. Moreover, $\zeta(r,z,t)$ is a single random phase realization, so point-by-point agreement with an observed snapshot is not expected by construction, the framework is meaningful only statistically. However, the text reports deterministic, location/time-specific extrema (e.g., "Mumbai … maximum ID of 14 m on 27 January," L551; the per-location, per-range readings of Fig. 8, L549–573). Please either provide absolute profile/snapshot comparisons to substantiate the deterministic claims, or restate all results as statistics (variance, spectra, distributions) and state that the framework does not provide deterministic predictions.

Ans: The primary objective of this study is to quantify daily (24-hour) variability in the hydrographic structure, rather than to reproduce the instantaneous phase of an individual internal wave. Since the GM formulation provides an IW field without prescribing the observed phase, point-by-point comparison of an instantaneous reconstructed profile with an *in-situ* snapshot is not an appropriate measure of model performance. Accordingly, Figures 2–4 evaluate the within-day variability, calculated from the hourly modelled profiles over each 24-hour period, and compare it with the corresponding variability derived from the *in-situ* observations at each buoy location for the respective analysis months. Thus, the validation specifically tests whether the coupled framework can reproduce the magnitude and vertical distribution of the observed daily variability.

The absolute hydrographic state is provided by the MITgcm/buoy fields used in the coupled framework, whereas the GM component represents the additional IW-induced variability. We have therefore clarified that the validation presented in Figures 2–4 is a validation of the variability, rather than of an instantaneous IW realization or its phase.

For Figure 8, the reported maximum ID values are intended to characterize the magnitude and spatial and seasonal distribution of IW displacement across the eastern Arabian Sea, rather than to represent deterministic predictions of an observed IW at a specific time and location. The analysis is used to identify regions and months with larger IW displacements and their relationship with the background hydrographic structure. We have revised the text to make this statistical and diagnostic interpretation explicit and have avoided wording that could imply deterministic prediction of individual internal-wave events (Lines 671–676).

M7 Validation: weak metric, questionable magnitudes, and the wrong baseline.

(a) Weak metric. Standard deviation is the only skill measure and is insensitive to phase, frequency, and vertical structure; two very different time series can share the same std. No error bars and no quantitative per-depth metric (RMSE/correlation) are given for any profile in Figs. 2–4(a–j). If E0 is tuned to match observed std, the agreement is not independent. (b) Discrepancies understated. Even on this lenient metric, clear mismatches exist, the coupled model departs at 150–200 m, and the observed spread at 50–100 m in Jun–Aug substantially exceeds the modeled envelope. "Good agreement" should be tempered. (c) Axis scaling and magnitude. The k–m panels are capped at 0.5 kg/m^3. A daily std of 0.5 kg/m^3 implies day-to-day excursions of order 1.5–2 kg/m^3, which is a large fraction of the surface-to-100 m density contrast, also the cap hides values that exceed it (e.g., September spikes in Fig.3k). Please convert the daily std to an equivalent vertical displacement (using local $d\bar{\rho}$_/dz) and check consistency with the IDs of Figs. 5/8. The large 5 m std is attributed by the authors to hourly surface-flux forcing (L357–359), i.e., PWP solar forcing rather than IW dynamics, at odds with the "IW-driven" framing. (d) Wrong baseline (Table 1). Table 1 compares the integrated model only against a bare MITgcm run, which contains no sub-daily variability by construction, a comparison designed to be won. The relevant control, integrated vs. the observation-initialized coupled model, is absent, thus the core claim (MITgcm substituting for observations) is never verified. Only July shows a clear reduction (0.62 to 0.44, still the largest).

Ans: We clarify that the primary objective of the coupled framework is to assess whether the daily (24-hour) variability of the vertical density structure can be reproduced, rather than the phase or frequency of individual IW events. Since the GM component does not prescribe the phase of the observed IW field, the standard deviation of hourly profiles within each 24-hour period is used as the primary metric. Figures 2–4 therefore compare the magnitude and vertical distribution of modelled daily variability with the corresponding *in-situ* observations.

We acknowledge the larger model–observation discrepancies at 150–200 m during June–August, particularly for the coupled PWP–GM configuration. These may partly arise because the model is constrained by *in-situ* T–S observations at 00:00 UTC, while subsurface variability below 100 m is less constrained and strong monsoon-related variability is not fully captured by surface forcing alone. We have therefore replaced the generalized statement “good agreement” with a more objective description of the results and differences (Lines 433–437).

The density standard-deviation values are calculated directly from the model output and are not restricted by the plotted colourbar range; the extended colourbar ends indicate values beyond the labelled limits. We also clarify that daily density standard deviation represents within-day variability, not the maximum density excursion. Thus, a value of 0.5 kg m^{-3} should not be interpreted as a 1.5–2 kg m^{-3} excursion. Furthermore, this variability reflects the combined PWP evolution and GM-induced displacement and cannot be directly equated with GM-derived IW amplitudes.

Finally, Table 1 is intended to assess the change in simulated daily variability resulting from applying the PWP–GM framework to the MITgcm-initialized state. The MITgcm-only case provides the reference background state, while the coupled configuration introduces sub-daily evolution. We have clarified this purpose and avoided interpreting the comparison as an independent model-skill assessment (Lines 501–517).

M8 Shallow v.s. deep configuration (Fig.7) does not support the claim.

The density RMSE difference is only ~0.014 kg/m^3 (~4%, shallow 0.336 v.s. deep 0.350), with interleaving profiles and a ~5-day observational record without error estimates, plausibly within noise. The buoyancy-frequency RMSE (shallow 4.702 / deep 6.246 cph) is comparable to the signal itself, so describing the stratification as "adequately represented" (L517) is not justified for either configuration. There is also a temporal/location mismatch: a May-2009 CTD at Station A is used to validate a framework configured for 2018 at different sites.

Ans: We clarify that the May 2009 CTD observation is used to evaluate the model for the corresponding May 2009 period and at the model location closest to the CTD station, and is not part of the 2018 simulations. This temporal and spatial correspondence has been clarified in the revised manuscript (Lines 597–600).

For consistency, the observed and simulated profiles are averaged over the upper 50 m and the corresponding day. The shallow- and deep-water configurations yield density RMSEs of 0.336 and 0.350 kg m^{-3}, respectively. Although the difference is modest, the purpose is to assess the relative performance of the two configurations for this observational case rather than claim a statistically significant improvement.

We also acknowledge the relatively large buoyancy-frequency RMSE and have removed/modified the statement that stratification is “adequately represented.” The revised text now presents the comparison more cautiously, noting the limitations of the short observational record and uncertainty in the derived buoyancy frequency (Lines 603–605). Thus, Fig. 7 is retained as a sensitivity assessment of the shallow- versus deep-water configurations without overstating the improvement.

M9 Open boundary forcing, volume balance, and spin-up.

Four open boundaries are forced with daily T/S and velocity (L180). Please clarify whether net volume is balanced (inflow = outflow), particularly given BoB intrusion via the West India Coastal Current. With velocity forcing plus ten tidal constituents, is the analysis window sufficient for the internal-tide field and adjustment currents to reach statistical steadiness? Please provide a spin-up diagnostic.

Ans: We thank the reviewer for raising this important point. In the MITgcm configuration, the four open boundaries are forced with daily temperature, salinity, and velocity fields, together with the prescribed ten tidal constituents. The boundary forcing allows the regional circulation, including the influence of the West India Coastal Current and associated Bay of Bengal water intrusion, to be represented.

The MITgcm configuration and its validation can be traced to Makar et al. (2025). In the present study, only the MITgcm output at 00:00 UTC is used for initialization of the PWP–GM model. The first 10 days are treated as a spin-up period to allow the model to adjust to the imposed boundary and tidal forcing. This duration is sufficient for internal tides to propagate across the model domain and is consistent with previous studies (Ma et al., 2023; Mohanty et al., 2018; Tang et al., 2023; Xu et al., 2016; Yadidya et al., 2022). These details have been clarified in the revised manuscript (Lines 212–218).

M10 Are the four months continuous or independent short integrations?

The MITgcm is configured "separately for four representative months" (L296–299), each with 10 days of spin-up. If these are independent short runs rather than one continuous annual integration, then inter-month differences may conflate genuine seasonal signal with differences in initial conditions and spin-up state. Please clarify, and if independent, explain how seasonal signal is separated from initialization drift.

Ans: The four representative months are simulated as independent MITgcm experiments, rather than as one continuous annual integration. For each selected month, the model is initialized using the monthly and year-specific mean temperature and salinity fields corresponding to that particular month and year. A 10-day spin-up was then applied before the analysis period to allow the circulation and internal-tide field to adjust to the prescribed initial, boundary, and tidal forcing.

Thus, the seasonal differences are not obtained by continuing one month's model state into the next month; rather, each experiment represents the hydrographic and dynamical conditions specific to its corresponding month and year. The 10-day spin-up removes the initial adjustment before the model output is used for analysis. The adequacy of the spin-up is further supported by the domain-integrated kinetic-energy evolution, which demonstrates stabilization before the analysis period.

Minor comments

1.(L39–41) A reference is needed for "IWs are influenced by the structure of the mixed layer." Please clarify whether "structure" refers to mixed-layer depth, the T–S structure, or something else. – **Ans:** Done

2.(L41–45) Please briefly explain the mechanism by which IWs lose energy to the mixed layer or bottom boundary layer under weak stratification. - **Ans:** Done

3.(L51) Mu et al. (2026) attribute ~41% of near-inertial IW energy variability to MLD. Since in this framework the GM field enters almost entirely via MLD/stratification through the PWP profile, please comment on how the remaining ~59% of the driving (wind, topography, mesoscale) is represented. - **Ans:** Done

4.(L80–81) A reference is needed for "sharp stratification contrasts, and energetic mesoscale and IW activity." - **Ans:** Done

6.(L108–133) The literature review is dominated by self-citation and a narrow set of Indian-coast studies; broader international work on PWP+GM coupling and IW acoustics is underrepresented. Please broaden the review to position the paper's specific increment.- **Ans:** Done

7.(L121–125) A reference is needed for the IT’s spatial and temporal variability in the AS. Authors only mentioned the eastern AS.- **Ans:** We thank the reviewer for the suggestion. However, the present study is specifically focused on the eastern Arabian Sea (EAS), and the analysis of internal-tide spatial and temporal variability is restricted to this region. Therefore, we believe that introducing a broader discussion of the entire Arabian Sea is beyond the scope of the present work. The relevant references and discussion have been provided specifically for the EAS to maintain consistency with the study objectives and analysis domain.

8.(L179–180) Both initial and boundary conditions derive from the relatively coarse ORAS5, which is later invoked to explain the integrated model's too-deep January MLD - **Ans:** We acknowledge that the relatively coarse ORAS5 initial and boundary conditions may introduce biases in the simulated hydrographic structure. However, the January MLD is dynamically simulated by MITgcm and is not directly prescribed by ORAS5. We have clarified this distinction in the manuscript (Lines 373-375).

(Figs. 2–4). Given that this is a known systematic bias, please justify the choice or provide a sensitivity test rather than attributing the bias post hoc.- **Ans:** The objective of this analysis is to examine the daily variability of density simulated by the coupled and integrated models, rather than to evaluate sensitivity to the initial conditions. The coupled model is initialized daily using buoy observations, whereas the integrated model is initialized from the MITgcm simulation, following the approach used in previous studies (e.g. Yadidya et al., 2021; Hareesh Kumar et al., 2010)

9.(L174–176) The eight-grid-point sponge layer is intended to suppress IW reflection. Please discuss whether it also damps the internal tides of interest, given that some are generated at sites close to the boundaries.- **Ans:** We thank the reviewer for raising this point. The eight-grid-point sponge layer is applied only at the open boundaries to minimize wave reflection and does not directly damp the internal tides in the interior model domain. Moreover, the simulated tidal signals are validated against tide-gauge observations, showing good agreement in amplitude and phase, as reported in Makar et al. (2025). This validation provides confidence that the sponge layer does not significantly affect the internal-tide variability of interest.

10.(L206) Please add references for the stability criteria (bulk/gradient Richardson numbers). – **Ans:** Done (Lines 241-242).

11.(L259–267) the density field is computed using the legacy UNESCO 1983 (EOS-80) formulation (Lines 259–261), whereas the sound speed follows the modern TEOS-10 standard (Line 267). While the numerical discrepancy is negligible for the final results, maintaining consistency within a single thermodynamic framework is recommended. - **Ans:** We thank the reviewer for this observation. EOS-80 (UNESCO, 1983) and TEOS-10 are indeed two internally self-consistent frameworks, each valid in its own right for the corresponding quantity: EOS-80 remains a well-established and widely used standard for seawater density, while sound speed is computed using the more recent TEOS-10 formulation, which is now the internationally recommended standard for sound-speed calculations. As the reviewer rightly points out, the numerical discrepancy between the two frameworks is very small for open-ocean waters of standard composition — the typical Absolute–Practical Salinity offset is only ~0.15 g/kg (IOC et al., 2010), corresponding to sound-speed and density differences well below the internal-wave signals in general. Given that this discrepancy does not affect any of the reported results or conclusions, we have retained the original formulation for this manuscript. The differences between the two frameworks are small for open ocean waters (sound speed and its comparison is done only in open ocean in our study) of standard composition and do not affect the reported results or conclusions. We have retained the original formulation and clarified this methodological detail in the revised manuscript. (Lines 332-335)

12.(L285–287) What ensures a smooth join between the PWP (0–500 m) and ORAS5 (>500 m) profiles? A discontinuity in T–S or N^2 at 500 m would contaminate the GM modal structure at deep sites, since this merged profile feeds the eigenvalue problem. - **Ans:** We thank the reviewer for raising this important point. The PWP model is constrained by the available *in-situ* observations in the upper 500 m, while observations are not available at greater depths. For the deep-water sites, the PWP-derived profiles are therefore smoothly joined with the ORAS5 profiles below 500 m to obtain a complete T–S structure over the full water column.

This is necessary because the GM modal structure is obtained by solving the vertical eigenvalue problem over the entire water column, and the deep-ocean stratification contributes to the eigenfunctions and eigenvalues, particularly for the higher modes. We use 20 vertical modes at sites deeper than 1500 m to adequately represent the vertical structure of the internal-wave field. The merged profiles were checked to ensure no artificial discontinuity in T–S or (N^2) at the 500 m joining depth. Thus, the ORAS5 extension provides the required deep-ocean stratification without introducing an artificial discontinuity into the modal calculation.

13. (L291) The longwave/shortwave extinction coefficients (0.6 m and 20 m) are prescribed as fixed constants; these control PWP daytime heating and hence the near-surface standard deviation. Please discuss their sensitivity. - **Ans:** The extinction coefficients used here (0.6 m and 20 m) correspond to the Jerlov Type IA open-ocean water type (Paulson and Simpson, 1977) and are standard for open-ocean PWP/KPP applications. In-situ optical profiles from DYNAMO closely match this Jerlov IA curve. Pei et al. (2018), as reported in Shinoda et al. (2021), found no significant difference in simulated SST between model runs forced with the measured optical profile and those using the fixed Jerlov IA coefficients. We therefore consider the fixed coefficients adequate for this study region.

14.(Figs.2–4 L238) A 2.7 km hydrostatic MITgcm cannot self-resolve the short-wavelength IWs the GM spectrum represents. This motivates the external GM field but also means the MITgcm cannot serve to validate it; please discuss (see also M2).- **Ans:** We agree that the 2.7 km hydrostatic MITgcm configuration does not explicitly resolve the short-wavelength internal waves represented by the GM spectrum. However, this is not the intended role of MITgcm in our integrated framework. The MITgcm provides the background temperature and salinity fields at 00:00, which are used to initialize the daily density structure, while the unresolved internal-wave-induced displacement is represented separately by the GM model.

15.(L292, L366) Please quantify the data exclusions (post-October records; the October 5 and 10 m near-surface data).- **Ans:** As stated in the manuscript, the 5 and 10 m observations of October -AD10 were entirely excluded from the analysis because they exhibit substantial near-surface noise and are therefore not representative of the density variability of interest. In addition, buoy observations were available only until October 2018; hence, no records after October 2018 were included in the analysis.

16.(L146) Please add the observation time period for the AD08, AD09, and AD10 buoys.- **Ans:** It is now mentioned (Line:171).

17.(L174-183) Key model parameters are not reported (horizontal and vertical mixing coefficients, sponge-layer strength, bottom-drag coefficient); only the 120 s time step is given. - **Ans:** Done (Lines: 206-218)

18.(Eqs.4–5) The two spectra are mutually inconsistent: the deep-water Eq. (4) uses

$\sqrt{(\omega^2+f^2)}$ with an ω^3 denominator, whereas the shallow-water Eq. (5) uses $\sqrt{(\omega^2-f^2)}$ with ω^2. The standard band-limited GM spectrum uses ω^2-f^2 and peaks near $\omega = f$; the "$+f^2$" in Eq. (4) removes the near-inertial peak and appears to be a typo. Please also check the $4/\pi$ prefactor and the normalization. - **Ans:** Done

19.(Fig.6) Fig. 6 is not an independent result: T, ρ, and c are all generated from the same displacement ζ via Eqs.(6)–(7), so the ID–sound-speed correspondence (L469–474) is an algebraic consequence of the construction rather than a validated finding. Any ID amplitude bias (see M3) propagates directly into the sound-speed excursions.- **Ans:** We agree that the temperature, density, and sound-speed perturbations are derived from the same isopycnal displacement and are therefore not independent validations. The purpose of Fig. 6 is not to present an independent validation, but to illustrate the two-dimensional spatial structure of the isopycnal displacement and its corresponding sound-speed variability.

20.(Figs.5, 8) Colorbar saturation: the plotted ranges (±17.5 m and ±10 m) do not span the amplitudes quoted in the text (see M3).- **Ans:** The colorbar limits are intentionally selected to emphasize the spatial structure and dominant variability of the isopycnal displacement. Extending the limits to include the extreme values would reduce the visual contrast over most of the location and make the spatial patterns less discernible. The extreme amplitude values is nevertheless retained in the underlying data and reported in the text.

21.(Fig. 1) The "Maldives" label and pointer target the northern segment of the chain, please reposition toward the central archipelago for a more accurate geographic representation.- **Ans:** Done

22.(L297) Please clarify whether the four monthly simulations were run from four different initial conditions or run from one simulations' pickup file.- **Ans:** Four monthly simulations were run from four different initial conditions (Lines: 213-214)

23.Inconsistent spellings throughout: Vellimalai/Vellimallai, Mangalore/Mangaluru, Khambhat/Khambat. Please standardize.- **Ans:** Done

24.(L355) "couple model" should read "coupled model."- **Ans:** Done

25.(L30, L134, L476, L608) The naval-operations motivation is repeated several times, in places nearly verbatim; please consolidate.- **Ans:** Done

Ans: All minor comments have been addressed, and the manuscript has been revised accordingly.

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