<u>Reply to Comments</u>

Reviewer 2

This paper, 'Internal wave-driven diurnal density stratification in the Eastern Arabian Sea', by Pragnya Makar, Ambarukhana Devendra Rao, Badarvada Yadidya and Vimlesh Pant, presents a modelling framework for the variability of near-surface temperature and salinity in the Eastern Arabian Sea. This framework is validated against *in-situ* observations at three locations.

The proposed model combines the Price-Weller-Pinkel prognostic mixed-layer model with an internal wave model based on the Garrett-Munk spectra. The resulting coupled model can be further combined with outputs from a 3D primitive equation numerical simulation for initialisation.

I have a few major concerns about the objectives and novelty of the results presented in this paper, which must be addressed before it can be considered for publication in Ocean Science. Firstly, the purpose of the paper is unclear. Is it about a modelling framework, in which case the main aim would be to validate the model against observations to demonstrate its predictive capacity and usefulness for further scientific studies? Or does it focus on using an existing modelling framework to unravel the mixed-layer and near-surface dynamics -- or other applications, such as predicting acoustic velocity modulation? I could not determine which of these aspects forms the core of the results presented in the paper, and I was left with the impression that it is actually none of them.

Indeed, unless I have misunderstood the information provided, the proposed coupled model was previously introduced by Yadidya et al. (2021) and validated against observations made at two different locations in the eastern part of the Andaman Sea. Therefore, the model is not new. It may still require further validation, but I doubt this would be sufficient to justify publication in Ocean Science. The only additional step I could find in this paper that was not already present in Yadidya et al. (2021) was the computation of acoustic celerity from the predicted density field. However, this is not validated against observations and essentially involves applying TEOS-10 to the density field. Therefore, there is nothing new here. Furthermore, the abstract and conclusions suggest that unravelling the surface and mixed-layer dynamics is not the paper's central outcome: the coupled model does not provide new insights into these phenomena and is not employed to explain the observations. Consequently, the remarks on the variability of the upper ocean structure could be derived from the observational dataset alone. While the introduction claims that 'this study provides new insights into the dynamics of IWs and their influence on coastal and open-ocean variability', neither the conclusion nor the body of the paper addresses this explicitly.

Therefore, I would be grateful if the authors could clarify the above points. What is this contribution really about, and what makes it a novel contribution -- compared to Yadidya et al 2021 -- to justify a new publication?

Below, you will find more specific remarks and questions about the paper.

Ans: We thank the reviewer for this important comment. We agree that the coupled PWP–IW framework itself was previously utilised by Yadidya et al. (2021), and we have clarified this point in the revised manuscript.

The primary focus of the present study is not the development of a new modelling framework, but its application to the Eastern Arabian Sea (EAS), a region with substantially different hydrographic and dynamical characteristics from the Andaman Sea. Particularly, we systematically examined the seasonal and spatial variability of internal-wave-induced isopycnal displacements and associated density variability across the EAS, considering both shallow/coastal and deep-water environments.

The study therefore addresses how the magnitude and vertical structure of IW-induced displacement vary among seasons and locations, including where and when the largest displacements occur. The reconstructed density and sound-speed variability provide an additional application of this framework, but acoustic prediction is not the primary objective.

We have revised the Abstract, Introduction, and Conclusions to clearly distinguish the established methodology for the new regional application and the seasonal and spatial findings obtained for the eastern Arabian Sea (Lines: 27-32, 160-164, 713-716).

Remarks and questions

- The abstract states that 'the model further captures diurnal variability and associated ID over spatial scales of up to 10 km', but, as far as I understand, this is not validated against observations. Rather, it is some kind of extrapolation: what is validated is the variance produced by the model. The phase-resolved, range-dependent structure comes from random realisations of the IW field (see my comment below for more information on these aspects).

  Ans: We agree that the phase-resolved, range-dependent IW field over 0–10 km cannot be directly validated against observations because observations are available only at specific locations. In the present study, the observational validation is performed using the 24-hour density standard deviation calculated from the hourly modelled and observed density fields.

  The 2-D GM formulation is used to provide a spatial realization of the IW-induced displacement field by combining the GM vertical-mode spectrum with horizontal wavenumbers and their corresponding spatial and temporal phase evolution. This allows the model to examine the spatial evolution of IW-induced displacement over 0–10 km at 1-km intervals, where direct observations are unavailable. Thus, the 10-km result is presented as a model-based spatial extension, rather than as a phase-resolved observational prediction. The 2-D GM formulation assumes an isotropic internal-wave field, with wave energy distributed uniformly across horizontal propagation directions. Therefore, the simulated 0–10 km displacement field represents an idealized spatial realization based on these assumptions rather than a directionally resolved observational prediction.

  We have clarified this distinction in the revised manuscript and modified the statement in the Abstract accordingly (Lines 17–20).

- 3.1: Figure 3 does not support that the PWP captures the variability during the summer monsoon months (l.340): the red profiles differ substantially from the data points

  Ans: We clarify that the PWP model is intended to represent the mixed-layer variability, and its applicability is limited to the mixed-layer depth (MLD). Below the MLD, the PWP-derived daily density variability is essentially zero, as expected from the model formulation.

  At AD09, the relatively strong southwest monsoon forcing and associated coastal upwelling produce a shallow and highly variable mixed layer during the summer monsoon, which contributes to the differences between the model and observations at some depths. Nevertheless, the coupled and integrated model and observations show comparable variability during most of the monsoon period. The larger discrepancy in September occurs during the transition from the southwest monsoon, when the atmospheric fluxes and upper-ocean structure are also undergoing rapid transition. We have clarified this limitation and revised the corresponding statement in the manuscript (Lines 454–456).

- sect. 3.2, l.421: what is the orientation of the slice? offshore/onshore or parallel to the coast? –

  Ans: The horizontal (spatial-range) axis in Figs. 5 and 6 represents the local horizontal-range coordinate generated by the coupled model's internal-wave/sound-speed realization scheme at each individual buoy site (AD08, AD09, AD10), rather than a continuous cross-shore or along-shore transect connecting the three moorings. This follows directly from the Garrett-Munk spectral formulation underlying the realizations (Garrett and Munk, 1972, 1975,1979; Flatté et al., 1979), which is horizontally isotropic and therefore has no preferred orientation — the GM spectrum describes a wave field that is horizontally isotropic, with waves arriving equally from all horizontal directions. (Lines 576-583)

- sect 3.2, starting at l.470 (see also l.585 in the conclusion): the discussion of the emulated perturbations of c is not very informative, as this is essentially by design: c(ρ) is a monotonic function. I cannot see the added value of the information provided here.

  Ans: We agree that the correspondence between density/isopycnal displacement and sound-speed modulation is physically expected because sound speed is a monotonic function of the hydrographic state. We did not intend to present this correspondence as a new physical mechanism. Rather, this analysis is included to demonstrate the acoustic implication of the reconstructed hydrographic variability, particularly the spatial modulation of the sound-speed structure associated with the simulated IW-induced displacement.

  We have therefore shortened and streamlined this discussion in the revised manuscript, while retaining the statement as a direct interpretation of the modelled hydrographic perturbations (Lines 583-584).

- sect. 3.3: I would suggest moving the first paragraph into the validation section 3.1

  Ans: We thank the reviewer for this useful suggestion. The first paragraph of Section 3.3 has been moved to the validation section (Section 3.1) in the revised manuscript, as suggested.

- It is unclear whether the model, and specifically the IW model, predicts variability (e.g. T and S variance) or actual phase-resolved isopycnal displacements. In particular, I am not sure to understand how IW-induced isopycnal displacement time series are constructed from the GM spectrum, which only provides variance information. Could you please clarify lines 225–227? Are you generating random time series that match the variance? Looking at Yadidya et al. (2021) and Evans (2000 -- grey literature), this seems to be the case. However, the generated time series are only 'plausible' time series, not phase-resolved predictions. Finally, why is the IW model 2D ($x-z$)? What is the underlying assumption?

Ans: The GM formulation provides the statistical energy/variance distribution of the internal-wave field, from which our 2-D model constructs a synthetic realization of isopycnal displacement by combining the prescribed modal and wavenumber components with randomly assigned phases. Thus, the model produces explicit displacement time series, but these represent plausible statistical realizations of the GM field rather than deterministic, phase-resolved predictions of the observed internal waves.

In the present study, the observational validation is therefore performed for the resulting 24-hour density variability, rather than for the instantaneous phase of density. We have clarified this distinction in Lines 436-442 and revised the terminology accordingly.

The IW model is formulated in 2-D (x-z) to represent the evolution of the IW field along a selected horizontal propagation/range direction while resolving its vertical structure. The formulation therefore assumes that the dominant horizontal variability occurs along this direction and neglects cross-range variability. This 2-D representation enables the spatial extension of the IW-induced displacement from the observation location to 10 km, where direct observations are unavailable. This assumption and its limitation are now explicitly stated in the revised manuscript (Lines 576–583).

- Fig. 8: I could not really relate the text to the Figure. What are the observed oblique patterns in these Hovmöller plots: IW propagation?

Ans: We clarify that Fig. 8 is not a Hovmöller plot and does not represent temporal propagation. It presents a single snapshot of the vertical distribution of model-derived isopycnal displacement over a 10-km offshore range, from Vellimallai, Kochi, Mangaluru, Goa, Jaigad, and Mumbai, at 23:00 UTC on the 20th day of January, April, July, and October 2018 using integrated model for shallow water.

The oblique patterns therefore represent the spatial structure of the synthesized IW-induced displacement in the depth–range plane at that particular instant, and should not be interpreted as observed IW propagation.

Minor comments and typos

- l.11: what are 'these processes' referring to? None of the items from the previous sentence (pynocline, ML and diurnal variability) are 'processes' – **Ans:** Done
- end of section 2: when initialising with MitGCM outputs, are you using a day average or instantaneous data? - **Ans:** The MITgcm temperature and salinity fields used for initialization are instantaneous outputs at 00:00 UTC, rather than daily averaged fields. We have clarified this in the revised manuscript (Lines: 373-378, 387)
- Figures: labels (a, b, c...) are missing – Its clearly there where ever I have mentioned in the manuscript - **Ans:** The panel labels (a), (b), (c), etc. are clearly included in the figures and are also referred to accordingly in the manuscript.
- l.355: "both the couple**d**"- **Ans:** Done
- sect. 3.2 could be significantly shortened. In its current form, it is a verbatim of what one can see in Fig.5. I would suggest focusing on the key aspects, since detailed numbers are visible in the Figure. - **Ans:** We have retained the detailed description in Sect. 3.2 because it provides quantitative information on the magnitude and spatial distribution of the isopycnal displacement, including the extreme values that are not readily evident from the figure. The colorbar limits are intentionally chosen to enhance the visualization of the dominant spatial patterns; therefore, the full range of amplitudes cannot be inferred directly from the figure. We believe that retaining these quantitative details provides useful context for interpreting the results.
- l.580: "up to" (missing space)- **Ans:** Done

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

References

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