Response letter to the referee #1

Presented is an improved version of the internal solitary wave model. The efforts of the improvements have been made through several aspects: inclusion of background currents, horizontally inhomogeneous stratifications, and many others. The results are very encouraging. This paper documents these achievements towards a better internal solitary wave model. The graphs are impressively beautiful. I would recommend it be accepted for publication after some clarifications and minor revision.

***Response*:**

We would like to thank the referee for the careful reading and valuable comments. In the revision, we have carefully considered them, and the necessary changes are provided to address them. Below, we provided point-by-point responses in blue to your comments.

Specific comments:

The title gives readers an impression that this is a forecast model. However, it has not been implemented as a forecast model yet. The numerical experiments reported in this paper are hindcast modeling. I would suggest to remove the keyword "forecasting".

***Response*:** We sincerely appreciate the referee's insightful suggestion. In accordance with this recommendation, we have systematically replaced the abbreviation "ISWFM" (Internal Solitary Wave Forecasting Model) with "ISWNM" (Internal Solitary Wave Numerical Model) throughout the manuscript. This revision ensures consistency with the technical scope of our model, which emphasizes numerical simulation capabilities rather than operational forecasting. All relevant figures and texts have been updated to reflect this terminology adjustment.

L17, "ISWFM-NSCS v2.0" can be replaced with "the new version".

***Response*:** "ISWFM-NSCS v2.0" has now been replaced with "ISWNM-NSCS v2.0".

L20, "in ISWFM-NSCS v2.0" can be removed.

***Response*:** "in ISWFM-NSCS v2.0" is now removed.

L21, "presented" should be changed to "used".

***Response*:** We now replace "presented" with "used".

L68, “by comparing with numbers of” can be changed to “through”.

Response**:** We now replace “by comparing with numbers of” with “through”.

L79, “by” should be changed to “by using” or “from”, and “a” should be “the”.

Response**:** We now replace “by a” with “by using the”.

L87, “well” should be changed to “good”.

Response**:** We now replace “well” with “good”.

L138-139, it would be good to provide some examples, e.g., Liu et al. (2000, 2008).

Response**:** We fully concur with the referee’s suggestion regarding the importance of exemplifying the interactions between ocean circulations and internal solitary waves. In revised manuscript, we have now integrated two pivotal studies by Liu et al. (2000, 2008) here.

Figure 5, the model name “MITgcm” should be changed to “ISWFM-NSCS v2.0” for a more specific case.

Response**:** We agree that the headings should be more specific and now replace “MITgcm” with “ISWFM-NSCS” in Figure 5.

L145-148, it is mentioned that background currents are added and that velocity data are also extracted from the HYCOM. However, it is not clear whether the HYCOM velocity data are used as the background currents. If so, please clearly state it in this paragraph. If not, then provide more information on what data are used for specifying the background currents or temperature and salinity fields in the ISWFM-NSCS v2.0. (This information is found later in the manuscript, L320-321. But it would be better to state that earlier in this paragraph).

Response**:** We sincerely agree that the information about how to extract HYCOM velocity and associated temperature and salinity fields as the initial and boundary conditions should be clarified in section 2.3. To demonstrate the source of background currents and hydrographic fields in the ISWNM-NSCS v2.0, we have now explicitly stated in the revised manuscript as follows:

“The background zonal and meridional velocity fields, associated with the corresponding temperature and salinity fields, are directly derived from the global HYCOM re-analysis dataset (https://www.hycom.org/, last access: 12 July 2024). These three-dimensional datasets are linearly interpolated onto the model grid to initialize the baseline dynamic conditions, while the time-varying velocity fields from the HYCOM dataset are imposed as lateral boundary forcing across all four domain edges, thereby continuously driving the internal circulation patterns through dynamic coupling."

Response letter to the referee #2

This paper presents an evolution of an already existing model focused on the internal solitary wave forecasting, known to be challenges for many physical reasons. Based on my own reading, the paper is clear, well written and present several important results that might interest the community. Three major evolutions are presented: modification of the eddy viscosity/diffusivity, add of a realistic non-homogeneous stratification and background current. The authors showed the impact of adding each component on the forecasting performances.

Response:

We would like to thank the referee for the careful reading and valuable comments. In the revision, we have carefully considered them, and the necessary changes are provided to address them. Below, we provided point-by-point responses in blue to your comments.

However, my main concern is about the choice of a constant eddy viscosity/diffusivity for their model. If the value is well suited for ISW as the authors explained, the values are far too large out of these phenomena, probably rapidly destroying the stratification, a problem they already faced in the first version of their model. I would suggest using an adaptative turbulent closure scheme, like the classical k-epsilon. At least, I would appreciate such a test or a discussion on that.

Response:

We sincerely appreciate the reviewer's insightful comment regarding the application of constant eddy viscosity/diffusivity and the suggestion to explore adaptive turbulent closure schemes. To address this concern, we conducted a sensitivity experiment using the K-Profile Parameterization (KPP) scheme (Large et al., 1994), a widely adopted turbulent closure model for oceanic boundary layers. Simulations incorporating the KPP scheme improved arrival time predictions (RMSD = 0.58 h vs. 0.63 h), but underperformed the control run (constant coefficients) in reproducing critical ISW properties, including maximum amplitude (RMSD = 37.22 m vs. 26.51 m), propagation direction (RMSD = 14.46° vs. 13.74°), and half-widths (RMSD = 0.25 km vs. 0.17 km). These discrepancies align with findings by Thakur et al. (2022), where the KPP scheme's excessive vertical mixing in stratified regions dampened ISW signals and degraded wave coherence. Statistical results and comparative metrics are detailed in Appendix A (Fig. A1). Given these findings, we retain the constant-coefficient configuration as the control run and relegate the KPP analysis to Appendix A to maintain focus on the optimized model setup.

Another important remark regarding the choice of the eddy viscosity/diffusivity, concerns the numerical diffusivity which is neither taken into account nor discussed. I would appreciate the author to warn on this potential issue, when they explain the choice of the values based on in situ measurements.

***Response*:**

We sincerely appreciate the reviewer's insightful comment regarding numerical diffusivity and its potential influence on our model's parameterization. Below, we have revised the relevant text to explicitly address this concern, while maintaining consistency with observational and theoretical justifications for our parameter choices. The added paragraph in section 2.1 emphasizes the distinction between physical and numerical diffusivity and acknowledges the latter as a factor requiring consideration. Specific revisions are detailed below:

"Numerical diffusivity, an artifact of discretization in the model's advection schemes, may also contribute to the effective mixing in simulations. While our parameter choices are grounded in observational constraints, the total diffusivity experienced in the model could include both physical and numerical components. To mitigate this, ISWNM-NSCS v2.0 employs a third-order nonlinear advection scheme to minimize spurious numerical diffusion (Adcroft et al., 2008). Nevertheless, future work will aim to explicitly quantify and disentangle these effects, as numerical diffusivity remains an important consideration in interpreting model-derived mixing rates."

Minor suggestions:

The introduction is lacking details on the amplitude/wavelength of the ISW in the region. It's important to be fully convince that you resolution is enough to really capture these small-scale waves. In addition, I fell that it is lacking geographical information about the region. Maybe you can add the quoted names on Fig 2.

***Response*:** We thank the reviewer for the valuable feedback. To address the concerns, we have added observed ISW characteristics in the NSCS (vertical displacements exceeding 200 m and wavelengths of ~3 km) to emphasize the necessity of high-resolution modelling. ISWFM-NSCS v1.0's horizontal (500 m) and vertical (90 layers) resolution is explicitly stated to align with these scales. Geographic clarity is enhanced in Fig. 2 with labelled features (e.g., Dongsha Atoll, Luzon Island, Taiwan Island, SCS, and West Pacific) and bathymetric contours.

Line 155 it is not clear if you are talking about 28 properties or 28 ISWs.

***Response*:** We fully agree that the original phrasing was unclear and have revised it to "five wave properties of 28 ISWs" to improve clarity.

Regarding the observations, even if they are already introduced in Gong et al 2023, I suggest to briefly introduce them: which instrument, how long ...

***Response*:** We entirely agree that a brief introduction of in-situ observation should be added and now revise the sentence as "To conduct a more detailed evaluation of the model's accuracy in predicting ISWs, we incorporate field observations from the Dongsha (hereafter DS) mooring station (117°44.7'E, 20°44.2'N; deployed from 1 August to 6 September, 2014). The mooring included ADCPs (2-min sampling; 16/8-m vertical bins) and distributed temperature, CTD, and CT sensors (10–15 sec sampling). More details can be found in Gong et al. (2023)."

In section 4, test the statistical significance of your differences. Especially in lines 310.

***Response*:** Thank you for highlighting the need for statistical rigor. We have now conducted independent two-sample t-tests (assuming unequal variances) to evaluate the significance of differences between in-situ observational wave properties and those in the numerical experiments (EXP. 1 – EXP. 5) in the revised manuscript.

Fig 8. Please add the legend in the panels b and d.

***Response*:** We have now added the legend in panels (b) and (d).

Fig 11: It is quite hard reading this figure. It would suggest to move to a table that summarize the averaged values should be much more readable. In particular, I recommend to separate the 15 first days and the 15 last days for your analysis.

***Response*:** We fully agree with the reviewer that the original Fig. 11 was a bit hard to read. The figure has been restructured into two columns: the left column displays biases for five ISW properties during the initial 15-day period, while the right column corresponds to the final 15 days. Additionally, we expanded Table 2 to summarize the root-mean-square deviations (RMSD) of these properties across both periods, enabling a clearer comparative analysis. Corresponding text revisions have been made to align with these updates. Thank you for this constructive feedback.