Response letter to the referee #3

This manuscript presents a realization of the MITgcm as a regional modeling tool. It is admirable to try to tackle a large geographical region such as the northern South China Sea. The model builds on an earlier iteration of modeling and thus I presume is already being used in one form or another.

I have used the MITgcm myself, and understand its utility in this context. My usual toolbox is more process study oriented and is typically high order. I am keenly aware that this cannot be the choice for the present authors. Nevertheless, the modelling presented in this manuscript requires more clarity.

***Response*:**

We would like to thank the referee for the careful reading and valuable comments. We entirely agree that the modelling presented in this manuscript requires more clarity. Therefore, 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.

The resolution is likely controlled by the need to resolve such a large area, but if the aim is to represent internal solitary-like waves, the large grid spacing needs justification. The reader should know how many points per wave a typical wave form, and how this compares to the standard in process studies. The resolution seems really low to me, but then I am one of those process study modelers I mentioned in my previous sentence.

***Response*:**

We sincerely thank the reviewer for raising this valuable comment regarding horizontal resolution and its implications for resolving internal solitary waves (ISWs). We fully agree that justifying grid spacing is essential for process-oriented studies, and we appreciate the opportunity to clarify this aspect.

In the original submission, the discussion of resolution sensitivity was omitted because the ISWFM-NSCS v1.0 framework (Gong et al., 2023, GMD) already evaluated the impacts of horizontal resolutions (Δx = 250 m, 500 m, and 1000 m) on internal wave reproductions. Key findings from that study are listed below:

(1) Δx =1000 m: Failed to accurately resolve key features of ISWs in the northern South China Sea (NSCS), such as waveform, maximum amplitude and characteristic half-width.

(2) Δx = 500 m: Captured the fundamental characteristics of ISWs (e.g., waveform, maximum amplitude, and wave-induced velocity) with reasonable precision, achieving approximately 6–8 grid points per characteristic half-width for typical ISWs in the SCS. This aligns well with standard practices in regional ISW modeling studies (e.g., Zhang et al., 2011; Lai et al., 20219), which often

employ 4–10 points per characteristic half-width depending on nonlinearity and nonhydrostatic effects.

(3) Δx = 250 m: Improved the accuracy of wave properties (e.g., a ~40% reduction in characteristic half-width bias compared to the Δx = 500 m case). However, this refinement increased computational costs by a factor of 5 for the same domain, posing significant challenges for operational forecasting timeliness.

In the current version (ISWNM-NSCS v2.0), our focus lies in optimizing initial/boundary conditions and turbulence configurations rather than further refining resolution. Nevertheless, we recognize the importance of contextualizing our chosen 500 m resolution. Accordingly, in the revised manuscript (Section 2, Lines 81–86), we have now explicitly stated the grid points per wavelength (6–8) for typical ISWs in the NSCS and compared this to established resolution standards in process studies (e.g., Zhang et al., 2011; Lai et al., 2019). Moreover, we now highlight the trade-off between computational efficiency and precision, emphasizing that 500 m strikes a balance for regional-scale forecasting while retaining dynamical precision, and reference the last version (Gong et al., 2023) for readers seeking detailed resolution sensitivity analyses.

We acknowledge that higher resolutions (e.g., 250 m) would benefit small-scale process studies, but our priority here is to advance the model's operational readiness for the SCS basin. In future work, it would be interesting to further explore adaptive mesh refinement to locally enhance resolution in critical areas (e.g., wave generation sites) without inflating unnecessary computational costs.

Similarly, the discussion of turbulence is a bit misleading. The authors quote a number of turbulence schemes applied at different scales and use this to justify constant eddy viscosity values in the vertical and horizontal. These values strike me as at least an order too high to me. The reader needs to know how a single ISW would be affected by these choices. Such model runs should take a day or so, and their results can be summarized in a table. These would provide an important counterpoint to the rather ambiguous validations provided in the present version.

***Response*:**

We sincerely appreciate the reviewer's insightful critique regarding our treatment of turbulence parameterization and the justification for constant eddy viscosity values. We acknowledge that further quantification of viscosity and diffusivity impacts on ISW strengthens model transparency, and we have conducted comprehensive sensitivity tests (EXPs. A1-A8, K1-K8) spanning four orders of magnitude (0.01× to 100× CTRL values from EXP. 1) to address this point.

In the EXPs. A1-A4, the horizontal viscosity coefficient (A_h) is ranging from 100×CTRL (1.0×

10^0 m^2 s^{-1}) to 0.01×CTRL (1.0×10^{-4} m^2 s^{-1}). As a result, the EXPs. A1-A4 reveal exceptional stability in four key ISW properties across different scaling factors. Specifically, the arrival time (0.64 h), baroclinic velocity (~0.40 m s^{-1}), and propagation direction (13.67°–13.74°) show negligible RMSD variations in EXP. A1–A4. Only maximum mode-1 amplitude exhibits mild degradation (i.e., 27.53–28.59 m vs. 26.51 m in CTRL), while characteristic half-width shows marginal improvement in EXP. A2 (i.e., 0.15 km vs. 0.17 km in CTRL). Conversely, the EXPs. A5-A8 show greater sensitivity for vertical eddy viscosity. In details, EXP. A5 significantly degrades propagation direction (16.05° vs. 13.74° in CTRL), and most sensitivity experiments for A_v worse reproduce maximum wave amplitudes. Although EXP. A6 slightly improves arrival time (0.58 h vs. 0.64 h in CTRL), no sensitivity experiments achieve >5% improvement across multiple ISW properties.

However, in the EXPs. K1-K4, modifications to horizontal diffusivity (K_h) yield mixed results. While arrival time and baroclinic velocity remain stable (±0.04 h and ±0.02 m s^{-1}), EXP. K3 (K_h=0.1×CTRL) substantially degrades maximum wave amplitude predictions (31.85 m vs. 26.51 m in CTRL). Characteristic half-width consistently worsens (0.18–0.22 km vs. 0.17 km in CTRL), though EXP. K4 slightly improves prediction of propagation direction (13.16° vs. 13.74° in CTRL). For vertical diffusivity (EXPs. K5-K8), extreme scaling causes pronounced effects. Specifically, although EXP. K5 (K_v=100×CTRL) shows a slight improvement in arrival time (0.55 h vs. 0.64 in CTRL), it significantly degrades four other wave properties simultaneously, namely maximum amplitude (31.92 m vs. 26.51 m in CTRL), baroclinic velocity (0.45 m s^{-1} vs. 0.39 m s^{-1} in CTRL), propagation direction (15.26° vs. 13.74° in CTRL), and characteristic half-width (0.28 km vs. 0.17 km in CTRL). Conversely, EXP. K7 (K_v=0.1×CTRL) improves maximum amplitude (27.10 m vs. 26.51 m in CTRL) and baroclinic velocity (0.38 m s^{-1} vs. 0.39 m s^{-1} in CTRL), but this is offset by half-width degradation (0.21 km vs. 0.17 km in CTRL).

Overall, no sensitivity experiment outperforms CTRL across all five ISW properties, but only isolated cases (e.g., arrival time in the EXP. A6) show >5% improvement in single metrics. The CTRL run maintains the most balanced performance, with all RMSDs within intermediate ranges. The fluctuations in ISW properties across all 16 sensitivity experiments confirm that viscosity and diffusivity configurations appear robust in the CTRL run (EXP. 1).

While not a theory paper, what is presented on ISWs is pretty dodgy. Gear and Grimshaw is a very old paper, the results of which have been superseded by other (often open source) tools. There are even monographs on the theory which would provide a more modern link to discussion, literature and codes.

***Response*:**

We sincerely thank the reviewer for this valuable comment regarding the theoretical foundation of our ISW discussion. We fully acknowledge that the original citation of Gear and Grimshaw (1983) represented an outdated reference point that failed to reflect contemporary advances in solitary wave theory. In this revision, we have carefully restructured the fundamental introduction and theoretical framework throughout the manuscript, eliminating obsolete citations and integrating modern developments. The revised text now builds upon seminal treatments from Grimshaw et al. (2010) for extended KdV theory and Simmons et al. (2010) for non-hydrostatic solvers. This restructuring provides deeper physical context for wave evolution dynamics relevant to the ISW simulations in the NSCS.

Furthermore, we have strengthened connections to modern computational tools by explicitly contrasting our MITgcm implementation against recently established community models such as SUNTANS (Zhang et al., 2011) and FVCOM (Lai et al., 2019). These enhancements appear primarily in Section 2.1 (Lines 26-38), where we now demonstrate how our approach aligns with contemporary numerical standards while addressing the specific numerical challenges in the South China Sea. We believe these revisions establish robust theoretical foundations while maintaining appropriate scope for an applied modeling study focused on operational forecasting.

The discussion needs to be cleaned up as some parts read very strangely. There are various tools the authors can use for this. In terms of content, the aforementioned turbulence models are presented as interchangeable when as a point of fact, they are designed for very different things (i.e. Mellor-Yamada versus Gent-McWilliams). There are also strange statements about the source of turbulence that seem at odds with my understanding of ocean physics.

***Response*:**

We sincerely appreciate the reviewer's insightful critique regarding the clarity and physical foundations of our turbulence parameterization discussion. We acknowledge that the original text contained ambiguous phrasing that inadvertently conflated distinct turbulence closure approaches while oversimplifying the physical mechanisms driving oceanic mixing. In the revised manuscript, we have undertaken comprehensive revisions in Sections 2.1 and 4.1 to rectify these issues through precise terminology and rigorous physical justification. The revised manuscript now explicitly differentiates between tracer diffusivity and momentum viscosity while clarifying their distinct physical origins. Moreover, detailed discussion on turbulence configuration is now added in Section 5.

Specifically, we emphasize that horizontal eddy viscosity (A_h =1.0×10^{-2} m^2 s^{-1}) parameterizes unresolved lateral dissipation from inertial ranges and mesoscale processes (Smagorinsky, 1963), while vertical eddy viscosity (A_v =1.0×10^{-2} m^2 s^{-1}) represents turbulence from shear instabilities and internal wave breaking. This configuration aligns with implementations for marginal China Seas using

MITgcm (Min et al., 2023; Vlasenko et al., 2018). To further validate our viscosity selections, a series of sensitivity experiments in Section 5.1 quantitatively demonstrate that the chosen values optimally preserve ISW characteristics relative to mooring observations. Moreover, vertical diffusivity in ISWFM-NSCS v2.0 is set to 1.0×10^{-5} $m^2 s^{-1}$ (Fig. 1), consistent with microstructure measurements of background diapycnal mixing in summer in the northern South China Sea (Shang et al., 2017). Horizontal tracer diffusivity adopts 1.0×10^{-2} m^2s^{-1}, following established subgrid-scale parameterizations for mesoscale-resolving models (Large et al., 1994; Griffies, 2004).

Again, we are grateful for this critique, which has significantly strengthened the methodological rigor of our turbulence treatment.

Finally, I was left wondering how the present methodology compares and contrasts with well-established models like Getm-Gotm when applied to something like the Baltic Sea. The two tools are different in purpose, but it would help the context to contrast them.

***Response*:**

We sincerely appreciate the reviewer's insightful comment regarding the comparison of ISWFM-NSCS with established models like GETM-GOTM (Burchard et al., 2004). While both tools are valuable for oceanographic studies, their design and applicability differ significantly due to distinct scientific objectives and regional dynamics.

GETM-GOTM is a well-validated modeling system tailored for estuarine, coastal, and shelf seas (e.g., the Baltic Sea, North Sea, and Wadden Sea), excelling in resolving baroclinic processes, tidal dynamics, sediment transport, and turbulence closure in shallow, stratified systems (Stips et al., 2004, 2008; Tiessen et al., 2012). Its capabilities in simulating wetting-drying cycles, biogeochemical interactions, and high-resolution coastal processes (e.g., dense bottom currents in the Western Baltic Sea or suspended matter dynamics in the Wadden Sea) make it a powerful tool for coastal regions with complex bathymetry and strong anthropogenic influences.

In contrast, ISWNM-NSCS (built on MITgcm) focuses on deep-water ISW dynamics, particularly in semi-enclosed basins like the South China Sea (SCS). To reproduce large-amplitude features of ISWs propagating over abyssal depths (>2000 m) in the SCS, non-hydrostatic terms are required in the framework. However, GETM-GOTM employs hydrostatic and Boussinesq approximations suitable for shallow systems. We agree that applying ISWNM to the Baltic Sea (a shallow, tidally dominated system) would likely underperform compared to GETM-GOTM, as the latter's explicit treatment of barotropic-baroclinic splitting, wetting-drying, and turbulence closures better aligns with Baltic Sea dynamics. Conversely, ISWNM-NSCS is optimized for deep basins where internal wave energetics dominate over tidal-driven mixing. To clarify this distinction, we have expanded our discussion in the revised manuscript (Section 5.3) to contrast the two models' strengths, limitations, and regional suitability.

We regard this comparison as a constructive pathway for future work, particularly in exploring hybrid approaches or cross-validation across diverse ocean regimes.