Review of "Conditional Modulation of Extreme Internal Solitary Waves in the Lombok Strait" by Wang et al.

This manuscript presents a valuable 13-month mooring dataset from the Lombok Strait, identifying 302 ISW packets and 618 individual solitons with amplitudes up to 104 m. The finding that extreme ISWs (>65 m) are associated with episodic ITF weakening rather than stratification strength is an interesting contribution to the field. However, the manuscript in its current form has several significant issues that need to be addressed before publication can be recommended. The authors have an excellent dataset, but the analysis and presentation do not yet fully leverage its potential. Therefore, a major revision is recommended.

Major Comments:

1. Geographic and mooring location context. The manuscript lacks a geographic focus figure showing the mooring location, bathymetry of the Lombok Strait, the sill region, and the surrounding Indonesian archipelago. This is a fundamental omission—readers cannot properly assess the observational context. The mooring location appears in the text, but without a map, the connection to the generation sites and the broader ITF pathway is lost. I recommend adding Figure 1 as a location map with bathymetric contours, the mooring marked, and ideally the strait's sill indicated.

2. What is genuinely new beyond satellite observations? The authors correctly note that previous studies (Matthews et al., 2011) have already established the seasonal cycle of ISW occurrence in the Lombok Strait from satellite remote sensing—peaking in boreal winter/spring and minimized in summer. While the 13-month continuous in situ record is impressive and valuable, the manuscript's novelty should not rest primarily on confirming this known seasonality. The authors need to more forcefully articulate and demonstrate what the mooring data reveal that satellite data cannot. For instance:

Northward- and southward-propagating ISWs are generated at the northern and southern sills (Aiki et al., 2011). Are their responses to ITF variability different? Does the ITF suppress southward-propagating waves differently than northward-propagating ones? The current analysis appears to treat all waves as a single population; separating them by propagation direction would be a significant advance.

Does the mooring capture mode-2 or higher-mode internal waves, or mode-conversion processes during propagation? The thermistor chain has sufficient vertical resolution to potentially identify higher-mode signals. Are there signatures of modal decomposition or transition?

Are solitary waves and wave packets generated with regular periodicity tied to the tidal cycle (e.g., spring-neap modulation), or is their generation more stochastic? The automated detection catalog (302 packets) could be used to examine the phase relationship with the tidal forcing—this would directly address whether generation is deterministic or modulated by background flow.

3. Effective phase speed calculation. The authors define effective phase speed as $c_{eff} = c_0 + V_{100\text{-}250}$, where c_0 is computed from a two-layer model. Given that the mooring has CTD and thermistor data providing continuous vertical profiles, why is the two-layer approximation used rather than a continuously stratified formulation? The Lombok Strait has a complex, multi-layered stratification, and the two-layer model may significantly misrepresent the vertical structure of mode-1 phase speed, especially when the thermocline deepens to ~168 m as observed in late 2024. Moreover, simply adding the mean thermocline velocity as a Doppler shift ($c_0 + V$) implicitly assumes that the background shear does not modify the wave's vertical structure. This is a strong assumption. A more rigorous approach would involve solving the Taylor-Goldstein equation for the stratified shear flow to obtain the intrinsic phase speed and the effect of background shear on wave stability. The authors should either justify the two-layer simplification with a sensitivity analysis comparing against continuously stratified solutions (using the observed density profiles), or adopt a more sophisticated method.

4. Event-scale correlation analysis (Figure 4). The authors present scatter plots of ISW amplitude versus four environmental parameters and claim significant correlations for c_eff ($r = +0.20$, $p < 0.001$) and V100-250 ($r = +0.21$, $p < 0.001$). However, the visual impression from Figure 4 is that the relationships are weak. For extreme ISWs (>65 m, stars), the sample size is only 26 events; the statistical significance is driven largely by the full dataset of 302 events, and the correlation coefficients explain only ~4–5% of the variance. Moreover, the linear fits shown in Figure 4 may not be appropriate—there is no physical reason to expect a linear relationship between amplitude and ITF velocity. The authors themselves describe a threshold behavior (no extreme ISWs when $V < -0.2$ m/s), which is better represented as a conditional probability or threshold analysis rather than a linear regression.

5. Separation of generation vs. propagation effects. The manuscript infers that the background flow compensation mechanism "reduces Doppler retardation" and thus "opens dynamic windows for nonlinear wave growth". However, the mooring is located north of the sill, so the observed waves have already been generated and have propagated some distance. The observed correlation between amplitude and background flow could equally reflect: (a) modification of the generation process at the sill (hydraulic control); (b) enhanced nonlinear steepening during propagation when the Doppler retardation is reduced; or (c) both. The authors discuss hydraulic control but do not attempt to distinguish these mechanisms. With a single mooring, this is admittedly difficult, but the authors should at least acknowledge this limitation explicitly and discuss what additional observations (e.g., a mooring array) would be needed to separate these effects.

6. Amplitude classification thresholds. The manuscript divides ISWs into three amplitude classes: small (25–45 m), moderate (45–65 m), and extreme (>65 m), with the 65 m threshold described as "identified by a natural break in the amplitude histogram (Supplementary Fig. S1)." The authors mention that varying the threshold between 60–70 m does not alter results. However, the basis for the 25 m lower bound is not explained—why exclude events <25 m? Is this a detection limit of the thermistor resolution, or a dynamical choice? If there are genuine ISWs with amplitudes <25 m, their exclusion may bias the seasonal statistics. Please clarify and, if possible, show sensitivity to the lower threshold as well. Additionally, the 65 m "natural break" should be shown or described more clearly in the main text, not just relegated to a supplementary figure.

7. Figure quality and readability. All figures in the current version have font sizes that are far too small for comfortable reading. Axis labels, tick marks, legend entries, and colorbar annotations are nearly illegible. Given that the figures are central to conveying the manuscript's key findings, this needs immediate attention. Please increase font sizes substantially and ensure figures are readable when printed at standard journal column width.

8. Missing context for ITF variability. The manuscript uses a single 13-month record and interprets the thermocline deepening from December 2023 to December 2024 partly as a seasonal signal and partly as an "interannual deepening trend". However, without a longer baseline or comparison to climatology (e.g., from GLORYS), it is impossible to assess whether 2023–2024 was a typical year or anomalous. The authors should compare their observed isotherm depths and ITF velocities to the long-term mean or at least to the 2023–2024 ENSO/IOD phase to provide context for the representativeness of this single year. The ITF is known to vary significantly with ENSO and IOD; was 2024 a neutral, El Niño, or La Niña year? This context is essential for interpreting the generality of the findings.