**Manuscript Number: EGUSPHERE-2026-1351**

**Title: Energetic near-inertial waves induced by winter storms and mesoscale eddies in the subtropical Northwestern Pacific Ocean**

August 12, 2026

FROM: Hongkai Wang, Zifei Chen, Xinyuan Diao, Fei Yu, Xingchuan Liu, Qiang Ren, Feng Nan

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Dear Reviewer,

Thank you very much for taking the time to review our manuscript. Your suggestions have helped us improve the clarity of the presentation, strengthen the methodological description, and refine the interpretation of the near-inertial energy evolution.

We have carefully considered each comment and revised the manuscript accordingly. Our point-by-point responses are provided below. All changes made in response to the comments are highlighted in blue in the revised manuscript. We hope that the responses and revisions can adequately address your concerns.

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**Response to Reviewer's Comment**

***This manuscript contributes to our understanding of atmosphere-ocean energy transfer, a key topic in physical oceanography, by investigating how energy is transferred from the atmosphere into the ocean interior through near-inertial waves and how this process is modulated by mesoscale eddies. Using data observations together with reanalysis products, the authors compare two winter storm events that differ substantially in their wind forcing and NIW characteristics, while exhibiting similar observed near-inertial kinetic energy levels. The study suggests that a combination of eddy-wave interactions and differences in the modal structure of the wind energy input may explain the contrasting behaviour of the two events.***

***The manuscript is generally well written, flows nicely, with appropriate citations, calculations, and figures to support the reasoning - I appreciate that you are not using a color palette that is difficult to read for colorblind readers. It represents a valid***

***contribution to the field, and the observational dataset is valuable. However, I have a few comments that I believe would help strengthen the interpretation and improve the robustness of some of the conclusions.***

***I'd like to note that I carefully read the manuscript both in its original form and in the revised version addressing Reviewer 1's suggestions, and in the first reading my main concerns were quite similar to those of Reviewer 1. I therefore really appreciated the revisions that were made, and I took this into account in my part of the review – page numbers are referred to the revised version of 12 July.***

**Response：**

We sincerely thank the reviewer for carefully evaluating both the original and revised versions of the manuscript. The detailed and constructive comments have helped us further improve both the presentation and physical interpretation of the results. In response, we have added a brief description of the regional circulation surrounding the mooring site. We also have revised the Abstract, Discussion and Summary to avoid attributing the observed near-inertial energy intensity to a single mechanism. The revised manuscript now explicitly acknowledges that wind forcing, mesoscale eddy-wave energy exchange, near-inertial energy radiation, and turbulent dissipation may all contribute to the observed differences in NIKE between the two events

**1.Major**

***On Major #1, already addressed: From my point of view, Reviewer 1's suggestion is sufficiently addressed in the introduction (though that is ultimately for them to judge), but a brief description of the physical setting of the mooring site would also be helpful. The manuscript focuses on the interaction between NIWs and mesoscale eddies, yet the reader is given little context regarding the regional circulation of the study area. Even a brief discussion or an appropriate reference around lines 69–70 could help provide this context.***

**Response：**

Thank you very much for this helpful suggestion. We agree that regional circulation surrounding the mooring site was not sufficiently descripted in previous version. The mooring at 25ºN, 146ºE is located near the northern flank of the North Pacific Subtropical Countercurrent (STCC) band. In this region, the shallow eastward STCC overlies a subsurface westward flow (Fig. R1 from Qiu and Chen, 2009), forming a vertically sheared current system that is favorable for baroclinic instability and contributes to the energetic mesoscale eddy field. We have added this regional circulation context and relevant references to the Introduction.

**The revised text is as follows (Lines 72-75):**

The mooring site at 25°N, 146°E is located near the northern flank of the North Pacific Subtropical Countercurrent (STCC) band. In this region, the shallow eastward-flowing STCC overlies the subsurface westward North Equatorial Current, forming a vertically sheared current system that is favorable for baroclinic instability and contributes to energetic mesoscale eddy field (Qiu, 1999; Qiu and Chen, 2010).

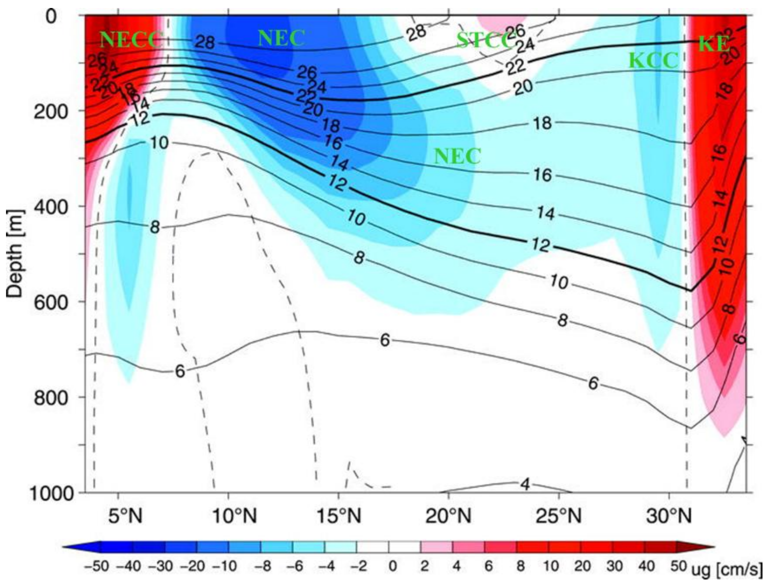


Fig. R1 from Qiu and Chen (2009). Latitude-depth section of temperature (solid contours) and zonal geostrophic velocity (color shading) along 137ºE. The geostrophic velocity is referenced to 1000 dbar and dashed lines denote the zero velocity contours. The acronyms NECC, NEC, STCC, KCC, and KE represent North Equatorial Countercurrent, North Equatorial Current, Subtropical Countercurrent, Kuroshio Countercurrent, and Kuroshio Extension.

***On Major #2, already addressed: The first version of the manuscript attributes the observed energy differences primarily to mesoscale eddy-wave energy exchange.***

***While this interpretation is plausible and supported by the diagnosed transfer rate P, other possible sinks and sources of NIW energy (e.g., turbulent dissipation, wave-wave interactions, and energy propagation out of the observed volume) were not considered in the first version, as already pointed out by Reviewer 1. I therefore appreciate the additional discussion of turbulent dissipation and wave radiation introduced in response to Reviewer 1. These additions considerably strengthen the interpretation of the NIW energy budget. Nevertheless, given the observational limitations, I would still encourage the authors to maintain caution when attributing the observed energy differences primarily to eddy-wave interactions. Explicitly acknowledging that multiple mechanisms may contribute to the observed behaviour would both strengthen the interpretation and help motivate future observational studies.***

***Another way to strengthen the discussion could be the following: as currently written, the manuscript still tends to lead the reader toward an eddy-centered interpretation of the observed differences between the two NIW events. Given the observational limitations, and the fact that other terms of the NIW energy budget cannot be quantified directly, the conclusions may benefit from a more explicit discussion of multiple contributing mechanisms. In particular, the modal projection of wind-generated near-inertial energy appears to provide an additional, and potentially complementary, explanation for the differences observed between the two events. Emphasizing the joint roles of wind forcing and eddy modulation would, in my view, result in a more robust interpretation of the observations.***

**Response：**

Thank you very much for this important and constructive comment. We agree that the previous version still placed excessive emphasis on an eddy-centered distribution of the contrast between the two NIW events. We have therefore revised the Abstract, Section 4.1-4.3, and the Summary to provide a more cautious interpretation.

In the revised manuscript, *P* is treated as a diagnosed local energy-conversion term between mesoscale eddies and the near-inertial motions, rather than as a complete NIW energy budget. The contrasting energy transfer rate between mesoscale eddies and near-inertial motions during WS1 and WS2 may contribute to the comparable NIKE observed during the two events. However, we do not interpret this process as the sole mechanism responsible for the comparable observed NIKE. We now explicitly acknowledge that the terms of near-inertial energy radiation out of the observed region and energy dissipation may also contribute to the comparable observed NIKE and cannot be fully quantified using the single mooring observations. **(Lines: 18-20, 303-304, 313, 384-386, 392-394)**

**2.Minor**

***(1) 2.1. Data: Adding the mooring line is a great way to help the reader visualize it, but I still suggest a bit more precision in the text — e.g., the depth of the mooring, the positioning of the two ADCPs (I assume one up-looking and one down-looking), and how the raw data from both the ADCPs and the CTDs were processed in the first place.***

**Response：**

Thank you very much. We agree that the previous description of the mooring configuration and observational preprocessing was not sufficiently detailed. We have therefore revised both Fig. 1b and Section 2.1 to provide the deployment depths, sampling ranges, and processing procedures of the raw data.

The mooring was deployed at 25ºN, 146ºE, with a depth of 5692 m. The upward-looking and downward-looking Teledyne RDI Workhorse Long Ranger 75-kHz ADCPs were mounted at 400 m depth, and jointly provided valid velocity measurements in the upper 900 m depth.

The raw ADCP data were subjected to quality control procedures including removal of bins affected by sidelobe contamination, screening based on correlation, percent good, velocity error, and removal of velocity spikes. The SBE 37-SM conductivity, temperature, and pressure records and the SBE 56 temperature records were processed using the manufacturer calibration coefficients and subsequently checked for spikes and physically unrealistic values. We have added this information to Section 2.1 of the revised manuscript. **(Lines 96-104)**

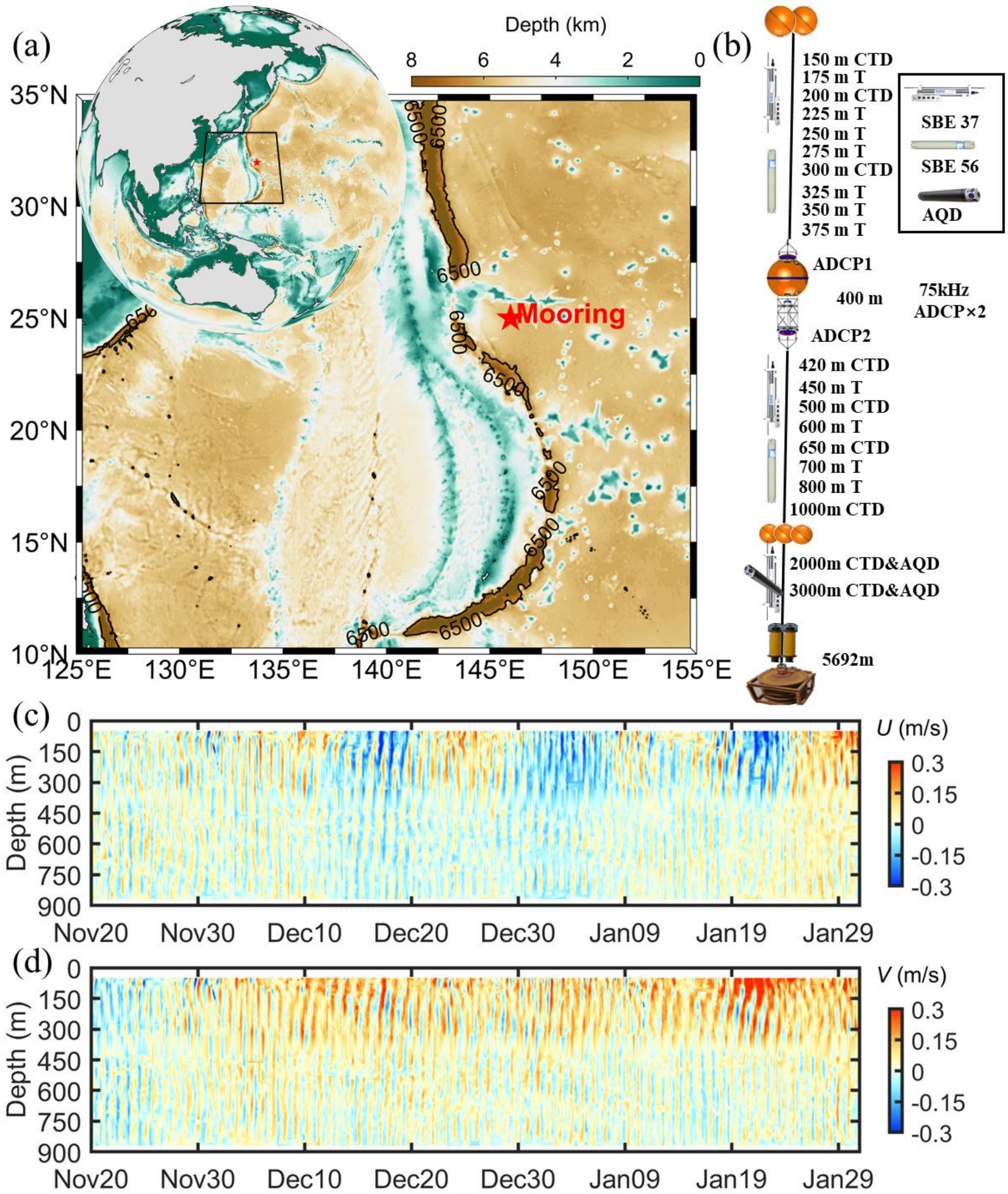


Figure 1 in the revised manuscript. (a) Location of the subsurface mooring (marked by a red pentagram), with color shading representing the topography. (b) Deployment depths of instruments on the subsurface mooring. (c) and (d) Depth-time plot of zonal and meridional velocities from 20 November 2017 to 31 January 2018.

***(2) Line 111: In response to Reviewer 1 comments on lines 99-103, you state here that MLD is calculated as the depth where temperature is 0.5°C lower than the SST, and in Supplement material you add more on the reproducibility of the variables. The agreement between reanalysis and Argo-derived MLD also appears satisfactory, nevertheless the manuscript does not clearly state the criterion used to define the***

***MLD. Given the relevance of MLD for the slab-model calculations, the subsequent interpretation of the NIW characteristics, and since temperature-based and density-based criteria may differ substantially, the adopted definition should be explicitly stated and justified.***

**Response：**

Thank you very much. We agree that the criterion used to define the MLD directly affects the slab-model response. In this study, the MLD is defined as the first depth at which temperature is 0.5°C lower than the sea surface temperature. The criterion draws on the research by Watanabe and Hibiya (2002), who applied it in their study of the slab model and storm-induced NIWs. **(Lines: 121-122)**

***(3) Results, line 172: Maybe "observed" instead of "suggested"?***

**Response：**

Thank you very much. We have revised it accordingly. **(Line 185)**

***(4) Line 194–198: The use of one-tenth of the maximum NIKE as a criterion for both propagation depth and decay time is still not entirely clear to me. Is this threshold adopted from previous studies, or was it chosen specifically for this analysis? If it is an empirical criterion introduced by the authors, I would suggest briefly stating so and providing a short justification for its use.***

**Response：**

Thank you very much for this helpful comment. We agree that the previous statement did not clarify the use of one-tenth of the maximum NIKE as a criterion for both propagation depth and decay time.

The threshold is taken from the study by Johnston et al. (2021), in which they used it to analyze the decay of storm-generated NIWs and the propagation depth into the ocean. We have clarified this in the revised manuscript. **(Lines: 207-208)**

***(5) Figure 6: in the revised version the x-axis label appears to be missing.***

**Response：**

Thank you very much for pointing out this omission. We have added the x-axis label to Fig. 6 in the revised manuscript.

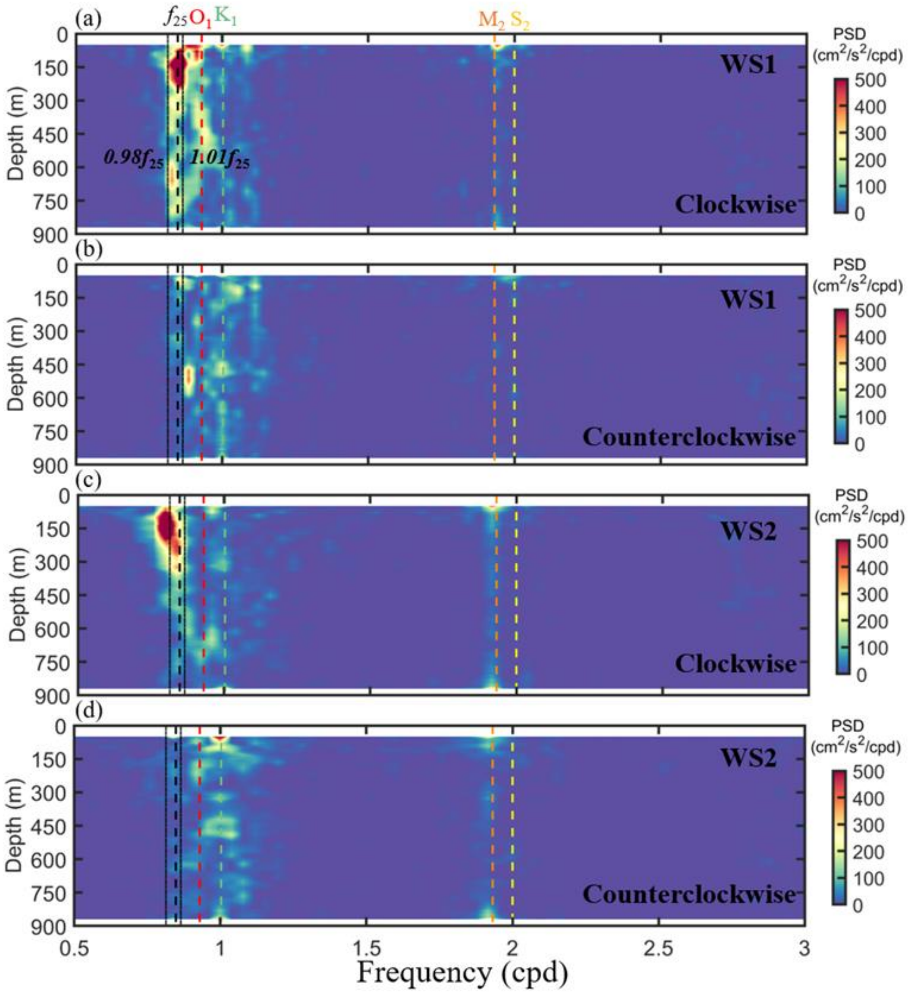


Figure 6. Rotary frequency spectra of raw velocities observed from ADCPs during WS1 (a and b) and WS2 (c and d). Clockwise and anticlockwise polarizations of velocities are shown in (a-d). The vertical dashed lines in panels represent the local inertial frequency, $f_0$, 0.98 $f_0$, 1.01 $f_0$, diurnal tidal frequency, O1 and K1, and semidiurnal tidal frequency, M2 and S2, respectively.

***(6) 4.1. paragraph, general consideration, with the Major #2 discussion in mind: the central conclusion here relies on the diagnosed energy transfer rate P. Therefore, the manuscript would benefit from a clearer description of the processing the variables entering the calculation have been submitted to, even if some details have already***

***been added in 2.3. For example, I wonder whether the filtering procedure applied to the horizontal velocity fields, namely a 5-day temporal low-pass filter, could affect the interpretation of the shorter event (WS2), which is only roughly twice as long as the filter window. A brief discussion of the robustness of the sign and magnitude of P with respect to the chosen filtering and averaging parameters could help clarify this point.***

**Response：**

Thank you very much for this insightful comment. According to your suggestion, we have expanded Section 2.3 to describe more clearly the processing procedures applied to the variables used to calculate *P*. **(Lines: 149-155)**

We also conducted sensitivity experiments using background-flow low-pass periods of 3, 5, and 7 days and averaging windows of 2, 3, and 4 inertial periods. As shown in Fig. R2(a), the temporal variation of *P* obtained using three low-pass filter periods exhibits a consistent pattern. Fig. R2(b) shows the differences in *P* obtained using three averaging windows. Although the *P* values vary somewhat, the event-mean magnitude and sign remain comparable and consistent. These results indicate that the inferred direction of the diagnosed energy conversion is robust to the selected filtering and averaging parameters. These results have also been added to the revised Supporting Information.

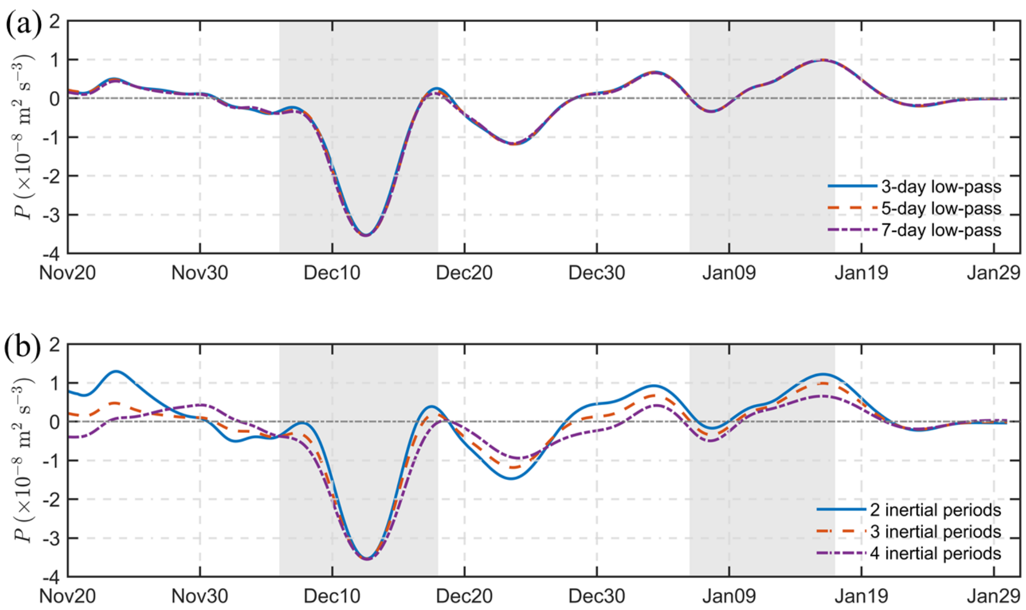

Figure R2. (a) *P* calculated using 3-, 5-, and 7-day temporal low-pass filters for the background horizontal velocity fields. (b) *P* calculated using averaging windows of 2, 3, and 4 inertial periods. The grey shading indicates the WS1 and WS2 periods.

***(7) In line 275, significant energy exchange is reported to occur primarily within the upper 200 m. However, the vertically integrated P is computed over the upper 300 m, while the profiles shown in Figure 9 extend to 400 m depth. A brief explanation for these choices would help the reader understand the rationale behind the selected integration and display ranges.***

**Response：**

Thank you very much for this helpful comment. We agree that the rationale for the selected integration and display depths should be clarified. Although significant energy exchange is concentrated primarily within the upper 200 m, the enhanced NIKE associated with both NIW events is mainly distributed within the upper 300 m, as shown in Fig. 5(c) in the revised manuscript. We therefore vertically integrate *P* over the upper 300 m.

The vertical profiles of *P* in Figs. 9(c) and 9(d) are displayed down to 400 m to show the vertical decay of the energy exchange between mesoscale eddies and NIWs and to demonstrate that *P* becomes weak below the selected 300 m integration depth. We have clarified the rationale for the 300 m integration depth in Section 4.1 of the revised manuscript. **(Lines 293-294)**

**Reference:**

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