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

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

Dear Editor, Dr. Sloyan

Thank you very much for your letter and for taking the time to review our manuscript. We also thank the reviewers for their constructive comments and suggestions, which have helped us improve the manuscript. We have carefully considered their comments and revised the manuscript accordingly.

Our point-by-point responses to the reviewers' comments are provided below. For easy of reference, revisions are highlighted in blue in the revised manuscript.

We sincerely hope that these responses and revisions satisfactorily address the reviewer's concern. Thank you for your time and consideration!

Sincerely,

Hongkai Wang

On behalf of all co-authors

Response to reviewer

(1) Line 123-125: I appreciate the authors for presenting the figures comparing the reanalysis data with the observation. These figures are helpful for readers to see the consistency between the reanalysis data and the observation. But, compared to the consistency of the potential density and N_2 profiles, the background horizontal velocities in the reanalysis data seem to be generally overestimated. I am concerned that these overestimates affect the subsequent estimation of several parameters (such as relative vorticity, S_s , and S_n). I think the authors need to discuss (or at least mention) the uncertainties that could be caused by the limitations of the reproducibility in reanalysis data.

Response:

Thank you very much for this valuable comment. We agree that uncertainties in the background velocity from reanalysis data directly affect the calculated results for relative vorticity, strain components, and the energy transfer between mesoscale eddies and NIWs.

Therefore, in the revised manuscript, we have acknowledged that the overestimation of background horizontal velocity in reanalysis data may introduce uncertainties in the calculation of strains and the eddy-NIW energy transfer. **(Lines: 305-308)**

(2) Line 138: Does this ρ_o represent the density (not the reference density)? In other parts of this manuscript, the density is simply referred to as ρ .

Response:

Thank you very much. ρ_0 represent the reference density. This is a typo, we have corrected it in the manuscript.

(3) Line 135: For ϕ_n , italicized and non-italicized forms appear mixed together. Please consider using consistent representation.

Response:

Thank you. We have corrected it and used a consistent italic form in the revised manuscript.

(4) Line 164: Is the velocity vector V in Equation (7) the same as (U,V) in line 149? (Is V also processed with a 5-day low-pass filter?) Please clarify it.

Response:

Thank you very much. Both the velocity vector V and the horizontal velocity fields (U, V) in Eq. (4) represent the background flow, but they are estimated from different datasets. The U and V in Eq. (4) are obtained from the reanalysis product, whereas velocity vector V in the ellipse analysis is obtained from the mooring ADCP observations. Both data were processed using a 5-day low-pass filter. We have clarified

this distinction in the revised manuscript. (Lines: 150-151)

(5) Line 165: Thank you for removing the absolute value signs as below: “ $\sim V$ and θ represent the velocity vector and direction of the mean current.” But, regarding Eq. 7, wouldn't it be correct to leave it as it was in the previous manuscript, as shown below? “ $\omega_i = \omega_o - |k_h| |V| \cos(\Phi - \theta)$ ”

Response:

Thank you for pointing this out. We agree with you. We have made the correction in the revised manuscript. (Line: 164)

(6) Line 166: Equations 8 and 9 are given by combining Eq.5 and Eq. 7 (not Eq. 6). Also, please use a consistent notation for equation numbers. For example, equation numbers are represented such as ‘Eq. X’ here, but they are represented as ‘Eq. (X)’ in line 241.

Response:

Thank you very much for pointing out these errors. We have corrected them in the revised manuscript. (Line: 166, 242, 243)

(7) Line 208: I appreciate for adding a new figure and explanation to describe the method for estimating the decay times. However, it is still unclear to me. Is the time it takes for a wave to propagate downward along the propagation path calculated based on the group velocity presented in Table 1? If so, I think it would be easier for readers to understand if the discussion regarding the decay time were presented after Subsection 3.3.

Response:

Thank you very much for this helpful comment. According to Johnston et al., 2021, the decay time is defined as the time required for the near-inertial energy to decrease from its maximum value to one-tenth of that value along the downward propagation path. We have clarified it in the revised manuscript. (Line: 210-211)

(8) Line 284-285: Please rephrase this sentence. In the present study, across both WS1 and WS2, negative P – the energy conversion from low-frequency flows to NIWs – corresponds to a positive OW parameter, which is the opposite of the condition of P in Jing et al. (2018). The results of this study should be accounted for by a mechanism different from the mechanism proposed by Jing et al. (2018).

Response:

Thank you very much for this comment. We apologize for the ambiguity in our original description.

The positive P indicates an energy transfer from mesoscale eddies to NIWs, and vice versa. Therefore, the negative P during WS1 suggests an energy transfer from NIWs to mesoscale eddies.

As reported by Jing et al. (2018), pronounced energy exchange between background flows and NIWs occurs primarily when the OW parameter is positive. Similarly, in our study, large magnitude of P occurs mainly during periods of positive OW (Fig. 9b in the revised manuscript). Thus, our results are consistent with the mechanism proposed by Jing et al. (2018).

(9) Line 297-298: Please consider rephrasing this sentence. Maybe ‘the remaining near-inertial energy’ instead of ‘the total near-inertial energy input’?

Response:

Thank you very much. According to this suggestion, we have corrected it in the revised manuscript. **(Lines: 298-299)**

(10) Line 299-301: Is the time-integrated energy here calculated as 25% of the total wind energy, same as the mean value calculated in the previous paragraph? Please clarify it. I also wonder why the time-integrated energy flux for WS1 has a minus value (-2.9 kJ/m²).

Response:

Thank you for pointing this out. The negative sign was a typo. The time-integrated energy flux during WS1 is 2.9 kJ/m². We have corrected it in the revised manuscript.
(Lines 301-302)

Yes, the time-integrated wind-generated near-inertial energy flux was calculated as 25% of the total wind energy, same as the mean value calculated in the previous paragraph.

(11) Line 301: Also, please check whether the values and positive/negative signs of the energy exchange between mesoscale eddies and NIWs are correct. I think the values for WS1 should have a minus sign (-1.3 kJ/m²).

Response:

Thank you very much for point this out. We agree with you that the values for WS1 have a minus sign. This is a typo. We have corrected it in the revised manuscript.
(Line: 302)

(12) Line 315: Why is Kunze et al. (1985) cited as a reference for the introduction to the GHP parameterization? Kunze et al. (1985) used data from microstructure measurements rather than finescale parameterization.

Please provide more detailed information on the estimation methods for the GHP parameterization in the Supplementary Information. While the formulation of the GHP parameterization is generally the same across most previous studies, detailed choices of some parameters for the estimation (e.g., the length of bin for calculation of the shear/strain variance, the integration intervals of spectra, the determination of noise spectra, etc...) differ among several studies (e.g., Kunze et al., 2006; Polzin et al. 2014). Or, if this manuscript follows exactly the same methodology as some previous study, please mention so. Such detailed methodological information is also useful when discussing the limitations of the finescale parameterizations.

Response:

Thank you very much for this constructive comment. We agree with your suggestion that the reference to Kunze et al. (1985) should be replaced with Kunze et al. (2006). **(Line: 321)**

In the GHP fine-scale parameterization, the vertical shear components u_z and v_z were obtained from the observed horizontal velocity. The shear profiles were interpolated onto a uniform vertical grid with a vertical resolution of 10 m and then divided into 240-m-long segments with a half overlap. Within each segment, the segment mean buoyancy frequency was calculated as

$$\overline{N} = \left\langle \sqrt{N^2} \right\rangle. \quad (S1)$$

A linear fit was removed independently from both the zonal and meridional shear profiles to remove the large-scale background vertical structure and isolate the finescale shear component. The detrended shear components were then normalized by the segment-mean buoyancy frequency as,

$$u_z^* = \frac{u_z - \overline{u_z}}{\overline{N}}, \quad v_z^* = \frac{v_z - \overline{v_z}}{\overline{N}}, \quad (S2)$$

where $\overline{u_z}$ and $\overline{v_z}$ denote the fitted linear trends within each segment. This treatment follows the finescale-processing procedure used in previous GHP parameterization, in which the large-scale vertical structure is removed before spectral analysis (Kunze et al., 2006).

Before spectral estimation, a 10% $\sin^2$ taper was applied to both ends of each segment to reduce spectral leakage. The buoyancy-normalized shear spectra were then estimated using Welch's method. Following Kunze et al. (2006), the normalized shear spectrum was

$$S\left[\frac{V_z}{N}\right](k_z) = \frac{k_z^2 [U_r^2(k_z) + U_i^2(k_z) + V_r^2(k_z) + V_i^2(k_z)]}{\overline{N}^2} S_{\text{correc}}(k_z), \quad (S3)$$

where k_z is the vertical wavenumber, U_r and U_i are the real and imaginary Fourier coefficients of u_z^* , V_r and V_i are those of v_z^* , and S_{correc} represents the spectral correction for attenuation at high vertical wavenumbers.

The normalized shear variance was obtained by integrating the corrected shear spectrum over vertical wavelengths of 60–180 m:

$$\left\langle \frac{V_z^2}{N^2} \right\rangle_{\text{obs}} = \int_{\frac{2\pi}{180}}^{\frac{2\pi}{60}} S\left[\frac{V_z}{N}\right](k_z) dk_z. \quad (\text{S4})$$

The wavelength range of 60–180 m was selected to isolate the finescale internal-wave band while limiting contamination from both lower-wavenumber background structure and higher-wavenumber noise (Kunze et al., 2006; Sheen et al., 2013; Waterman et al., 2013; Takahashi and Hibiya, 2021).

To normalize the observed shear variance, the corresponding GM reference shear spectrum was

$$S_{\text{GM}}(k_z) = \frac{3\pi E_0 b j_*}{2} \frac{k_z^2}{(k_z + k_*)^2}, \quad (\text{S5})$$

where

$$k_* = \frac{\pi j_* \bar{N}}{N_0 b}. \quad (\text{S6})$$

The parameters were set to $E_0 = 6.3 \times 10^{-5}$, $b = 1300$ m, $j_* = 3$, $N_0 = 5.2 \times 10^{-3}$ rad s⁻¹.

The corresponding GM normalized shear variance was then calculated over the same wavenumber interval as

$$\left\langle \frac{V_z^2}{N^2} \right\rangle_{\text{GM}} = \int_{\frac{2\pi}{180}}^{\frac{2\pi}{60}} S_{\text{GM}}(k_z) dk_z. \quad (\text{S7})$$

The normalized shear level was

$$E_{\text{sh}} = \frac{\left\langle \frac{V_z^2}{N^2} \right\rangle_{\text{obs}}}{\left\langle \frac{V_z^2}{N^2} \right\rangle_{\text{GM}}}. \quad (\text{S8})$$

The turbulent kinetic energy dissipation rate was estimated as,

$$\varepsilon = \varepsilon_0 \left(\frac{\bar{N}}{N_0} \right)^2 E_{\text{sh}}^2 h_1(R_\omega) j\left(\frac{f}{\frac{f}{N}}\right), \quad (\text{S9})$$

where the reference dissipation level used in this study was $\varepsilon_0 = 1.0 \times 10^{-10}$ Wkg⁻¹, and

$$h_1(R_\omega) = \frac{3(R_\omega + 1)}{2\sqrt{2}R_\omega\sqrt{R_\omega - 1}}. \quad (\text{S10})$$

The shear-to-strain variance ratio was set to $R_\omega = 7$, following Kunze et al. (2006), who showed that this value is representative of the observed open-ocean internal wave field.

The latitude and stratification dependent correction term was

$$j\left(\frac{f}{N}\right) = \frac{f \cosh^{-1}\left(\frac{N}{f}\right)}{f_0 \cosh^{-1}\left(\frac{N_0}{f_0}\right)}, \quad (\text{S11})$$

where f is the Coriolis frequency, and $f_0 = f(25^\circ)$.

Combining the above expressions, the estimated turbulent dissipation rate is expressed as

$$\varepsilon = 10^{-10} \left(\frac{\overline{N}}{5.2 \times 10^{-3}} \right)^2 \left[\frac{\int_{\frac{2\pi}{180}}^{\frac{2\pi}{60}} S_{V_z}(k_z) dk_z}{\int_{\frac{2\pi}{180}}^{\frac{2\pi}{60}} S_{GM}(k_z) dk_z} \right]^2 h_1(7) j\left(\frac{f}{N}\right), \quad (\text{S12})$$

We have added the GHP parameterization method to the Supplementary material (Text S4). **(Lines 317-318)**

(13) Line 320: These values (3.9×10^{-10} and 5.0×10^{-10} m²/s³) are time-averaged values? Please consider clarifying it.

Response:

Thank you very much. We have clarified this in the revised manuscript by adding “time-averaged” before the dissipation rates. **(Line: 324)**

(14) Line 321-322: I agree with the interpretation that turbulent energy dissipation is not a major reason why NIKE is comparable in these two events. While the conclusion is the same, I think that the manuscript should also point out that turbulent energy dissipation could be ignored because it is one order of magnitude smaller than the wind-energy input and the energy exchange between NIWs and mean flows. Given the uncertainties in finescale parameterization, this might be more

important than the fact that the turbulent energy dissipation is comparable between the two events.

Response:

Thank you very much for this comment. We agree with you that turbulent energy dissipation could be ignored because it is one order of magnitude smaller than the wind-energy input and the energy exchange between NIWs and mean flows. Therefore, despite the uncertainty associated with the parameterization, turbulent dissipation is unlikely to be a dominant pathway controlling the NIKE evolution during these two events. We have clarified it in the revised manuscript. **(Lines: 325-329)**

(15) Line 330-331: *Please check the grammar of this sentence.*

Response:

Thank you very much for pointing this out. We have corrected the grammatical error in this sentence. **(Lines 335-336)**

(16) Line 356-357: *The meaning of “in this situation” is unclear to me. Did the author mean “during WS1” here?*

Response:

Thank you for pointing out this ambiguity. “In this situation” refers to the conditions during WS1. We have revised the sentence. **(Lines: 361-362)**

Reference

Kunze, E., Firing, E., Hummon, J. M., Chereskin, T. K., and Thurnherr, A. M.: Global Abyssal Mixing Inferred from Lowered ADCP Shear and CTD Strain Profiles, *Journal of Physical Oceanography*, 36, 1553-1576, 10.1175/jpo2926.1, 2006.

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Polzin, K. L., Naveira Garabato, A. C., Huussen, T. N., Sloyan, B. M., and Waterman, S.: Finescale parameterizations of turbulent dissipation, *Journal of Geophysical*

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Takahashi, A. and Hibiya, T.: Influence of the Distortion of Vertical Wavenumber Spectra on Estimates of Turbulent Dissipation Using the Finescale Parameterization: Observations in the Antarctic Circumpolar Current, Journal of Geophysical Research: Oceans, 126, 10.1029/2020jc016613, 2021.