Temporal variability of measured vertical velocity over a canyon-incised slope in the Coral Sea

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Reply to Referees' Comments

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We would first like to thank Dr. Angelika Renner (Editor), Referee 1 (R1), and Referee 2 (R2) once again for their valuable comments and continued engagement throughout the review process. In the sections below, to clearly distinguish between the referees' remarks and our revisions, the referees' comments are highlighted in blue, while our modifications to the manuscript are indicated in orange.

This response letter is structured into 3 parts: Section 1 deals with R1's comments, Section 2 covers R2's comments, and Section 3 presents the other changes made to the manuscript.

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1 Comments of Reviewer 1

Robache et al. discuss vertical velocities measured at 50–400 m depth from four moorings over a sloping seafloor deployed for 11.5 months. They have a meaningful goal of trying to describe the various timescales at which vertical velocity varies. This has direct consequences for biology and biogeochemistry. The paper is organized well and the figures that are present are well designed.

Unfortunately, on account of the data contamination due to diel vertical migration (which the authors recognize but cannot sufficiently fix), I think the paper lacks clear results. Several attempts are made in the Discussion to interpret the results within existing theories, but not in a convincing way (more details below).

To turn this study into a publishable paper, my suggestion would be to simplify things. Spend more time presenting the data in its primary form (e.g., plots of velocity vs time) rather than abstract and derived quantities (e.g., IMFs from the EEMD or Q values). Also, lean more toward simple scaling analyses like that in Section 4.1 rather than the much more complex ideas in Section 4.2.

Listed below are comments separated into major and minor issues. Some of these elaborate on my points above.

We sincerely thank the reviewer for their constructive overview of our manuscript. We acknowledge the reviewer's concerns regarding the impact of DVM contamination and the complexity of some parts of the Discussion. Below, we address each of the major and minor points raised by the reviewer in detail.

1.1 Major comments

A key selling point of the paper is that there is an 11.5 month dataset of vertical velocities at four moorings. Yet at no point is the whole record shown in a simple manner. Most of the figures are abstract quantities like PDFs, power spectra, cross-correlations, etc. It's difficult to interpret these without seeing the original w data. And certainly it makes it harder to provide a useful review. I would suggest adding a figure near the start of the Results that shows the whole record for at least one of the moorings. Something like the current Figure 2a but for all 11.5 months. You could break it into multiple rows to get a reasonable aspect ratio.

The requested figure, showing several examples of time series, has been added to the main text

(Figure A of this letter, new Figure 2 in the revised manuscript). We found this representation more informative and easier to interpret than a heatmap, which can be more difficult to read. Nevertheless, the heatmap representation has also been retained and added to Appendix B for completeness (see Figure B). This has been referenced in the text: "Examples of the time series are shown in Figure 2. The complete vertical velocity record is also shown in Appendix B" (line 97).

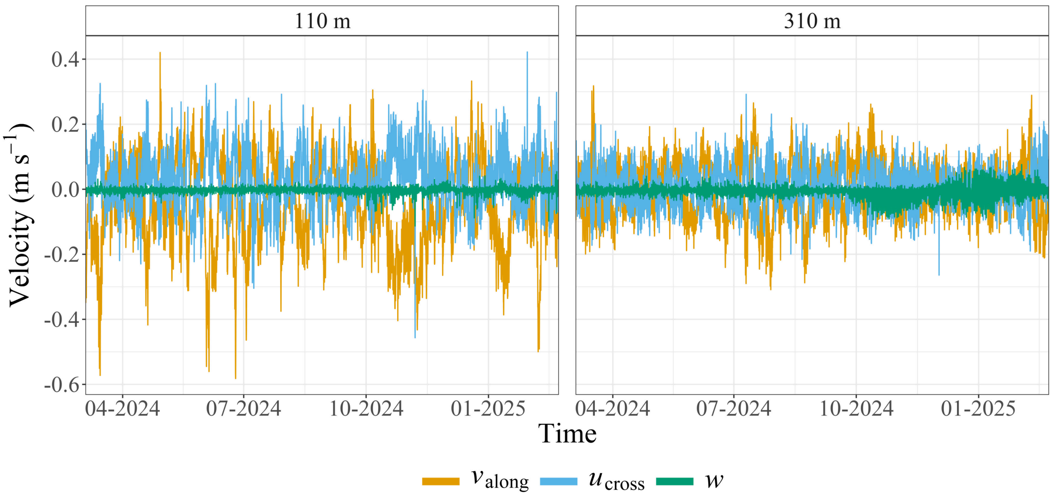

Figure A: Examples of time series of w, v_{along}, and u_{cross} recorded at the ADCP SW site at 110 m (left panel) and 310 m (right panel) depth over the entire study period.

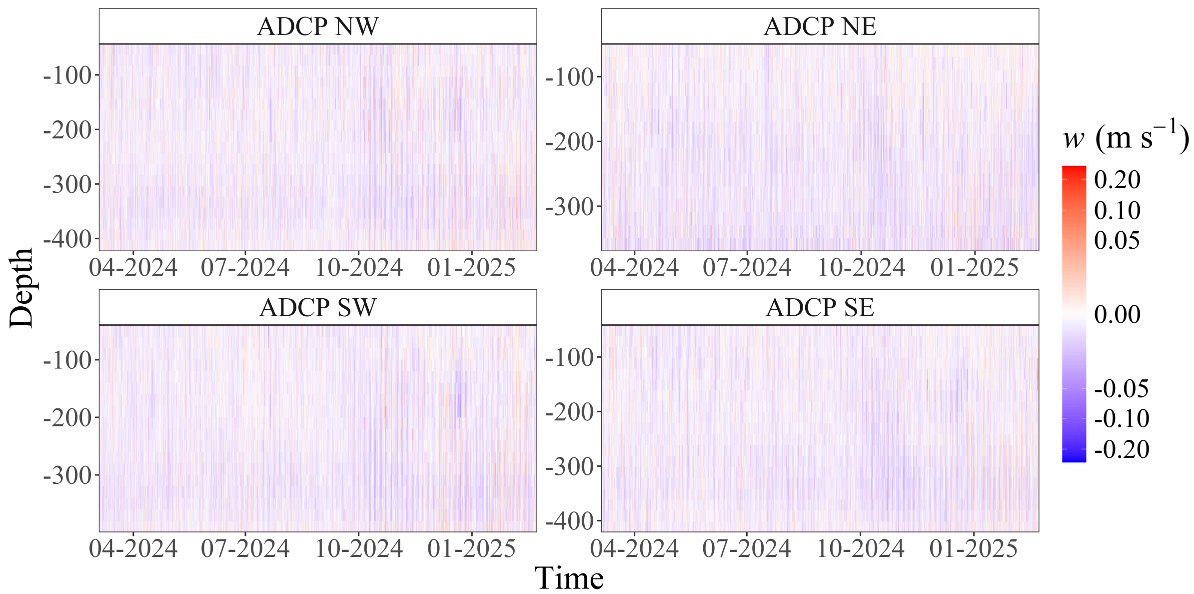

Figure B: Time-depth distribution of vertical velocity w for each site from 4 March 2024 to 22 February 2025.

Another key selling point is the multi-time-scale analysis, but I'm skeptical of its value. Figure 6 (and line 265) says that 90 % of the variance in the signal is captured by the lowest three modes.

But these modes are the 24-hour variability and higher harmonics. Elsewhere in the paper, it's clearly shown and recognized that this variability is biological (diel vertical migration, DVM), not physical. I realize that you state somewhere in the paper that you have to use w_{raw}, not $w_{\text{processed}}$, for methodological reasons. But it makes the analysis seem worthless. You've identified that the biological signal is unwanted in this analysis, but then you've gone ahead and ignored this major caveat in a lot of your analysis.

We thank the reviewer for raising this important and highly relevant point. We completely understand the skepticism regarding the use of w_{raw} and the heavy concentration of variance in the lowest modes (IMF 1–3). We agree that these modes cannot be interpreted as purely physical, since the biological DVM signal significantly contributes to this high-frequency variability. However, we would like to clarify two crucial points—one methodological and one theoretical—to demonstrate why this EEMD analysis remains physically meaningful and mathematically consistent.

First, the methodological constraint regarding data gaps: as the reviewer rightly noted, we had to apply EEMD to w_{raw} rather than $w_{\text{processed}}$. The EEMD algorithm fundamentally relies on cubic spline interpolations to compute the upper and lower envelopes of the signal. Applying it to $w_{\text{processed}}$, which contains data gaps resulting from the DVM filtering (typically occurring during sunrise and sunset transitions), would compromise the spline interpolation and generate numerical artifacts that could propagate through the extracted modes. Using the continuous w_{raw} time series was therefore a strict mathematical necessity for this specific analysis.

Second, the theoretical variance distribution of the physical signal: more importantly, we wish to highlight that the concentration of variance in the first three modes is also expected from a purely physical standpoint, even in the absence of biological signals. However, we acknowledge that the observed variance distribution reflects a combination of physical variability and the DVM contribution. As demonstrated by Flandrin and Gonçalvès (2004) and Wu and Huang (2009), EEMD exhibits a dyadic filter-bank behavior for broadband processes with power-law spectra $E(f) \sim f^{-\beta}$. For such processes, the variance V_m contained within the approximately dyadic frequency band associated with the m-th intrinsic mode function (IMF), with characteristic period T_m, theoretically scales as:

$$V_m \sim T_m^{\beta-1}$$

As shown in our spectral analysis, the physical vertical velocity variability (w) in our study area exhibits a relatively shallow spectral slope with $\beta \approx 0.4$. Applying this physical slope to the theoretical EEMD scaling yields:

$$V_m \sim T_m^{-0.6}$$

Because this exponent is negative, the variance naturally decreases as the period increases. This theoretical scaling indicates that a purely physical vertical velocity signal with a $\beta = 0.4$ spectral slope would inherently concentrate a large fraction of its variance within the first high-frequency IMFs. Based on this scaling, approximately 70–75 % of the total variance is expected within the first three IMFs. Therefore, the 90 % variance we observe in IMF 1–3 does not indicate that the EEMD analysis is dominated by an uninformative artifact. Instead, it reflects both the expected high-frequency variability of the physical w field and the additional contribution of the energetic DVM signal. The EEMD framework therefore captures the multi-scale organization of the observed vertical velocity field, while the higher-order IMFs provide information on lower-frequency oceanographic variability.

We agree with the reviewer that this nuance was missing from the text. We have thoroughly revised Section 3.2.2 to include this theoretical justification. We now explicitly state that the high variance in IMF 1–3 is consistent with the observed spectral scaling of w ($V_m \sim T_m^{-0.6}$), while these modes should be interpreted as containing both physical high-frequency variability and the biological DVM signature:

"Crucially, this dyadic property provides a theoretical framework to interpret the strong concentration of variance in the first three IMFs. For stochastic processes or turbulence time series displaying a power-law spectrum $E(f) \sim f^{-\beta}$, the variance σ_i^2 of the i-th IMF scales theoretically as:

$$\sigma_i^2 \sim \overline{t_i}^{\beta-1} \tag{1}$$

Given that the vertical velocity spectrum in our study area exhibits a relatively shallow physical slope of $\beta \approx 0.4$ (see Section 3.2.1), the theoretical variance scaling yields $\sigma_i^2 \sim \overline{t_i}^{-0.6}$. Because the exponent -0.6 is negative, variance is expected to decrease toward larger temporal scales. This scaling, together with the dyadic relationship $\bar{t}_i \sim 2^i$, suggests that a purely physical vertical velocity field with $\beta = 0.4$ would naturally concentrate approximately 70–75 % of its total variance within the first three high-frequency IMFs ($\leq$ 1 day), even in the absence of biological variability. Consequently, the observed 90 % variance in IMFs 1–3 reflects the combined influence of this expected high-frequency physical variability and the additional periodic contribution of DVM. These first IMFs should therefore be interpreted as representing the high-frequency variability of the observed vertical velocity record rather than as purely physical modes" (lines 312–323).

When the harmonics are first discussed at line 232, it doesn't appear to be recognized that these are expected. Consider this: idealize the diel vertical migration as a negative dirac delta function in the morning and a positive dirac delta function in the evening, and then repeat this every day to create an idealized time series. The power spectrum of this time series would include a peak at 1/day and higher harmonics (i.e., the 12, 8, and 4 hour peaks identified here). To me, this seemed obvious as soon as I saw it. The fact that the authors don't seem to recognize this reduces my confidence in the rest of their interpretation of results involving power spectra.

We agree that the peaks at 12, 8, and 4 h correspond to the expected harmonics of the diurnal signal. Because diel vertical migration is characterized by relatively abrupt transitions rather than a purely sinusoidal oscillation, its Fourier spectrum naturally contains higher-order harmonics. We have revised the manuscript to explicitly state this point and to clarify that: "These harmonics are expected because DVM consists of relatively abrupt transitions occurring twice per day rather than a purely sinusoidal oscillation. Such a periodic, non-sinusoidal signal naturally generates higher-order harmonics in its Fourier spectrum" (lines 250–252).

The harmonic peaks are also mentioned for the quantity dI at line 243, but again they are not given their simple explanation.

This has been added in the revised manuscript: "reflecting the non-sinusoidal, step-like profile of the DVM during sunrise and sunset transitions" (line 266).

How can the power spectra in Figures 4 and 5 extend below 0.001 per day? The record is only 356 days, which means the lowest frequency should be $1/356 = 0.0028$ per day.

The spectral values below $f = 1/356\ \mathrm{d}^{-1} \approx 0.0028\ \mathrm{d}^{-1}$ result from zero-padding applied during the Fourier transform calculation (used to interpolate the spectrum to a power-of-two grid). While zero-padding increases frequency sampling for graphical smoothing, it does not add independent physical information below the fundamental Rayleigh frequency resolved by our 356-day record ($f_{\min} = 1/T \approx 0.0028$ d^{-1}). To avoid any misleading representation of unresolvable low frequencies, we have updated Figures 4 and 5 in the revised manuscript by truncating the frequency axes at the physical limit $f_{\min} = 1/356$ d$^{-1} \approx 0.0028$ d^{-1}. The two Figures 5 and 6 (former Figures 4 and 5) have been modified accordingly and the text has been adapted to remove the part based on annual scale.

Related to the above comment, Figure 4's caption says spectra are 'smoothed using a mean per decade' (see also line 228). I do not understand what this means. I would've thought it is a moving mean with a window that has a $\log_{10}$ width $= 1$, but that would smooth out all the detail, and it's clearly not what the graphs show.

The expression "smoothed using a mean per decade" was indeed imprecise. The spectra were not smoothed using a moving average over one decade. Instead, the spectral estimates were averaged within logarithmically spaced frequency bins, with each frequency decade divided into 20 logarithmic bins. The frequency associated with each point corresponds to the average frequency within each bin. We have revised both the figure caption and the corresponding description in the manuscript to clarify this logarithmic bin-averaging procedure: "The spectra were smoothed by averaging the spectral density within logarithmically spaced frequency bins." (Figure 5 caption, former Figure 4).

Figure 5. It's concerning that the $w_{\text{processed}}$ spectra is above the w_{raw} spectra across a large band (all of $f > 0.03$ per day with the exception of the 1 per day peak and its harmonics). $w_{\text{processed}}$ is meant to be w_{raw} with the effect of diel vertical migrations subtracted. How does this lead to an increase in variance for $w_{\text{processed}}$ relative to w_{raw} for the range of frequencies in question?

In this figure, the spectra are not weighted; they are shown in arbitrary units. This explains the difference between the two spectra: a larger proportion of the relative variance appears over this broad range of scales because the influence of biological processes in the data is reduced. This has been added in the text: "In contrast to Figure 5, the spectra in Figure 6a are not variance-weighted but normalized and displayed in arbitrary units. Therefore, differences in amplitude do not represent differences in absolute variance but only changes in the relative distribution of spectral energy across scales" (lines 260–262).

240. Maybe it's okay, but it strikes me as odd to calculate a spectrum of speed V (i.e., a quantity that's always positive), when you could have just as easily calculated a spectrum for v_{along}.

Following the reviewer's recommendation, we changed the way the spectrum of V was estimated. Instead of computing the spectrum of the velocity magnitude $V = \sqrt{u^2 + v^2}$, we computed the spectra of u_{cross} ($E_{u_{\text{cross}}}(f)$) and v_{along} ($E_{v_{\text{along}}}(f)$). We then estimated the horizontal velocity spectrum $E_V(f)$ as follows, in order to preserve the effect of sign changes in the velocity components:

$$E_V(f) = \left\langle E_{u_{\text{cross}}}(f) + E_{v_{\text{along}}}(f) \right\rangle ,$$

where $\langle \cdot \rangle$ denotes the statistical average. This modification alters the shape of the spectrum. Consequently, adjustments have been made throughout the text: "While horizontal currents are mainly constrained by tridimensional turbulence and topography" (lines 7–8), "$E_V(f) = \left\langle E_{u_{\text{cross}}}(f) + E_{v_{\text{along}}}(f) \right\rangle$" (line 259), "A peak is also observed at 12 h for V, which, in contrast, is stronger than for the other time series" (lines 268–269), "The spectrum of V follows a slope of $\beta = 0.95$ below approximately 1.5 days ($f \simeq 10^{-0.2}$ d^{-1}), a slope of $\beta = 1.68$ between 1.5 days and 16 days ($f \simeq 10^{-1.2}$ d^{-1}), and a slope of $\beta = 0.05$ between 16 days and 250 days ($f \simeq 10^{-2.4}$ d^{-1})" (lines 278–280), "A spectral slope close to $\beta = -5/3$ was identified for the horizontal velocity over time scales ranging from 1.5 to 16 days. This $-5/3$ scaling is consistent with the classical homogeneous and isotropic three-dimensional inertial cascade (Kolmogorov, 1941; Obukhov, 1941). Conversely, at higher frequencies (time scales shorter than 1.5 days), an f^{-1} spectral decay ($\beta \approx 1$) is observed, representing the signature of dynamics constrained by physical boundaries and vertical shear (e.g., Katul and Chu, 1998; Katul et al., 2012; Renosh et al., 2014; Schmitt and Huang, 2016). At these higher frequencies ($<$ 1.5 days), structures interacting with the boundaries experience geometric confinement and shear, disrupting three-dimensional energy cascades and redirecting variance into an f^{-1} decay. While theoretical frameworks propose that this f^{-1} redistribution can arise from continuous internal damping, such as Heisenberg eddy viscosity (Katul et al., 2012), high-resolution numerical modeling would be required to explicitly isolate and validate this specific mechanism in our study area. For time scales larger than 16 days (up to at least 250 days), a flatter spectrum is observed, with a spectral slope close to $\beta \approx 0$ (a spectral plateau). In the literature, this spectral flattening is frequently associated with synoptic scales of energy injection (Richardson, 1922) or a "geostrophic eddy range" (Ferrari and Wunsch, 2009). These frameworks highlight large-scale structures generated by pressure and Coriolis forces. However, because a weak but non-negligible dynamical signal is still observed at these scales for the vertical velocity component w, our data suggests these structures are not purely geostrophic, but retain ageostrophic characteristics" (lines 450–477), and "The horizontal velocity exhibits characteristic $f^{-5/3}$ (1.5–16 days) and f^{-1} (< 1.5 days) spectral slopes, reflecting the 3D turbulence cascade and the strong influence of topographic confinement and bottom shear on the flow, respectively" (lines 590–592).

247. How do you know that what remains after the DVMs have been removed is still a biological signal?

The text has been revised to make this interpretation more cautious: "Hourly averages of w_{raw} and $w_{\text{processed}}$ were computed and are shown in Figure 6b. Although most of the DVMs have been removed, a cyclic signal remains in the time series. Its biological origin is suggested by its close resemblance to the daily cycle associated with DVMs. The residual signal can thus be interpreted as a quasi-sinusoidal diel cycle (as observed in previous studies; Chun et al., 2022), upon which two sharp, pulse-like features associated with the downward and upward migration phases are superimposed. This signal is cyclic and close to a sinusoidal shape around the mean value. For $w_{\text{processed}}$, the amplitude of this cycle reaches approximately 9 mm s^{-1}, amounting to 2.5 times the series mean ($\simeq$ 3.5 mm s^{-1}). This indicates that this cyclic variability remains a prominent component of the temporal dynamics" (lines 269–277).

259. Figure 6 presents the Intrinsic Mode Functions from the Ensemble Empirical Mode Decomposition. These aren't things commonly used in oceanography, so I would suggest you need a figure prior to Figure 6 that demonstrates the decomposition. The reader cannot make sense of the relative variance of the IMFs if they don't understand what its components are. Although you define everything in Section 2.2.2, there are only formulas. A visual is needed as well.

An example of the decomposition has been added to the Appendix (Figure C in this letter): "see Appendix C for an example" (line 284), "The EEMD decomposition applied to a w time series (arbitrarily chosen) yields eleven Intrinsic Mode Functions (denoted here as C_1 to C_{11}) along with a long-term trend (r_n), sorting the signal's variability from high to low frequencies. The first modes capture highly energetic, high-frequency fluctuations and transient anomalies. Lower-frequency modes (e.g., C_8 to C_{11}) isolate sub-seasonal to seasonal components, where

C_{11} highlights a broad annual or semi-annual cycle. Finally, the residual trend (r_n) describes a smooth, long-term evolution of the mean vertical velocity, reaching its lowest negative values by the end of 2024 before exhibiting a slight upward inflection" (lines 624–629).

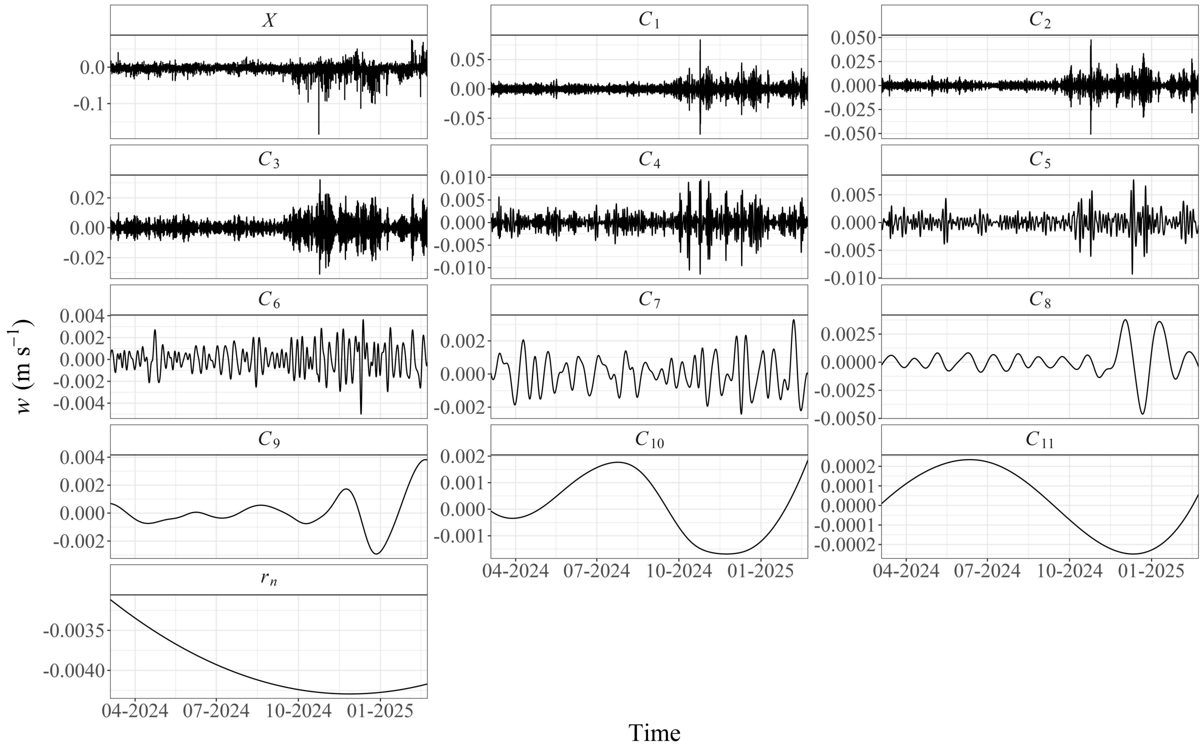

Figure C: Example of EEMD applied to the w time series measured by the ADCP SW at approximately 150 m depth. The original signal X, the resulting IMFs C_i, and the residual r_n are shown.

Section 4.2. A lot of this section seems hand-wavy. Take lines 393–401, for example. You discuss a single number (the spectral slope β) in relation to several processes (three-dimensional energy cascade, geostrophic eddies, scales of energy injection, Heisenberg eddy viscosity, bottom boundary shear). Everything is mentioned too quickly without any real justification. The same kind of thing happens at lines 404–415 when you look to explain $\beta = 0.3$–0.4.

We agree with your assessment. In our original draft, we overreached by definitively linking our calculated 1D temporal spectral slopes to complex 3D spatial phenomena. While these slopes are mathematically consistent with frameworks like Heisenberg eddy viscosity or specific energy injection scales found in the literature, we cannot justify these specific mechanisms using only a single temporal scaling exponent. To correct this, we have thoroughly rewritten Section 4.2. We have toned down the speculative language and reframed these physical mechanisms as plausible interpretations based on existing literature, rather than definitive conclusions drawn from our data. We have also explicitly acknowledged the limitations of inferring spatial cascade dynamics from single-point temporal time series:

"Since the time series are sufficiently long, it was possible to investigate their multiscale dynamics from a statistical perspective. First, a Fourier spectral analysis was performed. This analysis revealed a strong similarity in the multiscale dynamics of the time series across the four sites and for the different depths considered.

A spectral slope close to $\beta = -5/3$ was identified for the horizontal velocity over time scales ranging from 1.5 to 16 days. This $-5/3$ scaling is consistent with the classical homogeneous and isotropic three-dimensional inertial cascade (Kolmogorov, 1941; Obukhov, 1941). Conversely, at higher frequencies (time scales shorter than 1.5 days), an f^{-1} spectral decay ($\beta \approx 1$) is observed, representing the signature of dynamics constrained by physical boundaries and vertical shear (e.g., Katul and Chu, 1998; Katul et al., 2012; Renosh et al., 2014; Schmitt and Huang, 2016). At these higher frequencies (< 1.5 days), structures interacting with the boundaries experience geometric confinement and shear, disrupting three-dimensional energy cascades and redirecting variance into an f^{-1} decay. While theoretical frameworks propose that this f^{-1} redistribution can arise from continuous internal damping, such as Heisenberg eddy viscosity (Katul et al., 2012), high-resolution numerical modeling would be required to explicitly isolate and validate this specific mechanism in our study area. For time scales larger than 16 days (up to at least 250 days), a flatter spectrum is observed, with a spectral slope close to $\beta \approx 0$ (a spectral plateau). In the literature, this spectral flattening is frequently associated with synoptic scales of energy injection (Richardson, 1922) or a "geostrophic eddy range" (Ferrari and Wunsch, 2009). These frameworks highlight large-scale structures generated by pressure and Coriolis forces. However, because a weak but non-negligible dynamical signal is still observed at these scales for the vertical velocity component w, our data suggests these structures are not purely geostrophic, but retain ageostrophic characteristics.

In contrast to the horizontal component, the spectrum of the vertical velocity w exhibits a much weaker decay, characterized by a slope of $\beta \approx 1/3$ over a range of scales from 1.5 to 63 days. This reduced spectral attenuation highlights the fundamental anisotropy of the flow, likely resulting from stratification and geometric confinement suppressing the development of large vertical motions at low frequencies (Katul et al., 2012). Consequently, the variance of the vertical velocity is distributed much more uniformly across the frequency band, behavior typical of fields dominated by internal gravity waves. For context, the canonical model of Garrett and

Munk (1979) predicts a theoretically flat temporal spectrum ($\beta = 0$) for this component, while approaches based on the conversion of spatial spectra suggest a slope nearer to $\beta = 0.5$ (Klymak and Moum, 2007). The observed decay of $\beta = 0.4$ for w_{raw} (and $\beta = 0.3$ for $w_{\text{processed}}$) falls well within this expected theoretical range. While it is tempting to attribute the slight deviations from purely linear equilibria to nonlinear wave–wave interactions or local intermittency (e.g., Polzin and Lvov, 2011; D'Asaro et al., 2007; D'Asaro, 2014), our temporal data can only suggest this intermediate regime as a plausible hypothesis rather than a confirmed mechanism" (lines 447–493).

511–515. Again, there's too much hand waving going on. Take the sentence 'Such structures point toward the importance of secondary circulation, internal tides, baroclinic processes, and topographic interactions, extending beyond the classical Ekman framework.' That's a list of processes that incorporates too many things to be useful to the reader. This is soon followed by another long, wide-ranging list: 'Instead, they emerge from the interplay between topography, stratification, multiscale variability, and episodic processes.'

We agree with the reviewer that listing multiple physical mechanisms diluted the clarity of our conclusions. We have thoroughly streamlined both sentences in the revised conclusion. Instead of providing broad lists of candidate processes, we now focus explicitly on the core physical drivers supported by our findings—namely localized baroclinic dynamics, topographic forcing, and stratification: "Such vertical shear signatures highlight localized baroclinic dynamics and topographic forcing that extend beyond classical wind-driven Ekman frameworks" (lines 599–601), and "Instead, they reflect episodic, small-scale dynamics shaped by the combined effects of local topography and stratification" (lines 603–604).

As most of Section 2.2 is descriptions of textbook statistical analyses (except perhaps 2.2.2), most/all of it could be moved to an appendix. A lot of it also seems unnecessarily detailed, especially for the standard methods used in oceanography like Fourier transforms, cross correlations, and moving averages.

We thank the reviewer for this constructive feedback regarding the flow of the methodology section. While we completely agree that methods such as Fourier transforms and moving averages are standard textbook tools in physical oceanography, our manuscript is aimed at a highly multidisciplinary audience. Because the core of our results bridges physical dynamics with biological processes (DVM, biogeochemical implications), we initially chose to maintain a detailed methodology to ensure the analysis remains fully accessible to biologists and chemists who may be less familiar with signal processing. We would therefore prefer to retain this section in the Materials and Methods Section. However, if the reviewer still considers that moving this section would improve the structure and flow of the manuscript, we would of course be happy to make this change.

1.2 Minor comments

4. Replace 'slightly less than one year' with the actual length (11.5 months).

Done.

Figure 1. The labels for the arrows in the middle panel are too small to read.

The size of the labels has been increased (see Figure D).

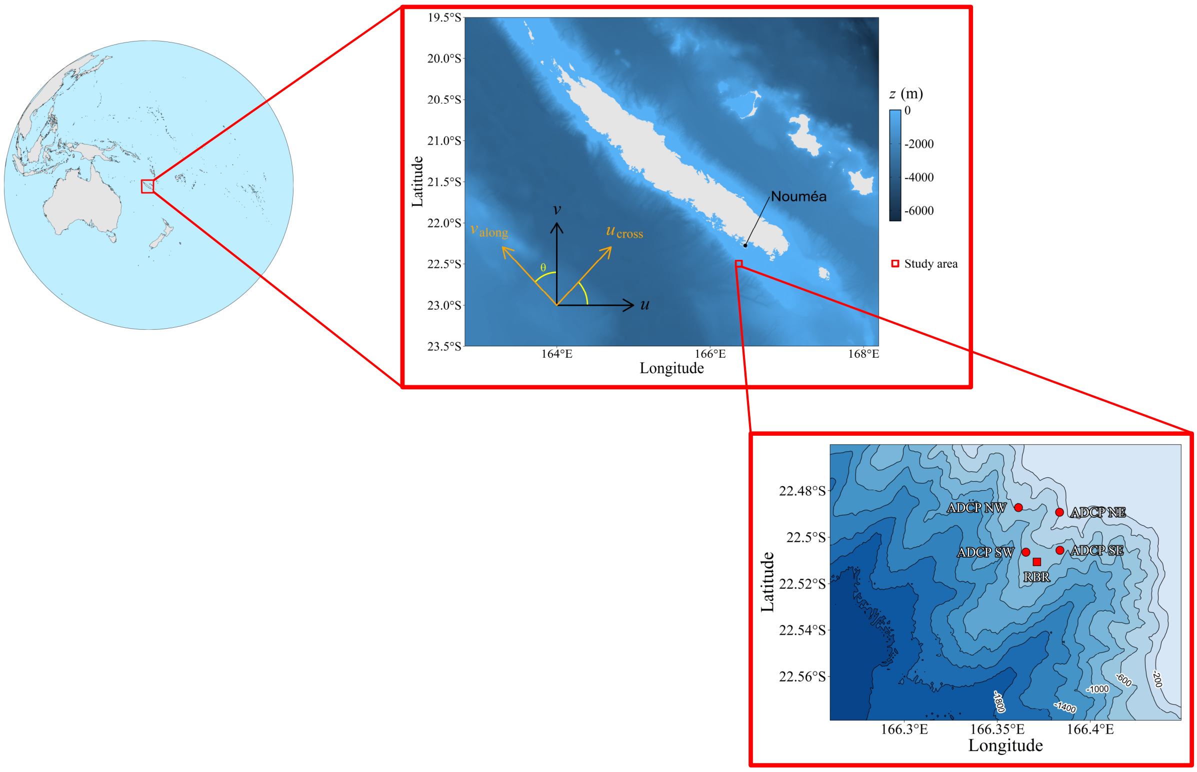

Figure D: Map of the study area located in the Coral Sea (Western Subtropical South Pacific) off Nouméa (Grande Terre island, New Caledonia, France). The location of the four ADCPs and of the RBR© sensors is shown.

178. 'Data presentation' is a very vague heading. Replace with something more insightful.

The title of the section has been changed: "Vertical velocity characteristics" (line 187).

184. Instead of 'An example is shown in panel (a) of Figure 2', you can just add '(Figure 2a)' to the end of the preceding sentence.

Done.

188. As above, replace 'panels (b), (c), and (d) of Figure 2' with 'Figure 2b–d.'

Done.

More generally, there are many places in the text where you use phrases like 'panel a' and 'panel b'. It would be simpler to just use Figure 1a, Figure 2b, etc. It possible to lose track of what figure you're referring to (e.g., at line 218), and having a reference to only panel a or panel b doesn't help.

These changes have been done all along the text as requested.

Figure 4 caption. The vertical lines are described as being at the Nyquist frequency, which you've correctly defined in the caption, but why are they are at $f \simeq 0.005$ per day? The Nyquist frequency would just be the upper frequency limit of your power spectra, so there's no need to draw a line.

The figure has been changed and this part of the text has been removed.

205. It's weird to say the 'NW, SW, and SE sites appear to be more affected' because there's only one other site. To say three of the four are 'more affected' seems more complicated than saying the other one is less affected.

This has been changed in the revised manuscript: "the NE site appears to be significantly less affected" (lines 219–220).

209–211. Define what metric is being used for the uncertainties. Is it standard deviation? Is it 95% confidence interval?

It is standard deviation, this has been precised in the text: "(μ)" (line 223), "while the standard deviations (σ) are relatively large" (lines 223–224), and "$\mu \pm \sigma =$" (line 224, 225, and 226).

209. It seems odd to say the NE site is 'larger and more variable' when its mean and un-

certainty are nearly the same as the other sites.

The values reported as "mean ± standard deviation" correspond to the temporal standard deviation of the vertical velocity time series, not to the uncertainty on the mean. While the mean values are indeed similar among the sites, the NE site exhibits the largest temporal variability (σ). We have clarified this point in the revised manuscript: "larger standard deviation, reflecting slightly greater temporal variability" (line 226).

212 and 502. 'persistent' seems like the wrong word. Maybe 'average' or 'net' describes the vertical motion? Persistent would imply it's always negative.

The word "persistent" has been replaced with "net" (line 227).

Figure 6a. This fit doesn't look that good to me. The real measure of how good this curve fits the data is whether it goes through the cloud of points for C2–C4, which it does not.

The relationship has been removed from the Figure 7 (former Figure 6) and from the text. It has been replaced by: "Beyond these short time scales, the cumulative variance rapidly reaches a plateau, indicating that lower-frequency processes contribute only marginally to the overall variability" (lines 295–297).

Table 1 caption. What is considered equal to 0?

Values equal to 0 correspond to measurements for which the ADCP-derived vertical velocity is exactly $w = 0$ m s^{-1} (within the instrument's velocity resolution). This has been precised in the text: "within the instrument's velocity resolution" (Table 1 caption), "Note that the instrument has a vertical velocity resolution of approximately 1 mm s^{-1}, which should be considered when interpreting values close to $w = 0$ m s^{-1}" (lines 230–231).

231. Same comment as for line 205 about 'more' for 3/4 sites.

This has been fixed: "with the lowest intensities observed at the NE site" (lines 248–249).

232. Peaks at higher frequencies are not 'subharmonics'. They're harmonics or higher harmonics.

We thank the reviewer for pointing out this terminology issue. The term "subharmonics" was incorrect in this context and has been replaced by "higher harmonics" throughout the revised manuscript.

232. '$f \simeq 10^{-2.56}$', not '$f^{-2.56}$'

This part of the text has been removed.

242. The peak described as 'less pronounced' is still very pronounced.

This has been added in the text: "even if it is still very pronounced" (line 264).

265. Related to the Figure 6a comment above, I disagree that the 'scatter plot follows the form of a power-law relationship'. Hence, I also don't see the value in fitting 'an asymptotic saturation model.'

As detailed in our previous responses, we have decided to abandon this empirical fit. The relationship has been removed from the figure and from the revised text.

270. The R^2 of 0.96 is less impressive than it sounds. The data going into the calculation are certainly not independent given there is a cumulative sum involved.

As detailed in our previous responses, we have decided to abandon this empirical fit. The relationship has been removed from the figure and from the revised text.

288. I'm unclear how you can meaningfully compute the cross-correlations for the highest mode IMF (i=11, top right squares in Figure 7). As stated in line 288, the associated time scale is ~1 year. But if that's the same length as your whole record, how is it meaningful?

We thank the reviewer for raising this important point. We agree that the interpretation of the cross-correlation involving the highest-order IMF (IMF 11) requires caution because its characteristic time scale is comparable to the duration of the observational record. With only approximately one cycle available, the statistical significance of this correlation cannot be assessed robustly. Our objective was not to infer a causal relationship from this single low-frequency mode, but rather to identify whether the lowest-frequency variability of w_{raw} and dI exhibited a common temporal evolution over the deployment period. We have revised the manuscript to clarify this limitation and to avoid overinterpreting this correlation. The corresponding sentence has been modified as follows: "Because the characteristic time scale of IMF 11 approaches the duration of the observational record, these lowest-frequency modes should be interpreted with caution. They nevertheless reveal a common low-frequency variability between dI and w_{raw} over the deployment period" (lines 336–338).

296. Remind the reader that Q was defined back in Section 2.2.4.

Done: "The PDF quotient Q (see Section 2.2.4)..." (line 346).

328-339. This recognition of the ADCP uncertainty is welcome, but what about uncertainty in how vertical the ADCP is? For example, if the ADCP was tilted from vertical by 1°, then $w = \sin(1)u = 0.017u$, which isn't necessarily negligible. Is this an issue, or did you ensure that verticality somehow?

The tilt, roll, and heading of the instrument were systematically taken into account prior to the analyses to correct for any deviation from verticality. This has been more clearly specified in the revised text: "Moreover, as previously mentioned, potential biases induced by instrument heading, tilt and roll were systematically corrected using the internal attitude sensor data before performing the velocity transformations, ensuring that deviation from true verticality does not artificially contaminate the vertical velocity estimates" (lines 389–392).

433. K1 has a period of 23.93 h, not 24.96 h.

Fixed.

2 Comments of Reviewer 2

This manuscript provides a valuable dataset of direct in-situ vertical velocity (w) measurements in a key oligotrophic region. The authors deliver important empirical insights, revealing a dominant background downwelling regime punctuated by short-lived upwelling events, and proving that 90 % of w variance resides at sub-daily timescales. These findings establish critical physical constraints for local carbon export models and future sampling strategies. Although the manuscript is not methodologically novel, it is observationally valuable. It is a solid observational manuscript, provided the authors improve the physical interpretation of the events and address the signal-to-noise/biological contamination limits more rigorously.

We sincerely thank the reviewer for their positive assessment of our manuscript and for recognizing the value of this direct in-situ vertical velocity dataset. We have taken the constructive suggestions fully into account to improve the manuscript. Below, we address all of the reviewer's specific comments point by point.

2.1 Major comments

The authors approach to removing DVMs relies on simple empirical time windows (1 around sunrise/sunset), which is quite basic compared to modern bio-acoustic or multi-frequency filtering techniques.

We agree that bio-acoustic or multi-frequency approaches provide more detailed information on the distribution and intensity of migrating organisms. However, the objective of the present study is not to quantify biological biomass or migration patterns, but to remove the first-order contamination of the vertical velocity signal associated with DVMs. Since the DVM signature in our dataset is strongly periodic and synchronized with local sunrise and sunset times, a time-window approach provides a simple and reproducible criterion to identify the most affected measurements. This choice is also motivated by the fact that the dominant DVM signal occurs at diurnal and semi-diurnal frequencies, which overlap with the main tidal periodicities at the study site (mixed tide). Consequently, applying more complex signal filtering carries a high risk of over-filtering and inadvertently removing genuine physical velocity components. Adopting a conservative time-window targeted strictly around solar transitions allows us to mask the most unambiguous biological bias while preserving the integrity of the physical oceanographic signal. We have clarified this methodological choice in the revised manuscript: "While more advanced bio-acoustic or complex filtering techniques exist, they carry a significant risk of over-filtering and inadvertently removing genuine physical w components that overlap in frequency or amplitude with biological signals (e.g., diurnal physical processes). Since our priority is strictly oceanographic, we opted for a conservative approach: isolating only the most unambiguous biological bias. A simple empirical time window targeted around solar transitions provides a transparent and reproducible way to mask these first-order DVMs without unduly distorting the surrounding physical oceanographic signal" (lines 199–204).

The authors need to explicitly explain this physical/bio-acoustic logic in the text rather than stating it as a given. Moreover, the acoustic backscatter (dI) can also be affected by physical processes like suspended sediment or turbidity layers in canyon environments, so attributing it solely to biological organisms without physical shear coupling needs a clearer explanation. For example See: "This link is not observed between the time series of w raw and V, suggesting a biological origin" & "Once again, this link is not particularly observed with V, suggesting rather a biological influence." Why can you state that?

We fully agree that in canyon environments, physical processes such as sediment resuspension or nepheloid/turbidity layers can alter acoustic backscatter. However, physical resuspension and passive transport are hydrodynamically driven by horizontal current speeds (V) or bottom shear stress. If the observed w_{raw} anomalies were caused by physical sediment transport, we would expect a strong coupling with elevated values of V. The fact that these high vertical velocities ($\sim$ cm s^{-1}) show no correlation with V, and yet are strictly phase-locked to solar transitions (sunrise/sunset), indicates that they are not passive physical tracers, but active directed movements (DVMs) of biological organisms. As suggested, we have explicitly detailed this physical and bio-acoustic rationale in the revised manuscript to avoid stating it as a given; "This correlation is not observed between the time series of w_{raw} and V (and only weakly with v_{along}) at the daily scale. Since passive particle transport or sediment resuspension would generally be expected to be associated with hydrodynamic forcing (e.g., enhanced horizontal currents or bottom shear), the strong coupling between w_{raw} and dI at this frequency, together with the absence of correlation with V and the close phase-locking of the high vertical velocities ($\sim$ cm s^{-1}) to solar transitions (sunrise and sunset), provides further evidence that this daily cycle (C_3) is associated with active DVM rather than a purely physical transport mechanism" (lines 329–334), and "Once again, no such correlation is observed with V at these longer time scales. This suggests that the seasonal acoustic backscatter variability is not primarily associated with changes in horizontal current intensity and is consistent with longer-term biological variability, such as seasonal changes in zooplankton biomass or community structure" (lines 339–342).

The authors calculate a theoretical vertical velocity using the 2D/3D mass continuity equation. However, estimating horizontal gradients from four discrete moorings separated by several kilometers over steep, highly non-linear canyon topography involves massive spatial interpolation errors. Can you explicitly state the assumptions and boundary conditions used to compute these derivatives, along with an estimated error bound on the resulting theoretical w?

We agree that estimating horizontal derivatives from four discrete moorings over complex canyon topography requires strong assumptions. We have clarified the assumptions used in this calculation: "including a linear variation of the horizontal velocity field between the moorings and neglecting unresolved spatial variability" (lines 412–413). We have also clarified that the resulting vertical velocities should only be considered as order-of-magnitude estimates. A quantitative uncertainty bound is difficult to derive because the dominant source of uncertainty is the unresolved horizontal structure of the flow rather than measurement errors. We have revised the manuscript accordingly: "Nevertheless, this estimate assumes spatially smooth velocity gradients and neglects variability associated with the complex canyon topography and mesoscale/submesoscale processes. A rigorous uncertainty estimate is difficult to obtain because the dominant uncertainty arises from unresolved horizontal variability between the moorings rather than measurement errors. Therefore, these values should be interpreted only as order-of-magnitude estimates" (lines 414–418).

The observed vertical structure during the upwelling events shows a sharp sign reversal at 200 m depth—positive w and shoreward flow above 200 m, and negative w and offshore flow below 200 m. The authors speculate about baroclinic adjustment, secondary circulation, or internal tides, but do not test these hypotheses. Can the authors calculate the gradient Richardson number ($\mathrm{Ri} = N^2/(du/dz)^2$) across the 200 m interface using their temperature (RBR) and velocity data? This would show whether the shear is strong enough to induce shear instability ($\mathrm{Ri} < 0.25$) or if it represents a stable, topographically trapped baroclinic response.

We thank the reviewer for this suggestion. We agree that the gradient Richardson number would provide valuable information on the role of shear in the observed vertical structure. However, the available hydrographic observations are limited to 9 RBR temperature measurements between the surface and 450 m depth, which does not provide sufficient vertical resolution to robustly estimate local stratification gradients across the ∼200 m interface. Any Ri estimate would therefore strongly depend on interpolation assumptions and would not allow a reliable assessment of shear instability. We have clarified this limitation in the revised manuscript and moderated our interpretation of the possible mechanisms responsible for these events: "consistent with the presence of two distinct overturning cells and potentially associated with strong vertical shear" (lines 551–552), "In particular, the available hydrographic resolution does not allow us to quantify the local Richardson number across the observed transition layer, which would be required to assess whether the observed vertical shear is sufficient to induce shear instability or whether the circulation remains dynamically stable" (lines 556–558), and "These features could contribute to the intensification of horizontal velocities and may enhance topographic interactions, potentially favoring the development of topographically induced upwelling (shelf-break upwelling; Kämpf and Chapman, 2016) and internal tide generation" (lines 566–568).

The manuscript mentions four mooring sites (NE, NW, SE, SW) with different bottom depths (396 m to 762 m), but many figure panels (e.g., Fig. 4, Fig. 6) aggregate depths or present spectra without clearly distinguishing how depth-dependent signal-to-noise ratios (SNR) near the surface (50 m) affect the results. Acoustic backscatter usually degrades near the surface due to side-lobe interference.

We agree that ADCP measurements can be affected by reduced signal quality near the surface due to side-lobe interference. Here, bins intersecting the sea surface were removed from the analysis. In addition, correlation, echo intensity, and percent-good diagnostics were systematically inspected to assess data quality and ensure the reliability of the retained measurements. This has been added in the text: "In addition, the uppermost bins potentially affected by side-lobe interference were removed from the analysis. Correlation, echo intensity, and percent-good diagnostics were systematically inspected to assess data quality and ensure the reliability of the retained measurements" (lines 92–95).

While the manuscript thoroughly catalogs the statistical properties of the w time series using EEMD and spectral analysis, it leans heavily on descriptive signal processing. To strengthen its scientific novelty, the authors should go beyond describing the statistical modes and better explain the exact physical mechanisms driving the observed episodic upwelling events (e.g., deeper analysis of the baroclinic shear, internal tide interactions, or cyclonic eddy passage shown in Appendix A).

We agree that identifying the physical mechanisms responsible for the mulitscale variability of w is an important perspective. However, the present study primarily aims to characterize the statistical properties of the vertical velocity field using long-term ADCP observations. While Appendix D (former Appendix A) provides evidence that some major events coincide with the passage of cyclonic eddies, a detailed investigation of the underlying dynamical mechanisms (e.g., baroclinic shear, internal tide interactions, or eddy dynamics) would require additional observations and/or dedicated numerical modeling beyond the scope of the present study: "While these combined factors (eddy dynamics, baroclinic shear, and internal tides) provide a compelling qualitative framework to explain the episodic upwellings, isolating their respective quantitative contributions would require dedicated high-resolution numerical modeling" (lines 570–572).

2.2 Minor comments

Consider putting abbreviation list in the beginning- dI, V, DVM, EEMD, IMF, etc..

A glossary has been added in Appendix A (see Table 1).

Table 1: Glossary.

Abbreviation	Meaning
ADCP	Acoustic Doppler Current Profiler
ACNC	Alis Current of New Caledonia
w	Vertical velocity
V	Horizontal velocity
v_{along}	Along-shore velocity
u_{cross}	Cross-shore velocity
dI	Acoustic backscatter echo-intensity
EEMD	Ensemble Empirical Mode Decomposition
IMF	Intrinsic Mode Function
DVM	Diel Vertical Migration
N	Brunt–Väisälä frequency
f_c	Coriolis parameter
Ro	Rossby number
Bu	Burger number
Fr	Froude number
σ	Standard deviation
μ	Mean value
$E(f)$	Fourier spectral density
$S(f)$	Variance-weighted spectral density
β	Spectral slope
PDF	Probability Density Function
Q	PDF quotient

The text alternates between "DVM", "first-order DVM", "biological vertical migrations", and "biological bias."

Only the terms "DVM" and "first-order DVM" have been retained throughout the text.

Line 217- panel b of which figure?

This has been changed: "Figure 4b" (line 235).

Fig.9 (d) the arrows are too tight. Consider plotting subsample.

Done (see Figure E).

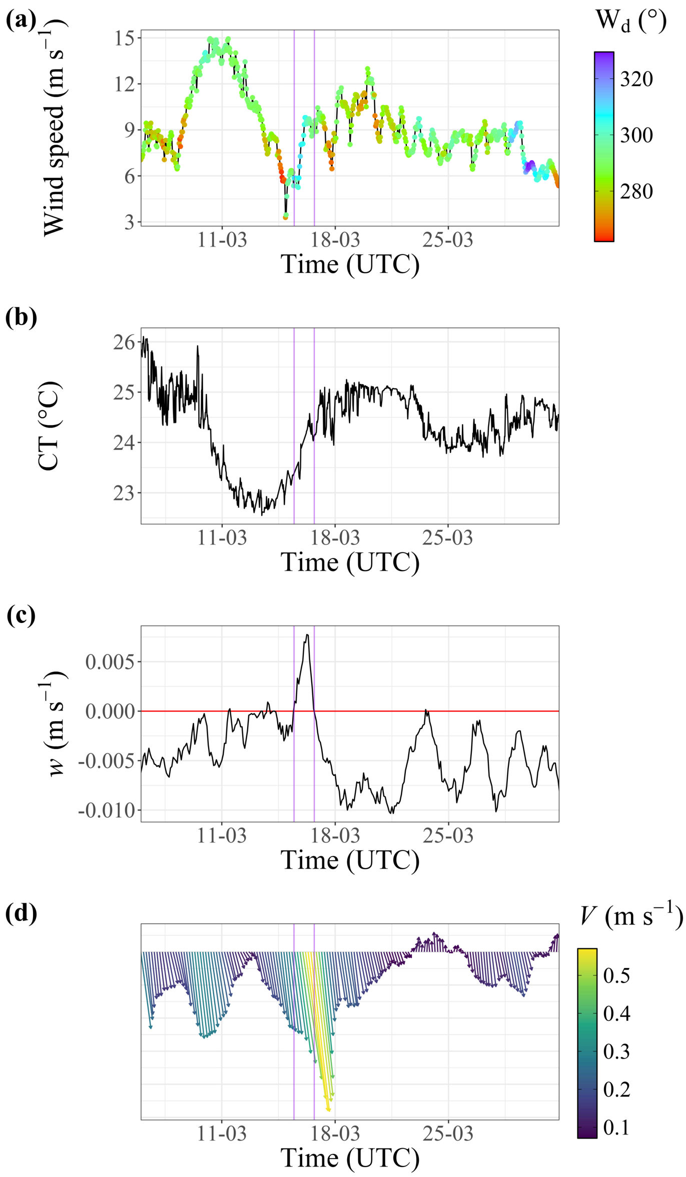

Figure E: **(a)** ERA5 10-meters wind data (Hersbach et al., 2020), **(b)** conservative temperature (CT) from the RBR© sensor at 50 m depth, **(c)** 1-day moving average of w and **(d)** stick plot of u_{cross} and v_{along} (also smoothed using a moving average) over the period from 6 to 31 March 2024 at 50 m depth. A displacement of the arrows along the x axis indicates a variation in u_{cross}, while variations along the y axis indicate a variation in v_{along}. The color represents the horizontal velocity. The vertical purple lines indicate the period from 15 March (starting at 12:00 h UTC) to 16 March 2024 (ending at 16:00 h UTC), corresponding to the upwelling period.

Lines 357-364 While calculating the specific numerical values for this deployment belongs in the Discussion/Results, establishing the theoretical framing (that submarine canyons in stratified, low-Rossby regimes inherently generate strong downwelling and internal waves) in the

Introduction would give readers the physical context before diving into the observational spectra.

This has been added in the Material and Methods part, when presenting the study area. We chose to introduce this physical context at the beginning of the Material and Methods section because introducing submarine canyons in the Introduction, before presenting the study area, felt somewhat abrupt. We found that placing it here provides a more natural transition from the general introduction to the specific physical setting of our study: "The study area is characterized by a steep continental slope incised by submarine canyons, which can strongly modify the circulation and vertical transport. In stratified oceanic environments, the interaction between along-shore currents and topography can disrupt the local geostrophic balance and promote enhanced vertical motions (Klinck, 1996; Spurgin and Allen, 2014). The interaction of currents with steep topography can also generate internal gravity waves, providing an additional source of vertical velocity variability (Bell Jr., 1975; Gordon and Marshall, 1976; Petruncio et al., 1998; Allen and Durrieu de Madron, 2009; Baines, 2022)" (lines 66–71).

Line 384 "First" and then line 386 "At first."

The beginning of the sentence "At first, a spectral..." has been replaced by "A spectral..." (line 450).

3 Other changes

- The cross-correlation comparison between the IMFs of w and v_{along} has been added to the new Figure 8, based on R1's comment regarding the spectrum of V.

- We added this sentence to further discuss the role of submarine canyons, as we felt that a link was missing in the discussion between the estimated values and the potential influence of submarine canyons on the observed downwelling: "These values suggest that, in our study area, the presence of submarine canyons may favor and enhance downwelling, while the strong stratification may tend to limit the associated vertical motions" (lines 441–443).

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