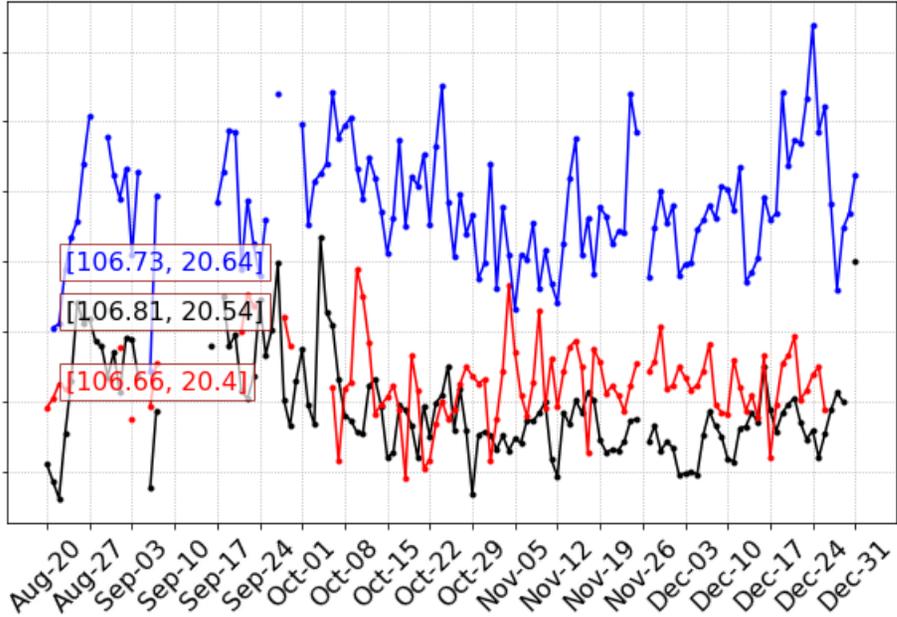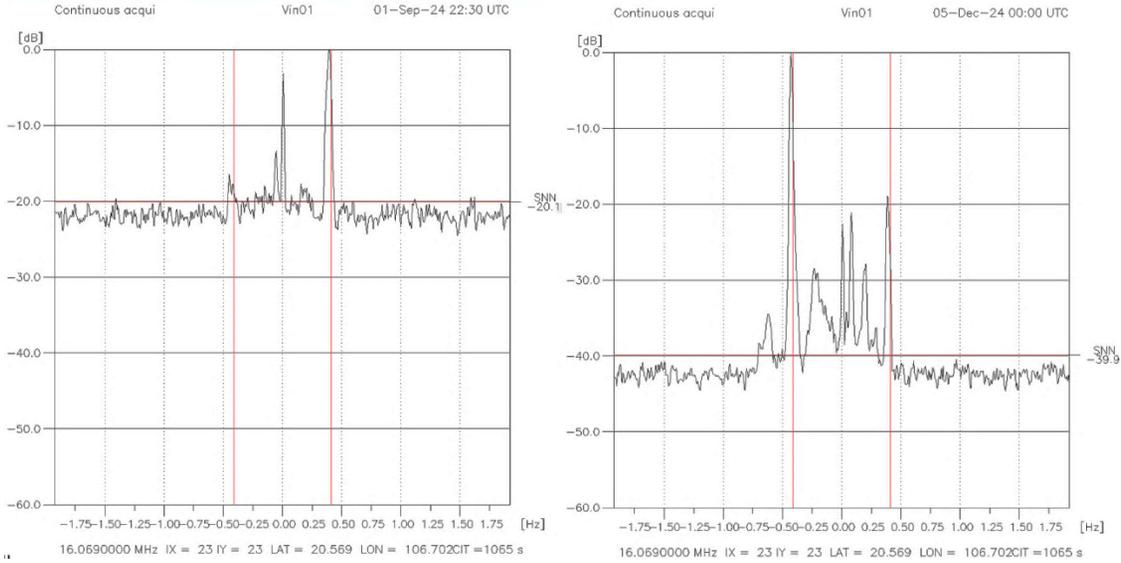**REVIEWER #1**

Dear Reviewer #1, we would like to express our gratitude for your insightful points regarding the impact of discharge on the accuracy of HFR measurements. Below, we would like to clarify in detail our responses to your comments, questions, and suggestions. The text in red (**Lines XX-YY**) indicates the modified lines/figures in the revised manuscript.

ID	Reviewer's comment	Authors' response
RC1#1	• The HFR data and drifter data generally agree. However, there is some question as to whether the accuracy of the HFR current velocity data remains the same even during periods of large river discharge.	• Unfortunately, we did not have a chance to release drifters during summer time due to some technical problems on field. It would have been a more comprehensive comparison, we totally agree. We added some paragraphs to discuss on sources of uncertainties in the flow fields into Section 4.1, including the impact of river discharge on range reduction **(lines 545-562).** And for more detail clarification, we would like to break down to some points as below:
	• Although the paper makes no mention of it, large river discharge leads to lower salinity and reduced electrical conductivity of the seawater. Since ocean radar waves propagate along the sea surface, low seawater electrical conductivity causes greater radiowave attenuation, reducing the signal-to-noise ratio (SNR) of the Doppler spectrum data.	• We agree the low seawater electrical conductivity can reduce working range of HFR system, consequently leads to the reduce in range and effective SNR (due to loss of propagation). The SNR, affecting accuracy of radial current measurements, is influenced by sources of errors that were investigated and discussed by Essen et al., (2000); Paduan and Graber, (1997); Paduan and Washburn, (2013) including sea states, radio frequency interference (RFI), pointing errors and geometric dilution of precision (GDOP). In addition, the accuracy of surface current velocity fields can be degraded in grid cells near river outflow jets due to strong shear and eddy activity, which cause a spreading of the first-order peak in the Doppler spectra. To investigate these effects, we selected three locations: in front of Van Uc (blue), off Van Uc (black), and in front of Tra Ly (red) (Figure below). We found that errors in surface currents (average for u and v components) within the Van Uc river plume (with a mean

ID	Reviewer's comment	Authors' response
		value of approximately 0.06 m s⁻¹) are generally larger than those observed at the other two locations (<0.04 m s⁻¹). However, these larger error values may also be partly attributed to high GDOP, as the area near Van Uc is located close to the baseline between the two radar stations. Furthermore, the accuracy of the surface flow fields does not appear to vary systematically with river discharge, as no clear correlation is observed between the temporal evolution of surface current velocity errors and the discharge time series (Figure 3 in the manuscript).
		Figure: Accuracy of surface current velocity (m s⁻¹) at different locations: in front of Van Uc (blue), off Van Uc (black), in front of Tra Ly (red)
		The constraint caused by baseline was overcome by the HF radar processing using MIO and 2DVar that has been described in the Method section 2.2.1 **(lines 165-182, Figure 4).**
	• I think this results in reduced current velocity accuracy.	• The low-salinity water can affect the uncertainties of the total current velocity at far range (overlapping area reduces), but not directly the accuracy of the radial currents,

ID	Reviewer's comment	Authors' response
		or so far no clear evidences on this argument has been proven. In addition, in the RR plume area, the influence of low-salinity water body does not extend more than 8km offshore and the salinity at the river mouth (Van Uc) is approx. 10-15‰ (Figure 3A, Sentchev et al., 2025), the HFR radars can work at this salinity level (Table 4, Mantovani et al., 2020). The accuracy of the radial currents reduces linearly with the width of the first-order peaks and depends on the number of samples (Parks et al., 2009). We compared two Doppler spectra at a point in front of Van Uc River mouth on the 1 September and 5 December, at comparable night time (to neglect day-night effects on radar performance). For the current measurement, only first order peak is considered. Both peaks were well-defined (sharp peaks, two figures below) from the noise level, even the noise level in September (20dB) is two times smaller than that in December (40dB).

ID	Reviewer's comment	Authors' response
RC1#2	This study performs 2D interpolation of current velocity vectors. When the number of measurement data used for the interpolation differs, the accuracy of the interpolated data also varies. Particularly when river discharge is high, there may be many missing HFR radial velocity measurements. Consequently, the resulting current field is primarily based on 2D interpolation results and is represented as a uniform flow, which may affect the analysis results.	Radial velocity fields were post-processed using the MIO radar-processing software before being used to reconstruct surface current vectors by 2DVar. The authors have furtherly clarified the processing procedure and its results in **lines 165-182 with a newly-added Figure 4.**
RC1#3	The comparison with drifter data was conducted during November and December, when river outflow is small. Therefore, the accuracy of the HFR velocity data is likely better than that during periods of large river discharge.	We agree with the viewer's comment. Please refer to our response to your comment in **RC1#1**
RC1#4	It is necessary to discuss how the number of Doppler spectra sufficient to determine the radial velocity differs between periods of low river discharge and periods of high discharge, and how this difference affects the accuracy of the 2D interpolated current velocity vectors.	In the radar configuration, we used 2048 samples with 512 samples each bin and the setup were the same for the entire measurement period. In MIO software, we increase the integration time to 30 minutes for better identifying the Bragg peaks and also using antenna subspaces for a better statistical distribution of doppler spectra and sources. The method applied in MIO software was described in (Dumas et al., 2025; Dumas and Guérin, 2023). We used the same configuration throughout the analysis period and projected the data onto a universal 1 km grid. The accuracy of the 2DVar interpolation is primarily affected by data availability within the interpolation domain—specifically, whether a given grid cell

ID	Reviewer's comment	Authors' response
		is constrained by observations from both radar sites or from only one site. This point is already addressed in our response to **RC1#1** above.
RC1#5	(1) Around Line 165: The explanation was unclear. Is this simply calculating the distance between the actual buoy and the virtual buoy every 30 minutes for up to 48 hours after buoy deployment? The explanation here seems different. t_i is time and N is the number of buoys, but are they adding these in Equation 1?	A clearer explanation has been provided in the revised manuscript **(lines 207-217),** together with a new **figure A1 added in Appendix A**.
RC1#6	(2) Figure 6: Where is the location? What about the confidence interval? Wouldn't the rotary spectrum be better?	We agree with the reviewer and calculated the rotary spectra. The authors replaced the old figure with a new figure (updated as **Figure 7**) showing mean CW and CCW rotary power spectra curve (of all points over the domain) with confidence interval at ±1 sigma, and modified **the Section 3.2** accordingly.

REVIEWER #2

At first, we would like to thank Reviewer #2 for your suggestions, especially on the explanation of dispersion regimes. Below, we provide detailed responses to your comments. The text in red (**Lines XX-YY**) indicates the modified lines/figures in the revised manuscript.

ID	Reviewer's comment	Authors' response
RC2#1	It is somehow surprising that, despite citing several papers (Bertin et al., 2024; d'Ovidio et al., 2004, 2009; Gough et al., 2016, etc.) employing advanced methods to characterize Lagrangian transport (for example those based on different types of Lyapunov exponents) which seem better suited to describe the filamental dispersion observed in the present data, they are not used here.	We are doing the FSLE method in our future work together with satellite data analysis. Within the scope of the paper, only relative and absolute dispersion were used. The future work is mentioned in Conclusions **(lines 685-689)** The introduction section was modified to clarify this scope. **(lines 101-113)**
RC2#2	Some additional data are needed about the drifters used (sect. 2.2.2), in particular their size, if they have some type of subsurface drogue or not, and what proportion of their heigh is exposed to the wind. These data are needed to estimate how much do these drifters follow the surface currents measured by the radar and how much are they influenced by wind. Some brief discussion on this is missed in the paper. In addition, an indication of the duration of the tracking time of the drifters is needed in sect. 2.2.2, not restricting this information to just what can be seen in the figures.	The buoy information has been added to the section 2.2.2 **(lines 187-192)**
RC2#3	The definition of the Lagrangian error previous to Eq. (1) is not clear enough. For example, what is the average indicated by brackets <> there?	A clearer explanation has been provided in the revised manuscript **(lines 207-217),** together with a new **figure A1 added in Appendix A**.
RC2#4	The discussion of the different relative dispersion regimes (Richardson, ballistic, etc.) is somehow superficial. At least some statement should be done on the fact that Richardson is expected in the direct 3d cascade and in the inverse-cascade regime in 2d, and	We thank the referee for raising this point and for suggesting some clarifications. The text in section 2.3.1 **(lines 232-246)** has been revised to clarify the theoretical context of the different relative dispersion regimes. In particular, we explicitly stated that Richardson's super-diffusive regime is expected in the inverse

ID	Reviewer's comment	Authors' response
	comment in which sense these may be appropriate for the spatial scales analyzed in this paper. Also, the comment on the case beta=-1 is inappropriate, since this is non-physical (infinite energy in the system) and also violates the locality assumption leading to the exponent 4/(3-beta) for the time exponent of delta^2.	energy cascade of 2D turbulence, which is relevant for horizontal transport at the sea surface. Moreover, in the Discussion (lines XX-YY) we have added explanations that at short time intervals (20–40 h) analyzed in our study, the dispersion is expected to reflect a transitional regime influenced by the inverse energy cascade, rather than a fully developed Richardson regime. This point is supported by scaling of $\delta^2(t)$ not matching exactly t^3. In the revised MS, **lines 232-237** *Dispersion at the sea surface is governed by turbulent motions spanning a range of spatial scales and is therefore closely linked to the kinetic energy distribution of the flow. Previous studies (e.g., LaCasce, 2008; Berti et al., 2011) have shown that, **under locality assumptions**, relative dispersion may follow a power-law growth of the form $\delta^2(t) \sim t^{4/(3-\beta)}$* *[... and the text continues ...]* We agree with the referee that a k^{-1} spectrum cannot correspond to a physically reliable inertial-range energy cascade and that such a spectrum does not satisfy the standard locality assumption. We have therefore clarified this point in the revised manuscript, **lines 243-246:** *"For $\beta=-1$, the particle-pair separation satisfies $\delta^2(t)\sim t$, reflecting a diffusive, non-local dispersion regime. In this regime, particle separations are driven by large scale velocity gradients*

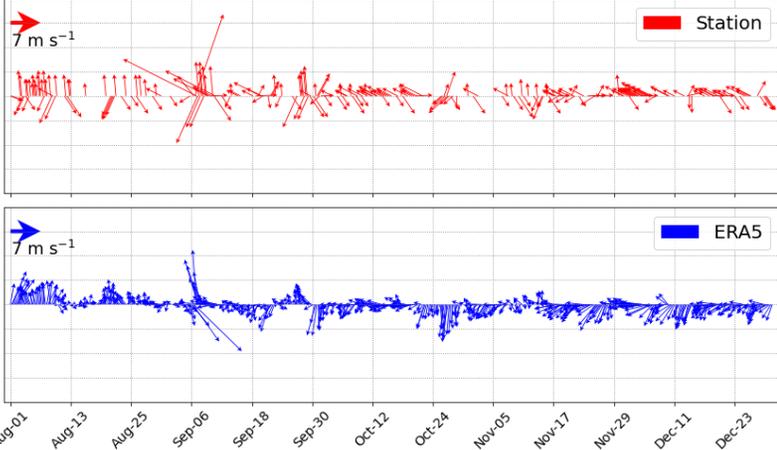| ID	Reviewer's comment	Authors' response
		rather than by eddies of size comparable to the separation scales. This case should therefore be understood as a formal limiting behavior, rather than as a physically realistic turbulent cascade. For β=−1, the particle-pair separation satisfies d2(t)~t, reflecting a diffusive, non-local dispersion regime. In this regime, particle separations are driven by large scale velocity gradients rather than by eddies of size comparable to the separation scales. This case should therefore be understood as a formal limiting behavior, rather than as a physically realistic turbulent cascade. "
RC2#5	There is a square root missing in some terms of Eq. 3a. Also, R_i^2(t) are the semiaxes length, not the axes length.	We modified toward your comments **(lines 250-252)**
RC2#6	The sentence starting at the end of line 235 seems unfinished.	We have corrected the sentence and moved this sentence to the **Discussion Section 4.1 (lines 542-543)**
RC2#7	In line 204: '… how large particles …'. Why 'large'?	We rmoved the word "large" **(line 257)**
RC2#8	Some indication on the duration of trajectories is needed in the caption of Fig. 4.	We added the duration of tracking to **figure 5** (in revised manuscript) toward your comments
RC2#9	Paragraph around line 240, and Table 1: minimum velocity values are mentioned, but not presented in Table 1. In addition, a definition should be given for the maximum velocity values: in turbulent flows this can depend on the time step on which velocities are computed, so this time step should be stated. Also, in Table 1, please define 'mean' and 'max' value of 'Sep. dist'.	We added the definition into the paragraph **(lines 285-288)**
RC2#10	Please state in section 3.3 or 3.4 how particles are initially distributed, instead of restricting this information to what can be seen in the figures.	The information of the initial distribution of particles was added in **section 3.3 (line 375-380)**
RC2#11	In Fig. 8, what does it mean 'Circles illustrate final positions'?	The caption of **Fig. 9** (in revised manuscript) was modified for clearer explanation.

ID	Reviewer's comment	Authors' response
RC2#12	• In many parts of the text statements such as '… followed by a Richardson's super-diffusive …' or '… dispersed with an exponential growth …' and similar ones appear. I do not see perfect power laws or exponential in most of the referred plots, as is expected from the lack of isotropy, homogeneity, stationarity, etc. of the studied flow. I suggest to somehow weaken this type of claims as for example '…approaching a Richardson …', '…approximately exponential …' at least in the more evident places. • Also, in most figures the statement 'black dashed lines are fitting curves' appears. But these lines do not appear to be fitting the data. Are they fits or guides to the eyes? If they are fits, please specify how are they calculated.	• We have modified the terminology for more accurate description of the dispersion characteristics in **Section 3.4, Discussion and Conclusion.** • The captions have been modified for more accurate definition of empirical curves used for comparison **(Figures 10 & 11 & 13)**

REVIEWER #3

At first, we would like to thank you for your very detailed, comprehensive, and constructive comments and suggestions. Below, we would like to clarify in detail our responses to each point of your comments. The text in red (**Lines XX-YY**) indicates the modified lines/figures in the revised manuscript.

ID	Reviewer's comment	Authors' response
RC3#1	**Abstract:** • I suggest changing the introductory sentence to something that describes the "why" of the study rather than jumping right into the methods. Why is this novel? Why is this important? • The second sentence starts with describing how much variability is in the system and ends with "permanent submesoscale eddies". I suggest breaking this into two sentences and/or clarifying what you mean by "permanent". I don't think "permanent" is used elsewhere in the manuscript to describe these eddies. • The last sentence of the abstract should include context as to what results from this study mean for the open questions in this region. How is this novel?	Regarding the Abstract, we have adjusted the abstract accordingly in the revised manuscript: • The answer for the question "why" was added in **Lines 12-15** in the revised abstract. • We removed "permanent" • We added the implication of the research results for the region (**Lines 24-26**)
RC3#2	**Introduction** • Line 50: resolution is presented as both a strength and a weakness of satellite data? I would leave resolution as only a weakness of satellite data, especially in comparison to HFR. • Line 60: "cannot be fully resolved by numerical models". I'm not sure if this is true. Perhaps this is true for the current state of models in the GoT? This is also not a great segway into the next paragraph, which doesn't mention the resolution capable of HFR observations and/or how they capture finer scale features than models.	Introduction • we removed resolution as a strength, updated in line … of the revised manuscript (**line 52**) • we remove the model, updated in **lines 62-63** of the revised manuscript

ID	Reviewer's comment	Authors' response
	• Line 61-65: I think it is worth going a bit deeper into how the HFR work, such as the doppler-shifted backscattering radio waves, that two radars are needed because each one gives a radial measurement, etc. • Line 85: I think the references here need to be broken up into several different claims. I agree with RC2 that it is strange that many advanced methods to characterize Lagrangian transport were cited but not employed (ex: FSLE in d'Ovidio et al 2004 and Hernandez-Carrasco et al 2011 FTLE in Gough et al., 2016). I suggest this sentence is broken into several sentences that outline the literature like below: Claim 1: Lagrangian diagnostics have been widely employed and they provide information that cannot be seen from Eulerian techniques (d'Ovidio et al 2009 and maybe a few more); Claim 2: These techniques can be deployed to HFR (Gough et al 2016, Veatch et al., 2024, McKee et al 2025, …https://academic.oup.com/icesjms/article/81/4/760/7633546, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2024JC022101; Claim 3: Past studies have used Lagrangian techniques in GoT with lower resolution (Sentchev et al 2025, Tran et al 2022)	• we elaborated more details on HFR technology in the revised manuscript **(lines 62-79)** • We modified the last paragraph in the Introduction chapter toward your comments in the revised manuscript. **(lines 101-113)**
RC3#3	**Materials and Methods:** • Line 101: is this a upwelling scenario? Explain for clarity and readability; Line 105: downwelling scenario? • Line 108: add Son Tay station to figure 1? • Line 113: are dams released when there is lots of runoff (large rain event?) or are the dam releases and the typhoon unrelated?	 • The extended explanation of upwelling and downwelling conditions has been added to the text **(lines 119-121, 122-123)** • We added the location of Son Tay station to a new figure, (**Figure 1a**) and coordinates in **line 197** • Yes. Dam releases during high runoff (including typhoons) are strictly regulated. As Son Tay is the last downstream station measuring discharge and water level for hydrodynamic

ID	Reviewer's comment	Authors' response
		modeling, it captures dam-release contributions, though this information is not essential for our dispersion analysis.
	• Figure 1: I suggest adding an inset of a map of the larger region for context. Were the C1 and C2 release locations in that area for a scientific reason? Was this an operational constraint? I think that is worth mentioning somewhere. Also, I recommend you add a kilometer scale to the map (key). • Figure 2: Why was ERA5 data used and not data from the Hon Dau meteorological station? How does ERA5 compare to observed data (if available)?	• Figure 1 was modified by adding **Figure1a** showing a bigger region (GoT), and Figure 1b showing the RR plume domain. Km scale bar was added in both two figures. The explanation of release locations was explained in updated manuscript **(line 185)** • We have compared wind data at Hon Dau with ERA5. The data at Hon Dau station was quite poor in quality, not representative and was very coarse in temporal resolution (4 measurements/day) and illustrated only 8 directions (please see figure below).

ID	Reviewer's comment	Authors' response
	• Line 134: How were the subperiods decided? It is not clear to me if these three subperiods correspond to those mentioned later in the text. Can you mark these subperiods on Figure 2 or 3?	• We explained the reasons for data interruption that led to the division of three subperiods. This was a bad explanation, we agree. We modified the text in the **lines 157-160**
	• Line 145 needs more info. How big were the deployed drifters? How much surface expression/draft of the buoy?	• We added the information of buoys in Section 2.2.2, **lines 186-189**
	• Line 175: How the buoy differs from simulated, massless particles is not mentioned here (ex. Friction, more influence of wind with surface expression, etc.)	• We added the information of buoys in Section 2.2.2 with the information of wind effects
	• Line 180: should be "2.3.2"	• We corrected it
	• Line 182: Are particle pairs those that are initially neighboring? Or particles from the whole cluster?	• We meant here the particles within the whole clusters (Eq. 2)
	• Line 185: Were particles released in a uniform grid? How many particles per cluster? What is the initial separation of particles? Are these the same clusters that were described in 2.3.1?	• We added information of number of clusters, initial spacing in **lines 375-378**. The virtual clusters used in the Section 3.4 (for dispersion behavior characterization purposes) are not the same with the ones used in 2.3.1 (for validation purposes).
	• Line 187: is the method described here the same that is used in the referenced manuscripts?	• Yes.
	• Line 194: This is a slightly confusing opening sentence. Does this theta(t) and R21 R22 give info on the direction of the dispersion? Which isn't given by delta squared(t)?	• The R_1^2 and R_2^2 and $\theta(t)$ give the information of the positional variances illustrated by the size and the direction of ellipses shown in **Figures 8, 11, 13**. Meanwhile, $\delta^2(t)$ gives information of spreading intensity in time dimension.
	• Line 210: "cluster" – all metrics calculated with the same release clusters?	• This "cluster" mentioned in line 210 of the original manuscript refers only to the general definition of the method. The method was applied to all clusters analyzed in the paper.
RC3#4	**Results:**	
	• Line 225: Move this to discussion and elaborate more?	• We agree and moved the text to Section 4.1 with more explanation of the sources of uncertainties (**lines 543-561**)
	• Figure 4: were releases during similar points in the tidal cycle?	• Yes, the real drifters were released sequentially within about one hour (≈15 min apart), under similar tidal conditions.
	• Figure 5: move title to y-axis label	• We changed toward your suggestion (now it is **Figure 6**)

ID	Reviewer's comment	Authors' response
	• Line 258-260 feels out of place. Plot tidal amplitude spatially to elaborate on this point? Or omit? (Spatial tidal amplitude example: Figure 7A Veatch et al 2025, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2025JC022447)	• We would like to discuss inertial forcing here, compared to tidal influences, to indicate which prominent processes contribute to the (rotary) power spectra (we replaced PSD with rotary PSD following Reviewer 1's suggestion, RC1#6), and to explain why inertial forcing becomes less important than tides, which can be a major contributor to coastal circulation variability in other regions. Besides, the domain is small (~75x75km) and the spatial variation of tidal amplitude does not vary much (5 to <10 cm) within the domain (Minh et al., 2014; Piton et al., 2020), therefore, we think it is not necessary to plot it in the paper.
	• Line 265: do these time periods correspond with those mentioned in the methods?	• Yes.
	• Line 315: move to discussion and elaborate? Or re-visit in discussion?	• Wind and discharge influences have been discussed in Discussion (Section 4.2, paragraph 1, 3). We added a discussion about tidal influence into Section 4.2 (**lines 590-591**)
	• Line323: how many particles in each cluster?	• We added information of number of clusters, initial spacing in **lines 375-378**.
	• Figure 8: using green and red together is not very colorblind friendly. Could you separate these two panels into four and not use so many colors?	• We changed all figures to colorblind friendly.
	• Line 363: tie bathymetry back to 20m isobath delineation shown in Figure 7? I think it would be nice to have a discussion section on the influence of bathymetry.	• The sentence was modified toward your comment **(lines 597-599)**
	• Figure 10: change "20241205TZ000" to "December 5th 2024 0:00" to be more readable	• We changed all dates to readable ones.
	• Line 409: add August 28 to supplementary material?	• The figures of surface currents on Aug 26 and 28 have been added into **Appendix A, Figure A2**.
	• Figure 11: see comment from Figure 10. Also, there are many colorblind friendly and perceptually uniform	• We changed all figures to colorblind friendly.

ID	Reviewer's comment	Authors' response
	colormaps available, such as those in cmocean (https://matplotlib.org/cmocean/) • Line 432: where are the "two coherent structures (eddies)"? one is deeper than 20m and one is shallower rotating from TraLy to ThaiBinh? • Figure 13: change dates and colormap to be more readable (see above suggestions)	• Yes. We modified the sentence for more readable and more precise (**line 490**) • We changed all figures to colorblind friendly.
RC3#5	**Discussion:** • Section 4.1 is really interesting! • Line 489: I think exploring more reasons why there is a 19% difference would be really useful here. We don't expect in situ buoys to behave exactly like virtual, masses particles. RC1 brings up a good point about the reduction in electrical conductivity of the seawater during larger river discharge events which could have a negative impact on the quality of HFR observations. I don't believe there is any current work-around to this problem, but it is worth discussing. • Line 496: explicitly stating that there is offshore flow in this scenario might make this section easier to follow. • Line 501-503: this type of discussion is missing from the downwelling scenario a few lines above. • Line 520: are these the same eddies referred to in line 432? • Line 526-531: restating results? Start with a conceptual take-away.	• We added some paragraphs to discuss on sources of uncertainties in the flow fields into Section 4.1 (**lines 543-561**) • We reworded to "offshore plume extension", it is not exactly offshore flow (**line 574**) • We are not sure which lines or scenario the Reviewer is referring to. Lines 500–505 of the original manuscript (**lines 579-581** in revised version) already discuss the winter scenario (i.e., the "downwelling" condition), characterized by enhanced vertical mixing and a river plume that is confined and narrowed near the coast. • Thanks for raising this unclear point, they are different eddies. The eddy was discussed here in at seasonal scales (referred to **Figure. 8** in updated manuscript). We reworded from "submesoscale features" to "seasonal submesoscale features" to improve clarity for readers (**line 600**) • We added a statement summarizing all four cases in response to the reviewer's suggestion. (**lines 606-607**)

ID	Reviewer's comment	Authors' response
	• Line 535: tie back to this study. Are these features resolved by the HFR? • Line 545: How does this fit into the current understanding of flow in the GoT? Line 545: How does this effect results? • I think the discussion would benefit from moving some "discussion" text from the Results to this section and expanding on a few topics such as (1) how the 20m isobath acts as a border between two different types of flows (2) forcings that create the eddy in Figure 11b and (3) the influence of the Typhoon on local circulation and dispersion.	• With a 1 km resolution, HFR can resolve flow motions at these scales. This statement is intended to explain why dispersion is more intense in summer than in winter. • We added the explanation and relation with previous studies and our study context (**lines 633-636)** • We would like to retain some case-specific discussion in the Results to facilitate reading, and then expand to a broader synthesis, referring to existing studies, in the Discussion section.
RC3#6	**Conclusion:** • Line 581: "structure with sizes of 10-30km" this information is missing from the discussion.	• We omit specifying the sizes of eddies. (**lines 679-680**)
RC3#7	I think further analysis with uniform grid releases over the whole domain could tell us more about the boundaries between the different oceanographic regimes that were suggested by the current results and the coherent structures (eddies) mentioned throughout the text. This further analysis could be as simple as the residence time experiments done in Kohut et al. 2018 (https://royalsocietypublishing.org/rsta/article/376/2122/20170 165/115543/Variability-in-summer-surface-residence-time) or the more complex Lyapunov experiments that are in many of the citations for this paper (Veatch et al 2024: https://academic.oup.com/icesjms/article/81/4/760/7633546). If this further analysis is not included in the final paper, I suggest that it is outlined in the Line 590 as a "future study".	Thank you for the suggestion. Actually, we have tested the approach already but we did not include in the current paper. For the future work, we would like to use similar approach with better representation of LCSs, i.e., FSLE. Thus, within this paper, we instead mention FSLE as a potential extension in the final paragraph of the Conclusion (**lines 685-670**).