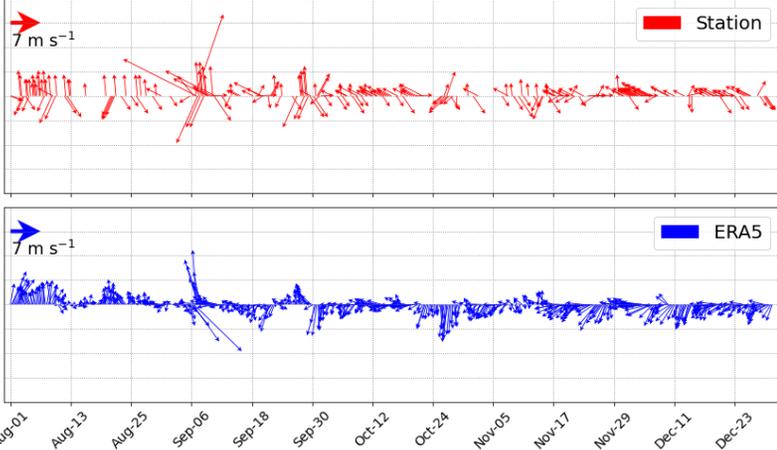


REVIEWER #3

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	<p>Abstract:</p> <ul style="list-style-type: none">• 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?	<p>Regarding the Abstract, we have adjusted the abstract accordingly in the revised manuscript:</p> <ul style="list-style-type: none">• 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	<p>Introduction</p> <ul style="list-style-type: none">• 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.	<p>Introduction</p> <ul style="list-style-type: none">• 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

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	<ul style="list-style-type: none"> • 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) 	<ul style="list-style-type: none"> • 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	<p>Materials and Methods:</p> <ul style="list-style-type: none"> • 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? 	<ul style="list-style-type: none"> • 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

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	<ul style="list-style-type: none"> 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)? 	<p>modeling, it captures dam-release contributions, though this information is not essential for our dispersion analysis.</p> <ul style="list-style-type: none"> 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). <p style="text-align: center;">Station HonDau vs. ERA5 1-Jun to 31-Dec</p> 

ID	Reviewer's comment	Authors' response
	<ul style="list-style-type: none"> • 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? • Line 145 needs more info. How big were the deployed drifters? How much surface expression/draft of the buoy? • Line 175: How the buoy differs from simulated, massless particles is not mentioned here (ex. Friction, more influence of wind with surface expression, etc.) • Line 180: should be “2.3.2” • Line 182: Are particle pairs those that are initially neighboring? Or particles from the whole cluster? • 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? • Line 187: is the method described here the same that is used in the referenced manuscripts? • 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)? • Line 210: “cluster” – all metrics calculated with the same release clusters? 	<ul style="list-style-type: none"> • 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 • We added the information of buoys in Section 2.2.2, lines 186-189 • We added the information of buoys in Section 2.2.2 with the information of wind effects • We corrected it • We meant here the particles within the whole clusters (Eq. 2) • 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). • Yes. • 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. • 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	<p>Results:</p> <ul style="list-style-type: none"> • Line 225: Move this to discussion and elaborate more? • Figure 4: were releases during similar points in the tidal cycle? • Figure 5: move title to y-axis label 	<ul style="list-style-type: none"> • We agree and moved the text to Section 4.1 with more explanation of the sources of uncertainties (lines 543-561) • Yes, the real drifters were released sequentially within about one hour (≈ 15 min apart), under similar tidal conditions. • We changed toward your suggestion (now it is Figure 6)

ID	Reviewer's comment	Authors' response
	<ul style="list-style-type: none"> • 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) • Line 265: do these time periods correspond with those mentioned in the methods? • Line 315: move to discussion and elaborate? Or re-visit in discussion? • Line323: how many particles in each cluster? • 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? • 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. • Figure 10: change “20241205TZ000” to “December 5th 2024 0:00” to be more readable • Line 409: add August 28 to supplementary material? • Figure 11: see comment from Figure 10. Also, there are many colorblind friendly and perceptually uniform 	<ul style="list-style-type: none"> • 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. • Yes. • 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) • We added information of number of clusters, initial spacing in lines 375-378. • We changed all figures to colorblind friendly. • The sentence was modified toward your comment (lines 597-599) • We changed all dates to readable ones. • The figures of surface currents on Aug 26 and 28 have been added into Appendix A, Figure A2. • We changed all figures to colorblind friendly.

ID	Reviewer's comment	Authors' response
	<p>colormaps available, such as those in cmocean (https://matplotlib.org/cmocean/)</p> <ul style="list-style-type: none"> 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) 	<ul style="list-style-type: none"> Yes. We modified the sentence for more readable and more precise (line 490) We changed all figures to colorblind friendly.
RC3#5	<p>Discussion:</p> <ul style="list-style-type: none"> 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. 	<ul style="list-style-type: none"> 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
	<ul style="list-style-type: none"> • 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. 	<ul style="list-style-type: none"> • 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	<p>Conclusion:</p> <ul style="list-style-type: none"> • Line 581: “structure with sizes of 10-30km” this information is missing from the discussion. 	<ul style="list-style-type: none"> • We omit specifying the sizes of eddies. (lines 679-680)
RC3#7	<p>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/376/2122/20170165/115543/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".</p>	<p>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).</p>