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

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	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 |

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		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.

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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.	• We have modified the terminology for more accurate description of the dispersion characteristics in **Section 3.4, Discussion and Conclusion.**
	• 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.	• The captions have been modified for more accurate definition of empirical curves used for comparison **(Figures 10 & 11 & 13)**