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# *Interactive comment on* "Relative dispersion in the South Western Mediterranean as derived from satellite-tracked surface drifting buoys" *by* Maher Bouzaiene et al.

**Anonymous Referee #3**

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The manuscript describes the relative dispersion of surface drifter pairs in the South Western Mediterranean (SWM) Sea. The topic has a high scientific relevance because the authors attempt to quantify the surface Lagrangian dispersion at the entrance of the SWM, where Atlantic water enters the region. However, the scientific quality of the study is rather modest. The authors invoke classical dispersion regimes, but these are not sufficiently discussed, and perhaps not very well understood. The arguments and plots to support the presence of different regimes in the SWM are not convincing. In terms of the presentation quality, the manuscript requires major revisions: several sentences seem incomplete or they are very obscure; dispersion plots do not show or mention confidence intervals, statistical significance nor any other argument to support



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their robustness. This should be done in a revised version.

Some specific comments are the following. There are no clear interpretations or discussions in some cases, and I will mention only three examples. First, the authors argue that the ballistic behavior is observed in other regions, e.g. in the Adriatic Sea and in the Santa Barbara Channel. However, the length and time scales in the Santa Barbara Channel are completely different, and in that case it is argued that shear dispersion might play a role on the shape of the dispersion curve (Ohlmann et al. 2012). What is the physical mechanism responsible for the apparent ballistic regime in the SWM? To examine this question, I think the authors should take into account more carefully the role of relevant circulation features in the region, namely, the strength, size and direction of the eastward Algerian Current (see Salas et al, J. Mar. Sys. 2001). A second point that demands further discussion is the behavior of the relative velocity in Figures 2 and 4. Panels a, b and c suggest that the velocities between particles are decorrelated after 25-35 days. It sounds that this is a long time, but the authors do not argue very much on why. Is it related with the presence of very persistent currents? A similar result would be found if only drifters near the North African coast are taken into account? Furthermore, the squared relative velocity is often discussed as a function of the pair separations (see e.g. Beron-Vera and LaCasce, 2016) in order to identify the decorrelation length scales; then, further inferences can be made regarding 2D turbulence or shear dispersion regimes. A third example is the interpretation that the Lundgren regime is observed for large initial separations. Other studies have indeed observed a similar exponential growth, but the reason cannot be attributed to the exponential regime in 2D turbulence, which relies on small initial separations within the enstrophy cascade subrange. So, if the authors report exponential growth is fine, even when the particles are initially very far from each other, but the interpretation should be more careful.

One way to improve the study (besides including statistical significance tests) is by exploring more metrics, and not relying only in the time-dependent relative dispersion.





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The authors should try to look at higher statistical moments or, more importantly, to the PDFs of separations. There are also scale-dependent metrics that might be considered (FSLEs). My general comment is that the statistical analyses of surface drifters might be incomplete because the observational evidence is scarce; thus, the actions to fix this problem is to consider a more ample number of statistical metrics. Also, it seems important to distinguish whether drifters in some particular regions generate some bias in the statistics, which reflects the relevance of local circulations.

One more comment: Two types of surface drifters with different nominal depths are considered, CODE (1 m) and SVP (15 m). The text should describe how many drifters of each type were considered, because in principle their functioning is different. The CODE drifters might be strongly influenced by the wind, while SVP drifters with a drogue tend to follow geostrophic currents, depending on the surface Ekman layer thickness. There should be some justificatory arguments to use both types in the statistics. Or, ideally, statistics calculated separately should be similar.

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