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Ocean Science Discussions

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 #4

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This manuscript presents simple analyses of basic two point statistics from a modest ensemble of surface drifters released in the South Western Mediterranean over the past 20 years. The authors claim that the results confirm, or are at least consistent with four standard scaling law regimes.

While the effort to synthesize existing surface drifter data in this region is certainly welcome, as is the attempt to organize and catagorize the results in the context of previously observed or predicted scaling behavior, the overall scientific quality of the present manuscript falls short of that required for publication. Most importantly, there is a distinct lack of quantitative analysis of the results to support the broad claims made

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in the abstract. Instead, the existence of various scaling regimes is vaguely intimated by simply plotting lines of different slope on log-log plots (often with axes extended well beyond the bounds of the data). Given the increasing availability of surface drifter data and the potential ability to connect two-point Lagrangian statistics to spectral properties of fluctuating surface velocity field, understanding and quantifying the existence of scaling regimes, their dependence on initial separation distance and how such properties vary between geographic locations is an important problem. Unfortunately, it is not at all clear how the results as currently presented in this manuscript clarify any existing questions about surface dispersion.

Specific points: (1) The authors concentrate on relative dispersion and its time derivative without providing any measure of the variability in these quantities or the statistical significance of the plotted estimates. What are reasonable error bars on the plots? How well (or poorly) is the data fit by any of the proposed scaling laws? Some reasonable, standard statistical hypothesis testing should be done to support the authors' claims.

(2) The choice of metrics is a bit narrow, and somewhat redundant. Diffusivity and 'characteristic dispersion time' are derivative measures of the relative dispersion, itself simply the time-dependent second moment of the distribution of separation distances. Given that the second moment is dominated large separation pairs, Richardson himself argued for consideration of the pdf. The finite-scale Lyapunov exponent (FSLE) would be a natural metric to consider when searching for exponential regimes.

(3) The authors combine data from two types of drifters, sampling different depths and presumably possessing different response properties. Can one show that these two instruments sample similar velocity fields? Are the resulting distributions of two-point separation statistics statistically similar?

(4) What is the effect of the substantial amount of data smoothing on the results? Is this the same for both the very near surface and deeper drifters? As explained in

section 2, the data are low-pass filtered with a 36 hour cutoff, 'to remove tides and inertial oscillations'. The effects of this smoothing are clearly seen in the results at early times. One could imagine that this smoothing preferentially effects the behavior at smaller initial separation distances. What is the reader to make of the short-time, 1-2 day results in the presence of such averaging?

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