

Interactive comment on “A gridded surface current product for the Gulf of Mexico from consolidated drifter measurements” by Jonathan M. Lilly and Paula Pérez-Brunius

Anonymous Referee #1

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Overview

This manuscript represents a vast amount of work to synthesize many different data sets, and is an incredible resource for future studies of circulation in the Gulf of Mexico. It should be accepted for publication.

Future users of these data, and readers in general, should pay careful consideration to the caveats noted in section 4. In particular, the drifters have various drogue depths, and some of the data sets do not include drogue presence flags. It would be very interesting to see a comparison of Fig. 2d with a similar map made from only the drogued drifters, and (for those without a drogue flag) drifters at wind speeds only below a rea-

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Discussion paper



sonable threshold (using winds from a satellite product). That may extend beyond the scope of this study, but would illustrate the impact of drogue loss. The authors should make clear whether the undrogued data (for those subsets with this flag) were used for Fig. 2d, Fig. 3, etc. It's clear how the data are flagged in GulfDriftersOpen, but now if the undrogued data were used here.

The authors note that simple bin average can produce spurious results due to biased sampling of particular mesoscale features during dense, short-term efforts such as LASER. They address this by averaging first in spatial bins with a one month time window, overlapping by half a month. In principle, this should be longer than the Eulerian time scale of the mesoscale variability, which the authors note later in the manuscript. It is reassuring that they applied the methodology to model output to validate it.

Specific comments:

38-40: "notwithstanding the global study of Laurindo et al. (2017) using exclusively Global Drifter Program drifters, no mean circulation maps for the Gulf of Mexico from a drifter-derived dataset have appeared in the literature since perhaps DiMarco et al.(2005) and Nowlin et al.(2001), .." Why does the map of Laurindo et al. (2017) or earlier versions cited therein, not count? This is unclear.

Section 5: it was very interesting to see the resolved vs. subgrid variance in these data. As noted by the authors, this highlights the energy levels in the submesoscale where large subgrid values are found - for example, in the Mississippi outflow. It would be interesting to examine if larger values are found in winter months, when the submesoscale may be stronger, in a future study.

Typos

30: "those derived from of dense surface drifter deployments".

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