

Interactive comment on “Vertical Structure of Ocean Surface Currents Under High Winds from Massive Arrays of Drifters” by John Lodise et al.

Anonymous Referee #2

Received and published: 2 May 2019

The paper by Lodise et al concerns the behavior of undrogued and drogued so-called CARTHE drifters. I agree with Ref #1 that lack of structure is a major problem in this paper, it is surprisingly hard to follow the authors as they describe their methods and present their results. A few recent papers are lacking, although Ref #1 mentions that as well.

What concerns me the most is the mix of tools used here in combination with a lack of uncertainty estimates: A coupled atmosphere-wave-ocean modeling system is used to provide the physical quantities needed to analyse the drift, but instead of using the ocean model results for the vaguely defined "pre-existing" circulation, the drifter trajectories are used to provide ocean circulation estimates that are later used in the analysis of the drifters themselves. Even though one can argue that the circulation estimates

C1

are independent of the drifter observations from the "high wind" periods later on, I still wonder how representative this "pre-existing circulation" is and feel uncomfortable about this circular use of the drifter data. Why discard the ocean model component entirely? Isn't the upper layer in the model even representative of the background circulation? If not, what is the point of the ocean coupling? And surely there are other ocean models that could be used, at least for comparison to the LAVA results? The model coupling will also need to be better described: How is the coupling implemented? Are wave-dependent air-sea momentum fluxes part of the coupling (both to atmosphere and ocean) for instance? It is precisely in situations with rapidly changing weather conditions that these couplings become important (see Rohrs et al., Ocean Dyn., 2012, for estimates of wave dependent fluxes from directional wave rider data and an analysis of upper ocean drift relevant to the paper under review). Although it appears to be a good idea to split the upper ocean drift velocity into "wave", "wind" and "background" components, it really depends on the ability to delineate them, and I'm not entirely convinced in this case. The CARTHE drifters are influenced by the Stokes drift, how much of the LAVA circulation estimates still contain some Stokes drift despite making model based corrections etc.? I'm not sure what the authors should do to present a more convincing case, but I would at least need to see a much clearer presentation with more emphasis on the various potential sources of error in the estimates. Alternatives to the LAVA circulation estimates should definitely be considered (and compared to the LAVA estimates). This work would constitute a major revision of the current manuscript.

Some minor comments: Eq. (1) isn't an average, "1/h" is lacking. A short description of the typical hydrographic structure of the upper ocean in the region (mixed layer depths, buoyancy frequencies, salinity/temperature profiles etc.) would be nice. Please add some info about the spectral resolution in the wave model as well. Eq. (2) is not correct for a model with the limit at infinity, there is a cutoff frequency, and presumably a specific shape of the spectrum is assumed.

C2

