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Ocean Science Discussions

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

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General comments: This paper presents very interesting data on near-surface drifter velocities that are interpreted in terms of near surface currents. This interpretation probably requires a clearer explanation of how the drifter velocity related to the surrounding water motion. I strongly encourage the authors to clarify this and resubmit their paper.

Specific comments:

1. The author write about "surface current" when they actually mean "drifter motion". The distinction is important as the drifter without the drogue will move due to the direct effect of the wind and of the waves, in addition to that of the current. In Novelli et

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al. (2017), the undrogued drifter moves at a speed that is significantly larger than the surface current (their figures 10 and 11), of the order of 10 cm/s for 10 m/s wind. However, it is unclear how this difference scales to the open ocean with very different wave ages and vertical mixing: this cannot scale with the Stokes drift (the Stokes drift in the lab is under 0.5% of the wind speed...). It is very unclear how the difference between water and drifter motion is estimate or corrected.

2. The idea of a "purely wind-driven current" should be clarified, in particular how the time varying wind produces a time-varying current, including a phase shift in time. On page 9, line 9, I guess there is some wind influence already in the "pre-existing regional circulation, u_rc"

3. There is not a single mention of density, temperature or salinity in the paper. It is expected that the surface response to the wind is very sensitive to the stratification (slippery layers, e.g. Kudryavtsev et al. JPO 1990). So that the present data is impossible to interpret without that information in the context of the wider litterature.

4. Numerical models or parameterizations of waves primarily design to get wave heights can disagree a lot on the short wave purely wind-driven current components that contribute to the Stokes drift (e.g. Peureux et al. 2008). Hence it would bee good to show a specific model validation on the wave spectrum in the 1 m to 40 m wavelengths regime that dominates the Stokes drift.

Technical corrections:

- Page 1: line 28: replace CARHTE with CARTHE

- in the paragraph "Observational data that captures the vertical shear within the first meter of wind-driven surface currents is very limited in the real ocean as well" the authors could reference some important work (Santala & Terray 1992)

- Page 2, line 28: "twice as fast as the average current over the first 1m and four times as fast" is misleading as a casual reader could think that over a 2 m/s Gulf Stream he

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would also have a 2 m/s wind shear. Please give a velocity difference in cm/s and / or scale it with the wind speed. Please also note that these shears should be mixed by wave breaking and should thus be much smaller in the open ocean than in the lab or in coastal areas / weak winds. As a result, lab studies are largely irrelevant for the open ocean. In that respect, Sutherland et al. (2016) is a relevant reference.

- Page 2: line 31. Classical Ekman theory stricto sensu (in particular the 45° !) does not apply to the real ocean. Please consider at least realistic mixing (Madsen 1977 or Rascle et al. 2009 are better).

Page 2: line 24: 0.5 m is optimistic,

Page 3, line 5: "anywhere from 0.4 % to 5 %" is not a scientific statement. The uncertainty is much less than this range, as most of the variability in horizontally homogeneous conditions is known function of the wind speed and stratification (Ardhuin et al. 2009). Besides, I did not find in Berta et al. a clear numbers on a "wind-only' component.

Page 4, line 31: please replace "current" with "drifter velocity"

Page 5, line 9: The acceleration is not just due to Stokes drift as shown in Novelli et al. (Stokes drift at low wind is under 1% of wind speed).

Page 5: lines 16-19: I would not expect that separation changes so much the mean wind speed in the ocean over the near-surface 10 cm. The radiation stress of the short waves dissipated / reflected by the obect can be relevant, see Longuet-Higgins 1977.

Page 5: Given the very different wave age in the lab and in the field, it is not clear at all that the "velocity slip" in the lab can be scaled to the field conditions.

Page 6: Please show / give reference to proper validation of wave model in terms of Stokes drift.

Page 10 line 4-5: please be more specific and replace "wind-driven velocities" by "wind-

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driven drifter velocities"

Page 12, line 31: "possibly the most significant" is a pretty bold comment given the history of the field (Munk 2002). I would contend that stratification is the elephant in the room here.

Page 13: Please do not use the word "current" unless you are properly explaining how you go from drifter velocity to water velocity.

Page 13, line 22: please clarify if that includes the Stokes drift or not. Also, it should be important to discuss the effect of proximity to coast as the wind-driven current are rectified by the shoreline in many datasets.

Page 15, line 1: "The momentum input from large breaking waves into the surface currents" what about rather, "the momentum input and surfing behaviour of undrogued drifters in large breaking waves"

Page 15 line 11: " twice as fast " does not make much sense, please provide some scale (wind, Stokes drift ...) you do not expect to go twice as fast in the top meter above a 2 m/s Gulf Stream. Also please discuss stratification.

References: Rascle, N., & Ardhuin, F. (2009). Drift and mixing under the ocean surface revisited: Stratified conditions and model-data comparisons. Journal of Geophysical Research, 114(C2). doi:10.1029/2007jc004466

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