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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.

We would like to thank Dr. Ardhuin for his constructive review of the current work.

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 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.

We agree that it is very difficult to determine how the wave action and especially the wind will affect the movement of the undrogued drifters during the conditions being analyzed here. For this reason, we do not make any correction of the drifter velocity, but choose to leave the manner in which we describe the "surface currents" unchanged. We believe it makes sense to describe the drifter velocities as the average current over the draft depth of the drifter, given the definition portrayed in Eq. (1). We have tried to be more precise in our description of the surface current estimate and the errors associated with the drifter movement due to velocity slip.

Page 6, lines 7-10.

Page 17, lines 24-26 and lines 10-13

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"

Effect of wind and waves on drifters during the low wind period over which the regional circulation estimates have been described in section 3.2, along with new Figures showing SST for validation (Figs. 6-7).

Page 10, lines 21-29

Response time of surface currents has also been addressed with supporting literature.

Page 11, lines 1-5

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 literature.

A new figure (Fig. 2) has been added to show a typical transects of salinity, temperature, and potential density across a frontal zone in the region measured during the experiment, however not during one of the high wind periods. Fronts like this one were frequently measured during this experiment showing transects very similar to the one shown in Fig. 2.

Page 7, lines 22-31.

Effects of stratification have also been referenced in Introduction

Page 3, lines 10-14.

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 be 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.

We've added a new Figure (Fig. 4) to the manuscript to provide wave validation with available observations from the model during one of the high wind events, showing significant wave height, mean wave direction, wavelength, and mean wave period. With only hourly averages from NDBC buoy 42040 to compare to the model too, we felt this figure was more appropriate than trying to calculate wave spectrum from hourly averaged data.

Page 8, lines 11-19

Technical corrections:

- Page 1: line 28: replace CARHTE with CARTHE

We have replaced this. Thank you

- 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)

We have cited Santala and Terray (1992)

Page 2, lines 13-15

- 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 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.

A reference velocity has been provided in regard to this description of the vertical velocity shear. A note about breaking waves and mixing in regard to decreased velocity shear has also been referenced, citing Sutherland et al. 2016

Page 2, line 24
Page 17, lines 22-26

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).

Rascle et al. 2009 has been described and cited.

Page 3, Lines 13-14

Page 2: line 24: 0.5 m is optimistic

0.5 m was the value from references listed.

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 number on a "wind-only' component.

This range of findings for the wind-driven current is based off the range of results reported by the different studies cited. The lines were edited for clarity. Berta et al. 2018 states their ageostrophic component, which is dominated by the wind, travels at ~2 % of the wind speed.

Page 3, lines 7-8

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

We would like to keep the present convention of referring to the drifter velocities as measurements of the surface currents themselves, with potential errors described.

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).

We *were referring to lab experiments where there was no wind influence. This point has been clarified in the text.*

Page 5, line 22

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.

The mechanics of radiation stress on the effect of the drifters is non-trival to diagnose, and is thus beyond the scope of this paper.

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.

We agree it probably cannot, which is why we choose not to perform a velocity correction to the measured drifter velocities.

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

Wave model validation provided in Fig. 4

Page 8, lines 11-19

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

Again, we would like to keep the present convention of referring to the drifter velocities as measurements of the surface currents themselves, with potential errors described.

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.

Paragraphs have been edited with the stratification mentioned as well.

Page 15, line 15.

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

We refer to the drifter velocities as measurements of the surface currents themselves, with potential errors described.

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.

Clarification of the inclusion of stokes drift and effect of shoreline rectification has been made.

Page 16, lines 4-9

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"

Statement has been changed to include this phenomenon.

Page 10-13

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.

Absolutely velocity measurements have been provided.

Page 17, lines 22-25