

Response to reviewer 1

I really enjoyed this article. It was well-written and showed how unexpected results of potentially great value can be extracted from data collected for entirely different purposes. There were a few minor things that, if addressed, could increase the value of the article, but it's already acceptable for publication. Here are my comments:

Thanks. Your comments are appreciated.

Introduction: the authors should cite Poulain (2013) as an example of mapping tidal currents with drifters. JGR Oceans 118, 1434-1444, doi: 10.1002/jgrc.20147.

*Added ***

121-126: the caveats are well stated here, as well as later in the manuscript. Likely more could be done to statistically assess windage and Stokes drift in a follow-up, but this first study is sufficient to show convincingly that the tidal signal is extractable.
148: it would help a reader not familiar with the tides of the area to state what fraction of the tidal variance at Liverpool is explained by M2.

*The Irish Sea is close to resonance with the semi-diurnal tide and diurnal tides are very small in comparison. The ratio of S2 to M2 is about 1/3, and so spring tidal currents (when M2 and S2 add) are about twice as great as neap currents. A note has been added to the text ***

246-247: later in the manuscript, this factor of 0.85 is called into question. Thus, it would add great value if the authors added at least a couple of sentences here summarizing how Pugh deduced this factor.

*Pugh, on page 243 of his book, gives a 'rough' guide for estimating the tidal current profile. The calculation is based on a power law description of the current profile above the logarithmic layer, in which velocity increases as height above bed to the power 1/m. For experimentally determined values of m in the range 5 to 7 this gives the ratio of depth-mean current to surface current 0.83 to 0.85. ***

We have therefore used an empirical relationship in this calculation. Physical arguments suggest that as the current increases, eddy viscosity also increases and so the ratio of mean to surface current will increase. It is hard to imagine it increasing much above 0.9, however.

279-280: is there any evidence that the birds preferentially seek times when the tidal current is fastest? If so, there might also be a spatial bias toward higher-current regimes.

The birds appear to follow a diurnal routine, flying during the day and resting at night. They may well seek out regions of fast currents however, if there is a benefit for feeding. We don't know this, although it has been suggested to us.

286-295: this is probably crazy, but is the M2 current signal in the model purely sinusoid? Could nonlinear effects alter this? The authors note that other, weaker con-

stituents can also play a role in modifying this signal.

*The model output is purely sinusoidal. The 'test' we refer to was adding a diurnal part to the curve fitted to the observations of bird speed. We should have made that clearer and have amended the text. ***

General question: is there any evidence of a time-mean current superimposed on the tidal signal? This is likely much more contaminated by windage and Stokes drift than the tidal signal, but it would be fascinating to see what the residual velocities look like after fitting and removing the M2 signal.

There certainly is a time mean current which is generally towards the east. The prevailing wind direction is from the south-west and so this is consistent with the residual flow being partly surface drift and partly windage on the birds. We have not tried to tally this with observations of wind speed in this study, however. Something for the future perhaps?

Response to reviewer 2

This is a very well written paper, and a splendid example of cross-discipline imaginative, even opportunistic, research. Definitely worth publishing, and fittingly short and focussed in presentation.

Thankyou, your comments are appreciated.

The technique is probably unlikely to be used routinely in preference to small non-biological drifters, as tagging and recovery are labour intensive, and without firm control of the locations and pattern distribution of the bird "deployments"; numerical modelling is even in this paper, considered to give better estimates of currents in areas of interest.

That's a good point. We don't see this as becoming an operational method, used by commercial oceanographers, for example, just yet. It's curiosity-driven science, limited by where birds go and are tagged. Still interesting, though.

Some specific comments follow:

Admiralty sources usually use the term "tidal streams"

Noted. The authors also like the use of 'streams' for currents and it wasn't our intention to use one term over another in the abstract. We have changed the 'currents' to 'streams' to be even-handed.

Line 34 can we have an indication of differences where currents are weaker?

Not really from this study, I'm afraid. There are no boxes with current amplitudes much less than 1 metre per second in the study area. We imagine that the error will stay much the same in areas of weaker currents (because it is caused by wind-drift, bird flapping etc.) and so will become a bigger percentage error in these places. The method definitely works best in places with fast streams.

Line 37 perhaps delete "great" see above.

Agreed

Line 50 location of expensive turbines would demand much more information than this technique can deliver. Don't oversell!

At this point we have not mentioned birds, just that a large number of drifters would provide useful information. That's true and fair enough we think.

Line 61 This is a critical assertion, skimmed over here. Are there other studies of the GPS-Razorbill behaviour to be more convincing?

As far as we are aware there have been no tests that the trackers make no difference to the behaviour of the bird. It is hard to imagine how it would affect the motion of the bird while it is sitting on the water, and so shouldn't affect the results of this paper too much.

Line 96 For a cross-discipline paper "colonial" could be defined en passant.

Done.

Line 107 need to specify precision/accuracy. One hundredth of a degree? How many metres?

Done

Line 145 using f for the multiplication factor is unfortunate as for tidal scientists f is used for the amplitude of the 18.6 years modulation of lunar tides. Alpha would be OK.

We think it's OK to use f here as it is defined.

Line 146 Scaling and normalising the observations according to the tidal range at Liverpool on the day is acceptable, but will also remove most of the variations due to diurnal and even shallow-water harmonics that are not local to Liverpool. There are elements of this procedure that will introduce some tidal noise.

True. The method as we have developed it so far is really only good for the M2 streams. As we say later, adding a diurnal component to the analysis doesn't make any difference and the higher harmonics will average out over a semi-diurnal cycle. So, things to worry about in the future but not at this stage.

Line 176 State that off shore tidal currents are seldom rectilinear, but take the form of ellipses. Hence the need for a definition.

Done

Line 189 need to say why this is done, having already normalised currents at line 145.

Line 189 refers to how the M2 currents were extracted from the model output. The earlier reference (line 145) was about how M2 currents were extracted from the bird movements, when we don't have a long enough time series to separate out M2 and S2.

Line 204 In relatively shallow water, it would be normal for the currents in the 6 hours before high water to be greater than on the ebb due to the progressive wave part of the tide here. Does the model show this? See also line 231 etc.

We have replaced 'high water' in this sentence with 'slack water'. In a sinusoidally oscillating current, speed plotted against time will have equal peaks before and after slack water, whether it is a standing or a progressive wave.

Line 210 could say here that generally in this area $u \gg v$.

Done

Line 219 Useful indication of the otherwise data quality, but how common are “timing errors”.

This is the only example that we saw and we are not sure that it is a timing error. The RSPB operatives assure us that they always use GMT in setting up the loggers. We have no explanation of this discrepancy.

Line 239 Wrong terminology. A degenerate ellipse is a point. Better to replace “degenerate” with “rectilinear”.

Done

Line 244 Replace “predicted” with “computed”.

Done

Line 247 while (Pugh 1987), a text book so simplified, is generally valid, it would be better to refer to more recent and original studies of the current profile in tidal streams. An alternative would be to interpret the results (and Figure 7) as a measurement of the factor they call f . Lines 286 to 288 could comment on this.

We have added more text here in response to reviewer 1

Line 255 is this an incipient bias in the method? Related to bird behaviour and travel time?

The observations are mostly concentrated around the nesting site on Puffin Island and become sparser with distance from there. We have chosen to include all boxes with SOME data in the analysis at this stage. Clearly things will improve as the data set grows.

Lines 260 to 263 would bear repeating in the Abstract.

It would, but the abstract is already pretty long. We'd prefer to leave it here, at the start of the discussion.

Line 265 Surely diving birds are diving for a purpose, and therefore very mobile when sub-surface! Probably also systematic eg upstream..

Good point. We got too excited when we first realised this possibility! We've taken out the reference to diving birds

Line 268 “The non-systematic issues may be fixed

...
”

Done

Line 270 delete “also”? Line 272 suggest paragraph break at “Anonymous”. Replace “corrected” with “eliminated”.

Done

line 284 But isn’t Figure 2 over the whole tidal cycle and without phase bias?

Not sure we understand this comment. There is no mention of phase in this part of the text. The ‘negative impact’ refers to the bird’s wellbeing.

Line 293 How does this relate to the normalising (line 1450) which should have removed diurnal effects inter alia?

The normalising is designed to bring each tidal cycle to a common tidal range. The problem referred to here is that the sinusoidal fit is better after slack water than before, for reasons we don’t understand. We wondered if this is because there is a significant diurnal current which could speed up the semi-diurnal flow before slack water and slow it down afterwards, but that doesn’t appear to be the case.

Line 299 see earlier. This could have been recast as a measure of the f factor as a function of current speed.

Yes, that’s a nice point, but at this stage we just want to point out the discrepancy between model and bird observations. Using the birds to quantify the ratio between depth-mean and surface currents is, we suspect, asking too much of the data. There are better ways of doing that.

Line 302 “unlikely” need justification. Hence the need to refer to original papers on profiles.

Reference added

Line 306 Somewhere near here the systematic possibilities of error could be summarised in bullet form for clarity and impact.

We don’t feel that this is necessary

Line 404 “curve is the sine curve..” say why these days were chosen?

Not sure we understand this. If the question is about why the particular observations were chosen for the fit the answer is that these are all the observations available in this particular grid element.

Figure 6 the ellipses could all have a dot marking the HW time for Liverpool

This has been looked into, and we feel that it over complicated the figure. The aim of the study is to demonstrate the differences over the study area, which we feel the figure does adequately.