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Biogeosciences Discussions

Interactive comment on "Depth-averaged instantaneous currents in a tidally dominated shelf sea from glider observations" *by* L. Merckelbach

Anonymous Referee #3

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This manuscript compares a method to estimate depth- and time-averaged currents using a glider dead-reckoning algorithm with observed current data from an ADCP moored within 10 km of the glider's path. The ADCP 10-min data is averaged every three hours, low-passed to remove the tides and then added to tidal predictions using a Kalman filter, and finally compared with the glider data. The prediction errors are then estimated.

The objective of this work is worthwhile as the estimated barotropic currents can be used not only in conjuncture with the data from the other sensors on the glider but could also be assimilated in numerical models and, of course, be used to estimate the underwater position of the glider for navigation safety purposes. The only surprise is that the manuscript is submitted to Biogeosciences: a more suitable journal would be J. Atmos. Oceanic Technology, for instance. However, I understand that it has been

submitted to a BG special issue dedicated to the COSYNA program. The Introduction discussed possible applications and highlights the possible benefits of using his method to interpret the other glider data. A possibility is fine, but are there studies using this type of data being published in this special issue? Elsewhere?

As I mention, this is a technical paper and it should be published. The text is clear but it could be sometimes difficult to understand for somebody not in the field. The figures are also clear, but small. Moreover, I have a few questions and comments.

- Page 4, line 9; I assumed that a harmonic analysis has been conducted on the ADCP data: how much of the tidal variance is being accounted by the M2 tides? Probably a great deal if we look at figure 3, but it would be nice to know.

- Page 4, line 16; adding more tidal frequencies might not be as trivial as the phase information needs to be included. I am also surprised that a linear model (eq. 9) works so well in a shallow (40 m) region. However, the non-linearities observed on Fig. 6 do look small.

- Page 8, line 23; there is also an error associated with the ADCP data. For a 600 kHz, the error associated with 10 min (32 pings) averages of 40 cm bins would be close to 2.5 cm s-1. Averaging again over 3 h would reduce the random error under the bias error. I haven't checked lately, but RD-Instruments was suggesting that we should consider a bias error of a minimum of 1 cm s-1. I am mentioning this because I think that the author is underestimating the various errors associated with his method (as in Table 1).

- Page 8, line 29; I doubt that the buoyancy currents scale, the internal Rossby deformation radius, would be smaller than 8 or 10 km (I don't have the data to compute it). This means the glider would have to be within 3-4 km of the mooring. I think the results presented here simply mean that the tide, as mentioned in the manuscript, is the dominant feature.

- Page 9, line 7; see my previous comment.

- Page 11, line 1; . . . “the errors seem Gaussian”. Has that been tested?

- Page 16, first paragraph; I am not convinced that this method will allow to incorporate the aspects of mixing in data analysis. I don’t see how, except maybe in a Simpson and Hunter (1974) frame: tidal and wind mixing by friction.

In summary, the manuscript is publishable as it introduces a simple and clever method to estimate the barotropic, time- and depth-averaged currents, using the glider positioning algorithm. I agree with the authors: it is most useful in tidally-dominated regions. In a highly stratified region, one would need to get closer than 10 km from a mooring to assess the influence of stratification. Finally, I don’t understand how the author can incorporated mixing with this method.

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