Editor Decision: Reconsider after major revisions (further review by Editor and Referees) (17 Mar 2017)
by Prof. Matthew Hipsey
Comments to the Author (pdf): hess2016389commentstoauthor.

Dear authors
Thank you again for the resubmission and improvements to the paper. I have 3 new referee reports that have come in, and they have highlighted the technical suitability of the work, but still issues are present around the readability, style and presentation of the paper. In order to resolve better the requirements for publication I have myself undertaken essentially a 4th review with detailed recommendations these are in the attached pdf with inserted comments (please let me know if you cant see the comments).
Dear Editor,
we would like to thank you for your assistance and for your valuable comments/suggestions in your review.
In this ultimate revision of the paper we have followed all your suggestions and we have modified our manuscript accordingly. Please, refer to the ultimate revision of the paper where point by point corrections have been marked.

Reviewer 2 has indicated the paper should be more site specific, which is different from my previous advice. Currently I think the focus is OK, but I have recommended in my comments the inclusion of a diagram to better communicate the flow of the framework (data streams> analysis> outcomes/findings). This also will help solving issues I have raised in the attachment around confusion of data and analyses that are being referred to in the text.
Following this comment, we have inserted in this ultimate revision a new Figure 1, showing the flow of our framework. Please, see the revised paper.

Please also address other comments raised by the reviewer in your resubmission.

Nonpublic comments to the Author:
The first major point is the readability and grammar. It simply is not at journal publication standard, and especially is not at a standard I would expect after going through a major revision process.
The manuscript has been revised by an English native speaker and corrections have been made to improve its readability and grammar

The content of the abstract, introduction and discussion is also far from publication quality. The introduction text must form a much more clear narrative outlining specific details. Currently their is vague mention of data and analysis for patterns and processes, but it is very confusing to understand what data is being referred to at certain points in the text, and in most of the introduction it is not clear WHY the data is being analysed. Detecting trends or patterns is not adequate what are you detecting trends in and why??? Please see more specific comments that have been made throughout these sections. I recommend that you take some time to find a book on scientific writing style that you could refer to in reforming the logical flow of the introduction.
Following this comment, abstract, introduction and discussion have been strongly modified and in this ultimate revised version mentioned data and analysis of patterns and processes have always been specified.

Review report of "Data sets characterizing tide and current fluxes in coastal basins" by Elvira Armenio et al.

Major comments:
In this paper the authors proposed a framework aiming to be applicable in any coastal sites to study the hydrodynamics using the field measurement data sets, and then use the Mar Piccolo semi-enclosed basin as a study case. The paper presents the time history records of tides, currents and waves, reveals the tide asymmetry and relationships between the tide record and the current speeds at different levels of water column. The results could be useful for understanding the basin hydrology especially the water transport.

We would like to thank the Reviewer for her/his appreciation opf our manuscript and detail revision work, which surely will improve our paper in its final form.

However, I have a big concern that a study case like Mar Piccolo is far not enough to support the aim of this paper, i.e. to set up a framework to study the hydrology at ANY coastal sites.

(1) not many coastal sites have continuous measurements of 3D currents;
(2) the currents at a coastal site are determined by many factors such as tides, winds, bathymetry and river discharge. A clear relationship between the amplitude of tides and the current speeds can only be found when two measuring stations are very close and impacts from other factors are small (as in the study case of Mar Piccolo). If the ADCP mooring station is far away from station B (for example in II Bay), the story will be totally different;

Answering to both (1) and (2) comment, we would punctuate that we agree with the Reviewer, considering that continuous measurements of 3D currents are difficult to be found in coastal sites, as well as measuring stations sufficiently close to allow data comparisons and correlations. In this sense, we think that our measurements are valuable and rare, thus deserving to be published. Furthermore, they are high quality measurements, as already written in the paper. In any case, it is evident that our proposed framework could find application in coastal sites where such monitoring stations and gauges are available. Namely, similar sites are those considered vulnerable and generally needing an environmental control (like the Mar Piccolo case). An example could be the platform 'Acqua Alta' installed in the Adriatic Sea close to the Venetian Lagoon.

(3) the data analysis methods and procedure used in the study case of Mar Piccolo such as data quality control, spectrum analysis and tide analysis, are general and traditional in coastal studies. It doesn't seem like a new study framework.

We thank the Reviewer for this comment. As also written in previous versions of the paper, we recognize that he adopted methods are not innovative, rather they are classic and well-established. Nevertheless, their use has an advantage because they are simple and immediate to be applied but at the same time they are not time consuming (in computation) and are able to provide good results, as shown in the paper. This concept has been more stressed in the ultimate revision of the manuscript.

(4) the shape of vertical current profile is supposed to related to the density stratification, i.e. the mode of internal waves determined by the vertical buoyancy frequency profile. This is not shown in the results.

We agree with the Reviewer, the vertical profiles of the current are the results of many forcing variables (wind, waves, T and S stratification, tide). Only some of these variables were measured during our investigation. The wave action was considered quite negligible (especially in the narrow channel) where waves arrived very smoothed. Wind action could affect the circulation only locally closer to the surface. About S and T we had not direct measurements on site and we referred to

previous results of numerical models, so that a direct correlation of currents with S and T gradients could not be shown in the results.

(5) the authors showed the monthly-averaged and some snapshots of current profiles, however no uncertainty is included in the results. So the results cannot be used reliably for forecast or management purpose.
The uncertainty of the measurements was indicated when the instrumentation was described, i.e. "The acoustic frequency of the ADCP is 600KHz and the velocity accuracy is 0.3% of the water velocity ±0.003m/s…. Values of tide levels have been acquiring with a sampling rate of 5Hz, while the gauge resolution is of 1 mm and its accuracy is of ±0.01m." Being the accuracy specified in this way, error bars were not added on the plots in order to not create confusion.
About the possible use of these results for forecasting purposes, as written in the discussion, "we could even attempt a forecasting of the response data over short timescales, i.e. time spans that are considerably shorter than the length of the investigated data time series." But the present results have not this ambition and further considerations should be done for providing forecasts (even estimates on the goodness of results) which could be the future step of this research

So I would suggest the authors to re-organise the paper to focus just on the hydrodynamics in Mar Piccolo itself, or to include more study cases to support your research aim.
Please, read the comment of the Editor on this point.

Another big concern is the writing of this paper. About half of the abstract and the conclusion are just repeating what have been included in the introduction. There are also many minor grammar and English errors. I pick up a few in next section 'minor comments'. But I recommend the authors to go through the paper to clean up the writing to make the paper more readable.
Also following the Editor's comments, abstract, introduction and discussion have been strongly modified and English has been corrected by a native speaker.

Minor comments (based on the last manuscript):
1. Abstract: most of the abstract is describing research targets and methods, but doesn't deliver the research results;
As previously written, the abstract has been strongly modified. Please refer to the ultimate revision of the paper.

2. Page 1, line 29-33: grammar errors;
3. Page 2, line 3-4: grammar errors;
Ok, thank you. English has been corrected.

4. Page 2, line 14-15: although ocean models are usually with resolution of >100m, there are many numerical modelling studies at estuaries/coastal bays with spatial resolution lower than one hundred metres;
We prefer to leave the sentence derived from (Samaras et al., 2016)

5. Page 2, line 26-28: the complexity of a numerical model is usually adjustable, you can choose 1D/2D/3D and different inputs/numerical schemes; numerical models are often overly simplified but not complex;
We agree with the Reviewer about the possibility to choose among 1D/2D/3D models characterized by different levels of difficulty (and accuracy of results as well). But actually, we believe that numerical models are often complex, referring to used algorithms and adopted numerical schemes. We refer to computational complexity.

6. Page 2, line 29: the concept of "data-driven approach" is very general and the author need to be more specific of which approach to use in this study;
The used approach and the aim of the paper have been better specified in this revision.

7. Page 5, line 26: typo, use "-" instead of "÷";
Ok, done

8. Page 8, line 37: typo, use "surface" instead of "superficial";
Ok.

9. Page 11, line 12 – 14: how does the fresh inflow change the current profile? i.e. the vertical buoyancy frequency profile will change according to density stratification, and therefore the vertical mode of current profile will change accordingly. Did you compare the vertical buoyancy frequency profile and the current profiles?
Please, refer to our previous answer to your major comments on this point.

10. Page 11, line 31-32: if we want to extrapolate the results for prediction, uncertainty of prediction must be known but that is not included in the results;
Please, refer to our previous answer to your major comments on this point.

11. Page 12, line 1-25: this part seems just simply repeating what has been included in the introduction.
This part was modified, also following the Editor's comment, and the limits and the advantages of applying this procedure have been better described.

12. Page 12, line 24: this paper doesn't include the interactions of waves with tide and currents.
We would like to thank the Reviewer for this comment.
In this paper, even if greatly smoothed in the narrow Navigable channel, waves have been analyzed by means of the FFT procedure, analogously to the other data sets. It was noted that, as expected, in their energy spectrum they were characterized by a frequency range very different from that of tide and current as well (namely, not overlapping). Therefore, they were not furtherly discussed in terms of correlation with current and tide. This concept has been added in the par. Of results in the ultimate revised manuscript.