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Hydrology and Earth System Sciences
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Discussions

Interactive comment on "An approach for data-driven characterization of tide and current fluxes in coastal basins" *by* Elvira Armenio et al.

Anonymous Referee #3

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The manuscript describes a field study of the dynamics of a semi-enclosed basin, influ- enced by waves and tides. The basin connects to the Gulf of Taranto through two main connections, an artificial one used for navigation and a natural one. The measure- ments focussed on the artificial channel where a bottom mounted ADCP, a wave array, and an acoustic level sensor were used to study the dynamics of the system. The flow in the channel are dominated by the semi-diurnal tidal forcing. Phase differences are found in depth, with a nearly progressive wave occurring at the surface, while near the bed the flow shows a phase difference of two hours with the water levels.

My main problem with the present manuscript is that its objective is unclear and not in line with the rest of the manuscript. The title, introduction abstract and conclusions seem to suggest that the manuscript introduces a novel data-driven methodology, or some unprecedented level of detail in the data collection.

In the abstract the authors state that the "work aims to demonstrate that a data driven approach [...] allows to directly identify key physical processes driving a coastal systems ...". Besides the ambiguous use of "data-driven" approach, I really do not see why we need a manuscript showing that collection of field data allows to identify key physical processes. Also, the measuring techniques used and the processing methods are not new and not particularly innovative or exceptionally detailed.

Reply: We would like to thank Referee #3 for his/her careful reading of our manuscript, positive comments and useful suggestions, which certainly will contribute to improve the paper in its final form.

The objective of the present paper is to show that the analysis of a high quality data set allows to individuate physical processes, on a small scale, that otherwise could be known only by means of numerical modelling. Nevertheless, also in this latter case they are difficult to be evaluated because of their spatial and temporal resolution, and in any way, they need for field validation.

Nowhere in the text we write that we intend to introduce a 'novel' data-driven methodology. Rather, we aim to use well-established procedures to analyze high quality data sets and to extrapolate information, identifying typical and recurring processes and trends in a coastal area. Surely we want to highlight that we refer to a *rare* data set, due to the quantity/quality of our acquired data. In this sense the level of detail of our data is undoubtedly difficult to find in analogous studies.
In fact, a data set should be considered of high quality and could be used to directly identify key physical processes only if it responds to some requirements:
1. it is sufficiently long in time, so to cover a period in which possible changes can be detected, i.e. the longer the record, the greater the chance to find evidence of seasonal or monthly trends/process. Moreover, in this way the possible periodicity and stationarity of the signal can be

detected;
2. each measurement is sampled for a sufficient temporal interval, to allow a significant time-average value, once filtered out noises. Also, a good sampling rate should be guaranteed, to permit the selection of turbulent features from the signal;
3. it is continuous, with no gaps, thus to allow statistical analysis;
4. quantities are measured in the same time and location, to investigate on their possible correlations in time and space.
All these points together are difficult to be respected in most cases and, as stated in the introduction of the original paper, many data sets refer to surveys limited in time and carried out along specific routes, often according on a commission basis. In this way, information referring to brief time periods and to limited sea areas are provided.

As an example, we can cite a very complete and interesting multiscale sampling experiment (Pinardi, N., Lyubartsev, V., Cardellicchio, N., Caporale, C., Ciliberti, S., Coppini, G., De Pascalis, F., Dialti, L., Federico, I., Filippone, M., Grandi, A., Guideri, M., Lecci, R., Lamberti, L., Lorenzetti, G., Lusiani, P., Macripo', C. D., Maicu, F., Tartarini, D., Trotta, F., Umgiesser, G., and Zaggia, L.: Marine Rapid Environmental Assessment in the Gulf of Taranto: a multiscale approach, Nat. Hazards Earth Syst. Sci., 2016, doi:10.5194/nhess-2016-179), during which four surveys were carried out in the Gulf of Taranto from 1 to 10 October 2014, with CTD probes. Specifically, in the open sea, two samplings were carried out at 15 km resolution over three days in 21 stations, while in the Northeastern shelf area the station spacing was about 5 km and the 24 stations were carried out in one day. Finally, the Mar Grande harbor scale was sampled at 1 km resolution and the sampling required 12 hours. In this way, Pinardi et al. (2016) deduced a first synoptic evidence of the large scale circulation structure and associated mesoscale variability in the Gulf of Taranto. But they also stated that regular sampling networks can capture most of the significant ocean variability if adequately calibrated for station resolution and that more observations will be required to map the seasonal variability and flow structure of the area.

As an example we can also refer to the work of Korotenko et al. (Korotenko KA, Sentchev AV, and Schmitt FG, Effect of variable winds on current structure and Reynolds stresses in a tidal flow: analysis of experimental data in the eastern English Channel, Ocean Sci., 8, 1025–1040, 2012), who described the contemporary acquisition of data in the English Channel from instruments such as ADCP, tide gauge, wave buoy and wind station. It can be noted that the wave buoy is 30 km distant from the other instrumentations and that the data set refers to a brief period of 6 days (in June 2006).
In many other cases, very long time series are available (even interannual), but they refer only to a single parameter, as for example in the case of national tide-gauge networks.
The contemporary acquisition of more parameters for long periods is still rare. It is worth noting that meteo-oceanographic stations providing more complete data sets are very few (particularly along the Italian coast) and very expensive, in terms of purchase and management, as well known.

In our case it is worth remembering that our data set is made by long term time series of many hydrodynamics parameters (wave eights and periods, tide levels, 3D currents measured along a vertical profile at intervals of 0.50m), acquired at each our for three months, uninterruptedly and in the same location.

In our opinion the use of the expression 'data-driven' is not ambiguous. In fact, in agreement with Reeve et al. (2016), we intend the data-driven approach as a technique that 'rely solely on the analysis of measurements … Attribution of particular behaviors found through the analysis to particular physical processes is through inference. Data-driven methods involve analyzing a sequence of measurements of forcing variables or coastal state indicators in order to find evidence of trends, cycles or other smoothly varying modes of change'.

Moreover, yes, 'we need a manuscript showing that collection of field data allows to identify key physical processes', because this is not so predictable and expected. To allow such deductions, the data sets must respected some requirements, as already written before. All our Figures from 7 to 15 show these key physical processes, relatively to fluxes in the channel, net flow at different depths, correlation between waves and current, temporal and spatial asymmetry of currents, ebb or flood dominance.

It is also true, as noted by Referee #3, that 'measuring techniques used and the processing methods are not new and not particularly innovative or exceptionally detailed'. But in our opinion the added value of our approach is exactly in the simplicity of the processing methods used. Not always increased sophistication of mathematical methods corresponds to better results.

Moreover the procedures we applied have restrictive requirements which are satisfied in our case only thanks to the high quality of our data. Specifically, the essential requirement for applying the classical FFT technique is the continuity in the data record. As written, this is difficult to be obtained and it is the principal reason why recently alternatives have been sought such as the Wavelets or the Empirical Mode Decomposition EMD, which can be applied to intermitted signals, but at the same time increase the difficulty in computation.

This notwithstanding, I think the added value is in the potential insights it might give concerning the specific functioning of the "Mar piccolo" system. Focussing on this aspect the authors might rework the manuscript such that it is clear what it contributes.

Reply: We agree with Referee #3 that our analysis provide insights about the specific functioning of the Mar Piccolo system. In any way, we still remark that the used method of data analysis is easily applicable and is independent from the site, so that can be employed in other semi enclosed coastal areas, providing the same type of results. We think that this concept was already clear in the original paper.

Another major remark concerns the analysis of the bridge vibrations and the discussion about whether or not this will affect the measured tidal signal. If anything, bridges might vibrate with waves, but I have never heard of a bridge vibrating at frequencies close to tidal frequencies and I cannot think of how traffic induced vibrations would affect the measurements of tidal water levels. I suggest the authors leave out this part of the manuscript.

Reply: On this point, please read our reply to your detailed comments further below.

Detailed comments: The introduction is very general, and could be the introduction of any article in which data is collected in a coastal environment. I do not think that collecting data (or data-driven approach, as the authors call it) is the novelty of the manuscript.

Reply: We do not agree with Referee #3 about this comment. The introduction presents the handled topic and shows the context in which this topic is inserted, explaining the necessity of the investigation. Then, the same introduction is better fitted on the topic. Specifically, 1. it describes the necessity of collecting data finalized to numerical modelling, 2. it gives evidence of the goal that high quality data allow to reach, independently from numerical modelling, i.e. the reconstruction of physical processes, especially on small scales that are difficult to reproduce with modelling. The aim of the study is therefore well expressed; 3. the used analysis and approach is also explained and linked to the specific case; 4. the organization of the paper is finally reported. Following the comment of Referee #3, a further point was added in the revised introduction, i.e. the fact that the used processing methodologies are well established and classical and not demanding from a computational point of view, thus resulting quite easy to be applied in similar context.

Probably the perception of 'general' is due to our deliberate intent to stress how our data approach can be of general application to other coastal monitoring programs.

As already written above, the novelty of the paper is not the collection of data. We do not write of novelty in our paper referring to data acquisition procedure, but we refer to a novel data set with respect to previous analyzed ones. Rather, the *rarity* of our data is highlighted, i.e. the availability of a high quality data set, characterized by long term time series of many parameters (wave eights and periods, tide levels, 3D currents measured along a vertical profile at intervals of 0.50m), acquired each our for three months, consecutively and in the same location. This is rare and in our opinion not easy to both acquire and/or examine.

I agree with the authors about the need for intensive field monitoring to complement numerical

modeling, but I do not see how this manuscript is bringing new insights here.

Reply: We wrote in the same introduction that data collection and analysis is surely necessary in conjunction with numerical modelling, but it should not be finalized only to this scope. In fact, it stands alone if data are of high quality, because, starting from this massive data, some recurring physical trends can be identified by using classical methods.

Section 2.1 Line 14: The two bays named "I Inlet" and "II Inlet" are not indicated in Figure 1 (Anyway I would not call give a bay the name "inlet").

Reply: Thanks for your comment. Changed.

Line 18: Please indicate location of the "Porta Napoli" channel in Figure 1 (does it correspond to St. Eligio pier?)

Reply: Ok, done.

Line 20: Funding information is typically included in the acknowledgments, consider removing it from here.

Reply: We agree with your observation and moved fund information in the acknowledgments as you suggest.

Line 28: Please detail how the bias from waves was determined and how the effect of side-lobes was considered in the exclusion of the upper layer

Reply: The producer of the ADCP instrument provided us the following information. As shown in the Figure below the transducer beam angles ca be oriented 20º (as in our case) or 30º from vertical. For the 20º transducer, the echo through the side lobe facing the surface returns to the ADCP at the same time as the echo from the main lobe at 94% of the distance to the surface. This means data from the last 6% of the range to the surface can be contaminated. (The concept is obviously the same for a 30º transducer, with contamination covering the 15% of the range).

In our examination an upper layer even more thick than 6% of the local water depth was assumed for caution and data from this upper layer were rejected.

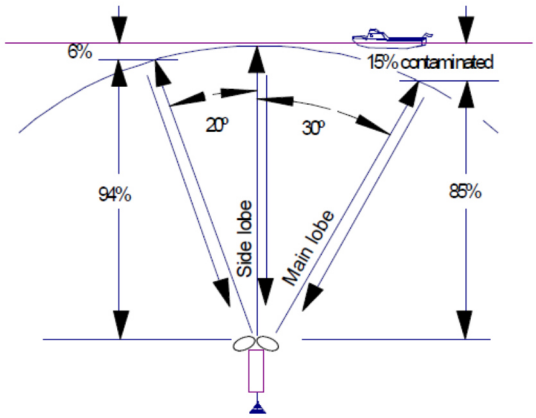


Figure. Relationship between transducer beam angle and the thickness of the contaminated layer at the surface.

Line 29: ...profiles have been collected... (instead of collecting) Line 31: ...wave height Hs have been acquired... (instead of acquiring)
Reply: Ok, thanks, corrected both

Line 32: move funding information to acknowledgments Lines 37-39: I do not think it is relevant to the manuscript who owns or manages the instruments.

Reply: We agree with Referee #3, being this information not relevant for the manuscript, but our administration requires this mention in the text, consequently also this information was moved in the acknowledgments.

Section 2.2 Title: I suggest "data processing"

Reply: We agree with Referee #3 and changed the title.

Line 6: The first sentence seems a bit out of place, since the rest of the Section does not seem a logical follow up of that first sentence

Reply: The sentence was slightly modified.

Section 2.2 already explains something about the processing of water level and current data. Subsequently paragraphs 2.2.1 and 2.2.2 give more details on these data, making it a bit confusing what exactly is explained in 2.2. Is this preliminary processing, or giving an overview of what is explained in greater detail afterwards?

Reply: Following the suggestion by the Editor, this is an overview of what detailed afterwards.

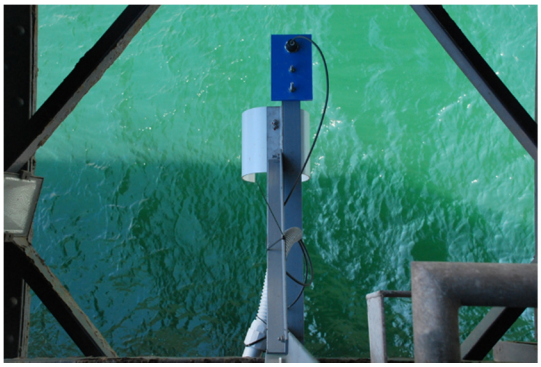
Section 2.2.1 Line 22: Tidal data were first examined (instead of firstly examined) Line 22: Once blanks were removed the data were checked ... (instead of Preliminary, once checked...)

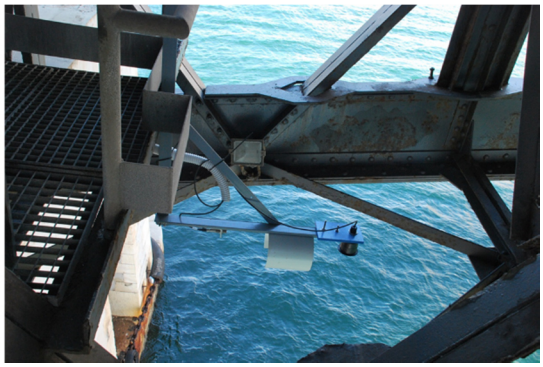
Reply: Ok, thanks, corrected both.

Line 31: "The assessment on possible traffic induced vibrations [...] was considered necessary and appropriate". Please elaborate on why this analysis was considered appropriate. I have difficulty to see the necessity for such analysis as explained above.

Reply: For the sake of brevity, the way in which the ultrasonic tide level gauge was mounted onto the bridge structure was not described in the original paper. Probably this could lead to misunderstanding/perplexity. The support which anchored the gauge to the bridge (please see the photo below) was made as rigid as possible, but caused by traffic, some amplified displacements could occur in anyway. Moreover, for technical limitations, the sensor was placed in such a position that eventual vertical movements could cause an enlargement of the cone of the ultrasonic signal thus intercepting the bridge piles and providing erroneous measurements. For all what written, we agree with Referee #3 that frequencies of vibrations and tides are in completely different ranges and that bridge displacements were expected not to interfere with the slow tidal movements, but cautionary we preferred to analyze the tidal signal in the frequency domain to be sure of the reliability of the tidal signal. This was done to be sure that technical problems (as written) were avoided during our experiment.

Therefore, in the revised paper the sentence of line 31 was modified.





Section 2.2.2 Lines 13-15 ... the net flows were estimated [...] it was approximated that the flow was uniform along the transversal axis... This is a questionable assumption, since significant variation can occur over the cross-section, comparable to the variations the authors observe in the vertical. Both the amplitude and phasing of the tide can strongly vary over the cross-section.

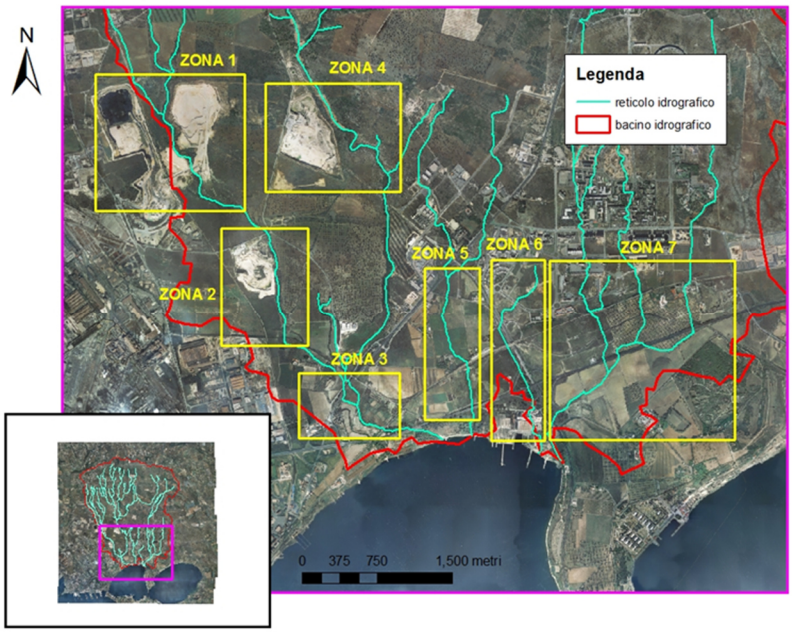
Reply: As also answered to Referee #2 on this point, the sentence had a misprint and it was

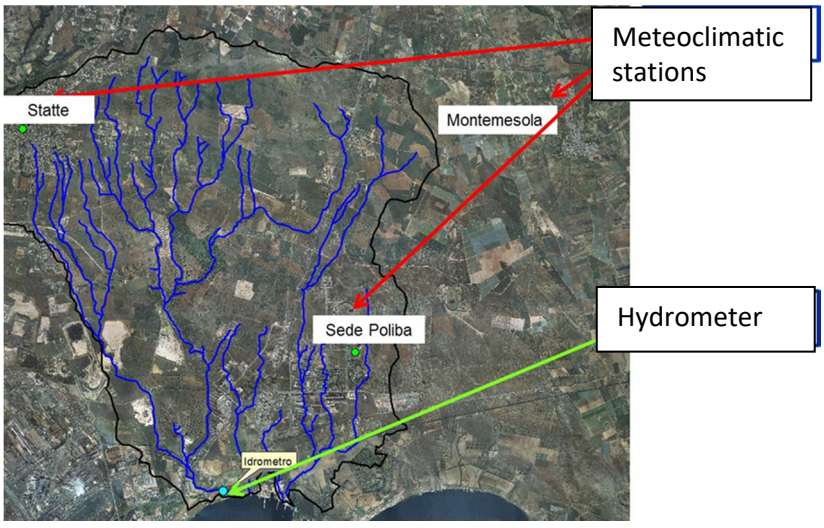
corrected in the revised paper, substituting 'transversal' with 'longitudinal'. Variations of the transversal velocity, even if they occur, are one order of magnitude lower than those observed in the longitudinal components, thus not taken into consideration. A strong variation of the features along the transversal direction are also not expected, due to the very limited transversal length of the channel (less than 60m).

Section 3.1 Line 4 ...semidiurnal tide, with two typical crests and troughs each day...: remove the sentence after "semidiurnal tide". This sentence is just repeating that the tide is semi-diurnal.
Reply: Ok, modified.

Line 7: "frequencies" should be "periods"
Reply: Ok, modified.

Line 11: "increased due to rainfall contribution": Please explain why this discrepancy promptly attributed to rain- fall.
Reply: Principally, the difference between the two signals in the two stations was attributed to the extreme event, relying on continuation of historic knowledge. We would mean that the Mar Piccolo basin is a catchment basin and this feature is well known and proved by many studies carried out in the frame of different national projects (RITMARE Project, as an example). In the Figure below the drainage network which feed the basin is shown (picture provided by the research group of hydrologists of our Department). The run off level due to this extreme event was recorded by the hydrometer in the Figure below. The increasing level in the basin was therefore expected and in fact it was recorded by our Station B.





Line 11: "about twice" is a bit strange in this context since it strongly depends on the reference of the levels measured. Twice the depth makes sense, but I doubt twice the level makes sense.

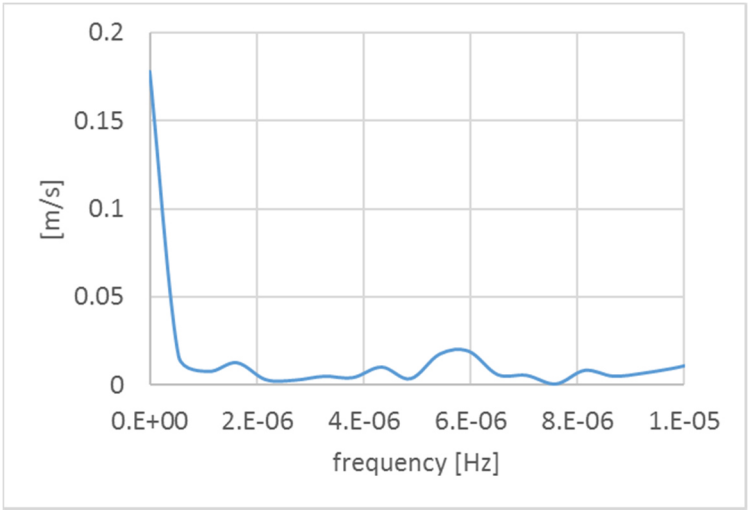
Reply: Actually, the mean of both time series (coming from both sensors) were subtracted from the signal, to allow a direct comparison of the levels, starting from the same reference. In any way the sentence was modified referring to depth rather than to level, to avoid possible confusion.

Line 18: "the frequencies of the two signals are not comparable, falling in completely different ranges": does it really need measurements to see that traffic induced bridge fluctuation are in a completely different frequency range than tides? I suggest this analysis is removed from the manuscript.

Reply: Some changes in the text were made in the revised version. Please refer to our previous answer to your comments of Section 2.2.1 line 31.

Section 3.2 Line 39: "monthly averaged" what the authors I trying to determine here, I guess, is the residual flow. Doing this with a monthly average might not result in the right figures, since spring-neap effects might still be in the residual. Why not do some low-pass filtering?

Reply: What discussed in these lines and shown in Figures 7a and 7b is the average over the month of each recorded longitudinal value of velocity, at each depth, respectively with its positive or negative sign. The spectra of the time series of the longitudinal current at all depths were already computed. If a low pass filter is used (thus cutting in the spectrum all the frequencies linked to the tide and higher), what remains is exactly the mean value of the recorded signal (according to the same definition of the FFT algorithm). In this reply to Referee #3, in the Figure below we show as an example the enlargement of the amplitude spectra of the current (of December) in the frequency range $0 \div 1 \times 10^{-5}$, for the point at depth z= -4.5m. It can be noted that the amplitude at zero frequency is equal to ~0.18 m/s and it corresponds to the monthly-averaged value of the time series. This prove that the monthly-averaged values are exactly the values of the residual current.



Section 3.3 Line 22: "the direction of the tide level reversed": The direction of a water level is undefined.

Reply: We agree with this notation and corrected the sentence.

Section 4 (Discussion) It seems the authors continue discussing more results in the discussion section. Give the content, I suggest the discussion is merged with the results and a proper discussion is included where the study is placed in a broader context discussing its relations to existing literature on the topic of the manuscript.

Reply: The results and discussion sections were divided in the original paper following the indication of the Editor. While the results section described what obtained separately for waves, tides and currents, the discussion section focused on the interaction between tide and current (once observed that the wave effect in the channel is greatly smoothed).
In any way Referee #3 suggestion was taken into consideration, so that a section 3.4 was added in which the correlation between tide and current was described, and a new discussion section was inserted in the revised version of the paper.

Section 4 (Conclusions): Change section number to 5
Reply: Thanks, done.

Line 3: "Our approach has significance due to the unique high quality/high resolution collected data set". I think the authors collected a rather ordinary set of data. Although strong indication of stratification is present in the data, no information on salinity and temperature is presented. In what way do the authors think the data set is unique?

Reply: Again we do not agree with Referee #3 opinion about our data set. Referring to this point, we think to have already extensively answered to this question in our previous replies. Excluding platforms equipped with great instrumentation in the Northern Adriatic Sea, we are not aware of other sets of data coming from monitoring stations like ours. About the lack of salinity and temperature data, we are pleased to inform Referee #3 that the daily monitoring of salinity and temperature along a vertical profile in the Navigable Channel is under arrangement (on the basis of other funds) and we are confident that we will have the possibility to acquire and examine these data in the near future.

Figure 3: Indicate the diurnal and semidiurnal period
Reply: Ok done

Figure 9: "current-velocities": add "(blue line)"

Reply: Following suggestion of Referee #1 and Referee #2, Figure 9 was completely changed in the revised paper, illustrating the phase averaged signals.

Figure 11: Exclude negative values from the ordinates axis, since R cannot become negative and add an R=1 line to clearly demarcate the ebb dominated from the flood dominated part

Reply: Even if is R>0, the ordinate axis started from zero to allow a more readable Figure 11, which otherwise results much flattened. The line for R=1 was added, in the revised version.

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