Reply to Reviewers' comments- Currents Generated by the Sea Breeze in the Southern Caspian Sea

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General comments

We thank the reviewers for their constructive comments. Most of the issues they bring up are relatively minor and were easily addressed, and we go through them all below. However, some larger issues were also raised, which we deal with first.

Reviewer1: The main comment and suggestion is to calculate and present the S_1 ellipses. I was very surprised that this was not done, taking into account that the second author is the main author of the world-known program of harmonic analysis of tides! The $>$ 1 year-long series of observed currents allow to separate S1 currents from K1 and P1 currents (e.g. Zaytsev et al., 2010). Besides, in fact, K1 , O1 and P1 tides in the southern Caspian Sea are negligible (Medvedev et al., 2017). Such constructed S1 ellipses would give lots of important information and enable the authors: (a) To see the main properties of the S1 currents, in particular, the amplitude, direction of propagation relative to the coastline and the exact phase; (b) To see the vertical structure of the S1 currents and the effects of the baroclinicity; (c) To effectively compare S1 currents observed at various stations; (d) To compare observed and modeled currents in a visible way. So, this analysis would make the paper much more interesting and understandable for readers! (see Zaytsev et al., 2010 as an example). BTW, it appears that the authors themselves understand the importance of the S1 ellipses and discuss this question in the end of their Discussion (Lines 501-504).

We did not perform a harmonic analysis mainly because in the initial stages of the analysis we concentrated on identifying sea breeze days, and analyzing those days alone, and only later found that averaging over the whole year was worthwhile and simpler due the large number of sea breeze days (harmonic analysis for the S_1 constituent and its multiples at S_2, S_3, etc. is roughly equivalent to averaging the daily cycle over the whole year). Of course, just because the initial analysis was done in one way does not mean that another analysis might be better. However, unlike the reviewer, we find the S_1 ellipses, which we plot below, somewhat opaque to understanding as they contain both barotropic and first baroclinic mode information, not easily separated. Further, presenting the information at single moorings in S_1 ellipse format obscures the relationship between the phase of along and across-shore currents, and time of day (for us at least). Time of day, i.e., morning or afternoon, is generally not important in tidal analysis, but it is clearly important to sea breeze mechanisms.

The only immediately obvious feature of the ellipse figure is that the Greenwich phase does not progress along the coast, but we already discuss this in Lines 180-185 (in contrast, phase does progress along the coast in our earlier coastally-trapped wave analysis for low-frequency variations, published in Masoud et al. (2019). Note that we are poleward of the critical latitude, whereas the area discussed in Zaytsev et al. (2010) is equatorward of this, so we do not expect propagation. Further analysis of vertical phase variations can be used to separate barotropic and baroclinic modes (as was done in Pawlowicz (2002)) but we think the procedure we used here is more straightforward and hence more informative overall, especially since Figures 7 and 9 are more similar to figures that have been used by others reporting on sea breeze and coastal wave problems. An obvious exception to this statement is the analysis in Zaytsev et al. (2010), and if we had known about this paper earlier it might have changed our analysis (we have added this reference at line 499 in revised version). However, we still have the problem that our measurements are missing the upper part of the water column, and so the issues of separating out the baroclinic and barotropic responses, and developing a model to explain them, (which seemed a reachable goal because we are beside "straight coastlines"), occupied more of our time instead of pursuing more complex correlation-type analyses.

Reviewer1: Section 2.1.1, Figure 3, rotary spectral analysis. This type of analysis is almost senseless here because stations are located close to the coast and currents are nearly rectilinear. It would be much more useful to show spectra of cross-shore and along-shore components (with the two spectra in one plot for better comparison). Such spectra would give much more helpful information! In fact, several following figures (Figures 7, 8, 9 and 10) are done just for cross-shore and alongshore current components.

This is a puzzling comment, since the 1 cpd currents and winds are not only NOT rectilinear, but in a number of cases (shown by the assymmetry between positive and negative amplitudes at the 1 cpd peaks in Fig. 3 - the point of using rotary spectrum is specifically to show this assymmetry) are clearly rotational, and this information would be lost in plotting along- and across-coast spectra. Indeed, overplotting the two spectra in a single plot, or alternatively plotting the positive and negative frequencies on the same axes, which is a useful method for comparing spectral levels of the broad-band "background" of red spectral noise, is not actually that informative when trying to see the relative amplitudes of the very narrow S_1 peaks (we concede that the figures in Zaytsev et al. (2010), which incidentally also present rotary spectra, do a good job with this).

Moving on to Reviewer 2:

Reviewer2: Overall an interesting set of observations worthy of publication in Ocean Sciences. My only real hesitance is I think the presentation of the work needs to be much more focused on the key results, as at present there is too much unnecessary detail obscuring the key results. In particular, it is not clear to me how the new theory presented differs from previous work on inertial oscillations in coastal settings. What new (generic) incites does this work give to the understanding of inertial oscillations in shallow seas? At present the paper gives the impression that the results are only really relevant to the Caspian Sea which is not the case. As such a recommend the paper to be more clearly focused on the key results (and new incites).

We assume that the reviewers use of the term "inertial oscillation" was a simple mistake, as the daily oscillations we discussed here are clearly NOT at the inertial frequency (see Fig. 3), and in fact we find the level of inertial energy

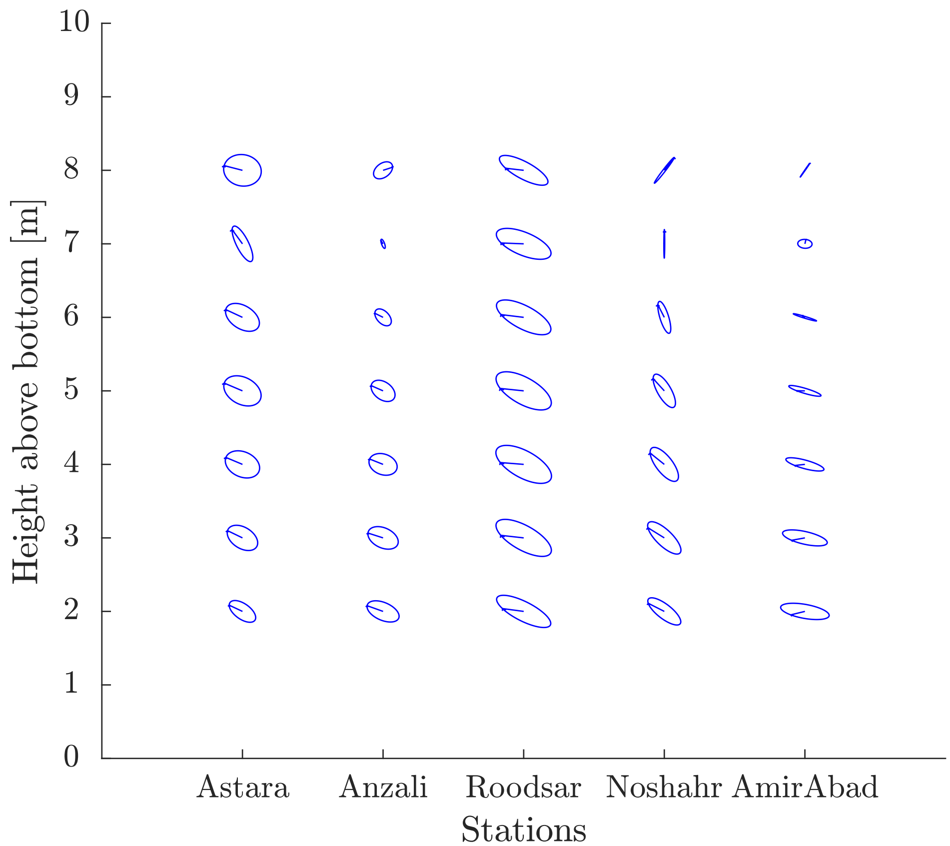

Figure 1: S1 tidal ellipses at the 5 stations, arranged from westernmost to easternmost. Ellipses are shown by their depth above bottom according to numbers on the vertical axis, but are scaled so that along-shore currents are parallel to the horizontal axis and cross-shore currents are parallel to the vertical axis in the correct aspect ratio . The bar radiating away from the center of each ellipse shows Greenwich Phase.

in these observations extremely low.

Having said that, we are somewhat at a loss here as how to respond. Perhaps the reviewer was hoping for a different paper about sea breezes in general? Being a paper entitled "Currents generated by the Sea Breeze in the southern Caspian Sea" we believe it is focused on the key result, which is that the sea breeze, present most of the time, is directly responsible for about the half of the variance of high-frequency currents in the southern Caspian Sea. Surely this result alone is worthy of being published! We additionally show this by developing a new model linking sea breeze to daily currents, so (at least in the results) it is not clear what details are unnecessary. We do consider inertial oscillations in the Discussion (lines 512-517) but mostly to contrast our weak levels near the coast with the strong inertial oscillations seen offshore.

The reviewer then wonders how the model is new. Lines 202-205 attempted to explain this: "Instead of following the depth-dependent oscillating Ekman-layer approach of Craig (1989b) with a vertical eddy viscosity, coupled to a barotropic mode [...] we restrict ourselves to a mathematically simpler coupled two-layer system [...] for which analytical solutions are easier to obtain." The model has analytical solutions, rather than requiring numerical solution (which makes untangling the relative effects of different forces easier to carry out); and the Ekman layer does not appear in this formulation. Further, adding friction and (possible) wave behaviors means that the model remains valid at all latitudes. Possibly a key difference is that the Craig approach is inherently based on unstratified conditions, although currents are horizontal, whereas our approach is inherently based on the existence of a stratification of some kind to restrict vertical motions. However, it turns out that the magnitude of the stratification, which would be important for (e.g.) determining propagation speeds, is here mostly irrelevant because the response does not propagate as we are poleward of the critical latitude.

The reviewer then asks what generic insights arise from this. We concede that this is a valid question. We have added the following sentences at the end of the paper (lines 526-530 in revised version) to address this comment:

"The observed strong and obvious sea breeze response in the South Caspian Sea which is well-modelled by our two-layer analytical model is likely present in other locations around the world where sea breeze systems exist. However, in those locations the diurnal water response to the sea breeze might easily be overlooked due to strength of tidal fluctuations. Therefore, we suggest more careful examination of motions at the S_1 frequency in other study areas. For example a diurnal sea breeze occurs all along the Chinese shelf (Huang et al., 2010) and so this might be a possible location of a widespread oceanic sea-breeze response."

Detailed replies

Continuing on to more detailed replies, we have included the original comments from the reviewers and then highlighted our replies by underlining.

Reviewer 1

- Page 2..., line ...:Abstract, Line 2: "from 2013 to 2014" Throughout the text it is written "December 2012 to December 2013". Be consistent! Right. It is now changed to late 2012 to late 2013.

- Line 37: "...28 m below sea level" 28 m below mean ocean level. Changed to mean ocean level

- Line 506: "...found clockwise and anticlockwise near circular diurnal motions in Northern and southern hemispheres" In the open sea, but certainly not near the coast! Their measurements are on the shelf area, close to the coast. Measurements in Rosenfeld (1988) study are at 30, 60, 90,150 and 400 meter from 5-20 km from the coast in California. Measurements in Rippeth et al. (2002) located at 10-70 km from the coast at 50 to 80 meter depth. Measurement data in Sobarzo et al. (2010) are within 5 km from the coast at around 30 meter depth.

- Line 512: "One surprising aspect of the analysis is the lack of energy at inertial scales on the shelf." There are lots of papers on inertial currents in the Caspian Sea. Actually, they are quite intense in this sea, but... in the open sea, not near the coast!

  Not sure how to respond - not only we are in an agreement with the reviewer's comment, but also this is just what we tried to say. Perhaps this rewording of Lines 516-518 (in revised version) helps?: "Consistent with this result, weak near-inertial motions are predicted near coasts of the Caspian basin with depths of less than about 20 m, but with quite large maximums in the energy of near-inertial motion occurring in the center of Caspian basin (Farley Nicholls et al., 2012)."

- Line 514: "maximums" → maxima (Latin word!) Done!

0.1. Reviewer 2

- Line 19: "However, it is often difficult to separate tidal, inertial, and sea-breeze effects in the coastal ocean response, since the time scales are very similar." Is this statement correct? I you need to be more precice?

  We are a little puzzled that this statement is apparently controversial. The inertial frequency is 1.2 cpd, the Sea Breeze frequency is 1.0 cpd and close tidal frequencies are O_1 (0.9295 cpd), K_1 (1.0027 cpd). These frequencies are so close to each other that they would be difficult to separate in a short time series (say, over a month), especially if the inertial peak is somewhat broad and the sea breeze is intermittent. Of course, opinions about difficulty can vary.

- 43-46: "The large-scale stratification in the Caspian's water column varies seasonally, with warm salty (20-30C, 12 PSU) waters in a relatively well-mixed layer about 40-100 m deep in summer and fresher, less warm (10C, 11 PSU) surface waters in winter (Zaker et al., 2007), above more stratified waters at depth." In terms of the modal structure of the inertial oscillations, the stratification, and it's evolution over the seasonal cycle is key. As such a more accurate description needs to be provided.

  Since we are concerned with current variations in depths of less than 30 m, the details of deeper stratification are not really relevant here (although they were important in our earlier paper on coastally-trapped waves (Masoud et al., 2019). The key sentence is the last one: "However, even within this mixed layer there is often a weak stratification", which again based on data shown in Zaker et al. (2007).

This is because, in terms of the modal vertical structure in these shallow depths, the details of the stratification are actually not that important - the first baroclinic mode will have a zero-crossing near the mid-point of the water column whatever the stratification, as long as there are no sharp steps (which appears to be the case). It is true that the propagation speed of internal modes will be strongly dependent on the magnitude of the stratification, but since we do not have propagating modes for the sea breeze response in this geographic region this effect is unimportant for our paper. The main effect of stratification, other than allowing for a two-layer response at all, is to set the offshore decay scale for the baroclinic adjustment to the coastal boundary condition - but as long as stratification is weak this decay scale is inshore of our mooring locations and so does not require more information (see explanation Lines 336-342). This is very fortunate for us, as there simply is no more information that we know of about seasonal changes in stratification.

Note, however, that if future observational programs include arrays of moorings across the shelf, possibly closer to the shore than this decay scale, better information about the stratification will be required.

- 56: "However, in other months when the temperature gradient between the sea and land surfaces is low, strong winds towards land at sea level can strengthen the sea breeze and generate precipitation." I found this section a little confusing. You need to be clear as to how the sea breeze evolves through the seasonal cycle.

We are simply pointing out that the daily wind cycle is not always forced by the land/sea temperature differences right at the coast. Other processes can also occur. However, although these other processes are important in understanding the mechanisms underlying the daily wind cycle, they are not really important for understanding the coupling to ocean currents.

The mechanisms for generating sea breeze system in the Caspian Sea were investigated by previous studies, and it is perhaps simpler not to try and explain these (sometimes complex) mechanisms here, instead we direct the reader to those references. We therefore reword line 56 as follows:

"However, in other months when the temperature gradient between the sea and land surfaces is low, other mechanisms, for example outflows from the Alborz mountains in winter known as Garmesh winds, can also increase temperatures in the coastal plain, generating a sea breeze (Khalili, 1971; Khoshhal, 1997; Karimi et al., 2016).

- 76: "Weather Research and Forecasting (WRF) model". I key wind characteristic in terms of generation and damping of inertial oscillations is the wind direction. You need to discuss the accuracy of the model wind predictions in these type of coastal situations at some point.

We have added more information about model accuracy, from an additional reference (Ghader, 2014). They carried out a statistical analysis, calculating correlation coefficients, the root mean square error (RMSE), bias and a skill core to evaluate the performance of the WRF model over the Caspian Sea. Based on their results, the estimated RMSE between WRF winds and offshore wind buoy, nearshore wind buoy along the South Caspian Sea (Anzali, Noshahr and AmirAbad) and QuickSCAT satellite data are less than 3.7 for six time periods of simulation (May 2001, Nov 2006, Aug. 2008, Dec. 2008, Aug-Oct 2011 and Apr-July2013). Over all simulated periods, the cross-correlation and skill core of simulated winds with observed wind data and Quicksat satellite data are mostly higher than 0.6 and 0.7 respectively. We have now added the following information to the text (lines 82-88):

"The accuracy of modelled winds has been evaluated by Bohluly et al. (2018) and Ghader et al. (2014) . The latter compared model winds with a variety of observed wind products over the Caspian Sea including one offshore buoy, three nearshore buoys and also data from the QuikSCAT satellite product. Qualitative and quantitative assessment of these comparisons showed that the simulated surface wind fields are in good agreement with the observational data and QuickSCAT satellite data. We also evaluated the accuracy of the WRF wind data ourselves, comparing with available wind buoy data at 3 locations during 2013 (the wind data is available mostly between May and Sep). The RMSE between WRF wind and observed wind is less than 0.1, 0.11 and 0.2 m s^{-1} at Anzali, Noshahr and AmirAbad respectively. "

We also add the following sentence in line 75:

"Some local observations of surface winds are available at three stations (Anzali, Noshahr and AmirAbad) out of our five stations during 2013, but even this data contains gaps, so for consistency we use winds at 10 m ..."

- 451: "This pressure gradient is anti-phase to the surface wind stress and so transfers the forcing to the whole water column with a 180 phase shift that leads to anti-phase motions in the lower layers." This is not stricktly correct - see Criag (1989). The phase shift is essentially a response to the presence of the coastline and the stratification. This paragraph will be removed.

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