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Ocean Science
Discussions

Interactive comment on "Tidal variability of the motion in the Strait of Otranto" *by* L. Ursella et al.

Anonymous Referee #1

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Review of Tidal variability of the motion in the Strait of Otranto by Ursella et al.

This paper analyzes current meter data collected at various intervals over more than a 10 year period in the Strait of Otranto for the influence of tides. Particular attention is paid to two records collected in the summer of 1995 that showed high intensification in diurnal currents at one station near the western shelf break.

General Comments:

The paper fails to take into account important Adriatic dynamics that could be acting to create or alter the measured signals and influence the tidal analysis of these signals. Adriatic seiches and P1 tides in particular could be playing important roles and are not discussed in the paper. These omissions call into question the conclusions of the paper. I recommend an extensive and major revision, changing the tidal analysis, adding analysis regarding seiches, and adding a more thorough discussion of the mechanism

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for generating a topographically trapped diurnal internal wave at this specific location.

Specific Comments:

I. The authors make no mention of Adriatic seiches, yet this is an important Adriatic response to forcing (Cerovečki et al., 1997; Leder & Orlić, 2004, and references therein). The fundamental period is between 21 and 22 hours which is close to the K1 tidal period at 23.9 hours. The data windowing used for the rotary spectra are too short to separate seiches from K1, and from Figure 12 it seems likely that the wavelet analysis cannot distinguish between these two periods either. The harmonic analyses over 3 months or even over 30 days should be able to separate these periods (some non-stationary seiche energy might bleed into UPS1 or OO1 constituent solutions). The model of Leder & Orlic (2004) shows 20 cm/s intensification during an Adriatic seiche at or very near the location of station St2 (Figure 13 in that paper). Some further analysis and discussion is needed to show that the non-stationary diurnal waves seen in Figure 10 (a & b) cannot be at least partially explained as Adriatic seiches.

Cerovečki, I., Orlić, M., Hendershott, M.C., Adriatic seiche decay and energy loss to the Mediterranean, Deep-Sea Research I, Vol. 44, No. 12, pp. 2007-2029, 1997.

Leder, N., Orlić, M., Fundamental Adriatic seiche recorded by current meters, Annales Geophysicae, Vol. 22, pp. 1449-1464, 2004.

II. The tidal analysis done over 2-3 month periods fails to account for significant Adriatic tides known to exist at P1 and K2 frequencies, and the use of 35 constituents over this short of a period will produce non-significant solutions for most of these constituents. The neglect of P1 is particularly significant to the findings of the paper because it beats with K1 frequencies at a 6 month period. A quick test with values appropriate for the Adriatic shows that the neglect of P1 in a 3-month long harmonic analysis will produce two peaks in solutions for K1 (one in summer and one in winter) with a false intensification of values around 13% higher. This effect could entirely explain the intensification observed at St3 and therefore speculation on pages 450-451 that this level of inten-

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sification could be influenced by diurnal internal waves should be removed. It seems unlikely that this effect could entirely account for the intensification observed at St2, but this possibility needs to be investigated in the paper before conclusions should be drawn about diurnal internal waves. Harmonic analysis can be done with large gaps in coverage due to the stationary of the tides, and therefore all seven of the principal tides of the Adriatic (Cushman-Roisin et al., 2001) could likely be resolved for all stations by analyzing the entire time records of the observations together. A focused analysis on summer intensification at St2 could then be done using wavelets or other non-stationary analyses on the tidal residuals. t_tide automatically produces an error analysis for tidal solutions that indicates signal to noise ration and marks constituents that cannot be significantly distinguished from noise or the continuum. Once such an analysis is done and significant tidal constituents are determined, there is no benefit to continuing to analyze for the non-significant constituents (fitting to noise). If they are kept, then there is no reason to report their values as in Figure 6 since these values are not significantly determined. Figure 6 could be made clearer if only 7 constituents were reported rather than 16.

III. There was insufficient analysis and discussion presented on the mechanisms for generating topographically trapped diurnal internal waves at this location. On page 452, it is stated that the presence of these internal diurnal waves was confirmed by the phase shift between the diurnal signal in the coastal sea level and in the currents at location St2, but no evidence is offered on how this phase shift differs from the general solution for sea level and current K1 phase difference, or exclusion of superpositions of barotropic K1 waves that might create the observed phase differences. Lack of coherence between sea level and currents is also given as confirmation of internal diurnal waves on page 452, but the statements on page 451 that multiple coherences are often close to 1 and that partial coherences with wind components were rarely significant implies that sea level and currents are coherent. The statement on page 451 seems to be backed up by Figure 13, panels b & d. In fact, I would expect that sea level and internal diurnal wave currents would be coherent if the currents are observed near the generation point of the diurnal internal wave as found by Beckenback & Terrill (2008). The analysis of VM-ADCP data from 2007 in Figure 15 and page 452 over two days duration are insufficient to draw conclusions regarding diurnal internal waves as processes such as seiches have not been excluded and differences from normal barotropic tide conditions are not discussed. The paper could benefit from some further analysis and discussion on the exact mechanisms of generation of topographically trapped diurnal internal waves in this region, their cross shelf structure, and their quantitative dependence on stratification rather than rely on qualitative comparisons to Beckenback & Terrill (2008).

IV. The authors discount the possibility for the extension of the low-frequency limit of the internal wave spectrum to diurnal frequencies at these latitudes on the basis of the stratification suppressing errors from using the traditional approximation for f. However, this is not the only mechanism for extension of the limit and any region with strong enough relative vorticity can effectively change the limits for the internal wave spectrum within the region (Kunze et al., 1995). For the shelf at Otranto, reasonable possibilities exist through either anti-cyclonic eddies propagating down the Italian coast or simply the anti-cyclonic inshore side of a sheared slope current. E.g., a 50 cm/s current shear over a horizontal distance of 20 km shifts the longest period for internal waves at 40°N to 25 hours, and therefore it would be possible for a diurnal internal wave to exist within the shear zone independent of topography. Although such explanations seem less likely than topographically trapped modes, this possibility should not be excluded on the basis of stratification alone as was done on page 449.

Kunze, E., Schmitt, R.W., Toole, J.M., The energy balance in a warm-core ring's near-inertial critical layer, Journal of Physical Oceanography, Vol. 25, pp. 942-957, 1995.

V. The observations used in this paper span a 13 year time period but there is no mention of nodal corrections being used in the tidal analysis. Modulation of diurnal tidal constituents is generally stronger than modulation of semidiurnal constituents. K1 amplification is 11% (Munk & Bills, 2007). This is unlikely to explain the intensification

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seen at St2, but it should be accounted for in all constituents used, especially when comparing tidal results a decade apart.

Munk, W., Bills, B., Tides and Climate: Some Speculation, Journal of Physical Oceanography, Vol. 37, pp. 135-147, 2007.

VI. Both Klaić et al. (2009) and Book et al. (2009) could be added to the reference list as the former paper is the most comprehensive study of sea-land breezes for the Adriatic and the latter shows that incident and reflected Kelvin waves and Topographic Rossby waves are all needed to describe diurnal tides for the Adriatic. Figure 8 (bottom left) from Klaić et al. (2009) is particularly supportive to the analysis that argues against sea-land breezes causing St2 intensification as it shows a minimum in land-sea breezes on the western side of the Strait of Otranto. Book et al. (2009) is relevant because a superposition of two oppositely traveling Kelvin waves in a channel will produce various phase differences between sea level and current in their combination and this could possibly explain the phase differences shown in Figure 14.

Book, J.W., Perkins, H., Wimbush, M., North Adriatic tides: observations, variational data assimilation modeling, and linear tide dynamics, Geofizika, Vol. 26, No. 2, pp. 115-143, 2009.

Klaić, Z.B., Pasarić, Z., Tudor, M., On the interplay between sea-land breezes and Etesian winds over the Adriatic, Journal of Marine Systems, Vol. 78, pp. S101-S118, 2009.

Technical Comments:

VII. picnocline on page 448 should be pycnocline

VIII. Shouldn't signal propagation on page 452 be phase propagation?

IX. Dark bands in panel a of Figure 2 marking P1, P2, and P3, completely obscure the bars that give the timing of available data.

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X. As stated above, the use of non-significant tidal constituents in Figure 6 makes the patterns of the significant ones harder to see.

XI. The use of black and grey bars in Figure 8 and Figure 9 creates a graphic that is difficult to understand. Why not use simple lines and points rather than an overlapping bar chart?

XII. There is a mathematical 180° ambiguity in tidal ellipse orientation, so the values in Figure 9 around 90° and those around 270° are really the same. 180° should be subtracted from all values that exceed 180° tilt in this Figure and the data should be replotted using a smaller range of orientations (maximum 180° range).

XIII. It is difficult to see the grey line in Figure 10 panels (a) and (b).

XIV. The notation used in Figure 13 is difficult to understand. Could notations like YX1-X2 be replaced with more explanatory labels like partial coherence U-wind?

Interactive comment on Ocean Sci. Discuss., 10, 435, 2013.