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Ocean Science
Discussions

Interactive comment on "Concomitant ocean acidification and increasing total alkalinity at a coastal site in the NW Mediterranean Sea (2007–2015)" *by* Lydia Kapsenberg et al.

Anonymous Referee #1

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In the present paper, the authors analyse two timeseries of carbonate system parameters (e.g. pH, alkalinity, CT and CO2) collected in a coastal site of the NW Mediterranean Sea during the period 2006-2015. A valuable description of the temporal variability at different scales and of recent trends is provided along with a discussion about the possible driving processes. The analysis presented in the paper provides important elements that can shed some light on the dynamics of carbonate system in coastal areas of the Mediterranean Sea. Therefore, the paper is worthy of being published in OS after few major concerns. Statistical methods are mostly appropriate, however the deconvolution analysis, the presence of two trend analyses and the relationship between atmospheric CO2 and sea water CO2 needs some clarifications (see major comments

2, 3 and 4). Results about Point B timeseries are well presented but I would suggest exploiting better the high-frequency timeseries at EOL boy (see major comments 5). Few points of the discussion section seem questionable and need some clarifications: that one about the relationship between the study site and the Adriatic Sea (major comment 6) and that one on the potential drivers (major comment 7). Finally, abstract and conclusion do not summarise exhaustively the valuable work and findings presented in the paper (point 1).

Major comments: 1) Abstract. I have found the abstract poorly informative, lacking to explain the main focus and the relevant findings. The first sentence is not clear to me. It is undoubtedly that monitoring in coastal area is important, however it seems to me that this sentence combines too many concepts. Please, review it. Line 18. The concept "faster-than-expected based on atmospheric carbon dioxide forcing alone", which is repeated twice (at lines 18 and 27), is not clear and needs some clarifications. Line 27. The sentence "localized biogeochemical cycling" should be made clearer. Line 22. The sentence "…its cause remains to be identified" is not consistent with the following "It seems therefore likely that changes in coastal AT cycling via a shallow coastal process gave rive to these observations". Please, review consistently. The sentence "Interesting, the increase …" (line 23) should be improved. Which "increases" is referred to? If the authors refer to the trends computed for each month, please make it clearer. Last sentence seems quite long and difficult to read. Please, rephrase it Keywords. The two keywords "global ocean change" and "near-shore" seems to me misleading. I would remove "global ocean change" since it is not a topic of the paper, and I would suggest using "coastal area" (as it used in the title) instead of "near shore" Conclusion. Few lines about the main findings of the analysis are maybe missing. This would help the paper to convey a clear take home message.

2) The deconvolution method (section 2.3) and results (section 3.2) should be revised. Authors should provide some details on their calculation method explaining how they deal with the hypotheses of linearity and of constant derivatives in time. These hypothe-

ses hold when dealing with annual values (according to Garcia-Ibanez et al. (2016)), but it seems they do not with the weekly data used in the present analysis. In fact, (as an example) the sum of the pH changes caused by the individual drivers differs of about 30% with respect to anomalies pH trends (21% to observed pH trends). It is said that these differences are negligible (line 774), however no clear explanation is given. A comment on this issue should be provided.

3) The authors state that the timeseries is "detrended for seasonality by subtracting monthly mean . . . resulting anomalies were analysed using a linear regression" (Lines 160-164). To my understanding the analysis is performed according to the approach provided by Bates et al., (2014). Given that, why is the linear regression computed on observations in addition to that one on anomalies? These two trend estimates (one on anomalies and one on observations, Table 2) are slightly different but no explanation or discussion is provided but they are used in different part of the text (i.e. the trends on anomalies are commented throughout the text, while the trends on observations are used in deconvolution analysis). This might be misleading. Author should decide which type of timeseries model they are proposing (i.e. first trend then seasonality or vice versa) and use only one.

4) Line 248-251. Not clear. Do the authors propose a linear model relationship between atmosphere CO2 trend and seawater CO2 trend? This should be clarified as well as the assumption of air-sea CO2 equilibrium. As a consequence, the discussion at line 299-300 seems not well supported by the result. A comment on this issue should be provided since it is claimed that this is one of the most important drivers (see lines 440-442 in discussion).

5) Section 3.4. EOL time series is very interesting and it could be better exploited. In particular, I would suggest that the search for event-scale effects should be made considering the variability of pH at local scale (and not using a threshold which is valid for open ocean). In fact, plot 6d shows the presence of daily pH variations larger than 0.05. These possible event-scale effects could be investigated. Most importantly,

authors could resolve the pH variability at daily, seasonal, events and interannual temporal scales producing an additional interesting result. I would encourage the authors to exploit this time series not only for validating the weekly one.

6) Discussion at lines 338-357. It is not convincing the claimed relationship between Point B and the Northern Adriatic Sea. While the Adriatic Sea has a negative relationship with salinity, Point B has a positive relationship with salinity (eq. 2 at line 274). Therefore, the comparison between the two sites is poorly informative of the behaviour of carbonate system at Point B. I would suggest reducing this part of the discussion to those elements that help in understanding Point B dynamics.

7) Discussion about the drivers of AT and CT trends (lines 376-449). This part, although very interesting, is maybe too long and sometimes not well connected to the results. I would suggest shortening this part, focusing on those drivers that are thought to play the most relevant role. Since the authors claim that terrestrial input are important, a description of the rivers and underground sources in the region of the Bay of Villefranche-sur-Mer (and their contributions to AT and CT) should be added into the introduction and used in the discussion for inferring the changes required to explain the observed trends. Moreover, a budget of the AT and CT for the Bay could be estimated, considering the volume of the bay, the exchanges with open sea, and the input terms (from atmosphere and terrestrial sources). This analysis can shed some light on the relative importance of the different boundaries to explain the observed trends, and , eventually, quantifying the missing term.

Minor points: Lines 107 and 128. Please provide the exact length of the timeseries.

Line 118. A description of the riverine input in the area could be of interest. Have rivers along the coast near the Bay of Villefranche-sur-Mer high AT (lines 118-119) or low AT (line 122)?

Line 215. Not clear what "exception" the authors refer to.

Lines 222-223. Why do the authors report that the T trend on anomalies is not significant for the period 1999-2014? Removing the last year (which has high T) seems a subjective choice that should be clarified. If, for any reason, the year 2015 is considered an outlier and it has to be removed, it should be done for all the variables.

Line 231-234. The sentence is quite long and difficult to read, please rephrase it.

Line 254. Does “which peaked in June” refer to parameters or to their monthly trends?

Section 3.3. Lines 262 and 274. It is not clear the message that the authors want to convey. At line 262 it is said that salinity is a poor predictor of AT, however the section ends with a salinity-alkalinity regression. Please, review this section consistently.

Lines 294-295. The sentence is long and difficult to read. Please rephrase it. Further, what do the authors mean for “morning sampling”? Is it referred to the sampling procedure of the Point B timeseries? If so, it should be introduced in Material&Method, and motivation explained if important.

Line 311-312. This sentence seems inaccurate. Which is the causal factor of CT increase due to AT increase?

Line 313. Which “spatial extent” do the authors mean?

Lines 336-337. The analysis of the coastal–offshore gradient would deserve some addition investigations, since offshore deep water is supposed to play a role for CT evolution at point B (at lines 323-325).

Lines 359-369. This part could be moved to introduction.

Lines 772-776. Table 3 caption reports not only the description of the table but also comments on results. Please remove the no necessary text.

Lines 799-801. Figure 6. Please use the caption to describe the plots without describing the results.

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