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Discussions

Interactive comment on "Space-time variability of alkalinity in the Mediterranean Sea" *by* G. Cossarini et al.

Anonymous Referee #2

Received and published: 6 October 2014

The paper by Cossarini et al. uses a model of the Mediterranean to infer information on the distribution of total alkalinity. The paper is, in principle, interesting but it lacks in-depth analysis of the results, and of the concept of alkalinity. The model seems to reproduce observations reasonable, although more efforts should be made to verify this particularly for critical regions close to major estuaries. The model thus provide the means of a more thorough analysis of the Mediterranean Sea alkalinity budget and processes. This potential is not used to its full extent, far from it. I think that the manuscript can be publishable in BG, but major revision would be needed, see below. Major concerns: Abstract lines 5-6: It is such an elementary statement (statement is repeat several times in the ms) that it does not belong in the abstract, if at all it should be phrased differently; it is well known that the alkalinity in the Med is high compared to the Atlantic, nothing new there. If the model did not reproduce this it would be dis-

astrous, at least. Introduction: The alkalinity definition and its dependence on various processes that effect alkalinity is poorly described and discussed. Alkalinity is not a simple function of salinity for a number of reasons; the authors tries to, but do not succeed, in conveying this message. This section needs to be greatly expanded. Section 2.2: There are additional data sets available in public databases (and in MedSea internal data bases) for DIC and Alkalinity that can be used for initialization of the model. Particularly there is an almost complete lack of data in the western basin, despite the availability of data there. This does not seem justified. A list of data is provided by the authors in table 2; why are these not used? Section 3: There is an almost complete lack of discussion of several potentially essential processes that are responsible for the observed distribution and variation of TA, both spatial and temporal. What about remineralization in the water column, on the benthos, sedimentation, budgets? This model frame-work has the potential to shed insight to many of these processes. The framework of a model allows for discussion on these themes; I would like to see this potential used by the authors.

Minor comments: Abstract, line 4: I suggest that the authors use the word "observations" (or "observational") rather than "experimental" here, and in other places in the ms where they refer to observations of alkalinity (or other properties). Abstract lines 8-10: There are not only west-east gradients, also north-south gradients (although mostly of secondary importance). This could be rephrased to reflect the influence on regional phenomena or processes. Abstract line 20: This statement is wrong; which has clearly been shown by others (Álvarez et al., 2014;Palmiéri et al., 2014). Also a statement that "which might indicate a higher buffer capacity" cannot be written in a paper; the buffer capacities can easily be calculated based on elementary chemistry of the carbonate system, the authors should do these calculations themselves rather than speculate, particularly when the speculations can be easily proven to be not correct. Abstract line 11: Why is dense water formation in the western Med not mentioned in this context? Abstract line 16: This is poorly phrased. There is always some fresh water influence (otherwise little S variability). I think the authors want to say that in some areas the

freshwater from major rivers has higher alkalinity than the Med, causing a negative S/TA relationship. Page 12873, line 3: remove the word "potentially" Page 12873, line 8: Why "ad hoc" studies? Page 12873, line 14: Add reference to Takahashi et al. (2014). Table 1: Is the alkalinity of the Tyrrhenian that high? I do not think so, correct or explain. Table 2. Please add information on the average TA content of the main rivers, not only the discharge. This is important to understand the intercept of the TA/S relationship. Figure 5. These figures need different scales to be readable. Also the legend needs to explain where these selected points are (ref to Figure 1). Figure 4: This is probably the highlight of the manuscript. It would be very interesting to see a similar plot made based on observational data. For this the authors probably need to explore the Med-Atlas collection as well as those already listed in Table 2. Deviations between model and observations needs to be discussed in more detail, and such a figure could be a vehicle for doing so. References: Álvarez, M., Sanleón-Bartolomé, H., Tanhua, T., Mintrop, L., Luchetta, A., Cantoni, C., Schroeder, K., and Civitarese, G.: The CO2 system in the Mediterranean Sea: a basin wide perspective, Ocean Sci., 10, 69-92, 10.5194/os-10-69-2014, 2014. Palmiéri, J., Orr, J. C., Dutay, J. C., Béranger, K., Schneider, A., Beuvier, J., and Somot, S.: Simulated anthropogenic CO2 uptake and acidification of the Mediterranean Sea, Biogeosciences Discuss., 11, 6461-6517, 10.5194/bgd-11-6461-2014, 2014. Takahashi, T., Sutherland, S. C., Chipman, D. W., Goddard, J. G., Ho, C., Newberger, T., Sweeney, C., and Munro, D. R.: Climatological distributions of pH, pCO2, total CO2, alkalinity, and CaCO3 saturation in the global surface ocean, and temporal changes at selected locations, Marine Chemistry, 164, 95-125, http://dx.doi.org/10.1016/j.marchem.2014.06.004, 2014.

Interactive comment on Biogeosciences Discuss., 11, 12871, 2014.