

Interactive comment on “Carbonate chemistry in sediment pore waters of the Rhône River delta driven by early diagenesis (NW Mediterranean)” by Jens Rassmann et al.

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In this manuscript Rassmann et al. present an extensive data set on pore water chemistry for sediments from the mouth of the Rhône River to the continental shelf. The data is of high quality and very interesting. Unfortunately, the discussion of the results is too general and poorly focused. Overall I think much more could be done with the data.

Much of the data interpretation is too speculative or is simply based on comparisons with what other workers have seen in these (and other) sediments. In many places the text reads more like a data report interspersed with comments about similarities between these results and results from other studies. The things that are new and exciting and different about this work, as compared to these other studies, are not

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clearly presented.

Questions about whether sediments such as these are alkalinity sources is an important one, and the authors note this in places in the text. While they do have some discussions of their results with such considerations in mind, the discussions are rather disjointed. At a bare minimum, Fig. 5 shows that all of these sediments are a source of alkalinity to the water column, although this simple observation seemed (at least to me) to get lost in the overall discussion. I would urge the authors to re-structure the paper so that this general topic is much more clearly examined with their data. In my opinion, this will make this paper one that people will want to read (and should read).

Before final publication the manuscript will need to be carefully copy-edited by a native or fluent English-speaker. There are many places where there are grammatical errors, awkward syntax, and curious phrasings.

One last general comment. When I read lines 91-94 and the sentence starting at the end of line 124 (“The sea floor in this region . . .”) I had the sense that these sediments have some degree of similarity to those of other large river deltas like, e.g., the Amazon (see, for example, Aller’s 1998 Marine Chemistry paper cited here). In contrast, much of the discussion of the data in the text takes a very traditional, steady-state “Froelich et al.”-type approach (see, for example, section 2.7 and much of section 4.1). To me, this approach seems to contradict the text on lines 91-94 and 124, and I think that some clarification is needed.

Specific Comments (line numbers in parentheses)

(215) I never realized there were 12 parameters of the carbonate system. Is this a typo or am I missing something?

(225) Here and on line 291 they talk about good agreement between measured and calculated pH values. It might be good to show this, and/or present some additional information like, e.g., the slope and r^2 value of a scatter plot of the two pH’s.

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(251) I would probably be good to list here what atmospheric $p\text{CO}_2$ was at the time of sampling.

(265) The way the pH data is plotted makes it hard to see things like differences in inflection points for different regions. It might be helpful to break Fig. 4 up into 3 panels like Figs. 5 and 6. It might also be useful to similarly sub-divide Fig. 2 (O_2 profiles) into 3 panels.

(286) Are these slopes statistically different in the three different regions? If not I would not report them separately but would simply list an overall slope for all of the sediments.

(317 -) Plotting sulfate concentrations and carbonate saturation state for each region on the same panels is very confusing. I would recommend separating them.

(405-) I would think that all of the things discussed here (organic matter oxidation state, carbonate precipitation, AOM) would affect the magnitude of the slope of a $\Delta\text{DIC}/\Delta\text{Sulfate}$ plot, and not the scatter around the best-fit line. I'm also surprised that the slope is ~ 2 despite all of these factors. Maybe they act (somehow) in such a way as to cancel each other out?

(473) I don't see any direct evidence in the paper that terrestrial organic matter is what is being degraded. It might be, but without evidence to support this I would not be so definitive.

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