

Interactive comment on “Organic and inorganic carbon and their stable isotopes in surface sediments of the Yellow River Estuary” by Zhitong Yu et al.

Anonymous Referee #1

Received and published: 8 December 2017

The author of this paper present new data of organic and inorganic carbon from the Yellow River Estuary. Authors describe the pattern observed, and conclude that it is a complex system and that some previous findings in the literature are probably right. In my opinion, this is insufficient to be published in Biogeosciences. I do not feel that the conclusions are novel, nor that they are actually based on the new dataset presented here. Find below my evaluation of the paper in regards to the criteria of BG.

1. Does the paper address relevant scientific questions within the scope of BG? No
2. Does the paper present novel concepts, ideas, tools, or data? Data are incremental to Liu et al., 2012, 2014, 2015 (etc.) who all reported similar data from the same region.

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3. Are substantial conclusions reached? No, the discussion is very descriptive and the conclusion overly vague: "Our study points out that the dynamics of sedimentary carbon in the Yellow River Estuary is influenced by multiple and complex processes, and highlights the importance of carbonate in carbon sequestration". In my opinion, this is not enough for Biogeosciences. I would expect the author to come up with a precise discussion of the potential processes and at least some hypothesis to test in the future. Furthermore, I would also expect some sort of quantification of the inorganic carbon sequestration, because how can one claim its important if not measured?

4. Are the scientific methods and assumptions valid and clearly outlined? Analytical methods seem fine but assumptions are not clearly outlined and it is hard to understand the logic behind the limited interpretation. Example: "Our analyses revealed a significantly positive correlation between TIC and TOC ($r=0.97$, $p<0.01$)". Which statistical test was performed? Is the distribution normal? It doesn't look like it from here. Also, what is the process potentially linking both?

Later in the text, it is stated that when TOC decompose it releases CO₂, which promote TIC precipitation. But then, why the relationship is positive and not negative? The relationship should be between TIC and the amount of TOC degraded. Would that be correlated to the total amount of TOC left after degradation? One can raise serious doubt about that. Especially with the relatively small range of concentration. Was any other potential relationship explored? The TOC/TIC and isotopic proxies seem to also follow the same pattern than the composition of the sediment (clay, silt, and sand). Could your distribution simply an effect of different sedimentation regimes?

5. Are the results sufficient to support the interpretations and conclusions? No, I feel the conclusion build more on previous study than the actual data presented here.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Yes

7. Do the authors give proper credit to related work and clearly indicate their own

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new/original contribution? Yes.

8. Does the title clearly reflect the contents of the paper? Yes

9. Does the abstract provide a concise and complete summary? More or less

10. Is the overall presentation well structured and clear? Yes

11. Is the language fluent and precise? Yes, in general

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Yes, the discussion should be improved, to discuss more in depth the different processes in order to come up with a more elaborated conclusion

14. Are the number and quality of references appropriate? Yes

15. Is the amount and quality of supplementary material appropriate? Yes, NA

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2017-353>, 2017.

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