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Biogeosciences Discussions

Interactive comment on "Modeling biogeochemical processes in sediments from the Rhône River prodelta area (NW Mediterranean Sea)" *by* L. Pastor et al.

Anonymous Referee #2

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In this study Pastor et al described the early diagenesis of organic matter (OM) in the Rhone prodelta. In situ-measurements and model data were used to understand the burial and mineralization efficiency at different water depths and the relative proportion of oxic vs anoxic mineralization. The paper is well organized and it reads well. Given the implications of this study and the role of river-dominated margins in the global cycling of reactive elements I strongly suggest this paper for publication in Biogeosciences.

On the other hand, I have one major criticism. My concern is mainly focused on the model and on his meaning in this river-dominated margin. Specifically, OMEXDIA was

developed and applied in systems whose steady-state nature allowed assumptions concerning both OM origin and sedimentological processes. In stationary conditions, the model generates steady state profiles of several parameters including oxygen, ammonium, nitrate, oxygen demand units organic carbon and particulate carbon. However, sedimentation in the shallow Rhone prodelta is highly episodic and I have hard time to understand how the OC burial, for example, can be assessed in sites characterized by event-deposition. In the open ocean, steady state accumulation rate can be assumed and confirmed by down-core profile of short-lived radionuclides (e.g., Epping et al., 2002, Progress in Oceanography; Van Weering et al., 2002 Progress in Oceanography). Conversely, a series of geochronologic studies carried out in the Rhone prodelta showed his non-steady accumulation characterized by transient sedimentary signals >10 cm in shallow waters ($\sim$20-30 m). Thus, how a 10-cm long sediment core can be representative of the burial in this system? Furthermore, despite the episodic supply of sediment, there are well characterized periods when the balance between accumulation and erosion is in favor of the latter (Marion et al. 2010), affecting the down-core profile of particulate OM. Similarly event-beds exhibit contrasting sediment texture, another factor that affects the down-core distribution of reactive OM.

Therefore, I do understand that model assumptions are important for the his proper implementation. However, some of these assumptions (such as the steady-state accumulation, no erosion, same sediment texture) do not actually find evidence in the current literature. Other examples include the bioturbation and OM origin. In the model Pastor et al assumed an ubiquitous mixed layer 13 cm thick based on SPI data (Desmalade, oral communication). However, it's hard to believe that the bioturbation just off the Rhone mouth (20 m) is the same as in the deep prodelta (90 m). For example, recently Drexler and Nittrouer (2008) using x-radiographs have grouped the Rhone prodelta in three sub-regions defined as physically stratified (i.e. laminae), partially stratified and bioturbated consistent with the conceptual model (accumulation vs bioturbation) developed by Wheatcroft et al. (2003, Progress in Oceanography). Similarly, the OM reactivity in the model is somehow simplistic accounting for non-reactive

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(burial), relatively labile, and particularly labile. However, it is well established that OM in marine sediments is extremely heterogeneous and exhibit a wide range of C/N ratios and reactivities that result in the selective preservation during diagenesis.

In brief, the authors should do a better job in explaining their assumptions. I definitely think that insights coming from this study are extremely interesting but I strongly suggest the authors to better justify their assumptions making sure that the reader is aware of the dynamic nature of this system. In fact, this is the first time that OMEXDIA was applied in a river-dominated margin and therefore the authors should explicate all their assumptions and relative consequences/interpretations taking into account the peculiarity and sedimentological features of the environment studied.

Specific details

- Given the not well constrained asymptotic down core content of OC, wouldn't it be better to come up with several potential scenarios? Maybe some literature research might help to define a series of asymptotic values (at least for a few stations). Burial occurs on long time scales and likely the thicknesses of sediment core collected just off the river mouth corresponds to seasonal deposition. So if nothing can be found for the Rhone prodelta I would suggest using asymptotic values of similar study areas (i.e. prodeltas having comparable mass accumulation rate and OC). I still think that Figure 4 must be shown but it should contain at least a supplementary Figure b that considers other asymptotic values. Would it be possible?

- The reader might find useful some formulas throughout the text as in Epping et al., 2002 (Progress in Oceanography). A table with all terms and acronyms used in the text it would be handy. In addition, the authors might consider to display a simple table showing the main pathways of organic matter degradation and reactions (aerobic respiration, denitrification, reoxydation of reduced ions, etc)

- The last paragraph is over two pages long and it should be split into two separate sections.

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Finally, I recommend the authors to read the following papers before starting the review process: - Drexler T.M and C.A. Nittrouer. Stratigraphic signatures due to flood deposition near the Rhone River: Gulf of Lions, northwest Mediterranean Sea. Continental Shelf Research 2008

- Miralles J, M. Arnaud, O. Radakovitch, C. Marion, X. Cagnat. Radionuclide deposition in the Rhône River Prodelta (NW Mediterranean sea) in response to the December 2003 extreme flood. Marine Geology 2006

- Marion C., F. Dufois, M.Arnaud, C.Vella. In situ record of sedimentary processes near the Rhone River mouth during winter events (Gulf of Lions, Mediterranean Sea). Continental Shelf Research 2010

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