

Response to Reviewer Amy East on “Late Holocene evolution of a coupled, mud-dominated delta plain–chenier plain system, coastal Louisiana, USA ” by M.P. Hijma et al.

Marc P. Hijma on behalf of Zhixiong Shen, Torbjörn E. Törnqvist, Barbara Mauz

We appreciate the review by Amy East. Below our responses to her comments are given in underlined italics.

The stated hypothesis to be tested, that cyclic Mississippi delta subshifting has influenced evolution of the chenier plain coast, sounds a bit tepid because a number of studies in the 1950s through early 2000s (cited in this paper) already showed pretty convincingly that the evolution of the chenier-plain coast and associated continental shelf are linked to the activity of various sublobes of the Mississippi-Atchafalaya delta. Framing this study’s objectives under a more specific hypothesis would set it up so that the results have greater impact (unless the authors intended to test an alternative hypothesis because they didn’t believe the findings of those earlier studies, which doesn’t seem to be the case).

Regarding our hypothesis, we disagree with the referee that the hypothesis was convincingly tested. Without the robust chronology that we presented it was also not possible to do so. Our work shows that the hypothesis is only partly valid and local/regional processes played an important role. We write that we want to test the hypothesis more rigorously and we still think that this a good way of describing the main core of our work.

The introduction presents well the larger scientific and management context of the work, which is substantial in scope and importance. It would be good to remind readers of this big-picture context again at the start of the Discussion section (beyond implications for inferring sea-level history), and revisit the implications further in the Conclusions. As is, the last several sections of the paper are focused so specifically on this immediate region that it may start to lose the broad scientific audience. It is also worth pointing out early in the Introduction (where it is mentioned that few studies address mud-dominated shoreline evolution) that the dynamics of mud-rich coasts are substantially different from those of sand-dominated shorelines, about which much more is known.

Both at the end of the abstract and the introduction we now highlight the broader implications of this study. In addition, we have added a paragraph in our discussion of the implications for coastal restoration that stresses the importance of using work like we presented here to improve numerical models since these latter will become increasingly important, also globally, in order to save delta from drowning due to sediment mismanagement and relative sea-level rise. Sentences of similar content have been added to the conclusions.

The section of the introduction that deals with background information on deltaswitching in the Mississippi system needs more complete referencing, e.g., the work of Coleman, additional work by Oscar Huh beyond that cited here, and others. There is a much larger body of literature on this than the text currently reflects.

Regarding more complete referencing. There is indeed a very large body of literature present on delta-switching in the Mississippi Delta Plain. We chose to refer to the latest review by Blum and Roberts, because it would not be feasible to include all previous work. We propose to add an additional reference to the Coleman et al. review from 1996. These two references should give readers a good starting point if they want to have more background on this topic.

p. 13, bottom: the “capture” of the mainstem Mississippi by the Red/Atchafalaya River is better constrained than this. It occurred because a meander bend of the Mississippi (Turnbull’s Bend) migrated laterally until it intersected the Atchafalaya during the 15th century, but the full capture has been both inadvertently assisted and now limited in scope by engineering works since the 1830s and especially since the 1950s. This bears mentioning in the text. I also couldn’t find mention in the paper of the fact that the Wax Lake Delta formed from an artificially engineered outlet during Atchafalaya River flooding. The authors are certainly aware of these facts, but please state them in the paper for the benefit of readers unfamiliar with this river system.

Capture of the Atchafalaya. We didn’t include any details of Turnbull’s Bend, because it was not directly necessary information to help us argue that the Atchafalaya River started to have significant sediment output only after 0.3 ka. We added one sentence to introduce Turnbull’s Bend and the importance of the removal of a large raft

Late in the Discussion and in the Conclusions, please expand on the potential broader implications for mud-dominated coasts beyond this field area in terms of coastal management, or landscape response to climate change and/or watershed-sediment-supply changes. The Discussion (section 6.1) does go into implications for inferring relative sea level from chenier coasts, which is an advance in understanding the dynamics of mud-rich shorelines and deltas better, and the paper does discuss implications for regional restoration scenarios: : : but can make further contributions by commenting on the additional broader issues just mentioned beyond this geographic region.

We included a reference to the Wax Lake Delta.

Technical corrections:

Section 3.2.1. ends abruptly; it’s unclear whether the last sentence was truncated inadvertently.

We removed the truncated part

Figure 14, caption, fix typo: “accumulation rate”

We fixed the typo.