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Earth Surface Dynamics Discussions

Interactive comment on "On the Holocene Evolution of the Ayeyawady Megadelta" *by* Liviu Giosan et al.

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Review of ESurf-manuscript 2017-064 (Giosan et al.)

Giosan et al. present new field data from one of the least studied large deltas on the planet. As such, this is a potentially useful contribution that might serve as a launching pad for more detailed future investigations. While I appreciate the challenges of working in a relatively remote and underexplored delta, the dataset presented here is very modest in size. As a result, several of the interpretations are tenuous, as detailed further below.

A significant portion of the study relies on geochronology, including a set of new OSL ages. OSL dating in these geologically young terranes has often proven to be challeng-

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ing, as exemplified by the nearby Ganges-Brahmaputra Delta where OSL chronologies have been notoriously problematic (but see a recent paper by Chamberlain et al., 2017, QG). Conditions in the Ayeyawady drainage basin may be different, however – something that would be worth addressing. For example, the authors might consider including some OSL decay curves to illustrate the dominance of the fast component in their quartz sands. Nevertheless, without verification of the OSL ages by means of independently obtained dating results (either historical ages or from other radiometric techniques), some caution is probably in order.

Setting these concerns aside, the stratigraphic context of the two OSL samples from natural levee-deposits near the delta apex is not well documented, preventing the reader from fully assessing their interpretation. The map (Fig. 2c) shows sample locations with respect to the surface morphology (including what appears to be oxbows) and the tables indicate the depth of the samples below the land surface. What is needed here is some subsurface information (i.e., cross sections) that shows the geometry and extent of the natural-levee deposits. With the information presented, all one can infer is that overbank deposition occurred around 1.5 ka. Likewise, a 14C dated wood trunk in a point bar doesn't really constrain anything. Assuming that it is contemporaneous with the point-bar deposits (which is by no means certain), the only thing it would reveal is that the point bar was actively forming at that time. When the associated channel belt started to form is an entirely different issue. Note that a rigorous sampling strategy is needed to determine the beginning and end of activity of channel belts in such settings; this would require considerably more subsurface data than presently available. Without such data, inferring avulsions remains a guessing game.

On the other hand, the interpretation of the beach ridge geochronology should be a little more straightforward. The possible temporal correlation of the oldest beach ridges with those in other SE Asian deltas is an interesting phenomenon to point out, even though the interpretation of potential causes must probably remain somewhat speculative at this point. Within this context, I would suggest the authors consider what may be a much simpler explanation. Assuming that modern sea levels in this part of the world were approached around 5000 years ago, I wonder how one can rule out that older beach ridges exist but are simply buried in the subsurface.

Returning to the inferred avulsion, it should be noted that avulsion is fundamentally an autogenic process, even though it can sometimes be triggered by allogenic forcing. Therefore, it seems unnecessary to invoke such mechanisms to explain a single avulsion. Given the overall setting that the authors describe (one with substantial Holocene aggradation) it is to be fully expected that many avulsions have occurred in this delta.

The inferences about subsidence rates beneath the Ayeyawady Delta based on comparison with the Lambeck et al. (2014) sea-level curve (lines 490-494) are untenable. If the authors want to compare their mangrove-based sample with a globally averaged sea-level curve in a meaningful way, they need to remove the effects of glacial isostatic adjustment that are significant virtually everywhere (see, e.g., Milne & Mitrovica, 2008, QSR). For example, hydro-isostatic effects (also known as continental levering) are potentially substantial along continental margins such as this one. In other words, accounting for these effects would require GIA modeling. Besides, inferring vertical stability in such a tectonically active setting seems like a dangerous proposition in the first place. And finally, the mangrove peat is unlikely to be compaction-free since it is not a basal peat (see below).

The supplementary information seems short enough that it could easily be incorporated in the main text. Otherwise, the manuscript is very long and could be shortened considerably without much loss of information.

Finally, these additional notes:

Lines 93-94: most readers are probably unfamiliar with these regional historic periods.

Line 278: “meandering belts” should be “meander belts” (or better yet, the more generic "channel belts").

Lines 387-388: "lower delta plain" is a more widely used term in this context than "outer delta".

Lines 392-393: or, alternatively, they have simply not been active very long.

Lines 410-411: note that basal peat is defined as immediately overlying a consolidated (commonly Pleistocene) basement. In this case, one would assume that weakly laminated muds are Holocene in age, which makes the mangrove peat an intercalated peat bed.

Line 484: while it is conceivable that there is such a thing as a paleovalley in the subsurface, it is a bit uneasy to just state this with no supporting evidence. I suggest some rewording, here and elsewhere.

Line 500: this looks like simple autogenic channel scour to me. Note that "erosion event" might be misconstrued by the reader to reflect floodplain degradation on a wider scale.

Line 519: since these are said to be rates, I suppose this should be m/yr or something of the like?

Fig. 1: please indicate the drainage basin of the Ayeyawady River; this is important, among others, in view of the comments above about OSL dating. A scale bar would be helpful too.

Fig. 3: the interpreted depositional environments include terms that are not mutually exclusive (e.g., floodplains are fluvial).

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