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

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RESPONSE: We appreciate the thorough review by Tor Törnqvist (in Arial, bold and italics) and provide our response below.

REFEREE: 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.

RESPONSE: Indeed we wrote this paper not to address specific problems in the

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Ayeyawady delta but to provide a basis for and highlight future lines of enquiry in a region little known before. In our revision we try to make this point clearer and provide interpretations with the proper degree of uncertainty suitable at this stage.

REFEREE: 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 challenging, 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.

RESPONSE: We appreciate the heads up on the recent paper by Chamberlain et al. The problems raised by the reviewer may apply to the fluvial sediments that we dated (levee samples). Nevertheless, all samples dated, fluvial and bech ridge, are dominated by the fast component. We provide an example in the revised supplementary data.

REFEREE: 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.

RESPONSE: Our sampling strategy was exploratory and suited to the field context and lack of accessibility due to habitation on delta highlands like fossil ridges and levees. But we agree with the reviewer that the presentation and interpretation can be improved on. Samples were taken from the top of levees to access the latest sediments deposited and deep enough to be undisturbed by human activity. We added text to explain this subsurface context. Indeed dating the inception of channel belt is not achievable with the strategy employed and was never our intention, as we now make clear. However, dating the activity on the youngest levee of a channel belt provides a limiting date for the abandonment of that belt. All dated sites have already been provided since the initial submission with lat-long information. We could include zoomed-in Google Earth images of the locations in the supplementary if the editor suggests it but readers can easily visualize locations at the resolution they need using information from Table 2.

REFEREE: 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.

RESPONSE: Indeed the interpretation provided is a starting point that will be explored in detail in the future.

REFEREE: 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.

RESPONSE: We are not clear what this comment means exactly. Based on previous reconstructions in South and Southeast Asia the sea level in the region most likely had an early to mid Holocene highstand above than present level. Based on the drill record at Kyonmangay the delta already reached there at that time. If they existed, older beach ridges would have been located more inland. Based on the morphology of the delta plain significant fluvial accretion that could lead to burial is limited to meander belt regions near the two main courses of the river well upstream of Kyonmangay. With sea level since 5000 years ago falling it is hard to envision burial of ridges on the delta plain south of Kyonmangay to the latitude of Labutte where the first ridge was dated at $\sim$4.6 ka. Published work cited by us for other deltas in the region that show ridges establishing themselves in late Holocene also do not indicate signs of burial.

REFEREE: 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.

RESPONSE: We agree with the reviewer on this point as we fully expect that the river avulsed many times during the construction of its Holocene delta. Nevertheless we refer to its last major avulsion that is indicated by the existence of the two alluvial ridges, one of which is now abandoned as a result. However, we removed the more speculative aspects of the story on the allogenic forcing, leaving it for future work.

REFEREE: 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).

RESPONSE: The reviewer is correct. A recent paper looking at GIA component in nearby regions does suggest subsidence and show a mid Holocene highstand. On the other hand preservation of deltaic beach ridges of late Holocene age argue for relative stability. We have rewritten the text to nuance our interpretation and eliminated the mention to stability from the conclusion and abstract.

REFEREE: 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.

RESPONSE: The revised supplementary has now more info that can be accessed by readers interested in data. However, we tend to disagree with the reviewer on the length of the manuscript. We ran an informal test on other colleagues on this point and they appreciated the comprehensiveness of the paper for a region that is little studied. Given that the journal has no printed version we see no reason to significantly shorten it but we'll do so if the editor suggests it.

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

RESPONSE: We provided more info in the revision.

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

RESPONSE: Fixed.

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

RESPONSE: Fixed.

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

RESPONSE: Fixed.

REFEREE: 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 peatbed.

RESPONSE: We debated this among ourselves as the muds are fluvial, freshwater in nature, rather than strictly marine mudflats. The text was modified to correct it based on the point made by the reviewer.

REFEREE: 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.

RESPONSE: We disagree with this view: our drill sites indicate the base of the Holocene below the present sea level. Based on what is know about the geodynamics and climate of the region we see no reason for a sediment-rich river like Irrawaddy not to have built a delta at any time during Pleistocene highstands most of which were higher than the present sea level. Pleistocene sediments have been described in oil exploration boreholes across the delta at greater depths than the present sea level. They are not differentiated at higher resolution age intervals in an attempt to delineate

the valley at this time as we would have liked.

REFEREE: 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.

RESPONSE: Fixed.

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

RESPONSE: Fixed.

REFEREE: 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.

RESPONSE: Fixed.

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

RESPONSE: Fixed.

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