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Earth Surface Dynamics Discussions

Interactive comment on "The Usumacinta-Grijalva beach-ridge plain in southern Mexico: a high-resolution archive of river discharge and precipitation" *by* Kees Nooren et al.

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I enjoyed reading this paper. It presents a laborious research that provided the refined chronology and morphological architecture of a huge coastal plain of Usumacinta-Grijalva in Mexico. While I would prefer to call this coastal system a wave-dominated delta rather than a beach-ridge complex, the paper presents valuable data which potentially provides insights into the nature of the beach ridge and associated long-term coastal evolution. You attempted making the paper focused and appealing by stressing the possible linkage of the beach-ridge height to the fluvial sediment supply, but the present data so far may not be convincing enough. With a nice combination of

OSL and radiocarbon chronology and Lidar elevation data, you found an interesting, negative correlation between the apparent progradation rate and the thickness of the aeolian accumulation on top of the prograded beach deposits, the core of beach ridge. The apparent progradation rate, however, in contrast to the title, is not related exclusively to the fluvial sediment supply, but also affected by several factors, such as the onshore wind strength and direction, and river avulsion and associated re-organization of the distributary channels. You are aware of this in the discussion. I also think that the river does not have to be an exclusive sediment source, as in the Gulf of Mexico, it is widely considered that the shelf sand is an important source of beach-ridge sands (e.g., Taylor and Stone, 1996, Journal of Coastal Research). You try to emphasize that the river has the large capacity of sediment supply with the aid of fragile Late Pleistocene volcanic rocks. However, I am curious where did the quartz grains, which are believed to comprise 50-65% of the beach ridge sand; L118-119), come from? I think they are unlikely from the volcanic rocks as volcanic quartz generally has inappropriate OSL properties for dating (e.g., Tsukamoto et al., 2003, Radiation Measurements). There should be a significant contribution of shelf sands as well, and thus the implied linkage between the beach-ridge height and fluvial discharge (as in the title) would be misleading. I suggest you reorganize the paper as just to examine the relationships between the ridge development and the apparent progradation, properly address the presence of other sediment sources, and then thoroughly discuss how the coast developed. Apart from this, I found numerous minor and moderate points that I hope you to reconsider as follows. In summary, I support the publication of this paper, but there is much to be reconsidered and refined.

Title: This coastal system looks like a wave-dominated delta rather than a beach-ridge complex. It would be evident that the fluvial sediment supply is significant if in the delta system. There is yet a large gap until you can use the beach ridge height as proxies of fluvial discharge and precipitation. In this paper you do not say anything about the temporal fluctuations in precipitation in the catchment, and so the title would be misleading. L81: insert Usumacinta-Grijalva before 'beach-ridge plain'. L85-86:

Why not cite the criticism by Tanner & Stapor (1971, Trans. Gulf Coast Assoc. Geol. Soc.). L117-127: This argument should be shortened and merged into the last one. L149: 'the same extent', bigger or smaller? L164-165: insert 'high' after '0.3 to 0.7 m'. L185-192: These arguments are more than the methodology. L265: 90Sr/90Y beta source L276-279: This argument is not necessary or accurate, and should be removed. L301-302: This does not make sense here before knowing the details of the dataset. L304-309: This section needs more detail. L312: should shore-normal, nor cross-normal. L319-320: You need core logs presented in the paper or at least in the appendix to estimate the thickness of the deposits. You may be confused with what is 'beach-ridge deposits'. It should not be so thick here, and does not include the subtidal shoreface deposits. Barrier sand or barrier deposits would be a broader term appropriate for what you call 'beach-ridge deposits'. L327-328: I am not convinced with your assumption of the constant run-up height. The boundary between beach/foredune could be the swash limit of the higher waves during the spring high tide. The spring tide range is 0.75 m and the swells can be up to 0.7 m high, yielding higher swash height than +0.5 m above the mean sea level. Besides, the NW swells can be up to 1.7 m high, implying the longshore gradient of swash height. L346: There appear to be two sets of definition of transects (A-C, and 1-15). This is confusing. L354: To me the faults look almost 45 degrees to the shore. L355: Not clear whether it means eastward tilting or westward tilting. L358: I cannot find any holes in Fig. 2b. L394-396: This may repeat the argument in the methodology, and strictly, is a bit different from that. Above you appear to have said the MAM was applied to all cases with OD >30%. L399-403: Delete. L410: The significance of the 600 years depends on the absolute age and so you should mention the absolute values of these two ages here. L424-426: Remove this argument, as it is not necessary. L454-461: I do not like so much discussion and interpretation presented in the result section. The last argument is problematic; how can you quantify the aeolian accretion rate separately from the chronology? L469-470: Remove this. L495: Be sure that the steepening of the shoreface is related to the coarser sand but not to the higher waves. L502: This is a microtidal system and

tidal current is minor. L520-521: I cannot follow this. It is too obvious to say that some aeolian processes act on the backshore and foredune ridges. L550-552: This sounds over-interpretation. Where is the swash bar welded to the beach corresponding the infill of a large runnel? L562-563: This is where you mention Tanner and Stapor (1971). L601: You should describe how you picked up the general trend. You appear to pick up the bottom of the trough/swale; is there rational behind this? L619-621: How did you define the thickness of what you call beach-ridge deposits at Keppel and Guichen bays, while in these sites the subtidal barrier deposits have not be explored. L636: This number highly depends on the thickness of the subtidal barrier deposits. L641: Why? Transect B occurs across the NE-SW to ENE-WSW trending coast, and the easterly wind blows from behind the foredune ridge and should not promote the onshore aeolian accumulation. L651: Fig. 3b is not informative enough. You need to show core logs at least in the appendix. L690-692: How can you tell the shape of the eroded, lost delta river mouth? Is it evident from Fig. 2a? L696: the marked increase is not evident in Transect A. L701-703: What data critically show this? L709: The break is not evident in figures you provide. There is no apparent age gap as well. How can you define it? L718-720: A similar view was given by Tanner and Stapor (1971). L739: cite Shepherd (1991, in Applied Quaternary Studies. Australian National University). L755: But other periods show more prominent ridges. L784: This is just what you imagine. How certain is it? L792: Hopefully you show some independent climate records here. Fig. 1b: Where did the sediment come from to form the updrift (eastern) part of the Phase 3 just next to Ciudad del Carmen. Fig. 7: What do you mean by 'beach/ridge'? 'Beach' is a broad term including the swash zone as well. Fig. 12: Why you show multiple plots for each episode of transect. Just one mean value for each is fine.

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