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Climate of the Past Discussions

Interactive comment on "Technical note: Optimizing the utility of combined GPR, OSL, and LiDAR (GOaL) to extract paleoenvironmental records and decipher shoreline evolution" *by* Amy J. Dougherty et al.

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General Comments:

Recent years have been exciting for research on progradational systems. It is during this time that scientists have been moving at an accelerated pace from the study of the evolution of beach ridges (and associated landforms) as geomorphic features to the use of these progradational systems as archives of past changes in sea level, sediment supply, wave energy, and storms, among others. These authors present some excellent thoughts and summarize several best-practices for using and coupling different topographic, subsurface, and geochronological datasets to achieve these latter goals. This manuscript presents a case for the integration of LiDAR-based topographic data, ground-penetrating radar (GPR) -based stratigraphic data, and optically stimulated luminescence (OSL) -based geochronologic data in reconstructing paleo-environmental change (storms, sea-level, etc) from beach- and foredune- ridge plains.

The manuscript is well written, well motivated, and very well organized, particularly the inclusion of general best practices for each of the tools, followed by discussion of a series of case studies which have successfully applied this approach. The basic premise is indisputable: the combination of multiple tools can allow for high-resolution mapping as well as comprehensive reconstruction of system evolution, and in particular those components which could impact paleoenvironmental reconstruction interpretations. Not following these, and other, best practices in the study of these clastic depositional systems can lead to misinterpretation of the paleoenvironmental record.

I suggest four general revisions for this manuscript: 1) Consider the addition of ground-truthing as a fourth approach, equally as important as LiDAR, OSL, and GPR. 2) Recognize the limitations of certain field sites and conditions which may make any one of the three (or four) “hat trick” components not possible, or not the best approach for a given site. 3) Consider adding examples from additional global sites; this does not need to be in the discussion of the three “case studies” as those are meant to be focused on single papers. 4) consider a more measured treatment of the Oliver et al (2017a,b) studies. I elaborate on each of these below, with specific examples and suggestions.

General Revision #1: I would argue that the “hat trick” described (LiDAR, GPR, OSL) might be better constructed as a “grand slam” (are we taking the sports analogy too far?). That is, sediment cores (or some other ground-truthing of non-invasive subsurface data) are a critical fourth component to these types of studies, equal in importance to remote stratigraphic data, high-resolution topography, and proper dating.

General Revision #2: The “hat trick” goal is not always achievable, as the authors recognize. Despite advances in data collection using balloons, kites, drones, as well as similar topographic mapping techniques using ground-based RTK-GPS mapping and aerial structure-from-motion, collection of high-resolution data over large areas (e.g., complete beach- and foredune- ridge plains) remains expensive. The best datasets tend to be those collected by government agencies and made publically available; while the accessibility of such databases (and other tools for mapping by individual scientists) is rapidly expanding, they are far from ubiquitous, especially in remote locations. Likewise, GPR suffers from signal attenuation by saltwater and muddy sediments; the latter is a particular challenge in trying to integrate stratigraphic data from modern beach systems. Finally, OSL is geographically limited to systems composed of quartz sand and, as commenter Shen notes, subject to methodological assumptions, error, and reporting inconsistencies. Other chronological techniques such as radiocarbon dating of (ideally in situ) shells, remain generally less expensive and applicable to coastal systems composed of, for example, mud, coarse gravel, or carbonate sand. For example, Long et al (2012; QSR 48:61-66) present a robust sea-level reconstruction from coarse clastic beach ridges in Svalbard based on bivalve 14C dating. Billy et al (2015; Geomorphology 248:134-146) do the same using GPR and RTK-GPS-based topographic mapping, but with OSL largely from sandy foredune ridges which overlie (and therefore may postdate by some unknown amount of time) a coarse clastic beach-ridge plain which could not be sampled for OSL. Thus, while the authors present what may be an “ideal” approach, it is important to recognize that site or data limitations do not invalidate studies that cannot achieve the GOaL “hat trick”. In particular, radiocarbon should certainly not be dismissed out of hand for all sample types and depositional environments.

General Revision #3: As this is a “technical note” and not a comprehensive review, it is understandable that it derives examples primarily from the authors’ study sites. However, the focus on Australia and New Zealand leaves out possible lessons some recent examples of comprehensive (“GOaL”) studies of beach-ridge plains; see, for example, Clemmensen & Nielsen, 2010 [Sedimentary Geology 223:281-290], Hede et al 2015 [The Holocene 25:1402-1414], and Nooren et al 2017 (Earth Surface Dynamics Discussions). That said, the authors do present a very recent case study from Portugal (Costas et al. 2016) of a sea-level reconstruction using this “hat trick” of approaches which they deem to be of highest quality (I agree that is a very robust study).

General Revision #4: The authors present GOaL studies of storm records and sediment supply from Oliver et al (2017a,b), from which Dougherty et al. draw inferences about missed opportunities to use some of the best practices described. Included in this is a comment (P12, L19-21) that ground-truthing with sediment cores would have improved interpretations, unintentionally bolstering my suggestion for a “grand slam” approach! While Dougherty et al make a strong case for additional data that could have been collected, analyses that could have been done, and several alternative (although not necessary superior) interpretations, I suggest a more measured approach that recognizes the creative approaches and robust data presented by Oliver et al. Here I elaborate further, reflecting the “interactive discussion” nature of the “Climates of the Past” review process.

The authors argue that the records from the barriers presented by Oliver et al (2017a,b) are incomplete. There is absolutely a case to be made for record (storms, sea-level change, etc) reproducibility within a single plain. Ideally any such record would integrate two or more parallel lines of radar, topographic, and chronological data extending across the entire plains; indeed this was a strength of the Costas et al (2016) study. However, the reality of field studies is a limitation of these studies. For example, rarely are large progradational plains preserved without later erosion or reworking by wind or water. Whereas a tidal creek has eroded/modified part of the Boydtown barrier (Dougherty et al, P13, L4-5) studied by Oliver et al 2017 (Geomorph), imagery published in that paper suggests that at least parts of every beach ridge are preserved (although, we all must recognize the possibility of erosion of one or more complete ridges entirely alongshore soon after deposition); Oliver et al appear to have sampled

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nearly every ridge across that plain. However, this identifies a second nearly ubiquitous issue with the study of progradational systems: although “natural” (undeveloped) plains best preserve natural topography, that same topography (and associated vegetation) can negatively impact the quality of GPR data, or render sections of the study area inaccessible. The latter appears to have impacted the studies of both Wonboyn and Boydtown. Nonetheless, although Dougherty et al make the important point that reproducibility (multiple, parallel plain-wide reconstructions) would surely have strengthened that record, the resulting storm record is not invalidated, as Dougherty et al. seem to suggest.

In this same vein, the Dougherty et al. (section 3.3) rightfully note that the integration of multiple, parallel, cross-shore topographic profiles from Seven Mile Barrier would have improved, and possibly altered, interpretations by Oliver et al 2017 (MarGeo). However, even from the data presented by Dougherty et al., the interpretation regarding the sediment fluxes between 6700 and 3600 years ago (P14, L12-14) would only subtly change: progradation/deposition perhaps did not “pause” or “halt” as interpreted by Oliver et al, but most certainly slowed quite significantly. Indeed, the vertical growth of foredune ridges can be interpreted as a slowing of progradation, allowing more time for sand to be transferred into beach-adjacent ridges (see, e.g., Nooren et al 2017, cited above). Likewise, for the modern system: Dougherty et al identify a substantially higher modern foredune ridge, which in the west of the plain appears to be transgressive in origin. This too suggests a decrease in sediment flux, or at minimum, a sediment flux unable to keep pace with accommodation creation (sea-level rise). The implication of this observation for future growth of this plain (P.14, L21-22) differs from the more conservative observation by Oliver et al., but perhaps not as substantially as suggested.

Thus, Dougherty et al identify some site-specific issues with studies by Oliver et al (2017a,b), which may have led to mis-/over-interpretation of data. Most importantly, Dougherty et al identify several transferrable lessons for the use of the GOaL techniques. However, these findings seem to impact the subtleties of the Oliver et al

(2017a,b) interpretations; their overall conclusions about system evolution or large-scale records contained within appear to remain robust. Oliver et al (2017a,b) push the envelope with creative uses of progradational records, and did well to integrate modern and Holocene geomorphic data and use the GOaL datasets creatively to investigate sediment fluxes and storms. Thus, while Dougherty et al make some excellent and worthy points, they might consider a more measured treatment of these two published studies, and focusing this section of the manuscript on the lessons for best practices.

Specific Comments

P6, L9 & Figure 2: this is focused on the utility of LiDAR, not the details of this study. However, it is not clear that the multiple sets of "prograded barrier islands" shown here were never a single island / beach-ridge plain. This is a great example of possible reworking of a non-continuous record, a limitation in reconstructing the evolutionary history of a progradational site, or the paleoenvironmental records contained within. This in fact may be a case for the use of subsurface data (GPR) to search for, e.g., landward-dipping beds at the landward side of each of those "islands" to try to infer if they formed as separate transgressive-regressive islands. LiDAR data here may in fact tell an incomplete story.

P8, L8-9: this statement would be stronger if supported with examples or details of how beachface mapping can be used to infer sea level, etc.

P8, L14: suggest being more specific. What about the change in reflection geometry indicates storms?

P8, L19-26: If the authors are going to discuss these aspects of GPR processing and interpretation (including the necessity of ground-truthing, as discussed earlier), then it is also worth noting some additional key processing steps for proper GPR interpretation. For example, migration, ideally using field common-midpoint (CMP) surveys, to determine radar velocities.

P10, L10: “calculations from the LiDAR data”. It may be worth noting that this would not have been possible without ample stratigraphic data from sediment cores, especially given the limited GPR penetration.

P12, L15: “gain control is high in the GPR data”. That could just be the way in which the GPR data are shown in published form; that can be a challenge to get right. Presumably the GPR data were analyzed in high detail, and through careful tuning of gains to ensure best analysis resolution. Only those authors can speak to this. This same interpretation could be applied to criticisms of Oliver et al (2017a) noted on P14, L7-8 (LiDAR color scheme chosen for publication display).

Technical Corrections

P2, L20-21: the point concerning collaboration between scientists with specific expertise in each of the tools (some of which [e.g., GPR or pre-processed LiDAR data] are perhaps easy to use, but not easy to use well!) described in an important one.

Figure 5 caption: “prograded normally for a while”. This is unclear, unspecific, and qualitative. (the term “normally” is applied on P11, L7 as well, and does not seem to necessarily indicate “normal [sediment surplus driven] progradation”. Correct?). “drastic shift in evolution observed . . .” this is not clear from the data presented, nor is it clear what would qualify as a “drastic” change

P11, L15: Costas et al 2016 is listed twice

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