Manuscript Review

Title: Technical note: Optimizing the utility of combined GPR, OSL, and LiDAR (GOaL) to extract paleoenvironmental records and decipher shoreline evolution
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General Comments

This is a re-review of a manuscript reviewed originally in March 2018. Please note that all page and line numbers given here are in reference to the version of the manuscript presented with changes tracked.

The authors have done a very nice job of responding to comments and concerns, and updating the manuscript in accordance with public reviews and comments. In particular, the authors' discussion around my first general edit ("consider the addition of ground-truthing as a fourth approach") was well thought-out, and the edits to the manuscript appropriately address this. I thank the authors for their thoughtful consideration of reviewer critiques and hope that my input was of use as they improved this manuscript.

My only response comment is to the final sentence of the Author Response to General Revision #2. The authors make a strong case here for the utility of OSL age dating (and a sub-set of the authors contributed to what I find to be a very compelling paper demonstrating the potential advantage of OSL dating over radiocarbon dating in Oliver et al., 2015 [Holocene]); however, I would be very hesitant to discount the use of radiocarbon dating of organic matter in all cases, even when a setting lends itself to determination of deposition age estimates using OSL. Radiocarbon remains a scientific standard and can be accomplished on very small (<20 ug) samples, providing the provenance of the organic matter is well constrained. Commercially, 3-4 radiocarbon samples can be analyzed for the price of one OSL sample, and in a matter of weeks rather than 6-12 months. Each approach has its place, and the authors may consider recognizing this better in the manuscript (this is done somewhat on P5, L21-23).

I find the manuscript much improved and, though we may still quibble on some points, certainly suitable for publication. The authors have done a particularly good job in:

1) clarifying the statement of purpose of the manuscript (P5, L2-6).

2) describing their treatment of each of the methods and clarifying the reasoning behind presenting a (relatively) low level of detail on each the technicalities of each underlying approach and on the methods for applying and analyzing associated data.

3) presenting clear best practices for use of each of the GoAL methods independently and in conjunction for these specific types of settings and studies

4) presenting a much improved Fig 7 with a clearer purpose and very nice demonstration, using their own data, of how proper use of gain (and I note again, with

ground-truthing) can improve analysis of GPR data (as opposed to simply pointing out perceived faults in the Oliver et al 2017 [Geomorphology] paper).

5) presenting a much more measured treatment of the Oliver et al 2017 [Geomorphology] and Oliver et al 2017 [Marine Geology] manuscripts. In particular, the authors benefitted from the publication of several Discussion articles in the months during which this manuscript was in review and revision, which allowed the authors to focus their discussion of these two prior studies on lessons for best practices in data collection and utilization. This was one of my primary critiques in the original manuscript and the authors have handled it very nicely.

My remaining suggestions are all very minor:

1) The manuscript would benefit from some additional detailed copy editing to correct issues such as (but not limited to):
(a) use of data as singular (perhaps the plurality of the word “data” is a personal preference rather than grammatical rule?) (e.g., P2, L30);
(b) missing closed parentheses (P3, L7)
(c) hyphenation of “three-step” (P4, L11)change “no” to “not” (P5, L22)
(d) comma missing after “used” (P5, L33)
(e) change “coasts” to “coastal” (P13, L21)
(f) consider not capitalizing “penetrating” and “radar” (P15, L7)
(g) “rendering of the LiDAR as well” (P26, L8) and “no GPR was” (P26, L27): consider adding “data were” after both “LiDAR” and “GPR”
(h) add comma after “how” (P26, L33)
(i) incomplete sentence in P27, L18-19.

2) Several suggested wording changes for clarity:
(a) change “coastal plains” (a geomorphic/geographic feature) to “coastal settings” (P5, L28)
(b) “there has been relatively little discussion about gain” (P15, L33) – where has there been little discussion? In the literature? Needs clarity.
(c) sentence structure in P17, L15-17 is very confusing
(d) “deep” coring (P18, L1): needs to be defined. Is this word even necessary?
(e) not sure “decipher” is the best word choice (P22, L18); consider “differentiate”
(f) consider adding something along the lines of “, therefore leading to incorrect or incomplete interpretations” after “overlooked” (P27, L1)
(g) Use of “evolution” is somewhat unclear (P27, L11).

3) P6, L123-14: the authors may consider a few sentences about other, newer methods/technologies for mapping morphology. I am specifically thinking about drone-based “structure from motion” (sfm). Unlike the earlier author-reviewer discussion we shared about the need for ground-truthing, this is not a significant consideration. However, it may be worth the authors mentioning the existence of other techniques

which could produce lidar-like and lidar-quality data across similarly large areas. Lidar data are still not available for large parts of the world. While, as the authors note, lidar benefits from penetrating vegetation which may obscure morphology (P6, L23-24), there are many coastal settings (e.g., coarse-grained and/or high-latitude beach ridges where vegetation is minimal or non-existent) where progradational beach- and foredune- ridge plains could be easily (at less expense and simplified logistics) mapped using, e.g., sfm. The point is that lidar is one tool of several for high-resolution, large-spatial extent topographic mapping, and data produced by another method may be equally valid for use in a GOaL-like approach.

4) P21, L24-25 and P27, L21-22: I would be remiss if I did not highlight the work in a very challenging environment to produce a sea-level curve using OSL dating and GPR, as well as detailed topographic mapping (using RTK-GPS as opposed to lidar) by my colleague Dr. Billy (Billy et al., 2015, Geomorphology). This paper is already cited in this manuscript, so this is not fishing for a citation, but rather highlighting a recent study that successfully used the exact approach proposed by the authors.

5) P17, L22-26: I recognize this addition is in response to another reviewer. I agree with this and have struggled with it myself, especially when presenting both radiocarbon (in years B.P.) and OSL dates in the same discussion. The difference matters little when discussing dates of >10,000 years, but can matter a lot for the last 1000 years. For those shorter time periods, simply converting the radiocarbon ages and presenting all dates in years CE is one solution my reviewers have agreed on.