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Interactive comment on "Reviews and syntheses: ^{210}Pb-derived sediment and carbon accumulation rates in vegetated coastal ecosystems: setting the record straight" *by* Ariane Arias-Ortiz et al.

Anonymous Referee #3

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General comments Overall, a very valuable contribution to the literature. This is a helpful synthesis of the literature that will be a go-to for those in the field, and it is also an interesting modeling exercise that sheds light on the processes producing various 210Pb patterns. My main concern is that the manuscript provides an overly optimistic view of the errors associated with complex 210Pb profiles, for reasons explained below.

Specific comments As the authors note in Table 4, patterns II, III, and IV can have multiple causes. Especially common, and especially problematic, is the difficulty in distinguishing between mixing and an increase in MAR. The simulation studies in this paper don't address this adequately because they separate the mixing simulations from

the increased sedimentation simulations. For the mixing simulations, for example, "the CF:CS model was applied below the depth of the visually apparent SML (3 cm) in scenarios A and B to avoid overestimation of MAR" (Appendix). But if you didn't know this profile was created by mixing, how would you know that you would be overestimating MAR rather than accurately estimating an increase in MAR? In other words, in the real world, how would you know whether it was mixing (so leave out the SML) or increased MAR? True, the mixing and increased sedimentation profiles in Figure 3 do look somewhat different, but I am not convinced that in the real world they are so easily distinguished. Bottom line: I am concerned that if the authors tested the error in non-ideal profiles without knowing what caused them, they would find higher errors than those shown in Figure 5.

Related to the above: the authors choose not to create a CRS estimate for the profiles with erosion. That is fine as long as one knows that erosion is a factor. In the real world, minor deviations from the ideal inventory (especially the small ones shown in the tidal marsh half of Figure3c) do not generally preclude investigators from applying the CRS method. I would strongly encourage the authors to apply CRS to these profiles to get a sense of how large the associated errors are. At a minimum, they should caution others not to use the CRS method with profiles that show deviations from the expected inventory.

The authors use their results to suggest in Figure 5 and Table 4 that pretty much any 210Pb profile is date-able (except those with extreme OM concentrations). However, in the real world, some profiles are likely to be altered in more complex ways than the simulations shown here – by mixing and erosion and different grain sizes. I believe that some profiles may just be too altered to be retrievable, and would suggest using extreme caution in interpreting Types V, VI, and VII. Section 4 of the paper is very helpful in suggesting alternative approaches that can help disentangle various factors, but it is in tension with Figure 5 (and the abstract), which suggest that those are not necessary, since maximum error is only 20% anyway.

It would be helpful if the Supplementary Tables in Excel had formulas rather than just values, to make it easier to understand how the simulations were done.

I think the authors could emphasize more strongly that they are looking at the 100-year average MAR and Corg-MAR, not the patterns over time. For example, the y-axis in Figure 5 (or at least the figure caption) could say “100-year Corg burial.”

Does this analysis only apply to Corg burial? There will be an audience interested in the equivalent of Figure 5 for the MAR itself, which presumably would be easy to make.

Table 4 is too long and repetitive; there must be a way to condense it, since the options for each outcome are the same.

I found the boxes helpful, except for Box 4, which is different from the others and not necessary in my opinion.

I understand the logic of including the methods in an appendix – mostly because they are quite long and detailed. But it is important for the reader to understand what the authors are doing. The authors might consider including in the methods a more detailed description than what is there now (but still less detailed than in the appendix).

Section 2.1 doesn’t seem like it should be in the methods.

The authors mention a literature review several times, but the only detail is provided on p. 4 line 27ff. in establishing that CIC, CRS, and CFCS are the most commonly used approaches. Is this the same literature review that was used to construct Figure 2? Please clarify. Also, they probably missed some of the literature by not including the term Pb-210, which is sometimes used instead of 210Pb. (There are almost certainly more than 150 uses of 210Pb in the salt marsh literature.)

The reason for excluding the CIC method – the absence of ideal profiles – is not persuasive as currently expressed. The other methods also suffer when there are deviations from the ideal profile, which is exactly what the authors explore. Perhaps more of a justification for excluding CIC could be given?

I’m not sure the distinction between Types VI and VII is necessary. They are both characterized by low inventories, regardless of profile shape.

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