April 1, 2023

Dear Marine and Coastal Fisheries Editorial Team:

We have extensively revised the manuscript "Collapsed oyster populations in large Florida estuaries appear resistant to restoration using traditional cultching methods — insights from ongoing efforts in multiple systems" following guidance from the Editor, AE, and two reviewers. Below we provide the original comment in italics and follow with our response.

*The reviews suggest that, subject to major revisions, your paper could be suitable for publication. All of the reviewers, including the AE and myself, commented that the text was overly long, sometimes confusing, and "preachy" in some cases. All of these lead to the key messages that are supported by the data being lost. All also though the manuscript addresses a very important topic and has important things to say.*

*Therefore, I invite you to respond to the reviewer(s)' comments and revise your manuscript. The revisions should address the comments and also involve a major rewrite and tightening.*

Thank you for the supportive comments. With the assistance of a professional copy editor, we have extensively revised the manuscript to better focus on the key results, improve clarity, and reduce “preachy-ness” of the manuscript. We also eliminated one analyses within the paper that simplifies the paper through a reduction in text and tables.

*Reviewer: 1*

*Comments to the Author*

*The work conducted here is an important contribution to the literature. In my review, I see three substantial issues.*

*1. As written, the manuscript is written with a great deal of jargon and coding and this obscures the work and the important findings. For example, for the sake of the modeling, time was divided into periods which seems appropriate. But then the authors keep that convention in the text and the figures, forcing a reader to decode odd and even numbers and do some math to figure out what year each period is in. Is there any reason that this can't be done?*

We appreciate the positive response to our manuscript. In revisions we have worked to reduce the jargon of the manuscript and simplify the language. For example, throughout we have revised the units of the assessment of oyster counts and cultch mass from a per quadrat framework to a per ¼ m2 (the size of the quadrat). We have also provided the conversion to the m2 unit requested by the reviewer where appropriate. We do caution on expansion of the mean values of these estimates because of the high dispersion and frequency of zeros. We have kept the analyses in terms of time periods because this provides analytical improvements by allowing for multiple samples to be considered in the same time step. This also allows for simple graphical comparisons of observed, standardized oyster spat counts on different substrate at the same time window. We have added additional information throughout to better identify the Period by year. This is explained in the figure captions and is described in detail including which months and samples are in each Period in Appendix 1. We attempted to add a second y axis with these labels, but were not successful in making this visually appealing.

*2. There is surprisingly little reference to work done in other areas. For example, there is virtually no mention of the work done in Mobile Bay by researchers based at the Dauphin Island Sea Lab specifically about reef height, location, etc. There is mention of work in Chesapeake Bay but the context for this work could be more inclusive of the substantial work done in oyster restoration.*

We have expanded references throughout the paper. However, if the reviewer is aware of specific articles from work in Mobile Bay by DISL staff, please provide the reference. We are aware of oyster restoration and management efforts in Alabama and are in regular communication with Alabama Marine Resources staff. We have also cited their ongoing restoration efforts in this manuscript.

*3. Inadequate attention is paid to alternative explanations for the observed data beyond issues with the cultching. For example, the question of larval supply is skimmed over in lines 640-644. Will putting cultch down work if we change how we do it, or have there been changes that may mean that putting cultch down may not work as a single solution? As noted, large sums of money and effort are put into these projects and it's not clear that these projects are working. (Lines 595-597 caught my attention.)*

All of these restoration projects are a test of a single hypothesis—that oyster populations have declined because of insufficient cultch. In the revised manuscript (lines 393-398) we begin the discussion by pointing out this simple fact and the follow this in subsequent paragraphs (lines 409-440) with discussions of alternative possible explanations. We have also provided refences to detailed assessments of the oyster population collapse observed in Apalachicola Bay in 2012 (line 409-415). We highlight that what we are assessing in this paper is response to restoration in three different estuaries (lines 417-419) which reduces the likelihood of bay-specific abiotic or biotic conditions limiting restoration success unless the limiting factor is an unknown regional limitation.

A recent newspaper series documented similar lack of response to restoration using similar methods in Mississippi waters

([https://www.propublica.org/article/mississippi-spent-millions-failed-to-save-oyster-reefs](https://nam10.safelinks.protection.outlook.com/?url=https%3A%2F%2Fwww.propublica.org%2Farticle%2Fmississippi-spent-millions-failed-to-save-oyster-reefs&data=05%7C01%7Cbillpine%40ufl.edu%7C3cfab13b6b944755001508db2e47e802%7C0d4da0f84a314d76ace60a62331e1b84%7C0%7C0%7C638154658661317625%7CUnknown%7CTWFpbGZsb3d8eyJWIjoiMC4wLjAwMDAiLCJQIjoiV2luMzIiLCJBTiI6Ik1haWwiLCJXVCI6Mn0%3D%7C3000%7C%7C%7C&sdata=Vp45PlhCTKNU4Lw46oGc2Um%2FAVNl7iB8khODMwtay1U%3D&reserved=0))

*lines 37-39 Awkward phrasing*

Revised as requested.

*Line 58 and throughout: 'Bays' should be capitalized, I believe, as those are all proper names of bays (Pensacola Bay, St. Andrew Bay, etc.)*

We are using the correct capitalization when referring to multiple bays based on the journal style manual and other publishing standards.

*Line 111. Should 'at' be 'and'?*

Revised as suggested.

*Line 125. What number of quadrants per site? Minimum number? Range?*

This information is included in the Supplemental Files, Appendix 1.

*Line 146: 'Staring' should be 'starting'*

Thank you, corrected.

*Line 281: over-dispersed was used as a term earlier but explained here. Might move that section in parentheses to the first mention of over-dispersed*

Revised as suggested.

*Line 404: Suggest 'intended' (or 'hoped') instead of 'designed'*

Revised as suggested.

*Acknowledgements need updated project numbers for the current placeholders 'YYYY-YYY'*

Revised as suggested.

We appreciate the detailed review by Reviewer 1.

*Reviewer: 2*

*Comments to the Author*

*Overall, I think that this is a very important study and one that should be reviewed and considered both by funding agencies and restoration practitioners. As is always the case with large scale environmental restoration efforts, it would be ideal for there to be more data available on project performance and environmental (both physical and biotic) conditions that can help explain the patterns observed. However, investments in these data are rare at the scale/level of resolution needed to be valuable. So, I think the authors are doing their best with a limited data set. At times, I think that the study verges on preaching to the restoration community and perhaps criticizing them with a tone that may not be conducive to provoking the changes that the authors recommend. Thus, I suggest a strong review of such statements and consideration of reframing them to be less confrontational and more, directly forward-looking. I am not an expert in the statistics so was not able to comment on their validity. However, I also did not see any particularly glaring issues with how the data were analyzed. I am favorable towards the study being published if the revisions suggested below are made.*

Thank you for the encouraging comments.

*Suggested revisions*

*L 19: shortcomings not short comings*

We have corrected.

*L36: some of these citations seem improperly formatted*

We have corrected.

*L 41: What is meant by a ‘low but apparently resilient state’ - resilient to evolving back into a more robust, living reef?*

We have removed this term.

*L 44: I this this phrasing is statement is not correct - “all restorations are expected to persistently increase… “. It is my perception that investments in restoration are made with the goal of locally enhancing oyster populations. But, there is not necessary the expectation that all project expect persistent increases in population size.*

We have revised this sentence (lines 35-38) to state “…many restoration efforts attempt to…”

*Research Questions:*

*It is unclear in Q1 whether these are oyster counts in the bays in general or on the restoration project specifically.*

We have clarified in the methods that all data are from restoration sites (lines 122-123).

*Q2 and 3: why only Apalachicola was investigated for these questions is unclear. I encourage you to provide some justification to support understanding by the reader.*

We have clarified that Apalachicola is the only system where different projects within one bay can be compared over time because none of the other systems have data available from multiple projects. We also clarified that Apalachicola Bay is the only estuary with regulated river inputs thus the only system where freshwater input can be modified via management (lines 103-118).

*Q3: In the framing of the paper in the earlier parts of the introduction, there is not really any motivation for understanding project to project differences… so this question comes out of nowhere.*

We have explained the reason for assessing freshwater inputs in revision (lines 103-118).

*Is there some reason to expect the projects to differ based on their design, placement, timing of deployment?*

Yes, the different projects use different materials. The different timing of deployment would potentially represent different abiotic and biotic conditions in the bays when the cultch material is placed.

*L72: the redundant use of leadership and leaders is a bit clunky*

This has been removed in revision.

*L73: There seems to be some underlying blame being suggested in this statement to those conducting restoration that they are not learning from prior projects. I think that this phrasing may act to repel such entities from wanting to read/engage with this work so suggest some rephrasing here.*

This has been removed in revision.

*L84-85: is there at lease a personal communication to cite about this statement?*

This information is included in Figure 2 and a reference to that figure has been added.

*L287: were none of the months or years significantly higher than average?*

This information is included in Figure 4.

*L.311-312: can you scale these estimates of spat per quadrat to be the number per meter square so they are more meaningful/interpretable?*

The units have been revised throughout to change the reference from a per quadrat unit to per ¼ m2. Scaling to m2 is also presented, and we have added a note to caution the user in expansion of reported estimates to larger spatial units given the overdispersion in the observed counts at the ¼ m2scale. Lines 305-313 as an example.

*Ln 335: I do not know what the SP term is*

This has been removed and clarified in revision. This is just the term for the random effect.

*Ln 350-358: What is the justification for using these river discharge values and the single period lags? Is there a connection between these discharge values and the salinity observed in Apalachicola? Is there a connection between this lag period and the ecology of oysters? I think these details would be helpful in the methods.*

This is discussed in detail lines 245-261.

*Ln 362: Isn’t this statement written incorrectly - it seems that the driver is predicted to be cultch biomass and response live spat, so I recommend reorganizing the statement.*

Thank you, this has been revised.

*Lb 403: Multiple use of ‘designed’ in this sentence is a clunky*

Thank you, this has been revised.

*Discussion;*

*Overall I appreciate the interpretation of the data in the discussion and find the conclusions drawn to be fair and appropriate based on the study’s findings and the literature. The one topic that the authors do not give much/any attention to is whether the broader food webs in these bays are now no longer conducive to supporting spat survivorship (i.e there are too many predators of spat that suppress their success) - this is just very briefly alluded to in the Future Directions section.*

Thank you for the encouraging comments. In revision we have expanded on “other topics” (lines 409-415; 416-440)

*Editor's Comments to Author:*

*Associate Editor*

*Comments to Author:*

*The manuscript reports on important findings of major interest. Nevertheless, I find the manuscript as submitted almost bafflingly over-long, repetitive, and difficult to follow. Presentations of data and statistics are difficult to interpret. There is a simple message in this paper: deploying cultch in these estuaries did not stimulate the restoration of oyster populations. It is not clear to me why this message should require nearly 80 pages.*

We have worked with a professional copy editor to revise the manuscript to focus the writing and to highlight the key messages. We have removed the formal analyses assessing relationships between live oyster spat and cultch mass. This was a complicated analyses which simplifies the message while providing the same information by simply referring the reader to plots of the data. Overall, the AE provides limited detail to address their concerns. We will discuss with the Editor.

*The discussion of reasons for failure barely acknowledges the most likely causes: predation on spat and early juvenile oysters, and possibly disease, especially Dermo. For both of these factors, salinity regimes are crucial, yet no salinity data are offered, only the proxy variable river discharge, which is far less informative. How could there be no salinity data available for these sites?*

In revision we have highlighted how continuous salinity data are not available in two of the three bays in this study and, in the one bay where it is available, it is not available to the restoration sites (lines 110-114). This lack of region-wide water quality monitoring has been highlighted in a previous paper (Dale et al. 2021). Apalachicola River discharge as a proxy for salinity, nutrients, and other factors and we have provided references to key documents that synthesize this information and recent debates (Kelly 2019 for example).

We appreciate the AE proposing additional factors including predation and disease. There is no ongoing oyster disease monitoring program in these systems. There is not a long-term monitoring program for oyster predators, and the role of oyster predators on the Apalachicola Bay oyster collapse in 2012 (and other factors) is widely debated as we identify in lines 409-415 in revision.

*The term "cultch biomass" is used throughout to express the amount of cultch material deployed. The term should be just "mass." Biomass on cultch, including oysters and all else, would be interesting, but is not mentioned.*

We have revised to only refer to this as cultch mass.

*Reef restoration, even without oysters, can be ecologically beneficial. I am also curious why some effort was not made to quantify cultch in terms of surface area rather than mass. It would be a more relevant variable, and computable from mass with some simple algorithms.*

We are unaware of these algorithms for irregular objects. We only have mass data available for analyses and introducing the uncertainty around a conversion to area would further complicate analyses.

Thank you for the opportunity to revise this manuscript. We look forward to publishing the paper with MCF soon.

Regards,

Bill Pine

For co-authors.