FYI, not too bad. I'll wait a few days and see if Kenny Rose will have a conversation about the paper to answer some questions I have before I start the revisions.

bp

-----Original Message-----

From: Kenneth Rose <[onbehalfof@manuscriptcentral.com](mailto:onbehalfof@manuscriptcentral.com)>

Sent: Thursday, February 2, 2023 3:13 PM

To: Pine, Bill <[billpine@ufl.edu](mailto:billpine@ufl.edu)>

Subject: Marine and Coastal Fisheries - Decision on Manuscript ID UMCF-2022-0059

[External Email]

02-Feb-2023

Dear Dr. Pine:

Your manuscript entitled "Collapsed oyster populations in large Florida estuaries appear resistant to restoration using traditional cultching methods — insights from ongoing efforts in multiple systems", which you submitted to Marine and Coastal Fisheries, has been reviewed. Two reviewers, the AE, and myself examined the manuscript. The reviewer comments are included at the bottom of this letter.

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.

To start the revision, please click on the link below:

\*\*\* PLEASE NOTE: This is a two-step process. After clicking on the link, you will be directed to a webpage to confirm. \*\*\*

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This will take you directly to the section of the site where you can submit your revision. Please (1) update the information provided as needed, (2) upload the file(s) containing your revised manuscript, and (3) upload a separate file containing detailed, point-by-point responses to the reviewers’ and editors’ comments. Using the drop-down menu, designate the manuscript files according to type (“Title Page,” “Main Document,” and so forth); designate the file containing your responses as “Response to Decision Letter.”

This link will remain active until you have submitted your revised manuscript. If you begin a revision and intend to finish it at a later time, please note that your draft will appear in the “Revised Manuscripts in Draft” queue in your Author Center.

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Because we are trying to facilitate timely publication of manuscripts submitted to Marine and Coastal Fisheries, your revised manuscript should be uploaded by 04-Mar-2023. If it is not possible for you to submit your revision by this date, we may have to consider your paper as a new submission.

Once again, thank you for submitting your manuscript to Marine and Coastal Fisheries and I look forward to receiving your revision.

Sincerely,

Kenneth Rose

Editor, Marine and Coastal Fisheries

[krose@umces.edu](mailto:krose@umces.edu)

Reviewer(s)' Comments to Author:

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?

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.

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.)

There are numerous minor suggested edits.

lines 37-39 Awkward phrasing

Line 58 and throughout: 'Bays' should be capitalized, I believe, as those are all proper names of bays (Pensacola Bay, St. Andrew Bay, etc.) Line 111. Should 'at' be 'and'?

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

Line 146: 'Staring' should be 'starting'

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

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

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.

Suggested revisions

L 19: shortcomings not short comings

L36: some of these citations seem improperly formatted L 41: What is meant by a ‘low but apparently resilient state’ - resilient to evolving back into a more robust, living reef?

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.

Research Questions:

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

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.

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

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

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.

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

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

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?

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

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.

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.

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

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.

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.

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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.

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?

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. 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.