Review of Belleza et al. for *PeerJ*

This paper compares environmental conditions and the densities ad behaviours of sympatric species of urchins between two spatially adjacent sites in Japan with different environmental and benthic conditions where the differences in benthic characteristics mimic two separate phases of kelp bed-urchin barren phase shifts. The authors assess environmental parameters between the two sites and observe differences in densities and movement and sheltering behaviour of two urchin species at each site. The authors provide inferences of why behaviour differs and what drives the environmental differences between the sites. Overall, I very much liked this manuscript. It was very well written, and I thoroughly enjoyed reading it. I thought the authors did a nice job designing the experiments and all data interpretations were reasonable. The data are useful, interesting, and certainly worthy of publication.

Nonetheless, I do have a few comments for the authors to consider. Notably, I thought that the Introduction placed a large emphasis on ecological phase shifts, but the Discussion didn't really bring the results of the experiments back into the light of ecological phase shifts in a meaningful way. Here, I suggest either adding Discussion on ecological phase shifts (I provide an example on how to do this in the specific comments below; see my comment on Line 342), or focus the Introduction more on the impacts oof human activity on habitat disturbance and effects on localized communities. Additionally, I thought that some aspects o the methodology could benefit from increased detail and transparency, and I provide some suggestions for further considerations in the Discussion.

Based on the number of comments and their content, I would call this a moderate revision, though I want to be clear that I do think this paper is worth publishing. If the authors have any questions or concerns regarding any aspect of my review, they should feel free to contact me directly at jeffery.clements@dfo-mpo.gc.ca

Specific comments

1. Lines 25-26: The English grammar could be improved here. For example, this should read "The benthic rugosity at both sites was also surveyed." While the paper is quite well written throughout, there are some other examples of these instances throughout the text. I would suggest having a native English-speaking scientist familiar with the subject matter review the manuscript for English language and grammar if possible.

2. Line 31: The word "experiences" inn this sentence should read "experienced", so that the manuscript is always written in the past tense. An example of where a native English-speaking proofreaders would be valuable.

3. Lines 36-39. This is a very long sentence. I suggest breaking it into two sentences.

4. Lines 40-41: I think explicitly indicating the size is worthwhile here since the reader has no context for what a small, medium, or large urchin is. Also see my comments re. size classes in the Methods.

5. Line 43: The word "shows" should read "showed" as per my comment on past tense above. Please be sure to check the entire manuscript carefully for consistency.

6. Lines 56-57: This sentence is quite vague and incomplete. What anthropogenic activity. The impact of anthropogenic activity on what? Ecosystems do more than "lessen the impact of anthropogenic activity. I would consider removing this sentence and starting the paragraph with something like "The resilience of natural ecosystems to natural and anthropogenic stressors is limited and..."

7. Lines 88-90: And, under certain circumstances, they can even actively predate on animals that are widely considered to be predators of the urchins themselves (at least in the lab; a neat example of predator-prey role reversal!)

   Clements et al. 2021. Ethology 127: 484-489.

8. Lines 102-103: It is not clear what specific "behaviour" the authors are referring to here. Feeding behaviour, perhaps? Some clarity is needed.

9. Line 125: Perhaps I am wrong here, but I think the authors mean to say that "...a particular species may display different behavioral patterns to other co-occurring species...". The term 'conspecific' means "off the same species", and I think the authors meant to convey that different species may behave differently. Please check and amend if necessary.

10. Lines 128-130: Cool! Sounds like a near system to work in!

11. Line 134: Again, "hypothesize" should read "hypothesized" for past tense. Please check the manuscript carefully throughout.

12. Lines 141-142: Some aerial or underwater images of the two habitats would be nice so that the reader can visualize the sites. If that authors have any photos, I suggest including them in Fig 1 with the map.

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14. Line 187: Please provide some written rationale for why points labelled as "other" were excluded.

15. Lines 189-195: I'm a bit confused as to why a single logger was placed between the deep and shallow transects, rather than placing a logger at each of the transects. Please provide some rationale or explanation for this.

16. Line 200: Missing comma after "long-spined black urchin"

17. Lines 214-215: How many urchins were not recaptured? I.e., what was the recapture success rate?

18. Lines 232-236: I appreciate the detailed analysis the authors provide here - it is analytically strong - well done! One thing i would like to see is some rationale for th distribution choices. Why did you choose the particular distribution you did for each dependent variable? This is done for algae on Lines 237-240 below, but it is missing here.

19. Lines 254-256: Again, some rationale for the choice of priors here would be useful here and for the other subsequent analyses.

20. Lines 277-282: The size classes are never strictly defined in the text and really need to be, and no rationale for grouping sizes into these specific classes is provided. While this is done in Fig. 5, size ranges for each size class should be explicitly stated here in the Methods. Also, from the ranges given in Fig. 5, the cut-off between a small (1-3cm) and medium (4-5cm) is not clear; for example, what was an urchin with a width of 3.5cm classified as? It is also not clear why the size ranges of each size class are not standardized. For example, the small size class has a range of 1-3cm (2cm span), while the medium size class range is 4-5cm 9only a 1cm span), and the large size class has an infinite span (>5cm). It would be worthwhile showing the entire distribution of sizes from the urchins obtained so the reader can see the true variation in sizes for the urchins in this study. I would suggest doing this by species (does each species occupy the same total size range?). Finally, there needs to be some rationale for dividing the size classes as the authors do, and it needs to be recognized whether these classes were defined before or after data collection , else one runs the risk of categorizing sizes in a post hoc manner that results in bias when analyzing and interpreting the data.

21. Lines 290-310: the symbol "pi" is typically associated with a precise number (i.e., 3.14...). This may confuse some readers and I would suggest using a different symbol.

22. Line 342: Since environmental conditions differed between these two sites at the same time, it makes biological comparisons to other ecological phase shifts difficult, as these phase shifts occur in the same place at different times, which may or may not have similar environmental conditions. As such, it is hard to say whether differences in urchin behavior observed here between the two habitats ar driven by aspects of the benthic habitat itself (i.e., vegetation and rugosity, which would bee comparable to an ecological phase shift) or the different environmental conditions between the two sites (which likely differ due to the concrete barrier between the site, and which may or may not be comparable between phases of an ecological phase shift). The effect of these spatial differences in environmental conditions, their influence on the biotic parameters of urchins measured in this study, and their comparability/applicability to conditions at the same spatial location during temporally different ecological phases should be explicitly mentioned in the Discussion. This is particularly important given the focus on phase shifts in the Introduction.

23. Line 374: Why was "D. setosum expected to be the most common..."? I don't remember reading this as a specific hypothesis. Should this simply read "D. setosum was the most common...'

24. Lines 386-412: These results and their interpretation are precisely why more detail ad transparency are needed regarding the choice of size classes.

25. Line 416: Again, why was his "expected"? There ws no direct, logical hypothesis presented for this to be expected.

26. Line 438: "evaluate" should be "evaluated". A reminder to check the manuscript thoroughly for consistent use of past tense.

27. Line 440: Again, need to use past tense: "...were highly adaptable and behaved differently..."

28. Line 518: Or is the barren state controlled by environmental conditions (sedimentation)?

29. Lines 529-531: Happy to see this limitation mentioned based on my comments on biomass estimations in the Methods. Good to acknowledge this - nicely done!

30. Lines 535-537: A good suggestion also due to the inherent variability of wet weight measurements which ca be an inaccurate representation of actual biomass. Good stuff!

31. Lines 558-560: Could increased movement in the IH also perhaps be due to it being a calmer, lower energy environment?

32. Lines 572-574: Perhaps the vegetation itself in the VH provided some refuge from predators, which could explain the more varied group sizes in VH.

33. Discussion, general comment: One thing I was left wondering about was whether the concrete barrier could act as a vector for speciation in this system. There were clear behavioural differences between the IH and VH and the barrier has been up for 50 years– do the authors think this could be actual adaptation (i.e., evolution), or is this just phenotypic plasticity? Is there any evidence of restricted gene flow between the two sites, either from genetic studies or hydrodynamic larval dispersal modeling? If gene flow is restricted ad there are some genetic differences, this is a cool example of human intervention driving an evolution on very small spatial scales.

34. Lines 622-644: The discussion is already quite long, but it is very well written (well done!). There was nothing new here in the conclusions that I didn't already read and gather from the Discussion. I am therefore not convinced that this conclusions section is necessary.

35. Figures, general comment: Very nice figures!