

This is a well-written, clear, and organized manuscript describing a reciprocal transplant experiment to test for evidence of local adaptation in native oysters. The experimental design is adequate, and the discussion stays within the scope of results. The results have clear implications for restoration strategies and future work. I recommend this paper for publication in *Marine Ecology* after some relatively minor revisions.

### Specific comments

Line 38: I appreciate the use of cautious language “may” but as you know resource managers and restoration groups have already done a lot of work with this species. The general reader would be better informed by a quick description (and citation) of the restoration activities that have already been undertaken for this species, especially in the Puget Sound.

Line 52: I apologize for the self-reference but Carson 2010 presents *O. lurida* data showing two peaks in settlement data (with a concurrent constant rate of brooding percentage). Those data seem very relevant to this discussion. *Population connectivity of the Olympia Oyster in Southern California. Limnol. Oceanogr., 55(1), 2010, 134–148*

Line 89: On behalf of my geologic colleagues I suggest calling Hood Canal a “fjord-like arm” or something similar. Apparently it is neither a fjord geographically (carved mostly out of unconsolidated material, not bedrock) nor geologically (the mechanism of carving was meltwater, not ice, making it a “tunnel valley”).

Line 105: It would be useful to describe the Clam Bay site a little more, perhaps in the preceding paragraph. Is it a “control” site only because it is not any of the three source sites, or for some other reason? Would you expect it to have characteristics similar to any of the three source sites that would make it less of a control?

Lines 111 – 116: More justification is needed here to support this choice. Was this because you feared survival would be very low at all sites if the oysters overwintered in place? It seems like “natural” overwintering conditions might be a part of local adaptation so it would be useful to know the strategy here.

Line 211: A placeholder (XY%,  $\pm$  YZ SD) was not filled in with numbers here.

Lines 231 – 238: It would be useful to include some of the actual percentages brooding here instead of just who was higher where.

Line 274: It’s unclear what is meant by “reproductively discrete”. It doesn’t seem like the data exist to judge degree of isolation, if that is what is meant. Define or remove.

Line 277: Typo “indicator OF local adaptation”

Line 285: I think you mean that the site had temperatures outside of Baker's thermal tolerance range for the species, but as written it implies that Baker measured temperatures at this site and that that range was exceeded.

Lines 282-299: This would be a good place to mention the experimental design on floating structures as opposed to "natural" oyster populations exposed to low temperatures at low tide, and how that might influence the results.

Lines 326-328: This statement needs a reference to either published literature or your own data/figures. It also should be expanded to make clear what you mean. Is the Chl-A increased overall in the south? Or does an increase come earlier?

Lines 350-352: Can you cite something here, perhaps from Mr. Blake's data or reports, regarding the distribution of oyster drills in Puget Sound?

Line 352: The shell thickness hypothesis seems like it would have been relatively easy to test after your experiment was concluded...

Line 391-398: In lines 67 – 70 you introduce two alternative hypotheses regarding false positives for local adaptation. I think it would be a good idea to make a clearer link between your language in this paragraph and those hypotheses.

Lines 417-524: This would be a good place to make some recommendations to restoration practitioners. Given your 3 concerns, what experiments or genetic tests might organizations ideally do before selecting broodstock or transplanting animals?

Let me know if you have any questions

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