

Reviews

Editor (Joseph Pawlik)

Dear Juan Carlos,

Thank you for your submission to PeerJ.

It is my opinion as the Academic Editor for your article - Spatial variation in allometric growth of invasive lionfish has management implications - that it requires a number of Major Revisions.

My suggested changes and reviewer comments are shown below and on your article 'Overview' screen.

If you address these changes and resubmit, there's a good chance your article will be accepted (although this isn't guaranteed).

Both reviewers noted a need to consider different or additional analyses and to improve the writing of this contribution. Along with your revised ms., please include the text of both reviews in you point-by-point responses to the reviewers' comments.

Although not a hard deadline, we expect you to submit your revision within the next 55 days (received Dec 18).

With kind regards,

Joseph Pawlik

Academic Editor, PeerJ

Reviewer 1 (Anonymous)

Basic reporting

The word choice needs to be consistent throughout the paper when referring to various items (see below). The word "major" is a subjective term and not appropriate to use in this manuscript. There is not a test for major or non"majorness", rather let the reader decide what is major. There are a few other word choice comments or clarity questions that I have, see below.

L 19: Using the word "major" is a subjective word and elicits an emotional response usually in those that read it. Please choose a less loaded word. Major to one person is not necessarily major to all people. Instead use substantial, significant, etc. Please make the appropriate changes throughout the manuscript as well (results L142; discussion: L 175).

Introduction:

- Line (L) 23: Capitalized "western" as was written in the Abstract and insert Ocean after "Atlantic". As it reads now it sounds like the Atlantic Sea.
- L24: replace "liberation" with "release".
- L 37: Albins and Hixon (2008) did their research in the Bahamas not Florida. Please correct.
- L47: Replace impacts with "negative effects".
- L61: replace "know" with "known".

- Fig 1. To stay consistent throughout the manuscript please refer to the general region of the Atlantic ocean the same way. Throughout the manuscript it is referred to as the Wester Atlantic, North Atlantic, North-Western Atlantic. Choose one way to refer to this throughout all text and figures (see lines: 11, 23, 23, 81, 145 158). Not sure I found them all.

Methods:

- L 82: The Bahamas are actually part of the Western Atlantic Ocean, not the Caribbean, please update your sample sizes.
- L 115: You changed how you refer to grams, here you use (gr) where as you used (g) before (see lines 100, 113). For all references to grams, both in the text and figures, please use (g).
- L 121: It is not clear to me what the TL (34,310) means. Can you re-write or clarify what is meant by this?
- L125: Will you add the github URL so the readers can find the data and code. It was not clear where the raw data would be shared either.

Results:

- L 134-135: Did you use all of the “different pairs of parameters” from all of the studies individually? Do I understand the right? If so can you adjust the words to be “each of the different pairs of parameters”?
- L 134-135: It is not clear what you mean by the lowest and highest weight estimates. For what size did you estimate these? These are pretty low weights so It must have been a small length, why just do it for a small length? Would you assume the effect to be the same among at all lengths? I suspect not with the exponential relationship.
- Fig. 3. Why are there two solid light green lines for the Sabido-Itza et al. 2016b? IN the legend for sex make sure that there are two dashed lines for female not just one short dashed line.
- Fig. 4. In the caption please indicate what “BC” and “PNAX” mean for the Sabido-Itza 2016b refer to. It is not clear to the reader.

Discussion:

- L 152: The phrase “variation is related to space” does not really say much. What do you mean by related to space? Do you mean that the is spatial heterogeneity in the parameters?
- L 152: I am not sure what word you mean to use when you said: “we shot that”. Please choose a different word.

Experimental design

- L 82-92: The authors do a good job indicating many of the differences among the studies, I wonder about the time of year, which might have a large effect on lionfish growth. One would expect both temperature and food availability to change. What time of year were these collected?
- L 92-94: Why include the Fogg et al. (2013) data when you already know that it is likely to underestimate the weight due to the use of spineless weight. What effect did this have on the analysis using data that you know to be different than the other data? Interestingly, Fig. 3 shows that the Fogg data has some of the largest weights-at-length, not the smallest.

Validity of the findings

- L 107-108: There appear to be quite a few differences in the sampling and processing methods among all of the studies (lines 82-92). Which the authors rightly discuss and support the conclusion that using local in situ, not ex situ parameters are critical to use. However, I wonder how this affects the data analysis. To use an ordinary least squares regression when all of the locations and study methods are likely to have an effect does not seem appropriate. I suggest using a mixed modeling technique with “study” as a random effect and the other parameters from equation (3) as the fixed effects, so running a LMM. If the error is not normal, as I suspect because you used a standard error correction you could then use a GLMM with a Gamma error distribution since the response variables are continuous and strictly positive. Doing this type of analysis would allow you to talk more specifically about the amount of variation that occurs based on the ex situ parameters to a site.
- L175: Remove the word major from the main conclusion. Use either key, central, or fundamental. Major seems to be too strong and subjective of a word.

Comments for the Author

Overall this study is a nice review of the existing data on allometric growth of lionfish. I like using new data as a case study to show local and regional differences and highlights the need to collect data at the local level to understand how this invasion manifest across the invaded region. I am interested to see how using a different statistical test might be able to shed some light on what the actual variation is among sights and using a mixed-model method should help you estimate this.

Reviewer 2

In this manuscript, the authors present an analysis of 17 previously published weight-length relationships for lionfish throughout the species invasive range, along with new data on the weightlength relationship for a population along the central Mexican coast. The authors report important differences in allometric scaling among studies and highlight the effects of applying mis-specified growth parameters to stocks lacking this information. The overall question is relevant to fisheries management and control of invasive lionfish populations, and the paper is well referenced. However, there are several aspects of the paper that would improve the contribution, both in their methodological approach and discussion of their findings. I outline areas for major improvement below.

1. The authors don’t compare the weight-length relationships statistically; instead they compare mean ratios of predicted to observed weight when the estimated parameters from each different study are applied to the range of body lengths they collected in their central Mexican regional study. The violin plot (Fig. 4) that shows these ratios is illustrative, but this isn’t the best approach for several reasons. First, ratio data isn’t distributed normally, and has unique behaviors near the tails of the distribution. It should always be transformed, preferably using a logit transformation, prior to analysis. Second, the parameter estimates from other studies are being applied to lengths of fish outside of the range of lengths that were used to generate the parameters. ANCOVA is the most powerful approach for comparing multiple linear relationships, as it specifically accounts for variation in the covariate (total length) among groups. The application of ANCOVA to the logtransformed weight-length relationships would identify if the beta (b) coefficient varied significantly among populations, and then which populations differed could be discerned using post-hoc tests.
2. The second major improvement would be inclusion in Table 2 of relevant information that would most likely have contributed to any differences in the weight-length relationships among the regions. Most importantly, this Table should include the range of total lengths that each study included, sex (which the authors already include), the depth at which most fish were captured (if reported in the original study), and the range of months (or seasons) during which fish were captured. The size range

is likely a major factor in driving differences, since most of the differences are observed at the largest total lengths, where data is likely sparse for many studies. Importantly, the authors should include a plot of the log-transformed data (reflecting the ANCOVA), with the plotted lines for each study only extending across the range of total lengths included in that study. Figure 3, showing the untransformed data, should be amended so that each fitted curve extends across the range of total lengths specific to that study. Fish occupying shallow versus deep habitats could demonstrate differences in growth and condition. Similarly, growth can vary considerably among seasonal periods, particularly seasons just prior to or during spawning.

3. The recommendation for major improvement is in the Discussion, related to the interpretation of their findings. The authors focus mainly on the implications of their findings, in a general way, but offer no potential explanations for the results they observed. Do the patterns of variation in weight-length relationships for invasive lionfish align with the genetic results that have been published? Are they aligned spatially....do populations that are closer geographically have more similar growth relationships? Are there potential effects of depth/season? Did any studies demonstrate a sufficient lack of contrast in total length data to generate a weight-length relationships representative of the entire population in that region? A paragraph or two of Discussion on plausible explanations for the patterns they observed would be a valuable addition.

Minor edits

1. In the Abstract, I would recommend changing the word ‘substantial’ to ‘significant’, assuming the ANCOVA detects differences in the beta coefficient. Also, ‘age-at-length’ should be ‘length-at-age’.
2. In the Methods section, the authors should refer to Tables and Figures in the order they are mentioned in the text. In the first sentence, they should refer to Fig. 1, not Fig. 3.
3. Methods lines 86 and 88: the authors refer to three and five studies in the text, but actually cite 4 and 7.
4. Methods lines 98-100: the authors refer to reduced gear selectivity of hand nets; it’s not clear how substantial this is, but is more reason to include details about sampling methods, size ranges included, depth, and month/season in the table that details the previous studies.
5. Methods: There is no need to include equation (3). The weight-length relationship in fishes is simply modeled as a power function, which can be log-transformed to generate a linear relationship. This is clear from equations (1) and (2), in fact, the log-transformation of this power function is standard enough that equation (2) does not likely need to appear in the manuscript, instead simply state the function was linearized using log-transformation.
6. Table 1 is not necessary as all of the included information is presented in Figure 2 or in the body of the text. Related to this, only include information on the weight-length relationship from the central Mexican coast in the text that is not already presented in Figure 2; there is no need to include it in two places.
7. Results line 140: stated predicted/observed ratios were 0.36 – 3.51, but in the sentences above the authors state the lowest was 0.80 and the highest was 1.76; if the above are means and the 0.36 – 3.51 refers to individual fish, this needs to be made clear, but I think reporting the means is sufficient, and most appropriate; particularly if the ANCOVA applied to the full relationships makes the analysis of the predicted/observed ratios redundant, and it is removed. Reporting the range in mean ratios would be sufficient along with an illustrative figure.
8. Remove the column for parameter c from Table 2. $a = 10c$, so there is no need to show both a and c in the Table. Change Fit to R^2 in the Table heading, so it doesn’t need to be defined in the caption.
9. Figure 3 caption: Why do the authors only show the relationship for 12 studies instead of all 18 that are included in Figure 4?