06-Aug-2023  
  
Dear Dr. Tejero-Cicuéndez:  
  
Thank you for submitting your manuscript, "Evolution along allometric lines of least resistance: Morphological differentiation in Pristurus geckos" (ID EVO-23-0198), to Evolution.  The evaluations of the Associate Editor Dr. Brian Sidlauskas and two Reviewers, as well as my own, indicate that your manuscript is potentially appropriate for Evolution.  Nevertheless, some substantive issues need to be addressed before the manuscript can go forward.  
  
The concerns of the Associate Editor and reviewer are detailed below, and include suggestions for how to improve  the presentation in ways that will substantially improve the paper (e.g., writing and figures).  
  
When submitting your revised manuscript please provide a cover letter documenting the changes that you have made to the original manuscript. To expedite the processing of the revised manuscript, please be as specific as possible in your response to the reviewer(s). The manuscript length guidelines are expected to be adhered to as much as possible during the revision process.  
  
Please note that issues are occasionally identified in the revision that were not observed in the original submission. Hence, there is no guarantee that the revised manuscript will be accepted even if the issues highlighted in the first round of review are addressed.  
  
All revised manuscripts must include two versions of the main text: (1) a clean document and (2) a document with highlighted and/or tracked changes. If you are unable to do this (e.g., because your manuscript was created in LaTeX), please contact the editorial office.  
  
DATA ARCHIVING: Please also remember that Evolution requires, as a condition for publication, that data supporting the results in the paper be archived in an appropriate public archive. Please ensure you have archived your data and included an updated link to the data in the Data Availability statement, if applicable.  
  
If you indicated in your submission that you will be archiving data on Dryad, the journal will notify Dryad of your acceptance decision to initiate curation and release of your data. Once your data have been deposited and released, please include the Dryad identifier (DOI) in your updated Data Availability statement. Please email the editorial office at [managingeditor@evolutionsociety.org](mailto:managingeditor@evolutionsociety.org) if you have any questions about this process.  
  
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When we receive your revised manuscript, the Associate Editor will review it and your responses before deciding whether further revision or information is needed.  The Associate Editor will then make a recommendation regarding final acceptance to me, and I will make a final decision on whether to accept the paper for Evolution.  
  
In order to facilitate the timely publication of manuscripts submitted to Evolution, we ask that you resubmit your revised manuscript within 90 days. If this is impossible, please send an email to our Managing Editor ([managingeditor@evolutionsociety.org](mailto:managingeditor@evolutionsociety.org)) to request an extension. Otherwise, any paper returned after the 90-day limit may be treated as a new submission.  
  
Once again, thank you for submitting your manuscript to Evolution and I look forward to receiving your revision.  
  
Sincerely,  
Dr. Tim Connallon  
Editor, Evolution  
Dr. Tim Connallon  
  
Associate Editor  
Comments to the Author:  
I have received two reviews from experts in this paper’s methodology, both of whom agree that the manuscript has improved substantially from the previous version.  Both also indicate some need for additional revision.  
  
Reviewer one asks for a phylomorphospace with fewer manipulations to the dataset prior to visualization, citing Polly (2013), who demonstrated that the use of a phylogenetic correction prior to comparative analysis can yield results that are difficult to interpret, or that differ substantially from results using ordinary PCA. While the supplement does include a phylomorphospace using raw trait values, it primarily illustrates that some species are larger than the others (PC1) without telling us much about proportional shape.   Presentation of a phylomorphospace based on ordinary principal components analysis of the size-standardized species means (e.g., without an underlying modification for the phylogenetic structure of the data) would better address this reviewer’s concern and would reveal whether any of the core results of the paper depend on the choice of ordination method.   The authors may want to place both visualizations in the main paper, or to place one in the supplement, but either way it would be valuable for readers to be able to see both versions of the phylomorphospace.  
  
Reviewer two penned a very thorough review and raised a concern about the possible conflation of static and ontogenetic allometry that needs to be addressed. Has growth really slowed enough at the ontogenetic stage examined herein for the variation to reflect pure static allometry?  If the variation seen here instead reflects a conflation of static and ontogenetic allometry, the paper should call this “intraspecific” variation, and should be more cautious in some interpretations, as the reviewer outlines.  
  
Reviewer two also indicates that the discussion seems weighted too heavily toward an adaptive interpretation of the observed variation in allometric trajectories and raises several good points that support that position.  While the authors of course have the leeway to argue for their preferred interpretation, they should consider these comments carefully.  They may wish to rewrite the discussion to focus more strongly on comparisons to allometric studies in other groups, to reduce the speculation about the adaptive value of allometric differences in these geckos, and to consider alternative explanations.  
  
With respect to the second reviewer’s comments about the figures, I agree that figure 3 is somewhat difficult to interpret and could be more effectively presented as a scatterplot.   I don’t share reviewer two’s concerns about figure 6, which I thought helped to tether the paper’s mathematical abstractions to the original observations nicely. That figure could, I suppose, be complemented with a more quantitative treatment, but I don’t think that the authors need to remove it.    
  
Overall, I think the manuscript is shaping up to be a fine contribution to evolutionary allometry and herpetology, but also agree with the reviewers that some items still need polish. Luckily, the remaining revisions should be relatively straightforward.  I look forward to reading the next version of the work.  
  
Literature Cited  
  
Polly, P. D., Lawing, A. M., Fabre, A. C., & Goswami, A. (2013). Phylogenetic principal components analysis and geometric morphometrics. Hystrix, 24(1), 33.  
  
Dr. Brian Sidlauskas  
  
Reviewer(s)' Comments to Author:  
  
Reviewer: 2  
  
Comments to the Author  
I thank the authors for revising the manuscript based on my previous comments, with most of the comments addressed in the updated version. There is one major aspect of the study that I'd like the authors to consider:  
  
The conclusion that rock-dwelling and ground-dwelling species show contrasting allometric trends (e.g. Ln 28–30) is based on residuals from PGLS on body measurements and SVL. This statement implies that these two ecological groups undergo opposite allometric trends/slopes, but we don't observe this (e.g., Fig. 2). I suspect that this observation is being made due to the compounding corrections of the original morphometric data (for allometry and phylogenetic relationship) that modify the multivariate data in increasingly unpredictable ways. In other words, these corrections make it more difficult to biologically interpret the results (refer to Polly et al. 2013 Hystrix). What I strongly recommend, as I did in the previous round of reviews, is to show a phylomorphospace with PC scores on raw measurements (and perhaps another with allometry-corrected values). Otherwise, it is genuinely difficult for me (and many others) to reliably interpret the biological phenomena occurring in this system.  
  
Reviewer: 1  
  
Comments to the Author  
General. The revision has tackled the problems of the statistical analyses of allometry and of disentangling evolutionary from intraspecific allometry. As a result, the new version of the manuscript is clearly improved. I am less convinced by the way the revision has handled the problem of the relative contributions of static versus ontogenetic allometry to the observed intraspecific allometry, and I think this needs some further work. Finally, given that this essentially is a study about allometry and how it evolves in this group, possibly in response to habitat switching, I am somewhat surprised by how much of the Introduction and Discussion are about things other than allometry. To me, much of this seems rather speculative and relies on interpretations of the results that rely in part on assumptions that cannot be tested with the available data. In some instances, I actually doubt the authors’ interpretations, even though I must admit that my knowledge of herpetology is very limited.  
  
Overall, I recommend a further round of revision. I am confident that the authors can address the remaining problem concerning the intraspecific allometry. For the problems of interpretation, I am coming to (or already venturing beyond) the limits of my knowledge of lizards and herpetology in general. Unless reviewer 2 is a herpetologist, I think it might be sensible to use a different reviewer with expertise in this area for a next round of review instead of me.  
  
  
  
Major points:  
  
1. Response to my point 1 (single origin of ground living): In their response to my comment the authors say that they “have modified the text adding some cautionary notes in the interpretation, especially regarding the ground-living species”. As far as I can see, this refers to the two sentences on lines 383–386 in the revised Discussion. As far as I can see in the remainder of the Discussion (overall 147 lines long), there are many places where associations of traits with ground living are interpreted as adaptation without any mention of this important caveat. I think this requires more changes throughout the Discussion.  
  
2. Response to my point 2 (ontogenetic stages): I think there was a bit of miscommunication between me and the authors here. I apologize if my original comments were unclear, and I encourage the authors to have another go at this problem. The authors have added a comment (lines 131-132) that all specimens included in the data were adults and that ontogenetic allometry therefore could not be considered in this study. This is true and I thank the authors for adding this comment.  
   The more serious concern, however, has not been addressed in the manuscript: the question whether what the authors call static allometry is indeed static allometry or some combination of static and ontogenetic allometry instead. In their response, the authors claim that “while geckos do display indeterminate growth, this diminishes precipitously post-maturation.” In the manuscript, there is no such explanation. If the observed intraspecific variation indeed is static variation, there should be no correlation between the sizes and ages of individuals within species. I am no herpetologist, and so I ran a simple literature search on Google Scholar with the keywords “gecko growth”; the very first paper that the search returned showed that there was just such a correlation in a different species of gecko and calculated growth rates seemed to decline rather gradually (<https://brill.com/view/journals/amre/27/3/article-p393_10.xml>). My point is not to start a debate about “precipitous” versus “gradual” decline of growth rate, but I am genuinely concerned about the interpretation of what is going on.  
   For instance, we might suppose that rock-dwelling species might be more limited in their growth than ground-living species, for which we might provide an adaptive speculation that this enables rock-dwellers to hide from predators in small crevices (I dimly remember reading something like this concerning other lizards) whereas larger individuals in open habitats can invest more resources in reproduction, or alternatively the more extensive growth might just be a non-adaptive feature of the ground-dwelling clade. In either way, intraspecific allometry would be clearer in the ground-living species because of the greater variation in body size from more extensive growth and therefore a greater contribution from ontogenetic allometry to the observed intraspecific allometry. For the same reason, under this scenario, the ground-living species would exhibit stronger intraspecific integration than the rock-dweller because the contribution of ontogenetic variation (expected to be concentrated mostly in the single direction of phenotypic space along the mean ontogenetic trajectory) produces higher integration.  
   The information I have seen is not about Pristurus, and so I cannot say whether such a scenario might be at all realistic. I see two possible ways to address this problem. If this information is available, the authors could provide convincing evidence in the manuscript (e.g. from literature) that in samples of adult Pristurus, such as those of this study, age and size are indeed uncorrelated (and therefore the bigger individuals do not tend to be the older ones). If this is impossible because there is no such information available (determining the age may require destructive sampling, as far as I understand), it would be sufficient to change the terminology by changing “static” to a more agnostic term such as “intraspecific” throughout the manuscript, and adding an explanation in the Methods section that, because of the indeterminate growth of these lizards, it is not possible to separate static and ontogenetic variation completely. None of the main findings of the paper would change, as far as I can see.  
  
3. Lines 228-235 and 237-252: For each of these comparisons, the manuscript does not present the actual patterns. I would be curious to see those, and there should be room in the manuscript (for evolutionary versus intraspecific allometry, it’s a table with just two columns of numbers, and manageably more for the habitat types…). Seeing the regression coefficients would enable readers to interpret what is going on in closer connection to anatomy, natural history, biomechanics etc….  
  
4. To my taste, the Discussion contains too much adaptive speculation that is presented without sufficient (or even without any) explanation or evidence. This starts around line 372, where the manuscript interprets shifts in allometric trajectories as a result of habitat-induced selection, and goes on to the end of the Discussion. The manuscript presents no evidence that shifts in allometric trajectories are the result of selection. There are many similar instances where the existence of differences is assumed to be adaptive and the result of selection.  
Related to this is also the selection of references, which is at least debatable in some places. The last instance of adaptive speculation in the Discussion is on the implications of arboreality, where the manuscript suggests that limited ranges in shapes and sizes of arboreal taxa are related to strong ecological selection (lines 459-461). The two references cited to support this are papers about the evolution of arboreality in salamanders. Even as an evolutionary biologist not particularly familiar with the study group, I can think of a closer example that provides a very different picture: Anolis lizards, where there is not just one but several types of arboreal niches and morphological ‘types’ of species inhabiting them. So the arboreal habitat seems to be very different for salamanders versus Anolis lizards; how it affects Pristurus is an interesting question that may go beyond what this study has to offer.  
I think a fundamental reworking of the Discussion to focus on questions of allometry, which is the subject of this paper, and removes adaptive speculation would greatly benefit this paper. Where adaptive interpretations are offered, this should come with substantially more detailed reasoning and specific evidence. I am not familiar enough with the biology of geckos to provide a competent review of such a revision; this is why I have suggested that a different reviewer should see a revised version.    
  
  
  
Minor points:  
  
1. Line 55: The phrase “allometric patterns manifest widely” is awkward (“differ” instead of “manifest”, or something like that?).  
  
2. Line 268: The arrow is the wrong sign for indicating a range (I might simply spell out that angles ranged from 5.8˚ to 7.2˚).  
  
3. Figure 3. What is the aim of this figure? If it is to show the connection between head residuals and limb residuals, why not produce a scatter plot of hwad residuals versus limb residuals and possibly project the phylogeny into the scatter plot (phylomorphospace with scatter plot of residuals as the morphospace instead of a scatter plot of PC scores). You could use different colors of symbols for the dots to indicate habitat types. I think this would be clearer, as I find it a bit tedious to relate the colors of lines in the two otherwise identical graphs, and spare a thought for a reader who might print out the figure on a black-and-white printer….  
  
4. Line 296: spell out that this is the phylomorphospace of the PCs of regression residuals.  
  
5. Line 366: Allometric patterns aren’t rock-like. Change the wording.  
  
6. Figure 6 is not entirely helpful because it is not consistent with the quantitative approach used in the rest of this paper. Why not draw some diagram that is based on the measurements actually used in the analyses, but without uncontrollable differences in other aspects?