Dear Author:  
  
One of our associate editors (Dr. Marcelo A. Aizen), two expert reviewers, and I have now read your manuscript. At this time, the Editorial Board remains unsure of the manuscript's suitability for the American Naturalist. Consequently, we request a major revision of your manuscript "Population-specific patterns of toxin sequestration in monarch butterflies from around the world" before we can reach a decision. Please be aware that some of the concerns raised below may not be fixable, and the manuscript may yet be declined.  
  
We all appreciated the effort you have undertaken to respond to reviewer comments. The reviewers, AE and I all agree that the manuscript is much improved as a result. However, with greater clarity sometimes we then find significant concerns with data or analyses that we missed previously, and I am sorry to say that is the case here. I am open to the possibility that you might be able to assuage our concerns, however at present I am doubtful that the manuscript can be revised to our satisfaction. Rather than assume this however, I am giving you the option to respond with suitable changes. But it may be that your path of least resistance would be to submit elsewhere (making suitable changes as you do so, of course).

**We appreciate the opportunity to revise and resubmit our work and understand that the concerns that you raised are substantial. We have made considerable changes to the text of the manuscript, most notably removing the section on inter-island comparisons of sequestered cardenolides between Guam and Rota and completely removing results that discuss sequestered cardenolides in terms of ratios. Below, we detail our responses to specific points raised by you and other reviewers.**  
  
The crux of my concerns regard analysis and interpretation. For one thing, analyses of ratios are famously problematic, because variation in the ratio is especially sensitive to variation in the denominator. Here, your sequestration ratio has plant cardenolide concentration in the denominator, and so variation in the ratio may be driven solely by variation in plant chemistry. Luckily there is a fairly simple fix here. One is to use the log of the ratio because ln(X/Y) = ln(X) – ln(Y) that linearizes the issue so your numerator and denominator contribute equally to variance in the log ratio. Another is to use the butterfly concentration as your dependent variable but put plant chemistry in the linear statistical model, then test whether there’s an effect of butterfly population or a population \* plant chemistry interaction. However, the current analysis of a ratio is not suitable.

**We agree that statistical approaches involving ratios can make it difficult to interpret results, especially in the case of our comparison of wild-caught butterflies from Guam and Rota in 2015. For this reason, and because of the inherent limitations associated with having only one “predator-free” environment, we have de-emphasized these results and moved the inter-island comparison to a supplementary appendix. The reason why we were originally inclined to use butterfly:plant ratios of cardenolides in this particular instance is that we had no knowledge of what natal plants the wild-caught butterflies from Guam and Rota had developed on; instead, we took a small sample of the milkweed plants available to them at the time of their collection from each location and divided each individual butterfly’s cardenolide concentration by the corresponding cardenolide concentration from these associated host plants. We later returned to Guam and Rota in 2018, with the intention of rearing these populations side-by-side in a controlled environment to eliminate potential effects of host plant chemistry. However, we were unable to collect any monarchs from Rota in 2018, leaving us with only data from wild-caught butterflies from 2015.**

**By contrast, for the much larger dataset comprised of greenhouse-reared butterflies, the response variable in all of our analyses is total hindwing cardenolide concentration (not a ratio). We feel comfortable using this as a response variable for two reasons. First, as you suggested, we tested a model using the subset of butterflies (n = 206) with paired host plant cardenolide measurements and did not find evidence for a positive correlation between natal host and butterfly cardenolide concentration. We intended to make this point during the last submission with a supplemental figure, which shows the raw data relating plant and butterfly cardenolide concentrations. Apologies that this connection was not made more clearly. We have added text to the methods and results that indicates that the cardenolide concentration of natal host plants was not a strong predictor of cardenolide concentration in wing tissue. Second, in our greenhouse rearing experiment, caterpillars were assigned to plants fully randomly; thus, each monarch population should have been exposed to a similar sample of host plants. This is contrast to the results from wild-caught Guam and Rota monarchs, where we could not experimentally disentangle the contributions of genetic variation in sequestration ability from environmental variation / host plant chemistry.**

This becomes a particular problem in Figure 5B&C: you don’t find a statistically significant difference between two butterfly populations (I again feel a twinge of hesitance over considering a pseudoreplicated comparison that tries to attribute a difference between two populations to an ecological cause (predation)). In this figure the difference becomes significant when you adjust for plant chemistry, but as noted above this might have nothing to do with the butterflies.

**Yes, we agree that these results were somewhat confounded and have removed them from the main text of the manuscript. This also entailed rewriting sections of the introduction and the methods of the paper. We feel that the greenhouse experiment is far more central to the economy of this manuscript and hope that de-emphasizing the results of the comparison between monarchs from Guam and Rota does not compromise the value of the manuscript. We have remade Figure 5 so that it now focuses exclusively on comparisons between Guam and other monarch populations from the greenhouse rearing experiment, where the results should be more easily interpretable.**

Consider this biological scenario: if butterflies asymptotically sequester up to some maximum concentration X, where two populations are the same (X1 = X2), but their plant chemistry is different (Y1 < Y2), then X1/Y1 > X2/Y2 even though the physiology of the insects are identical. Thus the data that you present in Figure 5 is not sufficient to convincingly make your case, and I am skeptical that it you can fix this. If my concern is warranted, then no further revisions will make this acceptable at this journal, but I am open to hearing your solutions if you have one. In doing so, the very vague  
“adjusted for the average cardenolide concentration” on line 288-290 will need to be made far more precise and clear.

**We agree that this presents a challenge to interpreting the results originally presented in Figure 5 and have downplayed these results, instead focusing only on greenhouse-reared monarchs from Guam where we can confidently assert that differences in levels of sequestered cardenolides are not driven by differences in plant chemistry.**

**With regards to the scenario of asymptotically sequestering up to a maximum concentration, there is some empirical support for this scenario from other rearing experiments (e.g. Figure 2 of Malcolm 1990, *Chemoecology*), although this pattern is much more apparent in *across-species* than *within-species* comparisons. Detecting an asymptotic sequestration relationship within species seems to depend on the level of intraspecific variation in plant cardenolides: if many plant genotypes / collection locations with a broad range of leaf cardenolides are considered, the chances of seeing non-linear sequestration patterns increase. By contrast, because we chose to rear butterflies on plants grown from a limited number of seed sources, this probably reduced the amount of variation in cardenolide “inputs” for caterpillars, and we did not see a noticeable pattern of asymptotic sequestration across any of our greenhouse-reared butterflies (see Supplemental Figure S5). Therefore, we continue to feel comfortable with our use of statistical approaches that treat total cardenolide concentrations as a normally distributed response variable, as we did not find evidence for an “upper bound” on sequestered cardenolides within any of sampled milkweed species.**  
  
In revising please consider a few other points (whether for this journal or others).   
  
First, your statistical models (line 231 for instance) are written in R’s idiosyncratic lme4 notation that is by no means universal, please consider a more formal standard statistical model way of writing this out that will be understandable to non-R-users as well.

**We have updated the section of the methods where we describe our statistical models. The resulting equations describing each linear model are somewhat cumbersome owing to the number of terms included in each model, but they are presented in formal linear model syntax. We will defer to you as to whether you prefer this formulation or some form of “pseudo-code” that describes our models without using R-specific syntax.**  
  
Second, I’m not a fan of using letter abbreviations like GOPH ACSU AINC ASFA…. To denote populations or species in figures or legends, when you can write out the text for greater clarity. This affects multiple figures (which otherwise are visually very appealing)

**We have updated all figures so that milkweed species are listed in binomial nomenclature (e.g., *G. physocarpus*). We also updated the labels used for each monarch population so that the only abbreviations are for Eastern North America (E. N. America) and Western North America (W. N. America). A few of the supplementary figures may still contain abbreviations; we will update these in subsequent revisions, if permitted.**  
  
Line 328 or thereabouts – you refer to Figure 4A, then to 4C, but there’s no in-text reference to 4B that I could find.  I also couldn’t find an in-text reference to Figure 5A, 5B, or 5C

**Thank you for pointing this out. We modified Figure 4 to substitute in a new panel, and we have checked to make sure that every panel in each of our main figures is now referenced in the main text.**    
  
Line 333: incomplete sentence.

**Apologies, this (and a few other instances) were formatting issues where the insertion of in-line figures cut off lines of text. We have moved all of our figures to the end of the manuscript to avoid this problem. The actual text of this sentence reads “Aspeciocide concentrations in Puerto Rican monarchs reared on *A. syriaca* were more than 23 times lower than for other populations, and 15 times lower on *A. speciosa* (Table S5).”**  
  
Where relevant report statistical interactions (e.g., Figure 4B is there a statistical interaction, and does that matter?), same with 4C.

**We added a statement to the caption of Figure 4 stating the monarch population x host species interaction (now shown in Figure 4C) is statistically significant.**   
  
In figure 4a, is there a biologically meaningful order to the x axis rather than arbitrarily using alphabetical order?

**We have rearranged axis labels so that populations are ordered according to the phylogram shown in Figure 1. We show the ancestral North American populations (Eastern and Western North America) first, followed by the Pacific lineages (Hawaii, Guam, Australia), and then Puerto Rico.**  
  
Line 357-358: this seems like a crucial negative result that rather undermines the case for the importance of these results. Moreover in this whole paragraph I struggled to determine whether you were using concentration or the sequestration ratio (adjusted) that you present in Figure 5C, for any given statement.

**We agree that this result (no overall difference in levels of sequestration between Guam and other populations) is important and continue to emphasize it in the reporting of our results and the discussion. Indeed, the only population-level differences that are detectable between Guam and other populations come when this population is reared on its sympatric host *A. curassavica*. This detail is important for two reasons: first, because *A. curassavica* is the sympatric host for Guam monarchs, natural selection has more opportunity to directly act on the process of sequestration from this particular host (relative to allopatric hosts, where we would expect relaxed selection). Second, *A. curassavica* is the lone species of milkweed for which substantial physiological costs of cardenolide sequestration have been detected; thus, it may be the only species where sequestration would be actively *disfavored* in the absence of bird predation. We have updated the discussion of the paper to hopefully make this interpretation more clear, and we also added references to a new preprint (Blount et al. 2021) and a newly published paper (Pocius et al. 2022) that help make this point.**  
  
Lastly, I agree that the readers need better assurance that the hindwing is a good proxy for whole-body sequestration, or that the ratio does not differ among populations (otherwise we might wonder if your results show us that populations differ in how they distribute cardenolides between wings vs thorax)

**We added the following text to the methods section to hopefully assuage concerns about using hindwing tissue as a proxy for the broader process of sequestration:**

**“We chose to measure cardenolides from monarch wings for a number of reasons: previous work has shown that wings are the tissue with the highest concentrations of cardenolides (Brower and Glazier 1975, Fink and Brower 1981), wing cardenolide concentrations are highly correlated with cardenolide concentrations in other tissues (Fink and Brower 1981, Freedman et al., unpublished data), and wings are the tissue most likely to be used by predators to assess monarch palatability (Fink and Brower 1981).”**

**The Fink and Brower (1981) reference is somewhat opaque in its methods, but the caption of Figure 2 states that they found a strong correlation (r = 0.88) between wing cardenolides and the wingless bodies of 100 monarchs from a Mexican overwintering site. Our mention of unpublished data is in reference to a follow-up experiment that we are currently conducting. As part of this experiment, we included a small number of paired measurements of hindwings and butterfly abdomens for monarchs from Puerto Rico and North America reared on *A. curassavica* and *A. syriaca*. We show these results below, although we would prefer to not include them within this manuscript, since they are part of a separate set of experiments that involved slightly different rearing protocols and cardenolide measurement protocols. We hope that these new data, along with our references to Fink and Brower (1981), will suffice to convince you and the other reviewers that hindwing cardenolide concentrations are correlated with cardenolide concentrations in other tissues, and also that this correlation does not vary majorly between milkweed species or across monarch populations.**



The revision should carefully address the concerns raised by the reviewers, Associate Editor, and myself, and be accompanied by a file describing your responses to our concerns. The AE and reviewers’ comments are at the end of this letter. I hope you find the comments to be constructive and that they help improve your paper.  Your revision may be subject to review.  
  
I want to be completely straightforward about this manuscript's prospects. The manuscript seems to have the potential to meet the American Naturalist's goals to publish papers that are of broad interest to the readership, to pose a new and significant problem or introduce a novel subject to the readership, to develop conceptual unification, and to change the way people think about the topic of the manuscript. Therefore, I want to encourage revision of this manuscript. But, the review process has identified important weaknesses in the present submission. We would like to see these weaknesses fixed, at which point we will be in a better position to judge the paper’s suitability. We must emphasize that, even in revised form, the manuscript ultimately may not be accepted.   
  
Please be careful in your revision to add as little as possible to the length of the text, and any ability to condense a bit here and there would be appreciated. Our journal is under extreme competition for space among many excellent articles, so we are forced to consider the value of a paper relative to the number of pages it requires. Therefore an idea needs to be presented concisely to maximize its probability of success in publication.

**We note that our revision is approximately 350 words shorter than our previous submission.**  
  
To ensure that your work satisfies The American Naturalist’s commitment to transparency and Open Data, your manuscript and data repository has been evaluated by one of our Data Editors. They generated a report identifying deficiencies and making suggestions to make your data/code repository complete and useful. I’m pleased to say that your data archive is in generally excellent shape, which is great! The Data Editor did have some recommendations. Our goal is to adopt a simple and painless process to improve compliance with data archiving, to avoid post-publication problems that may arise from incomplete data archives or poorly documented metadata.  We feel that review of data repositories is crucial to ensure that the work is reproducible. Recent evidence also suggests that a useful data repository will increase the impact of your work. If your data and code is accessible and usable, readers may be more likely to embrace and build upon your exciting findings!   
  
If you do opt to resubmit, please submit your revised manuscript to the AmNat web peer review website within 60 days. Please upload a detailed explanation of your responses to all of the comments (mine, the Associate Editor’s and the reviewers). Your responses will be available to any subsequent reviewers, so to maintain double blind review (unless you opt out), please do not include identifying information.   
  
Thank you for submitting your work to The American Naturalist. I look forward to seeing the revised version of your manuscript soon.  
  
Sincerely,  
  
Daniel I. Bolnick  
Editor-in-Chief  
The American Naturalist  
Professor, Department of Ecology and Evolutionary Biology, University of Connecticut  
  
  
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Associate Editor Dr. Marcelo A. Aizen's Recommendation  
  
Dear Dan,   
  
I read the revised version "Population-specific patterns of toxin sequestration in monarch butterflies from around the world”, and I am very pleased about how the authors addressed and incorporated the reviewers’ and our comments into this well-written MS.  Particularly, I appreciate the effort made by the authors to widen the scope and implications of their findings. They did succeed.  Also, I understand the reasons why not considering plant species and monarch populations as random effects.  In any case, a summary of their extensive arguments should be incorporated in the MS as many readers may wonder the same.

**As per your suggestion, we added a section the methods explaining our attempts to fit a series of models with random intercepts and slopes only, and we hope that readers will find this to be useful.**  
  
Also, when I read the original version, I had the same caveat as yours about the one-to-one comparison as evidence of reduced toxin sequestration when predation pressure is relaxed.  Even though I share your view that the geographical scale of the comparison does not lend itself to replication (and this is now well-recognized in the revision), I would suggest the authors to include one or two sentences strengthening the intended adaptive interpretation if this is possible rather than, or in addition to, the classical “future studies… “ statement.

**As part of our revisions for this iteration, we have tried to be more circumspect about the interpretation of our results from Guam. This includes fully removing the section on the inter-island comparison with Rota, as well as removing a paragraph from the discussion that suggested future studies. Instead, we try to emphasize the inherent limitations of our “n = 1” predator-release treatment and mention this in both the methods and the discussion of the manuscript.**  
  
Finally, but most important, two of the original reviewers kindly accepted to go over this new version. Overall, they are happy with how theirs and the other reviewers ’comments were addressed, just providing a few new specific comments for improvement.  However, Reviewer 3 made two important points, one about how representative is a measurement of toxin concentration made in a single hindwing (l. 164), and the other, about the interpretation of the Guam vs. Rota toxin-sequestration comparison depending on whether considering or not differences in cardenolide concentration in the plants (lines 349-351). This last comment is critical as points out again towards the need of strengthening the intended adaptationist interpretation of the results, particularly in light of limited evidence of local adaptation in toxin sequestration.

**First, for our discussion of interpretations based on measurements from a single hindwing, please see our response to the head editor above, as well as a more detailed response to reviewer 3 below. Second, for the Guam vs. Rota comparison, we have removed these entirely from the main text to avoid confusion stemming from using ratios.**  
  
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Reviewer #1:   
  
All of my comments were adequately addressed. As I stated previously, this is an interesting and well-written paper. I fully support the acceptance of the paper at this time.   
  
I had a few very small comments:  
1) Line 34: I suggest changing  "from predators" to "against predation" Otherwise it can read as if they are getting protection from their predators.

**We agree and have updated this wording.**

2) Line 217: Are the authors talking about R packages and functions here? This should be clarified.

**Yes, we are referring to R packages here and have updated the wording accordingly.**

3) Line 375: A word is missing here. Perhaps "Rican monarchs TO sequester"

**This has been updated.**  
  
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Reviewer #3:   
  
The author(s) has/have done a good job of responding to the comments of the reviewers.  They dealt with the major issue of there only being 1 replicate of the no bird situation and the discussion of this is well-done.  The request to make the introduction and discussion more broad was dealt with to some degree. This is difficult, as there are not that many systems of sequestering taxa that have been investigated in as much detail as monarchs.  The issue of sequestration efficiency was still a bit difficult for me (see point below).  There are a few other points that require some clarification.  
  
Specific comments:  
  
L. 46 and following:  should be nymphalid, with a small "n", and arctiid and zygaenid.  Throughout, when a family or tribe, etc. is used as an adjective, it should not be capitalized.  Check danaine on Line 60.  Also L. 132

**This has been corrected throughout the manuscript.**  
  
L. 54:  Muller et al.  2003 is not listed in references.  Also, the only Muller et al. 2003 paper I could find was C. Muller, N Agerbirk & C. Olsen.  This dealt with differences between 2 Pieris species, not within species differences.  So I'm not sure this is the correct reference.

**Apologies for not including this citation in our reference list; this has been updated. The citation is to a paper entitled “Chemical defense in a sawfly: genetic components of variation in relevant life-history traits” in the journal Heredity.**   
  
L. 144-145:  something missing here and what is missing is important because it describes the differences between Freedman et al 2020a and the current manuscript.  This needs to be rectified.

**Apologies, this seems to have been an issue with formatting. The full sentence reads “This experiment is the same as the one described in Freedman et al. (2020a), although here we focus on cardenolide sequestration as the phenotype of primary interest rather than larval growth rate.” We have moved all of our figures to the end of the manuscript to avoid similar formatting issues with missing text.**  
  
L. 164 and following:  you only measured cardenolides in a single hindwing.  How does hindwing cardenolide content relate to the total butterfly cardenolide content?  Is there some regression that relates hindwing cardenolides to total butterfly cardenolides?  Why only measure 1 wing; was the rest of the insect used for something else?  Are there data from elsewhere showing that hindwing cardenolide content is proportional to total butterfly cardenolide content?  This requires some additional explanation.

**Please see our response above to the head editor for a description of why we feel confident that the results obtained from a single hindwing can be a good proxy of whole-organism sequestration. Our original rationale for using wing tissue was that we were unsure of whether other tissues would be used for separate phenotypic measurements in another study of body allometry across monarch populations. While it is true that the composition of sequestered cardenolides seems to vary across monarch tissues (see Brower and Glazier 1975), the limited number of studies that have tested for correlations across tissues have all found strongly positive relationships.**  
  
L. 257:  related to the L. 164 comment, here it says "total cardenolide concentration in adults", but you didn't measure that.

**Thank you for making this point. We have updated the manuscript to explicitly state each time we mention "total cardenolide concentrations” that this is in reference to adult hindwings only.**  
  
L. 288 and following:  how did you adjust for the average cardenolide concentrations in natural leaf samples?

**We have omitted the section that involves comparisons between wild-caught monarchs from Guam and Rota.**  
  
Fig. 4B:  be sure that "faint lines in background" show up in final proofs.  They did show up on the high resolution copy of figures included with the revision.

**This seems to be an issue with the conversion of Word documents into PDFs through the Editorial Manager software (all R annotation layers involving the “alpha” argument are lost). However, as you point out, the same issues do not arise when we upload the PDF copies of figures by themselves without any conversion. We hope that this will not be a problem for any subsequent review.**  
  
L. 351 and following:  see comment on L. 288.  How are you accounting for average cardenolide concentrations of field-sampled plants?  Please explain how you account for plant cardenolide concentrations in presenting adjusted cardenolide concentrations (e.g., Figure 5).  I think I see what you are trying to do here—is this "sequestration efficiency"?  The reality is that actual cardenolide content of butterflies did not differ; however, given that plant cardenolides were higher on Guam than on Rota, you are suggesting that Guam insects were less able to sequester cardenolides than those from Rota.  I note the large standard errors for the plant data; were these significantly different?  Given this large amount of variation among plants within each population, it seems that making too much of these differences is problematic.  Not sure what to suggest here.

**Yes, you are correct that the original figure showed that the level of sequestered cardenolides between wild monarchs from Guam and Rota was not significantly different. The difference only arose after considering the level of cardenolides in naturally-occurring *A. curassavica* plants from each island. As you and the head editor pointed out, this approach is problematic because it requires using a transformed ratio of sequestered cardenolide (butterfly concentration / plant concentration). This was problematic because it was inconsistent with the rest of the analyses we conducted, may not have reflected any true physiological difference between monarchs from Guam and Rota, and required that we devote an entire section to explaining this different approach. For these reasons, we have chosen to remove this result altogether, and instead focus only on comparisons between greenhouse-reared butterflies from Guam and other populations, as we find that these data are much more straightforward to interpret.**   
  
Figure 5D.  In this panel, Guam is compared to all others, when larvae are reared on the same plant, A. curassavica.  What about comparing Guam to Rota specifically, when reared under these controlled conditions?  OK, I see, from looking at the supplementary data, you were not able to rear Rota caterpillars under these controlled conditions.

**Yes, as you note, we originally hoped to collect monarchs from Rota in 2018 to use in a controlled rearing experiment, which would have obviated the need for considering cardenolide concentrations in naturally-occurring plant tissue. Unfortunately, we were unable to find any monarchs when we visited Rota in 2018, and were left with only data from wild-caught butterflies in 2015.**  
  
L. 368:  this first sentence could use a little work—I know you are trying to keep it general, as suggested by the reviews, but maybe reword:  "We found strong evidence for GxE interactions in sequestration ability of monarchs; this suggests that other taxa that can sequester dietary toxins may also show spatially structured genetic variation in this ability"

**We have updated this sentence in accordance with your suggestion.**  
  
L 375:  "monarchs to sequester…"

**This has been updated.**  
  
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REPORT FROM DATA EDITOR  
  
Dear Authors  
As you may know, THE AMERICAN NATURALIST now requires that all data and analysis code associated with each submitted manuscript be uploaded to DRYAD/zenodo.  The rationale for this — as well as the expected style and contents of those repository files — is explained at: <http://comments.amnat.org/2021/12/guidelines-for-archiving-code-with-data.html>. Please refer to that blog post to ensure that your data, code, and README files are complete and usable.  
  
All papers accepted for publication at THE AMERICAN NATURALIST are also reviewed by a Data Editor who scans the paper and the associated data/code repository to check for completeness of the datasets and usability of analysis scripts. We are doing this to ensure that papers published in this journal achieve a high standard of reproducibility, and that all of those datasets and scripts are readily available and relatively easy to understand.  
  
We hope that you will find the recommendations below to be useful but please do let us know if you have any concerns or comments about this aspect of our manuscript reviewing process.  
  
I congratulate the authors on their sharing and reproducibility practices, using Dryad for your data and Zenodo for your code. You also provide an exemplary README file and very well commented code. I only have minor suggestions.   
  
When future users of your code and data download to their own computer, they will need a bit more information to run the cardenolide\_chromatograms.R code. Because the code is based on functions using the current directory, it would be useful for users to know that for the code to run they need to be within the /chromatograms folder (and unzip it before running). In cadernoiolie\_stats.R your are using relative paths to read the data file assuming a certain structure on the organization of folders which is not what happens when a user downloads the data and code in their own computer. To fix that I would simply add some workflow information to your README file explaining that data downloaded from Dryad are in a folder called data/ and code from Zenodo in another folder. Again here I understand that for experienced users this would not be a problem and they can figure this out on their own. Also please remove from your code any paths that are specific to your computer so that  
the code will run on a clean computer.  
  
In light of this assessment, I recommend:  
1. add workflow information to your README file  
2. remove computer-specific paths from your code

**Thank you for your careful review of our code. We have updated the README file accordingly and changed all file paths so that scripts should be executable by anyone who download the data repository. Finally, we also made some updates to the main analysis script (“cardenolides\_stats”) to address comments suggested by reviewers in this current revision. Our edits may take a few days to appear on Dryad but should be immediately accessible through the Github repository provided in our Data Accessibility statement.**   
  
Thank you for helping us make the papers published in THE AMERICAN NATURALIST as useful as possible.  
  
Sincerely  
Paula Lemos-Costa  
[plemos@uchicago.edu](mailto:plemos@uchicago.edu)  
  
  
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General Instructions  
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Upload your response to this decision letter as file type RESPONSE TO REVIEWERS  
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Please submit your revision file with comments and track changes turned off.   
If you wish to ALSO include a track changes version, attach it as file type "Other."  
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If you haven't already, please make your data available for peer review. When depositing it in DRYAD you can indicate on page 3 of the submission process that you are depositing the data for double-blind peer review. Dryad will generate a randomized URL. Please include that URL in the Author Comments field in Editorial Manager when you upload your revision. <https://datadryad.org/stash>  
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Tables in Am Nat follow the Chicago Manual of Style:  
<http://www.journals.uchicago.edu/journals/an/prep-table>  
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FIGURES CHECKLIST:  
  
1. Are all axes labeled?  
2. Do all axis labels have units?  
3. If there are multiple lines/dots, is each a different line style and color?  
4. Are all lines sufficiently thick (if you used matlab, and they are the default thickness, the answer is no)?  
5. Are all font sizes legible (i.e., about the same size as the fonts in the rest of the document/slide/poster)?  
6. If there are multiple lines, is there a legend or other textual description of each line?  
7. Are your axes 'tight' (that is, are the bounds of the axes just larger than the max and min of what you want to show)? If not, do you have a good reason for the excess?  
8. Are all graphics that can be vector graphics actually vector graphics? (i.e., never use a bitmap if avoidable)  
9. Do all axes have either clear tick marks or gridlines indicating magnitudes of everything?  
10. Are all the letters/numbers fully visible (i.e., not obscured by part of the figure)?  
11. Is the aspect ratio correct? (Hint: if you rescaled both the width and height, it is not.)  
12. Is there any use of Helvetica? (Helvetica can cause font substitution problems in the production systems)  
13. Production quality figures are vectored or high resolution (1200 dpi).  
14. If you are using color to make distinctions, are you including redundant signals such as shape, size, dashed/dotted lines, and labels? Redundant encoding helps make graphics more accessible.  
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If this is a multi-author paper, an Author Contribution statement is required if the paper is accepted. This statement should be carefully written to indicate the role that every author played in generating the paper. However, it is essential that authors do not get credited for steps in which they were not deeply involved. Sample roles are provided below but be sure to assemble a Contribution Statement that fits the specifics of your paper:  
  
Conceptualization; Funding acquisition; Methods development/experimental design; Data collection; Data analysis; Data validation;    Data visualization; Provided resources [including specimens, reagents, equipment]; Software development; Model analysis; Coding simulation;  Supervision; Writing - original draft; Writing - review & editing  
  
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The American Naturalist now has secondary, non-English abstracts in the languages of the location where research was done. There will be an opportunity to paste in an abstract when you return your revision.  
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