Dear Mr. Freedman,  
  
Greetings again from the editor's desk of Animal Migration. I am writing to let you know about your manuscript that was submitted for the special collection on monarch migration. Your manuscript has now been reviewed by two experts in this field, and based on the reviewer comments, as well as my own read of the paper, I have decided that it requires revision before it can be considered further.   
  
The comments from the reviewers are listed below. Note that none of the comments and suggested edits appear to be severe, and Reviewer 2 especially had only helpful suggestions for improvement.  
  
I note that Reviewer 1 had some specific points that I would echo. Specifically about the latitudinal effect you found (which I struggled myself to interpret). All of the work done thus far, including here, indicates that migration selects for larger wing size, and this explains the reason that overwintering monarchs are larger when they come from farther north, and it explains why N. American monarchs are the largest. But if latitude also is a factor, it seems to run counter to this theory. Reviewer 1 appears to favor the idea that selection must also be acting during the spring migration north, leading to larger monarchs recolonizing the northern latitudes. I believe I favor this as well, since it seems to make the most sense. I suggest you indicate in the paragraph where this is discussed (page 13) that is is the most parsimonious explanation. I think the host plant idea also is interesting too.

The discussion of the latitudinal effect has been updated and now highlights the potential discrepancy between latitude of collection and latitude of development. Regarding the host plant effect, we have also added a reference to a preprint (Boyle et al. 2018) suggesting changes through time in host plant abundance.  
  
Reviewer 1 also pointed out something about the latitude issue - that it was assumed the collected monarchs originated from the point of collection. This is a potentially big issue, and could complicate the latitude interpretation. While there is really nothing you can do about this, you should very clearly point this out, both in the methods, and also in this paragraph in the discussion.

The methods section currently includes a mention of this possibility, and we have updated the discussion to further highlight this point. As you suggest, this is not really a problem that we can address using this dataset, and   
  
Reviewer 1 pointed out that they found the "roundness" term to be unclear, and would agree. While I know what this term means, I suspect the many readers of this paper would be less clear. I do have a suggestion to fix this - it seems that roundness is simply the opposite of elongation (a term that is very easy to understand), so why not use this term throughout and simply change the phrasing of the sentences (i.e. instead of saying male forewings were less round than females, say male forewings were more elongated than females).

We have updated the text of the manuscript so that it discusses forewing shape in terms of elongation, rather than roundness. However, we still use roundness as the measurement that we use in all analyses.  
  
I also concur with this reviewer that the discussion about habitat fragmentation seemed to be a stretch, and could probably be removed with little loss to the paper.

This section was removed.

Next, given that this is a topic I am well-versed in, I also had some thoughts and suggestions of my own for improving the paper:  
  
First (and this is from a editor's standpoint), this paper would benefit greatly by including an example photo of one of the museum specimens, exactly as it was taken. This could then include some lines indicating the measurements taken. Keep in mind that this is an online journal, and color photos are encouraged, especially when the subject is a charismatic critter like the monarch.

This has been added and is now Figure 1.  
  
Also from an editor's standpoint, can you rearrange Figure 4 so that all graphs are side by side, instead of one on top and two below. this would allow it to fit better in the formatted paper.

Done.  
  
Next, I see that all wing areas were presented in cm2, which differs from most previous work (mm2). This is not a big deal, but to be consistent, it would be good to present the results and figures using mm.

Done.  
  
In the discussion, when the size differences between groups are given, these statements should also include the percentage difference, to indicate just how large these differences are. I see that percentages were given in some places but not others. I would also encourage you to include percentages in the abstract, which will be read the most by people.

We have updated much of the text to include percentage differences. The exception to this is for where the latitudinal effect is mentioned in the abstract, since this is a continuous predictor and would require some extra explanation to list in terms of percentages.   
  
There is an interesting paragraph in the discussion regarding the sex-based differences in wing size (page 11). In it, you refer to the 2009 paper showing changing sex ratios over time at overwintering sites, and you seem to imply that females may not be as successful in reaching Mexico because they are smaller - I think. If this is the case, it needs to be more clear here. Also, you could add that this idea does not match up with recent analyses of tagging data (Steffy 2015 Annals of the Ent. Soc) that indicated females have higher migration success, so in the end, there are contrasting bits of evidence. You could also highlight the fact that during mating, males have to carry the female around for a whole day, which could explain why they have evolved larger wings - i.e. it may have nothing to do with migration!

Yes, we meant to imply that males might be expected to be more successful migrants, at least based on their wing morphology. This paragraph has been updated to make this expectation more clear. We have added a reference to the Steffy paper that you mention here, and we also include mention of the idea that monarch mating / courtship could be implicated in the size differences.  
  
Finally, I believe the abstract could use a little improvement. One thing that is missing is one or two sentences explaining what these results mean. Currently, you only a vague mention of how you discuss these results in the paper. For example, after describing the eastern vs western size differences, say "this indicates eastern migration induces greater selection for large wings, probably because the eastern migration is longer." After the sentence on the time effect, say something like "this suggests selection for large wings is becoming stronger, perhaps as a result of a northward shift in breeding range and monarchs having to fly farther." Keep in mind that the abstract is what people will read the most (unfortunately, but this is a sign of the times).

Abstract was updated. It was originally written to be very concise, based on the requirements in the submission portal.  
  
If you decide to revise the work, please resubmit a revised version together with a cover letter describing all changes made and explaining how you have followed the referees suggestions. The revision should be made by following point-by-point the comments given below. If you do not agree with the comments made or there are any suggestions you have not considered, we also welcome your detailed justification.   
  
In addition to the editorial remarks, please take care that you have prepared the revised version according to the Journal's style - by carefully following the points indicated in our Guide for Authors at: [http://www.degruyter.com/view/supplement/s20848838\_Instructions\_for\_Authors.pdf](http://www.degruyter.com/view/supplement/s20848838_Instructions_for_Authors.pdf" \t "_blank)  
  
Please submit your revised version as soon as possible, no later than by 09/02/2018.  
  
To submit a revision, go to [https://ami.editorialmanager.com/](https://ami.editorialmanager.com/" \t "_blank) and log in as an Author. You will see a menu item call Submission Needing Revision. You will find your submission record there.   
  
Yours sincerely,  
  
Andrew Davis, PhD  
Editor in Chief  
Animal Migration  
  
Reviewers' comments:  
  
Reviewer #1:   
  
I enjoyed reading this manuscript.  It presents a large dataset on monarch wing morphology as it relates to geography, migration, and history.  In addition, an experiment and re-analysis of published data is presented; thus, this work represents a comprehensive study, adds new information, and clarifies some past unknowns.   
  
I confess that I don't have expertise in morphological analyses, but I expect that the editor will weigh in on this aspect of the methodology.  
  
I appreciated the authors discussion of alternative hypotheses for the latitudinal and temporal patterns observed.  A few nit-picky points about these.  1) I think the authors could be a bit clearer about what patterns could have been caused by phenotypic plasticity versus a response to selection. I think their logic here is sound, but the readers could benefit from clear explanations of which patterns could be caused by which factors (especially important in the discussion).  2) Relatedly, the authors correctly point out that selection may occur through "filtering" of butterflies as the migrate north - it may be additionally useful to point out that the evolution of size/distance is unlikely to be occurring over a multi-year process (I think this is right) because of the panmictic nature of the population.  3) with regard to the interpretation of plasticity vs. response to selection, it could be useful to explicitly explain how to interpret latitude of collection (do we  
assume this is the latitude the caterpillar grew at, and is this a reasonable assumption?). This could be highlighted in the methods and discussion. And finally, 4), the discussion presents a hypothesis based on habitat fragmentation - I find this particularly unlikely because the pattern observed occurred directionally over 140 years.  I cannot imagine h-frag occurring incrementally in this way.

1). We have added a supplementary table (Supplementary Table 2) that explicitly discusses some effects in the context of selection versus phenotypic plasticity. However, we note that this line may be a bit blurry. For example, the latitudinal effect that we observe could be driven both by selection/filtering (i.e. larger butterflies reach higher latitudes) as well as phenotypic plasticity, since milkweed species composition and temperature also varies according to latitude.

2). I agree that the ‘migratory filtering’ effect is captured in the discrepancy between overwintering and summer breeding butterflies. The across year effect could potentially be capturing overall evolution of the population, regardless of its panmictic nature. Although it is true that the North American population is indeed panmictic (at least at a genome-wide scale), it could still be the case that alleles associated with large size are becoming more frequent at the overwintering sites each year, and then these are the individuals that repopulate North America the following year.

3). We have added a sentence to the discussion highlighting the caveat about the potential discordance between latitude of collection and latitude of larval development. There’s not really any way to get around this with museum data. However, it probably is still true that latitude of collection and latitude of development are generally correlated (with the exception of overwintering butterflies, which we explicitly treat as a separate category), so we still think that it’s a worthwhile factor to include.

4). We have removed the section on habitat fragmentation.   
  
The one thing the authors may want to add is a very small additional statistical analysis.  In the discussion the authors state: "This result may be partly driven by the inherent correlation between forewing area and forewing roundness…"  Couldn't this be tested directly with ANCOVA-type analyses?  With an R2 of 0.25, the effect of the two could perhaps be untangled (perhaps not?).

We added this to the figure legend for Figure S2. When wing area is included in an ANCOVA with wing elongation as the response variable, variance gets reapportioned from sex and attributed to wing area. This analysis yields wing area as a significant predictor of wing elongation but not sex, suggesting that our intuition—sex-based differences in elongation are driven by the inherent correlation between elongation and size—is indeed correct.  
  
Other comments below are very minor.    
  
Abstract: Cut the word "Interestingly"

Done.  
  
Abstract: last two sentences are a bit vague and don't tell the story.  Make them more informative and tell us what the big gap is.

The end of the abstract has been updated.  
  
Line 36: perhaps change "prior" to "prior or during" since some animals like monarchs accumulate lipids during migration?

Updated.  
  
Line 37: replace "long" with "long-distance"?

Done.  
  
Line 44: Sentence beginning "This pattern has been…" is a little confusing… simplify and possibly cut into 2.

Done.  
  
Line 49: sentence beginning "Likewise…" is likewise too long.  Split by ";"?

Done.  
  
Methods: An additional sentence or two about roundness could be added. I understand that the author used a previously described methodology, but one sentence could be useful since this may not be common knowledge. An additional sentence (either at the end of the introduction or in the methods) would be useful to explain what roundness measures or approximates in a functional sense.  Area is somewhat intuitive (although not completely to me ;-) but roundness less so.

We added a brief description of what the roundness measure is capturing, and also refer to wing shape in terms of elongation now. We also added a figure showing the process by which images were measure that shows the formula for wing roundness.  
  
Methods: Give the host plant species names, tell us why you chose them, and explain why you combine the Gomphocarpus spp. for analysis (that seems a bit funky). You could just move the sentence from Table S1.

This has been added to the methods section. The species used in this experiment are admittedly not ideal for specifically addressing the hypothesis that North American milkweeds influence monarch wing morphology. They were chosen as part of an originally separate study to look for patterns of local adaptation to host plants across the monarch’s global range. Thus, the Gomphocarpus species were included because they are important host plants in Hawaii and Australia, and the Asclepias curassavica was included because it is the primary host in many other areas of the monarch’s non-migratory range.   
  
Lines 190-195: cut the "significants" (let the parenthetical stats speak for themselves), and replace with magnitudes of effect (what percentage is 0.32 cm2)?

Updated.  
  
274-279: this sentence is tooooo long, please cut it and rephrase.

Done.

317-318: give the percentage increase here as well.

Done.  
  
340: I would suggest replacing "the core" with "part" and "several" with "two to three" just to be clear.  The main reductions in milkweed are primarily in 4-5 states that account for the region where 30-40% of monarchs are "produced".

Done.  
  
Tables: be sure the chi-square symbols are correct.  
  
Done.  
  
Reviewer #2:   
  
This is an impressive data set spanning many years and locations, and some interesting patterns have been identified, with good explanations offered.  Other than suggestions regarding the section about sex differences, my comments are mostly fairly minor suggestions for revision.    
  
Line 16 - typo, delete "are"

Done.  
  
Line 240 - the wording here implies that citation (24) found or at least suggested that females prefer larger males, but the paper cited did not report or suggest that pattern - that paper primarily investigated the effect of wing coloration on male mating success, and found no evidence for female choice (male mating success varied with actual wing coloration, while wing color manipulations had no effect on male mating success).  The study also reported that larger males mated more often - the interpretation was that the study design didn't rule out female choice as an explanation, but there was also no evidence that larger males were preferred by females - they may just be better able to overcome female resistance.  So perhaps a more accurate way to word this sentence would be "…a result of sexual selection favoring larger males, possibly due to increased ability to overcome female resistance, female preference or sperm competition [24,25]."

You are correct that this is an important distinction and that the cited paper does not make the suggestion that females prefer to mate with larger males. The sentence has been updated accordingly.   
  
Lines 337-344 - This isn't a suggestion for revision, just a comment that I think these are really interesting ideas - that the increase in wing area over time could be explained by a shift in milkweed species abundance (rather than an evolutionary change in wing size).  Because A. syriaca thrives in disturbed areas, and there has likely been an increase in disturbed areas during the past century, and the study reported here shows a positive effect of this host plant species on monarch wing area, this seems like a plausible explanation.

A recently posted pre-print (Boyle et al. 2018) further corroborates the idea that milkweeds have changed in relative abundance over our sampling window, and we have included a reference to the preprint.  
  
Line 351 - delete "more"

This whole paragraph was removed, as per the comments from the other reviewer and editor.  
  
Figures 2 & 5 - I thought it was helpful that you included the sample sizes in Figure 5; this might be useful to include in Figure 2 as well (and promote internal consistency).  The colors in Figure 2 are useful, but not necessary in Figure 5, so I'd suggest using the same color on all 4 bars in Figure 5, but that's more a matter of personal preference.

This was updated.  
  
Figures 3 & 4 - What does the shading around the trendline show?  (specify in the figure legend)

The shading corresponds to 95% confidence intervals, which are the default produced in ggplot using the ‘geom\_smooth’ function. This has been added to the figure legends.  
  
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