Dear Professor Goodacre,

We would like to thank you and the reviewers for providing extremely constructive feedback on our manuscript entitled: “*Heritability and developmental plasticity of growth in an oviparous lizard*”.

We have now carefully considered all the comments and revised our main manuscript accordingly. Below, we provide a line-by-line response (in ‘blue’) to each of the comments raised by the Editor and three reviewers (in ‘black’). When helpful, we have pasted the section of our manuscript where we have made edits to provide clarity to what we have done to address comments. Additionally, we have included a document to aid in the review process that documents most of the changes we have made (in Track Changes).

We believe that our revised manuscript is significantly improved and hope that you now find it suitable for publication in *Heredity.*

Sincerely,

Fonti Kar and Daniel Noble

**\_\_\_\_\_\_\_\_\_\_\_\_\_\_\_\_\_\_\_\_\_\_\_\_\_\_\_\_\_\_\_\_\_\_\_\_\_\_\_\_**

**Editor (Comments to the Author):**   
  
Dear Fonti Kar and Daniel Noble   
  
Peer review of your manuscript "Heritability and developmental plasticity of growth in an oviparous lizard" is now complete and we invite you to revise your work based on the reviewers’ reports   
  
The reviewers are unanimous in their positive assessment of the novelty and value of your work. Nonetheless, they highlight a number of concerns regarding the methodology and interpretation of results. Specifically, the reviewers are concerned regarding the change of housing during the experiment, which may confound the obtained results. Similarly, the reviewers comment on the low power to detect multiple confounding factors with the current experimental design.   
  
Please find the detailed comments from the reviewers attached below and we look forward to receiving a revised version of your manuscript.   
  
Best wishes,   
  
Bastiaan Star

**Response:** Thank you Professor Star. We have now significantly revised our manuscript in accordance with the suggestions provided by each of the reviewers. Please see the line-by-line response below.   
  
**Referee #1 (Remarks to the Author):**  
This manuscript focuses on understanding the effect of temperature on the growth curves of lizards. The authors tested if a temperature treatment changed the average growth curves, but also how it may affect the sources of variation among-individuals in these trajectories. They analyze their experiment in a quantitative genetics framework in the context of GXE interactions. They found that lizards raised in colder temperatures where on average larger, but found no support for a strong treatment effect on the average curves or the relative contribution to the variation in mass of the different variance components. However, they find that the different sources of variation change through development regardless of treatment. The questions that were addressed in this study are interesting and the statistical analyzes were thoroughly performed. However there are some formatting issues that, at points, made it difficult to understand the results and need to be addressed. I also have some general and some specific comments that I hope the authors find useful.

**General comments**

What are the implications of differences between treatments in the growth trajectory to understand adaptation to extreme temperatures. For instance is the optimal growth trajectory dependent on temperature? Why is changing variance across the developmental trajectory important for adaptation? This may only be relevant if selection is age specific? The link between the experiment and the adaptive potential will be more clear if the expected relationship between the mass growth trajectory and fitness is explained more clearly.

**Response:**

**Detailed comments**

**Abstract**

Line 23-24. Only at the beginning or through the whole development?

**Response:**

Line 26-27. But in the results is stated that there are no ”significant” differences.

**Response:**

**Introduction**

Line 49: Chevin et al. (2010) is a relevant reference here. Line 83: ”ratio nature” is confusing

**Response:**

Line 84. Hansen et al. (2011) is relevant here.

**Response:**

Line 90. Why the non-genetic sources? While heritability may be dependent on the environ- mental variance, the expected evolutionary changes is determined by the genetic variance. Maybe develop this argument better?

**Response:**

Line 111. Is there any evidence that selection during development is age dependent, why is it important for adaptation that the genetic variance changes through development?

**Response:**

Line 127. Is the prediction related to the release of cryptic variance, if so, maybe refer to it here.

**Response:**

**Methods**

It was not clear the source of the parents. Where they born in the wild? Where the different source populations very different?

**Response:**

Line 134. Why did you chose this breeding desing. Is there are reason for only generating paternal half-sibs and not maternal half-sibs?

**Response:**

Line 154-155: Were all reproducing individuals caught in the wild? Or were some dams and sires in the 2017 trials born in the lab in 2016? This could potentially influence maternal effects. What was the proportion of individuals breeding in two breeding seasons?

**Response:**

Line 197: 94% of females had been sired by a single male? Do you mean offspring instead of females?

**Response:**

Line 196-199: How can you have sperm retention if there is only one male in the box?

**Response:**

Line 231. Non-informative for what, priors on the random effects are rarely non-informative for all the variance, covariance and correlation of a model. Specific?

**Response:**

Line 233. If posteriors are very skewed, the median provide a better central tendency measure.

**Response:**

Line 243: ”Overall. Heritability” has a misplaced period

**Response:**

Line 251. For people not familiar with WAIC, can you refer to the ”standard” difference threshold.

**Response:**

Line 279. Gavrilets & Scheiner (1993) is a relevant reference.

**Response:**

Line 291. Applying the logarithm of the response variable is already a way of mean stan- dardizing the variance. Therefore inferences of the variance components can be linked to evolvability as mean scaled variances (e.g. Hansen et al., 2011)

What about sex differences?

**Response:**

**Results**

314. The statement in the abstract about treatment differences in the variances should be toned down?

**Response:**

Line 317-322. Something is wrong with the figure legend.

**Response:**

Line 332. Heading out of place.

**Response:**

**Figures**

Age was z transformed but graphs are presented with natural scale. How was the back transformation done?

**Response:**

Most legends are out of place.

**Response:**

**Tables**

Line 390. Density of what?

**Response:**

**Discussion**

Line 437. ”have others” change to ”others have”

**Response:**

What are the consequences of choosing the wrong type of growth curve? Line 493. But there are lots of paternal half sibs right?

**Response:**

Line 560-562. Reference?

**Response:**

Line 573: Cold-reared lizards had larger variance in incubation time. But what about the mean incubation time?

**Response:**

Line 556-557. The fact that variance in body mass coincided with changing from single individual habitats to five individual habitats suggests that intraspecific competition may have triggered a response attributed to previous experiences by mothers. For instance see Marshall (2008).

One of the concluding remarks is that there are changes in the mean body mass but not in the genetic variance across treatments, but what would be the population-wide consequences of (environmentally driven) changes in the mean body size in response to temperature?

**Response:**

**References**

Chevin, L.M., Lande, R. & Mace, G.M. (2010) Adaptation, plasticity, and extinction in a changing environment: Towards a predictive theory. PLoS Biology, 8.

Gavrilets, S. & Scheiner, S.M. (1993) The genetics of phenotypic plasticity. vi. theoretical predictions for directional selection. Journal of Evolutionary Biology, 6, 49–68.

Hansen, T.F., P ́elabon, C. & Houle, D. (2011) Heritability is not Evolvability. Evolutionary Biology, 38, 258–277.

Marshall, D.J. (2008) Transgenerational plasticity in the sea: Context-dependent maternal effects across the life history. Ecology, 89, 418–427.  
  
**Referee #2 (Remarks to the Author):**   
  
I have read the manuscript Heritability and developmental plasticity of growth in an oviparous lizard by Kar and colleagues, where they address to which extent different incubation temperatures affect heritability, maternal effects and growth trajectory in skinks. In general, the sample sizes are good, the methods well described and the statistical framework solid. There is, however, a few major concerns that have to be properly dealt with in order to enable interpretation of data. The most serious concern is that hatchlings are housed in individual cages, but at a specific time point moved to a joint cage. After this, the heritability and maternal effects increased. This could, however, be due to that both heritability and maternal effects become more visible when competition and social stress is present as the lizards are housed in the same cage. The data will therefore need to be analyzed separately for the period in single cages and the period being co-housed in order to avoid this to confound the findings.   
  
**Major comments**   
  
The experimental design confounds the effects of socially interacting with other animals, where stress and competition arise, with time after hatching as the housing regime is changed in the middle of the experiment. This makes it impossible to distinguish between effects of timing. This has to be properly dealt with, including a thorough reanalysis of data where data is split in “single cage” and “5 individuals in the cage” data sets analyzed separately. The discussion and results and abstract will need to be rewritten based on these findings.   
  
What is cold and what is hot? While extremely stressing temperatures have been shown to give rise to a release of additive genetic variation, would this be expected for temperatures within the natural range experienced by the species? The motivation for the temperatures of interest and how extreme they are in relation to those in the literature should be introduced already in the introduction and materials and methods, and the findings should be discussed in relation to how extreme the temperatures in studies with similar or different results are.   
  
The presence of less than 1% multiple paternity could potentially give rise to spurious patterns in the animal model, it would be great to investigate if omitting these outliers change the findings.   
  
**Minor comments**  
  
L26-28 Are these significant differences or not?   
  
L28 This statement cannot be made given the current confusion with housing conditions   
  
**Introduction:**   
  
Motivate the choice of the egg stage for studying these effects.   
  
Add information on sex determination in relation to temperature for readers that do not know much about skinks   
  
When suggesting that plasticity might be a way to adapt to novel conditions, it is important to phrase it to reflect that adaptive plasticity typically acts within the range of conditions that are naturally experienced by the populations   
  
Add information on how extreme the conditions experienced were in relation to the range of conditions the organisms are exposed to in nature for the studies that are referred to that give contrasting patterns of additive genetic variation under stress   
  
  
**Methods**   
  
Include a discussion on how extreme the temperatures are in the experiment (e.g. in SDs or other measure that enables standardized comparisons), why these temperatures were deemed interesting, and how they are likely to affect the outcome in terms of release of additive genetic variation.   
  
L505-511 this information should be given already in the introduction and materials and methods   
  
L531 competition and stress are expected to change, leading to larger differences. See major comment, I suggest reanalysing these periods separately.   
  
L536 This will need to be rephrased after reanalysis.   
  
L571 I think constrained has other connotations not suitable here   
  
L581-582 I think the large differences in eclosion time and how they might interact with size and affect fitness is a bit neglected and could be mentioned here.   
  
**Referee #3 (Remarks to the Author):**  
This is a straightforward MS presenting the results of a well designed and executed quantitative genetic study in a lizard species (delicate skinks). The authors tackle the important question of whether genetic variance differs across environmental conditions. They use a paternal half-sib breeding design and a randomized split-clutch experiment to estimate genetic and maternal effects of size and growth and conclude that there is no strong environmental effect on these variance components. A major strength of the work is the verification of paternity via SNPs, which allows the variance components to be estimated using a genomic relatedness matrix.   
  
In general, I believe that this study should be published in Heredity, but I think the authors should consider a few things in a revision before the MS is published. I detail a couple of substantive concerns below, followed by a short list of copy-editing suggestions.   
  
The main concern I have with the study is the potential that the design does not have a great deal of power to separate maternal effects from additive genetic effects. Because it is a paternal half-sib breeding design, the power to estimate the additive genetic effect will come from having **multiple dams nested within sires**. Maternal effects will be more easily estimated when there are larger families, multiple generations, and/or when there are multiple sires per dam. Otherwise, there is the potential for a confound between the genetic and the maternal effect. Here, multiple paternity is low, and as far as I can tell, family size seems to be small. I notice that there are 144 dams but only 262 offspring. This means that the average family size is just under two. Then, when the offspring are separated into treatments, there seems to fewer than one offspring per dam in each treatment. Incidentally, it would be nice to have a table detailing pedigree statistics (sires, dams, family size, etc.) within each treatment.   
  
Comparing Tables S4 and S5 to Table S6 suggests that G and M may indeed be confounded. The M components are much larger (and G slightly smaller) in S4/S5 when the treatments are split than when they are both included. This suggests to me that G and M are confounded due to the lack of replication within dams in S4/S5, but that when all offspring are included, this is ameliorated somewhat, leading to a more reasonable value for M. There also seems to be a very strong correlation between slope and intercept for M, further suggesting that the model is overparameterized.   
  
I recommend that the authors consider comparing models that do not include maternal effects in their model comparison approach (Table 1). **If I am correct that there is a confound, I would expect G-only models to provide similar fit to G+M models**.   
  
As far as I can tell, Model 7 was selected based on the full dataset rather than the split data set (line 254). Is this correct? If so, the authors may be selecting a model to analyze their split dataset that has too many parameters.   
  
One of the findings is that the proportion of variance explained by maternal effects declines and rebounds. I believe that this conclusion hinges on analyses from the split data set (figure 3). If so, I would treat this conclusion with extreme caution given the potential confound mentioned above. I would also like to see how these curves look from the combined dataset.   
  
I found the statement noting the rarity of half-sibs in line 493 to be curious, as it seems to ignore paternal half-sibs, which should not be rare given the breeding design, and focus only on maternal half-sibs. I suggest editing this, as it has the potential to be misleading. That said, it would be nice to discuss the origin of the few maternal half sibs. Is this because females had stored sperm from previous matings in the wild, or because some females were mated multiply in the lab?   
  
I suspect that none of these considerations will affect the authors' main conclusion that G does not significantly differ across environments, but it would be nice to confirm that this result is robust to a model that does not include maternal effects.   
  
Minor copy-editing changes:   
30: Hyphenate "age dependent"   
85: I think this should be "have" instead of "has" here, because the clause modifies "traits", not "heritability". Also needs a comma before "which".   
393: "influenced" -> "influence"   
412: "is" -> "are"   
  
Supplement:   
35: "To avoid overfitting" is repeated