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Temperature variance, rather than mean, drives adaptation to local climate

[Fabien Aubret](#) based on reviews by Eric Gangloff and Ben Phillips

A recommendation of:

Jean-Claude Tourneur, Joël Meunier. **Thermal regimes, but not mean temperatures, drive patterns of rapid climate adaptation at a continent-scale: evidence from the introduced European earwig across North America (2019)**, *bioRxiv*, 550319, ver. 4 peer-reviewed by Peer Community in Evolutionary Biology.

[10.1101/550319](https://doi.org/10.1101/550319)

Submitted: 15 February 2019, Recommended: 29 May 2019

Cite this recommendation as:

Fabien Aubret (2019) Temperature variance, rather than mean, drives adaptation to local climate. *Peer Community in Evolutionary Biology*, 100074. [10.24072/pci.evolbiol.100074](https://doi.org/10.24072/pci.evolbiol.100074)

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Climate change is impacting eco-systems worldwide and driving many populations to move, adapt or go extinct. It is increasingly appreciated, for example, that species may adjust their phenology in response to climate change, although empirical data is scarce. In this preprint [1], Tourneur and Meunier report an impressive sampling effort in which life-history traits were measured across introduced populations of earwig in North America. The authors examine whether variation in life-history across populations is correlated with aspects of the thermal climate experienced by each population: mean temperature and seasonality of temperature. They find some fascinating correlations between life-history and thermal climate; correlations with the seasonality of temperature, but not with mean temperature. This study provides relatively uncommon data, in the sense that where most of the

literature looking at adaptation in animals in response to climate change has focused on physiological traits [2, 3], this study examines changes in life-history traits with time scales relevant to impending climate change, and provides a reasonable argument that this is adaptation, not just constraint.

References

- [1] Tourneur, J.-C. and Meunier, J. (2019). Thermal regimes, but not mean temperatures, drive patterns of rapid climate adaptation at a continent-scale: evidence from the introduced European earwig across North America. *BioRxiv*, 550319, ver. 4 peer-reviewed and recommended by PCI Evolutionary Biology. doi: [10.1101/550319](https://doi.org/10.1101/550319)
- [2] Kellermann, V., Overgaard, J., Hoffmann, A. A., Fløjgaard, C., Svenning, J. C., & Loeschcke, V. (2012). Upper thermal limits of *Drosophila* are linked to species distributions and strongly constrained phylogenetically. *Proceedings of the National Academy of Sciences*, 109(40), 16228-16233. doi: [10.1073/pnas.1207553109](https://doi.org/10.1073/pnas.1207553109)
- [3] Hoffmann, A. A., & Sgro, C. M. (2011). Climate change and evolutionary adaptation. *Nature*, 470(7335), 479. doi: [10.1038/nature09670](https://doi.org/10.1038/nature09670)

Revision round #2

2019-05-24

Dear Authors, Both reviewers and I thought you have done an excellent job revising the manuscript. Please find below some comments and minor points to address before I can reach a final decision. More importantly, please pay attention to accurately deal with the concepts of plasticity, early experience, canalisation, adaptive versus non adaptive. There are areas in the text that may potentially confuse the reader (see below).

L57: there or they are? L280: "would not be surprising" sounds a little too colloquial. Please rephrase L294 and L351 : there seems to be some confusion with phenotype Plasticity and early life experience – one could argue it really is the same thing. Please rephrase. Thus plasticity cannot be ruled out entirely before a proper experiment with naïve individuals is performed. L397: It's Fabien not Fabrice Aubret

Common garden experiments can indeed be powerful but they are also limited and deceptive. For instance, common garden experiments will not prevent maternal and grand maternal effects to play a role. Further, it's important to not over-simplify plasticity versus rigidity in the expression of traits. It's not often black or white. Some traits may be partially plastic, or the response can vary in intensity and slope, or can only show plasticity after a threshold etc. Also it might be useful to discuss the adaptive versus non adaptive value of plasticity. Mechanistic pathways may generate apparent plasticity, which is not necessarily an adaptation (although it may be picked up by selection if it provides a fitness advantage).

Reviewer #1 I think the authors have done an excellent job revising the manuscript. They may have claimed a little too far in their abstract with regards to "the observed changes in earwigs' life-history traits first emerged as a plastic response to the thermal constraints of the different localities, then diverged between populations through canalization, and ultimately became inherited traits." I am not convinced that their data speak to this pathway. I may be incorrect, but if not, I would suggest scaling this claim back to a speculation in the discussion, rather than a claim in the abstract.

Otherwise, only two minor catches, below. Specific comments L19: delete "in this adaption". L62: should you cite Chevin and Lande 2010 here?

Reviewer #2 suggested no changes.

Best regards, Fabien Aubret

Additional requirements of the managing board: As indicated in the 'How does it work?' section and in the code of conduct, please make sure that:

- Data are available to readers, either in the text or through an open data repository such as Zenodo (free), Dryad (to pay) or some other institutional repository. Data must be reusable, thus metadata or accompanying text must carefully describe the data.
- Details on quantitative analyses (e.g., data treatment and statistical scripts in R, bioinformatic pipeline scripts, etc.) and details concerning simulations (scripts, codes) are available to readers in the text, as appendices, or through an open data repository, such as Zenodo, Dryad or some other institutional repository. The scripts or codes must be carefully described so that they can be reused.
- Details on experimental procedures are available to readers in the text or as appendices.
- Authors have no financial conflict of interest relating to the article. The article must contain a "Conflict of interest disclosure" paragraph before the reference

section containing this sentence: "The authors of this preprint declare that they have no financial conflict of interest with the content of this article." If appropriate, this disclosure may be completed by a sentence indicating that some of the authors are PCI recommenders: "XXX is one of the PCI XXX recommenders."

Preprint DOI: [10.1101/550319](https://doi.org/10.1101/550319)

Reviewed by [Ben Phillips](#), 2019-05-23 04:46

General comments

I think the authors have done an excellent job revising the manuscript.

They may have claimed a little too far in their abstract with regards to "the observed changes in earwigs' life-history traits first emerged as a plastic response to the thermal constraints of the different localities, then diverged between populations through canalization, and ultimately became inherited traits." I am not convinced that their data speak to this pathway. I may be incorrect, but if not, I would suggest scaling this claim back to a speculation in the discussion, rather than a claim in the abstract.

Otherwise, only two minor catches, below.

Specific comments

L19: delete "in this adaption".

L62: should you cite Chevin and Lande 2010 here?

Author's reply:

[Download author's reply \(PDF file\)](#)

Revision round #1

2019-04-02

Dear Dr Tournier and Dr Meunier,

Your manuscript titled 'The successful invasion of the European earwig across North America reflects adaptations to thermal regimes but not mean

temperatures.' has now been reviewed by two referees - whose comments are appended below. As you'll see, both reviewers were fairly enthusiastic about your work and I join them to congratulate you on an impressive study. Both raised a number of significant concerns that you might need to address in a revised version of the manuscript (see below), with which I agree. Amongst those, one reviewer suggested a new angle to position your study within the existant literature. I agree that by doing so, you will reach more readers outside of the biological invasion world towards the climate change community. You should also address concerns about the interpretation of your results as stemming from plasticity (i.e. maternal effects) and/or genetic (static) effects.

Best regards, F. Aubret

Additional comment from the managing board:

As indicated in the 'How does it work?' section and in the code of conduct, please make sure that: -Data are available to readers, either in the text or through an open data repository such as Zenodo (free), Dryad (to pay) or some other institutional repository. Data must be reusable, thus metadata or accompanying text must carefully describe the data. -Details on quantitative analyses (e.g., data treatment and statistical scripts in R, bioinformatic pipeline scripts, etc.) and details concerning simulations (scripts, codes) are available to readers in the text, as appendices, or through an open data repository, such as Zenodo, Dryad or some other institutional repository. The scripts or codes must be carefully described so that they can be reused. -Details on experimental procedures are available to readers in the text or as appendices. -Authors have no financial conflict of interest relating to the article. The article must contain a "Conflict of interest disclosure" paragraph before the reference section containing this sentence: "The authors of this preprint declare that they have no financial conflict of interest with the content of this article." If appropriate, this disclosure may be completed by a sentence indicating that some of the authors are PCI recommenders: "XXX is one of the PCI Evol Biol recommenders."

Preprint DOI: [10.1101/550319](https://doi.org/10.1101/550319)

Reviewed by [Eric Gangloff](#), 2019-02-25 09:07

Tourneur and Meunier present an excellent data set of life-history observations across 19 populations of an invasive insect species across North America. The scope of the data is impressive and have the potential to offer important contributions to our understanding of variation in life-history traits in response to

environmental conditions. In its current form, however, the manuscript significantly oversteps in its conclusions about adaptation to novel environments and contains some important areas that require further explanation or justification. I hope that the two major concerns below, as well as minor concerns and editing suggestions, help to improve the manuscript for publication.

The major story of the paper is that variation in life-history traits is related to differences in temperature regimes and that this represents an adaptation to novel environments in North America. However, no information is presented on the native habitats of this species and how these might differ from those in North America. Such data are necessary to demonstrate that these are in fact novel adaptations rather than responses to climates already present in the native range. At the minimum, it would be useful to demonstrate that at least some of these climates are outside of the native range (for example, is the Montreal climate beyond the range experienced by this species in its native range?). If such a comparison is not possible, then I don't think that this paper demonstrates this response to temperature as an adaptation to novel environments. Nonetheless, the paper is certainly interesting and valuable in demonstrating the covariance of life-history traits with climate patterns.

My second major question is around the conditions in which animals were kept in the lab. Few details are provided other than that these conditions were those of Montreal. So were animals kept outdoors, subject to natural weather variation? Or in climate chambers mimicking natural conditions? Given that this is likely one of the coldest regimes experienced by this species in North America, using this as a common garden condition provokes questions about the observations: the design exposes insects native to different climates to one of the extreme climates. In other words, these results don't represent what insects do in the field in their (newly established populations), but rather how they respond to an extreme condition (Montreal's climate). While this does not invalidate the results, this would seem to indicate that the data do not address the question of variation across climate regimes, but rather response to an extreme climate. In my opinion, this is a major issue with the experimental design that would need to be addressed fully before publication.

In addition to these major points, I offer some small editing suggestions that I hope will improve future versions of the manuscript:

Line 25: The definition of 'thermal regimes' (as presented in line 60) would be useful to include in the abstract as well.

Line 31: There are numerous reviews and studies demonstrating this in insects and other invertebrates as well.

Line 42: Change 'in' to 'to'

Line 47: Change to "British Columbia" (not hyphenated)

Lines 51-52: It would be helpful to provide just a little bit of detail about why this species is considered a pest. What are its negative effects?

Lines 54-55: Change 'drove' to 'driven' (or consider re-wording sentence)

Line 56: Change to "Elderberry" (no hyphen)

Line 63: Change to "life cycle" (two words, no hyphen)

Line 65: The phrase 'depending on the population' is a bit ambiguous. Suggest changing to "with variation among populations" as this is more precise (and I think captures the intended meaning)

Line 113: Suggest changing "grounded" to "lined" or changing wording

Line 127: I don't think 'homogenous' is the word to express this idea. Do you mean that the data are unimodal, indicating a single continuous distribution of traits?

Line 141-142: Without information on the age at collection, I don't think that these observations can be used as a measure of longevity. As detailed later (Lines 280-282), these adults may have hatched at different times, thus confounding any measure of longevity of field-collected individuals of unknown age. While this is acknowledged, the potential to bias results and interpretation is not addressed.

Line 152: There appears to be a word missing after 'a priori' – maybe 'assumptions of data structure'?

Lines 173-175: These methods are unclear. The cbind command adds a vector as a column in a data frame. I don't understand how this relates to this analysis.

Lines 175-176: The meaning of this sentence is unclear. Does this mean that dependent variables were centered around a mean of 0? Where they also standardized to a SD of 1? This requires clarification.

Line 178: What was considered non-significant?

Lines 178-180: Was the FDR adjusted to provide an experiment-wide Type I error rate of 5% ($P = 0.05$)? This is generally the case and seems to be here, but should be specified.

Line 177: Change “residuals normality” to “normality of residuals”

Lines 232-236: This sentence seems to conflate plasticity in response to immediate environments with canalization within a population. The argument is that higher summer temperatures will accelerate physiology, which leads to earlier reproduction. However, the experiment was conducted under common garden conditions, which tests for either canalization or the effect of previously-experienced environments. This point also applies to the conclusions drawn in lines 244-247.

Line 252: The phrase “decreased together with autumn temperatures” is unclear. Suggest changing to “decreased with decreasing autumn temperatures” (I think this is the intended meaning).

Line 298: If the first documented introduction was in 1907, shouldn’t this be “a bit more than a century”?

Line 319: This closing phrase is awkward. Suggest “...to which the present study contributes.”

Table 1, Fig. 1: Change “Ashville” to “Asheville”

Table 2: It would be useful to provide Eigenvalues of each PC as well.

Table 3: The adjustment used for P-values doesn’t seem to make sense. For example, a range of raw p-values from 0.012 to 0.025 all result in corrected P-values of 0.041. Furthermore, some of the P-values don’t change at all.

Reviewed by [Ben Phillips](#), 2019-02-25 09:09

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Author's reply:

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