

Dear Fabien Aubret,

Thanks for your decision on our manuscript entitled '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'.

We are happy to see that both referees and you are still very enthusiastic about our work. We have followed all your suggestions and addressed all comments. In particular, we have edited the text to clarify our use of concepts such as plasticity, early experience, canalisation and adaptive versus non adaptive, as well as followed all the editorial changes that have been suggested.

You can find a point-by-point reply to the comments of each reviewers below, as well as a track-change version of our manuscript attached to this submission.

Sincerely,

Joël Meunier & Jean-Claude Tourneur

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?

It should have been « they are ». Sorry for the mistake. It has been changed (L53).

L280: "would not be surprising" sounds a little too colloquial. Please rephrase

We have changed the sentence into « A plastic response to warm temperatures on egg laying date could be expected in nature:» (L275)

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.

Sorry for the lack of clarity. We have edited this part of the paragraph to better disentangle the effects of plasticity and early life experience, as well as to emphasize that further studies are required to rule out the effect of plasticity.

The new part is: « *However, our results were obtained under common garden conditions, which reveals that the observed effects of thermal regime on egg laying dates are not a plastic response to their current environment, but are either due to the environment experienced during their early life development (i.e. before field sampling), or due to an inherited basis that possibly emerged through canalization*

(Nylin and Gotthard 1998, Van Buskrik and Steiner 2009). It has been proposed that traits tightly linked to fitness are more strongly canalized due to past stabilizing selection (Falconer 1990). Our findings may therefore suggest that the observed changes in the timing of first reproduction and females' reproductive strategy may have first emerged as a plastic response to the thermal constraints of the different localities, then diverged between populations through canalization and ultimately become inherited traits – all this in a maximum of 100 generations. Further experiments with naïve individuals remain, however, required to rule out an effect of early life experience.» (L282-293)

L397: It's Fabien not Fabrice Aubret

Sorry for that. It has been edited. (L389)

L345-367: 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).

It is true that our former paragraph put too much emphasize on the benefits of common garden experiment, without properly referring to their numerous limits (and to their importance in the interpretation of the present results). We have therefore edited the paragraph to better emphasize the limits of common garden experiments and their impact on the interpretation of our results.

The new paragraph is: « *All our results are based on a common garden experiment, a method that is often considered a powerful tool to disentangle the roles of phenotypic plasticity and genetic background on adaptation (Franks et al. 2014, Stoks et al. 2014, Blanckenhorn et al. 2018). Individuals reared under a common environment are typically expected to exhibit homogenized life-history traits if adaptation is the outcome of phenotypic plasticity, whereas they should exhibit population-specific traits otherwise. Our results are in line with the latter process for the great majority of the measured life-history traits (10 out of 13), therefore suggesting that the observed associations between thermal regimes and life-history traits do not stem from a plastic response to their current environment. Nevertheless, common garden experiments often have some limits: they do not prevent maternal and grand maternal effects, they cannot preclude the possibility of genotype-by-environment interactions on the measured life-history traits, and they are poorly efficient at shedding light on the multiple facets of plasticity (e.g. some traits can be partially plastic, the plastic responses can vary in intensity and slope, and plasticity may become apparent only after certain thresholds) (Franks et al. 2014, Merilä and Hendry 2014, Stoks et al. 2014, Bodensteiner et al. 2019). These limits can be particularly important here, as maternal effects and harsh environments shape the nature and outcomes of several family interactions in earwigs (Meunier and Kölliker 2012a, 2012b, Thesing et al. 2015, Raveh et al. 2016, Kramer et al. 2017). Concluding*

on the absence or limited role of plasticity in earwigs' adaptation to North American' thermal regimes would therefore need further empirical works exploring its multiple facets under several common garden conditions (Bodensteiner et al. 2019), and if present, demonstrating the adaptive value of this apparent plasticity.» (L339-360)

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.

That is a fair point. We have edited the sentence to tune down this interpretation.

The new sentence is now « *Furthermore, our use of a common garden setup reveals that the observed changes in earwigs' life-history traits are not mere plastic responses to their current environment, but are either due to their genetic background and/or to the environmental conditions they experienced during early life development.* » (L28-31)

Otherwise, only two minor catches, below. Specific comments

L19: delete "in this adaption".

Done.

L62: should you cite Chevin and Lande 2010 here?

We have followed this suggestion and have added this reference.

Reviewer #2

The authors have extensively revised this manuscript, including a re-framing of the data as an example of potential response to changing or novel climates. This revision is a significant improvement and has addressed my previous concerns. I offer a final set of suggestions below that I hope will aid in improving the published manuscript.

Lines 37-40: The second sentence of this paragraph provides a much more direct introduction to the paper. This first sentence is too broad and provides little context for the study. I suggest cutting this first sentence and beginning with the sentence which now starts on Line 40.

This is an excellent suggestion. We have therefore removed the first sentence of the manuscript to directly start with the second one.

The manuscript now starts by « *The dramatic acceleration of climate change observed over the last decades challenges the ability of resident organisms to track these changes and adapt their life histories accordingly (Meehl and Tebaldi 2004, Parmesan 2006, Williams et al. 2007). Over the last decades, modelling and theoretical approaches ...* » (L37-40).

Note also that neither Parmesan 2006 nor Merilä and Hendry 2014 appear in the Literature Cited section.

Sorry for this mistake (for these references and others in the manuscript). We have now updated the reference list (which did not do after the last series of corrections), as well as double checked that all cited references are present in the literature cited section.

Line 74: The use of “America” is unclear: should be “North America”, “the Americas”, or “the United States of America” depending on which meaning is intended (I think the first).

We have changed « America » into «North America ». (L69)

Line 112: This wording is a bit awkward. Suggest changing “identified to which thermal constraints they adapted to” to “identified the thermal constraints to which they adapted”

Changed accordingly. (L108-109)

Line 166: The “L” after species name is not necessary after the first mention of the full species name

We have removed the « L » here, and checked that it was not present at other places in the main text.

Line 171: change “species-specific” to “subspecies-specific”

Changed.

Line 166-173: It does not seem relevant to mention this genetic division here and then say that it is not taken into account. I suggest removing these lines altogether or, better yet, moving this section to the discussion where the possibility of locally-adapted subspecies/varieties can be explored. Since these genetic differences are not taken into account here, it does not serve the description of the methodology of this study.

This is an interesting comment. We had a deep thinking about where to put this information in the manuscript. We believe that this genetic division is a detail of our study and thus, that writing a full paragraph on this topic in the discussion would distract the readers from our main conclusions. However, we also think that this information is important for researchers aware of the presence of subspecies in the European earwig, and for those interested in comparing life-history traits among subspecies. For these reasons, we came to the conclusion that this information should be given in the material and method section. Hence, we would like to ask to keep it that way.

Line 185: Remove apostrophe from “adults”

Done.

Line 187: Change “age” to “ages”

Done.

Line 218: Change “adult’s” to “adult”

Done.

Lines 219-222: Thanks for the explanation of why the use of `cbind` is relevant here. Nonetheless, I think this information is a bit superfluous. Rather, it would be fine to just say: “In the GLM, the response variable was the ratio of iteroparous females per population, weighted by the sample size of its population...”

Thanks for this suggestion. Using the `cbind` function is statistically different than simply using a ratio (as it does some corrections). We therefore would like to keep the details of the statistical process in the main text. Note that this sentence is nevertheless very close to the one suggested by the reviewer, as it is: *“In the GLM, the response variable was the ratio of iteroparous females per population, which was entered using the command `cbind` in R (to weight each ratio by the sample size of its population) and fitted ...”*. (L215-218)

Line 274: Suggest changing to “both males’ and females’ experimental survival duration” or “experimental survival duration of males and females”

We have edited the sentence accordingly. The new one is: *“In particular, our data from 19 populations revealed that females changed their timing of first reproduction, their reproductive strategy and investment into egg production when facing different thermal regimes, while experimental survival duration of males and females varied accordingly.”* (L266-269).

Line 301: Change “reveals” to “reveal”

Done.

Line 314: As worded, it sounds like this is introducing a new species. It would be more clear to just say “in this species” so that readers know this is the same species studied here.

We have followed this suggestion and edited the sentence accordingly. This sentence is now: « ... it has been recently reported in several *Dermapteran* species, such as the species studied here (Koch and Meunier 2014, Van Meyel et al. 2019) and the maritime earwig *Anisolabis maritima* Bonelli (Miller and Zink 2012)” (L307-308)

Line 325: Change “were” to “was”

Done.

Lines 326-328: Change possessives to plural possessives (i.e., males’)

Done.

Line 334: Change to “adult”

Done.

Lines 365-368: A recent study does exactly this with turtle eggs and may be relevant to cite here as the patterns were surprising:
<https://onlinelibrary.wiley.com/doi/full/10.1002/ece3.4956>

Thanks for the suggestion. We have added this reference to the new paragraph. (L353 & L359)

Line 377: Change to “specific times” or “a specific time”

Changed in « a specific time ».

Line 397: Change “Fabrice” to “Fabien”

Done – with apologies.

Line 383: Note that the Huey et al. 2000 study does not appear in the Literature Cited. Suggest a careful review of citations and Literature Cited section.

We have updated (and double checked) our entire citation list. Sorry for this mistake.