Can intraspecific variation in an herbivorous mite alter responses to drought-stressed host plant? A common garden experiment in the context of climate change

Alain Migeon, Philippe Auger, Odile Fossati-Gaschignard, Ruth A. Hufbauer, Maëva Miranda, Ghais Zriki, Maria Navajas

https://biorxiv.org/cgi/content/short/2021.10.21.465244v1 version v1
Submitted by Alain Migeon 22 Oct 2021 14:56

Abstract
The effects of drought stress on plants and phytophagous arthropods are topics currently extensively investigated in the context of climate change. Dryness not only impacts cultivated plants but also their parasites, which in some cases are favoured by drought. It represents a major challenge that agriculture is facing in a perspective of intensification of drought. Direct effects of drought on herbivorous arthropods typically produce bigger offspring and faster development but attractiveness can also occur. However, how much responses to abiotic factors differ among populations of a species remains poorly documented. The impact of drought-stressed plants on key life-history parameters is here investigated for a major agricultural pest, the two spotted spider mite, Tetranychus urticae, depending on the climatic conditions of the localities at origin. Sampled localities represent a rather wide range of core climate conditions across the mite's native distribution area with contrasting climatic profiles, ranging from wet temperate to cool Atlantic localities to medium to dry hot Mediterranean localities. Plant drought stress effects on mites was estimated by measuring four life history traits: development time, fecundity, sex-ratio and emigration rate in a common garden experiment made of two modalities: well-watered and drought-stressed bean plants. Mites feeding on drought-stressed plants displayed shorter developmental time and attempted to leave leaf patches less often, and young females were more fecund. The mites originating from wet temperate to cool Atlantic localities respond more strongly to drought than mites originating from medium to dry hot Mediterranean localities, suggesting local adaptation of T. urticae populations to various aridity values and indicates that mite feeding behaviour is shaped by the climatic conditions they faced in the area of origin.
Keywords: Acari; Tetranychus urticae; Europe; Mediterranean; local adaptation; common garden experiment; life-history traits

Round #1

---

by Inês Fragata and Raul Costa-Pereira, 04 Dec 2021 18:32
Manuscript: **https://biorxiv.org/cgi/content/short/2021.10.21.465244v1**

Major revisions

Motivated by the current water crisis and severe droughts predicted for the near future worldwide, Migeon et al. investigated how the effects of water limitation on producers scale up to affect life-history patterns of a widespread crop pest, the spider mite *Tetranychus urticae*. The authors sampled spider mite populations (n = 12) along a striking gradient of climatic conditions (>16 degrees of latitude) in Europe. After letting mites acclimate to lab conditions for several generations, Migeon et al. performed a common garden experiment to

quantify how life-history traits of mite populations from different locations respond to drought stress in their host plants.

The manuscript was reviewed by three researchers with a large experience in eco-evo approaches to study insect-plant interactions. Overall, the three reviewers think, and both of us agree, that the manuscript addresses a timely question using an interesting study system. Figures are also clear and appealing; Figure 1 is excellent. However, their assessments also raised concerns about the writing style of the manuscript, methods, statistical analyses, and interpretation of the results. Therefore, we suggest the authors pay careful attention to the valuable comments provided by the three reviewers.

We recommend the authors rework the last paragraph of the introduction to communicate more clearly their a priori predictions on the effects of climate of origin and drought-stress on life history. In addition, we recognize the immense logistic challenges of studying life-history variation across populations, especially considering the spatial scale of the study. However, we can't help but notice that assaying populations at different moments (line 202) adds a relevant source of noise to the results. As noted by all the reviewers, there is not much to be done about this at this moment, other than acknowledging this issue and interpreting results accordingly. Reviewer 3 presents a suggestion to refocus the article on the intraspecific differences in response to drought stress, but using the intra-population variation, instead of between populations variation to drought response.

The three reviewers pointed out that many sentences of the manuscript are hard to follow because of troublesome grammar or structure. Also, all of them noted that the introduction and discussion could be substantially shortened and streamlined. For instance, the plentiful examples presented along the introduction are informative, but also distract the readers from the central message of the paper. We agree with the reviewers' assessment and believe that the manuscript would largely benefit from thoughtful proofreading and revision of text structure, not necessarily from an English-native speaker. Minor comments provided by the reviewers are helpful in this sense. Another consensus among reviewers is the need to change the title, which is long indeed, and should mention life history to communicate more clearly the scope of the study. Finally, as mentioned by the reviewers, several of the figures and tables may be moved to supplementary material, so that the readers can better focus on the main results.
We modified the title to shorten it and add life history traits.
Figure 3 and tables 3... were moved to Supplementary Material to shorten the text.

Reviews

Reviewed by Bastien Castagneyrol, 26 Nov 2021 14:53
Dear Dr Fragata,
Thank you for waiting an extra week before I could send my comments on the paper entitled « Can intraspecific variation in an herbivorous mite alter responses to drought-stressed host plant? A common garden experiment in the context of climate change ».
By comparing key life history traits of mites from 12 populations throughout Europe, the authors found that water stress had an overall positive effect on mite fitness. Interestingly, their results suggest that drought exacerbated inter-population differences in life history traits. There are also evidence for local adaptation of mite populations to climatic conditions.

The authors addressed a nice question, and collected a fair amount of data. However, my general feeling is that the data is underused. The paper is framed in a quite descriptive way. Although decsribing a biological system is always valuable, I believe there is enough material here to make the paper more appealing from an ecological point of view. The introduction and discussions are quite symptomatic of this. I appreciated the fact the authors illustrated their claims with examples, but I felt that often the main idea was diluted by the repetition of examples. I often missed the big question. It was the same with the results section.
Maybe I missed something important, but I understood that the main interest of the study laid in the differential response of different populations to water stress, and in the fact that population-specific response to drought could be explained by climatic conditions in the region populations originated. Statistically speaking, this would be an ANCOVA of the form

Life history trait ~ Drought * Climate

This brings me to another important comment. I understood that populations were tested separately « due to restricted space available » (L202). This surely have important statistical consequences as the effect of « Population » is confounded with the effect of « Date ». But assuming populations were essayed in a random order (which is not explained, but should have been), and not in a systematic order such that the effect of time would have been confounded with e.g. the temperature of areas populations originated from, maybe that's not a big deal. Or at least this is acceptable to test the effect of population properly. Please note that I am a bit puzzled with this last comment as on the one hand the authors explain they ran separate models for each population (which is reflected by e.g. Table 2) whereas on the other hand, the provide F values to test the effect of 'Population' in Table 3.
Repeating an ANOVA for each population is not a recommended approach because the greater the number of tests, the higher the probability to get a significant effect. Also, by doing so, it is not possible to test the effect of population. A clearer description of models that were used would have been useful, maybe with the equation of the model.
In terms of general presentation, I was not convinced that every tables and figures were necessary. In several instances I will list in detail below, information presented in tables could have been moved to the figures. It is a matter of taste and I am very open to contradiction, but now that raw data are made available together with the paper, I don't feel that systematically reporting raw means for every population and every trait, or the raw difference between water treatment is useful. Not if it dilutes the reader's attention and distract her/him from the biggest result.
You surely have noticed that my grammar is sometimes loose so I don't feel qualified to comment on that aspect but there were some sentences I was not comfortable with.
I hope the authors will find some of these comments useful.
Best regards
Bastien

##
Title – I was confused with the word "intraspecific", although it is perfectly correct. I am too far from genetics to be comfortable with the concept of local adaptation, and maybe it is not what was tested here, but "local adaptation to climatic conditions influences mite response to drought" could make it. That way one can get read of the second part of the title, which is a bit long, maybe.
We changed the title to shorten it.
L61 – 'Parasite' is too restrictive, and does not match with 'pest' that pops out next line (L62)
We replaced "parasite" with "plant pest".
L65-89 – This is a very long paragraph that could probably be shortened by referring to synthesis on the effect of drought on herbivory (there are a few meta-analyses on this topic).

The paragraph has been shortened

L96 – it would be great to tell what is the direction of the effect of the 'latitudinal gradient'.

We added main findings.

L107 – To me, it is acceptable to assume that there is no need to demonstrate or illustrate that 'intraspecific variation is common in many organisms'.

The sentence has been rephrased

L110-111 – This sentence give a very descriptive and a-hypothetical flavour to the paper. Yet it is surely possible to elaborate directional hypotheses, should the theoretical context of the paper be elaborated more in the introduction (see above).

We rephrased.

L122 – Why was it important to have green and red forms? The paper has some value even for non-mite people, but they need more information on the model system. Life history traits and their ecological significance are not explained. Generally speaking, I missed a paragraph on the biology of the model species. What is its distribution range. Its host range? What are green and red forms? What are the ecological implications of arrhenotoky or sex ratio in terms of plant-mite interaction?

The model presentation was developed as an introduction to the material.

Table 1 – It is unclear whether 'host plan name' is the host plant the population is specialized on, or the plant the initial mites were collected on. That's not exactly the same.

We changed "Host plant name" to "collected plant".

Note that Table 1 is largely redundant with Figure 1. I would report information in table 1 to Figure 1.

We choose to keep Table 1 because the information presented is important and cannot be added on Figure 1 which is focused on climate. We also added Dryness (GAI) values and categories

L132 – Add 'GAI' after 'Global Aridity Index'

We did it.

L144 – should 'to' be 'onto'?

The sentence has been rephrased

L148 – should it be 'and left THEM oviposit' ?

The sentence has been rephrased

L153 – I understood that SP-I population feeds on Phaseolus vulgaris. This mean that save for this population, all other mites had to face an host shift during the experiment. Could it have influenced the outputs? In a perfect world we would have used at least 3 host plant models, but I understand the technical constraints, one have to be realistic. But maybe the authors could mention this issue?

It has been addressed in the presentation of the model.

L192 – Were the same 10 plants used each time, or was it a random sample? This has consequences in terms of analyses.

Yes we used the same 10 plants, without mites to make the measurements.

L214 – add a ',' after 'sowing'?

The sentence was rephrased.

Figure 3 – I appreciate the authors show there experimental set up. It can be useful for people willing to use the same method (I may well do!), but maybe the figure would be better placed in Sup. Mat. ?

We moved Figure 3 to Supplementary Materials and added explanations.

L246 – As for the green vs. red types, it may be good justifying what why essays were conducted on 3- and 9-days individuals. Only when reading the discussion I approximated the rationale.

Some precisions were added in the presentation of the Material and Methods introduction (L115...).

L262 – Climate of the region populations originated from, or something along that? Also, I did not understood why it was necessary to use two databases. I assume that CGIAR brought something that was missing in worldclim, otherwise it would not have been used, but I don't know what.

We noted the parameters concerned in the text.

L273 and following – Please refer to my general comment on the modeling approach.

All the analysis has been changed.

Please refer also to general response.

L305 – 'a quite wide range of climatic conditions' is fairly subjective. For instance, how was this range compared to that experienced by the species throughout its whole distribution range?

The paragraph has been changed.

L304 – I didn't feel that this whole section and corresponding analysis was much useful, because if I understood well, coordinates on the PC axes were not used. If this analysis only aimed to roughly classify the populations, then it could have been moved to supp. mat. But maybe it can simply be deleted, with no hurt.

We removed all this part. See general response

Table 2 – data in columns 2 and 3 don't seem to have been logit transformed, as mentioned in the caption. Were they?

There has been no logit transformation in tables and figures. We changed the caption and indicated it the text.

L433 – I would have started with summary statistics (same for the next sections).

Note that I do not comment much on the results section as what I indicated so far also applies to other subsections.

We have changed the presentation of our results, adding summary statistics and removing many tables.

L509 – That would be great to have an overall statement of "the big result" here. At first, I thought one could say that "drought had a positive effect on mite fitness", but then I wondered whether if one can interpret the results that way, because there might be a trade off between reduced development time and total fecundity.

We have rephrased and changed the beginning of the discussion.

L522-524 – 'development time is an intrinsic parameter…'. I am not sure what to think about this sentence. When the plant is not stressed, there are no population-specific differences in development time. But drought revealed differences, suggesting that there was some kind of genetic differentiation among populations. The current sentence ignores this result, which is an important one. I admit I have difficulties to rephrase the sentence, but I trust the authors will do a great job (or explain me I am wrong, which is just fine too).

We modified the whole paragraph, trying to make the main ideas expressed clearer.

L535-539 – The two sentences does not flow. The 'for instance' is not appropriate here, as it seems the same idea is repeated.

We have changed and rephrased this part.

L541-547 – As there are obviously many differences between the present study and that by Ximenez-Embun, I don't think this bit is relevant. I would by far prefer that the author bring papers from other systems to generalize their results instead of compare very precisely their number with that of other papers.

We suppressed a sentence (L542-L547).

L566 – I don't think that the phrasing 'the hypothesis tends to…' is appropriate as the above sentence is not really an hypothesis. Also, because the above sentence is based on the literature, it is quite normal that it is supported by the literature. Maybe consider rephrasing?
We rephrased the sentence.
L572 – 'feeding' instead of 'exposed to feed' ?
We did change "exposed to feed" to "feeding".
L579 -580 – I understand the idea, but the present sentence is wrong, as it implicitly suggests that the mites – the individuals – experienced different conditions. This is true for the population, not the individuals.
We replaced "Mites" by "Populations".

Reviewed by anonymous reviewer, 18 Nov 2021 23:08
Please find below my review to the MS titled: "Can intraspecific variation in an herbivorous mite alter responses to drought-stressed host plant? A common garden experiment in the context of climate change" (2021.10.21.465244v1.full).
First I must say I found quite hard to read the MS as it is, most probably due to English issues. Although it is not my native language I find many sentences are not well written making some ideas not clear. I strongly suggest the author(s) to improve English with the help of a native speaker. Manuscripts like these are so much time consuming.
This research aims to evaluate the role of the geographic origins of mites in their response to climatic dry conditions in four life history traits: development time, fecundity, sex-ratio and emigration rate.
As important comentaries:
- I found the title is incomplete. One does not understand what kind of intraspecific variation on herbivorous mite authors are talking about. I would change it to: "Can intraspecific variation in IN LIFE HISTORY TRAITS OF an herbivorous mite alter responses to drought-stressed host plant? A garden experiment in the context of climate change".
We modified the title to shorten it.
- The MS needs the revision of an expert researcher from their laboratories and from a native English speaker.
The MS has been revised by an expert researcher and by a native English speaker.
- Considering Methodology: these mites come from different places and authors put them in chambers with the same t° and RH for their experiments. They do not explain if the selected t° and RH values represent the average from all the collection places. They need to explain how ad why did they choose these particular values. ± 10 % RH means a range of 50-70 % which I consider it represents a large RH variability that could be representative for mites behavior. How controlled were their experimental chambers is not explained. It is not clear for me either what are the treatments tested here. Finally, I ask myself what would be the consequences of doing so considering mites are naturally selected to different developmnetal conditions (even 6 generations later)? This is not measured. Any literature to support it?
RH in our climatic room is only down-regulated to avoid too much moisture which could be the main issue for spider mite rearing. The regulation of soil moisture allowed us to obtain contrasted values of stress markers for the two water regimes. The main goal was to test plant water stress on mites. In our opinion, despite variations of RH which are unavoidable, if only because of variations in the external environment, the experimental conditions allowed us to assess impact of water regime applied to host plants on mites. The parameters used correspond to "standard" values for rearing T. urticae. Laboratory selection of mite lineages has been reviewed by Sousa et al. (2019). We believe that the time chosen represents a balance between maternal effects and laboratory selection of lineages.

- In the rearing plants methodology, again, ± 20 % RH variability range means plants were reared between a 30-70 % RH which I find enormous! and potentially harmful for the experiment.
Here again ± 20 % RH in a glasshouse used for plant growing is a normal interval unless using very costly material over dimensioned for such experiments. Indeed, the experiments were not conducted in the glasshouse but in experimental rooms. Only first stage of plant growing occurred in the glasshouse.
- The total number of Tables and Figures in the Manuscript must be reduced / synthesized . Half of them must be chosen as Appendix for Supplementary Material.
The results part has been rewritten. Tables and figures changed or removed.
- Discussion is quite hard to read. On the one hand I may synthesize important ideas to make them clearer, on the other hand I may go deeper into other ideas such as the role of the different genotypes that could be explaining their results. Then in a Conclusion sub-part I will put some recommendations for further studies.
Discussion has been changed in accordance to the recommendations.
Please find below a line by line revision of the manuscript.

TITLE
Importantly, I found the title kind of incomplete? One does not understand what kind of intraspecific variation on herbivorous mite you are talking about. I would change it to: "Can intraspecific variation in IN LIFE HISTORY TRAITS OF an herbivorous mite alter responses to drought-stressed host plant? A garden experiment in the context of climate change"
We modified the title to shorten it.

ABSTRACT
L25-L29: Difficult to understand. For example, what is the relationship between arthropods' bigger offspring and faster development with attractiveness. Furthermore, of what kind of attractiveness are you talking about? Don't get it.
We deleted the reference to attractiveness from the abstract.
L31: What do you mean with: "depending on the climatic conditions of the localities at origin"? I don't think "depending" is the correct verb? Which localities at origin are you talking about?
The abstract has been partially rewritten.
L31-L37: Needs English review by a native speaker. I am not sure if I completely understood the message here.
The abstract has been partially rewritten.
L38: What do you mean with "leaf patches"? This must be explained somewhere before.
The abstract has been partially rewritten.
L39: What does "respond more strongly" means? More strongly on displaying shorter developmental time? attempting to leave leaf patches less often? young females more fecund? sex ratio? emigration rate? All of them? You must be explicit.
L39-42: We rephrased all this part and cut it in two sentences.
L41: Replace "aridity values" by "aridity levels" or "aridity ranges".
The abstract has been partially rewritten.
L41-L42: Results actually indicates that mite feeding behaviour contrasts with the climatic conditions they faced in the area of origin, isn't it? If I understood well.
Yes, it is correct.

KEYWORDS

L45: KEYWORDS, in plural
Corrected.
L47: "common garden" what does that mean? Backyard garden?
The keywords have been changed according to the new version of the manuscript
L47-L48: Please put keywords in alphabetical order.
Corrected.

INTRODUCTION
L52: I may replace "that was the title of an article" by "warned The New York Times magazine in ..."
We did it.
L52: Now, why use this article having hundreds of scientific appers telling the same thing? I would understand if it is something unique from investigation journalism, but this is not the case.
We agree with the reviewer. Using this article at the beginning of our paper seems important to us as it connects and replace research in a wider societal concern.
L54: REFs at the end of the sentence.
L56: REFs at the end of the sentence.
We deleted L54-56 that were redundant with the following sentence.
L56-L57: Put Mitlin et al. 2019 at the end of the sentence.
It was done.
L60: Water use will increase how much? Need numbers here, and REFs supporting them.
L62: More REFs here?
L63: REFs at the end of the sentence.
References have been added and the paragraph rephrased. The last sentence is the synthesis of the paragraph and ref are already provided to assert to the two facts.
L66-L67: How drought affects plants physiology?
It is in the reference quoted. We do not develop because it is not the subject of our study.
L68: What kind of changes in the amino-acids and free sugar balances were found in drought-stressed plants?
It is in the reference quoted. We do not develop because it is not the subject of our study
L93: "as a main way"
"a" was added.
L96: "... localities." Period missing here.
A period was added.
L107: I would be more modest. I would say "...intraspecific variation seems to be common for many many organisms in drought conditions"
The paragraph has been changed.

MATERIAL AND METHODS
L118-L119: I may put this sentence at the beginning of this paragraph, start with the main actor.
The paragraph has been rephrased.
L121: Sounds weird.
We changed "Mite material" to "Mites".
L122: OK, those green and red forms must be described in the Introduction. This is the first time you mention it. And moreover, what is the differene bewteen both in terms of physiology, metabolism etc?
We better described the mites in the first part of Material and methods.
L126: medium hot or medium wet? Please explain.

We changed to "wetter summer".
L142-L143: These mites come from different places and you put them in chambers with the same t° and RH. Are these average values from all the collection places? How and why did you choose these particular values? ± 10 % RH means a range of 50-70 % which I consider it represents a large RH variability that could be representative for their behavior. What are the treatments tested here? This is not clear for me. And finally, what would be the consequences of doing so considering they are naturally selected to different developemental conditions (even 6 generations later)? How do you measure this? Any literature to support this?
We explained better our choices in the text.
L144-L150: Why not to explain all this directly here? It would be easier to understand at once.
We agree that our experimental set-up description was confusing with doubloons. It has been revised to make it clearer.
L158_ Seeding time? Sorry don't understand this? Seedling?
We corrected to "Seedling".
L166: Again, ± 20 % RH means a variability range of 30-70 % RH which I find enormous! and potentially harmful for the experiment.
It is not possible to fix more precisely the RH in our greenhouse. It is only to produce the plants and not for the experiment itself (see previous comments).
L202-L205: Redundant. Delete. This was already said.
We kept this part here but deleted the details in the previous paragraph (L162).
L257-L259: How many generations in total?
There is just one generation.
L262: "World"
Corrected.
L286: Does the residuals followed a normal distribution in order to use this parametric test?
L297: Does the residuals followed a normal distribution in order to use this parametric test? ... and date meet at least half the ANOVAs assumptions? Particularly homoscedasticity of the variances.
L286-297 we provided responses to this concern in our general response.
RESULTS
L306: Where can you say they develop the better?
This part has been removed.
L316: OK, but I don't get the idea of taking mites from all these places to put them in one "same" t° and RH chamber, and test up to 6 generations the effect of drought in their life history. Based on the title of your work you expect variability from genotypes?
Our study was done after six generations and not during them. We do expect variability from the genotypes. See above for our explanations
L412: Replace "greater" by "larger".
We did it.

DISCUSSION
Discussion is quite hard to read. On the one hand I may synthesize important ideas to make them clearer, on the other hand I may go deeper into other ideas such as the role of the genotypes that could be explaining your results. Then in a Conclusion sub-part I will put some recommendations for further studies.

L509-L510: What? Rephrase.
We corrected the whole first paragraph.
L520-L522: Don't understand this first part of the sentence. Rephrase.

We rephrased the first part of the sentence.
L518-L532: Very hard to read and understand the central points. Try to synthesize the ideas here.
We modified the whole paragraph, trying to make the main ideas expressed clearer.
L542: "seen by" not "seen in"
We corrected this.
L581-L589: This sounds interesting. Could you develop the mechanisms here?
We agree with the interest of a such study. This aspect corroborates our results but it was not the core of our experimental study so we don't have materials, i.e. transcriptome analysis to go further.
L594: "for an example"? You mean "for example"?
We corrected this.
TABLES AND FIGURES
You will be asked to reduce / synthesize the total number of Tables and Figures in your MS. Half of them should be chosen as Appendix for Supplementary Material.
Figure 3 was put in Supplementary Material, as well as tables.
Table 1 lacks acronyms explanations. Please fill that in the legend.
We added the signification of DD and UK in the legend.
Figure 1 is interesting indeed, however the italian spot does not represent what in L125 says: "correspond to places with dry hot summers" which can be seen for the rest of the Country. In the end you have mites from 6 vs 4 different kind of summers (and not 5 vs 5 as it pretended to be). Is this important for your treatments?
Italia and Spain are intermediate as it is now clearly stated in the text.
In total, we have 12 populations (4 dry, 4 wet and 4 intermediate). These categories were used for analysis.
Figure 3. Legend lacks information about the picture's elements. It might not be considered for the main MS but as an Appendix in SM instead.
We moved Figure 3 to Supplementary Materials and added explanations.
Table 2 legend lacks information. Please fill it.
We clarified the legend.
Figure 5: Nice fig.
Thank you!

Reviewed by anonymous reviewer, 26 Nov 2021 17:57
Review of the pre-print entitled: "Can intraspecific variation in an herbivorous mite alter responses to drought-stressed host plant? A common garden experiment in the context of climate change"

In this manuscript, the authors characterized the intraspecific variability in the response of an herbivorous mite to drought-stressed host plants and aimed to assess if climatic differences in the geographic origin of mite populations explained the variability in their response. This was performed by sampling mite populations on different locations of a climatic gradient and, after 6 generations of acclimation to laboratory conditions, testing life-history traits of such populations on drought-stressed and control bean plants. The authors also assessed differences in dispersal attempts of all populations from drought-stressed and control bean plants.
Climate change affects plant-herbivore interactions, via changes in temperature, extreme drought-events, among other factors. Such effects may have important consequences for the management of crop production and control of crop pests. Recent work from several authors has focused on the effects of drought on different plant-herbivore systems, including herbivorous mites. Here, the authors, add relevant knowledge on intraspecific variability for

the response of herbivores to drought-stressed plants, assessing herbivore populations that were sampled in a climatic gradient having, therefore, experienced differently the effects of climate change.
The main short coming of this work, in my view, is that each population was assessed in separate moments (line 202), being impossible to disentangle if the observed differences among populations are derived from their genetic background, which may be linked to the climatic characteristics of their sample location, or from confounding effects pertaining from uncontrolled/unidentified differences between the experimental blocks. I understand the logistic limitations of performing a study with this number of field-collected populations, however this issue could have been solved if the 12-15 replicates per experimental treatment of each population were divided among experimental blocks containing many populations. Nevertheless, and being impossible to tackle this issue à posteriori, in my point of view, the information provided by the differences in life-history traits between drought-stressed and control plants for each population is very relevant for this research area. Considering intraspecific variability in the response of herbivores, whatever the cause, is key to the development of pest control strategies and to understand and predict the effect of climate change on plant-herbivore interactions in general. With this in mind, I highly suggest that the authors focus the scope of this manuscript on these intra-population differences, keeping the discussion of link between the differences in climate among the geographic locations of the samplings and the observed differences in life-history traits of this herbivorous mite as a possibility.
Another issue regarding the analyses of the results is that on experiment II the authors used 3-day old and 9-day old females to assess life-history traits. Even though, as I understood, both type of females were used on drought-stressed and control plants, they were used in different experimental blocks (line 246). If this is the case, I believe that is important to present the result for 3-day old females and 9-day old females (as the authors did) to show other types of intraspecific variability. However, I would not compare the results from females with different ages. If I understood it wrong, and the 2 batches of plants (referred to in line 246) were used at the same time, please clarify this in the text and ignore the rest of this comment.
The same batch of mothers was used to produce the 2 batches: one of 3 day- and one of 9 day-old females. Each was tested on a different batch of plants but always aged of 13 days after sowing.
Other then these two main issues regarding the analyses of the results I only have a few minor comments that I mention below:
C1: Regarding the title: In my view it is not intraspecific variation that alters the response of herbivores to drought-stressed host plants. I think that the question is "Is there intraspecific variation for…"
We modified the title to shorten it.
C2: Line 26-28 I don't understand this "…but attractiveness can also occur". Attractiveness of the herbivore offspring? Of the plants? Could the authors please clarify?
We deleted the reference to attractiveness from the abstract.
C3: In my view the first paragraph is too long and not directly linked to the main message of the manuscript. The sentences between line 52 and 60 could be summarized in one sentence.
We shortened the first paragraph by maintaining only its main aspects.
C4: line 107. This sentence is very broad, yes intraspecific variation is common in many organisms. Can the authors specify and maybe link this sentence to the previous paragraph?
We rephrased the paragraph.
C5: table 2: Df is not reference is degrees of freedom, please clarify this in the legend of the table. And also, where do the 8 degrees of freedom come from? Weren't there 12 to 15 replicates? (line 225)

Df is for Degrees of freedom as we have now specified in the legend.
There are 10 points of comparison (time) minus two conditions (= 8 Df). We added this in the legend of the Table 2.