Dear editors and reviewers

Once again, we thank the editors Inês Fragata, Raul Costa-Pereira and the anonymous reviewer for their work, comments and suggestions. We believe we have addressed all of them and to justify our choices we respond to each comment below. We also added all the necessary information regarding data and script access, funding and conflict of interest

We appreciate the authors' efforts to make the manuscript more balanced and to communicate the results more clearly and precisely. We would like to emphasize how the current version recognizes the study's methodological limitations, which we strongly believe is a positive aspect. As mentioned in previous reviews, some statistical analyzes could be more elaborate, but the current body of analyzes is not problematic to the point of compromising the interpretation. We would be happy to write a recommendation, after the authors address some minor points raised by the reviewer and ourselves.

Here I find a clear and streamlined version (v3) of a very interesting manuscript which will make a valuable contribution to the field. The authors addressed the comments made to the previous versions, clarifying the major issues pointed out.

Following, we address the main comments and corrections in order of the text

Given the previously mentioned limitations of the experimental design, I suggest a title implying less causality, for example: “The response to drought-stressed host plants varies among herbivorous mite populations from a climate gradient”.

We agree that the proposal implies less causality and we have changed the title to the one proposed here.

Line 46. Maybe a short sentence linking drought to production in field crops is missing here.

We have added a new part in the sentence, regarding crop yields, which now reads “Low water availability not only impacts cultivated plants by enhancing crop yield vulnerability to climatic events (Olesen et al., 2011) but also plant pests, which can be favored by drought (see Hamann et al. (2021) for a review).”.

Line 85. Do all these traits decrease with latitude? What is a decrease in sex-ratio? Moving towards male or female bias? Could the authors clarify how sex-ratio and dispersal change with latitude?

We have clarified the sentence, indicating the increase in male proportion and in dispersal, which now reads “Among the tetranychid mites, common garden experiments reveal a latitudinal gradient of life history traits (decrease in fecundity, shortening development time, increase in male proportion and in dispersal) from Western European core distribution of *T. urticae* to the northernmost part of the distribution area (Van Petegem et al., 2016).”

Line 119. Replace “Mites” with “Study System” and merge “Origin of mites” in this subsection. The next subsection “Plant material” could be renamed to “Host plant” or simply also included as a paragraph in “Study system”

We have changed the structure of Material and methods in a way we consider better. However, we did not merge mites and plants in a "Study system". In our point of view, keeping them separate makes easier structure, allowing to develop the basic knowledge of the mite useful to understand the herbivorous model, the core of the study.

The structure is now:

1 Mites

Biological features (added header)

Origin of mites

Mites rearing (moved here from a separate header)

2 Climate data

3 Plant material (just before experimental design)

4 Experimental design

5 Data analysis

Lines 303-306. What is the response variable in these regressions? The slope of the response of each population to drought conditions? Please clarify.

We have rephrased the sentence, explaining the variables used in the regressions, which now reads "We used linear regressions to explore how the climate of origin affected the magnitude of the differences of the response in the life history traits between watering regimes.".

Figure 5. Avoid using plot titles in all the panels.

We have redrawn the figure 5.

Lines 481-484. This is assuming that there is no other fertilization event, right? Maybe the authors could refer here to the first male sperm precedence pattern characteristic of this species or simply refer to a single fertilization event.

Indeed, there is no other fertilization event. We clarified this point and added a reference. "This phenomenon could be interpreted as a trade-off, but it could also be, more likely, the result of a decrease of sperm availability as a result of the effectiveness of the only first mating in spider mites (Helle, 1967), in nine-day-old females on drought-stressed plants as a result of the higher production of females in three-day-old female progeny (Krainacker and Carey, 1990)."

Lines 507-509 and 544-545. Alternatively populations from dryer climates may have adapted to those conditions, and, in the absence of costs, life history traits do not differ as much from those in control conditions in contrast with non-adapted populations. Although this alternative is not possible to test with the experimental design of this study, in my view it would be interesting to mention it in the discussion.

We agree that this point deserved to be mentioned and we addressed it in the conclusion, adding a sentence after the following one: "Our results, although not proving it, do suggest that the "over-reaction" of the French and British populations could have a cost, which might limit the response to drought of populations from dry locations." Added sentence: "However, populations from drier climates may also have adapted to those conditions, and, in the absence of costs, their life history

traits do not differ as much from those in control conditions in contrast with non-adapted populations.”.