Review of the manuscript entitled “Climate of origin influences how a herbivorous mite responds to drought-stressed host plants”, a third version of the pre-print “2021.10.21.465244”.

I apologize to the authors and the recommenders for the delay in this review.

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.

As such, I only have a few extra minor comments:

C1: 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”.

C2: 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?

C3: line 277 – library

C4: 303-306 – What is the response variable in these regressions? The slope of the response of each population to drought conditions? Please clarify.

C5: 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.

C6: 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 teste with the experimental design of this study, in my view it would be interesting to mention it in the discussion.