"*Individual differences in developmental trajectory leave a male polyphenic signature in bulb mite populations*" is an interesting study that examines the consequences of harvesting juvenile stages of different conditions on eco-evolutionary population responses such as population size and expression of male morph types. The authors first give a background on what kind of selection pressure act on anticipatory vs. mitigation developmental plasticity and how the expression of male morphs could be differentially affected under each case. To examine how harvesting different percentages of juveniles versus deutonymphs affect the expression of male morph type and whether the observed plasticity in the populations could be due to anticipatory/ mitigation development plasticity, lab-adapted bulb mite populations were put under five treatments for 302 days, (i) D100 treatment (100 percent of the deutonymphs removed), (ii) D50 treatment (50 percent of the deutonymphs removed), (iii) J-D100 (same percentage of juveniles removed as D100, barring eggs and deutonymphs), (iv) J-D50 (same percentage of juveniles removed as D50, barring eggs and deutonymphs), (v) C (controls, no individuals were removed), and population size counts and fighter morph proportions were examined in each treatment. Authors found that in D100, D50, and J-D100 populations, the juveniles undergo mitigation developmental plasticity as the expression of the fighter male morphs is suppressed. Authors suggest that juveniles mitigate the mortality risk under these conditions by reducing the investment in development and developing into a defenseless scrambler male. The D100 and D50 populations also evolved a higher population size as compared to the other harvested populations and it was suggested that a higher proportion of scrambler males in populations has led to an increase in mean population size. The adult body and deutonymph size variation were also measured at different time intervals to examine the mean shift in body size as the experiment progressed, and in the end, a life-history assay was conducted in all the treatments to examine if the observed differences in the morph expression were genetic or plastic. The results on morph expression being plastic or genetic were not conclusive.

I think it's a well-conducted study, the results are intriguing, and the experiments undertaken are laborious. The manuscript is also quite clearly written with the focus largely being on the problem that is being addressed and the results obtained. However, I feel there are some critical points that are missed while interpreting the results. I have a few suggestions that would be helpful for the readers and may clarify a few points.

Introduction

Line 64: It is not clear what "condition distribution" is in this context. Is it the distribution of fighter/scrambler morph proportion, food availability, or the presence of metabolic wastes in the food?

Authors may add a few lines explaining what kind of population responses are expected in anticipatory or mitigation developmental plasticity after line 67 which will help the readers to understand the expectations in each case.

Line 83-84: The working assumption that only juveniles of the good condition develop into deutonymph and all male deutonymphs molt into fighter males seem a bit farfetched given the

sample size in the Deere et al. 2015 study was just 11 for deutonymph males, and most of the deutonymphs molted into females.

Methods:

Line 124: Typo in the spelling of and.

Line 135 onwards: The following sentences are not clear as to in which proportion the other juvenile stages from the J-D 100 and J-D 50 were removed. Is it the proportion of deutonymphs in the D100 and D50 treatment and 100 or 50 percent of those in the J-D 100 and J-D 50 treatment for that particular generation or it is 100 or 50 percent of the total deutonymphs present in the population from which the juveniles are removed?

Even in the datasheet in the tab R population counts, it is not clear in what proportion other juvenile stages are removed. For example, in column 150-151 the total number of juveniles removed is neither a reflection of 50 percent of the current population's deutonymph number or 50 percent of the deutonymphs from the OA05 populations. Same is true for the other columns.

Additionally, I didn't see a column for the larvae that were removed in the OAC1 and OAC05, so it is not clear whether the larvae were removed or not.

Line 129: It would be important to state what proportion of deutonymphs are seen in the regular maintenance of these populations, thus the readers could get an idea of the relative magnitude of juveniles being harvested from the population.

More importantly, is the proportion of juveniles big enough to induce a stress response in the populations to express the mitigating developmental plasticity? I ask because as I see in the datasheet, the proportion of deutonymphs removed from the population is small as compared to the total juveniles present in the population at that time. I am skeptical about how this small percentage of removal of juveniles from the population is going to induce a plastic response in the population. Authors may add a line or two on why they think this proportion may be consequential.

Line 137&139: Typo: it should be 'were' instead of 'where'.

Results

Line 233: The authors say "Mean total population size had stabilized across all treatments at the start of harvesting (Fig. 2)". However, the mean total population size does not seem to have stabilized since the population size is still increasing for D-100, J-D-50 and control populations before the harvest began. Visually, it seems there is ~15-20% growth after the harvest started which is non-trivial. If population sizes are not stable or have not reached the equilibrium population size before the harvest began, there will be an interaction between the population growth rate and the harvested proportion of juveniles to influence the equilibrium population size, which may not represent the consequences of just harvesting on the population size. The authors need to address this issue in Discussion and Methods or find a way to control for the effects of such growth in their analyses.

Line 246: It is unclear why the proportion of fighters differs between the time periods. In the D-100 treatment, the proportion of fighter does not change at all time periods but in the D-50 treatment the proportion of fighters declines only in the last time period when the population size seem to have increased for D-50 (Fig.2).

Does the population size increase significantly in the last time period to influence the expression of the fighter morphs in D-50?

Line 291 onwards: I will suggest to not make inferences when the statistical differences are not significant, especially when the sample size for scrambler males is really low.

I think one of the important interactions that is not plotted is the total population size for all five treatments in different time periods (1, 2 and 3 which were used in analysis). It would be an important factor to know how decreased proportion of fighters would increase the population size. If the decrease in fighter proportion increases the population size then shouldn't the J-D 100 population should also have an enhancement in population size like D-50. This plot can be added to supplements if the authors think it makes the main article lengthy.

Discussion

It is important to highlight how an increase in population density could influence the expression of fighter morphs. Previous studies have shown that an increase in population density could lead to the suppression of fighter morphs, and as the population size is the highest in the D-100 treatment the exuding pheromones from this population could lead to decline in the overall expression of fighter, since the population size is the highest in the D100 treatment.

Another thing that I think is missing in the discussion is how the removal of deutonymph leads to removal of a higher number of potential females which could also affect the overall skew in the population sex ratio and how that would have affected the population size differently for D100 and D50 treatment. The removal of potential females could affect the egg output in the population and perhaps lead to a lower intensity of density-dependent selection as opposed to a condition when the female number is not altered leading to an increase population size.

Line 350 onwards: The explanation regarding the expression of more deutonymphs due to reduced olfactory cues seems unreasonable, because if the lack of deutonymphs affects the concentration of olfactory cues then the number of deutonymph should not be changing over the time periods for D100.

Line 380: I will suggest the authors to replace the term "carrying capacities" with "equilibrium population sizes" because when populations approach equilibrium, at that point due to higher population growth rates, the populations tend to overshoot the maximum sustainable population size.

Figures

Figure 3: Keeping the Y-axis same for 3A and 3B would be helpful for easy comparison.

Figure 4: Keeping the Y-axis same for 3 B, C and D would be helpful to compare the body sizes between males and females. Also, plot the Fig. 4 A, C, and D similar to 4 B, so that the consistency allows readers to compare the mean shift in body size for all the treatments and how that could have affected the mean population size or vice versa.