Peer review on manuscript

"Multiple cues favor [redacted] female preference in [redacted]"

by Peer 407

ADDED INFO ABOUT FEATURED PEER REVIEW	This peer review is written by an anonymous Peer PEQ = 4.6 / 5 Peer reviewed by 3 Peers

Introduction

Revision Recommendations	
Question:	Minor
Data:	Accept
Methods:	Accept
Inference:	Major
Writing:	Minor

The manuscript [1] presents an analysis of mating preferences in [redacted]. Males occur in four morphs that differ in [redacted], but also in their behavioral strategies. Females occur in three color morphs. The authors report that [redacted] females prefer [redacted] males, both in trials where females were presented with live males and when exposed solely to male chemical cues. Assortative preferences for [redacted] might facilitate the reproductive isolation between morphs and might therefore promote speciation. The authors also interpret their results as evidence for [redacted] preference, but I do not agree on this interpretation (see below).

Besides this aspect of assortative mating, the study addresses preferences for other male traits that vary continuously rather than in discrete morphs. The authors call these traits 'monomorphic' traits and postulate that mating preferences based on such traits are less likely to lead to rapid speciation. [redacted] female show a preference for males with [redacted], but the [redacted] preference seems to be less important than morph identity. [redacted] females do not show statistically significant preferences based on any of 7 morphological and color trait.

Overall, the study thus gives evidence for mating preferences conditional on [redacted] and multiple messages in mate choice of [redacted].

Merits

This is an interesting study and a well-written paper. The study is relevant beyond [redacted], because the central questions about the strength of assortative versus disassortative mating preferences [2] and whether different traits signal redundant or independent pieces of information [3,4] are of general relevance. The study has been carefully designed and the presentation of the methods and results is clear and easy to follow. Among other things, the study shows that [redacted] females prefer [redacted] males even in the absence of visual signals based on chemical cues alone. Hence, chemical cues seems to be correlated with [redacted] and thus seems to function as a backup signal for [redacted] and implicitly also for the behavioral strategies. The evidence for assortative mating preferences of [redacted] females is also intriguing and relevant to our understanding of discrete polymorphisms.

Critique

The manuscript is well written and the background on sexual selection theory is also interesting and relevant. Nevertheless, I have four general comments:

First, I am surprised about the use of 'monomorphic' versus 'polymorphic' traits. Since traits like [redacted] vary within the populations, it sounds counter-intuitive to call the trait monomorphic. The main difference seems to be between discontinuously (discrete) and continuously varying traits that are polymorphic within the population. The authors use the discrete versus continuous terminology in some places, but I would suggest holding this through the manuscript.

Second, I am not convinced that the correlation between [redacted] and [redacted] is informative with respect to the origin of [redacted] or about the mode of selection. Even if [redacted] correlates with any other trait, this is not necessarily evidence for correlational selection. I think what is lacking is a discussion of the possible modes of inheritance. It is possible, if not likely, that traits that vary in discrete classes are controlled by few major genes. Co-adapted gene complexes might be kept together by the mechanism of genetic inheritance (physical linkage, possibly even genomic inversions) rather than by correlational selection. Some genes influencing polygenic traits (such as [redacted]) might by chance be associated with the [redacted] gene(s). Hence, if there are discrete color morphs I find it not surprising if there are average differences with respect to all sorts of traits. The correlation alone is not informative about the underlying patterns of selection or about the origin of traits from a discrete polymorphism or from continuous variation.

Third, I don't agree on the conclusion of context-dependent mate choice that is also featured in the title. The 'context' in this study is the combination of male morphs available in mate choice trials, but the critical tests for differences in preferences among contexts (or among females morphs) is lacking. The contrast between a significant finding and a non-significant finding is not sufficient for

concluding that the two results are significantly different from each other! From eye-bowling Tables 2 and 4, I think that there is no context-dependent preference at least not in [redacted] females: They always prefer [redacted] males even if the different is statistically significant only in comparison to [redacted] males. The results would be completely consistent with hierarchical mating preferences in [redacted] females (primary based on morph, secondarily on [redacted]) and even [redacted] females show similar trends in the visual choice trials.

Fourth, I don't agree on the conclusion that [redacted] is monomorphic in the sense used in the manuscript. The correlation presented in Figure 4 is pretty strong and the text also states average differences among morphs. So it might not be a 1:1 match, but clearly [redacted] and [redacted] are correlated. The similarity in the coefficients of variation (L283) does not tell much.

Discussion

The manuscript is certainly very interesting, very well conducted and of general relevance. Some changes to the introduction and the discussion might make this a very convincing and important paper. In particular, I think the authors should consider and discuss patterns of inheritance rather than focusing all the theory and interpretation on correlational selection. Furthermore, with the evidence presented in the here, the manuscript would benefit from a more detailed discussion of possible mechanisms and prior evidence for the maintenance of the [redacted] polymorphism in the population (and indeed in many other [redacted] species). Finally, the authors should provide sound statistical evidence for context-dependent mating preferences or change their focus accordingly.

References

[1] Anonymous authors (2013) Multiple cues favor [redacted] female preference in [redacted] (unpublished manuscript) - Peerage of Science

[2] Jiang, Y. X., Bolnick, D. I. & Kirkpatrick, M. (2013) Assortative mating in animals - Am. Nat. 181: E125-E138.

[3] Johnstone, R. A. (1996) Multiple displays in animal communication: 'backup signals' and 'multiple messages' - Phil. Trans. R. Soc. Lond. B 351: 329-338.

[4] Candolin, U. (2003) The use of multiple cues in mate choice - Biol. Rev. 78: 575-595.

Additional comments for authors

L36-37: I think this hasn't been shown. It could be a hierarchical decision with morph ([redacted]) being more important than [redacted]. I don't see clear evidence for social context modifying preferences.

L38-39: I do not follow this conclusion. Does this imply that they cannot discriminate between [redacted]? It seems that they simply accept [redacted] sometimes, so they don't have a simple on/off preference for [redacted].

L44: Part of the sentence missing.

L59-60: I find the use of 'male genetic differences' a bit unusual (here and further below). It seems to refer only to genetic difference for the locus/loci that determine [redacted]. But surely there are genetic differences for other traits as well, even if they vary continuously.

L52: Full stop missing.

L66: Reference missing.

L79: flexible = conditional? I think this manuscript is really about preferences that are conditional on female morphs and not about flexible preference. I simply don't follow the conclusion that the results show context-dependent preferences.

L178: Space missing.

L180-181: Did you ensure that males participated in the same number of trials? Was there any evidence that males became more successful in attracting females the more trials they had? This might have influence the results.

L191: Redundant words.

L194-195: Why did you check female position only every 30 min? Could you provide arguments that this is sufficient?

L283: B, Y and O as subscripts.

L306-307: I understand what you mean, but I think the sentence could be rephrased for clarity.

L427: I don't agree that [redacted] are a precondition for speciation.

L578: What does '20' say? Table 2 and Table 4: The table should also show N, the sample size. Or - and this would be my preferred version - the columns "Chosen male" should show the number of

cases rather than the percentage of the traits. Table 3: Why are the main effects of status and treatment missing?

L636 also lacks the p value. Figure 1: Shouldn't the white and grey bars from each pair of bars add up to 100%? For example, the exceed 100% in the left part of panel 1a.

L742: As far as I understand, the plot shows trait means (and not trends).

Figure 4: Why aren't there three groups for the [redacted] score? This trait should vary discontinuously. It would also be nice to use separate symbols for the three morphs.

Final remark: I think ten days are too short for a thorough review. Two weeks should be the minimum.