

**BEAS-D-22-00080 - Our decision on your submission to Behavioral Ecology and Sociobiology**

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Theo Bakker <em@editorialmanager.com>

Lun 12 Sep 2022 12:31

Para: Katarzyna Wojczulanis-Jakubas <katarzyna.wojczulanis-jakubas@ug.edu.pl>

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CC: "Marcelo Araya-Salas" marcelo.araya@ucr.ac.cr

Ref.: Ms. No. BEAS-D-22-00080

Foraging, fear and behavioural plasticity, a lesson from hummingbirds  
Behavioral Ecology and Sociobiology

Dear Dr Wojczulanis-Jakubas,

I am very sorry to say that based on the Associate Editor's recommendation (two referee reports had been received and the AE evaluation) I have to reject your manuscript for publication in Behavioral Ecology and Sociobiology.

The reviewers' comments can be found at the end of this email or can be accessed by following the provided link.

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Thank you for giving us the opportunity to consider your work for publication in Behavioral Ecology and Sociobiology. I hope that the reviewers' comments will be helpful in preparing an improved manuscript for submission elsewhere.

With best wishes,

Theo C. M. Bakker

Editor-in-Chief for Behavioral Ecology and Sociobiology, vertebrates

REVIEWERS' COMMENTS:

Associate Editor:

I finally secured a second review for this paper. Unfortunately, although both reviewers found the topic to be relevant, both also raised serious questions. The use of a single visit by individual birds is problematic and may not fully represent the behaviour of particular birds; the order effect (presenting always control before experimental conditions) is also problematic and may have been avoided by testing on different days; analysing the different variables as proportions of time during the visit makes the different variables inter-correlated, and this lack of independence compromises the analyses; there are no clear predictions, and the interpretation of different results seems contradictory. My reading of the paper coincides with the reviewers' appraisals, and hence I cannot recommend this paper for publication in BEAS.

Reviewer #1:

I have found this paper a bit challenging. The experimental set up was in which the authors examined how hummingbirds foraged around three feeders with and without a bull ant placed on each feeder. The rationale for the work was not very clearly focussed, which was perhaps why the predictions were pretty vague and where the authors essentially said they were expecting everything to have an effect but couldn't suggest how. It would be much more useful in interpreting the data if the predictions could be more specific.

The part I found most challenging however was the Results section, which was in my view much too short, and did not contain clear descriptions. There was more in the Discussion than was in the Results, and I went back and forwards between them thinking I must have missed something or read something wrong. I am still not sure, but firstly on lines 239-240, the authors report: "All parameters and their interaction were significant in this model except for risk-avoidance (Table 2)." Furthermore, "Risk avoidance tended to lower foraging efficiency and did not differ between threat levels (Fig. 4)." I may be wrong, but I would interpret these outcomes as meaning that the birds did not treat the presence of the bull ants as a reason for delaying beginning to feed. If this is the case, then, it is not clear to me how the authors can consider the presence of the bull ants a 'high threat condition'. And in the Discussion the authors say: "we found that experimental exposure of an ant, potentially representing just a threat of being bitten (but not eaten), was enough for LBHs to exhibit higher risk-avoidance (longer latency to start to forage) and lower movement activity compared to the control conditions." I must have missed something?

I also feel that I must have missed something in the interpretation of the data on exploration and arousal. I understood the first to mean the number of feeders the birds visited/total duration at the feeder, and that birds visited more feeders but each for longer when there were no bull ants on the feeders than when there were bull ants on the feeders "...exploratory behaviour, which was positively related to foraging efficiency in low threat context but the opposite pattern was observed in the higher threat context (Fig. 4)". If the birds do not consider the bull ants a threat (i.e. no delay in beginning to forage), how do the authors interpret these findings?

Whereas the birds fed for longer when they moved around the feeders more, and they moved around the feeders more in the presence of bull ants ("Arousal was positively related to foraging efficiency and this was particularly pronounced when birds faced higher threat (Fig. 4)". Essentially the same comment as above: I just am not understanding what the data mean if the birds do not treat the bull ants as a threat.

Reviewer #2:

General comments:

I found this an interesting study that addresses an important topic - the effect of individual behavior on foraging efficiency. The authors place this study in the context of the tradeoff between energy gain and predation risk. The idea is that individual behavior can provide additional predictive power beyond the effects of predation risk on foraging. The presentation is excellent. Unfortunately, I feel that a couple aspects of the experimental design limit the generality of the findings.

One deficiency is that most, if not all, of the measurements taken in the study are based (in some way) on time during a single hummingbird foraging visit to the feeders. Because each foraging visit lasts for a certain duration, the authors are essentially dividing up the total time of each foraging visit and testing for relationships between these different time components or segments.

I'm generally wary of this approach because it seems this would almost guarantee relationships between the different measurements of behavior. For example, it seems expected that the latency to start feeding (i.e., risk avoidance) would reduce the time spent feeding (i.e., foraging efficiency) because there is only so much time available in a foraging visit. Perhaps the authors need to do a better job explaining how these different measurements are in fact independent.

Regardless of whether the different measurements are independent or not, the results of the study are only applicable to the behaviors an individual exhibits during a single foraging bout, a very short time scale. A more revealing approach would have been to measure behavior prior to measurement of foraging efficiency to estimate the among-individual component of foraging behavior (i.e., behavioral type), and then measure foraging efficiency at a later time. The benefit here is to test for a relationship that isn't just limited a single bird foraging bout, but is actually persistent over time. Unfortunately, the study itself only works with 12 birds, so estimating the among-individual variation (1 estimate per bird) would limit sample size to test for relationships.

Second, as the authors make clear in the manuscript, the control vs. risk treatments are confounded with time - the risk treatment was always conducted latter in the day compared to the control. While the authors provide some reasons why this isn't such a problem, I had a hard time following some of these arguments and am generally wary that such confounding would be so easy to interpret. It seems the authors could have conducted the two treatments on separate days (so they occur at the same time) and randomized the order of treatments over multiple days.

Specific comments:

I don't understand why the authors mention behavioral plasticity in the title - it really isn't the subject of the study.

Line 87 - What do the authors mean by "unpredictable" here?

Line 106 - What do the authors mean by "traditional" here?

Line 272 - Did the authors test for these effects of the risk treatment on individual behaviors like latency and arousal? It seems like the statistical models only explored the effects of individual behaviors on foraging efficiency. Looking at Figure 4, I don't see the evidence that the risk treatment

increased risk avoidance, like the authors claim here. I'm a bit confused on this.

Both the captions for Fig. 3 and Fig. 4 need more explanation. In Fig. 3, what does the point represent? What do the width of the data clouds represent? Also, for Fig. 4, what do the points represent? Does each point represent a single foraging bout of a bird and the behaviors measured during that bout? If so, perhaps the authors should color the points differently based on the bird ID (1-12).

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