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Notification of Decision for ANBEH-D-14-00261R1

2 messages

Animal Behaviour <yanbe@elsevier.com>

Wed, Jun 25, 2014 at 8:21 AM

To: david.awam.jansen@gmail.com, david.jansen.13@nd.edu

Dear Dr. Jansen

A decision has now been made on ANBEH-D-14-00261R1

Both Parents Respond Equally to Infant Cues in the Cooperatively Breeding Common Marmoset (*Callithrix jacchus*)

Below you will find the editor's decision letter, as well as the comments from the reviewer(s).

To access your comments and the decision letter, please do the following:

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Thank you again for helping us with this paper.

Best wishes

From the Editors of Animal Behaviour

To: ****

From: "Animal Behaviour" yanbe@elsevier.com

Subject: Your Submission to Animal Behaviour [ANBEH-D-14-00261R1]

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Date Revision Due Jul 25, 2014

ANBEH-D-14-00261R1

Title: Both Parents Respond Equally to Infant Cues in the Cooperatively Breeding Common Marmoset (*Callithrix jacchus*)

Dear Dr. Sánchez-Rodríguez,

thank you for submitting your revised manuscript to Animal Behaviour. Based on my own reading of the new version and the reviews, I regret I am unable to accept the study for publication in Animal Behaviour, at least in its present form. I should, however, be prepared to consider a new revised version that more comprehensively takes into account my own suggestions and those of the reviewers.

The research is focussed on parent-offspring vocal and olfactory communication, and yet the new introduction still does not adequately outline these research areas in general terms (see also my comment about L330 below). For example, the role of olfactory communication in the social lives of marmosets is not explained (except for a very brief mention in L101-102). I (and a reviewer) still have concerns about the robustness of the findings for both main parts of the work (vocal and olfaction). The results would be more convincing if vocal playbacks had been carried out with contact calls, and not distress calls. Indeed, it would be interesting to compare responses to contact calls (non-distress) versus distress calls, and that would probably be more biologically meaningful than comparing distress calls to olfactory cues. I think the writing of the manuscript needs to reflect this. For example, L29 and L99 (and elsewhere, e.g. Discussion) needs to be clear about what type of vocal cues were used and tested in the experiments, and their potential limitations.

L153. Calls emitted within the same bouts are far more likely to be homogeneous. You need to at least consider this in your experiments.

L330-333. Some brief form of this text should be in the Introduction, because this topic is not adequately "introduced".

Discussion

L364-365. If a result is not significant, it is misleading to describe it as higher/lower etc.

Sebe et al. (2010) is preferable to Searby & Jouventin (2003).

Sebe et al. (2010). Early vocal recognition of mother by lambs: comparison of low and high-frequency vocalizations. *Animal Behaviour* 79, 10555-1066.

References with more than two authors are not correctly cited in the text. Use "et al."

L467. Error in the reference list. Delete "Review".

When you revise your paper, you should prepare a detailed explanation of how you have dealt with all of the reviewers' and Editor's comments. Refer to the Instructions for Authors (on the main menu of the Elsevier Editorial System at <http://ees.elsevier.com/anbeh/>) for details of our house style and for a list of file types that are acceptable for revised papers. Login to the Elsevier Editorial System as an Author to submit your response to the comments and your revised paper. Changes in the revised paper should be highlighted in Word or underlined. Please submit both the highlighted version and the non-highlighted version of the revised paper.

We should like to receive the revised paper within 30 days. If you think you will be unable to revise your manuscript in that time please let the Journal Office know (yanbe@elsevier.com). Please do not reply directly to this email.

Best wishes,
Dr. Alan McElligott
Editor

Reviewer #1:

Although the revised manuscript is a big improvement to the previous version of the paper I feel the manuscript still needs a lot of work.

I think the authors need to be clearer at several points in the paper which current parents (e.g dependent (DI) versus independent (II) infants are meant. The authors claim that responsiveness to vocal cues is only strong when infants are depended. Ones infants are independent current parents basically become similar to non-current parents (NI). I think paper would really benefit from a clearer comparison between the responsiveness of NI versus DI versus II. Whilst the comparison between the various playback setups is interesting the real question is if there is a difference between the responsiveness towards the infant vocalization. The authors also need attempt to give a interpretation on why non-breeders are still responsive. The response of these non breeders raises the question of the claimed hormonal priming mechanism is the correct one.

The study is based on playbacks with infant calls that were recorded at the age of 15-17 days old. It is surprising that with the use of the same calls parents don't show discrimination at dependent stage between own and other infants, whilst this discrimination does occur in the independent stage. I am wondering if this could be due to the use of the "young" calls at the older stage.

The lack in responsiveness to odor cues is likely due to the wrong setup. Whilst a vocal cues can transfer between different chambers, ones can question if this is the case for the odor cues. And as responsiveness to vocal cues is absent and likely caused due to a wrong setup I think authors should consider removing in completely from the paper and focus on the vocal cues.

Below I have some more specific comments.

L23 I am not a native speaker, but "parentally-experienced" sounds a bit strange.

L25-27 Consider reorder the questions in a more logical order. I think 3, 1, 2 would be better.

L31-33 Is there really no difference between current parent and former parents (see comment above).

L34-35 Authors need to be careful so make claims regarding hormonal priming. Whilst it is a likely mechanisms, the authors did not specifically test hormonal responses.

L41 "among primates such as...." suggest that all the following species are primates... ..better would be to write "among mammals such as..."

L45-53 I am not sure if this paragraph is needed as it is partly repetitive with the previous paragraph. If authors want to maintain parts of the paragraph merge with the previous.

L56-57 Sentence needs a reference

L57-59 Reference

L63-67 Sentence is confusing and needs to be rewritten

L73 group does this refer to family or bigger groups. Generally the paper would also benefit from a short paragraph to describe the natural biology of the study species.

L78-82 Very long sentence consider rewriting. Also the use of however suggests a contrast, but this is not the case.

L157-162 rewrite this sentence

L200 versus L165 be consistent in use of terms ethanol:water degassed solution versus alcohol:water vehicle

P10 the methods part would benefit from some sub-headings. Also indicate which statistical program was used. See general comments regarding which comparisons would benefit the paper.

L214 change have considered to did

L229 which type of non parametric Tukey test was done

L302-304 Whilst is is true that responsiveness does diminish which in-dependency, the authors should also mention that the discrimination between own versus other infants does increase.

L305-308 Recently does not seem applicable as a paper of 1981 is cited.

L313-319 Rewrite the sentence

L330-343 Whilst the argumentation in this paragraph is fine, I think the authors also need to address the likely wrong setup (see general comments above).

L364-366 I think authors need to be careful claiming trends which such a small sample size.

P16 L364 - P18 L414 Whilst the discussion has improved from the previous version there is still space for improvement. If authors do the suggested changes with the analysis the discussion will also need to be changed. Also see the general comments above.

Figures/Tables

Table 1. The 3 page table is slightly confusing. Authors should consider renumbering them as separate tables (e.g. 1a, 1b, 1c)

Also in Table 1 it is confusing that in the OO and UO classes the latency in response time is 600s = 10min = max. observation time. This suggest that nothing happens during data recording, but there are values for frequency and time.

Additionally the authors need to show more consistency, which for instance how scientific names are introduced. Now there are instances where the scientific names are in italic and between brackets, italic between commas and italic between -

Reviewer #2: The authors" revisions have adequately addressed my major concerns. The sections discussing the change in the value of response to distress calls as infants age helps integrate the paper into a larger literature on the evolution of response thresholds, which is certainly apt here.

One thing that I noticed in reading that paper this time was that the authors appear to refer to cooperatively breeding and

bi-parental care as inter-changeable phenomena. It is true that the marmosets are both cooperative breeders and bi-parental care gives, but the two concepts are not the same. This is noteworthy since the opening paragraphs are about cooperative breeding, but the paper is really about the responses of parents. The fact that mothers show behavioral and hormonal changes in response to pregnancy is not surprising, though the mechanisms of how similar behaviors arise in fathers or helpers that provide care but have not been pregnant is certainly interesting. This paper, however, does not look at helpers and only looks at fathers and, as such, the emphasis on cooperative breeding is misplaced and could be revised.

Minor points

Ln 47 - The paper cited for insects does not show anything about bi-parental care. It is a paper about Hymenoptera where there is no male care (e.g. male honey bees explode during mate and thus cannot provide care).

Ln 317 - banded mongoose helpers are referred to as parental helper, but this is not correct.

Reviewer #3: Title: Both Parents Respond Equally to Infant Cues in the Cooperatively Breeding Common Marmoset (*Callithrix jacchus*)

I found this version much clearer and addressed major concerns raised with the original manuscript. The abstract is much stronger. The introduction is more focused. I can understand the specific groups being compared in the results more easily. The statistics are presented clearly and explicitly.

I have a few small suggestions, one related to the editor's suggestion about clarifying terminology related to "distress calls". My comments are fairly long-winded, especially on the topic of terminology related to distress vocalizations, but it should not be difficult to address these points.

1. Terminology and distress calls:

People working on infant distress vocalizations primarily work either on calls made when an infant is separated from an adult or a social group on vocalizations made when an infant is captured, usually by a human researcher but also by a predator. The former type of vocalization is usually referred to as "isolation calls" or "contact call" (and has also been called a "female attraction call" in some seal papers or a separation call). John Newman (2004, 2007 and other references) has done quite a bit of work on primate isolation calls.

I would say that the term "contact call" is a bit more ambiguous, because some people use this term for vocalizations made when parents and young are together, i.e., to maintain contact. The term "distress call" has usually been used when animals are captured by a predator or by a researcher. Even though this is a situation of distress, it is a very different context than that in which isolation or contact calls are produced, even if the sounds made have many similarities to isolation calls (Lingle et al. 2012).

Confusion can arise between "distress calls" (made when captured) and "distress vocalizations" as an umbrella term that includes vocalizations made when isolated or when captured. Confusion might also arise with use of contact call in situation of isolation or when parents and young are proximate to each other.

In any case, I think the usage of terms in your study should be straightforward for you deal strictly with a clearcut case of "distress vocalizations" that are widely considered "isolation calls" (e.g. Newman 2004, and 2007 as well as Lingle et al. 2012). I suggest you avoid the phrase "distress call" in your manuscript, because of its association with calls made when a predator or researcher captures the infant. (This context of distress call probably does not arise in the primate literature as much as elsewhere.)

I suggest use "distress vocalization" as an umbrella term that encompasses isolation calls and distress calls and other cries made when an infant is under distress. Use isolation calls a few times (including in the abstract) because that is what you did record and used. For the sake of shortening some sentences, I see no problem with using infant cry or infant cries periodically or even frequently instead of distress vocalization. Perhaps just use "infant distress vocalization (cry)" at first usage, and then use one or the other subsequently. I suggest going through the manuscript and replacing instances of "distress call" with one or the other (or with isolation call). You use "distress cries" at one point, which is redundant (as far as I know, cries are always made by animals in conditions of distress, excepting cases of humans crying in joy - I doubt such a context will enter the minds of your readers).

E.g., Line 74: change "distress cries" to "distress vocalizations (cries)"

Specific points

1. Line 29: I suggest using the phrase "isolation calls" in the abstract, and line 29 would be a natural place to do this (isolation calls instead of vocal cues). Vocal cues could mean everything (sounds made in many different contexts). It is

important to identify the context in which the infant sounds were made in the abstract, regardless of whether you call them isolation calls. I do think "isolation call" or "distress vocalizations" would be useful keyword(s).

2. Line 73-74: New question: You say that Zahed et al. 2008 found that fathers with infants responded similarly when presented with their own live infant as they did to the recording of their distress cries. I don't understand the comparison being made - I am missing something about the experiment. What did they do when presented with their own infant - didn't they hold on to the infant or carry it? And didn't they try to approach when hearing the infant's cries? How can that be the same response? Or were the infant's cries played in both cases (with the infant present and when the infant was absent)? That would make sense, but needs to be clarified.

3. Lines 154-161: Clarification and correction related to use of "distress call" phrase as mentioned above. As indicated above, please do not use the term "distress call" as the umbrella term. The umbrella term should be distress vocalization or cry, which is how Lingle et al. 2012 used these terms - they did not use "distress call" as the umbrella term, which is what you indicate here. They used "capture call" instead of "distress call" to avoid confusion, even though "distress call" is more typically used for sounds made when an animal is captured (e.g., Lingle et al. 2007).

It is fair to consider vocalizations made in isolation "distress vocalizations" or "isolation calls", for that is standard practice (as per the Lingle review but also a very large number of papers on isolation calls). Why not just say something like "We recorded all vocalizations infants produced during the 5 minute period of isolation, and refer to these vocalizations as distress vocalizations (isolation calls, cries)." You don't deal with the other contexts mentioned in the Lingle paper, so don't really need the following two sentences talking about the various behavioural contexts in which distress vocalizations are made.

4. Lines 305-308: I do not have the Wiesenfeld (1981) paper with me right now to check the following point. I seem to recall that they reported that both fathers and mothers responded equally to their own baby's cries, although mothers were better (but not very good) at identifying the context in which a cry was made. Please check the former point, for it seemed these authors made that point (recognition of own infant) in 1981, which does not make it a "recent" finding (first word on line 305). I know that the Gustafsson paper suggests that Wiesenfeld et al did not state that fathers were as capable of recognizing their own baby's cry, but I think their interpretation might be inaccurate. Please double-check to be fair to the original paper.

5. Line 361: Latin for mule deer is *Odocoileus hemionus*. (*O. virginianus* is white-tailed deer)

6. Lines 353-355: "Distress calls often contain fewer specific features for individual recognition (Rendall et al. 2009)." Rendall et al. suggest and test this for baboons in another paper published in 2009 (Rendall, Notman, Owren. 2009. Asymmetries in the individual distinctiveness and maternal recognition of infant contact calls and distress screams in baboons. *J. Acoust. Soc. Am.* 125: 1792-1805). They contrasted isolation calls (which were tonal coos) with harsh distress screams made when faced with a social adversary. So their "distress scream" IS NOT equivalent to your "isolation call", and you probably do not want to lump your isolation calls with their "distress screams".

In any case, I do not think there is general evidence for this claim, so I suggest you at least tone it down to a "Distress calls may contain fewer . . .". The difference in recognition may be associated with a difference between sounds containing a lot of broadband noise (as did the baboon screams) versus tonal sounds. However, many newborn animals make loud highly tonal sounds under extreme distress such as when attacked by predators (Lingle et al. 2012).

Sometimes acoustic distinctions between individuals are just as high for distress calls (i.e., capture calls) as reported for isolation calls made by other species, for which vocal recognition is reported to be quite high (see Lingle et al 2007a,b (tandem papers in *An Behav*) for some comparisons - in that study, females did not respond different to their own offspring, but there was a relatively high classification success for distress calls (capture calls) made by different individuals.

7. I like the discussion of the difference in function for olfactory cues versus vocal cues. That is very consistent with what we observe with the deer. Vocal cues appear to be used to solicit immediate assistance; olfactory cues for recognition such as before nursing.

Some copy-editing

Lines 31-32: (line 31) replace "by" with between and then (line 32) delete the next "by" to make the comparison between the two types of parents more clear.

Line 60: change "through to" to "through" or "to", but do not include both.

Line 331: a word seems to be missing. "than perceiving the infants" odour. . ." reads better if "than parents perceiving the infants" odour . . ."

Para lines 344-355: replace all cases of "distress calls" with either distress vocalizations or cries. Replace "similarcaregivers" response" with "similar response by caregivers" (line 356)

Susan Lingle

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Mon, Apr 29, 2024 at 10:29 AM

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