

Reply: *Dear Dr Paquet*

We are thankful to the reviewer and to you for checking our revised manuscript and providing these helpful comments. We respond to your comments below (replies are in italics).

We made these changes to the file at <https://doi.org/10.32942/osf.io/rc8na>

As before, the version-tracked file is in rmarkdown at GitHub: https://github.com/dieterlukas/FemaleDominanceReproduction_MetaAnalysis/blob/trunk/Manuscriptfiles/PostStudy_MetaAnalysis_RankSuccess.Rmd. In case you want to see the history of track changes for this document at GitHub, click the previous link and then click the “History” button on the right near the top. From there, you can scroll through our comments on what was changed for each save event and, if you want to see exactly what was changed, click on the text that describes the change and it will show you the text that was replaced (in red) next to the new text (in green).

We think the revised version is much improved due to your generous feedback!

All our best,

Shivani, Elise, and Dieter

by Matthieu Paquet, 07 Jun 2022 11:55

Minor revision

Dear Authors,

Your manuscript has been reviewed by one of the previous referees and based on their reviews and my reading, I am inviting you to revise it slightly according to their comments and suggestions. You will find their reviews below. In addition, I also have some comments:

Comment 1: Line 41 “mediate”. Although I appreciate that it is indeed a statistical “mediator”, perhaps using a less causal terminology would be less ambiguous e.g. relationship between rank and reproductive success were conditional on life history?

Reply 1: We agree that the term “mediate” has other implications that we do not want to refer to here, so we have changed the sentence as you suggested:

Abstract: “the relationship between rank and reproductive success is conditional on life-history mechanisms”

Comment 2: Lines 46-49: I would suggest avoiding the use of the term “complex” here as it can be rather subjective and such complexity levels are not referred to nor defined in the main text.

Reply 2: We had used the term “complex” here because the types of societies we described were those also identified in a recent article by one of us as different ends of a spectrum of complexity. We realize though that we do not introduce these concepts here in the introduction and that therefore it is better to directly describe the societies without trying to classify them into abstract systems.

Abstract: “dominant females showing higher reproductive success than subordinates in two different types of societies: first, effect sizes are highest when females live in cooperatively breeding groups composed of a single dominant female and one or more subordinate females; second, they are also elevated when females form differentiated relationships which occurs when groups are composed of unrelated females.”

Comment 3: Line 93: it sounds like all social mammals were included. Perhaps delete “all”.

Reply 3: Thank you, we have deleted the word “all”.

Introduction: “the effect of dominance rank on female reproductive success across social mammals”

Comment 4: Line 292-299 Apologies for not suggesting it earlier on but could it be possible to provide the resulting consensus phylogenetic tree used in the study as a Figure, possibly with illustrations (animal characteristic shapes) for different taxa? It would provide the reader with a fast and intuitive assessment of the phylogenetic diversity and representativity of the dataset.

Reply 4: This is a great suggestion. We have now added a phylogeny with the associated data to display the phylogenetic diversity and distribution of variation in our data.

Added new Figure 1

Comment 5: Line 306: not sure “estimated all models” is the best wording (they are rather built and fitted to the data, then parameters are estimated). I’d suggest merging with the following sentence e.g.: We fit meta-analytic multilevel mixed-effects models with moderators via linear models (function “rma.mv” in the package metafor; Viechtbauer 2010)...

Reply 5: Thank you, we have merged these two sentences as suggested.

Results: “We fit meta-analytic multilevel mixed-effects models with moderators via linear models using the function “rma.mv” in the package metafor (@viechtbauer2010conducting), taking into account the sampling variance as measurement error and including models that account for the potential correlations among effect sizes due to shared phylogenetic history among species (@nakagawa2012methodological).”

Comment 6: Line 312: this sounds like the sampling variance was ignored when using the metafor package. Is that the case? It is also unclear in the text whether there is a

difference in the way phylogeny was accounted for with the two approaches. See my main comment below based on previous reviews: a mathematical notation of the models (equations) would really help understanding what statistical models were built and whether they differ between the two approaches (beyond the existence of priors for the Bayesian approach). See e.g. Edwards & Auger-Méthé 2018 for guidance (<https://doi.org/10.1111/2041-210X.13105>).

Reply 6: We changed the sentence describing the metafor approach to clarify that also with this approach we incorporated the sampling variance.

Methods: “We fit meta-analytic multilevel mixed-effects models with moderators via linear models using the function “rma.mv” in the package metafor (@viechtbauer2010conducting), taking into account the sampling variance as measurement error and including models that account for the potential correlations among effect sizes due to shared phylogenetic history among species (@nakagawa2012methodological).”

Comment 7: Lines 319-337: please use mathematical notation. For example, use the symbol “beta” rather than spelling it out). The equations here seem like a mix between Ulam/Stan R code and equations and are hard to follow (is this due to a formatting issue?). Please only use mathematical equations (for the distinction, see e.g. in the “Statistical rethinking” book where both code and equations are typically presented). Note that LaTeX style equation writing should be supported by markdown. It would greatly improve clarity and make the models more understandable for people not familiar with Stan.

Reply 7: Yes, there was a formatting error. In addition, we have now switched the model description to the mathematical equations rather than the model implementation.

Methods: we now provide mathematical notation.

Comment 8: Line 604: did not “clearly” vary?

Reply 8: Thank you, we have added this clarification.

Results 1.5: “Effect sizes did not clearly differ depending on whether studies were conducted with captive”

Comment 9: Lines 625 and 626 are identical.

Reply 9: Thank you, we deleted the duplicated line

Comment 10: Lines 1040-1042: I am not sure why this and only this should introduce an interaction effect. I can see this happening if relationships are non-linear and/or if group size variation is different between cooperative and plural/associated breeders. Could you clarify?

Reply 10: We now realize that we were thinking more of a potential confound than an interaction. We now describe why we decided to build a model that includes both the breeding system and group size.

Result 5.2.4: “In mammals, groups of cooperative breeders never grow to the same size (in our data, median 2 females per group, $n=52$) as groups of plural/associated breeders (in our data, median 14 females per group, $n=392$), meaning that the negative relationship between group size and effect sizes that we describe above might arise because cooperative breeders have both smaller group sizes and larger effect sizes.”

Comment 11: Line 1209: “0-8 offspring”, I understand that more than 8 is unlikely but it still likely occurs when performing as many as 10000 simulations. Was the Poisson distribution truncated to ensure this maximum of 8? If not, just rephrase.

Reply 11: Thank you for noticing. We did not truncate the distribution. We have adjusted the text accordingly:

Discussion: “We assumed that each female in each group might have an average of 2 offspring, following a Poisson distribution, so most females have 1 or 2 offspring and very few more than 8 offspring.”

Comment 12: Figure 10: I don't understand why there are 5 dots for the quartiles. What is the furthest one on the left of each line?

Reply 12: Looking back at it, we are not entirely sure why the density function in R should display dots when plotted as lines. It might be an issue with using translucent colours in the plot, so we replaced these with solid colours.

Figure 11: replaced.

Reviewed by anonymous reviewer, 27 May 2022 10:47

Thank you to the authors for their careful attention to my comments on the previous version of the pre-print. I am happy with their answers, and I think the manuscript definitely improved after the last round of revisions.

I have just a few minor comments left:

Comment 13: LL151-L156: This sentence is confusing, and it is not exactly clear to me what studies are really excluded now. The sentence starts by stating which studies are excluded but then in the brackets its another exclusion of the exclusion. I think this could be phrased a bit more straight forward.

Reply 13: Thank you for noticing this. Part of this sentence was a remnant from the preregistration, which no longer worked in the context of the edits. We now split this into two sentences

Introduction: "For reproductive success, we similarly excluded studies that reported associations of dominance rank with traits whose links with reproductive success were indirect or had not been tested. Studies we excluded reported, for example, associations between dominance rank and mating frequency, priority of access to food resources, or differences in ranging behaviour."

Comment 14: LL925-926: the part after (iii) does not make sense to me.

Reply 14: We meant to express that some of the variation in the effect sizes appears to be linked to values being similar if they are reported in the same study or from species of the same Order. This variation is not directly linked to any of the variables we tested as predictors, so it might be because of differences in the methods that researchers used in a particular study or for a particular set of species. We have now reformulated this.

Results: “In terms of the approach, effect sizes of dominance rank on reproductive success were higher (i) when individuals were assigned a rank category rather than a continuous position and (ii) when rank was determined using indirect measures rather than aggressive interactions, plus (iii) variation in effect size was also influenced by differences not captured by our variables, with measures reported in the same study or from species belonging to the same taxonomic Order being more similar than expected by chance.”

Comment 15: L1240-L1242: It's a bit unclear as to what the authors exactly refer to when stating “Our results show that other factors, such as the relatedness among females have less of a role on the effect sizes in cooperative breeder than in plural breeders, [...]” Could you be a bit more precise here? Also how is this statement connected to the results presented in LL1061-1066? Here you state that effect sizes increase with increasing relatedness in cooperative breeders but decreasing in plural breeders

Reply 15: Thank you for noticing this. We were still thinking about the directional predictions, but had not referred back to this. We have now clarified the statement:

“Our results also show that other social factors, in particular the number of females in the group and their relatedness, influence effect sizes in opposite directions in cooperative breeders than in plural breeders. The observation that in cooperative breeders reproductive success is shared less in species with larger numbers of subordinates and higher relatedness among them is in line with theoretical predictions that complete monopolization of reproduction can be stable if subordinates are queuing to inherit the dominant position themselves (@kokko1999social).”