

Review on PCI Ecology ms by A Sevchik, C Rowney, CJ Logan, A Blackwell & D Lukas

The study of avian life history traits has fuelled many investigations in the field of evolutionary ecology, providing invaluable data and analyses on many topics such as evolution of trade-offs, parent-offspring conflicts, evolution of senescence, adaptation to a changing environment, etc.. There is one fundamental trait however, for which we still know relatively little in birds, and that is dispersal and in particular natal dispersal. This is because measuring the distance between place of birth and place of reproduction is quite another challenge, on the field, than counting the number of eggs or monitoring nestling survival. In many species, capture-mark-recapture analyses encompassing nestlings and breeding birds is not feasible at a relevant scale to study natal dispersal, and therefore, we still have very poor knowledge on the distribution of natal dispersal in both sexes. In this context, it is interesting to collect new data on dispersal in a variety of birds, with different mating and social systems, as this will help, in the end, in understanding the factors influencing dispersal.

This study aims at gaining insight on dispersal in male and female great-tailed grackles, by comparing the genetic relatedness among males and females in a breeding site of Arizona.

The manuscript is very clearly written and makes an enjoyable read. I do however have two major concerns that could heavily influence the data interpretation.

1. The authors choose to place their study in the general context of the resource-defence based monogamous mating system in many bird species which predicts females are more philopatric than males. Since the mating and social system of the focal species are different from this mating system described in many bird species and leading to this prediction of higher philopatry in females, the authors believe that their study can 'offer an opportunity to determine if and how these differences might influence the dispersal behaviour of both males and females', and this is where I think the authors are getting a bit carried away. With empirical data on one species in one population, the authors will at best be able to provide a useful example on a different mating and social system which will provide insight into whether dispersal in the focal species provides a counter-example to the classic prediction for monogamous species. I appreciated the effort to outline 4 different hypotheses in the introduction, leading to four different patterns of comparative dispersal in this species, however, an important caveat preventing the simple test of these four hypotheses is that the genetic relatedness data gathered in this study, although very valuable, does not allow to decipher between the effects of natal dispersal and breeding dispersal, yet the hypotheses outlined concern one or the other. In particular, the main hypothesis clearly states a prediction about sex differences in natal dispersal while Alternative hypothesis 3 relies on the expectation of high breeding dispersal. Since relatedness among individuals will result from both natal and breeding dispersal, I do not think that the data obtained can bring clear interpretations on either of the two processes. Note that the predictions made in L92-L105 at the end of the introduction are very clear and logical, yet they do not mention the main and alternative hypotheses, which is in a way a demonstration that these predictions do not have a direct and simple link with the hypotheses. This does not mean that this study is not interesting, but it does mean that the general framework may need a rethink, and the interpretation and conclusions should be toned down and address this issue of natal versus breeding dispersal. While the discussion is presently cautious, the conclusion of the abstract on the role of reduced resource competition in determining female philopatry is very much

speculative, and should be presented as such. I would reformulate the sentence L30 about what the results 'show' (at least change to 'suggest').

2. My deepest concern regards the mist-net sampling of non-breeding individuals. It is important the authors provide more natural history background about this species, and about the trapped birds. In particular, since trapping and sampling was done across a very large period (September 2017 to October 2019), covering many months that are not during the breeding period (which should be in April May?? this information should be provided) it is not obvious to me at all 1. whether the sampled birds were all breeding birds (if they were roosting birds during the winter, this is an important issue for the interpretation of the data!) and 2. Whether the highly biased sex-ratio for females is really representative of the breeding sex ratio (L233). I have discussed previously the fact that the results are influenced by both natal and breeding dispersal, but I hadn't even realized at first (before reading the methods and understanding that trapping was not limited to the breeding season) that the results could also be influenced by the birds possibly wide wanderings during autumn and winter. This species forms communal roosts during winter, and it is common in social species that roost composition is female biased (although I do not know whether this is the case in the focal species) and also that movements of individuals during winter are different between males and females. In short, since the sample could include transient birds, with a different probability of this for males and females, I do not think the authors can conclude that 1. The sample is representative of breeding birds, which influences the whole interpretation of results, and 2. That the highly biased sex-ratio is representative of the adult sex-ratio at the site, unless they have more information on the sampling that is not provided in the text presently.
3. For analysis i, I was surprised that the average relatedness across females and males was compared by resampling individuals from the same population, rather than resampling SNPs. Since the resampling is done within a dataset of 52 individuals where females are over-represented, I wonder whether resampling the genetic data is not less biased.
4. The sampling area is an urban environment. Although not much is known regarding natal and breeding dispersal in natural versus urbanised environments, it is highly suspected (and documented in some species see e.g. Partecke, J. & Gwinner, E. 2007. Ecology and review in Marzluff 2017 Ibis) that the fragmented landscape of cities will highly influence dispersal. Perhaps you want to mention this in the discussion, and whether this urban effect on dispersal could be sex-specific?
5. Note that I was not a reviewer of the preregistration for this study, hence I did not dwell on the section mentioning deviations from it.
6. Minor details
 - L14 : replace 'close' by 'closer' as male natal dispersal distance varies greatly across species, what is repeatedly observed is that they have shorter natal dispersal compared to females
 - L19: I would replace the term 'exact' by 'true' or by nothing. Often enough, our field studies measure proxies of these factors and not the 'exact factors' themselves.