



Letters

Small-Scale, High-Resolution Studies May Reveal Patterns Missed by Broad-Scale Analyses

Les études à échelle fine et à haute résolution révèlent parfois des patrons qui échappent aux analyses effectuées à des échelles plus grossières

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We thank Armstrong et al. (2008) for raising specific concerns about habitat fragmentation research in their paper “Avoiding hasty conclusions about effects of habitat fragmentation,” as these concerns are important and deserve to be highlighted. As a forum for exploring these issues, Armstrong et al. chose to single out our paper (Wittern and Berggren 2007) and structure their criticisms around three points with general relevance to fragmentation research; we discuss these criticisms below as they relate to our study:

(1.) The scale of the study system needs to be appropriate for the study species. Armstrong et al.’s first criticism of our research is that Tiritiri Matangi Island should never have been chosen as the location for our study on the effect of habitat fragmentation on North Island robins (*Petroica longipes*) because previous research (i.e., Armstrong and Ewen 2002) had conclusively shown that patch isolation was irrelevant to juvenile settlement. There are two points that need to be made here. First, to support their assertion, Armstrong et al. (2008) reproduce the best-supported juvenile settlement model from Armstrong and Ewen (2002) and state that “comparison to simpler candidate models showed there was no evidence of any isolation effect.” However, an examination of the candidate models

compared in Armstrong and Ewen (2002), rather than demonstrating no evidence of patch isolation on juvenile settlement, instead shows that there is some support for models that include this term (Akaike weight for the full model, which includes “patch isolation,” = 0.26). Thus, our study confirms, rather than “contradicts” their findings by showing patch size, patch occupancy, and patch isolation are all potentially important factors influencing early juvenile dispersal in this population. Second, the dispersal analysis undertaken by Armstrong and Ewen (2002) evaluated the effect of patch isolation on the location of juvenile settlement in a breeding territory in the following year; in contrast, our study focused on the initial dispersal phase after fledging—and we were able to closely follow juvenile movements because we undertook full population surveys every 5 days rather than just twice a year as in Armstrong and Ewen (2002). Thus, it is not surprising that our results might differ from those of Armstrong and Ewen (2002); our movement data were at a much higher resolution and our time frame for study was during a different life-history phase (i.e., we looked at initial juvenile dispersal, which is likely to be a much more sensitive indicator of resistance to crossing barriers, not juvenile settlement, which is the long-term outcome of this process and may overshadow the patterns we aimed to uncover).

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(2.) Interpretation needs to account for possible confounds. Armstrong et al. (2008) suggest that our study is flawed because the juveniles we observed did not have enough time to move from their natal territories; they argue that we did not observe true juvenile movement patterns, but simply were recording juveniles in areas close to their natal patch (with large well-connected patches producing more juveniles and, hence, producing the patterns we describe). This is not the case. The dispersing juveniles that we followed did not originate from more-connected patches (Kruskal Wallis test $\chi^2 = 5.0$, $p = 0.29$), were observed for a mean of 51 ± 4 days (\pm SE) after leaving their natal territory, their movement patterns from independence recorded every 5 days, and each juvenile observed up to 650 ± 70 m from the nest during dispersal. From mapping the positions of juveniles, we could see that they did not remain close to their natal areas (the aggression from conspecifics in these well-connected central areas tends to drive young birds out very quickly), and they had every opportunity to visit all areas of the 220-ha island during the observation period.

Armstrong et al. (2008) highlight a mistake we made in our analysis by using the number of habitat patches within connectivity classes rather than the total area. We admit this oversight. However, if we consider the total area available in habitat patches with high connectivity (<20 m) vs. those with lower connectivity (>20 m), we still see the same trend as reported in our study (observed vs. expected: 54% vs. 43% for highly connected habitat and 46% vs. 57% for lower connected habitats). We monitored all habitat fragments regardless of size (unlike Armstrong and Ewen (2002)), because we were interested in dispersal movement, not juvenile settlement. Indeed, we found juvenile robins and breeding adults in fragments less than half the size Armstrong et al. (2008) claim robins can settle in (0.07 vs. 0.18 ha). Because we were not interested in juvenile settlement in the following breeding season (as this was never the focus of our study), and given that we found robins in areas Armstrong et al. (2008) claim we should not have, we feel our decision to monitor all habitat fragments regardless of size is vindicated.

(3.) Increasing connectivity may not always benefit poor dispersers. Despite Armstrong et al.'s (2008) claim to the contrary, we never concluded or implied that conservation managers should create a network of habitats across mainland New Zealand

separated by an obviously impractical distance of less than 20 m. Instead, we showed that short-term natal dispersal of robins on Tiritiri Matangi Island was lower when patch isolation was >20 m. Based on this finding, we made the reasonable suggestion that, if this species is reluctant to cross open areas, then "the preservation or creation of habitat corridors...may increase the movement of individuals between isolated fragments." We were careful in trying to not overstate our case by reminding readers that "to estimate the dispersal behavior of the North Island robin in corridors and stepping stones, more studies are needed" and "more studies are needed on the effect of re-created habitats on individual dispersal in this species" (see Wittern and Berggren (2007): 11). We completely agree that dispersal limitation does not necessarily imply that higher connectivity would be beneficial to this species; however, it does not necessarily imply that higher connectivity would be "detrimental" either. The unorthodox idea that high connectivity is, in general, a bad thing is what the reader is encouraged to conclude, with Armstrong et al. (2008) suggesting that our conclusions are "worrisome," would result in "lost opportunities," and increase "threats" to this species via increased movement of "predators and pathogens" via corridors (all possibilities that are discussed in our original paper; see page 11 of Wittern and Berggren (2007)). Although we acknowledge that data are needed to quantify any benefit that might be achieved through improving habitat connectivity for North Island robins (as we have previously suggested), we are not willing to accept Armstrong et al.'s (2008) criticism that improving connectivity would harm conservation efforts directed at this species, without data showing that these threats are real.

Responses to this article can be read online at:
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