

The authors employed both individual based model simulations and an experimental manipulation to test the hypothesis that reduced landscape connectedness can yield pushed expansion dynamics by reducing dispersal. To evaluate the expansion dynamics of reduced connectedness and reference scenarios, the authors evaluated relative expansion velocities and changes in neutral genetic diversity at expansion edge vs. origin patches.

In the IBM simulations, patches on the edge of the expansion lost genetic diversity faster in reduced connectedness landscapes than in the reference landscapes. In contrast in the experiment, genetic diversity was lost faster in the reference landscapes than in the reduced connectedness ones and there was no difference in the velocity of the expansions. The authors argue that the discrepancy between experimental and simulation results can be explained by considering the effect of reduced connectedness on the strength of the density-dependence of dispersal. When the authors added positive density dependence in their simulation with reduced connectedness, genetic diversity loss in edge patches corresponded more closely with the observed losses in the experimental landscape.

I liked the parallel approaches of simulation models and experimental manipulations to test the hypothesis. It appeared from the presentation in the introduction that this experimental system is novel. If so, I think that the introduction of this system is itself a contribution. I thought the authors made a compelling argument for the need for experimental systems that can be manipulated to change factors that impact range expansion dynamics and the *Trichogramma brassicae* system appears to be amenable to these types of manipulations.

### **Major points**

I would like clarification about the degree to which changing the number of tubes between the vials in the experimental landscapes reduced dispersal between vials. I was unsure of how to interpret the reduction in mean egg laying distance between the reduced connectedness landscape and the reference landscape in terms of a reduction in dispersal. I found it more difficult then to compare the results of the experiment and simulation without knowing the degree to which dispersal was reduced in the experimental landscapes.

I do not have the expertise to comment on whether the IBM model setup or statistical analysis framework were appropriate. They were extensively described.

### **Minor points**

As far as I know, the presentation of the work within the context of prior literature was adequate. It was also clear from the introduction what the aims of the manuscript were.

The language in general was clear. There were a few instances of vague pronoun references ex. line 96 “This can increase the influence of population density on dispersal success...” It was unclear to me based on the previous sentence what “this” was referring to. Ex. line 100 “They are however much lower than those considered in most pushed expansion models...”; line 469 “This is not without precedent...”; line 473 “this shows more studies...”

It may be useful to add a figure illustrating the experimental setup

For clarity, I would recommend adding “(core patch  $t=0$ )” after “origin” in the Figure 5 key

Line 14: replace “these phenomena” with “range expansions”

Line 83: Does “changing the resource” refer to changing the quality of the resource to increase the strength of the density dependence of dispersal?

Line 311: Grammatical construction is a bit confusing. Perhaps “Relative velocities in the reference landscapes were close to those thus expected for pulled expansions”

Line 413-417: Am I correct in my understanding that the relative number of individuals contributing to a new population is higher under the reduced-connectedness scenario because the source populations in these landscapes are smaller (because of reduced dispersal) so the ratio of individuals that disperse from a source patch to the source patch population is higher? If I am correct, I would recommend that the authors clarify this sentence.

Line 444: I think it would be helpful to expand upon how the simulated expansions corresponded better to the results of the experiment. Is the result of this simulation presented in one of the figures?

Line 449: “spatial behavior in response, which evidence suggests they do...”

Line 460: I would recommend moving the explanation for why the authors do not believe that differences in equilibrium population sizes drove differences in genetic diversity losses between the experimental and control landscapes from the supplementary material to the body of the manuscript. My a priori assumption would be that this alternative explanation could drive substantial differences in the rate of genetic diversity loss and I think it would be useful to expand the discussion of this alternative explanation in the manuscript.

Line 469: clarify that “in these contexts” refers to “under reduced landscape connectivity”

Line 472: I think it would be helpful for the authors to clarify what they mean in advising empirical researchers to “be careful about which questions they ask” in light of their results.

Line 474: Does “types of pushed expansions” refer to types resulting from different drivers of pushed expansions?

Line 480: Perhaps it is a little strong to conclude that reducing connectivity may benefit expanding species based on a slowing of the decline in neutral genetic diversity. Perhaps this concern is addressed in the following citations; however, I think this argument could benefit from explicitly stating the evidence provided in these citations if that is the case.

Line 486: I think this sentence would be clearer if the structure were flipped. “We call for... because...”