Review of Preprint 2023.10.25.563987 – Rapid life-history evolution reinforces competitive asymmetry between invasive and resident species. Chapuis Elodie, Philippe Jarne and Patrice David

The authors present the results of laboratory experiments that characterize the interactions between an invasive species of snail (Physa acuta) and a resident species (Aplexa marmorata). The utilized P. acuta populations that had long since invaded new habitat (core populations) and those that were on the front of the invasion (front invasions), but emphasize that the front populations are derived from core populations so we should assume that all have adapted to the new environment. They compete them with A. marmorata from populations that have not yet been invaded or have only recently been invaded (N populations) with ones that have been occupied by P. acuta for an extended period of time (E populations). In earlier work they showed that the E. populations of A. marmorata were younger at maturity and had higher fecundity than N populations of A marmorata. There were more subtle differences in adult body size and fecundity between F and C populations of P. acuta – F populations tended to be larger and have lower fecundity. The new results in this paper show that P. acuta is competitively superior to A. marmorata and, paradoxically, A. marmorata from E populations are poorer competitors than those from N populations, meaning that their evolution in response to P. acuta invasion causes them to be poorer competitors. Simple ecological theory for co-existence predicts that the resident will go extinct, but they are continuing to co-exist with the competitor. The authors consider different explanations for this co-existence but miss a large body of ecological theory, originally proposed by Chesson (e.g. [1]), that propose general ecological conditions that can explain co-existence under such circumstances. While there is no need to go into these alternatives in detail, I think it is essential that they at least add a paragraph that refers to Chesson's models and considers how these alternatives might reconcile the observation of co-existence in spite of experimental data that suggest that co-existence is not possible.

Overall, I think this is a well-executed and well-presented experiment. I only ask that they adopt an interpretation that is less committed to their life history evolution explanation for the seemingly paradoxical results. It is natural to emphasize life history evolution since that is the one type of evolution that is well documented, but the ecological complexity of the actual invasion invites alternatives that need to be acknowledged.

Abstract: Rather than say that Aplexa marmorata evolves "towards an apparently more colonization- and less competition-oriented syndrome" I suggest that you state exactly how you know they evolved, which I assume is towards earlier maturity and higher fecundity. The reader should know the specifics after reading the abstract, but you never give them those specifics. Note that MacArthur and Wilson were not so specific in Island Biogeography about what the properties of a successful colonizer would be. Having a fast life history was one alternative, but being able to persist in the new environment was another. Likewise, later in the abstract when you refer to how A. marmorata's life history has evolved, you again just state that it has evolved a more colonizer lifestyle, leaving it to the reader to translate what this actually means. Please just give us the facts, not what you think the facts mean. The fact is apparently that they have evolved more rapid development. Your interpretation is that this means they have evolved a colonizer lifestyle.

Introduction: All that is said here makes sense and is well written. There is a good review of the relevant literature and a clear statement of how the resident species has evolved in response to the

invader. Lines 130 through 146 give a good summary of the earlier research that establishes that the invasion of P. acuta imposes some selection on the resident A. marmorata, but also states a reciprocal impact. The earlier descriptions lead me to expect an smaller effect of the resident on the invader than the reverse.

Table 1: I do not understuand what the second sentence (beginning with "changing the identity of the competitor…") of the prediction for hypothesis I means. It seems to repeat what is said in the first sentence. If so, delete it. If not, then it needs to be revised but I do not know what to recommend because I do not understand the intended message.

Methods: We need to be told how you provisioned the snails in the experiment. What sort of food was used, how often were they fed and was food availability limiting? This gives us some clue about the extent to which there was resource competition. Do they eat each other's' eggs or larvae?

Figures 2 and 3: Amend the captions to tell us that Figure 2 pertains to Model 1 and figure 3 to Model 2 of Table 4.

Lines 524-529: The absence of differences between front and core populations does not tell us that P. acuta does not evolve in response to A. mormorata. Earlier in the paper they state that front populations are derived from core populations, so the all have been exposed to A. marmorata, and the new habitat in general. From the evidence given here, it seems we cannot tell whether or not P. acuta has evolved in response to A. marmorata, but can certainly say that A. marmorata has evolved in response to invasion by P. acuta.

1. Chesson, P., *Mechanisms of maintenance of species diversity.* Annual Review of Ecology and Systematics, 2000. **31**: p. 343-+.