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RELOCATIONS, REPATRIATIONS, AND TRANSLOCATIONS OF AMPHIBIANS AND REPTILES: TAKING A BROADER VIEW

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THE review of "relocation, repatriation and translocation" (RRT's) of amphibians and reptiles by Dodd and Seigel (1991) provides a summary of the literature on the use of these techniques for conservation purposes. Their recommendations are generally sound, and apply not only to these conservation practices, but equally well to any of the myriad possible techniques used to help insure the preservation of a species. However, I believe that the evidence they use for support is weak, that their dissatisfaction with past efforts is only partially justified, and thus their conclusions extreme. Basically, the question that they attempt to answer is: given that conservation dollars are always limited, are RRT's cost effective and appropriate procedures for amphibian and reptile conservation programs? They find that these techniques have been successful in only a few cases, and thus they propose a rigid set of criteria to be addressed before any future attempts are begun. My comments on their work focus on two main points: whether amphibians and reptiles are generally poor candidates for RRT's, and how success should be determined.

REPTILES AND AMPHIBIANS AS RRT CANDIDATES

As Griffith et al. (1989) did for a much larger number of studies of birds and mammals, Dodd and Seigel reviewed RRT programs for 25 species of amphibians and reptiles and found that of the 11 projects that could be defined as successful or unsuccessful by their standards, five (45%) were successful. This is slightly higher than the success rate reported for 198 RRT's reviewed by Griffith et al. Even so, the use of this type of analysis is exceedingly crude, because it assumes that snakes, lizards, turtles, crocodilians, salamanders, and anurans have comparable potential for successful RRT. Certainly there is wide variation within each order as well as between them, and anyone considering an

RRT for a particular species should be mainly interested in experiences from similar species. For example, Griffith et al. (1989) found that RRT success varied dramatically between taxa in different trophic levels, and also that life-cycle stage when relocated was important. Dodd and Seigel also treat as similar those RRT programs that differ greatly in operating budgets, number of animals released, and origin of released animals (wild-caught or captive raised). Griffith et al. (1989) found all of these factors relevant to the success rate of RRT's for birds and mammals.

Because Dodd and Seigel did not control for important variables, their 25-study analysis is clearly a case of comparing apples to oranges. However, doing the comparison properly would be difficult, because the sample size is so small. Some additional studies to add to the list for anyone interested in attempting such an analysis are listed in Comly et al. (1991) [especially the 13 described by Cook (1989), but see also Humphrey et al. (1985), Stout et al. (1989*a*), Tom (1988), and additional references below].

Dodd and Seigel were unable to find any examples of successful RRT's for any species of snakes, turtles, anurans, or salamanders, despite the fact that the literature is replete with them [see Wilson and Porras (1983) for one recent relevant review]. Some of the examples that I cite below are "translocations" under the definition given by Dodd and Seigel, but because they involve species not recently native to the release area, they may also be called "invasions". I anticipate the objection that the deliberate or accidental release of a species that is later considered an invader is somehow different from the release of a species for conservation purposes. However, the distinction is important only in terms of human intentions and values (Price, 1989), and the theoretical and empirical studies on biological invasions are directly relevant to RRT's (Griffith et al., 1989; Konstant and Mittermeier, 1982; Pimm et al., 1988; Roughgarden, 1986*a*). Both involve the establishment of a species through the release of a small number of individuals into an area inhabited by few or no conspecifics. Attempts to identify the general life history and genetic characteristics of species that are either successful colonizers or extinction-prone have found little empirical support; for each generalization there are numerous exceptions (Burke and Humphrey, 1987; Ehrlich, 1986; Newsome and Noble, 1986). For example, elephants exhibit most of the traits commonly attributed to poor invaders and extinction-prone species, yet are pests in some areas. The main trait clearly shown to be useful in identifying extinction-prone species is initial rarity (Pimm et al., 1988; see references in Burke and Humphrey, 1987), which similarly characterizes both deliberate and accidental RRT's. Furthermore, conservationists may learn from a study of relevant invasions, because most invasions involve few individuals, released with a minimum of care in a strange environment, and as such are excellent examples of what can be done on a tight budget.

For snakes, the now 10 yr-old repatriation of *Nerodia sipedon* into a national park in New York (Cook, 1989) and *Boiga irregularis* in Guam (Savidge, 1987) are two examples of highly successful RRT's. The current discontinuous range of *Elaphe longissima longissima* is a result of multiple RRT's by the Romans some 2000 yr ago for rodent control in their temples (Mehrtens, 1987). For turtles, in California alone *Chelydra serpentina*, *Apalone spinifera*, and *Trachemys scripta* have populations clearly established by RRT's (Mooney et al., 1986). Similarly, *Trachemys scripta* has been firmly established through relocations to a variety of sites throughout the eastern United States (Conant, 1975). The tortoise *Geochelone pardalis* has been translocated into two nature reserves in South Africa, the first pre-1930 and the second pre-1966, and both populations are "flourishing" (Brooke et al., 1986). *Geochelone elephantopus hoodensis* has apparently been successfully repatriated now 15 yr after the initial release (Anonymous, 1986). For anurans *Rana catesbeiana* in the American southwest (Schwalbe and Rosen, 1988), *Xenopus laevis* in California (Mooney et al., 1986), *Dendrobates auratus* in Hawaii (McKeown, 1978), the repatriation of *Bufo calamita* into a British reserve (Raw and Pilkington, 1988), and the remarkable success of *Bufo marinus* (e.g., Easteal and Floyd, 1986) in numerous countries and habitats throughout the world are but a few of the many examples of successful RRT's. Examples of salamanders include *Ambystoma tigrinum* in the American southwest (Collins, 1981), *Necturus maculosus* in New England and apparently *Desmognathus quadramaculatus* into parts of Georgia (Conant, 1975). Finally, to add to Dodd and Seigel's list of successful lizard and crocodilian RRT's: *Chameleo jacksonii* and *Iguana iguana* in Hawaii (McKeown, 1978), *Anolis* sp. in numerous Caribbean Islands (Roughgarden, 1986*b*) and Florida (Wilson and Porras, 1983), *Anolis grahami* released in Bermuda to control mosquitos (Simmonds et al., 1976), *Hemidactylus turcicus* and *H. frenatus* into many tropical, sub-tropical, and even some temperate habitats all over the world, and *Caiman crocodilus* in Florida (Ellis, 1980) are just a few of the possible examples.

Finally on this topic, I agree with Griffith et al. (1989) that researchers and conservationists interested in understanding why some species under some conditions may be promising candidates for RRT, and others not, should investigate the literature on biological invasions, which has had several recent and thorough reviews (e.g., Castri et al., 1990; Drake et al., 1989; MacDonald et al., 1986; Mooney and Drake, 1986; Wilson and Porras, 1983). This body of literature reviews the data on successful and unsuccessful invasions by a number of species from a variety of taxa, and has a body of theory relevant to conservation issues (i.e., Ritcher-Dyn and Goel, 1972).

What Should We Call "Success"?

A second major thrust of Dodd and Seigel's essay is that some workers, particularly Burke (1989), have been premature in calling their efforts a "success". For their analysis of 25 RRT's reported in the literature, they defined a project as a success only if "evidence is presented that a self-sustaining population has been established", and that "the population is at least stable". It is not clear how they applied these criteria in the cases that they reviewed. For example, at what point can one call a population "self-sustaining", and how does one determine stability? They suggest that mere successful reproduction is insufficient. However, no population, "natural" or otherwise, can be defined as indefinitely, invariably stable, and the longer a population is monitored, the less stable it appears to be (Pimm and Redfern, 1988). Later, they suggest that a monitoring program of 10–15 yr for anurans and >20 yr for tortoises would be appropriate for determination of success. Again, it is not clear if they applied these criteria to the studies that they reviewed. Obviously, few RRT studies of this duration have been completed.

I welcome Dodd and Seigel's definitions of success for RRT's, and I encourage other interested workers to air their views on how to define success (e.g., Phillips, 1990). For example, rather than simply declaring a particular RRT a success, I stated that "the usefulness of relocation for tortoise conservation is unclear" (Burke, 1989: p. 295) and, later, that I had shown that "it is possible to relocate and reintroduce gopher tortoises *fairly* successfully" (Burke, 1989: p. 295, italics added here). These results were further presented in quantitative terms. Generally, I called the project "fairly successful" because the same 31 individual tortoises stayed at the release site (from which tortoises were extirpated before it became a county park) for 2 yr after release, they reproduced both years, and their offspring survived and grew. In addition, the release site was public land with a legal commitment to manage for maintenance of natural habitat in perpetuity, predator-control programs were in place, and the tortoise population exceeded the size that population simulation models suggested to be the minimum necessary for survival for at least 200 yr with a >90% probability under these conditions (Cox et al., 1987). This tortoise population continues to thrive, now 5 yr after release. I plan to write the 20 yr evaluation in due time.

Other than deliberate attempts to mislead readers, authors are not responsible for misinterpretations of their work, and I am unaware of any evidence that my results have encouraged the use of RRT's for gopher tortoise or any other amphibian or reptile. On the contrary, the appropriate regulatory agency, the Florida Game and Fresh Water Fish Commission, recently proposed making Florida tortoise RRT's obsolete with the consideration of an incidental take law which would allow the destruction of tortoises and habitat in exchange for fees. Few developers will go to the expense of a tortoise RRT unless legally required to do so.

Errors

Dodd and Seigel's essay has four additional problems that bear correction; the first three are relatively minor, but the fourth is more serious. First, Dodd and Seigel recommend that populations released as RRT's should mimic the demographic characteristics of "natural" populations. This is a point of some contention, and other views have been presented by Berry (1986) and Landers (1981). Based on the limited data available, these authors suggested that RRT's may be more successful if various manipulations, such as releasing female tortoises first or releasing fewer adult males, are used. My work (1989) addressed this in part, but this issue is not resolved and is likely to have different solutions for different species and release program combinations.

Next, they misquoted Burke (1989) as "claiming relocation had no effect on existing social structure of resident tortoises . . . despite data to the contrary on related species (Berry, 1986)." Both points are incorrect. There were no tortoises resident on the release site before that project, and I have never released tortoises into an area where there were resident tortoises. Apparently they misunderstood my research and results on the impact of social structure of the *released* population. Also, Berry (1986) did not present data on this specific point, but instead she postulated, from existing data on social behavior and movements, *possible* impacts on RRT success.

Later, they criticize the studies of Burke (1989), Fucigna and Nickerson (1989), Godley (1989) and Stout et al. (1989*b*) as being of too short a duration to justify claims of "long-term relocation success". I agree, but also point out that none of these studies claimed long-term success.

The fourth issue is that of population genetics and minimum viable population (MVP) analysis for RRT's. Dodd and Seigel focus on one small aspect of MVP analysis, that of population genetics, and point out that it has rarely been discussed in the RRT literature for amphibians or reptiles (but see Burke, 1989). I suggest that over the time frame relevant to most of these types of conservation efforts, population genetics is instead more important to another concern not addressed by Dodd and Seigel: the risk of mixing distinct gene pools through careless RRT's, as pointed out and documented by Greig (1979) and Templeton et al. (1986). Not only could such mixing threaten the survival of locally adapted populations, but current and future evolutionary studies on the species could be rendered impossible or misleading by careless RRT's. This reason alone is sufficient to recommend strongly that genetic studies be undertaken prior to RRT's (see, for example, Lamb et al., 1989), and that RRT's be carefully documented in the literature. It is also important to recognize that if a population is on lands scheduled for extensive alteration, any individuals that are not moved, but are killed instead, may represent genetic material lost forever.

Simberloff (1988), Shaffer (1987), and Lande (1988*a*) pointed out that MVP analysis (and its modern descendant, population viability analysis: Gilpin and Soulé, 1986) is based on more than population genetics, as genetic concerns are only likely to be important to a small population of a normally outbreeding species going through an extended, multi-generational bottleneck. They predict that under the

100–200 yr time frame considered by most conservation efforts, demographic and environmental effects will be more important, and thus most MVP and PV analyses do not take genetics into account (e.g., Burke et al., 1991; Cox, 1989; Cox et al., 1987; Grier, 1980; Lande, 1988*b*; Shaffer, 1983); thus the use of any sort of 50/500 rule is superseded. Population simulation for realistic and useful MVP analysis or PVA requires advanced computer programming skills and detailed knowledge of both the species' biology and the important environmental factors that impact populations. Current development of new PVA's, involving analysis of metapopulations subdivided into many subpopulations, promises to be particularly applicable to small, RRT-established populations. While a MVP analysis or PVA can be a useful component of a species recovery plan, it is not a trivial endeavor (Burke et al., 1991). Few have been completed for amphibians or reptiles (but see Cox, 1989; Cox et al., 1987; Soulé, 1989).

Recommendations

Dodd and Seigel's recommendations for future RRT's are generally sound, and I shall only comment on a few of them. Readers interested in reviewing these points in greater detail should see Price (1989). I agree that for no species of amphibian or reptile do we have a thorough knowledge of conditions that maximize chances for a successful RRT. I also agree that each RRT should have an experimental design allowing appropriate statistical tests of manipulations hypothesized to increase success. For species likely to be subject to many RRT's, a coordinated research program should be established to allow standardization of basic technique with replication and testing of suggested improvements. For example, the Florida Game and Fresh Water Fish Commission has permitted over 75 relocations (Dodd and Seigel, 1991), but it required only that applicants adhere to a general protocol, and did not recommend investigation of potential improvements. Funding for such programs should be available from the development forces that make them necessary.

Dodd and Seigel appropriately call for longer monitoring of RRT's, to insure that initial indications of success are borne out. They point out that this involves a substantial commitment of resources that in many cases may not be feasible. For example, when the proposal for tortoise relocation described in Burke (1989) was reviewed, the funding agency refused to fund more than 2 yr of follow-up, because current legal restrictions did not require more. This does not lessen the importance of long-term monitoring, only its likelihood. However, I would not draw the conclusion that further turtle RRT's should not be considered until 20 yr has passed to allow judgement on the success of those already done, for two reasons. First, extinctions of RRT populations must be considered against the baseline extinction rates of similarly sized unaltered populations. Thus, if 10% of the RRT's of a particular species fail, this may not be because of the RRT itself, but may be a rate characteristic of subpopulations of the species in general (Diamond, 1984; Karr, 1990). Secondly, conservation biology is correctly described as a "crisis science" (Soulé, 1985), and as such may not always be subject to the same statistical standards as most other scientific fields. In some cases, it may be necessary to accept higher than normal risk of Type 1 errors and to make decisions based on preliminary trends in data that may not reach the $P = 0.05$ level of significance, but are strongly suggestive of the value of a technique.

Dodd and Seigel also review criteria for choosing release sites, and thus generalize the example and discussion presented in Burke (1989). For example, there may be numerous appropriate sites for gopher tortoise re-introductions in Florida, areas from which tortoises have been extirpated, but are now relatively safe, and have low probability of natural recolonization (Burke, 1989). In a perfect world, potential RRT organizers would have sufficient time to study the biology of the species concerned, investigate a variety of potential release sites, and choose the best candidates. Inability to do this should be fit into the cost/benefit analysis for the RRT project; for example, if no good release sites are available, obviously an RRT is inappropriate.

Conclusion

Discussions of RRT's are important and useful, because RRT's may form an expensive part of the conservation program for a vulnerable species. For example, discussion between relevant agencies is underway on plans for a reintroduction of the endangered tortoise *Gopherus flavomarginatus* from Mexico into Big Bend National Park Texas (Morafka, personal communication), and for the captive-bred offspring of the world's rarest tortoise (*Geochelone yniphora*) to be used for both an introduction into entirely new habitat and to bolster extant populations (Burke, 1990). Several re-introductions are also being planned for *Sphenodon guntheri* (Daugherty, personal communication). The principal question remains as to whether RRT's are a cost effective method of improving a species' chances of survival. I suggest that generalization based on comparisons of results from a broad mixture of species and RRT techniques is not an appropriate way to resolve this question. Instead, relevant literature for the species under consideration should be reviewed, and the potential for success of an RRT should be considered in a cost/benefit or risk analysis (Price, 1989; Soulé, 1989). No one claims that RRT's are a panacea, but they should be considered an option in any recovery program.

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