Arguing over Little Things: Response to Haddad et al.

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The scientific debate over habitat corridors, played out largely in this journal, has been lively for more than a decade (Noss 1987; Simberloff & Cox 1987; Simberloff et al. 1992; Hess 1994; Beier & Noss 1998; Haddad et al. 2000). Although we think our time would be better spent addressing urgent conservation problems rather than engaging in yet another squabble over corridors, Haddad et al. (2000) have challenged our recent assertion that experiments on the movements of small animals in artificial corridors have little practical value for addressing conservation planning issues on broader scales and involving disparate species. So we feel obliged to respond to their challenge. We think, however, that there is more agreement between what we wrote (Beier & Noss 1998) and what Haddad et al. (2000) suggest than is implied in their paper. We never said corridor experiments are worthless. We explicitly questioned the value of fine-scale experiments based on small-bodied organisms and "artificial corridors" (i.e., those that have no close parallels in nature) for informing us about how to design landscapes for the species most likely to be vulnerable to habitat fragmentation at broader scales—large-bodied animals with large home ranges and low population densities. Hence, it is not experimentation per se, but rather the "little things," about which we disagree.

Conservation biologists need to give more emphasis to areas in which they agree than to areas where they differ. Decisionmakers will not come to us for advice if we are forever embroiled in academic disputes and appear to be incapable of reaching consensus on anything. Haddad et al. (2000) and we agree that experiments and observational studies on corridors are important and complementary. Haddad et. al (2000) suggest that "experiments, combined with observational studies, offer the best test of theory and the most likely source of general principles about the value of corridors in conservation." They strongly emphasize the value of experiments, whereas we stressed the limitations of experiments. Their enthusiasm for testing theory and deriving general principles warrants further comment. From a purely scientific or intellectual standpoint, tests of corridor theory are worthwhile. We question, however, how much ecological theory has contributed or might contribute to real-world conservation. (See the treatment of this issue by Shrader-Frechette and McCoy [1993], who concluded that theory has contributed little.) Theoretical ecology makes for interesting reading and sparks entertaining diatribes after departmental seminars. Sometimes it guides us reasonably well in our quest for knowledge. We are more interested, however, in learning how organisms sensitive to habitat fragmentation respond to landscape features in particular cases and how we might manage landscapes to enhance those features that promote connectivity and otherwise reduce the risk of extirpation. This is what the corridor debate should be about.

A series of case studies may indeed give us Haddad et al.'s desired "general principles"—we prefer the term *empirical generalizations*—which then might be used to guide action in situations where case-specific data are lacking and uncertainty is high, precisely the conditions under which decisions in conservation usually must be made. Empirical generalizations such as "a landscape that maintains natural kinds and levels of connectivity is preferable to a human-fragmented landscape" are well founded on observations from previous case studies, notwithstanding the paucity of evidence that corridors can mitigate the overall effects of continuing habitat loss (Harrison & Bruna 1999). Beyond such generalizations, conservation planners need to consider carefully the autecology and behavior of the species concerned as these relate to the particulars of landscape pattern. Experiments using species with radically different life histories and behaviors in thoroughly different settings are unlikely to provide any guidance beyond incremental support for empirical generalizations. A mouse moving through a strip of grass in a controlled experiment and a cougar (*Puma concolor*) moving through the complex landscape of southern California have little in common beyond similar mammalian physiologies and hormone-

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based drives. We know that individuals of different age, sex, and social status within species may differ in response to potential corridors and other landscape features (Beier 1993, 1995; P. Paquet, personal communication). Differences between species are more dramatic. Ungulates and carnivores, for example, respond very differently to wildlife underpasses in Banff National Park (Clevenger & Waltho 2000).

Haddad et al. (2000) accuse us of dismissing "science's most powerful approach to understanding—the experiment," which is not true. We expressed skepticism about the relevance of most previous experimental studies to the kinds of decisions that must be made about connectivity in land-use planning; we did not dismiss experiments generally. They also accuse us of dismissing "out of hand" the value of experiments with small-bodied organisms; we did not. Rather, we dismissed extrapolations from such studies to disparate spatial scales and situations. By no means did we intend to imply that small-bodied organisms should not be a concern to conservation biologists. Indeed, many little things are quite imperiled (McKinney 1999). We believe that small-bodied organisms are every bit as valuable as charismatic megavertebrates; both groups play critical roles in ecosystems (Wilson 1987; Terborgh 1988). We suggest, however, that systematic, integrated, regional-scale conservation planning (i.e., planning on a scale of thousands to millions of hectares) is usually more efficient than site-by-site planning, as is now well accepted by conservation biologists and practicing conservationists alike (Scott et al. 1993; Christensen et al. 1996; Noss et al. 1997; Ricketts et al. 1999; Margules & Pressey 2000; Poiani et al. 2000). Planning at regional scales involves assessment of biodiversity hotspots, representation of habitats, and the needs of focal species (Noss et al. 1999). Species sensitive to habitat configuration at coarse scales and vulnerable to disruption of connectivity at these scales are usually large-bodied vertebrates with large home ranges. Once a regional plan based, in part, on the needs of these species is developed, one can proceed to determine how smaller landscapes and sites within regions should be designed and managed. This is when more intensive consideration should be given to Haddad's butterflies (Haddad 1999*a*, 1999*b*), Rosenberg's salamanders (Rosenberg et al. 1998), and other "local-scale species" (Poiani et al. 2000).

We do not wish to disparage experimentation, just as Haddad et al. (2000) "do not wish to disparage observational studies of corridors." We agree wholeheartedly that the two approaches are complementary. Where we disagree is over the value of the "experimental model systems" approach (Ims et al. 1993), in which results from experiments at fine scales, using small-bodied species, are extrapolated to predict responses of large-bodied species at vastly coarser scales. We implore conservation biologists to learn more about the species-specific behaviors and anatomical and physiological constraints that determine use of corridors and other habitat features; this will require renewed, vigorous attention to natural history in our universities and elsewhere (Noss 1996).

Haddad et al. (2000) propose that "the challenge for conservation biology is to uncover general principles that predict behavioral and population responses to corridors across species and landscapes." We see a different and more pressing challenge. The general principles or empirical generalizations of conservation biology are already rather well established. Now we need to explore the details of particular species living out their lives—or failing to—in real landscapes, then apply what we have learned to the solution of urgent problems. The academic exercise of quibbling over theory and how one interprets experimental results does little to advance the real work of conservation.

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