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Fusion or Failure? The Future of Conservation Biology

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As many of the papers in this special section celebrate, the field of conservation biology has achieved a great deal in the 20 years since the journal bearing its name first appeared (and indeed over the 37 years since the first issue of *Biological Conservation*). The magnitude of the crisis facing wild populations and places has been better documented, threats to individual species and habitats elucidated, priority areas systematically identified, and remedial interventions proposed and in many cases put into practice. Yet with global rates of loss of populations and biomes probably averaging between 0.5 and 1% each year and with threats from unsustainable consumption, population growth, and greenhouse gas emissions continuing to rise (Harrison & Pearce 2000; UNEP 2002; Balmford et al. 2003; Mace et al. 2005), it is clear that although we may be winning a few battles, we are still losing the war. With perhaps only another 20 years or so left to turn the tide, it is worth asking why.

Unlike some (Whitten et al. 2001), we do not believe that this is the fault of conservation biology per se. We do not see the field as "a displacement activity for academia"—conservation biologists do what they do (often at the expense of giving up higher-impact research in other areas) because they are passionate about making a difference. Conservation as a whole is losing the war not because of any lack of commitment or focus but because of the sheer scale, growth, and complexity of the problems it faces. We think research has a vitally important role in finding solutions to these problems, but biology alone cannot provide all the answers. In some circumstances (such as managing populations in captivity or habitats in well-protected reserves) biological insights may be enough. But in the great majority of situations much more is needed. If we want to move beyond documenting losses or identifying specific causes of decline to understanding their underlying drivers and implementing interventions on anything other than a piecemeal basis, we need to undergo what one of us describes as "an epiphany for . . . natural scientists" (Cowling 2005): the realization that conservation is primarily not about biology but about people and the choices they make.

This in turn means that the key to increasing the future contribution of biologists to on-the-ground conservation interventions lies in accepting that reality and in working much more closely with experts from other disciplines, especially the social sciences. This is of course not a new insight (Hilborn & Ludwig 1993; Daily & Ehrlich 1999; Mascia et al. 2003). But at a time when some are calling for the recognition of new biological subdisciplines within the field (e.g., Whittaker et al. 2005), we think it is worth reexamining the case for looking outward rather than inward. For us, one very useful device has been to come up with a personal and doubtless eclectic list of 10 major and interlinked challenges (italicized) we think will become increasingly important aspects of conservation biology over its next 20 years.

Some of the problems we have selected are largely biological. For example, we suggest there is a clear need for *improving our understanding of how ecosystems change* in response to anthropogenic pressures. How widespread can we expect trophic and other cascades to be as the relative abundance of different species within systems alters (Paine & Schindler 2002; Springer et al. 2003; Frank et al. 2005)? Will ecosystem responses to pressures typically be nonlinear, with periods of little apparent impact giving way to sudden (and potentially irreversible) regime shifts (Scheffer et al. 2001; Folke et al. 2004)? And how can we better predict where thresholds for such shifts lie?

Linked with these questions, a great deal of biological input is needed to plan for persistence by explicitly *trying to conserve ecological and evolutionary processes*. Until recently, conservation planning focused largely on seeking to represent current patterns of biodiversity in as little area as possible (Margules & Pressey 2000). But maintaining diversity into the future clearly depends on

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conserving a set of underlying ecological and evolutionary processes too. Although several studies have tried to map out and then capture spatial surrogates for some of these (e.g., Cowling & Pressey 2001), how we go about spatially identifying and then conserving many other key processes remains poorly understood.

Implementing answers to these research questions will, of course, require working with practitioners from other disciplines, but for the other topics we considered, interdisciplinary collaboration is vital even at the research stage. For instance, conservation planning as a whole faces an implementation crisis (Knight et al. 2006*a*); although tools for systematic area-based planning and plans themselves have proliferated, very few have yet led to changes in land use on the ground. In our view this is because all too often research biologists interested in planning have worked in isolation. *Putting conservation plans into practice* requires close involvement from the outset with implementing agencies and other stakeholders, but it also needs biological analyses to be integrated with research on the economic and social consequences of conservation and on the institutional landscape for implementation (Pierce et al. 2005). As some of the ongoing South African bioregional planning programs illustrate (Knight et al. 2006*b*), these broader approaches are beginning to yield promising results.

Perhaps the most compelling evidence of the practical benefits of interdisciplinary conservation research comes from the emergence of ecological economics. By fusing perspectives from the natural and social sciences about the value of natural capital, this field of study has already begun to influence policy in organizations at many levels, from local to national governments and even the World Bank (e.g., Chomitz et al. 1999; Daily & Ellison 2002; Carret & Loyer 2003; Pagiola 2003; Millennium Ecosystem Assessment 2005). That said, we believe these positive impacts only serve to reinforce the need for much more work *documenting and understanding the benefits of natural systems for human well-being*. In our view, to be of greater use to policy makers we need many more examples, from a very broad range of countries, biomes, and services, that are underpinned by a stronger ecological and socioeconomic understanding of how individual services are delivered (Ekins et al. 2003; Kremen 2005). This work needs to express benefits and costs of conservation in different currencies (e.g., money, lives, votes), identify winners and losers, and suggest how knowledge of gains and losses can be used to devise equitable conservation interventions. Moreover, these services need to be mapped and included in assessments designed to identify conservation priorities because this will greatly increase the array of individuals and sectors available to implement the outcomes of such assessments (Molnar et al. 2004; Pierce et al. 2005). This research will require inputs not just from biologists and economists but also from health professionals, geophysicists, geochemists, sociologists, and anthropologists.

Equally diverse contributions are needed to address other emerging challenges. *Mainstreaming conservation into the everyday decisions of the business and public sectors* has in many ways only just started (Pierce et al. 2002). Although drawing on insights from ecological economics and related fields, its progress is likely to depend on new research into the impacts of conservation-friendly management choices on business performance and policy outcomes, as well as biodiversity. A major challenge in this regard is to persuade institutions in all spheres to adopt the strong sustainability development model—one that acknowledges that the foundation for social and economic sustainability should be a healthy environment—achieved by conserving those features of our natural environment that cannot be replaced by manufactured capital (Orr 2002; Dawe & Ryan 2003; Ekins et al. 2003)

Extensive collaborations with social scientists are also fundamental to better *monitoring and communicating the changing state of nature*. Recent assessments of progress toward the Convention on Biological Diversity's 2010 target for significantly slowing biodiversity loss concluded that we need far better spatial, geographical, and taxonomic coverage of changes in the quantity and quality of populations, habitats, and the benefits they confer on society (Balmford et al. 2005*b*; Green et al. 2005*b*; Pereira & Cooper 2006). Addressing this shortfall requires greatly increased inputs not just from field biologists but also from statisticians, the remote-sensing community, and social- and earth-systems scientists. On top of this, maintaining political commitment to this crucial target (and to similar targets in the future) will require close work with media experts to explore innovative approaches to raising public awareness.

Developing sophisticated approaches to communicating conservation findings is also likely to become an important part of *assessing and improving the success of conservation interventions.* Several initiatives—catalyzed by donors, practitioners, and facilitators such as Foundations of Success (http://www.fosonline.org/Site_Home.cfm)—have begun applying ideas from management science for improving the systematic design, implementation, and evaluation of conservation projects (e.g., Salafsky & Margoluis 1999; Salafsky et al. 2002; Belokurov et al. 2003). Quantitative analyses conducted with the resulting tools should help identify what works and what doesn't. Working with media experts could then improve the dissemination of results not just to the conservation community but also to a public becoming (we suggest) increasingly turned off by the idea that conservation is all about bad news.

We also see a growing need for interdisciplinarity in the core area of conservation biology—*tackling the major threats of overexploitation, habitat loss, climate change,*

and the spread of alien invasive species. In our view making progress against any of these will hinge on improved understanding of their nonbiological components. For instance, developing effective responses to climate change will require closer collaboration with climatologists to identify at the landscape scale what areas species will need to remain within their climatic realms (Hannah et al. 2002). Likewise, making progress on habitat conversion will in part depend on biologists working with agricultural scientists to resolve whether wildlife's needs are better met by extensification (which risks requiring more farmland) or by intensifying production on already-cleared land (see Green et al. 2005*a*).

Our final two challenges call for work that is barely under way. One, which cuts across just about all the others, is *building overarching, spatially explicit models of wild nature*—how wild nature is generated and maintained, and how it affects and is in turn affected by people (Balmford et al. 2005*a*). Such models require inputs from across the biological, physical, and social sciences and have to be constructed iteratively, with successive rounds of testing and improvement. But they would provide an invaluable framework for guiding monitoring efforts, understanding threats and how they interact, devising interventions to address them, and informing policy makers about the likely consequences of different courses of action. Climate-change models offer a compelling illustration of how influential such efforts can be.

Last, we see a great need for interdisciplinary efforts to tackle perhaps the most pervasive underlying threat of all by *reconnecting people and nature*. In our view even if all the other building blocks of effective conservation are in place, we will not succeed unless the general public cares, and they are unlikely to care enough if they no longer experience nature directly (Pyle 1993; Nabhan & Trimble 1994; Miller 2005). In the face of rapid urbanization and the explosion of sedentary entertainment in the form of television, electronic games, and the Internet, the proportion of time people spend in wild places, or even gardens, is decreasing dramatically, as is their familiarity with their natural heritage. Reversing these trends and encouraging people to care is an enormous but in our view inescapable challenge. These issues are being targeted by the emerging field of conservation psychology (Saunders 2003; Saunders et al. 2006 [this issue]), but alongside psychologists, inputs from educators, health scientists, economists, architects, marketing scientists, and conservation biologists will also be vital.

Readers will doubtless identify other major challenges besides our 10, but we suggest these too are likely to be characterized by a strong need for interdisciplinary research. Because of this, we hope that the next 20 years will see the progressive blurring of the edges of conservation biology into a broader conservation science and the emergence of several new transdisciplines—the products of disciplinary fusion and consilience (Wilson 1998; Max-Neef 2005)—that will enable us to tackle the enormous problems our species and all others now face.

Yet as we know, moving toward greater interdisciplinarity will not be easy. Given that nearly all conservation researchers to date have been trained in a single discipline, there are significant barriers between us (even within the natural or the social sciences) in terms of having different values and divergent views about what constitutes a useful answer to a problem (Campbell 2005; Lélé & Norgaard 2005). There are also practical difficulties—in language and in funding and publishing interdisciplinary work, for example. Breaking down these barriers will be extremely hard. We will need to form functioning interdisciplinary teams led by people with well-developed emotional intelligence, strong interpersonal skills, and an ability to focus on collective rather than personal goals (Goleman 1994). We also need to train genuinely transdisciplinary thinkers (Max-Neef 2005), which in turn will require us to break down traditional divisions within tertiary education. Above all, we will have to disperse from our familiar academic habitats into new and unfamiliar ones. We strongly believe, however, that the benefits of such dispersal, and of the outbreeding that will come with it, will be immense.

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