An African Conservation Agency's Perspective on Advocacy

Tracy and Brussard (*Conservation Biology* **10:**918–919) warn against the relaxation of scientific principles, contending that advocacy in scientific journals could result in a move away from scientific principles toward dogma. It is essential that conservation biologists maintain scientific standards (Murphy 1990), but does advocacy necessarily compromise these standards and do we have a choice not be advocates (Joslyn 1995)? We contend not.

We agree with Brussard et al. (1994) that we "are obliged to project as (our) goal not value-free or value-neutral science, but *unbiased experimental design and analysis*" (our emphasis). But the role of many conservation agencies is, in part, to inform and assist public groups in the wise use and conservation of biodiversity. Therefore, our job demands advocacy. So in addition to biology we may have to include sociological and political issues; decisions involving the latter may be based on incomplete information. This is where we need to be careful and base our viewpoints on the best science available.

Any research that we undertake must follow the rigors of the scientific method (Murphy 1990). The methods and results sections of peer-reviewed scientific publications must clearly show how the data were obtained and analyzed. Ensuring that this is done is incumbent on the authors, the reviewers, and the editorial boards of the journals. Advocacy, part of our job description, can appear in the discussion of the paper, but it must be based on the science presented. Again, ensuring that advocacy is not based on dogma is the responsibility of the authors, reviewers, and editorial boards.

The alternative of keeping scientific articles free of advocacy may be problematic. Presumably, the authors would then promote their particular viewpoints at a different forum. We maintain that it is better to promote a scientifically supported viewpoint in a journal because journals are peer-reviewed. By so doing we may be able to prevent a move toward dogma.

We write from the perspective of scientists employed by conservation agencies. We contend, however, that all conservation biologists can and should adopt this approach.

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¡Viva Caughley!

Conservation biology has a pendulum or two, as many fields do. One of these pendulums swings between those who feel that population viability analysis (PVA) and the genetics of small populations should be central issues in conservation biology and those who believe that most populations recover quickly and healthily if the factors that reduced them in the first place are removed. Graeme Caughley (1994) clearly outlined this dichotomy and was part of the swing away from an earlier emphasis in conservation biology on the genetics of small populations. Some are swinging back (Hedrick et al. 1996). Since Graeme Caughley is not able to respond to Hedrick et al.'s criticism of his paper, we respond instead, perhaps less eloquently and knowledgeably than Caughley would.

Hedrick et al. have constructed a straw man. They suggest that Caughley's distinction between the "small population paradigm" and the "declining population paradigm" is a false dichotomy. Caughley's point was that users were themselves creating a false dichotomy by ignoring process (the province of declining population paradigm) in favor of predictions of outcomes from current demographic parameters alone (small population paradigm). As Hedrick et al. say, and Caughley before them, when understanding of process is incorporated into sensible modeling of outcome, PVAs can be, and have been, useful. Caughley said this; Hedrick et al. quote him as saying so; Caughley gives examples of successful PVAs; and Hedrick et al. give a few more. We see little or no disagreement here.

Nevertheless, Caughley had some criticisms we think might still be valid. First, models are perhaps best used for sensitivity analysis, both of the models and of management techniques, rather than to predict outcomes (Wennergren et al. 1995), and yet do not users of PVAs (who tend to be managers, not biologists or modelers) still tend to pay more attention to the predictions (the population has a 50% risk of extinction in 100 years) than to the sensitivity analysis (predictions are worthless until we know more about young female adult survival)? Second, at the time Caughley wrote, there were few widely known studies that showed individuals (much less populations) of any species suffered from (as opposed to merely experienced) genetic homogeneity or inbreeding in nature. Most of the genetic worry came from knowledge of breeding of

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captive wild animals and of domestic animals, especially the latter. Now, increasing numbers of studies are finding genetic problems in small wild populations (Frankham 1995*a*,*b*), and we know there are theoretical reasons why we might expect a lag before sudden, severe genetic problems are experienced (Frankham 1995*a*,*b*). Nevertheless, it seems that most cases of population decline (as opposed to declines in individual fitness) attributed to inbreeding are only weakly inferential (Frankham 1995*a*). Stronger examples are much rarer (see Vrijenhoeck 1994 and Frankham 1995*a*).

Some populations appear to have survived very well after presumed extremely narrow bottlenecks: it has been suggested that the cheetah thrived for hundreds of generations since a major one, or even two bottlenecks (O'Brien et al. 1987). Similarly, several of the examples cited by Hedrick et al. in support of the importance of inbreeding for population viability are from populations that are stable despite lowered individual fitness (Packer et al. 1991) or populations that experienced many years of stability (Pettersson 1985) or growth (Wayne et al. 1991) after isolation only to crash many generations later for undetermined reasons. We still need to explain such cases.

Compare these examples to the many undisputed instances of populations suffering severe declines because of factors that were not attributed to genetic underpinnings, including drought, severe winters, disease, predation, succession, and of course human factors such as habitat loss, overexploitation, and introduced species (Primack 1993; Young 1995). That factors other than genetic problems can cause populations to decline is absolutely not to say there are no genetic limits to population viability. We still do not know, however, if the genetic factors are likely to be as important as other factors, even after allowing for the catch-all caveat that some population declines attributed to other factors may be related to genetic deficiencies.

The whole PVA enterprise, which is not cheap, could do with more critical analysis of itself. Although success stories exist, what about failures? And we need to ask of the successes: (1) Would the actions taken not have occurred without such analysis? (2) How successful were the actions in increasing measurable population fitness (other than via untested parameters in the models themselves [i.e., genetic diversity])? (3) Did the actions taken based on PVA shift resources away from actions that would have been more beneficial to population survival?

Graeme Caughley was not criticizing the PVA enterprise as a whole. How could he? He had been applying the equivalent of what we now call PVAs years before many of those apparently stung by his article. Caughley was criticizing the unthinking, blanket application of modeling of demographic outcome without due account of process of declines that was so prevalent in conservation biology in the 1980s. As far as we can tell from Hedrick at al.'s essay, and their choice of PVAs, they make exactly the same criticism. Perhaps now that some of the sting from Caughley's article has died down, we can agree that we need application of the understanding of both the processes by which populations respond to threats and the processes involved in the vulnerability of small populations to extinction.

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The attack by Hedrick et al. (1996) on Graeme Caughley is long in coming but not unexpected. Caughley proposed the idea that there are two paradigms within conservation biology: the declining population paradigm and the small population paradigm (Caughley 1994; Caughley & Gunn 1996). Hedrick et al. claim that such a distinction is "overly simplistic" and "should not be perpetuated," given that "hostile political forces are attempting to discredit many conservation efforts."

Despite our fear that we too will be brought before the Inquisition on charges of heresy, we suggest that Caughley's categories are not simple enough. The basic distinction in conservation biology is between field biologists and lab scientists. Caughley is clearly one of the former, whereas Hedrick et al. represent the latter. In our view computer modelers are as much lab scientists as geneticists.

The chasm between field biologists and lab scientists is clearly apparent in Hedrick et al.'s critique of Caughley. Hedrick et al. give numerous examples of the application of the small population paradigm to conservation problems without evaluating the success of these applications (see Caughley & Sinclair 1994). Hedrick et al. give an example involving predation by ravens on tortoises. They claim that Caughley would observe the salient instances of raven predation and assume this was the causal factor in the decline of tortoises. But according to Hedrick et al. (p. 1315), "one cannot always interpret the significance of deterministic factors unless a proper inclusive PVA is carried out." There is a delicious irony here in that, in criticizing Caughley, Hedrick et al. reveal that it is they who misunderstand the nature of scientific evidence. A population viability analysis (PVA) is simply a model of the system of interest. Manipulating variables in a computer is not a test of causal factors; one actually has to go out in the field and conduct an experiment in order to evaluate "how the ecology implied by the model differs from the ecology of real populations" (Caughley & Gunn 1996: 208). If Hedrick et al. were familiar with Graeme Caughley's work they would know that Caughley always stressed the need to diagnose all the probable causes of decline and then employ direct manipulative field experiments to test hypotheses (Caughley & Sinclair 1994).

The distinction between field biologists and lab scientists is a function of another distinction involving time and money. Field biology takes a great deal of time and there's no money in it. Lab techniques take next to no time and there's a great deal of money to be had. We do not suggest that lab scientists don't work long hours. Rather, lab techniques produce publishable results in a matter of weeks, whereas data in field biology usually take years to accumulate. This explains the greater publication rate among lab scientists. If we take as a starting point Soulé and Wilcox's (1980) book *Conservation Biology: An Evolutionary-Ecological Perspective*, it has taken 16 years for field biologists, namely Graeme Caughley and Anne Gunn (1996), to find the time to write the first textbook in conservation biology presented from the field biologist's perspective.

Why do the lab scientists have almost all the money? Because advanced capitalist economies are driven by the production and consumption of high-tech gadgetry (Galbraith 1972). Field biologists generally employ pre-industrial technologies like cage traps and bits of string to mark quadrats. Wildlife biologists that employ radiotelemetry are seen as "high-tech," but production of telemetry equipment is based on providing low-cost copies of existing high-tech technologies. In contrast, lab scientists employ the newest technologies produced by the largest transnational corporations. Geneticists are firmly ensconced in the medical-industrial complex.

Hedrick et al. claim that dissent among conservation biologists must be silenced for fear of unnamed "political forces." We think it far more productive to examine the relationship between the domination of conservation biology by lab scientists and the obvious political force wielded by transnational corporations. Grant allocations are made on the basis of policies set by government and industry. For public relations purposes, conservation concerns may be given the status of a subsidiary policy goal. The primary policy goal will always be to promote the production and consumption of advanced technology (Galbraith 1972). By providing grants to lab scientists, government and industry can achieve both policy goals. Grants to field biologists might be better in achieving the public relations goal of conservation, but they provide no benefit in terms of the primary goal of promoting new technology.

Contrary to government and industry, most conservationists (scientists and members of the general public alike) see conservation as the primary social goal for our time. By clearly documenting the existence of a dichotomy in conservation biology between the paradigms of "field biologist" and "lab scientist," Caughley has given conservationists the ability to perceive the degree to which the lab scientist is pursuing the goal of advancing technology rather than the goal of conservation. Caughley has opened our eyes, and Hedrick et al. worry that we might see that the emperor has no clothes.

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