Ten Years after "Reliable Knowledge": Are We Gaining?
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SPECIAL SECTION

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"There are no applied sciences. . .there are only applications of science and this is a very different matter. . .The application of science is very easy to anyone who is the master of the theory of it."

Louis Pasteur (1871). *Revue Scientifique.*

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Increasingly, The Wildlife Society journals reflect healthy discussion about the reasons and ways that science is conducted (e.g., Capen 1989, Matter and Mannan 1989, Romesburg 1989, Yahner 1990). Understandably, wildlife biologists and managers want to be among those to whom decision makers turn for advice about wildlife conservation and management (Mackie 1990: 42, 1991:3). Reliable advice, in turn, rests squarely upon the results of "good science." No one questions, then, whether to encourage "good science," and wildlife scientists, like others (Murphy 1990), need to ensure that science is conducted in a manner most likely to yield reliable knowledge and stand up to scrutiny (Murphy and Noon 1991).

Science is judged by peer review at 2 points in the course of research: when the research is designed and support is solicited, and when results are submitted for publication. It is with the latter that wildlife biologists and managers seem concerned principally, perhaps because credibility hinges on the quality of research as reflected by publications (Baskett 1985). Thus, any discussion about what constitutes "good science" has obvious ramifications for peer review and the editorial process (Baskett 1985:189). In many cases, research deemed acceptable to fund will not be published. But when publication is considered, editors and referees can (and do) use some different criteria to judge acceptability for publication than those used to judge acceptability for funding, for the former criteria necessarily deal with whether science is "good."

These are critical times for wildlife biologists and managers. They are (1) being called upon increasingly to be relevant and responsive to issues raised by both nonconsumptive and consumptive wildlife interests, (2) expected to become involved in complex sociopolitical and environmental issues in which traditional wildlife interests might constitute only a part, and, last but not least, (3) challenged with respect to the reliability of some of the "principles"of wildlife management. Established and new societies more and more are becoming involved in issues relating to the conservation and management of wildlife (Thomas and Salwasser 1989, Brussard 1991)—heretofore the almost exclusive domain of wildlife biologists and managers. So, the issue is important whether biologists and managers work on game or nongame species, belong to different scientific societies, or are employed as academics, by government, or by other organizations.

Ten years ago, Romesburg (1981) argued that much wildlife science, at least until that time, was compromised with respect to providing the reliable knowledge required to make management decisions. He offered that "good science" should be that best able to provide reliable knowledge and is, therefore, based on the hypothetico-deductive (H-D) method. The H-D method, as normally described, employs 3 steps: observation/induction, hypothesis formation, and experimentation. Romesburg (1981) pointed out that some accepted knowledge about wildlife might be better considered untested hypotheses about observations. In other words, wildlife research tended to go through the first 2 steps but left out the last one (Matter and Mannan 1989:1173). As a consequence, hypotheses advanced to account for observations might gradually evolve into explanations for them through a process Romesburg (1981) called retroduction.

Romesburg (1981) illustrated his thesis with a contentious example. Errington (1945) proposed that wildlife populations annually produced individuals doomed to die anyway, and that harvesting these individuals would not alter population size the next year. This hypothesis

became an undisputed cornerstone of wildlife management. Romesburg (1981) contended that Errington's hypothesis had never, in fact, undergone a critical test. Romesburg (1981) *did not* say that the hypothesis was "wrong," or even that there do not exist data consistent with its predictions. He merely pointed out that, in the absence of a test, there was really no good way to tell how reliable the notion might be. Whether research has, in this particular example, produced results consistent with predictions of Errington's compensatory mortality hypothesis, does not diminish the importance of Romesburg's (1981) observation that it had not been tested.

Wildlife scientists are no different from others when it comes to dealing with biases. However, wildlife science is frequently predicated on the idea that, at least implicitly, the results of research will be "useful" in some management context. Yet, definitions of wildlife management necessarily contain elements of subjectivity. Biologists or managers might decide what a goal of management might be (e.g., more deer) and conduct research to try to find out *how* to achieve the goal, or to justify it (Peterson 1991). The view that scientists should adopt advocacy stances (Lovejoy 1989) is widespread in society at large and is a serious misconception about the principal purpose of the scientific process. Science, rather, is a way of knowing (David 1975, Moore 1985). Romesburg (1981) offered that wildlife scientists infrequently ask *why* systems behave as they do (Gavin 1991) and, as a result, seldom question *whether* management goals are "correct," or even achievable (see also Gavin 1989). Romesburg (1981) contended that the means to deal with subjectivity, bias, and the acceptance of untested hypotheses as facts is to test hypotheses about the systems that wildlife scientists study with explicit experiments.

Some wildlife scientists receive this suggestion coolly. Challenges mounted to the H-D method can be reduced to 3 major types: (1) nothing is yet known about a system, so hypotheses are not apparent, (2) funding agencies do not support tests of hypotheses, and (3) the H-D method is impossible if experiments are impractical (see also Matter and Mannan 1989). Thus, 10 years after Romesburg's (1981) paper appeared, we are not sure that the H-D method is widely understood, considered, or practised as a means to acquiring reliable knowledge. It was in this climate that the idea for these peer-reviewed essays was born, with a goal to further discussion and to catalyze gains in reliable knowledge in wildlife science. We solicited essays from individuals who are academics, employed by government, and belong to different scientific societies and who, in our experience, had interests in the general topic about science in relation to management.

To the first challenge to the H-D method, the answer is fairly straightforward: if few data exist, then more are required, and there will always be room for them. Examples include information about habitat use or diets of rare or endangered species. However, wildlife science now requires fewer of the same kinds of food habits papers about the same kinds of species (Gavin 1989, Hunter 1989). Nevertheless, there is opportunity, even in relatively short-term research projects like those which result in graduate theses, to go beyond diet description to test hypotheses about *whether* and *why* food selection occurs (Keppie 1990, Sinclair 1991).

To the second challenge to H-D: attitudes of administrators in many agencies are changing. Examples include the U.S. Forest Service which has embraced the concept of adaptive resource management (Walters and Holling 1990)—in essence, management by experimentation, evaluation, and new experiments (Macnab 1983)—and the legislated evaluation of projects that receive funding under the North American Waterfowl Management Plan. Evaluations amount to tests of hypotheses that management has the effects it is supposed to. Further, administrators should be amenable to suggestions about why they need to spend money on controls and replicates, and over longer periods of time, because the H-D method recognizes and addresses their concerns—it explicitly evaluates the cost-effectiveness of management.

The answer to the third challenge to H-D is not as direct. The argument in support of this notion is founded on the assumption that "doing H-D" means doing only manipulative, controlled, replicated experiments. Because perfect experiments are often impractical or lack realism, the argument is made that the H-D method is inappropriate. But this argument rests on an overly narrow definition of experimentation. It does not recognize that what might be more critical than the kind of experiment employed is the attempt to falsify hypotheses and erect better ones (Sinclair 1991). Hypothetico-deductive research is not characterized by whether it

is experimental, because hypotheses can be tested with data not collected by experiment. This will have consequences for the reliability of the knowledge that results (because some best possible designs for data collection are more constrained than others), but even constrained H-D research should lead more efficiently to reliable knowledge than will the alternatives (Nichols 1991). Further, discussion resulting from Romesburg's (1981) paper seems to have centered on a use H-D versus do not use H-D dichotomy, but simply testing hypotheses will not make for reliable knowledge if inadequate attention is paid to details such as using unbiased sampling techniques, collecting adequate numbers of samples, and employing appropriate statistics. A variety of approaches to large-scale, field experimentation and appropriate statistical approaches are available (Matson and Carpenter 1990, Eberhardt and Thomas 1991); a nice example of H-D research and field experimentation also appears in this issue (Copeyon et al. 1991).

In the essays that follow, the authors elaborate on 2 major themes in Romesburg (1981). First, the results of research need to be able to stand up to the question "So what?," and such research will frequently be predicated with questions that begin with "why" or "whether" rather than "how" and "what." Second, wildlife research should expend more effort to test hypotheses by experimentation. Tom Gavin elaborates on his earlier thesis (Gavin 1989) about what kinds of questions wildlife biologists should consider asking, and Tony Sinclair discusses why wildlife science and management are not really separate pursuits. Dennis Murphy and Barry Noon write about the challenges from other quarters in society to knowledge about wildlife and about how the H-D method can help to meet those challenges in the context of forestry–spotted owl (*Strix occidentalis caurina*) interactions. Markus Peterson explores how policy is influenced by perspective, and the ramifications for deciding policy based on untested hypotheses, with reference to the effects of brucellosis in bison (*Bison bison*) and elk (*Cervus elaphus*) populations. Finally, Jim Nichols reviews the evidence for the effects of hunting on American black ducks (*Anas rubripes*) and the consequences of constrained experimental designs.

So, we are gaining. The growing literature about science and the scientific method in wildlife biology, conservation biology and, indeed, many other disciplines that deal with natural resource conservation and management testifies to these gains. Further gains will be made when, first, more consideration is paid to adopting the H-D method at the design stage of research and research funding is made conditional on it, and second, (as Louis Pasteur's quote suggests), less distinction is made between science and management as different and competing processes.

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LITERATURE CITED

BASKETT, T. S. 1985. Quality control in wildlife science. Wildl. Soc. Bull. 13:189–196.

BRUSSARD, P. F. 1991. The role of ecology in biological conservation. Ecol. Appl. 1:6–12.

CAPEN, D. E. 1989. Political unrest, progressive research and professional education. Personal reflections. Wildl. Soc. Bull. 17:335–337.

COPEYON, C. K., J. R. WALTERS, AND J. H. CARTER. 1991. Induction of red-cockaded woodpecker group formation by artificial cavity construction. J. Wildl. Manage. 55:549–556.

DAVID, E. E., JR. 1975. One-armed scientists? Science 189:679.

EBERHARDT, L. L., AND J. M. THOMAS. 1991. Designing environmental field studies. Ecol. Monogr. 61:53–73.

ERRINGTON, P. L. 1945. Some contributions of a fifteen-year local study of the northern bobwhite to a knowledge of population phenomena. Ecol. Monogr. 15:1–34.

GAVIN, T. A. 1989. What's wrong with the questions we ask in wildlife research? Wildl. Soc. Bull. 17: 343–350.

———. 1991. Why ask "why": the importance of evolutionary biology in wildlife science. J. Wildl. Manage. 55:760–766.

HUNTER, M. L., JR. 1989. Aardvarks and Arcadia: two principles of wildlife research. Wildl. Soc. Bull. 17:351–354.

KEPPIE, D. M. 1990. To improve graduate student research in wildlife education. Wildl. Soc. Bull. 18:453–458.

LOVEJOY, T. 1989. The obligations of a biologist. Conserv. Biol. 3:329.

MACKIE, R. J. 1990. President's corner. The Wildlifer 242:42.

———. 1991. President's corner. The Wildlifer 244:3.

MATSON, P. A., AND S. R. CARPENTER. 1990. Statistical analysis of ecological response to large-scale perturbations. Ecology 71:2037.

MATTER, W. J., AND R. W. MANNAN. 1989. More on gaining reliable knowledge: a comment. J. Wildl. Manage. 53:1172–1176.

MACNAB, J. 1983. Wildlife management as scientific experimentation. Wildl. Soc. Bull. 11:397–401.

MOORE, J. A. 1985. Science as a way of knowing. Am. Zool. 25:1–155.

MURPHY, D. D. 1990. Conservation biology and scientific method. Conserv. Biol. 4:203–204.

———, AND B. R. NOON. 1991. Coping with uncertainty in wildlife biology. J. Wildl. Manage. 55:773–782.

NICHOLS, J. D. 1991. Science, population ecology, and the management of the American black duck. J. Wildl. Manage. 55:790–799.

PETERSON, M. J. 1991. Wildife parasitism, science, and management policy. J. Wildl. Manage. 55: 782–789.

ROMESBURG, H. C. 1981. Wildlife science: gaining reliable knowledge. J. Wildl. Manage. 45:293–313.

———. 1989. More on gaining reliable knowledge: a reply. J. Wildl. Manage. 53:1177–1180.

SINCLAIR, A. R. E. 1991. Science and the practice of wildlife management. J. Wildl. Manage. 55: 767–773.

THOMAS, J. W., AND H. SALWASSER. 1989. Bringing conservation biology into a position of influence in natural resources management. Conserv. Biol. 3:123–127.

WALTERS, C. J., AND C. S. HOLLING. 1990. Large-scale management experiments and learning by doing. Ecology 71:2060–2068.

YAHNER, R. H. 1990. Wildlife management and conservation biology revisited. Wildl. Soc. Bull. 18:348–350.

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WHY ASK "WHY": THE IMPORTANCE OF EVOLUTIONARY BIOLOGY IN WILDLIFE SCIENCE

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Abstract: The kinds of questions we ask in wildlife biology are at least as important as the methods we use to get answers to questions in research. In this essay, I urge wildlife biologists to vigorously pursue "why" questions rather than "how" questions or descriptive studies that should serve only as a starting point for our investigations. Behavioral ecologists are currently involved in a debate over explanations for biological phenomena called "levels of analysis": how many are there, what terms and definitions apply to each level, and the importance of clearly identifying which level an explanation emanates from given that there are correct explanations for the same phenomenon at each level. Asking "why" questions should lead the wildlife biologist into the realm of evolutionary biology and should place greater emphasis on understanding spatial and temporal variability in reproductive success and survival of wildlife species. I argue that our most useful insights about populations and communities should develop from long-term studies of this type.

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In an earlier essay (Gavin 1989), I encouraged wildlife biologists to ask "why" questions, rather than to dwell strictly on descriptive relationships about wildlife and their habitats. My point is somewhat different from the other essays in this section, which emphasize the approach used to address research questions. They uniformly support the use of the hypothetico-deductive (H-D) method as recommended by Romesburg (1981). Nichols (1991) and Sinclair (1991) provide a particularly thorough review of many of the points made by Romesburg (1981). Although I strongly agree that we should aspire to the ideals of the H-D method, it would be redundant to reiterate a description of the method and supportive arguments already made by my colleagues.

My goal in this paper is to encourage greater emphasis in wildlife biology on the ultimate or evolutionary causes for the phenomena we observe in nature; by definition, this will force us to consider questions we have avoided in our research programs, possibly because we thought they were the sole responsibility of behavioral ecologists or evolutionary biologists who study basic biological problems. Ideally, it seems to me that if we understood *why* animals behave the way they do as individuals, then our understanding of dynamics at the level of the population would be more insightful. There is prob-