Deductive and Inductive Methods of Accumulating Reliable Knowledge in Wildlife Science

FRED S. GUTHERY,[1] *Department of Natural Resource Ecology and Management, Oklahoma State University, Stillwater, OK 74078, USA*

ABSTRACT My goal was to compare deductive and inductive methods of accumulating reliable knowledge in wildlife science. Under the hypothetico-deductive (H-D) method, observations are used to formulate explanatory or causal hypotheses, which serve as the basis for deductions (predictions) of expected events. Field experiments are designed to determine whether the deductions hold, in which case hypotheses are tentatively accepted or otherwise rejected. The H-D method provides the only way to test research hypotheses, but in field ecology it can lead to ambiguity and error. The method: 1) does not preclude confusion of correlation and cause, 2) might perform deceptively in multiple-cause venues, 3) is algorithmically blind to the fact that different hypotheses can lead to the same deduction, and 4) lacks an impartial means of determining whether a deduction has been observed and, therefore, whether a hypothesis is meritorious. Under the process of induction, the results of a study are presumed to hold generally and taken as knowledge accordingly. Induction is much maligned by logicians and philosophers, and wildlife scientists have built false knowledge inductively. However, wildlife scientists have auxiliary knowledge such as facts of natural history to screen inductions for validity. Both the H-D method and induction have important roles in the accumulation of reliable knowledge in wildlife science. (JOURNAL OF WILDLIFE MANAGEMENT 71(1):222–225; 2007)

DOI: 10.2193.2006-276

KEY WORDS deduction, hypothetico-deductive method, induction, wildlife science.

There is a tradition of opposition between adherents of induction and of deduction. In my view it would be just as sensible for the two ends of a worm to quarrel.—Alfred North Whitehead (cited in Peirce 2000:1340).

Like all human enterprises, wildlife science is a culture whose members perceive proprieties and improprieties in conduct. The philosophers of science, who tend to see physics as science (with deference to Charles Darwin), have laid down the proprieties. One is the notion that, philosophically, induction holds no claim whatsoever on truth (Howson 2000). The basic idea is that it is illogical to generalize from the results of a particular study to a general situation. Likewise, if a set of studies yields the same generalization, there remain no logical grounds for presuming truth has been discovered. Despite the misgivings about induction, it remains the workhorse of wildlife science (Romesburg 1981). Indeed, induction is common in our journals as often illustrated by the management implications section, which operates, to some degree, under the premise that the results of a particular study will hold in a more general spatial and temporal context than where and when the study took place.

Another propriety is the notion that the hypothetico-deductive (H-D) method, though imperfect, at least represents a logically supported approach to truth-seeking. "The H-D method," argued Romesburg (1981:295), "is a way of raising the reliability of speculation and, hence, the overall reliability of our knowledge." Accordingly, many wildlife scientists have extolled H-D experimentation since Romesburg's (1981) article, and I am among their number (Guthery et al. 2001*b*, 2005). Garton et al. (2005:46) asserted that research "should be" designed to test deductions from the H-D method.

I accepted the H-D propriety and looked askance at the alternative (induction) without critical thought. I now realize there may be a major disconnect between the thinking of philosophers and the reality of wildlife science. Moreover, I see a disconnect in the community of wildlife scientists between what we say we should do and what we actually do in research.

My goal is to place deduction and induction in a perspective relative to knowledge accrual in wildlife science. I argue that H-D experimentation is a way to do wildlife science but sometimes an ambiguous way. I argue that induction as employed by wildlife scientists is more highly evolved and better fit than the primitive notion of induction held by logicians. I conclude that deductive and inductive methods both provide important approaches to knowledge accrual in wildlife science.

H-D EXPERIMENTATION AND WILDLIFE SCIENCE

I refer to wildlife science as the gathering of knowledge relating to field ecology, where it generally is impossible to control factors in an experimental sense, though it is possible to identify and control for their effects with experimental design. However, there will always be effects, such as time effects (e.g., season, yr), that cannot be dealt with in design because they cannot be replicated or eliminated (unless studies are done at the speed of light).

There are some differences of opinion on the nature of the H-D method. Williams (1997) viewed it as a theory amended by hypothesis, which leads to deductions that can be tested in experiment. On the contrary, Garton et al. (2005) viewed deductions as hypotheses. Following Romesburg (1981), I view experimentation in the H-D mode as positing a research hypothesis (see below) and deducing observations from the hypothesis that can be tested with data; i.e., research is conducted to determine whether a deduction holds. If it holds, this outcome is taken as

[1] *E-mail: fred.guthery@okstate.edu*

evidence that the hypothesis is meritorious, though possibly false (Beveridge 1957). If the data fail to support the deduction, the hypothesis is presumed false, or at least the hypothesis is not supported by the current data. However, one can never be certain in field ecology that the presumption of falsity holds: "There are occasions when it is the experiment you want to reject or explain away, rather than the hypothesis" (Edmonds and Eidinow 2001:244). Thus, the H-D process does not free wildlife scientists from the need for judgment on the reliability of findings.

The obeisance paid to H-D experimentation as a revealer of truth has its roots in formal logic, where a deduction from a true premise is by definition (and obviously) true. A true premise does not permit a false deduction (Bandyopadhyay and Bennett 2004) because a false deduction from a true premise is, by definition, impossible. Thus, in an analogous manner, one might presume that an observed deduction from a research hypothesis supports but does not necessarily confirm the hypothesis. As I illustrate below, the observation, or lack thereof, of a deduction in a field experiment might leave the researcher with doubt over the merit of a hypothesis.

In general, research hypotheses ask whether, how, or why (Guthery et al. 2004). Scientists might deduce that a circumstance must exist for a hypothesis to hold. Confirming or refuting its existence (whether) seems a straightforward matter of field sampling under the appropriate design and intensity.

A research hypothesis might be a conjecture on how a phenomenon may be explained (e.g., how do birds find cached seeds). Suppose one hypothesizes they find caches by memory that is augmented by visual landmarks such as logs, rocks, and so on (Griffin 1992). We might deduce from this hypothesis that removing the landmarks would render a bird incapable of finding its caches and proceed with field or aviary experiments.

Questions of why seek to identify the cause of an event or pattern. The classical approach in experimental science would be to control all factors except the hypothesized cause, vary its levels, and then measure responses. The deduction here is simply that the hypothesized cause will result in the predicted effect if it is indeed a cause.

There seems to be no way to test how or why hypotheses except by testing deductions from those hypotheses. That is the supreme justification for H-D experimentation—that it permits testing of research hypotheses. As Dewey (1910:82, emphasis in original) observed, "The inductive movement is toward *discovery* of a binding principle; the deductive toward its *testing*—confirming, refuting, modifying it on the basis of its capacity to interpret isolated details into a unified experience." Romesburg (1981) lamented the failure of wildlife scientists to test hypotheses, especially those developed after the fact to explain an observation (retroductions). This failure was, I believe, his main concern for the reliability of wildlife science, and its annulment necessitates use of the H-D method.

Despite the apparent necessity of deduction in hypothesis testing, the H-D method is prone to error and ambiguity in field ecology. This circumstance prevails for ≥4 reasons. First, as Romesburg (1981) pointed out, it is conceivable that a true hypothesis could be rejected in H-D experimentation because some associated, unknown factor neutralized the effects of a perturbation. In a similar vein, the H-D method does not eliminate the possibility of erroneously assigning cause to associational or correlational factors rather than to true causes. By associational factors, I mean 2 events that seem to be related in a causal manner. If burning increases food supplies for an animal and density of the animal increases after fire, one might ascribe cause of the increase to food. However, a change in the structure of habitat might explain the increase just as well or better than food, or both factors might be involved to degrees.

Second, application of the H-D method might lead to misleading conclusions in the presence of multiple causes for some phenomenon. Suppose we want to know what causes large antlers in cervids. We might hypothesize (H_i) and deduce (D_i) along these lines:

H_1: Genotype governs antler size. D_1: Average antler size may be altered with selective breeding.
H_2: Nutrition governs antler size. D_2: Average antler size may be altered with variable nutrition.
H_3: Age governs antler size. D_3: Up to the age of senescence, antler size will be an increasing function of age.

We know these 3 hypotheses and deductions there from are simultaneously true to some fuzzy degree. In the presence of a single hypothesis on cause, the H-D method would lead to support for the hypothesis, yet that hypothesis would be an incomplete explanation. Therefore, the H-D method potentially leads to the propagation of incomplete knowledge (fractional truths). Indeed, uncritical application of the H-D method might be deceptive in multi-cause venues, which undoubtedly are omnipresent in ecosystems. Perhaps the best that can be done is to list the causes and assess their relative importance (Williams 1997).

The above example shows that the value of multiple hypotheses goes beyond that of protecting scientists from their ego, as Chamberlin (1890) argued. Multiple hypotheses also help protect against fractional truths possibly derived by H-D experimentation.

A third problem with H-D experimentation is that competing research hypotheses might lead to identical deductions (Guthery 2004). Hiller and Guthery (2005), for example, tested the competing hypotheses that heat avoidance versus predator avoidance governed midday covert selection by northern bobwhites (*Colinus virginianus*). Both hypotheses predict use of midday coverts that provide substantial structure for concealment and physical protection from predators and for provision of shade for its cooling effects. Accordingly, field research gave ambiguous results relative to the hypotheses. In the presence of a single hypothesis (heat or predators) and the absence of critical thought, however, application of the H-D method might have supported a hopelessly confounded cause as a full cause.

A fourth problem involves the drawing of conclusions from H-D experimentation. This problem does not exist, of course, in certain trivial circumstances. Many research articles nowadays are set up in the guise of H-D science when in fact the articles are perhaps better viewed as reports on the magnitudes of known effects. For example, we know that plant succession causes changes in wildlife communities. A hypothesis on successional effects and a deduction on community response are passé, yet such research is important for understanding the nature and magnitude of successional effects.

In nontrivial circumstances, there remains no faultless method for determining whether field results support or refute deductions. Romesburg's (1981) example involved 1) the positing of a research hypothesis, 2) the deducing of a numerical event under the hypothesis, which he called a test consequence, and 3) the application of a significance test to assess the merit of the deduction. Under his approach, the deduction is a 1-tailed alternative to the null hypothesis. This creates the curious if not paradoxical situation that a sample-to-population induction from a statistical test is at once a deduction from a research hypothesis.

Null hypothesis significance testing also brings to bear human arbitrariness in the conduct of science. For example, a difference might be construed significant at $P < 0.05$ but not at $P < 0.06$. In the limit of sample size, all differences will be significant (Johnson 1999). So, under Romesburg's approach, all test consequences might prevail and all deductions might be true if approached from the angle of null hypothesis significance testing with large samples. These arguments serve to further illustrate that there is no faultless method of determining support or lack thereof for deductions. However, pilot research followed by power analysis and the setting of an appropriate effect size can strengthen conclusions about support for deductions.

INDUCTION IN WILDLIFE SCIENCE

The alternative to H-D experimentation as a method of knowledge accrual is induction. Philosophers criticize induction because a sampled instance says nothing about the infinitude of not-sampled instances over which an induction prevails (Rosen 1991:70). Wildlife scientists can use experimental design to remove some competing hypothesis and thus purify the basis of induction. However, philosophers hold that generalizing the results of a perfectly designed study, or even a replicated set of perfectly designed studies on the same topic, represents induction. One counter example destroys an inductive hypothesis.

Certainly wildlife scientists have reason to be apprehensive about induction because that process has led to unreliable knowledge, that is, "false ideas that are mistaken for knowledge" (Romesburg 1981:293). A classic example is Leopold's (1933) law of dispersion (also known as the principle of edge). Based on reasoning and observation, he induced that the abundance of edge-obligate animals on an area is proportional to the amount of edge. Theoretical and empirical counter examples to the proportionality condition are known (Guthery and Bingham 1992, Guthery 1999, Guthery et al. 2001*a*).

Despite the philosophical and empirical dangers of induction, the working scientist has no alternative but to induce with some abandon; science would collapse otherwise (Brown 2000), in part because, strictly speaking, we gain no new knowledge from deduction (Campbell 1974). Campbell (1974) pointed out that the quality of an induction varies. We have no problem inducing whether we have diabetes based on a 1-mL sample of our blood. We can accept an inductive estimate of a population mean under random and sufficient sampling. Likewise, ecologists accept the results of a well-designed field study with sufficient sampling as inductively applicable to a more general situation. Their knowledge of the natural history of organisms under study might reinforce their trust in the generality of particular results, or call it into question. In a practical sense, induction does not take place in an epistemic vacuum (an absence of germane knowledge). "Rather, generalizations are accepted as a result of judgments made by skilled individuals who reflect on the information and the alternatives that are available in their field of expertise" (Brown 2000:196). This is an important point in the philosophy of wildlife science; it has no homologue that I know of in formal logic.

On the contrary, wildlife scientists might distrust results from a poorly designed study with insufficient sampling, or those that run counter to the observed ecology of the species. So there are implicit screens for how wildlife scientists deal with induction, the infinitude of not-sampled instances be damned. Portions of the discussion section of many articles could be viewed as active screening of inductions. These portions are comparisons of inductive findings of the present study with inductive findings from earlier studies. To the extent that inductive findings are replicable, credence in them grows (Johnson 2002), despite the philosophers' objections. To the extent that these findings are not replicable, acceptance is suspended until further research or unifying hypotheses clarify the situation.

A logical curiosity in field ecology is that inductions might be amenable to deductive analysis through thought experiment. Consider as an example the inductively derived migration corridors of waterfowl (Bellrose 1976). These corridors are based on generalizations to migratory populations from the locations of a sample of band returns. We could deduce the geographic locations of corridors for ducks with 2 inductive facts of natural history: ducks migrate and ducks are associated with water. The Mississippi River might be thus deduced as a migration corridor and that deduction would be amply borne out by band-return data. Here we have, as in the case of Romesburg's (1981) example of H-D experimentation, a deduction and an induction being the same species. I do not know whether deductive support for an induction strengthens the induction; such support might make an induction deductively plausible, at most. No wonder Whitehead likened induction and deduction to quarreling ends of the same worm (see epigraph).

DISCUSSION

Wildlife scientists toil in a rather bizarre philosophical setting with respect to the reliability of the knowledge they gather. (Of course, we could say that logicians and philosophers toil in a bizarre philosophical setting relative to field ecology.) We cannot test research hypotheses without using deduction. Yet we have ample reason to believe the H-D method might lead us astray if we do not invoke judgment.

Wildlife scientists have historical examples of spuriousness (e.g., principle of edge) and apparent trustworthiness (e.g., migration corridors) with induction. The philosophers would argue that no induction is trustworthy. Taken to its logical extreme, this argument implies a virtual absence of reliable knowledge, including natural history, in wildlife science. The implication is clearly absurd, the disconnect between philosophy and practice clearly apparent.

I believe the community of wildlife scientists also has a disconnect between its philosophy of what should be (H-D science; Romesburg 1981, Garton et al. 2005) and what is. We extol yet rarely practice H-D science. Twenty years after Romesburg's (1981) article, about 25% of the articles in *The Journal of Wildlife Management* that could have had research hypotheses had them (Guthery et al. 2004). This implies that $<$25% potentially used the H-D method, which would have entailed generation of deductions. The rising popularity of Akaike's Information Criterion–based model selection (Guthery et al. 2005) implies rising popularity of an inductive tool at the expense of H-D science.

I further believe the disconnect in our community is troubling only to the extent that it causes posturing over what should be. The H-D method is our only logical means of testing ideas, and it is essential on that basis. On the other hand, inductive and retroductive reasoning (after-the-fact hypothesizing on cause or process) are means of generating ideas in need of testing. Moreover, the inductive knowledge in wildlife science can be screened for validity with auxiliary knowledge. Knowledge accrual by both deductive and inductive processes is important to wildlife science.

MANAGEMENT IMPLICATIONS

The implications of my paper relate to application of the H-D method in field ecology and managing how we think about the proprieties of wildlife science. In application, the H-D method is associated with problems that oblige the scientist to exercise judgment on the implications of results. We generate ideas with induction and retroduction and test them with the H-D method, so thought process and method are interdependent. Both induction and deduction not only should be, but it would seem must be, integral components of wildlife science.

ACKNOWLEDGMENTS

I thank L. A. Brennan, B. Collier, J. J. Lusk, and M. J. Peterson for commenting on a draft of this manuscript. Financial support came from the Department of Natural Resource Ecology and Management, Oklahoma Agricultural Experiment Station, and Game Bird Research Fund. This paper is approved for publication by the Oklahoma Agricultural Experiment Station.

LITERATURE CITED

Bandyopadhyay, P. S., and J. G. Bennett. 2004. Commentary. Pages 32–39 *in* M. L. Taper and S. R. Lele, editors. The nature of scientific evidence. University of Chicago Press, Chicago, Illinois, USA.

Bellrose, F. C. 1976. Ducks, geese & swans of North America. Stackpole, Harrisburg, Pennsylvania, USA.

Beveridge, W. I. B. 1957. The art of scientific investigation. Vintage, New York, New York, USA.

Brown, H. I. 2000. Judgment, role in science. Pages 194–202 *in* W. H. Newton-Smith, editor. A companion to the philosophy of science. Blackwell, Malden, Massachusetts, USA.

Campbell, S. K. 1974. Flaws and fallacies in statistical thinking. Dover, Mineola, New York, USA.

Chamberlin, T. C. 1890. The method of multiple working hypotheses. Science 148:754–759.

Dewey, J. 1910. How we think. D. C. Heath, Boston, Massachusetts, USA.

Edmonds, D., and J. Eidinow. 2001. Wittgenstein's poker. HarperCollins, New York, New York, USA.

Garton, E. O., J. T. Ratti, and J. H. Giudice. 2005. Research and experimental design. Pages 43–71 *in* C. E. Braun, editor. Techniques for wildlife investigations and management. The Wildlife Society, Bethesda, Maryland, USA.

Griffin, D. R. 1992. Animal minds. University of Chicago Press, Chicago, Illinois, USA.

Guthery, F. S. 1999. Slack in the configuration of habitat patches for northern bobwhites. Journal of Wildlife Management 63:245–250.

Guthery, F. S. 2004. Commentary: the flavors and colors of facts in wildlife science. Wildlife Society Bulletin 32:288–297.

Guthery, F. S., and R. L. Bingham. 1992. On Leopold's principle of edge. Wildlife Society Bulletin 20:340–344.

Guthery, F. S., L. A. Brennan, M. J. Peterson, and J. J. Lusk. 2005. Information theory in wildlife science: critique and viewpoint. Journal of Wildlife Management 69:457–465.

Guthery, F. S., M. C. Green, R. E. Masters, S. J. DeMaso, H. M. Wilson, and F. B. Steubing. 2001*a*. Land cover and bobwhite abundance on Oklahoma farms and ranches. Journal of Wildlife Management 65:838–849.

Guthery, F. S., J. J. Lusk, and M. J. Peterson. 2001*b*. The fall of the null hypothesis: liabilities and opportunities. Journal of Wildlife Management 65:379–384.

Guthery, F. S., J. J. Lusk, and M. J. Peterson. 2004. In my opinion: hypotheses in wildlife science. Wildlife Society Bulletin 32:1325–1332.

Hiller, T. L., and F. S. Guthery. 2005. Microclimate versus predation risk in roost and covert selection by bobwhites. Journal of Wildlife Management 69:140–149.

Howson, C. 2000. Induction and the uniformity of nature. Pages 181–183 *in* W. H. Newton-Smith, editor. A companion to the philosophy of science. Blackwell, Malden, Massachusetts, USA.

Johnson, D. H. 1999. The insignificance of statistical significance testing. Journal of Wildlife Management 63:763–772.

Johnson, D. H. 2002. The importance of replication in wildlife research. Journal of Wildlife Management 66:919–932.

Leopold, A. 1933. Game management. Charles Scribner's Sons, New York, New York, USA.

Peirce, C. S. 2000. The probability of induction. Pages 1341–1354 *in* J. Newman, editor. The world of mathematics. Volume 2. Dover, Mineola, New York, USA.

Romesburg, H. C. 1981. Wildlife science: gaining reliable knowledge. Journal of Wildlife Management 45:293–313.

Rosen, R. 1991. Life itself. Columbia University Press, New York, New York, USA.

Williams, B. K. 1997. Logic and science in wildlife biology. Journal of Wildlife Management 61:1007–1015.

Associate Editor: Morrison.