Wildlife Science: Gaining Reliable Knowledge

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Like the Kaibab deer herd, progress in wildlife science may be headed for a crash under the weight of unreliable knowledge. Knowledge, the set of ideas that agree or are consistent with the facts of nature, is discovered through the application of scientific methods. There is no single, all-purpose scientific method; instead, there are several, each suited to a different purpose. When the set of scientific methods is incomplete, or when one method is used for a purpose better fit by another, or when a given method is applied without paying strict attention to the control of extraneous influences, then these errors of misuse cause knowledge to become unreliable.

Unreliable knowledge is the set of false ideas that are mistaken for knowledge. If we let unreliable knowlege in, then others, accepting these false laws, will build new knowledge on a false foundation. At some point an overload will occur, then a crash, then a retracing to the set of knowledge that existed in the past before the drift toward unreliability started. Every field that loses quality control over its primary product must undergo this kind of retracing if it is to survive. Of course, some unreliable knowledge inevitably creeps in—a researcher makes a systematic error here, or fails to do enough replications there. All science is prone to human error, and minor retracing continually occurs. But I think part of wildlife science's knowledge bank has become grossly unreliable owing to the misuse of scientific methods, and major retracing is inevitable.

I read published dissatisfaction on seemingly isolated topics as being symptomatic of past misuses of scientific method, e.g., Chitty's (1967) and Eberhardt's (1970) complaints over the continued confusion between correlation and cause-and-effect, Bergerud's (1974) case against the reliance on induction to generalize laws to the exclusion of testing research hypotheses, Hayne's (1978) dissatisfaction with poor experimental designs, Krebs' (1979) frustration with virtually every aspect of small mammal ecology, Caughley's (1980) claims that most large mammal studies "coalesce into an amorphous mass of nothing much" and that white-tailed deer (*Odocoileus virginianus*) and *Drosophila* are the most studied and least understood of animals, and Eberhardt's (1975) skepticism about the predictive value of computer simulation models of ecological systems.

What are these misuses of scientific method? Of the 3 main scientific methods used in virtually all fields, i.e., (1) induction, (2) retroduction, and (3) hypothetico-deductive (H-D), wildlife science uses the 1st and 2nd methods but almost never the 3rd. Induction and retroduction, by themselves, are inadequate for discovering some kinds of knowledge. Instead of realizing this limitation, wildlife science routinely stretches induction and retroduction beyond their limitation as knowledge-finding tools and unreliable knowledge results.

Let me show how this occurs by explaining each method. The method of induction (Hanson 1965, Harvey 1969) is useful for finding laws of association between classes of facts. For example, if we observed over many trials that the amount of edge vegetation in fields was positively correlated with an index of game abundance, we would be using induction if we declared a law of association. The more trials observed, the more reliability we'd attribute to the law. The method of retroduction (Hanson 1965) is useful for finding research hypotheses about processes that are explanations or reasons for facts. For example, if we observed birds caching seeds more on south slopes than on north slopes (facts), and our best guess for the reason of this behavior (our research hypothesis) was that south slopes tended to be freer of snow than north slopes, we would be using the method of retroduction to generalize a research hypothesis about a process providing a reason for the observed facts of bird behavior. The method of retroduction is the method of circumstantial evidence used in courts of law. Retroduction is not always reliable, because alternative research hypotheses can often be generated from the same set of facts.

The H-D method (Popper 1962, Harvey 1969) complements the method of retroduction. Starting with the research hypothesis, usually obtained by retroduction, predictions are made about other classes of facts that should be true if the research hypothesis is actually true. To the extent that experiment confirms or rejects the predicted facts, the hypothesis is confirmed or rejected. Thus, the H-D method is a way of gauging the reliability of research hypotheses acquired by other means.

Wildlife science's workhorse is the method of induction. I believe it is used in a way that gives reliable knowledge. However, induction has a limitation: it can only give knowledge about possible associations between classes of facts. Although this is undoubtedly useful for decision making (e.g., the correlation between a fish's weight and its length is a money-saving association), it cannot give knowledge about the processes that drive nature. Consequently, you can use induction repeatedly without diminishing the question "Why?". When we ask "Why?" we are asking for an explanation, an abstract process that provides a reason for

the facts. If the human mind didn't beg for reliable explanations, the method of induction would suffice. That not being the case, the method of retroduction was invented. It is reliable enough to be used in courts of law but, by itself, it is not reliable enough for science. Science has the most stringent standards of all endeavors. If courts of law followed science's strict standards, suspects identified through retroduction would be set free, and their guilt decided in accordance with whether or not the life of crime predicated for them turned up in future facts. That is, the courts would test a retroductively derived hypothesis using the H-D method.

Because wildlife science hardly uses the H-D method, it is stuck with no way of testing the many research hypotheses generated by retroduction. Herein lies the main cause of unreliable knowledge. The research hypotheses either are forgotten, or they gain credence and the status of laws through rhetoric, taste, authority, and verbal repetition. Leopold's (1933) book *Game Management* lists 9 entries under "hypothesis"; I think none has ever been tested by the H-D method. Errington's (1945) threshold-of-security hypothesis, a hypothetical process of winter mortality, is often stated as a law, but it is a retroductively derived hypothesis, and it strictly speaking, remains untested.

The normal pattern of university graduate and faculty research—spending hundreds of hours watching, describing, and quantitatively recording the habits of animals, relating their habits to environmental facts, analyzing the data using a computer and contemporary statistical analysis, and then drawing conclusions from patterns in the summarized data— produces reliable knowledge to the extent that induction and retroduction, properly used, will allow. But for me the reliable parts, inductively derived correlations about events, are often not interesting or even useful, whereas the interesting parts, the retroductively derived reasons for what is going on, are often unreliable speculation. The H-D method is a way of raising the reliability of this speculation and, hence, the overall reliability of our knowledge. It is not a cure-all. It cannot suggest good questions for research. It cannot be used to test every conceivable research hypothesis, for reasons of exprimental costs and lack of creativity on the part of researchers. It can be misused like any other method of science, but it can also lead to the discovery of reliable knowledge about processes.

The remainder of this paper will (1) explain the H-D method in detail; (2) show why the kind of general-purpose data routinely collected by game agencies is inadequate for testing research hypotheses; (3) show how an understanding of the H-D method resolves persistent confusions in wildlife science thought; and (4) contrast science with planning.

Essentials of the H-D Method

Terms critical to understanding the H-D method must first be defined: viz., theory, research hypothesis, and test consequence. The term *theory* means a broad, general conjecture about a process. For example, the Lotka-Volterra competition equations (Emlen 1973) represent a theory about the process of competition between 2 animal species. A *research hypothesis* is a theory that is intended for experimental test;

it has the logical content of the theory, but is more specific because, for example, the location and animal species must be specified. A research hypothesis must be tested indirectly because it embodies a process, and experiments can only give facts entailed by a process. The process itself is abstract, removed from the senses, and nonfactual. The indirect test is conducted by logically deducing 1 or more *test consequence(s)*, i.e., predicted facts, such that if the research hypothesis is true, then the test consequence(s) must be true, and the test consequence(s) must correspond to a feasible experiment, e.g., one that is not technologically impossible or so costly as to be impracticable.

For example, consider the question of how salmon find their way upstream to their home spawning grounds. The answer "Salmon navigate by vision alone" is a research hypothesis (H), i.e., a conjecture about a process of navigation. A test consequence (C) is "A group of salmon that has been captured and blinded as they begin their upstream migration will not reach their home tributary spawning grounds in numbers greater than expected by chance, whereas a nonblinded control group of equal size that was spawned in the same tributary as the blinded fish will return to their tributary in numbers greater than expected by chance." The fact of the test consequence C must then be obtained by experiment, e.g., tagging smolts before their migration to the lake or ocean, recapture of those returning to spawn, and subsequent recapture of blinded and control-group salmon after they have swum upstream.

The determination of whether or not C is true or false by reference to experiment requires a *statistical hypothesis* to be tested, e.g., the null hypothesis H_0,: "Control and blinded salmon return in equal numbers." Thus, a research hypothesis is a conjecture about a process, whereas a statistical hypothesis is a conjecture about classes of facts entailed by the process. In general, alternative test consequences can be used to test a research hypothesis. For example, an alternative test consequence is "When ink is metered into the stream so that vision is totally impaired, the fish will not reach their spawning tributary in numbers greater than expected by chance."

Because a test consequence prescribes the experiment necessary to ascertain the truth or falsity of C, the H-D method demands creative thinking. Creative researchers will search for test conclusions that require experiments beset by minimal statistical noise, that are cheap to perform, and that allow tight control of extraneous influences (note that the 2nd test conclusion is not as good as the 1st, because it doesn't allow for a control group). Successful researchers are defined, in part, as those who make a career of choosing the right trade-offs between these usually conflicting considerations.

The experiment's outcome determines whether C is judged to be true or false. If C is true, then H can be either true or false, and we say that the evidence supports or confirms the truth of H, i.e., is consistent with H being true. For example, consider the hypothetical limiting case in which somehow the truth or falsity of C is known with certainty, i.e., no test of a statistical hypothesis is required. If C turns out to be true, i.e., fewer blinded than nonblinded return, then support for the conjecture H that "salmon navigate by vision

alone" is evidenced. Further, the more replications carried out with the same outcome, the stronger the support is, although the the truth of H can never be declared with certainty because it is possible, for example, that H might really be false but other factors, such as a propensity for blinded fish to die, could be making C true.

On the other hand, if C turns out to be false, then H is false, provided that none of the background conditions required to make H entail C are violated. For example, if C is false, i.e., blinded and nonblinded return home in equal numbers, then H is false provided that H really does entail C. If blinded fish exhibit a schooling behavior not dependent on vision and get home by tagging along behind sighted fish, then of course, C being false is not justification for the statement that H is false. An experimenter can never gain complete assurance that the statement "The truth of H entails the truth of C" is true. Thus, even C being false does not provide complete assurance that H is false. However, the more certain a researcher is that the background conditions are indeed true, the more certain he will be in pronouncing H to be confirmed when C is true, and H to be falsified when C is false.

The details of the H-D method that fill out this brief outline are covered by Popper (1962), Platt (1964), Baker and Allen (1968), Harvey (1969), Medawar (1969), and Rachelson (1977). Bergerud (1974) used the H-D method to design a hypothesis test about the processes that cause caribou (*Rangifer tarandus*) populations to decline.

USES OF GENERAL-PURPOSE DATA

I call the kind of data routinely collected by game agencies "general purpose data." I ask whether they have scientific uses. Can they be used with the method of induction, method of retroduction, and the H-D method to discover reliable knowledge? The answer is yes and no.

I can see the method of induction being used with these data to obtain reliable laws, e.g., the correlation of the number of dead fawns with snow depth and duration. On the whole, however, I see unreliable knowledge resulting when the method of retroduction or the H-D method is used with these data. To understand a process of interest the process must be isolated from other processes by exacting experimental control. However, general-purpose data are not collected under controlled conditions.

Used with general-purpose data, the method of retroduction contains the flaw of incorporating the effects of unknown factors into the derived research hypotheses. Similarly, the H-D method can only produce reliable knowledge when background conditions are held to a tight tolerance. If the tolerance is lost, then the researcher will probably conclude something that in essence is more a result of error than substance. It goes beyond reasonable doubt for researchers to assume that nature delivers tightly controlled experiments without prompting. The creation of knowledge of processes from general-purpose data is therefore suspect.

History illustrates the pitfalls of loosely applying the H-D method to general-purpose data. Lauckhart (1955) and Lauckhart and McKean (1956) interpreted data from pheasant population studies as supporting the threshold-of-

security hypothesis, but the pheasant population data of Wagner et al. (1965) and Wagner and Stoker (1968) were interpreted as not supporting the hypothesis. Who can say that unknown factors are not giving conclusive results when there either are none or truly conclusive results are being obscured by errors?

OTHER PROBLEMS WITH WILDLIFE SCIENCE

Wildlife science has other problems befitting analysis. I will cover problems with concept definition, confusion between cause-and-effect and correlation, use of slipshod experimental controls, and the fixation on statistical methods.

Problems with Concept Definition

Key wildlife science concepts suffer from multiple and unclear definitions. For example, nearly 3 decades ago Edwards and Fowle (1955) concluded that more than a dozen different meanings of the concept "carrying capacity" were in use, and that most were vague and almost meaningless. They tried to right the situation by proposing a new, clearer definition. They failed; the confusion is undiminished today.

There is a mistaken belief that a profession sets the meanings of its concepts by decree. To be accepted, a concept must have appeal, and it can gain the necessary appeal in 2 ways. First, the concept can function in an inductively established law. For example, if it could be shown that a given definition of carrying capacity entered into inductively established laws with other concepts such as time, then the concept would gain appeal. Second, the concept can function in a law established by the H-D method. For example, if a given definition of carrying capacity functioned in a theory with other concepts, and if the theory became law through experimental test, then the concept would gain appeal.

The history of science shows that most of the concepts with staying power are those that function in laws established by the H-D method. For example, the concept of mass is substantiated by Newton's and Einstein's laws. When wildlife science decides to propose and test theories built around different concepts of carrying capacity, then the correct concept will emerge in those theories that pass experimental muster. When a science has no way of telling when a theory and the concepts it integrates are in error, then it has no way of telling which concepts are right.

Cause-and-effect vs. Correlation

One of the aims of wildlife science is to find cause-and-effect relationships among variables, for when cause-and-effect is found, then control may be possible. To say that a change in variable A causes a change (effect) in variable B, i.e., that B depends on A, requires a particular experiment: we merely introduce a change in A and see whether a change in B follows. If this occurs, and if in a control unit or group the variable A was not changed and a corresponding change in B did not follow, then we have evidence that A causes B. If A unerringly causes B over many trials, then at some point the

method of induction leads to the pronouncement of a causal law.

This is the basic method used in clinical experiments in medical research (Feinstein 1977:17-70). The variables A and B can be either binary or continuous. An example using binary variables is: A is a treatment variable taking on the states of (1) treatment applied of (2) treatment not applied, and B is an effect variable taking on states of (1) an effect is observed and (2) no effect is observed. An example using continuous variables is: A is the energy (kcal/kg) in a diet fed to deer and B is the change in a body weight (kg); Verme and Ozoga (1980) performed this type of study for white-tailed deer fawns under well-controlled conditions.

Chitty (1967) has proposed a similar but weaker method for binary variables. It differs from the method above in that nature selects the 2 settings of the supposed cause. Nature often won't yield control in field studies, so this design has appeal. The weakness, however, is that the data can follow the pattern required for saying A causes B, yet if we could seize control of A ourselves we might find that varying A produced no effect on B, i.e., the supposed cause-and-effect relation could stem from a 3rd variable that caused A and B to covary.

The scientific literature contains many examples of correlations between 2 continuous variables A and B being interpreted as one variable causing the other. For example in wildlife science, Eberhardt (1970) took 3 articles to task for inferring the existence of density-dependent regulation of populations from correlations. Bergerud (1974) questioned the logic in 2 articles in which observed correlations between fecal-pellet counts and the abundance of lichens were used to suggest that caribou require lichens for survival. And, Wagner (1978:198) used a correlation between the annual instantaneous rate of change in coyote (*Canis latrans*) populations and a black-tailed jack rabbit (*Lepus californicus*) population index to conclude that jack rabbit numbers are a determinant of long-term coyote density, and concluded that "if the mean jack rabbit densities over a period of years were increased, annual rates of change in coyote numbers would be largely positive for the years immmediately following."

Let us examine this last example in some detail. Clearly, there is a better but more expensive way to test the hypothesis that jack rabbit numbers are a determinant of long-term coyote density. The hypothesis would be supported if we took control of rabbit numbers and showed that changes made in levels of stocking were accompanied (with suitable time lags) by changes in coyote density. Failure to observe the effect, i.e., if coyote density followed no pattern as we changed stocking, would (if it could not be explained away as statistical noise) disprove the hypothesis.

On the one hand, we have an expensive experiment that can give reliable knowledge and a less expensive experiment involving correlations. Which is the better approach? Reliable and expensive? Or less reliable and less expensive? Depending on the costs, the case can be made either way. It is obvious, for example, that medicine could, at high moral and social costs, design experiments capable of producing reliable knowledge on the possible cause-and-effect link between cigarette smoking and lung cancer. But the costs are too high, and therefore medicine is restricted to making less reliable statements based on correlations observed in an uncontrolled setting. It is important to note that they chose this over the alternative of not studying the problem.

Correlation between 2 variable A and B, although a necessary condition for causality, is not sufficient. There is, however, justification for looking for correlations. Correlations offer weak support for making statements using the terms "causes," "determines," or "depends upon," but the history of science is replete with strong pronouncements of cause-and-effect based solely on correlations, only to find that later studies showed no such link.

Consequently, cause-and-effect should not be strongly stated when correlation is the sole evidence. If, however, a correlation is accompanied by other evidence (e.g., other corroborative evidence, the elimination by control of other variables conceivably responsible for the correlation, the demonstration of the correlation under a wide variety of circumstances that allow other possible influences to vary), and logically the dependence makes sense (e.g., the jack rabbit is a staple in the coyote's diet), then support for cause-and-effect is strengthened. Depending on the supporting evidence, the phraseology should range, I think, from "weakly supports," when a correlation is the sole evidence, to something short of "strongly supports," when correlation is accompanied by a variety of independent corroborating evidence. "Strongly supports" should be reserved for direct demonstration of cause-and-effect.

Thoughtful Use of Experimental Controls

I have selected 2 cause-and-effect studies, one from medicine and the other from wildlife science, to demonstrate the effect of experimental control on gaining reliable knowledge. The medical study (Nelson et al. 1980) tests the research hypothesis that the Leboyer Method of delivering babies in a dark, quiet environment results in the babies growing up to be healthier and calmer. One group of mothers received the Leboyer Method of delivery, the other group a conventional delivery, and the well-being of the babies was assessed for some months afterward and compared. The Leboyer Method had no effect. Thoughtful use of experimental controls greatly increased the reliability of the resultant knowledge. All conceivable alternative determinants of the babies' future health and calmness were controlled: the same obstetrical practice and the same delivery room were used, the group of mothers was selected on the basis of sharing certain common characteristics possibly related to the supposed effect, and the mothers were randomly assigned to the 2 methods of delivery.

Now consider a comparable study on the effects of vegetation interspersion on pheasant abundance (Taylor et al. 1978). Interspersion on a land unit in crops changed over a 20-year period, and an index of interspersion correlated with a pheasant density index. The authors conclude that the relationship is "useful for predicting changes in pheasant density, given anticipated land-use changes." How much more reliable the statement would be if controls had been used, if matched land units similar in all conceivable determinants of pheasant density except edge had been used and

the experiment conducted over the same time period. Looked at another way, suppose the medical study had been done the way the wildlife study was, not paying strict attention to controls. It would go something like this: a group of mothers who happened to be handy would be given the Leboyer Method of delivery in different hospitals by different obstetricians; 20 years later other mothers would get conventional deliveries; the well-being of the babies produced by the 2 groups would be compared. If medicine wanted unreliable knowledge, this is how they could go about getting it.

Research needs to be done correctly the first time. For example, Boag and Lewin (1980) tested the efficacy of different objects in deterring waterfowl from using natural and polluted ponds. They conclude their article with an apology for not doing the study under strictly controlled conditions: "... the causal elements in the corrlation reported herein need to be tested by using experimental and control ponds in given time period and thus avoid the necessity of using the same pond successively as a control and then as an experimental pond." I prefer conclusive over nonconclusive studies. Studies saying "here's how we did it; now here's how you can do it right" leave me cold not because they are wrong but because progress is unnecessarily retarded.

The reliability of knowledge is only partially determined by the dedication of researchers. No amount of dedication can make up for lack of experimental controls. In some studies the expense required to achieve control is not worth the expected gain in reliability. But for myself, I would rather see a handful of studies containing highly reliable knowledge than scores of studies containing something less. I would trade all of the studies that have been done on edge and interspersion for one carefully controlled study.

Fixation on Statistical Methods

In the wildlife literature of the past decade one finds an increasing use of nonparametric and multivariate statistical methods and computer analyses. There can be no doubt that progress in planning and scientific understanding is aided. I have noticed, however, scientific studies that lacked thought and were dressed in quantitative trappings as compensation. It's easy to collect data, perform statistical tests of hypotheses of the "no pattern" variety, and restructure the data using a computer. But all studies must be able to stand up to the question "So what?". I think that too many can't.

The mind must direct research. The processes of upstream salmon navigation are well understood (Hasler 1966, Hasler et al. 1978) because this research was directed at answering specific questions. Every phase of this research—from the initial generation of alternative research hypotheses explaining possible navigational mechanisms to the subsequent tests of these hypotheses using the H-D method—has been guided by a repeating cycle of questions, tentative answers, and tests of research hypotheses. Statistical analyses and the computer played an essential, but secondary, role. The questions and research hypotheses always directed the subsequent quantitative analyses.

Turning the process around and putting the quantitative analysis first is a quantitative natural history study. This can play a vital role at the start of research into an area where little is known by suggesting questions or research hypo-

theses. This is not bad, but it is time to shift the emphasis to hypothesis testing rather than hypothesis creation; otherwise we'll become swamped with untested ideas. In short, quantitative data analyses that are window-dressing should not be tolerated, those that are natural history studies should be tolerated, and those that play a role in the testing of research hypotheses should be encouraged.

Science and Planning

Science and planning are the respective domains of wildlife science and wildlife management. These domains are philosophically distinct, yet because each shares many of the same activities and tools, viz., data collection, statistical methods, and computer simulation models, their differences often pass unnoticed. Yet criticism of the use of commmon tools is baseless unless these differences in how the tools are used are understood.

Science and planning are different kinds of decision-making. Science (the H-D method) exposes alternative theories to facts, selects the best theory, i.e., that which agrees closest with fact, and gives it the name "law." Planning exposes alternative images of a future possible world to the decision-maker's values, or preferences, and selects the best image, i.e., that with the highest value. The essential difference is that science uses fact as its standard for selection, whereas planning uses values.

The images in planning are composed of scientific knowledge, common sense rule-of-thumb knowledge, and theories that are as yet untested, i.e., hunches (Boulding 1956, 1980). Because the image of the status quo will change over time due to influences outside the planner's control, planning is often necessary to counter these uncontrollable influences. Man's imperative to plan is so strong that planning routinely goes on even when scientific knowledge is totally absent from the planner's images. When the imperative to plan takes hold, a planner will enter into the planning process with the best knowledge, tools, and thought at hand, regardless of how imperfect they are. For example, this nation's macroeconomic policy is largely geared to project images made by computer simulation models of the economy. Yet the elaborate mathematical equations comprising these models represent untested economic theory, and by even the loosest standards of science their predictions fail to agree with economic fact as revealed in the future. Or consider that alternative plans for deployment of land-based intercontinental ballistic missiles are characterized by little scientific knowledge (Feld and Tsipis 1979): the probabilities of destroying the other side's missiles are crude hunches, and the probabilities of how the other side will target its missiles are based on a common-sense image of rational behavior. Finally, a recent wildlife management text (Giles 1978) draws only minimally upon the thousands of scientific articles that have appeared over the years in this journal. Yet no one would argue that planning for economy, defense, or wildlife should not be undertaken until every part of the images used in planning is substantiated by scientific study.

Science uses relatively absolute and tight tolerances for deciding which theories and hypotheses should be called law. Planning does not use tolerances for deciding what is the best plan, but instead defines "best" as relative to the set of alternative images. Thus, science many never arrive at laws in certain areas for no theory may be within the tolerance for truth, but planning will always arrive at a best plan. Although science and planning share common tools, science and planning have different norms for certifying ideas, and hence criticism of these tools must take into account the domain of their use.

Conclusion

Because the wildlife literature is taken as a role model for what wildlife science ought to be, and because it does not place the H-D method in a prominent role, widespread use of the H-D method is not guaranteed. I think the natural place to break this circle is in university education. As it now stands, education in the natural resource fields (almost everything I've said in this paper applies to the way all environmental sciences conduct themselves) does not provide training in scientific methods. Many, if not most, wildlife graduate students do not even understand the differences between induction and deduction.

Training is needed in all phases of science, and these principles need to be carried through in all wildlife courses. Students must be trained in the creative arts of asking the right questions, creating research hypotheses, using the scientific methods of induction and retroduction and the H-D method, designing efficient experiments (so as to avoid firing a cannon at a fly), and recycling the procedures so that the endless cycle of question and answer forms a unified whole. Students also must be trained in the ethics of science and planning; their teachers need to demonstrate these ethics in living form.

Wildlife science must try the H-D method. Without it the ability to detect errors in pronouncements of laws, the self-correcting feature science must have, is fatally lacking. All learning takes place in a feedback system in which ideas and reality interplay. The method of retroduction coupled with the H-D method is such a feedback system. Uncouple them and the ability to learn, to tell error from truth, is hindered, if not destroyed.

By themselves, scientific methods are impotent. Skills in using methods are the catalysts of potency. If, in a half century, the H-D method has been tried and shown to be impotent, then its judges must show that the cause was not the impotency in the skills and dedication of those who tried it.

I regard medical science and wildlife science as fields with equal potentials for achieving reliable knowledge. I think, however, that medicine has come closer to its potential, whereas wildlife science has lagged. I think medicine owes its success to the strict attention it pays to scientific method. Scores of books on the philosophy of clinical experiments have been published, yet I know of few comparable books in the natural resource sciences. Medical science obviously cares for and is committed to the quest for reliable knowledge. It is a good role model.

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Range Management: A Viable Science or an Indian Fakir Psuedo Religion

Dan Fulton

In our attempt to corral a few of the Sacred Cows which our good intentions have turned loose in the 100 years that have elapsed since this Range business started spreading over the Plains, it might be helpful if we look back into our history to see how we got here. That way we might see what we did wrong, regard some of our problems, wonder what brought them about, and speculate on possible ways to reach our objective.

In the early days of the Society there was considerable discussion on a name for the Society, whether to include *Management* in the name and whether to call it *Rangeland* or to call it *Grassland*. I feel very strongly that it was of some consequence and that we did reach the right decisions in both of these matters.

Then there were two schools of thought on membership requirements. When the subject of membership came before the meeting there was general agreement that 'technical' ranchers be admitted to membership but a proposal was made from the floor that only 'Conservation Ranchers' be admitted after examination by Society representatives showing that they had a 'good crop of grass.'

Fred Renner was the presiding officer and he 'innocently' suggested that this sounded reasonable but pointed out that in our democratic organization any such requirement ought to be applied to all members. He went on to say that if it were applied to land use project managers, national forest supervisors and regional graziers, the Society might find itself in the position of having to refund the membership dues of many who had already joined. In the ensuing laughter the group voted the broad membership requirements which we still have today.

But even after that there continued to be some dissatisfaction with our liberal membership policy. This is indicated by a letter written by Fred Renner in 1950 expressing his thoughts on the subject. Here is a sentence from the letter:

I am convinced that the conservation job in this country will never get done until the ranchers and other people who live on and make their living from the land assume the major responsibility for the job and undertake to get it done.

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Going through the pages of our publications we find this thought expressed over and over. In the February 1980 issue of *Rangelands* is an article by John Merrill, who became our President in 1981. From this I quote:

...the task was too enormous for anyone but the individual land-owners and operators themselves to accomplish. These farmers and ranchers had the desire, ability, and economic incentive to do a better job for themselves, their families and their communities...

In the March 1984 issue of the *Journal of Range Management* we find the President's Address by Gerald Thomas and again I quote:

. . .[we need to] emphasize the term 'management.' Research, understanding, management are our focus—not protection, per se.

In keeping with the 'management' theme we still need to place more emphasis on service to users of range land-particularly the livestock sector. . . . I still have a serious concern that the goal of certain environmental interests is to eliminate domestic livestock from public range lands.

As a long-time user of range lands I am acutely aware of the necessity of ownership or some form of stable, secure tenure to practice Range Management. This was pointed out in my book, *Failure on the Plains,* which Danny Freeman reviewed in *Rangelands*, August 1982:

Fulton strongly believes that the long-time maintenance of the public rangelands in the Northern Great Plains rests almost entirely upon the rancher, the user of the land. Government can not do this job. It is the man on the ground who will get the job done. He says, 'A big step in the right direction will be to give the user longer tenure.'

Why has the rancher not had tenure? Obviously it is not possible to develop and manage any natural resource without tenure. You can't manage it and you can't finance or spend the capital, the money needed for development of the land without tenure. Our government has always encouraged long and stable tenure of cropland for crop farmers. Sometimes it has been said nobody wanted tenure of the land on the Plains. As recently as June, 1984 I heard Secretary of Interior Bill Clark call it, 'The land nobody wanted.'

Is history what historians say it is, or is it what we who lived there have experienced? To use the vernacular of the attorney-at-law, 'Let's look at the record.'

The record is that ranchers have been trying for over 100 years to get tenure of grazing lands, and have had capital to