Alternative to Romesburg's Views (1981)

Wildlife research (really all ecological investigation) surely needs criticism but I believe Romesburg has missed the target by a wide margin in laying the blame on incorrect method. His descriptions of the methodology and logic of the hypothetico-deductive method are at best misleading and outdated. This is not to say that he can't have his own views but he has seriously misinterpreted Popper (1962) and his disciples Platt and Medewar.

Beyond misinterpretations Romesburg neglected any reference to original writings on scientific methodology published later than 1970. As will become apparent to anyone reading back from 1980, profound changes occurred in the general view on scientific method in that decade. The scientific method, unlike the 10 commandments, is always changing.

I can agree with Romesburg that there is much wrong with the current emphasis on modeling, use of statistics, and the education of wildlife students as well as other areas of ecology and natural resource management. I also agree that medicine has made more progress than wildlife research, but again I emphasize I do not believe it has much to do with the proper application of the hypothetico-deductive method.

Romesburg's explanation of the hypothetico-deductive method is much closer to that of 19th century writers on scientific method than it is to Popper (1962) and his disciples Platt or Medewar cited by Romesburg. Popper (1963) rejects verification (validation) (see Popper in Flew 1979) and insists that a sincere effort to refute your own theories is required. He claims, reasonably, that most scientists look for easy tests which will verify instead. He further states that although the mere proposal of a potentially refuting critical experiment qualifies a theory as "scientific"

Bill McConnell

and not "metaphysical" a theory no matter how scientific never is known to be true. It becomes "corroborated" by surviving tough tests but never proven.

Hanson's (1961) expalanation of retroduction is misunderstood by Romesburg but considering Hanson's ambiguous and confusing way of writing this is understandable. Retroduction is a word coined by Peirce in 1903 as a synonym for the deductive in hypothetico-deductive Reshe~~x~~r (1978). Retroduction is not included in any recent dictionaries of philosophy. Even Hanson omitted it in his last book on the philosophy of science (1969). Apparently it is unnecessary.

The current view on scientific method is similar to Poppers (1965) but less stringent (Lakatos 1978). Critical experiments resembling shootouts at the OK Corral don't work and nobody has ever done science that way anyhow according to Lakatos (1978). He points out that any refutation of a theory can be explained and used legitimately to continue research indefinitely as long as money is available. The history of physics, according to Lakatos, is replete with refuted theories making a comeback and defeating (temporarily) the refuting theory which only temporarily defeated it. Apparently Popper now agrees with Lakatos' views (according to Lakatos 1978). Feyerabend is the alternative modern Guru (to Lakatos and Popper) of scientific method and he says there is no such thing as scientific method (1978). This isn't to say that the most recent mainstream philosophy of science is the most correct, but it is usually considered to be the most authoritative. One can rationally reject it all but then he is on his own. Considering the changes in opinion of scientific method with time and the lack of agreement during any single year maybe Feyerabend is correct. For additional discussion see Brown (1977).

My chief objection to Romesburg's paper is not his misleading and outdated explanation of scientific method but rather his conclusion that wildlife science is in trouble because its researchers have been less than scientific. It is my opinion that the subject matter of wildlife ecology is the source of the difficulties and not the unscientific attitude of its practitioners. I will admit that the wildlife and fishery professions have a large share of naturalists who choose not to be critical thinkers but this is a result not a cause of the irregularity (charm?) of the subject matter. Anyone with an ambition of garnering the Noble prize is not likely to stick with wildlife or fishery science. By the definition of science, according to three influential philosophers of science, wildlife researchers are not scientists. Popper (1968) describes science as the explanation of impressive regularities, Toulmin (1960) as the investigation of the form of regularities, and Hempel (1965) as the hierarchy of regularities entailing the regularity to be explained. The search for regularities, or unexplained associations as Romesburg might put it, must precede attempted explanation by whatever method one chooses. Science doesn't start till a stockpile of regularities has been accumulated. Feyerabend (1978), Lakatos (1978), and Mach, Poincare and D~~ø~~uhem (see Flew 1979) believe (believed) that the only thing science produces are facts (correlations, regularities). They insist that the only purpose of theories is to motivate scientists to fool around and turn up new facts. China made superior technological advances without anything like western science (Needham 1954-1965). By simple inductions with no concern for causes the Chinese were technologically far ahead of the Greeks who had science and logic. Apparently discoveries are often stumbled on used and as an afterthought explained but not necessarily correctly.

Impressive regularities persist through many changes in causal theories without diminishing their usefulness (see Duhem in Flew 1979).

There are no original ideas in the following paragraphs but rather juxtapositions of cited views. I will state why, in my opinion, Romesburg is mistaken and how wildlife researchers in the broad sense (and sociologists, psychologists, economists, ecologists, and all other kinds of soft scientists) may stop wasting time trying to look like physicists.

When wildlife researchers consider the scientific method they should keep in mind that almost all philosophers of science are writing about physics. Those not doing so have great difficulty in trying to reconcile physics and biology, especially ecology (Toulmin 1960, Hull 1974). Physics may be defined as that subject which has selfishly assigned all highly regular phenomena to its domain and rejected the less impressive things like ecology. Popper (1963) states that to be amenable to the H&D method phenomena must be isolated [from accidents], stationary [unchanging], and recurring like the planets in their precise orbits. Irregular subjects like politics, sociology, psychology, and presumably ecology are not predictable by any method (Popper 1963). Toulmin (1960) insisted that biology was not science because of the nature of the subject matter (not the biologists). Peirce dismissed irregularities in nature as "unreal" and oustide the domain of science (Rescher 1978). Most nonphysicists plunge right past these warnings when considering the scientific method.

The first explanation of regularity itself in my reading is Eddington's (1928) explanation of predictability in physics as a simple exploitation of averages (see Bernoulli's Theorum in Flew 1979). Hiesenberg (1958) emphasized the impossibility of predicting the course or fate of one photon atom or electron (see also uncertainty principle in Flew 1979). According to

Eddington prediction is possible not because of underlying organization but only because we are predicting averages or aggregates. The movements of sun, earth, moon, and other cosmic bodies are predictable largely because of the huge number of atomic nuclei aggregated in their structure according to Eddington. Half lives of radioactive substances and Boyles gas laws are based on mass averages. Repeated random samples of all rocks in a streambed would also yield amazingly uniform mean weights (or volumes or whatever) if the samples were large enough, say 10,000 rocks. The saving principle being that the aggregate weight will be little affected by the few oddballs (defined by the mode) that will occur.

Mario Bunge (1963) hinted at the biological (ecological) implications of this phenomenon when he suggested that biology could become as precise as physics if we restricted our predictions to huge numbers of entities (organisms, lakes, valleys, etc.).

I could become an unerring predictor of fish harvest if I randomly sampled 10,000 lakes over the entire earth and developed a mean harvest to be applied to another 10,000 random lakes to predict their mean harvest.

A corollary holds true when a diligent researcher for example measures the association between fish harvest and gross photosynthesis over the entire spectrum of values encountered in a very large random sample in North America. Unless she (he) had extremely poor luck, a great range of pairs (X & Y) would be sampled and when graphed it would be necessary to compress the numerical scales for both variables. This would give a "beautiful" fit with very low relative scatter of points about the relation line. But, alas, when we cut out the part of the relationship which applied to lakes of Rocky Mountain Park only and expanded it to fill the graph paper, what seemed to be low scatter would become a disappointing array with a very low R^2 which tells the manager little for any one lake.

Bunge's (1973) suggestion that the precision of biology (and presumably ecology) can be greatly enhanced by dealing with sums or averages of large numbers rather than individual entities is really not an option for wildlife and fishery researchers. IF the average success rate for anglers is a fish per hour (good) but if most anglers catch 0 fish and a few catch all the fish we are in trouble. IF we manage deer on a correlation with an R^2 of 0.7 for predicting the size of 100 deer herds we are going to be way off on many herds and a lot of hunters will be unhappy.

To put the foregoing together a physical scientist comes to expect that his part of nature is going to produce impressive regularities (facts, associations) because he can choose to look at his things from a mass statistical viewpoint. He has a plethora of "magical" regularities to explain so the methodology of explanation dominates his view of science. Wildlife and fishery researchers have yet to find any impressive ecological regularities because of the way they must look at things. Attempting a common explanation for a series of unique events is a waste of time. Most of our classifications of habitat, climate, and natural events lump rather diverse things under one heading and we pay the price of being wrong very often.

When the physicist gets his nose pushed into the day to day world of human things and tries to predict the weather or its long term derivitive climate he is no longer engaged in exact science (Roberts and Landsford 1979). For one location mean annual temperatures are fairly predictable (not impressively so) but the meteorologists clinetele demands more. They want to know the weather for one specific day, often several months in the future (sports events, etc.). The best the meteorologist can do despite precise knowledge of the physics of captive water vapor, the scientific method, brilliant scientists, and a huge budget is a two day forecast and even this is pretty iffy. What success meteorology has is due more to monitoring than theory. Monitoring is technology and not meteorological science in the sense of scientific method because the regularities are in the ~~methods~~ tools not in the theories.

The most scientific of scientific methods isn't going to do wildlife (fishery, ecological) research much good until we find acceptable regularities (there are no absolute regularities). There may not be many to discover in ecology but we can't know that for sure. A helter-skelter search for regularities may be the best we can do but we must cease rationalizing every mediocre correlation we find. Perhaps the criteria justifying an association could be made much more stringent and related to costs of being wrong in one case. We do have useful regularities in molecular biology, genetics, and physiology in controlled environments but this is a form of technology not ecology. The uniformity of physiology, behavior, and reproduction of individual plants and animals is perhaps more to be marveled at than physics but it is highly contingent also. It only occurs <u>if</u> the creature lives and of course in the wild this means knowledge of population dynamics. This is begging the question we are trying to answer. The physiology, behavior, and reproduction of dead animals is not very uniform.

Controlled replicated experiments are a tempting solution to the unpredictability of wild nature but they too have their drawbacks. They may be justifiably rejected on the grounds that there is no reason to believe they represent nature. Any experiment to demonstrate this is defeated by the very attribute of nature that caused us to attempt the experiment in the first place, irregularity. Beyond this objection attempts to replicate natural chunks of the environment even in greenhouses seem doomed to failure if any degree of replicate uniformity much better than the maximum result (growth, production, etc.) being double the minimum is expected (Buck et al. (1970), McConnell (1977), Meyer (1978), Galat (1982). Buck et al. (1970) offers an explanation similar to that for the unpredictability of Vortex formation (Chaos theory). Namely that minute, unrecognized initial differences in replicates magnify differences in the development of the total system.

The question about the high variability in any ecological experiment then becomes: is the result useful for the purpose the researcher has in mind? There is no universal criterion of reliability like the one Romesburg alludes to. Even in "pure" physics research a given level of precision may support some hypotheses but is ambiguous with regard to others. The most pragmatic justification of any piece of research is that it helps avoid risk. IF a single error (loss of one fishery or deer herd) is "fatal" ecological research might not be worth the cost. IF any single error is relatively unimportant and only the overall batting average counts then research is often worthwhile. This may be more a matter of monitoring routine management operations than experiment, however. If the stakes are uncertain because criteria of success are diffuse or controversial reliability may be a meaningless term.

Why would researchers in natural resource ecology keep the faith and continue to rationalize? I suspect it's done naively and innocently in most cases. Two illusions seem to support the faith of most wildlife researchers. These are the illusion of uniformity of nature (see Hume, induction and uniformity of nature in Flew 1979) and the illusion of technique (Barrett, 1979).

The uniformity of nature illusion is what makes a wildlife researcher believe that a detailed description of the ecology of a few communities or a species tells how things will be for all instances of that kind of community or that species. It also makes wildlife researchers accuse each other of careless work when their findings disagree. We seem to be hypnotized by the impressive regularities of species physiology, behavior, and genetics plus the "miracles" of physics to the degree that we overlook or rationalize the highly contingent nature of life in the wild. We gain unwarranted comfort from explanations after-the-fact when our predictions fail. "My index would have worked if the weather wasn't unusual," or "The bass would have grown if the carp hadn't invaded the pond," etc., etc. Theories concerning highly irregular phenomena must be eternally disputed. Much wildlife research reads like a log of the numbers coming up on a subtlely rigged roulette wheel. There is no reason to believe that nature is uniform but to support actions we must stick our necks out on the basis of subjectively convincing evidence and judge a few cases to be more uniform than the rest. A good piece of research like a mathematical proof really proves nothing but lays out the evidence clearly (Wittgenstein 1983).¶The illusion of technique is manifested in the belief that there is a formal algorithm (words or numbers) which will lead us to the truth even if we don't understand it. Barrett (1979) makes evident the way in which all logic is ultimately supported by ~~invitation~~ intuition and grounded in assumptions, even mathematical logic as in the Principia Mathematica (see Flew 1979). As Lakatos (1978) (and others) points out all logic (deductive, inductive or informal) is about words and whether or not these words describe reality is a judgment.

Ecological knowledge at some unsophisticated level becomes basic in that it is true over a wide and well delineated domain. This level, however, requires that we make generalizations similar in precision to the ones the wisest rural bartenders and loggers make. Generalizations like clear deep lakes never produce many fish but shallow green lakes sometimes don't either; with no cover and edge game is scarce but it may be scarce even with cover and edge; a dry stream has few fish when it resumes flow but a stream with a permanent flow may have no fish either; no arctic hares no lynx, lots of arctic hares maybe still no lynx, etc., etc., etc. What was once successful research is now folklore. In criticizing ecological research we tend to overlook many past victories because they are now "crude" and all support negative rather than positive predictions. According to Popper (1963), "Every good scientific theory is a prohibition." Maybe we don't know where to look for our successes.

Literature Cited

Barrett, Wm. 1979. The illusion of technique. Anchor Press/Doubleday. Garden City, N.Y. 392pp.

Brown, Harold I. 1977. Perception, theory and commitment. The University of Chicago Press, Chicago. 203pp.

Buck, D. H., C. F. Thoits, and C. R. Rose. 1970. Variation in carp production in replicate ponds. Trans. Amer. Fish. Soc. 99(1):74-79.

Bunge, Mario. 1973. Method model and matter. Reidel, Dordecht, Holland. 181pp.

Eddington, A. S. 1928. The nature of the physical world. MacMillan Co., New York, N.Y. 373pp.

Feyerabend, Paul K. 1978. Against method. Verso, Great Britain. 339pp.

Flew, Anthony (Editor). 1979. A dictionary of philosophy. St. Martin's Press, New York, N.Y. 351pp.

Galat, David L. 1982. Primary production as a predictor of potential fish production: application to Pyramid Lake, Nevada. Ph.D. Dissertation, Colorado State University, Fort Collins, Colorado.

Hanson, N. R. 1969. Perception and discovery. Freeman Cooper, San Francisco, California. 435pp.

Hanson, N. R. 1961. Patterns of discovery. Cambridge University Press, Cambridge, U.K. 241pp.

Heisenberg, Werner. 1955. Physcis and philosophy. Harper and Row, New York, N.Y. 212pp.

Hempel, Carl G. 1966. Philosophy of natural science. Prentice-Hall, Englewood Cliffs, N.J. 122pp.

Hull, David L. 1974. Philosophy of biological science. Prentice-Hall, Inc., Englewood Cliffs, N.J. 148pp.

Lakatos, Imre. 1978. The methodology of scientific research programmes: philosophical papers, Vol. 1. Edited by J. Worrall and G. P. Currie. Cambridge University Press. 250pp.

McConnell, W. J., S. Lewis, and J. E. Olsen. 1977. Gross photosynthesis as an estimator of potential fish production. Trans. Amer. Fish. Soc. 106(5):417-423.

McConnell, W. J. 1965. Relationship of herbivore growth to rate of gross photosynthesis in microcosms. Limnol. Oceanogr. 10(4):539-543.

Meyer, David. 1978. The relationship between primary production and fish production. M.S. Thesis, Colorado State University, Fort Collins, Colorado. 56pp. Got thesis

Needham, Joseph. 1954-65. Science and civilization in China. 4 Vols. Cambridge University Press, Cambridge, U.K. 1923pp.

Popper, K. R. 1968. The logic of scientific discovery. Harper and Row, New York, N.Y. 479pp.

Popper, K. R. 1965. Conjectures and refutations. Harper and Row, New York, N.Y. 417pp.

Resher, Nicholas. 1978. Peirces philosophy of science. University Notre Dame Press, Notre Dame, Ind. 125pp.

Roberts, Walter O. and Henry Lansford. 1979. The climate mandate. W. H. Freeman and Co., San Francisco, Cal. 197pp.

Romesburg, H. Charles. 1981. Wildlife science: gaining reliable knowledge. J. Wildl. Manage. 45(2):293-313.

Toulmin, Stephen. 1960. The philosophy of science. Harper and Row, New York, N.Y. 176pp.

Wittgenstein, Ludwig. 1983. Remarks on the foundations of mathematics. The MIT Press, Cambridge, Mass. 444pp.