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HAMERSTROM SCIENCE FROM A "*Gabboon's*"[1] POINT OF VIEW

The rewards of scientific work include personal gratification gained from ingenuity, satisfied curiosity, recognition, and financial gain. Recognition by scientists of work by a peer is achieved in at least three ways: by citing a person's published paper, through awards from societies or institutions, and by attributing an idea or approach to a person.[2]

Frederick and Frances Hamerstrom have fared well in all of these recognition categories. However, because even the most valuable knowledge often is vague initially and not acquired in identifiable blocks, giving recognition can be difficult. Sometimes a "seed" for an idea is acquired but this seed can mature into a slightly different idea after nurturing. Furthermore, subtly different world views or paradigms can be acquired through someone else's influence and these can play an important role in the recipient's future. Because such subtle, conceptual acquisitions often fall through the sieve of the reward system, the purpose of this special "Hamerstrom Issue" of the *Journal of Raptor Research* is to pay tribute to recognizable and subtle contributions that Fran and Hammi[3] have made. Such contributions may have been made without the full awareness of the benefactor or Fran and Hammi.

A second purpose for this essay is to examine the Hamerstroms' approach to research from a methodological perspective. I compare what I recognize to be a Hamerstromian style in biological research to other approaches in science. My interpretation will no doubt reflect more of my own perceptions than those of Fran and Hammi, for the same reasons that science "... is not derived solely from what is immediately apparent to the eye and ear, but is also constructed by inference from all manner of other items of information."[4]

Having been in the forefront of a number of movements within ornithology and wildlife management according to some, the Hamerstroms have also been perceived as being on the periphery of mainstream biological science by others. Forefront contributions include, for example, the insightful study of dominance among individually marked Black-capped Chickadees (*Parus atricapillus*)[5] at a time when only loosely-conceived descriptive studies were commonplace in the ornithological literature. The Hamerstroms have championed bird and mammal trapping, marking and data recording methods; they have saved a population of an endangered subspecies, the Greater Prairie Chicken (*Tympanuchus cupido pinnatus*), from extirpation through innovative ways; and have made several significant conceptual contributions to conservation and population biology.[6] Yet, some of their approaches have seemed unconventional, and their abstinence from certain experimental and statistical approaches puzzling. In an attempt to explain this potential paradox, I examine two features of the Hamerstroms' approach to biology: their emphasis on natural history with a reluctance to wax theoretical, and their aversion for using analytical statistics.

In Fran and Hammi's own words, "Speculation (properly labelled) has its place." While conservative with speculation, the Hamerstroms stressed the need for prediction.[7] However, the tying of observations into a theoretical knot through imaginative speculation was done sparingly by them. Hamerstrom science seems to resemble the approach of a kind of purist. Interpretation was conservatively applied and speculation disciplined. I have witnessed the Hamerstroms' insatiable interest in discussing observations of natural events and patterns in nature. It did not seem to matter whether those patterns dealt with raptor biology or with an attempt to map the location of a human gene on a chromosome, a project my wife carried out. However, I detected comparatively less interest in discussing what predictions would follow from parental investment theory or from evolutionary stable strategies. Why this reluctance to move out on a theoretical limb, when going beyond the collation of individual observations and into the formulation of general statements is an essential part of science?

Despite its considerable power, the scientific method has limitations. According to T.S. Kuhn,[8] "philosophers of science have repeatedly demonstrated that more than one theoretical construction can always be placed upon a given collection of data." Often no one single method of investigating the unknown is clearly best. Nor should any one method be easily discarded because it has limitations, as an unlucky "carpenter may reject his tools."[8] However, the most capable carpenter is the one who produces a useful product despite the limitations his or her tools might have. The carpenter who is fully aware of the limitations of the tool and able to compensate for them is likely to be the most capable in the long run. The Hamerstroms' execution of the craft has much to recommend it.

Perhaps the Hamerstroms' conservative approach to theory was because of an awareness of the limitations in the scientific way of knowing. Albert Einstein explained his view of how scientific discoveries are made.[9] His de-

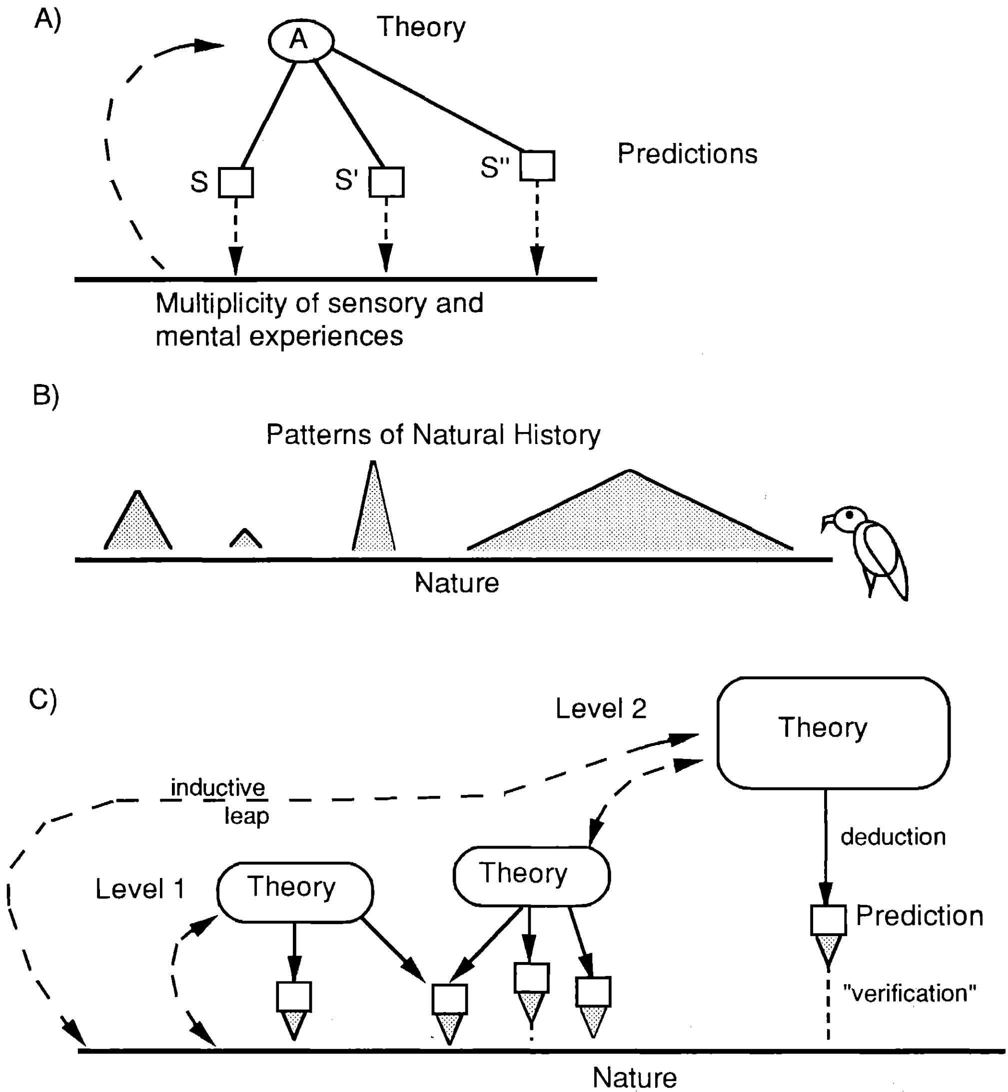

Figure 1. Three versions of how scientific discovery can be accomplished are presented. "A" is the version originally formulated by Albert Einstein, shown as adapted from G. Holton (op. cit.). Version "B" is intended to represent the Hamerstroms' style of science where data are often collected over the long term and conservatively interpreted within the context of natural history and functional ecology. Version "C" attempts to represent theoretical ecology where the source for ideas in the verification of predictions comes from theory. The connection with nature here often includes only a narrow window (e.g., short-term studies, specific data gathered; see also text).

scription went beyond the simplified textbook portrayal of the scientific method, described as hypothesis formation followed by logical deduction. Einstein recognizes four distinct components in scientific investigation which include: 1) the world around us is experienced through our senses, 2) these "sense experiences" are integrated with a person's prior conceptions and then formulated into a theory using intuition (induction), 3) logical predictions are derived from these theories (deduction), and 4) these predictions are "verified" using interpretation (Fig. 1A). The deductive connection between theory and prediction may be the strongest link in the chain of scientific discovery. Verification between prediction and reality relies on a considerable amount of interpretation and thus on the

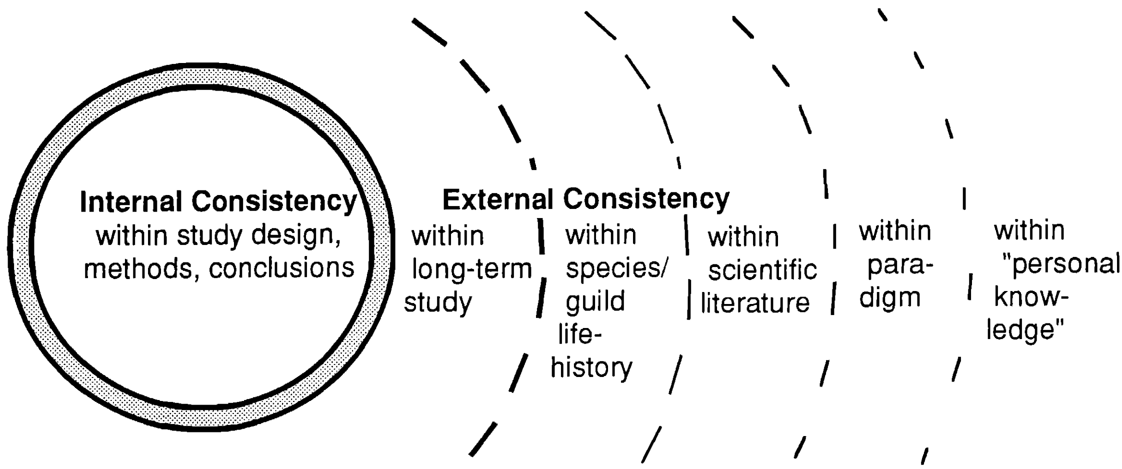

Figure 2. Different levels at which judgement of the rigor and value of an ecological investigation can be made. The levels are not mutually exclusive. See text for explanation.

accuracy of previously gained knowledge. Induction is potentially weak, because it is greatly influenced by the scientist's psychological nature. Theory is formulated through induction.

Using some or all of Einstein's components, biologists employ three identifiable approaches in research: 1) the mere description of natural events, 2) the description and explanation of repeated patterns of natural events (functional ecology) and 3) hypothesis testing. These approaches differ fundamentally. In the description of single events or patterns, the data source comes from nature. Symbolized in the form of a triangle, the triangle's base rests on the source of ideas, namely nature. The triangle's peak extends away from nature, little or far depending on the level of abstraction inherent in the interpreted explanation. The base of the triangle probably can never touch nature because the human interpreter's senses are naturally limited. These approaches (1 and 2), I believe, are compatible with the Hamerstroms' style of research (Fig. 1B). It is no coincidence that a Raptor Research Foundation award, established in the Hamerstroms' honor, recognizes individuals who have made a significant contribution to understanding the natural history of raptors.

In contrast to the description of patterns and events, in testing hypotheses the source of ideas does not come solely from nature. Ideas can be "theory-laden," derived from other theories (Fig. 1C). Testing theories that were derived from other theories and that relied on a series of *ad hoc* assumptions is not the Hamerstroms' style. When asked at the 100th annual meeting of the American Ornithologists Union in New York whether a student should concentrate on theoretical or descriptive biology, the plenary session speaker Gordon H. Orians advocated both.

In their own work, the Hamerstroms have stopped short of formulating highly abstract interpretations. As a result, many of their data went no further than the description of patterns and basic ecological interpretations. Twenty-two years of data on the behavioral ecology and population dynamics of the Greater Prairie Chicken, perhaps one of the largest and most comprehensive data sets on a natural population, have been underused from a theoretical point of view. It would appear that the Hamerstroms have shied away from using ingenuity to formulate intellectually challenging models to account for events in nature. Not so. The Hamerstroms have not down-played the mystery in nature. Instead, they have explored mystery through visual art and poetry, and sought it in music. Fran once deplored the trend in primary and secondary schools to stress the hypothetico-deductive link in scientific investigation while down-playing the personal dimension and mystery surrounding animals. Fran and Hammi feel strongly that youths should be encouraged to experience nature first hand, both out of doors[10] and within.

Another characteristic of Hamerstrom science, in addition to a reluctance to employ abstract theory, is the reluctance to employ analytical statistics. This does not mean that the Hamerstroms are uncritical in their thinking; on the contrary, critical thinking has been a prominent feature of theirs. Although statistical analysis was not a major focus in their university education, this paucity of "training" in statistical procedure has not been the determining factor in their style. They have collaborated with first-rate statisticians including F. Hilpert, G.W. Snedecor and statisticians at the Wisconsin Department of Natural Resources. Hammi and Fran have felt that the first choice was to present data in English and with revealing, legible figures. They disapproved of "cluttering any publication with non-essential mathematics." Usually, the Hamerstroms have delayed publication of data until the pattern was so clear that analytical statistics seemed superfluous. As a result, their investigation has been free of the constraints that are sometimes imposed by the use of statistical tools and design.

While I personally have never fully understood the reluctance by Fran and Hammi to employ a modicum of statistical analysis, their approach is well worth consideration. The issue touches on 1) what it is that makes a scientific conclusion rigorous and 2) on the sociology of scientists.

When a reader examines a manuscript, she or he can evaluate the work at many levels. These levels can be divided into two categories: internal and external consistency. An article describing the methods, results and conclusions of some investigation might be termed internally consistent if certain widely accepted criteria are met. Such criteria can include: posing a significant biological question, choosing methods that are currently accepted by peers, using and describing the methods adequately, providing conclusions that follow logically from the methods and results, and so on. Essentially, the criteria center around possible problems with the study in an internal, narrow sense. The view is "inward" with a concentration on procedure. The Hamerstroms in my view upheld many procedural expectations which included for example an elegant simplicity in the style of writing, a clarity of presentation and the use of proper terminology.

At another level (Fig. 2), a study that satisfies all or most of the procedural queries may still not "sit well" with the reader, it may be judged somehow "externally inconsistent." I have come across no scientists that have asked whether a conclusion "feels right" as often as the Hamerstroms have. This question of feeling right has sometimes elicited glib and condescending smiles.

Many nonscientists find the observation that two scientists given the same set of data can arrive at different conclusions very disturbing. Many nonscientists and scientists alike believe that knowledge is convergent; that eventually only one and the same conclusion will survive the ultimate test. The way in which scientists gain new knowledge is complicated and more tenuous than many care to admit.

The question of whether a conclusion feels right, however, has much in common with T.S. Kuhn's[11] notion of a paradigm, a fundamental guide for scientific inquiry. According to Kuhn, paradigms bridge the understanding that has been gained in the past with questions for the future. Paradigms "are the source of the methods, problem-field, and standards of solution" (p. 103). Paradigms are larger than theories because theories "must be restricted to those phenomena and to that precision of observation with which the experimental evidence in hand already deals" (p. 100). A paradigm is far less well defined than a theory and a paradigm changes as new information is gained and old information is rejected. A paradigm allows the independently thinking scientist to ask "What is my gut feeling about this?" By placing different levels of importance on each of a complex set of concepts contained within a "paradigmatic umbrella," scientists can legitimately arrive at different conclusions.

A paradigm, as a conceptual tool in making inferences through induction, may be situated close to the final explanation on an inspiration (least defined seed of an idea) to explanation (firmly defined concept) continuum. The remaining space along this continuum may be more aptly occupied by what M. Polanyi termed "personal knowledge."[12] The point is that knowledge does not simply flow directly from scientific "facts" and figures, but the information of knowledge involves a huge personal dimension. I believe that this personal dimension is largely ignored in most graduate student programs; it was valued and in evidence at the Hamerstrom household.

To think that only those who employ up-to-date statistical procedure carry out "good science" is flawed. The difficulties encountered in the study of complex natural events are so enormous that even approaches which are considered to be state-of-the-art by peers often are insufficient. S.H. Hurlbert[13] concluded that of 176 experimental studies published between 1960 and 1983, 27% were designed inappropriately. L.L. Eberhardt and J.M. Thomas[14] discuss the problems encountered in extrapolating from the "focal" to the larger "target" population in a reductionist approach. They pose the question "Should we, in some sense, revert to descriptive ecology?" Once more, the carpenter's tools have limitations. The chain is only as strong as the weakest link. Perhaps, the message from the Hamerstroms is not to use the term "chain" when the strength is equivalent to that provided by a "string." Much of what is considered "good science" is done not because the method warrants it or because a paradigm dictates it, but because it is considered the approach of choice by peers within one's "invisible college."[15]

The Hamerstroms have been highly independent in their thinking. They have been influenced little by the predominant "internal sociology of science"[16] or the "sociological setting"[17] which dictates scientific standards and procedures through consensus. For example, most geneticists agree that, when formulating a conclusion about heritability, gene-environment interactions need not be considered. This accepted omission is not because gene-environment interactions are not critical for the conclusion, but because the interactions are virtually impossible to measure. So, in many ways the "invisible college" has sanctioned a product even though the tools do not fully justify its production.

Hamerstrom science is reminiscent of a kind of investigation in natural history that is in danger of becoming extinct. L.L. Merrill[18] describes three views toward nature. The oldest view that prevailed for centuries is one in which things natural were romanticized; that which was natural was both beautiful and proper. Items contradictory to this view were ignored. In the 19th century, naturalists began to examine carefully every possible detail in nature. Observations were no longer edited, but data were collected rigorously and descriptions made critically. Views and approaches became measured, rational and precise. While natural history and science were frequently taken to mean the same thing, the two disciplines gradually diverged. Beside the natural history investigations of animals, plants and minerals emerged distinct "pure" sciences such as geology, biology and others. While natural history examined all of nature, science studied only a part of nature. Science became preoccupied with examining theories. "But even in the very different computerized climate of the late twentieth century, natural history remains popular, as an abundance of widely read modern writers attests—Joseph Wood Krutch, Rachel Carson, Edwin Way Teale, Aldo Leopold, Henry Williamson, Gerald Durrell, Archie Carr, Annie Dillard, John McPhee, and David Attenborough, to name but a few."[17]

Whether the relations between what I viewed to be "Hamerstrom science" and the science described by theorists exist in actuality may be debated. Most importantly, however, the Hamerstroms have caused me to try and look ever deeper at nature, the process of science, and the interaction between science and the public. I thank Samuel J. Barry, Patrick Colgan, Reg Fleming, Fran Hamerstrom and Gordon H. Orians for their insightful comments on an earlier version of this manuscript.—**Josef K. Schmutz, Department of Biology, University of Saskatchewan, Saskatoon, SK, Canada S7N 0W0.**

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[6] E.g., F. Hamerstrom 1986, Harrier, hawk of the marshes, Smithsonian Institution Press, Washington, DC.

[7] Hammi predicted the population explosion of White-tailed Deer in Wisconsin long before it occurred—in the 1930s.

[8] T.S. Kuhn 1970:76, The structure of scientific revolutions, University of Chicago Press, Chicago, IL.

[9] G. Holton 1978, The scientific imagination: case studies, Cambridge University Press, New York.

[10] F. Hamerstrom 1975, Walk when the moon is full, Crossing Press, Freedom, CA.

[11] op cit.

[12] M. Grene [Ed.] 1961, The logic of biology, pages 191–205 *in* The logic of personal knowledge, Routledge and Kegan Paul Ltd., London, U.K.

[13] 1984, Pseudoreplication and the design of ecological field experiments. *Ecol. Monogr.* 54:187–211.

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[15] G. Holton, op. cit.

[16] J. Ziman, op. cit., p. 4.

[17] G. Holton, op. cit., p. 5.

[18] 1989, The romance of Victorian natural history. Oxford University Press, New York.

Out of the Mews

I waited
 until the moon slipped her silvery body
 behind a cloud
Barefoot
 I slid into the mews
 and spoke to my eagle—softly—not loud
In the deep of the night
 the jesses you made my eagle
 moved onto each leg
 no fright.
Oh, beautiful night.

by Fran Hamerstrom
(Reprinted from The Falconer)