Book Reviews

Evolution, Brain, and Behavior: Persistent Problems

Edited by R.B. Masterton, William Hodos, and Harry Jerison
Lawrence Erlbaum Associates, 1976, Hillsdale, New Jersey, $14.95

Reviewed by
Dwight Hines
Bangor Mental Health Institute
Bangor, Maine 04401

The book being reviewed is a companion to another volume — *Evolution of Brain and Behavior in Vertebrates*, edited by R.B. Masterton, M.E. Bitterman, C.B.G. Campbell, and N. Hotton, 1976. Both volumes originated from a conference that was held in Tallahassee, Florida, in 1973. The purpose of publishing the volumes was to present "the facts and arguments of paleontology, neurology, and behavior," and both were "specifically written in a form usable to students in each other's field." The authors and editors have succeeded in their goals and have produced two books that provide excellent accounts of general and specific areas of evolution. In addition, the books provide much needed, comprehensive reference sources in the areas covered. Although the present review focuses on the second volume, both books are of equally high value. For these reasons, the books should be combined into one volume.

Methodology is the purported theme of the second volume; hence, the title *Persistent Problems*. The first chapter is titled "The Comparative Method of Investigation" and is, obviously, too large a topic for eleven pages. It is the third chapter, by Harry Jerison, "Principles of the Evolution of the Brain and Behavior", that will send students and teachers out to purchase Jerison's other book (*Evolution of the Brain and Intelligence*, Academic Press, 1973) for a more in-depth coverage of evolution and the nervous system. Jerison's work has assumed renewed importance since the coinage of the word "sociobiology". If "sociobiology" is to be more than a house of cards, works such as Jerison's must comprise the basic rules of the sociobiological paradigm. It would leave large lacunae within sociobiology if only E.O. Wilson and his students were used as the sole bases of theory and experimentation.

Glade Whitney writes an abbreviated but (for this volume) complete chapter on "Genetic Considerations in Studies of the Evolution of the Nervous System and Behavior". One point made early in the chapter is that the modern theory of evolution is a "synthesis of basic genetics with Darwinian natural selection". The discussion of population genetics is solid, but the role Whitney, following Mayr (1963), ascribes to behavioral

influences on "initiating new evolutionary events" is not self-evident. At best, the argument is misleading; at worst, it is teleological. Statements such as "behavioral modification may often precede evolutionary changes, at least in non-nervous system morphology", are worthless — for they cannot be tested in an ecologically valid manner — and lead to a reductio ad absurdum answer: How can a behavior change be transmitted without a change in the nervous system. I am not arguing that neural plasticity does not exist; I am arguing against a concatenation of pleiotropic-environmental-behavioral-evolution. The chain of events, and the proper timing, is nonsense.

Whitney does do an excellent job of explaining h^2 (the heritability of a given trait) and distinctly separating it from "heritability in the broad sense, or as the coefficient of genetic determination." The section on the h^2 of intelligence, however, is superficial and biased (as can be seen by the references cited) toward "intelligence" having a rather large h^2. It is confusing that egg production in both Drosophila and poultry have an $h^2 \approx .2$, yet intelligence has an h^2 of .5 to .8. Is egg production more complex than intelligence?

I don't know if "unbiased" testing procedures (*Neurometrics*, E. Roy John, 1978) will confirm coefficients this high. If I were black or a member of any other group that has had a history of oppression, I would oppose "unbiased" heritability measures of intelligence as strongly as "biased" measures.

In summary, the book is worth reading, but like most symposia the results have been scattered throughout the literature before and after the symposium. Publishers who wish to maintain sales, and librarians who wish to cut purchases might consider some type of ban on replicate publishing of symposium results. Perhaps such a policy could be made explicit to symposium participants. Because the book was published in 1976, it is imperative that *Science Citation Index* (or a similar search system) be used to locate recent advances in the neuropeptide transmitters in mammalian and reptilian brains as well as assessing the increased importance of mathematical models in evolutionary biology (*Science, 209*, 1980, 78-89). In a similar fashion, R. Ted Steinbock's *Paleopathological Diagnosis and Interpretation: Bone Diseases in Ancient Human Populations* (C.C. Thomas, Springfield, 1976) is definitely a book of interest to readers of *Evolution, Brain, and Behavior*. The difficulty concerning changes in human skulls found in different geographic and geologic states has a special value to students involved in evolution of the brain and behavior (see the *Paleopathology Newsletter*, Henry Ford Hospital, Detroit, Michigan: Attention: Dr. Adrian Cockburn).

Medical Technology and the Health Care System

A study of the diffusion of equipment-embodied technology
National Research Council, National Academy of Science, 1979, $10.95, 303 pages, Washington, D.C.

Reviewed by
Dwight Hines
Bangor Mental Health Institute
Bangor, Maine 04401

The editorial policy for book reviews of *The Journal of Mind and Behavior* (JMB) includes the caveat that books that are not worthwhile in content, organization, and cost will not be reviewed. The policy is based on the fact that negative book reviews can stimulate sales almost as well as positive book reviews: the "Banned in Boston" effect. In reviewing *Medical Technology*, I have found that the book is poor, misleading, and a waste of money. The consideration that the National Academy requires books not reviewed to be returned to them was the final blow.

Medical Technology was ostensibly written by the Committee on Technology and Health Care. The book was reviewed by other members of the Academy of Science, the Academy of Engineering, and the Institute of Medicine. The committee changed during the course of the study: the original chairman and four other committee members left and were replaced, I think. In addition, one member presented a dissenting opinion. The new committee, now approximately nine people, "met *four* times to deliberate the issues, reach conclusions, and develop a set of recommendations for policy and research that bear on medical technology." The committee also commissioned six study papers that they reviewed. The papers are included in the appendixes. The appendixes are the bulk of the book.

The scope and limitations — and they are large — of the study were restricted by the committee "to an examination of 'equipment-embodied technology,' which is the equipment, procedures, services, or systems that depend primarily upon capital equipment." p. V (preface). The questions asked were: (1) "What is the relationship of equipment-embodied technology to health care costs and social benefits?" What categories are worth their cost and not worth their cost? (2) "What factors determine the way decisions are made about the acceptance of new equipment-embodied technology into health care?" Are some technologies "adopted" too quickly or not quickly enough? Is public policy important? (3) "How is information about the effectiveness and efficiency of new technology developed and used?" Where in "technical change is information generated?" How can we improve development and diffusion of medical technology information? It is safe to say that this book did not answer these questions satisfactorily. The study noticeably ignored ethical and social issues. Also

ignored were the influences of industry upon technology. The report is virtually useless to people needing to make informed decisions about concrete problems.

The ninety-eight pages of the report, with recommendations in italics, read like a series of platitudes. So much thought and work was placed into this book that one of the figkres (p. 6) is directly taken from an earlier report by the Office of Technology Assessment (with acknowledgment, of course). One would hope for more, particularly since the mandate and the money for a unique report were granted. The recommendations of the committee in the summary are a good example of the level of effort and coordination entering into the total report.

Recommendation 1: Change reimbursement policy to promote adoption of technology . . . "the precise avenues of reimbursement reform must be chosen in a larger context. Prospective reimbursement of hospitals and the capitation method of payment are especially promising . . ." [Only promising?]

Recommendation 2: "Public subsidy" may be needed for information systems. [Imagine that!]

Recommendation 3: "A national coordinating body" is needed.

Recommendation 4: Technology should be adopted that does not strain ("burden") the resources of the regulating agency.

[It appears to me that the report suggests a new agency that should not have to think or work very hard.]

The report further vacillates when it deals with economic issues (remember, social and ethical considerations are not included in the analyses). The report continues to state on every page that more research is needed, or critical variables have not been analyzed. Yet, they make recommendations.

The discussion on the "Problems in Adoption and Use" of technology lists the examples of: (1) Fetal Monitors — "benefits unclear." (2) CAT Scanners — adoption of more than 500 in last 3 years — "Well in advance of the collection of information" on their effectiveness. [The committee needed a neurologist.] (3) Gastric Freezing — diffused widely and then dropped before it could be evaluated. [Does not appear to support the need for *another* agency.] (4) Medical Information Systems — significant barriers except in research centers to fund demonstration and development. [*Not true.* The Maine Medical Center in Portland, Maine, is a good example of a viable, working, system.] (5) Rehabilitative Technologies — have not lived up to expectations. [The above examples, except for gastric freezing, are hardly discussed. Except for the obvious note that CAT scans are the "procedure of choice" and that gastric freezing techniques were "self-limiting" (without more undue harm than "biofeedback" — the current untested technology), it appears that clinicians involved with gastric freezing did not need a new federal bureaucracy looking over their shoulders.]

It is intriguing that a regulation policy such as the state certificate of

need (appropriately called CON) has not had the desired impact on purchases of technology greater than $100,000, the typical limit imposed by the agencies. Fortunately, CON laws in one study were found to be significantly *negatively* correlated with adoption of some equipment and not related at all to the adoption of other equipment. One problem that the report admits to, almost halfway through, is that we need "study of the operational meaning of 'standard of care' for the diffusion of new diagnostic technology . . ." At this point I am in agreement with the minority report that the conclusion, "If 'merchandise' is a villain that causes people to spend money, the wisest course is to take money away or somehow make 'merchandise' undesirable," is disquieting.

At this point I'm going to cease critical evaluation of the study, although much more is needed, and concentrate on providing constructive suggestions for individuals who are in the trenches of medical care and need the technology available through automatic or computerized information systems.

Medical information systems have been around for a long period of time. Originally they were not known as "systems" because they were not electronic, not automated (electromatic), and were used primarily by the people who needed the systems — the physicians. They were called "case files." With the development of the "Buck Rogers" syndrome (heavy technological emphasis and sales pressure from industry), many individuals believed systems had to have their "blinking lights and cathode ray tubes" to deliver efficient and modern medical care and to attract patients. The individuals were not physicians. They were, and are, business managers. They realized the importance of the savings in manpower by using automated billing systems. Administrators in hospitals also realized the value of computers in billing and the importance of machines for the delivery of services, the enhancement of services, resources for education, scientific information, and the evaluation of services. Contrary to the report on Medical Information Systems in the book being reviewed, IBM is very much in the medical information business. They (IBM, Digital, Data General) have versatile hardware and software available for immediate general and, in some cases, specialized procedures. There are mini-computers that can network in a number of configurations; a large mainframe with terminals at the pharmacy, the nursing stations, and the labs, can also be purchased.

There are a few reasons for purchasing an automatic system: (1) the system reduces repetitive tasks, that are prone to error, that require qualified health personnel whose skills and training are better utilized for patient care; (2) there is a *substantial* reduction in paper handling, shuffling, verifying, and copying by clerical and professional personnel; (3) increases in staff are not necessary *for the machine system* even when there are increases in patient load; and (4) unless the machine is for improved medical care, such as ultra-sonic image enhancement, staff requirements for direct patient care should decrease. The decrease would be in phases so

individuals would not experience the trauma that, for example, automobile workers are presently experiencing in the United States. Any system that does not include *all* of the above four requirements plus development time should not be adopted. Thus, the above are necessary for an efficient but not a sufficient system economically and technologically.

Economically, there are definite constraints that must be considered before purchasing a *well known* and appropriate system. The constraints may be so strong as to preclude adoption of almost any configuration. I say almost because it is fairly obvious that a cost/benefit analysis of a family-hobby system is relatively expensive and has little value other than recreation (passive) and education (untested) for the purchaser. It is sobering, if not humorous, to see what trivial and abusive uses occur by people who cause enough trouble with pocket calculators, when they buy a "home computer." They are usually the same people who couldn't understand or use slide rules.

Middle and top level management must have a coherent policy or set of goals and objectives that are advanced by acquisition of a system. Preferably the goals and objectives reflect long-term planning. Too many machines are purchased on the basis of the advice of an outside "consultant" who spends a total of one week determining the mechanical needs of the user. Personal and agency long-term goals and funding problems are often not considered: maintenance contracts, room preparations, upgrading and expansion of machines and training of staff using the system (the hidden costs of computers) are often overlooked until after the machine is delivered. I wonder how long it will take for the pejorative tag of the "used car salesman" to evolve into the "computer salesman"?

In summary, *Medical Technology* is well deserving of the Golden Fleece Award. How much did the study cost? I estimate that it probably cost at least $200.00 per page. A very *low* estimate ($60,600). I would also like to propose that the National Academy of Science change their book review policy. I'll be writing a review of *Pharmaceuticals in Developing Countries* in the next issue of JMB. *Pharmaceuticals* is very much better than *Medical Technology*. This statement is not meant to be an accolade for *Pharmaceuticals in Developing Countries.*

Citation Indexing

by Eugene Garfield
John Wiley & Sons, New York, 1979, $15.95, 274 pages

Reviewed by
Dwight Hines
Bangor Mental Health Institute
Bangor, Maine 04401

> "First thing you have to understand is the way everything here is specialized. If it's matches, you go to one cafe. If it's women, you go to another. Furs are subdivided into Sable, Ermine, Mink, and Others. Same with dope: Stimulants, Depressants, Psychomimetics . . . Wpat is it you're after?"
>
> "Uh, information?" Gee, this stuff tastes like Moxie . . .
>
> "Oh. Another one." Giving Slothrop a sour look. "Life was simple before the first war. You wouldn't remember. Drugs, sex, luxury items. Currency in those days was no more than a sideline, and the term 'industrial espionage' was unknown. But I've seen it change — oh, how it's changed. The German inflation, that should've been my clue right there, zeros strung end to end from here to Berlin. I would have stern talks with myself. 'Semyavin, it's only a temporary lapse away from reality. A small aberration, nothing to worry about. Act as you always have — strength of character, good mental health. *Courage*, Semyavin! Soon all will be back to normal. But do you know what?"
>
> "I thought it was cigarettes."
>
> "You dream." He brings out a list of Zurich cafes and gathering spots. Under Espionage, Industrial, Slothrop finds three. Ultra, Lichtspiel, and Straggeli. They are on both backs of the Limmat, and widely spaced.
>
> "Footwork," folding the list in an oversize zoot-suit pocket.
>
> "It'll get easier. Someday it'll all be done by machine. Information machines. You are the wave of the future." T. Pynchon, Gravity's Rainbow, p. 300, 1973.

Information is now handled by machines. The individual most responsible for the machinery and the continuing conceptual development of automated information retrieval is Eugene Garfield. The first few chapters of the book are devoted to the how and what of citation indexing. They aren't exciting, yet the chapters are important in showing where subjective decisions occur in the Institute for Scientific Information (ISI).

Intermission. The book published by Wiley draws heavily on *Essays of an Information Scientist*, 2 Volumes, $25 for both, published by ISI. Because many of the references in *Citation Indexing* refer to *Essays*, I requested some of the references from ISI. I received both volumes. Being a bit compulsive, I actually read both volumes of *Essays*. In fact, if not for Aspirin and the essays on the most-cited articles and journals from lower estuaries — which I skipped — I could not have completed the books. Garfield is highly creative, a genius plowing an important field. Ironically, our government still does not take ISI proposals for funding seriously. Perhaps the National Academy of Science is afraid that the results from an objective data base will be compared to the results of the government's sub-contracting "studies" on scientific innovation (see Hines, 1980, for a review of *Medical Technology* and *The Health Care System: A Study of*

the Diffusion of Equipment-Embodied Technology, NAS, 1979, a book review in preparation for *The Journal of Mind and Behavior*, 1980).

I recommend *Citation Indexing*, but not *Essays*. *Essays* is too expensive unless, and this is important, you are specifically interested in the History of Science or Information Retrieval: Past, Present, and Future. The *Essays* is much more delightful to read than *Citation Indexing*, but the expense is not justified for individual purchase. *Essays* is a good example of the need for individual and institutional rates for such books. All decent libraries will purchase *Essays*, but you can't make cryptic notes in the margins of someone else's book.

End of intermission. Most people are not discovering Science Citation Index (SCI) until graduate school. Far too many don't learn of it there. Someone has to take a heavy blame for this ignorance. Most people know that if they have a good idea, and good ones are so rare, all that they need for a coverage of past literature is 2 or 3 good review articles. From there, primary sources are identified, and read when appropriate. The problem arises when the review articles are over 2 or 3 years old. *PARANOIA*. Your good idea obviously has been done by someone in the last 2 years. With SCI you have the references most appropriate to your idea and you can go *forward* in time. References give you bricks and mortar. SCI gives you security. Obviously, in writing your article you omit the studies that were poorly done and poorly interpreted. You should know it is far better to be criticized for omission than commission. Indeed, it is more fair not to reference a work than to do so because it will mislead others if it continues to be cited. Also, do not **ever** underestimate the power of the "invisible college." It's only money. So you think until you live on food stamps, or much, much less. Also, don't overestimate the power of the invisible college. Good research can be done without grants.

I am assuming you know there is a lag between publication of an article and its listing in SCI. Have no fear because *Current Contents* (CC) is published weekly, by ISI. Without a grant you may not have easy access to *CC*. You definitely will not have the luxury of having an assistant check the pages that you will read, or type reprint requests you may not receive. ISI has a rather good solution to not receiving reprints or the lag (and cost) of inter-library loan: OATS. OATS is cheaper and faster than Inter-Library Loan (ILL), as definite as death and taxes, and also honest to the original publisher of the article. The ignorance of tradition is too well established to break the ILL network. Don't even try. Save the Aspirin/Valium money that you will be forced to spend if you question the ILL network for buying good books.

Intermission No. 2. Empirical fascism is one way of describing the people who use *Citation Indexing* to determine tenure, worthiness, and quality.

Proverbs for Paranoids Number 1: If they get you asking the wrong questions they don't have to worry about the answers. Twenty-five percent of the papers published are never cited. The average citation is only a bit

above zero. Garfield wastes paper and time when he writes the final chapter, and several *Essays*, in a distasteful and forced *nondefense* of citation counts. Don't give the paranoids fuel for unnecessar fire. No light is generated.

End of second intermission. The meat of the book is that of nongenetic transmission and evolution of knowledge. Referencing behavior changes with time and is different for the different schools of social and natural science. Because progress in the sciences does occur, we hope the progress, whether it be incremental or schistic, is not only measurable *but predictable*. At present, citation information is being used descriptively, or for post-hoc analyses at very simple levels of description. Although there is a danger of presentism in exploiting a data base that is less than 20 years old, the benefits far outweigh any risks. The results do not appear equivocable, for example, in the DNA citation analysis study, cited below.

Garfield used citation analysis of "the development and validation of the DNA theory of genetic coding that controls protein synthesis." *The Genetic Code* (Isaac Asimov, 1963) was taken as the starting point and the "control" for the important DNA events. So, a "network diagram of the events and relationships described by Asimov" was produced and a second diagram was generated from the references in the first diagram. Did Asimov miss any important relations or events that citation analyses identified? Yes and no. Based upon a citation analysis historiograph, thirty-one relationships *not* noted by Asimov were identified. Some papers were identified that were written by researchers who were cited by "nodal" papers. Looking at co-authors not mentioned by Asimov and comparing their impact (citation counts) in 1961, with the impact of authors that were mentioned, Asimov's record (and memory) was supported (72%). Investigators credited by Asimov had a greater impact than those not credited. Only one paper, on the replication of RNA in the absence of DNA, found by citation analysis to have been important, was missed by Asimov. It appears that citation analysis can be used by a "non-historian" to provide an excellent reconstruction of important scientific events. I seriously doubt that Garfield and his associates can be considered nonhistorians although his interpretations of natural and social science documents are probably in error. Garfield states:

> For the comparative analysis, the three-year sample of 14,110 cited social science documents that qualified for pairing by meeting the citation-frequency threshold of 10 was compared to a one-year sample (1973 SCI) of 15,973 cited natural science documents that qualified for pairing by meeting a citation-frequency threshold of 15. The difference in the citation-frequency thresholds used was intended to keep the data bases from which pairs were extracted as close as possible in size.
>
> The major difference seen between the two structures was that the social sciences were much more tightly integrated. This was shown by several measures, the major one being the ratio of actual to potential pairs. By this measure, the degree of integration for the social sciences was 83% versus 56% for the natural sciences. (p. 136)

Garfield writes that "other measures of cohesiveness showed the same sort of difference." I disagree strongly with the interpretation of the differences

between natural and social sciences. Rather than measures of "integration" or "cohesiveness," I think the measures are better interpreted as incestuous or, more acceptably, miscegenation for the social sciences and natural sciences respectively.

In support of the incest interpretation, I would cite Kuhn (1972) and Price (1970, in C.E. Nelson and D.K. Pollack, Editors, *Communication Among Scientists and Engineers*), and Garfield, this book, p. 136. I'm tempted to cite myself but I fear that is unnecessary because of the general acceptance that creativity is the combining of disparate elements or concepts that lead to novel and useful interpretations of the old elements. The more miscegenation, or combination of concepts that do not appear to be related, the more chance *alone* is operating to produce novel (creative) shifts in our perception of the world. In poetry, "my love is like a red, red rose" does not have the metaphor distance of Miller's "The sun is like a bleeding anus." Whether poetry represents a paradigm shift is not important. The *idea* that metaphor distance can be useful in the sociology of science is damned important (Hines, D. The Application of Metaphor Distance to Scientific Documents for the Prediction of Future Basic Research Paradigms: The Trace Elements and Drug Metabolism. Grant, 1980, to be submitted).

Price (1970) has developed what he has termed Price's Index. The index is "the proportion of references represented in the last five years of literature." The Price Index "seems to correspond very well with what we intuit as hard science, soft science, and nonscience as we descend the scale." So, the hard sciences have an index ranging from 54% to 70%, the soft sciences cluster around 41.9% $\pm$ 1.2%, and the humanities from less than 10% to 33%. Price's Index appears to be a good measure of those areas that have a rapidly moving research front. We can see the results of the shift from calculators to computers in the hard sciences. The softer sciences are only marginally different from the humanities. If the effect of our advancing research front is to bury (compact) previous research, the social sciences are not burying or compacting the dead. Psychology's renewed interest in "cognition" is a case in point. It is interesting to observe the movement of clinical psychologists as they age from behavioral to Gestalt to cognitive and in some cases, eclectic biases. The analogue in physics would be a return to Thomistic logic, then to Aristotelian logic, and then to solipsism. It might be a good idea to encourage miscegenation of the social sciences with the natural sciences. The idea is testable using ISI's data base. I would predict, for instance, that metaphor distance is greater in the natural sciences (as exemplified via CI titles) than in the social sciences. After all, any group that finds that a subatomic particle has a mass and then states they now know the weight of the universe displays a great "metaphor distance." Empirically then, metaphor distance would be the sine qua non of a rapidly moving research front.

I am so impressed with *Citation Indexing* I think it should be incor-