Letters

Life of Scientific Publications

The philosophic appeal of Weiss' analogical description of the life functions of a body of knowledge [*Science* **131**, 1716 (1960)] is weakened by the way the illustrations are handled. To show the "real fate of plain recorded data" he selected lengthy series of several journals and tabulated the citations of earlier works in terms of the age of the reference at the time it was cited. The resulting frequencies were then transformed into percentages of *citations* and plotted against age of reference. The curves obtained all dropped sharply as the age of the reference increased. In some cases more than half of all of the references made were to works published within the previous 5 years. Weiss concludes: "the active life span of pure data is at any rate amazingly short: they die of either assimilation or oblivion."

Since the curves presented are based on percentages of citations rather than percentages of works published, conclusions drawn from them can refer only to citations, not to works published. The probability that a paper cited will be of a given age (which is what Weiss' curves show) is not the same as the probability that a paper of a given age will be cited (which is what he is concerned about). There is some evidence [Dennis, *Am. Psychologist* **13**, 457 (1958)] that the latter probability *increases* with age. Contrary to Weiss, Dennis found that the older the work, the greater the probability that it would be cited.

The source of the apparent paradox lies, of course, in the "population explosion" in scientific papers. The 19th century saw 15-fold increase in scientific publications between its first and last decades, over half of all of the papers being published in the last two decades of the century [Dennis, *Am. Psychologist* **13**, 457 (1958)], and the output seems still to be accelerating. If Weiss' curves were corrected for the actual number of papers there were of a given age, they would certainly flatten out considerably and they might even reverse their direction. His point, however, is well taken; while he has shown only that most of the papers that people refer to are new, it is also quite true that there are many more new papers than anyone can or ever will refer to. The problem which Weiss sees as one of senescence and decay appears to be more nearly one of infant mortality.

S. James Goffard
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I find the marginal comments by Goffard and Windle quite noteworthy. In theory, their plea for a correction factor for the proliferation of journals is well taken. In practice, however, the contention that the curves would then "certainly flatten out considerably and . . . might even reverse their direction" is invalid on several counts. (i) In both of the "several" journals sampled, the curves for the 1st and 10th years of the sampling periods are essentially the same, despite the "population explosion" of journals during that period. (ii) Curves for two different 10-year periods (1938–49 and 1950–59) of the same journal (*Biol. Bull.*) are essentially congruous. (iii) An "experimental" proof that correction for publication volume would not have altered the essential trend of the curves lies in the fact that the major temporary drop in publication volume during World War I registered in the annual curves only as a minor dip.

Since the terse treatment of the subject in my article does not reflect the volume of data from which the conclusions have been distilled, I appreciate the present opportunity for supplementary—and, I hope, clarifying—comment.

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The article by Weiss provides an interesting analogy between biological growth and the growth of documentation. I fear, however, that its simplicity may be misleading. There are other ecological factors operating in the field of documentation of knowledge which need to be considered. I mention only two of these factors here.

The editorial blue pencil still provides a kind of natural selectivity as a brake on the growth of documentation. The fields of science are now so well disciplined that it may be safe to say that papers which are not published are not worth publishing. It is the specialization and overspecialization in the sciences which brings new journals into being at a rate recently estimated as two a day. The editorial blue pencil is doing its best to control this tide of information. The actual need is for more information, not less, to increase the basic research which is the foundation upon which is built our expanding science. Somehow, we shall have to innovate our reporting devices to be sure that vastly increased amount of information does eventually become knowledge.

Another problem is the suggestion regarding the fast-aging and the slow-aging periodical. This neglects the painful problem of re-invention. Librarians have long been aware of the necessity of buying serials back to the first volume when funds, space, and availability permit. A thorough "search of the literature" is still a good safeguard against expending research funds on work which has already been reported elsewhere. If the fast-aging journal is discarded and its citations are not blended into any subsequent studies, what is to prevent the research it has reported from being done over again?

Actually, there appear to be two evaluations involved in the information-knowledge process. The first is editorial —whether to accept or reject a manuscript; the second is documentary and may occur years after the publication of the paper. The paper's research impact is then measured as a weight factor in a citation study. Its scientific durability may then be impartially assessed. I suggest reference to the paper by Raisig on "Mathematical evaluation of the scientific serial" [*Science* **131**, 1417 (1960)] for one recent, improved means of making this evaluation.

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Stern's View of Lewis H. Morgan

In a recent issue [*Science* **131**, 1435 (1960)] you published a review of Carl Resek's *Lewis Henry Morgan, American Scholar* in which the reviewer, comparing Resek's work to that of my late husband, Bernhard J. Stern, states that "in Stern's hands, Morgan, caught in a crossfire of Marxism and Boasian anti-evolutionism, suffers the worst of both worlds and emerges as a virtual class enemy as well as a 'not erudite,' unoriginal thinker with a few good ideas and more bad ones."

Not only is this estimate of Bernhard J. Stern's *Lewis Henry Morgan, Social Evolutionist* (University of Chicago Press, 1931) intemperate and lacking in scientific objectivity but it is totally erroneous.

Since Stern's book is out of print, may I request that you set the record straight by publishing some of his evaluations of Morgan. In the final chapter summarizing Morgan's contributions, he says:

"Pioneers in unploughed fields of science scrape the soil thinly leaving the more intensive work to be done by generations that follow. They may plant some seeds of thought that later prove infertile, for their knowledge of the character of the field is imperfect. Morgan was such a pioneer. He was among the first to extend the science of social origins into the remote past. In doing so he used an evolutionary method popular in his period but since discarded as applied to the study of culture. Divorced from its evolutionary setting much of Morgan's work remains a permanent contribution to the yet infant science of anthropology. His Iroquois study is still considered a classic. His discovery of the kinship systems was epoch-making and irrespective of his interpretations and his arrangement, his compilation in the field has proved to be a lasting storehouse of fact for all later anthropologists. . . ."

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Sweating in Man

Victor Cummings [*Science* **131**, 1675 (1960)], in his article on thermoregulatory and emotional sweating in man, is apparently unaware of the careful work of Chalmers and Keele [*J. Physiol.* **114**, 510 (1951); *Brit. J. Dermatol.* **64**, 43 (1952)], who demonstrated that neither type of sweating is blocked by an intradermal adrenergic blocking agent but that both are blocked by atropine; these results are essentially identical with Cummings'.

It is unfortunate that Cummings raised again the specter of adrenergic innervation of human sweat glands without presenting a more forthright analysis of the available evidence which tends to put the ghost to rest. The pertinent points, covered in the review of Randall and Kimura [*Pharmacol. Revs.* **7**, 365 (1955)] except where noted, are as follows.

1) Human sweat glands respond to directly administered epinephrine and related compounds, and to acetylcholine. Both substances act on the same glands [Mellinkoff and Sonnenschein, *Science* **120**, 997 (1954)].

2) The response to exogenous epinephrine is blocked by local or systemically administered adrenergic blocking drugs (for example, dibenamine).

3) Emotionally induced sweating is blocked by systemically administered dibenamine, but not by locally administered dibenamine; it is blocked by locally administered atropine.

4) Dibenamine analogs have been shown in other circumstances to have central blocking activity [Sawyer and Parkerson, *Endocrinology* **52**, 346 (1953)].

The simplest and most likely explanation of these observations is that there is no adrenergic innervation of human sweat glands and that adrenergic blocking drugs reduce sweating only by their central blocking action. The question of the physiological significance of the responsiveness of the glands to directly administered epinephrine remains open.

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