STIGLER'S LAW OF EPONYMY*

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No reader of Robert K. Merton's work on the reward system of science could fail to be struck by his insightful and engaging discussions of the role of eponymy in the social structure of science. The uninitiated should read (and reread) his 1957 address, "Priorities in Scientific Discovery,"[1] but for present purposes I must at least repeat his definition of eponymy, as "the practice of affixing the name of the scientist to all or part of what he has found, as with the Copernican system, Hooke's law, Planck's constant, or Halley's comet."[2] Merton went on to discuss three levels of a hierarchic order of eponymous practice: at the top there are a few men for whom an entire epoch is named, then comes a larger number of scientists designated as "father" of a particular science, and, finally, "thousands of eponymous laws, theories, theorems, hypotheses, instruments, constants, and distributions."[3] The present paper is an attempt by an Outsider to the sociology of science to shed some light on the workings of the eponymic reward system at this third level, and a report on a small statistical investigation into eponymous practices of my own field, statistics.

I have chosen as a title for this paper, and for the thesis I wish to present and discuss, "Stigler's law of eponymy." At first glance this may appear to be a flagrant violation of the "Institutional Norm of Humility,"[4] and since statisticians are even more aware of the importance of norms than are members of other disciplines, I hasten to add a humble disclaimer. If there is an idea in this paper that is not at least implicit in Merton's *The Sociology of Science,* it is either a happy accident or a likely error. Rather I have, in the Mertonian tradition of the self-confirming hypothesis, attempted to frame the self-proving theorem. For "Stigler's Law of Eponymy" in its simplest form is this: "No scientific discovery is named after its original discoverer."

Examples affirming this principle must be known to every scientist with even a passing interest in the history of his subject; in fact, I suspect that most historians of science, both amateur and professional, have had their interest fueled early in their studies by the discovery (usually accompanied by an undisguised chortle) that some famous named result was known (and better understood) by a worker a generation before the result's namesake. A detailed study of any scientific area will show, I would argue, that this phenomenon persists with a generality rivaling that of any other "law" in the social sciences, indeed even that of Merton's famous hypothesis that "all scientific discoveries are in principle multiples."[5]

Merton's hypothesis is related to, yet distinct from Stigler's Law (henceforth humbly referred to as simply the Law). It might appear that the Law is in fact stronger than the hypothesis, that the Law states that a discovery is always named after the wrong one of its multiple discoverers. But this is not a consequence of the Law; a discovery may in fact be named after someone who could not be reasonably counted as even one of its discoverers, much less the original one. Thus a scrupulous examination of the works of economist Robert Giffen has failed to reveal even a

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semblance of a statement (much less a proof) of what has come to be known commonly as "Giffen's paradox," although an earlier statement (published before Giffen's birth by Simon Gray) has been noted.[6] And St. Matthew did not discover the Matthew effect!

Evidence in favor of the Law is readily available in any field whose history has been subjected to serious scrutiny. Thus in my own field of mathematical statistics it can be found that Laplace employed Fourier transforms in print before Fourier published on the topic, that Lagrange presented Laplace transforms before Laplace began his scientific career, that Poisson published the Cauchy distribution in 1824, 29 years before Cauchy touched on it in an incidental manner, and that Bienaymé stated and proved the Chebychev inequality a decade before and in greater generality than Chebychev's first work on the topic. (Incidentally, in each of these cases there is evidence, sometimes even citation, to show that the earlier work was known to the later worker before he embarked on his investigation. These were *not* instances of multiple discovery.) Examples of this type are not in short supply, and they are not all cases where the discovery preceded its namesake: "Mayer's method" of combining inconsistent linear equations really first appeared in work of Laplace published a quarter century after Mayer died, and recent scholarship[7] has shown that one of the most famous of mathematical relations, the Pythagorean Theorem, was known before Pythagoras, was first proved after Pythagoras, and in fact Pythagoras himself may have been unaware of the geometrical significance of the theorem! But while such examples and other anecdotal evidence could be multiplied with ease,* a true defense of the Law would require a much more arduous examination of the rather ill-defined population of eponyms than I am prepared to undertake at present.[8] Instead, I shall accept the Law as true, and I shall concentrate on adducing reasons for its universality and implications for the reward system of science.[9]

One explanation for the Law has been given by a historian of science in these words: "Every scientific discovery is named after the last individual too ungenerous to give due credit to his predecessors." (That I do not identify the source of this quotation is due to a lack of information, not a lack of generosity.) This analysis of eponymic inaccuracy is witty, but surely false. Strictly interpreted it would imply that discoveries never receive lasting names (since ungenerosity shows no sign of being extinct), and such a claim would be easily refuted, witness the aforementioned Pythagorean Theorem, and Pascal's arithmetical triangle (which was actually published earlier by Pascal's teacher Hèrigone,[10] and was known in China before that). Even if we loosely interpret the statement, as blaming inaccurate names upon inadequate citation and a lack of corrective historical scholarship,[11] it is wrong: frequently posterity has pinned a label on a discovery despite the honored individual's citation of a worthy predecessor, or in the face of abundant historical evidence suggesting another candidate, as we shall see later.[12]

It is also not true that eponyms are bestowed capriciously. They are, as I have claimed, inaccurate as a means of identifying a discovery's originator, but it is rare that an eponym is awarded to an individual who has not done some work at least tangentially connected with the discovery, and rarer still that he has not made important contributions to his science generally. If the Law is not due to ignorance or caprice, and if other explanations such as stupidity or deceit are dismissed out of hand, to what *is* it to be ascribed? I wish to argue that the inability of eponymical practice to

*In fact, the Law is at once exemplified and self-exemplified in this statement from G. J. Stigler, *The Theory of Price*, 3rd edition (New York: Macmillan, 1966), page 77: "Here the P represents Hermann Paasche, who, like Laspeyres, was not the first to propose the index named after him. If we should ever encounter a case where a theory is named for the correct man, it will be noted."

meet the assumed purpose for the practice (to commemorate a discovery's original discoverer) is in fact a necessary consequence of the real role the practice plays, which Merton has taught us is as a key element of the reward system of science.

I begin with two observations. First, names are not given to scientific discoveries by historians of science or even by individual scientists, but by the community of practicing scientists (most of whom have no special historical expertise). Second, names are rarely given, and never generally accepted unless the namer (or accepter of the name) is remote in time or place (or both) from the scientist being honored. I shall present some evidence relevant to these claims (particularly the crucial second observation), but let us first pause to consider why they are true, and how they are related to the reward function of eponyms.

The most prestigious eponyms stand at the pinnacle of the scientific reward system—a scientist's name is enshrined in the literature as a mark of the enduring significance of his work, promising to remain there long after his work has ceased to be directly cited by the profession;[13] a kind of intellectual immortality is achieved. If these statements are to be true (and they must be widely seen as true or the eponym would cease to function as an important scientific reward), then the award of an eponym must not only be made on the basis of the scientific merit of originality, but more importantly it must be perceived by the community of scientists as based on merit and not upon personal friendship, national affiliation, or the political pressures of scientific schools. Historians of science may provide lists of nominations for eponymical recognition, but if an eponym is to be viewed as meritorious, then the community will look to specialists in the area of the discovery for guidance, not to historians who are usually specialists in no area. But more must be true—the scientists whose works are consulted for approval of the eponym must be seen as impartial, as only swayed by scientific judgment. An award of an eponym may be attempted by close friends, students, or political associates, but it will not be successful. It is the acceptance by the community at a distance, and thus the promise of immortality through acceptance by future generations of scientists, that gives the award its extraordinary prestige.

Some scientists, when first confronted with the Law, pause only briefly before reciting a string of supposed counterexamples. Many of these examples can be shown on further examination to be confirmations of the Law (although lengthy research may be necessary), but others fall in one of two general categories that would require separate handling in a more definitive investigation of this topic. Eponyms can be found in a wide variety of sizes (from the "F-statistic" to the "Fisher-Neyman-Halmos-Savage factorization theorem"), and in many flavors (from the redundantly reverential "Gaussian linear model" to the accusatory "so-called Cauchy distribution"[14]), and it would be impossible for any simply stated theory to accommodate all such current usage. One large category of examples that appear eponymous at first glance (and because of the close proximity in time and space to the individual named also appear to be exceptions to the Law) are in reality a short form of citation: as common knowledge of the (often important) cited article fades with the passage of time, so does the use of the "eponym," to be replaced by a more specific journal citation, where needed. The Law is not intended to apply to usages that do not survive the academic generation in which the discovery is made. I do not mean to exclude the possibility that an eponym may be contemporary to the discovery it names, although such cases are rare and according to my second observation the namer would have to be remote from the honored scientist in place or discipline. I do, however, insist that an eponym demonstrate its widespread acceptance as a name, and the test of time is the simplest way to show this.

Another class of eponyms that would deserve separate treatment in a deeper and more thorough investigation of this subject is that of multiple awards, such as the

aforementioned "Fisher-Neyman-Halmos-Savage factorization theorem." This example, which includes mention of two antagonists (at least three if the word is used in a philosophical sense) and salutes work done on two continents over a quarter-century span, is typical of many that achieve the requisite impartiality[15] by a very different route than does the simple eponym. This shotgun approach, which represents a statement that the development of the idea was the product of a community of scientists rather than a single individual, is more likely to hit a scientist who could be classified as the original discoverer (although there would be little agreement as to which of the many this was), and, in any case, the variation over time of the names included in the list renders the study of these cases extremely difficult.

The necessity of the appearance of impartiality to the award, and the apparent agreement that this appearance is best achieved through the distancing of the namer from the honored scientist, accounts for the general reluctance of scientists to propose their colleagues for eponymic recognition and the general resistance of the profession to such attempts. One famous example of this concerned the planet Uranus, discovered by William Herschel in England in 1781. Herschel attempted to name the planet "Georgium Sidus" after his patron King George III, but while it was briefly known as "the Georgian" in England, continental astronomers rejected the name as too narrowly nationalistic (despite its adherence to the Law). Ironically, Lalande in Paris suggested as a solution to the dilemma that the planet be called Herschel. This name enjoyed some currency on the continent (although less so in England), but either because the naming of a planet after a mere mortal was considered unacceptable, or because of the inexorable workings of the Law, "Herschel" eventually yielded to Bode's suggestion of "Uranus."[16]

There is another interesting phenomenon that can be explained by the necessity of the appearance of impartiality. I am aware of some notable instances of challenges to an eponymic award where, curiously, the challenge has been made by a student or countryman of the honored scientist in behalf of a scientist from some other country. For example, the claim that Bienaymé, in 1853, published what is known as the Chebychev Inequality (after an 1867 paper of Chebychev), and that Chebychev was well aware of Bienaymé's work, was most convincingly advanced by Chebychev's illustrious student Markov, and has recently been extensively discussed in a book, one of whose authors was born in the Ukraine.[17] I lack objective evidence as to how general this phenomenon is, but if it is widespread, as I suspect it is, it signals that the resistance to eponymic recognition of close associates may in fact be a norm of scientific behavior, one which serves the role of protecting the practice from degenerating to a regional or factional basis, with the consequent fall in the reward's incentive power.

An extreme case of inaccurate eponymous assignment, and important support for both the Law and the case I present for it, was pointed out to me by Robert K. Merton in a letter. I refer to the common practice of naming scientific units (such as the watt, ohm, and volt) after individuals other than their originators. This practice is particularly significant as it has been fully institutionalized in many cases, through bureaus of standards or nomenclature commissions which guarantee the accuracy of the Law by design. While these commissions seek an appropriate matching of individuals and units, they aim for general commemoration of excellence rather than the labelling of units by their inventors' names. The attaching of old names to new units guarantees the validity of the Law in this case, just as the international character of these bodies is intended to provide the appearance of impartiality required for the acceptance of the name.

If my claims are accepted, if eponyms are only awarded after long time lags or at great distances, and then only by active (and frequently not historically well informed) scientists with more interest in recognizing general merit than an isolated achievement (even a conspicuous one), then the Law can be seen to follow. For it should not then

come as a surprise that most eponyms are inaccurately assigned, and it is even possible (as I have boldly claimed) that all widely accepted eponyms are, strictly speaking, wrong. The very inaccuracy of the assignment stands as additional testimony to its remoteness and impartiality, and helps to guarantee its prestige and survival!

In the remainder of this paper I wish to present evidence, through a study of the pattern of acceptance of one eponym in my own field, of the rate and manner in which such names are adopted. In keeping with the Mertonian tradition, the study will be quantitative.

The discovery I have chosen to consider is the probability distribution with density

$$f(x) = \frac{1}{\sqrt{2\pi}} e^{-x^2/2}.$$

This distribution is today most commonly called the "normal distribution" or the "Gaussian distribution," after the great mathematician Carl Friedrich Gauss, who associated it with the method of least squares in his first publication on that topic, in 1809. Of course, the Law tells us that because $f(x)$ is now called the Gaussian distribution and Gauss did in fact study it, he must have been preceded. Indeed he was, for Gauss himself cites Laplace in connection with $f(x)$ in his 1809 book, and indeed Laplace did touch on the distribution as early as 1774.[18] But since a very few modern writers call it the Laplace or even the Laplace-Gauss distribution, we must look further back for its origin. Such a search would be rewarded, as current historical scholarship marks the distribution's origin as being a 1733 publication by Abraham De Moivre.[19] Interestingly enough, De Moivre's work was known by Laplace and Gauss, and his claim as the originator of the distribution is substantiated by the fact that *no* modern writer, as far as I am aware, calls it the "De Moivre distribution."

I selected the distribution $f(x)$ for study for two principal reasons. First, it has occupied a central position in mathematical statistics since at least 1810, and thus has been available for eponymical award for a long time. Second, there are several major candidates for the award, from different countries, and the relationship between nationality and acceptance of the name may be investigated. It is also true that the distribution has (and has always had) popular names such as "error curve" or "normal distribution" which may be used as alternatives to eponyms, and thus the use of an eponym may be regarded as a matter of choice, not necessity.[20]

An indication of the current state of eponymic designation of $f(x)$ can be found by consulting several recently compiled permuted title indexes of statistical journals and research papers. The most massive such index is *Index to Statistics and Probability: Permuted Titles,* published in 1975, edited by Ian C. Ross and John W. Tukey.[21] This index covers the literature through 1966, with the preponderance of titles dating from 1945. I find the index gives 1099 titles referring to $f(x)$ as "normal," "Gaussian," or "Laplace-Gauss," or some variation on these names. Of these, 18% (199) referred to Gauss, and 1% (7) referred to Laplace-Gauss, giving an eponymic total of 19% (206). The remainder of the references were to "normal." Another recent index, *An Author and Permuted Title Index to Selected Statistical Journals,* published in 1970, edited by B. L. Joiner, *et al.*[22] gives an eponymic total of only 11% (42 of 330), but that index is based upon only six Anglo-American journals (mostly from the 1960s), and misses what we shall see is a heavier continental usage of eponyms. A better indication of the worldwide practice can be found from the *Current Index to Statistics,* published annually since 1975.[23] The 1975 volume gives an eponymic total of 28% (44 of 159); the 1976 volume gives a total of 30% (56 of 185). All of these references were to Gauss, none to Laplace. Based upon these data, it seems fair to say that about 20% to 30% of all references to $f(x)$ in the titles of current research papers in theoretical or applied statistics are eponyms, and nearly all of these refer to Gauss. The question

then is, how was this level achieved? When, where, and at what rate was $f(x)$ awarded to this Titan of Science?

A permuted title index is not available for earlier literature; even if one were it is probable that changes in the character of the scientific literature generally (e.g., from monograph to research paper), and statistics in particular, would render it useless. I have therefore turned to another, more stable source of information, the textbook. We may expect textbook usage of eponyms to be at once more conservative and more liberal than the literature of active scientific research. Since a textbook, in reaching for a large market, may be expected to reflect the views and practices of only large segments of the scientific community, we may expect that it will be more resistant to new names than the active literature is, waiting until the verdict of the community is in. On the other hand, once a name has been accepted by a sizable fraction of the community, we may expect textbooks to be more generous than the community, by listing the name as one of several alternatives (whereas authors of research papers ordinarily use only a single name).

I selected for study a total of 80 textbooks, covering the period 1816 to 1976. All were what I would classify as statistical texts, although the emphasis varied from least squares for geodesists to correlation for economists and sociologists, and the levels varied from elementary to advanced. All made conspicuous mention of the distribution $f(x)$. The selection was not random. I canvassed my own library, and that at the Center for Advanced Study in the Behavioral Sciences, and I included a number of older texts in the Stanford University library which had not yet been placed in inaccessible "auxiliary storage." If the selection is biased, I believe the bias would be toward the inclusion of books by well-known authors, and toward frequently used, often reprinted texts (although in no case was more than one edition of a work included). Where conscious selection was most heavily exercised (recent English language texts), an attempt was made to choose general texts and avoid overrepresentation of any single orientation.

Each book was classified by country and year of publication, and according to how the author described the distribution $f(x)$. (Several authors used more than one description.) The data are presented in the Appendix, and summarized in TABLE 1. A brief explanation of the grouping of TABLE 1 is in order: All books were classified Eponymic (the author gave at least one eponymic description) or Other (he gave *no* such description). This classification differs slightly from that of the APPENDIX, where Noneponymic means the author gave at least one noneponymic description (but may have included eponymic descriptions as well). The countries of origin were grouped as Germanic (Germany and Austria), French (France and Belgium), Other Continental (Italy, Holland, Romania, and the Scandinavian countries), and Anglo-American (England and the U.S.A.). Most of the eponyms encountered (and all prior to 1920) referred to Gauss.

Perhaps the most striking features of TABLE 1 are the slow rate of acceptance of an eponymic description of $f(x)$, and the difference between the acceptances in the Anglo-American and Continental literatures. The first eponym encountered in the sample was in reference to Gauss in a book by F. R. Helmert published in Germany in 1872, 61 years after Gauss's relevant publication and 17 years after Gauss's death. Perhaps partly because of Helmert's identification with Gauss's homeland, the name was slow in gaining currency. An American text (by T. W. Wright) used it in 1884, but it is more likely that it was J. Bertrand's use of "loi de Gauss" in 1889 that signaled the name was acceptable to the larger community of scientists. The use of this eponym seems to have spread steadily after that, achieving some currency in Italy after the First World War, and being mentioned in all five of the post-Second World War Continental texts examined. The apparent recession in its use in France between the wars may be a confirmation that even the most impartial of scientific awards is not immune to political events.

The rate of eponymic acceptance in the Anglo-American literature, on the other hand, has not undergone any marked change since 1884. I suggest that this lower rate in England and America may be an instance of a generally lower use of eponyms in these countries than on the Continent, but the present data set does not permit the investigation of this hypothesis.

Another interesting aspect of these data is the pattern of references to Laplace, the only candidate alternative to Gauss receiving mention. The earliest mention of a Gauss-Laplace distribution in the sample was in an Italian work in 1920; of the four subsequent mentions of Laplace that were noted, three appeared in France. It would seem that after a century (Laplace died in 1827) French authors felt sufficiently

TABLE 1

EIGHTY BOOKS PUBLISHED 1816–1976, CROSS-CLASSIFIED BY YEAR AND COUNTRY OF PUBLICATION, AND ACCORDING TO WHETHER THE BOOK EMPLOYED AN EPONYMIC USAGE FOR $f(x)$ ("EPONYMIC") OR IT EMPLOYED NO SUCH USAGE ("OTHER")

a. Books published 1816–1884

	Eponymic	Other	Total
Germanic	1	2	3
French	0	7	7
Other Continental	0	2	2
Total Continental	1	11	12
Anglo-American	1	4	5
TOTAL	2	15	17

b. Books published 1888–1917

	Eponymic	Other	Total
Germanic	3	2	5
French	4	0	4
Other Continental	0	4	4
Total Continental	7	6	13
Anglo-American	1	8	9
TOTAL	8	14	22

c. Books published 1919–1939

	Eponymic	Other	Total
Germanic	0	1	1
French	1	3	4
Other Continental	2	2	4
Total Continental	3	6	9
Anglo-American	2	11	13
TOTAL	5	17	22

d. Books published 1947–1976

	Eponymic	Other	Total
Germanic	2	0	2
French	2	0	2
Other Continental	1	0	1
Total Continental	5	0	5
Anglo-American	4	10	14
TOTAL	9	10	19

distant from their countryman to advance his name for the award. I personally feel that Laplace's historical link to $f(x)$ is stronger (as well as earlier) than Gauss's, but as yet this eponym has not gained much currency. This may be due to eponymic inertia (it is very difficult to change an established name), or to an eponymic version of the Matthew Effect: the award of the prestigious $f(x)$ to Gauss may simply be a signal of the scientific community's verdict that Gauss was the greater mathematician. In any case, Laplace has in recent years been awarded, as a consolation prize perhaps, eponymic recognition through the spreading acceptance of "Laplace distribution" to mean the less important distribution

$$g(x) = \frac{1}{2} e^{-|x|}.$$

The data set I have presented is quite limited in scope, concerned as it is with but a single discovery in a single field. It does support the thesis that eponyms are only

awarded by the scientific community at a considerable distance from the recipient of the award, thus lending plausibility to the case presented for the Law. It is likely that studies of other discoveries in other fields would show considerable variation in the time lag between the discovery and the award, according to the importance of the discovery, the nation involved, and the institutional organization of the field involved.[24] We may expect that in years to come, Robert K. Merton, and his colleagues and students, will provide us with answers to these and other questions regarding eponymy, completing what, but for the Law, would be called the Merton Theory of the reward system of science.

Acknowledgments

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4. Merton, R. K. Ref. 1: 303.
5. Merton, R. K. Ref. 1: 356.
6. Stigler, G. J. 1947. Notes on the History of the Giffen Paradox. *J. Polit. Econ.* **55.** Reprinted as Chapter 14 of *Essays in the History of Economics,* George J. Stigler. 1965. U. of Chicago Press, Chicago, Ill.
7. Tian-se, A. 1978. Chinese Interest in Right-Angled Triangles. *Historia Mathematica,* **5:** 253–266.
8. An important source for future eponymic research would be *Eponyms dictionaries index. A reference guide to persons, both real and imaginary, and the terms derived from their names,* by James A. Ruffner (Detroit: Gale Research Press, 1977), but even this large volume is not without its errors or omissions. See, for example, the short review in *Language in Society,* **7** (1978):149.
9. Some of my colleagues, commenting on a draft of this paper, have intimated that the norm of humility is not the only reason for avoiding an autoeponymous designation for the Law. They have ventured the suggestion that the Law is not correct, and even helpfully supplied collections of what are purported to be counterexamples. Now it may in fact be that the literal absolute truth of the Law cannot be defended without occasionally descending to the argumentative depths of adumbrationism (for a discussion of which, see page 20ff. of R. K. Merton's *On Theoretical Sociology,* New York: The Free Press, 1967), or appealing to as yet unnoticed (and earlier) multiple discoveries, but this would not be crucial to the main line of the argument. While exceptions to the Law will only be granted after a struggle and on a case by case basis, all that is really necessary is that the reader grant the frequent truth of the Law, and agree to the unreliability of eponyms as guideposts to original discovery. For this, the examples presented (and appeal to the reader's own experience) should be sufficient proof.
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11. What Merton has called the palimpsestic syndrome; see his *On the Shoulders of Giants:* 218–219, 1965. Free Press, New York, N.Y.

12. For another, similar adumbration of the Law, see *The Collected Scientific Papers of Paul A. Samuelson* 1966. J. E. Stiglitz, Ed. Vol. 2, 1503. M.I.T. Press, Cambridge, Mass.
13. COLE, J. R. & S. COLE. 1973. *Social Stratification in Science:* 31. U. of Chicago Press, Chicago, Ill.
14. HEYDE, C. C. & E. SENETA. 1977. *I. J. Bienaymé: Statistical Theory Anticipated:* 90. Springer-Verlag. New York, N.Y.
15. In more standard sociological terminology, what I call impartiality, is called the norm of universalism. See Merton, Ref. 1: 270.
16. NEWCOMB, S. 1878. *Popular Astronomy:* 355. Harper & Bros., New York, N.Y.
17. HEYDE, C. C. & E. SENETA. 1977. Ref. 14: 121–124.
18. Laplace's first encounter with $f(x)$ was as an approximation to the posterior distribution of a probability, a very different use than that Gauss made of $f(x)$. If only the linking of $f(x)$ to least squares is considered, then the earliest publication yet found was by the American Robert Adrain in an obscure magazine dated 1808, a year before Gauss's book appeared.
19. WALKER, H. M. 1929. *Studies in the History of Statistical Method:* 13–19. Reprinted 1975 by Arno Press. New York, N.Y.
20. I am currently engaged in joint research with William H. Kruskal into the history of the use of "normal" in this and other connections. See W. KRUSKAL, 1978. Formulas, Numbers, Words: Statistics in Prose, *In: The American Scholar,* **47:** 223–229, for a brief statement of some of our findings.
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22. JOINER, B. L., N. F. LAUBSCHER, E. S. BROWN & B. LEVY. 1970. An Author and Permuted Title Index to Selected Statistical Journals. National Bureau of Standards Special Publication 321. U.S. Department of Commerce, Washington, D.C.
23. *The Current Index to Statistics.* 1975–76. Published annually by the American Statistical Association and the Institute of Mathematical Statistics.
24. Some additional evidence relevant to my case may be found in D. Beaver's"Reflections on the Natural History of Eponymy and Scientific Law," in *Social Studies of Science* (Vol. 6, 1976, pp. 89–98). Beaver shows that as of 1961, eponyms honoring twentieth-century discoveries in physics were far less numerous than those naming earlier scientists, when the different sizes of the scientific communities are allowed for: in Beaver's cross-sectional analysis, the population of eponyms did not seem to be growing exponentially. I take this to be most plausibly explained by a long (say, 30–60 years) average delay in the award of eponyms, such as found in the present longitudinal study, and thus at least consistent with the case presented here. Beaver's different conclusion (that either fundamental discoveries are becoming rarer, or eponymic practice is undergoing a marked change) does not seem to me to be warranted by the data.

APPENDIX

DATA ON 80 BOOKS' EPONYMIC PRACTICES

		Names Used		
Year	Country	Gauss	Laplace	Noneponymic
1816	France			*
1837	France			*
1838	England			*
1843	France			*
1846	Belgium			*
1852	Belgium			*
1860	Germany			*
1867	Italy			*
1869	Belgium			*
1872	Germany	*		*
1874	England			*

APPENDIX (*continued*)

Year	Country	Names Used		
		Gauss	Laplace	Noneponymic
1877	Germany			*
1877	U.S.A.			*
1878	France			*
1879	Italy			*
1879	England			*
1884	U.S.A.	*		*
1888	England			*
1889	England			*
1889	France	*		
1892	U.S.A.			*
1892	U.S.A.			*
1896	U.S.A.			*
1896	France	*		*
1897	Germany	*		
1901	England			*
1903	Holland			*
1903	Denmark			*
1906	England			*
1906	Italy			*
1906	Austria			*
1906	Germany	*		
1908	Germany	*		
1908	France	*		
1909	Germany			*
1909	France	*		*
1911	England			*
1912	England	*		*
1917	Denmark			*
1919	England			*
1920	Italy	*	*	*
1921	U.S.A.			*
1921	Italy	*		*
1921	England			*
1921	France			*
1921	Austria			*
1923	U.S.A.			*
1924	France		*	
1924	England			*
1925	U.S.A.			*
1925	England			*
1928	France			*
1928	U.S.A.	*		*
1930	France			*
1931	Sweden			*
1931	Italy			*
1931	U.S.A.			*
1937	U.S.A.	*		*
1937	U.S.A.			*
1939	U.S.A.			*
1939	England			*
1947	U.S.A.			*
1948	France	*	*	*
1950	U.S.A.			*

APPENDIX (*continued*)

Year	Country	Names Used		
		Gauss	Laplace	Noneponymic
1950	U.S.A.	*		*
1952	U.S.A.			*
1956	Austria	*		*
1957	Germany	*		*
1957	France	*	*	*
1960	U.S.A.	*		*
1962	England			*
1963	Romania	*	*	*
1965	U.S.A.			*
1967	U.S.A.			*
1968	U.S.A.			*
1968	U.S.A.			*
1969	U.S.A.	*		*
1970	U.S.A.			*
1970	U.S.A.	*		*
1976	U.S.A.			*

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