Part II. LIFETIME OUTPUT OF PSYCHOLOGISTS AND THEIR IMPACT ON THE FIELD

Chapter 1 only scratched the surface of a complex and difficult issue: What must a psychologist do to attain a high degree of recognition in the field? The three chapters of Part II subject this question to much greater empirical scrutiny.

Chapter 3. Individual Differences in Productivity and Eminence

Individuals vary greatly on most psychological attributes and behaviors, and psychologists are no exception. Here I review the two main ways that psychologists can differ with respect to their influence on the discipline. First, I review what has been learned about creative output, including the cross-sectional distribution, the relation between quantity and quality, the longitudinal stability of individual differences, contrasts in the type of contribution, and the basis for long-term influence. Second, I examine what we know about differences in eminence. In particular, I look at the degree of consensus, the cross-sectional distribution, the correlation between eminence and lifetime productivity, and the stability of eminence across time.

Spanish philosopher Ortega y Gasset (1932/1957),

"it is necessary to insist upon this extraordinary but undeniable fact: experimental sciences has progressed thanks in great part to the work of men astoundingly mediocre, and even less than mediocre. That is to say, modern science, the root and symbol of our actual civilization, finds a place for the intellectually commonplace man allows him to work therein with success" (pp. 110-111).

Nobel laureate, Sir Howard Florey,

"Science is rarely advanced by what is known in current jargon as a 'break-through,' rather does our increasing knowledge depend on the activity of thousands of our colleagues throughout the world who add small points to what will eventually become a splendid picture much in the same way that the Pointillistes built up their extremely beautiful canvasses" (quoted in Crowther, 1968, p. 363).

PRODUCTIVITY

Re: Wilhelm Wundt – E. G. Boring (1950),

his daughter's bibliography cites 491 items, where an "item" is taken as any writing, from one of less than a single page up to the entire 2,353 pages of the last edition of the *Physiologishe Psychologie*. If we exclude mere reprinted editions, but include all the pages of every revised edition, and adding-machine shows that Wundt in these 491 items wrote about 53,735 pages in the sixty-eight years between 1853 and 1920. In spite of all the many one-page items, Wundt's average adventure into print was about 110 pages long, with over seven such adventures in the average year. If there are 24,836 days in sixty-eight years, then Wundt wrote or revised at the average rate of 2.2 pages a day from 1853 to 1920, which comes to about one word every two minutes, day and night, for the entire sixty-eight years. (p. 345)

How to scale?

Page counts? 53,735 (Wundt) or 62,935 (Piaget), but …

Publication counts?

E. G. Boring 505, Wundt 503, Sigmund Freud 330, William James 307, Johannes Müller 285, Gustav Fechner 267, Hermann von Helmholtz 229, Alfred Binet 227, Francis Galton 227, Abraham Maslow 165, and Charles Darwin 119 (Bringmann & Balk, 1983)
Cf. Albert Einstein 607, Henri Poincaré 530.

Need for weighting? E.g.,

texts and other scholarly books = 20, edited books = 10, book chapters = 6, journal articles = 4, magazine articles = 2, reprint articles = 2, book reviews = 2, and unpublished reports = 1, with a complicated formula for apportioning differential credit in cases of multiple authorship (Furham & Bonnett, 1992).
Or according to journal impact?
(e.g., Clemente, 1973; Stephan & Levin, 1991).
What about multiple authorship?
(e.g., Kyvik, 1990)

Productivity score dependent on scale: Wundt vs. Piaget

Yet only for fine distinctions
For full cross-sectional variation:

"it don't make no nevermind" (e.g., Wainer, 1978).

Variation and Distribution

The normal, Gaussian, "bell-shaped" curve? No…

Dennis (1954c): "whether the aggregate publications of any generation of scientists are made up primarily of the work of the highly productive minority or are composed chiefly of the contributions of the less productive majority" (p. 191).

- Group I included 160 individuals covered in Carl Murchison's (1932) *Psychological Register*;
- Group II involved 587 individuals treated in a study published by Samuel Fernberger (1938), using *Psychological Abstracts* for output measure;
- Group III contained an unspecified number from a study later published by Kenneth Clark (1957), using *Psychological Abstracts* for output measure;
- Group IV concerned 229 individuals who had published original articles in the *American Journal of Psychology* (between 1887 and 1900) and the *Psychological Review* (between 1894 and 1900).

	Group				
Decile	I	II	III	IV	*M*
1	47	37	42	37	40.75
2	21	21	21	21	21.00
3	12	14	14	11	12.75
4	8	10	9	9	9.00
5	5	8	6	5	6.00
6	3	6	4	3	4.00
7	2	3	2	3	2.50
8	1	1	1	3	1.50
9	1	0	1	3	1.25
10	0	0	0	3	0.75

- Those psychologists who are in the top 10% in terms of output account for between 37 and 47% of all publications, with a mean of 41%.
- The bottom 10% ranges from 0 to 3%, with an average of less than 1%.
- The top half of the most productive psychologists are responsible for 90% of the total output, leaving the bottom half with the remaining 10%.

For Group I

The range of publications listed in the *Psychological Register* was from 0 to 130 (born prior to 1879 but who were still living in 1932)

"the most productive man published more titles than the 80 persons who make up the lower five deciles" (p. 191).

the 16 psychologists who make up the top decile represent almost uniformly a distinguished group: M. W. Calkins, June Downey, Knight Dunlap, C. E. Ferree, Shepard I. Franz, M. E. Haggerty, C. H. Judd, J. H. Leuba, Max F. Meyer, L. M. Terman, E. L. Thorndike, J. E. W. Wallin, H. C. Warren, Margaret F. Washburn, J. B. Watson, and R. M. Yerkes. Of these 16, only two, Haggerty and Wallin, were not among the 538 "important psychologists" identified by Annin, E. G. Boring, and R. I. Watson in 1968.

Same elitist distribution found in other disciplines:

E.g., Dennis (1955): American secular music, books in the Library of Congress, and publications in gerontology and geriatrics, geology, infantile paralysis, chemistry, and linguistics.

Hence, two laws:

1. The *Price law* of Derek Price (1963): if k is the number of scientists who have made at least one contribution to a given field, then half of all those contributions can be credited to $\sqrt{k}$. When this law is applied to the Dennis data, it appears that psychology is slightly less elitist that predicted. In Group IV, E.g., there were 229 psychologists, implying that 15 would account for half of all the publications. Yet the top decile, which consists of 23 psychologists, can claim only 37% of the total.
2. The *Lotka law* of Alfred Lotka (1926): the number of scientists who claim n publications is inversely proportional to the square of n, i.e., $f(n) = c\, n^{-2}$, where c is a constant that varies according the discipline and other factors; taking logarithms, the Lotka law becomes $\log f(n) = \log c - 2 \log n$, yielding a straight line (e.g., if $f(n)$ and n were plotted on double-log graph paper). When this line is fit to the appropriately transformed frequencies that Dennis (1954c) provided for Group I, 91% of the variance is explained.

Contradicts "Ortega hypothesis" – on the contrary

Cesare Lombroso (1891) *The Man of Genius*: "the appearance of a single great genius is more than equivalent to the birth of a hundred mediocrities" (p. 120).

Quantity and Quality

What is the minimum output necessary to become a great psychologist?

- E.g., Wayne Dennis 130 total publications,
- same as English-Canadian psychologist George Sidney Brett and Belgian psychologist Ovide Jean Decroly
- yet others more notable have less: Anna Freud, Leta Hollingworth, Pierre Janet, and Karl Lashley.
- And obscure psychologists who satisfy this same cutoff: e.g., M. E. Haggerty and J. E. W. Wallin

Hence, perhaps we should look at quality rather than quantity

- S. Cole and J. R. Cole (1973): fourfold typology
- *Silent* – low quantity and low quality
- *Prolific* – high quantity and high quality
- *Perfectionists* – low quantity and high quality
- *Mass producers* – high quantity and low quality

How to assess quality?
Answer: Citation counts using *Science Citation Index* or *Social Science Citation Index*

Criticisms of citations:

> (a) the lack of a universally accepted way to handle multiple authorship (e.g., Ashton & Oppenheim, 1978; Lindsay, 1980);
> (b) the biasing effects of the prestige of the institutions from which the publications originate (S. Cole, 1970; cf. J. A. Stewart, 1983);
> (c) the fact that many citations actually contain criticisms rather than endorsements (Moravcsik & Murugesan, 1975); and
> (d) the possibility that some scientists might "chalk up high citation counts by simply writing barely publishable papers on fashionable subjects which will then be cited as perfunctory, 'also ran' references" (Moravcsik & Murugesan, 1975, p. 91).

However, these and other problems have been shown to be insufficient to undermine the validity of citation counts (e.g., J. Cole & S. Cole, 1971; Rushton, 1984). Either these potential artifacts account for too little method variance or they prove to be too transient to affect the measures in the long term.

Given this operational definition, then the empirical literature has demonstrated most emphatically that quality and quantity are correlated, that is, both the number of citations and the number of cited publications are positive functions of the total number of publications, cited or not cited (Crandall, 1978). These relationships hold not just for psychology (e.g., Rodgers & Maranto, 1989; Simonton, 1992b) but also for all other sciences (e.g., Busse & Mansfield, 1984; Feist, 1993; J. A. Stewart, 1983). In psychology, the typical intercorrelations range between .50 and .70, meaning that between a quarter and half of the variance is shared between any two variables (Rodgers & Maranto, 1989; Simonton, 1992b). Moreover, these correlations are not contingent on the particular operational definitions used. E.g., essentially the same results obtain whether or not self-citations are included among the total citations (e.g., Helmreich et al. 1980), and alternative methods of handling multiple authorship yield pretty much the same results (J. Cole & S. Cole, 1971; Rushton, 1984).

The *equal-odds rule* (Simonton, 1997b):

- The ratio of citations to total publications (or the ratio of total cited publications to all publications) does not systematically vary according to a researcher's output (Simonton, 1985b; Platz, 1965; K. G. White & M. J. White, 1978).
- E.g., the number of citations per publication is not larger for those who are the most prolific (R. A. Davis, 1987; K. G. White & M. J. White, 1978).
- These findings are mathematically most consistent with a model that specifies the number of citations to be a positive linear function of the number of publications plus random error term that has roughly the same variance as total publications.

If anything, tendency is in the opposite direction;

- A secondary analysis of Group I of Dennis (1954c) actually found a slightly negative association between citations per publication and the total number of publications (Platz & Blakelock, 1960). The psychologists in the upper half of the productivity distribution had an average citation rate of 12%, whereas those in the lower half had a citation rate of 28%, a difference that came close to statistical significance ($p = .051$).
- Although this particular finding has not been replicated (e.g., K. G. White & M. J. White, 1978), the greatest psychologists are not necessarily Perfectionists, and might even have a leaning towards being Mass Producers (also see Feist, 1997).
- This contradicts the common view that a scientist's reputation is undermined by the production of low-quality publications (cf. S. C. Hayes, 1983).

Although quality and quantity are functionally related, the two factors have rather contrasting cross-sectional distributions. Specifically, the distributions are much more elitist for citations than for publications (Redner, 1998).

- Hence, if the Ortega hypothesis has been proven inconsistent with the publication data, it is even more flatly contradicted by the citation data (J. Cole & S. Cole, 1972).
- E.g., of 299 Australian academic psychologists studied in the 1970-1975 period, the top 10% in output accounted for 36% of the total publications, but could be credited with 60% of the total citations (K. G. White & M. J. White, 1978).
- E.g., of 196 American academic psychologists, 11% had no citations during a 3-year period, another 25% averaged 2 citations or less per year, and only 10% averaged more than 50 citations per year (Helmreich et al., 1980).
- E.g., of 48,903 psychologists who published in major English-language psychology journals from 1962-1967, more than half were cited only once, and a mere 6% were cited at least a half dozen times (Myers, 1970). Solely 18 out of the 48,903 received more than 200 citations, including such notable psychologists as Kenneth Spence (378), Neal Miller (362), Leon Festinger (298), Clark Hull (267), Edward Thorndike (241), and J. P. Guilford (201).
- Highly skewed statistics like these even emerge when the samples of psychologists are confined to those who are associated with major research institutions. E.g., of the faculty at the 100 top-rated university departments of the United States, Canada, and Great Britain, 22% received no citations in a single year, and only 3% had more than 100 citations in a 5-year period (Endler, Rushton, & Roediger, 1978).

What makes figures like these so remarkable is that the citations are aggregated across the cumulative work of any given psychologist.

- Therefore, when psychologists receive no citations in a given period, that means that they have not published a single item during the entire career up to the year of citation that at least one other scientist found worth mentioning. As academic psychologists, these uncited individuals must be publishing, but for the purposes of assessing impact they must be grouped with the Silent.
- Indeed, sociological research on the Ortega hypothesis has underlined the minimal impact of those scientists who publish only minor papers (J. Cole & S. Cole, 1972; Oromaner, 1985). Not only do influential articles disproportionately cite other influential publications, but also the unimportant publications even cite disproportionately the influential publications.
- One study of the criminology literature, for instance, found that "less important works (those with few citations) are rarely utilized by much more important papers (those with the highest citation counts)" (Green, 1981, p. 45). More than half of the research, in fact, was not cited at all! That represents a huge quantity of silent research.

One final aspect of the quantity-quality association must be noted. Although the correlation is very high, it is far from perfect. Even a correlation of .70 leaves room for considerable scatter around the bivariate regression line.

- As a consequence, nothing prevents an investigator from creating artificial groups of Silent, Prolific, Perfectionist, and Mass Producers from any given data set. All the researcher has to do is to split the productivity and citation counts at their respective medians. The result would be four groups defined by low productivity and low citations (the Silent), high productivity and high citations (the Prolific), low productivity and high citations (the Perfectionists), and the high productivity and low citations (the Mass Producers).
- Even so, whenever a researcher performs this methodological legerdemain on real data, the Prolific and Silent outnumber the Perfectionists and Mass Producers by well over 2 to 1 (see, e.g., J. Cole & S. Cole, 1972; Feist, 1997; Helmreich, Spence, & Thorbecke, 1981).
- Moreover, it is evident that the errors around the regression line are such that the Perfectionists and Mass Producers do not form taxonomically distinct groups (e.g., both lack identifiable clusters of outliers).

Accordingly, the safest scientific conclusion remains that quality is a positive linear function of quantity, but that the association is not so strong to rule out an occasional Perfectionist or Mass Producer.

Curiously, the preceding methodological tactic does suggest a potential operational definition of the great psychologist. Perhaps the minimum requirement for being called "great" is that the psychologist be in the upper half of the distribution with respect to both publications and citations. In other words, all Prolific psychologists make the first cut.

I express this as a necessary but not sufficient criterion because individuals who are above the median in both quantity and quality will constitute as much as a third of the total (J. Cole & S. Cole, 1972; Feist, 1997). For most tastes, that percentage might appear a bit on the liberal side. So it is very likely some additional criteria would have to be imposed.

Longitudinal Stability

Problem:

Publication and citation counts are often based on a thin slice of a scientist's career.

Hence why can't Silent become Prolific and visa-versa from one period to next?

Not likely for two reasons:

First, citation rates are extremely stable over time. This stability holds for all scientific disciplines, including psychology (e.g., Bonzi, 1992; Rushton, 1984).

- E.g., a sample of 82 personality and social psychologists who had received their PhD's in 1960 or earlier were scrutinized in three years five years apart, namely 1965, 1970, and 1975 (Helmreich, Spence, & Thorbecke, 1981). The 1965 citation rate correlated .46 and .42 with the rates in 1970 and 1975, respectively, and the citations received in latter two years correlated .88. The latter figure is more indicative of the temporal stability of citations, for by then the psychologists were at least a decade into their careers.
- In appraising these "test-retest reliabilities" it is essential to realize that citations counted within years will be less reliable than those counted within 5-year periods (Allison, 1977). E.g., a study of 69 eminent American psychologists (who were deceased as of 1969) counted the citations they received in the 5-year cumulative indexes for 1971-1975 and 1981-1985 (Simonton, 1992b). The correlation between the two counts was .94.

Second, publication rates also display considerable temporal stability, albeit not usually so much as citations (Blackburn, Behymer, & Hall, 1978; Bonzi, 1992; S. Cole, 1979).

- This stability has been demonstrated in several samples of psychologists (e.g., Over, 1982a, 1982b; Rushton, 1990).
- First demonstration was conducted by Wayne Dennis (1954b) for two groups.
    - Group I examined 43 psychologists born between 1850 and 1860 who had their publications listed in Murchison's (1932) *Psychological Register*. The sample included such notables as Havelock Ellis with 240 publications, Sigmund Freud with 201, and Hendrick Zwaardmaker with 162. Of course, there were many more psychologists who were much less productive (up to age 70, for this cohort). Dennis tabulated the output for each psychologist in five age periods (i.e., the 20s, 30s, 40s, 50s, and 60s), and then calculated the productivity correlations between consecutive and non-consecutive decades. From the 30s to the 60s adjacent correlation coefficients ranged from .71 to .82. Although the decade of the 20s had somewhat lower correlations with the other decades, the productivity during this period still correlated .58 with output in the 30s and .53 with output in the 60s (also see Horner, Rushton, & Vernon, 1986; Rodger & Maranto, 1989).
    - Group II of the Dennis study took a sample of 41 members of the National Academy of Sciences (excluding all psychologists), and obtained the same results. Again excluding the decade of the 20s, when most careers are still getting off the ground, adjacent decades were correlated between .79 and .86. Even productivity in that first decade was a reasonably good predictor of later output (also see Christiansen & Jacomb, 1992; Chubin, Porter, & Broeckmann, 1981).

To assert that citation and publication counts both exhibit temporal stability is not equivalent to asserting that these measures of output and impact just inexorably increase from year to year.

E.g., the cumulative output of some academic psychologists is affected after receiving tenure, productivity leveling off after having increased since the onset of the career (Bridgwater, Walsh, & Walkenbach, 1982). Yet this effect does not change the relative ranking of various psychologists. On the contrary, because the plateau does not appear for those psychologists who hold positions at high prestige institutions, the distance separating them from the less productive members of their cohort will actually increase (also see Blackburn, Behymer, & Hall, 1978). Hence, it is the relative standing of a psychologist within a given cohort that stays fairly stable, not the rate of output within each psychologist.

Contribution Type

If the great psychologists are those who publish many influential works, how does their output and impact compare with the acknowledged greats of the most successful scientific disciplines?

- The three illustrious mathematicians Augustin Cauchy, Leonhard Euler, and Arthur Cayley claimed totals of 789, 856, and 995, respectively, while the eminent chemist Wilhelm Ostwald accumulated an awesome bibliography of 5,545 publications (Bringmann & Balk, 1983).
- But disciplines differ in the level of output expected of their researchers. E.g., one survey of 27,000 faculty in American higher education found substantial variation in the current publication activities (Fulton & Trow, 1974). In the biological sciences, 84% were active researchers, a figure that declined to 75% for the social sciences, and 60% for education.
- Even within the social sciences substantial differences can appear, as is evident in the low scholarly productivity of those who serve on the editorial boards of social work journals (Duncan, 1976; Pardeck, Chung, & Murphy, 1995).
- And within psychology the clinical and counseling psychologists are noticeably less productive than are research psychologists (Brems, Johnson, & Gallucci, 1996).

In part, these contrasts may ensue from distinct norms operating in various scientific disciplines. But another part likely arises from the different types of publications that are likely to be made in various disciplines and subdisciplines.

Moreover, different types of contributions should differ not only according to the magnitude of effort that researchers must invest in their production, but also the contributions should vary according to the impact they are likely to have on the field. E.g., while it is true that E. G. Boring's total output slightly exceeded that of Wundt, a different picture emerges if books and articles are excluded from both counts.

- Boring's output includes 45 book reviews and 202 editorials (Zusne, 1984), whereas
- Wundt's includes 197 book reviews and 28 miscellaneous publications (Bringmann & Balk, 1983).

Because it is likely that editorials have less impact on the field than do book reviews, and all of these publications have less impact than articles and books, the edge would seem to reside with Wundt.

At the other extreme, what about the influence of books?

- A study of 69 eminent American psychologists has actually found some evidence for superiority (Simonton, 1992b). For each psychologist was calculated the proportion of his or her output that was represented by books rather than journal articles. This measure was then correlated with the number of cited publications, the total number of citations, and the number of citations to his or her single most cited work (using a 5-year accumulation for 1971-1975). In all three cases the correlations were statistically and substantively significant (i.e., about 10% of the variance was explained). Furthermore, although books only accounted for 17% of all the publications, books represented 45% of those works that received the most citations. Examples of such high-impact book-length contributions include Gordon Allport's *The Nature of Prejudice* (with 198 citations) and Carl Hovland's *Communication and Persuasion* (with 135 citations).
- Corroborative results are found in an earlier study that used a different methodology (Heyduk & Fenigstein, 1984). Eminent psychologists were asked to identify those "texts or articles … which have significantly influenced your work and though, both past and present, in your major area of psychology" (p. 556). As many as 10 works could be so identified by each survey respondent. Sigmund Freud's contributions came out on top, with such works as *The Psychopathology of Everyday Life*, *A General Introduction to Psychoanalysis*, *The Interpretation of Dreams*, and *Introductory Lectures on Psychoanalysis* leaving a mark on many great psychologists after him. Yet few articles were mentioned. And in every case but one, when a scientific paper was deemed influential a book or monograph by the same author proved even more so. E.g., Clark Hull's articles on "A Functional Interpretation of the Conditioned Reflex" and "The Goal Gradient Hypothesis and Maze Learning" were each mentioned twice, but his book on the *Principles of Behavior* was mentioned 7 times. Only one author out of the 39 most influential psychologists had more impact through an article rather than a book: Lee Cronbach, in his classic 1957 paper on "The Two Disciplines of Scientific Psychology." That means that fewer than 3% of these eminent contributors staked their fame on an article rather than a book. Furthermore, 92% of the works that influenced eminent psychologists were books or monographs, leaving only 8% to be credited to articles.

The above results suggest that to become great, psychologists should choose their projects wisely. Publishing exclusively articles, even if in the best journals, is not the optimal strategy. Every so often psychologists should consolidate their ideas in more comprehensive syntheses. This implies that the Prolific contributors tend to strike a delicate compromise between the Mass Producers who churn out paper after paper and the Perfectionists who concentrate on a magnum opus or two.

Long-Term Influence

Criticism of citation measures: distinct tendency for methodological works to be cited more than theoretical or empirical contributions (e.g., Folly et al., 1981; Peritz, 1983).

- Myers (1970) identified the most frequently cited authors were identified according to the references to their work over a 6-year period in over a dozen prestigious journals, including *Psychological Review*, *Journal of Experimental Psychology, British Journal of Psychology*, *Canadian Journal of Psychology*, *Journal of Personality and Social Psychology*, and *Journal of Abnormal Psychology*. The 99th percentile in citation counts included W. K. Estes, L. Festinger, H. F. Harlow, C. I. Hovland, C. L. Hull, J. Piaget, B. F. Skinner, and E. L. Thorndike. Yet this group also included A. L. Edwards, S. Siegel, E. F. Linquist, and B. J. Winer. Where the obvious luminaries claimed between 166 and 298 citations each, the non-obvious citation celebrities could boast between 224 and 377 citations each!
- A similar outcome appeared when researchers identified the 100 most-cited psychologists in the 1975 *Social Science Citation Index* (Endler, Rushton, & Roediger, 1978). The top 10 contained S. Freud, J. Piaget, A. Bandura, H. J. Eysenck, D. T. Campbell, E. Goffman, B. F. Skinner, and E. H. Erikson, as well as B. J. Winer and S. Siegel, the first in third place (after Freud and Piaget) and the second in tenth!

Yet this raises the issue of what happens to citations in the long term. It is one thing to show that citations tend to be relatively stable during the course of a psychologist's career, quite another to prove that the same stability holds after the career has ceased. Although the final count of cumulative output will seldom change after a scientist dies – except occasional posthumous publications – the influence that output has on the discipline might change in either direction. In some cases the scientist's work might have proven merely fashionable, and sink rapidly into oblivion. In other cases the life work might behave like a "sleeper effect," and gradually increase in impact. The more typical pattern, however, is probably somewhere between. Citation to the deceased author's work gradually decays over time, but never disappears altogether (Simonton, 1984g, 1992b). The first big drop likely occurs some time after the scientist dies, as the social obligation to cite an old mentor or colleague becomes less potent (Trimble, 1986). But after that, citation rates will continue decline either because the work becomes manifestly obsolete or because the research becomes fully incorporated into the "common knowledge" of the discipline (e.g., Abt, 1983; Barnett, Fink, & Debus, 1989; MacRae, 1969; Price, 1965). Nevertheless, if the contribution is truly one that survives the "tests of time," the citations will not reduce to zero.

An excellent illustration of this process occurring among the great psychologists is what happened to F. C. Donders (see Goodman, 1971). This Dutch ophthalmologist first introduced mental reaction times as a technique in "mental chronometry" back in 1865 (in Dutch) and 1868 (in German). Even after his method became assimilated as an integral part of experimental psychology, he continued to receive some degree of recognition. To be sure, mental reaction times are now most commonly used without any explicit reference to Donders, but from time to time various investigators made explicit psychology's methodological indebtedness to his pioneering work. These acknowledgments have come from psychologists eminent in their own right, such as James McKeen Cattell, Robert S. Woodworth, E. G. Boring, Donald E. Broadbent, Michael I. Posner, and Saul Sternberg. Donders still survives today. According to the *Social Sciences Citation Index Five-Year Cumulation 1981-1985* (1987), Donders received around 80 citations, about 70 of which can be credited to his reaction-time methods. To put these figures in context, of 783,339 papers published in scientific journals in 1981, 81% were cited 10 times or less, and 47% were not cited at all between 1981 and June 1997 (Redner, 1998). So Donders produced what can easily be considered "citation classics" (Goodman, 1971).

Nonetheless, the above example does urge caution in using citations as an index of impact. Because the probability of citation still tends to decay over time, a psychologist's contemporary influence must always be adjusted for his or her cohort (e.g., either birth year or year of highest degree).

EMINENCE

Citation indicators a recent development, and not the only route to assessing greatness.

An alternative: Galton (1869), who defined genius in terms of reputation, especially as posthumously revised by posterity.

- One method for implementing this definition was discussed in chapter 1 – when Annin, E. G. Boring, and R. I. Watson (1968) asked experts to identify the "most important psychologists" in the history of the field.
- Another option is to assess how much space is devoted to each candidate in standard histories of the discipline. Thus, one study examined the average percentage of space devoted to 570 deceased contributors according to 16 texts (Zusne & Dailey, 1982; cf. Zusne, 1987a). The texts included those by the Americans E. G. Boring and R. I. Watson, the British R. S. Peters and Robert Thomson, the German Wilhelm Hehlmann, and the Russian M. G. Yaroshevskii. Just eight notables could claim that they averaged at least 1% of the space in these texts: Sigmund Freud 3.23%, Wundt 2.46%, William James 1.76%, John Watson 1.46%, Descartes 1.35%, Fechner 1.13%, Hume 1.04%, and Locke 1.03%.
- Another option is simply to take those who have earned significant professional honors, such as those who received APA's Distinguished Scientific Contribution Award (e.g., Myers, 1970; Simonton, 1985b) or those who were elected to the APA Presidency (e.g., Gibson, 1972; Suedfeld, 1985). Unlike the previous method, which relies on the judgments of posterity, this operational definition involves the direct assessments of a psychologist's contemporaries, whether an award selection committee or the vote of members of a professional organization.

Below we shall examine more closely the implications of defining a psychologist's greatness in terms of such eminence measures. In particular, I will discuss four questions:

- Does a consensus exist on the relative distinction of those psychologists who have some claim to fame in the annals of our discipline?
- What is the cross-sectional distribution of eminence?
- How does eminence correlate with lifetime output?
- What is the transhistorical stability of eminence assessments?

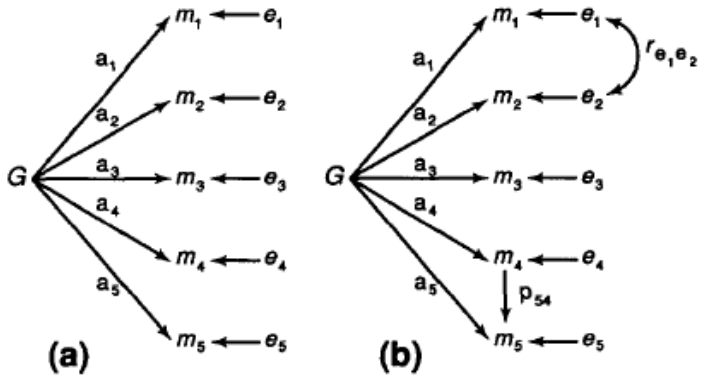
Galton's G*: The Greatness Consensus*

Skepticism:

- "Worldly renown is naught but a breath of wind, which now comes this way and now comes that, and changes name because it changes quarter," said Dante (c. 1307/1952, p. 69).
- Many psychologists have echoed this cynicism with respect to the so-called greats (e.g., Korn, R. Davis, & S. F. Davis, 1991; Ray, 1971).
- Some empirical investigations purport to show that a psychologist's fame is indeed very fickle. E.g., one investigator argued that surname counts derived from introductory psychology textbooks are orthogonal to those derived from history of psychology textbooks (Roeckelein, 1996b). The intercorrelations only ranged between .19 to .51, with a median around .30. Yet this gauge of agreement is misleading for two reasons.
  1. History and introductory texts focus on very different time periods in the development of the discipline, a factor that has been shown to contaminate indicators of eminence (Simonton, 1984g).
  2. These correlations pertain to only a select group of highly eminent psychologists: A. Adler, A. Binet, W. Cannon, H. Ebbinghaus, G. Fechner, S. Freud, F. Galton, H. Helmholtz, W. James, C. Jung, W. Köhler, J. Müller, I. Pavlov, E. L. Thorndike, E. G. Titchener, J. B. Watson, and W. Wundt. That's an *N* of only 17, the subjects spanning only the upper end of the eminence distribution. With such a draconian truncation of the variance, these coefficients must seriously understate the degree of consensus.

Yet, in fact, several studies have shown that a considerable agreement exists on the differential eminence of historical figures (e.g., Farnsworth, 1969; Kynerd, 1971; Simonton, 1983c, 1986a).

- This consensus even transcends various civilizations, nations, and subcultures (Simonton, 1984a, 1984g). E.g., the differential distinction accorded major figures in African-American history differs very little across majority (White) and minority (Black) cultures (Simonton, 1998a).
- Similarly, the relative eminence of contributors to Japanese civilization is substantially the same in East and West (Simonton, 1996b).
- Furthermore, the eminence consensus transcends alternative measurement methods, whether page counts in encyclopedias or histories, line counts in biographical dictionaries, frequency of inclusion in anthologies or collections, and so forth (Simonton, 1976f, 1977b, 1984a, 1987d).
- Linear composites of alternative indicators, no matter how diverse, will usually boast internal-consistency reliabilities (coefficient alphas) in the upper .80s and lower .90s (e.g., Simonton, 1984g, 1990d).
- Indeed, confirmatory factor analyses of data sets drawn from several distinct domains of achievement have shown that all alternative indicators can be adequately explained by a single latent variable that represents individual differences in attained distinction (Simonton, 1991c). So pervasive is this underlying factor that it has been christened "Galton's *G*" (Simonton, 1991c). This term makes obvious reference to the similarly ubiquitous "Spearman's *g*," the latent variable underlying performance on various intelligence tests (Spearman, 1927). This analogy reflects the fact that the two best established operational definitions of genius entail achieved eminence and exceptional intelligence (Cox, 1926; Galton, 1869; Hollingworth, 1926; Terman, 1925).


G
a1
a2
a3
a4
a5
m1
m2
m3
m4
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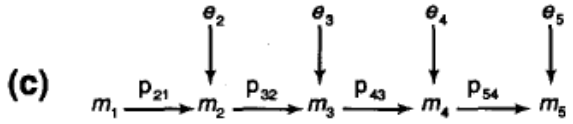
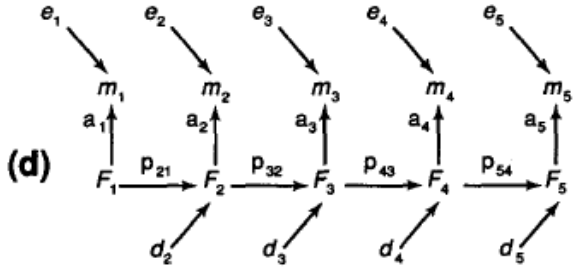
e1
e2
e3
e4
e5
(a)
(b)
r e1e2
p54


(c)
m1
m2
m3
m4
m5
e2
e3
e4
e5
p21
p32
p43
p54


(d)
e1
e2
e3
e4
e5
m1
m2
m3
m4
m5
a1
a2
a3
a4
a5
F1
F2
F3
F4
F5
p21
p32
p43
p54
d2
d3
d4
d5

Unfortunately, these powerful latent-variable modeling techniques have not yet been applied to multiple indicators of eminence in psychology. Even so, there is ample reason for believing that Galton's *G* would reappear in any sufficiently heterogeneous sample of psychologists.

- The broad agreement on eminence holds for extremely diverse groups of historical figures, including 342 European monarchs (Simonton, 1984f), 39 Presidents of the United States (Simonton, 1986f), 696 classical composers (Simonton, 1977b), and 772 visual artists (Simonton, 1984a).
- The consensus even emerges with respect to the differential eminence of two groups that include many figures in psychology's history, namely, the 2,012 philosophers (Simonton, 1976f) and 2,026 scientists (Simonton, 1991a) who populate the intellectual tradition of Western civilization.
- Even more significantly, inquiries into the differential eminence of psychologists find strong indirect evidence for the existence of a single-factor underlying their distinction. This indirect confirmation is most conspicuous in the differential distinction of 69 American psychologists active between 1879 and 1967 (Simonton, 1992b).
    - E.g., the expert ratings of Annin, E. G. Boring, and R. I. Watson (1968) correlated .85 with an index of the number of textbooks that discussed the psychologist's contributions (from Zusne & Dailey, 1982).
    - Moreover, a linear composite of these two measures plus a calculation of the amount of space devoted to each psychologist in those same textbooks (again from Zusne & Dailey, 1982) yielded an internal-consistency reliability (coefficient alpha) of .89.
    - In addition, this three-item composite correlated between .75 and .81 with three alternative space measures derived from Ernest R. Hilgard's (1987) more specialized text on the history of psychology in the United States.
    - The composite measure also correlated .62 with whether or not the sampled psychologist had been elected APA President. This correlation between contemporary and posthumous is quite respectable given that it entails the relation between a continuous and a dichotomous variable, and thus it is necessarily attenuated.
    - It is also noteworthy that the 3-item composite correlates positively with more general indicators of eminence, such as having an entry in the *World Who's Who in Science* (Debus, 1968) and *Webster's Biographical Dictionary* (1976). The two point-biserial correlations are .44 and .41, respectively, which are respectable given the highly selective nature of these two reference works. Thus, those who have outstanding reputations within the discipline also tend to have exceptional reputations in science as a whole as well as in the larger world. Curiously, the only conspicuous exception to this association was behaviorist Clark Hull who, despite his obvious fame as psychologist, managed not to win an entry in *Webster's*. Although Hull's influence was clearly on the wane in 1976 (Webster & Coleman, 1992), his omission must be certainly considered either an evaluative error or an editorial oversight.

  It is essential to point out that the above 69 psychologists still represented a fairly select group. The four who received the highest score on the 3-item composite were J. B. Watson, Titchener, Hull, and Tolman, whereas the four who received the lowest score were Ferree, Karwoski, Kuhlmann, and H. Seashore (but not C. Seashore, who was also among the 69). These four enjoyed sufficient renown to make it into the final 538 identified as "important psychologists" by Annin, E. G. Boring, and R. I. Watson (1968). Certainly the sample is far more selective than most investigations into Galton's *G*, which number into the hundreds and even thousands of clear celebrities and near nonentities (Simonton, 1991c). Accordingly, the consensus revealed by the reliability and correlation coefficients most likely understates the true magnitude of agreement for the discipline as a whole.

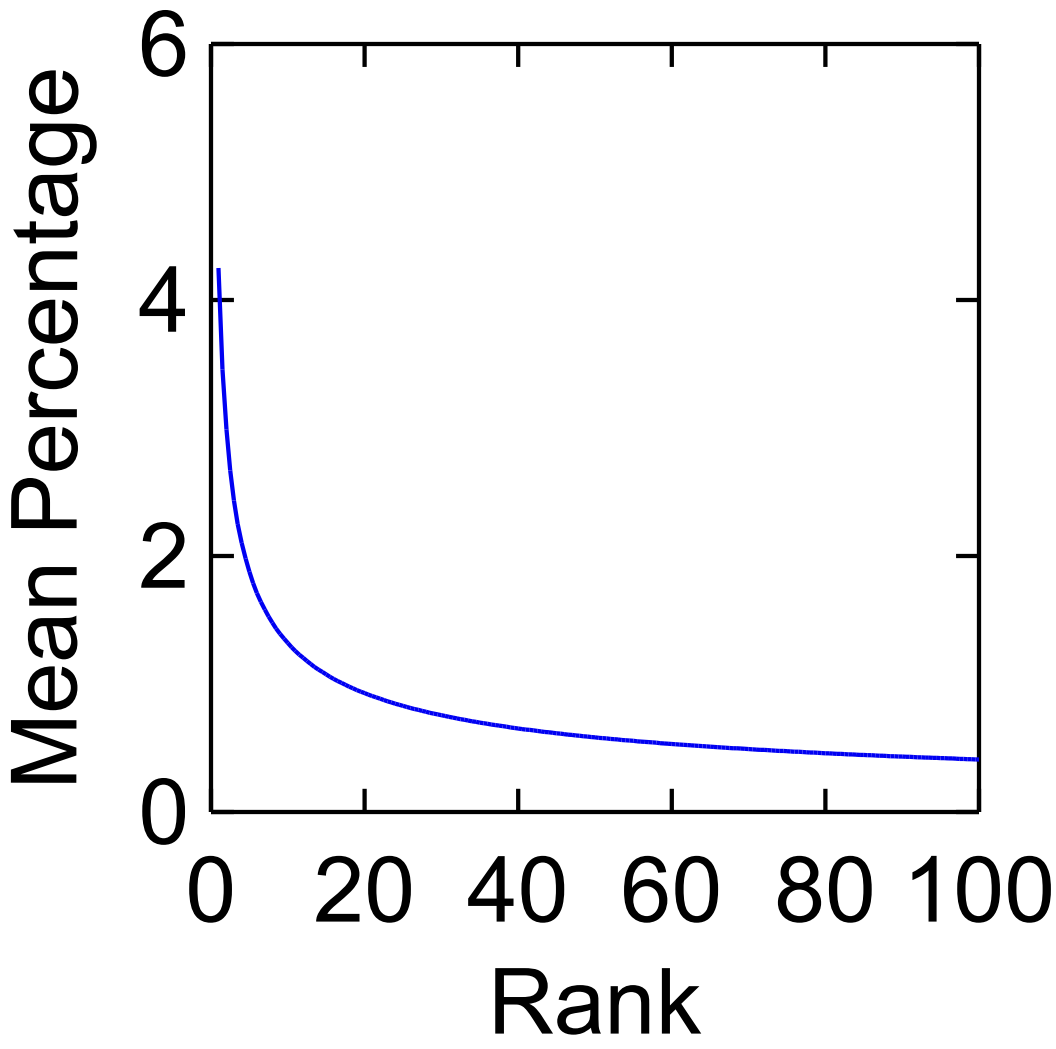
Variation and Distribution

- As discussed earlier in this chapter, the cross-sectional distribution of total lifetime output is extremely skewed, a minority of the contributors in any field deserving credit for the lion's share of the contributions.
- Also noted was the fact that the cross-sectional distribution of actual influence is even more elitist. E.g., the distribution of citations to scientific publications is far more skewed than the distribution of the scientific output subject to citation. It was for this latter reason that the Ortega hypothesis was so resoundingly disconfirmed.
- Hence, it should come as no real surprise that eminence also displays a highly skewed distribution, the most elitist by far. This hegemony of the cream of the cream holds for both the arts and the sciences. E.g., of 34,516 books written about 602 British poets, 9,118 are about Shakespeare, 1,280 about Milton, and 1,096 about Chaucer, the top 25 poets accounting for almost two-thirds of the books, and the top 12 for almost exactly half (Martindale, 1995).

Psychology is no exception to the overall pattern, a handful of psychologists striding like colossi over their less illustrious colleagues.

- This dominance is immediately apparent in how much space is devoted to various eminent psychologists in history of psychology textbooks. Consider the earlier study of the coverage of 570 deceased figures in 16 texts (Zusne & Dailey, 1982). The eight top contributors who accounted for at least 1% of the space each represent only 1.4% of the total contributors, and yet they are collectively receive over 13% of the coverage. Add another 10 contributors, and the proportion increases to about 22%.
- A follow-up investigation demonstrated the extreme nature of this distribution far more dramatically (Zusne, 1985). Again 16 history of psychology textbooks were used, but the sample of eminent psychologists included 697 deceased contributors plus B. F. Skinner (who was already past 80 and his place in history assured). Just 25 individuals, or 3.6%, account for half of the total number of pages in these texts. In rank order, these luminaries were S. Freud, Aristotle, Wundt, James, J. B. Watson, Plato, Descartes, Fechner, Skinner, Hume, Locke, Titchener, Kant, Helmholtz, Lewin, McDougall, Pavlov, Berkeley, Galton, Jung, Saint Augustine, Darwin, Herbart, Leibniz, and Hull. If the average percentage of space (Y) is plotted as a function of the contributor's rank (X), it is possible to fit a hyperbola according to the equation $Y = 0.0001 + 4.251X^{-0.508}$ (Zusne, 1985). Figure 3.1 shows what this curve looks like for the first 100 eminent contributors. Needless to say, adding the remaining 598 individuals would serve only to lengthen the right-hand tail all the more, as it asymptotically approaches the 0% level.
- Significantly, this hyperbolic function is not confined to history of psychology textbooks, for the same curve appears in introductory psychology texts, even when drawn from different periods (Roeckelein, 1996b; also see Coleman, 1991).

Hence, whether the measure concerns historical or contemporary fame, these eminent among psychology's eminent constitute an exalted elite.


Mean Percentage
6
4
2
0
0
20
40
60
80
100
Rank

Correlation with Lifetime Output

Yet the very extraordinary placement of psychology's greats, as so graphically depicted in Figure 3.1, may lead one to wonder about the wisdom of using reputation, contemporary or posthumous, as an indicator of a psychologist's true accomplishment.

"Reputation is an idle and most false imposition," warns Iago in Shakespeare's *Othello*, for it is "oft got without merit, and lost without deserving" (quoted in Evans, 1974, p. 1218).

If true, this would seem to invalidate Galton's (1869) attempt to ground genius in "the opinion of contemporaries, revised by posterity." Furthermore, psychology's own history is full of cases that may provide concrete evidence for Iago's assertion.

- With respect to "lost without deserving,"
    - scholars will sometimes lament that some relatively obscure figure is vastly underrated. This lamentation is most likely to be heard with respect to potential sexism, racism, or ethnocentrism in historical judgments (e.g., Korn, R. Davis, & S. F. Davis, 1991), leading various scholars to attempt to rectify what is perceived as a social injustice (e.g., O'Connell & Russo, 1990).
    - Yet, curiously, sometimes majority-culture males can appear the victims of posthumous neglect as well. A case in point is E. B. Twitmyer, whose independent discovery of the conditioned reflex did not earn him the same recognition in our discipline as I. Pavlov received (Coon, 1982). This differential is manifest in their opposed eponymic status: We commonly speak of Pavlovian conditioning, but never of Twitmyerian conditioning.
- With respect to "oft got without merit," sometimes scholars express wonder about the elevated placement of William James in psychology's pantheon. "Everyone acknowledged his greatness, yet it is difficult to point to specific achievements in psychology as the basis of his reputation," said one historian (Thomson, 1968, p. 125). "There is much that is paradoxical about William James and his role in American psychology," reads another history text (Schultz & Schultz, 1992, p. 173). "On the one hand, he was certainly the leading American precursor of functional psychology. He was the pioneer of the new scientific psychology in the United States and its senior psychologist, and he is still considered by many to be the greatest American psychologist who ever lived. On the other hand, James at times denied that he was a psychologist or that there was a new psychology. … He founded no formal system of psychology and had no disciples." Such remarks suggest that James' reputation is out of keeping with his genuine achievements.

Fortunately, an impressive accumulation of research proves quite conclusively that eminence is not very capriciously bestowed (Simonton, 1994a).

- On the contrary, for both the arts and the sciences, differential reputation is a conspicuous function of individual differences quantity and quality of lifetime productivity (Albert, 1975).
- Moreover, this linkage holds for contemporary acclaim, such as major honors and awards (Ashton & Oppenheim, 1978; S. Cole & J. R. Cole, 1973), as well as posthumous recognition, such as inclusion in biographical dictionaries, encyclopedias, and histories (Raskin, 1936; Simonton, 1977b, 1991b).

These associations have been most securely demonstrated in the sciences, since a considerable amount of metascientific research has been devoted to this very question (e.g., S. Cole & J. R. Cole, 1973; Feist, 1993; Simonton, 1991a).

- In fact, an exemplary study was conducted by developmental psychologist Wayne Dennis (1954a). He began with a random sample of 19th-century scientists with bibliographies in the *Catalog of Scientific Literature, 1800-1900*. These bibliographies were restricted to items that could be considered genuine scientific publications, namely articles published in professional journals. The 208 sampled individuals exhibited a fairly typical range of output, from 1 to 458 publications (cf. Bringmann & Balk, 1983; Feist, 1997). Also, as should be expected, the cross-sectional distribution of output was highly skewed: 50% could claim fewer than 7 publications, and 30% could boast just a single publication each. Dennis then checked to see who among these 208 attained sufficient distinction to earn a biographical entry in the *Encyclopedia Britannica*, a degree of eminence rarely achieved. Of those whose publication record placed them in the top decile, 9 out of 21, or nearly a half, received that honor. In contrast, only 6 of the remaining 187 attained that exclusive level of general recognition. Although Dennis did not do so, it is easy to convert these statistics into a measure of correlation (using the Phi coefficient; Simonton, 1984d). The result is .46, a fairly impressive figure given the elite nature of the eminence criterion. In addition, the advantage held by the highly productive persists even in the higher levels of the distribution. The top 10% with 50-50 odds of inclusion in a prestigious encyclopedia had produced 50 or more articles. But of those in the top 5% in output, who published more than 140 scientific papers, the percentage so honored increased to 70%. The connection between contribution and reputation is indubitable.
- Studies that specifically focus on psychologists find comparable results. Certainly the number of citations received in the professional literature correlates with peer ratings of eminence (Clark, 1957; Simonton, 1992b), election as APA President (Myers, 1970; Simonton, 1992b), having a biographical entry in *American Men of Science* (Myers, 1970), and receiving such honors as APA's Distinguished Scientific Contribution Award and the US National Medal of Science (Myers, 1970). The typical correlations range in the .50s and .60s, suggesting that between a quarter and a third of the variance is shared. Even among those who were already among psychology's elite, eminence was tied to output. APA Presidents who published more frequently are more prone to receive citations in 37 wide-used introductory psychology textbooks (Suedfeld, 1985).

Although the positive association between productivity and eminence is firmly established, one substantive issue remains unresolved: Which is more crucial to a psychologist's ultimate eminence, quantity or quality? One plausible causal model is depicted by a simple causal chain (cf. Dennis, 1954a):

QUANTITY (publications) → QUALITY (citations) → EMINENCE (reputation)

Assuming that the first two variables can be assessed with equal reliabilities, two necessary predictions follow: (a) quality should correlate more highly with eminence than does quantity and (b) quantity should have no independent predictive value with respect to eminence once the influence of quality is accounted for (Simonton, 1997b). Are these predictions justified by the data? The research on both scientists in general and psychologists in particular answer "no."

- In the former case, one study looked at the odds that scientists would be elected member of the National Academy of Sciences (Feist, 1997). On the basis of their publication lists and citation rates, the scientists had been classified into the Silent, Perfectionist, Mass Producer, and Prolific categories discussed earlier. Only 3% of the Silent and 14% of the Perfectionists received that honor. It may seem surprising to learn that the Silent had such a good chance to attain that status, but all of the scientists in the sample had had attained the rank of full professor at major US research universities, and hence they already represented a fairly select group. More fascinating, therefore, are the odds for the remaining two categories of achievement: 55% of the Prolific and 63% of the Mass Producers had been elected NAS! So quantity has an edge over quality. The same result was found for a more global measure of scientific eminence. In addition, contrary to the prediction of the simple causal-chain model, both quantity and quality were necessary for a complete prediction of differential eminence. Together the two indicators of output explained about half of the variance in eminence, with quantity explaining more than quality. It was certainly the case that quantity still predicts eminence even after quantity is statistically controlled.
- Comparable results appeared in a study of 69 illustrious American psychologists (Simonton, 1992b). In this instance there were two measures of output and two measures of eminence. Output was gauged by the total number of works cited in the professional journals (quantity) and by the total number of citations all of those works obtained (quality). Eminence was gauged by election to the APA presidency and by posthumous reputation, the latter using a 3-item composite measure. Control was introduced for cohort effects (i.e., birth year). The first, contemporary eminence measure correlated .48 with total cited works and .49 with total citations, pretty much a tie. The second, posthumous measure correlated .73 with total cited works and .66 with total citations, giving quantity a slight edge over quality. However, when the predictors were placed in a multiple regression equation, somewhat different results emerged. Election to the APA presidency was a function of total citations ($\beta = .44$) but not total cited works, putting quality ahead of quantity. In contrast, for posthumous reputation total works cited ($\beta = .34$) and total citations ($\beta = .32$) came in neck and neck as predictors. These results suggest that the causal-chain model is oversimplified. The model explains election to the APA presidency fairly well, but woefully misses in the case of posthumous reputation. Even so, the fact remains that eminence is prominently influenced by the quantity and quality, either singly or in some as yet undefined cooperation.

The last conclusion might lead us to suppose that eminence is not whimsical. Both contemporary and posthumous reputation, after all, can be grounded in productive output and impact. A slight qualification intrudes, however. Even with both quantity and quality as predictors, a considerable amount of cross-sectional variation in eminence stays unaccounted for. Moreover, the departures from prediction are most likely to reside in the upper end of the distribution, where the variation in eminence is largest. Therefore, to the extent that these statistical outliers exist, it could be argued that they represent a certain amount of capriciousness in contemporaneous and posthumous assessments. It is probably best to conclude with Thomas Carlyle that "fame, we may understand, is no sure test of merit, but only a probability of such."

Transhistorical Stability

Even if eminence is ultimately grounded in a psychologist's actual contributions, association is not static but rather dynamic. It often takes time for the psychologist's cumulative record to exert its effects on his or her colleagues. The impact often begins in a narrow circle of colleagues, expands slowly to national scope, and eventually, for the greatest psychologists, attains international prominence. Once that assimilation process is complete, the dynamic relation between contribution and reputation continues, but on a different level. The question then becomes whether the work will prove truly enduring or merely fashionable. Two interpretations:

1. An enforced continuity could result if the original assessments were contained in an especially well-written history. E.g., E. G. Boring's *A History of Experimental Psychology* has greatly influenced generations of subsequent historians since its first edition appeared in 1927. An analogous process has been argued to occur in the case of literary criticism, the evaluations of one generation of critics shaping the evaluations of the next (Rosengren, 1985). Any transhistorical stability then merely reflects these intergenerational borrowings of critical opinions.
2. If Galton's (1869) conception of genius is correct, the stability must ultimately rest on the individual's powerful contributions to a domain. Each generation would have to encounter these surviving works and on that basis make there own independent assessments. If the generation concurs with the earlier judgment, then the reputation will continue, but if it does not, then the individual will have undergone a reassessment. If a person's reputation is truly founded on a body of contributions that have genuine long-term merit, eminence will display a corresponding amount of transhistorical stability.

Which interpretation is most correct? It just so happens that these alternative accounts are empirically distinguishable (Simonton, 1991c).

- The first implies that the assessments across consecutive generations will be best described by an autoregressive model. A distinguishing feature of such a model is that the farther apart are two generations, the lower their magnitude of agreement. In fact, the consensus will necessarily decay over time, asymptotically approaching the point where the later generations will not agree at all with the earlier generations (Simonton, 1998a).
- The second, Galtonian interpretation, in contrast, maintains that all evaluations are a function of a single latent variable. This underlying factor reflects the intrinsic worth of a contributor's cumulative work. As a consequence, there will be no consistent tendency for the consensus to decay over time. To be sure, an individual's eminence may decline, as new findings and theories supercede his or her contributions, or as newcomers enter the competition for the attention of posterity. The old must often yield to the new. Still, on the average, a person's status relative to his or her cohort will remain more or less stable over successive assessments (cf. Farnsworth, 1969; Rosengren, 1985).

So far, what little data we possess on this subject indicates that transhistorical eminence ratings are best described by the single-factor model (Simonton, 1991c). Using the advanced techniques of covariance-structure modeling, consecutive evaluations have been conclusively shown not to exhibit the correlational pattern expected of the autoregressive model (e.g., simplex or quasi-simplex matrices). Instead, in every domain of achievement examined, the observed intercorrelations can be easily explicated by a single latent variable on which all eminence measures display respectable factor loadings. Very rarely must these Galtonian models undergo modification to obtain maximal fit to the intercorrelations, and in every instance these changes involve nothing more than accommodations for method artifacts (e.g., so-called "difficulty factors" that concern whether a measure is exclusive or inclusive). Admittedly, these critical tests of the two rival models did not specifically look at psychologists (Simonton, 1991c). Instead, Galton's *G* was fit on data involving the differential eminence of monarchs, presidents, classical composers, visual artists, scientists, and philosophers. Although psychologists were included among some of these groups, in no case were they singled out for scrutiny. Even so, I see no obvious reason to argue that the transhistorical stability of eminence operates in a totally contrary fashion in the psychological sciences.

In addition, highly supportive findings have been reported by a study that specifically examined the long-term stability of the scientific reputation of contributors to our discipline (Over, 1982c).

- The investigation began with the eminence that 52 American psychologists had attained as of 1903, according to peer rankings solicited by James McKeen Cattell (1906). The top 10 on this list were W. James, J. M. Cattell, H. Münsterberg, G. S. Hall, J. M. Baldwin, E. B. Titchener, J. Royce, G. T. Ladd, J. Dewey, and J. Jastrow – Cattell himself coming in second at the time! The bottom of these rankings was anchored by E. F. Buchner, A. C. Armstrong, and T. L. Bolton, psychologists not sufficiently eminent to make it into the list of 528 important psychologists compiled by Annin, E. G. Boring, and R. I. Watson (1968).
- The next step was to correlate these ratings with later assessments of the impact of these 52 psychologists. The main criterion chosen was the number of citations received in the journal literature from 1966-1970 (according to the *Social Sciences Citation Index*). To make the comparison more precise and just, only those citations were counted that referred to work published prior to 1903, when the peer rankings were carried out. This was necessary given that many of those rated were extremely active after 1903, including J. M. Cattell himself!
- The correlation was .72. Hence, over half of the variance continued to be shared after more than a half-century of temporal separation. This degree of transhistorical stability is sufficient to guarantee that "there was no individual who was markedly out of favor in 1903 but markedly in favor in 1966-70, or vice versa" (Over, 1982c; also see S. F. Davis, Thomas, & Weaver, 1982).

In all likelihood, the above correlation understates the true magnitude of the temporal stability.

1. Cattell's rankings and the citation counts represent different types of measures, with distinct psychometric properties and cross-sectional distributions that should attenuate the agreement (see Simonton, 1991c).
2. Cattell's sample included several younger psychologists whose careers had practically just begun. Margaret F. Washburn was only 31, and Robert S. Woodworth 34, for example.
3. The 52 American psychologists formed a comparatively elite group: All were "starred scientists" in Cattell's (1906) *American Men of Science*, a status that elevated them above others who received biographical entries, which itself was a mark of distinction. The 52 included 25 APA presidents and 12 members of the National Academy of Sciences, and all but two (H. R. Marshall and A. C. Armstrong) were still receiving citations to their cumulative work in the late 1960s. It seems reasonable to assume that if many more psychologists were selected, obtaining thereby a more heterogeneous sample, this correlation might get larger.

Hence, until a study is conducted that specifically tests the Galtonian and autoregressive models, the most secure conclusion is probably that eminence in psychology operates in the same manner as found for other achievement domains. The transhistorical stability can be mostly credited to Galton's *G*.