Chapter 5. The Creative Product in Psychology

This treatment of output and impact concludes by switching the unit of analysis from psychologists to the products on which their reputations rest. I begin by reviewing what we have learned about highly successful research programs, and then turn to the issue of why some publications have more impact than others.

Equal-odds rule implies failures as well as successes

So what does it take to produce a success, a high-impact work?

Histories of science are replete with explanations; e.g.,

- "Much of the appeal of even Guthrie's system rests upon its simplicity and its consistency over the years. It is easy to understand, especially when compared with more complex and mathematically based learning theories, such as Hull's" (Schultz & Schultz, 1992, p. 341).
- "Freud's theories, without doubt, have had greater impact on general psychology than any other single system. This was due partly to the very nature of his subject matter but, more importantly, to the creative manner in which the system was drawn together into a logical whole" (Capretta, 1967, pp. 168-169).
- "One of the reasons the Gestalt movement gathered momentum so rapidly was that its major hypothesis proved to be immediately applicable to a great number and variety of perceptual phenomena with which psychologists were already familiar" (Lowry, 1982, p. 184).
- In contrast to Structuralism which "was such a tightly knit and prefocused system that many historians regard this as the primary reason for its eventual demise" (Hillner, 1984, p. 17).
- "Freud's system has great practical application value and popular appeal because it can resolve practically every psychological phenomenon of interest and concern to the public at large" (Hillner, 1984, p. 201).
- "Phrenology was also popular because, unlike mental philosophy, it appeared to offer practical information" (Hergenhahn, 1992, p. 221).
- "Descartes' extraordinary influence in subsequent thought was probably due to the fact that he was one of the first major philosophers since Boethius (c. 500) to write for intelligent amateurs and gentlemen" (Capretta, 1967, p. 18, fn. 7).
- The "overwhelming stature and influence" of William James has been ascribed to the fact that he "wrote with a brilliance and clarity rare in science, then as well as now. There is magnetism, spontaneity, and charm throughout his books" (Schultz & Schultz, 1987, p. 136).
- Titchener's "impact on psychology was enhanced by a writing style that was both lucid and stimulating" (Kendler, 1987, p. 53), whereas
- "Wundt's works are little read today … [because] his writing style in German produces immediate discouragement" (Hothersall, 1990, p. 106).
- Kurt Koffka's 1935 *Principles of Gestalt Psychology* "was a difficult book to read and so did not become the definitive treatment of Gestalt psychology he had intended it to be" (Schultz & Schultz, 1992, p. 383).

But what has scientific research to say about this issue? Two issues:

1. What does it take to have a successful program of scientific research?
2. What is necessary for a particular product of a research program to have exceptional impact on the field?

GENERAL RESEARCH PROGRAMS

How does one assess the attributes of a program of scientific research?

One underutilized solution is to take advantage of titles.

- With the exception of some book reviews and commentaries, a title always graces the first page of every publication, whether it be book, book chapter, journal article, or research note.
- Titles are listed in every one's curriculum vitae and various bibliographic sources.
- More critically, according to the norms of scientific publication, the titles are supposed to be highly descriptive of a publication's contents (e.g., *Publication Manual of the American Psychological Association*, 1994). In a sense, titles are designed to be abstracts of the Abstracts found in professional journal articles.

Hence, content analytical procedures can be applied to these encapsulations in order to discern the nature of a scientist's research program (Whittaker, 1989).

This very approach was attempted in an investigation of 69 eminent psychologists (Simonton, 1992b).

- Because this study was designed to commemorate the 100th anniversary of the founding of the American Psychological Association, it concentrated on a sample of American psychologists.
- The subjects were born between 1842 (G. T. Ladd) and 1912 (C. I. Hovland), and were publishing between 1879 and 1967.
- Although 10% of the subjects were born abroad, all spent most of their professional careers in the United States (e.g., E. B. Titchener).
- To ensure that the study had adequate information on which to base a content analysis, all subjects had to claim at least 20 English-language titles spread over at least 20 years.
- The titles were taken from the bibliographic entries in R. I. Watson's (1974) *Eminent Contributors to Psychology*, which also provided an essential criterion for inclusion in the sample.

All titles were placed in machine-readable form, deleting solely those words that serve purely grammatical functions, such as prepositions, conjunctions, and articles.
These text files could then be content analyzed using TEXTPACK PC (Mohler & Zuell, 1990).
The utility and precision of this program had been previously demonstrated in a content analysis of the 154 sonnets of William Shakespeare (Simonton, 1989b, 1990c).
Like those earlier analyses as well, two types of measures were defined:

1. Both *primary* and *secondary* process imagery were calculated using the Regressive Imagery Dictionary (RID) of Colin Martindale (1975, 1990). This lexicon classifies words into categories for primary content (e.g., drives, sensation, defensive symbolization, and regressive cognition) and for secondary content (e.g., abstraction, instrumental behavior, restraint, and order). Although obviously inspired by psychoanalytic theory, the dictionary purports to capture a universal contrast between "primordial" and "conceptual" information processing (cf. Epstein, 1994; Suler, 1980). RID was used to compute total scores of the number of tag words in each of the two summary categories. These two scores were then divided by the total number of words in the title samples, to correct for the variable number of titles available for each of the 69 psychologists. Given the antithetical nature of primary and secondary thought, these two transformed counts correlated -.43.
2. TEXTPACK automatically calculated, for a given unit of text, a measure known as the *type-token ratio* (TTR). The TTR consists of the ratio of types (distinct words) to tokens (total words). A high ratio means that a text is riddled with lots of different words, whereas a low ratio means that a text has many repeated words. Normally this classic measure is used to assess linguistic complexity, it acquires rather different meaning when applied to the collection of representative titles. Now a high ratio indicates that a psychologist's life work addresses a considerable range of research topics, whereas a low ratio implies that the psychologist has concentrated his or her research program on a restricted number of scientific questions. A closely related measure was also calculated, namely an index of the proportion of *unique words*, which can be adopted as an indicator of how often a psychologist explores distinct topics in a presumably one-shot fashion (i.e., *hapax legomena* in the content analytical literature). This measure correlated .55 with the type-token ratio, suggesting that these two indices gauge very similar qualities of research programs. In titles, different keywords tend to be unique keywords.

To assess the differential impact of these 69 American psychologists, the *Social Sciences Citation Index Five-Year Cumulation 1981-1985* (1987) was used.
From this source were obtained log-transformed counts of

1. the number of total citations,
2. the number of total cited publications, and
3. the number of citations to the most cited work (the middle career landmark).

These three indicators were found to correlate with two of the content analytical measures.

1. The proportion of title words devoted to primary-process imagery correlated negatively with the number of citations, the number of cited publications, and the citations received by the psychologist's most cited work (*r*s = -.37, -.34, and -.41, respectively). Perhaps these negative correlations hint why Sigmund Freud's theory has often had a rough time permeating American academic psychology, given how many of his titles are rich with primary-process tags such as sex, anal, pleasure, eroticism, hypnotism, hysteria, dreams, and unconscious. We must also recall how Clark Hull was obliged to forfeit his research interests in hypnosis after becoming a Yale professor.
2. The type-token ratio also correlates negatively with the total number of citations, the total number of cited publications, and the total number of citations to the most frequently cited work (*r*s = -.38, -.31, and -.39, respectively). Hence, the greatest psychologists among the 69 are those who have the same title descriptors cropping up again and again throughout their publication lists. These scientists are not dilettantes who fritter around from topic to topic.

As further validation of the preceding conclusion, the type-token ratio also correlates negatively with long-term eminence, as assessed by posthumous reputation (*r* = -.30, after partialling out year of birth). Hence, no matter how impact is accessed, diversity of subject matter is not highly valued as a research strategy. Rather, professional success may require a high degree of continuity in a psychologist's research program (Crane, 1965).

Moreover, there is evidence that the benefits of a highly focused research program are not confined to these 69 deceased psychologists. Another investigation looked at 99 contemporary physicists, chemists, and biologists at top-rated research universities, almost a third of whom had been elected to the National Academy of Sciences (Feist, 1997). Again using TEXTPACK, the type-token ratio was calculated using the titles contained in their complete bibliographies (taken from their curriculum vitae). The TTR was negatively related to

1. the quantity of research,
2. the total number of citations received,
3. NAS membership, and
4. global eminence (an indicator consisting of peer ratings of creativity and historical significance, professional visibility, and the prestige of the highest honor received).

Although the TTR did not have an independent effect on global eminence once productivity was statistically controlled, the same held for the 69 psychologists as well.
This suggests that the depth of a research program affects a scientist's reputation largely through its positive influence on his or her total productivity.

However, some evidence seems to run counter to this conclusion.

- In one investigation of 2,030 scientists from 9 disciplines, those who had changed the subject-matter area of their articles actually displayed a slight increase in productivity as a consequence (Garvey & Tomita, 1972).
- In addition, authors of high-impact scientific contributions are more prone to display simultaneous involvement in several different research areas (Hargens, 1978; R. S. Root-Bernstein, M. Bernstein, & H. Garnier, 1993; R. J. Simon, 1974; M. S. Taylor, Locke, Lee, & Gist, 1984).

Yet these findings may not necessarily conflict with the inferences drawn from the content analyses of titles.

- For one thing, the samples and measures are not completely comparable.
- More importantly, close attention to the discrepancies reveals that all of these results may be subsumed under a single principle – the concept of a *network of enterprises* (Gruber, 1974, 1989).

Great scientists neither focus on a single narrow topic nor flip randomly around from topic to topic without rhyme or reason. On the contrary, the various subjects that constitute a highly successful research program are interconnected with each other, often in subtle or inexplicit ways. Indeed, often it is not until much later that the scientist comes to realize that two research interests have more mutual relevance than originally thought. The successful resolution of one puzzle may facilitate the solution to another, seemingly unrelated problem.

The great mathematician Henri Poincaré (1921) provided an illustration from his own career:

> I turned my attention to the study of some arithmetical questions apparently without much success and without a suspicion of any connection with my preceding researches. Disgusted with my failure, I went to spend a few days at the seaside, and thought of something else. One morning, walking on the bluff, the idea came to me … that the arithmetic transformations of indeterminate ternary quadratic forms were identical with those of non-Euclidean geometry. (p. 388)

Two previously disconnected research interests had become suddenly united, much as Descartes unified algebra and geometry into analytic geometry.

Hence, the low type-token ratio, the frequent subject-matter changes, and the simultaneous involvement in several research areas may all reflect the same underlining phenomenon, the operation of a network of enterprises. Such a network raises the likelihood that a scientist's research program will continue to be fruitful rather than becoming sterile. At the same time, the implicit linkages among the separate projects help ensure that discoveries will build upon each other, occasionally producing significant ideational syntheses. As Walter Cannon (1945), the great physiologist once noted,

> it seems probable that co-ordinate progress in research, process characterized by a natural development from one group of ideas to another, instead of a flitting from interest to interest in a quite inconsequential manner, is conducive to persistent effectiveness in productive scholarship. In this type of research, as studies advance and new facts are discovered, fruitful ideas accumulate and earlier ideas take on new meanings. As a result, fresh opportunities for exploration are frequently disclosed. (p. 218)

When one contemplates the careers of the truly great psychologists, the truth of Cannon's remark becomes most apparent. Freud, Pavlov, Piaget, and Skinner touched upon a tremendous diversity of topics and issues throughout the course of their long careers. Yet interweaving throughout each publication was a certain set of themes or principles or guiding metaphors.

The best illustration is the circumstance that surrounded the creation of the *Origin*. Darwin first began compiling a notebook on the subject of the "transmutation of species" in 1837, the year after his return from his voyage on the *H.M.S. Beagle*. In 1859 the first edition of the *Origins* was published. Between 1837 and 1859, inclusively, Darwin was engaged on a great many other projects. These included: several studies on the geology of South America (1837-1846), coral formation (1837-1842), volcanic islands and mountain chains (1838-1844), and geological formations in Scotland and Wales (1838-1842); preparation of the volumes reporting the zoological findings of the *Beagle* voyage (5 volumes on fossil mammals, mammals, birds, fish, and reptiles worked on from 1837-1845); extensive monographs on both fossil and modern cirripedes (1847-1854); plus a host of miscellaneous papers, notes, and reviews on topics as diverse as earthworms, mold, glacial action, erratic boulders, volcanic rocks, a rock seen on an iceberg, dust falling on ships in the Atlantic, the effects of salt water on seeds, seed vitality, the role of bees in the fertilization of Papilionaceous flowers, Waterhouse's *Natural History of the Mammalia*, and the species or genera *Rhea americana*, *Sagitta*, *Planaria*, and *Arthrobalanus* (1837-1858). That is an impressive range of topics, especially given that this period accounts for only about a quarter of his entire career as a scientist! Obviously, Darwin had many different things on his mind during the period that he conceived his theory of evolution by natural selection.

SPECIFIC SCIENTIFIC PUBLICATIONS

The within-individual variation in quality of output holds for scientists, including psychologists. According to the equal-odds rule, psychologists who generate more high-impact publications should also generate more no-impact publications.

- For instance, an investigation into the careers of 10 illustrious psychologists – all recipients of APA's Distinguished Scientific Contribution Award – revealed that 44% of their publications received no citations whatsoever during a 5-year time interval (Simonton, 1985b).
- Another study of 69 eminent American psychologists divulged that the single most influential work tends to account for over one quarter of the total citations received by his or her lifetime output (Simonton, 1992b).

This means that much of the differential fame attained by psychologists can be ascribed to the impact of their single best contribution. Cross-sectional variation with respect to that career landmark is immense. For the 69 psychologists, the most cited best work received 343 times as many citations as the least cited best work.

What can possibly account for this impressive variation in the impact of single publications?

- One factor has already been mentioned, namely the fact that books tend to be more influential than journal articles (Heyduk & Fenigstein, 1984; Simonton, 1992b).
- But this cannot provide a complete explanation, for even journal articles display substantial variation in citations (Redner, 1998).
- Another possible answer is that highly cited articles are published in high-impact journals, that is, the journals with high citation rates per article published. These journals are usually the most prestigious refereed journals in the discipline. In psychology, such journals include *Psychological Review*, *Psychological Bulletin*, *Journal of Personality and Social Psychology*, and *Developmental Psychology* (Buffardi & Nichols, 1981; M. J. White & K. G. White, 1977).

Yet the last explanation is not very satisfactory either.

- In the first place, the variation in impact is huge even for those articles published in the same journal. The variation is so large in fact, that the best papers published in second- or third-tier journals may prove more influential than the worst papers published in the top journals. In other words, the variance in article quality overlaps.
- Even worse, this answer begs the question. The better journals presumably have high impact because of their higher selectivity, as maintained by the editor, associate editors, editorial board, and ad hoc reviewers. Therefore, the question remains regarding the standards that must be satisfied to produce a high-impact piece of psychological science in the first place.

This question can be answered two major ways.

1. we determine the criteria that psychologists claim they use to judge the merits of scientific research.
2. we can scrutinize the criteria that psychologists actually apply in such evaluations.

The Ideal: What Psychologists Say

Peer review criteria: the quality of the presentation, the psychological value, social importance, theoretical significance of the results, the competence of the methodology, etc. (Wolff, 1973). Research suggests that some degree of consensus exists on the most appropriate criteria (Gottfredson, 1978).

Typical are the results of a survey of 66 editors of major journals in pure and applied clinical psychology, such as the *Journal of Abnormal Psychology*, *Journal of Clinical Psychology,* and the *Journal of Consulting and Clinical Psychology* (Wolff, 1970). They were asked to rank in order of importance 15 potential criteria for evaluating whether submitted manuscripts should be accepted or rejected.

- Top in the consolidated ratings were such standards as contribution to knowledge, research design, objectivity in reporting results, statistical analyses, writing style and readability, theoretical model, topic selection, and literature review.
- Toward the bottom in importance where such criteria as manuscript length, author's status and reputation, punctuation, and institutional affiliation.
- The coefficient of concordance was used to gauge overall inter-judge consensus. It came out to be .59, indicating a modest amount of agreement, despite the intrusion of some individual differences among the editors in their rating standards.

Significantly, the rankings of the criteria were strongly correlated with those found to govern journals in counseling and guidance psychology. The rank-order correlation was .91 (Wolff, 1970).

Other researchers have obtained comparable results by different means. For example, one inquiry led by Robert J. Sternberg first had 20 psychologists identify attributes of high-impact articles and then had 252 members of the American Psychological Society rate the resulting 45 hypothesized characteristics on 6-point scale of importance (Sternberg & Gordeeva, 1996).
A principal-components analysis (followed by a varimax rotation) yielded 6 factors: quality of presentation, theoretical significance, practical significance, substantive interest, and value for future research.

Hence, it appears possible to compile a set of clear-cut "do's and don't's" that detail what it takes for an article to leave an imprint on the field (see Gottfredson, 1978).
Yet something must be amiss! We have seen again and again that quality of output is a function of total quantity. Whether we are looking at individual differences or longitudinal changes, the equal-odds rule reigns supreme. Yet surely psychologists must be able to internalize the rules of the game so as to increase their odds. Over the course of their career, the hit rate should grow, and even the depressing rejections from journal editors decrease in frequency. But this does not happen. Not only is the ratio of successes to total attempts fairly constant across the life span, but the better-established psychologists may only escape painful rejections by switching from journal articles to book chapters as a main vehicle for communicating their ideas (Rodman & Mancini, 1981). What's going on here?

The Real: What Psychologists Do

Juxtaposed against this idealized portrait of the publication process in psychological science should be the following three complications:

1. If editors and reviewers exhibit such a strong consensus on the properties of a high-impact article, then that agreement should take the form of impressive inter-judge reliabilities in separate assessments of manuscripts submitted for publication. Many studies show that this is far from the case.

- A former associate editor of the *Journal of Personality and Social Psychology* calculated the reliability coefficients for referee evaluations of submitted manuscripts. For the evaluative criteria used for the editorial decisions, these reliabilities were: probable reader interest in problem .07, importance of present contribution .28, attention to relevant literature .37, design and analysis .19, style and organization .25, succinctness .31, and recommendation to accept or reject .26 (Scott, 1974; cf. Marsh & Ball, 1989; McReynolds, 1971; Scarr & Weber, 1978). Needless to say, if a submitted manuscript reported that its measures had reliabilities this low, it (probably) would be rejected for publication on methodological grounds!
- So poor is this consensus that most published articles in psychology journals would suffer rejection if resubmitted for publication, as has been empirically demonstrated (Peters & Ceci, 1982).
- Nor is psychology the only science to have such low levels of agreement (Cicchetti, 1991). Indeed, the evaluation process that underlies all peer-reviewed journals has been generally shown to be "a little better than a dice roll" (Lindsey, 1988, p. 75).
- Furthermore, the same minimal concordance confronts peer review when it is applied to research proposals submitted to major funding agencies (S. Cole, 1983). Indeed, the main predictor of whether or not a project gets funded is the total number of grant proposals submitted, as would be expected from the operation of the equal-odds rule alone (S. Cole, J. R. Cole, & G. A. Simon, 1981).

2. The judgmental criteria by which manuscripts are actually evaluated do not always operate as implicitly claimed by the evaluators.

- Often the assessment of a manuscript's quality is influenced by such extraneous factors as the prestige of the institutions with which the authors are affiliated, the existence of a special relationship between the authors and the editor or reviewers, the authors' gender, the professional status of the referees, and even the length of the submitted manuscript and the number of references it contains (Crane, 1967; Petty, Fleming, & Fabrigar, 1999; Stewart, 1983). Although the unfortunate effects of most of these contaminating factors can be ameliorated by the implementation of blind review process (Bowen, Perloff, & Jacoby, 1972), the factors can intervene elsewhere as well, such as the decision to cite someone's work once it is published (see, e.g., Ferber, 1986; Greenwald & Schuh, 1994).
- Even when a criterion is used, it is often is used in the wrong way. For instance, the quality of an investigation must be judged by both the importance of its research topic and the methodological rigor by which the topic is investigated. Yet the evidence tells us that methodological flaws are more likely to be overlooked if the topic is considered a highly significant one (Wilson, DePaulo, Mook, & Klaaren, 1993). That bias can even lead to the recommendation that methodologically weak papers be accepted for publication.

3. Given the low reliability of reviewer assessments, plus the introduction of various contaminating factors, it would seem rather difficult for the peer evaluations to have much connection with a publication's actual impact on the discipline. And, in fact, such ratings tend to have poor predictive validity (S. Cole, 1983; Gottfredson, 1978). At most only about 10% of the variance is shared.

- A study of research published in psychology journals found a correlation of .18 between rated quality and a log-transformed measure of citation counts (Shadish, 1989). Even worse, the publication attributes that predicted subjective quality evaluations were seldom the same as those that predicted objective citation measures.
- It was much easier to predict the quality ratings than the citation counts. On the one hand, about half of the variance in the subjective quality assessment could be predicted through a combination of predictors (although these predictors were themselves subjective, and therefore shared method variance with the criterion). On the other hand, only 10% of the variance in the objective citation counts could be similarly predicted.
- The same low predictive power of peer evaluations applies to grant proposals as well. That is, the priority scores given research proposals fail to predict the later impact of either funded or unfunded projects (S. Cole, 1983). Despite the supposedly objective nature of science, the ultimate influence of a scientific publication is not more readily predicted than that of a literary, artistic, or musical composition (see, e.g., Martindale et al., 1988; Simonton, 1980b, 1980c).

With the advantage of hindsight, it may seem obvious why these discouraging results must obtain.

1. Many of the criteria require highly subjective judgments about which there must be a diversity of opinions. For instance, who can objectively judge whether a journal's readers will find a particular problem interesting?
2. Some of the criteria require that the evaluator become a prophet, such as assessments about whether a study has important implications for future research.
3. The number of relevant criteria is very large, running into the dozens (Gottfredson, 1978; Shadish, 1989; Sternberg & Gordeeva, 1996). Although these criteria might be reduced to a subset of more inclusive judgmental factors, considerable variance is thereby left unaccounted for in the separate criteria. For example, when 45 distinct criteria are collapsed into 6 principal components, more than half of the variance in the items remains unexplained by the 6 factors (Sternberg & Gordeeva, 1996). Hence, evaluators are left with a bewildering array of standards for judging the merits of any potential contribution. This multidimensionally also implies that the attributes of a publication operate in a statistical rather than deterministic fashion to influence its ultimate success. The correlation of any one characteristic with a work's impact must necessarily be reduced as the number of participating factors increases.
4. It is very likely that the attributes that contribute to the impact of any product operate in a complex configurational manner (Simonton, 1999b). That is, interaction effects and curvilinear functions may dominate the determination (see, e.g., Simonton, 1980c, 1990c). What might be the best method to adopt for one substantive problem may be the worst for another; what might be an ideal way to organize one discussion might be horribly ineffective for another; 20 manuscript pages might be just right for treating one topic, but too long for another and too short for a third; and so forth. To assess a manuscript's scientific worth may thus require a subtle, probabilistic manipulation of multiple dimensions interlinked in complex relationships.

None of the foregoing would be especially problematic if the human cognitive apparatus were supremely sophisticated in its information-processing capacities. Yet the human mind is not by any means an ideal processor of information (see, e.g., Bruner, Goodnow, & Austin 1956; Faust 1984; Fiske & Taylor 1991; Kahneman, Slovic, & Tversky 1982; Meehl 1954; Miller, 1956). Instead, the human intellect is subject to many varieties of inaccuracies, constraints, and biases in perception, memory, thinking, and problem solving. Of special interest are the demonstrated human incapacities in the reliable inference of probabilistic, multidimensional, and configurational relationships among phenomena.

It also must be emphasized that these constraints, biases, and inaccuracies apply to everyone, including those who submit their manuscripts for publication.

- Authors are not necessarily any better at judging their own work than are journal referees, and sometimes authors may be even less adequate because of self-serving biases and other intellectual contaminants.
- Indeed, we know from the historical record that even the greatest scientists can get it very wrong. Gregor Mendel believed the theoretical significance of his research concerned evolution rather than inheritance. Specifically, his studies of peas were to shed light on the process of hybridization. The mathematical modeling of the inheritance process was only a secondary aspect of his classic studies. Hence, it could be argued that Mendel himself was not truly a "Mendelian" geneticist (Brannigan, 1981; Olby, 1979).
- All too often scientists have an overly high opinion of their own work, and thus find themselves cut down to size by those asked to serve as journal referees. This contingency we already noted earlier when we discussed the implications of the equal-odds rule, especially in its longitudinal form.

So what should those psychologists do who want to attain some degree of greatness in the discipline?

Well, certainly they should make every effort to produce work that meets the highest possible scientific standards. And, as examined earlier, it probably behooves the psychologist to engage in a network of enterprises rather than focus on an extremely narrow subject matter.

But beyond that, a tremendous amount of uncertainty remains about which of one's publications will have a major impact on the field and which will leave little or no impression on contemporaries and posterity. To the extent that the influence of a research program is governed by the equal-odds rule, it is best to exploit the odds by being extremely productive. As Wayne Dennis (1954a) expressed it,

> the correlation between fame and fecundity may be understood in part in terms of the proposition that the greater the number of pieces of scientific work done by a given man, the more of them will prove to be important. … Other things being equal, the greater the number of researches, the greater the likelihood of making an important discovery that will make the finder famous. (p. 182).

A similar idea was put forward nearly a century earlier by the philosopher Alexander Bain. In his classic psychological text on *Senses and the Intellect* is found the following passage: "The greatest practical inventions being so much dependent upon chance, the only hope of success is to multiply the chances by multiplying the experiments" (Bain, 1855/1977, p. 597).

Although you might become a mere Mass Producer by adopting such a career strategy, the odds still favor your thus joining the Prolific who may eventually figure prominently in the annals of our discipline. In contrast, if you strive to become one of the Perfectionists by meticulously trying to maximize all the supposed criteria for high-impact work for every item on your bibliography, you only do so at great risk. For you are then much more likely to end up among the Silent of psychology's history. In the final analysis, it is output that most securely and directly leads to impact.