Identification of scientists making long-term, high-impact contributions, with notes on their methods of working

Article *in* Creativity Research Journal · January 1993

DOI: 10.1080/10400419309534491

Robert Root-Bernstein
Michigan State University

Creativity Research Journal
Publication details, including instructions for authors and subscription information:
http://www.tandfonline.com/loi/hcrj20

Identification of scientists making long-term, high-impact contributions, with notes on their methods of working

Robert S. Root-Bernstein [a], Maurine Bernstein [b] & Helen Gamier [b]
[a] Physiology, Michigan State University, East Lansing, MI, 48824-1031
[b] University of California, Los Angeles
Version of record first published: 02 Nov 2009.

Creativity Research Journal
Volume 6 (4) 329–343 (1993)

Identification of Scientists Making Long-Term, High-Impact Contributions, with Notes on Their Methods of Working

Robert S. Root-Bernstein
Michigan State University
Maurine Bernstein
Helen Garnier
University of California, Los Angeles

ABSTRACT: A two decade (1958–1978) study was made of 40 male scientists, including four Nobel Prize winners. Multiple psychological tests were administered, along with interviews and analysis of publication rates and citations. The data yielded two factors that had very high predictive ability for identifying long-term, high-impact investigators: a series of five or more high-impact papers published by the age of 45 accompanied by simultaneous involvement in research in several areas. Scientists meeting these criteria all went on to produce high-impact papers into their late-50s and 60s, whereas the other scientists in the study did not. Other factors, such as number of publications, membership in the National Academy of Sciences, and the award of a Nobel Prize were not significantly predictive of continued impact. Thus, previous impact should not be used as a basis for further funding independent of other measures. Methods of working peculiar to long-term, high-impact individuals, such as frequent changes in the focus of their research, working on several problems simultaneously, and abhorrence of administrative duties, are also examined in light of Gruber's concept of networks of enterprise. The common preconceptions that a scientist's best work is done by the age of 40 and that productivity and creativity decline necessarily thereafter are shown to be common but unnecessary concomitants of aging. Possible reasons for exceptional cases are discussed.

As national science budgets peak or are curtailed, and as competition among an ever-

We dedicate the research reported in this article to the memory of the late Bernice T. Eiduson. We thank her husband Samuel Eiduson for permission to continue the study; Ivan N. Mensh of the Department of Psychiatry and Biobehavioral Sciences at the UCLA School of Medicine for his advice; and a Biomedical Research Support Grant through the Department of Psychiatry and Biobehavioral Sciences, UCLA School of Medicine, for funding. A Prize Fellowship from the John D. and Catherine T. MacArthur Foundation made possible the participation of Robert Root-Bernstein.

Correspondence and requests for reprints should be sent to Robert Root-Bernstein, Physiology, Michigan State University, East Lansing, MI 48824–1031.

growing body of scientists increases, it becomes important to identify and support those scientists most likely to make the greatest research contributions to their fields. We have found that a combination of two factors—a series of five or more high-impact papers published prior to the age of 40 accompanied by concomitant research in several different areas of a discipline (or several disciplines)—is an extremely high predictor of the ability of a scientist to carry out long-term, high-impact research. A "high-impact paper" means an article that elicits 10 or more citations in a single year, or 100 or more citations over 15 years. Less than 1% of papers achieve this distinction. Thus, peer evaluation suggests that such papers are considered unusually valuable. "Long-term" means an extended series of high-impact papers appearing with reasonable regularity over a span of 20 or more years. According to the research presented in this article, only about a quarter of those scientists publishing high-impact papers go on to become long-term contributors. In contrast, "short-term" contributors tend to have only a few high-impact papers that appeared in one or two brief periods lasting 2 to 5 years. Prediction of high-impact, long-term researchers may allow long-term or lifetime grants to be awarded to individuals most likely to continue to produce important scientific insights, at the same time fostering the eclectic, exploratory style of research that characterizes their work.

Methods and Materials

Data were analyzed from a long-term study of 40 male scientists begun by the late Bernice T. Eiduson in 1958 and carried through 1978 (Eiduson, 1960, 1962, 1966a, 1966b; Eiduson & Beckman, 1973). The scientists were fairly evenly spread among physics, chemistry, biochemistry, and biology, and no particular a priori criteria were employed in their choice other than willingness to participate in the study. Due to self-selection, this was not a random sample and the question of the generalizability of the results is therefore open to question. Personal interviews were conducted by Eiduson with each individual in 1958, 1964, and 1969, and by Eiduson or Maurine Bernstein in 1978. Rorschach and Thematic Aperception Tests (TAT) were administered at each interview in 1959, 1969, and 1978, and the Miller Analogies Test in 1969 and 1978. Bibliographies were compiled for each scientist and publication rates and citation data collected.

The mean age of the scientists in 1958 was 41.7 years (range, 29–59) and in 1978, 60.9 years (range, 50–79) (two of the scientists died during the course of the study). Four of the scientists were awarded the Nobel Prize. Two others were repeatedly nominated for the Nobel Prize and appear in lists of scientists holding the so-called "41st chair" (those generally considered to have deserved a Nobel Prize but not receiving one) (Zuckerman, 1977, pp. 296–302). An additional scientist became a member of a President's Science Advisory Committee. Eleven became members of the National Academy of Sciences, including all those receiving the Nobel Prize or nominations for that award. Some scientists achieved eminence in their fields as researchers, others as administrators and governmental advisors. A few produced only a handful of papers and never established significant reputations. In short, the group is heterogeneous.

The data were retrospectively analyzed to determine if any of the psychological tests, interview questions, or bibliographic material were predictive of high achievement. One publications-related factor was identified as a measure of achievement: the "impact ratio" or the ratio of total citations an author received in the *Science Citation Index* between 1964 and 1978 to the author's total number of publications over the same period of time (Ashton & Oppenheim, 1978; Garfield, 1970a, 1970b, 1973). Four clusters emerged: The highest had an average impact ratio of 15.4; the next highest 6.0; the third 3.7; and the lowest 1.6. The differences were statistically significant. The impact ratio was then used as the dependent variable in analysis of variance (ANOVA) testing for significant differences between groups of scientists. The groups were defined by age, cognitive, emotional, and motivational criteria, health, area of expertise, family background, TAT items, and Rorschach responses. Two approaches to the Rorschach data were essayed. In one instance the data were treated as a continuous dependent variable. In another, the scientists were divided into three groups based on Rorschach responses: those at least a standard deviation below the average number of responses of normal male subjects (i.e., 21.8 ± 5.1 [Exner, 1978]), those within a standard deviation of the average, and those at least a standard deviation above the average. In this case, the Rorschach groups were treated as an independent variable.

Additionally, a cluster analysis was performed on the publication and citation data. Four clusters were requested, but these yielded a poor distribution of cases in each group. The majority of cases were assigned to one cluster and the remaining three had only two members each. These groupings were inadequate as analytic tools, so a qualitative analysis of the publication and citation data was performed. This produced four distinct categories ranging from low to high impact on the scientific community: (a) scientists having one or more papers cited 100 times or more in the *Science Citation Index* in the period from 1964 to 1978 inclusive (this group included all of the Nobel laureates and those known to be nominated for this award); (b) those having at least one paper cited 10 times in one year (but not meeting category 1 criteria); (c) those not meeting category 1 or 2 criteria, but having at least one paper cited 10 times during the period from 1964 to 1978 inclusive; and (d) those scientists meeting none of the previous criteria. In other words, the impact of a scientist's work on his colleagues was allowed to determine his achievement ranking. The scientists were approximately evenly distributed among the four categories: 12 in category 1; 11 in category 2; 8 in category 3; and 9 in category 4.

The qualitative grouping was identified as the independent variable in a series of ANOVAs. The dependent variables included the scientists' background and psychological test variables (age, health, area of expertise, hobbies, parent background, the 50 cognitive, emotional, and motivational items from the TAT and 9 clustered factors that were derived from them [e.g., combined emotional responses], the number of Rorschach responses, and Miller Analogies scores—a total of 84 variables) at three time points each (1958, 1969, 1978, except for the Miller Analogies scores which were available only for 1969 and 1978).

Results

A significant difference was found between impact means (the ratio of total citations to total publications) with the interaction of age and area of expertise in a two-way ANOVA (but not with age or area of expertise alone): Middle-aged physicists had significantly higher impact than young chemists, older biologists, and young biochemists. It is not clear what the utility of this finding may be, other than suggesting that scientific impact peaks at different ages in different scientific professions (Simonton, 1988, 1991).

Impact also varied significantly by the Rorschach response group. Notably, the average number of responses was higher than the general population (34, with a range of 10–191) and decreased (insignificantly) with age from 36 to 31 responses between 1958 and 1978. The results were comparable to those reported by Roe (1953) in all parameters. Scientists with the highest number of Rorschach responses had a significantly ($p < .05$) greater impact ratio and a significantly higher number of high-impact papers than those in the middle and low response groups. This finding suggests that those scientists who are able to imagine the most alternative views of a set of data are likely to be the most successful at finding the correct interpretation of a scientific problem, and that scientists who imagine fewer possibilities are more likely to overlook the correct answer. There were, however, some striking exceptions to this statistical result. Two of the highest impact scientists (nos. 25 & 35), whose scientific creativity is beyond question, were in the lowest Rorschach response group. They were uncooperative. One (no. 25) for example, said:

> When I use my imagination, I would like to have it controlled by the situation, and what you're telling me to do is to put it in reasoning where there is no control. . . . In fact, I shy away from a situation where there are so many alternatives—I can't decide . . . I like to invent and then check. Otherwise there's no point.

This response suggests that one aspect of this man's scientific success was his ability to choose only those problems for which his imagination was likely to produce a startling, but verifiable answer. On the other hand, three of the lowest impact scientists were in the highest Rorschach response group, suggesting that an overly fecund imagination uncontrolled by factual checks may become paralyzing for a scientist. Thus, the number of Rorschach responses is highly associated with production of high-impact papers, but does not predict them in individual cases. Further study of exceptions in light of problem-solving strategies might prove enlightening.

No other significant differences that were consistent over time or between impact categories or professional groups were found using health, childhood interests or hobbies, parent background, Miller Analogies Test scores, or any of the 50 cognitive, emotional, or motivational variables from the TAT. In short, there appears to be no dominant psychological profile of the very successful or the unsuccessful scientist, at least according to the tests and definitions utilized in this study.

Nonetheless, we did find one unexpected differentiator that may be of some use in evaluating and funding scientists. Graphing the appearance of high-impact papers over time (Figure 1, p. 333) yielded an interesting pattern. High-impact scientists (those in Groups 1 & 2) tended to fall into one of two patterns: They either had one or two small clusters of high-impact papers during a brief period in their careers, or they produced many high-impact papers over a long period of time.

Publication patterns were investigated to determine whether or not there was something about the work habits of these different groups of scientists that might influence the differences in their impact patterns. It was discovered that long-term, high-impact scientists (Figure 2, p. 334) investigated a broader range of scientific problems than their colleagues, and they carried out research in several different fields simultaneously, which the short-term contributors did not. The range of problem areas was determined by classifying the publications into broad categories such as what might be found in separate chapters of introductory

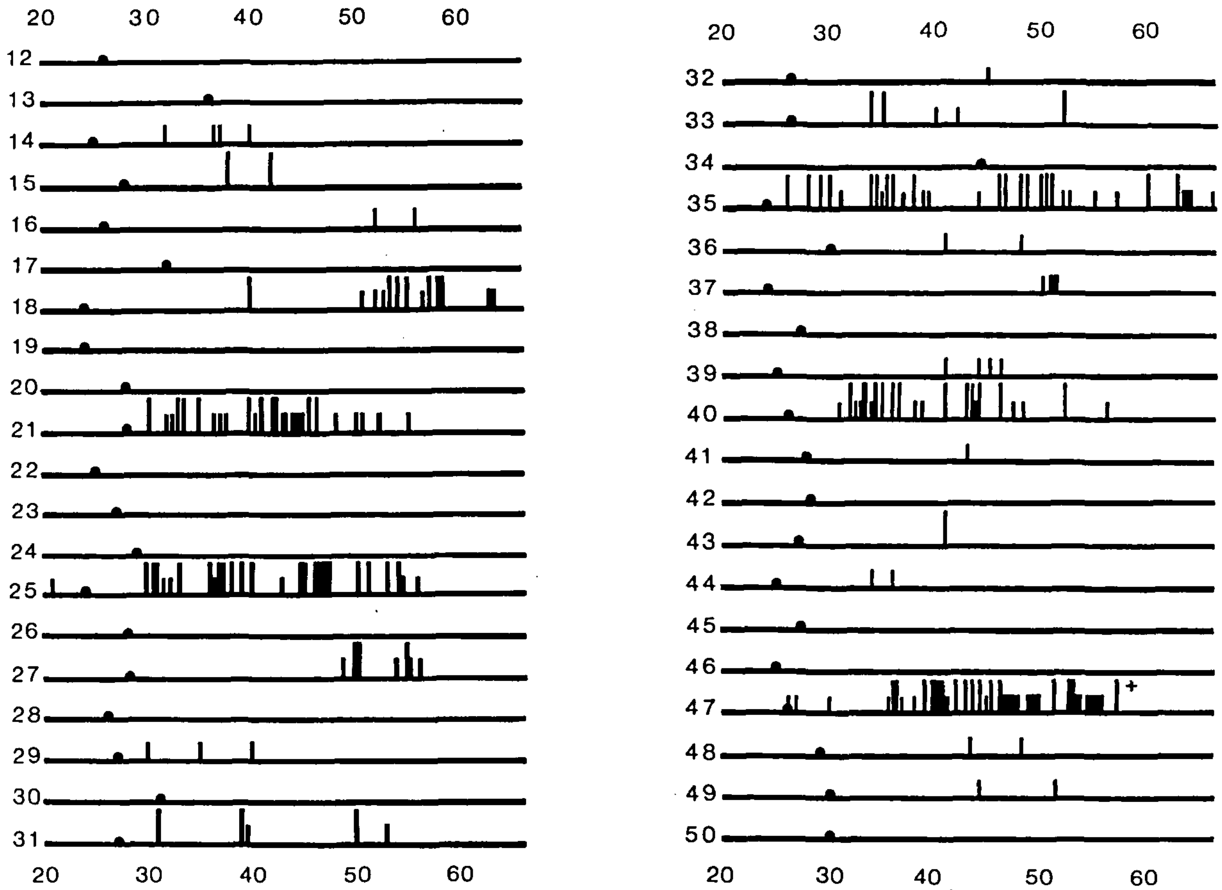

Figure 1. *Incidence of high-impact papers for each scientist (listed according to the code number at the left), plotted according to the scientist's age (20–67) at publication (top and bottom). Tall bars represent papers receiving more than 100 citations between 1964 and 1978 inclusive. Short bars represent papers receiving 10 or more citations in a single year, but less than 100 citations total. Dots mark the age at which the doctorate was awarded. One of the 40 scientists died so early in the study that his publication and citation record was not comparable to the others. Another (no. 47) died near the end of the study.*

textbooks in biology, biochemistry, chemistry, or physics. For example, one man performed only two types of research during his career: biochemical metabolism and polymer synthesis. Another worked solely on plant growth substances. A third worked on the genetics of corn, flies, and molds. A fourth worked on crystal structures, quantum mechanics, biochemistry, and immunology. In each case, the differences in techniques and subjects was so large that they are recognized as being different specialties by working scientists.

Once the publications were identified by area, they were graphed in the order in which they were published with a different symbol given to each type of research area. This procedure yielded Figures 2 through 4. Analyses of these figures revealed that the research patterns of the long-term, high-impact producers were more diverse and their foci changed more frequently than any other group. The long-term, high-impact scientists (Figure 2) averaged five major topics of research in their first 100 publications and switched back and forth between topics an average of 43 (range 35–51) times (n = 6). Short-term, high-impact scientists (Figure 3, p. 335) averaged three major topics of research in their first 100 publications and switched back and forth between these topics an average of only 16 (range, 10–22)

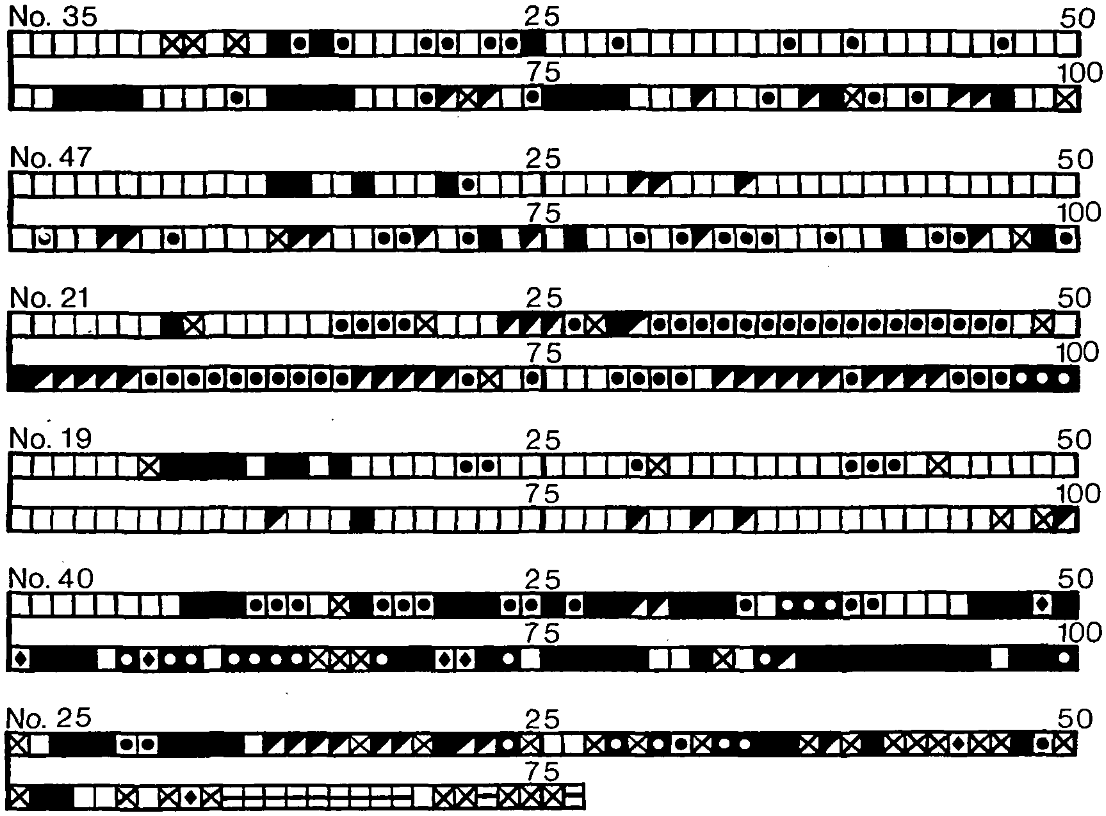

Figure 2. *Publication patterns of long-term, high-impact scientists (see text for definition of terms). Each box represents a publication. Each symbol within a box (blank, black, circle, etc.) represents a different field of research for that individual scientist. The xs represent miscellaneous papers on topics not repeated, book reviews, popular addresses, and so forth. In all cases, at least 15 years of publications are plotted serially, beginning with the scientist's first publication. One scientist published less than 100 papers during his career. Numbers at left are the scientists' code numbers that correspond to the numbers shown in Figure 1.*

times (n = 6). The difference between the two groups is significant ($t[1]$ = 5.64, p < .05) despite the small ns. There is, however, no significant difference between the publication patterns of high-impact, short-term contributors and scientists in lower impact groups (Figure 4, p. 336). Low-impact scientists (Groups 3 & 4) averaged two major topics of research in their first 100 publications (though few achieved 100 publications during their careers) and switched topics an average of 17 (range, 7–27) times (n = 17). Thus, this type of analysis is only useful for differentiating long-term, high-impact scientists from all others, but not for predicting impact ratios per se.

There was no apparent association between perceived eminence, honors and awards, and whether or not a scientist appeared in the long-term or short-term, high-impact category. Three Nobel Prize winners and a man repeatedly nominated for the award were in the long-term category. One Nobel laureate and a repeat nominee for that award were in the short-term category. The member of the President's Science Advisory Committee was in the second impact group and was a short-term contributor (scientist no. 39). All six of the long-term, high-impact scientists were members of the National Academy of Sciences. Three members of the National Academy were in the short-term, high-impact group (nos. 15, 27, & 31). Two members of the National Acad-

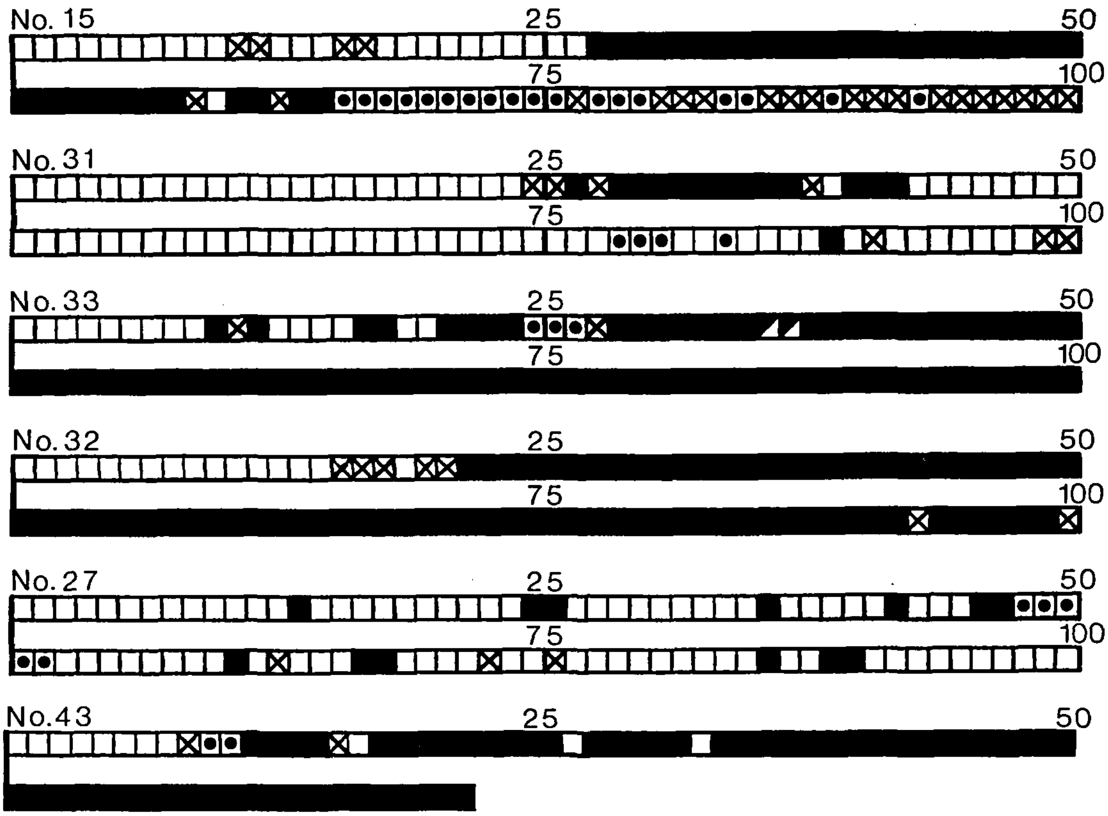

Figure 3. *Publication patterns of short-term, high-impact scientists (see text for definition of terms). Refer to Figure 2 caption for description of figure.*

emy were in lower impact groups (nos. 19 & 39). Three high-impact scientists (nos. 32, 33, & 43) did not gain admission to the National Academy. In short, the criteria do *not* suggest that one way of working is more successful than another, but only that a diversified pattern of research is predictive of *continued* scientific creativity after an initial success.

There was also no relationship between long-term or short-term, high-impact scientists and scientific field. Each group contains physicists, chemists, and biologists (no biochemists). This is a notable result because many scientists, especially in the so-called "hard" or theoretical sciences, believe that a scientist has done his or her best work by the age of 35. Two of the long-term, high-impact scientists were theorists in physics or physical chemistry. Two others were experimental chemists. The remaining two were experimental biologists.

The most interesting aspect of this pattern difference is that all but one of the long-term, high-impact scientists published at least five high-impact papers by the time they were 37 years old, and subsequently produced high-impact papers for 20 or more years. Because one measure of a high-impact paper used here was the occurrence of 10 or more citations in a single year, and most such papers (as well as most of the papers receiving 100 citations over 15 years) received this number of citations-per-year within 3 to 5 years of publication, these results suggest that it should be possible to identify with some precision potential long-term producers of high-impact papers between the ages of 40 and 45.

One other result is also worth highlight-

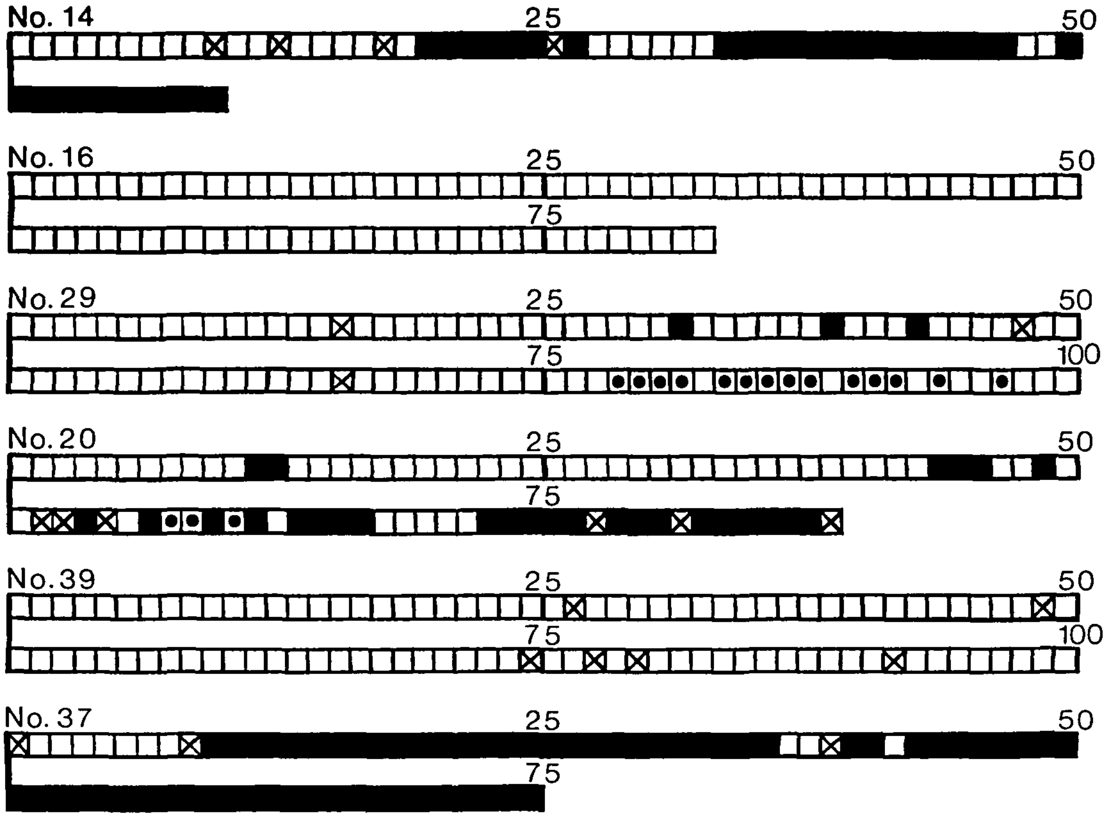

Figure 4. *Publication patterns of some typical low-impact scientists (see text for definition of terms). Refer to Figure 2 caption for description of figure.*

ing. It was surprising to observe that 2 of the 12 high-impact scientists (a Nobel laureate in the long-term group and another scientist in the short-term group) produced only about 75 papers during their entire careers (which in each case stretched over more than 30 years; see Figures 2 & 3). These men averaged only slightly more than two publications per year, which is about the rate of publication of the average scientist (1.55–2.01 at ages 40–60) and half the average of Nobel laureates (4.04–4.32 at ages 40–60). Conversely, two scientists published close to 200 papers but averaged less than two citations per paper. None of their papers accumulated as many as 10 citations during the 15 year period studied. Clearly, the perceived quality of a scientists' contribution is not a direct function of number of publications, and the uncritical use of this measure in peer review and in analyses of creativity is cautioned against (Andrews, 1979; Mellanby, 1974; Simonton, 1991).

Discussion

Despite a broad search for psychological factors that identify long-term, high-impact scientists from less distinguished and less productive colleagues, no significant or useful relationships were found. However, two factors concerning work habits that did seem to characterize long-term, high-impact scientists and differentiate them from short-term, high-impact scientists and from low-impact scientists were found: five or more high-impact papers published by the age of 45 and a repeatedly changing set of diverse research topics investigated concurrently. In short, a probable predictor of future creativity in science appears to be prior, acknowledged creativity (Shapero, 1985) combined with diverse scientific experience applied to a changing set of research topics. Age did not appear to be a determining factor of future creativity among the sample of scientists, despite much evidence suggesting that most scientists do their best work by the age of 35 or 40. Consider each of these points separately.

First, 95% of the high-impact papers in a scientific discipline are published by only 5% of the scientists (Carter, 1974). It is therefore clear that a small number of investigators direct the progress of science. Previous studies already have demonstrated that high-impact papers are predictive of the Nobel Prize (Ashton & Oppenheim, 1978). The difficulty is that most scientists appear to make only a single major contribution to science during their lifetimes. This was true of several of the scientists in this article's experimental group, one of whom received the Nobel Prize and another who was nominated for that award several times. One can also think of the career patterns of men such as James Watson or Jonas Salk, who produced only a single monumental discovery during their careers. Indeed, an analysis of the publication patterns of Watson, Salk, Avery, Joliot, Soddy, and Einthoven (Figure 5)—each of whom is known for only one major contribution to science—results in figures comparable to the short-term, high-impact scientists studied here. These men averaged three major research topics each in their first 100 papers and switched between topics 24 (range, 18–30) times. In

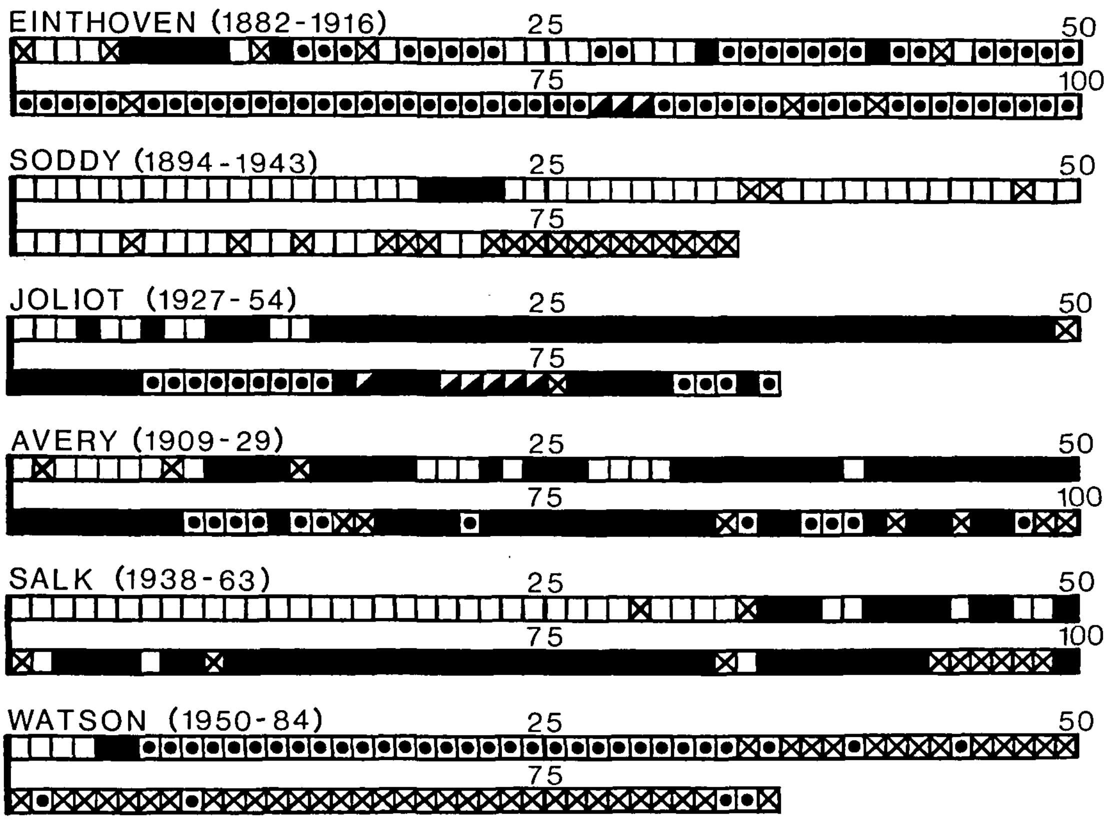

Figure 5. *Publication patterns of short-term, high-impact scientists not involved in this study. Historical research or (for living scientists) citation analysis reveals that each of the men had only one major contribution recognized by the scientific community (Root-Bernstein, 1989). See Figure 2 for further description.*

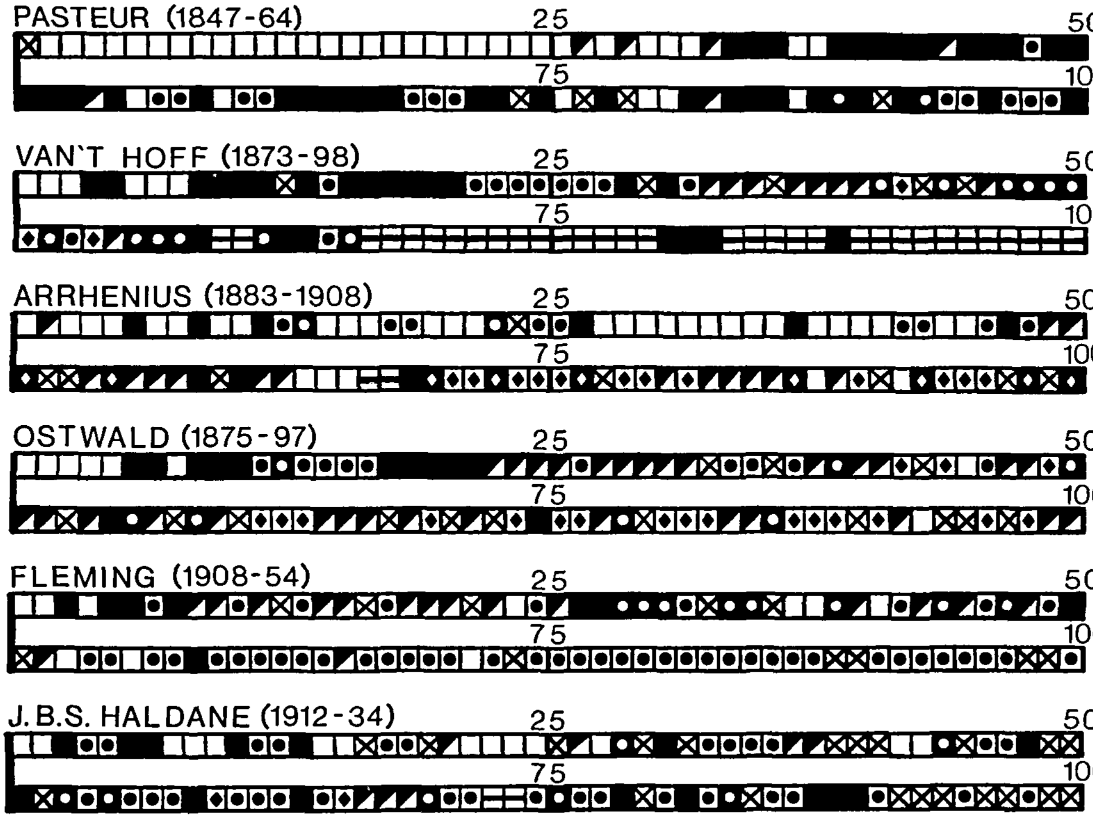

Figure 6. *Publication patterns of long-term, high-impact scientists not involved in this study. Historical research revealed that each man produced multiple contributions in several fields that were highly valued by their colleagues (Root-Bernstein, 1989). See Figure 2 for further description.*

comparison, Pasteur, van't Hoff, Arrhenius, Ostwald, Fleming, and Haldane (Figure 6)—all noted for several major contributions to several disciplines over several decades—resemble the long-term, high-impact scientists. They averaged six major research topics in their first 100 papers and switched topics an average of 55 (range, 46–64) times. Again, the difference between the two groups is significant, $t(1) = 7.57$, $p < .05$. (Combining the data from the present study with the historical figures yields long-term, high-impact scientists studying an average of 5.5 major topics in their first 100 papers and switching topics an average of 49 times; short-term, high-impact scientists studying three major topics and switching topics 20 times [each $n = 12$]).

Thus, it must be emphasized that although high-impact papers may in and of themselves indicate eminence, they are not sufficient to predict future research contributions. Future funding should not be tied solely to previous impact. Moreover, in this study and in the historical examples cited, staying within one's area of expertise after having made a major contribution is *negatively correlated* with making major research contributions subsequently. No scientist who remained in his specialty after publishing two high-impact papers succeeded in publishing a third. Only those who changed fields contributed more high-impact papers.

Anecdotal evidence both from historical sources and from interviews with the scientists in the present study suggest why a diverse research program may be particularly fecund in the long-term. For example, Nobel laureate Albert Szent-Györgyi wrote about the difficulty of remaining creative.

> When I saw actomyosin for the first time [one of the discoveries for which he was awarded the Nobel Prize], I was convinced that in a fortnight I would understand muscle completely. Then I worked 20 years more without learning a thing. . . . (Szent-Györgyi, 1966, p. 68)

Similarly, Hans Selye commented:

> As the years went by, I managed to acquire every available facility that modern science can offer in the way of the most up-to-date techniques of histology, chemistry, and pharmacology. I have been given the means to construct one of the best-equipped institutes of experimental medicine and surgery in the world and have acquired a staff of 53 trained assistants, technicians, and secretaries. Yet today as I look back upon those early observations in 1936, I am ashamed to say that, despite all this help, I have never again been able to add anything comparable in its significance to those first primitive experiments. (Selye, 1977, p. 287)

The problem, Szent-Györgyi said, is that "if one works for ten or twenty years on something, one needs a change of atmosphere. One gets stale; one doesn't see things" (Szent-Györgyi, 1966, p. 68). "Once a man has missed the solution to a problem when he passes it by," said Leo Szilard, "it is less likely he will find it next time" (Szilard, 1966, p. 28). As the long-term, high-impact scientists in this study made clear in their interviews, lifelong producers of breakthroughs are not content simply to rework old fields or to refine prior insights or to become administrators of other people's research. They desire more and therefore change fields periodically. Several did this as an explicit aid to inventiveness. As one man (no. 18) said: "My advice to people whose research productivity is diminishing is to change fields." Several other long-term contributors (e.g., nos. 25 & 35) echoed his advice.

It was also noticed that those most likely to change fields are those who are constantly exploring other research problems even as they focus on one or two major ones. Two aspects of this phenomenon are significant. First, several studies have shown that scientists rarely (less than 10% of the time) can make significant headway on a problem by a direct, prolonged attack on it. Most report that they must abandon a problem before the solution occurs to them, or they find that the solution only arrives as a result of addressing another, related problem (Fehr, 1912; Platt & Baker, 1931; Root-Bernstein, 1989). Thus, keeping several research problems going at once may benefit long-term, high-impact scientists by creating the best mental conditions for a high rate of insights (Jewkes, Sawers, & Stillerman, 1958). When one project is going poorly, another may be going well, and in the meantime, the scientist may have an insight concerning the first. In some cases, the scientists (e.g., nos. 21 & 25) reported that they kept problems "simmering on a back burner" until adequate data, a new technique, or some insight finally made them accessible.

The other aspect of constantly exploring a range of research problems is more obvious. By trying many things, long-term, high-impact scientists optimize the probability of finding new, significant, and important problems. Several of the scientists stated that they use strategies similar to that of Linus Pauling, who wrote that a scientist must "have lots of ideas and throw away the bad ones. And I think that this is part of it: that you aren't going to have good ideas unless you have lots of ideas and some sort of principle of selection" (Pauling, 1977, p. 44). The constant experimentation with new fields exhibited by the long-term, high-impact researchers would indicate the validity of

Pauling's insight. Studies of both industrial chemists and university biomedical researchers confirm that the most effective and creative scientists are those who combined several specialties or technical functions as part of their normal work habits (Finkelstein, Scott, & Franke, 1981; Jewkes, Sawers, & Stillerman, 1958; Pelz & Andrews, 1966). Nobel laureates Murray Gell-Mann, David Baltimore, Herbert Simon, and Vassily Leontief likewise agree that mastery of a wide range of tools and ideas from a diversity of weakly related disciplines combined with an understanding of emerging forms of mathematics are the keys to creating the sciences of the future (Branscomb, 1986).

Both the notion of exploring synergistic research areas and optimizing the probability of success by pursuing diverse interests are highly reminiscent of Gruber's (1988a, 1988b) notion of *networks of enterprise*. Gruber defined such networks as consisting of a person's organization of purpose or definition of his or her working self; a structure that organizes what may appear to be a bewildering miscellany of activities; an organization of goals that provide different levels of risk and reward at different levels of aspiration to fit different changing moods and needs; and finally a sense of what makes a person's work individual and unique (Gruber, 1984, 1988, 1989). It is clear from the results of this study that the most successful scientists in the group developed networks of enterprise as complex and varied as those Gruber described for Charles Darwin and some of the other scientists he has studied.

Finally, consider the question of age. A wide variety of reports in the literature, including comments by the scientists, the opinions of many members of the scientific community, and a number of formal studies, suggest that scientific creativity declines irreversibly with age. Novices—those under the age of 35 or 40, make the majority of breakthroughs in science (Diamond, 1986; Lehman, 1953; Lightman, 1984; Simonton, 1988, 1991; Thomson, 1957; Watson, 1979). One reason often suggested for this phenomenon is that older scientists get saddled with increasing obligations—speaking engagements, administrative work, committees, fund raising, reviewing—that keep them from active research (Ghiselin, 1989). On this point, it is interesting to note that only two of the long-term, high-impact scientists (nos. 18 & 35) tried administrative work. One immediately abandoned it, stating that it was unfulfilling. The other remained as head of his department for 5 years despite his vehement protests. Eventually a medical leave forced the department to replace him and he went back to research for the rest of his career. In contrast, all of the short-term, high-impact scientists spent most of their later careers as administrators. Whether this change in career emphasis resulted from a lack of new ideas or contributed to it is not clear. One study, however, reported that industrial chemists producing many patents rarely manifested an interest in administrative work, whereas those producing no patents *always* manifested such an interest (McPherson, 1964).

Another reason given for decline in creativity is the scientists inability to imagine making another discovery. As one of the short-term, high-impact scientists (no. 15) suggested, suppose you do something great as a young man:

> So they're all going to say, "He ought to do another thing like this." He knows that in a lifetime, his chance of doing this again—the equivalent of this—is almost zero—so he's going to be a little depressed by this, isn't he?

Setting impossible standards for oneself apparently interferes with ever trying anything again.

Yet another reason sometimes given for decline in scientific creativity is that it is physiologically or mentally "inevitable." J. Z. Young, the famous neuroanatomist, wrote that:

> There seems to be a limit beyond which new patterns and new connections are no longer easily formed. As we grow older the randomness of the brain becomes gradually used up. The brain ceases to be able to profit from experiment, it becomes set in patterns of laws. The well-established laws of a well-trained person may continue to be usefully applied to situations already experienced, though they fail to meet new ones. Here we see with startling clearness the basis of some of the most familiar features of human society: the adventure, subversiveness, inventiveness, and resource of the young; the informed and responsible wisdom of the old. (Beveridge, 1980, p. 101)

Although a distinction between intelligence and creativity may be necessary, it is important to note that the Miller Analogies Test scores of the scientists were constant (within experimental error) across the test period, and even into retirement. Thus, intelligence in the scientists did not seem to decline with age. Furthermore, the long-term, high-impact scientists demonstrated that scientific creativity need not decline. But note carefully the apparent reason: These men purposely placed themselves in the position of becoming novices again every 5 or 10 years. In effect they become mentally young by starting over again.

As several of them said in interviews, starting over again takes courage. It also apparently requires a different network of enterprise than the monolithic or monomaniacal style that characterizes one-time discoverers. Perhaps it is the courage to be ignorant again that fails most scientists as they grow older and not a matter of succumbing to physiological fatigue or set patterns of thought. Be that as it may be, it is evident that the *novice effect*, as it has been called (Root-Bernstein, 1984, 1989), worked for this select group of scientists. The novice effect may explain Simonton's (1988, 1991) observation that three factors all correlate with eminence in science: early age of first publication, late age of career landmark publications, and age of last publication. Those scientists who are effectively active over the longest time span alter science the most. The crucial adjective is "effectively": It is not publication, but impact that is significant. One thing is certain: Creativity and productivity do not necessarily decline with age. Some very successful scientists retain a youthful profile of scientific research activity and impact well into old age (Cole, 1979; McDowell, 1982; Stern, 1978; Zuckerman, 1977).

One other result—not statistically significant, but possibly instructive—is that one of the scientists (no. 32) received a lifetime research award shortly after receiving his doctorate (see Figures). His research was monolithic and was characterized by a single high-impact paper (placing him in impact group 2). Thus, guaranteed funding per se does not appear to spur scientific creativity, and not everyone may benefit from it. Ideas may need money for their development but money does not buy ideas.

Conclusions

Several important implications can be drawn from this study. The first is that the current system of science does not seem to foster long-term, high-impact scientists. Statements by a number of the scientists interviewed in this study as well as discussions with scientists at a variety of institutions (e.g., the Salk Institute for Biological Studies, UCLA, and the California Institute of Technology) indicate that increasing specialization, professionalization, peer review constraints, and budgetary pressures are making it ever more difficult for even the best established scientists to investigate several fields simultaneously or to change fields periodically (Root-Bernstein, 1989). Two Nobel laureates reported that although they can demonstrate a remarkable record of prior success as problem solvers, they cannot demonstrate any evidence of expertise in their new field. Their previous "track record" apparently is considered inadequate by many peer review boards. They also may be encountering unwarranted (in their particular cases) age discrimination. One Nobel laureate (no. 25) reported that he maintains his independence by refusing to play the grantsmanship game. He is fortunate to be at a university that permits him this idiosyncrasy and has sufficient resources to support his research internally.

Even more disturbing are reports by several of the scientists (and by commentators on drafts of this article) that young scientists displaying the sort of variegated research pattern predictive of long-term contributions are apparently discriminated against by more narrowly focused scientists who believe that if one has not settled upon a single area of research by the age of 30 or so, one has no future in science. A previous study by Jewkes, Sawers, and Stillerman (1958) on the conditions fostering inventions, and the work of Gruber (1984, 1988a,b, 1989) on networks of enterprise, indicate the opposite. If one can generate research valued by the scientific community, one's probability of continuing to do so depends upon investigating neighboring research areas and switching fields reasonably often.

If educated eclecticism in research, changing fields, and broad networks of enterprise are crucial factors in the ability of scientists to maintain long-term, high-impact research programs, then it is essential that scientists having the potential to do such research be fostered and not fettered or discouraged. One way of fostering their research would be to free them from the normal constraints of demonstrating their competence every few years in order to obtain or renew a grant (Burch, 1976; Mellanby, 1974; Yalow, 1986). It would be of great benefit to science if scientists displaying the unusual characteristics of work and publication described here were to be given lifetime grants or fellowships of some sort by their 40th or 45th year. A condition for tenure of such grants might be abjuration of administrative duties. Incentives might be added for changing fields periodically to enhance their natural predilections. It is also suggested that their methods of work, if emulated, might increase the effective creative lifespan of other scientists.

REFERENCES

Andrews, F. M. (Ed.). (1979). *Scientific productivity*. Cambridge, England: Cambridge University Press.

Ashton, S. V., & Oppenheim, C. (1978). A method of predicting Nobel prize winners in chemistry. *Social Studies of Science, 8*, 341–348.

Beveridge, W. I. B. (1980). *Seeds of discovery*. New York: Norton.

Branscomb, L. M. (1986). The unity of science. *American Scientist, 74*, 4.

Burch, G. E. (1976). On venture research. *American Heart Journal, 92*, 681–683.

Cole, S. (1979). Age and scientific performance. *American Journal of Sociology, 84*, 958–977.

Diamond, A. M., Jr. (1986). The life-cycle research productivity of mathematicians and scientists. *Journal of Gerontology, 41*, 520–525.

Eiduson, B. T. (1960). The scientist's image of himself. *Science, 132*, 552–554.

Eiduson, B. T. (1962). *Scientists: Their psychological world*. New York: Basic Books.

Eiduson, B. T. (1966a). Productivity rate in research scientists. *American Scientist, 54*, 57–63.

Eiduson, B. T. (1966b). Scientists as advisors and consultants in Washington. *Bulletin of Atomic Scientists, 22*, 26–31.

Eiduson, B. T., & Beckman, L. (Eds.). (1973). *Science as a career choice*. New York: Sage.

Exner, J. E. (1978). *The Rorschach: A comprehensive system. Vol. II. Current research and advanced interpretation*. New York: Wiley-Interscience.

Fehr, H. (1912). *Enquête de l'enseignement mathematique* [Investigation of mathematical work]. Paris: Gauthier-Villars.

Finkelstein, S. N., Scott, J. R., & Franke, A. (1981). Diversity as a contributor to innovative performance by academic physicians. In E. B. Roberts, R. I. Levy, S. N. Finkelstein, J. Moskowitz, & E. J. Sondik (Eds.), *Biomedical innovation* (pp. 135–143). Cambridge, MA: MIT Press.

Garfield, E. (1970a). Citation and distinction. *Nature, 242*, 485–488.

Garfield, E. (1970b). Citation indexing for studying science. *Nature, 227*, 669–671.

Garfield, E. (1973). Editorial. *Current Contents, 40*, 5–7.

Ghiselin, M. T. (1989). *Intellectual compromise*. New York: Paragon House.

Gruber, H. E. (1984). *Darwin on man: A psychological study of scientific creativity* (2nd ed.). Chicago, IL: University of Chicago Press.

Gruber, H. E. (1988a). Networks of enterprise in creative scientific work. In B. Gholson, A. Houts, R. A. Neimayer, & W. Shadish (Eds.), *Psychology of science and metascience*. Cambridge, England: Cambridge University Press.

Gruber, H. E. (1988b). The evolving systems approach to creative work. *Creativity Research Journal, 1*, 27–51.

Gruber, H. E. (1989). The evolving systems approach to creative work. In D. B. Wallace & H. E. Gruber (Eds.), *Creative people at work* (pp. 3–24). Oxford: Oxford University Press.

Jewkes, J., Sawers, D., & Stillerman, R. (1958). *The sources of invention*. London: Macmillan.

Lehman, H. C. (1953). *Age and achievement*. Princeton, NJ: Princeton University Press.

Lightman, A. P. (1984, March). Elapsed expectations. *New York Times Magazine*, p. 68.

McDowell, J. (1982). Obsolescence of knowledge and career publication profiles: Some evidence of differences among fields in costs of interrupted careers. *American Economic Review, 72*, 752–758.

McPherson, J. H. (1964). Prospects for future creativity research in industry. In C. W. Taylor (Ed.), *Widening horizons in creativity* (pp. 412–424). New York: Wiley.

Mellanby, K. (1974). The disorganization of scientific research. *Minerva, 12*, 67–82.

Pauling, L. (1977). *Linus Pauling: Crusading scientist* [Television interview]. Boston, MA: WGBH-TV. (Transcript of *Nova*, No. 417)

Pelz, D. C., & Andrews, F. M. (1966). *Scientists in organizations. Productive climates for research and development* (pp. 54–79). New York: Wiley.

Platt, W., & Baker, R. A. (1931). The relationship of the scientific "hunch" to research. *Journal of Chemical Education, 8*, 1969–2002.

Roe, A. (1953). *The making of a scientist*. New York: Dodd, Mead.

Root-Bernstein, R. S. (1984, April). Elapsed expectations. *New York Times Magazine*, p. 130.

Root-Bernstein, R. S. (1989). *Discovering*. Cambridge, MA: Harvard University Press.

Selye, H. (1977). Biological adaptations to stress. In W. R. Klemm (Ed.), *Discovery processes in modern biology* (pp. 266–288). Huntington, NY: Robert Kreiger.

Shapero, A. (1985). Managing creative professionals. *Research Management, 28*, 23–28.

Simonton, D. K. (1988). Age and outstanding achievement: What do we know after a century of research? *Psychology Bulletin, 104*, 251–267.

Simonton, D. K. (1991). Career landmarks in science: Individual differences and interdisciplinary contrasts. *Developmental Psychology, 27*, 119–130.

Stern, N. (1978). Age and achievement in mathematics: A case study in the sociology of science. *Social Studies of Science, 8*, 127–140.

Szent-Györgyi, A. (1966). [Interview]. *The way of the scientist* (pp. 111–128). New York: Simon & Schuster.

Szilard, L. (1966). [Interview]. *The way of the scientist* (pp. 20–36). New York: Simon & Schuster.

Thomson, G. P. (1957). *The strategy of research*. Surrey: University of Southampton.

Watson, J. D. (1979). [Interview]. *The eighth day of creation*. H. F. Judson (pp. 44–45). New York: Simon & Schuster.

Yalow, R. S. (1986). Peer review and scientific revolutions. *Biological Psychiatry, 21*, 1–2.

Zuckerman, H. (1977). *Scientific elite: Nobel laureates in the United States*. New York: Basic Books.