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Comparative Failure in Science

A recent study shows that this is not incompatible with stable careers for basic research scientists.

Barney G. Glaser

A perennial problem for some scientists is their *feeling* of *comparative failure* as scientists. This problem becomes clearer if we consider two major sources of this feeling that are inherent in the very nature of scientific work. (*i*) In science, strong emphasis is placed on the achievement of recognition (*1*); (*ii*) the typical basic scientist works in a community filled with "great men" who have made important and decisive discoveries in their respective fields; they are the acknowledged guiding lights. These esteemed scientists, who have attained honors beyond the reach of most of their colleagues, tend to become models for those who have been trained by them or who have worked under them. As Eiduson has put it in her recent psychological study of basic research scientists (*2*, p. 167): "Scientists are idols-oriented."

To take these honored men as models is important for training as well as for a life in research. During training, one learns to think creatively. Emulation of these models results in the internalization of values, beliefs, and norms of the highest standard. This emulation of the great continues and guides the scientist in his research work, however individual in style his work may be.

But it is precisely here that a feeling of comparative failure may arise. In emulating a great man the scientist tends to compare himself with the model. He estimates how closely he has equaled his model in ability to adhere to high standards of research, to think of relevant problems, to create "elegant" research designs, to devise new methods, to write clearly, to analyze data. In addition, because of the strong emphasis on attaining recognition for research contributions, the scientist perhaps will compare his own degree of success with his model's to gauge how he himself is doing. In using the great man's achievements and the recognition accorded him as criteria, the scientist may be motivated to strive continually and unremittingly toward greater heights (*3*). On the other hand, he may see himself, over time, as a comparative failure for not having attained a comparable amount of recognition (*4*).

Eiduson brings out the dynamics of this problem for scientists (*2*, p. 189): "The model, then, is the ego ideal figure, who represents the ultimate position, and in fact, defines what a scientist should do, how he should think, how he should act. *By comparison, everything else is inevitably of lesser worth* [italics mine]. We have seen the way the scientists in this group rebuke themselves as they become old, distracted, sit on committees or government advisory boards, or become administrators—and thus move away from the ideal. From this picture it is obvious that the scientist is hard on himself. He has a built-in, clearly marked scalar system, along which attitudes and kinds of performances are measured. When he moves away and deviates from the pattern, he becomes a maverick, or a person who has tossed aside the flaming torch."

Average Success

With this problem in mind, I recently made a study of the organizational careers of basic research scientists, one purpose of which was to ascertain the consequences, for the scientist's career, of receiving or not receiving an average amount of recognition (*5*). At the time of the study, these scientists were employed in a government medical research organization devoted to basic research. This was a high-prestige organization from the standpoint of scientists and was run much as though it were a series of university departments. The study is relevant to this discussion in showing something of the career history of basic research scientists, who are today in

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increasing proportions leaving the university setting to become affiliated with high-prestige organizations devoted to basic research. In these contexts organizational scientific careers are still primarily dependent on professional (not organizational) recognition (*6*).

By "average amount of professional recognition" I mean supervisor's favorable evaluation of the quality of the scientist's current research, and proper credit, through publication and through acknowledgment in the publications of others, for his contribution to the cumulative knowledge in his field. This definition gives the three major sources of recognition within reach of the typical scientist: references from superordinate colleagues, publication, and publication acknowledgments in the work of others. This "average" degree of professional recognition is attained by most of the country's scientists at any one time and by practically all scientists at one time or another. This degree of recognition is in marked contrast to the highly regarded, and restricted, high-prestige honors (in the form of awards, prizes, grants, lectureships, professorships, and so on) that are part of the professional recognition accorded those scientists who make great and decisive discoveries—the "great men."

Three general aspects of scientists' careers were studied: performance; security in, and advancement of, position; compatibility with others, and satisfaction with one's location in science. With respect to performance, an average degree of recognition was found basic to high performance. That is, recognition maintained high motivation to advance knowledge, and high motivation resulted in the scientist's devoting more of his own time to research; this, in turn, resulted in high-quality scientific performance, as judged by the researcher's closest professional colleagues.

Since, of course, such performance on the part of many individuals is the basis of organizational prestige, it was not surprising to find the organization providing, in return, a stable scientific career for a scientist who received average professional recognition. The scientists accorded this degree of recognition, in contrast to those accorded less, felt more satisfaction in their jobs and salaries. They tended to be more optimistic about their chances of promotion, and their rate of promotion was higher. With respect to the conditions for research—a most important consideration for basic-research scientists—they fared considerably better than scientists not accorded average recognition. They had more freedom to work on their own ideas, had more chance for originality, had more chance to use their current abilities and knowledge as well as to gain new abilities and knowledge, and had generally better research facilities and supplies. In sum, the "average" recognition accorded them was sufficient to give them security and advancement in their scientific careers.

Lastly, with average recognition, the high-quality performance and steady advancement could be achieved in a setting that provided personal satisfactions. The scientists accorded average recognition, again in comparison to those accorded less, were more content with their research and non-research colleagues. More of them felt intense interest in working with close professional associates. They were more satisfied with their assistants and with the other scientists, the organization leaders, their own supervisors and the directors of their particular institutes. They felt strengthened through belonging to work groups, such as sections and laboratories. They depended more on personal contacts for scientific information, both inside and outside the organization. They participated more in seminars, meetings, and the activities of professional clubs and other small groups.

Closely linked with this compatibility with their associates was a satisfaction with their location in the community of organizations of science. The scientists accorded average recognition, in comparison to those accorded less, felt strongly attached to their respective institutes and organizations. Indeed, they were more satisfied with the organization's reputation in the scientific world, and more of them felt that a sense of belonging to an organization which had prestige in both the scientific and the general community was of utmost importance. In comparing their own organization (from the standpoint of what job factors they deemed most important) with the "best" of universities, hospitals, industrial research organizations, and government research organizations, more of them consistently reported that their organization was generally better. In sum, the context of their careers in science was highly favorable.

Together these findings suggest that an average amount of recognition has a generally stabilizing effect for the careers of the scientists within the high-prestige organization of the study. (Even for individuals who received little or no recognition, the pressure on careers was not so great as to cause an exodus from the organization or from science itself. The great majority of these men thought the lack of recognition was only temporary and planned to continue in the organization, trying to advance knowledge.)

These findings suggest that career stability based on average professional recognition is probably found in other organizations similar in nature to the basic-research organization of this study, and that in organizations of lesser standing even less recognition may assure career stability.

In the light of these findings it appears that the feeling of comparative failure that may result when the average scientist judges his lesser success by the considerable success of his "great man" model tends to occur in many instances within the context of a stable, promising career. Further, most scientists can gain, if they do not have it currently, the degree of recognition necessary for a stable career. Comparative failure, then, is an evaluation resulting from a social comparison. It is not to be taken as absolute failure (loss of position as a scientist). A comparative failure can still be successful; an absolute failure is through.

The Scientific Career: A Carnivorous God?

Comparisons with great men are, however, taken not as comparative but as absolute failure by Kubie in his famous article "Some unsolved problems of the scientific career" (*7*). Kubie warns future scientists of the perils ahead when devoting themselves to that "carnivorous god, the scientific career." His criteria in warning of potential failure, are absolute (not comparative) judgments, based on the careers of the more notable great men of science. For example, he talks of the "ultimate gamble which the scientist takes when he stakes his all on professional achievement and *recognition* [italics mine], sacrificing to his scientific career recreation, family, and sometimes even instinctual needs, as well as the practical security of money." Implying again that the scientist whose success falls short of the great man's is an absolute failure, he characterizes the young scientist as having "a self deceiving fantasy: that a life of science well may be tough for everyone else, but that it will not be for him," and as having "ambitious dreams; unspoken hopes of making great scientific discoveries; dreams of solving the great riddles of the universe."

Kubie states that the young scientist "dreams unattainable dreams." More directly relating his judgments to great men, he cautions against choosing science as a career, because of the "many failures it took to make one Pasteur." He states that most young scientists, in using great men as models, unwittingly set themselves up to become failures: ". . . most young men view their prospect solely by identifying with their most successful chiefs, never stopping to consider how many must fail for each one who reaches this goal." Without making the distinction between absolute and comparative failure, this last statement clearly implies the former.

Admittedly, from this standpoint many must fail and few will attain the stature of their models, but this is hardly a reason for dissuading young men from becoming scientists. The chance is slight that they will equal or surpass their models, but they should be informed that most can gain the fundamental degree of recognition indicated in my study as necessary for a promising career in science. Surely the career to which they commit themselves need not be, as Kubie says, "devoid of security of any kind, whether financial or scientific."

Furthermore, these young men should be encouraged to enter science and take great men as their models, for most will be the artisans who do the commendable, but not earth-shaking, research which accumulates to form the foundation for future decisive advances. Kubie himself has recently, although somewhat ambivalently, recognized this, in comparing the typical scientist with the internationally famous scientist (*8*): "These little known and unrewarded men are the expendables of science. They are no less essential than are the few who reach their goals. Therefore, until many years had passed it would be hard to weigh which of these two men had had the more profound impact on scientific knowledge."

Perhaps my discussion draws the kind of "implication" from "statistics" that Kubie is looking for in future research when he says in his article on the scientific career: "It is the . . . duty of scientists and educators to gather such vital statistics on the life struggles of a few generations of scientists and would-be scientists and to make sure that every graduate student of the sciences will be exposed repeatedly to the implications such data may have for his own future." Career decisions are perhaps among the most important determinants of a man's fate, and anything which contributes to an understanding of the career in science may help people make these decisions more wisely.

References and Notes

1. Merton accounts for this in the following manner: ". . . originality can be said to be a major institutional goal of modern science, at times, the paramount one, and recognition for originality a derived but often as heavily emphasized goal" [R. K. Merton, *Am. Sociol. Rev.* **22**, 640 (1957)].
2. B. T. Eiduson, *Scientists: Their Psychological World* (Basic Books, New York, 1962).
3. See O. Klapp, *Heroes, Villains and Fools* (Prentice-Hall, Englewood, N.J., 1962), pp. 18–24 for some functions of role models. I have reference to the function of "providing the individual with self-images and corresponding motivation."
4. In their comprehensive statement on careers, Becker and Strauss note the relative nature of failure: "Of course, failure is a matter of perspective. Many positions represent failure to some but not to others" [H. S. Becker and A. Strauss, *Am. J. Sociol.* **15**, 257 (1956)]. The relative nature of failure can be seen in marked contrast to its absolute nature when a person simply has failed to keep a position. On absolute failure, see E. Goffman, *Psychiatry* **62**, 451 (1952).
5. B. G. Glaser, *Organizational Scientists: Their Professional Careers* (Bobbs-Merrill, Indianapolis, 1964).
6. C. V. Kidd, *Personnel Admin.* **15**, No. 1, 16 (1952); W. Kornhauser, *Scientists in Industry* (Univ. of California Press, Berkeley, 1962), pp. 131–133.
7. L. S. Kubie, *Am. Scientist* **41**, 596 (1953); *ibid.* **42**, 104 (1954) [reprinted in M. R. Stein, A. J. Vidich, D. M. White, *Identity and Anxiety* (Free Press, New York, 1960) and in B. Barber and W. Hirsch, *The Sociology of Science* (Free Press, New York, 1962)]. The remarks by Kubie are based on 30 years' observation. He sees these observations as "random," but their consistently negative character suggests that, by and large, they are observations of his analysands and are random only in that context. My references are to but one short section of an excellent article.
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News and Comment

JPL: Ranger VI Failure Increases Speculation on Jet Lab's Future Links with Space Agency, Caltech

Pasadena, California. The Jet Propulsion Laboratory (JPL) here, which is NASA's chief agent in the unmanned exploration of the moon, the planets, and interplanetary space, has lately been having some trouble with both its spacecraft and its image.

The Ranger VI spacecraft, which suffered an unscheduled TV power turn-on about 2½ minutes after it was launched and no turn-on in the crucial moments when it was approaching the moon, followed a series of five previous Rangers which had also encountered mishaps, although some of the earlier failures involved troubles with launching vehicles and guidance systems rather than the spacecraft themselves.

Ironically, Ranger VI appears to have performed its extremely difficult assignment admirably up until the big moment when the TV cameras were supposed to start sending back pictures of the lunar surface. No matter how near the miss, however, JPL's bad luck with the Rangers has to some extent diverted attention from the triumph of the Mariner II spacecraft fly-by of Venus and earlier achievements of JPL and California Institute of Technology, which manages the laboratory as a nonprofit institution.

NASA director James E. Webb, in a long Washington press conference devoted in substantial part to the unmanned program, made some remarks about the necessity of providing "a strong, hard-headed, industrial type of management of programs" for JPL. Newspapers in Southern California played up Webb's implied criticism, causing speculation about future NASA-JPL relations and anxiety among JPL wives.

Webb himself and Caltech president