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Comparative Failure in Science[1]

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Besides degree of clarity, another aspect of a demotion is the relative failure which it indicates. The clearer the forms of comparative failure, the more painful they are likely to be. However, a certain degree of failure may indicate nothing more than the lack of outstanding success, while indicating moderate success.

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 vary nature of scientific work. (i) In science, strong emphasis is placed on the achievement of recognition; (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 acknowledge 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 "Scientists: are idols-oriented."

To take these honored men as models is important for training as well as for a life of research. During training, one learns to think creatively. Emulation of these models results on 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

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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 towards greater heights. On the other hand, he may see himself, over time, as a comparative failure for not having attained a comparable amount of recognition.

Eiduson brings out the dynamics of this problem for scientists:

> 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 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 basic 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: At the time of this 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 affiliated with high-prestige organizations devoted to basic research. In these contexts organizational scientific careers are still primarily dependant on professional (not organizational) recognition.

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 acknowledgement in the publications of others, for his contribution to the cumulative knowledge in his field. This

definition gives three major sources of recognition within reach of the typical scientist; references from superordinate colleagues, publication, and publication acknowledgements 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 felt 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" 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 an 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 "Some Unsolved Problems of the Scientific Career." 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 and 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 the prospect solely by identifying with the 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 man 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 the earth-shattering, 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. "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.

Research on Comparative Failure in Science

While it is possible to be a comparative failure in virtually any occupation, the chances of becoming one are built directly into any occupation of scientist. Yet little or nothing is known about this area of comparative failure. Therefore, I wish to discuss a few of the properties of comparative failure that may be

useful for guiding exploratory research. To be sure, the most important and meaningful properties of this problem area are yet to be discovered. Since comparative failure is based on some social comparison, statements about it must always take into account a reference criterion. In this study, the reference individual is the "great man" model in science, and comparisons are based on his degree of success. The criteria for this invidious comparison must be established empirically for any occupation within its particular situational context. The problem of comparative failure may be seen as a specific one within the more general area of comparative reference group theory.

The relative nature of comparative failure is in marked contrast to the absolute nature of failure wherein one cannot hold an occupational position and therefore must leave it. Comparative and absolute failures vary independently. A person who is an absolute may or may not be a comparative failure. It is true that a scientist forced out of science because of mediocre work will probably feel himself a comparative failure with reference to his many former colleagues and models. However, if he can leave these comparative reference individuals behind during the "cooling-out" process attached to his loss of occupational position, he may not at all feel himself a comparative failure. More simply, if he realizes from his absolute failure that he was not cut out to be a scientist, then evaluations of comparative failure become superfluous and may not persist. Again, comparative failure can unnecessarily cause absolute failure; that is, cause a scientist to leave his profession or to stumble along feeling that he is an absolute failure.

Comparative failure takes on more strategic meaning when no absolute failure is involved. A major source of comparative failure is, of course, demotion or downgrading of one's rank in the occupation. This topic has been analyzed quite nicely for business and industrial organizations. It had yet to be considered for scientists. Another general source of comparative failure appears when an important reference individual outstrips the scientist. Statements about colleagues, such as "He advanced very quickly," can mean that the speaker was probably left behind with a lesser degree of success. Practically all scientists have classmates and colleagues who have been far more successful.

Another interesting and significant source of comparative failure was brought out in this volume. With reference to the

"great man" model, the typical scientist can have an objectively satisfactory career and yet feel a comparative failure! If this feeling persists, he may be oblivious to the stability of his promising career. If he actually lacks the fundamental range of recognition, and further, feels that his chances of gaining some recognition are hopeless when compared to his model, in focusing on the unattainable he is likely to ignore the general possibility of having a fruitful (if lesser) career by achieving the degree of recognition within reach of his ability.

The adjustment to comparative failure involves a "cooling down" of aspirations for success so as to be in accord with one's research ability and career prospects (rather than a "cooling out," as Goffman has described it for absolute failure, when one must give up all those aspirations linked with an occupation or position). The scientist as comparative failure does not have to go through the often very painful process of giving up commitment, involvement, and investment in his chosen profession. He need only set his achievement and career sights at a lower level than those of his models. Indeed, his models, who provide the basis for an unfavorable comparison, also provide a perspective on just where to see hopes for a career in science. This perspective is denied the absolute failure, since he is going nowhere.

Various aspects of both the scientist and his "great man" model will affect judgments of comparative failure. Aspirations for a success similar to the model's probably are highest immediately after graduate school, but they diminish as the scientist takes his own and his model's measure during the years of research maturity that follow. This change most likely will vary according to the age at which the model did outstanding research; in some fields, great men appear at an earlier age than in others. If he is in a field in which great discoveries come early, the scientist may feel that he is "through" soon after he has begun. If the discoveries come late, then he may believe he has ample time to equal his model - and his feelings of comparative failure may emerge much later.

The type of models that a scientist takes will also vary according to his age. The young scientist usually is focused on his classmates, equal colleagues, teachers, his work from supervisors, and current great men in his field. Hence, he has many reference individuals on which to base a feeling of comparative failure before he cools down his aspirations. In later years, as these

models no longer loom so large, the more successful scientist may switch to the non-living immortals of his field. Indeed, although a scientist may have become a great figure, he may still evaluate himself strongly as a comparative failure, judging himself in relation to the immortals he aspires to equal. Notwithstanding the many useful consequences of taking immortals as models, it is quite possible that comparative failure can be even more intense for a contemporary great man whose aspirations (in contrast to those of the typical scientist) may have been sharpened, not dulled, on the grindstone of experience. In other words, not only may a comparative failure be successful, but the most successful scientist may be the most intense comparative failure. Thus, comparative failure may be more pronounced among the beginning scientists and current great men than it is among others.

It should be noted also that I have dealt only with comparative failure as self-evaluation, not as other-evaluation. The latter also represents an area for research in the sociology of science. For example, a strategic aspect of advancement in the scientific career is the comparative evaluations given by the scientist's referees. These are speculations. Only future research can tell us of the processes leading to comparative failure; of its incidence at various stages of an objectively successful career; of the "cooling down" mechanisms by which scientists cope with such a feeling and are helped by others to cope with it: of its effect on creativity, motivation, and partial retreatism; and of its effects on absolute failure and organizational turnover.