The Impact of Rapid Discovery upon the Scientist's Career
Author(s): Fred Reif and Anselm Strauss
Source: *Social Problems*, Vol. 12, No. 3 (Winter, 1965), pp. 297-311
Published by: Oxford University Press on behalf of the Society for the Study of Social Problems
Stable URL: https://www.jstor.org/stable/798935
Accessed: 08-12-2019 01:06 UTC

goes to all of the top places so she meets the top people. . . .

The fact that the enactment of the call girl role requires little training, and the introduction of the girl to clients and colleagues alike is rather rapid, gives little time or incentive for adequate occupational socialization. It is perhaps for this reason rather than, for example, reasons related to personality factors, that occupational instability is great and cultural homogeneity small.

In closing, while it appears that there is a rather well defined apprenticeship period in the career of the call girl, it seems that it is the secrecy rather than the complexity of the occupation which generates such a period. While there is good evidence that initial contacts, primarily with other "working girls," are necessary for entrance into this career, there seems no reason, at this point, to assume that the primary intent of the participants in training is anything but the development of an adequate clientele.

THE IMPACT OF RAPID DISCOVERY UPON THE SCIENTIST'S CAREER

FRED REIF
University of California, Berkeley
AND
ANSELM STRAUSS
University of California, San Francisco

One of the most striking features of present-day science is the rapidity of scientific discovery, verification, and technological exploitation. This rapid rate of change is intimately connected with other outstanding characteristics of modern science, its bigness, visibility, and increasing professionalization. The number of men engaged in research is so large and the organizational framework so efficient that new fields get explored and developed with great speed. Thus Sir Neville Mott, talking about research in the field of semiconductors at an international conference on this subject in 1962, remarks: "These problems will hardly last us until 1970. . . . You have only to look back to the 1950 conference at Reading to see how rapidly we work out these problems."[1] While individual scientists still continue to make the major discoveries, the rate at which they do so has been enhanced not only by the existence of well-developed bodies of scientific theory, but also by the emergence in the realm of science of the kind of organization of men, technology, and capital investment so characteristic of the last decades.

This very organizational framework, while unquestionably furthering discovery, can however mitigate against the continuing creativeness of individual scientists. Men in many fields have complained about the restrictive aspects of working within the context of contemporary large-scale research organizations, or have murmured against the heavy administrative and consultative tasks into which cre-

[1] N. F. Mott, Report of the Int. Conference on the Physics of Semiconductors (Exeter), Institute of Physics, London, 1962, p. 3.

ative scientists are frequently drawn. The most creative years of research seem increasingly to belong to the young men . . . so much so that a recent editorial in *Science* called for "revitalizing the mature scientists."[2] Thus it appears that the scientist who wishes to keep contributing to the impressive rate of important discovery is operating against appreciable odds quite apart from his own biological slowdown.

Among the greatest of these odds, we shall suggest in this paper, are those stemming from the rapid rate of discovery itself—particularly in fields like physics, chemistry, and modern biology where recent activity and growth have been most pronounced. Rapid change may create situations of considerable strain for individual scientists. We shall discuss the areas where such strains are particularly manifest and consider their impact upon the scientist's career. Our discussion will deal mainly with the elite of science —those who make major fundamental discoveries—and will focus attention particularly upon those critical periods in the scientific career which are characterized by greatest personal strain and greatest hazard for scientific work itself. We shall then examine how scientists wishing to remain productive in research cope with the pressures to which they are subject, and how they adapt to their changing situations. Finally, we shall make some comments about the influence of rapid discovery and change upon the institution of modern science itself and about possible implications for its future.

THE EARLY RAPID PERIOD OF TRIAL

The admission to a graduate school is the first critical hurdle encountered by the prospective scientist. He begins early to enter upon a period of intense trial during which his future as a creative scientist, if he aspires to become one, is at stake. This period comes to an end only when, a few years after obtaining his Ph.D., it has become apparent to himself and others what his potentialities are likely to be and in what kind of working environment he is likely to spend his most productive years.

While a graduate student, he recognizes quite clearly that he has already embarked upon a period of trial during which he must prove that he has what it takes to become a good scientist, and to become accepted as such by people who, as yet, seem remote and formidable by virtue of their knowledge, experience, and reputation. Of course, he has the advantage of youthful vigor and of recent training. But this kind of advantage is rather abstract. The student is still faced with the concrete task of discovering his own abilities and getting them recognized in some tangible form. Even in graduate school his work is scrutinized. This process continues, of course, during the crucial years after his graduation until he can prove himself through significant contributions and get himself accepted by the rest of the scientific community.

Yet this process is not just one of growth and of the individual's "finding himelf," for the pressures of time are severe and are felt to be such. The opportunities for upward mobility are quite high today, as organizations compete for the relativly small number of competent scientists. Thus bright students can, only a few years after obtaining their Ph.D., become sufficiently well known by their work to obtain faculty positions at major universities or to figure prominently in scientific conferences. The fledgling scientist is therefore likely to see himself surrounded by people, only a few years older than himself, who have been very visibly successful. If he wants to "make good" and to follow their example, he feels that he has

[2] P. H. Abelson, *Science*, 141, 597 (16 Aug. 1963).

only a short time within which to prove himself.

The pressure of time looms also in a somewhat larger context. The ambitious young scientist is likely to perceive that achievement in science tends to occur at an early age and is concerned that he himself be productive before it is too late.

> Age is of course a fever chill
> that every physicist must fear
> He's better dead than living still
> When once he's past his thirtieth year.

This quatrain, attributed to the famous physicist Dirac,[3] expresses that kind of feeling in somewhat extreme form perhaps. But it is true that the aspiring young scientist does compare himself with role models many of whom have shown such early achievement. The young scientist may never have read Lehman's study[4] which shows that past generations of chemists did their most notable creative work between the ages of 26 and 30, physicists and mathematicians between the ages of 30 to 34, and geologists, bacteriologists, and physiologists between the ages of 35 to 39. But he is likely to have come across remarks of the type illustrated by the following comment in a textbook on quantum mechanics:

> Bohr was twenty-eight years old when he published his first paper on the theory of the hydrogen spectrum. In fact, the quantum theory has been from first to last a development by young men. Einstein . . . wrote his initial paper on the corpuscular theory of light at the age of twenty-six. Heisenberg was twenty-four years old when he laid the foundation of the matrix mechanics. Dirac and Jordan wrote their first important papers at the ages of twenty-four and twenty-three respectively. . . . Of course, the contributions of older men . . . have been exceedingly valuable, but one cannot but be impressed with the importance to science of a system of education which enables young men to finish their preliminary training and start their career of productivity while the extraordinary mental energy of youth is still in full vigor.[5]

Not only is the young scientist aware of the illustrious early achievers of the past; he encounters them very directly in the present. For example, it is only natural that his attention should be attracted by recent Nobel prize winners. During the past half dozen years, their mean ages at the time of the award have been between 45 and 50 years in physics, chemistry, and medicine.[6] But the young scientist is likely to note that some men were in their late twenties when they did the outstanding work leading to subsequent awards of the prizes; indeed, their work was done at an average age of less than 35 years.

Thus the young scientist feels that he must prove himself rather quickly. He has also learned to appreciate the importance of prestige in scientific work and he knows that he must gain recognition early to have a successful scientific career. The prestige he can command will bring him a host of extrinsic rewards. It will determine the positions he will be offered; the monetary income he can expect; the funds he will obtain to support his research; the students he can attract; the fellowships or awards he might receive; and the power he is likely to wield in his field, university, or larger community. At the same time, his prestige in the field will also have great intrinsic importance to him because it will help to determine his own feelings toward himself, his sense of self-esteem and self-perception as a success or failure.

The task thus faced by the aspiring

---

[3] Quoted in R. Jungk, *Brighter than a Thousand Suns*, Harcourt, Brace and World, Inc.; New York, 1958, p. 27.

[4] H. C. Lehman, *Age and Achievement*, Princeton Univ. Press; Princeton, 1953.

[5] E. C. Kemble, *The Fundamental Principles of Quantum Mechanics*, McGraw-Hill; New York, 1937, p. 2.

[6] L. Moulin (British Journal of Sociology, 6, 246 (1955)) finds similar mean ages for the Nobel prize winners up to the year 1955.

scientist is not an easy one, and the pressures felt by him are likely to be severe. His self-expectations are in large measure based on the role models which he has encountered. Since these models derive from outstanding scientists, his self-expectations can be quite demanding indeed. The young scientist also knows that achievement in pure science is characterized by the property that the best is very much better and more important than the second best. The best is also much more attention attracting; i.e., it contributes much more to one's prestige. Thus it is not really sufficient for the scientist to be merely competent. Although in more professional pursuits, such as some branches of engineering, a highly competent carrying out of daily tasks may result in very useful and respected accomplishments, this is not enough at the levels of scientific activity where originality is the essence. If he wants to be successful, the scientist cannot lapse into routine activities, but must continually strive to work near the limits of his capacity. His awareness of this state of affairs is accentuated by the knowledge that there exists a relatively small elite group of scientists who are responsible for most significant advances in his field and who command an accordingly disproportionate amount of prestige.

Quite naturally, the ambitious young investigator aspires to enter this elite. But, even if he should set his sights lower, there exist powerful outside pressures which insist that he be creative . . . and quickly. In earlier times creative originality was appreciated as a quality possessed by a few gifted individuals and it was recognized and rewarded as such. But in the post-war era the growth of science and technology have led to a situation where the demand for scientific creativity has also become institutionalized in ideological form. Creativity is commonly *expected* by employing institutions, whether industries or universities, because they are concerned with their own technological productivity or because they themselves gain institutional reputations through the productivity and prestige of their employees. Hence the employees are very aware that their overt creativity gets regularly assessed and rated on the basis of published papers or other measurable output. At the same time, the subject of creativity itself has become popular. It gets discussed at symposia and is studied in specialized research projects supported through industrial or government interest. It has become part of a new concern with highly skilled "manpower." Thus the young scientist cannot fail to be aware that he will work in a setting where he will be *expected* to be creative.

Quite apart from ideological considerations, there are very concrete conditions, inherent in the very structure of organized science, which necessitate the quick demonstration of talent. The expansion of science has given rise to a genuine scarcity of high caliber people to fill jobs opening up in industrial establishments and universities. There exists, therefore, considerable competition between the purchasers of scientific talent. To satisfy their needs, these institutions must be prepared to risk hiring young men whose promise is barely established. Yet they must select wisely; hence they look to the first rate university departments as the most likely source of low-risk recruits. In consequence, the young scientist's future career possibilities are already appreciably determined on the basis of the evaluation of his *pre*-doctoral work. Thus the scientist's critical trial period is initiated quite early and tends to be brief and intense. Metaphorically, the young man steps early onto a quickly moving career escalator. Starting from his entrance to graduate school, it may take only six to eight years to determine the kind or level of position the scientist is likely to occupy for the rest of his active career.

This situation is not without its dangers. It was already pointed out that employing organizations are interested in hiring young people of promise who are likely to grow in stature and to have many productive years ahead of them. They are also eager to attract older people with well-established reputations who are sure to provide leadership and prestige to the organizations. They are, however, much more reluctant to hire people who do not fit into these categories. If a scientist gets to be much older than 30 years, he is really too old for an assistant professorship. He has been out of school for too long, the salary provided for that rank seems too low to be appropriate, and he can no longer be assessed purely on the basis of promise. On the other hand, if his accomplishments of the preceding five years have not been reassuringly substantial, it is risky to appoint him to a permanent associate professorship. In short, since creativity has come to be demanded by institutions, there also exist well-defined expectations as to the level of accomplishment deemed appropriate at various age levels. The scientist must, therefore, meet these demands at the appropriate points in his career. Once he gets off the career escalator, he may miss opportunities which are available only at certain critical stages and without which it may prove difficult to scramble back onto the escalator.

The young scientist runs thus a real risk of getting started on the wrong foot or of getting out of step with career demands. He may do his graduate work in a second rate department and thereby diminish his chances for top positions. He may, even in an excellent department, choose his field of research unwisely, such as one which is falling out of fashion. He may imitate role models whose examples can only harm his own trial performances; for example, he may imitate men who are very careful in their work and may thus be led to publish very slowly by virtue of being overly cautious. He may be "scooped" in his early work by the competing research of more established men.

Since the young scientist realizes that he is in the midst of a fatefully short trial period during which he must prove himself, he tends to adopt tactics appropriate to the situation. Thus he tries to reduce the hazards of his decisions by hedging his bets. He is inclined to trim his sails toward the winds of caution and fashion; he is wary of attempting research that could be brilliant but might not pan out; and if he does attempt it, tries to carry on also some "bread-and-butter" work which is sure to lead to some kind of publishable contribution. As Harrison Brown has said:

> Pressures to publish force young scientists to pursue the obvious. That which is not obvious is risky—one might spend years without obtaining publishable results. . . . The fact remains that a published but trivial article is often valued more highly in university circles than the unpublished groping for an understanding of a particularly subtle facet of nature. . . .[7]

Those who do not emerge from the trial period with flying colors become the "journeymen of science." No denigration is implied by that term. A great deal of very competent and necessary work gets done by men who have failed to gain outstanding or brilliant reputations. In this respect, too, the graduate schools help to perform an important function: they help to channel these men into appropriate institutional settings. There some of them also become, at relatively early ages, scientific administrators who supervise the work of other highly competent journeymen and of less skilled scientific technicians.

[7] H. Brown, *International Journal of Science and Technology* (Oct. 1963) p. 113.

THE YEARS OF MATURITY

Assuming that the scientist has succeeded in becoming recognized as having genuine promise, we now wish to examine the effects of the current rapid rate of discovery upon his continued productivity. The scientist entering this stage of career is likely to conceive that he has only a limited number of years to do his best work, since the most original research seems associated with youthful minds. His anticipation tends to be progressively aggravated by the need to keep up with scientific fields which are advancing very rapidly. Thus it takes very much work just to keep up with the field; the task of staying in the forefront is correspondingly more difficult. The scientific literature being published has become overwhelming. New advances are made at a rate such that five years may see significantly new methods and techniques introduced into a field. Neighboring disciplines also tend increasingly to intrude themselves, particularly in the most active areas of research. For instance, experimental physicists in solid state physics have discovered that they must become familiar with some of the abstruse mathematical formalism of quantum field theory to understand the recent literature pertinent to their field. Thus the demand made on the individual scientist's adaptability is both great and continuous.

> This is a terrible problem for all of us, for all the professional scientists. It has become essentially impossible for physicists to keep up with the science of physics, let alone with the sciences of biophysics, chemistry, astronomy, and other neighboring sciences. There is just too much. . . .[8]

Similarly the editor of *Science,* describing some problems of the mature scientist, makes the following comments:

> Of the many factors which combine to diminish creativity in the maturing scientist, perhaps the most important are decreased motivation and obsolescence of his personal store of knowledge. . . . When the important problems are solved, a scientist should seek new interests. . . . He will realize, however, that his store of up-to-date knowledge may be smaller than that of graduate students. . . .[9]

This last point is well taken. The younger scientists are in many ways better trained to tackle new areas. They have been exposed to the latest points of view presented by various specialists, and now have a chance to synthesize all this information in pursuing the newer lines of research. The older scientist may have the advantage of long experience, but his continuing process of self-education is more likely to be piecemeal and limited to the confines of his immediate working interests. It is not uncommon for him to realize that his students may know more about many areas of his wider field than he does. In the words of Hans Bethe:

> . . . It is true that the younger people have more flexible minds and may be a bit more advanced. . . .[10]

Somewhat similar thoughts are vividly expressed by another scientist:

> Young scientists and young engineers eat up old scientists and old engineers. . . . Well, they beat them down . . . you don't remain a scientist in the active, progressive sense of the word for a great many years any more.[11]

Nor is it uncommon to find a scientist, still doing research but with his earlier excitement and productivity gone, speak of his

> . . . feeling that I'm worn out or burned out, and this is the end of my career. . . . I've some feelings of responsibility I wish I could get rid of, that I ought to be doing something.

---

8 Hans Bethe, from an interview with D. McDonald, Center for the Study of Democratic Institutions, 1962, p. 20.

9 Abelson, *op. cit.*

10 H. Bethe, *op. cit.*, p. 22.

11 R. C. Raymond. From an interview published in *International Journal of Science and Technology,* p. 50 (July 1962).

> It drives you into a locked position where you just can't do anything right.[12]

Since the complexity and volume of scientific activity is increasing, there exists, of course, a natural tendency to become expert in some smaller subfield. But specialization, although effective for keeping up with developments in a particular area in which one can be productive, involves a significant danger. In an age when science is expanding rapidly and is pursued by an increasing number of investigators, the continual emergence of new discoveries leads to fashionable lines of research which attract much attention before they are replaced by still newer developments. Thus the specialist who works in a certain field for some years may discover one day that he is no longer in the mainstream of scientific activity. There may be no dearth of unanswered questions remaining in his field, but these have become of minor importance as the center of gravity of the larger science has moved elsewhere.

There arises then the question of whether the scientist will continue to explore his chosen area or shift his research into newer directions so as to participate in more glamorous current developments. This question raises several issues. The scientist may be unable to shift his activities readily because of his existing commitments; or because his knowledge and training, although ample in his own area of specialization, are inadequate to make readily possible the transition to new lines of investigation. Furthermore, new developments take place constantly and are generated by new groups of people. How often and to what extent can the same scientist shift his own field of activity accordingly? Or at what stage does he resign himself to pursue his own specialty—even if it has become more remote from the mainstream of his science—and become content to watch younger people pursue the new areas?

The rapid developments result in a situation where the scientist's relation to his work changes not only by virtue of his own individual growth and aging, but also because the field in which he is active changes rapidly under his very feet. It has become increasingly difficult for an individual investigator to remain identified with a particular line of work for a long time in a satisfying way. He may start out in a field which is new, active, and exciting. Yet, after a few years, its essential features are understood, it has become systematized and more highly structured, and its problems are more prone to have acquired technological importance. "It is inevitable that most of the important problems in any field of research are soon solved, leaving those which are trivial or intractable."[13] Under these changed circumstances the scientific field is no longer congenial to some of its best workers who, individualistic and endowed with creative active minds, were particularly attracted to this field a few years earlier by the prospects of exploring new frontiers. The changed situation can lead to appreciable personal dissatisfaction and demands a readjustment of the scientist's attitude toward his work.

In a recent talk a well-known physicist has vividly described some reactions of a creative scientist to solid state physics, which has now seen some ten or fifteen years of active development:

> . . . We can see in Rutherford a symbol of the golden age of physics to which we of the silver age are tempted to look back with nostalgia. . . . It must indeed have been exciting to work . . . in those days when . . . every experiment might reveal a new phenomenon, every calculation dispel a new mystery. . . . The era of the great breakthrough is over . . . our physics

---

[12] B. T. Eiduson, *Scientists: Their Psychological World*, Basic Books, New York, 1962, p. 92.

[13] Abelson, *op. cit.*

> . . . now presents an orderly structure, with a great deal of detail to be filled in, but with no reasonable chance of being overthrown by any later discovery. . . .[14]

Indeed, he goes on to say that with all the industrial laboratories

> assiduously doing research, ten years is going to see the end of our games as pure physicists, though not as technologists. . . . What problem is going to stand the hard pounding that industrial research organizations are prepared to give to anything that looks like yielding cash or credit? . . . These examples reveal another phenomenon: the hordes of eager workers who rush in to tear the guts out of a problem. . . . The average first-class man isn't going to waste his powers on tidying up a little corner of science whose principles are well established. . . . What will happen when the cake is gone? . . . My guess is this: rather suddenly there will be a reaction among the most intelligent of the young away from our sort of physics as a career. . . .

But a field changes not only intrinsically in subject matter as the rich mines of potential discoveries become exhausted; its development also tends to affect its entire character in more extrinsic ways. Often it may come to involve large scale operations, team research, and the administrative responsibilities of handling big groups and budgets. It is also likely to become more relevant technologically, to induce many industrial laboratories to work in competition with academic investigators, and to attract new groups of people with values and temperaments significantly different from those of the earlier workers. Such extrinsic changes may make the field less congenial to some scientists originally active in it and may require readjustments to a significantly different situation. Thus Szilard, who left physics for biology, comments:

> . . . Physics has had a change of character. The interesting portions of physics have moved to higher energies where you have to have a Committee and Planning and getting the Machine and getting the Money for the Machine and the Committee deciding which Experiment should be done first. . . . I like the physics where I can think up an experiment today and do the experiment tomorrow.[15]

Similarly Hofstadter, a Nobel-laureate active in high energy physics, has described the trend of his field toward an organizational structure involving large apparatus and big staffs:

> An organization with this character exposes a new dimension to the life of the scientist working in it. . . . A physicist working with such a machine must become a politician as well as a good scientist. . . . The joyous feeling of individual accomplishment is no longer attainable, and many of our best young scientists are avoiding this type of research.[16]

Let us briefly summarize the main points of the preceding discussion. Rapid changes in his science tend to make an individual's knowledge obsolete and his field of specialization outdated; these trends aggravate his personal aging and natural loss of intellectual vigor. The whole character of a particular field of science is also likely to change appreciably during a time which is appreciably shorter than an individual's normal career span; the field may thus become less congenial to his particular temperament and set of skills. Nevertheless, the demands for recognition-producing work on the part of the individual remain high. These demands are generated from within, as a result of acquired norms and internalized role models; and from without, as a result of the institutionalized expectation of creativity existing in present-day science. At the same time, the rapid expansion of scientific activity generates a market for people to fill new roles, either within the field proper, as supervisors

[14] A. B. Pippard, *Physics Today*, 14, 38 (Nov. 1961).

[15] L. Szilard, interview in *International Journal of Science and Technology* (May 1962) p. 64.

[16] R. Hofstadter, *International Journal of Science and Technology*, p. 72 (August 1692).

or administrators, or in business or government. The cumulative effect of all these pressures tends ultimately to bring the scientist's productive research career to an end.

REMAINING A RESEARCH SCIENTIST

The preceding description has deliberately been somewhat overdrawn to highlight the difficulties confronting the scientist who wishes to remain productive in research. It becomes then of interest to examine the conditions under which he may continue to be productive, and the various ways in which he may cope with the pressures impinging upon him. In this context it will be helpful to differentiate between the situations facing technicians, journeymen, and elite.

Most persons trained in scientific fields are absorbed into the many positions created by the expansion of science and technology. They fill the demands of industrial and government laboratories for highly skilled technical personnel, and contribute importantly to technological advancement. For many, scientific research is a "job" not significantly different from other high level occupations, a way of earning a living which offers money, prestige, and satisfactory working conditions. Some entered their chosen field predominantly for such reasons. Others, having come to realize their own limitations of ability and motivation, have only later resigned themselves to regard their work as a job rather than a calling. They have thus relinquished the self-concept of the scientist as an original discoverer.

> A large number . . . take up scientific research as a career these days, but regrettably few of them are impelled into it by a passionate curiosity as to the secrets of nature. For the vast majority it is a job like any other job. . . . Indeed the relative security and stability of the research career are probably more attractive to mediocrities than the romance of inquiry is to the brilliant ones. And without this *great intellectual proletariat of research* [italics ours] how far should we get?"[17]

For most technicians and technologists there is neither much opportunity nor disposition to abandon the kind of research in which they are active; nor are they particularly sensitive to the types of pressures depicted earlier.

Let us now turn our attention to those journeymen scientists who are engaged in genuinely creative research. Most are not really outstanding, even though their work is highly competent and useful. "The large majority of scientists . . . have little prospect of great and decisive originality. For most of us artisans of research, getting things into print becomes a symbolic equivalent of making a significant discovery."[18] Most do not belong to the very small elite responsible for major breakthroughs in their field, and may never have expected or aspired to become members of this elite. Some, however, even when overtly quite successful, may view themselves as comparative failures since they are frustrated in their aspirations to emulate role models. Yet, moderately satisfying and creative work is open to them, and they adapt to the situation by striving for average recognition rather than for outstanding recognition.[19]

Indeed, this average level of competence is the norm in modern scientific research. Although the brilliant do get rewarded, most institutions of modern science are geared to the merely competent . . . and *average* recognition is relatively easily attainable. There *is* a large demand for research activity and for persons capable of training additional scientists; and financial support is given even to routine research. Work which is compe-

---

[17] From a letter by J. Gillis in *International Journal of Science and Technology*, p. 72 (July 1962).

[18] R. K. Merton, *American Sociological Review*, 22, 654 (1957).

[19] B. G. Glaser, *Organizational Scientists: their Professional Careers*, Bobbs Merrill, Indianapolis, 1964.

tent and sufficiently voluminous *does* attract the attention of scientists of similar competence, even though it might be ignored by the top elite. Thus the editor of *Chemical and Engineering News* comments:

> . . . the chances for widespread high recognition for scientific work are fewer relative to the number of scientists. Even as the number of available accolades has grown, the unique distinction of each thereby has tended to decrease. . . . It is perhaps worth some effort to install in young scientific scholars . . . the importance of the evaluation accorded by one's most respected peers of close scientific acquaintance. . . .[20]

On this more restricted scale, the journeyman can thus attain a fair amount of prestige and all of its associated rewards.

This kind of career is quite stable. Once the scientist has acquired a certain amount of recognition and a reasonable position, there are many factors which reenforce further pursuits along similar lines. Suppose, for example, that after a few years the scientist finds himself dissatisfied with his own accomplishments; or feels his field of specialization is becoming overworked and sterile; or would like to work at the forefronts, but finds his store of knowledge inadequate. Under these circumstances, his most likely course of action is to adapt as best he can by remaining in the situation, for there are very many factors in the institutional framework which encourage preservation of the status-quo. The scientist has a large investment in his extensive training. He has also spent quite a few years viewing himself as a research scientist pursuing certain lines of work, and it is not easy to change his self-concept, role-models, and values by turning to activities further removed from his science. Furthermore, the institutionalized nature of modern science makes for much inertia. There is likely to be some organization, consisting of the scientist's graduate students, postdoctoral fellows, and technicians, which implies a host of commitments. A continuing stream of students wishes to do research leading to a Ph.D. degree. They need to be given suitable research problems, and the most likely problems are along the lines of previous work. A similar situation prevails in obtaining contracts or grants. It is necessary to write contract proposals specifying the work to be done. Once again, the easiest thing to propose, and the one most likely to lead to the award, is research along lines similar to previous work. If the scientist really wanted to take time off to supplement his training and to pursue new work, he would face a much more difficult task.

On the other hand, the status quo, even when no longer very satisfying, can yet lead to many satisfactions. A secure established position offers many rewards and, even when research has become more sterile or less congenial, there are usually many ways of keeping busy fruitfully. Although a field may have become less exciting, considerable prestige accrues to the person who is expert in it. Indeed, specialization in some narrow area is one of the most successful ways of adapting to a rapidly changing science and to the inability of keeping in the forefront of knowledge. It makes for recognition, since requests for review articles, lecture invitations, and consultant positions are likely to be addressed to the narrow expert whenever his field seems pertinent in any other context. On the other hand, in an age of increasing specialization, a scientist whose work spans a large variety of subjects runs the distinct risk that no one group of scientists will view his whole work and thus recognize his merits consistently.

The elite scientist is in a somewhat more hazardous situation. Endowed with an active curious mind and im-

[20] Richard L. Kenyon, *Chemical and Engineering News,* 11 (Nov. 1963), p. 7.

bued with the self-concept of the creative scientist, he is particularly prone to feel dissatisfied when he finds his specialization running dry of important problems. He may, of course, be willing to settle for research activities less brilliant than those of his youthful years. Indeed, he may often follow this course and carry on his work as a highly gifted journeyman in a field which has lost its glamour and excitement. There is, however, another road potentially open to him: he may change his research significantly. If he switches into a relatively new and unexplored field, he is more likely to make important discoveries and to deal with questions of fundamental significance than in his own more fully worked-over—and possibly highly competitive—field. Furthermore, he may bring an unusual background of experience which may lead to fruitful originality in the new field. Lastly, fields differ in intrinsic difficulty and in the relative importance of intellectual brilliance versus accumulated experience in bringing about useful contributions. Thus a scientist may prolong his useful creative life by switching to a field which is somewhat less demanding and where the age of peak productivity tends to be greater than in his original field.

By such strategies, intellectually curious and adaptable scientists may attempt to prolong their productive careers. Indeed, in the recent issue of *Science* mentioned before, the editor makes a powerful plea for "finding better mechanisms for revitalizing mature scientists." After discussing perceptively some of the problems of maintaining individual creativity, the dangers of specialization in fields which become less fruitful, the "decreased motivation and obsolescence of the scientist's personal store of knowledge" and consequent increased readiness to accept administrative responsibilities, the editor to say:

> Many scientists would prefer . . . to reestablish their creative potential. What they need is an intellectual renaissance. This might take the form of comprehensive refresher courses followed by a dignified apprenticeship. . . . Industry, government, and the academic world should . . . establish mechanisms to enable him to do what is necessary to extend his creative life.[21]

The need for expressing such a plea makes it apparent, however, that switching fields is not easy and that there exist no accepted channels whereby the ordinary scientist can contemplate changing his research activities significantly without encountering severe obstacles. The very complexity and increasing specialization of modern science implies that the scientist must acquire much unfamiliar knowledge before he can be creative in a new field. He will also have to compete against the young people trained *ab initio* in that discipline; compared to them, he is likely to be working under a permanent handicap of significant lacunae in his knowledge of subject matter. Furthermore, to what extent is the scientist likely to be welcomed as an older man? How easily can he acquire the necessary training in institutions primarily geared to the education of young people? If he does emerge retrained in the new field and wants to begin doing research in it, will he have sufficient independent prestige to acquire necessary funds, research space, and students? Will the people in the new discipline accept him as a member of their group? For example, with what university department will he be affiliated? To these problems must be added a number of other obstacles. We have already mentioned the scientist's vested interest as an authority in his previous field, and his many commitments to the students and staff dependent on him. He is also likely to have family responsibilities. Under these circumstances, how does he solve some of the more immediate adminis-

[21] Abelson, *op. cit.*

trative and financial questions? Will he be given sufficient leave of absence to retrain himself in a new field? Does he relinquish all the contracts and grants which have supported his previous research? Can he get the necessary stipends or fellowships to support him during his period of retraining?

Here again the answers depend significantly on the status of the scientist in his previous field. If he is a member of the elite who has acquired considerable prestige as a result of his previous accomplishments, then he can exploit this prestige to facilitate plans for switching research areas. He is then more likely to get the necessary stipends and to be welcomed within the new discipline. But if the scientist is of lesser stature, then his mobility is much less and the questions raised above become much more formidable.

LEAVING RESEARCH

Yet many scientists, and especially the elite perhaps, will abandon doing active research at some point of their career. As we have noted, scientists tend to feel increasingly less creative in their own research. At the same time, the expansion of science has led to a vast expansion of alternative opportunities available outside of research proper. There are not only the traditional teaching activities, but numerous positions—such as department chairmanships or deanships—have become open in the universities which have grown in size and number and have tended to bestow an increasing proportion of their administrative positions upon scientists. Then, of course, there are a large number of industrial positions ranging from section heads to laboratory directors, plus a great variety of government jobs as consultants, advisors, and commission members. Such non-research positions become, in essence, additional steps added to the more traditional scientific career ladder.

These newer career steps are supported by the very institutionalization of science; i.e., people are needed to fill those positions. The steps are also justified by perfectly reasonable rationales quite apart from considerations of increased salary, prestige, or power; for it can be readily argued and believed that these non-research jobs are extremely important. Especially for the kinds of positions available to elite scientists, good justifications can be made in terms of "important and necessary work"—even if it is not the work of creative discovery. In the words of a distinguished physicist, now president of the National Academy of Sciences:

> . . . the corps of scientific administrators, most . . . centered in agencies in Washington, represent a highly elite and influential group . . . individually they are more important than all but the very best scientists whose work they support, both because their total number is very small and because individually they can have an enormous influence on the course of development of the entire country . . . the current system for supporting science . . . (I do not believe a better one exists), depends critically upon finding this small number of appropriately gifted individuals and matching them to the jobs. It is vitally important that we develop ways of inducing these young individuals to join the corps with the understanding that it can provide an appropriate means for self-expression . . . the role they can play is by no means passive.[22]

It is clear that these administrative positions may become increasingly attractive especially when the scientist is getting to feel less creative and less satisfied with his own research activities. Furthermore, his turning to such alternative administrative roles is significantly easier than switching to distinctly new research. Not only is the former path less arduous and risky, but it is facilitated by a number of accepted channels open to the individual who wishes to make the transition to such administrative tasks.

---

[22] F. Seitz, *Physics Today* (Aug. 1961), p. 36.

Again, this is particularly true for the elite.

The transition, once made, is highly irreversible. It is very unlikely that the scientist will, after several years of outside activities, return to research; under the present conditions of rapid discovery he will, have lost contact with the mainstream of his science. The transition requires thus an adaptation to a permanently new pattern of life, one which makes demands on the scientist's administrative and political abilities. It also requires a reorientation of values and aspirations. The scientist, raised in a tradition of science where the great discoverer is preeminent, does not find it easy to abandon his ego-ideal as active researcher and is prone to internal conflicts in his new roles. The older eminent scientist, although filling some key position important to the development of science in his country, may yet not find his office deeply satisfying. Looking back upon former days of active investigation, he may view his present activities as a "lesser task." Thus the justifications of the non-research position may not even convince the person himself, let alone his colleagues. Some scientists, commenting on colleagues who have become saddled down with administrative responsibilties, claim that these men "asked for it," that it is for them a "socially accepted escape from freedom . . . a decorous way of concealing that they are burned out."[23] Eiduson has described some older men, once active in research but now engaged in administration:

> they have guilt about whether their present activities are as valuable as the research they did earlier, and usually sense the feelings of impotence of which G. H. Hardy speaks when he describes mathematicians who have to write about mathematics because they no longer have the ability to contribute to new mathematics themselves.[24]

For the scientist who has internalized the norms of discovery, and especially if he has earlier proved himself an outstanding discoverer, the transition between active research work and the shift into non-research activities is thus fraught with personal difficulties and great demands on adaptability. The transition marks a critical turning point in the scientist's life and career: his "scientific menopause."

Trends for the Future

The rate of scientific discovery will, no doubt, increase even further as new fields of research continue to spring up and, after a brief time in the limelight of current fashion, in turn become well-explored, "classical," and replaced by more exciting fields. The pressures on the scientist are thus likely to become even more severe. Yet these pressures are not just felt by isolated individuals; they are becoming an intrinsic and, at least partly, recognized characteristic of the modern scientific enterprise. Correspondingly we venture to predict that the channels for coping with these pressures will also become increasingly institutionalized.

For example, it is likely that expectations regarding the scientific career are gradually going to change. The scientist himself, as well as his employing organizations, may come to accept as a normal part of his career that he will not remain active in research indefinitely, but will turn to other activities. This would imply a certain amount of age grading of the roles open to scientists, the younger being predominantly those entrusted with the task of carrying on research. After some time the role models acquired by students in their early training would change accordingly so as to encompass prominent scientists who perform tasks outside of research. This would tend to reduce the inner conflicts faced by older scientists when they ultimately turn to non-research positions. At the same time, the insti-

[23] B. T. Eiduson, *op. cit.*, p. 189.
[24] *Ibid.*, p. 158.

tutional expectation that research careers are short-lived would lead to more readily accessible opportunities whereby older scientists might make the transition to such non-research positions.

Of course, as remarked earlier, there is already emerging an increasingly large and significant number of scientists involved in supra-research and administrative functions: not only ordinary scientific administrators but also elite scientists who are concerned with policy decisions at the highest levels of government or industry. Most of these persons have themselves considerable past research experience. Indeed, the elite contains many scientists of great distinction and forms a very tight group of influentials. They have even been described as a "self-selecting core group that intercommunicates" and as an "oligarchy" who play a "game of musical chairs" with the available high level positions.[25] The prestige of the older scientists in these non-research positions derives partly from their past research accomplishments and partly from their present positions of power and influence. The esteem accorded to these scientists by the scientific community itself is not necessarily the same as that bestowed by the larger community. But, at least as far as the latter kind of esteem is concerned, one can confidently anticipate that such a group of elder scientists, who occupy increasingly positions of influence and perform useful functions, will increase its prestige standing in the society at large.

Under these conditions one can readily conceive that, faced with the cumulative pressures generated within their research activities on the one hand and with some of these prestigious alternatives on the other, older scientists may find the latter positively attractive . . . perhaps even seductive. Indeed, on the horizon one can see indications that the very research ideal of science may come to change its meaning. Some aspects of the transformation are already quite visible. Research has become less the exclusive province of great men, but has come to encompass a wide range of activities —from the pure to the applied and from the outstanding to the uninspired —carried out by a large pool of "scientific manpower." The more important potential transformation, however, concerns the status of research itself within science. Thus, whereas formerly research reigned supreme within science, it may come to share its supremacy with other activities like certain types of elite administration or policy making.

Historically, we are probably witnessing a transforming of the research ideal: from something which is *sui generis* a magnificent human achievement into something more like an amazing, but nevertheless regular, contribution by occasional intellectual geniuses to society. That contribution enables society to function better or worse, depending on whether the contribution is used wisely or not. But it is both expected with some regularity—indeed, socially and financially supported on that basis—and geared self-consciously into a much more embracing social organization than science itself.

Policy making and elite administration may not always remain the province of older men. Probably it is not even desirable that they remain so. One hears complaints that government boards and committees

> consist of the same old gang of insiders and that the members are too far removed from their days of active participation in the fields they represent. . . . Deliberate action will be necessary, for most methods of selection favor men who are widely known and older.[26]

There may thus come into being an in-

[25] M. Greenfield, *Science*, 142, 361 (18 Oct. 1963).

[26] From an editorial in *Science*, 141, 677 (23 August 1963).

creasing number of young scientists who set their sights from the very beginning primarily upon attaining some of the prestigious non-research career steps within science. They may then simply by-pass, or move very quickly, through the regular research career steps. This trend seems already underway.

In a context where science seems becoming increasingly institutionalized and where creative research is primarily the province of the young, one may even speculate about the possibility of young researchers coming to be conceived as merely the productive worker bees in an elaborately structured division of labor. The technicians and journeymen probably will increasingly approach that status. But really outstanding researchers are a very rare commodity, and their creative output is so important for society and so highly visible that the prestige attached to their particular activities is unlikely to diminish.

Since, however, prestige is a perishable commodity, even the outstanding research scientist is more than ever likely to move quickly into prized non-research positions in order to maintain his earlier hard-won prestige. Otherwise he may retain his personal reputation as an historically great figure, but be discounted on the current scene.

SEXUAL MODESTY, SOCIAL MEANINGS, AND THE NUDIST CAMP

MARTIN S. WEINBERG

Northwestern University

Deviant sub-systems have norms that permit, organize, and control the behavior which defines them as deviant. The nudist camp is an example of such a deviant sub-system, nudists being defined as deviant by their disregard for clothing when in the presence of others, particularly members of the opposite sex. This paper will describe the normative system of the nudist camp, its consequences for sustaining the definition of the situation common to this group, and the way it maintains those interaction patterns this sub-system shares with the outside society.

The general sociological framework to be used emphasizes the study of "social meanings" as a salient subject for understanding the realm of social organization. Such an approach was set forth by Weber:[1] in order to under-

Revision of a paper presented at the annual meeting of the Midwest Sociological Society, Kansas City, 1964. This represents one aspect of a theoretical treatment and study on Sex, Modesty, and Deviants. I am grateful to John I. Kitsuse for his encouragement, suggestions, and criticisms. I would also like to thank Raymond Mack, Scott Greer, Arnold Feldman, Walter Wallace, and Richard Schwartz for their valuable comments.

[1] Max Weber, *The Theory of Social and Economic Organization*, Glencoe: The Free Press, 1947, pp. 88ff. Also see: Alfred Schutz, *Collected Papers I: The Problem of Social Reality*, The Hague: Martinus Nijhoff, 1962, p. 59. Schutz insists that in considering these social meanings the sociologist develop "constructs of the second degree" out of the meaning constructs which typically guide the social actor. That is, the sociologist should abstract his more generalized model of social order out of the "order" which channels individual actors in their social behavior.