Age, Aging and Age Structure in Science

Harriet Zuckerman

and

Robert K. Merton

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Age, aging, and age structure in science

HARRIET ZUCKERMAN AND ROBERT K. MERTON

Scientists, and social scientists in particular, have come to recognize that increments of understanding are yielded by a conception of science that goes beyond cognitive structure and technological apparatus to include the social system of science. In this essay, Harriet Zuckerman and Robert K. Merton utilize this conception to broaden understanding in two major directions: the sociology of science and the sociology of age stratification.

By introducing relevant concepts of age stratification into the sociology of science, they develop new theoretical statements and fresh evaluations of available evidence on such subjects as the relation between the growth of science and the age structure of the scientific population, or the influence of scientific codification on age patterns of scientific behavior. They show how both historical trends and the socially patterned process of aging enter into the allocation of teaching, research, and administrative roles in science. In examining the extent of gerontocracy in science, they advance the basic idea that such inquiry must deal not only with the age hierarchy in distribution of powerful positions, but also with the consequences of this age structuring of power for younger scientists, for the accumulation of scientific knowledge, and for the relation of science to social policy. Through their discussions of collaboration in authorship and of shifts in scientific concerns, they illustrate the diverse ways in which the process of socialization intermeshes with processes of aging and cohort succession to affect the structure and culture of science.

The innovative treatment by Zuckerman and Merton of these central problems in the sociology of science is in itself enough to mark their essay as a notable contribution. But the importance of this contribution extends farther, for they have also expanded the potential of a sociology of age stratification, rather than simply drawing upon it. The authors have added to our still meager stock of finely honed conceptual tools. The happy choices of "juvenocracy" and "juvenescent population" and of "role-attrition" and "role-retention" are concepts and terms that will, no doubt, find permanent residence in the literature on age. More *profoundly. Zuckerman and Merton also add concrete illustration of the relation of age to social process, structure, and change. Their analysis of the growth of science, for example, points to ways in which the environing social system sets limits upon a changing age structure. Their discussion of codification in science reminds us that socialization (in this case the rapidity with which new roles are learned) can influence the allocative processes and press for a reordering of the age structure of power. Not least, their lucid examination of scientific roles, including collaborative roles, emphasizes the fundamental point that we must not allow unchanging role labels—parent, child, friend as well as scientist—to obscure the changes in role performances that actually take place historically and over the life course. In such manner does the theory of age stratification develop as it is applied.*

Critical overviews of a particular subject are ordinarily occasioned by the need to consolidate rapid advance in knowledge about it and to identify new directions of research. That is surely not so here. The best case that can be made for a chapter dealing with age stratification in science is that so little is known about it. In point of fact, systematic research over the years has been devoted to only one problem in this field: the patterns and sources of changes in the productivity of scientists during their life course. Beyond that, just about any methodical research on age, age cohorts, and age structure in science would qualify, through prior default, as a "new" direction.

The character of this chapter is therefore practically dictated by the twin circumstance that few theoretical questions have been addressed to the phenomena of age stratification in science and that only scattered investigations have dealt with these questions. Since the field is so short on facts, we must here be long on conjectures. Still the path of acknowledged conjecture, leading from mere guess and surmise to the neighborhood of grounded hypothesis, has its uses too. It can take us to a problematics[1] of our subject: the formulation of principal questions that should be investigated together with the rationale for considering these as questions worth investigating. For this purpose, we draw upon both the scattered evidence in hand and the model of age stratification developed by Matilda White Riley, Marilyn Johnson and Anne Foner.[2]

Put in utterly plain language, the aim of this chapter is to set out the little that we know and the much that we do not know but could know about the sociology of age differentiation in science and to explain why we should want to know it.

The paucity of materials on age differentiation in science appears to be the product of two distinct kinds of neglect in sociology. The first is the strangely slight attention given to age stratification generally even though age is recognized as one of the universal components of social structure. The second is the perhaps greater and now equally strange neglect of the entire sociology of science which, until the last decade or so, had enlisted the sustained interest of a remarkably small number of investigators. Neglect of this subject is odd if only because both advocates and critics of science are agreed that science is one of the major dynamic forces in the modern world. The convergence of the two kinds of neglect has meant that it was most improbable that sociologists would methodically investigate age differentiation in science. Nevertheless, enough work has been done to furnish a basis for formulating the problematics of the subject.

In this chapter, we deal with only a limited range of those problems. The first section is concerned with the connections between the growth and age structure of science; the second, with the relations between one aspect of the cognitive structure of science, here called "codification," and age-patterned behavior in science; the third, with age-related allocations of roles and with role-sequences in science; the fourth, with the operation of age-stratified structures of authority, principally in the form of gerontocracy; the fifth, with age- and prestige-stratified patterns of collaboration in science; and the sixth, final section dealing with age-patterned foci of problems selected for scientific investigation.

1 *Growth and age structure of science*

By all the rough-and-ready measures now available, science, with its various interacting components, has been growing more rapidly than most other fields of human activity. For example, the population of scientists, with a doubling time of about 15 years, is far outrunning the accelerating rate of increase in the general population. The science population of the United States, comprised of scientists, engineers, and technicians, grew from just under half a million in 1940 to 1.8 million in 1967. (About 300,000 of these in the latter period are estimated to be scientists [United States Bureau of the Census, 1970, p. 525].) The number of doctorates annually awarded in science has increased from about 60 in 1885 to some 16,000 in 1969, a growth rate of some 7 per cent per annum through this long period (National Academy of Sciences—National Research Council, 1970). And as Price (1963, Chapter 1; 1961, Chapter 5) has observed, the number of scientists and of scientific jour-

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[1] On the concept of problematics and of problem-finding, see Merton (1959, pp. v, ix–xxxiv).

[2] We refer in the main to Chapter 1 of this volume.

nals, articles and abstracts, and, more recently, the funds available for research have all been increasing exponentially at varying rates.

Of immediate interest to us is that this growth results from the recruitment into science of youthful cohorts, principally in their 20's, and practically not at all from the migration into it of older persons drawn from other occupations. And once in science, they tend to stay there. One now hears much about the so-called flight from science, but the historical pattern still remains. Scientists continue to be a tenacious lot, seldom leaving science for an entirely different occupation,[3] although there is an appreciable amount of transfer between fields of science at various phases in the life course. This raises a significant sociological problem in its own right, one that has been little explored in the sociology of occupations.

> *Query:* How are occupations arrayed in terms of rates of growth in personnel, and how do these rates and their components of entries and exits compare at different phases in the life course? What structural properties of occupations and characteristics of people recruited by them make for observed patterns of turnover?

Such detailed and systematic comparisons between age-stratified rates of turnover in various occupations are in short supply.[4] All the same, we suppose that classes of occupation have their exits and their entrances, distinctively patterned by age. In the case of science, transfers of personnel to other fields have been so conspicuously small that we take it to be in this respect near one extreme in the family of occupations. Once in the profession, scientists generally exit only through death or reluctant retirement.[5]

The patterns of growth and turnover in science are probably affected in at least three ways by the long period of socialization required to qualify for scientific work. Even in recent years, with the substantial funding of scientific education through fellowships and training grants in the United States, it takes an average period of 11 years after entry into college to obtain the doctorate in science (National Science Foundation, 1971, p. iii). First, the prolonged period devoted to acquiring the knowledge, skills, attitudes, values, and behavior patterns of the scientist means, as we have noted, that few enter the profession in later years after they have been at work in some other occupation. Second, this entails a considerable investment in the prospects of a career in science. The investment is not merely economic, with its financial costs of education and its foregone income during the period of preparation, but is affective as well. It is investment in a preferred way of life and commitment to it. Third, the high rate of attrition before entering the profession probably reflects a stringent process of social selection. Some 40 per cent of candidates for the doctorate fail to complete their program of graduate study (Berelson, 1960, p. 154), possibly leaving a more deeply committed and intellectually apt residue to go on into science.

Such modal patterns of entry and exit, coupled with the exponential increase in numbers of scientists, leave their stamp upon the age structure of the profession; and, as we shall indicate, this structure in turn affects both the normative system of science and its intellectual development. Figure 8 • 1, which compares the age distribution of scientific personnel with that of all employed personnel in the United States, suggests the ways in which the life-course processes of entry and exit combine with growth through cohort succession to yield the current age structure of science. On the one hand, the impact of delayed entry of scientific personnel into the labor force, necessitated by long years of prior training, is apparent from comparison of the age categories under 24 and 25–34. The relative size of the older strata, on the other hand, reflects the *juvenescent effect* of rapid growth on the age structure, as the size of entry cohorts expands. Despite the tendency of scientists to delay their retirement, the proportion of the scientific population in the older age strata is relatively small. Thus the age structure reflects the counteracting patterns of life-course and cohort flow.

The rate of increase in the population of scientists far outruns that of the American population at large, which results in an increasingly youthful population of scientists, both absolutely and comparatively. Their higher rate of increase means that scientists in the aggregate are also more youthful than the other professions. For example, more than half of those listed in the 1968 National Register of Scientific and Technical Personnel are under the age of 40 and 81 per cent of them under 50, compared with 63 per cent for lawyers and physicians (United States Bureau of the Census, 1970, pp. 65, 155, 525).

EDUCATION, AGE STRUCTURE, AND ALLOCATION

As with all social statuses and group memberships,[6] the status of "scientist" is not merely a matter of self-definition and ascription but also, and more significantly, of social definition and ascription. It is not enough to declare oneself to be a chemist or psychologist or space scientist; in order for the given status to have social reality it must be validated by "status judges," those institutions and agents charged with authenticating claims. Licensing boards for

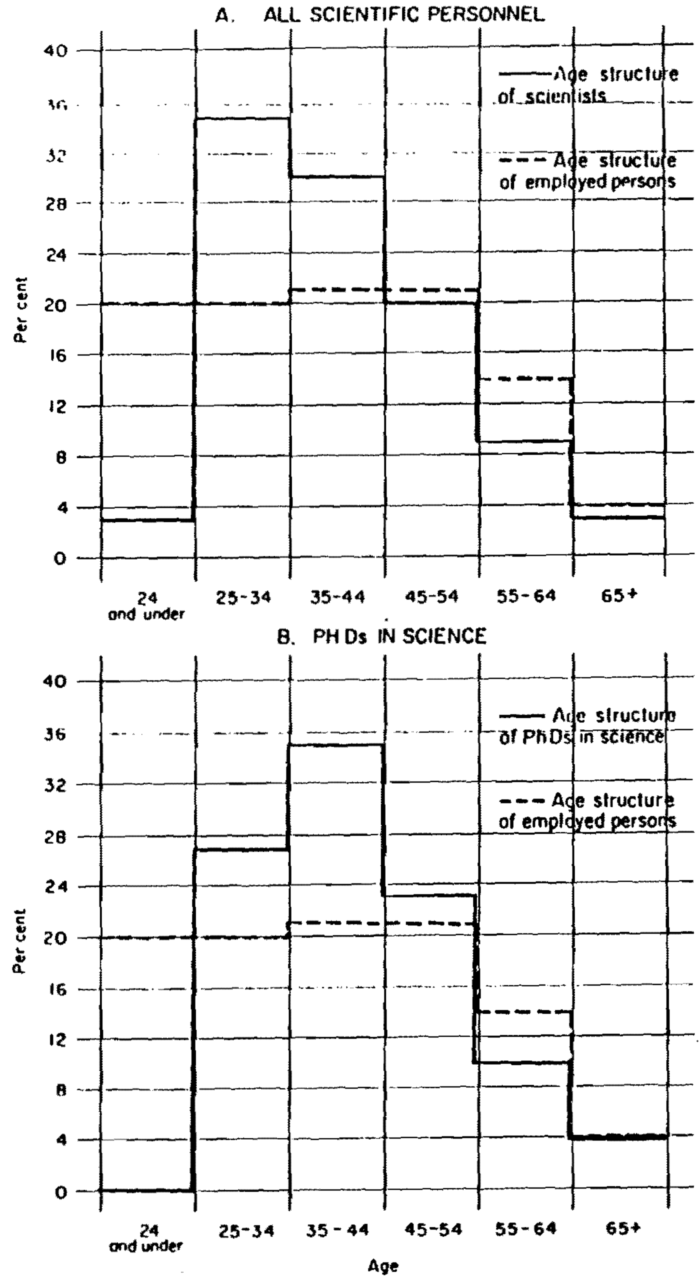
[3] This statement refers to those who have actually begun to work in science, not to those who have taken an undergraduate degree in science. In the United States, about half of all college graduates majoring in science and engineering go on into other occupational fields. See Brode (1971, p. 207).

[4] In the absence of suitable comparative investigations, Super (1957, Chapter 4) touches the periphery of this problem by impressionistically examining "occupational life spans."

[5] There is, however, a fair amount of mobility through the statuses and roles *within* the social system of science, as we shall see in a later section.

[6] For a theoretical analysis of group membership and nonmembership, see Merton (1968b, pp. 338–351); the concepts of status-judges and status-imputation are treated in Merton (1958).

FIGURE 8·1 *Comparative age structures: scientific personnel and total employed personnel, United States*



Source: National Science Foundation, 1969, p. 50; U.S. Department of Labor, 1968, p. 30.

many professions and specialty boards in medicine are only the more visible and firmly institutionalized specimens of status judges. The criteria for the sta- tus of scientists are typically educational attainment ("earned academic de- grees") and role-performance ("experience").

Since formal education is typically confined to the early years in life, the changing age structure of science is closely related to a concurrent educa- tional upgrading of the scientific population. As Harvey Brooks (1971, p. 319) has highlighted this trend:

> A striking fact is that in 1968 the percentage of the age cohort receiving Ph.D.'s in science and engineering and medical or dental degrees was higher than the percentage of the corresponding age cohort that received bachelor's degrees in science and engineering in 1920.

This rapid change in the educational composition of the science popula- tion is a special and illuminating case of the general rise in the level of educa- tion. As Riley and Foner have noted (1968, Volume I, Chapter 5), the long- range rise in formal education for the population at large has meant that, at any given time, older people are educationally disadvantaged in comparison with younger ones. In the special case of science, we deal of course with the uppermost reaches of the educational distribution, and this introduces dis- tinctive complexities. It may not be so much the social change in the level of attained education but the cultural change in the extent and character of scien- tific knowledge that presents the age cohorts of scientists with distinctive ranges of difficulties and opportunities. All this may lead to problems of career obsolescence and kindred difficulties for scientists in their life course. It also introduces various problems for the social organization of science; for exam- ple:

> *Query:* How do the social and cultural changes in the education of scientists and in the growth of scientific knowledge affect the working relations between cohorts of scientists? Do they make for social cleavage and discontinuity between them? What social mechanisms, if any, bind the cohorts together and make for continuity in scientific development?

Thus the changing age and educational structure of science is congruent with the self-image, prevalent among scientists, that pictures "science as a young man's game." But this expression, repeated on every side by scientists young and old, does not refer primarily to the age (or sex) distribution of sci- entists. Rather, it only announces a widespread belief that the best work in sci- ence is done at a comparatively early age. This posited linkage between age and significant productivity is still the focus of the little research that has been done on age stratification in science and we shall be examining that research in detail. But here, the imagery of science as the prerogative of the young gener-

TABLE 8 • 1 *Median age of Ph.D's[a] in selected science fields*

Field	Median age[b]
All scientific fields	41.2
Mathematics	38.2
Physics	38.3
Chemistry	41.0
Biological sciences	41.8
Psychology	42.2
Sociology	44.1

[a] When medians are computed for all personnel, with or without the Ph.D., the rank order is roughly similar except that sociology rises to third place.
[b] Computed from grouped data by straight line interpolation.
Source: National Science Foundation (1969, pp. 50 ff.) (adapted).

ates another kind of sociological question, this one dealing with the linkages between age structure, prevalent values, and social organization.

> *Query:* How do the age structures of occupations and their component specialties relate to age-connected values (e.g., youth as asset or liability)? How do they relate also to other forms of stratification within the occupation (e.g., allocation of authority-statuses and roles, of rewards, etc.)?

AGE AND EDUCATION IN THE COMPONENT SCIENCES

Turning from science in the large to the constituent sciences, we find that the median age of their personnel varies considerably, as appears in Table 8 • 1. These divergences reflect sociologically interesting differences in the social definitions of boundaries of disciplines and of criteria for admission into them. In part, the age differences in the various science populations merely reflect a differing mix of scientists, who often hold doctorates; engineers, who rarely do, and technicians who almost never do.[7] As can be seen from Table 8 • 2, some 37 per cent of American scientists held doctorates in 1968, but the figures ranged widely from 95 per cent (anthropology) to 7 per cent (computer sciences) among the disciplines. Thus the social science populations, with comparatively fewer technicians, have far higher proportions holding doctorates. Still, as we have seen in Table 8 • 1, when comparisons are confined to those holding doctorates, substantial differences remain in the median ages of workers in the various sciences.

Very likely, the differing age distributions of component sciences result largely from differing rates of recruitment of new scientists and interchanges of personnel between the sciences at various stages in their educational and

[7] The Committee on Utilization of Scientific and Engineering Manpower of the National Academy of Sciences reported in 1964 that fewer than 2 per cent of engineers had a Ph.D. No comparable counts have been published for technicians, no doubt because it would be a waste of funds, public or private, to search for what is known not to be there.

TABLE 8 • 2 *Percentages of scientists holding doctorates, according to fields of science, United States, 1968*

Field	Percentage holding doctorate
Anthropology	95
Psychology	64
Linguistics	62
Political science	59
Economics	53
Sociology	51
Biological sciences	48
Physics	44
Statistics	35
Chemistry	31
Mathematics	28
Earth and Marine Sciences	21
Agricultural sciences	18
Atmospheric and space sciences	9
Computer sciences	7
All fields	37

Source: National Register of Scientific and Technical Personnel (1968), adapted from Table, p. 23.

occupational careers. Although we assume that age-specific death and retirement rates are much the same for the various sciences, rates of transfer, which vary among age-strata, affect their age structures (National Science Foundation, 1971, pp. 16, 30). Substantial transfers from one science to others have been found in the course of undergraduate education (Reitz, 1972) and longitudinal studies have identified streams of interchange among practicing scientists (Harmon, 1965, Chapter 5). Yet, despite some strands of evidence, the sources of age differences among the sciences remain almost as much unexplored territory as the range of their consequences for the character and rate of development in the various sciences.

SLOWING GROWTH RATES OF SCIENCE

It has long been recognized in principle that the exponential growth of science at a rate greater than that of society could not continue indefinitely. Manifestly, the rate of expansion in the numbers of scientists, the amount of resources allocated to science and in the volume of scientific publication would have to slow down. Otherwise, as Derek Price (1963, p. 19) is fond of writing and as many others are fond of quoting, "We should have two scientists for every man, woman, child and dog in the population, and we should spend on them twice as much money as we had."

There are signs that, at least in societies with a substantial science base, the rate of expansion has tapered off. In the United States, for example, the resources expended for research and development have remained at about 3 per cent of GNP since 1964 (Martino, 1969, p. 769). There are indications, also, that the proportion of college graduates going into science has reached a ceiling (Brode, 1971, pp. 206–208).

Such equilibrium growth rates have direct consequences for the age and opportunity structures of science, as Martino has indicated (1969, p. 772). A direct consequence, of course, is an increase in the median age of scientists. Slackened increase in resources will mean fewer new research installations and new departments in new universities. In due course, the age structure of research groups will probably change with possible consequences for their productivity.[8] The rate of increase in the volume of scientific publication will tend to diminish. But if Weiss and Ziman are correct in suggesting that the proliferation of scientists and the big expansion in funds for research have resulted in an even more rapid proliferation of trivial research and trivial publications,[9] this decline in volume of publication need not mean a corresponding decline in the growth of knowledge. Finally, since the various changes do not occur uniformly in the individual sciences, the relative rates of development of long established and newly emerging disciplines will probably continue to differ (Rose and Rose, 1969, pp. 250–251).

2 *Age stratification and the codification of scientific knowledge*

In principle, the sociology of knowledge and, more narrowly, the sociology of science are concerned with the *reciprocal* relations between social structure and cognitive structure. In practice, however, sociologists of knowledge have dealt almost exclusively with the influences of the social structure upon the formation and development of ideas. And when sociologists of science have investigated the "impact of science upon society," this has been principally in the form of examining the, chiefly unanticipated, social consequences of science-based technology. In neither case is the effort made to trace the consequences of the cognitive structure of the various sciences for their distinctive social structures.

Yet the problem of the significance of cognitive structure for social structure is with us still. We propose to touch upon a limited case in point by examining one aspect of the cognitive structures of the sciences—what we call "codification"—in its relations to their distinctive age structures. Since so

[8] Vlachý (1970, pp. 132–134) and Thiemann (1970, pp. 1428–1429) maintain that the age structure of research groups affects their productivity, but little is yet known about this question. Available data reflect differences in the productivity of age strata rather than the various distributions of these strata.

[9] Weiss (1971, p. 135); Ziman (1969, p. 361) puts the thesis vigorously: ". . . the consequences of flabbiness [in science policy] are all too sadly evident in all quarters of the globe—the proliferation of third-rate research which is just as expensive of money and materials as the best, but does not really satisfy those who carry it out, and adds nothing at all to the world's stock of useful or useless knowledge."

little preparatory work has been done on this problem, our observations are altogether tentative, designed to raise questions rather than to answer them.

Codification refers to the consolidation of empirical knowledge into succinct and interdependent theoretical formulations. The various sciences and specialties within them differ in the extent to which they are codified. It has often been remarked, for example, that the intellectual organization of much of physics and chemistry differs from that of botany and zoology in the extent to which particulars are knit together by general ideas. The extent of codification of a science should affect the modes of gaining competence in it. Experience should count more heavily in the less codified fields. In these, scientists must get command of a mass of descriptive facts and of low-level theories whose implications are not well understood. The comprehensive and more precise theoretical structures of the more codified field not only allow empirical particulars to be derived from them but also provide more clearly defined criteria for assessing the importance of new problems, new data, and newly proposed solutions. All this should make for greater consensus among investigators at work in highly codified fields on the significance of new knowledge and the continuing relevance of old.

The notion that the sciences and specialties differ in the extent of their codification is not, of course, new. Some 20 years ago, James Conant introduced the counterpart idea that the various branches of science differ in "degree of empiricism." As he put it, "Where wide generalizations and theory enable one to calculate in advance the results of an experiment or to design a machine (a microscope or a telescope, for example), we may say that the degree of empiricism is low" (1950, p. 9). In our terms, the more codified a science or scientific specialty, the lower its degree of empiricism.[10]

By consolidating both data and ideas in theoretical formulations, the more highly codified fields tend to obliterate the original versions of past contributions by incorporating their essentials in the newer formulations. As Paul Weiss noted in his influential paper, "Knowledge: A Growth Process" (1960, p. 1347), "Each field of knowledge must be accorded its own merit ratio between generalization and particularization, taking for granted that assimilation will be driven to the utmost limits compatible with the nature of the field." In this sense, the more highly codified the field, the higher the rate of "obsolescence" of publications in it. Weiss measured differentials in obsolescence by the age distributions of references appearing in publications in various

[10] The recent distinction between "hard" and "soft" science also bears a family resemblance to the concept of codification. Storer (1967) uses that pair of terms to characterize sciences given more, or less, to quantification and to rigor. Price (1970) has suggested various features which are distinctive of the literatures of hard science, soft science, technology, and nonscience fields of learning.

sciences, a procedure which has since been described as "citation analysis."[11] Weiss found that references to recent work were much more frequent in "analytical physiology than in its more descriptive biological sister sciences" of zoology and entomology.

The general pattern has since been confirmed for a wider variety of disciplines.[12] The journals in fields we intuitively identify as more highly codified —physics, biophysics, and chemistry—show a larger share of reference to recent work; they exhibit a greater "immediacy," as Derek Price calls it.[13] By way of illustration,

> 72% of the references in *The Physical Review** are to papers published within the preceding five years, as are
> 63% in the *Cold Spring Harbor Symposium* on quantitative biology;
> 58% in *Analytical Chemistry*;
> 50% in the *Anatomical Record*; and
> 47% in the *American Zoologist*.*[14]

Similar citation data suggest that the social sciences are intermediate to the physical sciences and the humanities in degree of codification. The findings are notably consistent. As we have just seen, in the more analytical physical sciences, about 60 per cent or more of the citations refer to publications appearing within the preceding five years. In the humanities—represented by such journals as the *American Historical Review*, *Art Bulletin*, and the *Journal of Aesthetics and Art Criticism*—the corresponding figures range from 10 to 20 per cent. In between are the social sciences—represented by such journals as the *American Sociological Review* and the *Journal of Abnormal and Social Psychology* where from 30 to 50 per cent of the citations refer to equally recent publications.

[11] In his monumental *Introduction to the History of Science*, (1927–1948), George Sarton often quantified the citations of a major scientific work to previous works as one way of establishing its intellectual heritage. Eugene Garfield suggested the use of systematic citation indexing for historical research in 1955 and developed the computerized Science Citation Index in 1964 (see Garfield, 1955; Garfield, Sher, and Torpie, 1964). With its more than 20 million bibliographic citations, the SCI data base has greatly advanced citation analysis in historical and sociological investigations of science.

[12] Derek Price has done most to extend the use of "citation-and-reference-analysis" to distinguish modes of development in the various fields of learning. For early work, see Price (1963, pp. 78 ff. and 1965); for recent work, Price (1970). The abundance of citation studies includes Broadus (1952 and 1967); Burton and Keebler (1960); MacRae (1969).

[13] "Immediacy is much increased use of the last few years of papers over and above the natural growth of the literature and its normal slow aging." "A literature growing at the rate of 5 per cent per annum," Price goes on to calculate, "doubles in size every 13.9 years and contains about 22 per cent of all that has been published in its last five years of publication." This means, of course, that the extent to which a field focuses on new work and so makes for obsolescence of earlier work is measured by the excess of recent citations beyond what would be expected on the basis of growth in the literature (Price, 1970, pp. 9–10).

[14] The data identified by an * are from Price (1970); we have compiled the rest.

It appears, moreover, that the citation pattern of a science transcends national and cultural boundaries. At any rate, American, European, and Soviet journals of physics have been found to exhibit almost identical age distributions of citations (Dedijer, 1964, p. 461).

One limited aspect of the cognitive structure of the various sciences, then, is the extent of their codification. We want now to explore possible relations between codification and age-patterned behavior and processes in the sciences, along the following lines:

1. The extent to which codification affects opportunities for scientific discovery by different age strata;
2. age differentials in responsiveness to new scientific ideas in fields that are variously codified;
3. the effects of codification upon the visibility of scientific contributions; and
4. the linkages between codification, changing foci of research and opportunities for discovery.

CODIFICATION AND AGE DIFFERENTIALS IN DISCOVERY

We begin with the premise that up to a given age, older and more experienced scientists have an edge on their much younger colleagues in the opportunities for discovery. After all, they know the field as the novice does not. What needs to be explained, in our view, is not so much discovery by experienced and knowledgeable elders as discovery by newly trained youth. In this connection, we need to ask whether discovery by young scientists is more frequent in some sciences and, if so, how this comes to be.

Codification facilitates mastery of a field by linking basic ideas in a theoretical framework and by reducing the volume of factual information that is required in order to do significant research. This should lead scientists in the more codified fields to qualify earlier[15] for work at the research front—at least, as collaborators of more mature investigators. And early achievement in science may give an enduring advantage, by providing both increasingly abundant facilities for research and early access to the social networks of scientists at the research front where information and criticism are exchanged and motivation for getting on with one's work is maintained.[16] The best known because heavily publicized specimen of this process is that of the 25-year-old James

[15] This early start may also reflect early recruitment to the more codified sciences. We know of no data on the matter but share the widespread impression that decisions to go into mathematics and physics are made much earlier than decisions to enter the soft sciences. The significance of age at time of the decision to enter the field of medicine is examined by Rogoff (1957) and by Thielens (1957, pp. 109–129 and pp. 131–152).

[16] See the informed and astute account of how young scientists achieve entry into the "Invisible College" of their specialty by the physicist and sociologist of science, Ziman (1968, pp. 130–134).

D. Watson[17] soon finding his way into the center of work on the structure of DNA once he was sponsored by his intellectually influential teachers, Salvador Luria and Max Delbrück. In this process, intellectual mobility and social mobility (of a jointly sponsored and contest variety) are mutually reinforcing.

The organization of scientific inquiry and of training in science also promote early entrance into the research role. In a sense, young scientists are more apt than their expert teachers to be abreast of the range of knowledge in their field. Since advanced research in science demands concentration on a narrow range of problems at hand, the established specialist experts, intent on moving ahead with their own research, tend to fall behind on what others are doing outside their own special fields. But, at least in the best departments, students are trained by an *aggregate* of specialists at work on the research front of their specialties. This brings them up to date, if only for a time, in a wider variety of fields than their older and temporarily more specialized teachers.

This pattern we believe to hold in all the sciences but we suspect that it is more marked and efficacious in the more highly codified sciences, those that provide more powerful means for acquiring competence in consolidated current knowledge. The opportunity structure confers two advantages on the young in the more codified fields: the chance to begin research early as qualified junior colleagues and the chance to have training that is both up-to-date *and* relatively diversified. Both should advance their opportunities for making significant research contributions early in their careers.

It is from this standpoint that we come upon the one problem that has almost monopolized discussions of the, in fact, multiform connections between age and scientific activity: the time of life at which scientists do their most important work or, as it is sometimes put, the relations between age and scientific productivity. The data bearing on this subject are faulty or severely limited but, on first inspection, they seem to confirm our expectations. Various investigators report lower median ages for discoverers in physics and chemistry than in the more descriptive biological sciences, with these being lower in turn than in the behavioral sciences (Lehman, 1936, p. 162; 1953, p. 20; Adams, 1946, p. 168). Nobel laureates in physics, for example, were on the average 36 at the time of doing their prize-winning work; laureates in chemistry, 38 and those in medicine and physiology, 41.[18] This does not mean, of course, that a higher rate of discovery in youth is the norm in the more codified sciences, much less for them all. Apparently, we sometimes need to be

[17] Watson's detailed account (1958) of how all this worked out for him is one of the many features of *The Double Helix* that make it an unexampled personal document in the sociology of science.

[18] Data for 1901–1950 are drawn from Manniche and Falk (1957, pp. 302 ff.), and for the period 1951–1969 from Zuckerman (1972).

reminded that median ages at time of discovery tell us that half of the discoveries were made after the median age as well as before. In contrast to the usual emphasis, Lehman's findings could be reported, for example, as indicating that "fully half" of the discoveries listed in Magie's *Source Book of Physics* were made by scientists *over* the age of 38 or that "fully half" of the discoveries listed in genetics were by scientists *over* 40.

Beyond this, we need only mention other caveats in the use of these data on age and scientific achievements. They are faulty in two basic respects. First, they do not take into account the age structure of the scientific population. As we know from the exponential growth in the numbers of scientists, the young ones make up a hefty percentage at any given time and so they will produce a large aggregate of contributions. But of course this does not provide the needed evidence on comparative rates of scientific contributions at various ages. What is required are data not on the proportion of contributors in each age stratum but on the proportion of each age stratum making contributions. This requirement holds even in comparing age-linked rates of productivity among the various sciences since, as we have seen, the age composition of the sciences does differ appreciably.

Second, Lehman's studies do not take into account, as Wayne Dennis has emphatically demonstrated, the biasing effects of differing life spans on the distribution of achievements at various ages.[19] The fact that short-lived scientists are cut off from making any contributions in later years factitiously enlarges the proportions assigned to young scientists in data on age and achievement that do not take longevity into account. The essential issue is caught up in the lament of Newton over the premature death at age 34 of his protégé, the mathematician, Roger Cotes: "If he had lived, we might have known something." Or the similar observation by John Maynard Keynes about the brilliant young logician and mathematician, Frank Ramsey, robbed of his future at the age of 27.

We suggest, moreover, that the statistical bias in apparent age-specific productivity differs among the variously codified sciences. In doing so, we depart a bit from the ancient adage that the good die young. We propose a less crisp but more germane version: *comparatively*[20] more good mathematicians and physicists than good historians and sociologists die young. In saying this, we do not propose the improbable hypothesis that rates of premature mortal-

[19] Dennis (1956a, 1956b, 1956c, 1966). For a discussion of the fallacies in the interpretations drawn from the Lehman data and the implications of the Dennis data, see Section 3 of the Appendix; See also Riley and Foner (1968, Chapter 18 · B · 2).

[20] The emphasis on "comparatively" is intended to signal that we are not dealing here with the question of differences between the various fields in the talent of their recruits. That is an interesting question in its own right and in other connections; it is not a relevant question here.

ity are in fact higher in the more codified fields than in the less codified ones. We assume the same rates of age-specific mortality but an earlier age of prime achievement. Mathematicians of genius who died prematurely, such as a Galois dead at 21 or a Niels Abel dead at 26, have been enduringly identified by their early work as being of the first rank. But by hypothesis, sociologists or botanists of genius who die young will have fulfilled less of their potential in their early years and so do not even appear in the standard histories of their fields. This reconstruction is consistent with the Dennis data, which suggest that a far larger share of the total life output of long-lived mathematicians and chemists than of equally long-lived historians, botanists, and geologists is completed during their first decade of work. This sort of thing can thus foster the illusion that good mathematicians die young but that, say, good sociologists linger on forever.

To this point, we have centered on differentials in rates of scientific contributions by age-strata in sciences codified in varying degree. We have now to touch upon the further question whether the truly transforming ideas in science, the fundamental reconceptualizations, are more apt to be the work of youthful minds rather than older ones. T. S. Kuhn, in his vastly influential book on scientific revolutions, suggests that creators of fundamental new paradigms are almost always young or very new to the field (1962, pp. 89–90). A long and familiar roster of cases illustrates his suggestion. Newton wrote of himself that at 24, when he had begun his work on universal gravitation, the calculus and the theory of colors: "I was in the prime of my age for invention, and minded Mathematics and Philosophy more than at any time since." Darwin was 22 at the time of the Beagle voyage and 29 when he formulated the essentials of natural selection. Einstein was 26 in the year of three of his great contributions, among them the special theory of relativity, and finally, eight of the ten physicists generally regarded as having produced quantum physics were under the age of 30 when they made their contributions to that scientific revolution (Gamow, 1966).

Arresting illustrations of this kind are of course not enough to show that young scientists are especially apt to revolutionize scientific thought. But in the absence of systematic data on the age composition of scientists in various historical periods, they remain the basis for the generalization being widely accepted as commonplace. Yet, as Kuhn himself goes on to say, it is a generalization that "badly needs systematic investigation."

CODIFICATION AND AGE DIFFERENTIALS IN RECEPTIVITY TO NEW IDEAS

As we have suggested, new developments in the more codified sciences are closely linked to work done just before. These sciences grow, as Price puts it, "from the skin." As a result, awareness of new ideas and critical acceptance of them is especially important in the codified sciences. If certain age strata are more responsive to them, this increases their chances for making further discoveries.

It has often been said that aging leads to growing resistance to novelty and specifically, that older scientists tend to resist new ideas.[21] As Planck, who did not develop the idea of the quantum until he was 42, remarked: ". . . a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it."[22]

Observations of this sort, based on lore rather than systematic evidence, raise the perennial questions: is it really so? and if so, how does it come to be?

If there are these age-stratified differences in receptivity to new (and sound) ideas, this should be reflected in various behaviors. For one example, younger scientists should rely more on recently published research and cite it more frequently than older scientists in the same fields. For another, the age strata of scientists should differ on what they take to be the most significant contributions to their field. Some limited data in hand lend credence to these inferences.

We find (Zuckerman, unpublished data) for a small sample of scientists that those under 30 are more given to citing recent work (papers published in the five years preceding their own) than men in the same fields who were on the average 30 years older: 71 per cent of the references in the work of the younger investigators and 58 per cent by the older ones being to the recent literature.[23] But the pattern does not hold for all older scientists. Nobel laureates do not exhibit the same citation behavior as their less distinguished age peers. They appear to be just as responsive to new research as men 30 years their junior, with 73 per cent of their references being to the most recent literature. This finding bears only tangentially on the Planck doctrine but it does suggest that attentiveness to new developments in science is stratified by both age *and* scientific achievement.

Substantive differences in judgments of what constitutes important work may crystallize, as Hagstrom has noted, into "generational disputes." But, as

[21] *[But see the changes over the life course in political attitudes reported in Chapter 4 · 2 by Foner.]*

[22] Planck (1949, pp. 33–34). This must surely be one of the most frequently quoted observations of its kind in recent years. It was put to good use by Bernard Barber in his paper on resistance to scientific discovery (1961), and, among others, by Kuhn (1962, p. 150); Hagstrom (1965, p. 283), and Greenberg (1967, p. 45).

[23] Stephen Cole (1969) found the same pattern for a variety of scientific fields. Since the process of refereeing and editing scientific papers before publication probably tends to homogenize initial age differences in citations, even small differences between age strata in the published papers represent an *a fortiori* case.

he goes on to say, "even if some disputes are generational, they need not be simply 'innovative youth' versus 'conservative age.' Rather, the outlook of a generation is strongly influenced by events occurring when its members embark upon their careers. Age may be more radical than youth" (Hagstrom, 1965, pp. 284–285).

The evidence on age differentials in receptivity to new ideas in science remains thin and uncertain. But should further investigation find, as the widespread belief has it, that older scientists are indeed more resistant to new ideas, that would only raise a series of questions of how that comes to be. It would not follow, for example, that it results from physiological aging or senescence.[24] As Barber has noted in this context, *aging* "is an omnibus term which actually covers a variety of social and cultural sources of resistance." He goes on to suggest several possible social and cultural components in such resistance:

> As a scientist gets older he is more likely to be restricted in his response to innovation by his substantive and methodological preconceptions and by his other cultural accumulations; he is more likely to have high professional standing, to have specialized interests, to be a member or official of an established organization, and to be associated with a "school." The likelihood of all these things increases with the passage of time, and so the older scientist, just by living longer, is more likely to acquire a cultural and social incubus. But this is not always so, and the older workers in science are often the most ardent champions of innovation.[25]

This provides in effect, a formidable agenda for investigation of age-associated differences in receptivity and resistance to new conceptions in science.

CODIFICATION AND VISIBILITY OF SCIENTIFIC CONTRIBUTIONS

We turn now from considering possible age-patterned responses to new ideas, irrespective of their source, to consider possible differences in the responses to new ideas advanced by scientists of differing age. The visibility of new ideas in a discipline may be affected by the sheer volume of its literature, which may, in turn, be related to its degree of codification. That is not of interest here. However that may be, our interest lies instead in the direct implications of codification itself for the visibility of ideas introduced by scientists of differing age.

It would seem that new ideas are more difficult to identify as important in disciplines that are largely descriptive and only spottily and loosely organized by theory. In these less codified disciplines, the personal and social attributes of scientists are more likely to influence the visibility of their ideas and the reception accorded them. As a result, work by younger scientists who, on the average, are less widely known in the field, will have less chance of being noticed in the less codified sciences. Put another way, the Matthew Effect (Merton, 1968a, pp. 56–63)—the tendency for greater recognition to be accorded contributions by scientists of great repute—is apt to operate with special force in the less codified fields.

Correlatively, in the more codified sciences, new ideas, whatever their source, can better carry their own credentials. Important contributions by young scientists or older ones, for that matter, are not only more visible in the codified fields; they are taken more seriously since their theoretical importance can be more readily assessed. This tends to put the young on a par with eminent seniors in communicating ideas and in having them noticed.

Although Stephen Cole's studies of the Matthew Effect have been confined to physics, his findings are suggestive in this regard. In this highly codified science, the work of eminent investigators is incorporated into ongoing research only a little more quickly than contributions of comparable quality by less distinguished investigators. The age of physicists also has little effect on the speed with which their ideas diffuse (Stephen Cole, 1970, pp. 297, 299). In physics, then, the merits of the investigation seem to govern its reception with the attributes of the investigator playing only a small part. Comparative study is now needed to find out whether the strength of the Matthew Effect does in fact vary inversely with the extent to which sciences are codified.

CODIFICATION, INTER-SCIENCE TRANSFERS, AND DISCOVERY

Although, as we have seen, scientists seldom leave the occupation of science altogether, a considerable number transfer from one field to another. About a quarter of American scientists have made shifts of this kind, the rates of transfer differing among the various sciences of origin and, of course, among age cohorts (Harmon, 1965, pp. 50–52; National Research Council, 1968, pp. 59–62).

Scientists who have left the field in which they were trained, to work in another where they were not, constitute a special class of newcomers. In certain respects, these older neophytes are functionally like the younger novices in that field. Both are being rapidly introduced to the research front of the field although the older newcomers differ from the younger indigenous recruits in having been less comprehensively trained in the new field. Both include people bringing new perspectives on old problems to the degree that they have not acquired commitments to the definitions of problems or to the form of their probable solutions that are conventionally adopted in that field.

[24] For systematic review of the evidence on the multiple interpretations of such types of data, see Riley and Foner (1968).

[25] Barber (1961, p. 602). On much the same point, see also Hagstrom (1965, pp. 283–284).

The transfers, unlike their young colleagues just beginning to work in science, also bring with them styles of research new to their adopted field, as was dramatically the case when physicists turned their attention to biology and created the field of molecular biology.[26] In part, the contributions of newcomers derive from their tranferring to the new field standards and modes of investigation customary in their field of origin.

Some of the attributes of the more codified sciences that facilitate the fairly rapid learning of essentials by the incoming student thus facilitate as well effective transfer into the field by older, experienced scientists. Historical and statistical data (Harmon, 1965, p. 51) suggest that transfers tend to occur among sciences codified to about the same extent with a subsidiary pattern of movement toward less codified fields. There is little interchange between the extremes of codification: between physics and, say, botany or zoology.

The general pattern of such transfers has been connected by Joseph Ben-David to what he describes as "role-hybridization": applying the means usual to Role A in trying to achieve the goals of Role B. As he sums it up:

> Scientific disciplines differ in the degree of their theoretical closure and methodological precision. The phenomena most similar to role hybridization would be shifts from a theoretically and methodologically more advanced discipline to one less advanced. These must be distinguished from shifts between two disciplines of the same level and from less to more advanced disciplines (Ben-David, 1960, pp. 566–567; see also Ben-David and Collins, 1966).

The processes and consequences of patterns of transfer among variously codified sciences have only begun to be investigated. But something of the process can be pieced together for the eminent men who have changed fields. They often exhibit an almost playful arrogance about the time required for retooling. Symbolic stories abound. Leo Szilard is said to have taken all of three weeks at Cold Spring Harbor in order to effect his transformation from physicist to biologist, this at the age of 47.[27] Francis Crick's leap from physics into biology has been twice chronicled (Watson, 1968; Olby, 1970). The same theme of the rapid acquisition of fundamentals appears in Waddington's account of the European origins of molecular biology. He writes of the journey to the first conference of geneticists and crystallographers:

> Most of us tried to sleep on the benches in the general saloon, but Darlington and [the crystallographer] Bernal kept sea-sickness at bay by the former teaching the latter "all the genetics and cytology anyone needs to know" throughout the course of the night. Before dawn, Bernal had already decided that the mitotic spindle must be a positive tactoid. . . . (Waddington, 1969, p. 318).

This suggests not only that much can be learned in short order but also that knowledgeable newcomers accustomed to being in command of their field, even if they are not quite of Bernal's caliber, can achieve enough understanding of fundamentals to introduce new ideas at the outset.

These topflight migrants from one science to another seem unworried by their ignorance of the problematics prevailing in the new field. This keeps them from some stale preconceptions. Maria Goeppert Mayer, the Nobel laureate, provides an apt example. Her work on "the magic numbers problem" (a problem of such profound interest that it had been given a name) and her subsequent development of the shell model of the nucleus, she reports, depended on a specific kind of ignorance. Trained as a physicist but working mainly in physical chemistry, she was not brought up on the "Bethe Bible"[28] and so was unhampered by knowing "what everyone knew" about spin-coupling.[29] Focused naivete and focused ignorance evidently have their functions in science—especially for anything but naive and otherwise immensely informed scientists.[30]

Like geographic migration, intellectual migration in the form of transfers from one science to another should lend itself to cohort analysis. Are there certain times in the careers of scientists at which they tend to make the change? Do these patterns persist among successive cohorts or are they fairly constant? Have there been historical changes in the frequency and patterns of transfers? Are the migrants in science representative of the field, more able on the whole, less so, or bimodal in their distribution of capacity and achievement?

Whatever the patterns of transfer they need to be examined within the con-

[26] The *Festschrift* for Max Delbrück, one of the founders of molecular biology, consists largely of a remarkable series of lively and informative personal accounts of the beginnings of the field. See Cairns, Stent and Watson (1966). *What is Life?*, a short book by the physicist Erwin Schrodinger, proved decisive in transforming physicists into biologists. For a further account of the emergence of molecular biology, see Fleming (1969, pp. 152–189).

[27] Some 14 years earlier, Szilard had been told by the physiologist A. V. Hill that he could pick up the essentials of physiology by simply setting himself the task of teaching it (Szilard, 1969, p. 98).

[28] In a series of papers published in the late 1930's in *Reviews of Modern Physics*, Hans Bethe attempted to consolidate what was known then about the atomic nucleus. That these papers had unparalleled impact on physics is registered in the fondly respectful title by which they are known.

[29] J. H. D. Jensen, who independently solved the same problems, interestingly enough, was also unaware of prevailing ideas of spin-coupling.

[30] On the general idea of the uses of ignorance under certain conditions, see Moore and Tumin (1949, pp. 787–795). On the "outsider" in science and technology, see Gilfillan (1935, pp. 88–91); Ben-David (1960, pp. 557–59); Merton (1970b).

text of the intellectual organization of the sciences of origin and sciences of destination.

3 *Scientific roles*

INVENTORY OF ROLES IN SCIENCE

Like other domains of social life, the social structure of science has its distinctive array of statuses and roles, allocated to members through complex processes of social selection. We focus here on the status of scientist. But we should note in passing that the social structure of science, especially as we know it today after centuries of institutionalization and social differentiation, contains a variety of other statuses and roles. Often indispensable to the effective advancing of scientific inquiry, these parascientific roles include technicians of every stripe, the builders of experimental apparatus and instruments and the broad spectrum of assistants engaged in facilitating scientific work (for example, by preparing and taking care of experimental materials).[31]

Like other statuses, the status of scientist involves not a single role but, in varying mixture, a complement of roles. These are of four principal kinds: research, teaching, administrative, and gatekeeper roles.[32] Each of these is differentiated into subroles, which we only note here but do not consider in detail.

The research role, which provides for the growth of scientific knowledge, is central, with the others being functionally ancillary to it. For plainly, if there were no scientific investigation, there would be no new knowledge to be transmitted through the teaching role, no need to allocate resources for investigation, no research organization to administer, and no new flow of knowledge for gatekeepers to regulate. Possibly because of its functional centrality, scientists apparently place greater value on the research role than any of the others. As is generally the case in maintaining a complex of mutually sustaining roles, ideology does not fully reflect this differential evaluation of roles in the role-set: scientists will often insist on the "indispensability" and consequently equal importance of the ancillary roles. Yet, almost in a pattern of revealed preference, the working of the reward system in science testifies that the research role is the most highly valued. The heroes of science are acclaimed in their capacity as scientific investigators, seldom as teachers, administrators or referees and editors.

The research role divides into subroles, distinguished to varying degree in the different sciences. In research, scientists define themselves and are defined by others as experimentalists (or, more generally, empirical investigators) or as theorists, with occasional high-yield hybrids such as Enrico Fermi or Linus Pauling embodying both subroles effectively. This differentiation seems more marked in the more codified sciences. Little is known about the processes leading scientists to adopt one or another of these subroles. In the lore of science, this is not even problematic. Scientists are assumed to become either experimentalists or theorists as their highly specific capacities dictate. But it seems that the process is more complex than the simple matching of roles to self-evaluated capacities. It presumably involves, at the least, interaction between developing self-images of aspirants to scientific investigation, socialization by peers and mentors, and continuing evaluation of their role performance by peers, superiors, and themselves.

To the extent that the research role in science involves interaction between scientists, it also makes for some, often reciprocal, teaching and learning. The teaching role, particularly in the sciences, calls not only for explicit didactics but, probably much more in the sciences than in the humanities, for tacit instruction through observed example. The master-apprentice relation is central to socialization in the sciences, particularly in laboratories which provide for mutual observability by master and apprentice. This structural difference between the sciences and the humanities is reflected in the fact that the status of postdoctoral student is widespread in the sciences and rare in the humanities.

There is, in the normative system of science, an ambivalence toward the preferred relations between the research and teaching roles. For some, the norm requires the scientist to recognize his prime obligation to train up new generations of scientists, *but* he must not allow teaching to pre-empt his energies at the expense of advancing knowledge. For others, the norm reads just as persuasively in reverse. We have only to remember the complaints about Faraday that he had never trained a successor as Davy had trained him, yet consider the frequent criticism of scientists who give up research for teaching. There are indications, as we shall see, that the time scientists allocate to the roles of teaching and research changes during the life course.

A third major role of scientists is ordinarily (and not very instructively) caught up in the term "administration." The term often covers a wide gamut of quite distinct structural conditions, ranging from occasional service on advisory or policy-making committees, through direction of a small-scale research inquiry to full specialization in the one role as with full-time "science administrators" or "R & D administrators." What is described as the increasing bureaucratization of science often refers to the growing number of full-time administrative roles and their growing power to affect the course of scientific development. And such bureaucratization, precisely because it involves allocation of resources to the various sciences and to groups and individuals within them, also tends to engage more of the "non-administrator" scientists

[31] For a short inventory of roles in science, see Weiss (1971, pp. 29–30).

[32] For the general conception that each status has its distinctive complement of roles, or its role-set, see Merton (1968b, pp. 422–438).

in administrative activities: the preparation of prospectuses on work planned and of reports on work done, this in addition to the dissemination of the actual results of scientific investigation.

Although it is often (and loosely) included under "administration," a fourth role of the scientist needs to be distinguished from the others since it is basic to the systems of evaluation and the allocation of roles and resources in science. This is the gatekeeping role.[33] Variously distributed within the organizations and institutions of science, it involves continuing or intermittent assessment of the performance of scientists at every stage of their career, from the phase of youthful novice to that of ancient veteran and providing or denying access to opportunities.

The operation of the gatekeeper role affects contemporary science in its every aspect. First, with regard to the input and distribution of personnel, these scientists are asked to evaluate the promise and limitations of aspirants to new positions, thus affecting both the mobility of individual scientists and, in the aggregate, the distribution of personnel throughout the system. In American science, at least, and probably in other national communities of science, this gatekeeping function seems to involve a mixture of Turner's types of mobility: contest mobility based on role performance and reinforced by the norm of universalism and sponsored mobility in which elites or their agents help recruit their successors fairly early.[34]

Second, with regard to the allocation of facilities and rewards, the gatekeeper role, at least in the American social structure of science, operates largely through broad or narrow spectrum "panels of peers." These panels recommend and, in the usual event, determine the distribution of fellowships, research grants, and honorific awards. The term "panels of peers" refers to the fellow scientists of assumed competence in the fields in question and not, of course, to age peers, as we shall see in examining the age structures of groups of gatekeepers.

Third, with regard to the outputs of the variously allocated resources, the gatekeeper role is organized principally in the subroles of referees, charged with gauging the validity and worth of manuscripts submitted for publication, and of editors and editorial staff who make the final determination of what shall enter this or that archive of science.[35] Here again, we shall want to identify phases of their careers in which scientists tend to be most involved in these roles that help shape the permanent record of scientific work, and to find out whether there are distinctive age-related patterns in their performance of these roles.

[33] As is well known, the notion of the gatekeeper role was introduced into social science by Kurt Lewin (1943). Alfred de Grazia (1963) and Diana Crane (1967) refer to editors of journals as "the" gatekeepers of science. This usage is too restrictive; gatekeepers also regulate scientific manpower and the allocation of resources for research.

[34] For the general concepts, see Turner (1960); for their pertinence to the case of science, see Hargens and Hagstrom (1967); also Zuckerman (1970, pp. 243–247).

[35] Research on the operation of this role has lately burgeoned; for example, see Crane (1967); Whitley (1970); Zuckerman and Merton (1971).

ROLE-SEQUENCE AND ROLE-ALLOCATION IN SCIENCE

As we have noted, individual scientists have their own mixtures of these four roles, according different amounts of time and energy to each of them. At the extremes of specialization, scientists are engaged in one of these roles to the full exclusion of the others; more commonly, they will perform all of them in varying mix. For individual scientists, the question arises whether there are patterned sequences in preponderating roles during the life course. And for successive cohorts of scientists, the correlative question arises whether there are historical changes in the distribution of scientific roles.

In considering these questions, we should note that what is *role-sequence*[36] from the standpoint of the individual moving along the phases of his life course is *role-allocation* from the standpoint of the social system of science. Role-sequences, that is, the succession of roles or role-configurations through which appreciable proportions of people move in the course of their lives, are presumably affected by role-allocation, that is, patterned access to the structure of opportunity to engage in the various roles. The first deals with patterned career-lines, the other with (historically changing) processes and structures of role distribution. Concretely, individual preferences and social system pressures interact to produce the observed historical patterns of role-sequences.

Systematic data on role sequences are in short supply generally and all the more so in the sociology of science. An approximation to investigation of role-sequences is provided by studies of "occupational careers,"[37] which, however, tend to deal with patterns of mobility from one occupation to another rather than with patterned sequences of role-configurations for individuals remaining within the same occupation. For the field of science, there is a unique set of data assembled by Lindsey Harmon (1965), which traces the succession of role-complements for six cohorts of American scientists receiving their doctorates in ten major fields of science at five-year intervals from 1935 to 1960. Substantively, the Harmon Report provides incomparable clues to the patterned sequences of roles for American scientists in the last generation or so; procedurally, the Harmon data exemplify the great difficulties of disentangling from such cohort analysis the components of role-sequences in life course and of historical shifts in role-allocations, difficulties to which we have

[36] For the general conception of status- and role-sequences, see Merton (1968b, pp. 434–438).

[37] For instructive examples of the difficult art of "career analysis," see Wilensky (1960 and 1961); Miller and Form (1964).

been alerted by the Riley-Johnson-Foner model of age stratification [*see also Chapter 2 and the Appendix*].[38]

Table 8 • 3, drawn from the Harmon Report, summarizes the average proportions of time which six cohorts estimate[39] they have assigned to various role-activities at successive periods in their careers. The evidence bears upon both role-sequences in individual life courses and upon social changes in the role structure of science.

First of all, for each cohort, the figures across the rows show a steady decline during the *life course* in the proportion of time assigned to research and a steady increase in the time assigned to administration.[40] Teaching, like research, also tends to decline over the life course, with the interesting exception of 1950, when each cohort increased the relative time devoted to teaching. This deviation from the general life course pattern apparently reflects, as Chapter 2 alerts us to note, the impact of unique historical events upon the age strata. For the observed historical bump in role-sequence probably represents the additional teaching that came with the rapid expansion in "GI" programs of education just after World War II.

Second, Table 8 • 3 also reflects *historically changing* patterns in the distribution of time American scientists assign to their several roles at each stage of their careers. This can be seen by inspecting the left-to-right diagonals in the first two parts (a and b) of Table 8 • 3. Consider, for example, the top diagonal of those having just received the doctorate. Each more recent cohort tends to devote less of its aggregate time to teaching and more to research. The historical trend at each of the other stages of the scientists' careers also approximates this decrease in aggregate cohort time assigned to teaching and the increase in that assigned to research, except for the dramatic departure of the 1945 cohort (note that the 1945 row tends to fall out of line with each of the diagonals in parts a and b of the table). The experience of the great influx of World War II veterans into colleges and universities seems to have left an enduring imprint upon the 1945 cohort which, just entering upon their careers at the time, continue at each succeeding time period to interrupt the general cohort trend of less teaching and more research.

Interestingly enough, the cohort trends in teaching and research are not accompanied by complementary trends in administrative activity (shown in

[38] The cohorts of scientists in this study are thus of one kind identified in Chapter 1 • 1 • D: aggregates of people who share not a common date of birth but a common date of entry into the field.

[39] Grateful for these incomparable data, we do not here discuss the question of possible response error deriving from the fact that these retrospective estimates of the allocation of time cover periods ranging up to 25 years. Harmon is thoroughly aware of the problem and internal evidence suggests that errors in reporting are random rather than systematic.

[40] We report the full table from Harmon but attach no significance to the category of "all other" activities. At best, this is a catchall with unidentified ingredients. Moreover, it does not appear to be patterned by age in any systematic fashion.

TABLE 8 • 3 *Distribution of time assigned to their various roles by selected cohorts of American scientists*

(Years since Ph.D. shown in parentheses)

Date of Ph.D.	Work period: 1935	1940	1945	1950	1955
a. Percentage of time devoted to teaching					
1960					
1955					(0) 34
1950				(0) 40	(5) 34
1945			(0) 41	(5) 42	(10) 36
1940		(0) 42	(5) 30	(10) 33	(15) 28
1935	(0) 47	(5) 44	(10) 35	(15) 36	(20) 33
b. Percentage of time devoted to research					
1960					
1955					(0) 48
1950				(0) 45	(5) 41
1945			(0) 42	(5) 36	(10) 34
1940		(0) 42	(5) 40	(10) 36	(15) 33
1935	(0) 36	(5) 33	(10) 32	(15) 29	(20) 28
c. Percentage of time devoted to administration					
1960					
1955					(0) 8
1950				(0) 7	(5) 16
1945			(0) 11	(5) 16	(10) 22
1940		(0) 8	(5) 18	(10) 22	(15) 30
1935	(0) 8	(5) 14	(10) 23	(15) 28	(20) 30
d. Percentage of time devoted to other functions					
1960					
1955					(0) 9
1950				(0) 7	(5) 8
1945			(0) 6	(5) 7	(10) 8
1940		(0) 8	(5) 12	(10) 9	(15) 9
1935	(0) 9	(5) 8	(10) 10	(15) 8	(20) 9

Source: Harmon (1965, p. 65) (adapted).

the diagonals of Table 8 · 3, part c). The absence of any consistent trend here raises some question about the nature of the historically increasing bureaucratization of science. To be sure, each cohort of scientists devotes relatively more of their aggregate time to administration as they age. But, age for age, contemporary scientists devote no larger proportion of their aggregate time to administration than did scientists in past years.[41]

A third type of comparison of the figures in Table 8 · 3, comparison down the columns, reveals for each time period the combined effects of the life-course patterns, cohort trends, and unique historical events we have described. Thus the column for a particular year represents the structuring of scientific roles among the *age strata*. Here we find that, although the age strata do not differ substantially in time devoted to teaching, they do show striking differences in research and administration.

At any given time in the past quarter-century, the younger the stratum of scientists, the more of their aggregate time they devote to research and the less to administration. In 1960, for example, this ranges in linear progression from 26 per cent of all work time being assigned to research by the 1935 cohort (then 25 years past the Ph.D. and presumably in their early 50's) to 48 per cent by the most recent cohort of 1960 (then having just received the doctorate and presumably in their late 20's). Put more generally, these results suggest that the social system of science provides more time for the research role to younger than to older scientists. Like the youthful age structure of science generally, this distribution of roles accords with the widespread ideology[42] that holds that "science is a young man's game."

These data, representing aggregate averages for cohorts, of course provide only a rough approximation of individually patterned role sequences among scientists. They do not indicate the *composite* patterns of time that scientists allocate to their various roles at each phase in their careers. Nor, unlike panel data, do they indicate changes in these patterns for individual scientists during the course of their careers.

Partial but suggestive evidence on the individual patterning of roles can be found in the Harmon data (1965, Tables 9–11, pp. 19–21; Appendix 6, Tables A, B, C, and D). Of particular interest are the age patterns of specialization that can be identified. While there are no notable age differences in the proportion spending full time on some one activity in their current jobs[43]—on teaching or research or administration—there are striking differences among the age strata in the *type* of specialization that does tend to occur. These differences parallel those found in the cohort analysis of Table 8 · 3 above. Thus the young are far more likely than the old to give the major portion of their time to research; conversely, the older strata are more likely to specialize in administrative roles (while age differences in teaching are not pronounced). For example, Harmon finds that

> The proportion of scientists devoting no time at all to research on their present job is twice as large in the oldest age category as in the youngest; the proportion devoting full time to research is half as large.
>
> Among the oldest, the percentage spending full time in administration is four times as large as in the youngest stratum.

The same general patterns emerge in examining the *composites* of roles performed by scientists in the several age strata. For example:

> In the oldest age category, about half who do no teaching also do no research, most of these specializing in administration.
>
> In the youngest category, of those who do no teaching, 70 per cent spend their time predominantly in research.

The Harmon data contain additional clues that the greater emphasis on research among younger scientists reflects some attrition over the life course of the research role within the composite of roles performed by individuals. Comparing allocations of time on the first job held by scientists with allocations on the current job, Harmon emphasizes the modal tendency toward *persistence* of role-patterning by individuals over the life course.[44] His data also show, however, that among those who do shift, those decreasing the proportion of time devoted to research far outnumber those enhancing their research roles.[45]

Data such as these identify major patterns of role-specialization. But they tell us nothing, of course, about the kinetics of role-sequences and role-retention, the social and psychological mechanisms through which and the

[41] Aggregate data of this kind do not allow us to distinguish between role-specialization in the form of full-time administrators and other changes in the distribution of time among the several role-activities by individual scientists. As noted earlier in this section, both types of change are often and indiscriminately caught up in the phrase "the bureaucratization of science." It should be noted also that changes in the proportions of scientists employed by universities, government, and industry affect the observed historical patterns.

[42] We describe this as ideology since it includes both idea and norm, both what is assumed to be and what should be. It is of course only one component in the ideology about the roles of young and old in science.

[43] As can be observed by adding the relevant percentages in Harmon's Table 9 (p. 19), 31 per cent of the youngest age category, 27 per cent of the middle stratum, and 30 per cent of the oldest category devote full time to either teaching or research or administration.

[44] Harmon (1965, pp. 19–22). Note that all cohorts are combined in this portion of the analysis.

[45] Derived by comparison of the summated frequencies in the upper-right diagonal (increasers) with the lower left diagonal (decreasers) of Harmon's Table 11 (p. 21). A similar, though less pronounced, tendency to attrition is apparent through parallel analysis of individual shifts in the teaching role (Table 10, p. 20), clearly pointing to administration as the activity compensating for declines in research.

structural contexts within which the observed patterns come about. These largely remain matters for speculation.

MECHANISMS OF ROLE-ATTRITION AND ROLE-RETENTION

The ideological accent on youth in science provides part of the context for shifts in roles. In its extreme form, the doctrine holds that the best scientific work is done early in the career with nothing of consequence to be expected after that. P. A. M. Dirac, one of the more powerful minds in theoretical physics, found occasion to express this gloomy version, partly in parody, partly in sadness:

> 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.[16]

On this view, the scientists who have made significant contributions early in their careers burn out soon afterwards. And the many more who have done little in their early years can count on doing even less later on. For both, continuing to do research with advancing age is at best an act of self-deception. This extreme form of the ideology typically includes the premise that each scientist has within him a fixed quantum of contributions to make and that this is soon exhausted.[17]

A less severe version of the ideology, reinforced perhaps by Lehman's widely publicized and somewhat misleading data, holds that creative work peaks early in the scientist's life and diminishes more or less rapidly both in extent and consequence. The edge is sometimes taken off this version by noting that what age loses in creative powers, it can gain in mature experience. This provides a rationale for continuing in the research role with a degree of restrained optimism. John von Neumann, known for contributions of the first order to several branches of mathematics, took this to be the case for his own field:

> When a young man, he [von Neumann] mentioned to me several times that the primary mathematical powers decline after the age of about twenty-six, but that a certain more prosaic shrewdness developed by experience manages to compensate for this gradual loss, at least for a time. Later the limiting age was slowly raised (Ulam, 1969, p. 239).

[16] It is appropriate that Dirac should have formulated his mathematical theory describing the relativistic electron when he was 26 and that he became a Fellow of the Royal Society at 28 and a Nobel laureate just over the watershed age, at 31.

[17] The idiom puts it that scientists have no more than a paper or book "in them." Without at all subscribing to the total ideology of youth, Price and Beaver (1966) press the idiom further by referring to coauthors who manage to "squeeze out" of themselves the fraction of a paper that is in them.

Without assuming that ideology determines behavior, we should note that the extreme version of the ideology of youth in science thoroughly undermines the case for continuing in the research role, while the moderate version provides it with no great support. All apart from other considerations, this is the sort of ideological climate that we would expect to make for attrition of the research role, just as the Harmon data indicate.

There is reason to believe that the general pattern of shifts from research to other roles holds more for journeymen scientists than for the more accomplished scientists. Sociological theory leads us to expect and scattered evidence leads us to believe that the more productive scientists, recognized as such by the reward system of science, tend to persist in their research roles, forcing death rather than retirement to spell the end to their research careers. One piece of evidence deals with the scientific ultra-elite, the Nobel laureates. Compared with less distinguished scientists matched with them in age, specialty, and type of organizational affiliation, the laureates begin publishing research earlier in their career and continue to publish longer (Zuckerman, 1967, pp. 392–393). On the average, the laureates were not quite 25 years old at the time of their first papers, while scientists in the matched sample were past 28. What is more in point in the matter of role-retention is the publishing record toward the other end of the career. Of the nine laurates and their matches who had passed the age of 70, all the laureates but only three of the paired scientists continued to publish, indicating that they have more staying power in the research role. In part, this may result from their being subject to consistently greater expectations, both from others in the immediate and extended environment, to remain productive in research and in part, from their having established routines of work, also supported by the environment. One laureate, then past 80, reports that he feels no obligation to continue doing research—as he puts it, "After all, enough is enough"; nevertheless, his papers continue to appear in the scientific journals.[18] The oldest laureate, F. P. Rous, was described as "still hard at work" at the michelangelical age of 87.

Retention of the research role, or its attrition, among scientists ranked high in accomplishment seems to be affected also by their selection of reference individuals and reference groups for self-appraisal. Some take their own prior achievements as a benchmark and conclude that the prospects are slight for their maintaining that standard. They become more receptive to the opportunities for taking up other roles: administering research organizations, serving as elder statesmen to provide liaison between science and other institutional spheres or, occasionally, leaving the field of science altogether for

[18] In her restudy of 54 eminent scientists, 17 of them over the age of 65 at the time of her revisit, Anne Roe (1965) also found that they tended to persist in their research, even when taking up administrative roles.

ranking positions in university administration or international diplomacy. Other eminent scientists take the run of scientists as their reference group. They conclude that even if youthful peaking has occurred for them, they will continue to be far more productive, even on the assumed down slope of their careers, than most other scientists at the peak of their careers.

> *Query:* The generic problem of the determinants of selection of reference groups remains unsolved. Taking the matter of role-retention by scientists as a strategic case in point, we ask: What leads some scientists, highly productive in their youth, to take this as a reference mark and to anticipate *relative unproductivity* in the future while other scientists, equally productive in their youth, anticipate *relative productivity* as they compare their work with that of most scientists even in their most productive years.

The value system of science can make for a retention of the research role in spite of the ideology of research as essentially a young man's game. Of the various roles in the institution of science, greatest value is attached to research, theoretical and experiential. As a result, the self-esteem of scientists once effectively engaged in research depends greatly upon their continuing to do research, even though they may be plagued by doubts stemming from the ideology of youth. Beyond that, many scientists, precisely because they are minds trained in scientific inference, realize that even if scientific productivity or creativity does decline with aging for most scientists, it of course remains unsound to assume that this must hold for any particular scientist.

Conducing to retention of the research role is the comparative ambiguity about the kind and number of contributions to knowledge that would justify one's continuing in research. Since few make pathbreaking contributions, even an occasional craftsmanlike piece of work may be enough to maintain the self-conception of being engaged in research. This is particularly the case for academic scientists who, in the aggregate, appear to devote much the same proportion of their time—about one-fifth—to research during the greater part of their active career. It is the scientists in nonacademic employment, whose research productivity is presumably gauged in more utilitarian terms, that successively devote less of their time to research and more to administration.[49] This pattern suggests that the criteria of what constitutes "satisfactory research" differ within the social subsystems of science in academia and industry with consequent differences in rates of role-retention.[50]

Patterns of role-retention and attrition probably differ also among the various levels in the social stratification of science. For there are socially stratified differences in opportunity-structure and in socially patterned pressures in

[49] The data on these patterns are set out in the second career patterns report following up the Harmon Report (National Research Council, 1968, p. 53).

[50] For apposite observations, see Marcson (1960) and Glaser (1964).

science as in other departments of social life. Eminent research scientists are often subject to cross-pressures. On the one hand, in accord with the principle of cumulative advantage, their earlier achievements in research ordinarily provide them with enlarged facilities for research. On the other, the prestige they have gained in the research role often leads them to be sought out for alternative roles as advisors, sages, and statesmen, both within the domain of science and in the larger society.

In the main, however, the socially reinforced commitment to research seems to prevail in the upper reaches of the stratification system. This occurs even though there appears to be a ratchet effect operating in the careers of scientists such that, once having achieved substantial eminence, they do not later fall much below that level (although they may be outdistanced by newcomers and so suffer a *relative* decline in prestige). Once a Nobel laureate, always a Nobel laureate. But the reward system of science makes it difficult for laureates, if we may put it so, to rest on their laurels. What appears from below to be the summit of accomplishment becomes, in the experience of those who have reached it, only another way station. Each contribution is defined only as a prelude to other contributions. Emphatic recognition for work accomplished, in this context, tends to induce continued effort, serving both to validate the judgment that the eminent scientist has unusual capacities and to testify that these capacities have continuing potential. Such patterned expectations make it difficult for those who have climbed the rugged mountains of scientific achievement to call a halt. It is not necessarily their own escalating Faustian aspirations that keep the more accomplished scientists at work. They are subject to the enlarged expectations of their peers and reference groups. More is expected of them, at least for the time, and this environment of expectation creates its own measure of motivation and stress. Less often than might be imagined is there repose at the top.[51]

Although socially reinforced motivation for continuing in research may be greater for high-ranking scientists, they are far from absent for the rest of us who know ourselves to be, at best, journeymen of science. For one thing, our own more modest contributions can be compared with those of even less distinction, as we select reference groups and individuals that sustain our self-esteem. For another, the prevailing imagery of science as a vast collectivity in which each contributes his bit to build the cathedral of knowledge also helps to maintain the ordinary scientist in his research role.[52] Nevertheless, this would not seem to provide the same degree of social reinforcement that accrues to outstanding scientists.

[51] This account of the process of socially reinforced aspirations and of consequent role-retention draws largely upon Merton (1968a, p. 57).

[52] On this imagery of science and for evidence bearing on its validity, see Jonathan Cole (1970).

All these observations suggest the hypothesis that at[illegible]tion of the research role and enlargement of teaching, administrative, and other roles will tend to occur earlier and relatively more frequently among scientists lower in the stratification system of science. So far as we know, there has been no systematic investigation of this conjecture, although Harmon's cohort data for 10,000 scientists could be adapted to the purpose by incorporating indicators of standing in the field in the body of data already in hand. However, some evidence does bear tangentially upon the conjecture. Zuckerman (1972, Chapter 6) provides qualitative evidence for the reinforcing character of recognition in the early years of research for Nobel laureates and Stephen Cole and Jonathan Cole (1967, pp. 388–389; also J. Cole and S. Cole, 1972, Chapter 5) have found for a sample of American university physicists that the more recognition for their early work received in the form of citations by variously productive physicists, the more often they continued to be productive in research. Since degrees of recognition by the community of scientists make for location in the stratification system, this evidence is at least consistent with the hypothesis.

The patterns of shifting from research to other roles are not all of a kind. They differ phenomenologically and in their social and psychological mechanisms. In one pattern, the shift expresses a change in the values of the scientist or an enlarged access to alternative roles, which, in some sense, are more highly rewarding to him than research. In either case, the change represents a pull of the new role rather than a push from the old. The scientist searches out the shift rather than having it imposed upon him. He does not doubt his continuing competence to do research. He simply prefers another role that seems more significant to him. He may be responding to rapidly changing values in the larger society or modifying his values in more idiosyncratic fashion or simply finding an administrative post, with its better pay and greater power, more attractive to him. He perceives the change as one of extending his scope, perhaps by helping to shape the changing place of science in the society or by helping strategic publics to understand the risks, costs, and benefits of science and science-based technology.

In other cases, the scientist finds that his research no longer measures up to his standards and so takes little satisfaction in continuing with it.[53] He turns to an alternative role. This class of changing roles is sociologically unproblematical, requiring little interpretation.

[53] We should perhaps remind ourselves that to note the faults in the Lehman kind of data on age and scientific productivity does not mean that there is no relation between the two. The quantity and quality of scientific output may in fact decline for the aggregate of scientists in the later years and, in any case, such declines are known to occur for individual scientists (just as for occasional others, research continues unabated or occasionally expands). We refer here to those scientists who experience a declining research output and so are motivated to take up other roles in science.

A superficially similar but actually quite different pattern of shift in roles is that of the private self-fulfilling prophecy. In these cases, the scientist would prefer to go on with research. But he has become persuaded that he is approaching an age at which his creative potential, great or small, is bound to decay. Rather than continue in a role in which he believes himself destined to fall increasingly short, he makes a pre-emptive shift. He assumes new administrative responsibilities, turns more of his attention to teaching, becomes active in the public business of science. Once the premise of his prospective diminishing capacity for research is accepted, the pre-emptive adaptation becomes entirely sensible. But as with every kind of self-fulfilling prophecy, the question is, of course, whether the original premise leading to the behavior which seemingly validates that premise was sound in the first place.

Another pattern of shift from the research role also involves a self-fulfilling prophecy but in its more consequential public form. Here the shifts in role are system-induced, not personally induced. The process is set in motion not by the individual scientist's own definition of his capacity to continue doing research but by the institutionalized belief that the amount and quality of scientific output generally deteriorate badly after a certain age. To the extent that this belief is incorporated in policy, some older research scientists reluctantly find themselves elevated into administrative posts and others find their facilities for research limited. Subsequent declines in research output with age seem only to confirm the soundness of the policy and are taken as fresh evidence for the general validity of the belief in declining productivity with age.[54]

Both kinds of self-fulfilling prophecy, the self-generated and the socially generated, interact and reinforce each other. Social assessments of role-performance come to be reflected in self-images, and behavior in accord with those self-images tends to make for the patterned social assessments. What interests us here is the possibility that appraisals of the research may turn out to be stratified by age, with younger scientists being especially critical of older scientists. Consider as a case in point the psychological and sociological bases for the ambivalence of apprentices to their masters.[55] In the psychological analysis of the pattern, the apprentice esteems the master and takes him as a role-model while also aiming to replace the master who, after a time, stands in his way. Without assuming that such ambivalence is typical, we can readily identify many instances in the history of science: Kepler's strong ambivalence toward Tycho Brahe; Sir Ronald Ross's toward his master Manson in the quest for the malarial parasite, his devotion to his teacher pushing him

[54] For a statement of the social costs involved in policies of "premature retirement" from research in socialist countries, see Szafer (1968, pp. 33–34).

[55] The following passage on ambivalence draws almost verbatim on Merton and Barber (1963, pp. 92–93).

to extravagant praise, his need for autonomy pushing him to excessive criticism. Or consider, appropriately enough, the checkered history of psychoanalysis itself with the secessionists Jung and Adler displaying their ambivalence toward Freud; in sociology (to come no closer home to our own day), the mixed feelings of the young Comte toward Saint-Simon; in psychiatry, the mixed feelings of Bouchard toward Charcot; and in medicine, of Sir Everard Home toward John Hunter; and so on through an indefinitely long list of apprentice-master ambivalence in science.

The probabilities of such ambivalence of apprentices toward masters—or, to put the matter more generally, of younger toward older scientists—presumably differ according to the context provided by the social structure of science. For example, ambivalence may be more apt to develop when, owing to the paucity of major chairs in a field, the talented apprentice finds that he "has no (appropriate) place to go" after he has made his mark other than the place occupied by the master (or others like him). But if the social system of his science provides an abundance of other places, some as highly esteemed as that currently occupied by the stratum of masters, there is less structurally induced motivation for ambivalence. And by the same token, the masters, in the reciprocity of relations, may be less motivated to develop ambivalence toward the apprentices who, in more restricted circumstances, might be competing with them as "premature" successors.

> *Query:* Do age-stratified cohorts of scientists tend to adopt criteria differing in stringency if not kind in assessing the research of others or does a commonality of criteria transcend age differences? To what extent do cohorts agree in judging the research accomplishments and continuing potential of leading scientists in their field? Do these patterns differ according to the "market situation" in the various sciences and within the same science in various social systems that differ in the opportunity-structure for young scientists?

The various patterns of role-change in the life course involve an interplay between the individual's own expectations and those prevailing in the relevant social environment. This means, of course, that role-changes are affected both by developments distinctive to individuals and by trends in their environment. Individuals experience the social correlates of their own aging in particular social contexts. The contexts affect the meaning they attach to those changes and their adaptations to them. Early retirement from the research role should thus have different consequences for successive cohorts of scientists who come upon this experience at differing points in the historically evolving social structure of science. For scientific investigators to turn to the role of science administrator or science educator at a time in which such changes are relatively infrequent is quite another kind of experience than doing so when it has become common. In complementary fashion, both the probability and consequences of such shifts from research to other roles differ according to the changing degree of support—economic, technical, and social—available for research. The rapid growth in such resources has meant, for example, that the advanced graduate student or newfledged Ph.D. can now obtain technical help and services not available to seasoned investigators a generation ago (cf. Kusch, 1966, p. 12). This change may directly affect the age of entry into consequential research and indirectly affect the competitive positions of the various age cohorts of research scientists.

The Riley-Johnson-Foner model and Pinder's striking formulation of "the noncontemporaneity of the contemporaneous"[56] both suggest to us that the various age cohorts of scientists will tend to perceive the allocation of resources and the role structure of science from differing perspectives. For the newest cohorts, coming into science at a time of abundance, the availability of resources is largely a matter of ordinary expectation. After all, this is all they know from their own direct experience. The older cohorts tend to see this as drastic change, and not necessarily all for the better, as they nostalgically and sometimes invidiously contrast the current affluence to their own difficult days as novice investigators when outside resources were meager and inner resources all-important.

Other age-connected differences in perspective may derive from the allocation of roles within the changing status-structure of science. Younger scientists often come to see the positions of power practically monopolized by older scientists. For although the professionalization and institutionalization of science and the great growth in the resources of science have multiplied the number of policy-making roles, it may be that the exponential increase in numbers of scientists, all apart from other processes, has tended actually to decrease the proportions of the newer cohorts in these positions and to raise the age at which they enter them.

These few observations on the differing perspectives of younger and older scientists might seem to imply that the relations between these cohorts are dominated by stress, strain, and conflict. But to focus on the structure and

[56] "*Die 'Ungleichzeitigkeit' des Gleichzeitigen*" is the seemingly paradoxical phrasing adopted by the art historian, Wilhelm Pinder, to introduce his distinction between *Gleichzeitigkeit* (contemporaneity or temporal coexistence) and *Gleichaltrigkeit* (coevality, coetaneity or the condition of age cohorts). Consider this germane passage: "*Jeder lebt mit Gleichaltrigen und Verschiedenaltrigen in einer Fülle gleichzeitiger Möglichkeiten. Für jeden ist die gleiche Zeit eine andere Zeit, nämlich* ein anderes Zeitalter seiner selbst, *das er nur mit Gleichaltrigen teilt. Jeder Zeitpunkt hat für Jeden nicht nur dadurch einen anderen Sinn, dass er selbstverständlich von Jedem in individueller Färbung erlebt wird, sondern—als wirklicher 'Zeitpunkt,' unterhalb alles individuellen—schon dadurch, dass das gleiche Jahr für einen Fünfzigjährigen ein anderer Zeitpunkt* seines *Lebens ist, als für einen Zwanzigjährigen—und so fort in zahllosen Varianten*" (Pinder, 1928, Chapter 1 and p. 11). This sort of observation on contemporaneous age-cohorts and their perspectives is fully caught up in the Riley-Johnson-Foner model.

processes making for tension and conflict is not, of course, to say that these are all. We have noted the integrative aspects of complementary age-connected roles in the process of socialization in science where, perhaps more often than in other disciplines, the roles of teacher and student soon become transformed into those of research colleagues. The differentiated age structure of research groups provide bases for cooperation as well as conflict. It is probably in the politics of science that conflict between the age strata of scientists runs deep (Greenberg, 1967, Book One).

4 *Gerontocracy in science*

The claim that the organization of science is controlled by gerontocracy is anything but new. Complaints to this effect appeared as early as the seventeenth century and perhaps before. But the vast historical changes in the scale and power of science greatly intensify and complicate the problems of social control.

DYSFUNCTIONS OF GERONTOCRACY

Neither empirical evidence nor theoretical reason leads us to suppose that rule by elders is more characteristic of science than of other institutional spheres. Gerontocracy may turn out, in fact, to be less marked in science. But it can be argued that rule by elders is apt to be more dysfunctional for science than for other institutions. For although the Ogburnian notion that science and technology develop and change more rapidly than other parts of civilization and culture has yet to be empirically demonstrated,[57] we do know that the values of science call for maximizing the rate of developing knowledge and the procedures and equipments required to advance that knowledge. And with the great expansion of the personnel and resources of science, scientific knowledge has for some time been growing at an accelerating rate. Now it is an old and plausible sociological maxim, though one more often announced than confirmed by actual investigation, that the higher the rate of social and cultural change, the less the advantage of age, with its obsolescent experience.[58]

[57] Ogburn advanced this idea in his classic *Social Change* and developed it further in several monographs. Its validity has been questioned, principally by Sorokin; see Ogburn (1922); Sorokin (1937, Volume 4).

[58] The observation has been made in one form or another through the centuries. Here is how Roberto Michels put it in 1911: "The ancient Greeks said that white hairs were the first crown which must decorate the leaders' foreheads. Today, however, we live in an epoch in which there is less need for accumulated personal experience of life, for science puts at every one's disposal [such] efficient means of instruction that even the youngest may speedily become well instructed. Today everything is quickly acquired, even that experience in which formerly consisted the sole and genuine superiority of the old over the young. Thus, not in consequence of democracy, but simply owing to the technical type of modern civilization, age has lost much of its value, and therefore has lost, in addition, the respect which it inspired and the influence which it exercised" (Michels, 1949, p. 76).

It has been further argued, most emphatically by the distinguished physicist J. D. Bernal, whose own major contributions to crystallography have continued in his seventh decade, that "the advances in basic conceptions have become so rapid that the majority of older scientists are incapable of understanding, much less of advancing, their own subjects. But nearly the whole of what organization of science exists, and the vital administration of funds, is in the hands of old men"(Bernal, 1939, p. 116 and also pp. 290–291). Bernal suggests, moreover, that as science expands in numbers, complexity, and influence, and as it becomes more closely linked with government, industry, and finance, its control is increasingly exercised by older scientists.

Plausible as these observations are, they have yet to be systematically investigated. The fact is that we do not know the comparative extent of gerontocracy in science and in other principal institutional spheres. Nor do we know whether the vast expansion of science has brought with it enlarged control by gerontocrats. Nor, finally, do we know whether gerontocracy is more dysfunctional[59] for the development of science and for the society than alternative forms of age-patterned control, such as proportional age representation or, at the other extreme, juvenocracy.[60] Since comprehensive evidence on these complicated questions is absent, here are a few straws in the wind.

EVIDENCE OF GERONTOCRACY: THE NATIONAL ACADEMY OF SCIENCES

Consider the age composition of the National Academy of Sciences, the influential organization of scientists established during the Civil War and designated by Congressional charter to advise the federal government on matters of science. Apart from its own membership of some 900, the Academy draws upon thousands of other scientists through its operating adjunct, the National Research Council. Designed as an honorary society as well as an advisory body, the Academy is not likely to have a membership numerically representative of the entire national population of scientists: in regional distribution, university affiliation, age, or anything else.

The average age of Academy members is 62, with about a quarter of them being 70 or older. In 1969, three-quarters of the members of advisory committees and panels of the National Research Council were over 45; a third over 55. This contrasts with the median age of 41 for all scientists (holding doctorates) in the United States in 1968, with a quarter of these being under

[59] It has sometimes been said in wry or acerb mood that gerontocracy may even be a good thing in science; it leaves the young productive scientists free to get on with their research and helps to occupy the time of those who are no longer creative.

[60] To the extent that existing social structures tend to be reflected in language, there is perhaps a certain interest in noting that while the word "gerontocracy" has been around for at least two centuries, the word "juvenocracy" appears here, so far as we know, for the first time. It is, unfortunately, a hybrid. But a language which has absorbed such inelegant hybrids as "electrocution" and even "sociology" can surely make room for a much-needed another, such as "juvenocracy."

TABLE 8 • 4 *Mean age at election to National Academy of Sciences according to organizational affiliation of scientists, 1863–1967*

Affiliation	*Mean age*	*Number*
Major universities.	48.9	843
Government	51.5	141
Other universities and colleges	51.8	285
Industry	53.3	70
No affiliation	53.7	54
Retired	66.8	12
		1405
	No information	8
	Total	1413

Source: Zuckerman (1972, Chapter 6).

the age of 35. Contrasting age distributions such as these give high visibility to the pattern of gerontocracy.[61]

The elite character of the National Academy plainly affects its age composition. Scientists are seldom elected to membership on the basis of a single contribution to science, however outstanding: a continued record of contributions is ordinarily required. Young talented scientists are left to ripen on the vine before they are picked for membership. Moreover, scientists drawn from the various sectors of employment are evidently judged to meet the Academy's criteria for membership at different ages, as can be seen from figures in Table 8 • 4.

The scanty data we have assembled on the National Academy indicate no continuing historical trend toward recruiting older scientists. As early as the turn of the century, when the astronomer George Ellery Hale was elected to the Academy at the very early age of 35, he described it, in the words of a friend, "as more interested in keeping young men out of its membership than in acting as a vital force in the scientific development of the United States."[62] The mean age at time of election continued to rise until 1940 but has since remained fairly constant at a somewhat lower level as we see in Table 8 • 5.

Contrary to first impression, historical patterns of this sort may help account for the belief that positions of prestige and power in science are increasingly held by older people. For even in cases when the age at which they acquire these positions has stabilized or declined somewhat, this has been occurring in a period when exponential growth has been producing an increasingly youthful population of scientists. This results in widened discrepancies of age between the governing and the governed and might be enough to pro-

[61] Concerned with this and kindred problems, the National Research Council has established a panel to examine the composition of advisory committees. These figures are drawn from the preliminary report of that panel.

[62] True (1913, p. 73). We are indebted for this reference to the preliminary report of the NRC panel on advisory committees.

TABLE 8 • 5 *Mean age at election to National Academy of Sciences, 1863–1967*

Time of election	*Mean age*	*Number*
Before 1900	47.0	195
1900–1919	49.2	158
1920–1939	51.7	252
1940–1959	50.5	522
1960–1967	50.7	286
		1413

Source: Zuckerman (unpublished analysis).

duce a sense of increasing gerontocracy. Moreover, when, as in the case of the National Academy, the status, once acquired, is retained for life,[63] the aging of the group is encouraged by the increasing longevity of its members.[64]

The age-distribution of those occupying the positions of power in science does not tell us, of course, how that power is exercised. Systematic inquiry, rather than swift assumption, is needed to find out whether there are age-patterned differences in policies and the exercise of power. Such studies have yet largely to be made.

EXERCISE OF POWER: THE REFEREE SYSTEM

One recent study of the referee system in science (Zuckerman and Merton, 1971, pp. 92–94) touches upon the question. Drawing upon the archives for the nine years 1948–1956 of *The Physical Review*, the outstanding journal in physics, the study examines the behavior of scientists of differing rank and age in the gatekeeper's role. The referee system calls for evaluation of manuscripts by experts on their subject. It comes as no surprise, therefore, that referees for *The Physical Review* were drawn disproportionately from physicists of high rank.[65] Compared with the 5 per cent of the 1,056 authors (themselves in some measure a selected aggregate),[66] almost 12 per cent of

[63] Members of the Academy recently rejected the proposal that they should become emeriti after the age of 75.

[64] The longevity of college graduates has been increasing in the United States for at least the past century.

[65] In the first rank are those physicists submitting manuscripts who, by the end of the period (1956), had received at least one of the ten most respected awards in physics (such as the Nobel prize, or membership in the Royal Society or the National Academy of Sciences). Physicists of the second rank, although they had not been accorded any of the highest forms of recognition, had been judged important enough by the American Institute of Physics to be included in its archives of contemporary physicists. The remaining contributors comprise the third rank in this hierarchy. Referees are ranked by the same criteria.

[66] The special nature of this sample of authors must be understood as (a) resulting from considerable preselection through decisions to produce and submit manuscripts; (b) consisting only of sole authors of manuscripts, with joint authors excluded; (c) consisting of a 20 per cent sample of third-ranked contributors, but of *all* first- and second-ranked physicists submitting singly authored manuscripts during the study period. Had the sample included all single authors of every rank, fewer than 2 per cent would be included among the first rank.

the 354 referees assessing their papers were in the highest rank. Moreover, these 12 per cent contributed one-third of all referee judgments. They refereed an average of 8.5 papers compared with 3.8 for the referees of intermediate rank and 1.4 for the rank-and-file. And although 45 per cent of the referees were under the age of 40, thus giving major responsibility to the relatively young, we know that physicists are altogether a youthful aggregate and research physicists particularly so.[67] Fully 74 per cent of the papers submitted to *The Physical Review* came from physicists under the age of 40.

The referees, then, are older and higher in prestige and rank than the authors or the general population of physicists. But, as we have noted, such a skewed age distribution among those holding power is simply a static indicator of structure; it provides no information about the functioning and consequences of that structure. Age-distribution does not in itself represent gerontocracy. For even when used descriptively rather than invidiously, the word "gerontocracy" ordinarily carries with it the notion that power disproportionately placed in the hands of the elder comes to be used to their own advantage or, in more moderate version, that it results in policies and decisions that differ drastically from those that are or would be adopted by younger power-holders. In the case of the gatekeeper role, we want to know, then, whether the behavior of referees is systematically affected by their own age and rank as well as by the age and rank of authors.

One piece of evidence takes us a certain distance toward gauging the extent to which the rejection and acceptance of manuscripts for publication was affected by the standing of referees and authors. In examining this evidence, we should note again that eminence in science derives largely from the assessed quality of past and not necessarily continuing scientific accomplishments. And we have found that, in science, as in other institutional spheres, positions of power and authority tend to be occupied by older men. From these joint patterns, it would seem that if sheer power and eminence greatly affect the decisions of referees, then manuscripts submitted by older eminent scientists should have the highest rate of acceptance.[68]

But at least in physics, the distinctively young man's science, this is not what we find. As we see in Table 8 • 6, it is not the oldest scientists whose papers were most often accepted but the youngest ones. And the age-graded rates of acceptance hold within each rank in the hierarchy of prestige. Both eminence and youth contribute to the probability of having manuscripts ac-

[67] It will be remembered from the first part of this paper that physics has the lowest median age among the several major fields of science and from our discussion of role-sequence that the physicists engaged substantially in research are the youngest of the lot.

[68] On the general hypothesis, see Storer (1966, pp. 132–134). This hypothesis assumes that the identity of the authors of manuscripts is known to the referees; this is the case for *The Physical Review*, which does not try to provide for anonymity of authors, since, it is maintained, this cannot be achieved in most cases. Referees, however, are generally anonymous.

TABLE 8 • 6 *Rates of acceptance of manuscripts, by age and rank of authors* (The Physical Review, *1948–1956*)

	Prestige rank of authors							
	Higher rank physicists		*Intermediate physicists*		*Third rank physicists*		*All ranks*	
Age of authors	*% Accepted*	*No. Papers*	*% Accepted*	*No. Papers*	*% Accepted*	*No. Papers*	*% Accepted*	*No. Papers*
20–29	—	—	91	287	83	385	87	672
30–39	96	80	89	519	77	440	85	1039
40–49	95	58	83	236	73	79	83	373
50+	80	87	71	126	50	14	73	227
No information on age							61	423
Total papers							80	2734

cepted; youth to such a degree that the youngest stratum of physicists in the third rank had as high an acceptance rate as the oldest stratum of eminent ones[69] whose work, we must suppose, was no longer of the same high quality it once was.

This is a first indication that the "gerontocratic" body of gatekeepers does not exercise its power by denying or restricting access of younger physicists to publication in the most widely read and most influential journal in the field. This still leaves open the possibility that it is not the age of the author as such but his age *relative* to that of his referees which systematically influences appraisals of his manuscripts. Such biases in judgment might take various forms, depending upon the pattern of relative age.[70]

When referees and authors are age-peers, an hypothesis of *age-stratum-solidarity* would have it that referees typically give preferential treatment to manuscripts just as a counterhypothesis of *age-stratum-competition* would have it that, under the safeguard of anonymity, referees tend to undercut their rivals by unjustifiably severe judgments.

When authors are older than referees, an hypothesis of *age-status-deference* would hold that the referees give preferential treatment to the work of the older established scientists just as a counterhypothesis of *age-status-envy* would have them downgrade the work of older scientists.

And when referees are older than authors, an hypothesis of *sponsorship*

[69] Some caution must be exercised in this comparison, however, since most of the scientists for whom no information on age was available are in the third rank. These scientists have a relatively low acceptance rate and, should they also be disproportionately young, could depress the acceptance figure for the younger third-ranking scientists below that for the oldest first-ranking.

[70] On the concept of relative age, see the Riley-Johnson-Foner model [*Chapters 1 • 1 • D; 10 • 1; also 9 • 2 by Hess*] and the theoretical analysis by Eisenstadt (1956, Chapter 1, *passim*).

or patronage would maintain that referees are unduly kind and undemanding while a counterhypothesis of *age-oppression* would have them overly demanding of the young.

Differing in other respects, these six hypotheses are alike in one: they all assume that the relative age of referee and author significantly biases the role performance of the gatekeepers, either in favor of the author or at his expense. More concretely, all the hypotheses assume that the rates of acceptance of manuscripts submitted by each age stratum of authors will differ according to the age of the referees passing judgment on them.

The data assembled in Table 8 · 7 seem to run counter to most of these hypotheses.[71] For the most part, the relative age of author and referee has no perceptible influence on patterns of evaluation. With one exception, both younger and older referees are more likely to accept the work of younger authors, a finding that parallels the age pattern of Table 8 · 6. And each age stratum of authors, again with one exception, has the same proportion of papers accepted by referees of differing age. Interestingly enough, the pattern of evenhanded treatment by older and younger referees holds even for that class of physicists who were not well enough known to have their ages listed in any of the standard registries of scientists.

The one exception to the general pattern appears in the youngest stratum of authors who have more of their papers accepted by older referees than younger ones. This tentatively identified exception is consistent with the hypothesis of acute competition within the age-cohort in the earliest phase of their careers and with the hypothesis that older scientists are less demanding in assessing the work done by progressively younger scientists. The data allow no unequivocal choice between the hypotheses. Either or both may obtain. In any case, neither the general pattern nor the limited departure from it exhibits distinctively gerontocratic patterns of evaluation among the gatekeepers of this major scientific journal.

This particular case is enough to suggest the general point: a skewed age distribution of scientists assigned authoritative roles is one thing; what they do in exercising their authority can be quite another. But, of course, we cannot conclude from this one inquiry that there are no patterns of age-graded evaluations or policy decisions in science. Much more research will be needed to examine and develop this major question. For example, just as the several sciences differ in age-structure, so they may differ both in the age distribution of authoritative roles and in age-patterned performance of those roles. We hazard the conjecture that the more theoretically codified the science the greater the consensus among the age strata in their patterns of evaluation.

[71] The separate effects of each of these hypothetical processes cannot, of course, be weighed by examination of a single set of cross-sectional data, since several tendencies may operate in tandem or in opposition to produce observed proportions.

TABLE 8 · 7 *Referees' decisions*[a] *to accept manuscripts, by age of authors and referees* (The Physical Review, 1948–1956)

	Age of referees					
	Under 40		*40 and over*		*Total judgments by referees*	
Age of authors	*% Accept-ances*	*No. Judg-ments*	*% Accept-ances*	*No. Judg-ments*	*% Accept-ances*	*No. Judg-ments*
20–29	59	*106*	76	*136*	68	*242*
30–39	63	*193*	63	*189*	63	*382*
40–49	63	*65*	58	*71*	60	*136*
50 +	43	*42*	43	*61*	43	*103*
No information	53	*106*	52	*96*	52	*202*
All ages	58	*512*	61	*553*	60	*1065*

[a] The data refer to the number of judgments, not papers, made by 344 external referees and do not include judgments by the two editors. The table omits 18 cases in which there is no information on the age of the 10 referees judging them. Since papers judged exclusively by the editors are omitted, the analysis is based on judgments of fewer than half the total papers reported in Table 8 · 6.

The more codified disciplines such as physics should exhibit less disparity between age cohorts than less codified disciplines such as sociology on all manner of evaluations: the comparative significance of problems requiring investigation and of contributions to the field as well as such questions of science policy as the allocation of resources to various kinds of research.

It should be noted also that the question of gerontocracy in the formation of broad science policy remains as much a matter of conjecture as the question of gerontocracy within scientific disciplines. We know, for example, that the mean age of members of PSAC (President's Science Advisory Committee) has been about 50, with the Eisenhower advisors being somewhat older and the Kennedy advisors somewhat younger.[72] But we do not know how far the age composition of this and of other advisory and policy-making groups affects the substance of science policy.[73] In exploring this question, we need to distinguish the age composition and the rates of turnover of these influential groups, recognizing that each may have its independent effect. It would come as no surprise to find that optimum science policy is apt to be developed neither by gerontocracy nor by juvenocracy but, like the community of scientists itself, by age-diversified meritocracy.

[72]

Year	*Mean ages of members of PSAC*		*No. of advisors*
1958	Eisenhower	53.8	(18)
1962	Kennedy	49.0	(17)
1965	Johnson	50.3	(15)
1969	Nixon	50.5	(11)

[73] On the general issue, see Rose and Rose (1969, pp. 266–268).

The extent to which significant interaction takes place within age strata and between them, together with the consequences of such patterns, are no better known for the domain of science than for most other institutional spheres. Although there has been no investigation to determine which activities in science tend to be age-segregated or age-integrated, it is evident that some basic functions of science are served through institutional arrangements involving interaction between age strata rather than separation of them. First among these is, of course, education and training both in the narrow sense of transmission of knowledge and skills and in the broader sense of socialization involving the transmission of values, attitudes, interests, and role-defined behavior. Since he has himself been variously engaged in the process of professional socialization, just about every scientist has his own opinion about how that process actually works. Yet the plain fact is that there has been little methodical investigation of that process in science.[74]

Queries: Which components in the culture of science are principally transmitted by older to younger scientists? And which are largely acquired from age peers? Do these age-channeled streams of socialization merge or diverge? Which of the values, interests, and patterns of behavior derived from differing age strata are mutually supporting, complementary, or at odds? Do the observed patterns of socialization tend to persist so as to be much the same for successive age cohorts or are they subject to change, among other things, in response to the changing boundaries, technology, problematics, and substance of the sciences? How does the age-patterned process of socialization differ among the various sciences and these, in turn, from socialization in other fields of learning (such as the humanities and technology)?

GROWTH OF RESEARCH COLLABORATION IN SCIENCE

Just as the early years of education in science generally provide in varying degree for interaction between age strata, so in particular with that advanced form of socialization that takes place through collaboration in research.[75] This form of socialization takes on growing importance as the social organization of scientific inquiry has greatly changed, with collaboration and research teams becoming more and more the order of the day. One pale reflection of this change is the sustained growth in the proportion of scientific articles published by two or more authors. Table 8 • 8 shows each successive

[74] But see Becker and Carper (1956) for the case of physiologists; Underhill (1966) for physical, biological, and social scientists; and Aran and Ben-David (1968) for medical researchers.

[75] This discussion of age and collaboration in research draws extensively on Zuckerman (1965b, pp. 394–396).

TABLE 8 • 8 *Percentage of multiauthored papers in the physical and biological sciences, social sciences, and humanities,*[a] *1900–1959*

Date of publication	*Physical and biological sciences*	*Social sciences*	*Humanities*
1900–1909	25 (928)	—	—
1910–1919	31 (1,686)	—	—
1920–1929	49 (2,148)	6 (2,643)	1 (1,822)
1930–1939	56 (3,964)	11 (3,905)	2 (2,088)
1940–1949	66 (4,918)	16 (4,328)	2 (1,972)
1950–1959	83 (9,995)	32 (6,605)	1 (2,304)
Total	66 (23,639)	20 (17,481)	1 (8,186)

[a] The figures were compiled by counting the number of authors of articles appearing in a sample of journals for two of every ten years. The physical and biological sciences include the fields of physics, chemistry, biology; the social sciences include anthropology, economics, political science, psychology, and sociology; the humanities, history, language and literature, and philosophy.

Source: Zuckerman (1965, pp. 55–104) (adapted).

decade of this century registering a higher percentage of multiauthored papers in the physical and biological sciences.[76] The social sciences begin this practice later and sparingly but then rapidly increase the rate of collaboration. Both contrast with the humanities, which have practically no place for collaborative research reported in scholarly articles.

RANK-STRATIFIED RATES OF COLLABORATION

Imperfect indicators of the actual organization of research as they are, the data on multiple authorship nevertheless raise some questions and suggest some conjectures. Does the practice of collaborative research obtain to the same extent on all levels in the social stratification system of science? More concretely, how does this stand with an institutionally identified elite, such as Nobel laureates, compared with the collaborative practices found in a sample of scientists matched with them in terms of age, field of specialization, and type of organizational affiliation? And how do the rates of collaboration compare at various ages and phases in the scientific career?

As a crude approximation to an answer—crude since the data do not allow us to compare age cohorts throughout their life course—Table 8 • 9 presents age-specific rates of collaboration for the laureates and their less elevated counterparts. Laureates are somewhat more given to collaboration, with 62

[76] For data based on other samples showing similar results, see Clarke (1964, pp. 822–824). We do not examine here the relation between actual patterns of research (individual, with and without assistants; collaboration of varying numbers of peers; small-team research and large-scale research) and the number of authors of papers published by these types of research formations. There is evidence that research patterns and size of author-sets are correlated, though not as closely as is sometimes assumed by the many investigators who have used authorship data as indicators of research practices. The question is considered in some detail in Zuckerman (1965, Chapter 5).

per cent of their papers being multiauthored compared with 51 per cent of those by the age-matched sample. The difference holds, moreover, at every age. Table 8 · 9 also exhibits a slight curvilinear relationship between age at publication and multiauthorship for both laureates and the matched sample. We cannot explain these patterns, but other evidence enables us to speculate on their sources.

Consider first the seeming curvilinear pattern in which collaboration appears to be more frequent in the middle years. We say "seeming" pattern not because we really doubt this set of data but only because we know that more extensive data on age cohorts are required to *establish* the authenticity and to discover the generality of the pattern. It must be admitted also that we give credence to the numerical data in hand for the worst of reasons: they tally with our conjectures about age-connected processes making for collaboration.

These processes can be reconstructed in terms of age-patterned opportunities and age-patterned motivations for collaboration. As novices being inducted into the mysteries of the craft, young scientists who collaborate at all with mentors will typically do so with only one of them at any given time. Beyond that, the young scientists, sometimes at the urging of their sponsors, are motivated to do their own work and to establish a public identity within the field by publishing papers of their own. Not quite paradoxically, the motivation of beginning scientists to publish single-authored papers may only be strengthened by the great historical increase in papers with several, sometimes many, "authors." For the distinctive contributions of the individual get lost in the crowd of scientists putting their names to the paper and this, as they know, is especially damaging for young scientists who have not published independent work that testifies to their abilities. On this view, the smaller proportion of collaborative papers published in the early years of the career results in part from the often stressful operation of the reward-system that has developed in science.

Toward the other end of the career, the dropoff in published research,

TABLE 8 · 9 *Percentage of multiauthored papers by age at publication, for Nobel laureates and a matched sample of scientists*

Age at publication	*Laureates*	*Matched sample*
20–29	58 (523)	40 (288)
30–39	65 (1,382)	55 (756)
40–49	66 (1,641)	53 (590)
50–59	60 (1,198)	51 (622)
60 or over	55 (768)	46 (264)
Total	62 (5,512)	51 (2,520)

Source: Zuckerman (1967, p. 395).

which we have noted in the data put together by Wayne Dennis (1956a, 1956b, 1956c, 1966) may mean that collaborators are no longer as available as before. It is in this phase also that scientists often turn to broader "philosophical" or "sociological" subjects of a kind that have no place for collaboration.

It is the middle years, then, that presumably provide both the greatest opportunity and deepest role-induced motivation for collaborative work. Should the curvilinear pattern of collaboration turn out to be fairly general, it need not be counteracted by the historical trend toward more and more collaboration in the sciences; for the reasons we have indicated, it may even become more marked in successive age cohorts.

Consider now the consistently higher rates of collaboration among laureates at each phase of their life course. Throughout our interpretation, we make the rather undemanding assumption that, *on the average*, laureates exhibited evidence of greater talent for research than a random assortment of other scientists of their age in the same field. This perceptible difference would have set certain consequential processes in motion—processes such as self-selection and selective recruitment. In their 20's, as their capacities became identified, laureate-to-be were more often selected as apprentices by scientists of assured standing. (Although it would be too much to say that laureates are bred by laureates, it is the case that 42 of the 83 American prizewinners worked, as younger men, under a total of 54 older laureates.) There is reason to assume that these masters were more willing to grant coauthorship to their apprentices than were those of less elevated and secure standing.[77] This would make for the higher rate of published collaboration by laureates-to-be in their youth.

The life chances of scientists are greatly improved by having their substantial abilities identified early.[78] By the time they were in their 30's, every laureate had a position in a major university or research laboratory providing a micro-environment of other scientists in his specialty. By that time, many of the future laureates were making the status transition from junior to senior collaborator. They had acquired resources for research enabling them to sur-

[77] Only one laureate complained that he had been deprived of authorship by his senior collaborator when, in his judgment, it was deserved. Far more often, the laureates reported what they perceived as generous treatment in the matter of coauthorship with their typically eminent sponsors.

[78] On the bias in favor of precocity built into current institutions for detecting and rewarding talent, see Gregg (1957). The crucial point, which holds in the domain of science as well as in the field of medical practice of which Gregg writes, is this: ". . . *once you have most of your students of the same age*, the academic rewards—from scholarships to internships and residencies—go to those who are uncommonly bright for their age. In other words, you have rewarded precocity which may or may not be the precursor of later ability. So, in effect, you have unwittingly belittled man's cardinal educational capital—time to mature." For further sociological implications of this institutionalized bias, see Merton (1960, pp. 310–313).

round themselves with younger scientists wanting to work with them on the problems in hand.

RE-ENACTED ROLES IN AGE COHORTS

As they move into the role of senior collaborators, the laureates seem to reproduce the same patterns of collaborative work with youngsters that they themselves experienced when they were young. This may turn out to be one of several kinds of *re-enactment of role-defined patterns of behavior* at successive stages in the careers of scientists, especially those who occupy statuses comparable to those of their masters in the past. They are in a position to attract promising young scientists whose contributions are sufficient to merit coauthorship, much as the laureates, when they were young, were also included among the authors of papers. They are also in a position, even before receiving the Nobel prize (since most of them were eminent before being accorded that ultimate symbol of accomplishment), to exercise cost-free *noblesse oblige*, the generosity expected of those occupying undisputed rank, by granting authorship even to junior collaborators who, in the given case, may not have contributed much.

We cannot demonstrate that the laureates are more apt than less distinguished scientists to acknowledge the contributions of junior associates since we do not know how much the younger men had actually contributed. We can, however, compare the *degree* of recognition given to collaborators on jointly authored papers. We can approximate a check on this model of the laureates' re-enactment of collaborative roles as they move through their career by comparing the variously *visible name orders* of authors of joint papers published by the laureates and by scientists in the matched sample (who, we now report, were matched not only for age, specialty, and organizational affiliation but also for the initial letter of their last name).[79] A prevalent type of name-ordering gives prime visibility to the first author.[80]

The evidence is consistent with our model of complementary roles being re-enacted in the course of the life-work-cycle.[81] The laureates-to-be, when they were in their 20's, were first authors on nearly half of all their collaborative papers at the same time that scientists in the matched sample were first only a third of the time. In papers coauthored with their laureate masters, the pattern is even more marked, with the young scientists being first authors in 60 per cent of the papers and the laureates in only 16 per cent.[82] Moving into the role of senior collaborator, the laureates, by the time they are in their 40's, reduplicate the pattern taking first authorship on only 26 per cent of their collaborative papers at the same time that the scientists in the matched sample do so in 56 per cent of their collaborative papers. The negligible cost of this kind of *noblesse oblige* for scientists who have made their mark is put in so many words by a laureate in biochemistry:

> It helps a young man to be senior author, first author, and doesn't detract from the credit I get if my name is farther down on the list.

Substantial qualitative evidence obtained in interviews with laureates confirms that this kind of re-enactment of complementary roles often occurs when they have attained a status like that of their own masters. But this fact does not rule out other, not necessarily incompatible, interpretations of the numerical evidence on first-authorship. The dual pattern of authorship might also reflect age-associated changes and rank-stratified differences in the extent of contributions to collaborative papers. In their youth, the laureates-to-be might in fact have contributed more to jointly published papers than their age-peers in the matched sample and so appear as first author more of the time. And, in their maturity, the laureates, having attracted talented youngsters, might simply be re-experiencing the same phenomenon, this time from the perspective of the senior role, with their young collaborators making prime contributions and so being accorded first place. Correlatively, the age-matched scientists of less distinction, in their youth, will have contributed less and received first authorship less often in their collaborative papers with their less distinguished mentors just as, in their maturity, they re-enact the pattern by attracting, on the average, less talented youthful collaborators than those coming to the laureates and so would themselves turn up more often as first authors. It is the processes of self-selection and selective recruitment operating within the context of the reward-system of science, rather than any autarchic scientist-playwright, which recreate this drama in many times and places with the plot and roles remaining intact, and the only change being that the inevitably aging members of the cast now play, in the style of their mentors, roles complementary to the ones they played in their youth.

[79] This is designed, of course, to control for variations that would otherwise occur in cases of alphabetical ordering of authors.

[80] On the social symbolism of name-ordering among authors of scientific papers, see Zuckerman (1968).

[81] It should be emphasized that the samples consist of scientists working within a particular historical and institutional context: one sample comprises 41 of the 55 laureates at work in the United States (in 1963); the matched sample was drawn from *American Men of Science*. Obviously the attribution of authorship would be quite different in institutional frameworks where all or much of the research done in a laboratory or department is regularly ascribed to their chiefs. This is just another instance of institutional contexts serving to pattern interpersonal relations.

[82] Neither laureates-to-be nor their laureate masters were first authors in the remaining 24 per cent of papers. This situation is in marked contrast to papers coauthored by scientist peers, both of whom later became laureates. For these papers, one future laureate is just as apt as the other to be first author in papers having at least three authors.

The observed patterns of authorship might therefore involve the re-enactment of complementary roles at different phases in the career in an apparently different way than we had at first supposed. They might reflect the objective situation of differing extent of contribution rather than the exercise of *noblesse oblige* that came with established standing. In broader theoretical perspective, however, these hypotheses turn out to be much the same. They bring us back to the general idea, much emphasized in our discussion of gerontocracy, that the age distribution of power and authority in science as elsewhere is only a static structural fact and does not, in itself, tell us much about how that power and authority are actually exercised.

In the matter of deciding on authorship and name-order as symbolic of contribution, it is generally the senior investigator who has the authority. The exercise of that authority is hedged in by norms and by constraints of maintaining a degree of cooperation in the research group. As the laureates and the matched sample of scientists take over control of these decisions, they apparently do not exercise raw power by putting themselves uniformly in the forefront. At the least, the data suggest, they tend to accede to the norms governing authority; at most, and especially when secure in high rank, they engage in cost-free supererogation.

This much can be said then about our specimen of interplay between social stratification and age stratification in science. Having moved early into the higher reaches of the opportunity structure, the laureates are more apt to collaborate at every age than other investigators of less eminence. Their tendency toward collaboration we take to be reinforced by their ability to contribute enough to merit association and coauthorship with masters in the field when they are young and by status-supported dispositions to share authorship with the young when they are mature or old. And, to repeat, these patterns obtain within an institutional framework that calls for ascribing credit for research on the basis of contribution rather than having it uniformly assigned, as in an authoritarian framework, to the head of a department or laboratory.

But, as we have suggested earlier in this section, the change toward Big Science, partly reflected in the growth of multiauthorship, makes for a change in the structure of power and authority in science where, goodwill, *noblesse oblige*, and normative constraints notwithstanding, it becomes increasingly difficult and sometimes impossible to gauge the contributions of individual scientists to the collective product of ever larger groups of investigators. The possible consequences of this structural change on the information- and reward-system of science have been strongly formulated by Ziman:

> It is obvious, in the first place, that there is a grave threat to the convention of awarding promotion, or other forms of recognition, on the strength of published work. The mere fact that a candidate for a lectureship in elementary particle physics has his name amongst the dozens of 'authors' of some significant discovery says little about his scientific skill. In the long run, the leader of such a team gets the credit for its contributions to knowledge, but he must be already the selected and tested boss of a big group. Evidence of ability at a more junior level can only be assessed within the framework of the project itself, just as it would be in an army, a civil service or other bureaucracy. This . . . puts direct power into the hands of the seniors, and opens the way to careerism, personal autocracy, and other evils, as well as giving the advantage to the 'other-directed' personality, at the expense of those protestant virtues of being 'inner-directed' which have contributed so much, in the past, to the scientific attitude. . . . [O]ne of the primary functions of the conventional communication system is losing weight. The necessity of maintaining an open market for the creations of the individual scholar, as objective evidence of achievement and promise, is no longer evident (Ziman, 1970, pp. 191–192).

Without going into the matter further, it becomes evident that the apparently bland subject of age patterns of collaboration opens up into a large array of basic questions about the operation of contemporary science. Here, as elsewhere in the field, we are still long on demonstrable questions and short on demonstrated answers. But we have seen enough, in this section of our chapter and in the section touching upon gerontocracy, to identify a variety of related problems.

Queries: How much and in which respects do the structures of authority in Big Science and in Smaller Science actually differ? To the extent that they do, how does this affect the operation of the communication system and reward system? What are the consequences of various authority-structures for scientists of differing age and at different phases in their careers? To what degree are power and authority correlated with age in different sciences and in various national scientific establishments? Which decision points, at every level in the social organization of science, are most consequential for the advance of scientific knowledge? How can more headway be made in investigating the process of decision-making in science at the several levels of its organization?

6 *Age strata and foci of scientific interest*

Historical changes in the foci of scientific work are a matter of experience familiar to sufficiently long-lived scientists and a commonplace among historians and sociologists of science. But how these changes come about and how they are distributed through the community of scientists remains a long-standing and knotty problem,[53] which has lately attracted a renewed interest. As much else in the history, philosophy, and sociology of science, this recent development is a *self-exemplifying pattern* in which workers in these fields are registering a sort of shift in research interests much like that of scientists whose comparable behavior they are trying to interpret or explain.

Both reflecting and deepening the renewed interest in this problem is Thomas S. Kuhn's book, *The Structure of Scientific Revolutions*, which in less than a decade has given rise to a library of criticism and appreciative applications.[54] To judge from the assorted use of this book in just about every branch of learning, it has become something of a complex projective test, meaning all things to all men and women. We do not propose to add still another interpretation of the book in referring to its at least symptomatic relevance here. For our purposes, it is enough to note that Kuhn puts forward three relevant points in his book and supplementary papers. One, he joins Popper in a major concern with "the dynamic process by which scientific knowledge is acquired rather than . . . the logical structure of the products of scientific research." Two, central to this kind of inquiry is an understanding of "what problems [scientists] will undertake." And three, it "should be clear that the explanation must, in the final analysis, be psychological or sociological. It must, that is, be a description of a value system, an ideology, together with an analysis of the institutions through which that system is transmitted and enforced" (Kuhn, 1970, pp. 1, 21).

Kuhn thus reinstitutes as a concern central to the history and sociology of science an understanding of the changing foci of attention among scientists; more specifically, the question of how it is that scientists seize upon some problems as important enough to engage their sustained attention while others are regarded as uninteresting. But Kuhn seems to us too restrictive in saying that the sociological form of the answer to questions of this kind must ultimately be in terms of a value system and the institutions that transmit and enforce it. Sociological interpretations of extratheoretical influences upon the selection of problems for investigation in a science include more than its norms and institutional structure. They also include exogenous influences upon the foci of research adopted by scientists that come from the environing society, culture, economy, and polity, influences of a kind put so

[53] Among philosophers of science, Karl Popper has been concerned with the problem in a long series of books and papers at least since his *Logik der Forschung* (1935). See the translation in its second edition (Popper, 1960). For an early sociological effort to investigate what are described as the "foci and shifts of interest in the sciences and technology," see Merton (1970a, Chapters 2 and 3).

[54] Kuhn (1962). For a recent and, in some of its essays, penetrating examination of Kuhn's ideas, see Lakatos and Musgrave (1970). For an energetic attack on both Kuhn and Lakatos, see Agassi (1971, pp. 152–164).

much in evidence these days in the heavily publicized form of changing priorities in the allocation of resources to the various sciences and to problem-areas within them as to become apparent even to the most cloistered of scientists. All apart from such exogenous influences, there is the question, of immediate concern to us here, of (the largely unintended) influences upon the foci of research that derive from the social structure as distinct from the normative structure of science, that is, that derive from the social composition of scientists at work in the various disciplines.

In this paper, we happen to deal with the problematics of the age structure of the sciences and specialties within them as one part of their respective social structures just as others might deal with the problematics of their religious, ethnic, or political composition. We have noted in Section 1 that the age structures of the several sciences vary. This at least suggests that the age cohorts entering science at different times may have tended to find different sciences of prime interest to them. The further question whether the age strata *within* a science tend to focus on different problems and to approach the same problems in different ways remains, of course, moot. It is an exemplary question for the sociology of science directing us to one form of interaction between the social structure and the cognitive structure of science and inviting the thought that, in some of its aspects, the cognitive structure of a field may appreciably differ for subgroups of scientists within it.

Scraps of evidence as well as speculation suggest that there are age-patterned foci of research interest and theoretical orientations in the sciences. That this is the case is so fully implied as almost to be expressed in Kuhn's own observation, which we have encountered earlier, that "Almost always the men who achieve [the] fundamental inventions of a new paradigm have been either very young or very new to the field whose paradigm they change" (Kuhn, 1962, pp. 89–90). But Kuhn turns out to be instructively ambivalent about this statement. At one moment, he considers it common enough a generalization to qualify as a cliché and a point so obvious that he should hardly have made it explicit, while at the next moment, he thinks it a generalization much in need of systematic inquiry. In the conspicuous absence of methodical evidence as distinct from much anecdotage bearing on the matter, we are inlined to dissolve the ambivalence by plumping for further investigation.

The question of there being age-patterned foci of attention and theoretical perspectives in science need not be limited to the rare cases of fundamental changes in the structure of prevailing theory. There is reason to suppose that such age-stratified differences obtain more generally. Although the modal pattern is probably one in which the several age cohorts of investigators in a field center on much the same problems, we should not be surprised to find a subsidiary pattern in which young and older scientists tend to focus their work on different problems and so to attend to somewhat different segments of the work going on in the field. It should not be difficult to find out whether such subsidiary age-stratified patterns do occur. A systematic content-analysis[85] of papers published by scientists of differing age would yield the information needed on the foci of research just as systematic citation analyses[86] would yield the correlative information needed on the range of work to which they are paying attention.

Consider briefly how patterns of citation might reflect age-stratified differences in the foci of scientific attention. Our conjectural model of the sources and consequences of such differences involves a re-enactment of complementary role-behavior by successive age cohorts much like that which we have provisionally identified for patterns of scientific collaboration. We begin with one well-worn assumption and one familiar fact. The assumption (which is also adopted by Kuhn) holds that the time in their career at which scientists encounter ideas will significantly affect their responses to them. The familiar fact is the strong, and perhaps increasing, emphasis in science on keeping up with work on the frontiers of the field, i.e., with new work.

Whether the intensity of concern with keeping abreast of new work is age-stratified or not—we know of no evidence bearing on this—it should have somewhat different consequences among the age cohorts. Plainly, the work that scientists come to know as new when they enter the field ages along with them. As incoming cohorts move toward what in science is a swiftly approaching middle age, the work they had focused on in their youth has grown "old," as age of publications is judged in much of contemporary science.[87] The problems, new or old, which members of the older cohorts are investigating will often reactivate memories of pertinent work in the literature which they had encountered as new in years gone by. Meanwhile, the younger cohort working at the same time turn their attention primarily to new work (just as

[85] For rather primitive instances of such content-analyses of scientific work, which did not, however, go on to examine possible differences among scientists of differing age or rank, see the classification of papers in the *Philosophical Transactions* 1665–1702 and the research recorded in the minutes of the Royal Society in the seventeenth century (Merton, 1970a, Chapters 3, 10, and Appendix).

[86] Ever since the invention of the Science Citation Index, citation studies have been increasing at such a rapid rate that they threaten to get out of hand. Many methodological problems are being neglected in their frequently uncritical use. Moreover, the very existence of the SCI and the growing abundance of citation analyses (even for such matters as aids in deciding upon the appointment and promotion of scientists) may lead to changes in citation practices that will in due course contaminate or altogether invalidate them as measures of the quality of research. This would not be the first case where the introduction of statistical records of role performance has led to a *displacement of goals* in which the once-reliable statistical indicator rather than the actual performance becomes the center of manipulative concern. On the early use of citation analyses, see Garfield, Sher, and Torpie (1964); for a critical overview of methodological problems in citation-analysis, see J. Cole and S. Cole (1972, Chapter 2); S. Cole and J. Cole (1971); and Whitley and Frost (1971); on displacement of goals, see Merton (1968b, pp. 253–255); on displacement of goals in statistical measures of performance, see Blau (1955, Chapter 3).

[87] It will be remembered from Section 2 of this Chapter that the half-life of references in many of the sciences is 5 years or less.

their middle-aged colleagues did in their youth). But not having the same immediate knowledge about work which had been done in the, for them, remote past of 15 or 20 years before, they are less apt to be put in mind of earlier germane investigations.

In this model, scientists in each successive cohort re-enact much the same citation behavior at the same phases of their career. By doing so, younger and older scientists *to a degree* contribute differently to the development of science: the older scientists providing somewhat more for intellectual continuity by linking current work with work done some time before; the younger scientists pushing ahead somewhat more on their own, less "encumbered" by past formulations. We suggest that although the norms governing the communication of scientific knowledge are much the same for all, leeway in their observance is enough to allow for the occurrence of such unplanned, and often unnoticed, variation in the age-patterned reporting of scientific work.

If these conjectured differences do in fact obtain, they should be reflected in various ways. For one thing, younger scientists should be given more than older ones to making rediscoveries: findings and ideas, independently arrived at, that are substantively identical with earlier ones or functionally equivalent to them.[88] The Santayana dictum that those who fail to remember history are destined to repeat it should hold with special force in the domain of science—subject more than other spheres of culture to the objective constraints of finding authentic solutions to designated problems.[89] And if the conjectured differences obtain, we should also find age-connected patterns of references and citations along the lines suggested by the preliminary and tentative investigations by Stephen Cole and by Zuckerman,[90] which, it will be remembered from Section 2 of this chapter, found that older scientists are more likely than younger ones to cite older publications.

When scientists themselves do not understand this reiterative pattern of age-related foci of attention, they are ready to pass invidious judgments upon the behavior of those in "the other" age stratum. Older scientists then describe younger ones as parochial if not downright barbarian in outlook, little concerned to read and ponder the classical work of some years back and even less concerned to learn about the historical evolution of their field (the judges forgetting all the while that the new youth in science are only reproducing the

[88] On patterns of rediscovery, see Merton (1968a, Chapter 1).

[89] In suggesting this, we do not subscribe to positivistic or Whig doctrine.

[90] Stephen Cole (unpublished); Zuckerman (unpublished data). We should also note again that age-patterned differences in references and citations represent a finding *a fortiori* (i.e., under conditions tilted against the hypothesis). For any such differences observed in print are there in spite of countervailing suggestions by referees and colleagues (often differing in age from the author). Further investigation would compare the age-distributions of references in collaborative papers written by age-peers (old or young) and by authors of substantially differing age.

attitudes and behavior they had exhibited in their own youth). In turn, younger scientists deride the orientation of older ones to the past as mere antiquarianism, as a sign that they are unable to "keep up" and so are condemned to repeat the obsolete if not downright archaic stuff they learned long ago (the judges being all the while unable to anticipate their own future behavior that they will then likely perceive as providing needed lines of continuity in scientific development).

Historical changes in patterns of citation may provide context for these sequential patterns of citation during the life course. The exponential growth in the numbers of scientists and of scientific publication may result in changed norms and practices with respect to linking up work on the research front with that which has been done some time before. More specifically, this raises the question whether it is the case that, as science becomes bigger in every aspect, a not altogether functional adaptation develops in which successive cohorts of scientists give less and less attention to pertinent work of the past while scientific publications provide less and less space for it. This may be still another instance in which historical changes in the social and cognitive parameters of science interact with sequential patterns in the life course to produce both similarities and differences in the behavior of successive cohorts of scientists.

Concluding remarks

An exploratory paper like this one has no place for "conclusions" but it does call for a few afterthoughts.

Plainly we have only touched upon the problematics of our subject, not having discussed a variety of questions which even now could be examined to good purpose. Here is a scattering of examples:

—How is the *age of research groups* related to their scientific productivity?[91]

—What is the age distribution of the "founders" of new formations in the various sciences: e.g., new specialties, new forms of investigation (laboratories), new journals, scientific societies, etc.?

—What is the relation betwen the quantity and quality of scientific output at various phases in the scientific career?

—How is age variously associated with intellectual authority[92] and with bureaucratic authority in science, and what are the consequences of such differences for the development of scientific disciplines?

—What historical changes have occurred in the span of research careers and how do these relate to the durability of the intellectual influence of scientists?

—To what extent do age cohorts in science develop into age-sets with their continued interaction and solidarity to produce old-boy networks (not unlike new-boy networks)?

Perhaps enough has been said to indicate what we take to be the principal aim of investigating questions of this sort. That aim is to find out how age and age structure variously interact with the cognitive structure and development of science.

[91] For a critical examination of the question since the first studies by H. A. Shepard, W. P. Wells, D. C. Pelz, and F. M. Andrews, see Smith (1970); also Vlachý (1970).

[92] Most emphatically, peers in science need not be age peers. A century ago, William Perkins was, at 23, the world authority on dyes just as a Joshua Lederberg or a Murray Gell-Mann were authorities in their subjects at a comparable age today. This sort of thing should lead us to abandon the practice, common in the jargon of sociology and psychology, of having the word "peers" refer elliptically only to "age peers." As everyone else seems to know, "peer" refers to one who is of equal standing with another, in whatever terms that standing is gauged: political rank, esteem, authority, *and* age.

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