REVIEW PAPER

● *ABSTRACT*

This article is a critical assessment of research productivity through publication among scientists. The article scrutinizes the literature on correlates and determinants of publication productivity; provides an overview and organization of that knowledge; indicates gaps and shortcomings in the research; and hence makes clear the questions and issues which are both answered and unanswered.

Publication Productivity among Scientists: A Critical Review

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The most fundamental social processes of science are the communication and exchange of research findings and results.[1] The principal means of this communication is the publication process, which allows scientists to verify the reliability of information, to acquire a sense of the relative importance of a contribution, and to obtain critical response to work.[2] Correspondingly, it is through publication that scientists receive professional recognition and esteem, as well as promotion, advancement, and funding for future research.[3] And in fact, publication is so central to productivity in research that the work becomes 'a work' only when it takes a conventional, physical (that is, published) form, which can be received, assessed, and acknowledged by the scientific community.[4]

Yet, given the centrality of publication to scientific endeavour, the average rate of this productivity is low. For example, among physiologists in the United States, half of Meltzer's[5] sample had published five or more papers in the last three years, and the other half four or fewer. In a random sample of 238 chemists receiving doc-

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torates between 1955 and 1961, Reskin's[6] data show that 7.5% had published nothing the first decade following receipt of their degree, and 11% had published only one article. And in any given year, 60% of the chemists had not published a single article. Among physical scientists with appointments in American colleges and universities, Ladd and Lipset[7] report that almost a third had published nothing in the two years prior to the survey. Blume and Sinclair's[8] data, however, suggest that the publication of British scientists is higher than that of Americans, so that over a five year period 44% of the British chemists published fewer than 10 papers, 42% between 11 and 30, and 15% more than 30.

Data from social scientists show these groups to be much less prolific than the natural, physical, and biological scientists. In the early 1960s, one study examined the productivity of sociologists who had received their doctorates ten to twenty years previously, and reported that 60% of the group had published fewer than three articles during that time.[9] More recently, Yoel's[10] study of US sociologists who were promoted to senior level positions in 1970 and 1971 revealed that over one-eighth of those in the 'top nineteen' departments, and almost one-third of those in the remaining, good or 'adequate-plus', departments, had published no more than one article.

However, while the average rate of publication tends to be low, the variation between scientists is very high. Whether one looks to publication over a year, a five year span, or the entire professional life-time, productivity varies enormously between scientists. Since Lotka's[11] (1926) analysis of papers published in physics journals during the nineteenth century, it had been apparent that the distribution of publications is highly skewed, and that the great bulk of papers are produced by a small minority of scientists. These observations stimulated Derek Price's[12] study of the long-term historical patterns in publication, and led to his principle that scientific productivity conforms to an 'inverse square law' whereby the square root of the population of publishing scientists produce half of the work.

Other investigations both between and within fields indicate that publication is highly skewed, thus supporting Price's general conclusion about concentration of productivity. For example, in a sample of natural, biological, and social scientists, J. Cole[13] found that fifteen percent of the group accounted for half of all papers published. Reskin's[14] study of chemists similarly reports that fifteen percent of the scientists contributed about half of the total papers published in a sixteen year period. And likewise, Allison and Stewart[15] show highly unequal distributions in the publication productivity of chemists, physicists, and mathematicians.

Hence, the data on scientific productivity through publication show consistently that: (1) despite the salience and centrality of the publication to scientific endeavour, the average rate of this productivity is low; and (2) it is highly variable. But beyond these two items about rate and variation, agreement diminishes and explanation of these patterns is less resolute. In fact, the variation and determination of scientific productivity is cited as one of the most perplexing problems in the sociology of science.[16]

In this paper, we aim critically to review what is known and what is not known about the correlates and determinants of publication productivity among scientists. In doing this, we specifically aim: (1) to scrutinize the extant research on publication productivity; (2) to provide an overview and an organization of that knowledge; (3) to indicate the gaps and shortcomings in the research, and thus make clear the questions and issues which are both answered and unanswered. Throughout, the focus is upon productivity through publication among individual scientists (rather than research units or aggregates).

Perspectives on Productivity

Productivity and Individual-level Variables

In the search for explanation of productivity levels among scientists, researchers first looked to individual-level variables, such as psychological traits, work habits, and demographic characteristics. Among these investigations, a large number of studies have been those which explore the cognitive and emotional — or psychological — traits of scientists.

Psychological Characteristics

Among the psychological inquiries, one perspective has been called the 'sacred spark' theory[17] because it attributes productivity to ability and 'inner compulsion', which continue even in the absence of external rewards. And, indeed, certain investigations have reported that productive scientists,[18] and the eminent scientists especially,[19] are a strongly motivated group of researchers.

A second variant of the psychological perspective focuses not so much upon ability and attitude as upon 'stamina' or the capacity to work hard and persist in the pursuit of long-range goals.[20] Informed observers have long described high producing scientists as driving and indefatigable workers.[21] And empirical data on scientists and engineers do suggest that high performers are absorbed, involved, and strongly identified with their work.[22]

A third variety of the psychological perspective is represented by clinical investigations of the cognitive, emotional, and perceptual styles of the productive scientist. In the studies of personality structure, researchers report that productive scientists show high ego strength, personal dominance, preference for precision and exactness, strong control of impulse, and a preoccupation with ideas and things rather than people.[23] Aligned with these investigations of personality structure are studies which focus upon the biographical background of scientists.

These biographical studies have probed a wide range of items including early childhood experiences, sources of derived satisfactions and dissatisfactions, descriptions of parents, attitudes and interests, value preferences, self-descriptions and evaluations. The aim is to determine the biographical characteristics, experiences, and self-descriptions which differentiate the highly productive and creative, from the less productive and creative, scientist.[24] From these studies, one set of biographical attributes emerges strongly and consistently: eminent and productive scientists show marked autonomy, independence, and self-sufficiency early in their lives. This autonomy is apparent in early preferences for teachers who let them alone, in attitudes toward religion, and in dispositions toward personal relations.[25] In their personal relations, specifically, the creative and productive scientists tend to be detached from their immediate families, isolated from social relations, and attached, instead, to the inanimate objects and abstract ideas of their work.[26] These scientists emerge as 'dominant persons who are not overly concerned with other persons' lives or with attaining approval for the work [they are] doing'.[27]

In addition, other investigations have focused upon cognitive structure, particularly. These studies report that productive scientists have developed certain styles of thinking and perceiving, including a capacity to stave off immediate intellectual closure, play with ideas, differentiate stimuli, recombine familiar concepts, and tolerate ambiguity and abstraction.[28] Together, these cognitive traits are said to form the illusive 'creative personality'.

In these studies of cognitive traits, the emphasis is upon style, rather than level, of intellectual performance. The suggestion is that persons of equal capacity can differ markedly in the ways in which they deploy intellectual resources and come to grips with information,[29] and that the cognitive differences may explain, in turn, why some scientists are successful in their work and others are not. Yet few direct tests of the relationship between cognitive style and productivity, specifically, are available. One of the few reported tests of the relationship is Wilkes' study.[30] He finds, however, no simple relationship between scientists' cognitive style and their rates of productivity, and concludes that while stylistic differences affect the orientation of research, they do not affect research productivity as measured by rates of publication.

While studies from the psychological perspective have provided a rich array of findings, the investigations have certain shortcomings.

First, since scientists are a highly trained and rigorously selected élite, it is doubtful that scientific ability is distributed as unevenly as productivity.[31] Thus, it is questionable whether ability and talents are a sufficient explanation of variation in productivity. Among scientists with doctoral degrees, measured intelligence (IQ) is, in fact, very high.[32] But, within this already select group, IQ correlates very weakly with productivity and achievement in science.[33] It may be that IQ tests are too broad to differentiate within so select a group, or it may be that intelligence tests fail to measure the types of ability that are pertinent to performance in science.[34] Yet, however ability is measured, it is questionable whether it would be as highly skewed as productivity. While high IQ may be a prerequisite for doctoral training in science, it appears that once the degree is obtained, differences in measured ability do not determine subsequent levels of performance.[35]

In order to reconcile this disparity between the distribution of ability compared with productivity, Shockley[36] offered a 'multiplicative model of mental factors'. Specifically, he maintained that productivity is the result of factors such as ability to find and persist in the solution of important problems, and most critically, he maintained that these factors determine productivity multiplicatively rather than additively. Hence the resulting distribution of productivity will be more skewed than its determinants.

A second problem with the psychological perspective is that personality traits such as creativity do not exist in a vacuum.[37] Rather, social factors so affect the translation of creative ability into innovative performance that measured creativity is virtually unrelated to either the innovativeness or the productiveness of scientists' output. Accordingly, Andrews[38] reports that creativity may not result in productivity unless the scientists have strong motivation, diverse activities, and the capacity to exercise power and influence over decisions pertinent to their projects.

Other studies also report the critical importance of social and organizational context in translating creativity into productive output. Among a sample of natural scientists in both a large university and an independent laboratory, Connor[39] found no direct relationship between measured creativity and research performance. He concludes that although a scientist may possess creative ability, and although those abilities may be crucial to the research process, social and organizational variables interact with, and affect, the manifestation of that creative ability. Gordon and Morse report similarly that 'the interface between psychological capabilities and organizational requisites is the nexus around which to increase organizational effectiveness'.[40] However, unlike Andrews' investigation, the Connor and the Gordon and Morse studies do not actually test the extent to which organizational context mediates the relationship between creativity and output. Hence, the conclusions of these two studies are more conjectural.

Work Habits

Just as productive scientists have been said to have certain cognitive and emotional styles with which they approach their work, so, too, scientists' productivity may be associated with certain behavioural habits and patterns which act as tools or devices for accomplishing tasks. In fact, in his classic sociological essay, C. Wright Mills[41] maintains that 'intellectual craftsmanship' — the organization of time, space, and materials — represents a significant factor in productive work. Yet, with few exceptions, systematic research on work habits and productivity is lacking, and the commentary on the subject tends to be speculative.[42] Exceptions include Hargens'[43] study, which focused upon disciplinary context, and Simon's,[44] which focused upon the eminent scholar.

Hargens' work analyzes the association between scholarly output and practices in three disciplines — chemistry, mathematics, and political science. He reports that habits relate to productivity, according to the level of 'predictability' or 'routine' in the work and discipline. Thus, in chemistry, which is characterized by more predictable or routine procedures, investments of time and simultaneous investigations of several problems do affect productivity. But in mathematics, a discipline with little routinism in the work, such practices may have weak to nonexistent impact upon output. Further, the relationship between habits and productivity may vary across the history of a discipline, since research technologies, problems, and hence levels of 'routinism', vary over time.

In contrast to Hargens' investigation, Simon's focuses upon the work practices of the eminent — scholars judged by their peers to be 'the most outstanding' in their fields. Her data show certain patterns of practices: as a group, the eminent spend vast amounts of time in their research; they work on several problems at a time; and they tend to devote early morning to their writing.

Such studies of productivity and work habits are appealing inasmuch as work routines (unlike factors such as 'insight' or 'imagination') are more adaptive strategies at the disposal of the scholar and scientist. However, we must caution that the causal relationship between habits and productivity is uncertain, and that particular practices, such as investment of time, may be a function of productivity, rather than the other way around. The adoption of particular work practices may result, then, in little or no increase in the output of aspirants.

Demographic Characteristic: Age

In the search among individual-level variables for explanation of productivity, investigators have also looked to the effect of demographic characteristics, particularly age.

Some three decades ago, H.C. Lehman[45] published evidence that scientists' major contributions occur in their late 30s or early 40s, and thereafter decline. In subsequent documentation,[46] Lehman verified his observations, and showed further that the age peak occurred earlier in abstract disciplines (such as mathematics and theoretical physics) and later in more empirically based fields (such as geology and biology). Moreover, he observed that the age peak is sharper for major contributions and achievements, and flatter for minor scientific accomplishments.

In another investigation, Pelz and Andrews[47] likewise found a productivity peak in scientists' late 30s and early 40s; but they also observed a second peak ten to fifteen years later at age 50. Thus, in contrast to the continuing decline of Lehman's observations, Pelz and Andrews found a two-peaked curve of age and productivity. The disparity in the two sets of investigations is attributable in part to differences in the studies' dependent variables: Lehman found the sharp decline with age only for major contributions. Pelz and Andrews' performance measures, on the other hand, include a wide range of achievements in paper, patents, reports and manuscripts, and for this range of contributions, the age peak is less dramatic.

In analysis of data from a cross-section of academic scientists in seven fields, Bayer and Dutton[48] obtained results similar to those of Pelz and Andrews. Specifically, in five out of the seven fields, Bayer and Dutton observed what they call a 'spurt-obsólescence' function between age and articles published within the previous two years. This function represents a two-peaked curve, with the first peak reached at about the tenth year of career-age, followed by a second productivity peak as the scientists near retirement age. However, in this study, the reported relationships are weak in all fields, and age explains little of the variance in publication.

S. Cole,[49] on the other hand, reports a slightly curvilinear relationship between age and quantity of publications for a cross-section of academics in six scientific fields. Across fields, he finds that publication rates rise gradually with age, peak in the late 30s or early 40s, and then drop off. Additional longitudinal data on mathematics, which allow him to disentangle age and cohort effects, show the same pattern as his cross-sectional data: the relationship between age and productivity is slightly curvilinear, but productivity does not differ significantly with age.

A number of hypotheses have been advanced[50] to account for observed productivity declines with age. First, intellectual functioning of scientists may atrophy with age. Second, able scientists may be drawn off into nonresearch activities, and particularly administration, in their later years. Third, scientists may relax their zeal and motivation to achieve. And fourth, as scientists specialize, they may lose the fresh viewpoint needed for breakthroughs. In their scrutiny of these four hypotheses, Pelz and Andrews find support for only the third and fourth — that with age, scientists may lose motivation and fresh viewpoints. However, neither Pelz and Andrews' data, nor the existing studies on intellectual functioning, support the hypothesis of regression in intellectual functioning after the early 40s. And Pelz and Andrews' data, specifically, indicate that a mixture of research and nonresearch, including teaching and administration, *facilitate* rather than retard productivity.

An international study of performance in research units offers further evidence that administration responsibilities may foster rather than inhibit scientific performance. In a study of scientists in six countries, Knorr and her colleagues[51] argue that higher administrative positions (except those which move a scientist out of research altogether) provide the resources and task force which increase possibilities for publication. And in fact they do find that task environment, rather than age, accounts for variations in productivity. Once supervisory status is controlled, the correlation between age and publication reduces to insignificance, and supervisory status emerges in the analyses as the critical independent variable in the productivity relationship.

A recent longitudinal study of chemists in the University of California system challenges findings that productivity declines with age. In this study, Hammel reports that 'productivity increases strongly with age and decreases weakly with the square of age, so that the pattern is one of *gradually decelerating increase*'.[52] In contrast to previous studies, Hammel concludes, then, that scientific productivity increases with age, with some evidence of flattening, but not necessarily decline, with age. Moreover, he reports that increases in productivity are more marked for more recent cohorts, and that the declines apparent in any mean across persons are 'attributable largely to the "shooting stars" — the high producers who climb to a peak and then decline'.[53]

Hammel maintains that the failure of other studies to capture the pattern of decelerating increase with age lies in their failure to separate behaviour by birth cohort. Hammel's analyses, which do separate by cohort, show that only the very oldest cohort (age 61 +) manifest a decline in mean productivity with age over 40, and that the two cohorts prior to it (age 51-55 and 56-60) exhibit only a levelling of productivity with increasing age. He argues that those analyses which fail to separate relationships by cohort are skewed by the behaviour of the oldest cohort.

Productivity and Environmental Location

The second major perspective on scientific productivity through publication focuses not upon individual-level traits, but rather upon structural aspects of the scientists' environment — the calibre of the training institution, the prestige of the institutional affiliation, and other features of institutional location.

Socializing Environments: Graduate School Background

Graduate school is a critical socializing environment, not only because it develops knowledge, skills, and competences, but also because it cultivates norms, values, and attitudes.[54] Thus, graduate education shapes conceptions of the scientific role, standards of performance, and styles of work among its students.[55] In fact, most scientists do not significantly alter their ideas, approaches, and commitments after graduate school.[56] Graduate education and apprenticeship are particularly critical for the work behaviour and patterns of the eminent in science. Interviews with Nobel laureates indicate, specifically, that socialization during advanced apprenticeship is decisive in transmitting to the laureates standards of achievement, taste in selection of research problems, and confidence in work abilities.[57]

Given this salience of graduate school socialization, investigators have looked to graduate school background as an explanation of variation in productivity levels. Among these studies, an early investigation is Crane's[58] analysis of data from interviews with 150 scientists (biologists, political scientists, and psychologists) located at three universities of varying prestige levels. From these analyses, Crane reports that the setting in which a scientist receives graduate training has more effect on later publication than the setting in which he[59] works after obtaining his degree. Further, she reports that scientists trained at major universities are likely to be productive regardless of their current work environment; while scientists trained at minor universities are unlikely to be productive unless currently located at a major university. In accounting for the strong effect of current environment for graduates of minor versus major universities, Crane posits two influential factors — motivation and judgment in selecting research topics which lend continuity to research. More specifically, Crane reports that unless the scientist at the minor university has unusually strong motivation and a well-developed research agenda, he must depend upon the research opportunities and stimulation provided by the institution — and at the minor universities, these resources tend to be scarce.

In later studies of productivity and graduate school training, investigators have extended the inquiry by differentiating between particular training processes, refining the measures, and hence further specifying the effect of graduate school upon future attainment. Among these studies, Reskin's[60] drew data from a random sample of chemists who received doctoral degrees from US universities between 1955 and 1961. She analyzed the effect upon both pre- and post-doctoral publication and citation of three aspects of graduate school background — the calibre of the doctoral programme, training with a productive sponsor, and collaborative publication with a sponsor. For pre- versus post-doctoral productivity, Reskin's findings differ. Specifically, training with a productive sponsor and collaboration with a sponsor are associated with higher productivity during the *pre-doctoral* period, while calibre of the doctoral programme has no impact on publication during this period. For *post-doctoral* productivity, on the other hand, the findings reverse, so that calibre of the doctoral programme is important for productivity at the middle and end of the first post-doctoral decade, while sponsorship is not important for productivity during this period. Reskin's findings suggest, then, that although sponsorship may be important in launching scientists' *early* (that is, pre-doctoral) publication, the quality and vigour of their graduate department, as a whole, are more critical than sponsorship for *continued* productivity.

Other research on biochemists by Long and his colleagues,[61] however, challenges Reskin's conclusions about the effect of doctoral department. They report that while prestige of doctoral department, mentor's eminence, and selectivity of undergraduate institution are all positive in their effects upon productivity, the magnitudes of association are small and statistically insignificant. Instead, they find that the strong and direct determinant of future productivity is pre-doctoral productivity, and that the effect of doctoral department is only indirect, influencing productivity by way of its impact upon prestige of first appointment.

Subsequently, another group of researchers, led by Chubin,[62] has questioned the generalizability of these data on chemists to scientists in other fields. Replicating aspects of the analyses by Long's group on other data from a random sample of PhD recipients in fields of electrical engineering, physics, psychology, sociology, and zoology, as well as biochemistry, Chubin's team do support Long's contention that early publication predicts later publication. But they also maintain that prestige of doctoral programme predicts publication productivity.

In spite of some variation in findings on effects of graduate school education, two factors — prestige of doctoral programme and pre-doctoral publication — do emerge as predictors of productivity. Ironically, however, while early publication predicts future productivity, it may, by itself, have limited impact upon prestige of first position — a variable which, in turn, does affect productivity patterns.[63] Thus, in explaining scientific productivity, we must look beyond early environment toward the effect of scientists' subsequent location in department and institution.

Prestige of Department or Institution

In investigations of work location and productivity through publication, the research points specifically and consistently to the importance of *prestige* of the department and institution,[64] and suggests that the productivity of scientists is influenced not only by their own behaviour and attitudes, but also by the orientation and activities of their co-workers. Of course, the causal relationship between productivity and location might operate in the effect of productivity upon location as well as the other way around, so that more prestigious departments are selecting the more productive scientists.[65] But recent investigations point to the stronger causal effect of location upon productivity (rather than vice versa).

In contrast with earlier cross-sectional studies, these recent longitudinal investigations have monitored the publication histories of scientists between locations and over time, and hence they have permitted isolation of the effects of productivity and location. Among these studies, Long's[66] reports that whereas the effect of productivity upon prestige of location is weak, the effect of location upon productivity is strong. Specifically, he finds that for scientists moving into first academic position,[67] publication levels are not immediately affected by the prestige of the new department; rather, productivity levels are affected by early (that is, pre-doctoral) publications, and hence indicate a tendency among those who publish to continue to do so — for a while. However, after the third year in the appointment, scientists' productivity rates are more strongly affected by the prestige of their present location than by their predoctoral publication — so that those in prestigious departments increase their publication, while those in less prestigious departments begin to publish less. Among scientists who subsequently change institutions, the correlation between publication and new location is negligible at time of the initial move, but it increases markedly within five years — so that productivity comes to be related directly to the new departmental prestige level and unrelated to the old departmental prestige. In sum, Long's findings suggest a 'reinforcing process of advantage' whereby the initial appointment, which is independent of earlier productivity, has an impact on later productivity, and in turn, the prestige of second location.

In a yet more recent study, Long and McGinnis[68] extend this line of inquiry beyond academic departmental prestige to the effects of larger organizational context — classified as research university, nonresearch university or four-year college, and nonacademic or industrial sectors. They report that the chances of obtaining employment in one or another context are not related initially to productivity (measured as quantity of publication). However, 'once employment is obtained in a specific context, individual levels of productivity soon conform to characteristics of context'.[69] Specifically, location in four-year colleges and in nonacademic and industrial sectors, especially, depresses publication, while appointment in research universities fosters publication. Moreover, when changes occur in organizational context, the new context takes hold as the determinant of productivity — but only after three years on the job. The fact that it takes some time for the new context to emerge suggests that productivity levels are not simply the result of changes in scientists' goals or of global barriers to publication in certain settings.

While research has garnered support for departmental or institutional prestige as a determinant of productivity through publication, the shortcoming of these investigations lies in their failure to explain *how* prestigious departments and units foster, and minor institutions discourage, publication. Hence, the literature has failed to specify the extent to which major (that is, prestigious) departments and institutions promote productivity through available time, through research assistantship, or through a favourable reward structure. Among the environmental factors which might mediate productivity levels in major institutions, one variable, however, emerges with some consistency: namely, exchange and communication among colleagues and associates.

Because ideas cannot be born, nurtured and refined within a single mind,[70] and need, rather, to be tested through exchange, collegiality is an important process in scientific endeavour. Correspondingly, collegial dialogue and exchange may be an impetus to research activity and involvement. Blau[71] maintains that collegial discussion about discoveries and problems stimulates research involvement by activating latent research interests, and by providing rewards and reinforcement for work accomplished, as well as incentives for work still in progress. Notably, the prestigious academic departments which are said to provide a context favouring productivity, are also reported to have strong patterns of collegial exchange.[72] And the graduate seminar, found in research universities with doctoral programmes, provides a particular mechanism for the exchange and testing of ideas with a receptive and sensitive audience.[73]

In their investigation of scientists in academic as well as nonacademic settings, Pelz and Andrews[74] also report that productivity (which included but was not limited to publication) is, indeed, associated with scientists' communication among colleagues. Pelz and Andrews' measures of communication included contacts via memos and meetings, as well as conversation, and the relationship between communication and productivity held even after controlling for experience and supervisory status. Communication enhances productivity, they say, because it provides ideas, helps catch errors, and promotes competition and reward.

Collegial exchange may be particularly important for scholars and scientists who face conflicting demands for other-than-research performance.[75] Consequently, collegiality with active researchers may be especially important for the productivity of female scientists, who as a group, face conflicting demands for performance in domestic roles, and for scientists at minor institutions who encounter heavy demands in teaching and other nonresearch activities.

Of course, scientists' collegial networks clearly transcend the immediate environment of the department or institution,[76] but collegiality within the unit or department, itself, appears to be particularly advantageous for productivity. Ongoing and face-to-face contact seems to provide social and intellectual support as well as criticism, and this, in turn, facilitates scientific performance.[77]

Other Organization Variables

Other studies have looked to organizational freedom as it influenced productivity. While the findings are somewhat mixed, they tend to suggest that higher levels of freedom support publication-productivity.

In an investigation of scientists in eight industrial research laboratories, Box and Cotgrove[78] found higher levels of publication among scientists who were free to select, initiate, and terminate their own research projects, or to influence the process. Publication among scientists related also to the 'scientific commitment' in the lab — where commitment level was measured as the proportion of scientists in the organization who consider publication important and who consider the scientific community as their reference group. In fact, Box and Cotgrove report that this organizational commitment to science is a 'necessary condition' for a high level of publication. However, they add that commitment 'alone is not sufficient. The extra intensive effort required to transform research findings into a publishable paper is most likely within a context of a higher level of organizational freedom, including a more permissive attitude toward publication'.[79]

In another study of industrial scientists, Vollmer[80] likewise reports a positive relationship between productivity — quantity and quality of publication — and organizational freedom. More specifically, he finds that scientists with the highest levels of productivity are likely to be in locations in which they have freedom to select their research projects and to be involved in research activity that is separate from engineering development activities. Vollmer concludes that scientific productivity thrives under working conditions that approximate those in universities and in nonprofit laboratories.

These conclusions about organizational environment are supported, in turn, by Stahl and Stevens's[81] study of physical scientists and engineers in US Air Force research and development laboratories. Like Vollmer, they report that scientists' production of published papers is associated with organizational opportunities similar to those found in universities, including opportunities to teach, lecture, and instruct, to participate in decisions about projects, and to do independent research.

Vollmer's and Stahl and Stevens's data suggest that productivity is supported where scientists have flexibility and freedom with ideas, and where organizational goals do not conflict with individual interests and aspirations for basic research.

Further, in scientists' own subjective impressions about organizational climate and productivity, they frequently stress the importance of organizational freedom. For example, when a group of scientists (designated as 'highly creative' by their peers) were asked about the factors which stimulated productivity, 'freedom to work in areas of greatest concern' was the second most frequently mentioned item, ranking just behind 'recognition and appreciation for work'.[82]

Pelz and Andrews'[83] study, however, carefully qualifies the particular importance of organizational freedom as a factor supporting productivity among scientists. Specifically, they conclude that a combination of organizational freedom *and* organizational coordination are both feasible and desirable for effective and productive performance (including the publication of papers). Accordingly, they contend that decisions about work should be coordinated by some supervisory structure with substantial individual autonomy of scientists in the process.

In addition, conclusions about organizational freedom and productivity are confounded by uncertainties about causal relationships. In the available studies, it is not clear whether the more productive scientists gravitate toward settings which provide freedom to select and initiate projects and engage in research, or whether, in fact, those settings promote productivity among the scientists located in those places.

Information is limited on the relationship between publication among individual scientists and organizational variables other than organizational freedom and prestige. Studies of other organizational variables (such as style of leadership, degree of group-cooperation, or lines of communication) either focus upon the research aggregate rather than the individual scientist, or they focus upon productivity through patents, reports, or other products, rather than the publication productivity which is the central focus of this review. An exception, however, is Meltzer and Salter's[84] study of physiologists in research institutes and organizations. Analyzing the relationship between size and number of organizational levels and quantity of research papers published by the scientists within the previous three years, Meltzer and Salter report no relationship between size and productivity, and a curvilinear relationship between levels and productivity. Measures of organizational freedom (operationalized as lack of close supervision and opportunity to utilize abilities), which were treated as intervening variables were, in fact, positively related to productivity. But they bore no simple relationship to organizational structure.

Productivity and Feedback Processes: Cumulative Advantage and Reinforcement

Cumulative Advantage

The psychological theories assume, with few exceptions,[85] a simple additive relationship between individual-level traits and productivity. The environmental perspective, however, suggests certain associations between departmental prestige and productivity, and begins to indicate a 'feedback' process — whereby initial appointment has a major impact on later productivity, and in turn, the prestige of second department and subsequent productivity. These feedback processes are the very focus of a particular perspective — called 'cumulative advantage' — which attributes productivity differences to the differential resources that accrue to scientists because of their earlier productivity. From this perspective of cumulative advantage, scientists who experience early success are able to command or obtain the increased time, facilities, and support for continued research.[86] These resources and rewards then 'enrich the recipients at an accelerating rate, and conversely impoverish the non-recipients'.[87] Thus, the accumulation of advantage involves 'getting ahead initially and moving further and further out front'.[88] Early performance brings rewards and once these rewards are received, they have an independent effect on the acquisition of further resources and rewards in science.[89]

One special case of cumulative advantage has been called 'The Matthew Effect'.[90] This effect consists of the accrual of greater recognition to scientists of considerable repute, and lesser recognition to scientists of limited repute. Recognition can then be converted into resources for further research. The pattern of cumulative recognition favours eminent scientists principally in cases of collaboration and in cases of independent multiple discoveries by scientists of different ranks.[91] In both collaboration and multiple discovery, the already eminent scientist gets disproportionate credit.

While the perspective of cumulative advantage is well-developed, tests of the hypothesis are difficult, because direct assessment requires data on the particular research resources of scientists. Since investigators have lacked these data on resources, their findings support the cumulative advantage hypothesis, only indirectly. In his study of biologists, chemists, and physicists, Gaston,[92] for example, reports that as scientists go through their careers, the variability in their rate of publication steadily increases. In fact, he finds almost perfect linearity between time and increased variability in productivity levels — which indirectly suggests a pattern whereby productivity differences become ever larger between those initially advantaged and those not so advantaged. While Gaston's data are longitudinal, Allison and Stewart's[93] are merely cross-sectional. But by dividing the sample into age groups, they are able to assess the relationship between productivity and career-age. They, too, find strong linear increases in inequality with increasing career-age.

Each of these analyses, however, leaves unaccounted a major source of inequality: that which may exist, from the start, among the young cohorts of scientists. This initial inequality may reflect early differences in talent, ability, and motivation, and it may also reflect a strong reinforcement process which operates early in the educational process.[94] These sources of inequality raise a critical issue: the extent to which advantage is allocated on the basis of meritocratic, as compared to nonmeritocratic, principles. Because funds, facilities, and other scientific resources are limited, they will necessarily be allocated to some persons at the expense of others, and the resulting distribution will be unequal. The advancement and progress of science depends, however, upon the particular criteria for these allocations. If the accumulation of advantage rests upon differences in achievement and performance, then the resulting inequalities may contribute to progress and development in science. If, on the other hand, advantage stems from ascribed characteristics unrelated to performance, the process ultimately obstructs not only opportunity, but also the advancement of science.

Reinforcement

Literature on scientific productivity has often lumped reinforcement theory together with accumulation of advantage, and hence failed to distinguish between the two perspectives. Reinforcement, however, deals with *why* scientists continue to produce, and cumulative advantage deals, on the other hand, with *how* scientists are able to obtain the resources which facilitate research and publication.[95]

'Reinforcement' is the fundamental behaviourist principle[96] that activity which is rewarded continues to be emitted, while activity which is not rewarded tends to be extinguished. Applied to productivity, this means that: 'Scientists who are rewarded are productive, and scientists who are not rewarded become less productive'.[97] From this perspective, then, early publication and recognition (namely, citation) should result in continued performance.

It is difficult, however, to make a critical test of reinforcement as it relates to productivity. First, the social context of scientific productivity is much more complex and variable than the laboratory setting and animal experiments from which the principle derives. Second, while reinforcement and cumulative advantage are, conceptually, different processes, they are, nonetheless, related in such a way that makes it difficult to untangle their effects. Positive reinforcement can exist without cumulative advantage; but reinforcement will not account for much productivity unless accompanied by the cumulation of resources for research. Cumulative advantage, on the other hand, does not exist without some prior positive reinforcement; hence the process of reinforcement almost certainly accompanies enabling advantages.

In spite of difficulties in assessing the effects of reinforcement, certain studies have attempted to do so, and the data lend support to the reinforcement principle. For example, Lightfield[98] traced the publication records of 83 sociologists who received their doctorates between 1954 and 1958, and found that among those who published and received citations to their work in the five years immediately after receiving the PhD, the great majority (73%) continued to be active in publication and to be cited during the second five year period. In contrast, only six percent of those who published and were cited in the first period actually dropped out of sight in the second period, while two percent of this group 'bloomed' later in the second period. Most importantly, of the 21 sociologists who published but did *not* receive citations during the first five years, only one received citations during the second five year period. These data lead Lightfield to conclude that: 'Unless a person achieves a qualitative piece of research during his first five years, it seems unlikely that he will do so during his next five years — if at any time during his career'.[99]

The principle of reinforcement is corroborated by data from other disciplines. Among a sample of physicists, Cole and Cole[100] found that later productivity was substantially influenced by recognition of early work, so that those persons who received heavy citation continued to be highly productive, while for those who were not cited, productivity dropped off.

Similarly, among chemists, Reskin[101] found that early publication and citation to publication both contribute to productivity over the following decade. But she qualifies this finding by noting that the strength of the two effects varies with type of first employer. Specifically, early publication, in itself, is more important for those employed in research universities, while citation is particularly important for those employed in contexts without research emphasis. These patterns suggest that for scientists in research-oriented universities the immediate and informal collegial recognition which follows publication may be more important in maintaining productivity than the formal but more delayed reinforcement of citation. However, for scientists in less research-oriented settings, which do not provide informal and material rewards for publishing, the formal recognition of citation may be especially important, because citation can symbolize ties to the larger scientific community and encourage continued conformity to its norms. This highlights, again, the critical importance of organizational context in the operation of reward processes.

Summary and Conclusion

Currently, explanations of productivity form three major categories, respectively focusing upon: (1) individual-level characteristics (psychological characteristics; work habits; demographic characteristics); (2) environmental location; and (3) feedback processs of 'cumulative advantage' and 'reinforcement'. While certain variables from each perspective do correlate strongly with productivity, no one study or perspective explains the vast variation in scientific productivity, and the challenge for productivity studies lies in the capacity to combine perspective and untangle effects.

Among individual-level variables, certain psychological and attitudinal factors correlate with productivity in science. Specifically, strong motivation and autonomy or self-direction are characteristic of the most productive scientists. Ability and talent, on the other hand, may be a prerequisite for training in science, but once the educational degree is obtained, measured ability does not appear to differentiate productivity levels among scientists. Furthermore, these individual-level traits and characteristics are strongly affected by the social and organizational context in which they occur.

Compared to the large number of psychological studies, those of behavioural work habits are few. These studies are appealing, nonetheless, since work routines (unlike factors such as 'insight' or 'imagination') are more adaptive strategies at the disposal of aspiring scientists. However, the causal relationship between habits and productivity are uncertain in these studies, and research is needed to indicate whether, in fact, certain work practices will increase productivity among scientists.

For some time, observers of science have contended that productivity is governed by the life-cycle or age of scientists. Although the strength and particular form of the age and productivity relationship varies between reported studies, most investigations have shown that productivity tends to decline with age. The issue of productivity and age is important to scientific endeavour. The exponential growth in the scientific community is levelling off, and in the next 20 years the average age of scientists will probably increase, as there are fewer new positions for young scientists to fill.[102] If, in fact, productivity declines with age, then scientific capacity may be affected by an older age structure in science.

The second category of studies focuses upon the structural context often overlooked in the investigations of individual-level characteristics. This second group of studies has emphasized the effects upon publication of particular aspects of environmental location: the calibre of graduate school training, the prestige of scientists' institutional affiliation, and organizational freedom in the institutional location.

Among these studies, institutional prestige emerges as one of the strongest correlates of publication-productivity. Although the causal relationship between productivity and prestige of location can operate in either direction, recent longitudinal studies[103] indicate the stronger effect of location upon productivity rather than vice versa. Still, it may be that high status departments have the foresight to select those who will *become* productive,[104] and that the positions are allocated on the basis of potential (though not present) contribution. However, even if the stronger causal mechanisms are of location upon performance, the investigations have failed to demonstrate the particular ways in which major institutions foster, and minor institutions retard, productivity — through time, resources, or a favourable reward structure.

While the psychological theories tend to assume a simple additive relationship between productivity and individual-level characteristics, the environmental studies begin to suggest a 'feedback' process whereby the prestige of first appointment has an impact upon productivity and, in turn, upon the prestige of second and subsequent appointments. These feedback processes are the focus of the reinforcement and cumulative advantage perspectives which maintain that early success in science leads to increased support for continued research.

Although direct tests of reinforcement and cumulative advantage are lacking, the processes raise a critical issue — the extent to which resources and recognition are allocated on the basis of meritocratic, as compared to nonmeritocratic, principles. This issue is critical to science because it relates, as we have discussed, to certain pivotal processes, including the distribution of scarce rewards and the development of potential talent.

However, whether the process of allocation is meritocratic or not, it contributes, nonetheless, to the formation of élites and sharply graded rankings in science.[105] A meritocratic or universalistic process of allocation does help insure that scientists will be assessed on the basis of performance; but it does not guarantee that all persons will have equal opportunity to accumulate credentials and produce the performance.[106] Since élite institutions have resources to advance the work of their incumbents, talent that finds its way into these locations has a heightened potential for acquiring further advantage.[107] These patterns of social selection, resource, and reward help to create and maintain a class structure by providing a stratified distribution of chances for performance in science.[108]

Increased funds, facilities, and recognition for research may be especially crucial, then, to those scientists of high potential who are unable to acquire support so easily as the stars in science. 'Little can be done to affect the least productive, and nothing need be done that could affect the most productive'.[109] However, the scientists in the middle who offer a good deal but do not benefit from cumulative advantage may be an effective target for efforts to increase both opportunity and productivity in science.

• *NOTES*

For his careful reading and thoughtful comments on an earlier version of this paper, I am grateful to Frank M. Andrews.

1. Paul D. Allison, *Processes of Stratification in Science* (New York: Arno Press, 1980); Jonathan R. Cole and Stephen Cole, *Social Stratification in Science* (Chicago: The University of Chicago Press, 1973); Robert Merton, 'Priorities in Scientific Discovery', in Merton, *The Sociology of Science* (Chicago: The University of Chicago Press, 1973), 286-324.

2. See Nicholas C. Mullins, *Science: Some Sociological Perspectives* (In-

dianapolis: Bobbs-Merrill, Inc., 1973). However, this is not to deny certain alternative and less manifest functions of publication, such as perpetuation of class distinctions, support of partisan interests, and control of university systems via propagation of 'esoteric knowledge'. See Anthony Skiff, 'Toward a Theory of Publishing or Perishing', *The American Sociologist*, Vol. 15 (August 1980), 175-83.

3. Cole and Cole, op. cit. note 1; Jerry Gaston, *The Reward System in British and American Science* (New York: John Wiley, 1978); Michael Mahoney, *Scientist as Subject: The Psychological Imperative* (Cambridge, Mass.: Ballinger Press, 1976); Derek Price, *Little Science, Big Science* (New York: Columbia University Press, 1963).

4. B.H. Gustin, 'Charisma, Recognition, and Motivation of Scientists', *American Journal of Sociology*, Vol. 78 (1973), 1119-34; E.A. Shils, 'Intellectuals, Traditions, and the Traditions of Intellectuals', *Daedalus*, Vol. 101 (Spring 1972), 21-30.

5. Leo Meltzer, 'Scientific Productivity in Organizational Settings', *Journal of Social Issues*, Vol. 12 (December 1956), 32-40.

6. Barbara F. Reskin, 'Scientific Productivity and the Reward Structure of Science', *American Sociological Review*, Vol. 42 (June 1977), 491-504.

7. Everett Carl Ladd and Seymour Martin Lipset, 'Survey of 4,400 Faculty Members at 161 Colleges and Universities', *The Chronicle of Higher Education*, Vol. 15 (21 November 1977), 12, and (28 November 1977), 2.

8. S.S. Blume and Ruth Sinclair, 'Chemists in British Universities: A Study of the Reward System in Science', *American Sociological Review*, Vol. 38 (February 1973), 126-38.

9. Nicholas Babchuck and Alan P. Bates, 'Professor or Producer: The Two Faces of Academic Man', *Social Forces*, Vol. 40 (May 1962), 341-48.

10. William C. Yoels, 'On "Publishing or Perishing": Fact or Fable?' *The American Sociologist*, Vol. 8 (1973), 128-34.

11. Alfred J. Lotka, 'The Frequency Distribution of Scientific Productivity', *Journal of the Washington Academy of Sciences*, Vol. 16 (June 1926), 317-23. Lotka's findings have recently been questioned: for a sample critique, with relevant references, see Michael H. MacRoberts and Barbara R. MacRoberts, 'A Reevaluation of Lotkas's Law of Scientific Productivity', *Social Studies of Science*, Vol. 12 (1982), 443-50.

12. Price, op. cit. note 3.

13. Jonathan R. Cole, *Fair Science: Women in the Scientific Community* (New York: The Free Press, 1979).

14. Reskin, op. cit. note 6, and Barbara Reskin, 'Scientific Productivity, Sex, and Location in the Institution of Science', *American Sociological Review*, Vol. 83 (March 1978), 1235-43.

15. Paul D. Allison and John A. Stewart. 'Productivity Differences Among Scientists: Evidence for Accumulative Advantage', *American Sociological Review*, Vol. 39 (August 1974), 596-606.

16. See Gaston, op. cit. note 3; J. Cole, op. cit. note 13; Richard A. Wanner, Lionel S. Lewis and David I. Gregorio, 'Research Productivity in Academia: A Comparative Study of the Sciences, Social Sciences, and Humanities', a paper delivered at the annual meetings of the American Sociological Association, New York, August 1980.

17. Cole and Cole, op. cit. note 1, 115.

18. Donald C. Pelz and Frank M. Andrews (eds), *Scientists in Organizations: Productive Climates for Research and Development* (Ann Arbor, Mich.: Institute for Social Research, 1976).

19. Robert Merton, 'The Matthew Effect in Science', in Merton, op. cit. note 1, 439-59.

20. Merton, ibid.; Harriet Zuckerman, 'Stratification in American Science', in E. O. Laumann (ed.), *Social Stratification: Research and Theory for the 1970s* (New York: Bobbs-Merrill, 1970), 235-54.

21. See Jessie Bernard, *Academic Women* (University Park, Penn.: Pennsylvania State University Press, 1964); Bernice T. Eiduson, *Scientists: Their Psychological World* (New York: Basic Books, 1962); M.W. McCarrey, 'Research Climate and Scientific Accomplishment: An Interview with Gerhard Herzberg', *Studies in Personnel Psychology*, Vol. 13 (April 1971), 21-32; Max Weber, 'Science as a Vocation', in H.H. Gerth and C. Wright Mills (eds), *From Max Weber: Essays in Sociology* (New York: Oxford University Press, 1946), 129-56; Zuckerman, op. cit. note 20.

22. Pelz and Andrews, op. cit. note 18.

23. See R.B. Cattell and J.E. Drevdahl, 'A Comparison of the Personality Profile of Eminent Researchers with that of Eminent Teachers and Administrators, and that of the General Population', *British Journal of Psychology*, Vol. 46 (November 1955), 248-61; Robert Knapp, 'Demographic, Cultural, and Personality Attributes of Scientists,' in C. Taylor and F. Barron (eds), *Scientific Creativity: Its Recognition and Development* (New York: John Wiley, 1963), 205-16; Anne Roe, *The Making of a Scientist* (New York: Dodd, Mead and Company, 1953); Roe, 'Psychology of Scientists', in K. Hill (ed.), *The Management of Scientists* (Boston, Mass.: Beacon Press, 1964), 49-71.

24. Jack Chambers, 'Creative Scientists of Today', *Science*, Vol. 145 (11 September 1964), 1203-05; Anne Roe, 'A Psychologist Examines 64 Eminent Scientists', *Scientific American*, Vol. 187 (December 1952), 21-25; Morris I. Stein, 'Creativity and the Scientist', in B. Barber and W. Hirsch (eds), *The Sociology of Science* (New York: The Free Press, 1962), 329-43; Taylor and Barron, op. cit. note 23; Calvin W. Taylor and Robert L. Ellison, 'Biographical Predictors of Scientific Performance', *Science*, Vol. 155 (3 March 1967), 1075-80.

25. Roe, op. cit. note 24.

26. Chambers; Stein; Taylor and Ellison, op. cit. note 24.

27. Chambers, op. cit note 24, 1204.

28. See Eiduson, op cit. note 21; Gerald Gordon and Edward V. Morse, 'Creative Potential and Organizational Structure', in M.J. Cetron and J.D. Goldhar (eds), *The Science of Managing Organized Technology*, Vol. II (New York: Gordon and Breach, 1970), 517-31; Hans Selye, *From Dream to Discovery: On Being a Scientist* (New York: McGraw Hill, 1964).

29. A.J. Cropley and T.W. Field, 'Achievement in Science and Intellectual Style', *Journal of Applied Psychology*, Vol. 53 (1969), 132-35.

30. John M. Wilkes, 'Styles of Thought, Styles of Research, and the Development of Science' (Worcester, Mass.: Worcester Polytechnic Institute, Department of Social Science and Policy Studies, 1980, mimeo).

31. Allison and Stewart, op. cit. note 15, 597.

32. Price (op. cit. note 3), reports, for example, that among American physicists, the average IQ is 140 — which places these scientists in the upper one percent of the

distribution of IQ scores in the United States.

33. Alan E. Bayer and John Folger, 'Some Correlates of a Citation Measure of Productivity in Science', *Sociology of Education*, Vol. 39 (Fall 1966), 381-90; Cole and Cole, op. cit. note 1.

34. See Cole and Cole, op. cit. note 1, 248-49.

35. See Cole and Cole, op. cit. note 1.

36. William Shockley, 'On Statistics of Individual Variations of Productivity in Research Laboratories', *Proceedings of the Institute of Radio Engineers*, Vol. 45 (March 1957), 279-90.

37. Frank Andrews, 'Creative Process', in Pelz and Andrews, op. cit. note 18, 337-65.

38. Ibid.

39. Patrick E. Connor, 'Scientific Research Competence as a Function of Creative Ability', *IEEE Transactions on Engineering Management*, EM-21 (February 1974), 2-9.

40. Gordon and Morse, op. cit. note 28, 531.

41. C. Wright Mills, *The Sociological Imagination* (New York: Oxford University Press, 1959), 195-226.

42. See, for example, Arthur L. Stinchcombe, 'On Getting "Hung Up" and Other Assorted Illnesses', *Johns Hopkins Magazine* (Winter 1966), 25-30.

43. Lowell L. Hargens, *Patterns of Scientific Research: A Comparative Analysis of Research in Three Scientific Fields* (Washington, DC: The American Sociological Association, 1975); Hargens, 'Relations Between Work Habits, Research Technologies, and Eminence in Science', *Sociology of Work and Occupations*, Vol. 5 (February 1978), 97-112.

44. Rita James Simon, 'The Work Habits of Eminent Scientists', *Sociology of Work and Occupation*, Vol. 1 (August 1974), 327-35.

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

46. H.C. Lehman, 'The Chemist's Most Creative Years', *Science*, Vol. 127 (23 May 1958), 1213-22; Lehman, 'The Age Decrement in Scientific Creativity', *American Psychologist*, Vol. 15 (1960), 128-34.

47. Pelz and Andrews' study (op. cit. note 18), considers, but does not focus upon, individual-level characteristics such as age. The study's emphasis is upon organizational variables and productivity (which includes, but is not limited to, publication-measures).

48. Alan E. Bayer and Jeffrey E. Dutton, 'Career Age and Research-Professional Activities of Academic Scientists', *Journal of Higher Education*, Vol. 48 (May/June 1977), 259-82.

49. Stephen Cole, 'Age and Scientific Performance', *American Journal of Sociology*, Vol. 84 (January 1979), 958-77.

50. Pelz and Andrews, op. cit. note 18, 75-76.

51. Karin D. Knorr, Roland Mittermeir, Georg Aichholzer and Georg Waller, 'Individual Publication Productivity as a Social Position Effect in Academic and Industrial Research Units', in F. Andrews (ed.), *The Effectiveness of Research Groups in Six Countries* (Cambridge: Cambridge University Press, 1979), 55-94.

52. Eugene Hammel, 'Report of the Task Force on Faculty Renewal' (Berkeley, Calif.: University of California-Berkeley, Population Research, January 1980), 5. Hammel's productivity measures include gauges of teaching and university service

as well as research publications. Publications, however, correlate very strongly with the other productivity dimensions, and when the publication measures are separated, they show the same age and productivity patterns as the combined measures.

53. Ibid., 4-5.

54. See Howard Becker, Blanche Greer, Everett Hughes and Anselm Strauss, *Boys in White* (Chicago: The University of Chicago Press, 1961): Rue Bucher and Joan Stelling, *Becoming Professional* (Beverly Hills, Calif.: Sage Publications, 1977); Mullins, op. cit. note 2; Harriet Zuckerman, *Scientific Elite: Nobel Laureates in the United States* (New York: The Free Press, 1977).

55. Zuckerman, op. cit. note 54.

56. Thomas Kuhn, *The Structure of Scientific Revolution* (Chicago: The University of Chicago Press, 2nd edn, 1970); Mullins, op. cit. note 2.

57. Zuckerman, op. cit. note 54.

58. Diane Crane, 'Scientists at Major and Minor Universities: A Study of Productivity and Recognition', *American Sociological Review*, Vol. 30 (October 1965), 699-715.

59. Crane's sample is confined to males.

60. Barbara Reskin, 'Academic Sponsorship and Scientists' Careers', *Sociology of Education*, Vol. 52 (July 1979), 129-46.

61. J. Scott Long, Paul D. Allison and Robert McGinnis, 'Entrance into the Academic Career', *American Sociological Review*, Vol. 44 (October 1979), 816-30.

62. Daryl E. Chubin, Alan L. Porter and Margaret Boeckman, 'Career Patterns of Scientists', *American Sociological Review*, Vol. 46 (August 1981), 488-96.

63. See J. Scott Long, 'Productivity and Academic Position in the Scientific Career', *American Sociological Review*, Vol. 43 (December 1978), 889-908; Long et al., op. cit. note 61. Long et al. report, specifically, that predoctoral productivity is the strongest predictor, and that prestige of first employer is the second strongest predictor, of future publication. Pre-doctoral productivity, however, does not correlate strongly with prestige of *first* position.

64. Robert T. Blackburn, Charles E. Behymer and David E. Hall, 'Research Note: Correlates of Faculty Publications', *Sociology of Education*, Vol. 51 (April 1978), 132-41; Peter Blau, *The Organization of Academic Work* (New York: John Wiley, 1973); Long, op. cit. note 63; J. Scott Long and Robert McGinnis, 'Organizational Context and Scientific Productivity', *American Sociological Review*, Vol. 46 (August 1981), 422-42; Mary Glenn Wiley, Kathleen S. Crittenden and Laura D. Birg, 'Becoming an Academic: Early vs. Later Professional Experience', *Sociological Focus*, Vol. 14 (April 1981), 139-45.

65. Cole and Cole op. cit. note 1; Lowell Hargens and Warren Hagstrom, 'Sponsored and Contest Mobility of American Academic Scientists', *Sociology of Education*, Vol. 39 (Fall 1966), 24-38.

66. Long, op. cit. note 63.

67. This study limits its inquiry to effects of *academic* location.

68. Long and McGinnis, op. cit. note 64.

69. Ibid., 122.

70. Charles H. Anderson and John D. Murray, *The Professors: Work and Life-Styles Among the Academicians* (Cambridge, Mass.: Schenkman, 1971).

71. Blau, op. cit. note 64.

72. Talcott Parsons and Gerald M. Platt, 'Considerations of the American

Academic System', *Minerva*, Vol. 5 (Summer 1968), 497-523.

73. The undergraduate institution may offer seminars, but the experience and acuity of the audience is likely to be far more limited that that of the graduate audience.

74. Pelz and Andrews, op. cit. note 18.

75. Barbara Reskin. 'Social Differentiation and the Social Organization of Science', *Sociological Inquiry*, Vol. 48 (1978), 6-37.

76. See Judith R. Blau, 'Patterns of Communication Among Theoretical High Energy Physicists', *Sociometry*, Vol. 37 (1974), 391-406; Blau, 'Scientific Recognition: Academic Context and Professional Role', *Social Studies of Science*, Vol. 6 (1976), 533-45; Blau, 'Sociometric Structure of a Scientific Discipline', *Research in Sociology of Knowledge, Science, and Art*, Vol. 1 (1978), 191-206; Price, op. cit. note 3.

77. See Donald C. Pelz, 'Some Social Factors Related to Performance in a Research Organization', *Administrative Science Quarterly*, Vol. 1 (1956), 310-25.

78. Steven Box and Stephen Cotgrove, 'The Productivity of Scientists in Modern Industrial Research Laboratories', *Sociology*, Vol. 2 (May 1968), 163-72.

79. Ibid., 170.

80. Howard M. Vollmer, 'Evaluating Two Aspects of Quality in Research Program Effectiveness', in Cetron and Goldhar, op. cit. note 28, Vol. IV, 1487-501.

81. Michael J. Stahl and Arthur E. Stevens, 'Reward Contingencies and Productivity in a Government Research and Development Laboratory', a paper presented at the Joint National TIMS/ORSA Meetings, San Francisco, California, 9 May 1977, mimeo.

82. S.M. Parmerter and J.D. Garber, 'Creative Scientists Rate Creativity Factors', *Research Management*, Vol. 14 (November 1971), 65-70.

83. Pelz and Andrews, op. cit. note 18.

84. Leo Meltzer and James Salter, 'Organizational Structure and the Performance and Job Satisfaction of Physiologists', *American Sociological Review*, Vol. 27 (June 1962), 351-62.

85. An exception is Shockley's 'multiplicative model of mental factors', discussed earlier.

86. Gaston, op. cit. note 3.

87. Zuckerman, op. cit. note 54, 60.

88. Ibid., 61.

89. Cole and Cole, op. cit. note 1, 120.

90. Merton, op. cit. note 19. The term 'Matthew Effect' derives from St. Matthew's Gospel (13.12, Authorised Version): 'For whosoever hath, to him shall be given, and he shall have more abundance: but whosoever hath not, from him shall be taken away even that he hath'. (For an alternative version, see 25.29.)

91. Merton, ibid.

92. Gaston, op. cit. note 3.

93. Allison and Stewart, op. cit. note 15.

94. Ibid.

95. Gaston, op. cit. note 3, 144.

96. B.F. Skinner, *The Behavior of Organisms* (New York: Appleton-Century, 1938); Skinner, *Science and Human Behavior* (New York: Macmillan, 1953); Skinner, *Contingencies of Reinforcement* (New York: Appleton-Century-Crofts, 1969).

97. Cole and Cole, op. cit. note 1, 114.

98. Timothy E. Lightfield, 'Output and Recognition of Sociologists', *The American Sociologist*, Vol. 6 (1971), 128-33.
99. Ibid., 133.
100. Cole and Cole, op. cit. note 1.
101. Reskin, op. cit. note 6.
102. S. Cole, op. cit. note 49.
103. See Long, op. cit. note 63; Long and McGinnis, op. cit. note 64.
104. Long, op. cit. note 63.
105. Zuckerman, op. cit. note 54.
106. See Mary Frank Fox, 'Sex, Salary, and Achievement: Reward-Dualism in Academia', *Sociology of Education*, Vol. 54 (April 1981), 71-84, esp. 82.
107. Cole and Cole, op. cit. note 1; Robert Merton, 'The Sociology of Science', in Merton and J. Gaston (eds), *The Sociology of Science in Europe* (Carbondale, Ill.: Southern Illinois University Press, 1977), 3-141.
108. Merton, op. cit. note 107.
109. Hammel, op. cit. note 52, 12.

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