Chapter 4
The Sociology of Academic Careers: Problems and Prospects

Joseph C. Hermanowicz

Background

This chapter reviews major lines of research in the sociology of science to inform inquiry into the study of academic careers. The chapter seeks to serve as a bridge between the discipline of sociology and the field of higher education. The two fields have much to offer the other. At their best, the benefits are reciprocal. Studies in education can profit from sociological concepts and abstract theorizing as frames through which to ground scholarly inquiry. Studies in sociology can make much of educational processes, settings, and organizations to develop and refine concepts and theory.

Despite the professed benefits, the relationship between the fields has always been uneasy. Neal Gross labored to reveal the potential fruits of the union as early as 1959. After some development, and in the aftermath of expansion and heightened interest in the higher education system following World War II, Donald Light, drawing attention to the study of university faculty, sounded the concerns of an as-yet immature science:

...the sociology of academicians suffers from disorganization...First, the research is uncoordinated...The second weakness which keeps the sociology of the academic profession from maturing as a science is the lack of good theory on which to base research (Light 1973, pp. 2–3).

In his essay, “Development of the Sociology of Higher Education,” Burton Clark (1973) outlined major streams of research, future prospects, and potential pitfalls for a cross-fertilization of the two fields. The combination has worked only partially. Differentiated topical essays in Gumpert (2007a) bespeak significant knowledge consolidation and field maturation. However, despite this gain, the relationship struggles. As Gumpert has put it:

J. C. Hermanowicz (2017)
Department of Sociology, The University of Georgia, Athens, GA 30602, USA
e-mail: jch1@uga.edu

jch1@uga.edu
...in spite of the dramatic expansion within distinct lines of inquiry in this field over the
past 30 years—and perhaps because the research is pursued by individuals from differ-
ent professional contexts—the visibility of the sociology of higher education as a field
remains questionable...One unfortunate side effect of this separation is that the intellectual
resources each draws upon may be unduly narrow and partial by neglecting prior work
on a topic, such that the research fails to make a cumulative contribution that is seen by
researchers working from these different locations (Gumport 2007b, pp. 338–339, original
emphasis).

Sociologists in academic departments and higher education scholars in schools of
education still struggle to communicate effectively in scholarship that does indeed
cross boundaries between the fields. This stems in part from different scholarly
norms of communication—researchers in each field seeking to conform to valued
preferences of their most immediate peers—as well as from allegedly different pro-
fessional goals. An often overstated divide between basic and applied inquiry, be-
tween an emphasis on theory versus practice, handicaps a developmental goal of
field maturation. For many scholars, the divide is not merely overstated but falsely
drawn: “...some of the most important and difficult intellectual questions in sociol-
ogy turn on so-called practical problems” (Sampson 2010, p. 66). Marx, Durkheim,
and Weber, the theoretic triumvirate of sociology, were, first and foremost, con-
cerned with the practical problems of their day. They may be viewed as among the
greatest applied sociologists who made lasting theoretic contributions to the study
of society.

The ongoing confusion has come with costs. According to Stevens, Armstrong,
and Arum, the result is that “higher education [as a field of study] remains with-
out an intellectually coherent sociology. Instead, the varied and empirically rich
sociological work on higher education is scattered throughout the field, creating
at times a narrowness of analytic vision and inhibiting the benefits that can ac-
crue from integrated scholarly discourse” (Stevens et al. 2008, p. 128). While the
amount of work in the sociology of higher education has increased substantially
since Light and Clark commented on developmental issues within the field in
1973, aspects of the problems they identified continue to characterize attempts
at synthesis.

In this chapter, I focus on one domain of the sociology of higher education—
the study of academic careers—and organize a key sociological literature that can
help advance inquiry on the topic between fields. In the field of higher education,
systematic study of academic careers typically falls under the heading of and in-
corporates a literature on “faculty,” as indicated, for instance, in the organization
of the Association for the Study of Higher Education. In the field of sociology,
this study most readily draws on literature in the sociology of science. The sociol-
ogy of science “deals with the social conditions and effects of science, and with
the social structures and processes of scientific activity” (Ben-David and Sullivan
1975, p. 203). As a subspecialty area of sociology, “it resembles the sociologies
of art, law, religion, politics, economy, and the family, since each examines in-
itutional organization, structure, processes, contexts, and products” (Zuckerman
1988, p. 511).
Focusing the lens, a subset of the sociology of science has concerned the study of scientists and scientific careers.¹ To a very great extent, the samples upon which these studies are based, and the theorizing that has emerged from them, have been drawn on academics situated in departments and universities, rather than scientists working in industry or government. Herein lies the fundamental basis of the link between the sociology of science and a sociology of academic careers. It seems that a substantial portion of the work on the sociology of scientists was conducted as though one could substitute the term “professors” or “academe” in order to extend its reach. Thus, for example, when one reads in this literature of the “reward system of science,” one may extend the idea to the “reward system of academe.” Or in reading of “stratification in science” one may cognitively attempt a reconfiguration as “stratification in higher education” or in academic careers.

This is not to say that all of the conclusions drawn from this body of work, dealing as it largely does with physical and biological scientists, mathematicians and engineers, are applicable to academics across all fields. These extensions, extrapolations, and reconfigurations are precisely where new empirical work awaits to be done. This is a subject to which we shall return. The point here is that this is the closest sociological literature to the study of academic careers and it therefore holds the greatest potential for development, expansion, and generalizability across the sociology and higher education fields.²

The cross-fertilization and maturation of a field is dependent on two conditions. First, it is necessary to identify substantive problem areas of mutual concern to the fields, a condition sought to be satisfied by conducting the present review. Second, it is necessary to develop theory. Abstracting explanation from particularities serves the goal of transcending boundaries that falsely separate shared substantive areas. The inadequate use of concepts and theory formation has perhaps existed as the chief impediment to a bona fide sociology of higher education. The juxtaposition of terms underscores the point: to have a sociology of something, one must conceptualize problems abstractly in terms of systemic patterns of thought, behavior, action, or processes. By reviewing the most relevant work in the sociology of science, this chapter aims to make explicit the sociological concepts and frames of inquiry

¹ The sociology of science is wide-ranging, and only one part of it establishes scientific careers as an object of study. Other concerns consist, for example, in the construction of scientific knowledge. A major distinction in the sociology of science lies between institutional sociology of science, developed principally by Robert K. Merton and also known as “Mertonian sociology of science,” and constructivist sociology of science, developed largely (though not exclusively) following the bulk of Merton’s work and that of others whose writings closely paralleled the Merton tradition. For reviews of the sociology of science as a specialty, see Ben-David and Sullivan (1975), Hess (1997), and Zuckerman (1987).

² One might wonder why a cousin specialty, the sociology of education, did not develop a sustained research line into academic careers or, for that matter, into concerns of higher education more generally. The answer is that the classical sociological theorists confined themselves to schooling in the earliest years, believing, in Durkheim’s words, that this is “where real life began” (Durkheim [1925] 1961). For additional discussion, see Hermanowicz (2007b).
that may be used to expand toward a more theoretically guided study of academic careers.

Finally, it is worth noting that the very idea of a career is sociologically rooted (Barley 1989). We may understand a career “to be the set of hierarchically ordered and professionally relevant positions within a field or discipline in which entrance and progression are regulated by peers” (Lawrence 1998). This is to say that to have a career, and in particular one in academe, there exist: advisors, mentors, college origins, doctoral programs, social class backgrounds, colleagues, collaborative networks, postdoctoral appointments, employing departments, employing universities, disciplines, fields, professions, and reward systems—among other social constructions—that make careers possible and which differentiate them. To study careers is to examine how they are constituted and patterned by constellations of social conditions and social processes. Regardless of a scholar’s field, any inquiry into academic careers—if that is indeed what one is studying—requires sociological theorizing, since we cannot speak faithfully of careers independent of the social arrangements by which they are formed (cf. Finkelstein 2006).

In the next section, I review the major lines of inquiry that have defined the segment of the sociology of science most relevant to the study of careers. I organize the discussion topically, both to convey the breadth of prior inquiry and as a means to suggest applicability and points of departure in new work conducted by scholars in sociology and the higher education fields. The topics are arranged in four substantive parts: foundations; stratification and careers; gender, productivity, and careers; and the social control of careers. In the conclusion, I illustrate areas in which scholars across fields may expand and build upon the reviewed work, such that a prior sociology of scientific careers may more deliberately propagate and profit the theoretically guided study of academic careers.

**Learning from the Sociology of Science: Major Lines of Inquiry**

**Foundations**

Robert K. Merton (1910–2003), professor of sociology at Columbia University, is credited as the primary founder of the sociology of science (for a review of Merton, see Calhoun 2010 and Cole 2004). The Columbia “program in the sociology of science,” beginning in the 1960s and extending to the 1970s, consists of a sustained line of work by Merton, his students, and collaborators that helped to institutionalize a tradition of scholarship that came to be known eponymously across institutional and disciplinary boundaries as “Mertonian sociology of science.” Scholars not directly associated with Merton but conducting research on science and scientists in the Mertonian vein—during and following this period—are likewise associated with this tradition. The tradition is institutional in the sociological usage of the term:
the work sought to understand rule-bound and standardized behavior patterns associated with norms and roles comprising social systems, in this instance, the social system of science (for a discussion of institutional analysis, see Turner [1997]). Theoretically, this tradition of work was situated primarily in functional analysis, concerned as it was with the operation and maintenance of a social system and the actors who compose it. A variety of topics pertinent to careers were pursued by scholars of institutional sociology of science, which shall be explored in the pages to follow. No topic was pursued more extensively, however, than a body of work on stratification in science.

The paramount interest in stratification was borne of Merton’s claim that science has an “ethos,” indicated by a set of four delimitable norms that govern scientists’ behavior, which thereby form a theory of the normative structure of science (Merton 1973a). The norms, now widely recognized but also contested, include: universalism, communism, disinterestedness, and organized skepticism. The norm of universalism holds that assessments of contributions to knowledge should not be influenced by personal or social attributes of the contributor and that rewards should be conferred in ways commensurate with contributions. Universalism is set in contrast to particularism, which refers to factors such as age, race, gender, religion, political or sexual orientation, said to be functionally irrelevant to institutional operation but used in the evaluation of people and their work. The norm of communism (also “communalism”) holds that knowledge must be shared, not kept secret. The norm of disinterestedness holds that the motives and actual conduct of science should be driven without personal bias. The norm of organized skepticism holds that scientific judgments are to be held until all necessary evidence is on hand to make evaluations of scholarship.

The norms are said to bind scientists’ behavior. Like all norms, those that compose an ethos of science are understood to be acquired through socialization and internalized by rule-bound performance in a scientific career, subject to and reinforced by positive and negative sanctions. Merton claimed that the ethos of science is inferred from what scientists write about science and from how they behave, including observable reactions by the scientific community to those instances when norms are violated (Merton 1973a; Zuckerman 1988, p. 515).

Norms specify shared expectations of behavior. The ethos of science never meant that all scientists act always according to the norms. As Merton noted (1976, p. 40), there is at times a “painful contrast” between expectations and actual behavior. Zuckerman has reminded us that:

This ‘painful contrast’ does not mean that the norms of science do not exercise patterned control over behavior any more than occasional homicides mean that norms prohibiting murder are either absent or inconsequential. Sociologists seldom need to be reminded that norms and behavior are never perfectly correlated (1988, pp. 515–516).

In examining the norms, it quickly becomes evident that they focus especially on one of the several roles performed by academic scientists (and, again by extension, by higher education faculty): the role of research and scholarship leading to publication. Whether it is judging contributions, sharing contributions, curbing personal
bias in the motivation to contribute, or in attempting to acquire all necessary evidence in order to assess contributions, the subject of the rules is clear indeed.

The norms assume this particular formulation, and consequently are legitimized, because of the overriding institutional goal that Merton saw science serving: the goal of science to extend socially certified knowledge. In this light, the research role is paramount, and arguably remains so even amidst organizational goals and missions of various colleges and universities that may stress different roles, such as that of teaching, over others. This idea does not originate from opinion, preference, or personal points of view, however, rather from a theoretic consideration of how academic roles are functionally arranged as part of the social system of performing wide varieties of work in the modern-day college or university. In their formulation that warrants repeating, Merton and Zuckerman provide a self-exemplifying account for why the research role has been, is, and will always remain, more central than any other role that scientists perform:

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...The research role, which provides for the growth of 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 the 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 or editors (Merton and Zuckerman 1973, p. 520).

In light of how science is argued to operate systemically, attempts by colleges and universities are futile when they seek to "re-prioritize" roles or tamper with local reward systems in an effort to modify behavior. The formulation above does not diminish the importance of ancillary roles, although it may be tempting to draw this conclusion from the preponderance of subsequent scholarly attention that Mertonians gave to the research role of scientists. (For instance, one could also argue that if scientists did not teach at all, they would not have university jobs, or that if universities had no students, there would be nothing to administer. The advancement of science, however, does not depend on these conditions per se.) The formulation explains why the research role is functionally central to satisfying the institutional goal of science and why, consequently, practitioners and observers alike may always assign the role greater prestige, even in contexts of professed valuations of the other roles that academics perform.

This also accounts for the prominence played by peer recognition in Merton's discussions of scientists and their careers (Merton 1973b; see also Hagstrom 1965, esp. Chaps. 2 and 3). How may one view the phenomenon of recognition sociologically? In Merton's formulation, recognition is socially validated testimony that one
has fulfilled the goal of science—to extend certified knowledge (1973b). Recognition, therefore, is itself an institutionalized process in the system of science: it is both essential to progress and expected in trained individuals (cf. Glaser 1965; Gustin 1973).

These foundational formulations set forth a framework in which to examine careers. They made explicit a set of norms that may be taken to structure careers in professional and disciplinary milieux. The norms may be used as a basis to examine variation in and the stratification of careers. By the same token, an "ethos of science" specifies an order against which academics derive meaning about their careers, in various ways, and thus further establishes a basis on which to examine differences in the experience and practice of academic work. The importance attributed to recognition in this framework presents an additional lens through which to study career processes, including motivation, commitment, satisfaction, and productivity, among others.

Of all the norms guiding an ethos of science, universalism received the most attention in research. It became the main means by which to assess inequality in career attainments. It was, more fundamentally, the mechanism by which to examine the workings of the scientific reward system, to understand whether distributions of recognition resulted more so, or under what conditions, from universalistic versus particularistic criteria. A major line of inquiry on stratification in science developed to examine the operation of this norm in the larger context of the ethos of science.

Stratification and Careers

The literature on stratification and scientific careers may be considered via five organizing topics of research: processes of cumulative advantage and disadvantage; organizational bases of stratification; the construction of reputation, visibility, and influence; the relationship between age and achievement; and mobility patterns of scientists. It is important to bear in mind that discussion in several subsequent sections of this chapter also overlap with considerations of stratification (e.g., gender, productivity, and careers; recruitment and socialization; experience of work, etc.). However, as they tend to constitute distinct topics unto their own, they will be treated separately from the discussion that immediately follows.

Cumulative Advantage and Disadvantage

By one view, stratification in science may seem at odds with an "egalitarian ethos of science.” Zuckerman (1970) explained that stratification in science arises, amidst a correlation between contributions to science and investigators’ professional standing, from differential processes of resource allocation. These processes involve selective recruitment and socialization, access to publication and research facilities, and recognition of scientists’ work through citations of their published research.
Processes of cumulative advantage and disadvantage in science have been examined extensively to reveal stratification in the making. Cumulative advantage and disadvantage is a theory developed by Merton and elaborated by others to explain inequality in science. The theory explains how increasing disparities come to characterize the "haves" and "have-nots" over the course of a career.

Processes of individual self-selection and institutional social-selection interact to affect successive probabilities of access to the opportunity structure in a given field... When the role-performance of an individual measures up to demanding... standards... this initiates a process of cumulative advantage in which the individual acquires successively enlarged opportunities to advance his work (and the rewards that go with it)...[those who find their] way into [elite] institutions have[ve] the heightened potential of acquiring differentially accumulating advantage (Merton 1977, p. 89; quoted in Zuckerman 1988, p. 531).

Put differently, the theory holds that "certain individuals and groups repeatedly receive resources and rewards that enrich recipients at an accelerated rate and conversely impoverish (relatively) the non-recipients" (Zuckerman 1977, pp. 59–60). That is, the rich get richer at a rate that makes the relatively poor become even poorer. Zuckerman (1998) has elaborated on the development of this theory, and DiPrete and Eirich (2006) have considered its utility and application well outside of science.

The "Matthew Effect" elaborated by Merton is a special case of cumulative advantage. Named after the Gospel of St. Matthew, it holds that already-recognized scientists receive disproportionate recognition for subsequent contributions. "Eminent scientists get disproportionate great credit for their contributions to science while relatively unknown scientists tend to get disproportionately little credit for comparable contributions" (Merton 1973c), or, following the Gospel, "For unto every one that hath shall be given, and he shall have abundance; but from him that hath not shall be taken away even that which he hath" (Matthew 25:29). In social terms, the Matthew Effect, to the extent it may exist empirically (Cole 1970), is antimeritocratic since it violates the universalistic norm by favoring a particularistic attribute of an investigator (in this case, professional standing established by prior recognition).

Cole and Cole (1967) examined the operation of the reward system in science by focusing on the relationship between quantity and quality of publication among 120 university physicists. They found that quality of publication was more important than quantity in eliciting recognition in the form of awards, positions in prestigious academic departments, and renown among colleagues. In ways consistent with the theory of cumulative advantage, they concluded that the reward system operates to encourage creative scientists to remain productive while discouraging less creative scientists from further research.

Allison and Stewart (1974) incorporated the theory of cumulative advantage to account for productivity differences among scientists. They note that publication productivity among scientists (as among academics within any given field) tend to be highly skewed. Drawing on a sample of chemists, physicists, and mathematicians, they argue that productive scientists maintain or increase their productivity, while scientists who produce little go on to produce even less later on. They note the major implication of applying this theory to their data: the distribution of produc-
tivity becomes increasingly unequal as a cohort of scientists ages. The magnifying inequality over time is associated with change in the amount of time that scientists spend on research. Additional evidence in support of these claims was subsequently provided from a sample of true cohorts (as opposed to synthetic cohorts) of chemists and biochemists (Allison et al. 1982). In an extrapolation to the higher education field, Bentley and Blackburn (1990) assessed whether groups of institutions accumulate advantage relative to others by comparing the research activities across five Carnegie types of schools. They concluded that research advantages snowball in ways proportionate to the research scope of schools.

The book-length treatment, Social Stratification in Science, by Cole and Cole (1973), remains as much a treasure trove of ideas for further research today as when it was published. Using samples of physicists, the authors examine through multiple means the processes by which recognition is distributed among scientists, patterns that define the structure of stratification in the scientific community, and the manner by which the reward system of science operates to produce stratification. With only few exceptions, the authors conclude in this work that science operates largely universalistically.

The main purpose of the research reported in this book has been to investigate the extent and ways in which science departs from its rational and universalistic ideal. All our studies have focused on this one problem area. The general conclusion of our research has been that science does to a great extent approximate its ideal of universalism. In almost all cases where science departs from the ideal we find the process of accumulative advantage at work. People who have done well at time 1 have a better chance of doing well at time 2, independently of their objective role performance (Cole and Cole 1973, p. 235).

This conclusion would be subjected to considerable scrutiny and empirical work in the subsequent years, with numerous authors advancing competing claims. The review by Long and Fox (1995) summarizes the body of work that emerged to further assess the occurrence of particularism in science and indeed, conclusions evolved as additional evidence emerged, such that by 1992, Cole observed:

Is science universalistic? My answer now is substantially different from the one put forth in my 1973 monograph with Jonathan Cole...I conclude that the traditional studies failed to tap adequately particularism based on cognitive criteria and location in social networks rather than on statuses occupied by scientists (Cole 1992, pp. xi-xii).

*Cognitive criteria* refer to factors such as evidence, models, and theories used in people’s work; *location in social networks* encompasses the position of a social actor vis-à-vis the status of others with whom that actor interacts and/or by which the actor is influenced; *statuses* refer to professional stature as indicated by factors such as position, rank, and citation.

Still, the 1973 treatment is evocative. Consider the “Ortega Hypothesis,” that large numbers of average scientists contribute substantially to the advancement of science through their research (Cole and Cole 1973, pp. 216–234). The authors find the contrary, that scientific progress results not from the labor of all “academic classes,” but is rather primarily dependent on the work of an elite. They use this finding to question, then as one could today, whether the same rate of knowledge
advancement could be maintained even if the number of scholars were greatly reduced. The implications of the idea achieve still greater prominence in times of fiscal austerity, erosion of state funds for higher education, and clogged competition within federal and nonfederal granting agencies. It also relates to rising university expenses of journals and the proliferation of writing for publication, the concomitant ascendance of the research role across university types, consequent increases in referee roles and demands, and the organization of faculty time to “manage” what may largely consist of unnecessary work.

Zuckerman’s (1977) book on Nobel laureates in the United States simultaneously illustrates empirical concern for the operation of the reward system in science and a sustained inquiry into a specific stratum of individuals who comprise a larger occupational community. One could conceivably devise parallel inquiry into other important subsets of the academic community: for instance, community college faculty, the faculty of comprehensive universities, or the careers of part-time faculty, all of these groups ascending in their importance by their proportion among all faculty, by the proportion of institutions that employ them, and by the proportion of students they teach.

Zuckerman presents an exemplary occupational study. She theorizes about elites and their role, in occupations and in society. She situates her research subjects in the context of the Nobel Prize and its background. She then accounts for the Nobelists’ careers, first by examining their social origins and subsequently their schooling and mentors, followed by an analysis of the work that resulted in the prize and their careers following the award.

The author bases this work on biographical and bibliographic data on laureates, and incorporates data from interviews that she conducted with 41 prize-winners. The study is arguably the most sustained in its consideration of the theory of cumulative advantage. Well before the award, laureates are successively advantaged through stages of their careers. The advantages produce widening disparities between the elite and other scientists both in performance and in rewards, which create and maintain a system of social stratification.

The social class origins of scientists constitute another, though less extensively researched, area in which to consider the accumulation of advantage. In 1969, Crane found that class origin was related to holding academic positions at top-ranking universities (Crane 1969). The effect was traceable in part to the fact that lower-class academics were more likely to have received doctorates from lower-ranking institutions, which higher-ranking institutions were less likely to draw upon for their faculties. Crane found that even lower-class academics who had earned doctorates from high-ranking institutions still were less likely than their middle-class counterparts to obtain employment in “major” universities. By 1992, Xie drew different conclusions, albeit with different variables and measures (Xie 1992). Xie found that the social origins of scientists were largely homogenous across fields and that the disadvantages of having come from a low-status family were mediated by education.
Organizational Bases

A body of work has examined the role of department, university, (and, to a more limited extent, field) as organizational bases in stratifying scientific careers. Long (1978) investigated the relationship between productivity (indicated by publication and citation) and academic position using a sample of male biochemists. In this work, Long found a strong effect of departmental location on productivity, but a weak effect of productivity on the allocation of positions. Publication productivity was found to exert an insignificant effect on both the prestige of a scientist’s first academic position and on subsequent institution changes. However, this work revealed that while the correspondence between productivity and prestige of initial academic position is at first weak, the effect of departmental prestige on productivity increases over time. Moreover, for scientists who moved to other institutions, the prestige of the new department significantly affected their productivity in a positive direction.

In its time, this work emphasized the point that factors other than publication and citation, such as scientists’ graduate education, sponsorship, and postdoctoral study, played a more prominent role in initial academic appointment. Long observed that:

to the extent that the eminence of a scientist’s mentor and the prestige of his doctoral department, independently of the productivity of the scientist, are particularistic criteria for evaluation, a particularistic advantage accumulates, not an advantage initiated for universalistic reasons...Even if success in later job mobility is based more on objective criteria of productivity, the initial academic appointment, which is independent of earlier productivity, has a major impact on later productivity and hence the prestige of the second institutional location. Initial academic appointment significantly affects the chances of a scientist to become a successful researcher. Academic departments may recruit on the basis of the prestige of the mentor and the doctoral department because they have insufficient evidence of the young scientist’s productivity. But nonetheless, this initial decision to hire, based on where one studied and with whom, has a major effect on the career of the scientist (Long 1978, pp. 905–906).

Long does not state that publication productivity is unrelated to obtaining a first academic appointment. Here, the emphasis lies on other, particularistic criteria that exert a stronger effect on employment outcome. Thus, the data do not support a universalistic claim that scientists are allocated to positions on the basis of their scientific contributions. Long et al. (1979) repeated this finding in their study of entrance into academic careers.

The findings prompt speculation about how patterns might shift under different labor market conditions (i.e., whether the effect of productivity on initial academic employment is greater when academic jobs are scarcer). The findings also raise the question of how patterns change in light of further institutionalization of the postdoctoral stage of scientific careers, and how patterns vary among fields, particularly between those that evince relatively abundant productivity in early career stages (e.g., physics, chemistry) and those that are characterized by relatively less early productivity (e.g., history, classics). As Wanner et al. (1981) remarked in their comparative study of productivity in the sciences, social sciences, and humanities, “the mechanisms determining article and book count are quite different and are
systematically related to the relative roles of the two forms of output in the broad disciplinary categories examined" (1981, p. 250). Using field alone as the unit of organizational analysis, Hargens and Felmlee (1984) concluded that the distribution of recognition in a scientific field is conditioned by its rate of growth and the extent to which its members focus on recent as opposed to later work. They found that high field growth rates increase citations for senior members, which thereby increases the inequality of citation among members of a field as a whole. The citation of recent rather than later work, however, can reduce inequality by “discounting” the oldest contributions by a field’s senior members.

As a field’s growth rate declines, the career prospects of young scholars worsen. This is true not just with respect to one’s initial position, but for other forms of reward and recognition throughout one’s career...the baby boom of the late 1940s and 1950s greatly benefited scholars who had obtained positions by the middle 1960s...and the baby bust of the late 1960s and 1970s will greatly diminish the prospects of those currently entering academic positions...Large differences in scholarly recognition and rewards for equivalent performances will characterize the careers of these two cohorts of academics (Hargens and Felmlee 1984, p. 694).

We shall return to this last point, which has since emerged as a major structural issue of the academic profession, when discussing “Experience of Work” later in this chapter.

It also remains a puzzle as to what about prestige prompts productivity, a relationship also discussed by Zuckerman (1977, pp. 172–173) in terms of the “evocative environments” scientists attribute to have benefited their work. A partial answer is provided by Hermanowicz (1998), discussed later in this chapter, and lies in the ways highly productive academics, organizationally located in departments, socially control careers. Using interview data from respondents in the fields of biology, political science, and psychology, Crane (1965) found that scientists at major universities are more likely to be productive and garner recognition than scientists at minor universities. “Scientists trained and later hired by minor universities had difficulty developing continuity in their research activities and tended to be differently motivated than scientists trained and hired by major universities” (Crane 1965, p. 699). This is related to Long’s subsequent finding (Long and McGinnis 1981) that individual productivity conforms to the characteristics of the context in which a scientist works, a pattern elaborated by Braxton (1983).

It is also consistent with Allison and Long’s (1990) conclusion years later that the effect of department affiliation on productivity is more important than the effect of productivity on departmental affiliation. Despite the weak effect of preemption productivity on placement relative to ascriptive factors such as mentor, doctoral and postdoctoral origins, it is this productivity that remains the best predictor of later productivity (Long et al. 1979). In an earlier study, Clemente (1973) provided confirmatory evidence that only age at first publication and publication prior to the Ph.D. best predicts subsequent research productivity. The line of thought also underscores the great importance that the first academic position takes on: it structures the entire career, playing a prominent role in a stratification process that intensifies over time. As Reskin (1977) found in her work on chemists, early productivity most
accounts for productivity in the decade following the Ph.D., and the effect is most pronounced in strongly research-oriented universities. The stratifying effects of first employment could seemingly only grow greater in eras when academic jobs are scarce. This remains a topic for empirical work.

Reputation, Visibility, and Influence

For all the importance assigned to recognition—for scientists and for the institution of science—one may question how, in fact, professional reputations in science are made. Clearly, one answer is that scientists make a habit out of hard work (Hargens 1978; Simon 1974), devote many hours to their work (Blackburn and Lawrence 1995; Milem et al. 2000; Schuster and Finkelstein 2006), and persist in their efforts during frequent bouts with failure (Hermanowicz 2006). Using a sample of physicists, and making citation counts to analyze reputation, Cole (1970) concluded that a relatively small number of physicists produce work which is used to make subsequent discoveries in the field and, physicists do so regardless of their academic location. Physicists disproportionately use and cite the work of eminent scientists in their own work; the research contributions of most physicists are used only minimally. The patterns provide partial evidence in support of the Matthew Effect. Likewise, the paper by Cole (1970) in the same year and again using a sample of physicists found that, while top papers written by highly reputed scientists were no more likely to be widely used than top papers written by less-renowned scientists, lesser quality papers by reputed scientists did receive greater attention than papers of equal quality by less-renowned scientists. In this work, paper “quality” was determined by the number of citations to papers.

Combined with the research discussed in the prior section of this chapter, the patterns demonstrate how appointment to “top” departments facilitates productivity, increasing the likelihood of “hits” within that productivity, and thus the chances of recognition and the concomitant social testimony that one has “contributed” to the institutional goals of science. The constellation of findings also highlight the particularistic pattern wherein scientists employed outside of top departments, albeit less likely to produce as much, are also less likely to receive comparable recognition for their contributions when producing work of comparable quality.

The physicists in the research described above, however, work in fields characterized by high consensus (Braxton and Hargens 1996; Hargens 1975). Thus, one may be inclined to question how patterns of reputation, visibility, and influence possibly vary in contrasting fields. In a study of academic reputation situated in the field of sociology, a low consensus field, Clemens et al. (1995) examine how publication genre shapes careers. Unlike physics and the rest of the natural sciences, standard sociological work is produced in both article and book formats, and it is conceivable that different formats (or genres) establish grounds for differential reputation, visibility, and influence. Clemens et al. found that subject category in the discipline does not distinguish between the formats; rather, evidence and method do. Books are most often associated with the use of qualitative evidence; however,
the researchers found that the most cited books were those that used quantitative evidence.

In addition, the institutional origins of authors played a significant role in authorship patterns. Elite private universities were found to favor books, public institutions, articles. The preferences appear to be a part of socialization processes in different institutional types. With regard to the reception of work as indicated by citations, book authors at private institutions receive more citations.

The authors interpret the public/private difference in publication format in terms of scholarly networks. "Intellectual discussions at private universities typically span departments," and books more readily reach audiences outside of a given field (Clemens et al. 1995, p. 480).

At the large public universities, the cultures of production are less oriented to the university community than to the department's standing in the discipline's hierarchy (namely, the frequency with which department chairs produce 'bragging sheets' that compare departments by per capita publications in top journals). Whether the goal is attracting graduate students or protecting department budgets, publication in elite journals provides a highly visible marker of status within sociology (Clemens et al. 1995, p. 480).

Cole and Cole (1967) observed publication quality mattering more at private institutions and publication quantity mattering more at public institutions. Article publication satisfies such an organizational imperative at public institutions. Furthermore, associated with quality is the idea of "changing the direction of a field." Books may be viewed as more substantial and more compatible with this elite goal, thus constituting a different explanation for why books are favored by private universities. Such a goal is satisfied by articles in private institutions, especially in those fields where articles reflect the cognitive organization of the fields themselves, as in the natural sciences, where book publication is aberrant to the modal pattern of article publication.

The order of authorship on article (and book) publications constitutes a further mechanism in the construction of reputation (Zuckerman 1968). As collaboration in fields increases, taken by Hagstrom to be an indication of field maturation (Hagstrom 1963), visibility of individual role performance decreases. The ordering of authors' names on publication becomes, according to Zuckerman, an adaptive device which symbolizes relative contributions to the research. Working on the assumption that authors should be listed in the order of the value of their contribution, Zuckerman found, however, that Nobelists were often not listed first. She identified the practice of noblesse oblige, in which highly reputed scientists, despite their greater contribution, give credit to less eminent coresearchers, and do so increasingly as their eminence grows. Such a practice prompts the more general question of field differences in author-order norms. It may be generally assumed that, in the physical and biological sciences, authors are listed exclusive of having contributed to any of the actual writing of an article, whereas in the social sciences, the contribution of actual writing determines authorship listing.

These patterns bear on the related practice of listing doctoral or postdoctoral mentor names on mentees' publications: in the physical and biological sciences, the pattern may be parallel to that above, in which mentors' names are listed because
of any affiliation with the research, whereas in the social sciences, mentors' names may be listed only where actual writing contributions are made. These patterns, and their consequences for visibility and reputation formation, are in need of empirical treatment.

Just as different institutions appear to support different "cultures of production," according to Clemens et al., the careers of authors lend further support to the idea of differentiated scholarly worlds, at least in low consensus fields. The researchers found that almost half of the book authors and more than one-third of the article authors had never published in the other genre. The researchers discuss how their findings implicate (and complicate) disciplinary authority as well as the disciplinary legitimacy of careers.

Clemens et al.'s (1995) results also differentiate men and women authors. They found women eight times more likely to be first authors of qualitative articles in the *American Journal of Sociology*, and men six times more likely to be first authors of quantitative articles in the *American Sociological Review*. In addition, almost half of female first authors in their sample wrote books based on qualitative evidence; the modal career pattern among males was a quantitative article published in the *American Sociological Review* (see also Grant et al. 1987; Ward and Grant 1996).

In longitudinal work using samples from the fields of sociology and linguistics, Leahey et al. (2008) examined the effects of specialization on career success, as indicated by productivity and visibility. Leahey found that specialization enhances productivity but not visibility. Scholars who specialize may be visible within specific and relatively small networks of scholars, but not outside of them. The findings are consistent with a subset of those found in Clemens et al. (1995). Article authors—the types of authors examined by Leahey—are more likely specialists attempting to address more narrowly posed questions. The genre is less cited and recognizable outside of the small network to which such contributions speak. Specialization and visibility are also found to account for differences in academics' salary earnings (Leahey 2007). Women specialize less often than men, consequently publish less, and are thereby less visible. Leahey found women earn less than men as a result of these processes.

**Age and Achievement**

The relationship between age and achievement has long fascinated researchers from a variety of fields, spanning sociology, history, psychology, and higher education studies (Blackburn and Lawrence 1986; Clemente and Hendricks 1973; Simonton 1988, 1994). A related area concerned with "developmental approaches" to the study of faculty careers emerged in the field of higher education. Some studies in this area incorporate varieties of disciplinary views (Corcoran and Clark 1984; Lawrence and Blackburn 1985, 1988), others are psychologically oriented (Baldwin and Blackburn 1981; Baldwin et al. 2005; Caffarella et al. 1989).

In science, as in other institutional realms, a common belief is that individuals do their best work when they are young. It may also be believed that, in the case
of science, individuals are most productive in publication in their younger years. These notions were lent credence by the work of the psychologist Harvey Lehman (1958; see also Stephan and Levin 1992), who thought that there was a relationship between being young, creativity, and achievement. Despite the strength of these beliefs, now diffused throughout culture, empirical evidence does not support them (Bayer and Dutton 1977; Reskin 1979a). Moreover, Lehman’s work has since been found to be methodologically flawed (Cole 1979).

Instead, research has found that the relationship between age and scientific performance is modestly curvilinear; publication productivity is generally seen to increase modestly as scientists enter middle age and then decrease modestly as they advance further in age (Cole 1979). As a case in point, Zuckerman recorded that physicists were on average 36.1 years of age when they did the research that resulted in a Nobel Prize. Chemists were 38.8, biological scientists, 41.1 years of age on the average (Zuckerman 1977, p. 166).

As already made clear, organizational contexts of academic employment affect productivity; a concentration of highly productive colleagues creates an environment to stimulate high levels of research publication (Allison and Long 1990; Braxton 1983; Crane 1965; Fox 1983; Long and McGinnis 1981; Pelz and Andrews 1966; Reskin 1977). Furthermore, processes of cumulative advantage and disadvantage differentiate scientists as they age (Cole and Cole 1967, 1973). As Allison and Stewart (1974, p. 596) observed: “Because of feedback through recognition and resources, highly productive scientists maintain or increase their productivity, while scientists who produce very little produce even less later on. A major implication of cumulative advantage is that the distribution of productivity becomes increasingly unequal as a cohort of scientists ages.” This pattern was restated with slightly different emphasis by Blackburn: “Productivity does not decline with increasing age for all faculty...but only very slightly for many people. The more accurate statement is that there are productive and nonproductive people and the discrepancy between them increases over time as the productive people maintain their high level and the less productive become even less so” (1979, p. 25).

Publication patterns vary by academic field because of publication norms across fields (Bayer and Dutton 1977; Wanner et al. 1981). Publication patterns also vary by gender, to be discussed later in this chapter (Cole 1979; Cole and Zuckerman 1987; Fox 1985, 2005; National Research Council 2001; Reskin 1978; Sonnert and Holton 1995; Xie and Shauman 2003; Zuckerman and Cole 1975; Zuckerman et al. 1991). Overall, however, the evidence does not point to a strong relationship between youth and doing one’s best work.

How the reward system of science and how organizational settings of academic work influence productivity patterns have already been observed. In the case of the reward system of science, when early work is recognized, scientists are apt to continue to be productive whereas scientists whose early work is unrecognized are apt to become less productive, each respective process of advantage and disadvantage reinforced over time (Allison et al. 1982; Cole and Cole 1967, 1973; Zuckerman 1970, 1977). Zuckerman (1988) explained why it might be tempting to believe that there is a relationship between age and achievement. As so often is the case for
numerous purposes, the pantheon of science is invoked, enabling one to turn to the Newton at 24 for the invention of calculus, to the Einstein at 26 for the elaboration of relativity, to the Darwin at 29 for the theory of natural selection, and so on (Zuckerman 1988, pp. 533–534; also discussed in Merton and Zuckerman 1973 and Cole 1979). Rarely are epoch-defining events, or epoch-making individuals, generalizable to wider populations who follow in their long wakes, despite the inspiration that such events and individuals provide for those who follow. Such is true regarding the relationship between age and scientific achievement.

**Mobility**

A closely related body of work has examined faculty movement among ranks and among institutions as a means to uncover processes of stratification in science. Drawing on a sample of physicists, chemists, mathematicians, and biologists who changed institutions between 1961 and 1975, Allison and Long (1987) found that the major determinants of prestige of the destination department were: prestige of prior job, prestige of doctoral department, and the number of articles published in the 6 years prior to the move. When the job change involved promotion, the major determinants for elevation in rank included: origin rank, professional age, and citation frequency. (The authors are not able to provide support for differential patterns among the four fields studied.) Recall that Long et al. found in their 1979 research that publication productivity does not affect job placement, but that job placement does affect productivity. In light of the 1987 findings, this conclusion is thus amended when applied to job changes after the first academic job. The discrepant results seemed to indicate that hiring departments, at least in the historical context in which the researchers completed their work, paid more attention to research productivity when candidates are mature scholars with lengthier publication records than at the time of obtaining the first job.

Reskin and others have inquired into the effects of mentor characteristics on scientists’ careers. Using a sample of chemists, Reskin (1979b) found that an academic sponsor’s productivity affected predoctoral productivity in “offsprings,” and that the caliber of the Ph.D. department affected offsprings’ postdoctoral productivity. Integrated with the results above, these factors would seemingly play a prominent role in professional mobility after entry, if not also at the time of entry into an academic career. As Reskin observed: “sponsorship is vital for scientists’ careers. Both their [Ph.D.] departments and sponsors affect graduate students’ later performance as well as how they fare professionally” (1979b, p. 143). The net findings from these studies are logically consistent with Hargens and Hagstrom’s (1967) early article on the topic, where they found that prestige of doctoral department is closely associated with the prestige of the first employing department, even when the effects of productivity are controlled.

In a subsequent study, Long et al. (1993) researched rank advancement using a sample of biochemists who earned their Ph.D.s between 1956 and 1958 and all females who received Ph.D.s in biochemistry from 1956 to 1967. The authors found
that time in rank and number of publications in rank were the most important factors in determining rates of promotion. "There is little evidence that the quality of research, as indicated by citations to the articles or the standing of the journals in which the articles are published, affects promotion" (Long et al. 1993, p. 719). This finding, independent as it is from data on the departmental contexts of publication productivity, departs from Cole and Cole's (1973) earlier claim that publication quality matters more for success in prestigious departments whereas publication quantity matters more in less prestigious departments.

Long et al. also revealed that rates of promotion are lower for women. Moreover, employment in a prestigious department has a significantly more negative effect on promotion for women than for men. At both promotion junctures—from assistant to associate and from associate to full professor—women are promoted more slowly. Returning to the norm of universalism as their point of departure, the authors conclude that particularistic factors operate in processes of rank advancement in academe. If contributions to scientific knowledge were the chief criterion in promotion, the authors argue, then effects of citations to published work (for women and for men) should be stronger. What is more, while women were found to benefit more from each of their publications, this advantage only materializes into tangible results for the most published women scientists. The precise sources of gender differences in science have remained a puzzle on which researchers have worked extensively, producing a large body of work on the subject of gender, productivity, and careers (see the next section of this chapter).

In work that bears as much on mobility as on socialization, to be discussed later, Fox and Stephan (2001) considered the discrepancies between employment preferences and realities among 3,800 doctoral students in the fields of chemistry, computer science, electrical engineering, microbiology, and physics. In all fields, they found, students reported that their prospects for careers in research universities were less than good (and for men and women in physics and for women in chemistry, the prospects were rated as less than fair). In physics, nearly half of students preferred careers in research universities; frustration among recent graduates may stem from the discrepancy between reality and a desired future. The fraction of those students in microbiology who prefer careers in research universities is similar to that of physics; however, students are less pessimistic. Fox and Stephan attribute this difference to comparatively greater postdoctoral options open to microbiologists. A related issue is funding found in fields; depressed periods of funding in certain fields, such as physics (and in contrast to microbiology), may contribute to depressed sentiments about one's professional future.

Fox and Stephan (2001) also observed women reporting a greater preference than men for careers in teaching universities, which may reflect women's expectations that this is the option open to them. This channeling process of gender among the sectors of academic employment proves to constitute a major dimension that accounts for cumulative disparities between gender and publication productivity, a topic of the next section of this chapter.

The study of stratification in academic careers may be considered via five areas: (1) cumulative advantage and disadvantage; (2) the organizational bases of strati-
fication; (3) reputation, visibility, and influence; (4) age and achievement, and (5) mobility. These represent key domains in which scientific and academic careers are differentiated: by time (in the instances of cumulative advantage and disadvantage and of age); by place (in the instance of organizational bases—the departments, institutions, and fields in which academics work that constrain and shape careers); by medium (in the instance of reputation, visibility, and influence, and how these are constructed by quality and quantity of publication, publication type, and the citation of published work); and by the intersection of these dimensions (in the instance of mobility, patterns of which account for research productivity via the influence of timing of moves and lengths of accumulated publication records, the types of departments and institutions that foster, “send off” and “receive” individuals with these records, and how the records themselves are socially established by processes of citation and reputation formation).

Gender, Productivity, and Careers

No other area in the sociological treatment of scientific careers has received more attention than that of gender. Even in the larger context of stratification research on science, the area of gender has remained the most active. In their review of the subject in 1984, Cole and Zuckerman identified some 50 studies that sought to address “the productivity puzzle,” that is, why women scientists consistently publish less than their male counterparts. The volume of literature since their writing has only increased, with several book-length treatments having appeared (Eisenhart and Finkel 1998; National Research Council 2001; Sonnert and Holton 1995; Xie and Shauman 2003; Zuckerman et al. 1991; also Cole 1979) to complement the output of articles on gender differences in science careers. Again, the conceptual underpinning of the interest lies, as in the work above, in social stratification, and again, the interest originates (if not always stated explicitly) in ways the universalistic norm of science may fail—here, for how men’s and women’s scientific work is differently evaluated, for how their contributions are differently recognized and rewarded, for differences in structural barriers in their careers, and for how men’s and women’s careers may respond differently to feedback mechanisms that alternatively promote or inhibit productivity. While the topic of gender, productivity, and careers is sufficiently distinct to be partitioned as its own section in this chapter, the topic has grown so large that only reviews devoted to it entirely can grant the subject its justice (Cole and Zuckerman 1984; Fox 1985, 1995, 2008; Long and Fox 1995; Ward and Grant 1996; Zuckerman 1991; Zuckerman and Cole 1975). In light of our present aims, the treatment here is necessarily selective and understood as a means to illustrate central themes, including: the “productivity puzzle”; marriage, parenthood, and productivity; and social-organizational conditions of work environments.

Attempting to address the “productivity puzzle” and to settle some of its quandaries, Xie and Shauman (1998) develop a sample of doctoral scientists spanning a time period between 1969 and 1993 with regular faculty appointments across the
fields of the biological, physical, and social sciences, engineering, and mathematics. Whereas prior studies demonstrated that women published slightly more than half as many papers as men (Cole and Zuckerman 1984; Zuckerman 1991), Xie and Shauman’s findings demonstrate a significant narrowing of this gap, such that by the late 1980s and early 1990s, the gender ratio in productivity stood between 75 and 80%. The authors reason that the distribution of positions and resources, while still unfavorable to women, became more equitable in the last three decades of the twentieth century. It is important to note that this gender ratio encompasses overall sex differences; different fields may exhibit different gender productivity ratios.

Their data indicate that women are more likely than men to hold appointments in teaching colleges and are less likely employed at research universities. Across the time period of the researchers’ study, this pattern in institutional affiliation persisted but began to converge. Independent of institutional type, gender differences in teaching load significantly narrowed, as did differences in research funding, across the span of time studied. Resource conditions that affect productivity, such as teaching hours, research funding, and research assistance became more equally distributed between men and women (Xie and Shauman 1998, p. 859).

Some contradictory evidence is on hand about the effects of marriage, parenthood, and productivity. Hargens et al. (1978) report a negative relationship between childbearing and productivity. Hunter and Leahey (2010) found that productivity among women linguists and sociologists decline after the birth of a child. Other studies have found either a neutral or a modestly positive relationship (Cole and Zuckerman 1987; Sonnert and Holton 1995). Cole and Zuckerman (1987), using a sample from the fields of mathematics, the physical and biological sciences, economics, and psychology, found that women who marry and have children publish on average as many papers per year as single women, though the authors caution that the result should not be equated with a conclusion that marriage and children have no effect on the careers of women scientists. Indeed they do, but the effect is generally not observed in women’s research productivity. Throughout this body of research, it is abundantly clear that productive women scientists possess organizational qualities and organizational support that creates a ground for their work (Ward and Wolf-Wendel 2004). They are seen to eliminate from their lives “almost everything but work and family” (Cole and Zuckerman 1987, p. 125).

Xie and Shauman (1998) found married scientists to have significantly higher rates of productivity than unmarried scientists. Marriage may benefit productivity through the addition of economic resources and emotional support from a spouse. What is more, a spouse may provide domestic support whose net effect includes additional time for a scientist to work. Women scientists are, however, less likely than men scientists to be married (Marwell et al. 1979; Shauman and Xie 1996). Consequently, women scientists are on average less likely to benefit from marriage, but when they are married, are found to benefit equally with men (Xie and Shauman 1998, p. 860). Xie and Shauman (1998) argue that, when controlling for marriage, estimated gender differences in publication productivity are further reduced.
What can be made compositely of the patterns? Xie and Shauman (1998) argue that women scientists publish fewer papers than men because women are less likely to have the personal characteristics (as indicated by field, time lag between bachelor's and doctoral degrees, and years of experience beyond the doctoral degree), structural positions (indicated by university type), and facilitating resources (indicated by teaching loads, research grants and assistance) that foster publication. That is, men and women scientists often pursue (or are characterized by) different career tracks. Preponderantly, women and men scientists are located in different academic structures that entail varying access to the types of institutional values and resources that facilitate publication productivity. Personal values and career ambitions, the authors argue, may vary systematically, owing to gender-typed socialization (Xie and Shauman 1998, p. 864) and to significant gender differences attendant on child-bearing (Shauman and Xie 1996), creating further ground for the observed variation in outcomes.

Xie and Shauman's research plows substantial new ground on the long-vexing productivity puzzle; it also prompts new questions. For example, does its use of synthetic cohorts across four different time periods mask differences between men and women that otherwise would be observed by following the same scientists over time in their careers? Longitudinal data, nestled in organizational contexts, could parse the answer to the question in order to see how, and to what degree, demographic and contextual conditions differentiate careers.

Recent work has addressed some of these concerns. With regard to select demographic characteristics, Fox (2005) has found that, for women at least, the relationship between marriage and productivity varies by type of marriage and the occupation of a spouse. Women in subsequent (as opposed to first) marriages exhibited higher productivity. In subsequent marriages, women scientists are more likely to be married to another scientist, which Fox found to enhance productivity. Subsequent marriages may also be more stable and, by virtue of time, are likely correlated with a more mature research infrastructure within which one works, enhancing productivity.

With regard to select contextual conditions, Fox (2007, 2010) in other research has identified key social-organizational features of work environments that differentiate men's and women's careers in science and engineering fields: frequency of speaking with faculty about research in one's home unit; ratings of position and department; characterizations of departmental climates; and levels of interference experienced between work and family. Fox found that women are less likely than men to speak daily about research and more likely than men to speak of research less than weekly. The difference may be explained by lower integration or sense of membership among women in departments.

In addition, women rate their positions and their departments lower than men on aspects of human benefits and material resources necessary to work success (Fox 2010). These include such matters as access to equipment, a sense of inclusion among faculty in a department, and recognition from faculty for accomplishment. Correspondingly, women, more than men, characterize their home units as operating in ways other than consistently, neutrally, or "universalistically." Finally,
both men and women experience interference between work and family, but women more so, perhaps reflecting gendered expectations for women in households and families.

The findings are related to a theoretic orientation advanced by Sonnert and Holton (1995)—identified as the “deficit” and “difference” models—to explain gender differences in science careers. Building on the theoretical sociologist Georg Simmel’s formulation of “the stranger,” the deficit model holds that women are treated as strangers in science, whereas the difference model holds that women act as strangers in science. By the deficit model, women receive fewer chances and opportunities in their careers. The model postulates that women scientists’ goals are like those of men scientists, however, structural barriers keep women from accomplishing these goals. Much of the research on gender stratification in science, including that which explicitly seeks to partition universalistic and particularistic patterns, is consonant with the deficit model.

By the difference model, women possess “ingrained” contrasts in behavior, outlook, and goals compared to men. Differences are accounted for by gender-role socialization and cultural patterns, which may prompt varying professional orientations, ambitions, and attitudes between (and within) the genders (Sonnert and Holton 1995, pp. 10–17).

On this latter point, some gender differences, the researchers found, lie in stimulus-response behavior. For instance, more men than women interviewees in the study considered themselves self-confident. When questioned about whether they should have handled career obstacles in a different way, many more women than men claimed they should have been more confident or more assertive (Sonnert and Holton 1995, p. 139). The researchers also found women scientists, more so than men, more likely to have entered their careers “gingerly,” that is, by taking a step-by-step approach rather than by pursuing clear career goals at the outset. Three times as many women as men said they had unclear career aspirations when starting out in science (Sonnert and Holton 1995, p. 139). Cole and Fiorentine (1991) cautioned about the confusion between cause and effect in discrimination toward women in science. These results suggest patterned ways in which disadvantage may accumulate for women, both as gender-socialized agents in their careers and as subjects occupying specific structural positions.

Issues of self-confidence and assertiveness provide partial evidence for the former point (i.e., women as gender-socialized agents in their careers). Evidence for the latter point—women occupying specific structural positions—comes by way of the coping mechanisms that Sonnert and Holton found women scientists to employ in actively dealing with discrimination. They identified five chief strategies: ignoring, humor, compliance, deemphasizing gender, and avoidance (Sonnert and Holton 1995, pp. 130–131). Long and Fox (1995, pp. 62–63) postulated four conditions associated with the use of particularistic criteria on the basis of gender (and race) in academic science: the absence of information—when little information about the qualifications of scientists in on hand, particularistic factors are more likely to come into play; ambiguity of standards—particularism is more apt to be found in
those instances when criteria for evaluation are unclear; less developed scientific paradigms—particularism is apt to be correlated with low consensus fields characterized by a comparative lack of agreement on research problems, theories, and methods; and secrecy—particularism is likely to be more evident on those occasions when processes are not open and/or transparent, as in hiring, promotion, and the distribution of rewards.

Sonnert and Holton’s research underscores the importance of the idea of the “critical mass.” By this view, many differences in career outcomes are attributable to a low density of women in science, which varies by field (Fox 1995). Sonnert and Holton suggest that a comparative lack of women scientists leads them to adopt a distinctive “scientific style”: women frequently embrace a more meticulous and perfectionist approach to research and tend to more strenuously uphold traditional standards of science, such as carefulness, replicability, and connection to fundamental ideas. Women may embrace this style because the perception of a marginal status compels them to adopt extra-high standards of conformity in order to be viewed as legitimate members of the scientific community. Using Simmel’s formulations, the authors reason that women act in these ways because they are “strangers to science” and strive to be members of science. Here, the theory of cumulative disadvantage comes into play. Initially, small differences are accentuated in later career stages, resulting in disparities of performance (Sonnert and Holton 1995).

This temporal interpretation of gender differences in science careers is distinct from that promulgated by Xie and Shauman (1998) described above, which accounts for differences (and then only with regard to productivity), by women’s personal characteristics, structural positions, and attending resources. These three sets of conditions are themselves subject to accumulating or nonaccumulating returns; data on real, rather than synthetic cohorts, will allow future researchers to further examine the effects of gender on academic careers over time.

Gender constitutes the largest area of research on stratification in scientific careers. The preceding discussion has highlighted three central themes in this body of research: the “productivity puzzle” between men and women scientists; marriage, parenthood, and productivity; and social-organizational conditions of work environments. Since women and men are represented differently from field to field, despite various changes in the gender make-up of fields, it has become even more important to examine differences by field, which will reflect empirical realities more precisely than global assertions. In conjunction with the prior section on stratification and careers, research will likely profit from still greater in-roads made into how reward systems operate with and/or without regard to the use of particularistic criteria, including gender. The profit will likely be greatest (though not exclusive) at the level of department, not only because local work environments demonstrably condition the structure and experience of academic careers, but also because the outcomes of one’s situated work “feed” or, conversely, “starve” the operation of reward systems at institutional and professional levels.
Social Control of Careers

The social control of careers involves the ways by which careers are normatively ordered. Literature on the social control of careers in science originates not only from the sociology of science but also, in degrees, from the sociologies of work, occupations, and professions. Three main topics in the literature orient a consideration of how careers are socially controlled: recruitment and socialization; deviance; and the experience of work.

Recruitment and Socialization

Socialization refers to the transmission and internalization of norms such that they become self-imposed rather than exclusively managed by external regulation. Knowledge acquisition, intellectual and material investment, and role involvement may be understood as core elements of socializing processes (Antony 2002). While the majority of scholarship on professional socialization in higher education has concentrated on the transition from student-apprentice to scholar, inquiry need not be confined to this stage in the professional life course. Age norms socially control the timing, duration, and experience of the key events throughout the life cycle. While the change from lay person to professional may be viewed as particularly momentous, the occupational stakes in subsequent status transitions remain consequential to the very elements identified above that guide a socialization process—knowledge acquisition, intellectual and material investment, and role involvement (Hermanowicz 1998, 2009; Neumann 2005, 2009). Ways by which faculty members adapt over time in their productivity patterns, role sets, and professional identities are learned; socialization processes are as much a feature of ending careers in retirement as in beginning careers in graduate or professional school. Clearly, though, socialization researchers have been more caught up with early periods of professional socialization. Still other work has approached the subject in terms of a pipeline metaphor, investigating the sequences of events that result or fails to result in the production of the professoriate. Here, too, the focus lies in early rather than subsequent career matters of selection and socialization.

Cole and Barber (2003) follow this last approach in their examination of selection processes toward and away from academic careers among high-achieving minority students. The study is based on 7,612 graduating seniors at 34 colleges and universities across the United States. Occupational choice is treated as the dependent variable subject to the influence of various institutional characteristics, aspects of students’ experience of school and schooling, and input variables such as SAT scores, GPA, gender, and socioeconomic status. The authors contend that it is logical to examine academia as a career choice by studying the choices of high-achieving students, since high-achieving students may be most likely drawn to, and be encouraged to consider, an academic career.
The authors focused on three institutional variables: selectivity, student-body racial composition, and institutional orientation (i.e., research versus teaching). To test hypotheses using these variables, four main institutional types were included in the study design: Ivy League institutions (highly selective, predominantly white, and predominantly research-oriented); liberal arts colleges (generally highly selective; predominantly white, and predominantly teaching-oriented); state universities (generally less selective, predominantly white, and predominantly research-oriented); and historically black colleges and universities (included were HBCU's of varying selectivity and research orientation, all, of course, predominantly African American).

Drawing on Davis' (1966) classic article, "The Campus as a Frog Pond," Cole and Barber advance a theory of relative deprivation to explain several findings. The theory is based on the premise that satisfaction with a particular life condition is based less on objective conditions than on a comparison with those nearest.

Consider students at an elite school who receive a GPA of B-. These students are in the lower quarter of their class. When they compare themselves with classmates who have done better academically they are likely to conclude that academically they are average or below average... With such relatively low levels of self-confidence, these students might be less likely to pursue high-achieving occupations such as academia. If the same students had attended less selective schools they probably would have received higher GPA's since there would have been less academic competition at these schools, and they may have concluded that academically they were average or above average. Even if they had received the same GPA at a nonelite school, a GPA of B- has very different consequences for a student attending a school where almost everyone has a higher GPA than it does for a student attending a school where this is the average or above average GPA (Cole and Barber 2003, p. 201).

The researchers observe that affirmative action policies have steered many minority undergraduates to selective colleges where they do poorly, and further limit the "pipeline" into academe. What is more, Cole and Barber find that liberal arts colleges tend not to produce students with aspirations for high-achieving occupations, perhaps surprising given the low faculty-student ratios in such environments where many believe success is more readily facilitated. The research finds that role models do not exert a significant influence on decisions to become a professor. Students with same-gender or same-race role models were found no more likely to want to become a professor than those who had no such role model (Cole and Barber 2003, p. 185). Cole and Barber conclude their analysis with a set of important findings and specific recommendations, including greater student exposure to research experiences, steering of students to institutions that are not necessarily the most prestigious but ones where they are more likely to do well, and the development of institutional programs and career counselors dedicated to stimulating minority student interest in an academic career (Cole and Barber 2003, pp. 236–258; for related treatments, see Becker and Price 2009; Castillo-Chavez and Castillo-Garsow 2009; Leggon and Pearson 2009; Tapia and Johnson 2009).

By 2000, women accounted for almost half of doctorates across academic fields. However, their low representation has persisted in areas of engineering, physics, and mathematics. Throughout the sciences, career persistence and mobility after the
doctorate are considerably lower for women than men. Xie and Shauman (2003) examined the trajectories of women into science careers. Using demographic methods they apply a life course perspective to 17 longitudinal data sets in order to understand the interdependence and consequences of exit and entry across the domains of education, family, and work.

Xie and Shauman observe minimal gender differences in mathematical ability except at the upper extreme of the distribution. Male students, however, participate more in science curricula, and by the time students are high school seniors, many more male than female students have developed expectations to major in science or engineering fields in college. In college, women are more likely than men to major in some of the science fields, but these majors are less likely than other science majors to pursue scientific careers. Subsequently, married women, and married women with children, are less likely to pursue a career in science or engineering. The patterns are suggestive of the ways in which gender segregation by college major and subsequent familial roles differentiate ensuing career sequencing and attainment.

Later in the career, Xie and Shauman found that while female participation in science and engineering has increased over time, a gender disadvantage continues in employment and positional status. Recalling earlier theorizing, the disadvantage accumulates: it negatively affects geographic mobility. Affirming the authors’ prior work (Xie and Shauman 1998), they find again (Xie and Shauman 2003) that productivity differences between men and women are best explained by personal characteristics, structural positions, and facilitating resources.

The approaches above either adopt a “pipeline” approach to selection, recruitment, and socialization to the professoriate or, as in the case of Xie and Shauman (2003), adopt a perspective that functions as a critical response to the use of the metaphor. By contrast, other work focuses on a stage, and most often graduate education, to examine socialization issues. Austin’s work (2002; also Wulf and Austin 2004) exemplifies this approach.

Austin’s studies of graduate students and criticisms of their training yielded five recommendations: more attention to regular mentoring, advising, and feedback; structured opportunities to observe, meet, and talk with peers; diverse, developmentally oriented teaching opportunities; information and guidance about the full array of faculty responsibilities; and regular, guided reflection on the nature and content of faculty roles (Austin 2002, pp. 111–112). Most of these recommendations might seem obvious, but are hardly so when placed against data that indeed suggest how infrequently departments, programs, and faculty members engage student clientele in these activities (Ehrenberg et al. 2009). Concern for the efficacy in doctoral and professional training led the Carnegie Foundation for the Advancement of Teaching, among others, to complete intensive studies of training and socialization in several fields (Foster et al. 2006; Golde and Walker 2006; Sheppard et al. 2008; Walker et al. 2008).

A minority of research has examined aspects of socialization processes beyond the entry points of careers. Fox and Ferri (1992) examined the attributions that academics make for successful people in their fields. Using a national survey of academics across economics, political science, psychology, and sociology, the re-
searchers found that women make weaker internal (or individual) attributions but significantly stronger external (or structural) attributions than do men. Two possible explanations are offered to account for the differences. First, women are more aware of external barriers than are men. Second, the noncomparability of structural locations between men and women academics (a pattern subsequently confirmed and examined by Xie and Shauman [1998] as discussed above in the section on gender, productivity, and careers) evinces diverging sentiment about the sources underlying academics’ career success. The findings are suggestive of the way that academics, well into their careers, are conditioned to understand a system of stratification in which they are integral parts.

Concentrating on the field of biology, Sonnert (1995) inquired about the factors that led reviewers to make positive assessments of another scientist’s career. Among a variety of productivity and career measures, he found three factors to account for the majority of variance in reviewers’ evaluations: annual publication productivity, the existence of sole-authored publications, and graduate school prestige. The results suggest that academics are regarded as less successful when they publish a comparatively less amount of excellent work, do so (at least in part) with coauthors, and hail from a nonelite doctoral program. Success attributions are more readily assigned to those who are more prolific, independent of the quality of output, and even if variability in production quality cooccurs with prestigious doctoral origins. These patterns, too, reveal ways in which academic communities condition behavior and attitudes about performance in a scholarly career.

Using a sample of academic physicists, Hermanowicz (2006) asked: what does it take to be successful? The responses cast light on the moral order of physics by eliciting how members of academic construe the structure of success. Following Durkheim ([1915] 1965), a moral order refers to a group’s conception of how a life (or career) ought to be lived. Physicists identified a finite set of 12 qualities that they deemed necessary for success. In rank order of mention, these qualities included: persistent, smart, civil, creative, entrepreneurial, aggressive, tasteful, confident, adaptable, ability to communicate, service-oriented, and lucky. Over half the physicists concentrated on persistence as that attribute most responsible for success, whereas only a quarter of them identified the next most commonly attributed quality, that of being smart. Moreover, senior scientists were more likely to discuss persistence than junior scientists, suggesting that the importance of the attribute is learned via experience and indeed through brushes with failure. Gans and Shepard (1994; also Shepard 1995), who in studying the world’s leading economists, observed how frequently their articles—which went on to become classics, were routinely rejected in the early cycles of the reviewing process. “In the big leagues,” they wrote, “even the best hitters regularly strike out” (Gans and Shepard 1994, p. 166). Persistence allowed them to hit their home runs.

A concern for socialization has often prompted the question, “Socialization for what?” Bidwell (1972) addressed the question by answering: “For moral commitment.” Socialization processes and their efficacy immediately draw our attention to ideals of career success constituted by exemplary role performance. Few of those who embark on an academic career, particularly once beyond graduate or profes-
sional socialization, likely do so with low aspirations. Rather, ambition imbues the culture of professions (Bledstein 1976). It was with this backdrop that Westie (1972, 1973; Westie and Kick 1980) conducted his studies of academics’ expectations for professional immortality. Using a sample of sociologists, Westie examined academics’ aspirations for significant contributions and lasting legacy beyond their career spans. Almost half of his respondents entertained the desire to be included among the top ten leaders in at least one of their specialties, and more than half believed that their contributions in the form of research and publication would survive their career spans. Yet when presented with the task of identifying “luminaries” and their work on a list (which turned about to be the names of past presidents of the American Sociological Association), most of the people had been forgotten. Expectations for professional survival after one’s career are unrealistically high when compared with the fate academics accord many distinguished scholars.

As academic people we compete for rank, salary, recognition, as we are required to do. The system forces us to exaggerate our accomplishments and others’ definitions of our accomplishments. Because exaggeration is near-normative, we do it without knowing we are doing it, and we come to believe our exaggerations, again without knowing it...this academic normative order, which includes definitions of how one ought to evaluate one’s self as originator and lasting contributor, may be understood as part of the academic culture of legitimation, whereby the peculiar perquisites of academic people are perpetuated (Westie 1973, p. 32, original emphasis).

Rules of legitimacy are transmitted by socialization, both in doctoral training and throughout subsequent phases of the professional life course. On the one hand, these specific findings illustrate the prominence of exaggeration in the normative order of how academics make status claims. On the other, they underscore the importance to which academics assign recognition, when as socialized members of the academic profession, they seek, in Merton’s terms, to satisfy the institutional goals of science. To this end, Westie conveys something of the chances with which even once-prominent academics will be remembered for their work.

Deviance

The norms of science, discussed at the beginning of this chapter, prescribe behavior in how academic roles are to be performed and, conversely, call attention to the episodes in which scientists deviate from them. Zuckerman explains that the two most serious violations of the norms are fraud and plagiarism, which involve deliberately deceptive truth claims and deliberately deceptive claims to ownership of intellectual property (Zuckerman 1988, p. 521). These, however, are by no means the only ways in which scientists deviate from scholarly norms, though they may be viewed as the most serious and consequential because they interfere fundamentally with the development of knowledge, and thus with the foremost institutional goal of science (LaFollette 1992; Zuckerman 1988, p. 522).

results from the premium that the scientific community places on originality and recognition. By anomie theory, a discrepancy arises between desired goals and the means to satisfy them. Scientists unable to bring legitimate means to satisfy this institutional end are among those most prone to resort to deviance in order to gain desired rewards (Braxton 1993).

On the one hand, deviance from scientific norms is not surprising in the sense that, as in all norms, violations will invariably occur and, following Durkheim, must occur, in order to serve—with subsequent punishment—as reminders to a community of the rules to which members must adhere. In this regard, deviance is never eradicated; to have a collectively established idea of normative behavior, it is necessary also to have collectively recognized deviations from it, and incidents to make real distinctions in behavior.

On the other hand, it is not clear how much deviance exists. The actual frequency of deviance is not known, and no method has been developed to gauge the ratio of public cases with those gone undetected (Zuckerman 1988, p. 523). In the absence of systematic data on the subject, scholarly inquiry into deviance in academic roles is, while important, generally scant, and virtually no sociological treatments have been made. Studies that exist tend to be hypothetical in nature, considering either behavior that is plausibly regarded as deviant, or work conditions that plausibly give rise to its occurrence. The volume by Braxton (1999), which covers historical overviews, theoretical perspectives of deviance, and targeted empirical analyses, provides the best starting point for further inquiry.

Experience of Work

A final body of work focuses on how scientists experience the academic career and, in turn, how normative conceptions of work socially control subjective interpretations and passage through time. A number of topics discussed above are subsumed under this focus, including socialization, organizational bases of stratification and cumulative advantage, and age and achievement. However, as the term “experience” suggests, the emphasis in this latter work tends to be more social-psychological and biographically oriented as opposed to exclusively structural, as in much of the work discussed under the mentioned headings. What is more, some of these treatments in the experience of work, like the topic of socialization, are informed by a sociology of professions and occupations in addition to the sociology of science.

Hermanowicz’s studies (1998, 2003, 2005, 2007a, 2009) are part of a tradition of work in the field of sociology that dates to the 1940s and originates from what was called the “Chicago School of Sociology,” which studies how individuals are socially shaped by their interaction with institutions, including occupations (see Bulmer 1984; Hughes 1958, 1971, 1994). The Hermanowicz studies are also informed by the long legacy of work and protégés produced by Robert Merton in the sociology of science.

The concept of the subjective career situates the analyses. In one of Everett Hughes’ most notable formulations, careers are seen and studied for their “two
sides” (Hughes 1937; later developed by Goffman 1961; see also Hughes’ posthumous publication 1997). One side is the objective career, which consists of the sequence of statuses a person holds over time. The statuses may be indicated by positions or offices or titles, such as freshmen, sophomores, juniors, and seniors composing an educational career or assistant, associate, and full professor composing an academic career. The second side, existing in tandem, is the subjective career, which consists of the shifting personal perspectives individuals develop about themselves and their work as the objective career unfolds (Stebbins 1970).

Hughes marshaled the idea of turning points as a social mechanism that explains when and how change occurs in the subjective career as it engages in dialogue with the objective side (Hughes 1958). As lives and careers transpire, people undergo a series of changes, not only in their objective status, but also in the patterned subjective views they hold about themselves in light of this change. The young assistant professor comes to see him or herself in a substantially new and different light from that understood as a student undergoing intense training and socialization for the professorial role, just as the emeritus professor—at the other end of a long sequence in professional status change—comes to see him or herself differently than viewed through the lens of a once regular member of a senior faculty.

In The Stars Are Not Enough: Scientists—Their Passions and Professions, Hermanowicz (1998) sampled and interviewed physicists according to early, middle, and late career stages and by one of three types of university in which they were employed. The universities, which form a representative continuum, consist of those stressing research in the presence of teaching and other roles, termed elite; institutions that stress research and teaching as well as other roles, termed pluralist; and institutions that stress teaching in the presence of research and other roles, termed communitarian.

The same physicists were again interviewed in 2004–2005, creating a longitudinal design from which to study how academics, working in a variety of institutions, age in relation to their work (Hermanowicz 2009). The sequel study, Lives in Science: How Institutions Affect Academic Careers, thus allows one to see how academics’ perceptions of work evolve with felt costs and rewards, from early to mid career, from mid to late career, and from late to post career. Age and institutional location provide the structure to analyze individual, subjective careers through diachronic change, and the work is consequently in a position to address the following questions about scientific careers:

- How do scientists account for the unfolding of their careers in light of the goals and aspirations that socially situate their profession?
- What continuities and changes—in aspiration, satisfaction, motivation, commitment, and identification with work—mark the careers of scientists?
- What knowledge have scientists acquired about themselves, their institutions, and the academic profession in 10 years?
- How does this knowledge vary by individual age and type of university?

Hermanowicz (2009) generalizes about these careers by way of 20 analytic dimensions, ranging from overall modal career patterns to overall satisfaction and to work
attitudes. For example, with respect to overall modal career patterns, Hermanowicz (2009) found that in passing from early to mid career, elites stabilized and rededicated themselves to academe—to fulfilling the institutional goals of higher education by continuing in their research productivity. By contrast, pluralists experienced reversals. They questioned their interest and commitment to the profession and grew disillusioned with academic research. By mid career, most communitarians ceased in research. For communitarians, cumulative disadvantages accrued to the point of shutting down interest and motivation to continue in scientific research. Their career pattern may best be described as succumbing to a stasis—there was no forward progress.

In their mid to late career transitions, elites remained consistent in their identification with science and in their scientific productivity. Their publication productivity continued to accelerate. Pluralists either attempted to regenerate themselves following earlier fallow periods, or continued in the research that they had been doing. Communitarians entered into a demise; they decreasingly identified with research and became increasingly disaffected with their departments and universities, which they saw as having crippled their research aspirations. In moving from late to post career phases, elites for the first time lessened their intensity and embrace of research. Pluralists characteristically withdrew from work. Communitarians separated themselves completely from it, usually severing all ties with work and their employing organizations.

Patterns in modal careers are in turn associated with patterns in satisfaction and in attitudes about work. Among elites, satisfaction begins high and rises through the career. It then drops at the end. Among pluralists, satisfaction starts out on a high, drops and levels off. Finally, it rises at the end, coinciding with a time at which they withdraw from work. Among communitarians, there is a low in satisfaction throughout their careers, until the end. At the end of their careers, for the first time, communitarians experience the greatest high. Coincidentally, it is a time at which they are separating themselves altogether from work. Satisfaction operates both as a function of opportunity to undertake desired work, particularly research work, and as a function of the recognition garnered for work performed.

Elites possessed positive attitudes toward their work throughout most of their careers. Only in the end do their attitudes turn ambivalent—about what they have done, how much they have achieved, and where they stand professionally. Unlike any previous period in their careers, there is a sense of regret and resignation about their efforts and what they have achieved. Pluralists are, by turn, positive. Communitarians feel detached from work and institution. Their attitudes are far from the negative ones that were most common among them at earlier points in their careers.

Would scientists pursue an academic career again? Many would not. The notable trend is not that many would, as is also the case: one might anticipate that long training and preparation for a profession would coincide with commitment and satisfaction, indicated by a strong desire to pursue the same profession were people given the chance to start over. By contrast, what is noteworthy is the large fraction of faculty members who say they would pursue another line of work, an indication of
a profession’s possible lack of vitality, conditioned by the circumstances that faculty members confront in their institutional environments.

The picture that emerges is far from sanguine. In what direction do the patterns seem to be headed? Hermanowicz argues that increased emphases on research will likely be accompanied by increased probabilities of dissatisfaction throughout the system of higher education. As research is more greatly stressed, by institutions as by individuals, career expectations rise, in accord with attempting to satisfy external reference groups that are consistent with fulfilling the institutional goals of academe. As expectations rise, the likelihood of satisfying them decreases, because the expectations are defined by that not yet achieved and, ultimately, by the unachievable. Once again the idea of anomie is invoked. The relevance of anomie is readily found in the work conditions of contemporary academic careers, even as the origins of the idea trace to the social theorist Emile Durkheim ([1915] 1965, [1897] 1951), then subsequently pursued by Merton ([1957] 1968a, [1957] b), explored in later empirical studies of scientists (Braxton 1993; Hagstrom 1964, 1965, 1974; Hargens and Kelly-Wilson 1994), and suggested in others by virtue of work conditions outlined (Becher and Trowler 1989, esp. Chaps. 5 and 6; Zuckerman 1992). Many conditions of work appear to generate dissatisfaction and disaffection for academic careers.

Hermanowicz’s findings suggest that the present conditions of academe favor a decline in the attractiveness of academic careers. On many objective criteria, chances of success in academia across many fields are low and, where won, are hard-fought: obtaining regular employment, obtaining tenure, obtaining promotion through standard ranks, publication, citation of work, competitive salary, and competitive salary growth. These basic rewards are also arguably more difficult to obtain across institutional types than in any other historical time in the profession.

At stake on the one hand are individual satisfaction and moral commitment. These are significant stakes. When compromised, the institutional goals of science fail to be served. On the other hand, the overall welfare and functioning of the profession are at stake. Hermanowicz’s findings prompt the question of what types of people, with what levels of talent, the academic profession will be able to attract—a question centrally situated in the studies of recruitment and socialization discussed earlier in this chapter.

The social control of careers refers to the ways in which occupational groups—here, scientists and academics—regulate professional passage through time with socially enforced norms. Three principal areas situate work on the social control of careers: recruitment and socialization; deviance; and the experience of work. A major idea underlying social control involves the task of managing heterogeneity such that actors of numerous types, stripes, and persuasions conform, more or less, to group principles. Researchers can assess the extent to which this is actually the case. More significantly, researchers may draw new attention to the consequences of heightened heterogeneity. More people, from more diverse backgrounds, earn doctoral degrees and enter academic careers, having been trained and socialized at institutions and in programs that themselves speak of heterogeneity.
The challenge that heterogeneity poses to the social control of careers is not likely trivial. It may well condition how individuals are recruited and trained, their proclivity toward or desistance from deviance, and how they experience their work. In other words, we are drawn to additional dimensions by which to study and differentiate academic careers via social control. We are indeed thus also led to consider what a career means as well as what it means (if anything) to be a member of the academic profession, to the variety of people seeking to develop their livelihoods therein.

Conclusion

This chapter has aimed to organize the major lines of research in the sociology of science—the branch of sociology whose cumulative work is most closely allied with the more general study of faculty—in order to motivate a more theoretically grounded study of academic careers. These major lines include: *stratification and careers* (and attendant focus on cumulative advantage and disadvantage; organizational bases of stratification; reputation, visibility, and influence; age and achievement; and mobility); *gender, productivity, and careers*; and the *social control of careers* (and attendant focus on recruitment and socialization; deviance; and the experience of work). The discussion is not intended to be exhaustive of all work conducted in these major areas; as noted, the areas constitute domains for their own respective reviews. Rather, the goal has been to identify the principal topics of research, to call attention to the concepts that anchor the work theoretically, to summarize major findings, and, in so doing, to suggest connections to be forged between a sociology of scientific and academic careers.

Such connection can help stimulate research on many fronts and lead to greater synthesis between the sociology and higher education fields. In the field of higher education, the exchange will serve the goal of theory-building and concept-usage, which will in turn create greater ground for field maturation. In sociology, new work on academic careers, to the extent it makes use of a synthesis, will help reinvigorate institutional sociology of science. Readers will have observed (as explained in the account of the specialty’s mid-to-late twentieth century heyday as well as in note 1) that many of the bodies of work in the sociology of science are themselves aging. Some of them, such as the body of work on stratification, are in need of transfusion; in some areas, work ceased in the 1980s and 1990s. Methodical connection and systematic integration with higher education is a prime source of new blood.

A variety of problems and projects illustrate, but by no means delimit, work to be done in a way that makes productive use of field cross-fertilization. We may consider five of them. First, it is clear that if synthesis is to succeed, *samples must be drawn from varieties of fields* and not simply those in the biological and physical sciences, mathematics, and engineering. Braxton and Hargens (1996, p. 35) earlier sounded a similar call: “differences among academic disciplines are profound and extensive.” It is a noteworthy question to ponder how all of the findings discussed
in this chapter might vary from one cluster of fields to another if indeed samples extended beyond the sciences. Work by Becher and Trowler (1989) is suggestive of the pay-off. Are the dimensions and processes of stratification, for instance, the same between the sciences and humanities? Consequently, what are the possible differences in how reputations are made and in the resulting ways that visibility shapes the cognitive structure of fields? Are the career dynamics of women scientists similar to that of women humanists or social scientists or faculty of professional fields? In conducting these types of extensions and extrapolations, we might arrive at an altogether different conception of careers in contemporary American higher education, but nevertheless one that is decidedly more holistic, variable, and nuanced—and thus more representative of the empirical realities that define our very object of inquiry.

Second, we need to examine the interplay of professional with organizational reward systems. Study of “the reward system” in the sociology of science has rarely, if ever, been clearly specified. It is used as an abstraction and, operationally, appears to consist of the means by which recognition is distributed within a professional community. What is more, usage of the term is typically singular: the reward system of science. However, is there not more than one system of reward in which academics are integrated? Apart from a professional system of reward are organizational systems that vary in their operation among and within the types of higher education institutions; and in any given institutional type, there may in turn be seen to exist multiple systems of reward (e.g., faculty in the multi-university who are conditioned to be responsive to a “teaching track” as opposed to a “research track” in their work). There is a further point: nearly all work on academic careers is situated in research universities; exceptions are few (e.g., Braxton 1983). Yet many faculty members do not work in research universities. Continued exclusion of other employment sectors, such as community and liberal arts colleges and comprehensive universities, will continue to blunt our understanding of academic careers. In addition, different departments or fields may entail systematic contrasts in how faculty work is sanctioned. The complexity of how professional and organizational reward systems may interact awaits systematic investigation. The potential theoretic pay-off is large: it could reshape understanding of stratification and related career processes and, in doing so, inform such questions as how institutions structurally facilitate or constrain the advancement of knowledge.

Third, we may ask: what are careers in academe? At this time, approximately half of faculty appointments are not on a tenure track (Finkelstein et al. 1998; Schuster and Finkelstein 2006). Academic careers appear to be experiencing a transfiguration by employment type alone (Baldwin and Chronister 2001; Cross and Goldenberg 2009). Research is needed on several issues, among them: the consequences of employment type on the stratification of careers; the constitution and performance of roles in higher education institutions (e.g., teaching versus research classes of faculty members); the consequent quality of university functions, including teaching; mobility patterns; recruitment and socialization in light of incongruence between types of graduate school supplies and types of employing university demands; and on the very coherence of academe as a profession.
Fourth, the conditions above suggest deprofessionalization of academe. A recent series of treatments across a variety of topics illustrates the potentially wide-ranging effects of deprofessionalization in one of society's oldest and most vaunted professions (Hermanowicz 2011). With limited exception, the work of the sociology of science portrays its subjects as dedicated researchers, committed to the cosmopolitan goals of their fields, and identifying strongly with an intrinsic value assigned to their scholarly endeavors. Is this an accurate depiction of professors? Study of scientists and satisfaction reveals contradictory results (Hermanowicz 2003). We might hypothesize significant variation in career orientation in academics between types of institutions, but we have been less inclined to consider such variation within institutions, including the sector of research universities, which consists of significant internal variation. In light of dramatically changed and evolving work conditions, opportunities, and rewards, we need to inquire about the meaning of work for those who attempt to develop a career in academe. The study of subjective careers, as discussed earlier in this chapter, reveals not simply individual experience, but also the organizational conditions under which such sentiment is generated. The concept of the subjective career is viable to the extent that it is as much a lens on individuals as on the institutions involved in their reciprocal creation.

Finally, prevailing conditions of universities prompt the question of who will seek an academic career. The question is one of recruitment of labor stock. The implication is that degenerative organizational conditions will affect the quality of labor supplies. Prior discussion in this chapter and elsewhere (Bok 1993; Cole 1992) suggests that the most talented individuals will be drawn to professions viewed as more rewarding. If that is the case, we will need the means necessary to understand the consequences of such decline on the viability of academic careers, on the organizations in which academic careers are forged, and on the knowledge gains that are supposed to be made by the people who enter academic careers. This problem, like the projects outlined above, is likely too heavy a burden for just one research specialty to bear. The research agenda—illustrative, not exclusive—underscores the value of cultivating a courtship between sociologies of scientific and academic careers.

References


