

# The Proliferation of Publishing: Economic Rationality and Ritualized Productivity in a Neoliberal Era

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**Abstract** Article publication in the field of sociology has proliferated. Data are presented on change in the number of sociology journals over time, the patterns of publication productivity by faculty in differently ranked sociology programs, and the patterns of article output by faculty of different age cohorts located in top-, middle-, and bottom-ranked sociology departments in the United States. Data are inconsistent with findings from the sociology of science, which elaborate a pattern of generally low productivity, and which describe sharp differences in publication by the age and organizational location in which academics work. The reasons for proliferation, attributable to historic shifts that have affected the terms of academic work in the late 20th century, are discussed. Proliferation of publication is not unique to sociology, but neither are its effects on fields monolithic. Comparisons and contrasts to other fields are made throughout the discussion. The proliferation of publication poses significant implications for the status of sociology and sociologists. While increased output may be associated with the advancement of science and scholarship in some disciplines, it is both a consequence and cause of demise in others.

**Keywords** Publication · Articles · Science · Sociology · Academic capitalism · Neoliberalism

## Introduction

This article examines growth in the quantity of publication in the field of sociology. Publication has long been seen as a key, if not the most central, activity that academics perform. Yet up until the late 20th century, relatively few academics published, and still

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fewer published consistently. Focusing on the publication of journal articles, this article explores how publication has become an organizational imperative for the faculty role. I will present and discuss the reasons that account for why publication has proliferated in sociology in particular and in academia in general. The proliferation of publication may be seen by some as a sign of maturity and advancement of a field. There are contrary views, and in the case of sociology, the proliferation of publication is problematic. I will discuss the implications that publication growth entails for the field.

The article has three main parts. First, I frame and situate discussion of publication and its growth by work in the sociology of science. Findings from this work, much of which was published in previous decades, are deliberately employed to draw contrasts to patterns that have arisen in the contemporary period. In particular, I discuss the reward system of scholarship and how it is viewed to operate as part of a system of stratification in science. I focus on two domains—age (or career phase) and organizational location—in which stratification processes have been found to exert significant effects on academic careers.

Second, I present data used to examine article production in sociology. I present data on the number of journals over time as a means to consider publication opportunity and as a way to evaluate journals that one might judge to be “sociological.” Configuring information originally gathered by the National Research Council, I present data on publication productivity among sociology faculty who work in variously ranked departments. By turn, using data generated from the CVs of sociology faculty similarly employed in a range of departments, I examine publication productivity and how it varies both by organizational location and by career phase.

Finally, I discuss the ways in which patterns in the presented data differ from findings produced by the sociology of science. The differences pose critical implications for the conditions under which important sociological work is created, identified, and used. I discuss the reasons that account for a proliferation of publication, which are traceable to a transformation in the economic structure and organizational culture of modern American universities. The transformation entails dramatic and recurrently reinforcing consequences: for universities as organizations, for individuals and their faculty careers, and for sociology as a discipline. The discussion calls attention to the grounds on which science and scholarship advance. We are able to discern how proliferation of publication can sow some fields to strengthen, and how in others, it can over-seed them to break further down.

In this vein, while the article centers on sociology, many of the ideas raised are applicable to most other academic and professional fields—either by virtue of their differences from, or their similarities to, the patterns observable in sociology. These similarities and differences are raised throughout the discussion.

## **The Reward System in Science and Scholarship**

Scholars have argued that publication is the central activity of modern science and scholarship (Ben-David 1971, especially 139–168; Merton 1973a). It enables the communication of research findings. Thus it is seen as the foundation of fields of inquiry, encapsulating the ideas that form a field’s core, while suggesting what remains to be discovered on a field’s frontier. As Fox has observed: “Through publication,

scholars keep abreast of a field, verify information, obtain critical response to work, and redirect research interests” (1985, 255).

Publication is also viewed as central because all other roles performed by academic scholars and scientists are said to be contingent on it. The point, such scholars claim, is self-demonstrating, for there would be no codified knowledge to transmit through a teaching role, and no knowledge to apply through a role of service or consultation, were there not a body of systematically generated findings obtained by a process of research and imparted by the practice of sharing results through publication (Merton and Zuckerman 1973).

For these reasons, it is thought, publication assumes a privileged place wherein it constitutes the chief basis of recognition in science and scholarship. As Merton put it, recognition from those competent to judge a contribution is the prime indicator that scholars and scientists have fulfilled the core institutional goal of higher learning—to extend certified knowledge (1973b). Symbolized in honors, awards, appointments, citations, and the like, recognition may thus be viewed by members of a scholarly community as the prime reward for research achievement.

By this view, the reward system of science, operating at a social-institutional level, is monolithic. It is presumed to operate without regard to other levels of reward to which academics are socially and economically tied. Yet academics work in a spectrum of colleges, universities, and institutes, whose reward systems are plural. There is, therefore, likely incongruence and conflict in the operation of reward systems operating at different levels.

An older line of scholarship in the sociology of science found that, despite the purported centrality of publication, comparatively few academics actually engage in it. In a 1985 review of extant work in the field, Fox wrote:

Despite the centrality of publication to science and scholarship, average levels of performance are low. In a sample of academics from both natural and social sciences, J. Cole (1979) found that 1 or 2 years after the doctorate, 53 % had failed to publish a single paper and 34 % had published just one. With a national sample of faculty across fields, Ladd and Lipset (1975) also document astonishingly low levels of publication; over half of the full-time academics had never written or edited any sort of book; more than one-third had never published an article; and more than one-quarter had never published a scholarly work of any kind over the course of their careers (258).

Emblematic of the disparities it traces, this sociology is consolidated in an area known as “stratification in science” (e.g., Cole and Cole 1973; Hagstrom 1965; Long and Fox 1995; Zuckerman 1970, 1977). The theory of cumulative advantage and disadvantage informs much of the work in this area. The theory explains how increasing disparities come to characterize the “haves” and “have nots” over the course of a career. “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, 59–60).

Allison and Stewart (1974), using a sample of chemists, physicists, and mathematicians, incorporated the theory of cumulative advantage to account for productivity differences among scientists. They noted that publication productivity among scientists

tended to be highly skewed. Productive scientists maintained or increased their productivity, while scientists who produce little went on to produce even less later. Thus, the distribution of productivity 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. These patterns were corroborated in the 1980s using a sample of biochemists and chemists (Allison et al. 1982).

Likewise, S. Cole (1979), using publication patterns in the years 1965 to 1969 across six fields, explained change in productivity as scientists age by the operation of the scientific reward system. The reward system is thought to encourage scientists to continue publishing when their (early) work is favorably received and, by contrast, discourage those whose work is not favorably received. Consequently, over time, the reward system will work to reduce the number of people who are actively publishing. “Those who continue to publish throughout their careers are a ‘residue’ composed of the best members of their cohort. Increases in productivity through the thirties and into the forties are shown to be a result of command over the resources necessary to be highly productive” (S. Cole 1979, 958), an empirical manifestation of cumulative advantage.

Related work examined the organizational bases of stratification in scientific careers. Using a sample of biologists, political scientists, and psychologists, Crane (1965) found that scientists at major universities are more likely to be productive and garner recognition than scientists at minor universities. This finding, at least for its time, is consistent with patterns of cumulative advantage discussed above. Major universities may not only recruit highly motivated personnel, but also confer resources that further fuel productivity. Further, Long (1978), using citation counts from 1955 to 1973, found that the effect of departmental prestige on productivity is demonstrably positive over time. One’s work stood a greater chance of being recognized when the author was located in a prestigious department or university.

These patterns in turn bear on the work of Long and McGinnis (1981), who, basing their work on the population of biochemists who earned their doctorates in 1957, 1958, 1962, and 1963, found that individual productivity conforms to the characteristics of the context in which a scientist works. Allison and Long (1990) reinforced this observation by concluding that the effect of department affiliation on productivity is more important than the effect of productivity on departmental affiliation.

The body of research on social stratification in science established two modes by which academic careers are differentiated: *career phase* and *organizational location*. Studies of cumulative advantage conveyed how research performance is stratified over time, that is, over a set of phases in an academic *career* in which scientists age. Early success spelled continued productivity; lack of recognition brought about productivity decline. Studies of scientists working in departments and universities conveyed how *organizations* conditioned research performance. Advantage existed in an elite, where scientists were socially controlled by immediate peers to produce and where their productivity benefited from organizational resources.

There is little reason to suspect that the general patterns (e.g., in age and organizational effects) and processes (e.g., of cumulative advantage) of publication extracted from samples of natural scientists would not have also applied to academics in the social sciences and humanities studied *during the same periods of time*. Social scientists and humanists, like natural scientists, are embedded structurally in both

organizational and social-institutional systems of reward. Patterns of age and organizational effects, and processes of cumulative advantage, may vary among fields. Such variation is likely produced by the time that is necessary for patterns to emerge, owing to differences in field norms about the practices of work. The body of work on stratification in science, which, with some notable exceptions, decreased in volume by the 1980s, may serve as an historical back-drop against which to inquire about contemporary publication patterns, with a focus on the academic field of sociology.

With the exception of the humanities, articles are the most standard publication genre across academic fields. It is true that sociologists, especially in the United States, publish books, as do members of other social sciences (e.g., anthropology, history), and that in some academic departments of sociology books are co-standard with, and sometimes more standard than, articles. (This, too, illustrates the plurality and complexity of organizational reward systems discussed above). For the purposes of the present discussion, article publication in journals is taken to serve as the analytic unit.

## The Article in Sociology

### *How May One Construe “Opportunity” to Publish? and How Are “Sociology” Articles Construed?*

To begin an empirical examination of article publication in the field of sociology, we may consider the journal outlets available to people in which to publish. Opportunity to publish is a *de facto* constraint on careers and thereby on institutions, since careers and institutions, be they departments, colleges, universities, institutes, or the larger system of higher education, interactively condition each other (Barley 1989). Opportunity is more or less constraining because there are a finite number of journals in which to publish and a finite number of pages allocated for articles in those journals.

But while finite, the number of journals has been anything but constant over time. Table 1 records the number of journals in the field of sociology in 15 year intervals between the year 1970 and 2015. Based on the *Social Sciences Citation Index*, 78 sociology journals existed in 1970. In 2015, there were 142, an average increase of 1 %, or 1.5 journals, in each of the 45 years covering the period. The upward pattern in journal number is not unique to the field of sociology. It is a general pattern observable in a large number of academic and professional fields.

This pattern portrays only part of the picture. Over this period of time, the domain of work considered sociological expanded. Major quarters of what may be taken as the sociological field are increasingly interdisciplinary, as evidenced in areas such as cultural studies, urban studies, science studies, women’s studies, environmental studies, African-American studies, Latin and Caribbean studies, and the like. These areas of inquiry are part of a sociological domain; over time the areas may be cast apart in a movement toward their institutionalization. The founding of journals, academic programs and departments, and professional associations are indicative of this institutionalization. The areas of criminology and demography reflect among the strongest institutional claims. (Criminology is sometimes jointly housed and titled in sociology departments; demography, owing largely to its technical basis, is enshrouded in

**Table 1** Number of journals in sociology and selected cognate fields, 1970–2015

Field	Year			
	1970	1985	2000	2015
Sociology	78	83	96	142
Education	103	114	96	231
Criminology	18	22	20	58
Demography	11	18	16	26
Urban studies	13	25	29	39
Women's studies	–	7	25	41
Communication	11	24	43	78
Gerontology	7	10	23	32
Environmental Studies	15	34	47	103
Cultural studies	–	–	–	38
Family studies	–	23	31	42
Total	256	360	426	830

Sources: Institute for Scientific Information 1979 and 1987. *Social Sciences Citation Index*. Philadelphia: Institute for Scientific Information; and, *Social Sciences Citation Index, 2000 and 2015*, Thompson Reuters, <http://ip-science.thomsonreuters.com>

prestige, emblematic in the journal by the same name and by the Population Association of America.)

Thus to consider only “sociology” journals in the story of the academic publishing industry in sociology underestimates the amount of production. Table 1 also lists the number of journals over time in selected fields that are arguably cognate to sociology. The list is, however, but a sampling. It is necessarily incomplete, and thereby underestimates production volume by the people who call themselves sociologists. The list thus self-demonstrates the point: where does one cross the line from sociology into another field? (Sociologists publish in the *American Journal of Public Health*, the *American Journal of Surgery*, *The Hedgehog Review*, and so on, among the many outlets not captured in the list.)

Using this sampling for illustration, there were a total of 256 journals in sociology and selected cognate fields in 1970. The number steadily increased, such that by the year 2015 there were 830, an increase, albeit an undercount, of 324 %. Put differently, by a conservative measure, an average of nearly 13 new journals availed themselves *each* of the 45 years between 1970 and 2015 for sociologists to publish their work.

### How Does Article Publication Vary by Department Rank?

It may seem logical to suspect that the bulk of publishing takes place in the most reputed departments.<sup>1</sup> For example, some might believe that the sociology faculty at Chicago, Wisconsin, UC-Berkeley, Michigan, and Harvard are more prolific than faculty members elsewhere. The most reputed departments are the most strongly

<sup>1</sup> *Department* and *program* are terms used here interchangeably.

associated with graduate education (they produce the largest share of doctoral students); these departments also attract some of the best students who become co-authors and thereby accentuate publication productivity. Such departments are housed in what many believe are prestigious universities whose acclaim may stem from a history of achievement by their faculty and graduates in research.

The National Research Council (NRC) has conducted assessments of graduate programs in the United States. In all but the last assessment, published in 2011, a reputability measure was included among the items provided to characterize programs. Because the 2011 assessment excludes a reputational measure, data used below are taken from the prior assessment published in 1995. In this assessment, 95 graduate programs in the field of sociology were evaluated (Goldberger et al. 1995). Among the measures, the “scholarly quality of program faculty” measure is the broadest or most over-arching. Programs may be ranked using the measure. Burris (2004) has demonstrated that most programs, departments, and universities change little in rank over time. The present selection of ranked programs is thus relatively stable in time.

Table 2 depicts a clustering of sociology programs into three tiers: the top, middle, and bottom ten programs. Ten programs, and not some other number, are used to constitute tiers because they capture sufficient variation at three intervals along a scale from 1 to 95. In the 1995 assessment, the NRC collected data on the percentage of faculty in a program who had published in the preceding period 1988 to 1992.

Table 2 aggregates the NRC data. It shows *the percentage of programs, clustered by tier, that had 33 % or more, 50 % or more, and 66 % or more, of their faculty members publish between 1988 and 1992*. Thus, all of the top and middle programs had at least 33 % or more of their faculty publish; only *one* of the bottom tier programs had *under* 33 % of its faculty publish. By turn, 90 % of the top programs had 66 % or more of their faculty publish, whereas this number for the middle programs was 70 %, and for the bottom programs, 60 %. In different terms, even at the bottom tier, a majority of the programs had a majority of their faculty publish. In still plainer words, most sociologists, everywhere, are publishing.

**Table 2** Sociology Faculty Publication Productivity, by Program Tier

Faculty who publish	Tier		
	Top 10 programs	Middle 10 programs	Bottom 10 programs
≥66 %	90.0	70.0	60.0
≥50 %	100.0	100.0	70.0
≥33 %	100.0	100.0	90.0

The percentage of programs, clustered into three tiers of ranked programs, who had 33 % or more, 50 % or more, and 66 % or more, of their faculty members publish in a preceding period of 1988–1992. Rankings by National Research Council, using scholarly quality of program faculty measure (93Q).  $N=95$ , ranked sociology programs. Source: Goldberger et al. (1995)

### ***How Many Articles Do Sociologists Publish? and How Does Article Publication Vary by Career Phase?***

Perhaps sociologists are just publishing an article (or some other type of publication) sporadically. The NRC question used above captures merely the fact that a faculty member published *something* in the prior 4-year interval of time. What is more, the rise in the number of journals as depicted in Table 1 can simply mean there are more options among the outlets in which to publish. Up to this point, the data do not speak to faculty publication volume.

What is more, publication productivity may vary by phase in an academic career. Research discussed earlier found that productivity continues for the best producers, whereas it lessens or stops for the weakest producers. Further still, publication may vary by career phase within program tiers, owing to variation in organizational resources that condition productivity.

To address the question of publication quantity, the publication lists, as of 2015, for faculty members, were obtained in each of the programs in the three tiers constructed from the NRC data described above. Publication lists are available from CVs, which are now readily available on individuals' pages of departmental web sites. Across the internet sites of academic departments in the contemporary American university, the occurrence of the unavailable CV has become a rarity. The ubiquity of the CV is itself data which may speak to a standardization of academic roles, perhaps especially research, across an otherwise diverse spectrum of departments and universities.

Each CV was coded for the faculty member's number of peer-reviewed journal articles. Excluded in the count are books, edited books, monographs, book chapters, book reviews, reprinted articles, replies, responses, commentaries, newsletter articles, essays, reports, conference presentations, and articles under review. For the purposes of coding, sole-authored, multiple authored, first-authored, and non-first authored peer-reviewed articles are counted equally. Only regular faculty members, that is, tenured and tenure-line faculty who have the rank of assistant, associate, or full professor are included in the analysis.

To ascertain publication patterns by career phase, the sample was divided into three cohorts, corresponding roughly to early, middle, and late career phases, as indicated by the year in which the sociologists earned their Ph.D. Cohort I, corresponding to late career phases, includes sociology faculty members who earned their Ph.D. prior to 1990. Cohort II, corresponding to mid-career phases, includes sociology faculty members who earned their Ph.D. between 1990 and 2005. Cohort III, corresponding to early career phases, includes sociology faculty members who earned their Ph.D. after 2005.

Table 3 presents the data on journal publications by program tier and cohort. Several patterns are noteworthy. First, examining the *median* number of publications, faculty members in top programs wrote 34, 18, and 9, articles, from the eldest to youngest cohorts respectively. In the middle tier programs, the respective medians were 30, 16, and 7. In the bottom tier programs, 35, 15, and 5. The difference between total average medians between the top (20) and middle (17) programs is quite small. The difference between the top (20) and bottom (12.5) is only moderately greater. In different terms, while the difference in rank between top and bottom sociology programs (1–10 and 85–95) may be perceived as significant on several dimensions (such as departmental prestige

and individual renown), the overall difference in terms of article production amounts to a median number of 7.5 articles.

Second, examining the *mean* number of publications in comparison with the medians across cohorts, the differences are small with limited exception. For example, in the bottom tier, the median and mean number of articles are roughly similar, differing only within a magnitude of between 2 and 3 articles. This pattern, while still strong, begins to lessen in rising across the middle and top tiers. Thus, for example, the difference between the mean and median number of articles in the eldest middle tier cohort (39.5 versus 30) is 9.5; among the eldest top tier cohort (46 versus 34), 12. The *maximum* producers in the eldest cohort published 193, 177, and 116 articles from the top tier down. These collective patterns suggest that some of the cohorts have mass producers who elevate the average away from the median, and that these producers are most likely found in the eldest top and middle tier cohorts.

**Table 3** Journal Publications among Sociologists, by Program Tier and Cohort

Program tier	Cohort I	Cohort II	Cohort III	Total
	Ph.D. Pre-1990	Ph.D. 1990–2005	Ph.D. Post-2005	
<b>Top 10</b>				
N:	95	100	52	247
Minimum:	3	3	1	1
Maximum:	193	95	74	193
Median:	34.0	18.0	9.0	20.0
Mean:	46.0	22.0	11.4	29.0
<b>Middle 10</b>				
N:	61	68	51	180
Minimum:	5	1	0	0
Maximum:	177	43	42	177
Median:	30.0	16.0	7.0	17.0
Mean:	39.5	17.0	9.7	23.0
<b>Bottom 10</b>				
N:	16	43	43	102
Minimum:	5	5	0	0
Maximum:	116	58	30	116
Median:	35.0	15.0	5.0	12.5
Mean:	38.0	18.3	7.3	17.0

Source: The curriculum vitae of each sociology faculty member as available and downloaded June 8–10, 2015 from departmental web pages. Includes peer reviewed journal articles only. Excludes books, edited books, monographs, book chapters, book reviews, reprinted articles, replies, responses, commentaries, newsletter articles, essays, reports, conference presentations, and articles under review. Journal articles are counted up to and including the last year listed on each vita

Vitae are for regular faculty (i.e., those listed as assistant, associate, or full professor). Faculty count excludes instructors, lecturers, adjunct, visiting, and emeriti faculty, and non-tenure line faculty. Of the 291 regular faculty in the top ten sociology programs, 247 (85.0 %) vitas were available online. Of the 208 regular faculty in the middle ten sociology programs, 180 (87.0 %) vitas were available online. Of the 140 regular faculty in the bottom ten programs, 102 (73.0 %) vitas were available online. Number of ranked sociology programs=95 (Goldberger et al. 1995)

Finally, the patterns indicate that a majority of cohort members are publishing across the tiers. Indeed, comparing medians, the eldest cohort of the bottom tier out-produced their counterpart in the top tier (35 to 34). This is made all the more remarkable by the fact that the former tier was comprised by just 16 people, in contrast to the 95 who comprised the later. If we examine the median number of articles at each end of the program continuum (bottom and top tiers), the late career cohort published 35 articles to the top's 34; the mid-career cohort published 15 articles to the top's 18; and the early career cohort published 5 articles to the top's 9. Publication activity, including the behavioral pattern of sustained publication, is by no means concentrated among an elite; it is, rather, exemplified throughout academic sociology.

## Implications and Discussion

The present-day patterns of article publication in sociology are not consistent with observations drawn from earlier work on stratification in science. First, it is no longer the case that only a minority of scholars publish, as Fox summarized in 1985. At least in academic sociology, the present data show that virtually *everyone* publishes. The quantity of publication has been accommodated by steep increases over time in the number of journals. There appears to be no reason why this pattern would be unique to sociology; rather, it likely applies to most academic and professional fields of the modern American university. Whether one field or all, the result is a very large proliferation of publication.

Second, in terms of age, scholars across cohorts publish, and successive cohorts publish more in *total number* of articles than their younger counterparts. This does not necessarily obviate the occurrence of cumulative advantage and disadvantage, since no assessment has been made of (a) the recognition garnered by sociologists, as measured, for instance, by the number of citations to their published articles, and (b) the quality of journals in which sociologists publish subsequent to their early work. For example, sociologists who are highly recognized for their early work (regardless of tier) may go on to publish articles in highly esteemed journals, whereas those who are comparatively less recognized may still go on to publish, but in less esteemed journals. (We shall return below to the subject of journal quality and professional status in sociology.)

S. Cole's (1979) more general point, however, was that, if relatively unrecognized, scientists will not continue in an activity as difficult as research and publication. "Most people will not continue an activity as arduous as scientific research unless they are rewarded for it" (969). In Mertonian sociology of science, the reward system operated functionally such that scientists producing the best work would be encouraged to continue publishing while scientists producing work of less quality would be encouraged to stop publishing and more fulsomely embrace activities in other roles. "By encouraging those scientists who produce the most favorably received work and discouraging those who produce work that is not favorably received, the reward system works to reduce the number of scientists who are actively publishing" (S. Cole 1979, 958). It is far from apparent that only the most recognized sociologists keep

publishing. Net of all other conditions, sociologists continue to publish as the age of a cohort increases.

Finally, in terms of organizational context, scholars across ranked tiers publish and do so in amounts that are roughly equal across tiers. In median number, scholars located in bottom-ranked programs publish roughly as much as those in the top-ranked programs (cf. Turner 2013). Here again we are unable to assess processes of cumulative advantage and disadvantage. In theory, the accrual of advantage would successively benefit scholars in the top programs to produce the “best” work compared with all others, and benefit scholars in the middle programs compared with those in the bottom. An assessment of article quality would shed light on whether “writing from the top” is more advantageous over time compared with “writing from the middle” or the bottom.

The fact, however, that sociologists everywhere are publishing suggests that stratified patterns in publication quality are not as clear across organizational lines as espoused by the functional view. Crane put it this way in 1965: “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” (699). Today the words are nonsensical. What is more, given her sampling of departments with graduate programs, the “minor” universities of Crane’s time are likely “major” ones today, reflecting significant change in the faculty composition and organizational goals of U.S. universities across an historical period (Gonzales 2013).

Nor do the present findings coincide with Long and McGinnis’s (1981) work: individual productivity cannot meaningfully be said to conform to the characteristics of the context in which scholars work when the variation by context (in median article output) is so small. The findings also challenge Allison and Long’s (1990) claim that a significant effect is exerted by department affiliation on productivity. The present data suggest that departments exert a demand on individuals to publish, but as a *universalized* function of individuals’ university membership, not as a means to differentiate the effects of one type of department from another. The present data are not consistent with organizational claims spelled out by the earlier sociology of science. Net of all other conditions, the publication of journal articles is widespread and customary among faculty throughout the spectrum of sociology departments.

The assessment of quality in the field of sociology is problematic. This in turn bears on the construction of professional status and the logic of stratification in this field. Sociology is typified as a low consensus field, in contrast to, for example, physics, chemistry, genetics, or mathematics (Braxton and Hargens 1996). Consensus refers to the extent of agreement among members of a field regarding such matters as problems worthy of inquiry, methods for research, and theory used for testing and explanation (S. Cole 1983; Hargens 1975). Consensus also encompasses journals. In sociology there is little agreement about which are better than others. Some sociologists would claim that there are two prestigious general journals in the field: the *American Sociological Review* and the *American Journal of Sociology*. Others would add *Social Forces* to the list; others would put it in another tier, and others still would disregard it altogether—a demonstration of low consensus. But while these journals may be said to be general, most sociologists do not publish in them. Indeed even in some quarters there is an *anti*-ASR-AJS sentiment. The reasons for such sentiment, with these and with other sociology journals, are various: editorial decisions seem arbitrary; biases

exist against areas, styles, theoretical approaches, and methods; reviewers are mismatched for reviews; the quality of reviews is low; the review process is encumbered by second and even third and fourth rounds with new reviewers added in new rounds; acceptance has become a “crap shoot,” and so on. These sentiments are also indicative of low consensus (Fox 1989).

Some sociologists would claim that *ASA journals* are the gold standard, representative of what clearly is sociology and sociology at its best. But there are just eight ASA journals (if we exclude the journal of book reviews, *Contemporary Sociology*). This compares to the 142 listed in the *Social Sciences Citation Index* (Table 1). ASA journals do not encompass the sociological field by the most liberal criteria. Many sociologists do not publish in them either.

What comprises an elite and who is in it? In fields such as physics and chemistry, members can rank each other and those who compose an elite are readily identifiable (Hermanowicz 1998). In sociology the exercise is more complicated. Organizational location, for example, can enhance status but is clearly not its sole, or even most important, determinant. Elite sociologists are found throughout departments of sociology in the U.S., just as there are members of what are considered elite sociology departments who cannot hold a candle to the wind. Elite sociologists have been made by virtue of publication in journals such as *ASR* and *AJS*; some considered elite have published but sparingly in those outlets. Still others have achieved a high status without having published in those journals. Some sociologists achieve renown for theoretical contributions, others for methodological ones. All, or nearly all, journals become defensible ground for establishing merit in sociology. That many sociologists can fashion themselves as elite or something approaching it before some kind of audience of sociologists, is perhaps not an exaggeration. The proliferation of journals, concurrent with a plurality of performance standards, illuminates a disintegration of a discipline.

...disintegrated disciplines with many different and incompatible standards for what is good tend to be precarious in academic settings... What is good in them does not depend on the disciplines' corporate existence; what is bad in them discredits the disciplines. ...[S]ociology has a dim future first because it is unlikely to develop much consensus on who best represents the sociologists' sociologist... [and] because it is unlikely to be able to argue with one voice about what is “elementary,” and how what is elementary is connected to what is first class... (Stinchcomb 2001, 85–86).

Furthermore, while the use of citations to indicate article quality is customary in the natural sciences, this, too, is problematic for the humanities and many of the social sciences, including sociology (Najman and Hewitt 2003). For these reasons, the use of citation to judge article quality is not fully institutionalized as a means of performance evaluation in sociology. Rather, it seems to be used in cases where judgments stand to benefit the person whose work is being evaluated. Work in sociology is not as cumulative as work in the natural sciences. Consequently, there is not a clear genealogical order of contributors from which to deduce who should receive recognition from a

scholarly community and what amount the community should bestow (Lindsey 1988).

Status in sociology (and in other fields in which citation counts are problematic) is more largely structured by reputation. For example, a scholar of sociology or English or history may be known and regarded by the book she or he published independent of citations to it. Academic reputation is not subject to commensuration akin to citation (Espeland and Sauder 2007; Espeland and Stevens 1998). While reputation operates as a function of scholarly contributions, it is a social construction. Thus, “good reputations” can be crafted from achievements that are by turn various (Hermanowicz *Forthcoming*). This partly explains how sociologists maintain commitment to publication. It is difficult to be judged as pedestrian by the system, and sociologists need not accept one set of criteria of judgment as legitimate (cf. Cole 1970, 401). Especially in low consensus fields, the proliferation of journals facilitates the construction and conferral of esteem.

Drawing on samples of academics and universities of the 1950s and 1960s, earlier sociologists of science saw the institutional reward system shutting down (or at least decreasing) publication among many scientists. In the present-day, the *reverse* characterizes academics. The proliferation stems from discrepancies between *institutional* and *organizational* systems of reward. The institutional reward system does not keep people from publishing even minor or unnoticed work because organizational incentives to publish have become sufficiently strong. Short of receiving recognition bestowed by a scholarly community, the scholar and scientist receives increases in salary (and other rewards) for continued publication productivity. Even small percentage salary increases are sufficient incentive, suggesting that both money and honor are at stake in a production process. Most faculty members do not want to lose out in the competition; both their salary and reputation, however local or cosmopolitan, would suffer. This pattern is predicated on a pervasive *competition* in a new *economy* of academic work.

From top to bottom, faculty members—like the organizations that employ them—have become entrepreneurs. This development bespeaks social and historical conditions that have prompted the change. It is the argument here that this development is an outcome of *neoliberalism* and its ascent in American higher education. Neoliberalism is a “theory of political economic practices that proposes that human well-being can best be advanced by liberating individual entrepreneurial freedoms and skills within an institutional framework characterized by strong private property rights, free markets and free trade” (Harvey 2005, 2; quoted in Slaughter 2011, 267). A radical restructuring of American universities has entailed a shift in which academic work and education, once viewed predominantly as public goods, become the principal matter of free enterprise (Berman 2012). “[A] neoliberal state shifts higher education from a public good knowledge/learning regime...to an academic capitalist knowledge/learning regime” (Slaughter 2011, 267), a trend that gained strength in the 1980s, wherein universities and faculties are increasingly characterized by market behavior (Slaughter and Leslie 2001).

In an academic-capitalist age, economic returns coupled with prestige are the central objects in which institutions and faculty members compete. Research and publication are a main currency in which prestige is traded. The currency is regulated by audit systems that have themselves become institutionalized as part of the way in which an

academic-capitalist economy operates. Universities now require faculty members to account for themselves by engaging in rituals of verification—documenting and recording their activities, including research in progress, articles under review, accepted for publication, and published, on an annual basis (Power 1997). These rituals incentivize the proliferation of publication, however minor or unimportant to the stock of knowledge.

Administrators are charged with generating evaluations of how individuals and units perform (Miller and O’Leary 1987). These practices encourage what has been called an “accountability regime,” wherein academic work gets reconfigured in a metric reality (Hopwood 1987; Tuchman 2009). The practices create a new standard of economic rationality in university decision-making (Geiger 2004) and, consequently, in faculty behavior. “Rather than universities being subordinated to the production and transmittal of knowledge, knowledge is now subordinated to the needs of universities for profit and recognition” (Tuchman 2009, 11). The new academic economy exacts motivational behavior for publication. Increase in the number of journals is a pattern accommodative to neoliberalism.<sup>2</sup>

The proliferation of publication creates a further set of consequences for higher education, especially in low consensus fields. The suppression of an institutional reward system by organizational ones maximizes a specific type of faculty member: *ritualists*. In Merton’s terms, ritualism is an individual mode of adaptation (Merton [1957] 1968). Here it is adaptive behavior to academic capitalism. As ritualists, people fail to believe in the goals of a system, but retain belief in the means toward satisfying those goals. Throughout the tiers of departments, the penalties of not publishing are now perceived as high (Tables 2 and 3). Lack of faith in academia can stem from a perception that system goals fail to be served by all the writing; that is, the majority of publication is perceived not to advance knowledge.<sup>3</sup> As Cole observed of physics: “...the data indicate that most research is rarely ever used by the bulk of the physics community, and even more sparingly used by the most eminent scientists who produce the most significant discoveries. ...Clearly, most of the published work in even...an outstanding journal makes little impact on the development of science” (Cole 1970, 400). If this is a conclusion drawn about a high consensus field, we may surmise that the effects are even more pronounced in a low consensus field.

We can therefore question the problems that are created for a discipline when an social-institutional filter of publication breaks down, such that weak

<sup>2</sup> Proliferation of publication is but one consequence of the ascendance of neoliberalism in academia. Its effects are multiple, including a re-structuring and re-stratification of faculty, in recruitment, rank, tasks, and compensation, throughout higher education (see Schuster 2011).

<sup>3</sup> By the same token the pressures to publish, now felt everywhere, may also establish conditions for a rise of *innovators*, that is, those who believe in systems ends, but not the means by which to achieve them (Merton [1957] 1968). The socially manufactured desire for recognition becomes so great, because it is so consequential for academic careers, that individuals resort to inappropriate and illegal behavior to obtain it. The result is deviance in science and scholarship. Reports of fabricating and falsifying data have become commonplace (e.g., Callaway 2011; Carey 2015), but we do not know how rates of deviance in research and scholarship have changed over time (Zuckerman 1988).

producers (however defined) ritually continue to produce in response to organizational incentives. One problem is that mediocre academics “pass” for productive and creative. While egalitarianism may be appealing to some individuals because of an enhancement in status or because of ideological commitments to equality, ambiguity of performance criteria erodes the intelligibility of a field. At this social level, everyone loses.

Another problem is that it becomes increasingly difficult to locate important work. The problem develops not only because the principals disagree on what work is “important,” but also because of sheer production volume. To use the vernacular, piles of garbage accumulate inordinately. This in turn makes the task of producing important work more difficult. It is also likely that, because of volume, some important work goes unnoticed or under-utilized. This impedes scholarly and scientific advancement.

Yet another problem is that the range and quantity of work (by virtue of its varied quality) is further fragmentary in its effects. For a field such as sociology, whose core is already contested, the proliferation of publication weakens its disciplinary claims. Coinciding with the data on the number of journals in Table 1, the field mushroomed after the late 1960s. It has expanded to the point where it is difficult to say when work falls in or out of a disciplinary boundary (Cole 2001a; Smith-Lovin 2007; Stinchcomb 2001; Turner 2006; Wallerstein 2007). As such, a field is compromised in its legitimacy. A de-legitimized field bears significant intellectual, educational, and political consequences which testify to the confusion about its own identity.

An important irony also appears to emerge. Fields can only advance in their state of knowledge by scholarly contributions. Therefore, a logical statement would be that the more a field produces, the greater chance it has for advancement. A body of work on the conditions of scientific advancement has examined how constellations of ideas lead to development in a field (e.g., Ben-David and Collins 1991; Clark 1973; Cole 1992, chapter 9; Menard 1971).

But just the opposite seems to be the case in sociology: the more that a field produces, the worse the field has become. This prompts the question: at what point does a field cross a threshold from progress to decay, and what causes this transition? The thought that sociology is in trouble has been around for some time. It is a post-1960s concern (Cole 2001b; Collins 1986; Turner and Turner 1990, 179–197; Wallerstein 2007). This is to say that the concern covers a period approaching half of the field’s life-time. The concern centers on substantive, theoretic, and methodological diversity and polarization.

The prior proposition—that the more a field produces, the greater its chance for advancement—makes an assumption that there is coherence, organization, and an above average degree of cumulateness to the work produced by a communicating group of scholars. It is an assumption inapplicable to sociology (Collins 2001). In the absence of these conditions, growth in the volume of work produces successively greater disarray. This enables us to speculate that increase in the number of journals in high consensus fields could enhance the occurrence of important work, since members of the field produce work in accord with some set of shared understandings, frameworks, and goals. By contrast, growth in the number of journals in low consensus fields fosters a

disproportionate growth in the amount of unimportant work, since members of the field work in much less agreement about their field. In the former case, increased production strengthens a field; in the latter, it weakens it.

## Conclusion

For sociology, the proliferation of journals and publication is associated with a weakening, rather than a strengthening, of the discipline. The proliferation occurred not only as a result of a 1970s surge of sociology Ph.D.s to place their work (Hargens 1991), since proliferation actually accelerated long after this period. Nor was it solely a consequence of increased expectations in article production for tenure (Perrucci et al. 1983), though this was a component of a larger historical shift. The proliferation has resulted from a transformation in the economy of universities and academic work, a transformation embodied in a philosophy of neoliberalism, which grew from antecedent roots (Berman 2012; Sorokin 1937–41) and gained unprecedented political and social momentum in the 1980s and thereafter. Publication in and of itself no longer separates academics; “stardom” now does (Slaughter 2001). Gone are the days of the one-good-book career, the preponderant teaching career, and the “nice set of articles” career. In all reaches of the contemporary university—felt by the hand of academic capitalism—the current state reveals a wide-sweeping organizational creation of ritualists who, by the terms of their work, spread the world with publication.

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