ADVANCES IN MOTIVATION AND ACHIEVEMENT

A Research Annual

WOMEN IN SCIENCE

Editors: MARJORIE W. STEINKAMP
MARTIN L. MAEHR
Institute for Child Behavior
and Development
University of Illinois, Urbana-Champaign

Volume 2 . 1984

JAI PRESS INC.

Greenwich, Connecticut *London, England*

JAI PRESS INC.
3 Henrietta Street
London WC2E 8LU
England

ISBN: 0-89232-288-8

Manufactured in the United States of America

THE PRODUCTIVITY PUZZLE:
PERSISTENCE AND CHANGE IN PATTERNS OF PUBLICATION OF MEN AND WOMEN SCIENTISTS

Jonathan R. Cole and Harriet Zuckerman

ABSTRACT

More than 50 studies of scientists in various fields show that women publish less than men. Moreover, correlations between gender and productivity have been roughly constant since the 1920s. The existence and stability of gender differences in productivity continue to be puzzling.

Drawing **upon** data on publications by and citations to 263 matched pairs of men and women scientists ($N = 526$) who received doctorates in 1969-1970, we examine productivity and impact over the first 12 years of scientists' careers to determine whether disparities observed in prior studies persist and, if so, to what degree and why.

Aggregate gender differences in productivity in the cohort **of** 1970 are much like those in earlier ones. Women published slightly more than half (57%) as many papers as men, with that proportion decreasing somewhat as time passed. However, women now account for 26% of the most prolific scientists in the cohort (those who published at least 1.6 papers annually) as against **just** 8% in the cohort of 1957-1958 in comparable years. Since highly productive scientists contribute disproportionately to the literature and presumably to the development of scientific knowledge, the increased representation **of** women in this group is significant.

Advances in Motivation and Achievement, Volume 2, pages 217-258.

ISBN: 0-89232-288-8

Here, as earlier, women's papers had **less** impact than men's (59% as much), when impact is gauged by frequency of citation. Prior studies suggesting that women are cited **less** even when productivity is held constant are not bome out. Paper **for** paper, women are cited as often or slightly more often than men. However, since women publish less, their work has less impact in the aggregate.

Three explanations for gender differences in output and citation are examined. There is no support for the view that women publish less because they collaborate less often than men, when collaboration is measured by extent of multiple authorship. Nor are overall gender differences in citation attributable to gender differences in first authorship, Women are first authors as often as men. Last, there is support for a hypothesis attributing differentials in productivity to differential reinforcement. Women are not only **less** often reinforced than men, reinforcement being crudely indicated by citations, but they respond to it differently. Women were less apt to maintain or increase their output and more apt to reduce it than comparably cited men at the same levels of prior productivity. Reinforcement has less effect on later output of women than of men.

Although gender differences in output and impact are statistically significant, their substantive importance is not self-evident. It is not clear whether observed gender differences signal real disparities in contribution to science. Nor is it clear how productivity should be measured over the course of scientific careers. Questions are also raised about the nature of age, period, and cohort effects at the institutional and societal levels.

INTRODUCTION

"Work, Finish, Publish." It is said that a sign bearing these words hung in the laboratory of the great British physicist and chemist, Michael Faraday. **Now,** as then, this directive calls attention to three fundamental requirements of the scientific role. In this chapter, we focus on publication. The reason for doing so is clear. Science is public, not private, knowledge; publication is a necessary step in the process by which new knowledge comes to be certified by scientific peers (Merton, **1938;** Ziman, 1968). **It** should not be surprising then that sociologists and other observers of the scientific enterprise have focused considerable attention on publication practices and particularly on variations in such practices among those of differing age, field, degree of scientific achievement, social and educational origin, and sex.

More than 50 studies covering various time periods and fields of science report sex differences in published productivity, more specifically, that men publish more than women, even when age and other important social attributes are taken into account.[1] Moreover, gender differences in publication rates appear to have persisted for decades. So far, efforts to account for these differences have not been successful; their existence continues to be a puzzle.

Two classes of explanations have been examined empirically: one dealing with gender discrimination of various kinds and the other with women's greater obligations to marriage and family. The former, for example, has emphasized gender differences in access to the means of scientific production, including resources associated with high academic rank, university affiliation, grants, graduate students, and time for research, as well as differential treatment by referees and editors of scientific publications. The latter, by contrast, attributes the observed disparities to the time women devote to household and childcare at the expense of their research. Neither class of explanation has found much empirical support. Married women are as productive as single women and women with two or fewer children just as productive as those without children. (Centra, 1974; Cole, 1979; Reskin, 1978a, p. 1241). Moreover, observed disparities in productivity between the sexes have not been eliminated by taking into account variables such as rank and institutional affiliation, although such disparities are reduced when this has been done.

Research on sex differences in role performance in science is, of course, politically charged and there are some who think it cannot yet be done. For example, the Committee on the Education and Employment of Women in Science and Engineering of the National Research Council (1979, pp. xiv, 87-88) observed:

> Research productivity cannot be used yet as an overall comparative measure of male and female academic scientists' performance. In most fields in research universities, there are not yet enough women faculty who have held professional positions with the necessary perquisites long enough to make such comparisons meaningful.

Such statements confuse the existence of a phenomenon with its explanation. First, it is not the case that there are too few women scientists for quantitative comparative study. Individuals can be aggregated across universities to provide samples of sufficient size for statistical analysis. Second, even if the Committee's conclusion proves to be correct, it would be appropriate to see whether differences in rates of publication actually exist and, if so, to try to account for them in terms of the "perquisites" identified as significant. We do not think that it is premature to examine sex differences in published productivity; rather, their extent, sources, and consequences merit serious attention since studies to date have yet to provide satisfactory explanations for the observed disparities.

We therefore take up the productivity puzzle once again, this time focusing on a cohort of young scientists (those who received doctoral degrees a decade or so ago) to determine whether the sex differences reported again and again for older cohorts persist or whether signs of change toward greater sex equality can be detected. The question, of course, is whether younger women scientists who were professionally socialized in the early years of the women's movement and Affirmative Action have fared any differently from older women who began their careers under quite different circumstances. We also take up the related question of the comparative impact of publications by men and women scientists on the development of their fields. Earlier studies of impact, measured crudely by the number of citations in the scientific literature to men and women authors, seemed to show that papers by women had less impact than those by men even when disparities in productivity were taken into account (Cole, 1979; Reskin, 1978a).

More specifically, we examine the publication histories of matched samples of men and women scientists who eamed PhDs in 1969 and 1970 in order to determine:

- Whether there are gender differences in rates of publication and, if so, of what magnitude?
- If such differences are observed, are they more or less constant over the period under review (1968-1979) or do they grow or diminish **as** time passes?
- How do "profiles" of published productivity, the distribution of high and low producers, among men and women compare?
- How do pattems of productivity in this cohort compare to those observed in earlier ones?
- Are women scientists given to collaborative publication to the same extent as men? Do such proclivities change over time?
- Is there evidence here for differences in the impact of publications **by** men and women authors and, if so, are they attributable to productivity differences alone?
- To what extent can differences in the impact of papers by men and women, if such differences exist, be accounted for by differences in the extent of primary authorship? (Since citations are available only for primary or solo authors, sex differences in authorship practices may produce artifactual differences in citation counts.)
- And finally, is early recognition of scientists' work related to their subsequent rates of publication?

Before we examine evidence bearing directly on these questions, a number of historical observations are in order.

HISTORICAL CONTEXTS

For centuries women were considered unfit for intellectually demanding careers, science being no exception. Few women became scientists, and those who did encountered ideological and structural barriers to productive work. **As** late as the first decades of the twentieth century, scientists as well as the general public believed that half of the species was intellectually feeble and emotionally and physically frail.

By way of example, G. Stanley Hall, one of America's foremost psychologists, suggested (as quoted in Gould [1981, p. 1181; originally in Hall [1904, p. 194]) that the higher suicide rates of women were

> one expression of a profound psychic difference between the sexes. Woman's body and **soul** is phyletically older and more primitive, while man is more modem, variable, and **less**

> **conservative. Women are always inclined to preserve old customs and ways of thinking. Women prefer passive methods; to give themselves up to the power of elemental forces, as gravity, when they throw themselves from heights or take poison, in which methods of suicide they surpass man. . .**

Indeed, as late as 1924, Felix Frankfurter wrote in support of "protective legislation" for female workers: "Nature made men and women different. . .the law must accomodate itself to the immutable differences of Nature."'

A heavy burden to carry for the few women embarked on scientific careers. Given such public sentiment about women, it is surprising that women scientists in the early decades of this century contributed as much as the evidence indicates they did.

For a small sample of 53 American men and women who received PhDs betwen 1911 and 1913, Cole (1979, p.242) found that men were more apt to publish scientific papers in the first 5 years of their careers than women ($r = -.46$ where the coding scheme produces a negative correlation if men are more productive and a positive correlation if this is so for women). This difference between the sexes moderates somewhat as time passes; it is reduced to less than half its size at the end of 10 years ($r = -.20$) and then rises slightly after 15 years ($r = -.26$). Given women's exceedingly limited access to research facilities generally and their virtual exclusion from major professorial and research posts specifically, these differentials might well have been larger.

It is also surprising that the relationship between gender and published productivity remains so stable among cohorts entering science in the United States in successive decades, in spite of marked changes having taken place in the larger society, higher education, and the social organization of science. Again, using a coding scheme in which negative correlations signal greater productivity among men, Cole (1979, pp. 64,242)[3] reports the following correlations between gender and productivity after the first 15 years of scientists' careers:

Year of PhD	*Zero-Order Correlation Between Gender and Published Productivity*
1922	–.24
1932	–.30
1942	–.25
1952	–.27
1957/58	–.30

It appears, then, that men have published more copiously than women in each decade for which data are available. Moreover, these differences seem to hold for English and Canadian scientists as well as for Americans (Blackstone & Fulton, 1974, 1975; Endler, Rushton, & Roediger, 1978). So much then for the historical context.

There is now accumulating evidence that sex role attitudes of American men and women became more egalitarian in the 1960s and the 1970s, though not to

the same degree or at the same velocity in all domains of social life (Mason, Czajka, & Arber, 1976; Duncan & Duncan, 1978). Such changes should mean that the youngest cohort of women scientists are less hobbled than their predecessors by sex role stereotypes, their own and those held by men. To the extent that role performance is affected by such attitudes, gender differences in productivity should be reduced. Moreover, changes in sex role attitudes should also be followed by structural modifications of the sort that make it easier for young women to be productive scientists. To the extent that such structural modifications have been made, they too should reduce gender differences in productivity. What do the data on productivity show for the 1970 cohort? Is the productivity puzzle in the throes of solving itself?

THE DATA

Data were collected on productivity patterns of 263 pairs or a total of 526 men and women scientists in six scientific fields: astronomy, biochemistry, chemistry, earth sciences, mathematics, and physics.

The sample, drawn from ***American Doctoral Dissertations*** (ADD),[4] is composed of all women listed in ***ADD*** who earned degrees in 1969-1970 in five out of six fields. In chemistry, women PhDs were sufficiently numerous to permit selection of every other woman. Male "matches" were selected from among those who received degrees from the same departments in the same years. Thus the samples of men and women have the same educational origins and have received degrees in the same years in the same fields. In accord with the vast differences in the numbers of PhDs in these fields, the cases are unevenly distributed, with only 4 astronomers having been included along with 24 earth scientists, 134 chemists, 168 biochemists, 130 mathematicians, and 66 physicists.

Although this cohort is young, chronologically and professionally, it may not be young enough to show the effects of the social and cultural changes we seek. It might have been preferable to focus instead on the youngest cohort of scientists (those who earned PhDs in the 1980s) since they presumably would show these effects more strongly than older scientists. But the youngest cohort has the disadvantage for our purposes (unlike the cohort of 1970) of not being far enough along in their careers to provide sufficient data for analysis. Choosing the cohort of 1970 has an added benefit. We know from studies of scientific careers (Berelson, 1960; Cole & Cole, 1973; Crane, 1969; Gaston, 1978; Zuckerman, 1977) that scientists' standing in the stratification system is fairly well set by the end of their first decade of work. Thus what we learn about members of this cohort will not only tell us about their past but will also provide a good basis for predicting their futures—although more in terms of their career attainments than extent of individuals' publications. .

Drawing on the ***Source Index*** of the ***Science Citation Index, (SCI)*** we traced the publication histories of men and women for a 12-year span beginning in

1968 (1 year prior to their receiving their doctorates) up to 1979. These publication data are close to complete since the ***Source Index*** covers all the major and the great bulk of the minor scientific journals. In 1979 it recorded papers published **in** 2,993 scientific journals.[5] However, the ***Source Index*** does not list publication in book form. Although most sciences are, as Derek Price (1971) has observed, ''papyrocentric'' rather than "bibliocentric," given to paper rather than book publication, we cannot estimate the effects of this omission on productivity counts. Nonetheless, it is not immediately evident why one sex should publish books more often than the other. If the data are biased, then the bias should not systematically penalize or benefit men or women.

Data were collected on the total number of papers each scientist published in each of 12 years. This total was subdivided into papers published by the scientist alone and those published in collaboration with others. We also noted the number of times the scientist in our sample was the prime author in the author set.[6] All citations, year by year, to the author's cumulative publications were enumerated as they were listed in the SCI, although self-citations were excluded from the counts.' Productivity and citation counts can, of course, be aggregated over any number of years within the 12-year interval, enabling us to compare earlier and later patterns of scientific publication and citation. Data on the field and prestige rank of the doctoral departments (Roose & Anderson, 1970) from which these scientists received their degrees were also coded.

This set of data is limited. Information is not included on the social origins of the 526 scientists, their jobs, promotions, and honors. This phase of data collection is not yet complete. In fact, we do not know yet which of these scientists remained in academic life after receiving their PhDs. If unequal proportions of men and women have remained in academia, the results of this study could be biased, since academics tend to publish more than government and industrial scientists. Studies of the distribution of men and women PhDs among sectors of employment suggest, however, that sex differences in this regard exist but are not large. (Astin, 1969; Harmon, 1965). By way of example, the National Research Council reports that 67% of the women who earned PhDs in 1977 in science and engineering were employed in educational institutions of all kinds, with 61% being located at 4-year colleges and universities. Correlatively, *55%* of the men were also employed in educational institutions and 53% at 4-year colleges and universities. Not surprisingly, a smaller proportion of women held jobs in industry than men. Similar percentages of both sexes were employed in govemment, although men were more apt than women to work for the federal govemment. (Committee on the Education and Employment of Women in Science and Engineering, 1979, Table 4.1, p. 58.) If women in this sample tend more often than men to be employed in academic jobs and if academic scientists publish more than others, then in the aggregate, differences between the sexes in sector of employment should, if they have any effect at all, work in favor of rather than against women.

FINDINGS

Published Productivity of Men and Women Scientists

As the data in Table **1** show, men were more prolific than women, on average, publishing 11.2 papers compared with 6.4 for women. Thus the ratio of productivity of women to men was .57. (Comparisons of medians yields similar results: 7.6 papers for men and 3.2 for women, for a ratio of .42.) Putting aside the question of substantive import of such differences, these findings are, in a purely statistical sense, highly significant.

Gender differences are maintained when we examine rates of publication for different segments of the 12 years under review. During the first 7 years of scientists' research careers, the "tenure-relevant'' years, we find that the women publish about two-thirds (.63) as many papers as men on the average but only half (.51) as many in the next 5 years. Turning from means to medians, the female to male ratios are reduced: in the first 7 years (1968-1974) to .51 and to .30 in the next 5 years (1975-1979). There are then increasing differences in publication rates between men and women as they progress beyond the first 7 years of their careers.

It also turns out that men publish more than women, **in** five fields out of six, although there is some variability in the ratios. Astronomy, with but four cases,

Table 1
Published Productivity of 526 Men and Women Scientists: Summary Statistics

	Men *(N = 263)*	***Women*** *(N = 263)*	*Ratio (Women/Men)*
Mean Total Number of Papers (1968-1979)	11.2	6.4	.57
(S.D.)	(12.5)	(9.5)	
Median Number of Papers (1968-1979)	7.6	3.2	.42
Mean Number Early Papers (1968-1974)	5.7	3.6	.63
(S.D.)	(5.5)	(4.9)	
Mean Number Later Papers (1975-1979)	5.5	2.8	.51
(S.D.)	(8.5)	(5.7)	
Median Number Earlier Papers	4.4	2.2	.51
Median Number Later Papers	2.3	0.7	.30

is the exception. Although the female to male ratio of mean publications in mathematics is 0.46; in physics, 0.44; in chemistry, 0.48; in biochemistry, 0.68; and in earth science, 0.25; it is 5.7 for the four astronomers. In all fields but astronomy, which simply has too few cases for meaningful interpretation, the direction of the inequality in publication is consistent. There is sufficient variation in the ratios among the several fields to raise questions about whether the conditions for research productivity for women in biochemistry, for example, differ from those of women in the earth sciences, physics, or mathematics. This question cannot be answered with these data but we note that women are far more numerous in biochemistry and the biomedical sciences than in the physical sciences and mathematics and it has been suggested that women encounter less discrimination when their relative numbers in social groups increase beyond a given point (Kantor, 1977).

Despite the variability among fields, we have pooled the productivity data for the analyses that follow. When field differences depart substantially from the aggregate data, such differences are noted.

Another way of describing gender differences in research publication is the zero-order correlation of −.21 for the entire sample of 526 men and women. That is, productivity is negatively associated with feminine authorship. The strength of this association is essentially unaffected by transforming the productivity counts into logarithms ($r = -.24$), or by eliminating from consideration those scientists who had failed to publish a single scientific paper in the 12 years under examination ($r = -.19$).[8]

These gender differences in research productivity in the cohort of 1970 are consistent then with data on earlier cohorts. Significant differences between men and women persist, and their extent is similar in magnitude to those found for groups of men and women who received PhDs as far back as the 1920s.

Measures of central tendency may, of course, mask important gender differences in the distribution of scientific productivity. As Table 2 shows, the separate distributions of productivity for men and women exhibit considerable skewness. It also shows that twice as many women scientists as men failed to publish a single paper during the 12 years under review: ***22%*** against 11%. The disproportionate representation of women among "silent" scientists is again consistent with other studies of earlier cohorts of men and women (Cole & Cole, 1973). Correlatively, women are underrepresented among "prolific" scientists, those who published approximately 1.5 to 2.0 papers annually or a total of at least 16 publications for the 12-year period. Although more than a quarter of the men published this many papers, about one-tenth of the women did so. At the same time, the 70 "prolific" men (27 percent) published more papers overall than the remaining 193 men in the sample while the small proportion of "prolific" women scientists (7%) account for an even higher proportion of the total output by all women.[9]

The distributions in Figure 1 show that the most productive 15% among men

Table 2
Distribution of Published Productivity (1968-1979)
for 526 Men and Women Scientists

Total Number of Papers: 1968-1979	*Men* %	*Cum. %*	*Women* %	*Cum. %*
0	11.4	11.4	21.7	21.7
1	9.1	20.5	10.6	32.3
2-3	12.2	32.7	19.8	52.1
4-6	13.3	46.0	17.9	70.0
7-10	17.5	63.5	11.7	81.7
11-15	9.9	73.4	9.5	91.2
16-20	8.0	81.4	2.4	93.6
21-24	7.9	89.3	1.1	94.7
25 or more	11.0	100.3	5.6	100.3
	100.3		100.3	
(*N*) =	(263)		(263)	

account for almost half (49%) of all the papers published by the men in the sample, whereas the most productive 15% among women account for an even larger proportion, 57%, of **all** papers published by women. These "profiles" of productivity show slightly greater skewness for women than men; "prolific" women scientists generate a larger share of all papers published by women than their prolific counterparts among men.

When the data for men and women are combined, what proportion of the top stratum of producers (taking 15% as an arbitrary figure) is composed of women? To be included in this group, scientists must have published as many **as** 19 papers in the period being reviewed. Of this group of the prolific scientists, only 20 are women," or about one-fourth (24%).

Paired Comparison of Scientists: Productivity

Thus far, we have focused mainly on aggregated productivity patterns of men and women scientists. But since we selected these scientists using a matched sampling procedure designed to hold constant field of PhD and place of graduate education, we are also able to compare the productivity of pairs of men and women scientists. Three aspects of the paired comparisons are of interest: first, the proportions of pairs in which scientists of one sex published more than those of the other or of those in which there were no such differences; second, the mean differences in output, pair by pair; and finally, the distribution of the differences between the pairs. Table 3, which presents paired comparisons, shows that in 60% of the 263 pairs, men published more than their women "matches," and in 35% of the pairs, women published more than their men matches. **In** *5%* of the cases there were no differences."

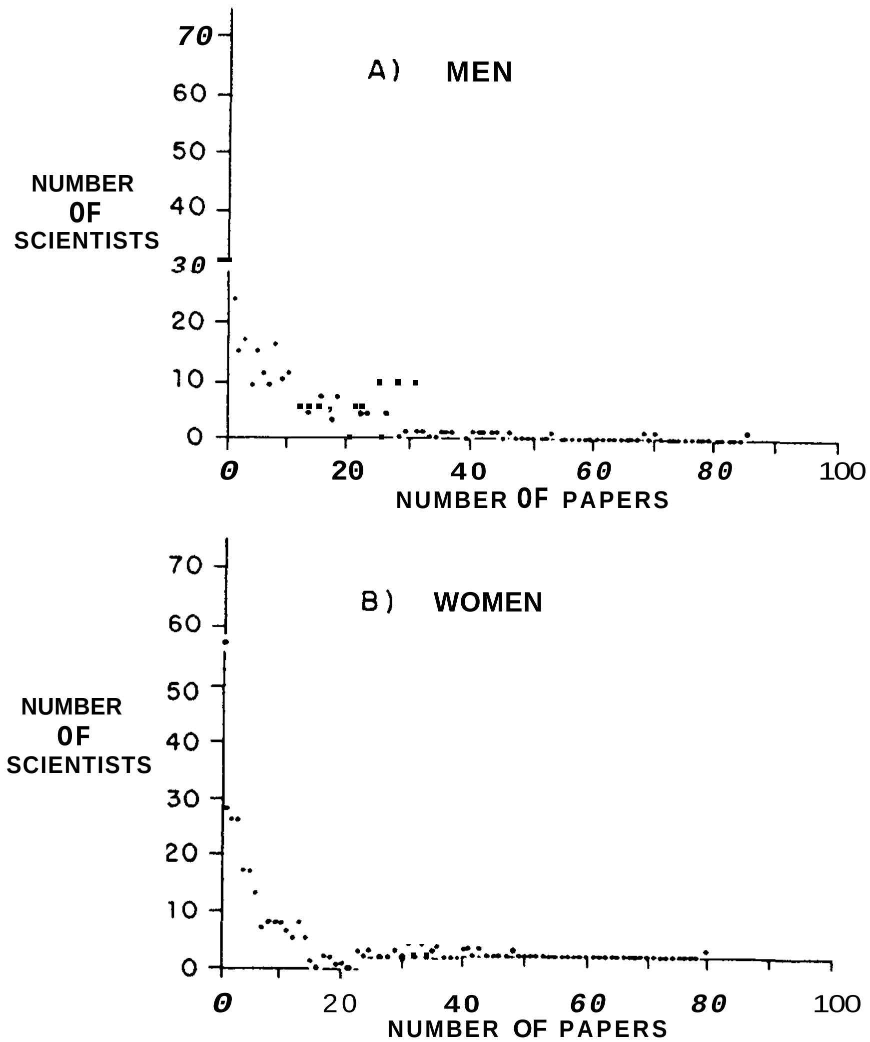


Figure 1. Distribution of total publications (1968-1979).

Table **4** shows the distribution of differences for pairs in which women outproduced men and men outproduced women. In more than half of the 91 cases in which women published more than men, the margin of difference was small (between one and five papers) but in about one-tenth of the cases, that difference was as much as **21** papers or more. When men published more than women,

Table 3

A Comparison of the Total Number of Papers Published by 263 Pairs of Men and Women Scientists, According to Differences within Pairs

Male-Female Comparison	*Percentage*	*N Pairs*
Male Published more than Female	*60.5*	(159)
Female Published more than Male	34.6	(91)
No Difference in Number of Papers Published	**4.9**	(13)
	100.0%	(263)

Total Cases: *526* Scientists

Table 4

Distribution of the Difference in Number of Papers Published by 263 Pairs of Men and Women Scientists'

	Differences in Numbers of Papers Published by Each Pair				
	1-5	*6-10*	*11-20*	*21+*	*Total*
Male published more than female	33	23	22	*22*	100% (159)
Female published more than male	*56*	*21*	12	11	100% (91)

'No difference for 13 pain
Mean difference = **4** 80 (men higher)
Median difference = 2 **46** (men higher)

they were apt to do so by a greater margin. In one-third of the 159 pairs the difference was relatively slight (one to five papers) but in as many as one-fifth of the pairs, they did so by 21 papers or more. Not only were men substantially more apt to publish more than their women matches, they were also more apt than women to do so by a considerable margin. Put another way, among pairs of men and women who received doctoral degrees from the same departments, men outpublished women 64% of the time and women outpublished men 36% of the time (excluding the 13 ties). The data that we have in hand do not allow us to account for these differences.

Change in Productivity Distributions

Although these data largely conform to those recorded for earlier cohorts of scientists, one important change can be observed: the proportion of women turning up in the most prolific stratum of scientists has greatly increased. In an earlier study of men and women who received degrees in 1957-1958, Cole (1979) found that approximately 13% of the men had published more than 20 papers during the first 12 years of their careers as against just 1.2% of the women.[12] In the recent cohort, however, prolific men and women are considerably more numerous, with 19% of the men and 7% of the women having published that many papers. Although data for just 2 time periods do not constitute a trend, this change may be significant since prolific scientists contribute far more than their proportionate share to the development of scientific knowledge.

The same findings can be reported in a different and, in this instance, preferable manner.[13] Comparing the proportions of women among "prolific" scientists in the 2 periods, we find that fully 26% of those who published as many as 20 papers in the cohort of 1970 are women as against just 8% in the 1957-1958 cohort. When the ante is raised even further to those who published 25 or more papers, the feminine presence is even more marked: one-third of these in the cohort of 1970 are women.

Although important changes may be underway, differences in output between men and women scientists persist and are not well understood. As we noted, one important class of hypotheses for gender differences focuses on discrimination against women and their limited access to the means of scientific production. With the data now in hand, we can begin to see how well one version of the discrimination hypothesis actually squares with the evidence.

Gender differences in productivity have been attributed in part to differential access to scientific collaboration and thus to shared publication. Given the considerable role of collaborative research in contemporary scientific inquiry (many problems simply cannot be worked on by a single investigator), such differences in access, if they exist, would be important not only for women's rates of publication, but also for their chance to participate in much of mainstream science. Reasons given for differential access vary. Some think that women have difficulty establishing collegial relationships with men (Kaufman, 1978; Reskin, 1978b). Others think that women have less access to appropriate collaborators because they are affiliated more often than men with small colleges and universities whose faculties are of limited size and scientific diversity. Whatever the reason, if it is so that women participate less often than men in collaborative research, then a larger share of their publications should be published under their names alone, regardless of field. What do the data actually show on the extent of solo authorship by men and women?

First, we find no differences in the average proportions of solo-authored papers published by men and women: 24% of papers by men have one author, as do 23% of those by women (after eliminating scientists who have not published at all from the computation of averages). In keeping with their greater overall output, men publish a larger absolute number of solo-authored papers than women: 3.09 papers as against 1.89 ($p < .01$). Nor do we find that women show a greater proclivity to solo authorship as time passes and they make the transition from student to mature investigator. The rates of solo authorship remain roughly the same in the later period (23% for men and 22% for women). These data are consistent with those reported by Over and Moore (1980) for psychologists in Australian universities. Although further inquiry may show that women collaborate less often than men, these data on authorship pattems provide no evidence for it.

Thus far, the data on gender differences in productivity raise a number of important questions, not all of which can be answered immediately. One set of questions deals with the character of scientific output and proper units of analysis and another with multiple consequences of observed sex differences in productivity.

Little work has been done on identifying the appropriate unit of analysis for studies of scientific productivity. Is the paper really the right item to analyze? Many scientists have told us that they publish "clusters" of papers on one or more related experiments and "strings" of papers on successive experiments. Do men and women have different propensities to produce clusters or strings of papers? Are their clusters or strings apt to be of different sizes? And if so, do gender differences in these propensities account in part for women's lower absolute number of publications? In turn, why should women exhibit different patterns of production?

All apart from matters of measurement, to what extent do observed gender differences affect individual careers and the growth of scientific knowledge? Prior studies show that differences in output are correlated with differences in rewards allocated to men and women scientists (Cole, 1979; Reskin, 1978a). How important should it be that in this sample men publish 0.9 papers annually as compared to 0.5 for women (11 papers in 12 years as against 6)? To be sure, the normative framework of science requires that rewards be in rough accord with individual contributions to science (Merton, 1942/1973) but it is not clear how differences in output at this level of magnitude (5 papers on the average in 12 years) should translate into differences in scientific contribution. Although these gender differences in productivity are statistically significant, it is not self-evident whether such differences should or should not be significant in allocating symbolic or material scientific rewards. Shifting our focus from individuals to the population of men and women scientists, are such differences consequential for the growth of scientific knowledge? Perhaps so, perhaps not. Many scientists complain that too many papers are published, that many are written to satisfy officers of granting agencies or are "requirements" for promotion and salary raises, regardless of their real contributions to the advancement of knowledge. It may be that men contribute more to cluttering the literature than women. And it may be that women publish a higher density of papers that are useful to their scientist colleagues. And it may be that there is a threshold of productivity that is required before an individual's work can become known and thus be useful. The fact is that we know little about matters of this kind.

We now tum to an analysis of citations to the work of the 526 scientists. This should give us some sense of the comparative impact of the publications of men and women on their fields.

Patterns of Citation to Scientists in the 1970 Cohort

Citation countsregister the "impact" or "influence" of scientific publications, as we noted earlier. This is so because recognizing the cognitive contributions of others in footnotes or references is well-established practice in science. Thus the number of times a particular paper has been cited is a rough indicator of the number of different occasions on which other authors have taken note of it (Cole &Cole, 1971; Garfield, 1979, especially chap. 10). Citation counts for individual authors are, as we have said, significantly related to a variety of forms of scientific recognition such as prizes and awards, as well as to independent peer assessments of the significance of scientists' contributions. In fact, citation counts are a better predictor of influence or impact of contributions by individual scientists as they are measured by awards than are publication counts.

Output (measured by publication counts) and impact (measured by citation counts) have been shown to be highly correlated, by and large from .5 to .75 depending on the sample being studied. (Allison & Stewart, 1974; Cole & Cole, 1973; Gaston, 1978; Hagstrom, 1971; Long, 1978). The more scientists have published, the more apt they are to be cited by others. This is as much the case for this sample of young scientists as for earlier ones where the correlation between publication and citation counts runs to .62 ($p < .001$).

The earlier studies have also shown that impact and output are not the same, that impact is not merely a function of output. Scientists who publish a great deal also tend to publish particular papers which are influential in and of themselves (Garfield, 1981). The same pattem turns up in the cohort of 1970. If we **look** only at each scientist's most cited paper—the one receiving the most attention from other scientists (here, in 1979)—the more prolific the scientists are, the more citations their most-cited paper receives ($r = .59$). If we aggregate the citations to scientists' two or three most-cited papers, the outcome is much the same ($r = .62$ and .63, respectively). Being highly cited therefore is not just the outcome of copious output, but is also associated with having particular papers in print which have received a great deal of attention.

Since we know that women have published less than men, we would expect that they are also less often cited. Earlier studies report, however, that the association between gender and citations is modest and ranges from .1 to .3 in various fields and for various samples even though men are consistently shown

to be more often cited than women. For example, Cole (1979, Table *3-3*, p. *64)* found a zero-order correlation of −.19 between total citations and gender for the group of 1957-1958 PhDs he studied, although the figure varied somewhat among disciplines ($r = -.33$ for 61 chemists; −.21 for 297 biologists; −.22 for 159 psychologists but was .21 for 44 sociologists). Cole also reports a correlation of −.14 between gender and citations for a sample of 248 biologists of various ages. His data are consistent with those reported by Reskin (1978a, **p.** 1240) who also finds differences in the extent of citation to the work of men and women chemists.

Do these small but significant gender differences in impact also appear in the cohort of 1970? They do, but not to the same degree. Again, using a coding convention in which negative correlations indicate that men have been cited more often than women, we find that the correlation between gender and total citations for the 12-year period is −.11 ($p < .01$),[14] somewhat smaller than the correlations reported in earlier studies.

These correlations cannot, of course, convey anything about orders of magnitude differences in citations between the sexes. As Table 5 shows, women average 32.3 citations over the 12-year period and men, 55.1, or a ratio (w/m) of 0.59. But women in the cohort of 1970 do relatively less well than men as time passes; in the first 7 years, they received 0.66 as many citations as men

Table 5
Citations to 526 Men and Women Scientists: Summary Data

	Men (*N* = 263)	*Women* (*N* = 263)	*Ratio* (*Female/Male*)
Mean Total Number of Citations (1968-1979)	55.14	32.27	.59
(s.d.)	(135.4)	(69.8)	
Median Number of Citations (1968-1979)	13.67	5.40	.40
Mean Number: Early Citations (1968-1974)	16.51	10.86	.66
(s.d.)	(50.9)	(26.6)	
Mean Number: Later Citations (1975-1979)	38.63	21.40	.55
(s.d.)	(90.9)	(48.3)	
Median Number: Early Citations	3.61	1.34	.37
Median Number: Later Citations	7.42	2.41	.33

but only 0.55 as many in the next 5 years.'' We shall have more to say later about increasing disparities in citations to men and women and what consequences this pattem might have.

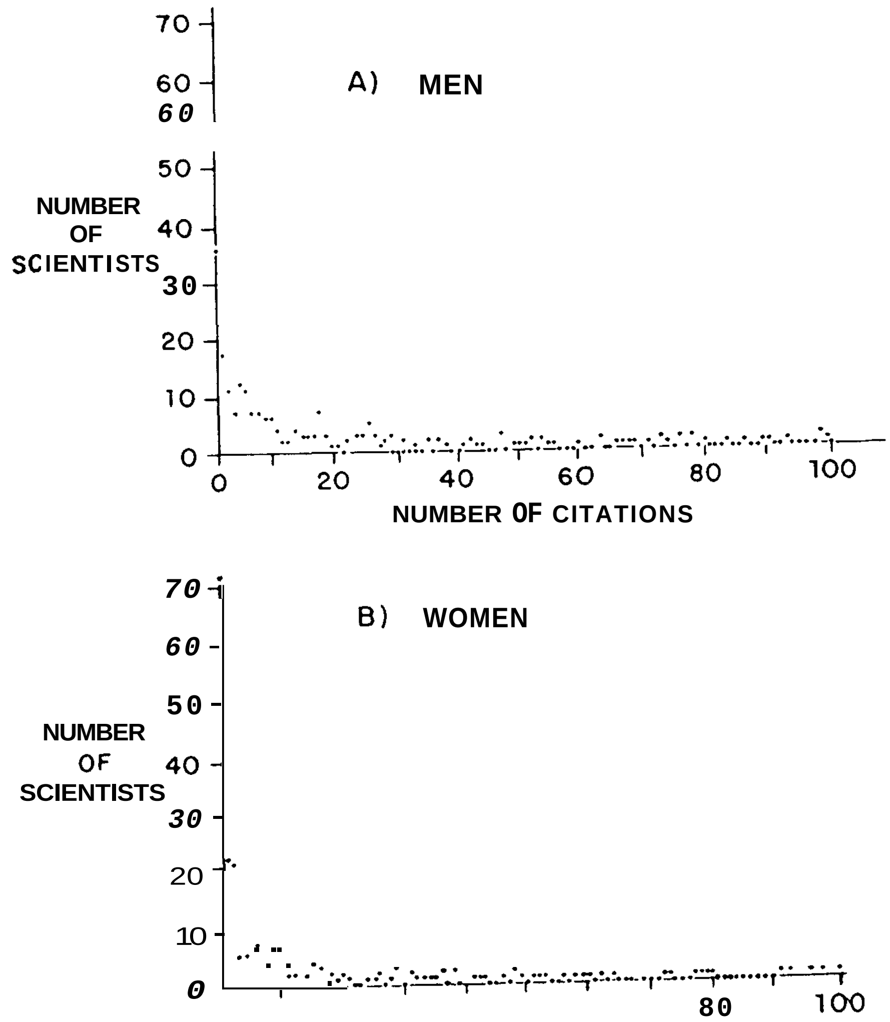
As we found in the case of output, there is considerable inequality in citations within each sex. Some women are very heavily cited, as are some men, and these scientists contribute disproportionately to the cumulative numbers of citations earned by those of the same sex. Table 6 and Figure 2 show that the distributions for men and women are sharply skewed (Gini coefficient = .76 for women and .70 for men). Thus women are no more like one another than are men when it comes to the distribution of citations; a few in each group receive the lions' share.

We noted that differences in cumulative rates of citation for men and women are statistically significant. But are they "real" or an artifact of sex differences in authorship practices and of procedures used in the ***Science Citation Index*** for recording citations? Since the ***Science Citation Index*** was first published, citations have been enumerated only for solo-authored papers and those on which a given author was first in the author set. This convention means that citation counts are underestimated for the subset of authors who collaborate more than usual and whose names tend not to be first in the author sets.

Before we proceed to any further analysis of gender differences in citations, we need to know whether they simply reflect differences between men and women scientists in authorship practices. We have already seen that women are no more given to solo authorship than men. But some have claimed, for example, that women are less often first authors on collaborative papers because their contributions are taken less seriously than those by men (Epstein, 1970). It has also been suggested that women are less often heads of laboratories and therefore less often in the position to insist on primary authorship (Reskin, 1978b). What

Table 6
Distribution of Total Citations 1968-1979 to 526 Men and Women Scientists

Total Number of Citations	*Men* %	*Cum. %*	*Women* %	*Cum. %*
0	13.3	13.3	27.4	27.4
1-5	22.0	35.2	22.8	50.2
6-10	11.4	46.7	11.4	61.6
11-20	11.0	57.7	9.5	71.1
21-50	15.2	72.9	11.0	82.1
51-100	11.8	84.7	8.4	90.5
101+	15.2	99.9	9.5	100.0
	100%		100%	
(*N*) =	(263)		(263)	



do the data show on such authorship practices of men and women in the cohort 1970?

When it comes to rates of primary authorship, we find no differences at all between men and women. On the average, men and women take **first** position in author sets on **43%** of their multi-authored papers. Nor are there any detectable differences in first authorship in the early and later periods, although both men and women show a decreasing tendency to be first authors as they mature, a finding consistent with earlier studies of authorship practices (Long, McGinnis, & Allison, 1980; Zuckerman, 1968).[16] And finally, when rates of first authorship and solo authorship are combined, we also find no gender differences which might account for differences in citation rates, since men occupy one or the other of these positions on 66% of their papers and women on 59%. For the cohort of 1970, then, gender differences in citation rates do not appear to be an artifact of authorship practices of men and women.

So far, the data on citations indicate that publications by men have more impact than those by women. To what extent are these differences a function of differences in output that we described earlier? Are publications by men cited more often simply because there are more of them to cite? The answer to this question is more complicated than it might at first appear.

One way to assess the influence of output on impact for men and women is to compare their average numbers of citations. Although men are cited more often overall, it tums out that there are no gender differences in average citations per paper. Women's papers averaged 5.02 citations and men's, 4.92 (p = n.s.). Paper for paper, then, women's publications are just as influential as those by men." Moreover, this seems to be true in both the early and later periods. In the first 7 years of their careers, women earned an average of 3.02 citations for each paper and men, 2.89. In the next 5 years, citations per paper increased for both groups to 7.6 for women and 7.05 for men. In short, it appears that men are cited more often largely because they publish more."

Even so, it is still possible that we have not detected differences in impact between men and women who publish at different rates. The most productive women may be cited far more often than comparably productive men while less productive men are cited more than equally unproductive women. **As** Figure 3 shows, average citations per author are much the same for men and women at each level of productivity, with the women authors having a slight edge in each group. (But it should be noted that there are more women in the less productive group and only a third as many women in the highly productive group.) Again, it seems appropriate to conclude that gender differences in impact are largely a function of gender differences in output.

The picture becomes somewhat more complicated, however, when we look at those papers by men and women which have had the most impact, that is, have been most often cited. Taking citations in 1979 as an indicator of citations in other years (year-by-year correlations of citations are high for this cohort as for others), we find that most-cited papers by men have been cited more on the average than those by women, with papers by men averaging **3.7** citations and those by women, 2.7 citations, although the medians show somewhat smaller gender differences (1.2 vs. 0.7 citations). Combining citations to each scientist's two or three most-cited papers does not change the findings to any significant

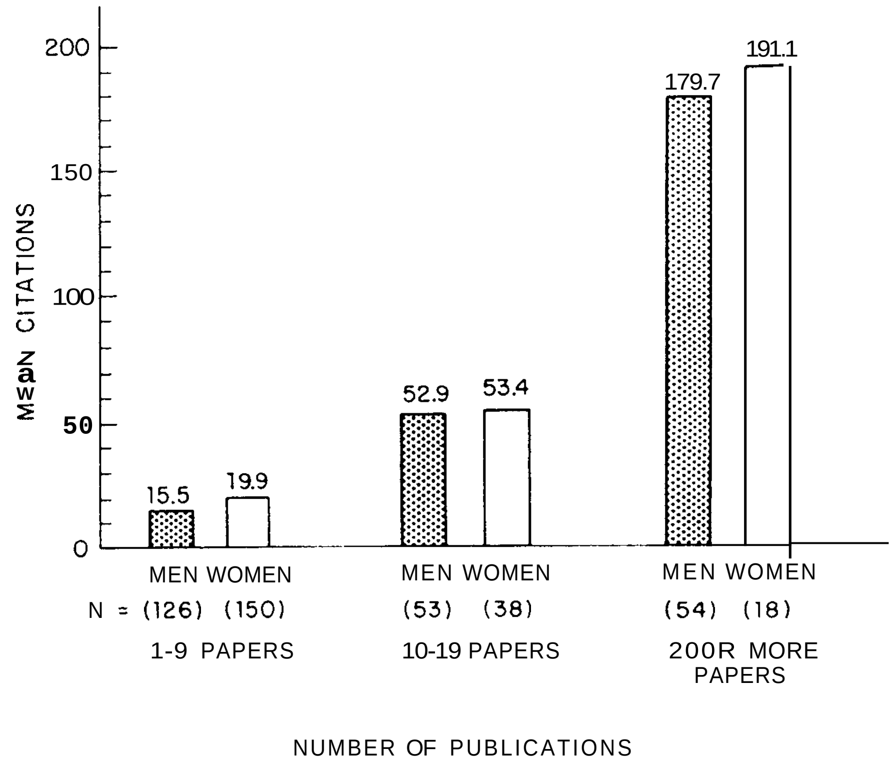


Figure 3. Mean number of citations to men and women authors, according to their productivity (1968-1979).

degree. Why should it be that women's papers on the average are cited **as** often as those by men but their most-cited papers are cited less often?

The answer seems to lie in the distribution of output and citations for men and women, as Figure **4** shows. Scientists who are highly productive are highly cited, not only because they have published a great deal, but also because some of their papers are themselves highly cited. This is as much the case for women as for men. The most-cited papers of highly productive women scientists are cited at about the same rate as such papers by equally productive men, 10.8 as against 10.4 times. At the same time, there are three times as many men (54) as women (18) in the highly productive group of scientists and it is this which produces the higher overall average citations for most-cited papers by men.

On the average, then, papers by women are cited as often as those by men.

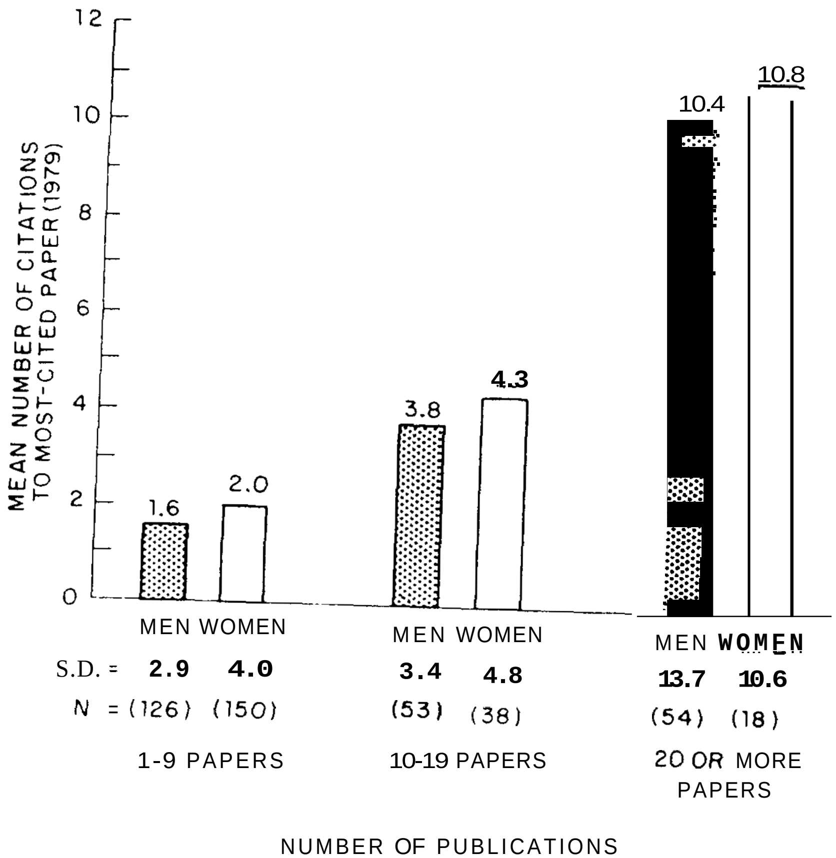


Figure 4. Mean number of citations to the most-cited papers of men and women authors (*1979*), according to their productivity (*1968-*1979).

There is no indication of gender differences in impact when we examine average publications. Another conclusion to be drawn that is just as warranted is that men publish more and as a consequence are cited more in the aggregate. Later on, we shall have more to say about how gender differences and gender similarities may be interpreted.

Paired Comparison of Scientists: Citations

We turn now to patterns of citations to pairs of men and women. **As** we observed earlier, comparison of pairs allows us to see how scientists trained in

the same departments fare when it comes to output and citations to their work. Table 7 presents data on the proportions of the 263 pairs appearing in each of three categories: those in which the male partner in the pair was cited more often, those in which the female partner was cited more often, and those in which there were no gender differences observed in citation.

The first column in Table 7 shows that men were almost twice as apt as their women counterparts to have been more often cited (61% vs. 32%). However, as column 2 shows, when the average number of citations per paper is considered for each pair, the differences between men and women are substantially reduced. In 52% of the pairs, men had the higher average per paper as against **41%** of the pairs in which the women's average was higher.

Turning from averages to the distribution of differences in citations per paper in Table 8, we see that men and women look much the same when the ***margin*** of difference is considered. That is, when men are more often cited, the extent to which their citations exceed those of women is no greater than the margin of difference observed when women lead in citations. If anything, the margin of difference is slightly greater in those cases where women have been cited more often.

One more comparison of citations to pairs of men and women merits comment. We wanted to see whether pairs of men and women differed when we compared citations to their most cited papers in 1979. Column 3 of Table 7 shows that in 44% of the pairs the men were more often cited as against 32% of the pairs in which women were more often cited. This suggests that the gender differences we have observed for overall citation are somewhat reduced when **we** consider only the most heavily cited papers but that men do better on this criterion also.[19]

Citation Counts and Their Significance

Before considering changes in output for men and women over the course of time, some comments are in order on citation counts and the meaning of differences in citations among groups of scientists.

The data we have reported so far, which detail differences between men and women in the extent that their work is cited, do not lend themselves to unambiguous interpretation. For one thing, there is no agreement among sociologists and historians of science on the substantive significance of unequal rates of citation. We can say that citation counts are strongly correlated with scientists' reputation and the prestige of their honorific awards (Cole & Cole, 1973; Gaston, 1973; Zuckennan, 1977). However, we cannot say whether the gender differences in citations that we have described indicate that research by men really has greater impact than research by women. It is not at all clear that scientists whose **work** is cited an average of 55 times over 12 years (the figure **for** men in the cohort of 1970) have had substantially greater impact than those whose work is cited an average of 32 times (the figure for women). To make the point

Table 7
Comparisons of Citations (1968-1979) to Pairs of Men and Women Scientists Who Received Doctoral Degrees from the Same Departments

Result of Paired Comparison	*Total Citations 1968-1979* %	(N)	*Citations Per Paper* %	(N)	*Single Most-Cited Paper* %	(N)
Men More Cited than Women	60.8	(160)	52.5	(138)	44.5	(117)
Women More Cited than Men	31.6	(83)	40.7	(107)	31.6	(83)
No Gender Difference	7.6	(20)	6.8	(18)	24.0	(63)
	100.0	(263)	100.0	(263)	100.0	(263)
Mean Difference:	-22.87		.10		1.00	
Median Difference:	-8.27		.20		[illegible]	

Table 8
Comparison of Citations (1968-1979) per Published Paper for Pairs of Men and Women Scientists According to the Extent of Differences between Pairs

	Extent of Difference in Per-Paper Citations				
Result of Paired Comparison	*.01-1.50*	***1.51-5.0***	*5.01-10.0*	***10.1+***	***Total***
Men More Cited than Women	38 4	34 1	15 9	11 6	100% (138)
Women More Cited	31.4	29.9	17.8	15.0	**100%** (107)
No Gender Difference = 18 Pairs					

more generally, not enough is known about the connections between citation counts and contributions to scientific knowledge or about the comparative impact of work by scientists who are cited *n* times as against those who are cited a fraction of *n* times. We can say how both groups compare to the average cited scientist in the *SCI*[20] but more information is needed than numbers of citations to assess comparative contributions to the development of scientific knowledge. In short, we report gender differences in citation counts but underscore our conviction that the connections between such counts and the extent of contributions to knowledge have not been satisfactorily established.

Another set of questions about the interpretation of citation counts deals with their distribution among the set of papers published by each scientist. Have two scientists who have each been cited 50 times in a given year had the same impact when one has published 25 papers, each of which has been cited twice, and the other has also published 25 papers but has two papers which received 25 citations each, with the rest having received none at all? How are these scientists' patterns to be compared to others who also have received 50 citations but who have published only 5 or 10 papers? Is the impact of the work published by all these scientists the same? We do not know. Preliminary evidence for physicists, collected almost 15 years ago, suggests that those who were highly cited but who had a relatively modest output (a group dubbed "perfectionists") tended to have received greater formal recognition than scientists who had approximately the same number of citations but who had published more papers (these dubbed, "prolific" scientists) (Cole & Cole, 1973). But the detailed relationships between the distribution of citations to scientists' publications, patterns of the relation between number of citations and quantity of published work, and appraisals of impact or influence of work remains substantially uncharted territory. This is still another reason why it is difficult to assess the precise meaning of the gender differences in citations that we have described.

And finally, we note that the attribution of citations to men and women authors contains its own ambiguity. Earlier, we reported apropos solo- and multi-authorship that 76% of the papers published by men in the cohort of 1970 and 77% of the papers published by women on the average were collaboratively authored. But in the absence of detailed information on the sex composition of sets of authors, we cannot say precisely how often papers "by men" and "by women" are cited. On the basis of their incidence in the population of scientists, men are probably more apt to work with men than they are with women. There are simply too few women scientists to make incidence of all-female and cross-sex collaborations as frequent as all-male collaborations. Numbers aside, it is still not clear how best to count citations when the sex composition of collaborative groups varies. At the minimum, classification of papers by the sex composition of the author set is needed to permit comparison of citation counts to subsets of authors.

Reinforcement of Published Output: Its Effect on Men and Women Scientists

Last, we take up the evidence bearing on an hypothesis which may account in part for gender differences in rates of publication. If women receive less response to their research contributions than men (are less often reinforced), they may become discouraged and publish less as time passes. If the hypothesis of differential reinforcement is correct, then we would expect to find declining rates of publication more common among women than men. This is precisely what we find. A much larger proportion of women published fewer papers in the second 6 years of their careers than did men. In fact, 50% of the women who had published almost a paper a year (five papers or more) in the **first** 6 years dropped below that number in the second 6 years in contrast to just 31% of the men. Correlatively, a much smaller proportion of women than men increased the pace of their output: 12% of the women did so as against **24%** of the men." Moreover, when productivity of men and women is compared over time, we found, as noted earlier, that disparities between the men and women increased. In the early period women published 63% as many papers as men and, in the later period, 51% as many. So far, then, the data are consistent with the hypothesis of differential reinforcement.

Lacking better indicators of reinforcement, such as appointments in prestigious departments, early promotion, research grants, honorific awards, and other symbols of the good opinion of colleagues, we use citations to scientists' research as an approximation. **As** we noted earlier, citations register the impact of a scientist's work or how much attention it receives. This is the case because poor or uninteresting research tends to sink without a trace; other scientists do not cite it. By contrast, research which provokes others sufficiently to try to disprove

it does have impact and is cited. In this sense, other scientists pay attention to the work even if they are not sure it is correct.

Plainly, we do not know for sure whether citations provide incentives for further work, whether scientists are aware of the extent to which they are cited, and if so, how they experience citations by different classes of scientists. One citation from an important reference individual may be more sustaining than numerous citations from the rank and file. In the absence of fine-grained data on linkages between types of citations and reinforcement of scientific work, we use citation counts with caution.

We have already seen that women are less often cited in the aggregate than men and also that the extent to which this is so increases over time. **As** we noted, women received 66% as many citations as men in the early period but only 55% as many later on. These data then are also consistent with the hypothesis of differential reinforcement.

Prior studies have shown that early recognition, as indicated by use through citations, is positively related to later productivity. Scientists whose early publications evoke no response (are uncited), are less apt to continue to do the difficult work required for publication than those whose papers are recognized by citations (Cole & Cole, 1973, pp. 110-14; Reskin, 1978a, p. 1240). As it turns out, the data for the cohort of 1970 provides further support for this hypothesis.

Table 9 presents data on early publication and recognition and their relation to subsequent publication. First, note that rates of publication for the majority of scientists are quite stable during the two periods. The last row of Table 9 shows that 62% of those who had published as many as five papers in the early

Table 9
The Effects of Early Recognition on Later Productivity Among Scientists According to Levels of Early Productivity

	Percent Having Published 5 or More Papers in Later Period (1974-1979)		
	Early Productivity: 1968-1973		
Early Recognition: Citations: 1968-1973	*0-4 Papers*	*5 or More Papers*	*(N)*
0	9	46	
	(185)	(13)	(198)
1-5	25	58	
	(114)	(55)	(169)
6 or more	29	66	
	(55)	(104)	(159)
Total (%)	17	62	
(*N*)	(354)	(172)	(526)

years of their careers continued to publish that many later on. Similarly, it can be inferred from the same row that 83% (100% – 17%) of scientists who had published fewer than five papers also continued at the same slower pace. Among those scientists whose publication rate changes, we find that reduction, not increase, is the more common pattern.[22]

The columns of Table 9 show the effect of differing degrees of early citation on scientists who had been differentially productive in the early period. It is evident from data in both columns that the more cited scientists have been, the more likely they are to move into or to remain in the prolific category. As many as 29% of those who had published four or fewer papers in the early period and who were cited comparatively often increased their published output, but just 9% of those who were uncited and unrecognized published more than they had earlier. Similarly, as many as two-thirds (66%) of those who had both been prolific and were cited comparatively often in the early period continued to be prolific in contrast to fewer than half (46%) of those who were equally as productive but who had not been cited at all.

Does recognition affect men and women scientists in the same way? The data in Table 10 suggest that women scientists in the 1970 cohort are ***slightly more*** responsive than men to the lack of reinforcement and ***considerably less*** responsive than men to positive reinforcement. Put in a different way, women scientists seem more readily discouraged and less readily encouraged by varying degrees of citation to their work. The first row of Table 10 shows the effect of silence

Table 10
The Effects of Early Recognition on the Later Productivity of Men and Women According to Levels of Early Productivity

	Percent Having Published 5 or More Papers in Later Period (1974-1979)				
	Early Productiviry: 1968-1973				
Early Recognition: Citations: 1968-1973	***0-4 papers***		***5 or more papers***		***(N)***
	Men (1)	***Women*** (2)	***Men*** (3)	***Women*** (4)	
0	11 (75)	7 (110)	50 (6)	43 (7)	(198
1-5	36 (53)	16 (61)	64 (39)	44 (16)	(169
6 or more	39 (23)	22 (32)	73 (67)	54 (37)	(159
(N)	(151)	(203)	(112)	(60)	(526

or the absence of citation. Among those men and women who published a small number of papers in the first 6 years, women were slightly less apt than men to increase their rate of productivity in subsequent years, and among those who start out at a reasonably fast pace (columns 3 and 4), women are slightly more apt than men to reduce their rate of publication.

A modest reinforcement (1-5 citations) has a more pronounced effect on subsequent publication by men than by women. This level of encouragement is more often followed by greater research activity among men than women. Indeed, 36% of the slow-starting men compared with only 16% of comparable women later increase their rate of publication after receiving modest reinforcement in the first 6 years of their careers.

Such modest reinforcement also appears to have a slightly more stabilizing effect on men than women. Among men and women who began by publishing five or more papers and whose work was modestly reinforced, 64% of the men compared with 44% of the women maintained that pace of publication. Among the comparatively prolific scientists who were also significantly reinforced in the early period (with six or more citations), 73% of the men compared with 54% of the women continued to publish at their earlier rate. Turning the data around, we see that almost half of the women who start off being comparatively productive fall back to a slower pace, despite the reinforcement received through citations, in comparison to slightly more than a quarter of comparable men.

These findings on differential responsiveness of men and women to varying degrees of reinforcement suggest that women may need more encouragement than men to maintain the level of publication they set for themselves earlier in their careers. These aggregated data are inconsistent with Reskin's (1978a, p. 1242) findings for an earlier cohort of chemists. She reports that women are ***more*** responsive than men to formal recognition in the form of citations although the models she uses are somewhat different from those presented here. When we examine this reinforcement pattern for chemists alone, we find results that are more consistent with Reskin's. This suggests that there may be significant differences in the effects of reinforcement in the several scientific disciplines. In attempting to predict later productivity, we may find substantial interaction between reinforcement through citations, scientific field, and gender. It is not clear at this juncture why this should be so nor is it clear whether the same pattern of response to recognition would be observed if we were in a position to examine the effects, for example, of recognition in the form of appointments, research support, or honorific awards. We do not yet have these data in hand. But we do know from extended interviews with scientists that many are aware of whether, when, and how others are making use of their work even if they do not know how many times they have been cited.

What then can we say about the hypothesis of differential reinforcement and its possible contribution to growing disparities in publication between men and women? First, the data are consistent with the hypothesis. Women are reinforced less than men, to the extent that citations are indeed a reasonable measure of reinforcement. Moreover, women appear to respond differently to the reinforcement they receive. At similar levels of reinforcement, women are less apt than men to maintain or increase their rate of publication.

The hypothesis of differential reinforcement only goes part of the way, of course, in accounting for increasing gender differences in publication. This social psychological explanation is consistent with the sociological explanation which emphasizes women's unequal access to the means of scientific production. There is ample evidence that women achieve high academic rank (and its perquisites) later than men (Zuckerman & Cole, 1975) and also that they are less apt to be promoted than comparably qualified men (Astin & Bayer, 1975; Bayer & Astin, 1975; Cole, 1979, p. 246). Since a variety of resources for research are associated with high academic rank, the difficulties and delays women encounter in achieving it may well contribute to their falling farther behind men in publication in the post-tenure years. When data are in hand on the career histories of the 1970 cohort, we should be able to say more about the relative contributions of structural impediments and differential reinforcements to disparities in publication between men and women scientists.

SUMMARY AND DISCUSSION

By now it should be clear that social change has not eliminated gender differences in productivity and thereby solved the productivity puzzle. Disparities in publication between men and women scientists in the 1970 cohort are about as large as those observed in earlier cohorts. The data for the 526 men and women indicate that men publish almost twice the number of papers as women in the first **12** years of their careers. The correlations between gender and productivity (about $-.2$ to $-.3$) have remained fairly stable since the first decades of this century. Moreover, the findings for the cohort of 1970 are consistent with those reported in scores of other studies.

At the same time, these data on recent PhDs suggest that some important changes may be under way. We have found an increased proportion of women among the most prolific scientists (those who publish about two papers annually) and who contribute a hefty share of all publications in science. Thus an increasingly large subset of women scientists now resemble, for good or ill, those men who start publishing early and do so copiously year after year.

Citation differences between men and women seem to be largely a function of their different rates of publications. When average citations per paper are compared for men and women, we find no difference. This means, of course, that the papers published **by** women on the average have as much impact as those by men, but overall, owing to the weight of numbers **of** men's publications, women's **work** has less impact.

In some sense, these findings raise as many new problems about the character

of scientific productivity as they resolve. How important for individual reputations and for the development of scientific knowledge are strings and clusters of papers? What is the threshold of output required for a scientific contribution to become visible? Do thresholds differ among fields and historical periods as well as with the contributor's age and prior achievements? At what point do scientist's publications become overabundant, mere clutter in the literature? [23] And finally, since the per-unit effect of papers by men and women does not differ, sheer output may take on added significance in assessing their relative contributions.

Before we can be sure that differences in cumulative citations to men and women are really a function of the size of their respective bibilographies, additional data are needed. We need to know, for example, whether men and women are represented among authors of the most-cited papers in each field in proportion to their representation in those fields. Although a number of the most-cited papers are technical or instrumental rather than substantive contributions, most scientists would agree that they are important papers which have been useful and have had considerable impact. It would be helpful to have such data because the measure we use here, the average count per paper, provides no clues as to the distributions of citations to specific papers or subsets of them.[24] If women are represented among the authors of most-cited papers in proportion to their approximate numbers in each field or specialty, then there will be good reason to conclude that differences in citation levels are mainly a function of differences in output. If, however, women are underrepresented among authors of most-cited papers, we will have to be more tentative about this conclusion.

The data presented here allow us to discard for the moment several explanations for the productivity differentials. Women are just as apt as men to publish alone and, correlatively, they are just as apt as men to publish collaboratively. Differences in output are not a simple function of lack of access of women to collaboration. However, these data do not show whether men and women participate in collaborative groups of the same size and duration nor do they say anything about the relative ranks of male and female co-workers. We do not know whether men and women are equally apt to collaborate with status peers and with status unequals and what effect such relationships between co-workers have on published output.

We do know that women are as apt as men to be first authors on their publications and this dispels the notion some have that women have fewer citations than men because they are less often first in author sets. But we still do not know whether women are less apt than men to be accorded first authorship on papers judged by the authors to be important contributions[25] and, conversely, more often give "token" first authorship on papers judged to be routine.

The data presented here also suggest that women are less likely than men to translate high early productivity into high later productivity when they have experienced early positive reinforcement in the form of frequent citation. The regression analyses presented in Tables 11A and 11B summarize these results. In Table 11**A**, where men and women are aggregated in one group, we find that the three independent variables—early productivity, gender, and early citations—each have significant independent effects on later productivity. Early productivity is, of course, the strongest predictor of later productivity, but both gender and reinforcement are "independent" predictors of later productivity. At the same time, gender has less effect than early reinforcement in the OLS (Ordinary Least Squares) regression equation.

Table 11B presents the effects of early productivity and early citation on later productivity for men and women separately. Here we find a substantial reinforcement effect for men, but no reinforcement effect for women. The regression coefficient in standard form for early citations is beta = .19 for men and beta = .02 for women. These regression results are, as we would expect, consistent with the results reported earlier in the paper.

What we do not know yet is why the early positive feedback has different effects for the men and women. Reskin (1978a) has suggested that early reinforcement is more effective for women than for men. Yet these results in the

Table 11A
OLS Regression of Later Published Productivity (1975-1979) on Early Productivity, Early Citations ("Reinforcement") and Gender (N = 526)

Independent Variables:	*B*	β	*F-ratio**
Gender	**–1.65**	-.10	**1.32**
Total productivity:T_1	.86	**.46**	**134.10**
Total citations: T,	**.04**	.15	**14.23**
(Constant)	**3.63**		
R = .57			
R = .32			

*f > 3.78 = p< .01

Table 11B
OLS Regression of Later Productivity on Early Productivity, Early Citations ("Reinforcement") for Men and Women as Separate Groups

	Men (N = 263)			*Women (N = 263)*		
Independent Variables:	*B*	β	*F-Ratio**	*B*	;β	*F-Ratio**
Total productivityT_1	**.96**	**.46**	12.32	**.86**	.56	**55.79**
Total citations:T_1	.05	.19	**12.68**	.01	.02	.07
(Constant)	**1.44**			**.62**		
R = .54				R = .58		
R^2 = .29				R' = .33		

aggregate indicate the contrary; while reinforcement may be more important for women, it is apparently not translated into high continued output in some scientific fields.

Finally we must emphasize that while the difference between the sexes in research output is both significant and puzzling, gender **per** se does not explain much of the variance in published productivity or citations. Variability between the sexes in productivity is not nearly as great as variability within each sex.

Although we have shown that differences in the research productivity persist among young men and women scientists, we have not explained them. The data necessary for an adequate explanation of the phenomenon will come from improved quantitative data, which expands the types of "explanatory" variables employed, and from increased attention to qualitative data including detailed accounts by scientists of the standards they subscribe to, the decisions they have made, and the constraints they have encountered which affect publication. We are now collecting these types of data.

It is premature now to speculate on how the productivity puzzle will be solved but several points should be made to help redefine the pieces of the puzzle in such a way as to make its solution more likely.

First, structural determinants of differing rates of output have not received sufficient attention. As many working scientists know, the social structure and composition of research laboratories (the relative numbers of senior researchers, postdoctoral fellows, and graduate students as well as the status differences within these groups) affect group and individual productivity.[26] Scientists who must work alone or with undergraduates are not only limited with respect to the sorts of problems they can tackle, but also in the numbers of papers they can produce.

Second, it is often assumed that high rates of productivity are associated with long hours spent at the bench. It appears from the interviews we are now conducting with scientists that this is misleading and that roles in the laboratory change markedly with upward mobility and over the course of the scientific career. Although senior scientists may have begun by spending many hours at the bench and in the lab, their responsibilities for and involvement in the finished product usually changes. Authorship for this group is almost always collaborative and no longer derives from their having "done the research" but instead from having set the problem for those who actually carry out the experiments, from raising the funds to keep the laboratory going and to support those at the bench, from discussion of results, and from helping to draft the papers submitted for publication. These patterns are apt to be conditioned by the character of research, by discipline or specialty, and by organizational context, but whatever form they take, scientists' involvement in research and participation in publication changes in ways that affect rates of publication but are not visible in straight counts of papers.

Third, publication counts alone provide no clues as to varying norms regarding publication in different fields and specialties and, more importantly, for different statuses and organizational settings. In some settings there are strong normative obligations to publish (and incentives provided for doing so), and in others there are almost as strong prohibitions (and probably some disincentives). Correlatively, certain statuses carry with them the obligation to publish and to help others to publish, principally students and postdoctoral fellows. Little attention has been paid to the way in which normative prescriptions and proscriptions contribute to scientists' output and these need to be understood better than they are now to account for differentials in output between men and women.

Clearly, considerable empirical work is needed on the fine structure of scientific productivity before persistence of gender differences in output in successive cohorts can be understood fully. Equally clearly, the theoretical contexts and implications of the productivity puzzle need further development. The questions we have investigated here are connected to the growing body of work on age stratification and the interplay of age, period, and cohort affects in social life (Riley, Johnson, & Foner, 1972; Riley, 1980; Zuckerman & Merton, 1972).

We sought to identify changes in the role performance of women in this cohort of young scientists because we assumed that they, more than women in earlier cohorts, would be affected by widespread cultural change in attitudes toward women. We suspected that being professionally socialized during the emergence of the women's movement might have led to their having more egalitarian attitudes, aspirations, and role definitions than women in earlier cohorts. We also thought that the same cultural changes would affect men in all age cohorts in ways that would facilitate women's role performance.

At the same time, the impact of these changes might not be as great in science as in the society at large, not because women scientists are more conservative than others but rather because the opposite may be so. Women scientists who received their PhDs **2** or 3 decades ago may have held more egalitarian attitudes than their age peers among women as compared with women scientists in the 1970 cohort relative to their age peers. Thus the differences in attitudes between different cohorts of women scientists may be smaller than they are between cohorts of women of the same ages generally. Social and self-selection may produce cohort differences within institutions which are less (or more) marked than in the society as a whole.

Important countervailing forces have also been at work which affected the cohort of 1970. They have been plagued by greatly reduced job opportunities in colleges and universities and by reductions in research funding brought about by changes in govemment policy and high rates of inflation. These changes in the opportunity structure for doing science have affected all cohorts but perhaps the youngest ones most strongly. If women scientists have benefited from Affirmative Action (and there is some question about whether this has been so in posts other than those at entry level), both men and women in the cohort of 1970 did not when they started out and do not now face futures as rosy as did their age peers who benefited from the expansion of higher education in the

1950s and 1960s. How these changes in opportunity structures have affected the role performance of successive cohorts of scientists is not known.

Finally, our studies of age, period, and cohort effects in science suggest the need for an extension in the analysis of these effects. Until now, attention has focused primarily on these effects as they operate at the societal level. A new perspective is required which differentiates such effects at the societal level from those which operate in particular institutions and, most important, treats the reciprocal relations between these effects at different levels of analysis. Cohorts of scientists are a strategic research site for the study of these reciprocal relations.

This is so because multiple layers of age, period, and cohort effects that interact with one another in science can be readily identified. By way of example, the sciences seem to have their own period effects separate from those which have impact on the whole society. There are times of much cognitive development (or little) and times when resources for research are ample (or meager). There also appear to be marked cohort effects in the sciences. Becoming a scientist in the 1960s when there were many posts being created was surely quite different from becoming a scientist in the 1930s when the Depression made itself felt in science and academic life as well as the economy at large. There are also cohort effects in the sciences which are associated with cognitive change. Thus, those scientists who came of age after relativity and the quantum revolution or after the discovery of the helical structure of DNA share perspectives and research agenda different from those of their predecessors. And, finally, there may be institutionally specific age effects that are different from age effects that operate in other institutions or in the society at large. By way of example, the association between age and authority in science may be much weaker in science than is generally the case. Scientists who are 35 or 40 years of age are considered fully qualified for elite status if their contributions merit it, whereas those of the same age may be considered still rather young in law or business or medicine regardless of their achievements.

These observations are meant to suggest that the productivity puzzle is connected to a series of theoretical questions about the relations between social and cultural change, aging, and cohort flow, questions which we have only begun to articulate.

Faraday's edict, "Work, Finish, Publish," still holds—at least for most scientists—and now probably as much for women as for men. But since gender differences in published productivity persist, the productivity puzzle has yet to be solved.

APPENDIX

The following empirical studies, mostly published since 1973, examine aspects of published productivity of men and women scientists. They vary in kind and quality. Some directly address the question of gender differences and similarities in the productivity patterns; others simply examine the extent to which women scientists are represented in scientific publications in rough proportion to their numbers in a field; still others focus on the reward system of science and use productivity **of** scientists, male and female, as one explanatory variable. The scientific fields represented differ; the quality and size, as well as the type, of samples vary. The adequacy of the methods and presentations also vary. This list (compiled **by** Jan Sedofsky) is confined to studies that indicate simple bivariate differences in research output by gender; whether or not the studies purport to "reduce" the differential through multivariate analysis is not considered here. For a list of pre-1975 papers on this subject, see Cole (1979).

American Astronomical Society, Report of the Committee on the Status of Women (1979)
Astin (1978)
Astin and Bayer (1975)
Astin and Bayer (1979)
Bayer and Astin (1975)
Blackbum, Behymer, and Hall (1978)
Blackstone and Fulton (1974)
Blackstone and Fulton (1975)
Bryson, Bryson, and Johnson (1978)
Bryson, Bryson, Licht, and Licht (1976)
Centra (1974)
Chubin (1974)
Clemente (1972)
Clemente (1973)
Clemente and Sturgis (1974)
Cole (1979)
Cole and Cole (1973)
Converse and Converse (1971)
Fitzgerald, Pasewark, Thomton, and Sawyer (1975)
Freeman (1977)
Guyer and Fidell (1973)
Hamovitch and Morgenstem (1977)
Hansen, Weisbrod, and Strauss (1978)
Hargens, McCann, and Reskin (1978)
Heckman, Bryson, and Bryson (1977)
Heins, Smock, and Martindale (1978)
Helmreich, Spence, Beane, Lucker, and Matthews (1980)
Katz (1973)
Ladd and Lipset (1976)
Loeb and Ferber (1973)
Over (1980)
Over (1982)
Over and Moore (1980)
Pasewark, Fitzgerald, Thomton, and Sawyer (1973)
Pasewark, Fitzgerald, and Sawyer (1975)
Persell (1978)
Reskin (1978)
Simon, Clark, and Galway (1967)
Teghtsoonian (1974)
Widom and Burke (1978)

ACKNOWLEDGMENTS

This research was supported by a grant from the National Science foundation (SES-80-08609). We thank the staff of the Center for the Social Sciences, Columbia University, and in particular Jan Sedofsky, Margaret Carey, and Madeleine Simonson, for their assistance. Jan Sedofsky compiled the Appendix.

NOTES

1. The number **of** studies reporting data on published productivity **of** men and women scientists has grown rapidly. **More** than 40 studies are listed in Appendix A. Just one of these is purported

to show that women publish more than men (Simon, Clark, & Galway, 1967), and several others show no zero-order difference. In fact, Simon et al. do not show that women publish more than men, although they are often cited as demonstrating that this is so. As the authors say, "To summarize, of the four measures of productivity, the two most direct ones, numbers of articles and books published, married women publish as much or more than men, and unmarried women publish slightly less than men. The differences on the whole are not great." (Simon, et al., 1967, p. 231). The Simon, Clark, and Galway paper presents an interesting case in the sociology of knowledge. This paper has often been misread as evidence for one social fact (that women publish more than men), when it does not actually provide evidence for that position. The misreading is consistent with a particular ideological stance. It turns out that the paper, which really shows one thing, acquires a quite different symbolic meaning. This new meaning is reinforced through the subsequent citations to it as a demonstration of a "falsely" attributed fact. Several questions arise from this single instance of the wider phenomenon of "mis-citation" and reification. First, what produces the original mis-classification and citation? **Is** it a simple misreading of the text? Is it "intentional" and motivated distortion? Is it a socially conditioned misreading of the paper, influenced by strongly felt personal values and beliefs? In sum, how is the original error made? Second, how does the original error come to be perpetuated; how does it take on an independent life of its own? Plainly, this case is one among several classes of errors in references and citations, each of which would make interesting cases in the sociology of knowledge. For discussion and examples of other types of errors in use of evidence and in scientific scholarly research, see, among many others, Merton (1965; 1973, pp. 402-412); Altick (1963); Gould (1981). The list in Appendix A is not exhaustive. A significant number of these studies make use of the same data sets such as the data on the faculty collected by the American Council on Education and thus are not, in fact, fully independent inquiries.

2. Such attitudes were widespread in both the United States and Europe during the later part of the nineteenth and early twentieth centuries. One only has to look at the work of sociologists such as Le Bon, Durkheim, Spencer, Comte. and at the works of Freud and his contemporaries to get ample doses of such expressed beliefs. For examples, see Cole (1979) and Gould (1981).

3. The number of cases on which these correlations are based increases as we move toward the current period. As noted, for the 1911-1913 group, the number totals only 53; by 1952, the **number** is 383, and is 561 for the 1957-58 group of scientists.

4. ***American Doctoral Dissertations*** (ADD) and ***Dissertations Abstracts*** (DA) list degree recipients annually and there is a great deal of overlap between their entries. However, we found delay in entry dates in DA with some 1970 PhDs not listed in DA until 1971. We therefore used **ADD** as our prime source for the sample of women and men scientists.

5. In 1968, the first year of our data collection, the ***Source Index*** covered 1,968 source journals. Coverage of journals has expanded yearly.

6. "Authorship" includes the following types of publications by a given scientist: first, papers that were written and published by the author alone and, second, all collaborative papers on which the scientist's name appears. If two scientists collaborated to produce one paper, that paper would count in the totals of each author. We did not differentiate between collaborative papers involving different numbers of authors. A paper on which there were two authors or six were counted as one paper and contributed equally to the total publication counts for each scientist. Third, we made *no* distinction between collaborative papers on which there were only male authors, only female authors, or a combination of male and female authors. We know of no study that has distinguished between collaborative papers involving *only* women, ***only*** men. or the ***combination*** of men and women. This last distinction could be important. Studies of productivity and citations attribute authorship to men or to women but do not acknowledge the existence **of** a third category of papers authored by men and women jointly. Publication and citation counts for men and women, to be precise, should take into account this class of papers

7. Vigorous discussion continues between those who advocate the use of citation counts **as** measures of "influence" **or** "impact" and those who object to their use because scientists' citation

practices are not well understood and because the procedures used to count citations introduce artifacts and errors that are hard to estimate (Cole & Cole, 1971; Garfield, 1979; Goudsmit, 1968). We have tried to minimize the more common errors and artifacts in citation counting (those introduced by homonymns, for example) by requiring detailed checking of the fields represented by the citing joumals listed under the name of a given scientist (for example, citations in astrophysics journals to a scientist we know is a biochemist were deleted). This may have produced underestimates of citations but such errors are much smaller than those introduced by homonyms when such deletions were not made. We also carefully reviewed cases where copious citation was associated with sparse publication to be **sure** that references really were to scientists in the sample.

It would have been preferable to have had complete citation counts for the full bibliographies of the scientists in the sample instead of being confined to "straight" counts for papers published as solo or first authors. (The SCI does not list citations to papers where a scientist was a secondary author.) But the disparity between "straight" and "complete" counts is small; the two are correlated on the order of .80 to .96 in various fields (Cole & Cole, 1973, p. 73; Long, McGinnis, & Allison, 1980, p. 134). This correlation is greater for younger scientists than older ones since the proclivity to first authorship declines with age. (Long, McGinnis, & Allison 1980, p. 139). Others, however, claim that first-authored and solo-authored papers are not a representative sample of authors' publications and would restrict the **use** of straight counts (Lindsey, 1980). The Institute for Scientific Information now has an algorithm for counting all citations to a given author, irrespective of his location in an author set. When this is made available to researchers, the controversy on straight counts should be moot.

8. This correlation is based on 439 cases ($p < .001$).

9. These results conform closely to pattems described by Lotka (1926) and later by Price (1963).

10. The total number of papers produced by the top 15% of men scientists exceeded the total number of papers by all of the female scientists.

11. When no differences were found, men and women may have published no papers at all or they may have published precisely the same number of papers.

12. Cole (1979, p. 63n). These data are for matched samples of men and women PhDs in chemistry, biology, psychology, and sociology-different fields from those treated here. It should be noted that the increased presence of women among high producers may have resulted in part from the inclusion of biochemists in the 1970 sample. Biochemists tend to publish more papers than other scientists.

13. Since the fields represented in the two time periods differ, it is more appropriate to examine the sex distribution among prolific scientists than to examine the proportions of men and the proportions of women publishing as many as ''n'' papers

14. The association between gender and number of citations is weaker for the first 7 years ($r = -.07$) than for the next **5** ($r = -.12$), which is consistent with the increased difference between men and women in output that we noted earlier.

15. Comparisons of medians show somewhat greater inequalities, with the 12 year ratio being 0.40; for the early years, 0.37; and for the later ones, 0.33.

16. Moreover, there were no significant gender differences in first authorship within four of the six fields; in one, the earth sciences, men are more often **first** but in astronomy, the opposite is true. The number of cases is so small in both fields that departures from the overall pattern should not be given much weight.

17. The fact that the median citations to all papers by women is 5.4, and not much larger than the average citations per paper for women, suggests of course that women do not publish many papers in the aggregate.

18. Average citations per paper for men and women are much the same in each of four fields: biochemistry, chemistry, mathematics, and physics. There are too few cases in astronomy and earth sciences to draw reliable conclusions.

19. In previous studies of citation practices, the **use** of most-cited papers represented an attempt to control for the effects of productivity on citation totals. Since most scientists are infrequently cited, the correlation between total citations to scientists' work in any given year, such as 1979, is highly correlated with the total number of citations to their most-cited paper. For these scientists that correlation was $r = .69$. To extend this a bit, the correlation between total citations and citations to the two most-cited papers is .73 and $r = .75$ between the total and the three most-cited papers, For men as a group, the zero-order correlation between total citations in 1979 and the citations to the top paper was .69 and $r = .71$ for women. When we extend the number of most-cited papers to two, the correlation for men is .73 and is .74 for women. And finally, the correlations between the three most-cited papers and the total are $r = .76$ for men and .75 for women.

20. The number of citations to the average cited author in the SCI has remained fairly stable for the last decade. In 1969, each author was cited an average of 8.12 times; in 1979, 8.05 times; and in 1980, 8.28 times. This number of citations is much higher than one would find if citations for all authors, cited or not, were computed since most authors are uncited in any given year.

21. At the same time, the majority of scientists, men and women, maintained about the same rate of productivity over this period. The data reported in this section have been divided into two equal periods of 6 years rather than being broken into the first 7 and the later 5 years as was the case earlier.

22. Using five or more published papers as a cutting point between "lower" and "higher" productivity, we find that only 17% of the low producers in the initial 6 years became high producers in the next 6, but 38% of those who began as high producers in the first 6 years move into the group of low producers in the subsesquent 6 years. The correlation between early and later productivity is .60. The correlation between early and later productivity is actually higher for women than for men, that is, there **is** greater stability over time in the level of output for women than for men. This is shown in the following table:

Transition Probabilities of Men and Women Scientists for Stability and Change in Levels of Productivity between Earlier (1968-1973) and Later (1974-1979) in the Career

	T2 Productivity					
	Women			Men		
	Lower (*0-4* papers)	Higher (*5+*)		Lower (*0-4*)	Higher (*5+*)	
TI Productiviry:						
Lower (0-4)	.88	.12	(203)	.76	.24	(151)
Higher (5+)	.50	.50	(60)	.31	.69	(112)
			(263)			(263)

The data indicate that the primary reason for the greater association for women than for men is the clustering of 178 women among **lower** producers in both time periods.

23. These findings also suggest why many studies report that sheer output of papers has little independent effect on peer recognition when total citations are taken into account. This is so because citations are a function of total output to a significant degree and thus total ouput independent of citations has little influence.

24. Paired comparison of citation counts to the most-cited papers of the 1970 cohort showed that papers by men were cited more often in 44 percent of the pairs; and those by women in 32 percent. This provides a limited example of distributions of citations to particular papers by men and women.

25. Such calculations are made. Zuckerman (1968) found that Nobel laureates-to-be often exercised "noblesse oblige" in authorship by giving first authorship to younger colleagues-except on those papers they knew to be important and signal contributions

26. Absolute group size alone, as Cohen (1980) has shown, is not associated with output

REFERENCES

Allison, P.D., & Stewart, J.A. Productivity differences among scientists: Evidence for accumulative advantage. *American Sociological Review,* 1974, *39,* 596-606.

Altick, R.D. *The arr of literary research.* New York: Norton, 1963.

American Astronomical Society. *Report of the committee on the status of women.* Center for Astrophysics, Cambridge, October 1979.

Astin, H.S. *The woman doctorate in America.* New York: Russell Sage Foundation, 1969.

Astin, H.S. Factors affecting women's scholarly productivity. In H.S. Astin & W.Z. Hirsch (Eds.), *The higher education of women: Essays in honor of Rosemary Park.* New York: Praeger, 1978.

Astin, H.S., & Bayer, A.E. Sex discrimination in academe. In M.T.S. Mednick, S.S. Tangri, & L.W. Hoffman (Eds.), *Women and achievement.* (Originally published in *Educational Record,* Spring 1972.)

Astin, H.S., & Bayer, A.E. Pervasive sex differences in the academic reward system: Scholarship, mamage and what else? In D.R. Lewis & W.E. Becker (Eds.), *Academic rewards in higher educntion.* Cambridge: Ballinger, 1979.

Bayer, A.E., & Astin;, H.S. Sex differentials in the academic reward system. *Science,* 1975, *188,* 796-802.

Berelson, B. *Graduate educarion in the Unired States.* New York: McGraw-Hill, 1960.

Blackbum, **R.T.,** Behymer, C.E., & Hall, D.E. Research note: Correlates of faculty publications. *Sociology of Education,* 1978, *51,* 132-141.

Blackstone, **T.,** & Fulton, **O.** Men and women academics: An Anglo-American comparison of subject choices and research activity. *Higher Education,* 1974, *3,* 119-140.

Blackstone, T., & Fulton, **O.** Sex discrimination among university teachers: A British-American comparison. *The British Journal of Sociology,* 1975, *26*(3), 261-275.

Bryson, R., Bryson, J.B., & Johnson, **M.F.** Family size, satisfaction, and productivity in dual career couples. *Psychology of Women Quarterly,* 1978, *3,* 67-77.

Bryson, R.B., Bryson, J.B., Licht, M.H., & Licht, B.G. The professional pair: Husband and wife psychologists. *American Psychologist,* 1976, *31,* 10-16.

Centra, J.A. (With the assistance of N. Kuykendall). *Women, men, and the doctorate.* Princeton, N.J.: Educational Testing Service, 1974.

Chubin, D. Sociological manpower and womenpower: Sex differences in career patterns of two cohorts of American doctorate sociologists. *The American Sociologist,* 1974, **9,** 83-92.

Clemente, F. *A note on* **sex** *differences in research productivity.* Unpublished manuscript, 1972.

Clemente, F. Early career determinants of research productivity. *American Journal of Sociology,* 1973, *79*(2), 409-419.

Clemente, F., & **Sturgis,** R.B. Quality of department of doctoral training and research productivity. *Sociology of Education,* 1974, 47, 287-299.

Cohen, J.E. Publication rate as a function of laboratory size in **a** biomedical research institution. *Scientometrics,* 1980, 2, 35-52.

Cole, I.R. *Fair science: Women in the scientific community.* New York: The Free Press, 1979.

Cole, J.R., & Cole, S. Measuring the quality of sociological research: Problems in the use of the Science Citation Index. *American Sociologist,* 1971, *6,* 23-29.

Cole, J.R., & Cole, S. *Social stratification in science.* Chicago: University of Chicago Press, 1973.

Committee on the Education and Employment of Women in Science and Engineering. *Climbing the academic ladder: Doctoral women scientists in academe.* Washington, D.C.: Commission on Human Resources, National Research Council, National Academy **of** Sciences.

Converse, P.E., & Converse, J.M. The status of women as students and professionals in political science. *PS,* 1971, *4*(3), 328-348.

Crane, D. Social class origin and academic success: The influence of two stratification systems on academic careers. *Sociology of Education.* 1969. 42, 1-17.

Duncan, B., & Duncan, O.D. (with the collaboration of J.A. McRae). *Sex typing andsocial roles: A research report.* New York: Academic Press, 1978.

Endler, N.S., Rushton, J.P., & Roediger, H.L. Productivity and scholarly impact (citations) of British, Canadian, and U.S. departments of psychology (1975). *American Psychologist,* 1978, *33,* 1064-1082.

Epstein, C.F. *Woman's place: Options and limits in professional careers.* Berkeley: University of California Press, 1970.

Fitzgerald, B.J., Pasewark, R.A., Thomton, L., & Sawyer, R.N. Research activities of experimental psychologists. *Professional Psychology,* 1975, *6*(2), 114-115.

Freeman, B.C. Faculty women in The American University: Up the down staircase. *Higher Education,* 1977, 6, 165-188.

Garfield, E. The 1,000 contemporary scientists most cited 1965-78. Part I. The basic list and introductions. *Current Contents,* 1981, *13,* 5-14.

Gaston, J. *Originality and competition in science.* Chicago: University of Chicago Press, 1973.

Gaston, J. *The reward system in British and American science.* New York: Wiley Interscience, 1978.

Goudsmit, S.A. Citation analysis. *Science,* 1968, *183,* 28.

Gould, S.J. *The mismeasure of man.* New York: W.W. Norton, 1981.

Guyer, L., & Fidell, L. Publication of men and women psychologists: Do women publish less? *American Psychologist,* 1973, 28, 157-160.

Hagstrom, W. Inputs, outputs and the prestige of American university science departments. *Sociology of Education,* 1971, *44,* 375-379.

Hall, G.S. *Adolescence. Its psychology and its relations to physiology, anthropology, sociology,* **sex,** *crime, religion, and education* (2 vols.). New York: Appleton, 1904.

Hamovitch, W.. & Morgenstern, R.D. Children and the productivity of academic women. *Journal of Higher Education,* 1977. *48*(6), 633-645.

Hansen, W.L., Weisbrod, B.A., & Strauss, R.P. Modeling the earnings and research productivity of academic economists. *Journal of Political Economy,* 1978, *86*(4), 729-741.

Hargens. L.L.. McCann, J.C., & Reskin, B.F. Productivity and reproductivity: Fertility and professional achievement among research scientists. *Social Forces,* 1978, *52*(3), 129-146.

Harmon, L.R. Profiles of Ph.D.'s in the sciences. In *Career Patterns Report* ***No.*** *1.* Washington, D.C · National Academy of Sciences, National Research Council.

Heckman, N.. Bryson, R., & Bryson, J.B. Problems of professional couples. A content analysis. *Journal of Marriage and The Family,* 1977, *39,* 323-330.

Heins, M., Smock, S., & Martindale, L. Current status of women physicians. *International Journal of Women's Studies,* 1978, *1*(3), 297-305.

Helmreich, R.L., Spence, J.T., Beane, W.E., Lucker, G.W., & Matthews, K.A. Making it in academic psychology: Demographic and personality correlates of attainment. *Journal of Personality and Social Psychology,* 1980. *39*(5), 896-908.

Honig, M., & Hanoch, G. *Age, cohort and period effects in the labor market behavior of older persons.* (Preprint No. 83.) New York: Columbia University, Center for the Social Sciences.

Kantor, R.M. Some effects of proportions on group life: Skewed sex ratios and responses to token women. ***American Journal of Sociology,*** 1977, 82, 965-990.

Katz, D.A. Faculty salaries, promotions, and productivity at a large university. ***The American Economic Review,*** 1973, *63*(3), 469-477.

Kaufman, D.R. Associational ties in academe: Some male and female differences. ***Sex Roles,*** 1978, *4*(1), 9-21.

Ladd, E.C., Jr., & Lipset, S.M. Sex differences in academe. ***Chronicle of Higher Education,*** 1976, *12*(11), 18.

Lindsey, D. Production and citation measures in the sociology of science: The problem of multiple authorship. ***Social Studies of Science,*** 1980, *10*, 145-162.

Loeb, J.W., & Ferber, M.A. Representation, performance and status of women on the faculty of the Urbana-Champaign campus of the University of Illinois. In A.S. Rossi & A. Calderwood (Eds.), ***Academic women on the move.*** New York: Russell Sage Foundation, 1973.

Long, J.S. Productivity and academic position in the scientific career. ***American Sociological Review,*** 1978, ***43,*** 889-908.

Long, J.S., McGinnis, R., & Allison, P.D. The problem of joint-authorship papers in constructing citation counts. ***Social Studies of Science,*** 1980, *10*, 127-143.

Lotka, A.J. The frequency distribution of scientific productivity. ***Journal of the Washington Academy of Sciences,*** 1926, ***16,*** 317.

Mason, K.O., Czajka, J.L., & Arber, S. Change in U.S. women's sex-role: Attitudes, 1964-1974. ***American Sociological Review,*** 1976, *41*(4), 573-596.

Merton, R.K. ***Science. technology and society in seventeenth century England.*** New York: Howard Fertig, 1938.

Merton, R.K. ***On the shoulders of gianrs: A shandean postscript.*** New York: The Free Press, 1965.

Merton, R.K. The normative structure of science. In R.K. Merton, ***The sociology of science: Theoretical and empirical investigations.*** (N.W. Storer, Ed.) Chicago: University of Chicago Press, 1973. (Originally published, 1942).

Over, R. Research productivity and impact on men and women in departments of psychology in the United Kingdom. ***Bulletin of The British Psychological Society,*** 1980, 33, 385-386.

Over, R. Research productivity and impact of male and female psychologists. ***American Psychologist,*** 1982, *37*(1), 24-31.

Over, R., & Moore, D. Research productivity and impact of men and women in psychology departments of Australian universities, 1975-1977. ***Australian Psychologist,*** 1980, *15*(3), 413-418.

Pasewark, R.A., Fitzgerald, B.J., Thomton, L., & Sawyer, R.N. Icons in the attic: Research activities of clinical psychologists. ***Professional Psychology,*** 1973, ***4*(3),** 341-346.

Pasewark, R.A., Fitzgerald, **B.J.,** & Sawyer, R.N. Psychology of the scientist: **XXXII** God at the synapse: Research activities **of** clinical, experimental, and psychological psychologists. ***Psychological Reports,*** 1975, *36*, 671-674.

Persell, C. ***Explaining productivity among male and female applied researchers.*** Mimeo, 1978.

Price, D.J. de S. ***Little Science, big science.*** New York: Columbia University Press, 1963.

Price, D.J. de S. Citation measures **of** hard sciences and soft science, technology and non-science. In C.E. Nelson & D.K. Pollack (Eds.), ***Communication among scientists and engineers.*** Lexington, Mass.: D.C. Heath & Lexington Books, 1971.

Reskin, B.F. Scientific productivity, sex, and location in the institution of science. ***American Journal of Sociology,*** 1978, ***83*(5),** 1235-1243. (a)

Reskin, B.F. Sex differentiation and the social organization of science. In J. Gaston (Ed.), ***The sociology of science.*** San Francisco: Jossey-Bass, 1978. (b)

Riley, M.W. Age and aging: From theory generation to theory testing. **In H.** Blalock, **Jr. (Ed.),** ***Social theory and research: A critical appraisal.*** New York: The Free Press, 1980.

Riley, M.W., Johnson, **M.**, & Foner, A. **(Eds.).** ***Aging and society: A sociology of age stratification*** (Vol. 3). New York: Russell Sage Foundation, 1972.

Roose, K.D., & Andersen, C.J. ***A rating of graduate programs.*** Washington, D.C.: American Council of Education, 1970.

Simon. R.J.. Clark. S.M., & Galway, K. The woman Ph.D.: A recent profiie. ***Social Problems,*** 1967, *15*(2), 221-236.

Teghtsoonian, **M.** Distribution by sex of authors and editors of psychological journals, 1970-1972: Are there enough women editors? ***American Psychologist,*** 1974, 29. 262-269.

Widom, C.S., & Burke, B.W. Performance, attitudes, and professional socialization of women in academia. ***Sex Roles,*** 1978, *4*(4), 549-562.

Ziman, J. ***Public knowledge.*** Cambridge, England: Cambridge University Press, 1968.

Zuckerman, H. Patterns of name-ordering among authors of scientific papers: A study of social symbolism and its ambiguity. ***American Journal of Sociology,*** 1968, ***74,*** 276-291.

Zuckerman, H. ***Scientific elite: Nobel laureates in the United States.*** New **York:** The Free Press, 1977.

Zuckerman, H., & Cole, J.R. Women in American science. ***Minerva,*** 1975, *13*(1), 82-102.

Zuckerman, H.. & Merton, R.K. Age, aging, and age structure in science. **In** M.W. Riley, M. Johnson. & **A,** Foner (Eds.), ***Aging and society: A sociology of age stratification*** (Vol.3). New York Russell Sage Foundation, 1972