NOTES AND LETTERS

● *ABSTRACT*

Previous studies in the sociology of science have relied on measures of production and citations which have failed to take account of multiple authorship. This study indicates that these previous (and still currently used) measures introduce intolerable error and often profoundly influence substantive interpretation. To address the problem of multiple authorship in the measurement of publications and citations, a revision of current indices is presented. Previous measurement error may well require a re-analysis and rethinking of previous reported studies in the sociology of science.

Production and Citation Measures in the Sociology of Science: The Problem of Multiple Authorship

Duncan Lindsey

The academic science community has always evaluated its members in terms of their scientific contribution to knowledge. Within the emerging subfield of the sociology of science, increasing attention has been paid to the factors which influence this scientific contribution. Two variables have been used to assess this contribution; scientific production, and its quality. Production has been determined by counting the number of scientific papers and books published by a scientist. Quality, with its aesthetic dimension, has proven more elusive to measurement strategies. The approach most frequently used is citation counting. I will argue that one of the most serious errors in empirical judgment made in the sociology of science has been to count both publications and citations with procedures that take no account of multiple authorship. Measure-

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ment thus introduces increasing errors as multiple authorship increases. Since scientific publication is now characterized by extensive multiple authorship, this error has become critical.

There has been a dramatic trend toward multiple authorship in science. Price considers this to be a salient feature of the shift from little science to big science.[1] It has also been evident in the social sciences.[2] But there have been few concomitant empirical studies in the sociology of science which take account of this trend with modified measurement strategies.[3] I will examine the history and reasons for this puzzling neglect of multiple authorship.

In counting publications or citations, how are multiple-authored papers to be treated? When they are relatively rare, the problem can be treated lightly: most studies have simply allocated full credit to all contributors — that is, they have employed a 'normal count procedure' (see Table 1, which lists the strategies employed in a sample of studies in the general field of 'social studies of science'). However, when more than half the published work in almost all of the physical and natural (and many of the social) sciences is multi-authored, a more sophisticated solution must be developed.

Straight Counts

The Coles claim that 'the omission of collaborative citations to papers on which the author was not the first among collaborators does not affect substantive conclusions'.[4] They therefore recommend such omission — a strategy which they call '*straight counts*'.[5] This strategy has two results. First, it 'solves' the problem of distributing credit for multiple-authored work by disregarding all but the first author (who receives all the credit). Second, it greatly reduces the work required to collect data on any sample of scientists. Since this process of data collection can be quite tedious, the 'straight count' procedure sounds attractive. It does not require taking note of multiple authorship; thus less information needs to be collected. Nor is it necessary to track down papers where the scientist was not the first author — nor citations to these papers. Where multiple authorship is common, the tracking down procedure introduces a multiplier effect. For instance, one begins with a sample of 50 scientists: if eighty percent of these scientists write three or more multi-authored papers for which they are not the first

TABLE 1
Accounting Procedures for Production and Citation Counts Used in a Sample of Social Studies of Science

Study	Production		
	Articles	Books	Citations
Aiyepeku (1976)	N		
Axelson (1960)	A		
Babchuk & Bates (1962)	N		
Bayer & Astin (1975)	N	N	
Bayer & Folger (1966)	N	N	S
Chubin (1973)	N	N	N
Clemente (1973; 1975)	N	N	
Clemente & Sturgis (1974)	N	N	
Cole & Cole (1973)	N	N	S
Crane (1965; 1969)	N	N	
Dennis (1954)	N	N	
Dent & Lewis (1976)	N	N	S
Eiduson (1966)	N	N	
Glenn & Villemez (1970)	N	N	
Guyer & Fidell (1973)	N		
Hagstrom & Hargens (1968)	N	N	S
Hargens & Hagstrom (1967)	N	N	
Hayes (1971)	N	A	
Heffner (1976)	N	N	S
Holley (1976)	N	N	
Knudsen & Vaughan (1969)	N	N	
Larson et al. (1972)	A		
Lazarsfeld & Theilens (1958)	N	N	
Lehman (1962)	N	N	
Lightfield (1971)	N	N	S
Manis (1950)	N	A	
Meltzer (1949)	N	A	
Price & Beaver (1966)	N	A	
Quandt (1976)			S
Reskin (1976; 1977)	N	N	S
Roche & Smith (1976)			S
Roe (1965)	N	N	
Shaw (1967)	A	A	
Shockley (1957)	N	N	
Simon (1967)	S	S	
Stallings & Singhal (1970)	A	A	
Strans & Radel (1969)	N	A	
Sturgis & Clemente (1973)	N	N	
Teghtsoonian (1974)	N		N
Voeks (1962)	N	A	

N = normal count (all items counted once)
A = adjusted count (each item divided by number of authors and then summed)
S = straight count (only first author or solo authored items counted)
For details of the studies, see Appendix.

author listed, and the first author is not in the original sample, then a new sample of 120 or more additional names have to be searched. Since accounting procedures (for both production and citations) can be restricted with a 'straight count' approach, there is a substantial reduction in effort.[6]

Sampling Assumptions

The straight count approach is a sampling strategy: as such, it should be examined in terms of its representativeness and convenience.[7] Its convenience is clear. However, it is in its representativeness that its limitations are most pronounced.

The straight count procedure presupposes that the set of papers on which a scientist's name occurs first (including solo-authored papers) is a representative sample of all of that scientist's papers. Although Cole and Cole (and others who recommend this approach) do not explicate its rationale, except on a priori grounds,[8] there are a number of reasons which might support its use. First, it would be reasonable to assume that the name order of authors listed on a given paper reflects the level of their contributions — with the greatest contributor listed first, and so on in descending order. In a study of the patterns of collaboration in four fields, Heffner found that there was little evidence to contradict this hypothesis.[9]

Second, it might be argued that, since authorship is probably allocated in a reasonably fair fashion, straight counts provide a *fair* measure of production. However, it would be insensitive to those members of a collaborative team who consistently occupy a junior role, while overestimating the contribution of the senior partner.[10] As Narin has noted, this implies that 'a researcher who is a second author of five papers may receive no citations under his own name, even though the papers he co-authored may be highly cited.'[11] Moreover, if the order of attribution is determined by level of contribution, first author papers should be a *unique* (and hence unrepresentative) subset of all of an author's papers. In a large sample, this uniqueness may average out, scientist by scientist. Or it may be that this unique subset is representative of those contributions for which the author had primary responsibility — and hence is the most appropriate for judgment.

Another problem with a straight count is that it may discriminate against those scientists whose name appears late in an alphabetic listing. The Coles claim that the position of an author's name in an alphabetic listing does not influence the allocation of credit.[12] But this has been contradicted by a more comprehensive examination of 1500 chemists conducted by Rudd, who found a greater percentage of first authors among those with last names beginning with A to F compared with G to M, and with G to M compared with N to Z (the percentages of first authors in the three groups were 56.8, 29.9, and 13.3 respectively).[13] I have examined the question with a cohort of professional literature published in 1970, and consisting of 1300 articles selected by a stratified random sample of publications in seven fields: (1) biochemistry; (2) economics; (3) gerontology; (4) psychiatry; (5) psychology; (6) social work; and (7) sociology.[14] I investigated the extent to which name ordering in this sample followed alphabetic order — believing that the important question is not where an author's name appears in a listing, but whether or not that listing is alphabetic. Of the 1300 articles, 527 were multi-authored: the proportions of alphabetic to non-alphabetic name ordering in these 527 papers are presented in Table 2. There is a tendency towards alphabetic name ordering in the double-authored papers, and this trend continues (but at a reduced level) for triple-authored papers. With four authors, alphabetic ordering does not appear to occur more frequently than non-alphabetic. Overall, therefore, there is a significantly greater probability of alphabetic ordering: in other words, among members of a collaborative team, the member whose name occurs first alphabetically is more likely to appear first in the list of authors.

Other factors are also likely to influence the sampling procedure implied by the 'straight counts' strategy. For instance, contributors to a scientific paper are frequently enmeshed in subordinate supervisor roles that could exercise an independent influence on any decision on author listing.[15] One cannot assume that the authoritative allocation of power and authority in the scientific environment has negligible influence on the allocation of credit.

To summarize, the measurement procedure recommended by the Coles essentially amounts to a sampling strategy where first author papers are defined as representative of a scientist's total production. The underlying assumption is that the influences affecting the sampling process are random. There is neither strong empirical evidence nor theoretical rationale to support this assumption. Rather, what limited evidence is available suggests that the sampl-

TABLE 2
The Order of Authorship

Two Authors:							
Alphabetic	Yes	203	Z = 2.94, p = 0.0016				
	No	148					
Three Authors:							
		1&2	*1&3*	*2&3*			
Alphabetic	Yes	70	62	56			
	No	46	54	60			
Four Authors:							
		1&2	*1&3*	*1&4*	*2&3*	*2&4*	*3&4*
Alphabetic	Yes	32	28	33	31	31	35
	No	28	32	27	29	29	25
Total:							
Alphabetic	Yes	581	Z = 3.17, p = 0.008				
	No	478					

For further discussion of part of this data, see Lindsey, op. cit. note 14, Chapter 2.

ing process is *not* random. If a straight count procedure is not used then the problem of distributing credit for multi-authored publications arises.

Accounting for Multiple Authorship

Previous studies of co-authorship have indicated the near impossibility of assessing relative contributions in collaborative work.[16] The concern for precisely measuring production requires removing the bias introduced by the straight count procedure. Since solo-authored and multi-authored papers are similar in impact, it seems reasonable to distribute the credit for a multi-authored work equally among the contributors. But they are still *single* papers: in terms of production, one solo-authored paper is equal to one multi-authored paper. The substantive question is the allocation of credit for the production of that one paper. Various weighting schemes are possible — such as, for instance, awarding first author two-thirds credit, second author one-third, and so on.[17] In any such scheme, the weights must sum to one, because what is being measured is 'one scientific paper'. Although this is the logical procedure, it violates the intuitive judgment of many scientists. Nudelman and Landers conducted a mail questionnaire survey to determine how a random sample of sociologists would allocate credit to multi-authored productions: their data indicate that multi-authored work consistently receives proportionately more perceived credit, and they conclude that 'Even though an article is the work of only one author, sharing authorship with colleagues is a useful strategy, resulting in a mean credit gain of 96 percent for every three publications "coauthored".'[18]

There are variations in the nature and style of collaborative work. At one end of the continuum are the partners who routinely include each others' names on their papers, despite having had minimal scholarly or scientific interaction. At the other extreme is the team whose members interact and stimulate each other in a dynamic creative process, and whose subsequent work reflects this interchange. It is likely that this latter style produces more important work than would be achieved in solo investigation. If collaboration does lead to higher quality work, then a precise measure of production will include it.

Besides having more authors, are there any measurable differences between multi-authored and single-authored papers? Oromaner traced 219 articles (114 single-authored and 105 multi-authored) appearing in core sociology journals over a ten-year period, to determine if there were any differences in impact as assessed by citations. His findings suggest that multi-authored papers have a slightly greater impact.[19] I have investigated the impact of the 1300 articles in my literature cohort by examining how the median number of citations varies with the number of authors, in each of the seven fields. The results are found in Table 3. The data obtained with this primitive measure (median measures of citations whose distribution is exponential) indicates an inconsistent pattern, both within and between fields.

TABLE 3
Citations by the Number of Authors for Seven Fields

Biochemistry:

	N	%	Median[a]	Mean
one author	29	19	4.0	8.45
two authors	72	46	8.0	18.11
three authors	34	22	12.5	15.06
four or more	20	13	4.5	9.85
	155			

Economics:

	N	%	Median	Mean
one author	128	83	1.20	4.16
two authors	24	16	3.25	5.04
three authors	2	1	0.5	0.5
four or more	0	0		
	154			

Gerontology:

	N	%	Median	Mean
one author	64	53	0.9	1.48
two authors	34	28	2.0	2.47
three authors	16	13	1.1	2.25
four or more	7	6	1.0	2.86
	121			

Psychiatry:

	N	%	Median	Mean
one author	97	53	0.95	2.29
two authors	50	27	0.7	2.26
three authors	17	9	4.0	9.71
four or more	18	10	3.17	6.17
	182			

Psychology:

	N	%	Median	Mean
one author	93	45	2.0	7.74
two authors	74	36	2.4	5.88
three authors	30	15	3.0	6.43
four or more	8	4	5.5	7.75
	205			

Social Work:

	N	%	Median	Mean
one author	259	75	0.45	1.16
two authors	68	20	1.0	1.75
three authors	13	4	1.25	1.85
four or more	6	2	1.5	2.33
	346			

Sociology:

	N	%	Median	Mean
one author	103	75	1.39	4.07
two authors	29	21	4.0	7.41
three authors	4	3	1.0	2.0
four or more	1	1	20.0	20.0
	137			

a. Median and mean citation counts are for the 1970-75 period inclusive. The source for biochemistry data was the *Science Citation Index,* for the other fields the source was the *Social Science Citation Index.*

Adjusted Counts

Narin claims that 'There does not seem to be any reasonable way to deal with the attribution problem, except to attribute a fraction of a publication to each of the authors.'[20] I propose an accounting procedure for measuring production which involves dividing the paper produced by the number of authors. For example, to assess article production for any one author would require dividing each article produced by the number of authors, and summing across articles:

$$\text{Adjusted total articles} = \sum_{i=1}^{j} (1/n_i)$$

(where n_i represents the number of authors to article i, and i through j represents the first to last article produced by the author). This 'adjusted total articles' measure is not intended to partial out the influence of collaboration on production: if co-authorship leads to greater production, then the measure should reflect this. My adjustment is intended to control for the bias in overestimating production when the full value of a co-authored paper is awarded to all contributors. The suggestion is that this 'normal count' procedure tends to inflate the score of those who produce multi-authored papers. To examine this, I calculated the differences which arise from applying the two accounting procedures ('normal count' and my 'adjusted count') to members of an editorial board sample who were in the bottom twentieth percentile in terms of collaboration.[21] The data (in Table 4) indicate only a very small difference (although it is just barely significant at the 0.05 level). The mean difference between the measures of low collaborators is 0.0965, with a standard deviation equal to 0.346. But among high collaborators (the 20th percentile for the sample) the difference in measurements is substantial: the mean difference is 7.633, with a standard deviation equal to 6.034. The large discrepancy at the high collaboration level *forces a judgment* regarding the most valid indicator of production. The difference can only be accounted for by the heavier weighting assigned to multi-authored works by the unadjusted measure. Price and Beaver provide an illustration of the bias:

> It is especially noteworthy that nobody who worked without collaborators or with only one coauthor succeeded in producing more than four papers in the five-year period, whereas everybody with more than twelve collaborators produced fourteen or more papers in the same time.[22]

TABLE 4
Comparison of Production Accounting Procedures for High and Low Collaborative Authors

Accounting Procedure for Total Articles	Level of Collaboration Low: (bottom 20% of sample)		High: (top 20% of sample)	
	Mean	s.d.	Mean	s.d.
normal count	6.82	11.41	18.05	13.62
adjusted count	6.73	11.07	10.42	7.91
N = 57 (for both Low and High groups)	t = 2.1 Mean difference = 0.097		t = 9.47 mean difference = 7.633	

This heavier weighting for collaborators represents measurement bias intorduced by the 'normal count' procedure: the 'adjusted' procedure reduces the bias by using the limited information available.

An Illustration of the Error

So far the analysis has focused on the theoretical problems of current measurement strategies — that is, 'normal' and 'straight' counts. It might be allowed that these problems exist, but that they do not pose serious problems in practice. Reducing measurement error involves an economy of choice. Therefore, if adjusted counts reduce error, they must reduce it beyond what might be gained by the larger sample size a similar amount of effort would permit with straight or normal counts. It has been noted that in actual practice measurement error often cancels out. For instance, Tukey observes that '...under the usual circumstances where many effects are small, while a few are large enough to be detected, we may confound effects with error and still obtain reasonable analyses of the result.'[23]

To examine the consequences of the measurement bias introduced by ignoring multiple authorship, I recomputed data from a previously published editorial study so as to make use of both 'unadjusted' and 'adjusted' measures.[24] This study assessed those factors that best predict the degree of influence an editorial board member exercises in the manuscript review process. A regression analysis on an 'editorial power index' was performed, where 'editorial power' was defined as the number of manuscripts reviewed during a year, times the rate of favourable recommendations, times the percentage of journal concurrence with board member's review. The independent variables in the analysis were 'adjusted' measures of production and citations, factor scores of 'quantitative' and 'qualitative' orientation,[25] and prestige measures of the board member's doctorate and place of current employment. The revised data are presented in Table 5, where 'equation one' refers to the original published data (using adjusted measures), and 'equation two' refers to the same regression computation, but using *unadjusted* measures. The differences between the two measurement strategies are obvious. Using equation one, three of the variables exert a significant influence. In contrast, with equation two, none of the variables are significant at the alpha error level, and the size of the beta weight for production is substantially reduced from the result with the unadjusted measure (0.406 to 0.207). A similar shift has been reported by Reskin;[26] a straight count citation measure also reduces the figure (from -0.456 to -0.314). Both these reductions in the value of the beta weights (which are the largest in the table) indicate substantial measurement error. Although the direction of the relationship among the set of variables is the same with both measures, the relative influence of the variables in the regression equation has shifted. One of the most notable differences is that the beta weight for production, which was twice that of 'quantitative' orientation with the original equation, is equivalent to it with the second equation. This would alter substantive analysis of the data.

In summary, the general effect of using straight counts of citations, and normal counts of production, on this example has been to obscure the influence of the main variables. This could be partly a result of the small sample size. However, the sample size is in a range where standard errors begin to stabilize. (It should also be emphasized that this illustration is from the field of psychology, where multiple authorship is not as frequent as in the natural and physical sciences.)

TABLE 5
Determinants of Editorial Power for Psychology Journals

Determinant	Equation 1 Beta weights	Zero-order correlation	Equation 2 Beta weights
Origin of Doctorate	-0.036	0.109	-0.079
Adjusted Production	0.406*	0.032	***
Unadjusted Production	***	-0.070	0.207
Quantitative Orientation	0.238*	0.243	0.208
Qualitative Orientation[a]	0.069	0.154	0.094
Present Employment	0.019	0.044	0.004
Adjusted Citations	-0.456*	-0.214	***
Straight Citation Count	***	-0.225	-0.314

*p 0.05 (N=81)
***Variable not included in the equation.
a. This variable was omitted in the original presentation (see Lindsey, op. cit. note 21). Since equation one was calculated with this variable included, it should have been displayed.

Conclusion

I have argued that counts of either production or citations which ignore multiple authorship introduce considerable measurement bias. Since both production and citations are most frequently used as intervening variables, this bias appears to be unacceptable. If the concern of the investigator is with either an individual scientist or an aggregated unit, the measure of production (or citations) must include an adjustment for multi-authored work. Until it becomes possible to assess the relative contribution of collaborators, division by the number of contributors, and equal allocation of credit seems the best course.

Theoretical development in the sociology of science will advance hand in hand with the conceptual and technical improvement of measurement strategies (in this case, of scientific contributions to knowledge) which provide the data for developing, testing and revising theories. These comments are intended to serve that effort.

But the more serious problems remain: these are conceptual, and have to do with the meaning of citations.

• *APPENDIX*

Details of the studies referred to in Table 1 are as follows:

Aiyepeku (1976): W. O. Aiyepeku, 'The Productivity of Geographical Authors: A Case Study from Nigeria', *Journal of Documentation*, Vol. 32 (1976), 105-17.

Axelson (1960): L. J. Axelson, 'Graduate Schools and the Productivity of their Graduates', *American Journal of Sociology*, Vol. 66 (1960), 171-75.

Babchuk & Bates (1962): N. Babchuk and A. P. Bates, 'Professor or Producer: The Two Faces of Academic Man' *Social Forces*, Vol. 40 (1962), 341-44.

Bayer & Astin (1975): A. E. Bayer and H. S. Astin, 'Sex Differences in the Academic Reward System', *Science*, Vol. 188 (23 May 1975), 796-802.

Bayer & Folger (1966): A. E. Bayer and J. Folger, 'Some Correlates of a Citation Measure of Productivity in Science', *Sociology of Education*, Vol. 39 (1966), 381-90.

Chubin (1973): D. Chubin, 'On the Use of the *Science Citation Index* in Sociology', *American Sociologist*, Vol. 8 (1973), 171-91.

Clemente (1973): F. Clemente, 'Early Career Determinants of Research Productivity', *American Journal of Sociology*, Vol. 79 (1973), 409-19.

Clemente (1975): F. Clemente, 'Race and Publication Productivity', *Journal of Black Studies*, Vol. 5 (1975), 157-66.

Clemente & Sturgis (1974): F. Clemente and R. B. Sturgis, 'Quality of Department of Doctoral Training and Research Productivity', *Sociology of Education*, Vol. 47 (1974), 287-99.

Cole & Cole (1973): Op. cit. note 4.

Crane (1965): D. Crane, 'Scientists at Major and Minor Universities: A Study of Productivity and Recognition', *American Sociological Review*, Vol. 30 (1965), 699-714.

Crane (1969): D. Crane, 'Social Structure in a Group of Scientists: A Test of the "Invisible College" Hypothesis', *American Sociological Review*, Vol. 34 (1969), 335-52.

Dennis (1954): W. Dennis, 'Predicting Scientific Productivity in Later Maturity from Records of Earlier Decades', *Journal of Gerontology*, Vol. 9 (1954), 465-67.

Dent & Lewis (1976): R. L. Dent and D. J. Lewis, 'The Relationship between Teaching Effectiveness and Measures of Research Quality', *Educational Research Quarterly*, Vol. 1 (1976), 3-16.

Eiduson (1966): B. T. Eiduson, 'Productivity Rate in Research Scientists', *American Scientist*, Vol. 54 (1966), 57-63.

Glenn & Villemez (1970): N. D. Glenn and W. Villemez, 'The Productivity of Sociologists at 45 American Universities', *American Sociologist*, Vol. 6 (1970), 298-303.

Guyer & Fidell (1973): L. Guyer and L. Fidell, 'Publications of Men and Women Psychologists', *American Psychologist*, Vol. 28 (1973), 157-60.

Hagstrom & Hargens (1968): W. O. Hagstrom and L. L. Hargens, 'Mobility Theory in the Sociology of Science', unpublished paper presented to the Cornell Conference on Human Mobility (Ithaca, NY, October 1968).

Hargens & Hagstrom (1967): L. L. Hargens and W. O. Hagstrom, 'Sponsored and Contest Mobility of American Academic Scientists', *Sociology of Education*, Vol. 40 (1967), 24-38.

Hayes (1971): J. R. Hayes, 'Research, Teaching and Faculty Fate', *Science*, Vol. 172 (16 April 1971), 227-30.

Heffner (1976): Op. cit note 9.

Holley (1976): J. W. Holley, 'Tenure and Research Productivity', unpublished paper presented to the annual meeting of the Midwest Sociological Society (St Louis, Missouri, 22 April 1976).

Knudsen & Vaughan (1969): D. D. Knudsen and T. R. Vaughan, 'Quality in Graduate education: A Re-evaluation of the Rankings of Sociology Departments in the Cartter Report', *American Sociologist*, Vol. 4 (1969), 12-19.

Larson et al. (1972): R. F. Larson, M. L. Petrowsky and J. S. Vandiver, 'Journal Productivity of PhD Sociologists', *The American Sociologist*, Vol. 7 (1972), 9-11.

Lazarsfeld & Theilens (1958): P. F. Lazarsfeld and W. Theilens, Jr, *The Academic Mind* (Glencoe, Ill.: The Free Press, 1958).

Lehman (1962): H. C. Lehman, 'The Creative Production Notes of Present Versus Past Generations of Scientists', *Journal of Gerontology*, Vol. 17 (1962), 409-17.

Lightfield (1971): E. T. Lightfield, 'Output and Recognition of Sociologists', *American Sociologist*, Vol. 6 (1971), 128-33.

Manis (1950): J. G. Manis, 'Some Academic Influences upon Publication Productivity', *Social Forces*, Vol. 29 (1950), 267-72.

Meltzer (1949): B. N. Meltzer, 'The Productivity of Social Scientists', *American Journal of Sociology*, Vol. 55 (1949), 25-29.

Price & Beaver (1966): Op. cit. note 22.

Quandt (1976): R. E. Quandt, 'Some Quantitative Aspects of the Economics Journal Literature', *Journal of Political Economy*, Vol. 84 (1976), 741-55.

Reskin (1976): B. F. Reskin, 'Sex Differences in Status Attainment in Science: The Case of the Postdoctoral Fellowship', *American Sociological Review*, Vol. 41 (1976), 597-612.

Reskin (1977): Op. cit. note 10.

Roche & Smith (1976): T. Roche and D. L. Smith, 'Use of the Citation to Evaluate Journals, Departments, and Individuals: A Study in the Stratification of Sociology', unpublished paper presented to the annual meeting of the American Sociological Association (New York, September 1976).

Roe (1965): A. Roe, 'Changes in Scientific Activities with Age', *Science*, Vol. 150 (15 October 1965), 313-18.

Shaw (1967): Op. cit. note 17.

Shockley (1957): W. Shockley, 'On the Statistics of Individual Variations of Productivity in Research Laboratories', *Proceedings of the IRE*, Vol. 45 (1957), 279-90.

Simon (1967): J. Simon, 'The Woman PhD: A Recent Profile', *Social Problems*, Vol. 15 (1967), 221-36.

Stallings & Singhal (1970): Op. cit. note 17.

Straus & Radel (1969): M. A. Straus and D. J. Radel, 'Eminence, Productivity, and Power of Sociologists in Various Regions', *American Sociologist*, Vol. 4 (1969), 1-4.

Sturgis & Clemente (1973): R. B. Sturgis and F. Clemente, 'The Productivity of Graduates of 50 Sociology Departments', *American Sociologist*, Vol. 8 (1973), 169-80.

Teghtsoonian (1974): M. Teghtsoonian, 'Distribution by Sex of Authors and Editors of Psychological Journals, 1970-72', *American Psychologist*, Vol. 29 (1974), 262-69.

Vocks (1962): V. W. Vocks, 'Publication and Teaching Effectiveness', *Journal of Higher Education*, Vol. 33 (1962), 212-18.

• NOTES

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1. See D. J. de Solla Price, *Little Science, Big Science* (New York: Columbia University Press, 1963).

2. See N. Patel, 'Quantitative and Collaborative Trends in American Sociological Research', *American Sociologist*, Vol. 7 (1972), 5-6; R. K. Merton, *R. K. Merton, Sociological Ambivalence* (New York: Free Press, 1977), 51-53.

3. Since measures of production and citations are most frequently treated as intervening variables, they are particularly sensitive to measurement error. For a general discussion of this issue, see H. M. Blalock, *Causal Inferences in Nonexperimental Research* (Chapel Hill, NC: University of North Carolina Press, 1964). Allison examines this issue with a specific focus on production and citation measures: see P. D. Allison, *Processes of Stratification in Science* (unpublished PhD dissertation, University of Wisconsin, Madison, 1976), 10-17.

4. J. Cole and S. Cole, *Social Stratification in Science* (Chicago: The University of Chicago Press, 1973), 33.

5. Although the Coles specify citations, their recommendation would also apply to publication counts.

6. For a number of investigators (primarily those with substantial grant support), the chore of collecting data is substantially reduced because of access to Institute for Scientific Information (ISI) tapes of both the *Science Citation Index* and the *Social Science Citation Index*. The citation index lists only first authors. Since it is a machine-readable source, investigators using the ISI tapes need not hand-collect any data. It might be possible to use ISI tapes and include multiple-author information, but it would appear that the file manipulations involved could be quite extensive and costly. See E. Garfield, 'The 250 Most-Cited Primary Authors, 1961-75. Part 3: Each Author's Most-Cited Publication', *Current Contents*, Vol. 51 (19 December 1977), 5-20.

7. See N. K. Denzin, *The Research Act* (Chicago: Aldine, 1970).

8. See Allison, op. cit. note 3, 11-12.

9. See Allen Heffner, *Patterns of Collaboration in Science* (unpublished PhD dissertation, Purdue University, 1976). It is somewhat ironic that Heffner employed the straight count procedure, which disregards collaboration, in his study of collaboration. Similarly, the Coles have refuted the 'Ortega hypothesis' (the notion

that the average scientist actually makes a small contribution to science, and that these small contributions accumulate and form the foundation for major scientific achievements). With measures that allocate all credit in multiple-authored work to the first author, they demonstrate that the contributions of distinguished scientists (who usually appear as first author) could be achieved without the contribution of the average scientists (who usually appear as junior authors). Actually, the more serious error here is the assumption that what is measurable (in this case publications and citations) is what is important. It may be that what is important for many scientists is their *applications* work, even though it rarely culminates in publications. See J. R. Cole and S. Cole, 'The Ortega Hypothesis', *Science*, Vol. 178 (27 October 1972), 368-75, and op. cit. note 4.

10. Reskin provides an illustration of the bias introduced by straight counts:

> Since citation counts underestimate the number of citations for junior authors, I tried to assess the effect of this bias by examining the relations among the 98 chemists who had no junior-authored early publications. Among this subsample, the independent effect of early citations was 40 percent stronger than it was for the entire subsample.

See B. F. Reskin, 'Scientific Productivity and the Reward Structure of Science', *American Sociological Review*, Vol. 42 (1977), 491-504, quote at 500-01.

11. F. Narin, *Evaluative Bibliometrics: The Use of Publication and Citation Analysis in the Evaluation of Scientific Activity* (Cherry Hill, NJ: Computer Horizons, 1976), 125.

12. See J. R. Cole and S. Cole, 'Measuring the Quality of Sociological Research: Problems in the Use of the *Science Citation Index*', *American Sociologist*, Vol. 7 (1971), 23-29, at 28. The problem with this comparison is that both measures are inadequate. In this test, the Coles compare a straight count with a normal count and find the normal count wanting. Later in this paper, I propose a count adjusted for multiple authorship. This measure would be the more appropriate comparison.

13. Ernest Rudd, 'The Effect of Alphabetic Order of Author Listing on the Careers of Scientists', *Social Studies of Science*, Vol. 7 (1977), 268-69.

14. For further details, see D. Lindsey, *The Scientific Publication System in Social Science* (San Francisco, Calif.: Jossey-Bass, 1978).

15. See D. J. de Solla Price, 'Ethics of Scientific Publication', *Science*, Vol. 144 (8 May 1964), 655-57.

16. See, for instance, H. Zuckerman, 'Patterns of Name Ordering Among Authors of Scientific Papers: A Study of Symbolism and its Ambiguity', *American Journal of Sociology*, Vol. 74 (1968), 276-91; J. Simon, 'A Plan to Improve the Attribution of Scholarly Articles', *American Sociologist*, Vol. 5 (1970), 264-67; D. Spiegel and P. Keith-Spiegel, 'Assignment of Publication Credits', *American Psychologist*, Vol. 25 (1970), 738-47.

17. For discussion of some proposals, see B. T. Shaw, *The Use of Quality and Quantity of Publication as Criteria for Evaluating Scientists* (Washington, DC: Agriculture Research Service, USDA Miscellaneous Publication No. 1041, 1967), and W. M. Stallings and S. Singhal, 'Some Observations on the Relationship Between Research Productivity and Student Evaluations of Courses and Teaching', *American Sociologist*, Vol. 5 (1970), 141-43.

18. A. E. Nudelman and C. E. Landers, 'The Failure of 100 Divided by 3 to Equal 33⅓', *American Sociologist*, Vol. 7 (1972), 9.
19. See M. Oromaner, 'Collaboration and Impact: The Career of Multi-authored Publications', *Social Science Information*, Vol. 14 (1974), 147-55. Since Oromaner did not report a test of the probability of this finding, I computed a chi-square for his data (op. cit., 151), and found no significant difference in impact at the 0.05 probability level ($\chi^2 = 2.41$, $df = 2$, $N = 127$).
20. Narin, op. cit. note 11, 125.
21. For a discussion of this sample, see D. Lindsey, 'Distinction, Achievement, and Editorial Board Membership', *American Psychologist*, Vol. 31 (1976), 799-805.
22. D. J. de Solla Price and D. B. Beaver, 'Collaboration in an Invisible College', *American Psychologist*, Vol. 21 (1966), 1011-18, quote at 1014.
23. J. W. Tukey, 'The Future of Data Analysis', *Annals of Mathematical Statistics*, Vol. 33 (1962), 1-67, quote at 41.
24. For further details of this study, and discussion of the measures used in it, see Lindsey, op. cit. note 21.
25. For further discussion of this distinction, see D. Lindsey and T. Lindsey, 'The Outlook of Journal Editors and Referees on the Normative Criteria of Scientific Craftsmanship', *Quality and Quantity*, Vol. 12 (1978), 45-62.
26. See Reskin, op. cit. note 10.

Duncan Lindsey was a post-doctoral Fellow in the Research Program on Social Analyses of Science Systems (SASS) at Cornell University at the time this was written. He is now an Assistant Professor at the George Warren Brown School of Social Work, Washington University. His published work and current interests focus on the scientific publication system. He is the editor of *Children and Youth Services Review* and the author of *Children of the Asylum* (Oceanside, NY: Dabor Science Publishers, 1977) and *The Scientific Publication System in Social Science* (San Francisco: Jossey-Bass, 1978). *Author's address*: Box 1196, Washington University, St Louis, Missouri 63130, USA.