

Lord Kelvin is reputed to have published about 660 scientific papers in his working lifetime—almost one excellent paper every five weeks for a period of 67 years.<sup>1</sup> With typewriters and computers, not to mention laboratory technicians, why shouldn't we expect at least a few of our modern geniuses to be as prolific?

I say this because a number of readers expressed a certain skepticism about our recent data on most-cited authors.<sup>2</sup> R. A. Good, for example, was the author of at least 694 journal articles, 1961-1976. Since we excluded conference proceedings, books, reports, etc., from our analysis, certain authors may have written much more than our figures indicate. Sir Gustav Nossal was listed as the author of 99 journal articles, but we know that he was the author of at least twice this number of publications during the period.<sup>3</sup> However, it seems reasonable to expect that few modern authors are as prolific as Kelvin.

Figures, however, may be distorted by national customs. Professor Nossal, Director of the Hall Institute in Melbourne, points out that there is a great cultural difference between the British and Amer-

ican traditions of authorship: "In the United States it is virtually obligatory for the head of a group to put his name on a paper, even if he hasn't done much or perhaps any of the relevant laboratory work.... In the United Kingdom, on the other hand, it would be quite common for younger people, even postdoctoral fellows, to publish under their own names and only give an acknowledgement to their 'boss' by way of a footnote at the end of a paper."<sup>3</sup>

An institutional approach to citation analysis may solve some of the problems created by "cultural" differences. About one-third of the authors on our most-cited list were affiliated with about 5 universities or well-known institutes. (A complete table of affiliations will appear in a future essay.) But team research, however it is handled, raises important ethical questions about scientific authorship.

The dictum that "one should give credit where credit is due" is simplistic, to say the least. For example, there is no sure method for assigning credit when one author is the "major" contributor and two authors are "minor" contributors. Is the major author the one who originated the basic idea while his

"minor" collaborators merely did the laboratory work? And how major is the role of the person who did the writing job? Ghost-writing is not unknown in the scientific community. Indeed, the role of the technical reports writer is critical in scientific and technical communication. Brilliance in science is not always accompanied by literary talent.

Should authorship also be extended to administrators? And what about librarians or others who help collect data? Indeed, what about the colleague who was called in for professional advice?

In the days when most papers were written by one author, attribution of credit was not a problem. But the trend to multiple authorship has increased the importance of these ethical questions. Back in 1963 Derek de Solla Price noted in *Little science, big science* that the number of papers with three authors was already increasing faster than those with two. And four-author papers were increasing even more quickly than those with three.<sup>4</sup> More recent studies have substantiated the growth in multiple authorship.<sup>5,6</sup> A quick scan of *Current Contents*<sup>®</sup> attests to the preponderance of multiple-author papers, especially in the "hard" sciences.

Scientists have come up with many schemes for arranging the order of author by-lines to indicate the magnitude of each author's contribution. Sociologist Harriet Zuckerman has reported on several

of these methods. One "logical" technique involves a display of names in descending order of contribution. Thus, the first author contributed most, the last author least.

Another method is to place all other co-authors in alphabetical order, while the most significant contributor's name is placed first or last.<sup>7</sup> This is often done by an individual who has already received widespread recognition. And sometimes the most important contributor is designated as the reprint author.<sup>8</sup>

Certain research teams make no attempt to designate a single major contributor. The equality of all members is indicated by rotating names, alphabetically or otherwise, on a series of papers.<sup>7</sup> I believe this method also tends to increase the number of papers. Findings published in a series often could be published in one paper. This practice artificially inflates the cost of publication and indexing.

Some might argue that the best solution to this problem is to eliminate real names entirely. Thus, a group of mathematicians adopted the pseudonym "Bourbaki." The elimination of authors' real names was characteristic of Chinese publications in the days of the cultural revolution. But now we see in Chinese publications a return to papers on which the names of the authors are specified.

Many times the solution adopted is really no solution at all. For example, a wide practice, especially

in the United Kingdom, is for authors simply to list their names in alphabetical order. Thus, the reader has no idea which author was the major contributor, if there was one.

From the above mentioned practices it becomes evident that the use of so many different ordering patterns by journals only adds to the confusion. How is one to know from one article or journal to another which system is in operation? The problem is further complicated by other factors which militate against the usefulness of name-order patterns.

Price claimed that "new information-handling methods have a heavy impact on the intuitive ethic, such as it is, that governs the awarding of credit by deciding who is listed as first author in multiple authorship.... There is now at least ten times as much value in being listed first on the by-line as there is in being anywhere else on the rapidly growing list."<sup>9</sup> In most circumstances, it is claimed, the first author's name will be remembered and credited with the work if none of the authors is well known. In the *Science Citation Index*<sup>®</sup> this problem is accentuated because we use the first author's name as a simple means of abbreviating the citing and cited work.

No matter what order is adopted, the "Matthew Effect," a phenomenon described by Robert K. Merton, will cause misallocation of credit. Merton adopted the term "Matthew Effect" from a Bible

verse: "For unto everyone that hath shall be given, and he shall have abundance; but from him that hath not shall be taken away even that which he hath"<sup>10</sup> (Gospel according to St. Matthew). In other words, the work will be associated with the best-known person in the group.

Zuckerman noted that the "Matthew Effect" occurs frequently among Nobel laureates.<sup>11</sup> Although Nobelists frequently exercise *noblesse oblige* and give primary authorship to junior colleagues,<sup>12</sup> the work is usually attributed to the Nobelists by the scientific community.

Another factor working against name-order patterns is the policy of some *journals* of placing authors' names in alphabetical order — no matter how the authors would have arranged them themselves.

Alphabetizing names gives rise to the "second author syndrome," the semi-serious belief by co-authors that they are the victims of a subtle but vicious form of discrimination by the alphabetizers.<sup>13</sup> This is a variant on the "alphabetic disorder" described by Weston.<sup>14</sup>

Some people think that a journal's alphabetic arrangement of author names is of minor importance. But two psychologists provided evidence that this policy significantly influenced the content of one journal. They found that researchers whose surnames began with the letters P-Z avoided publication in the journal to a statistically significant degree.<sup>15</sup>

Probably no single pattern of name-ordering will ever be accept-

able to everyone. But there is no reason why authors must rely on name-order alone to indicate their contributions.

A 1970 study conducted by Don Spiegel (Brentwood Hospital, Los Angeles) and Patricia Keith-Spiegel (San Fernando Valley State College) surveyed 746 psychologists from a cross-section of specialties on the problems and ethics of assigning credit to authors.<sup>16</sup> Many of the respondents said the ideal solution to the multiple-author problem would be to explain each author's contribution in a footnote, thus eliminating ambiguities created by a particular ordering system.

Herbert Dardik, a fellow of the American College of Surgeons, supports this idea by suggesting that science follow the example set by the arts and music, where the contributions of many are clearly defined.<sup>6</sup> For example, in a theatrical production the director, set designer, stage manager, as well as the actors are clearly credited in the play bill. The members of the audience can judge for themselves the major and minor contributions to the production because they are aware of the value of each role. Unfortunately, the roles are not always that clear-cut in science. However, it would be refreshing to see an admission in print that "the following paper is based on an idea I heard at a party."

The convention of explaining contributions in a separate para-

graph can help alleviate the problem of allocating authorship credit. Actually, there are two aspects to this problem. One is the problem of the non-contributor who should not get co-authorship status, but obtains it by virtue of his or her power. The other is the problem of the real contributor who does *not* receive co-authorship status when in fact it is warranted.

In their book *Ethics in social and behavioral research*, Edward Diener (University of Illinois) and Rick Crandall (Texas Christian University) write that "little has been written on the guidelines that govern publication credit. Yet, publication issues often come before professional ethics committees."<sup>17</sup>

An informal survey of selected professional societies revealed that the ethics committees of these organizations acknowledge the existence of the problem. But they say they rarely, if ever, receive complaints.

Only the American Psychological Association reported that complaints about unfair assignment of authorship credit are received on a regular basis, perhaps five to ten a year. Maybe this is because APA is one of the few professional societies that has issued guidelines about authorship.

However, in a letter to the editor of *Drug Intelligence and Clinical Pharmacy*, three anonymous authors attacked the practice of su-

periors receiving authorship status for work performed completely by their subordinates.

"This is not only an act of egomania, but a means by which non-achievers fabricate achievement," the letter states. "A non-achiever could actually obtain a job over the achiever, simply because he has more publications listed on his curriculum vitae, thereby appearing to be more accomplished.... If the person(s) did not intend to use the false-authorship for further self-benefit, then why is it so important that they demand their name(s) be included? The reasons are obvious."<sup>18</sup>

Since the authors wished to remain anonymous, it is unlikely that we will ever know all the facts of their case. Stories of graduate students and others who are denied authorship credit are also difficult to substantiate.

What we do know from such letters and stories is that some members of the scientific community feel that the ethics of publication have not been defined clearly enough to meet the problems encountered today.

In my own experience I suspect that my mentor and colleagues at the time were more than generous in giving me credit as a co-author.<sup>19</sup> Under other circumstances I might have received a note in the last paragraph thanking me for preparing the chemicals involved or for confining all explosions to my side

of the laboratory. But in another instance I had to fight the resistance of my former boss when I tried to publish my first paper in a documentation journal.<sup>20</sup>

The Spiegel survey probed these problems, and the authors were able to draw some conclusions from psychologists' responses about the ethical determination of authorship status. They state: "It is unethical to give co-authorship to someone of higher status in one's organization unless he makes a substantial contribution to the project.... The results [of the survey] suggest that neither power nor status should be determinants of credit assignment."<sup>16</sup> By "substantial contribution" they mean actual work on the project.

Diener and Crandall concur: "The general principle governing publication credit is that authorship is assigned to individuals according to the magnitude of their scientific contribution to the study."<sup>17</sup> They define scientific contributions as the concept and design of the study and the writing of the report.

These general statements are less than definitive answers to the ethical questions of multiple authorship. Individual cases do not lend themselves to "prepackaged" solutions. However, general criteria are necessary so that decision-making on specific cases may operate within some broad standards.

Diener and Crandall also address a related problem: students who put

professors' names on their papers—not at the professors' behest—but because they are grateful to their teachers or feel their work will receive greater recognition if the higher-status individual's name appears along with their own. "Authorships should never be given out of gratitude or deference to persons of higher status," Diener and Crandall assert.<sup>17</sup> Again, the rule of thumb is the measure of significant contribution.

The Spiegel survey also elicited responses from psychologists on the rights of sub-doctoral-level workers and of scientists only tangentially involved in a project.

The Spiegel report concluded that paid personnel below the doctoral level who are part of the research team are entitled to the same credit as doctoral-level participants, if they make similar contributions. However, activities that do not affect the scientific character of the study do not deserve publication credit.<sup>16</sup> Diener and Crandall mention typing the report, computer programming for data analysis, and clerical work as activities within this category. These individuals, however, may deserve footnote credit.<sup>17</sup>

They add, "A person who only analyzes the results of a study usually does not deserve authorship unless the analysis represents an important contribution to understanding or rethinking the study. A

person who only collects data usually should not be an author, whereas a person whose contribution also includes planning the study or writing it up will deserve authorship."<sup>17</sup>

When the idea for the project comes from a colleague not part of a research team, respondents in the Spiegel survey suggested giving credit in a footnote to the originator. However, some respondents also noted that before work has begun, the originator of the idea should be invited to collaborate on the project.<sup>16</sup>

When a colleague provides assistance for a small portion of the study which requires his professional skill, recognition should be indicated in a footnote, according to respondents.<sup>16</sup>

To avoid the conflict over assigning credit after the project has been completed, Spiegel and Keith-Spiegel recommend a thorough discussion of assignment of responsibility and subsequent credit before work has begun. They caution to maintain flexibility, however, so that if individuals do not fulfill their responsibilities, adjustments in credit can be made. If all else fails, a neutral third party should be chosen to serve as an arbitrator.<sup>16</sup>

The conclusions of Diener and Crandall and Spiegel and Keith-Spiegel are drawn from the attitudes and circumstances of researchers in the behavioral sci-

ences. Perhaps the professional societies of other disciplines should run their own surveys and create guidelines appropriate to the work in those fields. If strict guidelines were adopted, they might deter the powerful from using their influence to get their names on papers to which they did not contribute. With guidelines, workers below the doctoral level and colleagues tangentially involved in the work would be apprised of their rights. They would feel less at the mercy of the persons running the project.

However, Spiegel and Keith-Spiegel conclude that guidelines, however fair and comprehensive they may be, will still not guarantee an equitable distribution of credit if the decision is left to a manipulative, egotistical, or unethical individual: "Unfortunately, it is the person with the most status and power who usually makes the ultimate credit determination. The lower status individual, if dependent on the higher status individual for his job, may be reluctant to even bring up the issue of credit."<sup>16</sup>

One of the problems with scientific journals is that they do not pro-

vide enough of an outlet for administrators. They should be encouraged to write periodic review papers in which they can demonstrate their role, if any, in the work reported.

In the absence of specific guidelines or other restraints, Dardik's words may serve as a strong warning and a general guideline: "Publications serve as the concrete art form for the scientist. It is his *modus operandi*. Authorship is akin to success and achievement. It cannot and should not deteriorate into a bargaining tool or commodity."<sup>6</sup>

Just as authorship can be abused so can citation practices. Referees have a right, indeed an obligation, to verify the claims of authorship just as they ought to insist on the proper and adequate selection of works to be cited. Only the most outrageous and persistent egomaniac could put his name on papers that were not his. But it is even more improbable that this same individual would be cited with a frequency that would distort the perception of his peers as to his real contribution to science.

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