Other Viewpoints about Authorship

The viewpoints about authorship criteria presented in the "Dialogue" section come out of the biomedical community and specifically from journal editors in that community. As the scientific publishing community works toward effective standards or guidelines for scientific authorship, the views and concerns of the scientific authors themselves need to be heard. To promote a discussion, we invited several scientific authors and author's editors from a variety of disciplines within the life sciences to present their views of the current "Dialogue" and to educate us about practices within their fields.

MM Tacker

John C Bailar III
Chair and Professor
Department of Health Studies
University of Chicago
Chicago, Illinois

In observance of the fine traditions of both science and CBE, I disagree with everybody, at least in part. Smith argues for replacing current notions of authorship with brief descriptions of what each person has done, but as he notes, this would be "truly a paradigm shift." A paradigm shift must be rare: a concept is not a paradigm in this sense unless it is deeply embedded and widely accepted. But he has not shown that the problem requires any such radical solution, and there are problems in his descriptive approach, too. One is the likelihood of overstatement of what each person contributed to the common enterprise. We would be very likely to find that the sum of the parts exceeds the whole manifold. Another is that the listing is certain to require substantially more space than a simple list of names of authors, with the added material of very little interest to anyone other than the authors themselves. Subscribers, readers, libraries, and others already have a hard time with both costs and storage, and it is not clear that paying for and filing descriptions of contributions would be in their interest.

Pitkin and Utiger argue, instead, for preserving the traditional concept of authorship, and would do so by asking investigators to be good citizens. This approach suffers from the serious flaw of not really addressing the present deep and broad problems. It is at least remotely possible that many investigators are not good citizens—at least in this respect. Perhaps they see inflated lists of publications as a victimless crime (it is not), or even as an acceptable way to get ahead (again, it is not).

The concept of responsibility, which implies a guarantee of the paper as a whole, is critical to the progress of science. If that kind of guarantee is not possible in any specific instance, the project is not appropriate for a single report as an original article. The work should be disassembled and reported in 2 or more papers for which each author can appropriately accept responsibility for the whole and agree to do so. Few authors, or none, should be cited on more than one of the papers. A integral part of this is my view that most papers have far too many authors anyway. (I often think of some unknown sage's proposal that editors allocate only a fixed amount of page space to listing the whole team: A single author might be recognized in 24-point type, while the names of 30 authors would become nearly illegible.) Authorship is not infinitely divisible; each added name dilutes the credit that can be attached to the others.

Where disassembly of a complex project is required, a review paper of the more traditional variety (again with one or few authors) should be used to bring them back together, with the author(s) of the review determined in the usual way for such work. Overall, this would preserve the present system, it would allocate responsibility in a more precise way (and, I hope, to smaller numbers of people per paper), it would facilitate reference when parts of the paper are used in other ways, and it would help preserve the useful distinction between what is an original contribution in the form of observations and what is a contribution in the form of synthesis. While this approach entails some risk of further reductions in the "least publishable unit," I think this risk is small. It would also, unfortunately, require something more in the way of journal space (I have not forgotten my comments above), but I would expect that in many cases the extra clarity and greater precision of citing components would be of value to readers.

Smith and Pitkin and Utiger agree that "honorary authorship" is inappropriate, despite its frequent appearance in all of our journals. I concur; it is generally nothing more than an exercise in power, and in reading papers and evaluating curricula vitae I customarily give no weight at all to the person named last.

Data documenting that many medical investigators believe that they were wrong fully excluded from authorship are useful. The question, as stated, is not symmetric (how often were they included when they should not have been), and even if it were, authors or would-be authors are not necessarily the best persons to judge who should be listed and who should not. This is especially true when most of them are not familiar with recent recommendations about standards and criteria.

Overall, I am convinced that authorship should be far more tightly restricted, and that the "rules" about authorship should be revised to hold each author responsible for the whole of what appears under his or her name. Even when we cannot disassemble a complex project into smaller parts that can be fully vouched for by individuals, we must remain responsible for our choice of colleagues. There is precedent: Business leaders are responsible for the whole of the organization for which they have oversight, and if something goes seriously wrong, they are held accountable, even though they had no prior knowledge of the matter. The same is true for military commanders. Should science settle for standards of responsibility that are lower than those in business or the military? I think not.
Elizabeth Wager
Medical Writer
Janssen-Cilag Ltd
Buckinghamshire, United Kingdom

Research sponsored by pharmaceutical companies involves a complex interplay of people from inside and outside the sponsoring company. The sheer number involved, even in straightforward trials, makes the allocation of authorship difficult. The Vancouver criteria assume that the designers of a study also performed it, analyzed it, and wrote up their findings. When the protocol is developed by a committee and refined by internal scrutiny, patients are recruited by a larger group of doctors, data are analyzed by a statistical department, and a report is written by a professional writer with the whole process coordinated by another company employee, the neat divisions of design, interpretation, and writing become blurred.

A major problem of the Vancouver criteria is their failure to acknowledge the contributions of the foot soldiers (such as the recruiting doctors and technicians) who often contribute more hours work to the study than the officers who designed the protocol. Although participating doctors are paid, they also hope to see their names on papers. Companies are aware of this and may include as authors the doctors who recruited the most patients, regardless of the doctors’ involvement in other aspects of the study.

Abstracts may be submitted to different meetings, quite legitimately, to encourage discussion before full publication. Multidisciplinary studies may have several audiences, and presenters may be allocated accordingly. For a small group of authors this poses few problems, but for large studies the author carousel may be confusing and even lead to allegations of misconduct (1,2).

Beliefs about readers’ perceptions may encourage authorship omission. Even when company funding is acknowledged, the marketers may still prefer not to see employees named as authors, although this practice seems to be decreasing (3). Just as gift authorship may occur in academia, including a “big name” doctor who had limited involvement with a study may be condoned because it gives a paper more credibility.

Stringent audit requirements from regulatory authorities mean that industry-sponsored studies are very strictly policed. Ultimately, it is the sponsoring company who takes corporate responsibility for the study (and whose profits will be affected by the outcome). Thus, institutional responsibility might apply as well to commercial as to academic institutions. I recently surveyed company employees involved with publications and found that only 55% were familiar with the Vancouver authorship criteria and only half of these found them useful: most found them too restrictive. About the same number (and the same people) said their company had an authorship policy, but I have never seen these policies published (unpublished data submitted for the 1997 International Congress on Biomedical Peer Review and Global Communications).

Roy Pitkin talks about the “maintenance of the highest possible standards” as if the Vancouver criteria were widely known, widely accepted, and working well. I suggest that, within industry, the criteria are not well known and even less well used. An impractical standard is not a high standard: good criteria should encourage openness and be perceived as fair. Readers and editors need accurate information to judge potential conflicts of interest and inappropriate behavior: restrictive criteria do not encourage such disclosure (4). Employees and external investigators need to feel that their efforts are justly rewarded. Criteria that achieve both openness and fairness will have no difficulty achieving wide acceptance. Abandoning our old notions of authorship and reorganizing the reward system that has built up around them will be a harder task.

References
port, explain, and enforce them (as they would copyright or similar regulations). When author-clients ask me what happens if existing criteria are violated, I can only answer "it depends." To my knowledge (and I'd be happy to be corrected), the *American Journal of Obstetrics and Gynecology* is the only peer-reviewed journal that has published a list of sanctions (1).

To restate the obvious, no set of authorship criteria will please everyone. And it's true that we'll never prevent all instances of gratuitous, omitted, or "surprise" (2) authorship. So, to paraphrase Smith, Why bother? Because authorship remains not only the coin of the academic realm but an integral part of the research and publication process in which we are all involved. Reliance on self-described "contributorship" would not prevent abuses—it would simply change the way in which they were perpetrated. I agree that changes are needed. I simply don't think the bathwater is that dirty.

Published discussions like this one are a step in the right direction, but they reach a limited audience. I would very much like to see the so-called Vancouver group broaden to include representatives of "hard science" journals, given the increasingly indistinct lines between basic and clinical research, and the very different authorship cultures of these once-separate groups. The Vancouverites should invite input from, if not formal liaison with, groups such as AAAS, NAS, the Association of American Medical Colleges, and their counterparts in other nations. Also, I think it's time to bring the debate to those who are most affected by the outcome—the authors. This could be done through an organized series of discussions and debates at the meetings of medical and scientific associations and societies, perhaps under the aegis of CBE.

References

Stuart I Offenbach
Professor
Department of Psychology
Purdue University
West Lafayette, Indiana

Dialogues about authorship have continued in the "Letters" section of *Science* (1) and in *The Scientist* (2,3). Electronic dialogue also has appeared, notably on a list called "SciFraud". Add to these the "op-ed" pieces by Richard Smith and by Roy Pitkin and Robert Utiger in *CBE Views*, and we now seem to have an issue to be debated. Authorship is, as your commentators point out, the "coin of the realm" in academe and is used to "buy" promotions, salary increases, graduate students, and personal recognition. As Pitkin and Utiger correctly note, this system works most of the time. In fact, only last year my faculty colleagues in the Department of Psychology reemphasized their intent to rely on 1st-author publications (in refereed journals) as a (perhaps THE) significant measuring stick used to select mentors for graduate students, be promoted, and receive departmental resources. So if the system is acceptable, what is the problem? The "problem" is not really whether your name at the head of a manuscript means you are the creator of the text. Rather, the problem is what readers, typically one's peers and colleagues, assume about your relationship to the published work. The members of the academy do not wish to pay out that coin if the product is overvalued.

We assume that an "author" has played a (perhaps THE) significant role by producing the ideas of the study, designing the test, and using his or her professional expertise to analyze the results. The problem is that many articles now have more than 1 person listed as an author, and some readers have lost confidence that being an author still has the same meaning. Why have we lost confidence? Two significant factors may be a) the financial difficulty of carrying out one's own research and b) the consequent increase in multiple authorship. Much of today's research requires, at the very least, contributions from others of equipment, financial resources, time, and energy. The issue is how to reward or acknowledge such assistance in "our" research. The apparent solution has been to pay our "debts" in the standard "coin of the realm" by making that contributor an author. One result of that practice has been an increase in articles with multiple authors.

Certainly that seems to be the case in developmental psychology. I examined several issues of the journal *Child Development* to check the accuracy of my perception. In 1977, 12% of the published articles (4 of 50) had 4 or more authors. In 1996, 67.5% of the articles (17 of 43) had 4 or more "authors", a 4-fold increase. Over the same period of time, the average number of "authors" per paper increased by one (almost a 50% increase).

These data may not reflect a change in what it means to be an author, but how are today's readers to tell? What we readers want to know is whether our assumptions about the involvement in the research by each author are correct. For example, I had no way to decide whether any or all of the 139 authors of the 1996 *Child Development* articles met the stringent criteria for authorship described in the articles by Pitkin and Utiger and by Smith. The group probably included students learning to conduct research, post-doctoral students honing their skills, and skilled professionals and technicians operating complex equipment or analyzing data. Not knowing their contributions, some have assumed that not all authors met the criteria and have lost confidence in the system. Now
those scholars are seeking a solution.

I do not agree with Smith, who wants to replace the system. I am not certain that such a change could be achieved or that the resulting dislocation would be "cost-effective". Instead, I think, as apparently do Pittkin and Utiger, that some changes could restore our confidence in authorship. I suggest that 2 changes be made. First, every article published should include a footnote describing the contributions of each author. That is a rare practice today, at least in the Developmental Psychology articles I examined. There, only 10 sets of authors attempted to tell me anything about their contributions. These articles included a footnote that described the nature of the authors' contributions (3 were dissertations, 3 were master's theses, and 4 described the contributions by some or all authors as equal). Such a footnote should be mandatory for all articles that have more than 1 author. Second, I would encourage adoption of a practice of listing some authors as "Contributing Authors". For example, an individual who had responsibility for an experiment reported in a multi-experiment report would be listed as a contributing author. Perhaps individuals responsible for designing the analysis of experiments might also be so listed. In any event, an individual would be seen to have made a concrete (and significant) contribution to a published report. I believe these 2 changes would at least be possible, and that we in the academy could learn to interpret multi-authored publications fairly.

References

Authorship in botany and related plant sciences seems to be based on the widely believed but uncertain criteria of "ownership" and "responsibility". Two traditions predominate.

1. The dependence of principal investigators (PIs) on research grants has had the same consequences in botany as in other fields. Graduate students or individuals who "did the work" are 1st authors, principal investigators are the last authors, and the rest are packed in between. Neither the writing author nor the corresponding author receive preferential treatment. A recent development is the footnoted co-1st-authorship (I first saw it in The Plant Cell), where 2 of the 3 authors were footnoted with a categorical statement that they had both made equal contributions and should both be considered to be the senior author. The plant ecological community, which relies on big field teams, seems to give authorship to students with a thesis resting on the work or to the PIs and leave the rest to acknowledgments.

Some leaders of big laboratories have their name on every paper. The decision is never a conscious one. All new members of such labs soon learn that this is the cultural expectation.

2. Authorship in biological taxonomy brings a continuing recognition because the author or authors who first describe the taxon are named in the complete citation of the scientific name. Most papers describing new species or providing monographic revisions have only 1 or maybe 2 authors. Taxonomy and systematics relies both on experimental and observational data and very heavily on informed judgments, which may lead a student and a professor to disagree about a particular taxonomic decision. This often leads to single authorship by a student and acknowledgment of the supervising scientist(s). The arrival of molecular biology may lead to major taxonomic revisions being multi-authored. We will have to see how the community deals with this.

My institution, the University of British Columbia, has recently developed a policy on scholarly integrity. It reads:

"All authors listed should have been involved in the research. Each is expected to have made a significant intellectual or practical contribution, understand the significance of the conclusions, and be able to share responsibility for the content and reliability of the reported data. All authors listed should have seen and approved a manuscript before submission. The concept of 'honorary authorship' is unacceptable. There should be guidelines developed and discussed within each unit regarding conditions of authorship for research trainees. These guidelines should be discussed with the trainees before the research is begun or they become involved in it." (University of British Columbia, Policy Handbook, Policy #85, 1997)

I am not aware of any journals that publish specific guidelines on authorship. Taxonomists have the tradition that authors' names are attached to taxonomic names. Other disciplines are more driven by the changing scene in experimental, nonclinical biology. My guess is that more than 99% of botanical authors have not heard of the Vancouver Group! The Canadian Journal of Botany (see Can J Bot 1997;75:1-1) and several other journals require some document (usually the submission letter) from the corresponding author affirming that all authors...
have read and approved the manuscript.

I believe that I and other botanical authors would support the presentation by Pritkin and Uijter. I am not ready to see authorship reduced to a percentage footnote. The question, beyond the facts of tradition and professional assessment, lies in the fact that authorship assigns credit and priority to the individuals who do the work. It is common to hear complaints about multi-authored papers that leave us wondering which author was the bus driver who brought the PI to work on the day the paper was written! Several (not me) also view the prestigious journals with suspicion, especially in the "hot" areas where the gossipindicts the Ph.D.s with grant writing and little more. It is not uncommon in botany, especially for students or post-docs supervised by older colleagues, to see papers published by a graduate student or a post-doc alone with the PI being acknowledged. Certainly, statistical analysis often earns authorship, but that may be because those who need statistics are also the active researchers and will become deeply involved in a project.

Poly M Potter
Chief
Publications
Centers for Disease Control and Prevention
Atlanta, Georgia

Like most groups that have wrestled with the issue of authorship, the Centers for Disease Control and Prevention (CDC) recognizes that no single set of rules is likely to settle all arguments, resolve all questions, or even apply to all cases. Authorship problems vary with the environment in which a manuscript is produced. In government, as in many other areas, publishing an article is the natural by-product of completed work; however, a publication is often not seen as an individual achievement but rather as an agency product performed as public service and distributed without copyright. In some cases, the authors are not even listed on the final publication. The Morbidity and Mortality Weekly Report, published by CDC, is a case in point. But even in an environment where authorship is not showcased, procedural questions remain: When should officials from various states working with CDC authors on epidemiologic investigations be listed as authors? Should scientists be named as authors when not listing them may jeopardize the collection of surveillance data from a state? To answer these and other questions, to address concerns specific to government publishing, and to facilitate the decision-making involved in the authorship of hundreds of scientific articles submitted to peer-reviewed journals each year, CDC's Excellence in Science Committee, Procure-

ment and Grants Office, and Office of General Counsel have set up general guidelines (CDC-69, rev. 1995).

According to the CDC guidelines, the planning and development of a project and the preparation of manuscript(s) for publication should include defining responsibility for the various work components of a manuscript and establishing minimum basis for authorship. Authorship requires active participation in all of the following: the conception or design of work; the collection, analysis, or interpretation of data; drafting of the manuscript or reviewing and revising critical sections (a critical section is defined as the specific portion of the manuscript for which a coauthor claims expert defense responsibility); and assuming responsibility for the final version of the manuscript.

CDC's authorship criteria also cover multi-authorship, including cases warranting exception; determination of the primary author; listing of secondary authors; and credit lines other than authorship. Even though multi-authorship is not a problem in itself, it raises many issues, such as criteria for inclusion as an author, ability of each author to evaluate and defend all aspects of a study, order of listing coauthors, and separation of results to increase numbers of communications and authorship citations. Therefore, in interdisciplinary studies, authors may prepare brief statements of the exact contribution of each author to the work described. To maintain productive relationships with outside agencies and individual scientists, authors who represent CDC are urged to be sensitive and systematic in assessing coauthorship expectations, entitlement, and listing of coauthors. Primarily authorship requires actual contributions in the conception, planning, and execution of the study. Secondary authorship is determined according to the order of magnitude and pertinence of the author's input. Collaborators whose contributions do not warrant authorship credit are listed in the acknowledgment.

CDC's editorial staff (if not the CDC authors themselves) and the committee that produced the authorship guidelines were familiar with the "Uniform Requirements"; however, by drafting a more agency-specific guide defining the various components of a manuscript for which various authors could take direct responsibility, the committee was perhaps moving away from a monolithic set of guidelines and toward a guide that more specifically addresses each author's contribution to a manuscript.

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Who Cares about Authorship Rules? Editors Do, Authors and Candlestick Makers Might, Readers Do Not

Annette Flanagan
Associate Senior Editor
JAMA
Chicago, Illinois

Last July, George Lundberg and Richard Glass published an editorial (1) in JAMA describing their journal’s policy on authorship and the recently reaffirmed statement on authorship by the International Committee of Medical Journal Editors (ICMJE). The ICMJE statement contained 1 modification: “Editors may ask authors to describe what each contributed; this information may be published” (1). The JAMA editorial was written partly in response to discussion that occurred during the ICMJE’s annual meeting, which was held 2 days after a conference on authorship was convened by Richard Horton (from The Lancet) and Richard Smith (from the British Medical Journal) in Nottingham (2,3). At Nottingham, editorial pundits criticized the ICMJE authorship criteria as being impractical, too rigid, and largely unknown by biomedical authors, at least on 1 side of the Atlantic.

In their editorial, Lundberg and Glass explained that, although agreement on the criteria for authorship was not unanimous among the editors attending the ICMJE meeting, consensus was reached. Recognizing that some editors, including Drummond Rennie (another JAMA editor), believe that change is still needed, Lundberg and Glass invited readers and authors to submit their views on the usefulness of the ICMJE statement on authorship. Few responses were received, and those that came in reflected a disappointing range of attitudes—unfocused ambivalence to concerned concurrence.

Two replies were published in the correspondence columns of a subsequent issue of JAMA. Both letters agreed with the principles of the ICMJE criteria. The author of 1 of the letters explained how he uses and enforces the ICMJE criteria in his role as a managing editor (4). The other letter suggested that the criteria be modified to allow computer programmers to be listed as authors (5). Lundberg and Glass (6) replied that anyone who meets the 3 criteria for authorship is an author regardless of their job title. Here, care should be taken to avoid a narrow interpretation of the 1st ICMJE criteria “conception and design; or analysis and interpretation of data.” A programmer might easily be part of analysis and interpretation. As long as a programmer, or even a baker or candlestick maker, also meet the 2nd and 3rd criteria (drafting or critically revising the article and approving the final version to be published), they are entitled to authorship.

References