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## CSE TASK FORCE ON AUTHORSHIP

### DRAFT WHITE PAPER

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### Introduction

#### Richard Horton

The White Paper Group wants to provoke as well as to summarise, inform and propose. The contributors, if I may betray an immediate bias, have done so by, first, questioning as many assumptions underlying the concept of authorship as they can discover, and then, second, by questioning those questions again. This meta-questioning – of credit, responsibility, disclosure, intention, entitlement, order, replication, rules, integrity, authority, the nature of research work, scientific relationships, the scientist as person, interdisciplinary histories, and ghosts and guests – is necessary because, despite the advent of contributorship in 1997, its impact on the academic community has been only modest. Several editors have used the debates surrounding authorship as a peg to hang further calls for a careful prising apart of promotion from publication, an indecent liaison that lies behind much authorship abuse. These pitiful cries have mostly drifted silently into academic winds of indifference.

By the press of a button, this equilibrium has been violently disturbed. Harold Varmus, once

a student of Romantic poetry and now, the poetic legacy having left its mark, a quiet revolutionary leading the US National Institutes of Health, has tipped the science publishing community on its head. With e-biomed, Varmus is asking scientists to challenge the power of publishers to control scientific communication. The only surprise is that it has taken him so long to do so. If his proposal is implemented, the status of the author will rise to more than mere supplicant to the editor. All to the good, perhaps, but that shift in power will also change the nature of authorship since lines of accountability may be diluted if editors become simply file managers rather than awkward critics. An alternative, altogether more positive view, of course, may be that a vast global database of research, with no space in elite journals to compete for, will democratise science and thereby lessen the greed for publication and possibly even diminish the temptation to commit fraud, authorship or otherwise. It is time to engage one another in persistent and difficult argument. There has never been a more important moment to define the scientist as author, or to locate, if one can, the author as arbitrator of not only the scientific record but also our epistemological traditions.

## **2. The Problem**

### **Mario Biagioli**

Like a coin, authorship has two sides: credit and responsibility. One receives professional credit from his/her publications and takes responsibility for their contents. Few would dispute the basic fairness and clarity of this position. But while two sides of a coin, once stamped on it, are locked into a stable complementary position, credit and responsibility are much harder to keep together under the category of authorship. This is not only because of a human tendency to try to maximize one's credit while minimizing one's responsibility, but also because common definitions of credit and responsibility do not always fit the reality of research practices. They create loopholes in which misconduct can breed or, alternatively, impose unrealistic demands on the practitioners.

While credit and responsibility are difficult to define under any circumstance, the problem has become more acute in modern research contexts – contexts in which the traditional figure of the individual author has been replaced by large groups of collaborating practitioners with different disciplinary expertise, different tasks, and often working in different locations and institutions. Many of the current problems of authorship can be traced back to the application of an outdated conception of individual authorship to research environments that have outgrown it. A redefinition of authorship is, in practice, a definition of multi-authorship.

The main question about credit is: credit for what? The ICMJE guidelines list the kind of contributions that qualify for authorship credit, leaving the rest, presumably, for the acknowledgment page: "Authorship credit should be based only on substantial contributions to (1) conception and design, or analysis and interpretation of data; and to (2) drafting the

article or revising it critically for important intellectual content; and on (3) final approval of the version to be published. Conditions 1, 2 and 3 must be all met" (1).

This definition begs a number of questions: Should authorial credit be limited to conception, data analysis, writing and the other tasks listed by the ICMJE or should it be extended to those kinds of contributions that provide the broader conditions of possibility for the study? How cognitively tenable or professionally fair is the distinction between authors and acknowledgees? Is this a distinction about cognitive and non-cognitive tasks or is it the result of the social organisation of research? Are there ways to qualify and quantify credit over a continuous spectrum without introducing a two-tier system? Should credit be attached only to the cognitive results of the research effort or should it be about work and labour? Is some level of misrepresentation of the research process introduced by not mentioning contributors who (like laboratory technicians and professional writers) may be satisfied with monetary rewards but no authorial credit? Should their contributions be publicly credited anyway? In sum, definitions of credit are not just about credit but also about disclosure and transparency.

Different constituencies may attach different significance to these questions. Researchers (especially junior ones) sometimes complain that they have been denied credit as authors, or that at least their authorial contribution has been underrepresented in the final publication. From the legal point of view, this could be read as an accusation of plagiarism, but one could also see it as the result of a too narrow definition of credit that failed to recognize certain kinds of contributions. It could be a problem of taxonomy, not just of bad intentions or of excessive perceptions of one's entitlement. Symmetrically, cases of honorific and gift authorship point to situations in which authorial credit has been extended too widely. While these practices are unacceptable, they also point to a deeper problem with the categorization of authorial credit – a categorization that denies credit to senior practitioners whose work has been important, in some ways, to the production of the published results. The practice of honorific authorship and the denial of credit sometimes experienced by less powerful members of the research community may share a common root in a too narrow definition of credit. That some people receive too much credit while others receive too little may have to do with a two-tier definition of credit and the abuses it is likely to engender (no matter what the direction of the abuse might be).

The predicament of journal editors is different from that of practitioners, but they too have to confront the problem of authorship credit. Although editors do not (and probably cannot) certify the fairness of the distribution of credit among a paper's authors and contributors, they do not want to become tacit ratifiers of unsavory practices or to become entangled in disputes about authorship that they are not in a position to control or settle. Having each contributor sign off on the arrangement of the author byline may provide some technical and legal protection for journals. But that should not stop editors from wondering whether the process that led to that document has been as fair and democratic as it should have been (especially knowing that any two-tier definition of credit is likely to create tensions and possible abuses). Additionally, editors are confronted with the question of how to assess and reward the work of reviewers whose role is crucial to the peer-review system and to the assignment of authorial credit. Should reviewers be given explicit credit for their work? If so, what kind of credit? Would that credit violate the anonymity of the peer-review process?

If so, can we live with that or should the referees pay for the anonymity we see as integral to peer-review? Do we know how reviewers feel about that?

Definitions of credit are crucial to academic institutions and funding agencies as well. How can they assess the value of the currency of authorship credit as they go through the process of promotion or grant evaluation? What is the relationship between authorship credit and academic credit? How can an institution assess an individual's authorial contribution to a multi-authored publication? What is the meaning of the order of authorship? Furthermore, the same tensions found in the ICMJE guidelines between the assigning of credit to cognitive tasks or to the work done by each and every contributor re-emerge in the context of institutional decisions about academic reward. If credit were to be attached only to work then it would make sense to base these decisions on a consideration of how much work the candidate had done, that is, on the quantitative sum of his contributions. But this approach would blur the distinction between quantity and quality (assuming that we have clear and distinct ideas about what that distinction means). The paradox, then, is that if we define credit in terms of work we may get a fairer description of the research process (and one that would discourage abuses in the assignment of credit) and yet we may end up weakening the role of authorship as a tool for academic reward. If, instead, we define authorship credit narrowly, we may preserve the value of the coin, but may also open the door to a "black market". These tensions do not mean that we should be skeptical about the possibility of a workable system of reward, but simply that there may be no magic bullets, only democratically negotiated compromises.

Moving on to responsibility – the other side of the authorship coin – the questions cluster around the issue of misconduct and fraud. Unlike credit, responsibility becomes an issue only when the content of a publication is challenged. Most frequently, definitions of responsibility have been about identifying the person or persons on whom responsibility can be pinned. Less frequently has the question been asked about who is responsible to whom (and not just for what). The elision of the second part of the question may have to do with the difficulties in coming to terms with what responsibility means. The ICMJE defines the author as the person responsible for all aspects of the publication. If there are many authors, each of them is responsible for the entire publication, not just the specific sections or aspects they have worked on (2). The logic of this position is as clear as the image of the two-sided coin, but its clarity fades when we look at the details.

In what sense can we say that an author is responsible for the work done by a team? The ICMJE definition of responsibility uses an individualistic notion of authorship and applies it to collective contexts of research. It does make sense to say that an individual author is responsible for what he/she makes, but how is one supposed to be responsible for someone else's work short of replicating that work? How can we equate "checking" to "doing"? As one critic put it, the assumption that an individual could and should be responsible for a team's work casts such an individual as omniscient, but omniscient authors wouldn't need collaborators to begin with (3). That the conceptual problems inherent in the extension of individualistic notions of responsibility to collective contexts have been ignored by the ICMJE indicates that the social need to attach responsibility somewhere is so strong that it must be answered, no matter how. It is a bit like passing laws to contain a problem that must be contained and yet cannot be legislated away.

The problem, then, is not to do away with responsibility, but to discuss what we really mean by it before we focus on identifying the person on whom responsibility should be pinned. From a legal point of view, fraud is a puzzling notion. Technically, it could be seen as libel against nature. But nature is not a legal subject and it cannot take its libelers to court. Therefore, the party damaged by fraud and misconduct is not "truth" itself but has to be the scientific community and, in certain cases, the funding agencies. If we agree on this, the problem of responsibility is recast in terms of what are the specific damages produced by fraud, and who is affected by these damages. The focus shifts from an exclusive concern with identifying the potential "culprit" (as the ICMJE tends to do) to figuring out how the person responsible for the problem should do reparation (for what and to whom). The problem with current definitions is that they are more punitive than operational and they cast responsibility as an object rather than as a multi-layered process.

From the practitioners' point of view, the problem is to define responsibility in ways that make sense within their research practices and, therefore, make it enforceable. How can responsibility be defined in ways that allow it to be distributed fairly among contributors? How can endless finger-pointing be avoided or controlled? What kind of definition of responsibility would enable due process for those accused of misconduct and fraud? In addition to these questions, editors are concerned with the role of responsibility in the peer-review process. How much responsibility (if any) should be shared by editors and referees? And how? How should editors handle allegations of misconduct? Who should pursue them? Who should be responsible for retracting a flawed publication? Finally, definitions of responsibility are not limited to the content of a publication but extend to its funding and the possible conflicts of interest that may entail.

In short, responsibility means many different things, and not all of them are about authors. The challenge for future definitions is to establish the many shapes of responsibility, the processes through which it can be assessed and contested, and damages established and redressed. More importantly, these definitions should not focus only on authorial responsibility but also on the mutual obligations between authors, editors, and institutions – issues that have been largely overlooked in the past.

## References and footnotes

1. *JAMA* 1997; **277**: 928.
2. "All members of the group who are named as authors, either in the authorship position below the title or in a footnote, should fully meet the above criteria for authorship." *JAMA* 1997; **277**: 928.
3. Loehle C. Responsibility of co-authors. *Science* 1997; **275**:14.

## 3. The current research environment

## Pamela Derish

*In our society there had always been a preoccupation with the scientific hero who comes back with a major revelation after having wrestled with his angel in self-imposed isolation (i.e., Newton, Röntgen) or in relative obscurity (i.e., Curie, Einstein).*

Gerald Holton, *Thematic Origins of Scientific Thought Kepler to Einstein*, 1973 (1)

*You are too clever not to see the good of a bold fresh mind in medicine, as well as in everything else; and as to cholera, I fancy, none of you are very sure what you ought to do. If a man goes a little too far along a new road, it is usually himself that he harms more than anyone else.*

George Eliot, *Middlemarch*, 1871-2 (2)

The long-standing image of the scientist as isolated from life – the lone genius – is one that appeals to Nobel Committees, promotion committees, corporate and university public relations, as well as to science journalists, all of whom like to associate a unique *idea* with a unique person (3). The public image of scientists as ruffled and idiosyncratic – picture Einstein on his bicycle – is currently being mined in advertisements to sell computers, under the rubric "Think different." Einstein himself wrote of scientists "who found favor with the Angel" as mostly "rather odd, uncommunicative, solitary fellows" (4). He was referring to his fellow physicists, yet, writing in 1973 on modern science and the intellectual tradition, the physicist and historian of science Gerald Holton noted that the typical physicist now often belonged to "a sizeable group working under a contract with a substantial budget" (5). He added that science has, as result, "become a large-scale operation with a potential for immediate and world-wide effects," and that although the results are a "splendid increase in knowledge", there are also side effects "analogous to those of sudden and rapid urbanization – a strain on communication facilities, the rise of an administrative bureaucracy, the depersonalization of some human relationships" (6).

Echoing Holton some fifteen years later, the National Academy of Sciences published a report called *Responsible Science*, which, in exploring the customs and traditions of the current research environment, identified several factors as contributing to the evolving research scene, including increasing complexity of contemporary research problems and instrumentation, and the changing character of collaborative efforts (7). Ethnographers and historians of laboratory science have detailed the "heterogeneity of elements involved in scientific work" and have described science "as heterogeneously engineered" (8). Biomedical research has been likened by one sociologist to "more an orchestrated production than a creative act by individual scientists" (9). Contemporary sociological understanding of science suggests that many scientists believe there are distinctive norms that guide their practice, but that a major gap exists between scientists' self-representation and the representations of scientists by those who study them (10). Many scientists have, perhaps without knowing it, given voice to this tension by acknowledging that the intense pressures on senior scientists to secure funding have come at a high cost to their traditional

mentorship role (11-15).

A duality exists within the current research environment: science is often a collaborative pursuit in terms of finding "the truth", but it is always an individual pursuit in terms of reward. This was recently illustrated by awarding the Nobel Prize for the discovery of the polymerase chain reaction (PCR). PCR itself and the refinements that made a great idea a technical reality were achieved through teamwork at Cetus – a corporation that had embodied the best of collaborative science, rewarding individuals for their contributions irrespective of their degrees. The ultimate reward for "the discovery" went to just one scientist, however – to Kary Mullis. His fellow scientists at Cetus voiced conflicting reactions to the award: gladness to see the technique recognized, but frustration that awarding the prize to Mullis alone lent credence to Mullis's "fable" or "PCR creation myth" (16), in which he cast himself in the mold of scientific iconoclast, much along the lines of the scientific "hero" described by Holton. These scientists had viewed PCR as a powerful means to enable them to do further innovative research; Mullis appears to have viewed it as a means to celebrity (17). Sour grapes? Perhaps.

One sociologist, Robert Merton, points out that the main rewards scientists desire for their achievements are community recognition and prestige (18). That community is still governed by traditions derived from an earlier model, a community of independent scholars with a reward system "influenced largely by research performance and productivity, sometimes measured by the number of publications or total amount of research support acquired by an individual faculty member" (19). Academic physicians in medical schools have not been assimilated quite equitably into that community, as it is generally acknowledged that basic or "bench" research is associated with greater prestige and greater potential for academic success than is likely to be rewarded by tenure and promotion committees (21). Another inequity lies in funding for clinical research, which – as compared with funds for basic biomedical science – has been relatively hard come by, enhancing the conviction within academic medicine that MDs in general, and clinical investigators in particular, are at a disadvantage when competing for NIH grants (22).

Despite the academic physicians' obligation to render ample clinical activities in service to the institution (23), an obligation that has intensified under managed care (24), their rewards still depend on productivity measured chiefly by their publications and research funding. Nonetheless, when they contribute to the large-scale multicenter clinical trials that offer the greatest promise for shaping therapeutic approaches, their names are deemed worthy of publication by *The New England Journal of Medicine* only if they fit into a single column of print (25), a disincentive if ever there was one (26).

*For centuries, scientists have looked at their endeavors as the central, truth-seeking process in modern culture; the embodiment, as it were, of the classical virtues of truth, of goodness, and of beauty. Scientists have been proud to claim their work as embodying the best of the ethos of practical goodness in this imperfect world through the largely self-corrective practice of honor in science and through its tendency to lead to applications that may improve the human condition.*

Gerald Holton, 1993 (27)

*[D.] Lydgate certainly had good reason to reflect on the service his practice did him in counteracting his personal cares. He had no longer free energy enough for spontaneous research and speculative thinking, but by the bedside of patients, the direct external calls on his judgment and sympathies brought the added impulse needed to draw him out of himself.*

George Eliot, *Middlemarch* (28)

Eliot's physician has intrinsic flaws that conspire with his circumstances – among them, the need to make a decent living – to stifle his research ambitions. However, as to those circumstances, it is noteworthy that during the nineteenth century, medical school and hospital appointments were generally awarded on the basis of social and political factors rather than accomplishments or even interest in research, much less a publication list (29). In the current research environment in the United States, the liberal financial support from the Rockefeller Foundation that made academic medicine possible by relieving medical scientists of the need to generate their own incomes from medical practice (30) no longer exists, and in fact physicians in biomedical research universities not only have funded their own research with the earnings from their clinical practice, but may even see those earnings applied to fund other others' basic research through institutional grants.

The NIH states emphatically that physicians trained to perform clinical research are vital to the NIH mission, that without them, the mission will fail, and that even suggesting that the mission of the NIH might fail is unacceptable because "The future health of all Americans and of the world's population depends on the continued success of the NIH." (31). The solution seems obvious.

## **Acknowledgements**

I would like to thank Susan Eastwood and Melinda Derish for their insights and suggestions, which have greatly influenced the content of this manuscript.

## **References and footnotes**

1. G Holton. *Thematic Origins of Scientific Thought Kepler to Einstein*. Cambridge: Harvard University Press, 1973: 400.
2. Cholera figures only briefly in the novel, but there had been a major outbreak of the disease in England as late as 1866, five years before *Middlemarch* was published (WJ Harvey, *Introduction to the Penguin English Library Edition*, Hazell Watson & Viney Ltd, 1965 (reprinted in 1981), p.17). The "man" referred to in the quote is Dr. Lydgate, probably the first fictional *new kind* of doctor to be delineated with historical precision – his use of the stethoscope and his wish to conduct post-mortems place him in the "avant garde of medicine" (*ibid*,p. 20). George Eliot herself asks her readers, "Does it seem incongruous to you that a Middlemarch surgeon should dream of himself as a discoverer? Most of us, indeed, know little of the great originators until they have been lifted up among the constellations and already rule our fates" (p.175). Harvey adds that by hoping to advance medical research *and* practice in the provincial town of Middlemarch, Lydgate is a prophetic figure, and that his story is "in large part the defeat of the man of the future by the stubborn

conservatism of the present." (p. 20).

3. The quote appears in Holton's summary of an address Einstein gave in 1918 in honour of Max Planck. *Thematic Origins of Scientific Thought Kepler to Einstein, op cit*, p 376. Thought he refers only to physicists, the "image" seems to have crossed the borders into other scientific disciplines. The image itself seems to have distinct roots in the Judeo-Christian prophetic tradition.

4. An early and significant example is provided by the historian Nancy Tomes in her book, *The Gospel of Germs: Men, Women and the Microbe in American Life* (Cambridge, Harvard University Press, 1998). She notes that although the modern germ theory of disease is often credited to Pasteur, it actually "emerged through a far more collaborative sharing of ideas and research" by a small group of natural scientists and physicians who began to investigate the relationships between microbes and disease after being inspired by early reports of Pasteur's work on fermentation (p 32). According to Tomes, not until the 1870s after the germ theory of disease had already been articulated, did Pasteur start "his celebrated research on anthrax and rabies" (p 31).

5. G Holton, *Thematic Origins of Scientific Thought Kepler to Einstein, op cit*, p 455).

6. *ibid.*

7. *Responsible Science. Ensuring the Integrity of the Research Process*, Volume 1. Panel of Scientific Responsibility and the Conduct of Research. Committee on Science, Engineering, and Public Policy. National Academy of Sciences, National Academy of Engineering, Institute of Medicine. National Academy Press, Washington DC, 1992, p71.

8. JH Fujimura. *Crafting science: a sociohistory of the quest for the genetics of cancer*. Harvard University Press, Cambridge, 1996: 185.

9. D Nelkin. The performance of science. *Lancet* 1998; **352**:893.

10. The anthropologist Paul Rainbow notes that the classic formulation of these norms comes from the work of the sociologist Robert Merton, who, in the 1930s, argued that four interrelated, mutually reinforcing norms guided the practice of empirical research – universalism (scientific truths are held to be impersonal and independent of the scientist who makes a discovery and the place of discovery), communalism (science is pre-eminently a social activity building on previous efforts and shaping future ones), disinterestedness (the commitment to truth above all other motives), and organised scepticism (the social evaluation of truth claims through open debate, peer review, duplication of work, and the like). P Rainbow. *Making PCR: a story of technology*. University of Chicago Press, Chicago, 1996: 13-14.

11. FE Bloom. Scientific conduct: contrasts on a gray scale. *Science* 1995; 268:1679.

12. PJ Friedman. Research ethics: a teaching agenda for academic medicine. *Acad Med* 1990; **65**:32-33.

13. RG Petersdorf. The pathogenesis of fraud in medical science. *Ann Intern Med* 1986;

**104:** 252-54.

14. AL Singer Jr (with G Jones G, J Gurley, L Backus L, T Meyer). Postdoctoral researchers: a panel. In: Ethics, values, and the promise of science. Forum proceedings. Sigma Xi, The Scientific Research Society, Research Triangle Park NC. 1993, pp 47-59.

15. JD Watson. Lecture presented at the Sixth International Conference of the International Federation of Science Editors (IFSE). Marine Biology Laboratory, Woods Hole MA, October 1991. In Eastwood S: *Summary. Sixth International Conference of IFSE: Science Editing in the Age of Global Communication*. IFSE [July] 1993, pp 4-14.

16. One fellow scientist at Cetus, Randy Saiki, expressed his reservations that receiving the Nobel Prize would give Mullis "a wider audience for his story, which changes a bit from year to year. It's a fable; it's not really how the science was done at Cetus...I believe if it had not been for this group being there, nothing would have come out of it. He would have dropped it and moved on to something else." Rainbow P. *PCR: a story of technology*. University of Chicago Press, Chicago, 1996: 160.

17. Another Cetus scientist, Henry Erlich, observed that Mullis "had a great idea, which he followed up with years of misrepresentation and self-promotion. Rewriting history was more productive than rewriting papers." Erlich adds that Mullis's "myth" was "accepted in part because it was never challenged. Many of us who were at Cetus were functionally muzzled. We were told not to upset Kary because they thought it might endanger the patent. There was no assurance he would support the Cetus part of the patent. They had to take great efforts to mollify him, make sure that no one at Cetus was going to do anything that was going to offend him. So as a result, his assertions have not been challenged or refuted by any of the people involved." *ibid*.

18. According to Rainbow, in Merton's view, the ingenuity of the system is precisely that scientists, by acting in their own self-interest, collectively reinforce the public good. *ibid*.

19. Responsible Science, *op cit* p 75.

20. WB Fye. Medical authorship: traditions, trends, and tribulations. *Ann Intern Med* 1990; **113**: 320, citing Relman AS. Publications and promotions for the clinical investigator. *Clin Pharmacol Ther* 1979; **25**: 673-6.

21. This was one of four main problems impeding the recruitment of physicians into clinical research, according to an NIH Panel Subcommittee on Training/Job Opportunities for Patient-Oriented Research. Others included Academic Health Centers' not introducing medical students to clinical research nearly as readily as they introduce them to basic research; the length of time needed for clinical training, including specialty and subspecialty training; and the high level of debt with which physicians leave medical school. The Panel on Clinical Research was convened by Harold Varmus, Director of the NIH, in the spring of 1995, "because the perception of crisis in clinical research that had simmered for decades had intensified by a funding shortage induced by managed care and new restrictions on the Federal budget" (p 5). The Panel was organised to review the status of clinical research in the United States and make recommendations about how to ensure its effective continuance

(p 1). The Executive Summary of the 12/97 Report notes that "it seems impossible to believe that a crisis in clinical research is at hand because a career in clinical research today would appear more rewarding than ever." It is generally feared that clinical investigators "are not sufficiently renewing themselves and are therefore an endangered species facing extinction" (p 5). The Panel gathered data to show that, over the past 30 years, the ratio of MD to PhD applicants for NIH support has progressively fallen, even though success rates for the two types of applicants are similar (p 6). More important, the Panel notes that the absolute number of MD applicants has fallen further in the past three years, and that MDs whose initial applications do not receive fundable priority scores are less likely to reapply than are PhDs. The Panel's view is that this "represents a dispirited attitude among MD faculty members that bodes ill for the future of academic medicine and the public's health. The sense of excitement, opportunity and determination that should permeate the field is compromised by financial and career anxieties" (p 6). *The NIH Director's Panel on Clinical Research, Executive Summary, 12/97*, available on the World Wide Web at <<<http://www.nih.gov/news/crp/97report/execsum.htm>>>.

22. *ibid*, p 5.

23. One conclusion reached by The NIH Panel on Clinical Research was that – although much of the responsibility for conducting translational research (the "seed corn" for future progress in clinical medicine) is properly assigned to academic health centers – "their financial capacity is under severe stress because of changes in the financing and delivery of health care, as well as Federal budgetary practices." The panel noted that, during the past 35 years, these academic health centers have relied heavily on patient-care revenues to "fill the gap in meeting their responsibility for research and teaching." (*The NIH Director's Panel on Clinical Research, Executive Summary, 12/97*, p 20). They also emphasise that one of the most serious consequences of managed care "has been the rise of financial constraints leading to increased demand on the time that clinical investigators must devote to delivering health services rather than to research (*ibid*, p 7). In short, MDs in academic centers are "taxed" by their institutions in order to do their own research and to make it possible for bench researchers to do theirs. Moreover, academic research physicians are also under pressure to accept pharmaceutical industry sponsors for their research – a partnership that raises issues including conflict of interest and even censorship of results (D Rennie. Thyroid storm. *JAMA* 1997; 277: 1238-43; RA Deyo, BM Psaty, G Simon, EH Wagner, and GS Omenn. The messenger under attack – intimidation of researchers by special-interest groups. *N Engl J Med* 1997; 336:1176-79.) Under managed care, MDs have less time than ever to devote to pursuing grant funding for their research, let alone *do* research, but even in these times, promotion committees still emphasise the "publish or perish" ethic (WB Fye. Medical authorship; traditions, trends, and tribulations. *Ann Intern Med* 1990; **113**: 320). Publications are therefore key to a clinical investigator's academic and economic survival. Unfortunately, the NIH report does not address the issue of the need for academic health centers to reassess their promotion criteria for clinical investigators – perhaps they should be rewarded for clinical excellence and their contribution of clinical data to multicenter trials, rather than for a long list of publications reporting trivial studies.

24. JP Kassirer, M Angell. On authorship and acknowledgements. *N Engl J Med* 1991; **325**: 1510-12.

25. One respondent to Kassirer and Angell's *NEJM* editorial asserted that clinical research "is difficult, time-consuming, expensive, and absolutely necessary," that obtaining funding for clinical research is almost impossible and receiving academic credit for doing clinical research almost unheard of." The correspondent, a group chairman of a large cancer clinical trials group, also charged that "making it harder to receive credit or acknowledgement in journals [such as the *NEJM*] is only going to lessen the worth of the clinical investigator," which amounts to taking a step backward "by allowing only those who report small or large nonrandomized trials to be recognised and large-scale clinical research to be shortchanged." (PP Carbone. On authorship and acknowledgements. *N Engl J Med* 1992; **326**: 1084. On this same topic, see also EJ Topol. Drafter and draftees. *Lancet* 1998; **352**: 897-98.). Another respondent noted that clinical researchers "forego more remunerative clinical practices in order to contribute...to the advancement of medicine. Academic departmental chairpersons often advise their faculty members not to become involved in multicenter trials because the academic rewards for the individual investigators are too meagre." (RG Hart. On authorship and acknowledgements.

*N Engl J Med* 1992; **326**: 1085). In their sanctimonious reply, Kassirer and Angell admit only "disappointment" over the fact that "some believe our policies will discourage physicians from participating in clinical trials." They note that such a belief implies that physicians participate "only to see their names in a list of acknowledgements" and that they believe that most members of the medical profession participate in trials "for loftier reasons." (JP Kassirer, M Angell. On authorship and acknowledgements. [Reply to Correspondence] *N Engl J Med* 1992; **326**: 1084-5). Maybe the editors of the *NEJM* need to reacquaint themselves with *Middlemarch*.

26. G Holton. The responsible conduct of research: the historical underpinnings, the current NIH policy, definitions of terms, and the value of virtue. In Fishbach R (ed). *Educating for the responsible conduct of research. NIH policy and other mandates*. Boston: PRIM&R, 1994, p 22.

27. *Middlemarch*, English Library Edition. Middlesex: Hazell Watson & Viney Ltd. 1965 (reprinted in 1981), p 721.

28. *op cit* Fye, p 319

29. *ibid*.

30. *The NIH Director's Panel on Clinical Research, Executive Summary*, 12/97. NIH report, p 6. Available on the World Wide Web at <http://www.nih.gov/news/crp/97report/execsum.htm>

#### 4. What is an author?

## Mark Gruber

Practitioners of medical research have been sorely exercised by the contemporary ethical and professional problems associated with scholarly authorship for some time. A steady stream of articles, proposals, assessments and conferences have proceeded; and the medical community's process of dialogue, should it be leading to practical resolutions and applications, must be in its early stages.

One impediment to the progress of the dialogue about authorship may well be located in the nature of research skill proper to a majority of medical writers. In the hugely subspecialised domains of medical research and journalism, the most successful writers are adept at isolating data out of complex matrices, and applying intensive analytical scrutiny to them. Their competence in this utility is the literary analogue of the microscope (1).

The issue of medical authorship, however, may not be profitably abstracted out of its broad philosophical and historical matrices, at least not at first, until the practitioners of the medical arts observe its etiology therein. Moreover much is gained by considering how a disparate range of disciplines have long framed the problems of authorship and addressed their resolutions. But such considerations as these require the utility of the literary analogue of a telescope. Much is to be gained, then, from surveying the issue in its widest context and greater depth. Thereafter, some of the seemingly more intractable problems of medical authorship may yield to further analysis.

At bottom the question of "authorship" is founded upon an even more basic foundation of social structure: "authority." The exceedingly close linkage of the two ideas functions to remind us that taking credit for a certain publication is assuming power in a certain institutional setting. Remarkably the correlation of the one "author" in authorship and authority is regularly unobserved, granting this linkage the power of an unreflective assent (2). The value of this simple lesson is underscored by the fact that issues of authorship miraculously precede the development of writing itself, and actually comprise a fundamental cause for its genesis. That is to say, the names of contractors in business transactions are among the first logographic symbols at the base of man's first alphabets.

From the beginning, people have been asking the "who" about writing as much as they have been asking the "what" of its content. In this respect, the contemporary measure of professional status by literary output is part of long tradition. Searching a book's bibliography to find one's own name or typing it on the search engine of the Internet is in the same lineage of measuring one's importance as by the number of imprints of one's logo an ancient might find on the clay bullae in the imperial archives of Mesopotamia.

Moreover, the literature which comprises the base of civilisation is a welter of contestation. Foundational texts from Homer to Plato; from Genesis to the epistles of Paul; from Hippocrates to Ptolemy; from the rule of Benedict to the plays of Shakespeare, are subjected to endless debate as to what authors really stand behind them. Such is not merely an arcane speculation, but real forces struggling for power in universities, churches, boardrooms, parliaments, and courts stake their claims about who authored the texts from which they trace their formation. Bismark's theologians, faithful exponents of their master's

nationalistic aspirations, broke with longstanding tradition that St. Matthew wrote the first gospel (for Matthew gives real prominence to Peter, i.e., the candidate of Papal claims) and insisted instead that St. Mark wrote it (where Peter comes off rather badly by comparison). Hence the burning authorship controversies in Biblical hermeneutics often reflect political ecclesiastical turf wars more than theological interests. To what extent do secular authors transcend analogous concerns?

Contemporary philosophers have created a whole discipline around the analogy of the written text as vehicle for social control and institutional hegemony. By the "deconstruction" of any texts they assert that we can observe who is grasping for power and how. Research assistants and medical graduate school students can attest to this literary theory without philosophical training. But if this analysis is even partially correct, medical professionals can operate within this context to at least insure the content of their literature is as valued and valid as the author(s) claimed; and that the author(s) claimed are named with due recognition of reputations already possessed and reputations legitimately aspired towards.

On evidence that the deconstructionists are correct, is the fact that when once the institution of religion, government and education were administered by relatively small bodies of self-perpetuating elites, nobody doubted that Moses wrote the "Exodus," or that Hippocrates wrote the "Oath." However, as the segmentations of a pluralizing society proliferate the question of who wrote what burns more hotly, and the question is no less burning about foundational cultural texts than about contemporary research articles in *The Lancet*. If there is a proper limit to power then medical writers must seek the proper measure of credit.

## References and footnotes

1. At a recent retreat conference on medical authorship sponsored by the CSE, Feb. 1998, a four page compact bibliography on "Authorship in Science," was supplied as a handout. Not one listed article, paper, report, editorial, case, or letter concerned the issue in its broader aspect as embedded in larger literary or historical movements.
2. From the Latin, "auctor," meaning, "enlarger"...having the twin appellation of enlarging upon a subject (author) or enlarging the self upon an audience (authority), authorship questions are ultimately questions about sizing or measuring how large or specific contributions are to be rightly credited.

## 5. Solutions

### Drummond Rennie

The concept of an "author" grew up when there was only one, and that person took all the credit while shouldering any blame. But numerous examples of ghost and guest authorship,

and of scientific misconduct centering around published articles, have demonstrated that too many scientists are failing to treat their manuscripts with the reverence demanded of what Lederberg has called an "inscription under oath" (1). There is therefore abundant evidence that the concept of authorship, when applied to co-investigators in biomedical research, is inadequate and the system is truly broken (2,3,4,5,6,7). Since priority is important to a scientist, and since promotion may depend to a large degree on authorship, the credit associated with authorship is essential both psychologically and practically. The cause of problems with authorship stem from the weight authorship is required to bear. But it is probably connected with the dilution of accountability consequent on the steady increase in the number of co-authors per paper and the consequent disconnect between credit and accountability (6,7).

Solutions to the problem obviously include teaching scientists, both young and old, about the virtues of responsible authorship and about the consequences of breaching the trust between author and reader. Such efforts often form a part of the ethics courses mandated, over the past decade, for young researchers receiving funds from the US Public Health Service. Whether they decrease problems with authorship is unknown.

Various suggestions have already been made to decrease credit, thus reducing the importance of a co-investigator's name appearing on the byline, and so, in theory, reducing the temptation to sin. Publishing articles anonymously, or having names on the byline always arranged in alphabetical order, are suggestions that, to no one's surprise, given the importance of credit, have been shown to be ineffectual (2,6).

Scientific journals have considerable power to dictate policies for their own authors. In 1985, The International Committee of Medical Journal Editors (ICMJE or Vancouver Group) addressed the issue. In the Uniform Requirements for Manuscripts Submitted to Biomedical Journals, (8) the definition states that:

*"All persons designated as authors should qualify for authorship. Each author should have participated sufficiently in the work to take public responsibility for the content. Authorship credit should be based only on substantial contributions to a) conception and design, or analysis and interpretation of data; and to b) drafting the article or revising it critically for important intellectual content; and on c) final approval of the version to be published. Conditions a), b), and c) must all be met. . . . Any part of an article critical to its main conclusions must be the responsibility of at least one author. Editors may require authors to justify the assignment of authorship". This last has since been changed to "Editors may ask authors to describe what each contributed: this information may be published" (9).*

Their statement is useful as a document that properly concentrates on the issue of responsibility, but is seriously inadequate as a solution. The biggest problem is over the issue of whether all individuals should be required to take responsibility for all parts of the article, given that those individuals got together in the first place because they each brought differing talents and expertise to the group (6). In addition, it does not make sense for the contribution to be disclosed to the reader merely at the discretion of the editor.

The fact that so few authors know about the statement after 14 years of its publication in the major medical journals (10), and that a minority of authors fulfilled even a lenient definition

of these criteria, (11,12) while many scientists disagree with the statement, show that no matter how laudable its goals, this cannot work as a solution.

The essential practical step is to link credit and accountability, specifically, tightly and publicly. The linkage is best made by thinking not in terms of "authorship" but more simply in terms of "contributorship": what did each of the co-investigators actually do for the study.

The proposal is to list contributors to scientific papers according to their contribution, the latter to be listed for the reader, not just for the editor, in a footnote (6,7). The co-workers would meet at the end of the project to decide on each person's contribution to be disclosed, and the relative contribution would determine the order on the byline. The contributors and editors would decide what degree of contribution merited a place on the byline. It is often unrealistic to demand that all contributors take responsibility for all aspects of the study. Certain of the co-workers should assure themselves of the integrity of the whole, and appear as guarantors of the entire article. Guarantors would note this added function to the reader in their disclosure of contribution.

It is important to distinguish this from the list of credits at the end of a film, which indicate the title rather than the work done. It is of the essence that work actually done be disclosed, if honorary, guest authors are to be eliminated, and if the reader is to know who did what.

This proposal will not eliminate all abuses of authorship. However, for co-workers to state what they contributed in front of their colleagues will be a healthy exercise. It will make overt the discussion of who contributed what, and those who would abuse the system would have to do so publicly. It will be of great assistance to readers, who will include people responsible for promotion and tenure, for example, whose work would be aided by a description of the candidate's work, decided by his or her peers at the time the work was done. It would make it far easier for the reader to identify the expert whom they might contact directly. In addition, it might make it harder for the colleagues of fraudulent workers either to have their names unwittingly added to fake articles, or, when they have allowed their names to be added, to defend their ignorance of the content. Apart from the fact that there is no good moral argument against it, this solution has the added advantage of making redundant those elaborate, but obscure, systems whereby position on the byline is supposed to convey, to those who possess the key to the code, information about responsibility (6).

*The Lancet*, the *British Medical Journal*, the *American Journal of Public Health*, the *Annals of Internal Medicine*, and *Radiology* are among the journals that have adopted variations of this plan. There have been no problems and the lists take up little space (11,12,13,14,15,16)

## References and footnotes

1. Lederberg J. Communication as the root of scientific progress. *The Scientist* February 8, 1993; **7**:10-14.
2. Rennie D, Flanagan A. Authorship! authorship! Ghosts, guests and grafters, and the two-sided coin. *JAMA* 1994;**271**:469-71.
3. Smith R. Authorship: time for a paradigm shift? The authorship system is broken and

- may need a radical solution. *BMJ* 1997;**314**:992.
4. Horton R. The signature of responsibility. *Lancet* 1997; **350**:5-6.
  5. Smith R. Authorship is dying: long live contributorship. The *BMJ* will publish lists of contributors and guarantors to original articles. *BMJ* 1997; **315**: 696.
  6. Rennie D, Emanuel L, Yank V. When authorship fails: a proposal to make contributors accountable. *JAMA* 1997; **278**: 579-85.
  7. Rennie D, Yank V. If authors became contributors, everyone would gain, especially the reader. *Am J Public Health* 1998; **88**: 828-30.
  8. International Committee of Medical Journal Editors. Uniform requirements for manuscripts submitted to biomedical journals, 1994.
  9. International Committee of Medical Journal Editors. Uniform requirements for manuscripts submitted to biomedical journals. *JAMA* 1997; **277**: 927-34.
  10. Hoen WP, Walvoort HC, Overbeke JPM. What are the factors determining authorship and the order of the authors' names? *JAMA* 1998; **280**: 217-18.
  11. Horton R. The unmasked carnival of science. *The Lancet* 1998;**351**: 688-89.
  12. Yank V, Rennie D. Disclosure of researcher contributions: a study of original research articles in the *Lancet*. *Ann Intern Med* (In press).
  13. Proto AV. Radiology — 1998 and the future. *Radiology* 1998; **206**:1-2.
  14. Northridge M. Annotation: new rules for authorship in the journal: your contributions are recognized-and published! *Am J Public Health* 1998; **88**: 733-34.
  15. What *AJPH* authors should know. *Am J Public Health* 1998; **88**:721.
  16. Information for authors. *Ann Intern Med* 1998; **128**:111-16.

## 6. Further questions

### Judith Crane

#### *Ghost/guest authorship*

Although everyone agrees that medical publishing is to an important degree dependent on the medical industry, one questions this relationship when those writers who are legitimately authors of promotional material or proceedings of industry-sponsored activities extend their

role to the authorship of articles submitted to peer-reviewed journals (1). This situation inevitably involves a ghost author, a guest author, and an honorarium, thus calling into question the definition of authorship as well as one facet of the issue of conflict of interest. The degree to which researchers are willing to consider themselves authors in such situations and their concurrent failure to declare, or even regard, this relationship as a conflict of interest puts editors in the role of judges, faced with the decision of whether to judge for themselves or to provide that which is necessary for their readers to make an informed judgment.

The fact that many journals accept submissions directly from professional organizations, that these organizations do not hesitate to contact editorial offices for information concerning the editorial process and decision, and that guest authors, when confronted by an editor concerning the design of a study, sometimes claim never to have read the paper, does not render credible the current practice concerning letters of submission and disclosure. The difficulty here is to walk the fine line between protecting and policing, the first of which editors would consider among their obligations to both their readers and to legitimate contributing authors, the second of which has unwelcome connotations.

It seems unlikely, however, that even researchers who resent the idea of editors creating guidelines and who reject outright the idea of editors defining authorship, would defend the ghost/guest situation unless they were, or anticipated being, the guest; nor would they wholeheartedly discredit the idea of editors taking note of such situations and distinguishing between guest authors and genuine contributors. Medical editors who would proceed in doing both are urged to refuse submissions from medical writers or industry representatives, to decline to communicate directly with manuscript managers, and to require and publish disclosure of professional manuscript preparation. Authors are urged to reconsider associating one's name with a study one did not perform or a document one did not write.

### ***Gift authorship***

The question of gift authorship should be distinguished from that of guest authorship, although they have been described concurrently as honorary authorship (2). Gift authorship which has been bestowed as either a tribute or as a ploy for recognition, within the context of a reciprocal exchange or as the consequence of a dependence, is distinctly different from guest authorship which has been paid for. As such, gift authorship is more difficult to eradicate, a process which will involve the direct participation, or at least the agreement, of research institutions and funding agencies, in addition to a process of weaning away from long-standing academic tradition. Crucial to this is that it must be agreed by researchers rather than imposed by editors. Given the fact that many researchers are unaware of either the existence of established criteria for authorship or the details of such criteria, and that once made aware, many authors disagree with the restrictive nature of the criteria and believe that non-compliance reflects upon the credibility and authority of the guidelines (3), it is not surprising that they are unwilling to make way for a new tradition, nor that they believe themselves in a position to do so before the research institutions and funding agencies accept and adopt a more creative view of contributorship in the context of reporting and rewarding research results. The most appropriate role for editors is that of innovator in presenting alternatives and of advocate for their adoption by both the

contributors to medical scientific literature and the institutions and agencies upon which they are dependent.

### ***Conflict of interest***

The current controversy over the degree to which editors should concern themselves with conflict of interest is evidence both of its widespread existence and its elusive nature. Conflict of interest has been defined as "a set of conditions in which professional judgment concerning a primary interest (such as patients' welfare or the validity of research) tends to be unduly influenced by a secondary interest (such as financial gain)" (4). Another definition describes conflict of interest with respect to a given manuscript, indicating that neither the author, nor the reviewer, nor the editor should be excluded from scrutiny regarding "ties to activities that could inappropriately influence his or her judgment, regardless of whether judgment is, in fact, affected" (5). More recently, in the interest of making it more palatable and thus more easily disclosed, conflict of interest has been redescribed as "competing interests" (6). The degree to which authors are unwilling to declare a conflict of interest, however, or are willing to declare the absence of a conflict of interest, argues against the preferable solution of voluntary disclosure simply. The discrepancy between the *New England Journal of Medicine's* manifest refusal to publish editorial material written by authors with a conflict of interest, in conjunction with their disclosure mechanism in the form of a questionnaire (7), and *The Lancet's* more liberal application of the embarrassment test (8), in conjunction with their option of publication with full disclosure, has not been resolved and probably should not be. This sort of debate will keep the issue of conflict of interest in the spotlight and ever-present in the minds of those who stand to gain as well as those who risk being misled.

### ***Implications of Internet***

Concurrent with the project of redefining authorship, editors are faced with the increasing need to redefine the term "publication" in light of the fact that most medical journals have instituted websites. The internet provides an alternative to expensive and time-consuming printing and binding, as well as to the cost and delay implied in delivery by post; a website avoids the problem of space and page limitations, and thus liberates editors from the necessity to limit acceptance of correspondence; web technology facilitates the search process and expands the possibility of access from within information-poor areas of the world; and the web dramatically reduces the delay between acceptance and dissemination (9). At the same time, publication via the internet requires an ongoing awareness of the possibility that such shortcuts may compromise the quality of the literature and thus requires a certain vigilance on the part of editors so that the transition to a more expedient and democratic mode of publication is not at the expense of integrity (10). This will involve an openness to the differences between an author of the printed page and an on-line author, and will require an anticipation of the direction that electronic publication may take and the distances to which it may go, including the possibility of authors bypassing editors.

### ***Research***

There has evolved, and will continue to evolve, an area of research that measures and

analyses the trends in authorship and publication. The possibilities for research in this area are endless, ranging from a consideration of who is determining the new definitions and who is most threatened by them, to a measure of the rate of acceptance of and transition to the new definitions. Within the realm of medical scientific publication in journals, traditional definitions of authorship in a strict sense have for the most part disappeared. Authors in this realm are denied that which artists generally take for granted: the rare privilege of being the sole originator of his or her creation (11). An author here is not the « fashioner of words » that Marianne Moore described, "stamping the words with his or her own personality". Rather, co-authorship is contribution to a shared project and, at its best, acceptance of personal responsibility.

## References and footnotes

- 1 *The Lancet* 1993; **342**:1498-499.
- 2 Flanagin A et al. Prevalence of articles with honorary authors and ghost authors in peer-reviewed medical journals. *JAMA* 1998; **280**:222-24.
- 3 Bhopal R et al. The vexed question of authorship: view of researchers in a British medical faculty. *BMJ* 1997; **315**:1009-12.
- 4 Thompson D F. Understanding financial conflicts of interest. *N Engl J Med* 1993; **329**:573-76.
- 5 International Committee of Medical Journal Editors. Conflict of interest. *Ann Intern Med* 1993; **118**: 646-47.
- 6 Smith R. Beyond conflict of interest. *BMJ* 1998; **317**:291-92.
- 7 Stelfox, HT et al. Conflict of interest in the debate over calcium- channel antagonists. *N Engl J Med* 1998; **338**:101-06.
- 8 The politics of disclosure. *The Lancet* 1996; **348**: 627.
- 9 Delamothe T, Smith R. The BMJ's website scales up. *BMJ* 1998; 316:1109-10.
- 10 Horton R. *The Lancet* 1997; **349**:1411-12.
- 11 Games people play with authors' names. *Nature* 1997; **387**: 831.

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