The Instability of Authorship: Credit and Responsibility in Contemporary Biomedicine

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In the past decade, the definition of authorship has been the topic of many articles and letters to the editor in scientific and especially biomedical journals. The official position of the ICMJE (International Committee of Medical Journal Editors) has been, and continues to be, that authorship must be strictly individual and coupled with full responsibility for the claims published. But the applicability of ICMJE guidelines has come under increasing debate. Indeed, about a year ago, some journal editors called for a paradigm shift in the definition of authorship, while others argued that "it is time to abandon authorship" altogether (1). But if the problems with traditional definitions of authorship are at this point clearly laid out and a few proposals put forward, a new comprehensive paradigm has yet to emerge (2).

Coming to this debate as a historian with a background in the early modern period, I am struck by the similarity between the current emphasis on the coupling of scientific authorship and responsibility and older, premarket definitions of the author. Before the emergence of the figure of the intellectual property-holder in the late 17th and early 18th century, the author was construed by the state, the prince, or the church as the individual responsible for the content and publication of a given text (3). The author was not seen as a creative producer whose work deserved protection from piracy, but as the person upon whose door the police would knock if those texts were deemed subversive or heretical.

Although the Office for Scientific Integrity's increasing commitment to democratic due process sets it apart from its inquisitorial ancestors, the current definition of scientific authorship still shares its early modern cousin's relationship to responsibility (4). Today, if scientists publish dubious claims, they are not accused of *lese majesté* against their absolute ruler or of subverting the church's absolute control on theological doctrines, but they *are* represented as responsible for something that is deemed to be equally absolute: truth.

Although I do not see clear continuities between the 16th century and the 1990s, there is something to learn from the genealogy of the figure of the author in science and other fields in reference to how credit and responsibility have been differently defined, joined, or separated in different disciplines after the demise of early absolutist regimes. In particular, the reward system of science and the liberal economy have developed in parallel as two distinct and yet complementary systems since the 17th century, and so the definition of scientific authorship has not been framed by the logic of the reward system of science alone, but by the intersection of these two economies and the way they have carved out different categories of credit and responsibility in relation to each other.

The troubles of scientific authorship have been emphasized by the development of corporate-style contexts of research and professional ethos in the last two decades. The problems, however, were already there. The stress produced by the increased proximity of the complementary economies of science and the market has only highlighted tensions *within* the logic of each system. Authorship is caught between these two tectonic plates, and the pressure is mounting. Although this article cannot prevent earthquakes, I hope it will help locate some of the fault lines and stress points underlying current discussions about the coupling of authorship and responsibility in biomedicine.

TWO COMPLEMENTARY ECONOMIES OF AUTHORSHIP

In a liberal economy, the objects of intellectual property are artifacts, not nature. One becomes an author

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Abbreviations: ICMJE, International Committee of Medical Journal Editors; OSI, Office of Scientific Integrity; OSIR, Office of Scientific Integrity Review; ORI, Office for Research Integrity.

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by creating something new, something that is not to be found in the public domain. A common view is that copyright is about 'original expression,' not content or truth (5). If you paint a landscape, you can claim intellectual property (a form of private property) on the painting (the expression), but not on the landscape itself (the content). Also, copyright does not cover facts or ideas per se. Therefore, though researchers (or journals) can copyright scientific publications and gain some protection from having articles appropriated or reproduced without consent, their rights do not and cannot translate into scientific credit. Saying that they are scientific authors because their papers reflect personal creativity and original expression (the kind of claim one has to make to obtain copyright) would disqualify them as scientists because it would place their work in the domain of artifacts and fiction, not truth. Nor can scientists copyright the content of their claims, because nature is a 'fact' and facts (like the landscape represented in a painting) cannot be copyrighted, since they belong to the public domain. In sum, copyright can make scientists authors, but not scientific authors.

Like copyright, patents also reward novelty, since they cover 'novel and nonobvious' claims. But, unlike copyrights, such claims need to be useful to be patentable. Scientists, then, can become 'authors' as patent holders, but cannot patent theories or discoveries per se (either because they are 'useless' or because they are about something that belongs to the public domain) (6). It is becoming increasingly common for scientists (mostly geneticists) to patent natural objects, but they do so by making them potentially useful by carving them out of their state of nature (7). Nature becomes patentable by being turned into something that is less natural and more useful.

As with copyright, the patent system may provide scientists with an authorship venue, but not with scientific authorship. Scientists can patent useful processes stemming from their research, but scientific authorship is defined in terms of the truth of scientific claims, not of their possible usefulness in the market. In sum, according to definitions of intellectual property, a scientist qua scientist is, literally, a nonauthor. While novel claims are the objects rewarded by both intellectual property law and the reward system of science, the 'unit of credit' is dramatically different in these two economies. A new, dramatic discovery that may warrant a Nobel prize cannot be translated, in and of itself, into a patent or a copyright. Likewise, a scientist's copyrights and patents will not earn him/her such an award. It seems, then, that scientific authorship is not 'independent' from the logic of the market, but that its definition is complementary to that of market-based authorship as articulated through the copyright or patent systems.

From this complementarity it follows that the primary currency of scientific credit is not money per se, but rewards assigned through peer review (reputation, prizes, tenure, membership in societies, etc.) rather than transacted according to the logic of the market. Intellectual property rights can be exchanged for money because they are a form of private property, and money is the unit of measurement of the value of that form of property. For the same reason, the kind of credit held by a scientific author cannot be exchanged for money because nature (or claims about it) cannot be a form of private property, but belongs in the public domain. Of course, scientists can operate simultaneously in academic and market economies, but, with the help of university lawyers, they need to keep the boundaries between these two systems as distinct as possible. They can also work in industry or government, in which case authorship may be contractually relinquished in accordance with terms of employment. Again, the logic behind the reward of scientific work with 'honorific' credit is not independent from, but complementary to, that of monetary economy.

A number of consequences follow. The first is that authorship credit distributed by the reward system of science has to be attached to a scientist's name and cannot be transferred. It is not transferable because scientific authorship cannot be a form of private property, and only private property (like copyrights and patents) can be tranferred from one individual to another. The reasons for attaching authorship to a scientist's name, instead, follow from how the notion of truth is construed by the reward system of science.

Truth, unlike private beliefs, is generally defined as something that ought to be public. The accessibility of truth (or simply true information) in the public domain is that which legitimizes liberal democracy as an egalitarian state form (8). This assumption also justifies the presence of inequality in the private sphere. If you are not as rich as your neighbor, the story goes, you can't blame it on the fact that you lacked access to the same information that was available to the person next door. Empirically, this reasoning may be questionable, but that does not stop it from being widely used as one of the fundamental justifications for the distinction between the public domain and the private sphere (and private property).

The definition of scientific truth as public is usually presented as an epistemological rather than an economic or legal axiom. Truth is defined as public or, more emphatically, as universal, because it is assumed to be transparent and recognizable by anyone who is competent. Truth should be as public and accessible as its object, nature. Putting on hold, for the moment, the question of whether such a definition reflects actual practice, there are other logical reasons for defining truth as public—reasons that stem from the complementarity between the categories of the reward system of science and of liberal economy.

The relationship between truth and private beliefs is parallel to that between the public domain and private property. Both relationships hinge on the distinction between private and public—a distinction that is integral to both science and liberal economy. Private property is 'private' because it is complementary to the public domain, a vaguely defined category that nevertheless provides the conditions of possibility for private property (9). Likewise, private beliefs are private because they are complementary to public truth, and are made possible by that very category. For instance, when expressed in material forms such as a literary text, a music score, a painting, or a patent, private beliefs (here broadly construed as any personal thought or conception that deviates from the common stock of knowledge and cultural expressions found in the public domain) become the object of intellectual property. Considered as fictions or artifacts, private beliefs may be 'bad' from an epistemological point of view, but are simultaneously very 'good' in the eyes of liberal economy because they make intellectual property possible.

Whether or not scientific truth is universal, it still has to be defined as such to maintain the logical coherence of both liberal economy and the reward system of science. A notion of universal scientific truth legitimizes private property defined as the result of specific 'deviations' from public, 'universal' knowledge, and it confirms the epistemological status of science by virtue of being an activity that is outside of monetary economy and private interests. Universal truth is value-free because it is literally defined as valueless, and yet it is the mother of all property values.

How, then, can scientific credit be defined? Intellectual property is often represented as the result of taking as little as possible from the public domain (the shared 'pool' of cultural and natural resources) and transforming it into some kind of 'original expression' (10). But a scientist is not represented as someone who transforms reality or produces original expression out of thin air, but as a researcher who, with much work, 'detects' something specific within nature—the domain of public and 'brute' facts. For that finding to be recognized as true, s/he has to put it back in the public domain (here construed as the 'public sphere', which includes, but is not limited to, the community of scientific colleagues). Although this is a loop that begins and ends in some version of the public domain, fundamental changes take place along the way. The starting point is *generic* nature, but the result is a *specific* item of true knowledge about nature. Whereas the production of value in a liberal economy involves a movement between two complementary categories (from generic public domain to specific private property), in science the movement is within the same category (the public domain) and goes from unspecified to specified truth. Both cases involve a tranformation from something unspecific to something specific. But if in the case of intellectual property such transition can be legally tracked (as it moves across two different categories), scientific credit is much trickier, because the movement from nature and the public domain to a specific true claim about nature does not cross any recognizable legal threshold. As a result, it cannot be legally tracked or monetarily quantified.

Another way to put it is that in the case of intellectual property, one can rely on the distinction between the form and content of a work (between 'original expression' and the 'public domain') to determine authorship and property rights. In science, however, a claim cannot be attributed a 'form' (in the legal sense of the term), since that would categorize it as an artifact. But, at the same time, a scientific claim cannot be like nature itself, it cannot just be 'content'. There is a bit of a paradox here. The transition from unspecified to specific truth cannot be attributed to nature, as nature does not investigate itself. Yet that work should not result in a commercially transactable intellectual property because that would destroy its status as truth. Nevertheless, such a transition has to be marked somehow, not only because the scientists deserves fair credit for it, but because it has to be marked in order to exist, to be recognized as a specific truth, not just a chunk of undifferentiated, undescribed nature.

Historically, the solution to this paradox has been to attach scientific credit to the scientist's *name* while construing such credit as nonmonetary (11). That scientific credit is honorific and attached to a scientist's name is a default solution to a problem posed by the inapplicability of the taxonomies of liberal economy to the case of science and, at the same time, by the need to find a solution that does not delegitimize those taxonomies. Such a definition of scientific credit is the result of a metrological necessity. The scientists' 'disinterestedness', therefore, is not the cause for scientific credit being honorific, but a professional value practitioners accept or develop by working in an economy that logically requires their credit to be nonmonetary.

But what also needs to be attached to a scientist's name is responsibility, not just credit. If a true claim about nature were like an artifact, a novel expression, or a piece of literary fiction, responsibility could be negotiated legally. In market environments, an author's responsibility is construed as financial liability, that is, as a matter of property and damages. Also, the legally responsible author may not be the actual producer of those claims, but rather the individual or corporation that paid the producer for his/her labor or rights in those claims. But this cannot apply to true claims about nature because they are in the public domain,—a category complementary to that of property and monetary liability. Therefore, in the reward system of science, responsibility for scientific claims falls on the scientist who produced them simply because that individual is the only 'hook' on which the movement from unspecified to specified truth can be pinned.

Furthermore, responsibility in science is absolute. It is as absolute as truth because, like scientific credit, truth and responsibility *cannot be quantified*. Responsibility becomes absolute by default. That is why, within this logic, scientific authorship has to be defined in strictly individual terms. If truth and responsibility are absolute, they cannot be attached to a corporate author since that would parcel out something that has to remain absolute. But a new paradox emerges from such a solution: Truth (defined as universal, permanent, absolute, etc.) ends up being hinged on something that is extremely local and ultimately transient—the scientist's name. And such a name needs to be a proper name that is unequivocally connected to a person's body, not to a corporation—*a persona ficta*.

This may cast some light on why scientific fraud is seen as a fundamental aberration, not just a serious problem. Commercial fraud—fraud about property—is certainly not a trivial issue, but it can be handled legally; it can be *quantified* (more or less adequately) in terms of financial damages. Scientific fraud, instead, tends to assume a more ominous status, and does so in part because it *cannot* be properly measured in terms of damages. Fraud shares in the absoluteness of truth and the responsibility it is seen to subvert. Of course, legal and administrative actions can be taken against fradulent scientists. Universities can fire them, and funding agencies can sue them for misuse of research funds. However, this is a bit like getting Al Capone for tax fraud when he could not be charged with murder. Adapting the False Claim Act of 1865 (developed to curb the delivery of substandard equipment to the army) to sentence scientists with punitive damages up to three times the amount they received from funding agencies shows that the reward system of science cannot prosecute scientific fraud per se, but is forced to step outside itself and adopt the logic of commercial fraud (12). The emotions stirred by scientific fraud and its moral condemnation as a 'crime against the truth' may reflect the fact that while fraud rattles the logic of the reward system of science, its punishment cannot be logically commensurate with the 'crime'.

In the next section I argue that while the complex relationship between authorship, responsibility, and credit has been highlighted by contingent concerns with scientific misconduct, its roots lie in the tensions between (and within) the economies of science and of the market. After being historically and logically constituted in opposition to each other, these economies are now brought into a closer and uneasy proximity by the development of increasingly large-scale, collaborative, and capital-intensive contexts of research. The grassroots emergence of corporate views of scientific authorship that erode individual responsibility is perhaps the most conspicuous hybrid that has resulted from this process.

BIG SCIENCE AND THE REACTION AGAINST CORPORATE AUTHORSHIP

Historically, the debate on authorship and responsibility in biomedicine has developed in response to two distinct trends: the sharp increase of multiauthorship related to the transformation of biomedicine into a 'big science', and the emergence of well-publicized cases of scientific fraud (or alleged fraud). That biomedicine is as much about truth as about healing the taxpayers' bodies has added further urgency to the problem of responsibility and has made it an unavoidable focus of the debate.

Quantitatively speaking, the scale of multiauthorship in biomedicine still lags behind that of physics (13). But if articles with hundreds of authors resulting from large multicenter clinical trials are relatively rare, bylines including six or more authors are not. Journal editors and other commentators began to notice this tendency in the 1970s and usually interpreted it as resulting from the need to pool together different skills and specialized knowledge within increasingly large and collaborative research projects (14). The multiauthorship trend could have opened the door to the acceptance of a corporate notion of authorship as a way to distribute credit in large cooperative research programs, but it clashed with the requirement of individual responsibility.

What did trigger concerns about responsibility was the growing awareness that a given scientific paper may have required the work of a biostatistician, although that person may have had little or nothing to do with the collection of the data s/he eventually analyzed; or that several contributors who may be considered authors (in the sense that they made important contributions to the project) may not be able to defend the work (or perhaps even understand the tasks) accomplished by some of their other colleagues (15). This state of affairs would pose no problems in market-based fields where responsibility, credit, and intellectual property rights can be negotiated contractually, but such techniques are not acceptable in science.

While some literary theorists have argued that the emergence of large-scale book markets led to a 'death of the author', there has been no parallel movement occasioned by the big science trend in biomedicine (16). On the contrary, the more collective, corporate, and industrial-style the research contexts become, the more one finds a resistance to accepting the implications this trend is having on the notion of authorship and responsibility. Beyond resistance, there may even be a reaction to the erosion of a notion of individual authorship. For instance, over the years the ICMJE has issued increasingly stricter guidelines about authorship; these guidelines struggle to attach authorship to the 'crucial' contributions to a project and to develop taxonomies of credit that would distinguish between authorship (defined as responsibility) and other forms of recognition to be listed not in the authors' byline, but in a separate 'acknowledgments' section that, according to some other proposals, could resemble a 'film credits' list (17).

The section on authorship of the ICMJE 1997 ''Uniform Requirements for Manuscripts Submitted to Biomedical Journals'' reads:

> ''All persons designated as authors should qualify for authorship. The order of authorship should be a joint decision of the coauthors. 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 (1) conception and design, or analysis and interpretation of data; (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. Participation solely in the acquisition of funding or the collection of data does not justify authorship. General supervision of the research group is also not sufficient for authorship. Any part of an article critical to its main conclusions must be the responsibility of at least one author. Editors may ask authors to describe what each contributed; this information may be published.
>
> ''Increasingly, multicenter trials are attributed to a corporate author. All members of the group who are named as authors [...] should fully meet the criteria for authorship as defined in the *Uniform Requirements*. Group members who do not meet these criteria should be listed, with their permission, under acknowledgments, or in an appendix [...].''(18)

Overlapping with analyses of the increasingly corporate structure of research, one also finds frequent expressions of concern about the changing ethos of biomedicine. Big science is often equated to big business, an analogy that points as much to the large role of private sector funding as it does to the increased scale of biomedical research (19). Accordingly, commentators note that the multiauthorship trend reflects not only the increased complexity of modern research, but a growing entrepeneurial ethos. They see it as a problematic response to an increasingly competitive publication-based regime of credit and professional advancement (20). Under pressure from this complex, entrepeneurial, and competitive environment, practitioners have been alleged to associate authorship more directly with credit than with responsibility, that is, to treat authorship as ''a trading chip in an economic game.''(21) Those who are concerned with the trading chip attitude about authorship acknowledge that scientists have serious concerns with professional credit and career advancement. These commentators refer quite explicitly to authorship as the primary 'currency' in science, but then deplore the 'inflation' that excessive multiauthorship might bring to such a currency (not to mention fraud and other unsavory practices that could be elicited by the same pressures toward the accumulation of scientific credit) (22). They criticize the 'capitalistic' ethos that seems to be taking over biomedicine, but end up casting the threat to science in terms of 'inflation'. In the end, they use a category that reflects an acceptance of the very market logic they want to resist.

Similarly, the commentators regret, but do not deny, that in practice the quantity rather than quality of publications is often the leading factor in promotion cases, distribution of research funds, etc. (23). While deploring the situation and suggesting improvements (such as quotas on the number of publications submitted for promotion cases), they admit that the big business mentality that has pervaded biomedicime is here to stay. This means that in a large-scale and extremely active professional environment there are material constraints on the time and energy that can be allocated to a review of candidates' or applicants' work on the basis of quality rather than quantity (24). Practices that have been condemned for their dubious ethics thrive: reliance on 'salami science', LPUs (least publishable units), and the rotating distribution of first authorship in a series of related papers published in different disciplinary journals.

FRAUD AND THE GEOGRAPHY OF AUTHORSHIP

Against the background of these changes in the structure of biomedical practice, a series of cases of scientific fraud, or alleged fraud added urgency to debates about scientific authorship (25). The Darsee and Slutsky cases, and more recently the so-called (or misnamed) 'Baltimore case', have received much attention in the popular press, a publicity that has put further pressure both on the scientific community and policy makers. In some instances, senior scientists whose name was on an article's authors' byline argued that they were not responsible for the mistakes or misconduct of their junior colleagues. Accordingly, the blame was to be placed on Darsee, Slutsky, and people like them, not on the honest (if busy) directors of their labs or the department chairs who had agreed to have their name on the articles according to a practice that has since been labeled 'honorific' or 'gift' authorship (26). Similar cases and similar justifications continue to this day (27).

Even though not all instances of scientific fraud can be reduced to situations in which a senior researcher did not take responsibility for the work of a junior associate, it is interesting that this aspect of the problem has received the most attention and that honorific authorship has become a sort of fighting word in debates about scientific misconduct. The high visibility of some of the senior scientists and their institutions (UCSD, Harvard, MIT) does not fully account for the emphasis that has been placed on this aspect of fraud. Although it is not my business to absolve or condemn those involved in these cases, I believe that the primary focus on honorific authorship signals a difficulty in coming to terms with the fact that the practitioners' perceptions of due credit and responsibility may be informed by their location and role within a collaborative project, and that blame, as appropriate as it may be, is not going to eradicate the sociological roots of the problem.

Scientific authorship has a geography as well as a logic. However, the geographical variability of attitudes about authorship is downplayed by the fact that the term science tends to cast an aura of homogeneity on a vast range of diverse disciplines and differently situated individuals and institutions (28). In science as in society, workers, managers, and lawmakers are not the same people (though they may be citizens of the same state), and such differences are constitutive, not erasable. But scientific culture, because of its emphasis on values such as trust, collegiality, and disinterestedness, has few ways to acknowledge and negotiate these tensions and power differentials. In contrast, liberal economy has abundant categories to explain its litigiousness, and plenty of legal infrastructures to manage it. Therefore, if liberal economy has no problems admitting the sharp economic conflicts at play behind current disputes about intellectual property law, science is inherently ill-equipped to acknowledge that the debate about responsibility, credit, and authorship may reflect struggles among different constituencies (29).

However, one legacy of the fraud scandals of the early 1980s is precisely the mapping of the different interests and positions of at least three different constituencies: *1*) Congress and funding agencies; *2*) universities, research institutions, and academic journals; and *3*) the practitioners themselves (this group can be further divided into junior and senior researchers).

Some members of Congress, funding agencies, and scientists whose job is to monitor other scientists became concerned about the misuse of research funding and the disrepute that fraud cases bring to biomedical research and to the politicians and institutions that support it (30). Given their role and interests, it is not surprising that these constituencies identify authorship with responsibility and see honorific authorship as emblematic of how well-funded scientists have become overconfident in their belief that they do not need to earn the freedom from external regulation that has been granted to them but denied to other professions (31). According to this constituency, science must cleanse itself from misconduct, establish its own policing infrastructures, or face the possibility of government regulation. To some extent, this possibility materialized in 1989 with the development of the Office of Scientific Integrity (OSI) and the Office of Scientific Integrity Review (OSIR) within the Public Health Services (PHS), with OSI and OSIR being reorganized into the Office for Research Intergrity (ORI) in June 1992 (32). The same year, a U.S. attorney went so far as to suggest that the legal system could take over the adjudication of claims of scientific misconduct—an option that, to the displeasure of universities, has been increasingly exercised (33).

Universities, research institutions, and journal editors were quick to respond to these moves, but universities and journals have different stakes in these matters. Universities would like to rely on journals and their editorial practices (refereeing system, etc.) to certify good science, detect misconduct, and possibly alert them about potential problems (34). Journals, on the other hand, claim that although they do their best to ensure the publication of quality articles written by the authors named in the byline, it is not their duty to play judge (35). After all, their editors are not in the lab and do not have the material resources to push the evaluation process beyond refereeing. In any case, journal editors make decisions based on what scientists themselves (as referees) report to them. When it comes to authorship, most journal editors (in particular those who endorse the ICMJE guidelines) now require all authors to sign a statement such as this:

> AUTHORSHIP RESPONSIBILITY: ''I certify that I have participated sufficiently in the conception and design of this work and the analysis of the data (when applicable), as well as the writing of the manuscript, to take public responsibility for it. I believe the manuscript represents valid work, I have reviewed the final version of the manuscript and approve it for publication. Neither this manu-

script nor one with substantially similar content under my authorship has been published or is being considered for publication elsewhere, except as described in an attachment. Furthermore, I attest that I shall produce the data upon which the manuscript is based for examination by the editors or their assignees if requested.''(36).

Such statements cast journals in a curious role. They end up representing themselves as credit-givers while simultaneously minimizing their responsibility for the credit they give. In the end, it is not clear whether journals are casting themselves as publishers or printers. What I find surprising is not that editors are understandably cautious about their practical ability to certify true knowledge, but that their policies put the onus of assessing scientific authorship and the truth value of claims completely on the scientist's shoulders as if the reward system (of which journals are a crucial element) had little to do with certification.

Insistence on the individuality of authorship and its coupling with complete responsibility is so categorical that it amounts to a demand that authors do what the reward systen and peer review should but cannot quite do. Ready or not, practitioners are being volunteered for a sort of 'mission impossible'. I am not suggesting that referees should be formally coresponsible for the articles they review, but that the current definition of scientific authorship casts peer review not as a system of certification but as little more than a free (and responsibility-free) consulting service for editors (37). Although journal editors quite laudably do their best to eradicate the problem of honorific authorship, they do not seem to realize that the reputation of their own journals is constituted through a process that is structurally similar to honorific authorship. If the articles they publish are praised, the journal's credit grows accordingly. But if something goes wrong, the editors can say that they (and the referees) are not responsible for the problem and only the authors are to blame.

This would not be a problem outside of science, where the limits of certification are accepted as a fact of life. For example, the U.S. Patent Office may grant a patent without checking whether the device or process actually works. Preliminary checks are conducted to detect conspicuous overlaps between a given application and other existing patents, but it is then up to the inventor to find people who would appreciate the value of his/her idea, as it is up to the inventor to defend the patent in court against competing claims. The same can be said about copyrights. In market environments, then, authorship is not absolute, but a resource to be developed (and perhaps defended) through further work, time, and expense.

In contrast, according to the logic of the reward system of science, authorship is as absolute as the truth of the claims on which it rests—a truth that is not to be negotiated in court or through contracts. And authorship credit is construed as something almost instantaneous. You produce a true claim, you take responsibility for it, you publish it, you get credit. Unlike other products, truth does not need to be developed to be recognized. In science, the work for which an author gets credit does not extend past the 'filing'. The logical function of the peer review system is the certification of truth. It is as though you deposit a check, the bank 'reviews' it, and you get the money.

But, in practice, the bank (the peer review system) cannot function so thoroughly and swiftly. A scientist receives full credit for the amount of the 'check' s/he deposits, but the funds can be taken back at any later time if it is contested. The check clears immediately and, at the same time, it never really clears. In a liberal economy, the granting of a copyright or a patent is a way of saying that your check looks potentially good, but that it is up to you to develop its actual value in a market. The limits of certification are acknowledged, but there are a range of tools to manage them.

In practice, the reward system of science is faced with the limits of the peer review system, but cannot fully admit them without jeopardizing its own logic—a logic hinged on the absoluteness of truth. Such a contradiction is not solved, but displaced in time in the hope it will never express itself, that is, that scientific claims will never been assailed as fraudulent (38).

SPECIFICITY VS. CONDITIONS OF POSSIBILITY

If Congress, universities, and journals couple authorship with total responsibility, the practitioners themselves tend to stress the links between credit, labor, and authorship, while attaching them to a notion of limited responsibility. When practitioners write responses to editorials about authorship policies, they point to the power differentials that frame the debates about authorship (39). Journal editors assemble themselves in committees, issue guidelines that may reflect their needs and wishes (more than the daily realities of the researchers), and can easily air their opininions in the pages of their own journals (40). Most individual scientists do not have that kind of power. It is almost as though scientists express a wish to 'unionize', as they seem to feel they are at the receiving end of authorship policies whose development they do not control.

Expressions of discontent from scientists have become increasingly frequent. In 1988, one could find a letter to the editor stating:

> ''I wish to comment on the preposterous suggestion, being seriously advanced in some

quarters, that all of the authors of a given paper are responsible for all of the material that appears in that paper. If that rule were adopted, it would bring multidisciplinary research to a virtual halt.''(41).

Recently, letters to *Science* were peppered by remarks like, ''it is ridiculous to think that each author can or should be able to vouch for each of the others,'' or:

> ''If marriage partners are not held liable for the actions of their spouses, why should we assume that scientific collaborators are liable? In both cases, liability would be tantamount to an assertion of omniscience, and an omniscient scientist would probably be in no need of collaborators.''(42).

A third writer voiced frank skepticism about the idea that responsibility for a multiauthored paper be shared by all its authors by saying that:

> ''This amounts either to banning all papers with more than one author or enshrining a kind of chivalry where scientists agree to destroy their own careers if they happen to work in the same lab as a scientist who commits fraud.''(43).

Earlier this year, a report based on a questionnaire circulated through a broad cross section of biomedical practitioners at the University of Newcastle showed that a substantial portion of the scientists could not recall the basic authorship requirements issued by the ICMJE, and when told what they were, found them inapplicable. Many of the respondents (49%) had also experienced situations in which, according to their perception, authorship had been deserved but not awarded (44).

In sum, researchers seem to favor a notion of authorship that entails limited rather than global responsibility, and view authorship as something that should be extended not just to those who allegedly would be able to defend all results, but to anyone who worked at making a trial possible, such as laboratory workers or the many general practitioners who provided and followed patients but may have contributed little or nothing to data analysis (45).

A perception of authorship as primarily linked to credit and labor, rather than absolute responsibility, is not limited to the 'workers' but also to some senior scientists, and it informs the credit arrangements they may adopt (implicitly or explicitly) with their junior colleagues and assistants. For instance, the occurrence of honorific authorship is important evidence of a perspective according to which a director of a lab who provided space, equipment, or prestige and facilitated access to funding and publication is seen (by him/herself and perhaps by the associates) as an investor. The role of a 'remote' lab director is not unlike that of a general practitioner who provided patients but did not necessarily analyze the data or write up the final papers. Perhaps neither the director nor the general practitioner had much to do with the specific results, but they nevertheless made those results possible. In some ways, honorific authorship resembles a common phenomenon of the early modern period: the dedication of a book to the patron who supported the author or, through his high social status, could help him gain legitimation, visibility, and even some protection from plagiarism for the claims published in that book (46). If one finds such behavior proper then but not now, this does not mean that early scientists were unethical, but simply that professional ethics is not a matter of ahistorical first principles, but has evolved alongside the reward system of science.

The practitioners who resist a definition of authorship as something that is inherently individual (rather than collective) and tied to absolute (rather than limited) responsibility seem to think in terms of corporate credit and investments—investments they 'pay back' by giving authorship credit. Even though it is easy to see how these behaviors clash with the logic of the reward system of science, they accurately reflect the outlooks practitioners develop when operating in large resource-intensive projects that are extended both in space and time. They are 'grassroots' and quasi-capitalistic. I say quasi-capitalistic because authorship in academic biomedicine remains a matter of name, not money. It is capitalistic only to the extent that authorship is treated like having stock in a particular project, and responsibility is also treated in a corporate manner. Responsibility in this 'take' is not an absolute notion (as the reward system of science would require), but is something limited to one's stock in the project.

A way to summarize the differences between the positions of the ICMJE and those of some of the participants in large projects is to say that the ICMJE focuses on the *responsibility for the specific claims* that emerged from a study, whereas the participants attach authorship to those who have provided the *conditions of possibility* of that study. This kind of demarcation is not new. It reproduces (in logic but not in content) the type of distinctions discussed earlier: those between the public domain and private property, and in the case of science, between unspecified nature/truth and specific truth claims.

The ICMJE states that ''participation solely in the acquisition of funding or the collection of data does not justify authorship,'' that is, they attach authorship only to those tasks that made a *difference*, not just provided a *possibility* (18). In this logic, the general practitioner who provided patients (or in other settings, an instrument maker, a laboratory assistant, a maintenance technician, or a 'remote' lab director) are seen as people whose work was not *specific* to that project. They did contribute to its *happening*, but not to the fact that the result was X rather than Y. The author, then, is cast as the individual who was *irreplaceable*, someone whose involvement in the study was both necessary and sufficient to its result.

Conceived in those terms, the author would be a sort of bodily counterpart for what has been called the crucial experiment. Accordingly, truth is the outcome of an experiment conceived in a way that only one of its various possible outcomes can be the result of that which is hypothesized as its true cause prior to the conducting of the experiment. The ICMJE's test for authorship seems to translate such a view of natural causality into the domain of human agency. An author is the person, and the only person, who 'caused' the outcome of a research project. Of course, more authors can be attached to an article, but the ICMJE guidelines break multiauthorship down to an assembly of separate authors, each fully individual and fully responsible. Coauthorship cannot mean corporate authorship.

But if the ICMJE's position is coherent, it begs at least two further questions. One is whether the view of natural causality entailed by the crucial experiment can be applied to environments where human agency is temporally intermittent and spatially distributed. Unlike gravity acting on all apples all the time, many different people work at different aspects of a research project, often at different times and at different sites. The second is the practical feasibility (rather than conceptual robustness) of a taxonomy of reward that distinguishes true authors from other practitioners eligible only for 'acknowledgment' credit.

To begin with the second question, it would appear that, in principle, the widening of categories of scientific credit through the introduction of currencies other than that of authorship could rechannel the pressures that have led to the corporate uses of individual authorship toward the use of other forms of credit-giving. But, at present, having one's name in the acknowledgment section or in some other appendix (as requested by the ICMJE) does not do much good to many practitioners, since these credits are not usually retrievable through computer searches. And in biomedicine today, such searches play a crucial role in the production of the author function. Furthermore, such a reform of authorship would work only if accompanied by a serious reeducation not only of the researchers, but also of those who evaluate them for jobs, promotions, and funding.

COMPRESSING TIME, SPACE, AND LABOR

Going back to the first, more difficult question, I believe that a two-tier system distinguishing between full authors responsible for the truth of the published claims and all the others who provided 'only' the conditions of possibility for those claims would introduce not a graduated credit scale, but an incommensurability between two classes of contributors.

The ICMJE guidelines that try to reduce the entire range of collaborative projects distributed in time and space to the model of individual effort and total responsibility reflect a literal extension of the image of the individual author. In doing so, these guidelines fit in a long tradition that has emphasized the agency of the individual author at the expense of other contributions to the knowledge-making process. In literature, the legal concept of the author was developed in the 18th century largely as a way to include immaterial objects such as expression and creativity under the category of private property, a category that until then was about material entities (47). Both writers and publishers were faced with the financial costs of piracy, the result of an early perception of books as objects one bought, claimed as property, and could use in any way one wished (including reproducing them). The figure of the author as the holder of intellectual property rights was developed as a way to limit the property rights of the book buyer by saying that there was more to a book than its materiality, that there was something that could not be relinquished in the act of selling a book. The author, then, was a market construct, one that made both booksellers and writers very happy.

But one can argue that the focus on the individual author as the holder of such newfangled property rights misrepresented the long chain of human agency that produced a literary work. It involved compression and selection. The historical figure of the individual author as romantic genius is the epitome of such misrepresentation through the compression of human agency. Accordingly, the 'work' is seen as emerging from an instantaneous act of creativity, not from the time-extended labor of paper makers, font cutters, editors, typesetters, printers, binders, and booksellers (not to mention the body of previous literary works from which the author drew his/her 'inspiration') (48).

A similar, if less drastic, compression and selection of the chain of human agency is found in the depiction of the figure of the scientific author. Since the emergence of experimental philosophy in the 17th century, the notion of the individual author was often constituted through the erasure of the contribution of instrument makers and laboratory technicians who, because of their low social status and credibility, were not perceived as true knowledge makers and whose names were omitted from the published reports (49).

Historically, then, the author has always been more of an efficient accounting device for intellectual property or scientific credit than an accurate descriptive tool of knowledge-making practices. The tensions produced by author-based forms of accounting have been there since the beginning, and have been made only more conspicuous by the increasing complexity and changing scale of knowledge production (in both the scientific and market economies). The logic behind the two-tier taxonomy of credit one finds in the ICMJE guidelines is homologous to that behind these historical cases. In both cases a line is drawn between the author and those who provided the conditions that made the author's specific results possible. In contemporary biomedicine, the definition of the author does not turn on his/her creativity or original expression, but on his/her responsibility. The parameters that constitute the author are different, but its logic cuts across disciplines and, to a lesser extent, across historical periods.

In fact, the author described in the ICMJE guidelines is not a hyperindividualized romantic genius. Such a figure worked well to legitimize the author's (or his/her bookseller's) claims of intellectual property by representing literary production as an act that borrowed little or nothing from the surrounding culture. However, the journal editors' primary concern is not the maximizing of intellectual property, but the management of responsibility. Therefore, the editors use the figure of the individual author not as a creative genius, but as the person responsible for those aspects of the research process that can be represented as constant and stable throughout the process of knowledge production: the conception of the work, the analysis of the collected data, and the writing of the article. In intellectual property law, the individual genius is the one who creates out of nothing, whereas in science the individual author is the one who gives continuity and consistency to a heterogeneous process. Although one emphasizes instantaneity and the other constancy, both figures work as ways of demarcating the final product from its conditions of possibility.

By drawing a wedge between conception and execution, the ICMJE guidelines carve out research practices in two categories: one that is unified, stable, and allegedly laid out since the beginning of the project, and one made up of diverse activities, ideas, and insights that may have developed along the way at different times and places—items that are much more difficult to subject to a neat accounting. The focus on data analysis rather than data collection as a fundamental aspect of authorship is an attempt to compress temporally and spatially diverse labors to an activity that took place in a specific place at a specific time. Similarly, though no one would question that the writing up of the results is a crucial contribution to any scientific project, a text, being an object that is physically well circumscribed and easily accessible, is also very handy for accounting purposes. It is a stable inscription whose content is frozen in time, available in many locations, and yet always the same, as opposed to the complex temporally and spatially dispersed activities it is seen as summarizing.

But as reasonable and convenient as this approach may be, it does not guarantee that the conception of the work can always be located in one or a few distinct individuals or that, together with the division of tasks among the various practitioners, it could have been laid out once and for all at the beginning of the project. Similarly, this does not imply that, in principle, the writing of the final paper should be a task rewarded with a kind of credit (authorship) that is incommensurable with (rather than simply more important than) the credit to be given to those responsible for other tasks.

It is not that the 'conception of the work' is a convenient fiction, that the focus on the written outcome of research is simply fetishistic in nature, or that the ICMJE guidelines are wrong. The policies proposed are predetetermined by the logic of the reward system of science. The point is that the choice of the features deemed to be constitutive of authorship reflects an accounting rationale shaped by a symbiosis with liberal economy. It deploys categories that facilitate the accounting process better than the global description of research practices.

No one, I think, would object to the necessity for accountability or responsibility in science. What is happening, however, is that responsibility is treated as something that preexists and is independent from its accounting protocols. But authorship is not just a *result* of the accounting of human agency and responsibility in the knowledge making process: it is a category that provides the condition of possibility for such an accounting. *Authorship is both accounted and accounting*. What we take to be authorship in science or intellectual property in a liberal economy are coexistent with the accounting systems that rest on those categories as constitutive assumptions, not as empirical categories that exist independent of the system in which they operate.

CONCLUSIONS

Historically, the law has been continuously modified and articulated to manage the emergence of new forms of production and of new interest groups. Unlike various national legal systems, the reward system of science has remarkably fewer tools to adjust to contingent historical changes. In my opinion this is because its logic has been historically tied to an absolute concept of truth and responsibility. However, I believe that an overhaul of the reward system of science in a market direction would not give authorship its desired flexibility.

For one thing, liberal economy and the reward system of science are not independent, but complementary, and are joined at the hip, so to speak, by the hazy category of the public domain. It is the public domain that, in one case, legitimizes liberal democracy and its notion of private property and, in the other case, grounds the notion of truth as universal, transparent, disinterested, etc.

Science and liberal economy both construe value (be it a true scientific claim or intellectual property) as a process of specification that, depending on the economy, is either from the public domain to private property (via the individual's creative expression) or within the public domain from unspecified nature to specified truth claims (via the individual scientist's responsibility). In short, the fundamental dichotomy in liberal economy between public domain and private property is found, *mutatis mutandis,* in the scientific realm in the distinction between conditions of possibility and specific claims at the roots of credit, authorship, responsibility, and truth.

In both cases the fundamental distinction between specific, individually produced claims and products and the 'stuff' that made them possible is both necessary and inherently unstable. Consequently, definitions of authorship in science and in the market—definitions rooted in this distinction—reify such instability. Thus, the conceptual tensions that underlie scientific authorship would not be solved by moving toward more corporate, market-based notions. Although I have a taste for hybrids, in this case a cross-breeding of scientific and market authorship would not join two different and mutually strengthening entities, but two categories that have evolved together (though complementarily) and are both cracking under similar kinds of stress. The cross-breeding would be sterile.

At the same time, authorship policies like those of the ICMJE that reinforce the separation of the economies of science and the market are likely to produce more discontent than sustainable solutions. Erecting stronger boundaries between the two systems is not going to solve the problems, because the problems are *within* each of the two systems. The increased proximity of these different and complementary economies have only enhanced the visibility of previously existing problems.

In sum, I do not think that the conditions for revolutions and new paradigms for scientific authorship are readily available. So much has been hung on it from different sides that, despite its inherent instability, scientific authorship has become virtually unmovable. While there is an implicit awareness that the category of authorship needs to be reconstituted, I think that the proposed solutions find themselves chasing their own tails, often reproducing some of the very tensions they try to solve. And this is not for lack of effort or acumen. Therefore, rather than pursue the chimera of the one conceptually 'right' definition, one may take a more pragmatic position by acknowledging that authorship (scientific or not) has always been a matter of compromises and negotiations, and that no new conditions have emerged to change that.

But the logic of compromise begs the question of what are the constituencies that should negotiate it. The current debate and policies, however logically coherent and well-intentioned they may be, have a predominantly 'top-down' quality to them. This points to the need of appropriate infrastructures to enable a representative number of practitioners with different roles and seniority to participate democratically in the legislation of future authorship protocols. Having been discussed mostly by editors and administrators, authorship has been framed as an administrative problem. It is, instead, an issue whose roots spread so far and wide that its solution may require something of a 'constitutional amendment' to the logic of the reward system of science. In the end, the real challenge may be precisely the development of infrastructures to make these broader discussions possible and to provide the conditions of possibility for a workable definition of scientific authorship. FJ

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ENDNOTES

1. Richard Smith, ''Authorship: Time for a Paradigm Shift? The Authorship System is Broken and May Need a Radical Solution,'' *Br. Med. J.*, Vol. **314,** 5 April 1997, p. 992; Richard Horton, ''The Signature of Responsibility,'' *Lancet*, Vol. **350,** July 5, 1997, pp. 5–6. See also Richard Horton, Richard Smith, ''Time to Redefine Authorship,'' *British Medical Journal*, Vol. **312,** p. 723; Fiona Godlee, ''Definition of 'Authorship' May be Changed,'' *Br. Med. J.*, Vol. **312,** 15 June 1996, pp. 1501–1502; and Evangeline Leash, ''Is It Time for a New Approach to Authorship?'', *J. Dent. Res.*, Vol. **76,** No. 3, 1997, pp. 724–727.
2. The most innovative proposal to date has been put forward by Drummond Rennie, Deputy Editor (West) of the *Journal of the American Medical Association.* At a conference on scientific authorship held in June 1996 at Nottingham and sponsored by *Lancet*, the *Br. Med. J.*, Locknet (an international peer-review research network), and the University of Nottingham, he proposed to replace 'author''' with 'contributor'. Contributors should be listed in the byline and the nature of their contribution described in a footnote. In addition, some contributors who are the most familiar with all aspects of the project should be termed 'guarantors', and should be in charge of answering any question

that may be elicited by the publication (Evangeline Leash, ''Is It Time for a New Approach to Authorship?'', *J. Dent. Res.*, Vol. **76,** No. 3, 1997, pp. 726).

3. Michel Foucault, ''What is an Author?'', Donald F. Bouchard (ed), Language, Counter-Memory, Practice, Ithaca: Cornell University Press, 1977, p. 124. See also Carla Hesse, *Publishing and Cultural Politics in Revolutionary Paris*, pp. 1789–1810, Berkeley: University of California Press, 1991.
4. However, some critics have argued that scientists investigated for misconduct may get less than a fair trial from the ORI Among other problems, the ORI mixes investigatorial and prosecutorial tasks and holds hearings (if the defendant so reguires) only after it has found misconduct. (Louis M. Guenin, ''The Logical Geography of Concepts and Shared Responsibilities Concerning Research Misconduct,'' *Academic Medicine*, Vol. **71,** No. 6, June 1996, pp. 598–599). Similarly, in his discussion of the 'Baltimore case', Daniel Kevles argued that: ''Imanishi-Kari was, to all intents and purposes, prevented from mounting a genuine defense. The OSI [ORI's institutional ancestor] combined the duties of investigator, prosecutor, judge, and jury, and pursued them all in the manner of the Star Chamber.'' (''The Assault on David Baltimore,'' *The New Yorker*, May 27, 1996, p. 107).
5. James Boyle, *Shamans, Software, and Spleens: Law and the Construction of Information Society*, Cambridge, Mass.: Harvard University Press, 1996, pp. 51–59.
6. Jeremy Phillips, Alison Firth, *Introduction to Intellectual Property Law, 3rd Edition*, London: Butterworths, 1995, pp. 39–42.
7. Eliot Marshall, ''Companies Rush to Patent DNA,'' *Science*, Vol. **275,** 7 February 1997, pp. 780–781, provides a review of recent trends. See also ''Gene Fragments Patentable, Official Says,'' *Science*, Vol. **275,** 21 February 1997, p. 1055. For an earlier overview on these issues, see Dorothy Nelkin, *Science as Intellectual Property*, New York: MacMillan (for AAAS), 1984.
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9. Jessica Litman, ''The Public Domain,'' 39 Emory law Journal 965, 999 (1990); David Lange, ''Recognizing the Public Domain,'' *44 Law and Contemporary Problems 147* (1981); James Boyle, ''A Politics of Intellectual Property'' (unpublished manuscript).
10. Views of copyright framed by beliefs in the romantic figure of the author tend to be more extreme as they present the author's work as coming out of thin air, not from the reelaboration of materials found in the public domain. The origin of this view is discussed in Martha Woodmansee's ''The Genius and the Copyright: Economic and Legal Conditions of the Emergence of the 'Author','' *Eighteenth-Century Studies*, Vol. **17** (1984), pp. 425, 443–444.
11. Harriet A. Zuckerman, ''Introduction: Intellectual Property and Diverse Rights of Ownership in Science,'' *Science, Technology, and Human Values*, Vol. **13,** Nos. 1 and 2, Winter and Spring 1988, pp. 7–16; Robert K. Merton, ''The Normative Structure of Science,'' *The Sociology of Science: Theoretical and Empirical Investigations*, Chicago: University of Chicago Press, 1973, pp. 273–275; Robert K. Merton, ''Priorities in Scientific Discovery,'' *The Sociology of Science: Theoretical and Empirical Investigations*, pp. 294–295, 323; Robert Merton and Harriet Zuckermann, ''Institutionalized Patterns of Evaluation in Science,'' *The Sociology of Science: Theoretical and Empirical Investigations*, p. 465. Important views on the relationship between scientific and economic credit have been presented in Pierre Bourdieu, ''The Specificity of the Scientific Field and the Social Conditions of the Progress of Reason,'' *Social Science Information*, Vol. **14** (1975), pp. 19–47; and in Warren O. Hagstrom, ''Gift Giving as an Organizing Principle in Science,'' Barry Barnes, David Edge (eds), *Science in Context*, Cambridge, Mass.: MIT Press, 1982, pp. 21–34.
12. Paulette V. Walker, ''1865 Law Used to Resolve Scientific Misconduct Cases,'' *The Chronicle of Higher Education*, January 26, 1996, p. A29. Subsequently, some uses of the False Claim Act have been challenged in court (Paulette V. Walker, ''Appeals Court Overturns a False-Claim Ruling Against U. Of Alabama at Birmingham,'' *The Chronicle of Higher Education*, February 7, 1997, p. A37).
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20. Marcia Angell, ''Publish or Perish: A Proposal,'' *Annals of Internal Medicine*, 1986, Vol. **104,** pp. 261–262; R. L. Engler, J. W. Covell, P. J. Friedman, P. S. Kitcher, R. M. Peters, ''Misrepresentation and Responsibility in Medical Research,'' *New Engl. J. Med.*, 1987, Vol. **317,** No. 22, pp. 1383–1389; Drummond Rennie, Annette Flanagin, ''Authorship! Authorship!,'' *JAMA*, 1994; Vol. **271,** pp. 469–471; J. Smith, ''Gift Authorship: A Poisoned Chalice?'', *Br. Med. J.*, 1994, Vol. **309,** pp. 1456–1457; D. W. Shapiro, N. S. Wenger, M. F. Shapiro, ''The Contributions of Authors to Multiauthored Biomedical Research Papers,'' *JAMA*, 1994, Vol. **271,** pp. 438–442.
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22. Daniel M. Laskin, ''The Rights of Authorship,'' *J. Oral Maxillofac. Surg.*, 1987, Vol. **45,** p. 1; Addeane S. Caelleigh, ''Editorial: Credit and Responsibility in Authorship,'' *Academic Medicine*, Vol. **66,** No. 11, November 1991, pp. 676–677; Drummond Rennie, Annette Flanagin, ''Authorship! Authorship!,'' *JAMA*, 1994; Vol. **271,** pp. 469–471;. William J. Broad, ''The Publishing Game: Getting More for Less,'' *Science*, Vol. **211,** 13 March 1981, pp. 1137–1139; Robert N. Berk, ''Irresponsible Coauthorship,'' *AJR*, Vol. **152,** pp. 719–720, April 1989.
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29. James Boyle, *Shamans, Software, and Spleens: Law and the Construction of Information Society*, Cambridge, Mass.: Harvard University Press, 1996, pp. 35–60.
30. Walter W. Stewart and Ned Feder's painstaking analysis of the publications of John Darsee is emblematic of this trend. Its conclusion is that ''Scientists have to an unusual degree been entrusted with the regulation of their own activities. Self-regulation is a privilege that must be exercised vigorously and wisely, or it may be lost.'' (''The Integrity of the Scientific Literature,'' *Nature*, Vol. **325,** 15 January 1987, pp. 207–214).
31. According to Congressman John Dingell, ''Scientists need to understand that the best way, perhaps the only way, to avoid the threat of 'science police' is for scientists themselves to show that they have the ability and the will to police themselves. It is a matter of morality, but also of self-interest.'' J. D. Dingell, ''Shattuck Lecture—Misconduct in Medical Research,'' *N. Engl. J. Med.*, June 3, 1993, Vol. **328,** No. 22, p. 1614.
32. Donald F. Klein, ''Should the Government Assure Scientific Integrity?'', *Academic Medicine*, Vol. **68,** No. 9, September Suppl. 1993, pp. S56–S59.
33. Breckinridge L. Willcox, ''Fraud in Scientific Research: The Prosecutor's Approach,'' *Accounting in Research*, Vol. **2,** 1992, pp. 139–151. Rex Dalton, ''Heat Rises over UCSD 'Misconduct' Charge,'' *Science*, Vol. **385,** 13 February 1997, p. 566; Paulette V. Walker, ''2 Lawsuits May Change Handling of Research-Misconduct Charges,'' *The Chronicle of Higher Education*, June 6, 1997, pp. A27–A28.
34. According to the editors of *JAMA*: ''The parent research institutions rely on publications as the coins academics must use to get through the tollgates on their way to academic promotion. And if the promotion committees function well, they weigh as well as count the coins.'' Drummond Rennie, Annette Flanagin, ''Authorship! Authorship!,'' *JAMA*, 1994; Vol. **271,** pp. 469–471. See also Marcel C. LaFollette, *Stealing Into Print: Fraud, Plagiarism, and Misconduct in Scientific Publishing*, Berkeley: University of California Press, 1992, pp. 156–194.
35. A reasonable articulation of this position is in Drummond Rennie, ''The Editor: Mark, Dupe, Patsy, Accessory, Weasel, and Flatfoot,'' Editorial Policy Committee, CBE, Ethics and Policy in Scientific Publication, Bethesda, Md.: CBE, 1990, pp. 155–163. See also Don Riesenberg, George D. Lundberg, ''The Order of Authorship: Who's On First?'', *JAMA*, October 10, 1990, Vol. **264,** No. 14, pp. 1857; Helmuth Goepfert, ''Responsible Authorship'' (editorial), *Head and Neck*, July/August 1989, pp. 293–294; A. N. Schechter, J. B. Wyngaarden, J. T. Edsall, J. Maddox, A. S. Relman, M. Angell, W. W. Stewart, ''Colloquium on Scientific Authorship: Rights and Responsibilities,'' *The FASEB Journal*, Vol. **3,** Feb. 1989, pp. 209–217 (especially Maddox's statement on p. 214).
36. *JAMA*, 13 October 1989, Vol. **262,** No. 14, p. 2005.
37. P. V. Scott, T. C. Smith, ''Definition of Authorship May Be Changed: Peer Reviewers Should Be Identified at the End of Each Published Paper,'' *Br. Med. J.*, Vol. **313,** 28 September 1996, p. 821.
38. The remarkable paucity of analyses of the peer review system before the wave of fraud scandals and the early tendencies to underestimate the frequency of misconduct may reflect a built-in tendency to denial that, far from being arbitrary, is connected to the structural blind spots of the reward system of science. Until recently, the peer review system had been the subject of few sustained analyses (Daryl E. Chubin, Edward J. Hackett, *Peerless Science*, Albany, N.Y.: SUNY Press, 1990, is a notable exception). But several conferences, studies, and publications have emerged since, such as ''Guarding the Guardians: Research on Editorial Peer Review,'' special issue of *JAMA*, March 9, 1990, Vol. **263,** No. 10.
39. Michael E. Dewey, ''Authors Have Rights Too,'' *Br. Med. J.*, Vol. **306,** 30 January 1993, pp. 318–120. See, also, comments on Dewey's piece in the *Br. Med. J.*, Vol. **306,** 13 March 1993, pp. 716–717, which include remarks like ''Authors need to organise themselves to redress the current imbalance of power,'' ''The International Committee of Medical Journals Editors should consider the sort of issue discussed by Dewey,'' and ''how a mechanism might be set up to allow authors' grievances to be aired.'' A radical revision of the relationship between journal and contributors has been recently proposed by *Lancet*'s editor (Richard Horton, ''The Signature or Responsibility,'' *Lancet*, Vol. **350,** July 5, 1997, p. 6).
40. Occasionally, some editors seem uneasy about this state of affairs. Commenting on a meeting of the ICMJE, the editor of *Lancet* claimed that ''medical editors should be banned from assembling in more than twos or threes, lest they conspire to present a homogeneous front to contributors and readers.'' (''Editorial Consensus on Authorship and Other Matters,'' *Lancet*, 1985; Vol. **2,** p. 595).
41. Avram Goldstein, ''Collaboration and Responsibility,'' *Science*, 23 December 1988, Vol. **242,** p. 1623. See also Arnold Friedhoff's letter on the same page.
42. Letters by Jay M. Pasachoff and Craig Loehle in ''Responsibility of Co-Authors,'' *Science*, Vol. **275,** 3 January 1997, p. 14.
43. Letter by Tobias I. Baskin, ibid.
44. Raj Bhopal, Judith Rankin, Elaine McColl, Lois Thomas, Eileen Kaner, Rosie Stacy, Pauline Pearson, Bryan Vernon, Helen Rodgers, ''The Vexed Question of Authorship: Views of Researchers in a British Medical Faculty,'' *Br. Med. J.*, Vol. **314,** 5 April 1997, pp. 1009–1012.
45. Domhnall Macauley, ''Cite the Workers,'' *Br. Med. J.*, Vol. **305,** 11 July 1992, p. 6845; Ian W. B. Grant, ''Multiple Authorship,'' *Br. Med. J.*, Vol. **298,** 11 February 1989, pp. 386–387. See also letters to the editor (*N. Engl. J. Med.*, Vol. **326,** No. 16, April 16 1992, pp. 1084–1985) published in response to J. P. Kassirer, M. Angell, ''On authorship and acknowledgments,'' *N. Engl. J. Med.*, 1991, Vol. **325,** pp. 1510–1512. A few editors have taken these complaints seriously. An editorial in *Lancet* a few months ago argued that: ''Many researchers think this definition [ICMJE's] is out of touch with their own research practice. It leans toward being a senior authors' charter, falling short of providing explicit credit for those who actually do research [...]. And, in an era of highly technical, multidisciplinary research, how can all authors be expected ''to take public responsibility for the content''? On

balance, the definition seems to fail important tests of relevance and reliability.'' (Richard Horton, ''The Signature or Responsibility,'' *Lancet*, Vol. **350,** July 5, 1997, pp. 5–6).

46. Mario Biagioli, Galileo Courtier, Chicago: University of Chicago Press, 1993, pp. 103–157; Mario Biagioli, ''Etiquette, Interdependence, and Sociability in Seventeenth-Century Science,'' *Critical Inquiry*, Vol. **22** (1996), 193–238.
47. Mark Rose, Authors and Owners, Cambridge, Mass.: Harvard University Press, 1993); Peter Jaszi, ''Toward a Theory of Copyright: The Metamorphoses of 'Authorship','' *Duke Law Journal*, 1991, pp. 455–502; Martha Woodmansee, *The Author, Art, and the Market*, New York: Columbia University Press, 1994; Roger Chartier, ''Figures of the Author,'' *The Order Of Books*, Stanford: Stanford University Press, 1994, pp. 25–59.
48. Martha Woodmansee, ''The Genius and the Copyright: Economic and Legal Conditions of the Emergence of the 'Author','' *Eighteenth-Century Studies*, Vol. **17** (1984), pp. 425–448. The problems posed by the romantic view of authorship on contemporary intellectual property law are one of the foci of James Boyle's *Shamans, Software, and Spleens: Law and the Construction of Information Society*, Cambridge, Mass.: Harvard University Press, 1996.
49. Steven Shapin, ''The House of Experiment in Seventeenth-Century England,'' *ISIS*, Vol. **79** (1988), pp. 373–374; and ''The Invisible Technician,'' *American Scientist*, Vol. **77** (November-December 1989), pp. 554–563.