RIGHTS OR REWARDS? CHANGING CONTEXTS AND DEFINITIONS OF SCIENTIFIC AUTHORSHIP

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I. INTRODUCTION

This essay is about the attribution of authorship in academic science, with special emphasis on extensive collaborative projects, or "Big Science." These environments are characterized by large-scale multi-authorship, and may produce articles with hundreds of names stretching the author's byline over a few pages.[1] While the problems raised by such levels of multi-authorship are acute, they are not anomalous and foreground with great clarity the problems of attribution typical of scientific authorship in general. Here I limit my discussion to academic scientific authorship, that is, I do not look at the large and increasingly complex domain of university-industry relations, commercial biotech, the human genome initiative, and the challenges these developments pose to patent law.[2] After a discussion of the conceptual problems posed by the attribution of scientific authorship, I analyze two new frameworks for the definition of authorship (one from particle physics and one from biomedicine). These remarkably divergent proposals indicate that scientific authorship may be becoming something that has little to do with authorship as we know it.

II. AN OUTLINE OF THE PROBLEM

Attribution is a particularly thorny issue in science because of the logic of its reward system – a logic that, as I hope to show, is quite distinct from (and usually complementary to) that of intellectual property law.[3] Moreover, definitions of scientific authorship, far from being codified in a corpus of doctrine like intellectual property law, do change across disciplines and institutions.[4] This, however, does not mean that scientific authorship represents a prolifer-

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1. *See infra* Appendix A.

2. The literature on these issues is vast and developing at an exponential pace. A survey of the last two years of journals like Nature and Science would provide a quick synopsis of the state of the debate.

3. Case studies on different aspects of the genealogy of scientific authorship and of its differentiation from the logic of intellectual property are in WHAT IS A SCIENTIFIC AUTHOR? (Mario Biagioli & Peter Galison, eds., forthcoming Spring 2002).

4. Different universities have substantially different definitions of scientific authorship. Often, these differ from the definitions adopted by scientific journals, which, in turn, are far from homogeneous.

ation of ad hoc devices. While the many disciplinary expressions of scientific authorship are indeed varied and apparently contradictory, the logic underneath those positions is fairly consistent and therefore analyzable.

Like copyright, scientific authorship concerns something fixed in a medium (an article, a book, an abstract). But the analogy between scientific claims and the objects of copyright ends quickly. Most of the differences between the two can be traced to the fact that scientific authorship is not about property rights but about true claims regarding nature. This fundamental distinction is played out at many levels, some theoretical, some mundane. To begin with a mundane example, a non-scientific work is protected by copyright just by virtue of its being fixed in a tangible medium (without the further requirement of publication), but a scientific claim does not count as such unless it is made public and subjected to peer-evaluation. In the case of copyright, an author obtains rights in the material inscription of his or her originality precisely because it is produced by something – personal expression – that is considered to be his or hers to begin with. Whether or not other people see or appreciate it as a result of its publication is not relevant to the author's rights in it.[5] Instead, a scientific claim is not seen as the material inscription of the scientist's personal expression, but as an objective statement about nature. Consequently, it cannot be the scientist's property. This means that he does not have inherent rights in a scientific claim in the way a "normal" author has rights in the product of his personal expression simply by virtue of being the creative producer of that inscription. From this, it follows that unless it is published and evaluated by peers, a scientific claim does not count as such and does not bring rewards to the scientist who produced it. Scientific authorship is not a right but a reward. And such a reward is not bestowed by the state according to the law, but by an international community of peers, according to often tacit customs.[6]

Furthermore, scientific credit is not monetary but "symbolic."[7] This is probably not the right adjective, but it tries to capture the fact that scientific credit is about professional recognition that can be transformed into money (in the form of jobs, fellowships, and grants) but is not money-like in and of

5. SHELDON HALPERN ET AL., FUNDAMENTALS OF UNITED STATES INTELLECTUAL PROPERTY LAW 40-44 (1999).

6. In this sense, a scientific claim is more like a generic commodity (not the product of personal creativity) that is "sold" to a community by its "finder." I use quotation marks because, of course, the kind of transaction between the scientist and his peers is not a sale. No property is exchanged and the reward is not money but authorship itself. Also, a scientist is no mere finder as the production of a scientific claim entails a hefty dose of human agency, not just serendipity. What I am trying to stress here is the paradox that while a scientific claim is the basis for scientific authorship, that claim is not an authorial product in and of itself. Actually, it is precisely its status as a non-authorial (i.e., objective) claim that may constitute its producer as a scientific author.

7. *See* Pierre Bourdieu, *The Specificity of the Scientific Field and the Social Conditions of the Progress of Reason*, 14 SOCIAL SCIENCE INFO. 19 (1975); Robert K. Merton, *Priorities in Scientific Discovery*, *in* THE SOCIOLOGY OF SCIENCE: THEORETICAL AND EMPIRICAL INVESTIGATIONS 294-95, 323 (1973).

itself.[8] Some have argued that science works like a gift economy in which a scientist gives publications to his peers as a gift and receives credit from them as a counter-gift.[9] But whether or not the notion of the gift can capture the peculiar logic of scientific rewards, what is clear is that credit is attached to qualitative notions such as truth, novelty, and scientific relevance that have been proven very hard to quantify precisely because they operate (and need to operate) in an economy that is distinct from capitalistic economy.[10] Truth is priceless not only in the sense of being such an expensive commodity that no amount of money can buy it, but in the sense that it should be priceless because it cannot belong to the logic of interest and its ubiquitous unit of measure: money. The opposition between truth and interest is one of the pillars (perhaps a rhetorical one) of the logic of scientific authorship.

Once we rule out the possibility of quantification through something like money (and especially when we exclude the logic of exchange-value from science), however, the attribution of scientific credit and authorship becomes a very tricky matter of qualitative judgment.[11] Traditionally, peer review has been cast as the process through which scientific credit is reliably assessed, but recent studies have opened up this venerable black box showing its many limitations, especially when a single publication has been produced by many people with different fields of expertise and disciplinary affiliations.[12] The frequent complaints that the quantity rather than quality of a candidate's publications seem to be the major factor in promotion cases stem from these difficulties.[13]

8. For a discussion of how symbolic credit is transformed into financial and material resources and back into more symbolic credit and so on, *see* BRUNO LATOUR & STEVE WOOLGAR, LABORATORY LIFE: THE SOCIAL CONSTRUCTION OF SCIENTIFIC FACTS 187-233 (1979).

9. Warren Hagstrom, *Gift Giving as an Organizing Principle in Science*, *in* SCIENCE IN CONTEXT 21 (Barry Barnes & David Edge, eds., 1982).

10. There is a widespread awareness about how difficult it is to quantify scientific credit and that no one seems to have a solution for that problem. For instance, "The coin of publication has 2 sides: credit and accountability. On the credit side, *no one has the least idea what the coin is worth*, or who should be awarded coins, or how the coins should be lined up for inspection . . . ." Drummond Rennie et al., *When Authorship Fails: A Proposal to Make Contributors Accountable*, 278 JAMA 579, 580 (1997) (emphasis added). But while most scientists and administrators cling to the hope that there must be a solution for that problem, I am much more skeptical and believe that the problem is inherently unsolvable because scientific credit works precisely by being unquantifiable.

11. While this essay deals only with academic scientific authorship, several of its claims can be applied to the social sciences and humanities as well by translating "true claims about nature" with "scholarship."

12. Until recently, the actual workings of peer review in science had received scant attention, a surprising pattern given the fundamental role everyone attributes it. The most notable exceptions are DARYL CHUBIN & EDWARD HACKETT, PEERLESS SCIENCE (1990); PEER REVIEW IN SCIENTIFIC PUBLISHING (Council of Biology Editors, Inc. ed, 1991); *Peer Review Theme Issue*, 280 JAMA 212 (1998).

13. *See* David Hamilton, *Publishing by – and for? – the Numbers*, 250 SCIENCE 1331 (1990); Drummond Rennie & Annette Flanagin, *Authorship! Authorship!*, 271 JAMA 469 (1994); *Are Academic Institutions Corrupt?*, 342 LANCET 315 (1993) (editorial); Marcia Angell, *Publish or Perish: A Proposal*, 104 ANNALS OF INTERNAL MED. 261 (1986); Barbara

The in-depth evaluation of a candidate's work is a time-consuming process, and time is a most rare commodity in science. However, the time constraints or laziness of a review committee cannot fully explain the tendency to rely on quantitative assessments of a candidate's publications. The more serious, structural problem is that, especially in large-scale multi-authorship contexts, the qualitative evaluation of a candidate's work turns out to be a conceptual nightmare, not just a very onerous task. Evaluation is a complex (and inherently contestable) process even in the case of a single-authored publication. But when a curriculum vitae includes dozens of articles co-authored with dozens of other scientists, the complexity and ambiguity of evaluation grows exponentially, thus stretching (or breaking) the credibility of the entire process.

What evaluators have to contend with is not just their desire to be somewhere else rather than in front of a tenure file, but with two thorny and potentially intractable questions: What is the overall value of the article I'm reading, and what is the "share" of this value that I should attribute to the candidate? It seems that precisely because of the difficulties produced by defining scientific credit as something that cannot be quantified, scientific credit often ends up being quantified by default and in the most crude manner: by adding up the articles bearing the candidate's name. Scientists, editors, and administrators realize very clearly that this situation is irreconcilable with their views about how science ought to operate. And yet, it is far from clear how these problems could be solved within the very logic of the scientific economy they wish to uphold.

Another peculiarity of the attribution of credit and authorship in science is that it is deemed inseparable from the attribution of responsibility. A scientist gets credit, but has to take epistemological (and perhaps legal) responsibility for the truth of the claims that he or she publishes. These issues have become increasingly pressing in the wake of numerous cases of scientific fraud and misconduct. The development of large-scale collaborations and the publication of articles with hundreds of authors has only escalated this problem by making it harder to figure out which names listed on the byline should carry the burden of responsibility. Some proclaim that each co-author should be responsible for the entire publication. Others, instead, contend that responsibility should be limited to the extent of one's contribution. As with the definition of credit, these discussions are still waiting for closure and it is not clear how (or whether) that closure will come about.[14] What is clear, however, is that the pressure is building toward the reform of (or revolution in) the definition of scientific authorship.[15]

Culliton, *Harvard Tackles the Rush to Publication*, 241 SCIENCE 525 (1988); John Maddox, *Why the Pressure to Publish?*, 333 NATURE 493 (1988).

14. For a recent assessment of the state of the debate authorship in biomedicine, *see* Frank Davidoff, *Who's the Author?: Problems with Biomedical Authorship, and Some Possible Solutions*, 23 SCIENCE EDITOR 111 (2000).

15. *See* Richard Smith, *Authorship: Time for a Paradigm Shift? The Authorship System is Broken and May Need a Radical Solution*, 314 BRIT. MED. J. 992 (1997) (editorial); Richard Horton, *The Signature of Responsibility*, 350 LANCET 5-6 (1997); Richard Horton & Richard Smith, *Time to Redefine Authorship*, 312 BRIT. MED. J. 723 (1996); Fiona Godlee, *Definition of "Authorship" May be Changed*, 312 BRIT. MED. J. 1501 (1996); Evangeline Leash, *Is It Time for a New Approach to Authorship?*, 76 J. DENTAL RES. 724 (1997).

III. THE PECULIAR ECONOMY OF SCIENTIFIC AUTHORSHIP

In liberal economy, the objects of intellectual property are artifacts, not nature. One becomes an author by creating something original, something that is not to be found in the public domain. Copyright is about "original expression," not content or truth. Scientists, therefore, cannot copyright the content of their claims, because nature is a "fact" and facts are in the public domain. Also, saying that scientists are authors because their papers reflect personal creativity and original expression (the kind of claim that justifies copyright) would actually disqualify them as scientists because it would place their work in the domain of artifacts and fictions, not truth. A creative scientist (in the sense that IP gives to creativity and originality) is a fraudulent one. The only thing researchers or journals can copyright with regard to scientific publications is the "form" they used to express their factual findings. This gives them some protection against piracy, but copyrights in these texts do not translate into academic scientific credit because scientific credit is not about property rights, at least for the time being.[16]

Like copyright, patents also reward novelty as they cover "novel and non-obvious" claims. But, unlike copyrights, such claims need to be potentially 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" by virtue of being "pure science," or because they are about something that belongs to the public domain.[17] While it is increasingly common for scientists (mostly geneticists) to patent what might appear to be natural objects, they do so by arguing that these objects have been extracted from their original state of nature and packaged within processes (usually diagnostic tests) that are deemed useful.[18] Scientists can patent useful processes stemming from their research, and yet academic scientific authorship is defined in terms of the truth of scientific claims, not of their possible usefulness in the market. According to the categories and tools of intellectual property, a scientist qua academic scientist is, literally, a non-author.

16. While academic credit is officially construed as something that is based on peer evaluation, not property rights, one could also argue that the situation may be changing, and changing quickly. As universities rely more and more on income from royalties and licensing fees, it is not unthinkable that a scientist's ability to produce patentable claims (like his or her ability to attract substantial grants) may play a role in his or her hiring or promotion. To the best of my knowledge, however, the potential role of such considerations has not been made explicit by academic institutions.

17. *See* JEREMY PHILLIPS & ALISON FIRTH, INTRODUCTION TO INTELLECTUAL PROPERTY LAW 39 (3d ed. 1995).

18. Eliot Marshall, *Companies Rush to Patent DNA*, 275 SCIENCE 780-81 (1997) provides a review of recent trends. *See also* Gretchen Vogel, *Gene Fragments Patentable, Official Says*, 275 SCIENCE 1055 (1997). For an earlier overview on these issues, *see* DOROTHY NELKIN, SCIENCE AS INTELLECTUAL PROPERTY (1984).

Intellectual property rights are justified by saying that the author takes as little as possible from the public domain, or "previous art," and that, by adding to and transforming what she has taken from the public domain, she produces an original work or non-obvious useful device or process.[19] But a scientist is not represented as someone who transforms reality or produces "original expressions." And contrary to patent applicants who try to minimize their overlap with "previous art," scientists buttress their new claims by connecting them as much as possible to the body of previous scientific literature.[20] Fencing off a work from the commons of the public domain or "previous art" is a smart move if you want to secure private property. But it is a plainly self-defeating tactic if the claim you are putting forward is not about property, and if it can bring you credit only by being endorsed, used, and cited, but not bought as property, by your peers. The business practice that comes closest to science may be the "free software" movement.[21] Another partial analogy between science and IP may be found in the legal notion of "compulsory licensing," as the author, in exchange for a certain reward, relinquishes the right to control who may use her work. In science, however, one does not get monetary rewards but only citations from such a "licensing".[22]

The production of a "work" protected by IPR and that of a scientific claim follows radically different trajectories and attributes very different roles to the name of the author. A scientist is seen as a researcher who, with much work, detects something specific within nature – the domain of public and "brute" facts. Then, for that finding to be recognized as true, she has to put it back in the public domain and share it with the scientific community. 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. While the production of value in 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 it goes from "unspecified" to "specified truth."

19. This foundational assumption of intellectual property law has been challenged by several legal and literary scholars. Examples of this literature are ROSEMARY COOMBE, THE CULTURAL LIFE OF INTELLECTUAL PROPERTIES (1998); JANE GAINES, CONTESTED CULTURE: THE IMAGE, THE VOICE, AND THE LAW (1991); JAMES BOYLE, SHAMANS, SOFTWARE, AND SPLEENS: LAW AND THE CONSTRUCTION OF THE INFORMATION SOCIETY (1996); Martha Woodmansee, *The Genius and the Copyright: Economic and Legal Conditions of the Emergence of the "Author,"* 17 EIGHTEENTH-CENTURY STUDIES 425 (1984); Jessica Litman, *The Public Domain*, 39 EMORY L.J. 965-99 (1990); Peter Jaszi, *Toward a Theory of Copyright: The Metamorphoses of "Authorship"*, 1991 DUKE L.J. 455 (1991); MARTHA WOODMANSEE, THE AUTHOR, ART, AND THE MARKET (1994).

20. Greg Myers, *From Discovery to Invention: Writing and Rewriting of Two Patents*, 25 SOCIAL STUDIES OF SCIENCE 57-105 (1995).

21. I owe this point to Phil Allred. For references to the "free software" trend, *see* Eric Raymond, *Open Source and Hacker Anthropology*, *at* www.tuxedo.org/esr/writings/ (Aug. 24, 2000); *Open Source: Software Gets Honest*, *at* www.opensource.org; Free Software Foundation, *What Is Free Software?*, *at* www.fsf.org/philosophy/free-sw.html (last modified Sept. 24, 2000). For a recent discussion of the state of the debate, *see* Steve Lohr, *Code Name Mainstream: Can "Open Source" Bridge the Software Gap?*, N.Y. TIMES, August 28, 2000, at C1.

22. *See* PHILLIPS & FIRTH, *supra* note 16, at 29.

Both cases involve a transformation 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, the case of 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. The unique role of the author's name in science stems precisely from these difficulties. The name becomes the only device left to mark the production of a scientific claim out of nature.

IV. AUTHOR AS CAUSE OR AUTHORSHIP AS REWARD?

The definition of scientific authorship is further complicated by the fact that notions of credit and attribution of authorship are not only fuzzy, but their "fuzzinesses" are co-dependent. In IP, the definition of the author in terms of his creative contribution and personal expression provides the legal axiom for construing his products as objects in which the author ought to have rights. For instance, the 1976 Copyright Act does not define "author" but uses it as a primitive notion.[23] Ownership issues begin with the axiom that an author is one "to whom anything owes its origin . . . ."[24] The author is the prime mover who "causes" the product, thereby constituting it as his intellectual property. But, as I have argued, such a causal framework is inapplicable to science as it would undermine its epistemological authority by casting its claims in the category of artifacts. This creates a no-win situation – though a conceptually intriguing one.

The inapplicability of the traditional figure of the author as creator sets the definition of scientific authorship adrift because it is not clear what notions of authorial agency could be put in its place to draw the line and articulate the connection between the author and the credit he is due, while simultaneously upholding the epistemological status of scientific claims as non-fictional. One of the consequences of this conundrum is that what becomes conceptually destabilized is not just the definition of authorship, but also that of authorial credit. This problem is evidenced in the current debates among scientists, editors, and science administrators. While in IP the articulation of authorial rights follows from the assumption about who an author is and what she does, in science we see that that relationship is not one of one-way causality. It oscillates back and forth between the definition of author and that of his credit.

It is not uncommon to see the author defined in terms of what kind of credit is deemed to be authorial.[25] This would be like having IP start with

23. HALPERN ET AL., *supra* note 4, at 54.

24. Burrow-Gilles Lithographic Co. v. Sarony, 111 U.S. 53, 58 (1884), *cited in* HALPERN ET AL., *supra* note 4, at 54.

25. For instance, the International Committee of Medical Journal Editors (usually referred to as "ICMJE" or "Vancouver Group") frames its authorship guidelines not in terms of what an author does, but in terms of the kind of contributions that qualify a

rights and then move back to picture what kind of subject those rights could be attached to. For instance, if you say that data collection constitutes authorial credit, then the data collector is entitled to have her name in the byline. If not, she ceases to be an author and ends up listed in the acknowledgment section. Depending on the discipline, one may encounter either scenario. In sum, the scientific author oscillates between being the producer and the product of what she produces. (This dovetails with my previous suggestion that scientific authorship is not about rights, but about rewards).

V. The Coupling Of Credit And Responsibility

A reader familiar with the discourse of IP – a discourse that focuses on rights rather than responsibilities – might be surprised to see how frequently the inseparability of authorial credit and responsibility is invoked in discussions of scientific authorship.[26] If a claim about nature were like a product its author could sell in the market, then responsibility for its "faults" could be negotiated legally and monetarily in terms of liability. But this cannot apply to claims about nature because they are not owned by anyone, cannot be sold, and therefore appear to be alien to the logic of monetary liability.[27] While it sounds quite reasonable to say that a scientist should be responsible for what she publishes, it is much more difficult to figure out exactly what that means. Undoubtedly, scientific responsibility "sounds good," but what kind of object is it?

Technically, scientific fraud amounts to lying about nature. But what crime or misdemeanor is that? As a thought experiment, one could say that fraud is like "libeling" nature, but then nature is not exactly a legal subject entitled to the legal protection of its reputation. One could also look at other scientists – not nature – as the damaged party and argue that a fraudulent paper misleads other scientists into wasting time and resources doing work that rely on those fraudulent claims. But those scientists did not purchase that fraudulent paper the way a consumer may have purchased a flawed product. The fraudulent paper was in the public domain, and it was those scientists' choice to pick it up and use it. Of course things are much more complicated than this, especially because the economy of science is inherently based on trust and it is not clear whether it could operate outside of that framework. The point of my exercise in casuistry here is that, like credit, responsibility is simultaneously essential to the operation of science and yet impossible to reduce to one clear definition. I find it interesting that despite

researcher as an author *See* ICMJE, *Uniform Requirements for Manuscripts Submitted to Biomedical Journals*, 277 JAMA 277, 927 (1997). This subtle difference may be easily lost in the shuffle, but is conceptually crucial. Authorship is seen as a reward, not a cause (not unlike a Ph.D. degree conferred to students after they have fulfilled the appropriate requirements).

26. Sometimes, the essential inseparability of credit and responsibility is represented through the figure of the coin (authorship), with two sides (credit and responsibility). *See* Rennie et al., *supra* note 9, at 580; Davidoff, *supra* note 13, at 115.

27. These difficulties may contribute to the complexity of current debates about scientific fraud and how it should be punished.

the sense of moral outrage stirred by cases of scientific fraud, there are few tools to punish its authors besides firing them, denying them access to future funding, or in certain cases asking them to pay back the funds they have misused.[28] Most of these actions are, in effect, forms of exile or ostracism from the community, but carry few or no tangible legal consequences.

Both in the case of credit and responsibility, the problem is that a scientific claim is neither simply natural nor simply artifactual, in the sense that "natural" and "artifactual" assume within a logic that opposes public domain and private property. A scientific claim is neither nature itself nor an artifact in the traditional and legal sense of the word. As such, it operates in a legal no man's land. As in the case of credit, the default solution to the dilemma posed by the attribution of responsibility has been to attach it permanently (whatever "it" means) to the scientist's name. Intellectual property rights and responsibilities can be transferred contractually, but scientific credit and responsibility are seen as inalienable – that is, inseparable from the name of the original author. But while the coupling of credit and responsibility to the scientist's name is, I believe, a default move, it is not an arbitrary one.

Because it is not clear what "axioms" one could use to define credit and responsibility in science and to determine how they should be related, it appears that those categories can be defined only in the negative, as categories that are complementary to their counterparts in IP. For example, scientific authorship is not like IP authorship, scientific credit is not like intellectual property rights, scientific responsibility is not like financial liability, and scientific credit cannot be transferred like intellectual property rights. In sum, the coupling of credit and responsibility and their inalienable link to the scientist's name may be seen as a desperate one – one that is overdetermined by the lack of other possibilities.

This might be a bit speculative, but please indulge me. If you cannot treat scientific authorship as IP authorship, neither can you say that the author of science is nature itself, then you need to redefine the authorial function of the scientist in a way that does not turn him into an IP-style author and yet acknowledges the human cause of that claim about nature. This, I believe, has been achieved by treating the scientist not as a legal subject (who operates in an IP context), but only as a body with a name. Of course I am not saying that the people who practice science are not legal subjects, but simply that, insofar as they work as scientists, they operate in a peculiar economy in which what matters is their name (and the fact that there is a real person behind that name), not the rest of the "bundle of rights" that, as legal sub-

28. Adapting the False Claims 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 out of itself and adopt the logic of commercial fraud. Paulette Walker, *1865 Law Used to Resolve Scientific Misconduct Cases*, Chron. Higher Educ., January 26, 1996, at A29. Subsequently, some uses of the False Claim Act have been challenged in court. *See* Paulette Walker, *Appeals Court Overturns a False-Claim Ruling Against U. of Alabama at Birmingham*, Chron. Higher Educ., Feb. 7, 1997, at A37.

jects or citizens of specific nations, they may have attached to their names.[29] Scientists qua scientists are humans, but not quite legal subjects.

VI. Too Many Names, Too Few Names

Until the emergence of large-scale multi-authorship, science administrators and editors were able to treat scientific authorship as a non-problem, similar to its literary cousin. It seemed plausible to think of the scientist as the person who had the idea, did the work, wrote the paper, and took credit and responsibility for it. Despite all the differences between credit and responsibility in science and literature, the individuality of the scientific author seemed to provide a container for its hard-to-define functions.

Multi-authorship has unhinged this unstable but plausible-looking conceptualization, and has produced divergent reactions among science administrators and practicing scientists. Science administrators have tried to hold on to traditional notions of individual authorship and to treat multi-authorship as an aggregate of individual authors. For instance, the International Committee of Medical Journal Editors ("ICMJE"), an influential body representing hundreds of anglophone biomedical journals, has required that each name listed in an article's byline must refer to a person who is fully responsible for the entire article, no matter how long that byline might be, and not just for the task he may have performed.[30]

This stance emerged also as a response to the finger pointing that tends to develop among co-authors accused of having published fraudulent claims. In some of these cases, senior authors listed in the byline have argued that they were either unaware that their name had been added to the author list, a sort of "inverse plagiarism" aimed at increasing the publication chances of the article, or that, although they did participate in the research, they had nothing to do with the fraudulent aspects of the publication.[31] While these claims were found ad hoc and self-serving in some instances, they did match the investigators' findings in others.[32]

Additionally, the ICMJE has been concerned with what it saw as the inflation of authorship credit due to multi-authorship. For instance, how can one be sure that all these names refer to people whose diverse skills were actually necessary for and contributed to such a large project? The ICMJE's overall

29. As puzzling as it may sound, this peculiar image of science has a history. It can be traced back at least to the idea of the "republic of letters" – an imagined community to which many early modern scientists claimed to belong.

30. "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." ICMJE, *supra* note 24, at 928.

31. *See* A.S. Relman, *Lessons from the Darsee Affair*, 308 New Eng. J. Med. 1415 (1983); R.L. Engler et al., *Misrepresentation and Responsibility in Medical Research*, 317 New Eng. J. Med. 1383 (1987); *President of Royal College Resigns*, 309 Brit. Med. J. 1530 (1994); Claudia Court & Luisa Dillner, *Obstetrician Suspended After Research Inquiry*, 309 Brit. Med. J. 1459 (1994); Jane Smith, *Gift Authorship: A Poisoned Chalice?*, 309 Brit. Med. J. 1456 (1994).

32. Engler et al., *supra* note 30, at 1383-89.

response has been to put forward stringent definitions of authorship in an attempt to control the scale of multi-authorship, rein in inflation, and facilitate the enforcement of authorial responsibility. Rather than developing a radical redefinition of authorship in the light of the new conditions of production brought about by large-scale collaboration, the ICMJE has gone back to reinforce the figure of the individual author – the only figure it saw fit to sustain the credit-responsibility nexus.

Accordingly, what qualifies a person for authorship are her intellectual contributions, not other forms of labor that are deemed non-intellectual.

> 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 all be met. Participation solely in the acquisition of funding or the collection of data does not justify authorship. General supervision of the research group is not sufficient for authorship.[33]

That is, the scientific author is separated from and placed above those "workers" who contributed to the production of that text but did not contribute to its uniqueness, to the specificity of its claims and its epistemological status.[34]

Several practitioners have objected to this definition. Others simply never noticed or ignored it, despite the fact that they published in journals that had endorsed and published such a definition of authorship.[35] The critics' position has been that they cannot be responsible for those aspects of a project that fall outside of their work and expertise.[36] They have also argued that a narrow definition of authorship is unfair to many scientific workers who, while not engaged in the conceptualization and writing of a certain publication, still made such work possible.[37] If these contributors do not receive authorship credit, they will receive no credit at all. Being thanked in the

33. ICMJE, *supra* note 24, at 928.

34. For a more extensive discussion of the ICMJE's distinction between "intellectual" and "non-intellectual" contributions, *see* Mario Biagioli, *Aporias of Scientific Authorship: Credit and Responsibility in Contemporary Biomedicine*, *in* The Science Studies Reader 12, 21-24 (Mario Biagioli, ed., 1999).

35. *See* Raj Bhopal et al., *The Vexed Question of Authorship: Views of Researchers in a British Medical Faculty*, 314 Brit. Med. J. 1009 (1997).

36. Avram Goldstein, *Collaboration and Responsibility*, 242 Science 1623 (1988) (*but see* Arnold Friedhoff's letter on the same page). *See also* Letters by Jay Pasachoff, Craig Loehle, and Tobias Bsakin in *Responsibility of Co-Authors*, 275 Science 14 (1997).

37. *See* Domhnall Macauley, Letter to the Editor, *Cite the Workers*, 305 British Med. J. 6845 (1992); Ian Grant, Letter to the Editor, *Multiple Authorship*, 298 British Med. J. 386 (1989). *See also* the letters to the editor in response to Jerome Kassirer & Marcia Angell, *On authorship and Acknowledgments*, 325 New Eng. J. Med. 1510 (1991) (editorial) published in 326 New Eng. J. Med. 1084 (1992) (correspondence). A few editors have taken these complaints seriously. An editorial in *Lancet* a few months ago argued that: "Many researchers think [ICMJE's] definition is out of touch with their own research practice. It leans toward being a senior authors' [sic] charter, falling short of providing explicit credit for those who actually do research. . . . On balance, the definition

acknowledgment section is not something one can put on her curriculum vitae. Researchers in large-scale biomedicine projects tend to think of authorship in corporate terms, that is, as stocks in a company that carry credit and responsibility in proportion to their share of the total value of the enterprise. To them, their names are literally their stocks.

While one can empathize with the critics, their position is fraught with as many tensions as that of ICMJE. Their "corporate" perspective would require a means to demarcate and quantify their contributions and responsibilities that flies in the face of the current logic of the economy of science (especially that of responsibility). In some ways, they are trying to apply the categories of liberal economy to something that, instead, is complementary to it. At the same time, the ICMJE's attempt to control the problems of authorship simply by controlling the number of authors smacks of well-intentioned wishful thinking, and is at odds with the changing realities and intricacies of large-scale collaborative research.

I believe a co-authored scientific publication makes for a very unusual pie whose features resist, in different ways, what both the ICMJE and its critics would like to do to it. Surprising as it may sound, cutting it in thin slices does not necessarily reduce the value of each slice. Rather, it leaves that value undetermined. As a result, multi-authorship does not produce credit inflation (as the ICMJE fears), nor does it allow for a quantitative division of the "shares" (as the critics would like). *Mutatis mutandis*, this is not unlike what we find in copyright law where all "authors of a joint work are co-owners of copyright in the work[,]" which means that "each joint owner of a work may exercise *all the rights* of a copyrights owner with respect to that work . . . ."[38] Of course, an author of a joint work cannot simply sell it and take off with the bundle. She is legally accountable to the other joint authors. For instance, she has to share the profits with them and may not sell or license the work in a way that would curtail the rights of the other joint authors, as by giving out an exclusive license to a third party.[39] What is interesting here is that even copyright law, despite the range of legal categories it can draw upon, is unable to divide up the pie of authorial rights among the co-authors. All it can do is make each joint author responsible for splitting the income deriving from the uses of those rights, though even then the modalities of that split remain a matter of negotiation.

While, as I have tried to argue, scientific authorship is not about rights, and therefore the IP doctrine of the indivisibility of copyright among co-authors cannot be applied to it, I still think we have a family resemblance here in the sense that, like the rights in a co-authored work, scientific multi-authorship is not a zero-sum game. The main difference in these two cases is that in a co-authored work, one can draw the line between the indivisible

seems to fail important tests of relevance and reliability." Richard Horton, *The Signature of Responsibility*, 350 LANCET 5, 5 (1997).

38. HALPERN ET AL., *supra* note 4, at 55 (emphasis added) (citing 17 U.S.C. § 201(1994)).

39. *Id.*

rights in the work and the monetarily divisible income from those rights; in the case of scientific multi-authorship such a line is nowhere to be found because a scientific claim is not about property rights.[40] So adding a name to the byline of a scientific article does not reduce the value of the other authors' contributions by any tangible amount because it is not clear what might be the overall value of that text or its parts.[41] In the end, scientific authorship seems to work like a hologram in which each fragment contains the whole.[42] However, it is not that each name contains full authorship in a determinable, positive sense. It works that way, but only as a negative, default effect. In science, a co-author becomes a full author because it is not clear how one could deny her that status given the chain of indeterminacies surrounding the function of the scientist name and the value of a scientific work.

VII. FROM AUTHORSHIP TO CONTRIBUTORSHIP AND GUARANTORSHIP

Recently, two radical reframings of scientific authorship (coming from two very different disciplines) have been put forward and implemented, if only within limited constituencies. Although it is unlikely that they will settle all debates about authorship, at least they are expanding both the practical options and the conceptual vocabulary for dealing with these issues.

The first example emerges from the biomedical community. In a recent article published in *The Journal of the American Medical Association* ("*JAMA*"), Drummond Rennie (one of *JAMA*'s deputy editors) and his collaborators have radically departed from the traditional definition of scientific authorship:

> Because the current system of authorship is idiosyncratic, ambiguous, and predisposed to misuse, we propose in its place a radical change: a new system that is accurate and discloses accountability. We propose

40. Things are more complicated in the case of scientific multi-authorship because the value of a scientific work is not expressible in a standardized unit of measurement. So, while the joint author of a copyrighted work can at least use money as a unit of measurement in negotiating the distribution of income generated by that work, scientists and their administrators do not have that option (at least not within current definitions of scientific credit).

41. "So the expansion in numbers of authors per article has tended to dilute accountability, while scarcely seeming to diminish credit." Rennie et al., *supra* note 9, at 580. While the scarce diminution of credit is cast as a pathology by Rennie et al., I believe that what they have correctly observed is a structural (not abnormal) feature of scientific authorship.

42. Other factors may contribute to this. Readers or evaluators experience a scientific publication as a whole, not an assemblage of authorial contributions. That has much to do with the way an article is written and printed. The names of the authors are presented at the beginning, but their specific contributions are not flagged within the technical narrative. The "voice" of that narrative is a unified one, no matter how many people may be behind it. Therefore, the readers' perception of a work as an entity casts its authors as the producers of a whole. Consequently, more names on a byline does not mean more "owners" of identifiable and quantifiable shares of the work, but more authors of the same whole.

> the substitution of the word and concept contributor for the word and concept *author*. . . .
> Abandoning the concept of author in favor of contributor frees us from the historical and emotional connotations of authorship, and leads us to a concept that is far more in line with the actuality of modern scientific cooperative work.[43]

Rennie and his collaborators have struck a sympathetic chord among other editors and, within two years, leading medical journals like *JAMA*, *Lancet*, *Annals of Internal Medicine*, *British Medical Journal*, and *American Journal of Public Health* have implemented versions of their proposal.[44]

According to Rennie, each person who "has added usefully to the work" should be listed as a "contributor."[45] Journals should not limit the number of contributors.[46] Each name should be attached to a verbal description of that person's contribution, and the contributors' list should be published on the article's first page. These blurbs are reminiscent of film credits, but are much more descriptive and do not need to make use of standardized job titles. The contributors are asked to write down what they did, without packaging their work into pre-existing categories. The team is then asked to ratify these self-descriptions, and is also given the opportunity to attach numerical values to each contribution as a percent value.[47] These percentages would not represent absolute measurements of those contributions' value, but only the group's local assessment of them. Collectively, the contributors should also choose the names to be published in the byline if space constraints make that necessary (though both those listed or not listed in the byline are treated as contributors and have their tasks described in the contributor list). The order in which names are listed in the byline should reflect the importance of their contribution, in descending order.[48]

This proposal's goal is explicitly pragmatic: to add transparency to a traditionally opaque process, and to reduce its arbitrariness for authors, editors, and users. The additional information provided by the contributors' job descriptions would give the reader a much better understanding of who did what. Similarly, tenure committees and institutional evaluators would have their work simplified (though not necessarily reduced) by these short narratives.[49] This information would also provide the authors themselves with some safeguard against arbitrary distribution of credit, because potential credit "usurpers" would have to write down and therefore make explicit the

43. Rennie et al., *supra* note 9, at 582 (*emphasis in original*).

44. For an early assessment of the experiment, *see* Veronica Yank & Drummond Rennie, *Disclosure of Researcher Contributions: A Study of Original Research Articles in* The Lancet, 130 ANNALS OF INTERNAL MED. 661 (1999).

45. Rennie et al., *supra* note 9, at 583.

46. *See id.*

47. *See id.* at 582.

48. *See id.* at 583.

49. The qualitative information included in the contributors' job description might force the evaluators to stay away from quantitative analyses of the publication list. Their evaluations, therefore, may become more accurate but perhaps even more time consuming.

credit they are taking away from colleagues. For the same reasons, they could also play an important role in assessing responsibilities in the case of fraud allegations by holding the contributors responsible for what they claimed to have done. Furthermore, the order of the byline would cease to be tied to local disciplinary customs – a practice that is made increasingly problematic by the confluence of many different subdisciplines and subcultures into large-scale projects.[50]

This proposal also introduces important conceptual innovations. The ICMJE's two-tier distinction between the names of authors and those of people entitled only to an acknowledgment credit is virtually erased. The categorical hierarchy between the author as the "creator" of the distinctive traits of the work and the "helpers" who provided only the background conditions for the creator's work is replaced by different degrees of contributorship. Every person who added something to the project is treated as a contributor, provided he or she is willing to write down what he or she did.

Moreover, while the name of the contributor would continue to work as an entity that constitutes a text as a "work," it would also become simultaneously circumscribed by a description of its own agency. In other words, the contributors' names do not work like names of traditional "certifying" authors, like those of IP authors. Rather, they are names of workers whose claims of contributorship should be assessed by the readers (that is, by the "market") based on the description of what they have done. This clarifies one of the crucial issues we have encountered earlier on: scientific "authorship" is about rewards, not rights. The "author" is the producer of the work, but he is also "produced" (i.e. recognized and rewarded as such) by his peers.

While this proposal reconceptualizes authorship credit and distances it from the figure of the traditional author, it does a more conservative job when it comes to scientific responsibility. But the innovation, however modest and unarticulated, provides interesting food for thought.

Contributors are to be paired with "guarantors," people whose role seems to resemble that of the traditional and all-responsible scientific author envisaged by the ICMJE:

> All contributors are fully responsible for the portions of the work they performed and have some obligation to hold one another to standards of integrity. At the same time, special contributors must be designated and disclosed as guarantors of the whole work. Guarantors are those people who have contributed substantially, but who also have made added efforts to ensure the integrity of the entire project. They organize, oversee, double-check, and must be prepared to be accountable for all parts of the completed manuscript, before and after publication. In this way the role of the guarantor is precisely defined and differs from that of "first author" or "corresponding author" or "senior author". . . .[51]

50. Usually, the order of authorship is a matter of disciplinary conventions, though the first and last author tend to be considered the most important ones, leaving the names in the middle somewhat unranked.

51. Rennie et al., *supra* note 9, at 582.

At first, the proposal seems to combine the two conflicting notions of responsibility put forward by the ICMJE and its critics. Contributors are responsible for their share of the work, but then there are also one or more guarantors who are responsible for all of the work. Judging from the reception of the proposal, many readers and editors have had a hard time telling the guarantor and the traditional author apart. Only one journal, in fact, has decided to experiment with the idea of the guarantor.[52]

But there may be the germ for a new and interesting notion of responsibility somewhere in here, though one that is resisted by Rennie himself.[53] While the proposal does a careful job articulating the role of the contributor, it only offers an example of a "bad" guarantor (Felig) and of a "good" one (Collins):

> A Yale advisory committee found that Felig had exercised "poor judgment" in not aggressively investigating charges that his junior had doctored data. In contrast, it seems that Collins, director of the National Center for Human Genome Research at the NIH, responded with dispatch. Accepting responsibility for the aftercare of his work, Collins quickly corrected the published literature by exposing tainted data in five articles, *thereby preventing other researchers from wasting further efforts in trying to replicate the faulty reports.*[54]

While I do agree with Rennie et al. that, under the circumstances, Collins did the right thing, it is not clear how his behavior matches all the features of what they take to be a good guarantor. If the guarantor is supposed to insure the integrity of the entire project and to organize, oversee, and double-check the publication, then Collins failed. Yet, he is presented as an exemplar of what a good guarantor should be and do.

There is a subtle but important conceptual difference taking shape here. According to the ICMJE guidelines (but also according to half of the definition of the guarantor), Collins was a "bad" author or guarantor because his name appeared on a fraudulent paper. If one sticks to an absolute notion of responsibility, Collins could be said to have been responsible for fraud. If instead one reinterprets the role of the guarantor as that of an auditor, we get a very different picture. Collins may have failed as an auditor (he did not catch the fraud before publication) but that does not make him responsible for that fraud. His responsibility would be limited to the auditing process, but would not extend to the production of the product he is auditing. The latter kind of responsibility should belong to the contributors.

Another important difference between traditional notions of responsibility and what we find, in potential form, in Rennie's proposal emerges when we focus on the guarantor's role as the person responsible for the aftercare of the publication and not just the process that leads to its publication. Collins

52. This is the case with *The British Medical Journal.*

53. Drummond Rennie, audience comment during author's discussion at Council of Biology Editors Retreat on Authorship, Montreal, May 1999.

54. Rennie et al., *supra* note 9, at 582-83 (emphasis added).

is presented as a good guarantor largely because he cleaned up the mess produced by the fraud. In sum, one could redefine the guarantor as the person who is responsible for (1) the auditing, not that which is audited, and (2) the clean-up operations after fraud allegations are raised, but not for the mess she has to clean up.

I do not know whether this interpretation is something scientists and their administrators would accept. What interests me here are the slippages between very different views of responsibility that seem to be happening in this proposal as it tries to define the guarantor – slippages that may be pointing to a differentiation developing within the category of responsibility. Moreover, like credit, responsibility appears to be turning into a more "operational" category and less of an "essential" feature attached to the name of the author. This turn toward operational views of credit and responsibility seems to be coupled with an increasing subdivision and distribution among different people of the functions that used to be kept together under the all-encompassing figure of the author. Scientific authorship as we knew it may be falling apart, or it may be simply stripping itself of all those functions it could no longer juggle.

VIII. The Corporate Unburdening Of Authorship

Another, completely different notion of scientific authorship has emerged at about the same time, but in a very different discipline and independently from the debates that have occupied biomedical practitioners and editors. Its introduction has not been the result of the kind of heated debates found in biomedicine. The proposal has not even been published, but only distributed electronically and posted on a laboratory's internal web page. While it still makes use of the term "author," the concept behind the word is not something an IP lawyer would be familiar with.

A few years ago, a team of high-energy particle physicists working at Fermilab appointed a committee to develop bylaws for regulating their multi-institution (and multimillion-dollar) collaboration. It was felt that the collaboration had greatly expanded in size and level of complexity, but was still operating according to traditional customs known by a few elderly participants who were now approaching retirement age without having consigned their wisdom to paper.[55] As part of these bylaws, the committee articulated the definition of authorship and the modalities of its management.[56] The proposal was approved in 1998. Similar authorship guidelines are now being considered at other large laboratories, like CERN in Europe.

55. John Huth, Professor, Department of Physics, Harvard University, Lecture at the Harvard Department of History of Science (November 1998). John Huth was heading the committee in charge of formulating the authorship guidelines for CDF.

56. "As it turns out, CDF has absolutely nothing written down on authorship guidelines until I started writing them. What you have is the closest approximation to what I could term a 'oral tradition.' Nonetheless, it is widely agreed upon." Email from John Huth to author (April 9, 1998) (on file with author).

The Collider Detector at Fermilab Collaboration ("CDF") is a consortium of institutions and universities that support and staff the laboratory. Potential members are engineers, students, and physicists who are said to be "blessed" (i.e., selected) by their home institution for work at Fermilab. To be approved for actual membership, a Ph.D. physicist is required to dedicate at least fifty percent of his research time to CDF experiments over a three-year period.[57] Graduate students, instead, are required to work fulltime in the Collaboration, and technical personnel gain membership by "making major contributions to CDF experiment."[58]

The CDF Collaboration has stipulated that every publication emerging from the lab should include all names included in the so-called "Standard Author List."[59] This list includes hundreds of names, which are to be included in the byline in alphabetical order, independently from what their specific contribution to that paper might have been.[60] The Standard Author List is updated bi-annually by a committee that reviews the authors' fulfillment of membership requirements in the Collaboration.[61]

All members are entered in the Standard Author List, but only after they have done one FTE-year service work in the Collaboration.[62] This simple bureaucratic requirement speaks volumes about the different conceptions of authorship held by CDF and ICMJE. What differentiates a member from an author is not a professional hierarchy. Students, technicians, and Ph.D. physicists are all eligible for authorship, although the ICMJE guidelines effectively exclude laboratory technicians from authorship. The kind of work they do does not matter either, unlike what we find in the ICMJE guidelines that restrict authorship only to those in charge of the more conceptual tasks. Instead, at CDF only a member who has paid his dues through labor becomes an author.

The "labor mentality" that seems to characterize CDF, as opposed to the "originality mentality" that frames IP and the ICMJE guidelines, is inscribed in CDF's leave policies. A member is allowed up to one year's leave of absence without losing her author status during that period.[63] This means that for up to a year her name appears on all publications produced while she is not there, based on research she may or may not have directly contributed to. Similarly, a person who ceases to be a CDF member remains on the Standard

57. *See* Bylaws of the CDF Collaboration, Part III ("Membership") [hereinafter "Bylaws"]. The version of the Bylaws of the CDF Collaboration used in this article was received from John Huth, via email, on April 9, 1998 and is on file with the author.

58. Bylaws at Part III(1)(3).

59. Guidelines for Authorship in the CDF Collaboration, Section 0(ii) [hereinafter "Guidelines for Authorship"]. Visitors may be added to the list after approval, while people already on the list may elect to have their name not included in specific publications. The version of the Guidelines used in this article was received from John Huth, via email, on November 4, 1998 and is on file with the author.

60. *See* Guidelines for Authorship, Section 5. *See also infra* Appendix A.

61. *See* Guidelines for Authorship, Section 7.

62. *See* Guidelines for Authorship, Section 1.

63. *See* Bylaws, Part III.

Author List for a year after her departure.[64] This kind of authorship in absentia would be anathema to the ICMJE and to Rennie (and would probably puzzle more than a few IP lawyers). But it makes perfect sense if you think of authorship in terms of credit for accumulated labor. A member does not receive authorship credit until she has worked for a year, and maintains author status for a year after she stops working. To use an image that seems ubiquitous these days, she earns her "stock options" in CDF, and sells them back to CDF when she leaves.

These policies suggest that physicists do not think of responsibility in the same terms biomedical practitioners do. The very idea of an absentee (that is, a de facto "irresponsible") author would be inconceivable in biomedicine. But CDF physicists do not have a lax attitude about responsibility. Responsibility is simply managed and distributed in ways that make it independent from the presence or absence of an individual author. While both the ICMJE and Rennie's proposal stress individual responsibility, CDF treats it as a corporate matter.[65]

The reasons behind the specific notions of authorship, credit, and responsibility developed at CDF have much to do with the internal structure, physical location, and culture of that community. Biomedical practitioners participating in large clinical trials do not tend to work in the same lab. Like the sources of their data, they may be scattered over hundreds of miles and various institutions. Several of them may be only marginally familiar with each other. Physicists, instead, have only a handful of places where they can detect particles. As a result, CDF represents a kind of collaboration that is tied to a specific apparatus (from which it derives its name). Significantly, its stated objective is:

> [T]o provide the basis for the participation of the Members and Collaborating Institutions in the construction and operation of the Collider Detector at Fermilab, and the analysis of data obtained from the Collider Detector at Fermilab (CDF).[66]

Although they are affiliated with different home institutions, the CDF members work at the same site (which they also help build) for a substantial portion of their research time. Opportunities for getting to know their colleagues are plenty. Operating in a bureaucratized environment structured by bylaws, committees, and procedures reinforces their sense of corporate identity—one that would be hard to find in biomedicine.

The bureaucratization of the author's name at CDF indicates that authorship credit and responsibility is not crucial in that setting, and it is not crucial because those functions have been taken up by other relations. Authorship has become more of a "fact of life" than a struggle for professional life as it is in biomedicine. Credit does not reside primarily in one's publication list sim-

64. *See* Guidelines for Authorship, Section 8.

65. I am referring to the responsibility of the contributor, not the guarantor, as outlined in the proposal by Rennie et al.

66. Bylaws, Part II ("Objective").

ply because everyone develops similar lists in the period he is part of the collaboration. Credit develops through the professional appreciation one gains from colleagues by working with them on a regular (if part-time) basis throughout the length of the project. Credit seems to travel through letters of recommendation or personal communications more than through publications lists. And given the remarkable size of the collaboration, and the presence of scientists from many different institutions, one's colleagues within the Collaboration may already constitute a very large portion of one's disciplinary peers and potential employers. Such a relatively close and inclusive community may reduce the role of the c.v. as "professional passport" – a role that is crucial in more dispersed and less interdependent communities like those of biomedicine.

As with credit, CDF's approach to responsibility is also framed by the structure and scale of its community. Nowhere in the CDF bylaws or in its authorship guidelines can one find the biomedical mantra about the inseparability of credit and responsibility and their essential link to the name of the individual scientists. What one finds, instead, are detailed corporate protocols for the internal review of manuscripts to be submitted for publication. It seems that the physicists at CDF do not need to rely on the name of the scientist as a device to keep credit and responsibility together simply because they are comfortable with the procedures they have developed for managing these two issues separately.

When a sub-group of CDF wishes to publish an article or to present a conference paper, the text goes through three rounds of internal review.[67] The first is a preliminary approval from the publication committee, the last two take place on CDF's internal webpage. The text is posted and all members of the collaboration are asked to comment electronically. After comments are sent and answered, a revised version is posted and the process starts again. After two rounds of revisions, those whose name is in the "Standard Author List" may withdraw their name from that publication if they are unsatisfied with the end product.[68]

Interestingly, an article carrying fewer names would appear to be less (not more) credible than one with more names – a scenario that is exactly opposite to what happens in biomedicine. Given the remarkable size of the collaboration in relation to the size of the field, most of the competent reviewers are inside of the CDF. So more names on the byline mean more peer-endorsements, especially because those are the names of the peers who would have most to lose if the article turned out poor or, worse, fraudulent. The function of peer-review – a function that in biomedicine is constitutive of authorship but is farmed out to colleagues external to the project – is performed internally. While this would be unacceptable in biomedicine, or could even be seen as a clear case of conflict of interest, here it is a non-problem because the inside and the outside of the community of peers overlap quite substantially.

67. Guidelines for the CDF Publication Process.
68. Guidelines for Authorship, Section 3.

Like peer-review, issues of misconduct are handled internally. CDF members can be involuntarily removed from the Collaboration if they are found responsible for professional misconduct. Fraud and misconduct do not seem to have assumed the heated moral connotations they have in biomedicine. Interestingly, the sanctions leveled against those found responsible for misconduct are exactly the same as those who do not live up to their labor commitments.[69] They are simply fired. Misconduct is assessed by specific committees operating according to the rules specified in the CDF bylaws without input from other agencies and institutions.[70]

One might think of expulsion, a form of exile from the community, as a fairly mild punishment. But because there is not much community outside that community and because the Collaboration includes representatives from many institutions and universities, expulsion is likely to have fatal professional consequences. In fact, I believe that it precisely because of the community's ability to enforce these sanctions, and because of the effectiveness of these sanctions, that talk of responsibility is minimal at CDF. If you can enforce responsibility, you do not need to legislate (or obsess) endlessly about it as seems to be the case in biomedicine.

IX. Conclusions

Despite the vast terminological and substantial differences between the CDF guidelines and those put forward by Rennie and his collaborators, they share a common denominator. No matter what names are given to it, scientific authorship is losing (or has already lost) its role as the containment vessel for credit and responsibility, and the vast problems posed by their definitions. The development of large-scale multi-authorship is directly responsible for that. While the names of the scientists remain crucial to the economy of science, the logic of that economy, and the role of the name within it, is changing. The various functions of authorship are being redistributed among different people within a team or are taken up by corporate bodies and procedures. The shift from "essentialism" to "operationalism" seems clear.

What is also clear is that there are no good or bad definitions of credit or responsibility. My brief description of CDF's protocols may cast it as a success story compared to the apparent chaos found in biomedicine. But CDF's ability to reframe authorship in ways that seem satisfactory to its members is predicated on the very specific internal structure, size, and facility-based nature of that community. As I have tried to show, the vast differences between their authorship practices and those found in biomedicine can be directly related to their different professional ecologies. I am as certain as I can be that biomedicine (as it is today) could not adopt something like CDF's guidelines.

The inherently community-specific nature of scientific authorship is not a problem but a necessity. We cannot come up with a unified notion of scien-

69. *See* Bylaws, Part III.
70. *See id.*

tific authorship in the same way some would like to achieve the globalization of intellectual property and the notion of "author" behind it. Scientific authorship is a misnomer, a historical vestige. It is not about legal rights, but about rewards. Similarly, scientific responsibility is not a legal category, but a set of relations among colleagues. As such, they cannot be conceptually unified under legal axioms. It make sense, therefore, that scientific "authorship," whatever shapes it might take in the future, will remain tied to specific disciplinary ecologies.

APPENDIX A

List of Authors in the CDF Collaboration*

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