

Documents of Documents

Scientists' Names and Scientific Claims

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THIS CHAPTER IS ABOUT the name of the scientist, how it functions, and the kinds of relationships it documents. When we think about documents in science, what comes to mind most naturally is scientific evidence: measurements, diagrams, photographs, statistics, inscriptions of particles' trajectories, fossils, specimens, and so on. A scientist's name printed at the beginning of an article does not stand out as an obvious document, but may be taken to be part of the post facto packaging of a claim, of what happens when a claim is written up, made public, and credited. Names may seem to belong to an article's prefatory matter, the part of the text one glances at on the way to its real content.

I argue, instead, that authors' names are crucial documents of the workings of the economy of science, of the process through which scientific documents are constituted. Names work as the hinge between two apparently distinct moments of scientific production: the development and the publication of a claim. Such a demarcation plays a constitutive role in the economy of science as it strives to separate "natural" scientific facts from the "social" actors who produce them, but is also fraught with unavoidable tensions. By following the scientists' names around we can trace some of these tensions while also mapping the ways in which different communities connected to science (scientists, administrators, policymakers, and science studies practitioners) attach different documentary value to these names.

What counts as a document or evidence to a scientist is quite different from the range of materials that science studies practitioners look at to

understand how science works. In its attempt to map out the processes through which scientific authority is constructed, science studies considers a far broader spectrum of documents than the evidence scientists present in their publication to buttress their claims.¹ Teasing out the many components of their “authority,” their genealogies, and how they are brought and held together in specific contexts has directed the field toward increasingly detailed analyses of scientific practices and their documentary traces—documents that may have looked like trivia to previous generations of historians and especially philosophers of science who based their work on the analysis of printed sources only. The documentary trend associated with sociocultural analyses of scientific practices has expanded the range of relevant documents not only to manuscript drafts of printed texts, but everything from laboratory notebooks, instruments, correspondence, gifts and exchanges, patent applications, illustrations, accounting documents, citation patterns, literary, artistic, and filmic representations of science, oral histories, institutional ecologies, water-cooler gossip, architectural layouts of laboratories and departments, and a slew of other objects and texts.²

It is, then, surprising to see that such an empirical focus on the construction of scientific authority seems to have left scientific authorship by the wayside.³ Scientists’ names have not been ignored by science studies, but have been usually treated within macroanalyses of citation patterns (which tend to treat names as “units” to be counted, not as documents to be opened up), or as traces of struggles for scientific authority.⁴ Most science studies practitioners (and, I believe, quite a few scientists) would say that the presence of a name on the byline of a science article should not be taken at face value as a document of the agency behind the production of that text. Rather, the presence of a name, or its position in relation to other names, could be the result of negotiations, tactical decisions, or even usurpations. But while such a perspective has added complexity to our understanding of how credit and agency may or may not be attached to names, it has also upheld the apparently natural connection between agency and authorial names while acknowledging that social negotiations or power play may modify, amplify, or distort such a connection in specific contexts. Even those who have proposed epistemological models in which nonhuman actants, instruments, or cyborgs share agential roles with social actors have not expanded their critiques of human agency to include what may be its very epitome: authorship.⁵

In sum, while science studies has moved in different directions toward questioning traditional notions of agency and individual creativity that had

helped construe scientific authorship as an unproblematic concept, in the end it has stopped short of opening up the authorship “black box.” That is, the field has not asked the fundamental question of how and why the name of the scientist has become the fundamental unit in the “metrology” of both professional credit and epistemological responsibility of academic science. This essay looks at the space between the scientific text and the scientist’s name to understand the unstable relations that link and constructs them as two different kinds of documents: one of nature, the other of human agency.

Scientists’ Mundane Philosophy of Their Names

Authors’ names are of a peculiar kind, and not only in science. At first they seem to have a simple relation to the work, like cause to effect or, as it is often said, like father to son.⁶ But with a little more work one can see that the kinship relation moves in other directions too: personal names (even some that do not refer to actual people) become authors’ names by being linked to works they are deemed to (but may not) have produced. The kinship between authors and works is a tricky two-way street, if indeed “street” is the appropriate model.

Legal and literary studies have analyzed the inherent instability of authorship—an instability that affects the relationship between the authors’ name and the work, but also extends to the categories of “author” and “work” themselves (e.g., Foucault 1977, 113–38; Coombe 1998; Rose 1993; Hesse 1991, 1789–1810; Woodmansee and Jaszi 1994; Gaines 1991; Boyle 1996). Many of the tensions surrounding the name of the author take different shapes in relation to the specific kinds of work and the economies in which they circulate. For instance, the scientist’s name functions very differently from the name of the literary author or the patent holder. This follows from the way “work” is specifically defined in academic science, and from the kinds of functions (credit and responsibility) that are attached to the author’s name within that economy. As I will discuss in a moment, an academic scientist is not an author who can hold intellectual property rights on his or her claims, nor one who receives credit by producing artifacts (novels, music, paintings, and so forth). The author function in science is related to intellectual property law, but that relation is one of complementarity, not analogy.

It is not completely clear why a scientific text would need to have an author, as one could argue that the epistemological status of a scientific text

ought to be grounded in nature, not in an author's name. However, far from fading in importance or dwindling in number, more and more names are being attached to scientific articles, and their placement order in the byline has become an increasingly complex and contentious issue (e.g., Fye 1990, 317–25; Huth 1990; Strub and Black 1976; Regalado 1995, 25). Furthermore, the recent development of large-scale multiauthorship has added a new important twist (perhaps much more than a twist) to the function of the scientist's name. These days, especially in biomedicine and particle physics, it is not uncommon to find articles with hundreds of authors name listed on them, papers whose authors' byline may take up as much space as the technical part of the text (fig. 4.1). Because of the sheer amount of work entailed by "big science" projects and the corresponding need to bring together different skills within one research team, multiauthorship has become a fact of scientific life.

Quantitative change has triggered a qualitative change that, in turn, has pushed the scientific author-function into unmapped territories. In the last ten years or so, hundreds of articles, editorials, and empirical studies published in scientific journals have tackled the thorny problem of redefining authorship in the context of large-scale collaborations.⁷ Much of this literature reflects widespread concerns about the definition of authorial responsibility (not just authorship credit). The embarrassment produced by a series of well-advertised cases of scientific fraud, the extensive finger-pointing and self-exculpations among the coauthors of these publications, and the politicians' growing interest in regulating science have driven the scientists' analyses of authorship as much as their concerns about the distribution of credit in collaborative scenarios (Relman 1983; Engler et al. 1987; Stewart and Feder 1987; Braunwald 1987; Institute of Medicine, Committee on the Responsible Conduct of Research 1989).⁸

The new contexts produced by multiauthorship, I believe, escape much of the traditional theoretical discourse about authorship—a discourse that, despite its analytical power, has been typically constrained by the figure of the single author and the conceptual scenarios it makes thinkable.⁹ Current discussions about definitions of authorship developed among scientists, science administrators, editors, and funding agencies do acknowledge the remarkable instability of authorship, but have had little or no use for the tools developed by critical legal scholars or literary critics.¹⁰ This reflects the specificity of the problems connected to scientific authorship as well as the very different division of labor between users and critics of authorship that we find in science and literature or legal studies. While authorship

studies outside of science have been mostly the domain of theorists and critics (not authors or consumers), the discourse about scientific authorship has been developed by its very users and producers. The scientists' discourse about authorial names is in and of itself a document of differences between disciplinary economies.

The indigenous discourse about authorship in science was set in motion by practical, rather than theoretical questions. Most of the time, scientists use "authorship" in a very specific, documentary sense: the physical presence of one's name in the author's byline. But as the range of questions spiraled quickly in all directions, the discourse about whose name should or should not appear on a byline has become necessarily more comprehensive and theoretical. Scientists and their administrators started out by confronting issues such as these: How are institutional evaluators and funding agencies supposed to assess the credit to be attached to each name listed in the byline? How is that credit to be weighed according to these names' placement and modalities of order? How does the name of the journal in which an article is published affect the credit to be bestowed on the name of the author? How can readers be sure that those long strings of names are neither too inclusive nor too exclusive? Are senior practitioners given authorship without having done the work, and are junior researchers unjustly denied it? And, if a paper is deemed fraudulent, on whose name should that responsibility fall?

These questions extended seamlessly to other aspects of the system of scientific authorship.¹¹ For instance, given the specific economy of scientific publications (an economy where the role of the market is deemed to be replaced by that of peer evaluation) should referees and editors share in the author function? Should the referees' names be withheld or published? And what about the names of the so-called ghost writers, professionals who, while not scientists themselves, may take over the writing, that is, the very emblem of traditional authorship? Furthermore, does peer review certify the truth of an article? If not, what can peer review do, and how does it really work?¹² And how is the status of a work modified by the fact that it may have been funded by the private sector? How is the function of the author's name modified by appearing together with another name, that of a pharmaceutical company that sponsored the research?¹³

As the scientists and their administrators try to deal with these issues in a pragmatic, managerial way, it has become increasingly evident that we are no longer just talking about the name of the author (or of several authors), but about many different names (of journals, institutions, editors,

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We describe a measurement of the charge asymmetry of leptons from W -boson decays in the rapidity range $0 < |y_l| < 2.5$ using $W \rightarrow e\nu, \mu\nu$ events from $110 \pm 7 \text{ pb}^{-1}$ of data collected by the CDF detector during 1992–1995. The asymmetry data constrain the ratio of d and u quark momentum distributions in the proton over the x range of 0.006 to 0.34 at $Q^2 \approx M_W^2$. The asymmetry predictions that use parton distribution functions obtained from previously published CDF data in the central rapidity region ($0.0 < |y_l| < 1.1$) do not agree with the new data in the large rapidity region ($|y_l| > 1.1$). [S0031-9007(98)08026-0]

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referees, "ghost writers," private-sector sponsors, and public funding agencies) and their complex relations. The multitude of these relations indicates that a scientific publication is not so much a "work," that is, a well-demarcated object produced by one author. Rather it is something whose boundaries are harder to define, something that, while attached to the name of an author, was constituted through the work and resources of many other actors (who, nevertheless, may not be called authors). In sum, the authorial names on an article's byline begin to appear more as the "packaging" of this product, and less as its sole, necessary "causes."

Observers familiar with literary and legal analyses of the author function would notice that the scientists seem to have gone a long way toward deconstructing scientific authorship while still using it almost as a term under erasure. In literature, the mapping out of the many actors behind a text (the papermaker, the font-cutter, the typesetter, the printer, the editor, the publisher, and other works that may have inspired the author) is a move historically associated with critiques of intellectual property rights (Woodmansee 1994). The author is often presented as little more than a convenient legal device developed to rationalize yet another version of the fencing of the commons, this time into privately held intellectual property rights. The mapping of the collective nature of cultural production is seen to expose the problematic logic of intellectual property, and to support arguments for the curtailing of the legal and financial privileges attached to the name of the author as the sole holder of intellectual property rights. In science, instead, we find that the names of the other actors behind the publication of an article are all acknowledged as part of a constructive process (though the modalities of that acknowledgment might be quite complex). In sum, the discourses on the author function in literature and in science are not substantially different in terms of content, but while the former appears to belong to the genre of critique, the latter seems much closer to that of management.

Names and Their Geography

These differences can be traced to the different professional ethos of those who produce these discourses, but there are underlying logical distinctions as well. The peculiarity of scientific authorship is that, provided the many actors of the economy of scientific production and publication are attributed an appropriate place, their presence does not weaken, but rather strengthens the role of the author. Instead, in literature (or other fields

operating within intellectual property law) the name of the author functions by casting a caesura between the “work” and the “rest”—the public domain whose role must be minimized as much as possible so as to enhance the creative contribution of the author.¹⁴ This caesura is crucial (if problematic) because it is through this that an author’s work can be constituted as property and thus generate rewards through its sale or licensing. On the contrary, in science the “rest” is the author’s ally, not a nemesis. By not acknowledging other scientists’ work in footnotes, or by denying the role of editors and peer reviewers, authors would damage (not increase) the value of their work.¹⁵ This is because a scientific publication does not bring rewards by being sold as property, but by being accepted as true. And the “truth effect” is best achieved by listing all actors and resources as distinctly as possible so as to transform them into many “testimonials.” In sum, academic science does not construe the function of the author’s name through a dichotomy between the holder of intellectual property rights and the public domain, but breaks up and rearranges such a dichotomy into a series of demarcations between different names. Many of them are people’s proper names; others are names of instruments, reagents, institutions, and journals. But all of them are acknowledged while being put in different places and assigned different functions. The scientific author cannot exist as a point of production but only as a distinct link in a network that is extended chronologically and spatially.¹⁶ Its function is maximized by making that link as distinct as possible, while making the chain as long as possible. In this sense, the scientific author is always a coauthor, even when a single name appears in an article’s byline.

The extended figuration of scientific agency is clearly inscribed in its publications. If we look at a scientific article as a whole (not only at the “main” body of the text) we see that several of the extended actors’ names (sponsors, home institutions, acknowledgees, and the authors of other publications the work draws from) do appear on the same paper, but in different locations, usually printed in different fonts (the acknowledgments section, the footnotes, conflict-of-interest statements, the authors’ addresses, and so on). Other actors (editors, reviewers, administrators, policymakers, for example) may not be mentioned in the scientific article itself, but are not erased either. They are simply discussed in other kinds of texts that may still be published in that same scientific journal (though often in other issues) and cast in other genres (the first page, the editorial, the letter to the editor, policy discussions, invited commentaries, or, say, a special issue on peer review). These background actors—the “circumfer-

ence of authorship,” as an editor has aptly put it—are not usually linked explicitly to any of the specific articles published in the journal. But any member of the scientific community who reads these journals on a routine basis (the way a nonscientist would read a daily paper) understands that his or her work counts as a “work” precisely by being constituted within this complex economy of names.¹⁷

Novels do not carry all this information between their covers, and for good reasons. Their value as commodities (or perhaps even their author’s intellectual property rights) would only be damaged by a documentation of the borrowings and collective contributions that went into their production. Usually novels do not even have an acknowledgments page. If they do, it is not about their borrowings from the public domain, but rather about close kin who have more or less suffered the neglect imposed by the allegedly necessary creative isolation of the author—an isolation that is reiterated even when framed within a thankful gesture.¹⁸

The different function of the name of the author in science and literature is also inscribed in the material appearance of the work. Literary works tend to have covers, but scientific articles do not. A novel is materially contained by two covers (usually decorated to stress the message that “this is a cover”), with the author’s name conspicuously printed on one of them. There is a material and functional symbiosis between the name and the covers in that, together, they circumscribe the novel as a singular object and isolate it (materially as well as symbolically) from other literature and, more broadly, from all that stuff called the public domain. Scientific articles, instead, appear in a much less demarcating material packaging. The author’s name still separates one article from another, but its demarcating function is much less stark (either because it may appear with a hundred others, or because the reader’s eye might focus more on the descriptive title than on the author’s name). Furthermore, an article’s pagination (uninterrupted by covers) casts it not as an isolated object, but as one product of a discipline (whose news, politics, and debates are clearly laid out in other sections of the journal).

As a genre, the scientific journal (especially the major ones like *Science* or *Nature* that have a weekly schedule) is perhaps closer to a newspaper than to a novel. A newspaper article is separated from other articles reporting other events (though several of them may appear on a same page), and yet they are all framed as the “news of the day” or, as the *New York Times* says, “All the news that’s fit to print.” The function of the novels’ covers is taken up by the daily demarcation of one newspaper issue from the next.

The calendar, not the authorial name, circumscribes the text. These different “cover functions” flag what is in between the two covers in dramatically different ways: in one case we are told we are reading a product of individual creation; in the other we have reports of facts and events. In a sense, the author authors a novel, but the day authors the news.¹⁹ The scientific journal is a hybrid of these two genres. Like the newspaper, the scientific journal casts itself as a weekly slice of science, yet it does not present its articles as reports of events that have just “happened.” They did happen, but they happened because they were produced, and produced through a lot of labor and resources. The material appearance of the article inscribes this hybridity. Like a news article (and unlike a novel) a scientific article is part of a larger printed product, but unlike a news report it comes with an apparatus (the long authors’ byline, the footnotes, the description of the apparatus and methodology, the acknowledgments, and so on) indicating that it is not just a report of the news of the day, but a product of science.

As a corollary to the discussion of the scientists’ own discourse about the function of the authorial name, the notion of scientific authorship as a figuration helps explain why scientists seem able to criticize a crucial category of their economy while holding on to it, if only temporarily. No one talks about the “death of the author” in science, and no one argues that authorship is not their problem and that only “critics” should bother with it. The casting of authorship as a wide (if high-maintenance) figuration seems to allow the scientists to treat it the way they would treat a problematic but wide-ranging scientific theory—a theory that leaks in many spots and yet holds the picture together and therefore should not be dropped unless something better comes along. Paradoxically, the spreading of the instabilities of names in science seems to stabilize their role while making its problems all the more visible. The problems of scientific authorship are seen as anomalies (that is, challenges), not death knells.

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From Names to Addresses

The image of the coin appears in some of the scientists’ analyses of authorship. We are told that, like a coin, authorship has two inseparable sides: credit and responsibility (Rennie and Flanagan 1994, 469–71). Such an emphasis on the inseparability of credit and responsibility hinged on the author’s name may come as a surprise, as the issue of responsibility is marginal in authorship studies outside of science—studies that focus almost

exclusively on the name as a function of authority, or on the rights of the author.²⁰ One could say that the emphasis on the nexus between authorship and responsibility in science is a direct result of the recent visibility of scientific fraud, but the logic and modalities of that nexus predate them. To understand that logic we need to take a roundabout path and look in some detail at how authors' names work as both designators and descriptors.

In "What is an Author?" Foucault argues that the name of the author works like a proper name, but not completely so. Following Searle, he points out that proper names perform more than an indicative function (like, say, a finger pointed at someone). Proper names are not just indicative but also descriptive, as we always attach some additional information to a proper name in order to link it to a specific person (like "Annelise Riles is the person who has edited this volume"). Often the name of the author works like a proper name (as when we say that Aristotle is the author of the *Posterior Analytics*), but it can also function as a label that does not refer to any specific person and yet constitutes a certain body of texts as a unified whole (as in the case of Hermes Trismegistus, the "author" of the Hermetic corpus). Far from being arbitrary, the distribution of these different functions of the author's name reflects the different ways in which regimes of fields and disciplines operate:

To learn that Pierre Dupont does not have blue eyes, does not live in Paris, and is not a doctor does not invalidate the fact that the name, Pierre Dupont, continues to refer to the same person; there has been no modification of the designation that links the name to the person. . . . The disclosure that Shakespeare was not born in the house that tourists now visit would not modify the functioning of the author's name, but, if it were proved that he had not written the sonnets that we attribute to him, this would constitute a significant change and affect the manner in which the authors' name functions. Moreover, if we establish that Shakespeare wrote Bacon's *Organon* . . . we would have introduced a third type of alteration which completely modifies the function of the author's name. [Moreover], it is altogether different to maintain that Pierre Dupont does not exist and that Homer or Hermes Trismegistus have never existed. While the first negation merely implies that there is no one by the name of Pierre Dupont, the second indicates that several individuals have been referred to by one name or that the real author possessed none of the traits traditionally associated with Homer or Hermes. Neither is it the same thing to say that Jacques Durand, not

Pierre Dupont, is the real name of X and that Stendhal's name was Henri Beyle. (Foucault 1977, 122)

He continues by linking the role of the author's name to the construction of what we called a "work":

[The author's name] is functional in that it serves as a means of classification. A name can group together a number of texts and thus differentiate them from others. A name also establishes different forms of relationships among texts. Neither Hermes nor Hippocrates existed in the sense that we can say Balzac existed, but the fact that a number of texts were attached to a single name implies that relationships of homogeneity, filiation, reciprocal explanation, authentication, or of common utilization were established among them. . . . We can conclude that, unlike a proper name, which moves from the interior of a discourse to the real person outside who produced it, the name of the author remains at the contours of texts—separating one from the other. . . . The name of an author is a variable that accompanies only certain texts to the exclusion of others: a private letter may have a signatory, but it does not have an author; a contract can have an underwriter, but not an author; and, similarly, an anonymous poster attached to a wall may have a writer, but he cannot be an author. (124)

Foucault then focuses on the function of the author's name in modern scientific texts, and on how that function differs from that of the author in both modern literature and ancient scientific texts. He places the emergence of the modern regime of scientific authorship somewhere in the seventeenth century, around the time of the development of so-called scientific method. In his view, scientific method took over some of the certifying features of the author and reframed the role of his name:

Texts that we now call "scientific" . . . were only considered truthful during the Middle Ages if the name of the author was indicated. Statements on the order of "Hippocrates said . . ." or "Pliny tells us that . . ." were not merely formulas for an argument based on authority; they marked a proven discourse. In the seventeenth and eighteenth centuries, a totally new conception was developed when scientific texts were accepted on their own merits and positioned within an anonymous and coherent conceptual system of established truths and methods of verification. Authentication no longer required reference to the author who had produced them; the role of the author disappeared as an

index of truthfulness and, where it remained as an inventor's name, it was merely to denote a specific theorem or proposition, a strange effect, a property, a body, a group of elements, or pathological syndrome. At the same time, however, "literary" discourse was acceptable only if it carried an author's name; every text of poetry or fiction was obliged to state its author and the date, place, and circumstance of its writing. The value and meaning attributed to the text depended on this information.

(126)

More recent historical scholarship and the current discussions around definitions of scientific authorship have questioned several of Foucault's empirical assertions, though they have not undermined his claim that scientists' names do not function like those of Shakespeare, Pliny, or Hermes Trismegistus.²¹ That claim too, however, needs some substantial reframing. In love with his notion of episteme, Foucault was carried away by his belief that the scientific method (the apparent embodiment of science's rules of discursive formation) had supplanted the name of the author as the entity demarcating scientific texts from other kinds of works. Instead, the name of the scientist has always remained crucial. While it no longer needs to be an especially authoritative name like Pliny or Hippocrates (as Foucault had pointed out), it still has to be the name of a specific person, a person with a valid address. The author's name may have become "banal" in modern science, but its role is more crucial than ever.

Foucault focused on the epistemological and authoritative dimensions of scientific texts but missed the fact that responsibility is a constitutive element of the scientific author-function. Curiously, responsibility played a major role in other aspects of his discussion of authorship, as when he claimed, correctly, that before the author had emerged as a category within the intellectual property system, it was construed by the state as the person legally responsible for the content of a text.²² Books could not be published without the name of the author, of the printer, and the printer's address because the police needed to know on what door to knock if that book was deemed subversive.

Similarly, in today's science, the name is the point around which credit accrues, but it is also the name of the person who is responsible for the content of that publication. Unlike modern literary authors (but like renaissance translators of the Bible) the scientific author needs to have a real name (not a pseudonym), and a real address to be included in the article itself (something we do not see in literary publications). Like Foucault's "Pierre

Dupont,” scientific authors can live wherever they please, have blue or brown eyes, be famous or not, belong to whatever nationality, race, religion they happen to belong to. At the same time, the scientist’s name has a strong (if remarkably narrow) descriptive function in that it must refer to a *physically traceable body*. To perform its function, the scientific author does not need to be an Author (that is, an auratic entity) but does need to be a point on a geographical map. Similarly, the voice of a scientific text may be most impersonal, but it works only by being directly traceable to a person’s name (as opposed to a literary text where the voice of the narrator may sound most personal but is usually uttered by a fictitious character that may bear little or no resemblance to the author of the work). In sum, the name of the scientific author does not need to refer to a special person, but to a specific one. His or her name needs to be as specific as the claims the author produces.

To put it differently, the scientific author is not the creator of a certain text, but functions more like one of the “initial conditions” of a study or an experiment—conditions that may (or may not) be crucial to a given result but need to be specified nevertheless. As a thought experiment, try to think of the author’s name and address as the settings of a detector, as the dilution of a certain reagent, as the brand and model of an instrument. This analogy is certainly problematic as one could say that the scientist’s identity should have nothing to do with the epistemological status of a scientific claim. And yet, the analogy between the scientist’s name and experimental conditions is useful in pointing out that the name must be part of a series of very mundane specificities that, in and of themselves, do not constitute a certain claim as true and yet cast it as a candidate for a true or false statement within the discursive regime of science.

It is precisely because of this string of specificities that a scientific claim can be made “universal” and apparently author-independent (in the same way a “fact” comes to be seen as independent from the instrument that “detected” it only after that instrument is deemed to have been “black-boxed,” that is, after it has become epistemologically “banal”). Modern science would see an author’s name like “Hermes Trismegistus” as illegitimate because, while high-sounding, it lacks specificity.

The Peculiar Economy of Scientists’ Names

The unique relationship between scientists’ names and the claims attached to them may be better understood by looking at the specific logic of the

scientific economy of authorship and how it differs from that of intellectual property law and, more generally, from liberal economy.

In liberal economy, the objects of intellectual property are artifacts, not nature (see Biagioli 1999). One becomes an author by creating something new, something that is not to be found in the public domain. Copyright is about "original expression," not content or truth. If you paint a landscape, you may claim intellectual property (a form of private property) on the painting (the expression), but not on the landscape itself (the content). Scientists, therefore, cannot copyright the content of their claims. Facts (like the landscape that provides the subject of a painting) cannot be copyrighted because they are not the result of the author's original expression, but belong to the public domain. Also, saying that scientists are authors because their papers reflect personal creativity and original expression (the kind of claim one has to make to obtain copyright) would actually disqualify them as scientists because it would place their work in the domain of artifacts and fictions, not truth. Therefore, while researchers (or journals) can copyright scientific publications (i.e., the "form" they have used to express the "facts") and gain some protection against piracy, their rights in these texts do not and cannot translate into scientific credit. In sum, copyright can make scientists authors, but not scientific authors.

Like copyright, patents too reward novelty as 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" by virtue of not having practical applications yet, or because they are about something that belongs to the public domain) (Phillips and Firth 1995, 39–42). While it is increasingly common for scientists (mostly geneticists) to patent what would appear to be natural objects, they make these objects patentable by extracting them from their original state of nature and by packaging them within processes (often diagnostic tests) that are deemed useful or potentially useful.²³ Scientists can patent useful processes stemming from their research, but academic scientific authorship is defined in terms of the truth of scientific claims, not of their possible usefulness in the market. In sum, according to the categories and tools of intellectual property, a scientist qua academic scientist is, literally, a nonauthor.

Moreover, intellectual property is deemed to result from taking as little as possible from the public domain and transforming it into some kind of "original expression." But a scientist is not represented as someone who

transforms reality or produces “original expressions” out of thin air, but 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, the scientist 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 scientists). 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 claims.

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. As a result, it cannot be legally tracked or quantified monetarily. 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. It also becomes the only possible tool for marking scientific credit.

The pinning of the epistemological responsibility for a claim on the author’s name follows from a similar logic. 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 usually construed as financial liability. Also, the legally responsible author may not be the actual producer of those claims, but the individual or corporation that paid the producers for their 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, therefore, to monetary liability. As a result, the responsibility for scientific claims is made to fall on the scientist who produced them simply because his or her name is the only hook on which the movement from unspecified to specified truth can be pinned.

While intellectual property works through three related but distinct devices (the object of intellectual property rights, the name of the holder of such rights, and their monetary value), science has only the name to work

with. The author's name marks the object, designates the person who has produced (and is responsible for) that object, and embodies the credit for its production. This last statement may sound paradoxical, and it probably is. What I am trying to convey through the awkward notion of embodiment is that the name of the scientist becomes something other than the point around which scientific credit accrues. Such a picture would assume there was one thing called "credit" and another called "owner of that credit." But because in science there is neither an owner, nor a property, nor a unit of measurement for such a property, everything (whatever that "everything" is) gets folded into the name (and onto the body attached to that name).

In science, credit is attached exclusively to the author's name, construed as symbolic and nonmonetary, and assigned through peer recognition (reputation, prizes, tenure, membership in societies, and so on) (Bourdieu 1975). Some have argued that science works like a peculiar gift economy (Hagstrom 1982). Furthermore, credit accrues on a scientist's name during his or her career, but each "unit" of credit results from the scientist's ability to produce new claims, that is, to be recognized as the first to have made that specific claim. There is no Coca-Cola and Pepsi-Cola in science. The first "cola" takes all. Credit in science does not come from market shares, but from first discovery (Merton 1973, 294-95, 323).

The use of eponymy in science reflects such a name economy. Discoveries, laws, and theories are sometimes attached to a scientist's name (for example, Boyle's law, Golgi's apparatus, Fermat's theorem, Feynman's diagrams). But these associations indicate neither actual property (as in "this house belongs to Robert Boyle"), nor certify the "authenticity" of a product (like trademarks attached to sneakers or designer clothing). Rather, eponymy works as a form of symbolic capital (because monetary capital or material property cannot translate into scientific authorship). Eponymy usually comes into play only after the scientist's death, strengthening its role as a "monument" rather an acknowledgment of property.²⁴

But while one can map in some detail the logic of scientific authorship, the quantification of scientific credit remains elusive.²⁵ This has to do with two related issues. The first is that scientific credit is defined not as the outcome of an algorithmic procedure but of peer judgment.²⁶ A claim's empirical basis may be tied to standardized techniques of observation and data analysis, but the relevance of such a claim and the credit its producer should receive remains a matter of expert judgment. The second issue is that this community of peers (while distributing various kinds of rewards

and awards) does not operate in an economy regulated by units of measurement of value (such as money in a capitalistic economy). For lack of a better analogy, I would say that science's predicament resembles art connoisseurship without an art market. While in art the critics or the connoisseurs express qualitative judgments that are then variously translated into the artwork's monetary price, in science the expert judgment and the currency through which value is expressed are both qualitative. This makes sense if we realize that, unlike objects traded in a public market (a context in which producers are by and large distinct from evaluators and buyers), academic science is done, evaluated, and "consumed" by the same people.

The lack of standardized protocols and units for the quantification of scientific credit is simultaneously a curse and a blessing. It makes promotion cases and other kinds of professional evaluation difficult, and casts an aura of arbitrariness about them.²⁷ At the same time, it also helps to construe scientific credit as "pure" (not something like a currency among others), thus reinforcing the perception of science as dealing with truth. However, while everyone agrees that not all articles have the same value (and actually only a minority of all published scientific articles are ever cited), there is also a widespread awareness that, in practice, the number of one's publications is a reliable index of one's chances of succeeding in science's extremely competitive marketplace (Hamilton 1990, 1332; Rennie and Flanagin 1994, 469; Angell 1986; Culliton 1988; Maddox 1988). There is, in practice, a gap between the qualitative logic of scientific reward and its *de facto* everyday quantification.

These two apparently incommensurable positions are, in my view, necessarily linked. The routine complaints about the fact that promotions should be judged on the quality rather than quantity of publications indicates that scientific credit, precisely because it is defined as unquantifiable, often ends up being quantified, by default, in the most crude manner: by adding up the articles bearing the author's name. While the judgment-based logic of scientific reward and value is predicated on the assumption that the evaluators' time is a limitless resource, in practice time is anything but limitless (and most, if not all, decisions about scientific value are made in contexts structured by severe time constraints). Perhaps one could see this *aporia* as the mirror image of what happens in liberal economy, that is, in an exchange system whose logic is buttressed by the assumption that information about value and price is free and equally available to all players when instead liberal economy works (and can only work) through the selective, limited availability of that information (Boyle 1996, 40-41).

There are other interesting differences between liberal and scientific economies and the ways they shape the function of the author's name. When we buy a novel we do not expect to receive the producer's address. We simply take the risk of paying twenty dollars for what could turn out to be a disappointing read. Neither the author nor the publisher guarantee that we will be satisfied. We do not buy truth but only the possibility of intellectual pleasure, and the cash transaction between us and the bookseller is the beginning and end of the story. Alternatively, when we buy a product based on its performance, we expect it to work according to its specifications, and we may decide to hold on to the receipt in case something goes wrong. And if something does go wrong, we start our chain of grievances from the store who sold us the product and find our way up to the original producer only if we need (and have the time) to do so.

In science, instead, the name and address of the author is all a reader gets. The store owner, the wholesaler, the distributor, the importer, and all the other intermediate actors in a chain of commercial transactions are not entities that have an analog in science. While the production of a claim is indeed lengthy and involves many people and institutions, all these steps are represented as irrelevant to the epistemological value of that claim—a value that rests solely on the published author and his or her name. And such a value is of a peculiar kind anyway. A scientist who reads another scientist's work does not do so by buying it, either because journal subscriptions are usually provided by institutions, or because the scientific author does not receive royalties for the published research. (If this does not apply to textbooks, it is because they are not seen as presenting new research claims and fall, therefore, in the category of commercial books.)

Furthermore, upon reading a scientific article, a scientist may make professional decisions, such as investing time and money in a related line of research. These decisions are based on a claim the scientist has not purchased and therefore cannot "return" in case the claim turns out to be "defective." Again, the author's name is the only entry point for a grievance process (though it is not clear what shape a grievance can take beyond the production of damaging critiques of the original author's reliability and competence). Also, while in science the "producer" may lose symbolic capital as a result of such complaints, the "consumer" gets little or no reparation. In this sense the scientific consumer is in a position similar to a customer who has bought a bad novel that can't be returned (though, in science, that text would have cost the consumer nothing).

Too Many Names, Too Few Names

The scientist's name looks plain and unproblematic, and it is precisely because of its apparent naturalness that it can cover up the inherent opacity of its functions—functions it can perform only by leaving them shrouded in an aura of tacitness and unspecificity. Because the peculiar logic of scientific credit and responsibility prevents the construction of scientific authors either as holders of intellectual property rights or as workers paid for their work, it is hard, perhaps impossible, to find appropriate legal or economic categories to manage the function of the name. Scientific authorship, then, is a vast “underground economy” regulated by practices that are inherently administrative and private because they cannot be explicitly articulated in legal or economic terms. Not only do different institutions, disciplines, and journals have different authorship policies, but it is not even clear who should have the authority to legislate on these matters. Legally speaking, authorship definitions seem, at most, private contracts between scientists and journals, scientists and their institutions, or scientists and the colleagues who make up their research project. I believe that if these arrangements seem to survive (with some difficulty), it is because there is no body of legal doctrine that would challenge their arbitrariness. At the same time, such an absence is not an accident but the very result of the peculiarity of the scientific economy. In a sense, scientific authorship is a paralegal discourse predicated on the absence (perhaps the impossibility) of a Law.

Until the emergence of large-scale multiauthorship, science administrators and editors were able to treat scientific authorship as something similar to its literary cousin (or, more specifically, to the literary author before the development of intellectual property law). After all, a scientist is a 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 an envelope to contain its hard-to-define functions.

Multiauthorship has unhinged this unstable but plausible-looking conceptualization, and has produced opposite reactions among science administrators and practicing scientists. Science administrators have tried to hold on to traditional notions of individual authorship and to treat multiauthorship as an aggregate of individual authors. For instance, until recently the

ICMJE (International Committee of Medical Journal Editors), an influential body representing hundreds of anglophone biomedical journals, has required that each name listed in an article's byline (no matter how long that byline might be) must refer to a person who is fully responsible for the entire article (not just for the task he or she may have performed).²⁸

This position emerged also as a response to the finger-pointing that tends to develop among coauthors 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 (Relman 1983; Engler et al. 1987). While these claims were found ad hoc and self-serving in some instances, they did match the investigators' findings in others.

Additionally, the ICMJE has been concerned with the potential inflation of authorship credit due to multiauthorship. 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 response has been to put forward stringent definitions of authorship in an attempt to control the scale of multiauthorship, rein in inflation, and facilitate the enforcement of authorial responsibility. Rather than developing a radical redefinition of authorship in the light of conditions of production brought about by large-scale collaboration, the ICMJE has gone back to, and reinforced, 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 intellectual contributions, not other forms of labor that are deemed nonintellectual:

- 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. (ICMJE 1997, 928)

Like the so-called romantic author, 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.

The "workers," of course, have objected to this definition. Many scientists feel that they cannot be responsible for those aspects of a project that fall outside of their work and expertise, and have 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. If these contributors do not receive authorship credit, they would receive almost no credit at all. Being thanked in the acknowledgment section is not something one can put on one's vita. In sum, the "workers" of large-scale biomedicine 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.

But while one can empathize with the "workers" (as I tend to do), their position is fraught with as many tensions as that of ICMJE. Their perspective would require a means to demarcate and quantify their contributions and responsibilities that, as I have tried to show, flies in the face of the current logic of the economy of science and of authorial names. In some ways, they are trying to apply the categories of liberal economy to something that, instead, is complementary to it.

It is easy to slip into familiar social imagery and picture the tensions between scientists and the ICMJE as those between workers and their bosses (as I have just done here). But that misses the nature of the economy of scientific names, as well as the relationship between editors and scientists. Scientific authorship would then appear to be something like wages or, perhaps, stock options. But authorship is not something editors alone can "give" to scientists in exchange for their work, and it is not that editors become "poorer" by giving them authorship. Similarly, while one can conceive of "authorship inflation" in qualitative terms, it is not at all clear how one could measure it.

Scientific authorship makes for a very unusual pie because cutting it in thin slices does not necessarily reduce the value of each slice. As surprising as this might sound, it 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 copyright owner with respect to that work" (Halpern, Nard, and Port 1999, 55). Of course, an author of a joint work cannot simply sell it and take off with the bundle. Authors are legally accountable to the other joint

authors, that is, they may have to share the profits with them and may not sell or license the work in a way that would violate the rights of the other joint authors (as by giving out an exclusive license to a third party) (55). But 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 authorship pie. All it can do is to make each joint author responsible for splitting the income deriving from the uses of the pie (though even then the modalities of that split remain a matter of negotiation).

Things are much more complicated in the case of scientific multiauthorship because the value of a scientific work is not expressible in a standardized unit of measurement. 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). As a result, scientific authorship is not a zero sum game. Adding a name to the byline does not reduce the value of the other authors' contributions by any tangible amount because it is not clear what the overall value of that text (or of its parts) might be.²⁹ In the end, scientific authorship seems to work like a hologram in which each fragment "contains" the whole.³⁰ 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, coauthors may become de facto full authors because it is not clear how one could deny them that status given the chain of indeterminacies surrounding the function of the scientist's name and the value of a scientific work.

In sum, both the ICMJE and the "workers" are walking on logical quicksand as they try to legitimate their views on the role of the name—views that, nevertheless, are not unreasonable in and of themselves.

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Conclusions

I do not see this situation as a recipe for chaos or paralysis, but only for the proliferation of new arrangements of the function of the name—arrangements that, sooner or later, will clash with some aspect of the current logic of the economy of science. New interesting proposals are being floated and tried out. Some of them may lead to more radical redefinitions of credit and responsibility and their nexus (Davidoff 2000; Rennie, Yank, and Emanuel 1997; Rennie and Yank 1999; Smith 1997; Horton 1997; Horton and Smith 1996; Godlee 1996; Leash 1997). Or perhaps the dramatic

development of patenting activities by geneticists (academic and not) may not be an isolated trend, but may be marking the beginning of more sweeping changes of the economy of science (including academic science) in the direction of intellectual property law.

Whether scientific claims will continue to circulate mostly in the public domain (as public documents) or will be carved away more frequently as private property (covered by patents or copyrights), it appears that the name of the scientist is not going to lose its central role in the economy of science (though the logic, relations, and objects of that economy may change substantially in the near future). But, independently from what the future may bring, I hope to have shown that the mundane problems surrounding the role of the scientist's name point quite directly to more general, theoretical tensions (perhaps aporias) stemming from the current conceptualization of scientific authorship and the contradictory notions of human agency it is asked to juggle.

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NOTES

Debbora Battaglia, Pierre Bourdieu, Sande Cohen, Yves Gingras, Annelise Riles, Marilyn Strathern, and Rochelle Dreyfuss (and the students in her seminar at NYU School of Law) provided important comments and criticism on previous versions of this essay. I wish there was a way to credit them without making them coauthors of my mistakes. Written in 2000, this essay reflects the state of the debate at that time.

1. It would be impossible to discuss the many positions, approaches, and methodologies through which science studies has dealt with the construction of scientific authority without writing a very long review of the field. However, several key examples of this literature can be found in Biagioli 1999, while critical synopses of the main research questions in the field can be found in Jasanoff et al. 1995.

2. The trend toward the analysis of an increasingly broad range of mundane documents of the construction of science, however, has not gone unchecked. It also hit some serious snags when confronted with the issue of tacit knowledge, skill, and other forms of bodily knowledge—limits that have brought into question the epistemological status that science studies had conferred to the notion of “practice” (Turner 1994). Turner is a critic of science studies' conceptualization of the causal role of tacit knowledge and skill, but his work provides a good survey of the key literature and of the debate.

3. Biagioli and Galison (2002) attempt to initiate a discussion about the disciplinary and historical specificity of the author function in science.

4. Many examples of the first approach can be found in the journal *Scientometrics*. The second kind of analysis is much more diversified across periods and disciplines and hard to map in a footnote, but some examples are plagiarism and various accusations of appropriation that often emerge within priority disputes; the

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erasure of women's authorship (or women's use of pseudonyms) in the early modern period; the erasure or limited acknowledgment of technicians' work (from the early modern to the present); the use of collective names in early modern academies, contemporary "big science" (e.g., ATLAS Collaboration, CDF Collaboration, etc.) or in collective authorship experiments like the Bourbaki group in mathematics; the "Mathew Effect" studied by Robert Merton (i.e., the inordinate accumulation of scientific credit on the names of famous scientists, though they may have not been the authors of that work, e.g., Boyle's Law, actually discovered by Robert Hooke); the many disputes about the ordering and contents of author bylines in multiauthored publications (discussed later in this essay); the construction and crediting of mythological authors such as Hermes Trismegistus; the use of anonymity or pseudonymity as a protective shield, especially in the early modern period; and the so-called reverse plagiarism, that is, the publication of one's work under the name of another scientist.

5. Examples of these directions are Haraway 1992, Latour 1999, and Pickering 1999.

6. This topos emerged in the very early debates about copyright in eighteenth-century England (Rose 1993, 38).

7. My own bibliography on this topic (available upon request) contains about eight hundred items. A selected and updated bibliography on scientific authorship in biomedicine (and a discussion forum) can be found at www.councilscienceeditors.org/services.

8. One of the most visible and more studied instances is the so-called Baltimore case (see Kevles 1998).

9. There are exceptions. Art historians have dealt with the complicated figure of the author in early modern artists' workshops (Alpers 1988). The trend can be traced to contemporary forms of artistic collaboration (Afterimage 1999) and to the development of the artist's studio into a space in which social relations of production are not unlike those of the factory or the laboratory (Jones 1996). The other exception is anthropology, where authorship of artifacts within non-Western cultures tends to be treated as a cultural or collective (rather than individual) category.

10. I have not found one reference to Foucault, Barthes, Benjamin, or other analysts of the author function in the scientific literature about authorship.

11. Davidoff gives a very recent synopsis of the state of the debate on authorship in biomedicine (and its boundaries) (2000, 111-19).

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. Daryl E. Chubin and Edward J. Hachett (1990) is the most notable exception. Since this chapter was written in 2000 a number of publications have substantially corrected the literature gap.

13. Even conflicts of interest, in fact, can be seen as an authorship problem, that is, as a lack of transparency about the author's ties to "background authors."

14. The tensions between privately held intellectual property rights and the public domain have been discussed elsewhere (e.g., Woodmansee 1984, 425-45; Boyle 1996; Litman 1990).

15. Greg Myers (1995) provides an analysis of the diametrically opposed ways in which scientific articles and patents buttress their claims.

16. Here I draw from the so-called actor-network theory (see Callon and Latour 1981).

17. The remark by Richard Horton, editor of *Lancet*, was made at the Council of Biology Editors' retreat on authorship, Montreal, May 1999.

18. Academic publications in the social sciences and humanities occupy the middle ground between literature and the sciences. There we do find extensive acknowledgment sections thanking colleagues, librarians, archivists, research assistants, editors, informants, etc. In this case, the author function is still very close to the individual figure of the literary author, while the acknowledgment page points to the fact that, in practice, the "figuration" of the author is already expanding, though not as widely as in the sciences. One could also think of the "writing culture" debate in ethnography as being about more than the construction of ethnographic authority, but also the very author function of the ethnographer vis-à-vis that of the informant.

19. By "day," of course, I mean the social day, that is, what people (not only nature) have done in that slice of time.

20. Besides Foucault, the other scholars who have looked into the connection between authorship and responsibility are those who have studied the early modern period, when the state control of the press was a crucial factor in framing the author function. The other (partial) exception can be found in modern and contemporary debates about pornography and so-called obscene art. But even here the focus is more on the definition of "pornography" or the "obscene" rather than on the detailed articulation of the author's responsibilities.

21. An interesting reassessment of Foucault's views on the author function can be found in Roger Chartier's *The Order of Books* (1994, 25-59).

22. "It is important to notice, as well, that its [the work's] status as property is historically secondary to the penal code controlling its appropriation. Speeches and books were assigned real authors, other than mythical or important religious figures, only when the author became subject to punishment and to the extent that this discourse was considered transgressive" (Chartier 1994, 124).

23. Eliot Marshall provides a review of recent trends (1997a, 1997b). Dorothy Nelkin gives an earlier overview on these issues (1984).

24. There are exceptions. Feynman's diagrams, for instance, were given that name during his life (Kaiser 2000).

25. As Drummond Rennie, deputy editor of *JAMA*, candidly puts it: "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 the coins, or how the coins should be lined up for inspection" (Rennie, Yank, and Emanuel 1997, 580).

26. Figures from citation indexes may be used as "supplements" in the evaluation of an article's impact, but do not exhaust that process.

27. Although here I am dealing with the evaluation of scientific publications, many of these issues surface also in the assessment of "scholarship" in the humanities and social sciences.

28. "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 1997, 928).

29. "[T]he expansion in numbers of authors per article has tended to dilute accountability, while scarcely seeming to diminish credit" (Rennie, Yank, and Emanuel 1997, 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.

30. 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.

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