The Attribution of Credit and Responsibility in Science: Changing Contexts and Definitions

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University of Houston Law Center/ Institute of Higher Education Law and Governance (IHELG)

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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. In sum, 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).

Furthermore, scientific credit is not monetary but "symbolic". Probably this is not the right adjective, but it tries to capture the fact that scientific credit is about professional recognition which can be transformed into money (in the form of jobs, fellowships, and grants) but is not money-like in and of itself. Some have argued that science works like a gift economy in which a scientist give publications to his/her peers (as a gift) and receives credit from them (as a counter-gift). 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. Accordingly, 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.

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

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 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 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? In sum, 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.
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. Perhaps, the business practice that comes closest to science may be the "free software" movement. 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 his/her work (though in science one does not get monetary rewards but only citations from such a "licensing").

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, s/he 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".

Both cases involve a transformation from something unspecified 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.

- 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/her creative contribution and personal expression provides the legal axiom for construing his/her products as objects the author ought to have rights in. For instance, the 1976 Copyright Act does not define "author" but uses it as a primitive notion. Ownership issues begin with the axiom that "an 'author' is one to whom anything owes its origin." The author is the prime mover who "causes" the product, thereby constituting it as his/her 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 s/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 does, in science we see that that relationship is not one of one-way
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: scientific authorship is not like IP authorship, scientific credit is not like intellectual property rights, scientific responsibility is not like financial liability, scientific credit cannot be transferred like intellectual property rights, etc. 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 can't treat scientific authorship as IP authorship, but 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/her 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, in so far 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 subjects or citizens of specific nations, they may have attached to their names. To put it differently, scientists qua scientists are humans, but not quite legal subjects.

- 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 and treat it as something 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 containment vessel 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 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/she may have performed). This position 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. While these
critics would like to do to it. Surprising as it may sounds, cutting it in thin slices does not necessarily reduce the value of each slice, but it also leaves that value undetermined. As a result, multiauthorship 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 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 copyrights owner with respect to that work." Of course, an author of a joint work cannot simply sell it and take off with the bundle. S/he is legally accountable to the other joint authors. For instance, s/he 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). 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 to 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 undivisibility 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 multiauthorship is not a zero sum game. The main difference in these two cases is that while with a co-authored work one can draw the line between the indivisible rights in the work and the monetarily divisible income from those rights, in the case of scientific multiauthorship such a line is nowhere to be found because a scientific claim is not about property rights. 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's not clear what the overall value of that text (or of its parts) might be. In the end, scientific authorship seems to work like a hologram in which each fragment "contains" the whole. However, it is not that each name "contains" full authorship is an 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 him/her that status given the chain of indeterminacies surrounding the function of the scientist name and the value of a scientific work.

- From authorship to contributorship and guarantorship

Recently, two radical reframings of scientific authorship (coming from two very different diciplines) have been put forward and implemented, if only within limited constituencies. While 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 one comes up of debates within the biomedical community. In a recent article published in The Journal of the American Medical Association, Drummond Rennie (one of JAMA's deputy editors) and his collaborators have put forward a radical departure from 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 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
is the producer of the work, but s/he is also "produced" (i.e. recognized and
rewarded as such) by his/her peers.

But 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 insure the
integrity of the entire project. They organize, oversee, and double-check, and
must be prepared to be accountable for all parts of the completed manuscript,
before and after the publication. In this way the role of the guarantor is
precisely defined and differs from that of the "first author" or "corresponding
author" or "senior author"... □

At first, the proposal seems to put together 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 is also one or more
guarantors who are responsible for all of it. 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. □

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. □ The proposal does a careful job at articulating the role of the
contributor, but 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 5 articles thereby preventing other
researchers from wasting further efforts in trying to replicate their faulty
reports. □

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

All members are entered in the Standard Author List, but only after they have done 1 FTE-year service work in the Collaboration. This simple bureaucratic requirement speaks volumes about the different conceptions of authorship held by CDF and ICMJE. What differentiate a member from an author is not their professional hierarchy. Students, technicians, and PhD physicists are all eligible for authorship (while 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/her 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 its leave policies. A member is allowed up to a year of leave of absence without losing his/her author status during that period. This means that for up to a year his/her name appears on all publications produced while s/he is not there, based on research s/he may or may not have directly contributed to. Similarly, a person who ceases to be a CDF member remains on the Standard Author List for a year after his/her departure. 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 s/he has worked for a year, and maintains author status for a year after s/he stops working. To use an image that seems ubiquitous these days, s/he earns her "stock options" in CDF, and sells them back to CDF when s/he 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, de facto "irresponsible") author would be inconceivable in biomedicine. But CDF physicists do not have a lax attitude about responsibility. Simply, as I will discuss in a moment, responsibility is 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.

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:

To 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).
responsible of professional misconduct. Fraud and misconduct does not seem to have assumed the heated moral connotations it has in biomedicine. Interestingly, the sanctions leveled against those found responsible of misconduct are exactly the same applied to those who do not live up to their labor commitments. 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.

One might think of expulsion (a form of exile from the community) as a fairly mild punishment. But because there isn't 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 responsibility talk is minimal at CDF. If you can enforce responsibility, you don't need to legislate (or obsess) endlessly about it as it seems to be the case in biomedicine.

- Conclusions

Despite the vast terminological and substantial differences between the CDF guidelines and those put forward by Rennie et. al., I believe 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-authored 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 scientific authorship in the same way some would like to achieve the globalization of intellectual property and the notion of author behind it. As I have tried to show, scientific authorship is a misnomer, a historical vestige. Scientific authorship 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.

[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.]

For a very 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", Science Editor, 23 (2000): 111-119.


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/her ability to attract substantial grants) may play a role in his/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.


I owe this point to Phil Allred. For references to the "free software" trend, see www.tuxedo.org/esr/writings/; www.opensource.org; and www.fsf.org/philosophy/philosophy.html#AboutFreeSoftware.


Burrow-Gilles Lithographic Company v. Sarony, 111 U.S. 53, 4 S. Ct. 279 (1884), as cited in Sheldon W. Halpern, Craig A. Nard, Kenneth L. Port,


ibid., p.55.

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 don't have that option (at least not within current definitions of scientific credit).

"So, the expansion in numbers of authors per article has tended to dilute accountability, while scarcely seeming to diminish credit." Drummond Rennie, Veronica Yank, Linda Emanuel, "When Authorship Fails: A Proposal to Make Contributors Accountable", JAMA 278 (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.

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.
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 4 November 1998.

- Guidelines for Authorship in the CDF Collaboration, Section 5.
- Guidelines for Authorship in the CDF Collaboration, Section 7.
- Guidelines for Authorship in the CDF Collaboration, Section 1.
- Bylaws of the CDF Collaboration, Section III "Membership".
- Guidelines for Authorship in the CDF Collaboration, Section 8.
- I am referring to the responsibility of the contributor, not the guarantor, as outlined in the proposal by Rennie et. al.
- Bylaws of the CDF Collaboration, Section II "Objective".
- Guidelines for the CDF Publication Process (ASK JOHN)
- Guidelines for Authorship in the CDF Collaboration, Section 3.
- Bylaws of the CDF Collaboration, Section III "Membership".
- Ibid.