

THE DEATH OF THE SCIENTIFIC AUTHOR

Multiple Authorship in Scientific Papers

William Vesterman

Movement between scientific and literary authorship is a long-standing tradition. Alan Sokal's parody of hermeneutic theory, published in *Social Text* in 1996, is only a recent example of switching genres. A search of the literatures reveals, for instance, that Thomas Huxley ("Darwin's Bulldog") died before completing a long Swiftian satire on opponents he had fought throughout his career by both literary and scientific means. His motives were the same as Sokal's—to ridicule the misuses of science by literary meddlers and to do so by taking the war into the enemy's own territory. Huxley's quarrel was, again like Sokal's, with those who misrepresented science for moral and political purposes. In a new biography, Adrian Desmond describes Huxley's efforts to overthrow all religious explanations of natural facts and to "pull ethics out of Nature." Huxley succeeded in discrediting "a might-have-been Nature, obeying the divine Edict, legitimating the National Church. . . . The 'new Nature'—that lawful, causal, agnostic bundle—had triumphed. It was called Science."¹

Taking ethics out of nature was the only ethical option for Huxley and his

1. Adrian Desmond, *Huxley: From Devil's Disciple to Evolution's High Priest* (Reading, MA: Addison-Wesley, 1997), 603.

allies, given their commitment to a disinterested observation of facts. Yet while science may have triumphed in the struggle to interpret nature, scientists still felt put upon by sore losers who willfully confused a lack of ethics in what was observed (nature) with a lack of ethics in the observers themselves. Desmond writes that, in “Science and Morals,” Huxley allegorically “portrayed the winning ways of innocence. The young waif Science eyed her old sisters, Theology and Philosophy”:

Cinderella . . . lights the fire, sweeps the house, and provides the dinner; and is rewarded by being told that she is a base creature, devoted to low and material interests. But in her garret she has fairy visions out of the ken of the pair of shrews who are quarreling downstairs. She sees the order which pervades the seeming disorder of the world; the great drama of evolution, with its full share of pity and terror, but also with abundant goodness and beauty . . . and she learns . . . that the foundation of morality is to have done, once and for all, with lying; to give up pretending to believe that for which there is no evidence.²

Agnosticism, the word that Huxley had newly coined, expresses the same contempt for lying—in the sense of “pretending to believe that for which there is no evidence.”

Huxley was not alone in seeing the moral possibilities of an extra-moral view of nature. Like Huxley’s Cinderella, the White Knight of the mathematician Charles Dodgson in *Through the Looking Glass* provided a contemporary literary image of noble scientific innocence. In *Some Versions of Pastoral*, William Empson says of the White Knight that

he stands for the Victorian scientist who was felt to have invented a new kind of Roman virtue; earnestly, patiently, carefully (it annoyed Samuel Butler to have these words used so continually about scientists), without sensuality, without self-seeking, without claiming any but a fragment of knowledge, he goes on labouring at his absurd but fruitful conceptions.³

The labor might be at once absurd and fruitful in the way that, say, a fascination with mold might lead to an antibiotic. Yet Butler had a right to be annoyed by pretensions to motiveless benignity, and one more example of Huxley’s authorship shows one more reason why.

Besides satire and allegory, Huxley explored many other forms of writing, including the textbook, which was not yet—in the nineteenth century—always already beyond the pale of intellectual respectability. Desmond tells this story too:

2. Desmond, *Huxley*, 552–53.

3. William Empson, *Some Versions of Pastoral* (London: Chatto and Windus, 1979), 264–65.

The Church Scientific was about to get its bible. The four years of experience with schoolmasters went into Huxley and Martin's book of "*practical dodges*" (Foster's apt description), the seminal "how-to" lab Manual, *A Course of Practical Instruction in Elementary Biology*. It was distilled by Huxley and his demonstrators from the Kensington course. They all had a hand in it, and Martin got his name on the title page in 1875 through Foster's angelic intercession. They knew it would increase his prospects of a job.⁴

The Church Scientific may have acquired its bible, but by the same stroke the buyers and sellers and the changers of money were established inside the temple. The good book did in fact so increase Newell Martin's prospects that he soon became a professor of biology at the newly formed research university, Johns Hopkins (Huxley sent a copy in 1876 to support Martin's candidacy). The annunciation of authorship in Martin's case transfigured him: no longer a relatively unknown Cambridge protégé of Michael Foster's, he was now a man able to help protégés of his own through angelic intercession with the matter-of-fact marketplace.⁵

Authorship was a very valuable commodity. But was it a *fact*? "They all had a hand in it," Desmond says, but only Huxley and Martin were listed on the title page. Unlike Cinderella, Johns Hopkins believed that for which there was no evidence, apart from Huxley's *suggestio falsi*.

It is now notorious in the humanities that the modern understanding of the literary convention known as authorship was formed in the Enlightenment, when the war between ancients and moderns also began to escalate. Huxley fought battles in that enduring war, just as Alan Sokal and Jean Bricmont continue the good fight of antifaith today in their *Impostures Intellectuelles*, a book for which Sokal's parody was a stalking horse. But in eighteenth-century Britain, *science* meant merely any systematic knowledge. What we now call *science* was then manifested in only a few of their many studies, all of which marched under the general banner of literature and for the greater glory of the republic of letters. The republic had an honorary head of state, Samuel Johnson ("The Grand Cham of Literature"), who loved to perform chemical and other experiments and who welcomed the chemist George Fordyce as a member of his Literary Club, which included so many leading minds of the British Empire in so many fields. Boswell tells us that Johnson, being asked one day about the basis of copyright,

descanted on the subject of Literary Property. "There seems, (said he,) to be in authors a stronger right of property than that by occupancy; a metaphysical right, a right, as it were, of creation which should from its nature be perpetual."⁶

4. Desmond, *Huxley*, 455.

5. Desmond, *Huxley*, 711 n. 75.

6. James Boswell, *Boswell's Life of Johnson*, ed. Lawrence Powell, rev. George Hill (Oxford: Clarendon, 1971), 2:259.

The idea of the author as creator may seem at odds with the scientist as disinterested observer and discoverer of facts, but Darwin's scramble to establish his priority in the discovery of natural selection is a good example of how important the paradox of scientific authorship has been. All of Darwin's closest friends knew that he had been working for twenty years on evidence and arguments for the mechanism of evolution before Alfred Wallace came up with the same general idea. "His terms now stand as my chapter heads," Darwin wailed about his unfinished book when he read the essay that Wallace had sent him.⁷ Told the facts, Wallace graciously agreed that separate pieces by each author should be published at the same time. Today, well-informed biologists refer without irony to the Darwin-Wallace Theory of Natural Selection—so holy in the Church Scientific is the sacrament of priority through publication. The disinterested observer of natural properties must obtain an interest in a literary property through the metaphysical right of authorship.

1

That Wallace should seem to get equal credit for natural selection may seem strange to an outsider, but it is easier to understand than some, more modern difficulties of multiple authorship in the sciences. In a 1997 essay titled "What Is an Author?" a professor of neuropathology, William Rosenbaum, tries to pick through the tangled issues that make his title's question so hard to answer in the context of institutional scientific research.⁸ The issues surrounding scientific authorship are so tangled because (though Rosenbaum does not put the point in so literary a way) contemporary Funding combines the roles of wicked stepmother and fairy godmother. On the one hand, Funding sternly demands productivity, self-abnegating work in the scullery of hard facts; and she will starve science if Cinderella does not work—though, paradoxically, work is among Cinderella's dearest dreams. On the other hand, productivity may be blessed with handsome coach accommodations to meetings with the aristocracy of science, plus lots of mice to toy with at home. But for her dreams to come true, Cinderella must believe in fairy tales—among them, that productivity is measured by authorship, especially "principal" authorship, and that authorship means what everyone involved knows very well it does not mean. Scientists today get frequent practice believing in the impossible, just as the White Queen in *Through the Looking Glass* recommends.

Sorting through the facts, Rosenbaum says that the many questions they

7. Charles Darwin to Charles Lyell, June 18, 1858, *The Correspondence of Charles Darwin, 1858–1859*, ed. Charles Darwin, Frederick Burkhardt, and Sydney Smith, vol. 7 (New York: Cambridge University Press, 1991), 107.

8. William Rosenbaum, "What Is an Author? The Responsibilities of Authorship," *Academe* 83.6 (November–December 1997): 34–37.

raise may be “recast as a single query: what are the responsibilities of authorship?” (34). Halfway through his discussion, he answers this single query, but not quite so simply as he posed it: “When a scholarly paper has multiple authors, the responsibility for its contents should rest fully with each author” (35). Should? In the context of Rosenbaum’s evidence, this “should” sounds not firm, but extremely wistful. His evidence shows that responsibility in the case of multiple authorship is as unclear as it was in Huxley’s day and that the concept of authorship continues to cloud responsibility rather than to clarify it. The cause of this effect is based on well-known facts, among them careerist “practical dodges” in the tradition of Huxley’s textbook.

In the beginning, there is the first author, according to the creation myth of scientific authorship. Also called the “principal” or “senior” author, he or she appears first on the list (or sometimes last out of noblesse oblige) and is presumed to get, as Rosenbaum says, “the lion’s share of the credit” (34). Rosenbaum tells unflinchingly a story of naturalists red in tooth and claw, but he does not acknowledge that at least two inquiries define the prey: “What is credit?” and “Credit for what?” The first question is the easier. For the individual, “credit” is an invidious measure of “production” and thereby both a store of value and a medium of exchange for “funding.” What the credit is granted *for* in any given case is a greater mystery, in spite of Rosenbaum’s many examples, because the literary convention of authorship is incapable of expressing the facts of “production.” The vestigial convention has a built-in narrative insufficiency.

Rosenbaum holds that appearing as first author is justified for contributing in “a significant way” to (a) designing the study, (b) gathering the data, or (c) interpreting the data. He adds:

Often, interpreting the data is coupled with writing the paper, but sometimes the senior author may do nothing but write. Even then, placing that name first may be justified, because writing a paper can be hard work and because the way in which data are presented often determines their acceptance or lack thereof. (34)

“Nothing but write” makes the last of four separable activities that, alone or in some combination, might define the contribution of the first author in a paper with multiple authors. And while being the first author *may* be so justified, the problem remains as to whether the first author *is* so justified. Though it is easy to imagine that the coauthors agree on any one of the four significant factors as the most significant in a given case, it is harder to imagine the bases for the agreement; and it is absolutely impossible for a simple sequence of authorial names to tell us anything about any agreement or its grounds. The sequence of authors turns the stylistic fact of position into a conventional expression of importance, but the nature of the importance is unexpressed thereby.

I have begun with problems of style and content that resist even good faith, but the narrative insufficiency of the convention of multiple authorship invites and even begs for bad faith. Here is an example: “Often a principal or ‘senior’ author is identified because he or she has a higher academic rank than the other authors” (34). In this case, the literary property secured by Dr. Johnson’s “meta-physical right of creation” has been evicted by the right of “occupancy.” But it is occupancy in *another* sequence, that of academic rank. Again, nothing of this new equation of rank and credit is expressed by the sequence of authors’ names. Moreover, in the world of scientific authorship, money talks—or at least, money seems to write: A principal author, Rosenbaum adds, may be the one who “provided financial support for the project and the salaries of the other authors” (34). That is to say, authorial credit is sometimes given for the funding that authorship is supposed to purchase. And what if money were *all* that was provided? “In the United States such a colleague would almost always be listed among the authors even if no intellectual contribution was made to the publication.” In fact,

productivity is judged not only by the quality of one’s papers but also by their quantity. It is therefore important for a paper reporting on a project to list anyone whose funds supported the project. Including the names of such persons in the list of authors enables them to get grant funds that will support their own work as well as that of the unfunded students or junior colleagues who produced the study. A footnote crediting the funding investigators for their material support would not accomplish the same thing. (34)

Why not? Why not render unto Caesar what is Caesar’s and unto science what pertains to science? Why should scientific publications credit the claims to authorship of “funding investigators” and (this is a new one) “guest authors”? The multiplying demand for credit is producing inflation in the medium of exchange—authorship—which is thereby diluted as a measure and store of value.

The demoralization of science in the name of an inherited literary convention continues in the tradition of Huxley and his fortunate protégé: “Sometimes scholars have a junior partner whose career they want to advance,” Rosenbaum reports, then adds: “Getting a faculty job often hinges on whether students have published as principal authors” (34). Again, what *principal author* means in practice is opposed to what it is supposed to mean, but in the context of modern laboratory science even the *reverse* of giving false credit to a junior colleague must be considered:

When a finding is unique or controversial, it may be to the advantage of a junior investigator for an uninvolved senior investigator to place his or her name on the paper reporting the finding. The reader gets the

impression that the more experienced and respected senior author is vouching for the validity of the data and the interpretations on the paper. But this assumption may not be correct. (34)

Which assumption? That the uninvolved senior “author” is vouching? That the order of precedence is naively read? That it is skeptically read? Rosenbaum explains that, when “the fame of the senior workers is great, they may get the credit no matter where their names appear in a list of authors unless a clear disclaimer is made. Such misunderstandings are likely when the senior worker travels around the world presenting the work of the entire team” (35). Misunderstandings of what? is a question that appears to be unanswerable, since the fictive is consubstantial with the facts of scientific authorship.

The muse of fiction bestows other powers. As in novels, an invented author can invent still other authors in a deepening regression from the world of fact. Since “students sometimes spend a lot of time performing routine technical tasks,” Rosenbaum writes, they may be listed as authors “to encourage other graduate students to take on technical tasks in laboratories.” To those outside, this way of determining authorship may seem unjustifiable, even a little mercenary; but for insiders, the “system can be deemed fair to students because some day, as senior professors with their own laboratories, the students will be able to perpetuate the system and ask their own students to perform grunt work” (35).

The paradox now appears in all its paradoxicality. Only the metaphysics that science defined itself by escaping can hope to define the concept of scientific authorship as it is understood today. The soul of scientific publication needs saving, though its sense of election refuses to acknowledge the need for repentance. Just as Sokal and Bricmont in *Impostures Intellectuelles* rightly complain about literary critics’ misuse of precise scientific terms (Gödel’s Theorem, for example), scientists should be called to account for the imprecision in their own uses of the word *author*. Here, in summary, is a list of the identities that, according to Rosenbaum, might define the coauthor of a scientific article:

- a funder,
- a lab grunt,
- a junior colleague or graduate student in search of a job,
- a senior colleague who made no scientific contribution but has a higher rank than the others listed,
- the director of what Rosenbaum terms a “research factory,”
- someone who collected data,
- a famous scientist vouching or wishing to appear to vouch for unique or controversial data,

- someone who actually made an intellectual contribution to the paper by designing the experiment,
- someone who actually made an intellectual contribution to the paper by interpreting data,
- whoever wrote the paper.

Rosenbaum's is not an exhaustive list. Statisticians, donors of rat blood samples or lab space, makers of graphs, along with many other functionaries, from pilots on down—all these have in fact been credited with the coauthorship of scientific papers. To mandate, as Rosenbaum tries to do, that the responsibility for a paper's contents should rest fully with each such author addresses only that part of the problem having to do with responsibility for fraudulent data. There is also—unless we are nonmetaphorically in the realm of fiction—a problem of fraudulent credit.

2

Perhaps authorship is an inadequate and atavistic convention for modern research. Why, then, do “authors” continue to create scientific “literature”? There would seem to be an almost Darwinian kind of adaptive advantage to multiple authorship, since it maximizes credit and minimizes responsibility. Discovery of fraudulent data is a rare, time-consuming, often unrewarded, sometimes penalized process, and responsibility for fraud is diluted by multiple authorship. On the other hand, credit is immediately and uninvitably granted to coauthors after the principal author has taken the lion's share (assuming that the king of beasts can be identified). To have one's name appear on as many papers as possible, and do as little for each as possible, may maximize one's participation in the gene pool of production. However, the very success of this scheme has produced its own problems. Those responsible for evaluating productivity for purposes of funding and academic promotion find their tasks increasingly difficult, especially since many candidates now appear *only* as coauthors. Creators of reference indices are similarly burdened—some have reduced to a mere twenty-four the number of authors they will list for a single paper!⁹

Out of the dozens of millions of scientific papers published since the launch of Sputnik, when the really big research money started to flow and multiple authorship became the predominant mode of publication, I have been able to find references in the literature to only a dozen proposals to change the system. At

9. Drummond Rennie, Veronica Yank, and Linda Emanuel, “When Authorship Fails: A Proposal to Make Contributors Accountable,” *Journal of the American Medical Association* 278.7 (August 20, 1997): 583.

the time of writing, the most recent recommendations have been made in Drummond Rennie, Veronica Yank, and Linda Emanuel, “When Authorship Fails,” in the *Journal of the American Medical Association* 278.7 (August 20, 1997). Rennie, Yank, and Emanuel claim nothing for their ideas in terms of priority or originality, and Rennie has pointed out elsewhere that at least nine groups have made suggestions similar to those in the *JAMA* article since 1969. The reform proposed comes down to dropping the word *author* altogether in favor of *contributor*, with each contributor’s specific contributions explained parenthetically. In addition, at least one of the contributors would also be named *guarantor* for the data’s validity. The proposal has an added democratic benefit: it would eliminate the distinction between those listed as authors (“scientists”) and those more often acknowledged in footnotes (“technicians”).

Like Rosenbaum, the contributors to the *JAMA* article give amusing examples of the difference between what titles like “principal investigator” seem to express and the facts at hand. One is especially interesting: “Twenty researchers worldwide, more than half being heads of biomedical laboratories, published once every 11.3 days throughout the 1980s. It is certainly conceivable that some of their contributions were minimal” (580). We learn further that, in one survey, only seven out of thirty-nine editors of clinical journals knew (because there were written policies they were obliged to follow) what the order of authors meant in the articles they published. Still, policies differed widely from journal to journal, such that the same scientific functions would command various positions on the lists (581).

According to the reform proposed in *JAMA*, both aspects of the dual problem of credit and responsibility would be successfully addressed in a fairer and clearer system by identifying specific contributions and requiring guarantors. Yet a glance at how that system works for the *JAMA* article itself suggests that many problems would remain:

Dr. Rennie coconceived and codeveloped the idea for the paper; codeveloped and corefined the intellectual content; contributed articles and cases collected since 1978; wrote numerous drafts; discussed the ideas at scientific meetings worldwide; and contributed editorial expertise. Ms. Yank codeveloped and corefined the intellectual content; contributed to earlier drafts; produced the final draft; coordinated the project; and contributed historical expertise. Dr. Emanuel coconceived and codeveloped the idea for the article; codeveloped and corefined the intellectual content; wrote the first draft; commented on subsequent drafts and contributed expertise on ethics. All 3 contributors are guarantors for the integrity of the article as a whole. (579)

Even though this paper has fewer contributors than most—no data collectors, statistical analyzers, or funding investigators—the problem of credit does not

seem solved, and listing guarantors for a paper of this kind appears unnecessary, if not self-congratulatory. The concept of cocontributing clouds the issue almost as much as coauthoring, and the difference between “corefining” and “codeveloping” content seems both overrefined and underdeveloped. And how does either differ from writing a “draft”? In the jargon of literary criticism, the underdetermined has become the overdetermined.

We do not, here, have the makings of a paradigm shift. As in contemporary subatomic physics, there would seem to be no limit to the multiplication of particles: self-interest tends to produce greater and greater numbers of subdivided contributions, creating a nightmare of information overload for editors, evaluators, and indexers. In a brief aside, the article in *JAMA* speculates that generalized job categories may eventually evolve as they have in the movies (another highly successful, modern collaborative enterprise) (582). The scientific equivalents of Gaffer, Key Grip, and Best Boy might simplify some of the difficulties of friability for the contributor model, but it seems to me that such functional categories would work well only for those now usually called technicians. In any case, this projected shape of things to come begs the question of quantification. Is the business of scientific publication now in its *Citizen Kane* phase and heading toward its *Titanic*? But these are details. Meanwhile, neither the metaphysical status of coauthor, nor a social contract among contributors enforced by guarantors, accurately represents the relation of scientist to science in a scientific paper. And the facts offered in a scientific paper will continue to begin with the first sentence—decidedly not with the list of authors’ names.