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COLLABORATIVE AUTHORSHIP IN THE SCIENCES

Anti-ownership and Citation Practices in Chemistry and Biology

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Some years ago at a national writing conference, researchers reported on a campus-wide study of faculty understandings of plagiarism: not only did they find that scientists rejected the use of quotation marks, but also they learned that verbatim copying from textbooks was fine with them because they believed textbooks contained only "common knowledge." Corroboration of this finding has proven elusive over the intervening years, but this indication of how diverse the understandings of plagiarism can be has led to many interesting conversations with science and non-science faculty. While no one we interviewed in biology or chemistry was accepting of students' verbatim copying of the "common knowledge" found in textbooks, we did find that plagiarism bothered them far less than did the concern they held for the integrity of data. Further, the fundamentally collaborative nature of science became a major player in our investigation into the problems that arise from collaboratively authored texts and into the foundational premise of science as a pursuit of truth, and public truth at that.

For this study, we interviewed ten faculty members in academic departments of biology and chemistry—men and women

1. We would like to thank our informants who gave generously of their time, willingly explaining scientific jargon and practices. We'd also like to thank Ximena Hernandez and Jocelyn Graf for their efforts to bridge the gaps between science and writing.

in both fields—from five institutions in two states, using the research questions common to the contributors of this text (appendix A) to investigate definitions and practices of intellectual property in these disciplines. We provided our research subjects with the questions in advance of our interviews. Eight interviews were conducted face-to-face, one was a series of e-mail exchanges, and one was conducted by phone. Our informants were from four-year, masters granting, public teaching institutions as opposed to research institutions, or commercial or other nonacademic lab settings. While all of our subjects were engaged in research and publication, they typically acknowledged, either tacitly or explicitly, that their roles as teachers were equally important as their roles as researchers.

The biologists we interviewed were from molecular genetics and physics, theoretical or quantitative ecology, ecological management, plant eco-physiology, and neuroscience physiology education. Not surprisingly, all those willing to take the time to be a part of this project already had tenure, and most were full professors. They averaged fifteen years of teaching in the university and all noted additional time in post-docs. One also had fifteen years of teaching high school before he returned for his doctorate. The chemists specialized in inorganic and organometallic chemistry, catalysis, mechanistic organic photochemistry, and bioorganic chemistry. They averaged eighteen years of teaching in the university; all were tenured, most were full professors, and three either were or had been chair of their departments.

While traditionally, biology or chemistry may be thought of as single disciplinary categories, in reality, each breaks down into subfields. This is particularly true in biology, wherein one type of biologist uses terminology and thinks of the world quite differently than do biologists in a different subfield. A botanist, for example, and a geneticist working on the genome project are both biologists and are both concerned with living organisms, but their subjects of study, their vocabulary, their scope, and their day-to-day research have little in common. Further, the complications of intellectual property as it is legally configured, play a large role in scientific production and publication. As a result, the limits of this project would not allow us to examine the many disparate subfields of biology and chemistry, and we decided to limit our literature research predominantly to the exploding areas of biomedicine, its struggles with fraud, and the resultant hotly contested concerns about varied forms of collaborative authorship.

In this chapter, we focus on three primary areas: *anti-ownership, collaborative authorship* and its attendant complexities, and the *teaching of citation practices* to students. First, the underpinnings of scientific disinterest demand an attitude of anti-ownership in order to free scientists to pursue hypotheses without vested interests or prejudice toward potential outcomes; we found, however, that the language used by interviewees frequently evoked types of ownership. Second, in exploring the problems inherent in collaborative authorship, we examined in some depth the concept of rewards and responsibilities in the sciences, the growth of fraud, and some suggested reforms in authorship guidelines. We also discovered that scientists were much more concerned about the integrity of data than about plagiarism. And finally, we discovered that in their endeavors to teach the practices of proper citation to undergraduates aspiring to the profession, the scientists we interviewed tended to use various methods of trial and error. Marcel Lafollette (1992) says, "The trust that society places in science, traditionally assumes . . . assurances of authenticity and accuracy in all that science does or recommends" (1); clearly, our subjects understood that passing on this tradition is vital to maintaining society's trust in their discipline, which means imparting to students the conventional and ethical methods by which scientists use and acknowledge their sources.

ANTI-OWNERSHIP IN ACADEMIC SCIENCE

What does ownership mean? In a capitalist culture, we immediately consider the monetary dimension of ownership—of buying, selling, and being paid for our work—but "property rights in science are whittled down to a bare minimum by the rationale of the scientific ethic. The scientist's claim of his intellectual 'property' is limited to that of recognition and esteem" (Merton 1973, 273). Because this seems ideal, we felt a need to examine closely how academic scientists try to distance themselves from the notion of ownership constituted as a private possession; we begin by focusing on anti-ownership.

Today, the public view of science is often that of a corporate enterprise as much as an intellectual pursuit. As we began this study, we imagined that scientists, at least potentially, were "owners" in many ways. We assumed that inventions, medications, formulas, and patents all were owned and returned monetary rewards. But in traditional science, in "pure" academic science, it is much more difficult to identify what scientists own. Our subjects reported that, for example, if money were the reward they sought, they wouldn't work at universities. As one chemist put it, "If I wanted those things, I'd go work for Dow." It isn't that money is uninvolved, but according to more than one informant, U.S. federal grants are managed by sponsoring universities, which garner nearly 50 percent for overhead, including facilities, health care, etc. Consequently, such grants do not lead to significant additional income for the scientists, even for principal investigators (PIs). When patents are secured (a rarity among our interviewees),[2] the university or the granting entity typically holds proprietary rights. A couple of interviewees pointed out that it was possible to work as a consultant outside their university laboratories and that they might then receive additional pay from a company. But grant work secured in their roles as professors keeps them busy and intellectually stimu-

2. No patents were held by our interviewees in their current positions, although one chemist held more than one patent from his years at research institutions. Patents are a legal area of intellectual property that we did not delve into deeply and that merit comparisons across institutions.

lated enough that, save for sabbaticals, they tend not to look for extra money.[3]

Remuneration is but one facet of ownership. "Academic capitalism,"[4] occurring at the juncture between the academy and the consumer economy, has been the topic of much interest in the sciences. In our research, biotechnology is one field straddling exactly that juncture (Swanson 2007, Carey 1982). Pure science is under attack from the encroachment of growing corporate funding of research, which is tied directly to the legal aspects of intellectual property. While not the focus of the study here, it is nonetheless useful to consider that

> intellectual property is defined in contradistinction to a conceptual space—namely, the public domain [. . . . I]ntellectual property law polices the knowledge that can be owned, the realm of artifact, while the university polices the knowledge that cannot be owned, the realm of fact and universal truth. (McSherry 2001, 6)

This explanation fits snugly with the anti-ownership that defines science. Scientists pursue the truths of nature through their hypotheses until their data demonstrate knowledge that they believe to be new and replicable, which they then publish. The furtherance of science—shared knowledge—is achieved through publication. In fact, according to Patricia Woolf, in her remarks in 1987 to the American Association for the Advancement of Science, "the notion of ownership has no meaning until ideas

3. In an e-mail with Jocelyn Graf, July 1, 2008, she pointed out that this was not necessarily true in Korea, where she is the assistant director of the Hanyang University Writing Center. She says a number of the science faculty work for their own private companies and that the administration does not discourage this.
4. See Slaughter and Leslie's (1997) *Academic Capitalism*, which situates academic scientists' and university administrations' increasing interest in corporate funding of research, especially when federal funds decrease. They report on a growing trend of research being market-driven rather than the result of following hypotheses generated through scientific curiosity. Examination of the legal disputes discussed by Nelkin (1984) and McSherry (2001) also demonstrate the paradigm shift based in both the changing economy driving science and issues of ownership that arise because of those changes.

are shared" (qtd. in Lafollette 1992, 104). One of the biologists we interviewed described it this way: "Ideas are owned, but they're disseminated. Ownership is gone once published. Sometimes you might keep a particular idea under your hat, but ultimately, science belongs in the public domain." A chemist said, "It's important to advance science more than for career gain." Intellectual property or an ownership of ideas may result from authorship, but since publication literally returns the scientific findings to the public domain,[5] any sense of personal ownership is fleeting at best. One biologist said that he wasn't sure "how much writing is owned in science." Perhaps this is, in part, because scientists typically sign away copyright to publishers "in exchange for the reputational and career benefits that will accrue from the broad circulation of their work" (Birnholtz 2006, 1760). In *Who Owns Academic Work?* Corynne McSherry (2001) uses the term "nonproperty" and describes how the necessary disinterest of scientists creates the non-ownership they espouse. McSherry points out that, in theory, academic scientists seek recognition rather than money, which makes them "immune to the influence of politicians and/or corporate executives" (17). As Mario Biagioli (2003) explains, "a scientific claim is not rewarded as the material inscription of the scientist's personal expression, but a nonsubjective statement about nature. Consequently, it cannot be the scientist's property" (84).

With ownership comes rewards, and even if the notion that the ideas or data are owned is anathema to scientists, they do seek the attached symbolic rewards that accrue to publishing. Publications of scientific endeavors are rewarded in many ways: grants, science-index citations,[6] tenure, promo-

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5. While making research "public" was how our informants phrased it, their publications are often some of the least available to the actual public. Without scientific research library access, many scientific publications can be expensive or inaccessible, even to scientists—from community colleges and liberal arts schools to periphery countries' national universities (Graff 1992). Therefore, "public" in this context, may mean "other research scientists" as much as it means all people.
6. Cronin (2005) reports that although persistent concerns arise question-

tion, prizes, journal editorships, and honorary society memberships. Being the first to publish results about the development or improvement of a technique or process leads to citations by others, which leads to more name recognition and thereby prestige. However, being first means publication, and it brings more than symbolic rewards; publishing and getting grants typically provide the quantifiable achievements needed for promotion and tenure in the university system. "Those who are most successful in advancing their careers are not necessarily those who make the most interesting and original contributions" (Schmaus 18). Symbolic rewards also lead to more tangible ones as they provide the cachet among colleagues and thus garner more grants, top students, and speaking opportunities. One biologist spoke animatedly about being able to travel as a result of his research. He said that being able to meet people around the globe whom he had e-mailed for years—or even already shared publication with but never met face-to-face—was exciting. He also found it rewarding to take his students to other countries and expose them to the world in ways he didn't achieve until much later in life.

Some of the scientists with whom we spoke said data are—or can be—owned; others said the opposite. If data are owned and if multiple scientists have been involved in the creation of those data, yet they are not working as a collective entity, then the question arises: who has the right to publish—anyone in the project, only the PI, or the sponsoring institution? Patents are owned as are copyrighted materials such as textbooks. But what about source code, especially as open sourcing becomes more common? This gets to one nexus of change in today's scientific arena—computerization and the World Wide Web. Several of our informants mentioned uncertainty about intellectual ownership issues as they emerged on the Web. All of our informants agreed that for someone to take something directly from the

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ing the reward signified by citation, several studies of the sciences and hard social sciences report "citations as reliable predictors of pecuniary success within the academic reward system" (2005, 133).

Web without the permission of the author constitutes plagiarism. However, as one biologist put it: "In eco-informatics, for example, optical data is being gathered and made available on the Web. We're trying to establish rules for contribution and attribution, but it's difficult."

This is even true of teaching materials. A biologist who has moved into education scholarship spoke of the free exchange of syllabi and course activities that once occurred among her colleagues. Today, however, with such materials on the Web, and a need for them as part of tenure and advancement review in her department, teaching materials are more likely to be seen as property with individual ownership by faculty in ways they never were before. Of course, such sharing of course materials has been commonplace for years, but in the past, the mere mechanics of the process—asking for and receiving actual paper copies from a colleague—often meant that permission for the use of such materials had been granted, at least tacitly. One high-profile lawsuit over teaching materials is the late '80s case of Weissmann v. Freeman, which is all the more complex because the material was developed in collaboration. In this case, when one collaborator later used part of a previously co-authored paper with his name alone as part of the materials for a course, he was sued by the other co-author[7] (Mervis 1989).

One biologist who is collaborating with scientists all over the globe on a project that posts databases to the Web spoke of anti-ownership as a guiding principle of the project. He said that those involved wanted the data to be accessible to other scientists so that retesting for replicability as well as manipulation of the data could occur, continuing the scientific enterprise. However, the process stalled over concerns about how to

7. This case was further complicated by the earlier mentor relationship between the two collaborators and by the gender dynamics of a female suing a male who had erased her contribution by removing her name. It may also be a demonstration of the willingness of younger scientists to value ownership more personally than their older counterparts and to take legal action to ensure the rewards tied to that ownership. (See McSherry 2001.)

maintain the integrity of the data. He explained that it was vital that each data set stay tied to the parameters that created it so that misuse through miscalculation would not occur, but that wasn't easy to establish in a Web environment. The issue seemed to come down to trusting individuals who downloaded the data to be ethical in their usage.

TRUTH IN AUTHORSHIP

The scientists we spoke to were far more concerned with the integrity of data than with the possibility of plagiarism.[8] Our subjects revealed little in this area; they seemed to take for granted that scientists present their results honestly. "The opposition between truth and interest is one of the pillars (perhaps a rhetorical one) of the logic of scientific authorship" (Biagioli 2003, 85). Truth is the bedrock of science; the exchange of information operates in what has been theorized since the '60s as a "gift" economy (Hagstrom 1965). In this gift economy, moral obligations to truth and thereby trust in one another as scientists hold the structure together. "Knowing that one stands either (i) to gain credit for making an important contribution, or (ii) to lose credibility if one's findings later prove to be unreliable, scientists are motivated to produce results that are generally reliable" (Wray 2006, 509). In terms of authorship, the gift economy fits with our subjects' views that when they publish, they no longer own their ideas because those ideas become part of the public domain; thus, the scientists "gift" the world with their knowledge. However, the prolific discussion of the inherent rewards of publication contradicts the notion of a gift economy (Biagioli 2003, Birnholtz 2006, Merton 1973, Wray 2006). Bruno Latour and Steve Woolgar (1979) describe a "cycle of credit" in which scientists make results available in exchange for credit that leads to more funding and more research. Further, the growing interactions of IP and trade-secret law along with the growing litigation of copyright and trademark all signal that

8. This emerged primarily in their discussion of teaching students, which we will explore later.

even if science were once the land of the gift, it is now fully participatory in an economic exchange that challenges the notion of the selfless gifts of scientists (McSherry 2001). One specific place to witness the blurring of the boundary between gift and money economies would be in the concern over financial ties between pharmaceutical companies and authors. In light of growing public concerns, the *Journal of the American Medical Association* (JAMA), along with other medical publications, has begun requiring statements of "competing financial interests" in submission disclosures ("A Matter of Trust").

Whether through the fabrication of data or the plagiarizing of another's work, fraud is not a new phenomenon. Yet authorship lists that sometimes number in the hundreds because of the international and interdisciplinary natures of big science, the shifts in economic relations, computerization, and even the sheer growth in numbers of scientists, all contribute to increases in fraud. In the 1960s and '70s, cases of "faked data or plagiarism were dismissed as aberrations, as unrepresentative of the integrity of scientists overall" (Lafollette 1992, 1). Then came the '80s with a well-publicized rash of scientific fraud, including plagiarism by Elias K Alsabti; fabricated data and contaminated cell lines by John Long at Massachusetts General Hospital; data-faking by several scientists, including oncologist Marc Straus, who falsified patient records for a clinical trial; and Phillip Felig, who resigned as chief physician at Columbia Presbyterian Medical Center after failing to act decisively when a junior co-author admitted falsifying data and plagiarizing (Woolf 1981, 9). In 2005, another rash began with the Korean stem cell researcher, Woo Suk Hwang, whose work with embryonic stem cells was discredited, and continued with the announcement of false data in Norwegian researcher Jon Sudbø's cancer publications (Couzin 2006). But perhaps the most incredible event of 2005 occurred when the first scientist was incarcerated in the United States, Eric Poehlman, for "scientific misconduct unrelated to patient deaths" (Couzin 2006, 1853). "Poehlman acknowledged falsifying seventeen grant applications to the National Institutes of Health (NIH) for nearly $3 million, and fabricating data in ten published articles" (Dalton 2005). Data fraud creates varied problems as can be seen in the stem cell example. Hwang fabricated "data" about his ability to cure Alzheimer's disease, which led to other researchers falling behind in their efforts to build on his work, as well as losing time and the potential for grants, thus putting those who were following a different line of inquiry far ahead of the game. The domino effect created by scientific fraud wastes time and money, but perhaps more importantly, it erodes the public's trust and, in cases of medical research, delays treatments of the sick.

COLLABORATION AND AUTHORSHIP

In our interviews, one of the most striking findings is how fundamental collaboration[9] is in the creation of scientific knowledge. Collaboration in the sciences is so basic and elemental an assumption as to be all but invisible; for example, when asked about collaborative work, one of the chemists said she "didn't do much," yet when pressed to include students in that equation, she stated, "Oh, of course I collaborate with students." Other than one chemist,[10] virtually everyone we spoke with shared her same mild bemusement at our questions about collaboration, which is so much at the heart of what scientists do

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9. In our research, everyone talked about collaboration within a lab, but that may not be the only or the most common type emerging. "When I hear the word 'collaboration' in science, I think, 'collaboration between labs' not individuals. The basic unit of identity is the lab, not the individual. There are vertical and horizontal collaborations. Vertical collaboration deals with research staff at various levels of expertise within the lab; horizontal collaboration is across two or more labs where each lab contributes different things or do exactly the same thing, such as each studying a portion of a sample" (Graff 1992).
10. One chemist had worked for ten years at a research university before his move to start a new program at a brand new institution that would offer only undergraduate degrees for eight to ten years and then begin masters programs. He said that he owned more items individually and collaboratively. His list of owned items included patents, molecules, and research publications in journals, books, and abstracts. The difference in this response supports our concluding call for more research in this area.

that it disappears. However, it should also be noted that her lack of inclusion of students in her initial equation also comes into play because determining the boundaries of whose work is considered a contribution at the level of collaboration, and thereby authorship, is one of several central issues.

Since World War II and the advent of "big science," collaboration has been a fast-growing feature of scientific work. This is due to the size of the problems being tackled—putting people into space or mapping the human genome—as well as the resources and equipment needed for such exploration (Cronin "Hyperauthorship"). Much of this research has also created the need for interdisciplinary teams and the opportunity for international ones. Several researchers provide literature reviews of the documented growth in collaboration of specific, yet when collected, random assortments of fields, journals, and date ranges (Wray 2006, 507; Cronin 2001, 560–63; Zuckerman 1968, 277). For example, Harriet Zuckerman and Robert K. Merton found that from 1900–1909, 25 percent of published papers in natural science were collaborative, but by the 1960s, over 80 percent were co-authored (cited in Wray 2006, 507). Similarly, the numbers of co-authors has been rising. King found that from 1945–1995 the average number of authors per scientific article rose from 1.8 to 4.6 in the *Journal of Neurosurgery* and *Neurosurgery* combined (cited in Cronin 2001, 561). This growth in the number of authors is nowhere more evident than in high energy particle physics, as examined in studies by Mario Biagioli (2003), Jeremy Birnholtz (2006), and Peter Galison (2003). They describe physicists working at the Collider Detector at Fermilab (CDF), the European Council for Nuclear Research (CERN), and the Stanford Linear Accelerator Center (SLAC), respectively. Papers by scientists from these facilities often have author lists in the hundreds. While each has its own unique policies for how the author lists are created and ordered, as well as how responsibility is ensured in the process, collectively they demonstrate how one subfield of science has delineated authorship guidelines.

Although we interviewed no physicists, the research in this area provides a touchstone for the collaborative authorship occurring in biology and chemistry, especially in terms of how the challenges of rewards and responsibility are addressed. The significance of including research on collaborative authorship in physics is threefold for our purposes here: (1) specific guidelines have been spelled out and followed for decades; (2) while independence is maintained on some levels, for the most part, physics provides a model of truly corporate authorship where individual contributions of varying sorts intentionally cannot be identified; and (3) no contribution can be hierarchically weighed against another.

Clearly, the sheer number of participants in and "authors" of these large scientific enterprises has necessitated that these physics labs develop policies and guidelines for determining authorship; however, these policies are in stark contrast to our traditional notions of sole authorship. In "Beyond Authorship: Refiguring Rights in Traditional Culture and Bioknowledge," Peter Jaszi and Martha Woodmansee (2003) point out that

> even in the face of contrary experience, [which] tells us that our creative practices are largely derivative, generally collective, and increasingly corporate and collaborative, . . . we nevertheless tend to think of *genuine* authorship as solitary and originary. (195)

They further explain that until the eighteenth century, "in the sphere of science, invention and discovery were viewed as essentially incremental—the inevitable outcome of a (collective) effort on the part of many individuals applying inherited methods and principles to the solution of shared problems" (196). Despite this evidence of the collaborative nature of creativity—whether scientific or poetic—as Jaszi and Woodmansee argue, most modern copyright, intellectual property, and patent laws reinforce this Romantic conception of the "individual genius" at work, thus "obscuring the reliance of these writers on the work of others" (196).

Recently an essay by Mott Greene in *Nature* proclaimed, "The lone author has all but disappeared" (2007, 1165). The *day* had been coming, a decade ago, Drummond Rennie et al. wrote: "With modern research by multiple investigators, the authorship model is outmoded, stretched: it no longer fits" (1997, 582). The traditional definition of authorship as the one who pens or even computer processes words onto the page is no longer sufficient. In a world of not just collective but collaborative authorship, the problem of defining it grows (Lafollette 1992, 91; Wray 2002, 152). In the sciences, several of the problems of collaborative authorship can be seen in the prolific terminology used to describe it. We've broken the terminology we found (but which we do not believe to be exhaustive) into three categories: (1) the commonplace—*lead, first, last, senior, single, plural, collaborative, contributing, corresponding*; (2) the hyphenated—*co-, multi-, sub-, hyper-*; and (3) the emerging[11]/problematic—*corporate, collective but non-collaborative, ambiguous, honorary, gift, guest, promiscuous, surprise, ghost.* For the most part, the terms in our first category are common and do not need explanation, although a few of them have specific definitions in science. A *corresponding* author is the person who submits an article to a journal for review and thereby is the conduit of information between a journal and multiple authors (Ilakovac et al. 2007). The label *senior author,* as it sounds, refers to one's seniority or prominence, but this label is attached to various problematic behaviors to which we will return. In our second category, the hypenated *sub-authorship* is typically used by someone citing a *multi-authored* text in which names at the top or bottom

11. While some of these labels are not new, they are emerging in the sense of growth which challenges accepted ethical standards. For example, ghostwriting is certainly not a new concept. However, there is a world of difference between a biography which is ghostwritten, rendering the prefix "auto-" inappropriate, and a scientific article on a clinical trial for a new drug which appears with the name of a seemingly disinterested scientist, often someone in the forefront of the field, who did neither the research, nor the writing, but merely lent his or her name in exchange for cash from the pharmaceutical company producing said drug.

of the list are well known, but others are not; thus they become sub-, something less. *Hyperauthorship* is a term coined by Blaise Cronin (2001), which refers to articles listing more than a hundred names in the byline.

In the third category of emerging/problematic terminology, several labels deserve clarification. *Corporate* and *collective but non-collaborative* are terms used to signify particular kinds of group authorship. A *corporate* author refers to a list of authors who have created for themselves a group identity such as often occurs in physics; this type of authorship is designed to diminish the sense of individual ownership and, in some cases, to increase the sense of individual responsibility (Biagioli 2003). The label *collective but non-collaborative* sends the opposite message; it allows for the contributions of individuals to be listed in some form, perhaps by directly identifying contributions or by an author order based on contribution. *Ambiguous* authorship simply arises from the context of multiple authors with neither of the above conditions.

All of the other labels in this third category are problematic in one way or another. The types of authorship included below have come under increasing scrutiny in recent years, chiefly because of the ways that authoring is obscured in a list on a publication. According to a review of literature examined by Cronin, the increase of undeserved authorship in one field rose 21 percent when the number of co-authors exceeded six, while in another field, 19 percent of reports carried the name of at least one honorific author (Cronin, 2001, 563). Honorary, gift, and guest authors are all names appended to a document for reasons that do not include actual intellectual contributions or labor in the research and resulting publication. These types of authorship are most often granted to senior scientists, lab "owners," and grant recipients or PIs who do no more than sign their names to projects. These are then sometimes considered promiscuous authorships as well because they are handed out liberally. At times, such authorships surprise the named individual who had not been consulted and who did not expect to be named. This may seem odd, but we found in both our interviews and our literature review that senior scientists were likely either to (1) expect the inclusion of their names without necessarily being involved, or (2) give authorship to students even when the senior was the primary conceiver of the project.

And finally, a term common in biography is the “ghost author.” While two of our subjects spoke of a variety of services for which a technician, statistician, or scientist might get paid rather than receive authorship credit on a project, none of them mentioned the ghost writer, perhaps because the recent growth of this phenomena has been predominantly in the biomedical arena—especially the pharmaceutical—and none of our subjects works in that subfield. In *Ghost Marketing*, Barton Moffatt and Carl Elliott (2007) examine the practice of pharmaceutical companies hiring communications companies to write favorable reports of their products and then enlisting well-known academics to publish them without disclosure of the research origins (18). This ghostwriting process hides a commercial enterprise in the cloak of academic scientific purity, producing something that appears honest but that violates the public scientific trust. Such ghostwriting provides useful “marketing tools precisely because they appear to come from a disinterested source” (27), which creates a “patina of undeserved academic credibility” (29). This practice clearly blurs the property line between commercial product and intellectual property.

Regardless of the label attached, collaboratively researched and written scientific texts raise many intertwined issues that are problematic to both the reward and the responsibility inherent in the professional sphere. Collaboration undertaken by a large group—sometimes numbering into the hundreds, as with physics, rather than just two or three people—makes determining “author credit” in the listing of names extremely complicated and potentially controversial. These include author order, contribution donor names, and the additional cultural forces that lead to honorary, gift, and guest authorship. According to Zuckerman

(1968), there are three predominant ordering principles: equality (alpha forward or backward), first or last author out-of-sequence, and alphabetically random (278–79). The first principle is self-explanatory and is in use in the hyper-textuality of physics. But when no author stands out as primary, who gets credit, and who gets blame? While physics has dealt with the responsibility problems that can be created by this ordering,[12] it has not solved the rewards problem, which can be seen in Birnholtz's (2006) research at CERN. Since individual publication does not occur, Merton's Matthew Effect holds true, wherein a scientist prevents her/his credibility from being subsumed by a more senior scientist. Birnholtz's interviews with physicists revealed that "getting noticed" became an alternative and was crucial to a credit system internal to CERN. Rather than department faculty who might be unfamiliar with one's research and publications, CERN scientists create a small enclave of physicists who believe they know everything about each other. Birnholtz's interviews revealed that young physicists were required to do something that made them stand out from the masses of scientists, technicians, and engineers who worked on any given project.[13]

The second pattern, first or last author out-of-sequence, allows for one author to stand out among equals, so one name stands out as primary. The third pattern, alphabetically random, is indiscernible to the common reader, though insiders to the project have criteria for the ordering based on amounts and types of work. These are most problematic with regard to reward but not necessarily to responsibility.

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12. As an example, at CDF, Biagioli (2003) describes the "Standard Author List" as containing all members associated with the institution including technicians and students (100). In this particular system, drafts circulate for three rounds of revision to members who may "opt out," if, after revisions have been completed, they do not accept any or all of a document. What this creates is a system where a shorter author list represents a more suspect piece of work than one that contains the full list (102).
13. Getting noticed could take place because of a variety of behaviors: being dependable and diligent, coming up with novel solutions, giving talks and presentations that offer visibility, and providing leadership through additional responsibilities to those originally assigned.

Most of the scientists we talked with said the most important author position in a listing "usually" comes last and goes to the PI who received the funding for the research. That they reported this occurrence as "usual" indicates the instability of this expectation within both fields. On the surface, the significant position of being last appears to be Zuckerman's (1968) second category of one standing out among equals. One chemist we spoke to reported that, generally speaking, untenured faculty are listed first, tenured faculty next, and the "key" faculty member last. However, they never mentioned alpha ordering as an option. Their descriptors suggested that criteria of contribution factors drive the sequencing of names. Thus, they seemed to be using Zuckerman's third indiscernible pattern with a nod to a senior scientist as last author. Their assertion that the last place was primary generated a surprising benefit for one of the biologists we interviewed, who had moved into biology education as a primary field; she expressed her pleasure with the fact that education emphasized the first listing as most important, which meant that when she co-authored papers, there was often room for two authors to receive primary credit from their respective peers—she for her listing as first author and a co-author in biology who received equal credit from peers for being listed as the final author.

When criteria are used (i.e., amount of work, intellectual contributions, actual writing), as our interviewees took for granted, which criteria are most valued? That was less easy to assess. While all reported that the author positions were based on the roles of the various participants, they were far from consistent in their determinations of which activities garnered the best positioning on the list. Some said that the amount of work someone contributed figured into the ordering of names. One chemist argued, for instance, that students who do very little but end up with authorship credit in a publication are in fact committing a form of "plagiarism," taking credit for work that is not really their own. A biologist, however, said that he didn't need the credit and felt strongly that students who put in the many "man hours" it takes should be rewarded. He said that to give a student the "gift" of being primary was entirely up to him. "It's none of my campus colleagues' business if I choose to list my students' names on articles. My intellectual property, my academic freedom." Clearly, amount or type of contribution are not as important as professional status and power in listing order.

It seemed obvious to all we interviewed that those who contributed significant intellectual insight deserved authorship, as did the actual writer(s), but other roles—and there can be many—were less clear. The scientists told us that contributors of data or ideas (such as suggestions for ways to improve a study) or lab assistants who contribute their labor to a project may be listed as authors or credited in the methods or acknowledgment section. These citations of contribution are not simply different in location, but in value; one of the chemists was a bit dismissive of acknowledgments, stating that "no one cares much, since you can't use it." According to Cronin, the acknowledgment section "serves as a parking lot for miscellaneous contributions, cognitive, technical, and social" (2001, 564). He also points out that the line between authorship and acknowledgment is neither universal nor consistent, which was exemplified in our findings.

Technicians, lab workers, and statisticians have traditionally been part of the "work" force rather than the "intellectual" contributors and have not received author credit (McSherry 2001, Rennie et al 1997). Complicating the matter further, Cronin points out that on the Web there are "ever increasing numbers of nontextual objects" contributed which don't deserve authorship credit but which are nonetheless part of the product demonstrating the research (2001, 564). If a suggestion by someone on a project turns out to be crucial, that person may end up being listed as a co-author even without being one of the central figures. One biologist reported that it was possible for students to get authorship listing if they contributed significantly even though they didn't understand the entire project. On the other hand, the same biologist pointed out that graduate students might not get authorship credit on their own theses. A chemist said that he had not received top billing on his thesis because his mentor expected that his senior status entitled him to place his own name in the position of power. Such are the vagaries of the scientific authorship mentor system.

This brings us to a final important consideration in scientific authorship, that of the "senior author." This term is easily recognizable as the scientist in the listing with the most prestige or power, but it isn't a term commonly used in other academic disciplines for authorship. In science, the term "senior" is so common that we heard it from every interviewee multiple times and found it in most of the literature we read. It is not the same as PI, though in everyday conversation they may seem synonymous; rather, it is a term used to identify the known name in a list of co-authors and is most predictably last, or in some subfields, first. While this is the expectation in science, it wasn't fully borne out by Zuckerman (1968). She studied the name orders of works with and without Nobel laureates and interviewed several of the Nobel winners. While a hypothesis that Nobel laureates would have their names in the power positions more often was proffered, the findings were that "*noblesse oblige* is exercised more frequently as the eminence of individual scientists increases" (288). This, too, fit with our research, since several of our interviewees pointed out that either they themselves or other senior scientists occasionally give credit or authorship to students or those on a team who might traditionally be deemed "unworthy," such as those doing the often tedious labor of an experiment. We also found a type of ownership embedded in this concept of "senior" scientist. When asked about intellectual property and ownership, none of our interviewees said that labs were owned, yet in the process of discussing author order, several referred either to the "owner of the lab" or used the possessive, such as Dr. Johnson's lab. Obviously the imagined Dr. Johnson does not literally own the lab, its space, or its contents, but by managing it, acquiring grants, and hiring students and lab techs, Dr. Johnson enacts a type of ownership within the lab environment and its results—publication.

Our subjects reported that in some cases it was easy to determine authorship. The first author does most of the writing and others offer feedback, demonstrating that the first author understands the project most fully and others merely contribute. It should be noted, however, that in our interviews the scientists said "first," meaning most important, while also saying clearly that the actual location in the list would be last. Several scientists we spoke with also tend to decide authorship order early in projects so that no surprises occur.[14] Given the amount of dissonance surrounding collaborative authorship, we were pleased to know that it was possible for the criteria to be clear to insiders, at least some of the time.

Definitions of collaborative authorship may be expanding and uncertain, yet the social structure of science demands authorship, not only to confer symbolic and remunerative rewards, but also, and equally important, to secure the responsibility of researchers. With rewards, the primary concern is whether scientists get proper credit. As it stands, they may be awarded too much or too little, depending on the ways that author listings are both arrived at internally by the authors and understood externally by those who hire, promote, and tenure them. When a listing is alphabetical, how can those who offer rewards do so equitably regarding the type and amount of contribution? On the other side of the coin, when lists are arranged by some internal criteria order, external readings of that order must assert values

14. Not unlike the problems we uncovered in scientific co-authorship, in working on this chapter we did not decide author order or particular roles in our collaboration prior to embarking on the work. After extensive reading about the ways that scientists now try to distinguish who "authored" what (see Lafollette 1992; Cronin 2005; Rennie, Yank, and Emanuel 1997; Zuckerman 1968) and considering such descriptions for ourselves, we decided that our collaboration was such that we could not parse the particularities. Instead, we opted for an alpha-order listing. Denise, recognizing her destiny near the end of every such listing, hopes that the prized place of "last" author in some scientific spheres might accrue to her, even though her field is not among them.

for the ordering that cannot be fully known. Both our informants and our literature made abundantly clear that even when scientists assert that the last author gets primary citation credit, the last author may have been relatively uninvolved in the actual project. It's no wonder that our informants did not feel ownership of their publications. While theoretically this is due in part to the anti-ownership underpinnings of science, the lack of clarity of author orders must also play a significant role.

Ownership means not only getting credit but also taking responsibility for one's work (Birnholtz 2006). Or to put it another way, with rewards come responsibilities. In collaborative authorship, determining who contributed what is problematic at best. When falsified data, plagiarism, or some other type of fraud is discovered, it's unclear which scientist(s) should be held accountable. In the '80s, after a rash of fraud cases came to public light, Woolf (1981) suggested two primary reforms: She asserted that granting agencies needed procedures that would prevent dishonest scientists from obtaining further research support and that journals needed to have retraction policies (10). A decade later, when the next round of substantial scientific fraud hit the media, Rennie et al. (1997) called for initiatives from four sectors: universities, professional societies, outstanding researchers, and journals. They also proposed very specific policies for authorship: (1) that contributions be specific and visible for each author so that they are thereby held accountable for their portions of the project, and (2) that guarantors be established as overseers of a project, who are able to "vouch for the whole work" (582). They further describe ways in which indexing services, universities, granting agencies, and professional societies "can influence the culture substantially" (583).

Rennie et al. recognized that it takes multiple forces to change a profession. Now another decade has passed, and though several publications have established submission forms with detailed contribution, retraction, and duplicate publication policies, the problems persist. In "Even Retracted Papers Endure," Katherine Unger and Jennifer Couzin (2006) note that retraction does not stop citation of the original publication. This means that even retracted work might lead to problematic research down the line, even though electronic databases now have the capacity to flag retracted articles when they are downloaded, which can reduce the likelihood of the problem going forward. Detailed contribution forms may be less reliable than they at first appear, at least if that information is conveyed through a corresponding author. In "Reliability of Disclosure Forms of Author's Contributions," Ilakovac et al. (2007) report on a study including over 900 authors of over 200 articles in medicine in which they found that there was inconsistent reporting of contributions in multiple ways. While a single study is not generalizable, it does give pause as to whether or not contribution listings solve the multiple problems raised by collaborative authorship. As Cronin writes, "While listing contributions may clarify the nature of coworkers' participation and, thus, both reduce the incidence of honorific authorship and ensure more equitable allocation of credit, it does not necessarily address the thorny issue of ultimate responsibility for the overall integrity of the study" (2001, 566).

Clearly, there is work to be done to stabilize authorship so that ethical practices are transparent and so that individuals and collaborators can be held accountable when necessary. It appears that this is an issue of scientific culture that will not be easily fixed by mandates from any single source, but as Woolf notes in her conclusion, without substantive response to these growing concerns, the professionalism of science is at risk.

LEARNING/TEACHING CITATION PRACTICES

The scientists we interviewed all spoke of citation as largely (though not exclusively) done to put one's contribution into context, in the form of a literature review, for example, to show where this new work fits and how it complicates or adds to the existing body of knowledge in a particular area. These expectations for citations are true both for themselves as scientists and for their students as emerging scholars and writers.

Our interviewees share the fairly conventional belief that students (or anyone) must be meticulous in citing their sources of information accurately. One informant said that the use of secondary sources was absolutely forbidden because it was crucial to "fully understand primary sources." However, citation is less a demarcation of "ownership of ideas" than of providing a context that demonstrates their credibility as scientists who are contributing to their field. They do so by adding research data that either reproduces the work of others or examines an altered or new hypothesis that will then also need replication. These pragmatic contextual needs drive the process, but citation is also a means of showing respect for the work of other scientists. One of the biologists called it a "professional courtesy." Another said it was done "out of respect and appreciation."

In terms of learning about citation practices, there appears to have been a paradigm shift between the time our informants were students and today. Scientists reported that they mostly learned to give proper attribution for sources implicitly. As graduate students, some of the scientists were given pointers by mentors on how to give credit, but for one respondent—a chemist near retirement age—it was never explicitly discussed or taught, so he learned to cite sources only by modeling and implication. As he put it, when he began teaching, "It was 'don't ask, don't tell'—it's OK to talk about what you teach, but never *how* you teach," a prohibition that applied to teaching practices including how to teach citation.

In their own teaching practices, however, these scientists tend to be much more explicit than their teachers were about how they expect students to cite sources. One biologist said, "[Teaching citation] is evolving. It's not something I ever learned explicitly. . . . We put emphasis on this in the classroom here more than I got." A paradigm shift was evident; we were surprised by how matter-of-factly our interviewees explained the teaching of citation conventions as part of their own responsibility and role as faculty members. Not all approached it as a rote part of their curriculum, but if and when they discovered that their students were having trouble understanding citation practice, they ensured that the individual or the class learned the expectation of the field. This was true despite the fact that they hadn't anticipated having to do so when they began teaching and despite the fact that none of them had ever explicitly been taught the conventions themselves. As one chemist said, when she found that her students didn't understand how to quote, cite, or paraphrase sources accurately, "I felt I had to intervene." A biologist put it even more simply: "I expect mistakes." Not surprisingly, they saw this practice through a scientific lens, stating that, "trial and error is to be expected." A student writer can't be expected to get it right the first time, much as an experimenter can't expect to get the result that demonstrates the hypothesis the first time out. It takes practice; mistakes are part of the process of learning in science. They didn't see such errors as evidence of moral failure and jump to accusations of plagiarism; rather, they believed they had a responsibility to teach their students how to demarcate the sources used in their research. This coincides with Woolf's system of scientific social controls to prevent fraud; she says that "fledgling scientists" learn to develop an "internal monitor" from mentors that teaches them that, "the aim of the enterprise is reliable new knowledge" (1981, 11).

Several of the scientists spoke of receiving papers with "too many direct quotes" copied verbatim from Web sources, especially from non-majors, and either too little or too much citation as the primary attributing errors. One of the chemists encourages his students to paraphrase rather than quote, in part to keep them from "plagiarizing," but also to help the students extract and comprehend the meaning of what they are citing better than they do when they are simply copying quotes verbatim. As he said, "Students will often use a quote but not put it into quotation marks. They think that if they put it [the citation] in a footnote, that's OK, that it's not plagiarism because they've attributed the concepts or ideas. But they've still stolen the actual words." While many of their students felt that the shared, common knowledge comprising the "facts in textbooks" do not need quotation or even citation, much as our opening anecdote suggests, the faculty we interviewed did not totally agree on this point. Citation mattered greatly to them, though for most, quotation was disdained as inappropriate to their field. This disagreement about use of textbook material resides, in part, in whether scientific facts are seen as stable. In not quoting or citing, the assumption is stability. One of the biologists pointed out that students need to be dissuaded from the idea that facts are stable entities because the enterprise of science constantly challenges the already known. Similarly, one chemist spoke of how students learn about these evolving concepts in the field when they do research, so that students who actively engage in conducting their own research understand more about their field than students who don't.

As we've stated, in their teaching of citation practices, all of these scientists expect a certain amount of error from their students. Perhaps because their own learning of these conventions didn't occur until graduate school, because the acceptance of failure is seen as part of the scientific process, or because citation practices have typically been taught in an English context where direct quotation occurs more than citation of findings—whatever the reason(s)—these faculty were calmly accepting of their students' difficulties and willing to work with them as they struggled to figure out how to cite properly in these disciplines. Notably, the biologists pointed out that in their field no single citation style has been identified as the standard, so they understand students' struggle more clearly than others might who take a particular practice for granted.

Clearly, the faculty we interviewed reflected good Writing in the Disciplines (WID) pedagogy. They understood the need to teach citation and science-writing conventions explicitly and to create opportunities for revision in a variety of ways. One biologist said, "I talk about [citation] theoretically initially—purpose and why it's important in the academy. [Then] I model it using student examples."[15] When their students fail to meet assignment expectations for citation, they are most often given a chance to revise. This might happen in the paper they are working on or it might happen in a future paper of a similar type.

However, in addition to concerns about falsified or inaccurate data, the scientists we interviewed, all of whom are teachers, did voice some concern about classroom plagiarism. For a junior-level writing class in chemistry, one of the chemists described his practice in assigning a 10–page paper which counted for 30–40 percent of the students' grade in the course: the students choose a topic from a list provided, and after they have done a literature search of databases in which they find twenty-five papers on their chosen topic, they must choose six papers from their own lists and write a review of only those six. Because they can include references only to the six papers they have chosen, the instructor feels that it is impossible for the students to plagiarize from other published sources. For example, if a citation to a work outside of the six a student has chosen appears in his or her paper, the instructor is alerted to the possibility of the review being taken from another source. Using this method, he has encountered very little plagiarism; in the year prior to our interview, he said that he had had two instances of plagiarism in one quarter, but that was the first time ever. In good WID fashion, he also had his

15. An interesting aside: At a national WAC (Writing Across the Curriculum) conference, after a presentation about a writing-intensive course for nurses, Lise asked the presenter, a community-college nursing professor, whether she taught citation practices as part of the course. With a bit of apparent confusion, the woman replied, "They've taken their English." To her credit, the professor demonstrated that her focus in the class was on researching and critical-thinking skills and that she relied a great deal on small-group collaboration and active learning, but her assumption about students' citation practices being both someone else's responsibility and something that once taught was "done" seemed to be in opposition to the one held by our respondents: that they have to teach it, whether students have "taken their English" or not, and that they may have to attend to it more than once. However, no doubt there is more diversity among science faculty in higher education in their teaching of citation practices than our sample from teaching-centered universities suggests.

students doing at least some in-class or outside writing every day, not only making the students aware of his expectations for frequent written work but keeping himself familiar with the students' abilities.

Another of the chemists said that despite the difficulties she had witnessed in her students' ability to handle citation practices, she had not yet encountered any problems with actual plagiarism. Her method is to "call attention" to any potential problems "in big red letters" on the students' work, and then, as she stated, "the problem ceases." She also pointed out that many of the students are pre-med, so while they may not care much about the class itself or about the citation conventions, they *do* care about passing and getting a good grade. Another member of her department concurred, stating that introductory classes are "mined" for good students, who may stay in a given lab group with a particular faculty member for several quarters or even years, perhaps the whole time they're in school. The chemistry department has a small, unchanging population, so students are motivated to succeed, for reasons of self-preservation if not scientific integrity or ethics.

Biology and chemistry students, like all students, are in the process of learning the conventions of writing and citation practices in their fields. As such, they struggle with what to cite (are textbook facts cited?), when to cite (what constitutes common knowledge?), and how often to cite (do I reference everything I find everywhere?). We discovered that these science faculty dealt with all of these enactments as teachable moments—approaching them directly, matter-of-factly, and without moral outrage—because they expected trial and its co-requisite error. The biologists and chemists we interviewed recognize the dissonances their students experience, students who have likely had their only writing instruction come from English/writing departments, which emphasize the importance of direct quotation. This led to the problematically high number of quotations they experienced in their general education courses, but even there, they were likely to teach to their disciplinary expectation rather than penalize students who did not know their conventional practices.

DISCIPLINARY HABITS

The scientists we interviewed demonstrated the tradition of the sciences in many respects: trust is vital, the gift exchange is primary in conferring rewards, and noblesse oblige is intact. Intellectual property is thought of very little; rather, anti-ownership is the normative expectation in "pure," academic science. Findings must be released to peers and the larger public to continue the pursuit of knowledge. Collaboration is expected. The ordering of authors is based on criteria, albeit with varied hierarchies: the amount and types of work performed, and most importantly, the level of status of those involved, especially the senior scientist. Plagiarism was less a concern than was the integrity of data. Citation practices are crucial to provide historical context.

Our research also revealed the edges of change. The ordering of names in author lists is unstable and can present problems because no one can be certain how to "read" the meaning of ordering beyond a key position of senior author at the end. The disciplinary habit of scientific ethics may need to be taught more explicitly to budding scientists, as was the explicit practice of teaching citations among our subjects.

What our interviews did not reveal was the growth of fraud and the attendant problems of responsibility found in large collaborations. This should not be too surprising; as Woolf says, "Scientists as a group are generally reluctant to acknowledge falsification of data as a pervasive problem and seem unwilling to take formal notice of this serious deviation from prescribed scientific norms" (1981, 9). While our interviewees never mentioned fraud in any grave way, or at all in terms of professional production, we were alerted to look at the issue of fraud because of the deeply expressed concern that students not falsify data.

It appears that the growing trend of larger and larger collaborations, spanning the disciplines and the globe, is challenging the professional dimension of the sciences. In the last thirty years, calls for reform have been unevenly enacted. Simultaneously, commercial science has grown and begun to overlap with academic science. This challenges notions of intellectual property, whether thought of as individual ownership or the public anti-ownership of ideas traditionally espoused by science. To us, it looks like a paradigm shift in the meaning of authorship and its attendant rewards and responsibilities has begun but is far from complete. McSherry sees technology as a major player in the paradigm shift that is in process and sees the management of complexity as vital (2001, 20). This fits with our interviewees reports of future concerns, most of which congregated around technology. The integrity of data on the Web and all that is entailed in electronic sharing of information and the potential loss of originary citation came up in several biology interviews. The possible diminishment of the peer-review system with the growth of rapid online publication and acceptance of "personal communication" as authorial is also an electronically based concern that was voiced. As the human genome project matures, questions about who will own genetic information arise. Issues of ownership at the junctures of industry and academe concerned one chemist, as did a growing concept among colleagues that "if you don't sell something, it's OK to use it freely." Here, too, appear telltale signs of commercial science (selling a discovery) challenging and changing the expectations of pure, academic science (using discoveries freely and publicly).

Academic habits of thought, influenced as they are by our disciplinary training, too often do not include a conscious awareness of what we consider to be intellectual property. Going into this project, we had imagined that scientists would be much clearer about what they owned as scholars, since their research is based in more tangible media than is the ephemera of "personal expression," as Biagioli refers to it (2003, 84). We discovered, however, that the public nature of science combined with many unexamined assumptions about ownership meant that most of those we interviewed did not have ready-at-hand responses to questions regarding intellectual property and plagiarism. In our view, this lack of focus on ownership comes at least in part from the fact that our research was conducted with faculty at teaching universities. Whether or not these findings would differ at research institutions, we cannot be sure, but more investigation into this area is warranted.

This research, both our own and that of our colleagues in this volume, has made us ever more aware of the importance of recognizing the differences across the disciplines of what constitutes plagiarism and its basis in the shifting sands of authorship and intellectual property. In lieu of the media witch hunt for electronic plagiarists and the burgeoning market for ways of catching students who plagiarize, it is more important than ever that we tread more cautiously and approach the matter from a critical, educated perspective—especially in the sciences, where a concern about plagiarism is not as strong as a concern about data falsification and where the shifting sands of change make it imperative that intellectual property be publicly constituted so that academic scientists maintain their professional disinterest in results.