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Credit History
The Changing Nature of Scientific Credit

Joshua S. Gans and Fiona Murray

4.1 Introduction

We are interested in the institutional conditions confronting scientists engaged in cumulative innovation and the ways they have changed since Vannevar Bush famously evoked the image of an endless frontier of scientific progress. While many scholars have explored changes in outputs, that is, in the rate and direction of the scientific frontier, we examine changes in the production of scientific knowledge. In seeking to explain how the organization of knowledge production has changed, scholars have focused on two critical factors: the vast number of new technologies that enable scientific progress (Mokyr 2002; Agrawal and Goldfarb 2008) and the increasing burden of knowledge needed for cumulative progress at the frontier (Jones 2009). We propose a third, previously overlooked, factor grounded in the formal institutions and norms of science: the assessment and allocation of credit. The institutions of credit are central to the incentive system of open science (Merton 1957; Dasgupta and David 1994). While not as easy to observe as the growing array of new equipment that fill laboratories today, and harder to conceive of than the notion that to contribute to the frontier

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today means knowing more than those contributing fifty years ago, we argue that credit has also changed in a range of critical dimensions in the years since the "endless frontier."

We start our essay by focusing briefly on economic and sociological perspectives on the nature of scientific credit. In section 4.2, we then develop our perspective on the core organizational choices made by scientists as a way of motivating the central importance of scientific credit in the ways in which knowledge production is organized, using an example from number theory in mathematics to highlight these choices. Section 4.3 elaborates our "credit history"—how the institutions and norms of scientific credit have changed over the past fifty years. We do so by exploring three debates that have animated the scientific community over the past fifty years. Building on the qualitative insights from the past fifty years, section 4.4 lays out a formal model that places credit allocation alongside the changing technical costs and knowledge burden of research to explore the relative importance of these three factors. Section 4.5 considers predictions for how science is likely to change going forward and implications of the continually changing nature of scientific credit for the scientific community. Section 4.6 concludes.

4.2 Credit and the Organization of Science

4.2.1 Choices in the Organization of Science

The post–Vannevar Bush years have seen growing interest among scholars in disciplines spanning the history, economics, and sociology of science in understanding the production and accumulation of scientific knowledge (rather than simply a focus on scientific knowledge itself). While economists such as Arrow (1962), Nelson (1959), and Rosenberg (1990) have famously pursued questions regarding the rate and direction of scientific progress, sociologists including de Solla Price, Garfield, and Merton were among those highlighting the social and institutional nature of scientific progress at the frontier. More recently, the economics of science has been concerned with the institutions that shape knowledge accumulation, among them, decisions regarding the production and disclosure of knowledge (Azoulay, Graff Zivin, and Manso 2010; David 2008; Furman and Stern 2011; Mokyr 2002; Murray and Stern 2007; Murray et al. 2009).

As they work at the knowledge frontier, scientists (in a variety of organizational settings) also make a range of meaningful organizational choices, although these have been less widely examined by scholars of science. Particularly for those researchers working within academia, there is considerable flexibility with regard to a range of choices. Indeed, one can think of academic laboratories as small enterprises in which the faculty scientist is effectively a chief executive officer (CEO) with significant autonomy. Among their autonomous organizational choices, two are critical: First, the scope of the project—whether to undertake a small research project and then disclose or to undertake a more substantial but possibly longer project and disclose at a later time. Second, whether and with whom to collaborate with, who to have as a coauthor, and so forth. We place these choices at the front and center in our analysis of the organization of knowledge production and its transformation over the past fifty years.

A useful way to understand scientists' organizational choices is to build on the conceptual approach developed by Green and Scotchmer (1995) in the context of cumulative innovation in the private sector and by Aghion, Dewatripont, and Stein (2008) with regard to scientific research. Accordingly, research follows a particular "line" that sets an intellectual trajectory for progress and along which research can be understood as taking place in discrete stages or "chunks." At each stage, scientists (or those who fund them) have the freedom to determine specific organizational arrangements and control rights and rewards within the constraints of the broader institutional context. With respect to the organizational arrangements made by scientists working within academia, we argue that three elements are critical: First, they must determine a sequence of cumulative projects that follow along the line they are pursuing; that is, they set a particular intellectual trajectory and map out two or more projects along that line. Second, they must determine the optimal way to approach these projects with respect to collaborative choices. Third, they must determine their disclosure choices for these projects. Taken together, these three elements lead to three distinctive organizational outcomes for any two steps along a research trajectory (and can thus be generalized along a much more significant path):

- Integration. Under integration, scientists may choose to undertake both projects in a line themselves (i.e., within their laboratory with no external collaborators) and only then publish both steps.
- Collaboration. Under a collaboration strategy, scientists bring in collaborators (from other laboratories presumably with complementary skills) to complete both the projects in a line and publish a paper describing both steps with coauthors.
- Publication. Rather than collaborate or integrate, a scientist may choose to publish the first stage in the line and then simply wait to be cited (in the market for ideas) by follow-on researchers who pursue the second stage of the line at some later point.

While conceptual in our exposition, these three organizational alternatives reflect the very real choices made by academic scientists throughout the course of their careers as independent investigators. They are sharply illustrated in the recent case of discoveries in number theory.

On April 14, 2013, Dr. Yitang Zhang, a previously unknown lecturer at the University of New Hampshire, submitted a paper to the *Annals of Mathematics* that purported to prove that there were infinitely many pairs of consecutive prime numbers with a gap of, at most, seventy million. This was the first such bound established and one of the most significant steps toward proving a long-standing conjecture that there exist an infinite number of twin primes (that is, a bound of size 2, the smallest possible). The paper was accepted for publication on May 14, 2013.

Zhang's contribution reflects a strategy that we would describe as "publication"—it was sole authored but he chose to publish as soon as he had established an advance rather than take the next step, that is, follow an integration strategy. What is interesting is what has happened since that time: in subsequent months, other researchers showed that the seventy million bound established by Zhang was capable of significant refinement. By July, it had been reduced from seventy million to just 5,414. With each advance, Zhang's contribution became more significant.

What is salient for the purposes of our analysis is the way in which follow-on researchers chose to chart the continued research trajectory. And as the bound fell to 400,000, there were contributions from a number of individual researchers who raced to publish even the smallest improvements; that is, they followed a publication strategy. However, in June 2013, Field's medalist Terence Tao proposed a change in organization. He set up the bounded primes problem as a polymath project. Polymath projects are online collaborative endeavours in mathematics using many researchers to solve unsolved problems in a short amount of time. There had been seven such projects over the previous two years, all with some measure of success. The important feature of the polymath project is that all conjectures, failed routes, and advances are made public and transparent. For the bounded primes problem, in just a month the bound fell from just below 400,000 to its current level. The end result was a many-authored paper with this final result and proofs of varying efficiently, that is, a modern form of collaboration.

The bounded primes example vividly demonstrates the range of organizational choices that can be pursued, as well as the changes in organizational modes scientists pursue today. It highlights the importance of thinking more deeply about organizational choices and credit in science. The simple dichotomy between sole authored and collaborative works does not capture the richness of the scientific knowledge production process. Here we argue that much can be gained by explicitly considering publication (and citation) as an organizational model for cumulative scientific endeavours alongside integration (sole authorship and secrecy) as well as collaboration.

4.2.2 Organizational Choices and Institutions of Credit

Our organizational perspective highlights the factors that influence scientists as well as the central role of credit in the organization of science along research trajectories. Without the consideration of credit, the reward structure that lies at the heart of scientific work is ignored and our explanations of knowledge production are inadequate. Our argument is as follows: selecting whether to integrate, collaborate, or publish (and rely on the citation market) depends at least in part on the ways in which scientists' believe that they will be rewarded for each of these alternatives. Specifically, a scientist choosing among these options must consider the cost of pursuing each project along the line as well as the time it will take to accumulate the relevant specialized knowledge—the traditional factors thought to shape the organizational calculus made by researchers from one laboratory to another. Nonetheless, these explanations are incomplete. While the costs and benefits of the necessary technology and specialization are critical, scientists must also consider the benefits and costs in terms of the level of credit they will receive under different organizational arrangements.

Under *integration* a scientist receives all the credit for a substantial amount of research progress along the line, but must balance this against the potential costs of acquiring the specialized knowledge and accessing relevant technology. In contrast, the attractiveness of *collaboration* depends upon the trade-offs between the benefits of additional resources (expertise and technology) brought to the project by coauthors and the possible costs of how credit is allocated and shared between scientists and their collaborators (see Bikard, Murray, and Gans [forthcoming] for an empirical elaboration of this issue). Last, under the *publication* choice, citation markets provide an alternative form of credit—in the form of citation and acknowledgment that may itself be valued by researchers and those who evaluate them—that must be considered as a scientists may then receive credit for the first-stage project in the form of publication and credit in the form of citation recognition from the second stage researchers.

The trade-offs driving scientists' organizational choices emphasize the importance of credit as a more institutionally grounded, but nonetheless important, countervailing set of costs and benefits that balance the technical costs and benefits of pursuing particular organizational strategies along a given research line. The role of credit as a central institutional feature shaping the organization of science came to the attention of economists upon publication of Dasgupta and David's influential 1994 paper "Towards a New Economics of Science." In it, they highlighted the importance of reexamining the organizational structures as well as the institutions and policies of science. The paper argues that science "is a system that remains an intricate and rather delicate piece of social and institutional machinery" (489), and emphasizes the importance of the norms and general "institutions" governing the production of knowledge. In focusing on the less tangible features of scientific work, Dasgupta and David build upon a long sociological tradition examining the institutional arrangements found in academia.

Distinguished sociologist of science Robert Merton identified the central role of credit and the informal norms regarding credit, describing them as the "psychosocial processes affect[ing] the allocation of rewards to scientists for their contributions—an allocation which in turn affects the flow of ideas and findings through the communication networks of science" (Merton 1968, 56). Merton also argued that credit could be "mis-allocated" under some conditions noting, famously, that "[e]minent scientists get proportionately great credit for their contributions to science while relatively unknown scientists tend to get disproportionately little credit for comparable contributions" (Merton 1968, 443)—a feature of scientific credit Merton dubbed the "Matthew Effect."

While the Matthew Effect emphasizes a specific instantiation of credit and its (mis-) allocation, Merton's other work and that of subsequent sociologists have explored a variety of ways in which the scientific reward system and credit serve key elements in the institutional life of scientists. For example, in shaping the career trajectories of scientists (most especially tenure), credit was traditionally allocated through processes that take place within a small "inner circle" of scientists who adjudicate claims for priority and, therefore, credit on the basis of close personal relationships (Crane 1969). Beyond credit allocation within closed social contexts, Hägstrom (1969) recognized that citations to prior publications by colleagues also provided an additional reward to researchers as part of an exchange relationship, whereby credit and recognition are placed at the center of a system for knowledge disclosure with information provided in return for credit in the form of citations (see Murray [2010] for an exploration of this process in the context of patent rights). Cole and Cole (1973) further elaborated our understanding of credit and rewards by considering the different types of rewards scientists' accrue: professorships in leading departments, honorific titles, and wide citation being among the most salient.

Most recently, historians of science working in these institutional traditions have sought to use the idea of credit as a way of explaining key historical events in the scientific community. Perhaps the best example are the lengths to which Galileo went to ensure that his novel telescope (and his unique access to its design and production) became a means of receiving both financial and reputational credit from a variety of patrons across Europe, carefully balancing his maintaining control of the telescope to garner further credit with sharing with others so as to ensure that his scientific claims could be validated and thus given appropriate credit (Biagioli 2003, 2006).

Dasgupta and David (1994) can be credited with incorporating this line of thinking from the sociology of science into an economic framework. Their approach has been to examine the reward system as a central element in science and argue that "an individual's reputation for 'contributions' acknowledged within their collegiate reference groups is the fundamental 'currency' in the reward structure that governs the community of academic scientists" (498). This reference to credit as currency highlights the notion of credit and currencies of credit as central both to the norms of science and to its economic foundations. From the perspective of a social planner, the importance of priority in credit speeds up discovery along research lines and ensures their disclosure. From the perspective of a scientist organizing to pursue a given research line, issues of credit allocation shape their organizational choices. However, what remains to be understood, and what serves as the central focus of the remainder of this chapter, is the way in which credit and the changing nature of credit more precisely and more generally shapes the organization of science.

The notion that credit is a critical factor driving the organization of science is in counterpoint to prior approaches taken in the academic literature. Traditionally, scholars have examined two main determinants of the organization of research: the technology of knowledge production and the burden of knowledge. With regard to the influence of technological change, a significant body of knowledge has argued for the important (albeit complex) role of new technologies in facilitating the pursuit of scientific progress (see Mokyr 2002, 2010). From Boyle's air pump (Shapin and Schaffer 1985) to Volta's pile (Pancaldi 2005), new technologies have enabled scientists to pursue more complex and distinctive research lines.

In the post–Vannevar Bush period, technology has been of particular importance in areas such as biology and physics (Knorr-Cetina 1999). In biology, the invention and automation of the polymerase chain reaction (PCR), DNA sequencing and DNA synthesis have, among other technologies, opened a wealth of new biological research lines and, at least in part, been the driving force behind new modes of organization (Huang and Murray 2009). Likewise in physics, the development of new, more powerful telescopes and massive particle colliders (each with their attendant computing power) have enabled the exploration of new knowledge frontiers, while at the same time changing the lives of physicists and their ways of collaborating and organizing of research (Galison 1997). Beyond the specific technologies of knowledge production, recent work (e.g., Agrawal and Goldfarb 2008; Ding et al. 2010) has highlighted the ways in which the coming of the Internet shaped the organization of research and the extent and nature of collaboration versus integration. In particular, data (from engineering research) show that faculty in middle-ranking universities have seen the greatest organizational change, becoming more likely to be engaged in top-tier collaborations than prior to Bitnet introduction.

An alternative, or perhaps more accurately a complementary, perspective on the changing organization of science is articulated by Ben Jones who outlined the importance of the burden of knowledge on a researcher's organizational choices (Jones, Wuchty, and Uzzi 2008; Jones 2009). His line of argument focuses on the growing length of scientific training as scientists seek to accumulate an ever-growing body of knowledge in order to make contributions at the frontier. As a corollary to the increasing requirement for training, scientists are accordingly becoming narrower in their expertise and more highly specialized—an effect he refers to as the death of the Renaissance man (Jones 2009). According to the burden of knowledge argument, the combined need for more and more specialized knowledge leads researchers into pursuing their chosen research lines through higher levels of collaboration. In support of this perspective are data on the rise in the number of authors on scientific publication across all fields (Wuchty, Jones, and Uzzi 2007).

Our argument is that changing the technology and specialization costs (and benefits) of scientific knowledge production takes us only part way toward understanding the organizational choices of researchers at any given moment in time. Moreover, it fails to account for the fact that, while technology and specialization has surely changed over the past fifty years, so too has the nature of credit. While less observable in laboratories than the changing equipment and less immediate than the growing shoulders of giants upon which all scholars must stand, credit too has likely changed. In particular, with the expansion of the scientific community (both within the United States and more globally), simple informal networks and scientific inner circles are less likely to be effective at adjudicating questions of credit. Likewise, the observable rise in collaboration challenges traditional institutions of credit and its allocation. Thus, credit becomes central to the calculus of academic scientists. In what follows, we offer a limited "credit history" of the past fifty years as a window into the changing credit allocation process and the exchange rates in the currencies of credit.

4.3 Credit History

The intangible nature of credit and its allocation makes it challenging to trace and demonstrate. To overcome this invisibility, we develop a short history of the debates around credit and the scientific reward system as told by scientists themselves in the pages of their journals: a window into a narrative we refer to as a credit history. This history is basically gleaned from the issues that animated the editorial pages of major research journals—*Science* and *Nature* as well as some medical journals, and the *Chronicle of Higher Education* (CHE). While not comprehensive, our explication of the credit arguments that animated scientists serve as a window into the challenges that they confront as they wrestle with their autonomous credit system.

Two debates of particular import can be traced through the historical record that are salient to the link between credit and the organization of scientific knowledge production: first, authorship conventions (including ordering and ghost authorship), which link to trade-offs around credit and collaboration; second, salami slicing, which speaks to the role of credit in choices of integration versus citation markets.

4.3.1 Authorship Conventions and Credit

The most important debates that animate scientists as they consider the role of credit are those that explicitly link authorship and credit allocation

(Cawkell 1976). Gaeta[1] (1999) coined the term authorship "law and order" to connote not only the rules of authorship, that is, the "law," but also the specific role of ordering of authorship (see Gans and Murray [2013] for a more comprehensive theoretical treatment of author ordering and credit and Engers et al. [1999] for a theoretical treatment). At its core the debate raises the possibility that while some genuine changes in collaboration may be taking place (see Price and Beaver 1966) driven by specialization, thus accounting for a rise in average authorship, gratuitous authorship may also be increasing, particularly from the 1970s onward (Alberts and Shine 1994).

To highlight this possibility, Broad provides an example in his 1981 article on the topic. He notes: "The fellowship application for the American College of Physicians asks a candidate to list percent participation in studies in which he is a listed author. Though seemingly a workable solution, the accuracy of the resulting judgments has been called into question. In at least one instance, when a whole research team applied for fellowships, their total participation came to 300 percent" (Broad 1981, 1138).

Changing authorship norms (specifically norms adding further authors) places an increased burden on the reward system of science, particularly in the evaluation of young faculty at key career milestones. Over a fifty-year period, when the number of researchers (and their specialization) has increased, the evaluation of individual biographies (i.e., published contributions) has grown increasingly complex. Even in 1981, an editor of the *New England Journal of Medicine* noted that "You have to know the journals, and what impact they have. . . . You have to know the institutions, the people, the meetings. . . . It's a ticklish matter" (quoted in Broad [1981], 1139). Today, evaluative choices for promotion, tenure, grant making, and a wealth of other forms of scientific credit rely on publishing records, even while those records are increasingly murky and hard to interpret (see Simcoe and Waguespack [2011] for an analysis of the impact of missing authors). Likewise, scientists themselves must make organizational choices over collaboration, integration, or publishing in the shadow of a complex credit allocation process: one that is beset with indeterminacy over credit and authorship norms (Häussler and Sauermann 2013).

In responding to this debate, a number of scientific journals have taken the lead in asking authors to carefully document their contributions to scholarly publications. Most notably, on New Year's Day 2010, Bruce Alberts, editor-in-chief at *Science* magazine, published an editorial promoting scientific standards and focusing on authorship issues. In it, he described a change in policy to discourage "honorary authorship" in which:

> before acceptance, each author will be required to identify his or her contribution to the research (see www.sciencemag.org/about/authors). Sci-

1. http://www.ncbi.nlm.nih.gov/pubmed/10230981.

> ence's policy is specifically designed to support the authorship requirements presented in *On Being a Scientist: Third Edition*, published by the US National Academy of Sciences. That report emphasizes the importance of an intellectual contribution for authorship and states that "Just providing the laboratory space for a project or furnishing a sample used in the research is not sufficient to be included as an author." (Alberts 2010, 12)

This view was echoed in a 2012 column in *Nature*, in which the author argued that "[w]hen it comes to apportioning credit, science could learn from the movies" (Frische 2012, 475).

In other instances, faculty themselves have developed internal "lab norms" to adjudicate authorship claims. As recently described in *Science*, Harvard psychology professor Stephen Kosslyn has developed his own points system, which he describes in detail on the website for his laboratory. "Anyone who works with him on a project that results in a paper can earn up to 1,000 points, based on the extent of their contribution to six different phases of the project: idea, design, implementation, conducting the experiment, data analysis, and writing. The first and last phases—idea and writing—get the most weight. Those who make a certain cut-off are granted authorship, and their score determines their order on the list" (Venkatramen 2010). While this example is unusual in its specificity, local norms at the field level are continually evolving and fields such as high energy physics have moved to norms that rely on alphabetical ordering of the many hundreds of authors that are often part of a paper that relies on massively costly, centralized technology for the production of knowledge.

Not only an issue for credit, authorship conventions—rather than authorship that simply reflects changes in underlying research organization—also raise questions of responsibility and liability for research findings and for the potential "false science," including fraud (see Furman, Jensen, and Murray [2012] and Azoulay et al. [2013] for a broader analysis of retractions in this context). To combat this challenge and the liabilities (as well as the credit) that arises with publishing, *Science*, in the same editorial noted above, also outlines a policy in which senior authors record that they have personally examined original data and attest to its appropriate presentation (Alberts 2010).

4.3.2 Salami Slicing and Credit

A second major theme that emerges in our credit history takes the colourful label "salami slicing." It describes an ongoing debate regarding the "size" of the least publishable unit (LPU) along a research line. This debate emphasizes the organizational choices between integration (leading to a larger published slice) on the one hand and publication (of a smaller slice of the research line) with follow-on citation by other researchers (or by self-citation by the research team). The question posed by scientists is whether or not to publish the small step embodied in project 1 and wait for citation by another researcher pursuing project 2 (or pursing self-citation) or to complete projects 1 and 2 before publishing a larger slice of research, thus making a larger contribution.

There is clear statistical evidence from the 1970s onward to support the claim that publication length, at least in the biological and physical sciences, is shortening (although Card and DellaVigna [2013] show evidence that in contrast, publications in economics are longer). While not conclusive evidence of the rise of LPUs, anecdotal evidence supports the publishing dilemma of young faculty and the link between LPUs and credit. One dean of science described the dilemma in an article in CHE as follows: "In order to appear to have more publications on their CVs, young scholars are often advised to break their research down into pieces and publish those pieces in multiple articles—i.e., LPUs. . . . Having a couple of LPUs will ensure that the bean counters cannot assail her record. We both know that there are those among us who would easily ignore her aggressive pursuit of grants and a single brilliant paper in *Cell* if her four years here did not include the magic two papers."[2]

Far from being a new issue, discussions over LPUs and the link between publication strategies and credit can be traced back through the editorial pages of *Science* at least to 1981 (Broad 1981). In a provocative article, the careers of young scientists in the 1980s—who typically had between fifty and one hundred publications at the time of promotion—were contrasted with scientists from the late 1950s such as James Watson who, when being evaluated by peers had only eighteen papers (albeit one that described the structure of DNA). Broad notes the emergence of the LPU and argues that "the increases stem not from a sharp rise in productivity but rather from changes in the way people publish. Coauthorship is on the rise, as is multiple publication of the same data" (1137). He also notes the challenge for credit allocation arguing that, in combination, LPU practices and changing coauthorship obfuscate the effort made by young scholars, making evaluation and credit much more challenging.

More recently, in 2005, the journal *Nature Materials* explored the impact on the sustainability of scientific publishing of what it referred to as "fragmenting single coherent bodies of research into as many publications as possible—the practice of scientific salami slicing."[3] Scientists also speculate that salami slicing potentially leads to a much greater likelihood that publications will be plagiarized (at least in part), be overlapping, or in other ways cross the boundary into false science. It also has important implications for the effectiveness and capacity of scientists to engage in meaningful peer review and credit allocation. As the editors of *Nature Materials* outlined, poor practices associated with salami slicing "deny referees and editors the

2. http://chronicle.com/article/In-Defense-of-the-Least/44761.
3. http://www.nature.com/nmat/journal/v4/n1/full/nmat1305.html.

opportunity of assessing the true extent of its contribution to the broader body of research" raising the question of credit allocation for researchers selecting between integration and publishing.

While incomplete in their coverage, these two examples of credit history highlight both the central importance and the critical challenges associated with the role of credit in the organization of science. Far from being a wrinkle in the changing tapestry of science, scientists are making a range of organizational choices in the shadow of the complex and changing beliefs about the nature and meaning of scientific credit. In what follows, we bring clarity to these issues by developing a formal model that links credit with technology and specialization.

4.4 Formal Model

The combined elements of technology, knowledge burden, and credit institutions together shape the observable outputs of science—the number of publications, the number of authors, the rate of progress and its direction. More importantly, they inform the underlying organization of science: decisions made by scientists about "laboratory life" as it pertains to any particular research line the laboratory is pursuing. With this in mind, we provide a formal model of the drivers of the (optimal) choice of organizational form for cumulative science and, in particular, how this choice is driven by institutions to allocate credit to individual scientists for their role in knowledge production.

4.4.1 Basic Set-Up

We consider an environment whereby knowledge is created by cumulative scientific research. Specifically, we focus on a pioneer scientist's (P) decisions with respect to an initial scientific project, 1. Following Green and Scotchmer (1995) (and also Bresnahan 2011), we assume that the (opportunity) cost to the scientist of pursuing 1 is c_1. The stand-alone (expected) quality of project 1 is x.[4]

A follow-on project that builds on 1, project 2, may also be possible and can be conducted by P or another follow-on scientist, F. For a scientist, i, with in-depth knowledge of project 1, the probability that they perceive the project 2 opportunity is p_i. To acquire the necessary in-depth knowledge of project 1 costs a scientist, C_i, so long as they have access to project 1's knowledge in the first place. The idea here is that, while project 1 knowledge may be disclosed (say through publication or communication), understanding it

4. It is useful to note that the publication and associated citation plays a similar role to ex post licensing in the Green and Scotchmer (1995) model except that the key parameters are market determined rather than determined through bilateral negotiation. That said, Green and Scotchmer (1995) do bring some of those factors into play when they consider how a planner might set patent length as well as antitrust policy.

in a way that leads to follow-on research takes additional effort (for instance, by undertaking replication studies). That said, an alternative interpretation of C_i is as a communication cost. If project 2 is possible, it comes with an expected quality, y, and research (opportunity) cost, c_{2i}.

As noted in our discussion above, there are three ways research into projects 1 and 2 can be organized. First, under *integration*, P conducts both projects before publishing the results under sole authorship. Second, under *collaboration*, P collaborates with another scientist, F, over both projects. In this situation, each focuses on aspects of the project they can do at least cost but pool their skills and communication in understanding the implications of project 1 for the project 2 opportunity. Third, under *publication*, P publishes project 1 results and then F conducts research into project 2, citing back to P's initial contribution. Under both collaboration and publication, the market awards P and F with attribution regarding each scientist's contributions. A key focus here will be on how that attribution takes place.

Integration. In this option, P pursues both projects. Of key importance is that the entire quality of research, should it take place, is attributed to P. Thus, P's expected payoff is:

$$v_P^{Int} = \max[x - c_1 + \max[p_F(y - c_{2P}) - C_P, 0], 0]. \tag{1}$$

Note that it is entirely possible that project 1 has no stand-alone value (i.e., $x = 0$) and its value rests solely on its ability to lead to research in project 2.

Collaboration. Under collaboration, P identifies F ex ante, and they choose to pursue both projects jointly.[5] The first consequence of this is that the costs of understanding the implications of project 1 for project 2 are shared across scientists. To this end, we assume that these costs are C_{PF} and can be allocated to P and F through internal bargaining, with P's share being s. Similarly, we assume that the consequent probability that project 2's opportunity is perceived is p_{PF}.

The second consequence of collaboration is that coauthorship is formally given to both P and F on projects 1 and 2. Of course, one can imagine a scenario whereby this is only done with respect to project 2 but, as explained below, this does not necessarily lead to different conclusions regarding whether collaboration is chosen. If both projects are successful, the research quality of their collaborative effort is $x + y$. However, the market—comprised of scientific peers—will award each with personal attribution of that output. We assume that the attribution going to scientist i is α_i rather than simply equal sharing on the basis that the market may have some reason to assign differential weights to each scientist. Otherwise we assume that attribution has to be the same in equilibrium; that is, $\alpha_P = \alpha_F = \alpha$. Importantly, we make no assumption that $\alpha_P + \alpha_F = 1$. Indeed, (as observed in the example

5. There is an issue associated with whether F can be simply identified or not. As we note below, publication can work without this condition.

from medical research above) credit could be greater than 1, although we assume that the total quality from the projects can be no greater than $x + y$.

Under collaboration, the expected payoffs to each scientist are:

$$v_P^{Col} = \max[\alpha_P x - c_1 + \max[p_{PF}\alpha_P y - sC_{PF}, 0], 0], \tag{2}$$

$$v_F^{Col} = \max[\alpha_F x + \max[p_{PF}(\alpha_F y - c_{2F}) - (1 - s)C_{PF}, 0], 0]. \tag{3}$$

This reflects the notion that P has the lowest cost associated with conducting project 1 and that P and F choose the scientist with the lowest cost to conduct project 2. The allocation of the costs, C_{PF}, is assumed to be determined internally. To keep things simple, it will be assumed that all of the internal bargaining rests with P and so s is the minimal amount (if it exists) that will ensure that F collaborates.

To see what s will be, let us assume that it is jointly profitable for project 1 to be investigated and, individually, profitable for project 2 to proceed (i.e., $\alpha_F y \geq c_{2F}$). In this case, from equation (3), the minimal s that allows F to earn a positive return is:

$$\hat{s} = 1 - \frac{\alpha_F x + p_{PF}(\alpha_F y - c_{2F})}{C_{PF}}. \tag{4}$$

Thus, for P, its expected return is:

$$v_P^{Col} = (\alpha_P + \alpha_F)(x + p_{PF}y) - c_1 - C_{PF} - p_{PF}c_{2F}. \tag{5}$$

Note that the total surplus from collaboration is:

$$\max[x - c_1 + \max[p_{PF}(y - c_{2F}) - C_{PF}, 0], 0]. \tag{6}$$

Importantly, while the market can potentially award P and F a "free lunch" if $\alpha_P + \alpha_F > 1$, total surplus only involves the "real" variables.

Publication. The third organizational option for cumulative science is for P to research and publish the results of project 1 and then for another scientist, F, to investigate this project outcome and (potentially) perceive and research project 2. For F, should P publish their research from project 1, they will have a choice as to whether to conduct an in-depth investigation of that research and, if that provides an opportunity, research and publish project 2. It is assumed that if project 2 is published that it includes a citation to P's research in project 1. The market will then partially attribute credit for some of project 2 to P as a share β_P of y and attribute β_F of y to F.

Given this, F's expected payoff following a publication by P is:

$$v_F^{Pub} = \max[p_F(\beta_F y - c_{2F}) - C_F, 0]. \tag{7}$$

If F's expected payoff is negative, we assume here that publication is infeasible as, if they are given the choice, P would prefer integration to publication. However, if F's expected payoff is positive and research into project 2 goes ahead, P's expected payoff is:

Table 4.1 **Value attributed to each scientist**

	P	F
Integration	$1(x + y)$	0
Collaboration	$\alpha_P(x + y)$	$\alpha_F(x + y)$
Publication	$x + \beta_P y$	$\beta_F y$

$$v_P^{Pub} = \max[x - c_1 + p_F\beta_P y, 0]. \tag{8}$$

In this case, social surplus from publication is:

$$\max[x - c_1 + p_F(y - c_{2F}) - C_F, 0]. \tag{9}$$

Again, we assume that even if $\beta_P + \beta_F \neq 1$ the social surplus from project 2 if it is successful is y.

Table 4.1 summarizes the value attributed to each scientist under each organizational mode.

4.4.2 Equilibrium Choices

We now turn to consider P's organizational choice for research. While the specification above allows for the possibility that under some organizational forms both stages of research will be completed while under others one or neither will be completed, this model can be more easily exposited if we assume that, regardless of organizational mode, both stages of research are completed.

To that end we assume the following:

$$x - c_1 + p_F(y - c_{2P}) \geq C_P \tag{A1}$$

(both projects are undertaken under integration);

$$(\alpha_P + \alpha_F)(x + p_{PF}y) - c_1 - c_{2F} \geq C_{PF} \tag{A2}$$

(both projects are undertaken under collaboration); and

$$x - c_1 + p_F\beta_P y \geq 0 \quad \text{and} \quad p_F(\beta_F y - c_{2F}) \geq C_F \tag{A3}$$

(both projects are undertaken under publication).

Thus,

$$v_P^{Int} = x - c_1 + p_P(y - c_{2P}) - C_P \tag{10}$$

$$v_p^{Col} = (\alpha_P + \alpha_F)(x + p_{PF}y) - c_1 - C_{PF} - p_{PF}c_{2F} \tag{11}$$

$$v_P^{Pub} = x - c_1 + p_F\beta_P y \tag{12}$$

$$v_F^{Pub} = p_F(\beta_F y - c_{2F}) - C_F. \tag{13}$$

In addition, we assume that:

$$\text{(A4)} \qquad c_{2F} \leq c_{2P}.$$

That is, F has a comparative advantage in conducting stage 2 (e.g., P has a higher opportunity cost of their time). Under these assumptions, P will choose $\max\{v_P^{Int}, v_P^{Col}, v_P^{Pub}\}$. It is instructive to consider the pairwise choices between these organizational forms.

Collaboration versus Integration. Collaboration will be chosen by P if:

$$\text{(14)} \qquad v_P^{Col} \geq v_P^{Int}$$
$$\Rightarrow (\alpha_P + \alpha_F - 1)x + ((\alpha_P + \alpha_F)p_{PF} - p_P)y$$
$$\geq p_{PF}c_{2F} - p_P c_{2P} + C_{PF} - C_P.$$

By contrast, collaboration is socially superior to integration if:

$$\text{(15)} \qquad x + p_{PF}(y - c_{2F}) - C_{PF} \geq x - c_1 + p_P(y - c_{2P}) - C_P$$
$$\Rightarrow (p_{PF} - p_P)y$$
$$\geq p_{PF}c_{2F} - p_P c_{2P} + C_{PF} - C_P.$$

It is clear that in our modeling set up, the social choice and P's choice will coincide if and only if $\alpha_P + \alpha_F = 1$, a result that sharply contrasts to scientists' own discussions and informal evidence which, as we outlined, suggest that many scientists believe that $\alpha_P + \alpha_F > 1$. Based on our model, if collaboration allows knowledge transfer costs (C_{PF}) to be shared between both scientists, overweighting the collaboration rewards would encourage over-collaboration.

Publication versus Integration. Publication will be chosen by P if:

$$\text{(16)} \qquad v_P^{Pub} \geq v_P^{Int} \Rightarrow p_F\beta_P y \geq p_P(y - c_{2P}) - C_P.$$

In this case, publication is socially preferable to integration if:

$$\text{(17)} \qquad x - c_1 + p_F(y - c_{2F}) - C_F \geq x - c_1 + p_P(y - c_{2P}) - C_P$$
$$\Rightarrow (p_P - p_F)y + p_F c_{2F} + C_F$$
$$\leq p_P c_{2P} + C_P.$$

Note, however, that because we assume that, under publication, F will choose to conduct project 2, $p_F\beta_F y \geq p_F c_{2F} + C_F$. Thus, if publication is chosen by P we know that:

$$\text{(18)} \quad (p_P - p_F\beta_P)y - p_P c_{2P} - C_P \leq p_F\beta_F y - p_F c_{2F} - C_F$$
$$\Rightarrow (p_P - p_F(\beta_P + \beta_F))y + p_F c_{2F} + C_F$$
$$\leq p_P c_{2P} + C_P.$$

This is a necessary condition for publication to be chosen by P. Thus, if $\beta_P + \beta_F \leq 1$, if publication is chosen in equilibrium then it is socially preferable to integration. However, if $\beta_P + \beta_F > 1$, it is possible that publication will be chosen in equilibrium when it is not socially preferable to integration. Specifically, equation (18) may hold when equation (17) does not.

Publication versus Collaboration. Publication will be chosen by P over collaboration if:

$$(19) \qquad v_P^{Pub} \geq v_P^{Col}$$
$$\Rightarrow (1 - \alpha_P - \alpha_F)x + ((\alpha_P + \alpha_F)p_{PF} - p_F\beta_P)y$$
$$\leq p_{PF}c_{2F} + C_{PF}.$$

In this case, publication is socially preferable to collaboration if:

$$(20) \qquad (p_{PF} - p_F)y + p_F c_{2F} + C_F \leq p_{PF}c_{2F} + C_{PF}.$$

As above recall that, $p_F\beta_F y \geq p_F c_{2F} + C_F$. Thus, if publication is chosen in equilibrium by P, then

$$(21) \quad (1 - \alpha_P - \alpha_F)x + (\alpha_P + \alpha_F)p_{PF}y - p_F\beta_P y \leq p_{PF}c_{2F}$$
$$+ C_F \Rightarrow (1 - \alpha_P - \alpha_F)x + (\alpha_P + \alpha_F)p_{PF}y$$
$$- p_F\beta_P y \underbrace{- p_F\beta_F y + p_F c_{2F} + C_F}_{<0} \leq p_{PF}c_{2F} + C_F.$$

This is a necessary condition for publication to be chosen by P. Thus, if $\alpha_F + \alpha_P = 1$ and $\beta_P + \beta_F \leq 1$ then, if publication is chosen by P, it will also be socially preferred to collaboration. However, if $\beta_P + \beta_F > 1$, then it is possible that publication will be chosen in equilibrium when it is not socially preferable to collaboration. Specifically, equation (21) may hold even when equation (20) does not hold.

4.4.3 Optimal Attribution

The above analysis suggests that setting $\alpha_F + \alpha_P = 1$ and $\beta_P + \beta_F = 1$ may have some desirable properties. However, it also demonstrated that inefficient outcomes can arise involving each of the three evaluated organizational choices. Consequently, we consider what levels of $(\alpha_F, \alpha_P, \beta_F, \beta_P)$ might generate an efficient outcome, the idea being to imagine that these parameters were chosen by a planner and to evaluate their properties exploring how this is in accordance (or discordance) with what actually happens in science. To this end, we follow Green and Scotchmer (1995) considering situations where the follow-on scientist, F, may earn no surplus as a convenient means of avoiding having to deal with the potential range of parameters that may give rise to an efficient outcome. The idea here being that since P's choice determines the outcome, it makes sense to ensure that as much of the surplus goes to P as possible.

PROPOSITION 1. *There exists* $(\alpha_P, \alpha_F, \beta_P, \beta_F)$ *such that* $\alpha_F + \alpha_P = 1$ *and* $\beta_P + \beta_F = 1$ *that results in an equilibrium choice for P that is efficient.*

The proposition is easily proved by finding the $\hat{\beta}_F$ such that $v_F^{Pub} = 0$ and letting $\hat{\beta}_P = 1 - \hat{\beta}_F$ and substituting $\alpha_F + \alpha_P = 1$ so that:

$$v_P^{Col} \geq v_P^{Int} \Rightarrow (p_{PF} - p_P)y \geq p_{PF}c_{2F} - p_P c_{2P} + C_{PF} - C_P$$

$$v_P^{Pub} \geq v_P^{Int} \Rightarrow (p_P - p_F)y + p_F c_{2F} + C_F \leq p_P c_{2P} + C_P$$

$$v_P^{Pub} \geq v_P^{Col} \Rightarrow (p_{PF} - p_F)y + p_F c_{2F} + C_F \leq p_{PF}c_{2F} + C_{PF}.$$

These are identical to conditions in equations (15), (17), and (20). This demonstrates that the choices among each of the organizational forms are driven by the same conditions as the socially optimal choices.

Specifically, note that:

$$(\hat{\beta}_P, \hat{\beta}_F) = \left(\frac{p_F(y - c_{2F}) - C_F}{p_F y}, \frac{p_F c_{2F} + C_F}{p_F y}\right), \tag{22}$$

such that so long as they sum to 1 it is arbitrary what α_F and α_P are. The reason for this is quite intuitive: with collaboration, we allowed P and F to negotiate cost sharing, but given the structure this allowed them to transfer utility. Thus, the decision was driven wholly by the joint market reward to collaboration rather than the precise division. No such instrument existed for publication and hence, the market rewards needed to determine division as well as overall value in order to generate a socially optimal outcome.

It is useful to consider how Proposition 1 might change if, in fact, s (the share of costs accruing to P under collaboration) was fixed and nonnegotiable.

PROPOSITION 2. *When s is fixed, there exists* $(\alpha_P, \alpha_F, \beta_P, \beta_F)$ *such that* $\alpha_F + \alpha_P = 1$ *and* $\beta_P + \beta_F = 1$ *that results in an equilibrium choice for P that is efficient.*

The proof proceeds using the same method as Proposition 1. In this case, the range of β_i remains as in equation (22). By contrast, the market weights for collaboration become:

$$(\hat{\alpha}_P, \hat{\alpha}_F) = \left(\frac{x + p_{PF}(y - c_{2F}) - (1 - s)C_{PF}}{x + p_{PF}y}, \frac{p_{PF}c_{2F} + (1 - s)C_{PF}}{x + p_{PF}y}\right). \tag{23}$$

In this case, it can easily be demonstrated that the payoffs realized are the same as in Proposition 1.

4.4.4 A Note on Social Surplus

Thus far we have been somewhat loose in our consideration of socially optimal outcomes. The propositions focus on efficiency, which is the expected difference between research quality realized and the costs incurred under the chosen organizational form. However, this is distinct from social efficiency that would take into account the broader impact of the research. For a very significant medical breakthrough, for example, the social surplus from that research may vastly exceed the assessed quality of that research accruing to scientists. In this case, the costs realized by these scientists would be very small relative to social outcomes. Thus, to generate a socially efficient outcome would require giving scientists kudos well above 1 even for integrated research lines.

Given this, how should we interpret Propositions 1 and 2? The first reasonable assumption is that a research paper has an assessed quality that is independent of the organizational form of the team that generated it. This is what the parameters x and y capture. As a result, we interpret $x + y$ as the maximum kudos that can be given to a research line that is sole authored. Given this, the shares, $(\alpha_P, \alpha_F, \beta_P, \beta_F)$, represent the relative shares given to collaboration or publication compared with that for a sole-authored paper. Thus, Propositions 1 and 2 ask if it is efficient to give collaboration or publication a different weight to integrated paper kudos that is fixed. We find that such a distinct weight is not warranted. However, that is a distinct weight relative to the level of kudos under integration. If that kudos could be aligned with social value, then that could generate a socially optimal outcome. For the moment, an interpretation as a relative weight is as far as we can go here.

4.5 Some Implications

Having constructed a model of organizational choice for cumulative science, we now turn to consider a number of issues raised in our credit history and discuss what insights the model gives us into how these may be reasonably resolved. We must emphasize that this analysis is suggestive rather than conclusive. Specifically, we do not know what determines the allocation of credit in science. Our formal model tells us what that allocation might look like if it was indeed efficient but, in fact, the processes by which these actually arise have likely been changing over time and are subject to various informational limitations that will lead to allocation being an inferred outcome rather than a precise one.

4.5.1 Collaborative Bias

The first element of our credit history explored the relationship between credit and decisions to collaborate. In particular, we illustrated the concerns scientists have expressed over rising collaboration and the attendant difficulties in credit allocation. This is particularly troublesome if there are trends that lead to overcollaboration. Specifically, it has been claimed that the market weights on a collaborative research project are greater than the weights that would arise if that project were not collaborative. This goes beyond the potential higher quality of such projects to whether, in fact, the market does and should divide the quality of collaborative projects by the number of authors when assigning attribution of credit. In other words, what are the implications of an inflated assessment of $\alpha_P + \alpha_F$ on the overall organizational form chosen?

As it turns out, if $\alpha_P + \alpha_F$ is set exogenously (assumed here to be > 1), then only one market weight remains to be determined: β_P. This is because β_F is determined as the value that leaves $v_P^{Pub} = 0$ and so is unchanged from Propositions 1 and 2. The issue becomes that β_P must do two things. It must continue to balance *P*'s incentives with respect to publication versus integration. And it must now rebalance *P*'s incentives with respect to publication and collaboration. It is easy to see that that latter task means that the optimal attribution, $\hat{\beta}_P$, will lie above the levels in Propositions 1 and 2. Thus, as a result of a bias to market weights on collaboration, not only will we observe socially suboptimal overcollaboration, but also overpublication as well. The point here is that these decisions interact and so any analysis of patterns must take into account all of the organizational form options facing scientists.

4.5.2 Blurred Contributions and Salami Slicing

Both with regard to the link between collaboration and credit, and in considering salami slicing, scientists have argued that a major challenge lies in the increasingly blurred relationship between actual observable effort in knowledge production and credit allocation. Among other things, this blurring arises because of the rise and increased geographic dispersion of the scientific community. More specifically, this likely means that the roles of individual scientists in collaborative endeavors have become increasingly blurred. Propositions 1 and 2 both suggested that optimal attribution would depend on factors specific to the project but importantly specific to the scientists themselves and their roles (as pioneer and follower, respectively).

If these factors are less known in more recent science compared to that in the past, what impact might this have on the choice of organizational form for cumulative science? The challenge here is to consider whether blurring will systematically bias organizational choices. To that end, we focus here on the new prominence given to citation counts. Basically, the value of a given research output is increasingly measured by the number of citations it receives above and beyond other factors. Within the context of our model here, that means that there is a systematic increase in β_P if there is follow-on research, while there is no necessary trade-off between the level of β_P and the level of β_F.

The direct impact of this, as predicted by the model, would appear to be an increasing bias toward publication as an organizational choice at the expense of collaboration and integration that may be more efficient. In other words, this result provides a direct link between blurring of credit, a rise in the use of citations, and the move to salami slicing that has been so widely documented and discussed among scientists.

4.5.3 The Matthew Effect

A third issue that has animated scholars of science more than scientists themselves is the Matthew Effect, which, as noted earlier, argues that more famous scientists (in terms of their past achievements or positions at elite institutions) receive more credit—in citations and kudos in collaborative projects. The issue is whether such credit is proportionate to their actual contribution (which may be high for the same reasons they are famous) or disproportionate. There is, to our knowledge, no formal economics model that derives the Matthew Effect in its disproportionate form as an equilibrium phenomenon.[6] It is instructive, therefore, to discuss this in the context of the model presented here. Specifically, if the market weights to collaboration and publication are determined optimally, what does this say about the Matthew Effect?

To consider this, let us focus on the pioneer scientist (P). There are several parameters that relate to P's ability to contribute to project 2. There is p_P, the probability that P has an insight that perceives the opportunity of project 2. There is C_P, the costs associated with understanding project 1 that will generate that insight. There is c_{P2}, P's costs associated with carrying out project 2. Finally, in a situation where it is exogenous, there is s, P's contribution to insight in a collaborative venture. The only time a contributive driver for P impacts a market weight is for s, when it is exogenous in a collaborative venture. In this case, if s is higher, P will receive more of the weight in kudos associated with collaboration. However, the implication is that F will receive too *little* weight in such collaborations and so collaboration will be infeasible. This will lead to more publication/integration than is efficient. However, in both those organizational forms there is no Matthew Effect distortion realized. The conclusion here is that for a pioneer scientist, the Matthew Effect is not a clear prediction of this model.

When it comes to a famous scientist's role as a follow-on researcher (F), there is more impact. If that scientist finds it less costly to engage in project 2 research (c_{F2}), then the market weights F receives under publication and collaboration both rise. Otherwise, drivers that are specific to F only impact the weight a scientist receives under publication; specifically, the higher are p_F and C_F, the more diminished is the market kudos flowing back to P for a citation. Note here, however, that while it is often said that the Matthew Effect works to provide a famous scientist with more citations, here it is operating to deny kudos flowing back to earlier researchers.

That said, the market weights for publication and collaboration are, in reality, given by market assessments of the underlying drivers as in equations (22) and (23). If, because of fame, these assessments are distorted upward for one scientist, this may have an impact on the relative choices of organizational forms. In particular, a market bias in favor of P that is understood

6. There are, however, evolutionary and other models where the Matthew Effect arises. For instance, Price (1976), David (1994), and Simkin and Roychowdhury (2005a, 2005b, 2006, 2007). However, the issue is that when scientists expect a Matthew Effect but are interested in extracting out a signal of actual contribution, it is difficult to sustain signals with a bias in equilibrium. Of course, this may be a flaw with the standard economic approach to equilibrium rather than a statement about the plausibility of the Matthew Effect in science itself.

by both scientists may render project 2 under publication infeasible for F (as their expected surplus was zero based on real variables and with a diminished market weight it will be negative). This would rule out publication as a choice for P. In addition, unless they can internally negotiate taking into account such biases, a diminished weight for F will render their participation in collaboration infeasible. Thus, a disproportionate weight on a famous P would have the effect of pushing organizational choice outcomes toward integration. Ironically, if this were done, the Matthew Effect would not be observable at all as it only arises under nonintegrated organizational forms.

4.6 Conclusions and Future Directions

In October 2013, François Englert and Peter Higgs were awarded the Nobel Prize for what is known as the "Higgs mechanism" that fills empty space that absorbs forces that were missing from observations. Robert Brout, who collaborated with Englert, had passed away but would have otherwise shared in the prize. As it turns out, that was just the beginning. There was a set of follow-on research that took the initial idea and built up the theory. Some of this was done very quickly but also independent of the initial contributions. Then the research program was "completed" by a very large team operating the Large Hadron Collider in Switzerland. This example shows not only how credit can be allocated, but also how follow-on contributions enable the value of initial contributions. Not surprisingly, this has given rise to debate as to whether Nobel prizes should include teams of research—both formal or part of a research line. Of course, in this case, the credit went to the pioneers, or stage 1, researchers.

Our chapter suggests that the experience in particle physics is commonplace and also that how credit is allocated will impact upon whether research lines are undertaken by formal teams or through a publication mechanism. Our formal model suggests that such choices have become important and have efficiency consequences. In addition, we suggest that the allocation of credit needs to reflect the balance of incentives in the organizational choices facing scientists.

However, this is just the beginning. What we have not addressed is how credit allocation arises in reality. While one can imagine institutions that may allocate credit to foster efficiency, it is not clear how existing credit allocations mechanisms including citations, collaboration, name ordering, careers, tenure, prestige, prizes, and phenomena naming actually function and what ends they promote. In another paper (Gans and Murray 2013) we consider how a market might evaluate the contribution of individual researchers when there is "team output" and show that the processes and mechanisms for credit allocation interact with market inferences and, hence, overall efficiency. But broader themes still remain including the impact of quantitative counts of credit (i.e., citation-based metrics) and the ongoing issue of the

Matthew Effect. These theoretical and empirical developments remain a task for follow-on research and the continuation of scientific investigation that we have continued here in this chapter.

References

Aghion, P., M. Dewatripont, and J. Stein. 2008. "Academic Freedom, Private-Sector Focus and the Process of Innovation." *RAND Journal of Economics* 39 (3): 617–35.

Agrawal, A., and Avi Goldfarb. 2008. "Restructuring Research: Communication Costs and the Democratization of University Innovation." *American Economic Review* 98 (4): 1578–90.

Alberts, Bruce. 2010. "Promoting Scientific Standards." *Science* 327 (5961):12.

Alberts, Bruce, and Kenneth Shine. 1994. "Scientists and the Integrity of Research." *Science* 266 (5191): 1660–61.

Arrow, K. 1962. "Economic Welfare and the Allocation of Resources for Invention." In *The Rate and Direction of Inventive Activity*, edited by R. Nelson, 609–25. Princeton, NJ: Princeton University Press.

Azoulay, P., J. Furman, J. Krieger, and F. Murray. 2013. "Retractions." NBER Working Paper no. 18499, Cambridge, MA.

Azoulay, P., J. Graff Zivin, and G. Manzo. 2010. "Incentives and Creativity: Evidence from the Academic Life Sciences." *RAND Journal of Economics* 42 (3): 527–54.

Biagioli, M. 2003. "Rights or Rewards: Changing Frameworks of Scientific Authorship." In *Scientific Authorship: Credit and Intellectual Property in Science*, edited by Mario Biagioli and Peter Galison, 253–79. New York: Routledge.

———. 2006. *Galileo's Instruments of Credit: Telescopes, Instruments, Secrecy*. Chicago: University of Chicago Press.

Bikard, M., F. Murray, and J. S. Gans. Forthcoming. "Exploring Trade-Offs in the Organization of Scientific Work: Collaboration and Scientific Reward." *Management Science*.

Broad, W. 1981. "The Publishing Game: Getting More for Less." *Science* 211 (4487): 1137–39.

Card, David, and Stefano DellaVigna. 2013. "Nine Facts about Top Journals in Economics." *Journal of Economic Literature* 51 (1): 144–61.

Cawkell, A. E. 1976. "Citations, Obsolescence, Enduring Articles, and Multiple Authorships." *Journal of Documentation* 32 (1): 53–58.

Cole, Jonathan R., and Stephen Cole. 1973. *Social Stratification in Science.* Chicago: University of Chicago Press.

Crane, Diana. 1969. "Social Structure in a Group of Scientists: A Test of the 'Invisible College' Hypothesis." *American Sociological Review* 34:335–52.

Dasgupta, P., and P. A. David. 1994. "Towards a New Economics of Science." *Research Policy* 23:487–521.

David, P. A. 1994. "Positive Feedbacks and Research Productivity in Science: Reopening Another Black Box." In *Economics and Technology*, edited by O. Grandstrand. Amsterdam: Elsevier.

———. 2008. "The Historical Origins of 'Open Science': An Essay on Patronage, Reputation and Common Agency Contracting in the Scientific Revolution." *Capitalism and Society* 3 (2): article 5. doi:10.2202/1932-0213.1040.

Ding, W. W., S. G. Levin, P. E. Stephan, and A. E. Winkler. 2010. "The Impact of Information Technology on Academic Scientists' Productivity and Collaboration Patterns." *Management Science* 56 (9): 1439–61.
Engers, M., J. S. Gans, S. Grant, and S. P. King. 1999. "First Author Conditions." *Journal of Political Economy* 107 (4): 859–83.
Frische, Sebastian. 2012. "It Is Time for Full Disclosure of Author Contributions." *Nature* 489 (7417): 475.
Furman, J., K. Jensen, and F. Murray. 2012. "Governing Knowledge Production in the Scientific Community: Quantifying the Impact of Retractions." *Research Policy* 41:276–90.
Furman, J., and S. Stern. 2011. "Climbing atop the Shoulders of Giants: The Impact of Institutions on Cumulative Research." *American Economic Review* 101 (5): 1933–63.
Gaeta, T. J. 1999. "Authorship: 'Law' and Order." *Academic Emergency Medicine* 6 (4): 297–301.
Galison, Peter. 1997. *Image and Logic: A Material Culture of Microphysics*. Chicago: Chicago University Press.
Gans, J. S., and F. Murray. 2013. "Markets for Scientific Attribution." Unpublished Manuscript, Massachusetts Institute of Technology.
Green, J., and S. Scotchmer. 1995. "On the Division of Profit in Sequential Innovation." *RAND Journal of Economics* 26 (1): 20–33.
Hagström, Warren O. 1965. *The Scientific Community*. New York: Basic Books.
Häussler, C., and H. Sauermann. 2013. "Credit Where Credit is Due? The Impact of Project Contribution and Social Factors on Authorship and Inventorship." *Research Policy* 42 (3): 688–703.
Huang, Kenneth, and F. Murray. 2009. "Does Patent Strategy Shape the Long-Run Supply of Public Knowledge? Evidence from the Human Genome." *Academy of Management Journal* 52 (6): 1193–221.
Jones, B. 2009. "The Burden of Knowledge and the 'Death of the Renaissance Man': Is Innovation Getting Harder?" *Review of Economic Studies* 76 (1): 283–317.
Jones, B., S. Wuchty, and B. Uzzi. 2008. "Multi-University Research Teams: Shifting Impact, Geography, and Stratification in Science." *Science* 322 (5905): 1259.
Knorr-Cetina, K. 1999. *Epistemic Cultures: How the Sciences Make Knowledge*. Cambridge, MA: Harvard University Press.
Merton, Robert. 1957. "Priorities in Scientific Discovery." *American Sociological Review* 22 (6): 635–59.
———. 1968. "The Matthew Effect in Science: The Reward and Communication Systems of Science are Considered." *Science* 159 (3810): 56.
Mokyr, Joel. 2002. *The Gifts of Athena: Historical Origins of the Knowledge Economy*. Princeton, NJ: Princeton University Press.
———. 2010. *The Enlightened Economy: An Economic History of Britain, 1700–1850*. New Haven, CT: Yale University Press.
Murray, F. 2010. "The Oncomouse that Roared: Hybrid Exchange Strategies as a Source of Productive Tension at the Boundary of Overlapping Institutions." *American Journal of Sociology* 116 (2): 341–88.
Murray, F., P. Aghion, M. Dewatripont, J. Kolev, and S. Stern. 2009. "Of Mice and Academics: Examining the Effect of Openness on Innovation." NBER Working Paper no. 14819, Cambridge, MA.
Murray, F., and S. Stern. 2007. "Do Formal Intellectual Property Rights Hinder the Free Flow of Scientific Knowledge? An Empirical Test of the Anti-Commons Hypothesis." *Journal of Economic Behavior and Organization* 63 (4): 648–87.

Nelson, R. R. 1959. "The Simple Economics of Basic Scientific Research." *Journal of Political Economy* 67 (3): 297–306.
Pancaldi, G. 2005. *Volta: Science and Culture in the Age of Enlightenment*. Princeton, NJ: Princeton University Press.
Price, D. J. 1976. "A General Theory of Bibliometric and Other Cumulative Advantage Processes." *Journal of the American Society for Information Science* 27:292–306.
Price, D. J., and D. B. Beaver. 1966. "Collaboration in an Invisible College." *American Psychologist* 21 (11): 1011–18.
Rosenberg, N. 1990. "Why Do Firms Do Basic Research (with Their Own Money)?" *Research Policy* 19 (2): 165–74.
Shapin, Steven, and Simon Schaffer. 1985. *Leviathan and the Air-Pump: Hobbes, Boyle and the Experimental Life*. Princeton, NJ: Princeton University Press.
Simcoe, T. S., and D. M. Waguespack. 2011. "Status, Quality, and Attention: What's in a (Missing) Name?" *Management Science* 57 (2): 274–90.
Simkin, M. V., and V. P. Roychowdhury. 2005a. "Copied Citations Create Renowned Papers?" *Annals of Improbable Research* 1 (1): 24–27.
———. 2005b. "Stochastic Modeling of Citation Slips." *Scientometrics* 62:367–84.
———. 2006. "Theory of Aces: Fame by Chance or Merit?" *Journal of Mathematical Sociology* 30:33–42.
———. 2007. "A Mathematical Theory of Citing." *Journal of the American Society for Information Science and Technology* 58 (11): 1661–73.
Venkatramen, Vijaysree. 2010. "Conventions of Authorship." *Science Careers*. http://sciencecareers.sciencemag.org/career_magazine/previous_issues/articles/2010_04_16/caredit.a1000039.
Wuchty, S., B. Jones, and B. Uzzi. 2007. "The Increasing Dominance of Teams in Production of Knowledge." *Science* 316 (5827): 1036.