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Scientists as Agents

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A large proportion of the time and effort of scientists is spent in activities that have no obvious place in the traditional model of "basic science discovery leading to application in a marketable product." Some of this time is spent on basic science that does not lead to application in a marketable product; this can be assimilated, though very problematically, to the traditional model by regarding it as failed effort, or the production of a different kind of good, such as generally available scientific knowledge that can be seen as a "public good." But an overwhelming proportion of the time and effort of scientists is spent on a series of activities that fits neither the core model nor its variations: writing grant proposals, negotiating revisions of proposals, evaluating proposals; evaluating other scientists for promotions or appointments, writing and reading letters on their behalf; evaluating students and postdocs, in ways ranging from grading students in classes to making admissions and funding decisions about them; reading, as a peer-reviewer, articles, notes, abstracts, and the like submitted for publication or to conferences, and evaluating, as an editor, the comments made by referees; evaluating other scientists for prizes or awards, membership in honorific bodies; serving as a consultant and evaluating proposals, scientists, or ideas for firms; performing site visits on behalf of funding agencies, accreditation agencies, and the like.

These evaluative activities are tremendously expensive. If one calculated the time that scientists spend on them at the same rate as consulting fees, the costs would be astronomical. But they are inseparable from the way science is presently conducted. Although obviously some of these activities can be internalized to firms, and costs reduced, it is not clear that it would be possible for there to be "science" anything like the sci-

ence we are familiar with if all of these activities *were* internalized to firms producing for the market. Firms, indeed, rely on the existence of these activities in many ways, from obtaining financing to gaining acceptance for their products, and this has been the case at least since the era in the middle of the nineteenth century in which chemical fertilizers were introduced.

In this chapter I will attempt to say something about the economic character of the "evaluative" part of science, the part that is most completely hidden by usual accounting methods. Yet despite the fact that the costs are hidden, there is no question that these activities involve transactions. The transaction that occurs when a scientist agrees to spend an afternoon evaluating the work of a colleague at another university who is applying for promotion is sometimes a cash transaction: some universities pay a small honorarium for this service, and scientists are paid consulting fees by firms for professional opinions of various evaluative kinds that are substantively identical to those that they give within a university as part of their job, for "free" in connection with colleagues, or as part of their membership on an editorial board, as a journal reviewer, or for a nominal fee as part of a grant-reviewing team. So for the most part the "payments" are implicit. And we lack a coherent understanding of what goes on in these transactions: why they exist at all, what the larger functions they serve are, and what sorts of choices, markets, and competition is involved in them. These transactions, markets, and competitions are the subject of this chapter.

Getting a Grip on the Economics of Evaluation

The costs of the these "evaluative" activities are borne in peculiar ways that have the effect of hiding them: it costs about $50,000 to evaluate and editorially prepare an article for a major medical journal, including the costs of editing and the operation of the editorial office, but not including the cost in time of peer reviewing. These costs are paid through journal revenues, which are largely the result of sales to libraries, and partly through advertising and sometimes membership dues of professional associations. So libraries contribute to the support of this expensive machinery, as do peers, who donate their valuable time, which is in turn paid for by universities and research institutions. If the journal is subscribed to by practitioners, costs are borne ultimately by customers, in this case patients and the third parties that pay on the behalf of patients. The time of scientist professors is paid for by universities, and the journals are paid for out of library budgets. These costs are in turn borne by funding agencies in the form of indirect costs, by students in the form of tuition, and so forth. So the payments are indirect and much of what makes the journal valuable—the contributions of the authors, editorial board, and peer reviewers—are part of a system of implicit payments whose character is difficult to grasp in terms of standard economic models, but nevertheless is quantitatively enormous if we consider the time and effort that these activities involve.

The peculiarly indirect and complex structure of actual payments involving science makes it very difficult for economic analysis to deal directly with flows, transactions, and choices. And since science is paid for in significant part by a complex system of state subsidies, it is difficult to value it. It is even difficult to say what the portion is. The difficulties are evident in the structure of expenditures of universities themselves, which operate through an elaborate system of cross-subsidies. Tuition is paid at one price for undergraduates and for large categories, such as graduate students, without regard for differentials in actual costs of instruction. There are consequently huge cross-subsidies, most of which cannot be determined from an examination of university budgets. The internal economy of universities is itself complex and often purposely mysterious. For example, university managers construct elaborate strategies to deal with fact that there are restrictions on different kinds of funds, and that much of the management of funds consists in using funds collected under one pretext for other purposes. Donations from aged alumni, for example, rarely support daily operations and are often for things that the university could do without. But there are ways of making assets acquired with restrictions, such as buildings, produce funds that the university can employ for other purposes. Thus a dormitory donated by an alumnus can be used to extract fees that can then be used for other purposes. Similarly, of course, for tuition fees.

One of these means has a great deal to do with science: "indirect cost" payments for grants are for the most part payments for assets the university not only owns but in many cases can raise funds (and thus double bill for), such as buildings, library assets, and so on (Turner 1999c). So universities can construct strategies or simply benefit from past donations in the form of money that is unrestricted in its uses and only very indirectly connected to present costs. Indeed, in some universities, particularly state universities where there are legislative restrictions on the use of particular funds, there is more or less open trading in types of funds, such as funds that may be spent on equipment or maintenance as distinct from funds that can be spent on alcoholic beverages, whose

exchange value varies in the way that the exchange rates of different currencies vary. Needless to say, these strategies are not reflected in the "books" in any easily accessible way, and it is even very difficult to ask such apparently simple questions of managerial accounting as "which departments do we make money on and which do we lose money on?"[1] This means that it is also very difficult to estimate the total expenditures that universities make on science, and consequently the share of non-grant funding in the total cost of science, and indeed the total cost of science itself. If we are to include the costs of evaluation, the subsidization of scientific publications by library budgets, and so forth, the picture would certainly look different.

There are other ways in which the economic processes in this domain can be approached. One can begin piecemeal with studies of such things as the value of a Nobel Prize in raising money for a biotechnology venture, for example, and such studies certainly point to the profound economic significance of evaluations in science. But because of the peculiar history of science studies, there is another option: one can reinterpret prior findings and theoretical efforts from other fields, notably sociology, in economic terms, in order to get a general understanding of the processes, an understanding that can subsequently be tested against piecemeal studies. Within this strategy there are a number of different possible options, and the approach I will take here reflects a set of choices in the face of these difficulties that needs to be explained in advance.

The first choice is theoretical. There is no question of simply "applying" market models to the activities I will consider. But I do not propose, or see the need for, market analogies, such as the notion of a marketplace of ideas or such notions as public goods. The main difficulty with studying and conceptualizing the vast body of activity I have de-

1. An interesting internal study of this question at the University of Chicago done in 1971, when the university, responding to undergraduate student activism, considered converting to an entirely graduate institution, attempted to do this; the problematic character of the assumptions that the author, economist and dean David Gale Johnson, was forced to make indicate something about the difficulties here, but also point the way to the possibilities of empirical analysis. Interestingly, at this time, which in retrospect is now considered a period of prosperity for the sciences (though shortly after the beginning of a sharp decrease in the rate of growth of funding), the sciences were not money makers for the university. This hints that as competition for funding became more intense and more universities competed, the sciences required even greater subsidies, and perhaps even that the increases in tuition in the following period ultimately, through the complex cascade of cross-subsidization that is characteristic of universities, actually wound up supporting science.

scribed above results from the weakness of conventional models in cases in which the usual simplifications cannot be matched up with economically measurable quantities. The activities themselves, however, are not merely analogous to markets. They are genuinely economic activities, which have real (if almost always implicit and indirect) costs and consequences, and they are engaged in both rationally and self-consciously strategically in a setting in which choices are made that have direct consequences for who gets what and how. Choices are made by agencies that dispense and receive real funds and sign real contracts. The choices that are made greatly affect the decisions of the scientist participants in these activities about how to choose what to do, and what to invest time and effort in.

This approach contrasts sharply with some other approaches. Uskali Mäki in a recent (1997) paper comments on his disappointment with Coase's paper on the marketplace of ideas (1974), which, Mäki observes, is actually about publications such as newspapers (and their costs) rather than about ideas and knowledge.[2] As my comments suggest, I think that basic facts about the costs of activities in science such as the costs of journal articles have been ignored, perhaps because of the difficulties of understanding the web of implicit and indirect transactions involved. And I suspect that the basic facts about the costs of tangible and tangibly economic activities provides a necessary, and perhaps sufficient, place to start. I leave the construction of marketplace analogies and the kinds of analogies needed to apply notions like "public goods" to others, but my bias is that they are unneeded and, in the case of the evaluative activities I will consider here, actually serve to obscure the way in which scientists and the evaluative institutions of science create value. And in the end I think an answer to *this* question suggests a transformation of the way in which science has been understood, both by economists and by the community of science studies scholars.

My second choice has already been alluded to. My approach to the economic structure of these evaluative activities will itself be somewhat indirect. The activities can be interpreted in noneconomic terms, but the noneconomic terms point to and illuminate their economic structure. My core argument will be that there is a clear and an important element that can be understood in terms of, and resonates with, though it does

2. Actually it is about the First Amendment, and the strange contrast between attitudes toward the regulation of newspapers and similar sources of opinion and the regulation of other products.

not precisely match, some familiar economic conceptions associated with principal-agent theory. Some of the similarities are obvious. Clearly scientists act and use science and have their science used in chronic situations of information asymmetry, or to put it somewhat differently, in situations where the users are at best only partly competent to judge the validity of the science that they use, and are also characteristically not even competent to fully understand the science that they use. This situation of chronic lack of ability to fully understand makes the notion of "information," i.e., something that might not be known by one party to a transaction would be transparently understandable to them, of very problematic applicability in these settings. Nevertheless, asymmetries are obviously central to the activity of evaluation in science, and so is overcoming them.

The application of these concepts is obviously not easy even in the paradigm cases of principal-agent theory, and is substantially more difficult in the complex system of mutual monitoring and agency risk spreading that I will discuss. Thus while the general similarity between the problem of principal-agent theory and the problematic of monitoring in science is undeniable, because of the implicit character of transactions in science, there is no straightforward way to "apply" these notions. But there are both actual transactions and implicit transactions with significant actual costs and detectable pricelike variations throughout this system. For example, in some respects the incredibly expensive and time-consuming processes of evaluation discussed above are surrogates for the kinds of monitoring expenses employed in the regulation and approval of drugs by regulatory agencies, which require expensive research conducted according to standardized protocols. But in another sense, these monitoring processes are inseparable from the evaluation system of science and dependent upon it: the research must be conducted by qualified researchers, i.e., those who have subjected themselves to many of the monitoring activities discussed above by becoming Ph.D. scientists. In addition, drug companies are eager to have peer-reviewed researchers publish material related to drug testing.

To ask why this should be so is to enter directly into the complexities of concern to this chapter. A simple, but central, issue is the obsessive concern by the participants in science over pecking orders, particularly with respect to such arcane questions as the value of publishing in one journal over another or the much more extreme difference of, say, an abstract published in an obscure proceedings of a meeting and distributed and printed by a drug company. Are these obsessions irrational?

The facts of science—the "informational contents"—are literally the same in each case: nothing about the "application of basic science" or the public goods model distinguishes the kinds of publication. But drug companies and researchers invest very heavily in these distinctions, with real money and costly time and effort. Are they irrational? Or do these approaches to the economics of science simply fail to understand what these activities and choices are about?

Norms and Counternorms: A Typical Principal-Agent Structure

We may begin with a simple observation. In one of the most famous papers ever written on science, "A Note on Science and Democracy," Merton described four "norms" of modern science: universalism, disinterestedness, "communism," and organized skepticism. Merton inferred these norms, we are told, from the practice of scientists (1942; cf. 1973). Three of the four relate directly to evaluative processes, and the fourth, "communism," turns out to be even more directly relevant to the concerns of this paper.

Merton's analysis has been controversial, but the controversy is itself revealing. One of Merton's critics, Ian Mitroff, argued that the "norms of science" as formulated by Merton were essentially backwards (1974). What scientists actually valued was exactly the opposite of, for example, disinterestedness. They valued passionate dedication to the goals of a project and fervent beliefs in the ideas that were being developed or the methods that were being used. Something similar could be said about the other norms. Yet Merton anticipated much of this criticism, and noted especially that, despite the norm of community possession of knowledge, there were intense priority disputes, and argued that the peculiarity of priority disputes is that they were not matters of egoism alone, but were themselves normative in character. The signpost of their normative character is the fact that disinterested observers often enter into the discussion of priority disputes to secure the norm (Merton 1973, 292). Michael Mulkay (1976) treated the Mertonian norms as an "ideology" in the pejorative sense of a story told to the public (though he conceded that this was also a story that could be employed within science by scientists, i.e., a political mythology), but also argued that, in fact, there are no strongly institutionalized norms of this sort in science. The mythological character of the "norms" turns out to be a useful clue.

All norms exist to restrain impulses of some sort, so there would be no "norms" unless there was something valued, desired, or sought for

them to be "counter" to. However, there is a special class of human relationships in which self-denying norms are especially prevalent, and it is this class of relationships that is the subject of principal-agent theory. The theory concentrates on those relationships in which we rely on someone whose interests conflict with ours, and on the costs of dealing with the problem of conflicts of interest. A good paradigm case of this is the relationship between a client and a lawyer. The client trusts the lawyer to exert himself on behalf of the client. However, the client, not being a lawyer, is not in a position to effectively judge whether the lawyer is properly representing the client or giving the client adequate legal council and advice. This is *why* trust is required in this relationship. Not only is the client suffering from a deficiency in information or inability to make judgments, but the lawyer is a person with interests as well which the lawyer can advance, potentially, by cheating the client. Another case of an agency relationship is the relationship between a client and a stockbroker. The stockbroker benefits, as the lawyer might, by doing commission work for the client. The stockbroker also advises the client on what work needs to be done. Similarly the lawyer advises a client not only about what legal steps to take but benefits from the client's decision to take those legal steps that are necessarily costly to the client and beneficial to the lawyer who is paid for carrying out those steps.

The term "trust" has a certain seductiveness, but it is a seductiveness born of the illusion that something is being explained by it. Unfortunately, in the writings of more than a few commentators, trust functions as an unmoved mover, or as a kind of mystery that produces results without having a cause. Although principal-agent theory is not the only approach to trust, it deals with the costs of routine systems of trust that are impersonal and rely on the existence of, or beliefs about, the incentives under which people operate. Incentives means tangible, if sometimes indirect and difficult-to-measure, costs. Just as love is free, but roses are not, the cognitive respect we give science is not purchased by science directly, but is maintained through activities that do have costs.

In the cases of lawyers and stockbrokers, matters are simple: the self-denying normative structure of the codes and rules that these two professions operate under is clear. A stockbroker should be aggressive and motivated by money. A stockbroker who is not would be unlikely to do what he needed to do to keep your business. Similarly, a lawyer needs to be aggressive and motivated by money in order to do anything on your behalf. Yet, at the same time, both lawyer and stockbroker must substitute the client's interest for their own interests. The "norms" of legal representation and stockbroking are characteristically statements of the absolute subordination of the stockbroker's and lawyer's interests to the client's interests. The "counternorm" is that the stockbroker and lawyer should be tenacious and highly motivated, and motivated by the fact that someone is paying them for these services. Even lawyers, when they hire other lawyers, want the shark to be an entirely altruistic shark who puts their interests before the shark's own interests in every respect. And we know that there are some mechanisms for punishing a lawyer who violates the rules, and that indeed the bar association disbars people, uses its dues to support actions involving the punishment of lawyers, and so forth.

At first glance this seems to have little to do with science. Sharks in science are not sharks on behalf of the interests of a client. But science is also about ambition, score keeping, playing by particular rules of the game, and being a shark in debunking false claims. Scientists do all of these things in the context of a large and complex set of institutions, a large number of which are devoted to regulation, which in one fashion or another involve people acting authoritatively on behalf of science. Science is thus "political" in a mundane sense. As I have suggested, scientists make authoritative *decisions* in the name of others, such as decisions on behalf of organizations or collective bodies or in the name of science itself, and have such decisions made about them. Many of these are "gatekeeping" decisions, and indeed the business of gatekeeping is perhaps the primary means of exercising authority in science.

The making of evaluative decisions and the exercise of authority or advisory authority is a pervasive fact of scientific life: in directing the work of subordinates, in asking funding bodies for resources, and the like. But this political "decision-making" character of science is also a largely undiscussed fact—whether by commentators on science, philosophers of science, or sociologists of science.[3] The reason for this neglect, in part, is that these decisions occur under a particular theory or ideology: the idea that the scientists making the decisions are operating neutrally or meritocratically, and that the public role of science itself is neutral. Science is thus mundanely political, but its overtly political features are conceived to be unpolitical. This line of reasoning has the false implication that the activities are somehow inessential to science, or that it is transparent that there are merely administrative, and that they work so effectively because scientists do in fact always agree on merit.

3. There is a good but small literature on peer review (cf. Chubin and Hackett 1990) and on ethical issues (La Follette 1992).

The problem of concern to me in the rest of this discussion is the problem of what this "regulatory" activity means. My concern is not to debunk the "meritocracy" argument so much as to explain the mystery that it produces—the mystery of why, if merit is so transparent, there are so many and such expensive mechanisms for assuring it. There is a sociological literature, to be discussed below, that treats it as a matter of the maintenance of "cognitive authority," and this is certainly part of the story. But reading institutions politically is not the same as explaining why they exist, how they came to exist, and why they take the form they do. The fact of decision making on behalf of "science" is the feature of these institutions that is the topic of the norm-counternorm structure, and it is this activity—judging and evaluating—that produces many costs and incentives, such as the incentive of publication in a prestigious journal or winning a Nobel Prize that is the proximate goal of many scientists' efforts. But the activity itself deserves some consideration, for it is unusual, especially when compared to the usual subjects of principal-agent theory.

Representation, Bonding, and Membership

All collective or political activities depend on some model—an "ideology," in the nonpejorative sense—of representation (Pitkin 1989). As I have noted, one peculiarity of decision making in science is the idea that any scientist, or at least the most accomplished scientists, can truly represent or speak for "science" as a judging or evaluative body, without being elected or otherwise "chosen." Thus scientists engaged in peer review are understood to represent not their personal or economic interests but to be impartial judges evaluating from the point of view of scientific opinion at its best. Although in practice it is accepted that scientists who have made accepted research contributions to a particular area are the most competent to judge new work in that area, it is also believed that they "speak for science" rather than for themselves when acting as a representative.

Why do these arrangements exist at all? As we have seen, this is an ambiguous question, in that one might choose to interpret decision-making processes in accordance with the political mythology of science and say that they are part of the administration of things rather than the governance of people. Some scarce resources—jobs, research opportunities, and so forth—do exist, and allocating these requires some administrative expense. It might be thought that mere administration—at least if the mythology were true—would be a matter whose costs could be minimized, delegated to scientists who function essentially as clerks, leaving talented scientists for more important work. Obviously that is not how these processes work. And as difficult as it might be to estimate the costs, it is clear that the costs go very far beyond administrative expense, and even the administrative expenses are largely connected with a degree of evaluative scrutiny that occurs in these cases. So the problem of the economic function of these expensive activities is intact. Why are they performed in the way they are?

Central to what follows is the relation between *who* evaluates and the value of the evaluation—something that the "transparent merit" model necessarily denies, and that conflicts with at least some understandings of Merton's "norm" of universality. It is obvious that some evaluations are more valuable than others because the evaluators are more valuable as evaluators, the distinction is more competitive, or the journal more prestigious. The pursuit of more valuable evaluations or certifications has real costs—the costs of investing the time and effort to publish in an especially competitive journal, for example. My thesis here is that there is a market for evaluators and evaluations, and that the existence of this market is critical to understanding science. The market is bilateral; that is to say the evaluators compete with one another—how and why will become clear shortly—and the scientists who are being evaluated compete as well, but also make complex market choices between the forms of evaluation available to them. The market is competitive on both sides. But there are also potential problems with these markets that can make them seriously defective, and it is a matter of policy interest, and interest to scientists, to consider the effects of the workings of these markets, and to alter institutional arrangements in response to these defects.

The markets are about agency relationships, and to understand them it is necessary to understand something about the complexities of the agency structures that occur in science. When scientists exert discipline, reward, exclude, and accept, they act as "agents" or representatives, and when it is done on behalf of a journal or professional society, the society itself is the political embodiment of a collectivity. The simplest agency structure is "representation." The decision maker is an agent of a collectivity, either an actual one, such as the American College of Obstetrics and Gynecology, which is in the business of certification (quite literally)

for practitioners, has a publication, and selects scientific articles through the actions of editors who act as representatives, or a hypothetical one, such as the "scientific community" or "science" itself. The agency relationship has costs for the members and for the agents, though the costs may be difficult to estimate and the transactions themselves may be implicit.

Science differs from medical practice in a decisive way. In medical settings (like the law and stockbroking) there are typically single certifiers, such as the American College of Obstetrics and Gynecology, or the bar association. In these monopolistic cases, there are costs, benefits, enforcement, and so forth, but no markets in which certifying organizations compete. In science, matters are considerably more complex. The basic logic of the relationship is nevertheless the same: getting a Ph.D. or submitting a paper to a journal is a transaction in which someone pays for a form of certification. The difference is this: in medicine or law, one either passes one's exams and is certified or one fails. In science, the individual pursuing a career can choose between different forms of certification, add different forms of certification to one's CV, pay different "prices" for different forms of certification and thus develop and act on a strategy for accumulating forms of certification. These certifications are then valued in various marketlike transactions—such as employment decisions—by buyers who themselves can think and act strategically about the market of certifications.

What function does this system of agency relations perform? There are various possible ways to answer this question, but the most straightforward and obvious one is this. The agents or evaluators by the act of evaluation assume risks. The economic function of the activity is to spread risks—risks that arise from wrong answers, scientific error, bad scientists, and so forth. The process here has many imperfect analogues in more familiar economic activities: cosigning for a loan, bonding, signaling, and so forth. My preference is for the notion of bonding: an act in which an agent pays for an assurance (in this case an assurance by other scientists) that the agent will act in accordance with a principal's interests in a situation of information asymmetry (cf. Jensen and Meckling 1976). But the analogy is imperfect in several respects. The asymmetry is not simply a matter of "information" and the assurances take various forms, such as assurances about minimal competence as a scientist, or minimal adequacy sufficient for publication in the case of a journal article. In each case—for example when an academic program awards a degree or a journal accepts an article, the program or journal assumes a risk that its assurances of adequacy will be found out to be false, and the consequence of error is damage to "reputation," which translates into a loss of the value of future assurances of the same type. This feature is central—and for this reason, and for convenience, I will retain the term "bonding."

The term seems merely to be about reputation. Reputation is a deceptively simple notion, however. There is a sense in which reputation is contrasted with reality, and thus that there is some sort of falsity or illusoriness of reputation. But in the case of the bonding that happens in science, this is potentially misleading, for it suggests that no real value is created by the activities of evaluation. Something like this suggestion appears in what Merton called the Matthew effect (1973), by which scientists whose achievements are recognized in various ways "accumulate advantage" so that a scientist who has gone to the right schools, published in the right journals, and won the right prizes is more likely to have his achievements cited. The implied distinction is between the intrinsic value of the science done by the scientist and the increased impact that results from advantage. But if we think of the process of accumulating advantage in terms of bonding, it becomes clear that at each point of accumulation something has actively been done, at a cost, to create value through reducing risks, specifically by distributing risks to people other than the scientist accumulating the advantages. So the total value of the "product" in question, the science, is not only the ideas, the intrinsic value, but the guarantees that come along with it, in the form of risk-bearing actions taken by editors, hiring departments, and prize givers, each of whom has put the value of their journal, department, or prize at risk by their actions. The accumulation of advantage is thus like the accumulation of cosigners to a loan. So where Merton, operating in terms of notions about merit, and concerned to make the argument that science proceeds successfully and without external interference in terms of merit, finds it puzzling and problematic that advantage accumulates, the "bonding" approach finds it understandable.

Why is there so much "bonding" in science? One of the concerns of principal-agent theory is the problem of adverse selection, and there is a sense in which one can see the web of evaluation activities that is so important to science itself as a means of avoiding a large number of potential adverse selection problems. Scientists themselves need to select ideas to pursue, to believe, and to take seriously. Academic departments and businesses employing scientists need to make hiring decisions.

These decisions are almost always made in the face of asymmetries, and where there are risks of making bad choices. "Bonders" provide ways of spreading these risks. To put this very simply, because in science only a few can seriously evaluate an idea, the rest must rely on their certifications of the idea, and thus certification takes on a massively important role.

A somewhat more puzzling question is why are there so many *forms* of bonding in science. The system, incarnated in the CV, is one in which single distinctions do not suffice. Not only does a scientist seek many kinds of certifications, they are sought from, and available from, a wide variety of sources. Understood as a total market or system, it is complex in the following ways: most of the certifications are indirect with respect to matters of truth. What is judged is the minimal adequacy or interest of a journal article or Ph.D. dissertation. The effect is that a scientist acquires various "certifications" from a variety of sources, that these certifications overlap, sometimes involving the personal quality of the scientist, sometimes of a particular article, sometimes, as in the case of prizes, for a "discovery" or "contribution" that spans many years and consists of many items. The fact that they overlap amounts to the building in of redundancy. An established scientist will have passed through many tests, of which the CV is the archaeological record. The existence of all this redundancy is a relatively recent phenomenon in the history of science, and it deserves its own discussion, which I will provide shortly. It must suffice to say that there is obviously a market here, with both demand for bonding and an incentive to supply it. So far I have focused on the demand side of this relationship. I now turn to supply.

Bonding and Value

What I have said here about bonding suggests that assuming risks is a major activity of science, that one might expect that it will show up in the form of transactions, and points out that it does, for example in the phenomenon of accumulated advantage. But this account assumes that there is something of value already there for the transactions to be about, and in a sense this is the central puzzle of the economics of science: what is the value of knowledge? Several points ought to be made about scientific knowledge that bear on this question. First, scientific knowledge is "embodied knowledge," in the sense that it has little or no value as pure information. One needs to know what to do with it, and this requires an investment in scientific education.

This simple fact is critical to understanding the way that scientists solve the problem that any possessor of embodied knowledge faces: how to convert one's knowledge into money. One way is to employ it in the production of things that can be bought and sold—this is the model around which much discussion of science has revolved, and it produces the familiar puzzle of the value of basic science, and such solutions as the idea of science as a "public good." Patent law is a kind of extension of the notion of product that creates an artificial right to produce something that can itself be bought and sold but is valuable only if it either produces something or prevents something from being produced. But much of science cannot be converted into such a right simply because there are no products with sufficiently high market value that patenting makes any sense, and the patent process is limited to things that it makes sense to create rights about. But there are other options for scientists to convert their embodied knowledge into money.[4] One widely employed option is consulting. Here there is no mystery about the existence of a market, the existence of competition, and the existence of market pricing processes.

A primary way in which knowledge can be converted into money is through the transmission of that knowledge to others, at a price. Teaching is a primary source of income, or a large source of income, for the possessors of many kinds of knowledge, such as musicians. Musicians are also paid for performances, and this is a useful model for understanding the value of scientific performances. Musicians, however, are not, for the most part, their own judges. There is a public audience for music. Science, however, is different. There is a public audience for science, to be sure, but it is an audience that assesses science in very general ways, and through its products, but does little to distinguish among scientists, and to the extent that it does, is inclined to grant much greater credit

4. Elsewhere I have argued that it was fatally misleading for Polanyi to have used the word "knowledge" with respect to these competencies or skills (Turner 1994). As I have repeatedly pointed out since, "knowledge" used here is an analogical notion but one in which the distinctive characteristics of knowledge are absent precisely because of the nature of the stuff that is being captured by the analogy (1999a, 1999b). Philosophers sometimes think of knowledge as justified true belief, for example, but the stuff that is being analogized is not, except analogically ("implicitly"), beliefs, and by definition not warranted nor justified in the usual public sense of this term, and consequently not the sorts of things one can say are true. I have elsewhere suggested that the best one can do with these notions is to talk about the habits that one acquires by successfully going through certain kinds of public performances (Turner 1994). It is these habits that I have in mind with the problematic phrase "embodied knowledge." The issues here are primarily of importance to social theory and philosophy, but it is worth at least alluding to them to indicate that there is no easy way of eliminating them in favor of some simple substitute notion.

to the kinds of scientists who write for the public than scientists themselves do.

If we invert the usual way of thinking about the relationship between these performances and judging and think of them on the analogy of the kind of music teacher who charges, and raises, tuition on the basis of successful performances, we have a model for the role of scientific publication, as well as a very good explanation for scientists' concerns about having their successes credited to them. Performances, or discoveries, are a source of demand for the other things that the scientist can do, such as teach and make judgments. This close link suggests an alternative view of Merton's norm of "communism." There is no need for musicians to have an "ethic" of public performance, since performance is, in effect, a condition of their making money through tuition. One can of course, as a musical nonexpert, judge the products of musical tuition, so the link is not so close. Still, performing demonstrates one's possession of the embodied knowledge that one charges tuition to transmit to students.

In science, unlike art, production and assessment are closely related, and indeed, since developing hypotheses and testing them almost invariably have implications for the validity of previous hypotheses, there is a sense in which all science is authentication. Yet the activity of criticism and production is separated in various ways, in that there is a moment of pure thought, so to speak, in which hypotheses are formulated; and other moments when hypotheses are, sometimes very expensively, tested; and another when scientists assess the work of other scientists by means such as peer review or simply reading and criticizing the works of other scientists. Finally, with art, there is an object whose value is increased by the efforts of art historians. In science, matters are much more obscure. People are bonded; ideas are bonded; some bonding, such as degree granting, is the sort of thing one pays for, some, such as publication in a famous journal, is not.

One might say, however, that this pattern is characteristic of the old economics of science, in which a particular kind of competition for talent, between university departments of physics, chemistry, and the like, was central. Whether there has been, or might be, a radical change as a result of present changes, such as the corporatization of bioscience, is a question I will take up in the conclusion.

The Market for Bonders

The problem of judgment of peers is that these judgments themselves require bonding because there are known conflicts of interest that arise in judgments of competitors and judgments of work in the same specialty. Evaluators may be biased in judging the general significance of work in a particular area that resembles theirs, and, if they are competing for fame and fortune, they have a general interest in seeing work that cites and extends theirs being funded. By the same token, they may have an interest in impeding the research of a competitor. Competition of exactly the same kind exists between "bonders" as such. Journals compete, prizes compete, and departmental programs and universities compete. Departments compete with corporations in the hiring of scientists. And there are various incentives to the creation of novel bonding mechanisms, such as official journals and scientific societies.

The potential risk, from the point of view of the journal or the department, is the risk of bonding too much, or the wrong things, so that credibility as a bonder (and consequently the value of implicit "certification") is diminished on the one hand. The more demanding the standards, the more prestigious the certification might be. But if the standards are different in such a way that there are well-recognized achievements that fail according to these standards then the value of certification is likely to diminish generally, for the users of certification will be unable to say when the certification is useful and when it is not. Overly generous certification obviously risks the same effect, especially if it is potentially biased, for example in the direction of allowing dangerous or useless therapies. The market, in short, demands a certain uniformity and punishes those who deviate from it, though the uniformity is in effect a moving equilibrium. But the "market" is not closed: the rise of new forms of scientific activity may make previously important certifications peripheral or worthless.

Central to what I have argued here is that there are different kinds of bonding—from journal gatekeeping, to grant giving, to degree granting and award giving—that are redundant and overlapping. By this I mean that no single mechanism is sovereign or final. It must be noted that it is part of the founding mythology of modern science that no person is authoritative in science. The experience of the prosecution of Galileo by the church was formative: scientific bodies were generally reluctant to certify scientific truths, to avoid the risks of error. If we recognize the lack of sovereignty in the agency activities of scientists as bonding representatives, it becomes obvious that under normal historical cirsumstances—that is to say circumstances of polyarchy rather than a Stalinist uniformity—there will always be a variety of overlapping communities, consumer groups, and forms of certification that will sometimes agree and sometimes fail to agree, sometimes produce equivalent bonding results and sometimes not.[5]

The model I have constructed here points toward some way of characterizing the intentions of the parties to at least some of these processes. Buyers of science have various purposes, such as being cured or annihilating their enemies by the use of nuclear weapons. The consumers have a need for certified knowledge because it is cheaper for them to accept certification than to find out for themselves. But there may be competing certifiers or bonders. In the simplest case there is a kind of equilibrium of bonding. The "results" of science are thus standardized so that one can choose between the scientifically bonded and the unbonded. In situations of scientific change, the situation will typically be more complex. Users may be buying, and happy with the results of, scientific work that is not universally certified or bonded by scientists but only by some small faction. This may persist indefinitely, though to retain the credibility of bonding as such, scientists have an interest in giving some sort of final judgment on particular claims, especially if those claims have market appeal and are bonded by competing bonders. To fail to recognize a genuine new achievement is to become less valuable as a bonder. And it is perhaps for this reason that bonding agencies seldom subject themselves to the risk of making direct and unqualified endorsements of scientific claims. In the end, the majority does not rule, but rather the credibility market and its demands rule.

It is this market that produces the distinctive norms of science described by Merton. "Disinterestedness" is just a norm of bonding as such. A journal or prize that was known to be biased would lose some of its value for bonding. "Organized skepticism" is not so much a norm as a description of a situation in which participants recognize the inadequacy of the means they possess to judge the claims of science—the very situa-

5. Latour's actor network theory (1987), from the point of view of this chapter, is in effect a description of the connections between parts of the process of the rise and fall of scientific ideas, but it is not an explanatory model. It works, to put it very simply, by a kind of inverted behaviorism. Rather than denying intentionality to human beings and explaining them and their interactions as though they were purely causal phenomena, as behaviorism attempted to do, actor network theory inverts this explanation and grants a kind of quasi intentionality to all sorts of objects, including the subject matter of science, making networks into quasi-intentional conglomerations. Nevertheless, this actually explains nothing because the relevant intentions are not themselves explained by anything. What is right about the Latourian model is the denial of a point of epistemic sovereignty, such as "the scientific community." What is lost is any account of the intentions themselves, and how they relate to one another to produce the particular social forms that arise in science.

tion that produces the demand for bonding. The term "organized" is an important qualification: there are actual means of bonding, such as degree granting and the like, between which the skeptic chooses. "Communism" or publicity is another matter entirely, and I think one on which, as I have suggested, Merton was simply in error. There are advantages to keeping trade secrets if they are secrets one can exploit. There is a patent mechanism for preserving the secrets' use and protecting it from copiers. Because science can be produced as a public performance or, sometimes, kept secret and used as a product, scientists have a choice: the choice is dictated by their interests and those of their funders, and does not always lead to making scientific results public. "Universalism" is a descriptive feature of the market and another way of expressing the political mythology of representation. If by some bizarre circumstance, scientists were confined to something like a sovereign buyer such as Stalin, it would be rational to be nonuniversalistic, as indeed Lysenko and his associates were, and of course Hitler's scientists were as well. Is this, or Islamic science, some sort of normative aberration? In the sense that granting epistemic sovereignty to any particular tradition or agent is a violation of the basic political mythology of science that is the foundation of its notion of representation, it certainly is. But universalism does not demand universal agreement, just the agreement of representatives speaking properly for science.

The New Situation of Science

The basic argument of this chapter has been simple. Bonding is an economically essential feature of science that is a result of the high cost of assessing alternatives personally. Bonding, certifying, accepting, using, and so forth are real acts, acts that are not at all cost free. In the end, however, the overall benefits are very close to those conferred by the rule of law and the reduction of transaction costs that the rule of law enables. The real scientific revolution is the revolution that substantially reduced those costs by the emergence of an effective bonding market, itself the product of incentives, notably incentives rooted in the imperative of making money out of the possession of knowledge.

Scientists and nonscientists alike rely on, or treat as information, things that scientists say without having any knowledge of the opinions of this handful of certifying figures, and the certifying figures are themselves certified and recertified by the many and highly redundant indirect means discussed above. Instead, there are many certifying mechanisms of the sort I have described that the user does rely on, such as the fact

of acceptance in a journal, the granting of research funding and support of the project, the high academic position of the person reporting the finding, and so forth. The striking development from the kind of gemeinschaft of early science is that science is now able to proceed effectively by accepting the certifications and recognizing the certifying mechanisms of other groups in science. And, unlike face-to-face mechanisms, these mechanisms of certification can be extended in such a way as to permit an enormously complex division of labor in which no one's understanding extends very far. If we can trust the market for bonding to produce adequate bonders, we can trust the results of science where there are such markets. In short, it is the market for bonding in which we place our trust, and on which our trust in science properly rests.

From the point of view of this chapter, the basic question raised by the corporatization of science, and especially biotechnology, is whether the system described here is historically doomed—bound to an era of academic competition that perhaps has already passed its peak and is being transformed into something else entirely. The usual way of thinking about what it might turn into is a world that is indirectly dominated by the demands of investors, of the source of the funds necessary to produce science. Since these funds far outstrip the funds available through teaching, it stands to reason that they will become—as government funding became—the locus of competition. But government funding of science was in effect an extension of the academic system, in which academic prestige governed peer review and if anything simply made it more powerful. So the new question is this: is the autonomy of science in the sense of scientific control over the standards of science compromised by these new funding sources?

To begin to address this question it is perhaps useful to discuss some ways in which the academic-governmental system itself has sometimes gone wrong or been accused of going wrong. It is well known, for example, that there are critics of the HIV-AIDS connection who argue that the huge AIDS research establishment is committed to a hypothesis and refuses to fund other approaches. High-energy physics research facilities and telescopes, notoriously, are scarce resources: opportunities to do research are allocated by like-minded committees, with the effect that the possibilities of research are limited. When there are no alternative sources of funding, these committees are effectively monopoly buyers, and "market" competition disappears in favor of science by decision. Nuclear power researchers failed to preserve their independence from the industry (Morone and Woodhouse 1989). In many disciplines, notably economics and sociology, it has been argued that the power of certain top journals, together with the peculiarities of competition for space in those journals, has served to elevate particular styles of research and exclude others, thus distorting the standards of the disciplines and the disciplines themselves. In each of these cases there is a dominant player whose conduct dictates the conduct of others, including the standards of evaluation.

These problem cases raise some serious questions about whether the arrangement I have described in this chapter, in which a scientist's acting as an agent for science provides certification or what I have called bonding independently, competitively, and in different but overlapping ways, is vulnerable to a shift in the weights of forms of market power of the sort that corporate science represents. In its most advanced form, the form familiar to us, it created a powerful internal market and set of incentives that propelled the development of science, reduced the risk of bad science. But the "system" I have described is an oddity in the history of science. University science and the research university came to their position of dominance in science only during the twentieth century. The idea of scientific authorship and the system in which a scientist acting as an agent certified the science of others, which is so characteristic of twentieth-century science, was very weakly developed.[6]

It would be ironic, but hardly surprising, if the success of science governed by this evaluative market were to lead to the demise of the system itself. The system depends on mutual monitoring through implicit agency transactions. But corporations do not participate in this process. They don't benefit by editing journals, and the journals would probably not be trusted if they did. Yet the problems of monitoring and evaluation that this loose system solved do not vanish. The "information asymmetries" that always exist in science still exist. Investors may need, or come to need, more and different types of monitoring and information. Conflicts of interest would still arise over evaluations. And the fact that cash has already invaded the "old" system of bonding in the form of payments for evaluation, suggests that a surrogate for the old system of cashless "credit" is in the process of emerging. This will almost certainly be a system that is less "public" than academic evaluation was, and in which academic science and its scheme of competitive incentives is marginalized and made ineffectual, and its web of indirect devices replaced by more direct forms of monitoring and assessment. The real irony is that this replacement will almost certainly, and justly, command less respect by the consuming public. The strong resistance to genetically modified foodstuffs in much of the world testifies to the suspicion in which corporate science will likely be held. Replacing the old evaluative system may thus kill the goose that laid the golden eggs.

6. I am grateful to David Stodolsky for bringing the fascinating work of D. A. Kronick (1988) to my attention. Kronick discusses the evolution of authorship, and the previous use of anonymity and collective publication, and the demise of these alternatives to the present system. Kronick, however, uses the language of Merton, and thus runs together two things that from the point of view of the present paper ought to be sharply distinguished: the collective nature of science and the act of publication of particular anonymous contributions by particular scientific collectivities. The latter I take to be a case of collective endorsement that effaces the contributor. Kronick seems to think of it as a microcosmic instantiation of the collective nature of science itself.

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PART V

Contours of the Globalized Privatization Regime

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