Science as a Market Process

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To allocate resources in the pursuit of chosen ends is an economic matter: a matter of costs and benefits, of investments, risks, and payoffs—above all, a matter of choices and trade-offs. The allocation of *cognitive* resources in the pursuit of *knowledge* surely must be a case in point. In science, we may devote all our efforts to making a few extremely precise measurements, or we may achieve a greater number of measurements by sacrificing precision. We may spend years attempting to solve a particularly significant theoretical problem—at the risk of complete failure—or we may choose safer, less-significant problems. Trade-offs are associated with collaboration versus independent work, with the strictness of one's standards for accepting experimental results and other researchers' findings, and with the choice between adopting a new theory or continuing to work within the old. Thus, collaboration brings the benefit of others' expertise, but the coordination of multiple efforts takes time and imposes limits on individual initiative; strict epistemic standards carry the benefit of minimizing error at the risk of rejecting truth; adopting a new theory involves an investment in learning to use it, the risk that it will prove fruitless, and the opportunity cost of results that might have been achieved with the old theory, but it also involves the potential payoff of achieving revolutionary advances.

This article is intended as a manifesto for an economic theory of scientific inquiry. My focus is not on traditional economic concerns about how societal resources are allocated to the funding of science and how scientific research contributes to technological advances and economic growth. Rather, my attention centers on using economic concepts to illuminate the conduct of scientific inquiry itself.

This work was originally conceived in the mid-1980s independently of the few then-existing efforts along roughly the same lines and before I was well acquainted

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with the Austrian paradigm that now informs it. In the years since, a number of works have appeared that recognize the relevance and application of economic concepts to issues in scientific research. Nevertheless, several major features of the present essay offer distinct contributions:

- The theory is grounded in the outlook of a particular school of economics, the Austrian school, which I claim lends itself especially well to extensions of economic thinking beyond its traditional sphere.
- The relevance of an economic point of view is demonstrated through numerous parallels between science and traditional economic activity.
- A proposal is made to extend the concept of the "market" to encompass a broader range of transactions than fall traditionally within its scope, thereby opening the door to a conception of science as a market process. In the scientific market (or "marketplace of ideas"), a process of exchange takes places in which citation is the payment for use of another's published work; nevertheless, the right to receive citation is not usefully characterized as a property right.
- Economics is applied as a critical perspective on several classic approaches to understanding the process of scientific inquiry: logical methodology, evolutionary epistemology, Mertonian norms, and Kuhnian revolutions.
- Together with insights adopted from the modeling approach to philosophy of science, economic thinking is used to shed light on the nature of scientific change and scientific rationality.

Among those who have argued for the relevance of economic concepts to an understanding of scientific inquiry, Radnitzky (1987a, 1987b) and Rescher (1989) have emphasized a cost-benefit approach. Diamond (1988), Goldman and Shaked (1991), and Wible (1998) have offered mathematical models of, respectively, theory choice, truth acquisition, and misconduct in science, based on the principle of utility maximization by individual scientists. Numerous authors have drawn attention to one economic concept or another—such as exchange (Storer 1966), competition (Hagstrom 1965), and division of labor (Kitcher 1990)—in discussing scientific inquiry. Polanyi (1951, [1962] 1969, [1967] 1969), Ghiselin (1989), Railton ([1984] 1991), Bartley (1990), and Lavoie (1985) are among others who have developed extensive economic parallels. Economists who in recent years have been taking seriously a comprehensive economic approach to science include Dasgupta and David (1987, 1994), Stephan (1996), Stephan and Levin (1992), Leonard (1998), and Wible (1998).

In a recent book, Wible (1998) seeks to establish an economics of science, examining various aspects of scientific inquiry on the assumption that the scientist is a rational economic agent. Our approaches differ entirely in that Wible adopts a mainstream rather than an Austrian economic perspective and does not consider scientific inquiry to be a market process; he also considers a rather different mix of issues. Among his main concerns are scientific misconduct and deficiencies in the institutional "self-correctiveness" of science, with implications for its ability to serve society; how scientists choose research problems and programs; and the self-referential nature of an economic perspective on economics itself, which is taken to be a science.

Leonard's (1998) work appears close in spirit to my own. Leonard advocates "using economics to study science and its product, scientific knowledge" (2). He sees science as an "invisible hand" process in which competition among self-interested agents—whose interests are not purely epistemic—leads very successfully to the production of reliable knowledge. He carefully contrasts the economic perspective with traditional philosophical as well as postmodern views.

Thus, the work presented here finds its place within a growing body of literature devoted to or touching on scientific inquiry as an economic process.

The Economic Point of View

The Scope of Economics

In the view I am adopting, the scope of economics is not limited to such traditional concerns as the creation of wealth or transactions involving money or even the allocation of scarce resources among competing purposes. Human beings pursue their individually chosen goals through purposeful action; economics is the intellectual discipline that traces the consequences of that fact. This conception is close to that which Israel Kirzner (1976), in reviewing the history of attempts to define the nature of economics, identifies as originating with the Austrian school, of which Ludwig von Mises, F. A. Hayek, and Kirzner himself have been prominent members. Mises's magnum opus *Human Action* ([1949] 1996) provides a comprehensive exposition of economic theory according to the Austrian school and serves here as my standard economics reference.

Mises defines *human action* as purposeful behavior, as aiming at ends and goals ([1949] 1996, 11). He uses the term *praxeology* to refer to the general study of human action so defined (3, 12), reserving the term *catallactics* for the subset of problems that fall within the traditional scope of economics. Significantly, he emphasizes that no strict boundary can be drawn to demarcate catallactics from the rest of praxeology (3, 10, 232–34).

Mises uses the word *economics* flexibly. In some passages, such as the following one from page 3 of *Human Action,* he clearly means traditional economics or catallactics: "Out of the political economy of the classical school emerges the general theory of human action, *praxeology.* The economic or catallactic problems are embedded in a more general science, and can no longer be severed from this connection. No treatment of economic problems proper can avoid starting from acts of choice;

economics becomes a part, although the hitherto best elaborated part, of a more universal science, praxeology." Elsewhere, as on page 266, he speaks of economics in a broad sense, as equivalent to praxeology itself: "Economics is, of course, not a branch of history or of any other historical science. It is the theory of all human action, the general science of the immutable categories of action and of their operation under all thinkable special conditions under which man acts." Economics in this broad sense is to be *distinguished* from "the field of catallactics or of economics in the narrower sense" (234).

In this article, *economics* is understood in its broad sense. The limited sphere of traditional economic applications is referred to as *traditional economics*. Just as traditional economic activity (which I sometimes refer to as *business*, for short) is to be regarded as only one imprecisely delimited province of the larger realm of human action amenable to economic analysis, science is another such province. Sometimes I refer to *scientific inquiry* in place of *science* in order to emphasize the activities, choices, and interactions of scientists more than the subject matter, data, and theories.

Clearly, economic insight is not a substitute for specialized knowledge and experience, in science or elsewhere. Economics can no more tell a scientist whether a theory is correct or how to apply it or how to devise an experiment than it can instruct an automotive engineer how to design a reliable motor. Note, however, that the engineer's knowledge is not by itself sufficient to determine the parameters of the motor that will actually be manufactured. Different sizes and designs will offer different levels of power, durability, and fuel economy; will require more or less expensive materials and more or less time to develop and build; and will ultimately prove more or less profitable. Thus, the problem remains of choice and trade-offs among alternatives. This problem exists in science, where the many trade-offs include those identified in the opening paragraph of this article as well as those in business and all other realms of human endeavor. It is the problem at the heart of economics.

In recent decades, a number of overt extensions of economic analysis beyond its traditional scope have been made. Becker (1976; see also Tommasi and Ierulli 1995) has applied economic reasoning to subjects typically associated with such fields as sociology, political science, law, and even psychology. Contributors to Radnitzky (1992) and Radnitzky and Bernholz (1987) promote an economic approach to a variety of fields. The Public Choice school has examined from an economic perspective the interaction of politicians, bureaucrats, and special interests in the political arena (see Gwartney and Wagner 1988). Sowell's *Knowledge and Decisions* (1980) offers a nontechnical economic analysis of social, legal, and political institutions. McKenzie and Tullock (1989) built an introductory text around diverse nontraditional applications of economic thinking. Thus, economic interpretations of scientific inquiry fit into an existing body of extended economic scholarship. Such extensions are not universally welcome, and it is perhaps ironic that the Austrians, from whose perspective these extensions so naturally flow, did not take the lead in developing them.

The Market

Nevertheless, from the Austrian point of view, to develop an economic interpretation of scientific inquiry is simply to apply praxeological analysis to an area of human action. What I am proposing, however, goes a bit further: it is not just an *economic* perspective, but a view of science as a *market* process. For this proposal to succeed, the concept of the market must be broadened beyond its traditional meaning in a way that parallels the broadened understanding of economics.

The market is ordinarily defined in terms of or associated with buying, selling, and setting prices, and that is how Mises clearly portrays it in many passages of *Human Action*. Thus, on pages 232–34, he refers to *market phenomena* as "the determination of the market exchange ratios of the goods and services negotiated on markets, their origin in human action and their effects upon later action"; he says, "The subject matter of catallactics is all market phenomena with all their roots, ramifications, and consequences," and "Market exchange and monetary calculation are inseparably linked together." But *Human Action,* like other treatises on economics, is really about *traditional* economics, even though Mises devotes considerable space to grounding the subject in the larger field of praxeology. As we move the focus of our attention beyond a limited subject area, surely it is reasonable to entertain a broader application of the terminology that had been defined for use primarily within that limited area.

A passage on page 258 leaves the door at least slightly ajar: "The market process is the adjustment of the individual actions of the various members of the market society to the requirements of mutual cooperation. The market prices tell the producers what to produce, how to produce, and in what quantity. The market is the focal point to which the activities of the individuals converge. It is the center from which the activities of the individuals radiate." Within traditional economics, prices do inform the process of "adjustment of the individual actions . . . to the requirements of mutual cooperation." But cooperation also occurs outside the realm of traditional economics; such cooperation must involve a process of adjustment of individual actions, and that process must be informed in some way. To the extent that the process involves exchange, it deserves to be called a market process.

Let the concept of the marketplace, or simply the market, encompass the entire array of institutions and customary modes of interaction through which people engage in exchange in pursuit of their individually chosen goals. Must exchange involve buying, selling, and establishing prices? No. I argue, in partial agreement with a number of authors, that cooperation in science is mediated by a process of exchange that does not possess such features—namely, the practice of citation. It follows that scientific inquiry is characterized by a market (the "scientific market") that is distinct from the market of traditional economics (the "traditional market"). This scientific market is indeed the focal point of the activities of the community of scientists, where they offer the results of their own research and acquire access to the research of others, where they give and receive proper credit.

Economic Modeling

Assumptions and Scenarios

In economics, idealized scenarios or models are employed to gain insight into complex systems of interaction (Mises uses the term *imaginary constructions* [(1949) 1996, 236–37]). These scenarios embody assumptions concerning the goals and preferences of the people acting and the constraints and influences under which they act. A highly idealized scenario may serve as a basis for devising more realistic ones by incorporating additional assumptions. To investigate the benefits of exchange, we might consider the plight of an isolated individual attempting to supply his basic needs self-sufficiently. We might imagine two farmers seeking to maximize their cash incomes through cultivating adjacent plots of land independently and compare that situation with one in which they pool their efforts. We might foresee complications that will arise when additional farmers and plots of land are brought into the cooperative effort, and we might anticipate institutions that might evolve to handle those complications. An economic scenario might be constructed on the premise that individuals seek to maximize their cash incomes in a free market with no governmental constraints other than the enforcement of contracts and the punishment of aggression. This scenario might then be modified by allowing for a wider range of individual goals (status, security, altruism) and imposed constraints (taxes, quotas, regulations, prohibitions).

Similarly, by developing idealized scenarios of scientific inquiry based on simplifying assumptions about scientists' motives and the constraints and societal influences under which they act, we might understand better the observed features of the research process and anticipate how the institutions and progress of science may vary with different circumstances.

I propose to adopt, as a first approximation, the assumption that scientists are motivated by a desire for recognition by their professional peers. Of course, they have other motives as well, which vary in relative importance from one individual to the next. Someone might pursue scientific research purely out of curiosity, with no thought of recognition, just as many people engage in hobbies and charitable work with no expectation of financial reward. Such purposeful behavior still falls within the scope of economics as construed here. Nevertheless, it is clear that most scientists seek professional recognition, either for its own sake or as a key to other rewards such as job tenure and financial gain. Priority disputes (Merton [1957] 1973) and near-universal anxiety over having research findings anticipated (Hagstrom 1965, chap. 2) indicate what a powerful incentive recognition is.

Professional recognition is not to be confused with public acclaim. What I am taking to be the prime motive is recognition for contributing to the advance of science, as judged by experts in the field. (If professional recognition is sought as a means to public acclaim, then my assumption is still good. To the extent that scientists seek public acclaim that is not grounded in professional recognition, the assumption is inadequate and perhaps misleading.) Even a perfectly selfless seeker of truth might well consider recognition to be a useful form of guidance from the scientific community, an indicator regarding the effectiveness of his research efforts; those efforts might be, in effect, directed toward the pursuit of recognition.

As for constraints and influences that arise from outside the scientific community, our first approximation might be simply to ignore them. Let us imagine that scientists communicate only among themselves, that they have independent sources of income to support themselves and their research, and that they are not subject to external forces, such as censorship. The resulting picture of scientific inquiry as a self-contained competition for collegial recognition will be taken for granted in much of this article, with additional assumptions—for example, about how science is funded or about what motivates scientists other than recognition—brought in where they are salient.

Mathematical Models Versus Idealized Scenarios

Scientists employ idealized models of physical systems. An example from physics, which is used to gain insight into the electronic structure of solids, describes a single electron that is free to move in only one dimension, subject to a simplified potential energy function (the "periodic square-well potential"). A real solid is a three-dimensional array of atomic nuclei and electrons, perhaps dozens of electrons per atom. Nevertheless, the extremely idealized model elucidates major differences in the electrical and optical properties of metals, semiconductors, and insulators. Experience gained with this model facilitates the development of progressively more sophisticated ones that incorporate realistic potential functions, lattice vibrations, impurities, defects, three dimensions, and so forth. Through such models, one gains insight into the properties of known materials and predicts the properties of others that might be fabricated—for example, variously doped semiconductors.

Clearly, some parallels might be drawn between the use of idealized models of physical systems and idealized scenarios of human action. How deep does the similarity run? In particular, given that analytically powerful models of physical systems tend to be formulated or articulated in terms of mathematics, should we expect corresponding mathematical models of *human action* (here, models of scientific inquiry) to be similarly fruitful? In Diamond (1988), Goldman and Shaked (1991), Kitcher (1990), and Wible (1998), the focus is indeed on mathematical models in which functions with adjustable parameters are said to characterize the various options, propensities, and outcomes. As is typical of work in mainstream traditional economics, these authors even invoke a maximization of expected utility (or "optimality analysis") in much the same way that a physicist might employ, say, maximization of entropy or minimization of energy.

But the systems studied by physicists differ radically from human action in ways that cast doubt on the suitability of a mathematical approach to modeling the latter. Silicon atoms are identical. Electrons are identical. Relevant properties of silicon atoms and electrons are measurable and characterizable mathematically, once and for all, in terms of a few parameters. There is little doubt that the known equations of quantum mechanics correctly describe the interaction of silicon atoms and electrons in a semiconductor crystal. We may need to use approximations and idealizations, but we obtain quantitative results by solving mathematical equations. We can perform repeated experiments on the same sample of silicon or on different samples identically prepared, with repeatable quantitative results. Testing quantitative theoretical predictions with precise, repeatable measurements is the key to refining our models.

Unlike atoms, human beings are unique individuals. Each makes frequent choices on the basis of individual goals, preferences, and capabilities that are subject to continual change and are not fully articulable. In striking contrast with the realm of physics, there are no fundamental or enduring numerical constants characterizing human action (Mises [1949] 1996, 55–56, 118). Nor is human history subject to controlled, repeatable experiments. In these circumstances, how meaningful is it to represent interacting humans by means of mathematical functions? When the equations are solved, do the results have any significance? Such concerns have provoked trenchant Austrian-school criticisms of econometrics and mathematical modeling in traditional economics. (See, for example, Mises [1949] 1996, 350–57, and Yeager [1957] 1991) These same concerns must cast doubt on mathematical modeling in an economic perspective on scientific inquiry.

Exploring idealized scenarios through verbal reasoning stands as the alternative to mathematical modeling. A great deal of knowledge regarding human action is available to us through introspection and common experience, but this knowledge is qualitative, not quantitative. We are aware of the diversity of human motivations, aptitudes, and circumstances. We know that humans pursue goals; that to pursue goals requires pursuing the means to those goals; that action involves choices among alternatives; that people prefer to receive benefits sooner rather than later; that they communicate, cooperate, and compete. In exploring idealized scenarios, we can draw on all our knowledge without articulating it fully in advance (which would be impossible anyway). Simplifying assumptions that are expressed verbally carry with them a large component of tacit understanding. As the consequences of our assumptions are explored, the assumptions themselves become refined and clarified.

The verbally described scenario therefore comprises a wide range of qualitative and even unarticulated knowledge in a process that involves active reasoning throughout, in contrast to a mathematical model, which generates only numerical output in response to numerical values of a few input parameters. Verbal scenarios can provide only qualitative results, but the quantitative output of mathematical models represents little or no advantage because, given the degree of uncertainty and idealization involved, little significance can be attached to the precise values of numerical inputs and outputs.

To trust mathematical models of human action as something more than suggestive or illustrative Tinkertoys might be profoundly misleading. Mathematical models in the physical sciences routinely provide a basis for the design and control of physical systems to serve useful purposes—a basis for *engineering*. That major, enduring, spontaneously evolved, ecologically complex, and interdependent human institutions might be redesigned, replaced, or substantially improved through analogous *social* engineering is highly questionable. The danger of mathematical models with regard to human interaction or institutions is precisely that they may lend false plausibility to misguided utopian schemes.

Consider Philip Kitcher's 1990 paper "The Division of Cognitive Labor." Given the existence of competing experimental methods for solving a particular problem in science, Kitcher assumes that the probability of success of each method can be expressed as a mathematical function of the number of scientists utilizing it. When these functions are known, it is a simple matter to calculate the distribution of scientists among the competing methods such that the total probability of success is maximized. (Kitcher offers a similar analysis with regard to competing theories.)

Unfortunately, neither the functions nor the experimental methods are simply given to us, and their discovery itself becomes a difficult problem requiring the allocation of intellectual resources—how? Scientists are not mere interchangeable parts that can be counted out by the dozen. And who decides in the first place which problems are most worthy of investigation? Never mind. Kitcher, a distinguished philosopher, sees the way clear to redesigning the very institutions of science on the basis of such optimality analyses! Here are his conclusions:

> we can ask how, given all the aims that we have for ourselves and our fellows, we should allocate resources to the pursuit of our community epistemic goals. Given the solution to this optimization problem, we know the size of the work force that the sciences can command. We can then ask for the optimal division of labor among scientific fields, and, finally, proceed to the question that has been addressed in a preliminary way in this essay: what is the optimal division of labor within a scientific field, and in what ways do personal epistemic and nonepistemic interests lead us toward or away from it? That question ultimately finds its place in a nested set of optimization problems.
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> . . . [I]t would be highly surprising if the existing social structures of science, which have evolved from the proposals of people who had quite different aims for the enterprise and who practiced it in a very different social milieu, were to be vindicated by optimality analysis. How do we best design social institutions for the advancement of learning? The philosophers have ignored the social structure of science. The point, however, is to change it. (1990, 22)

I find it impossible to take this passage seriously. What it amounts to is nothing other than a proposal for central economic planning, an idea that received devastating theoretical criticism from Mises and Hayek in the 1920s and 1930s and failed in practice everywhere it was tried. As John Ziman points out, if central planning will not work in the traditional economic realm, it certainly will not work in science: "Everybody now appreciates the practical impossibility of planning in advance, from a single centre, the routine manufacture of all manner of standard products to meet the foreseeable needs of a nation: it is scarcely credible that this approach could succeed [in science] where every item is novel, where the means of production are uncertain, and where the needs to be met are not even clearly conceived" (1994, 118).

I have pursued the contrast between idealized scenarios and mathematical models at length in this section in order to make clear that my use of the former and avoidance of the latter reflects a deliberate, principled choice. In the traditional economic realm, the existence of numerical data such as prices, total expenditures, and the unemployment rate lends at least superficial plausibility to mathematical modeling through which one might hope to relate, reproduce, or even predict trends in those data. In philosophical and related studies of scientific inquiry, however, the deployment of mathematical and logical formalism appears to offer little benefit by way of insight or application, and the display of technical virtuosity may lend undue credence to insubstantial claims and misguided policy recommendations.

An idealized scenario is of course a kind of model. Devising and exploring a verbally described scenario is a kind of model building. Because *model* is the term in common use, and because modeling in general is an important theme later in this article, I feel free to refer henceforth to an idealized scenario as just an economic model.

Similar Features of the Traditional and Scientific Markets

Specialization

Specialization naturally arises as individuals pursue their own self-interest. In the traditional market, each person can achieve greater prosperity through specialization and trade than through self-sufficiency. In the scientific market, specialization is the key to achieving recognition, for no one can hope to master all of science sufficiently well to produce results that will achieve recognition.

Although Francis Bacon is credited (Cohen 1985, 151) with originating the concept of division of labor, he was talking about scientific research, not business. Later, when Adam Smith discussed the division of labor in his *Wealth of Nations,* he counted intellectual specialization as a case in point:

> In the progress of society, philosophy or speculation becomes, like every other employment, the principal or sole trade of a particular class of citizens. Like every other employment too, it is subdivided into a great number of different branches, each of which affords occupation to a peculiar tribe or class of philosophers; and this subdivision of employment in philosophy, as well as in every other business, improves dexterity, and saves time. Each individual becomes more expert in his own peculiar branch, more work is done upon the whole, and the quantity of science is considerably increased by it. ([1776] 1937, 10)

Smith ([1776] 1937, 17) noted that the degree of specialization (the wellspring of enhanced productivity, in his view) is limited by the extent of the market, and Ghiselin (1989, 116–17) has pointed out that this insight applies equally well to science. The "extent of the market" means, of course, the extent of exchange—whether of goods and services or of research results and recognition.

If a larger market permits greater specialization, an increasing volume of knowledge requires it because the knowledge possessed by any individual becomes a progressively tinier fraction of the whole. Thus, it is often said that no one knows how to make even a common lead pencil. That is, "no single person . . . knows how to mine the graphite, grow the wood, produce the rubber, process the metal, and handle all the financial complications of running a successful business" (Sowell 1980, 48). Similarly, it may be that no individual scientist knows how to make a successful scientific model. A stellar astrophysicist, for example, devises a mathematical simulation of a supernova explosion. Can he reconstruct from scratch the body of nuclear and atomic theory; the experimental measurements of nuclear reaction rates and photon absorption cross sections; the astronomical observations involving photometry, spectroscopy, and astrometry; and all the other ingredients that go into such a model? Perhaps, but certainly not quickly enough to obtain results that will achieve recognition.

Exchange

Specialization is accompanied by exchange, for if one specializes in producing certain goods or services, the rest of one's needs must somehow be obtained from others. The traditional market is the arena for exchanges of goods and services. What sort of exchange, then, might be going on in the scientific market? Without intending to promote an economic analogy, Hull aptly describes a vital exchange mechanism underlying the cooperative enterprise of science as we know it:

> The most important sort of cooperation that occurs in science is the use of the results of other scientists' research. . . . Scientists want their work to be acknowledged as original, but for that it must be acknowledged. Their views must be accepted. For such acceptance, they need the support of other scientists. One way to gain this support is to show that one's own work rests solidly on preceding research. . . . One cannot gain support from a particular work unless one cites it, and this citation automatically confers worth on the work cited and detracts from one's own originality. (1988, 319)

Thus, scientists choose from among other scientists' published research that which they need as a basis for proceeding with their own work. The use of other scientists' work requires the *payment* of recognition in the form of citation. So we have a system of publicly offering, choosing from what is offered, and paying for what is used: in short, we have a market for exchange, and scientific inquiry is therefore properly viewed as a market process.

Investment and the Structure of Production

To make research contributions at the frontier, one must acquire techniques and background knowledge relevant to one's chosen area of specialization; that is, one must invest time and effort in the acquisition of *cognitive capital*.[1] For experimental work, further investment may be needed in the construction of apparatus (or perhaps in preparing grant proposals and in other lobbying efforts aimed at acquiring access to experimental apparatus). Such investment clearly parallels the capital investment needed to generate goods and services profitably in the traditional market.

Just as in the traditional market the payoff anticipated from various investments serves as an incentive to direct more investment into some areas rather than into others, so it is that anticipated recognition influences scientists' choices with regard to research topics. There are speculative investments in difficult problems offering much recognition but carrying the risk of complete failure. There are safer investments in problems offering modest recognition but nearly certain success. Opportunities suddenly arise for those who can respond quickly, as when an unexpected observation in astronomy elicits a flurry of theoretical papers and follow-up observations. Scientists make long-term investments as they develop a systematic research program and contribute steadily to progress in a particular area. If an investment is not paying off, the time comes to abandon it, cut one's losses, and invest elsewhere.

In the traditional economic realm, a process of production utilizing tools and machinery transforms raw materials through stages of partially finished goods into finished goods. Austrians have emphasized the importance of this "structure of production" ever since Carl Menger, the founder of the Austrian school, introduced the concept of lower- and higher-order goods in his *Principles of Economics* ([1871] 1994). In Menger's scheme, the consumer goods at the end of the chain of production are the goods of lowest order. Goods of higher order are those utilized in the production process: capital goods. Thus, goods of first order are brought together to produce the consumer goods; goods of second order are brought together to produce goods of first order; and so on. The point is not, of course, that we can assign every good to a specific order. The point is that we can trace the origin of each consumer good (and each capital good) back through a chain of production in which ingredients have been brought together and transformed at each step.

In science, we find a structure of production that, with a bit of interpretation, corresponds directly to that found in the traditional economic realm. Observational and experimental data are the raw materials that science acquires and transforms—through a process utilizing specialized instruments, techniques of analysis, and preexisting theory—into new or improved theory and tools for learning still more. In addi-

1. Rescher uses the term *cognitive capital* (1989, 4). Giere (1988, 214–15) and Hull (1988, 284, 514) refer to *cognitive resources*. Economists (Diamond 1988; Stephan 1996; Stephan and Levin 1992) tend to use the more generic term *human capital*.

tion to the existing scientific instruments and the literature available in journals and texts, a very substantial component of the stock of scientific capital lies in the human capital of expertise acquired in formal education, in research apprenticeship (thesis and postdoctoral work), and in lifelong self-education.

Scientific theory, equipment, and data must have the character of capital goods because they are utilized in the ongoing enterprise that adds to and improves existing theory, equipment, and data. Are we left, then, with a purely circular process in which the means of production are used to produce the means of production of still further means, and so on? One way to break out of the circle is to recognize that scientific knowledge is used in the production of consumer goods outside of science itself, in the traditional economic realm. Yet much or most academic science is pursued without thought of application, and some scientific results, particularly in particle physics and astrophysics, appear to offer no practical applications in the foreseeable future.

Scientists by and large are genuinely curious and eager for new knowledge about the world. Whatever delights and satisfies that curiosity is valued for its own sake; it is in effect a consumer good. Within our model of science as a self-contained competition for peer recognition, scientific knowledge therefore serves both as capital goods and as the goods of lowest order that are generated by the scientific structure of production. This dual nature is common in the traditional economic realm. For example, electricity and gasoline are capital goods when they power factories and farm tractors, but they are consumer goods when they power our televisions and recreational vehicles. In any case, once goods of lowest order are identified, circularity of the process is not an issue.

Consider an application of the structure-of-production concept that directly parallels the Austrian explanation of business cycles in traditional economics. Let us begin by recognizing that society's stock of capital comprises goods in various stages of completion as well as productive capital goods at various stages of their life cycle. Because of the time lag between investment and final output, this structure is more heavily concentrated toward the earlier end in a rapidly growing industry than it would be in a static or slowly growing industry. Consider an industry in which rapid growth has established the corresponding time structure of capital. Suppose now that demand continues to grow, but at a somewhat slower rate. Room remains for growth at the late end of the capital structure, but the reestablishment of a slower-growth structure requires liquidation of capital investments at the early end.

This example provides a very apt description of the boom-bust cycle that played out in academic science from the late 1950s to the early 1970s in response to changes in government research funding (Stephan and Levin 1992, 94–96). When growth in federal research and development (R&D) funding slowed, suddenly there was an oversupply of new cognitive capital, the human capital acquired through college and graduate study, so the job market for fresh Ph.D.s in the sciences collapsed. No cutoff in federal funding or even a decrease was required to generate pain and frustration among younger scientists as anticipated career paths failed to materialize—only a slowing in the rate of increase.

Entrepreneurship

The entrepreneur who intends to bring a new good to market faces uncertainty and possible losses because costs of development and production are necessarily incurred before the hoped for profit can be realized. Will this entrepreneur succeed in producing the intended good from the available resources? Will it be accepted in the marketplace and distributed widely? Will the payoff justify the investment, or has our entrepreneur mistakenly passed up a better opportunity in order to pursue this one? Will another entrepreneur get to market first with the same or a similar good and capture the profit?

The scientist seeking to make a significant research contribution faces remarkably similar conditions. Even though he eventually may complete the intended research successfully, someone else may obtain the same results first, or the results may lose significance because of advances on other fronts, or it may become apparent that the researcher's time could have been put to better use. The research results must also become known to and accepted by other scientists and attributed by them to our scientist-entrepreneur. Even if accepted for publication in a research journal or as a book, a contribution can be lost easily in the flood of papers and books published each year. Aggressive self-promotion may be necessary, particularly on the part of a younger scientist who has yet to establish a reputation.[2]

To the extent that neoclassical mainstream economics treats of entrepreneurship at all, the attempt is made to fit it into the same mold as goods and services in the marketplace in order that it may be accommodated within the professionally accepted practice of generating mathematical models of equilibrium. The assumption therefore is that there is a supply curve and a demand curve for entrepreneurship, and in equilibrium a certain amount of entrepreneurship is being generated. But because entrepreneurship has to do with economic change—with *dis*equilibrium—the neoclassical mainstream vision fundamentally distorts its nature and obscures its significance.

Consider two visions of entrepreneurship: one sees it as an "equilibrating" process, and the other sees it as a "disequilibrating" process. The former, associated with Israel Kirzner (1973, 1979, 1997), apprehends the entrepreneur as alert to errors—that is, to suboptimal allocations of productive resources. These errors present themselves as profit opportunities. The paradigm case is arbitrage, in which an alert individual notices that items being sold for a certain price would command a higher price elsewhere or in a reconfigured form. By taking advantage of the opportunity to buy low and sell high, the entrepreneur plays the role of middleman and establishes an avenue of trade that was needed but previously overlooked, thereby

2. As Overbye puts it in his popular history of modern cosmology, "The glory and honor justly go to those who are willing to stand up for an idea and commit themselves and their tenure prospects to the ego-grinding process of convincing their colleagues, pushing it on the colloquium circuit, and generally making noise about it" (1991, 202, see also 314–15). For a mildly cynical view of self-promotion in science, see Ghiselin 1989, chap. 10.

incrementally refining and improving the operation of the market. As such profit opportunities are exploited and errors thereby corrected, the market converges on a sort of optimum or equilibrium.

Suppose an entrepreneur discovers a low-price supplier of a particular good and a high-price purchaser for the same good. Once he takes and fully exploits the opportunity to serve as middleman, that particular opportunity is no longer available for entrepreneurial profit. The transactions that have been set in motion become part of the economic routine. What is true of arbitrage is true in this respect of other entrepreneurial action: to exploit a profit opportunity is at the same time to eliminate it as such (Selgin 1990, 38–41). The approach to equilibrium consists in a drying up of profit opportunities on account of entrepreneurship.

The *dis*equilibrating vision of entrepreneurship is associated with Joseph Schumpeter ([1934] 1983). According to him, true entrepreneurship consists in what he calls "economic development": the carrying out of new combinations of productive resources, the successful introduction of major innovations in the marketplace. Perhaps the paradigm case, from our viewpoint half a century after Schumpeter's death, is the introduction of the personal computer. Instead of reducing the number of profit opportunities, such innovations generate a vast array of new opportunities; rather than converging toward equilibrium, the market is displaced from equilibrium.

For Schumpeter, arbitrage did not count as entrepreneurship but fell within what he termed the *circular flow,* meaning the equilibrium of neoclassical economics. For Kirzner, on the other hand, the concept of arbitrage extends even to the introduction of a major new good in the marketplace; that is, acquiring factors of production and assembling them into a profitable new good are just means of exploiting an opportunity to buy low and sell high.

Let us adopt the Austrians' elementary concept of entrepreneurship: to seek to profit from economic change through efforts undertaken in anticipation of change or in an active attempt to initiate change. The result of such efforts, when successful, may be an incremental modification of the structure of production and its output that reduces the opportunities for further entrepreneurship along the same lines, or it may be a radical innovation that opens up an array of new profit opportunities. The claim here is not that every example must fit neatly into one category or the other; rather, at two ends of a continuum, we find two different kinds of entrepreneurship—or at least two different results of entrepreneurship. One kind has a Schumpeterian flavor, the other a Kirznerian.

Research contributions in science similarly may have either an incremental or a radically innovative character. Most research yields refinements and applications of established theory or experimental results that fit passably well with established theory. As each of these contributions becomes accepted, the opportunities for further research along the same lines are diminished. But some research developments (such as the theory of evolution, the discovery of radioactivity, the quantum hypothesis, and the structure of DNA) change the course of science by overturning established theory or by opening up entirely new experimental or theoretical research areas that attract substantial intellectual resources.

I am not claiming merely that science possesses an entrepreneurial character, but that scientific research is more essentially entrepreneurial in character than the marketplace activities traditionally studied in economics. One can imagine a world in which land, labor, and capital are used to produce the same goods in the same quantities by the same methods, by and for the same people, over and over. Productive economic activity continues, but the established routine persists, and thus no entrepreneurship occurs. By contrast, the very essence of science is the production of new knowledge or at least of new insights. Repeating the same experiments and theoretical analyses again and again does not count. One might imagine a situation in which all the loose ends are worked out in established theory and all scientific applications thereof are pursued to everyone's satisfaction, but in that case—in the absence of further change, of further scientific entrepreneurship—science as a process will have ceased altogether.

Organization

Relatively few individuals operate as independent entrepreneurs in the market. They find it advantageous to commit themselves to more formal, structured cooperation in partnerships and hierarchical firms—or, within science, in collaborations and research teams. In a famous paper, Coase (1937) explained that firms exist in order to reduce transaction costs, and the insight carries over directly to scientific research. Two or three scientists, especially if they bring complementary talents and expertise to a problem, may each be able to achieve more rapid progress—and more recognition—through collaboration and coauthorship than by working independently because direct communication is more efficient than the formal process of communicating through publications and citation.

On the other hand, collaborations of equals tend to break down if there are too many collaborators, in part because collaboration requires rather detailed and continued agreement on goals and methods, and in part because an equal sharing of recognition among coauthors may be unacceptable to those who believe they are contributing more to the final result. A hierarchical research team—in which the leader decides on goals and methods, selects team members, and distributes rewards—can accommodate a larger number of scientists effectively. Nevertheless, beyond some point the difficulty of monitoring individual performance and of acquiring and utilizing knowledge dispersed among the many members makes this form of organization also vulnerable to centrifugal forces.

Consider a small research team in which a professor directs several postdoctoral fellows and graduate students. Let us ignore money payments, such as stipends and tuition, because they are not essential to the analysis. Within the scenario of science as a self-contained competition for recognition, this team resembles a small firm operating in the traditional marketplace. The professor is like the proprietor who, by combining his own efforts with those of hired labor, seeks to profit more than would be possible by working alone. The "hired labor" consists of individuals who want to do scientific research and have been pursuing an education, which in its latter stages includes one or more apprenticeships with established researchers. In addition to the experience they gain from their work under the professor's direction, the postdocs and students obtain recognition from junior coauthorship of papers and the professor's recommendation for future positions. The professor obtains their assistance in completing substantial research projects quickly enough to obtain recognition. All parties stand to benefit from the exchange.

The considerations described in this section obviously parallel those in traditional economic studies regarding the existence and nature of the firm. (A discussion of Coase's work and of later work in this area may be found in Williamson and Winter 1991. For a discussion of the firm as it relates to the organization of scientific research, see chapter 9 of Wible 1998.)

Self-Regulation

Markets develop self-regulating mechanisms. For example, the peer review process for scholarly journals acts as a quality filter, a function performed in the traditional market by established wholesale and retail outlets as they select products to carry. Plagiarism and phony data are discouraged by the prospect of having one's future work ignored as unreliable, just as dishonest business practices and shoddy workmanship are discouraged by the prospect of losing one's reputation and with it one's customers (Ghiselin 1989, 135).

An element of conservatism stabilizes the market against wasteful fluctuations. Incremental innovation is encouraged, but radical innovation is inhibited because people have a large investment in established goods and methods of production (traditional market) and in established theories, models, and associated analytical techniques (scientific market). Radical innovation renders previous investments obsolete and eliminates the return (in money or continuing citation) derived from them. New investment is required in order to exploit the innovation, and in the scramble to do so the same individuals who were most successful previously (hence, most influential currently) may not come out on top again. (Roughly the same observation appears in Reder 1982, 20.)

Yet radical innovations are proposed, and some of them do succeed; the result can be sudden and sweeping change. Successful innovations in science make a transition from the research frontier—with its testing, competition, and uncertainty—to the core of accepted knowledge (Cole 1992, 15–17), as, for example, the personal computer made it through a shaking-out process in the marketplace from inventor's garage to nearly ubiquitous indispensability. Radical innovations in a given field often come from outsiders and from the young. Perhaps this tendency is owing to a fresher perspective on their part, but perhaps an element of incentive also is involved, in that newcomers to a field have less to lose and more potentially to gain from radical change (Stephan and Levin 1992, 43).

The activities of many specialists are coordinated through market interaction, so that the diverse ingredients of, say, lead pencils and supernova models do get assembled into their completed products. In the traditional market, coordination is facilitated by prices established in free trading, which convey essential information regarding the availability of and demand for various factors of production. For example, the physical and electrical properties of silver recommend it for use as electrical wire, but the price of silver makes clear that the combination of its scarcity and its alternative uses renders it unsuitable for wiring homes. Without market prices, to attempt such an evaluation for all the factors of production of even relatively simple goods is inconceivable.

In science, coordination is facilitated by the record of citation established in formal communication, which conveys information regarding the reliability and relevance of other researchers' contributions. The point here is not to try to draw an analogy between prices and citation, but only to appreciate that the great multitude of market transactions generates evaluative information in a form readily accessible to each participant. The citation record provides a set of formal reputations for researchers and their work. The most reliable and relevant work in a field or on a problem tends to be cited frequently by the most reputable researchers working in that field or on that problem. The most reputable researchers, by and large, are those who generate the most reliable and relevant work. It is very difficult for a scientist to make rapid progress in an established field without this sort of information.

Market Failure

The existence of self-regulating mechanisms might not prevent markets from malfunctioning. Putative failure conditions include market power and externalities. In the former case, a single producer or consumer (or small group) dominates a market and stifles the benefits of competition. In the latter, market mechanisms do not sufficiently capture or account for benefits and costs, resulting in underproduction of some goods ("public goods") and overproduction of others. Austrian school economists, one might notice, tend to be more skeptical of such concerns than mainstream economists.

If we stay within the simple model of science as a self-contained competition for collegial recognition, it is easy to imagine that the "market power" of a few dominant researchers or journal editors might suppress new ideas for a while. Nevertheless, in principle, dissenters can always break away and establish their own societies and journals, and the verdict of nature with regard to scientific theories cannot be evaded forever. If we bring into consideration, however, the need for external funding of research, then perhaps entrenched interests' control over the purse strings may block the carrying out and effective dissemination of research that would discredit the ruling orthodoxy. (Market failure in science, especially in the form of what I am calling market power, is extensively discussed by Wible 1998 and in the references cited therein.)

It is easy to identify apparent shortcomings in the institutional arrangements that have evolved for the conduct of scientific research. For example, the system relies on referees to screen research contributions for quality and originality, lest the journals be flooded with substandard and redundant offerings, but it provides little reward for the conscientious performance of this role. On the contrary, the near-universal anonymity of the referee appears to offer an opportunity to block one's competitors while stealing their ideas. We do not find in science the well-differentiated role of public critic that exists in the arts. Another valuable role that evidently falls through the cracks in the reward system is that of teacher, although teachers can at least hope for the gratitude of their students and take satisfaction in their later success. We may assume that a more effective reward system for referees and teachers will make all parties better off, but what specifically will the modifications be and how will they be implemented?

Again, recognition goes to *original* research, not to the replication of what has already been done. Does this shortchange an important contribution, that of replicating and thereby testing previous work? Does the importance of being first lead to unproductive priority disputes? When several researchers or teams are racing to solve a problem and one succeeds just ahead of the others, does the redundancy of effort constitute a waste of resources (Dasgupta and David 1994)? Such concerns appear throughout the historical, philosophical, and sociological literature on science; they are the perennial complaints about wasteful competition in a market economy transferred to science.

Defenders of an evolved market process offer the usual two responses: first, the competitive system is not so bad; and, second, the alternatives are likely to be worse. Thus, for example, competition among independent researchers actually may serve the purpose of replication fairly well, in that if one announces an incorrect result, others will have the expertise and the incentive to find the error (Leonard 1998). Moreover, aside from a few famous cases with Nobel Prize implications, competition in research does not typically have the character of a winner-take-all sweepstakes. Even where one person makes a fundamental advance, there is room for others to make solid contributions. Priority disputes are an imperfection of the competitive system, but perfection is not a likely option. Shall we empower a research czar to assign scientists to research projects? What drawbacks might that system have? Here, the point is not to consider and resolve such issues, but only to note that one encounters in scientific inquiry the same general concerns about market failure—along with the same general responses from defenders of the market process—as are found in the traditional economic realm.

The comparison may be carried yet further. Like business, science has been a realm of individualistic competition with highly disparate rewards, with an explosive growth of activity that emerged from Europe and spread around the globe, precipitating rapid change, taking root, and flourishing in some cultures much more than in others. These qualities have long made business ("capitalism") an object of disapproval in some quarters, notably among academics in the humanities. In recent years, similar complaints have been leveled against science: that, as practiced, science is undemocratic, racist, sexist, and culturally hegemonic; that it is dominated by and serves a power elite; that its rewards are unfairly distributed; that it is dangerously out of control. The point, again, is not to try to deal with these complaints here but only to note that they are the same complaints.[3]

Differences: A Market Without Money

The identification of separate provinces of human action—such as traditional economic activity, science, and politics—may be compared with the subdivision of traditional economic activity itself into different areas: agriculture, manufacturing, transportation, and so forth. The various segments of the traditional market have many similar features—prices, investment, specialization, and so on—as well as dissimilarities, which include, at minimum, the differences by which the segments of the market were distinguished from one another in the first place. By the same token, although we have found many similarities between the scientific and traditional markets, we cannot expect to find a useful parallel between every feature of the former and some corresponding feature of the latter.

It is tempting to try to draw a correspondence between money or currency in the traditional market and citation or recognition in science, as Barnes (1985, 43–47), for example, does. After all, citation does serve as a payment for the use of another's work, and the accumulation of sufficient recognition may be the means to other rewards, such as additional research grants, the security afforded by academic tenure, and a higher income. And, yes, the citation record does play a role roughly comparable to that of market prices in transmitting evaluative information.

Unlike citation, however, money is an *incremental medium* of exchange. In the traditional free market, a cash transaction takes place at a price agreed to by seller and buyer. If agreement is not reached, there is no transaction. A higher price demanded by a seller is likely to attract fewer buyers than a lower price. A higher price offered by a buyer is likely to attract more sellers than a lower price. Such considerations underlie the concept of supply and demand curves in traditional economics. In science, on the contrary, the "price" to be paid for using someone's work is fixed: one must cite that work. Although some citations may be worth more than others (citation by a Nobel laureate versus citation by an obscure researcher), it is not the case that the author of a paper can "hold out" for a higher price before permitting others to use its results.

3. For an explicit example, see Ross's (1996, 1–15) introduction to the *Science Wars* volume.

The origin and primary function of money lie in permitting transactions to be accomplished indirectly. Rather than attempting to trade goods and services directly through barter, people engage in intermediate transactions involving a durable, fungible, universally accepted commodity. Thus, money has arisen and has served people's purposes in a free market (Menger [1871] 1994, chap. 8; Smith [1776] 1937, 23–29). Citation and other forms of recognition do not serve as intermediate goods in indirect exchange. Recognition *is* the reward received when a scientist's work is cited. Recognition so achieved attaches to the individual as a badge of merit, and on that basis sufficient recognition may bring other rewards. One's reputation may fade with inactivity or may be forfeited through blunder, but it is not spent like money.

I also regard as potentially misleading a trend in the literature to characterize research contributions as the "intellectual property" of their authors and thereby to interpret citation as the purchase of a property right: a payment for the use of one's intellectual property (Ravetz 1971, chap. 8; see also Dasgupta and David 1987 as well as Stephan 1996). The results of research just completed may be submitted for publication, consigned to the flames, or stashed away at the scientist's discretion (assuming that no obligation to the contrary has been accepted, perhaps in return for financial support). At that point, those results may be viewed as the scientist's property. Submission of the results for publication initiates a process of exchange that is completed by the accrual of citations from other researchers. Nevertheless, once the findings have been submitted and published—in a telling phrase, "made public"—the author has no control over them. No one else can be excluded from using them, and the right to receive citation (the "income" derived from the "property") cannot be transferred to others. Having become a research *contribution,* the research findings can no longer be viewed as the author's property.

By contrast, patents and copyrights, which are intended to protect intellectual property rights in business, are exclusive and transferable. Indeed, someone (usually the author or the publisher) will retain copyright control over every research *article* in regard to its reprinting—say, in a book—but not over the research findings themselves.

Human interaction gives rise to evolving traditions of customary rights and obligations, from common law to common courtesy. People are customarily entitled to recognition for a great variety of achievements, performances, courtesies, and contributions to group endeavors. One is entitled to thanks for holding a door open, applause for a musical performance, congratulations for scoring the winning goal in a hockey game, and for a substantial charitable donation. Similarly, according to the etiquette or the recognized tradition that has developed in science, one is entitled to have one's research contribution cited when others make use of it. Nothing is gained by characterizing this entitlement as a property right because in the absence of exclusivity, of control, and of transferability the concept of "property" (in the sense of ownership) carries no explanatory or conceptual load.

Economics as a Critical Perspective

On Methodology

A prominent theme in the philosophy of science has been the articulation of a logical methodology to govern scientific judgment. (As representative examples, see Harre 1983 and Salmon 1967 and 1989.) What are the processes by which theories are to be tested and the criteria by which they are to be evaluated? How do we know when a theory has become well established, disproved, or superseded? How much confidence are we to place in a scientific inference? What constitutes a valid scientific explanation? By raising such questions, philosophers have shed light on the scientific enterprise. Yet there is no credible prospect of prescribing a set of rules by which scientists are to do science (or of capturing in a set of rules the way scientists actually do science). While philosophical schools debate perennial issues, science proceeds apace.

An economic perspective offers insight into this state of affairs because to see how a market economy works is at once to see limitations of logical methodology. Unlike methodology, which seeks to prescribe the correct judgment, the market takes advantage of differing judgments. People act on the basis of their individual judgments. Different judgments lead to different choices. Diverse options are explored, and the results can be compared. In science, this process generates settled knowledge that is not, as a matter of common experience, much amenable to further challenge on methodological grounds.

Cannot methodological considerations usefully inform individual judgment? Yes, but only with considerable uncertainty and subject to rapidly diminishing returns. As Feyerabend (1975) emphasized at length, successful scientists historically have broken all the rules and generalizations set down by philosophers. Furthermore, logical methodology utilizes explicitly articulated knowledge, but knowledge is in fact to some degree dispersed among many individuals, and much of it is tacit.[4] Articulating it and assembling it in one place, even if possible, must require much time and effort, which is to say in economic terms that the decision-making costs are high. The market embraces and utilizes dispersed and tacit knowledge and takes account of decision-making costs by providing incentives for timely as well as correct decisions.

Science rapidly generates information that humans judge to be true, and a market economy copiously produces goods and services that humans value. In both cases, the key element is individual liberty to produce and to choose. The rewards of recognition or financial gain provide incentives that steer productive activities toward that which human judgment validates.

It may be objected that an economic criticism of methodology begs the fundamental question of how to establish truth about the world that transcends mere con-

4. On tacit knowledge in science and in traditional economics, see Lavoie 1985 (chap. 3 and appendix) and references therein, especially Polanyi 1969 (105–80) and Hayek [1962] 1967.

sensus of human judgment. Economics indeed cannot answer this question—how humans are to establish truth about the world independent of human judgment—but neither, patently, can logical methodology do so. How can we possibly perceive truth aside from our judgment of what is true? Even the very question of what methods to adopt (or the prior question of whether further inquiry into a subject is worth one's time) can be, for humans, only a matter of human judgment, and that judgment is itself subject to revision on further reflection or experience. In traditional philosophy of science, proposed methodological prescriptions are rejected again and again on the basis of counterexamples, but the very acceptance of a counterexample as such implies a primacy of human judgment over fixed methodology.

The point here is not to equate truth with what humans judge to be true, but rather to suggest that humans must judge, and judge individually, whether the evidence is convincing, whether an argument is valid, or whether to accept the voice of authority (and, if so, whose). We must make choices, and it is a specifically economic contribution to illuminate trade-offs associated with different choices, with different ways of choosing, with different social, cultural, and institutional frameworks within which choices are made.

Cole (1992) contrasts the logical methodology of the traditional ("positivist") philosophy of science with the relativism of a recently influential group of sociologists known as social constructivists. On one hand, "According to positivist philosophy of science, the objective validity of a scientific contribution could be determined by using a set of rules to evaluate evidence" (5). Relativism is antithetical to such a view. On the other hand, "social constructivists . . . believe that the substantive content of scientific theories is socially determined or constructed." Moreover (and this is the statement of a radical relativism), "They argue that the empirical world has little, if any, influence on what is accepted by the scientific community" (229). The picture (perhaps something of a caricature) one gets is that whereas traditional philosophers have placed so much emphasis on logical methodology that scientists as human beings have disappeared from their treatment, social constructivists have placed so much emphasis on social interaction that the external world studied by scientists seems almost incidental.

The parallel between science and traditional economic activity provides a concrete, familiar, external perspective from which to examine the issue of relativism in science. Consider the structure of production and the array of consumer goods in a market economy. That they are socially constructed and perhaps to a significant degree socially determined does not imply a minimal or nonexistent role for the external world. Yes there is a contingent element. People could have chosen differently. There might be more rail transport, with fewer cars, trucks, and major roads. Clothing styles might be entirely different. But enduring characteristics of nature ensure that elephants will not be employed to haul fresh vegetables across continents and that people in cold climes will not insulate their clothing with steel wool.

Similarly, scientific theories and models are indeed constructed and tested through social processes, through which they are either discarded or make their way from the research frontier to the core of accepted knowledge, and through which they may be brought to bear on a range of scientific and technological problems. It defies credulity, however, to suggest that the results of this process are little constrained by the properties of the external world.

From an economic perspective, relativism may be a less significant issue than either its detractors or its proponents suppose. Economics points out that where beliefs have consequences for the pursuit of human goals, incentives tend to modify beliefs. If people who rely on magic end up less affluent, healthy, and secure than people who rely on science, an incentive exists to abandon magic in favor of science. This tendency is a selection process that acts on individual judgment as well as on the shared practices of groups and the knowledge-generating and knowledge-applying institutions of societies, resulting in an evolution from less successful to more successful judgment, practices, and social institutions.

Similarly, if the properties of the external world matter to the application of science in the service of human purposes, then those properties will tend to constrain very effectively the results of scientific inquiry if there are mechanisms through which the success of practical applications can influence the conduct of science itself. The industrial funding of research is such a mechanism. That such discipline does not appear to be highly attenuated in areas of science far removed from application may be attributed at least in part to the overlap of research specialties (or "neighborhoods"; see Polanyi [1967] 1969, for example). In effect, the areas of expertise of the many individual scientists link up and extend a network of mutual oversight across the broad range of the sciences.

In sum, to the extent that logical methodology is intended to contribute something useful to scientific inquiry, it is a tool, a resource—and economics has something relevant to say about the effectiveness of tools and resources. In the case of scientific inquiry, it says that competition in the marketplace of ideas makes use of dispersed and tacit knowledge, including the diversity of methodological and theoretical opinion among scientists, and brings it to bear in a way that appears to leave little scope for the effective implementation of a prescriptive methodology. If scientific practice may be enlightened by thoughtful reflection on the history and methods of science, there is nevertheless no substitute for individual judgment. Where individuals are free to pursue their goals and to judge for themselves whether those goals have been satisfactorily met, relativism is an issue of questionable significance.

On the Darwinian Analogy

The evolution of economic activity in a market possesses many of the general features of Darwinian evolution, thus making the latter a suggestive analogue for the development of science (Callebaut and Pinxten 1987; Hull 1988). Both involve a blend of cooperation and competition. In both, a process of selection operates on continually arising "variations": biological mutations or economic innovations. Economic activity and scientific inquiry, however, are realms of human action; Darwinian evolution is not. Hence, it should not be surprising that the major *dissimilarities* between economic development and Darwinian evolution favor the former as an analogue for science.

In the market, as in science, innovations are proposed purposefully by human beings. They do not arise randomly as genetic mutations do. The selection process, too, involves purposeful choices by human beings. Nothing either in science or in the market is comfortably analogous to the genotype/phenotype dichotomy in biology, in which the propagation or extinction of the genotype depends on the success of the phenotype in survival and replication, with the genotype remaining insulated from the experiences and acquired characteristics of the phenotype.

Just as, in principle, publication makes information available to all of one's fellow scientists, so offering goods for sale makes those goods available to all of one's fellow human beings. Both science and the market benefit directly from their failures as well as their successes because human beings note the failures and take account of them in making their future plans. By contrast, genetic inheritance is available only to an organism's descendants. Thus, nature "learns" only from its successes because the failures die, leaving behind no genetic information.

On Mertonian Norms

Human interaction gives rise spontaneously to behavioral norms, including customs, rules of etiquette and grammar, and the common law. Norms are inculcated through example and become internalized, representing a tacit consensus about expected and acceptable behavior, though they may to some extent be identified and articulated explicitly. Violating the norms may generate confusion, distrust, and hostility among others who expect compliance. Even in the absence of explicit sanctions, a violator risks some degree of ostracism from that web of human interaction we call society. Sixty years have passed since sociologist Robert Merton identified "the normative structure of science" ([1942] 1973). Others, such as Storer (1966), have elaborated extensively on the theme. In what follows, I rely largely on Ziman's (1984, chap. 6) pedagogical formulation and discussion of the Mertonian norms.

Ziman identifies five norms: *communalism* ("Science is public knowledge, freely available to all" [84]); *universalism* ("There are no privileged sources of scientific knowledge" [84]); *disinterestedness* ("[Scientists] should have no personal stake in the acceptance or rejection of any particular scientific idea" [85]); *originality* ("scientific research results should always be novel" [85]); and *skepticism* ("Scientists take nothing on trust" [85]). These norms "define an *ideal* pattern of behavior, which scientists should endeavor to follow" (1984, 87, emphasis in original). Obeying them is claimed to be a matter of morality. In his 1942 paper, Merton speaks of "mores," an "ethos" of science, a "scientific conscience," a "moral consensus," "moral indignation," and a "moral compulsive for sharing the wealth of science" ([1942] 1973, 269, 274). Storer speaks of "sins" (1966, 86, 101) for which people are "punished" (86) and of the "moral failure" of applied research (112) because it violates the norm of disinterestedness.

But how much of scientific behavior is truly a matter of morality? To be sure, annoyance and resentment may arise when someone does not behave as expected or desired. I am not denying the relevance of moral considerations altogether. Honesty stands as a virtue in science as in other human activities; representing someone else's work as one's own is simply fraud. Nevertheless, aggressive self-promotion, polemical disputes, and even "sharp" practices are far from rare in the research world, and their significance may not be entirely negative. Hull observes that "the least productive scientists tend to behave the most admirably, while those who make the greatest contributions just as frequently behave the most deplorably," and he even argues that "the existence and ultimate rationality of science can be explained in terms of bias, jealousy, and irrationality" (1988, 32).

An invisible hand explanation? Indeed, many of the behavioral regularities ascribed to norms (*and* to behaviors that beneficially violate putative norms) can be understood straightforwardly on the assumption that scientists pursue their own self-interest in the form of professional recognition. For example, the norm of communalism, according to Ziman, demands that "[s]cientific discoveries should be communicated immediately to the scientific community" (1984, 84). Clearly, however, the self-interest of a scientist provides an incentive to publish quickly, for to delay is to risk losing recognition if someone else publishes the same findings first. True, there also may be incentives to maintain secrecy for a while. By withholding results until they can be thoroughly checked and confirmed, the researcher avoids the embarrassment of a faulty publication and the annoyance of fellow researchers who may have been misled for a time. (See, for example, Smoot and Davidson [1993, 246–49, 270–72] on how the Cosmic Background Explorer team kept its findings in strict confidence for months until the results were secure.)

Less benignly (or seemingly so), an incentive may exist to withhold findings until they can be "milked" for additional recognition-generating advances. Perhaps the most famous example of such intrigue is portrayed in James D. Watson's (1968) candid account of the discovery of the structure of DNA. From a position outside the process, it might be easy to criticize secrecy as counterproductive on the assumption that a communalistic sharing of findings permits those who can use them most effectively to do so. On the other hand, Schumpeter's (1950) discussion of monopolistic practices in capitalism suggests what is arguably a more sophisticated view—namely, that the availability of such practices in a competitive system may permit producers to move forward more rapidly into uncharted territory, knowing that if successful they can hold off competitors long enough to secure a sufficient return on their investment. Restriction of output is among the practices that may, in Schumpeter's words, "protect rather than impede . . . a long-run process of expansion. There is no more of paradox in this than there is in saying that motorcars are traveling faster than they otherwise would *because* they are provided with brakes" (1950, 88, emphasis in original).

The norm of universalism, according to Ziman, requires that "discovery claims and theoretical arguments should be given weight according to their intrinsic merits, regardless of the nationality, race, religion, class, age—or scientific standing—of the person who produces them" (1984, 84). He suggests that the "tendency for groups of specialists to discriminate against the opinions of outsiders and laypersons" is inconsistent with this norm. It is, however, a matter of self-interest for a scientist not to ignore the work of others on the basis of irrelevant personal characteristics, for to do so is to cut himself off from potentially useful information. On the other hand, given that time is limited and the scientific literature is vast, it makes sense for the scientist to concentrate attention on the work of those whose contributions seem most likely to be valid and useful: specialists in the same field. In a free market, scientific or otherwise, people have an incentive to "do business" with others on the basis of strictly "business" considerations.

The norm of disinterestedness, according to Ziman, "forbids any open manifestation of the psychological commitment that scientists usually feel to their own discoveries" (1984, 85), and he warns that without this norm "the scientific communication system might be opened to straightforward advertising" (87). It is, however, a matter of self-interest for individual scientists to maintain a professional demeanor that will not undermine their colleagues' trust in their objectivity and honesty. Similarly, rejecting advertising may be as much a matter of credibility, hence self-interest, for a scientific journal as it is, say, for *Consumer Reports* magazine.

Is the "tragic experience" of Soviet genetics under T. D. Lysenko truly "direct evidence of what can happen when the norm of universalism is not respected" (Ziman 1984, 87), or is it more incisively viewed as an example of government interference in science? Do we really need the norm of originality to prevent scientists from "spend[ing] their time ritually performing old experiments" (87), or can we rely on the self-interest of scientists who know that journals will not publish the results of such experiments because their readers will not be interested in them?

I am not claiming that no norms exist or that norms do not to some extent serve some of the functions that have been attributed to them. The successful operation of markets must rest to a significant degree on the observance of behavioral norms. Sociologists, for their part, will hardly deny that the effectiveness of social sanctions in the enforcement of norms rests at least to some degree on individual self-interest.

Nevertheless, it must not be supposed that the Mertonian-sociological and economic perspectives converge happily on some middle ground where social norms and individual self-interest get equal billing as behavioral determinants. Consider an oft-quoted passage from Merton's 1942 article: "The institutional goal of science is the extension of certified knowledge. The technical methods employed toward this end provide the relevant definition of knowledge: empirically confirmed and logically consistent statements of regularities (which are, in effect, predictions). The institutional imperatives (mores) derive from the goal and the methods. The entire structure of technical and moral norms implements the final objective" ([1942] 1973, 270). In this view, science has a goal, and norms are the mechanism whereby individual human beings are directed to serve that goal. By contrast, from an economic perspective, individual human beings have goals, and science is but one of the realms of human action in pursuit of those goals. In this view, if norms and other regularities of human interaction can be said to have a purpose, it is to facilitate the pursuit of individual goals. An abyss separates these two perspectives.

The Economic Structure of Scientific Revolutions

Thomas Kuhn's (1970) historical interpretation of scientific change has been widely debated, and it has been profoundly influential. I assume the reader's general familiarity with Kuhn's work because to attempt a coherent summary of both it and the discussion it stimulated would carry us well beyond the scope of this article. My aim here is to consider some of the key elements of Kuhn's thesis from an economic perspective. Much of what has appeared problematic in that thesis gains cogency via reinterpretation in economic terms, particularly in conjunction with a modeling approach to the philosophy of science.

Kuhn's Political Metaphor

In chapter 9 of his *Structure of Scientific Revolutions* (1970), Kuhn develops a metaphorical parallel between scientific and political revolutions. According to him, a crisis arises in which a challenger confronts the prevailing theoretical paradigm (or political regime). A definite choice is required, and if the new paradigm wins, then the old one is abandoned. Kuhn emphasizes the *breakdown* of the old paradigm and suggests that people are forced to try something new. He observes that when political recourse fails, "the parties to a revolutionary conflict must finally resort to techniques of mass persuasion, often including force" (93).

However, even if one were to grant that major changes in science are aptly characterized in terms of crises and confrontations between old and new, surely it is not the case in science, as it is with political revolutions, that the outcome is decided coercively or by some extraordinary process of ratification. Instead, it is decided by the free choices of many interacting individuals, as is the outcome of competition in a market economy. Indeed, the very aim of political action is control over the coercive power of government, whereas nearly everyone agrees that coercion has little or no proper role in science.

Kuhn rigidly links adoption of a new paradigm with rejection of the old, claiming that "the assimilation of all new theories and almost all new sorts of phenomena has in fact demanded the destruction of a prior paradigm" (96), and he uses the triumph of relativity theory over Newtonian mechanics as an example. Appealing to a comparison with the defunct phlogiston theory of heat, he emphasizes that Newtonian mechanics must be regarded as *wrong* (98–100).

Newtonian mechanics, however, is still a vital part of both the physics curriculum and the toolkit of scientists and engineers, and it is still being developed and applied in new ways. The study of chaotic phenomena arose out of Newtonian mechanics, after which physicists struggled to find chaos in quantum theory. The political metaphor does not allow for such long-term coexistence and independent development of old and new theories, but an economic parallel does. Televisions and jets represent technological advances over radios and propeller-driven aircraft, but the latter still have their uses and are still being refined. On the other hand, many old products really have been abandoned, such as slide rules, which were completely displaced by electronic calculators. An economic interpretation remains apt, regardless of whether a surpassed theory is discarded or continues to be utilized and developed.

Kuhn affirms that in science "there can be small revolutions as well as large ones" (49), but then he himself undermines the parallel with political revolutions (as he appears to acknowledge on page 92). *That* parallel seems apt only as long as we focus on a few cataclysmic episodes. Radical changes in theory and practice, however, take place continually on all scales, from the narrow research specialty to whole sciences and beyond. Such a seething cauldron of discovery and innovation is better captured by comparison with entrepreneurial activity in a market economy than by the metaphor of political overthrow.

With regard to "the" Scientific Revolution that climaxed with Newtonian mechanics in the seventeenth century, I. B. Cohen's *Revolution in Science* (1985) provides additional reasons to prefer an economic parallel over a political one. Cohen points out that "unlike political revolutions but like the Scientific Revolution, the Industrial Revolution was spread over a long period of time, covering some seven or eight decades in two centuries" (266). Furthermore, "The Industrial Revolution resembles the Scientific Revolution also in the way in which some historians have tended to see both revolutions as continuing processes, lasting up to the twentieth century or even to our own days" (268). Cohen also says, "The process of continual change was institutionalized in the form of journals for the publication of results, repositories for the registration of discoveries to ensure priority, and prizes for the most revolutionary advances. I know of no other revolution, or revolutionary movement, which so institutionalized the continuing process of revolutions to come" (83). But the Industrial Revolution also brought about the same sort of outcome, as legal institutions sprang up to facilitate the continuance of dynamic economic change: free trade, joint-stock companies, protection of property and enforcement of contracts.

Normal Science Versus Scientific Revolutions

According to Kuhn, during periods of "normal" science, established theory is being refined, articulated, and applied. During a "revolutionary" episode, established theory is overthrown, and a new theory becomes accepted. These two aspects of change in science match up well with the two visions of entrepreneurship—incremental and innovative—discussed earlier.

Thus, where Kuhn (1970, 80) tells us that the object of normal science is to solve puzzles for whose very existence the validity of the prevailing paradigm must be assumed, we may note that an arbitrageur works quite similarly within an existing market structure, as does the small business proprietor who observes a growing demand for a particular service and moves in to take advantage of increasing prices, and as does the manager who finds ways to improve efficiency by incrementally reorganizing the workplace.

Turning to scientific revolutions, Kuhn remarks that "if a new candidate paradigm had to be judged from the start by hardheaded people who examined only relative problem-solving ability, the sciences would experience very few major revolutions. . . . [T]he issue is which paradigm should in the future guide research" (157). Let us note that, in a similar situation, if start-up firms had to turn a profit from their first day, far less entrepreneurship would take place in the market economy. Many such firms gain support from their promise, even though they may not reach profitability for years.

Kuhn remarks, "The man who embraces a new paradigm at an early stage must often do so in defiance of the evidence provided by problem-solving. He must, that is, have faith that the new paradigm will succeed with the many large problems that confront it, knowing only that the older paradigm has failed with a few" (158). Replace *paradigm* by *innovation,* and do we not have the faith of a Schumpeterian entrepreneur, who bets everything on his vision and works for years to bring it to fruition?

Perhaps the essential common element between a revolutionary new Kuhnian paradigm and a major innovation in business is that for their success both require a substantial change in the prevailing structure—of theory or of production. They both require a reallocation of investment, a revision of the productive routine, and above all a choice by many individuals to adopt, to work with, and to align their plans with the requirements of the new paradigm or the innovation. There is no *incrementally* profitable way for our entrepreneur—or our entrepreneurial scientist—to get from here to there. As Schumpeter puts it, we are talking about "that kind of change arising from within the system *which so displaces its equilibrium point that the new one cannot be reached from the old one by infinitesimal steps*" ([1934] 1983, 64, emphasis in original).

Insights from the Modeling Approach

The economic perspective complements in a very congenial way the modeling approach to the philosophy of science that Giere (1988) introduced and that Teller (1999) further developed and promoted. Proponents of the modeling approach reject the traditional view that science has as its object the discovery of exact, general, exceptionless laws of nature—that it aims at representations consisting of statements that are true of nature itself (what I call "thing-itself" truth). Rather, science generates models of nature, and scientific laws are true of the models. The relationship of the models to nature is one of similarity in certain respects and to certain degrees. How is that similarity to be judged? As Teller has suggested, it must be on the basis of how well the models serve the goals and purposes of those who construct and utilize them.

Science brings us truth about the world, but not thing-itself truth; it brings us, rather, the truth that certain models serve our purposes in certain respects, to certain degrees. Models may enable us to predict, to build technology, and to control nature. They may gratify our sense of curiosity. And, I would add, they may bring us professional recognition if our colleagues judge them favorably. We as individuals choose our purposes and judge whether and how well a model has served them. Choice in the pursuit of goals: a quintessentially economic matter.

From this perspective, scientific revolutions take on a wholly different character. Kuhn saw anomaly, a deepening sense of something gone awry, a crisis in which people resort to desperate measures. From the standpoint of modeling, it is only to be expected that our models have limitations. Refinement, articulation, and application of a promising new model (or theory as model-building kit) may pay large dividends, but eventually diminishing returns are encountered. For scientists who have pursued their entire careers within a given theoretical framework, who have seen it refined and applied with success upon success to inspire confidence in its correctness, it may well be an emotionally wrenching experience to find that nature, beyond some point, refuses to conform to that framework. If there is frustration, however, there is also opportunity: for the more entrepreneurial of scientists, the potential gains of trying something new outweigh the risk of failure and the forgone opportunities remaining within the old model.

Nevertheless, we have no reason to assume that a new model must completely displace the old to be successful. Newtonian mechanics, special relativity, and nonrelativistic quantum mechanics have survived and find much use today. And yet it also may happen that an old model—for example, phlogiston—is left without any advantages and is thus abandoned.

From this perspective, problematic Kuhnian claims invite reformulation as commonsense observations. For example, Kuhn originally saw crisis as a necessary precondition for scientific revolution and went so far as to equate rejection of the prevailing paradigm (during normal science) as rejection of science itself. The commonsense observation is that proposals for radical change are unlikely to receive consideration while rapid progress is being made within currently accepted theory; until diminishing returns are encountered, proponents of radical change are likely to be ignored and thus marginalized.

According to Kuhn, proponents of rival paradigms belong to different language communities, their communication hampered by incommensurability of meaning—where the same words may be used to mean quite different things, where observations are interpreted so differently (and theories judged by such different standards) that there exists no solid ground on which to debate. The commonsense observation is that a one-to-one correspondence need not exist between the elements of different models or between the elements that are referred to by the same terms. Moreover, because what is at stake is not isomorphism but similarity with nature, which may be in different respects for models that were generated with different purposes in mind, it may be no straightforward matter to sort out the relative merits of competing theories.

Where Kuhn saw the switch from old to new paradigm as a nonrational conversion experience or Gestalt switch (1970, 111–12, 150–51, 204; 1973, 338–39), let us simply note that detached weighing of evidence pro and con may not always provide a sufficiently rich experiential basis for choice. Immersion in a new model for a time may be needed to get a feel for how it works, *that* it works, and what purposes it may serve well.

Philosophers have strongly criticized the *Structure of Scientific Revolutions* as denying the rationality of science. To be sure, the book advances a number of theses—the incommensurability thesis in particular—that are famously pessimistic with regard to the traditional vision of scientific rationality: that scientists open-mindedly entertain competing theoretical hypotheses, test them observationally and experimentally through rigorous application of universally valid standards of evidence, and thereby arrive at or progressively approach truth. In the absence of definitive criteria for theory choice, what guarantee is there that scientists get the right answer? How do we know that science progresses? How secure is the boundary between scientific knowledge and nonscientific and pseudoscientific beliefs?

One cannot cogently criticize a work, however, for failing to satisfy standards that are themselves untenable or highly problematic. As Kuhn noted nearly three decades ago in "Objectivity, Value Judgment, and Theory Choice" ([1973] 1977), even at that time little optimism remained for the philosophical project of establishing a rigorous logical methodology to govern scientific inference. Scientists do share a number of values that they look for in a theory, but they differ in their interpretations and applications of the values and in the relative emphasis they place on each value. In fact, it is essential that scientists have room to disagree so that they will explore and evaluate diverse options (as I noted earlier, in discussing logical methodology). Furthermore, the traditional ideal of scientific progress, whereby science approaches more and more closely the truth about nature itself, is rendered suspect by the actual history of physics: from Newtonian mechanics to special and general relativity to quantum mechanics, the sequence of incompatible de facto ontological commitments simply defies the portrayal of science as converging toward any stable picture.

The perennial philosophical agenda of grounding scientific judgment in formal logic can succeed only to the extent that our models and observations of the world can be captured in statements possessing rigorous logical relationships. If A implies B, then not-B implies not-A; letting A be a theory and B an observational prediction deduced from the theory, the inference yields a brittle falsificationism—itself a crude model that offers a modicum of insight into one aspect of scientific judgment. After decades of being elaborated and hedged about with qualifications to overcome counterexamples, such logical constructions cannot capture much of the subtlety of human judgment that is demonstrated in the actual deployment, testing, and application of scientific models, much less command allegiance as a standard of rationality.

The preceding paragraphs should suffice to identify a traditional ideal (too familiar to be dismissed as a straw man) of scientific rationality that Kuhn rejected and that I would not care to build on, for the reasons indicated. I do not see where Kuhn himself came up with an adequate vision of scientific rationality, and I attempt no survey of other philosophical efforts toward that goal. In the next section, I briefly and somewhat speculatively offer a vision of rationality that flows from economic and modeling-approach insights, illustrated via a rudimentary sketch of an economic model of the scientific enterprise.

The Rationality of Science as Economic Rationality

That individuals choose goals and action in pursuit of their goals is the basis of economic analysis. From an economic perspective, rationality has to do with the effectiveness of action in utilizing the available means to achieve those goals. Mises ([1949] 1996, 19) took the position that all action is rational because people will do the best they can, with the background knowledge they possess and the resources and technology at their disposal, to achieve their goals. If individuals appear to behave irrationally, it may be because their background knowledge is inadequate or because we are misinterpreting their goals or because they are continually setting aside long-term goals in order to pursue immediate goals. The worthiness of the goals themselves is not a matter for economics to judge. That saints, criminals, entrepreneurs, and bums choose action in pursuit of their goals makes their action amenable to economic analysis and, in that sense, rational. Individual choice directly implies that satisfaction is equally a matter of individual judgment. To the extent that individuals differ in their goals, in the resources they can bring to bear, and in their judgments of outcomes, a diversity of rational choices exists for different individuals facing ostensibly the same alternatives.

Nevertheless, even given the same background knowledge, resources, technology, and goals, some individuals may be able to project more accurately the outcomes of different alternatives and thereby select more effective action. Furthermore, the institutions of some societies may influence and constrain the choices of individuals in such a way that their efforts are better coordinated and their goals typically better served. Better individual judgment and better institutional coordination lead to a more effective application of knowledge in the service of individual goals and, in this sense, reflect a higher degree of rationality.

In a radical departure from traditional philosophical views of scientific rationality, we are starting with what is similar about science and other human activities rather than with what is purportedly unique about science. To both Kuhn and his philosophical critics, science was a special pursuit, carried on by a special community, its rationality closely tied up with what distinguishes the scientific community from other communities. For traditional philosophy, this distinguishing feature is the employment of logical methodology; for Kuhn, it is the possession of a shared paradigm. By contrast, from an economic perspective, science is just one of many realms of human action in the pursuit of individual goals, and the rationality of science is nothing other than the rationality of human action generally—nothing other than economic or praxeological rationality.

To devise an economic model of science requires that we narrow our view to that field; adopting the modeling approach, I take science to be the realm of human action in which intellectual models of nature are constructed and evaluated. (This statement is not intended as a rigorous definition, and I doubt that any strict demarcation can be drawn between science and nonscientific activities.) We also need to postulate what goals are being pursued within that realm. Earlier I adopted as a first-order approximation that scientists seek professional recognition from their peers. I now move beyond that limited assumption in two ways: first, scientists seek to understand nature, and they evaluate research contributions with regard to the sense of improved understanding that those contributions afford; second, scientists seek resources from society at large to support their research, and people in society at large value the technological advances that science makes possible. The first assumption identifies a basis for peer evaluation, and the second assumption connects the purposes of scientists with those of people in the wider community.

In taking into account that scientists seek to understand nature, I refer not to understanding the way nature actually is ("thing-itself" knowledge), but rather to the psychological satisfaction of sensing that one understands. We emphatically do not have direct access to nature itself, to the external world, against which to check our sense of understanding. To assume that we do have such access leads trivially to self-contradiction, for in that case we must accept as true the finding of physiological psychology that all we have are perceptions resulting from complicated neural processing of electrical signals from the cells of sensory organs. (To deny the results of physiological psychology looks like a dead end to me.) Our *sense* of understanding nevertheless serves our purposes with some degree of reliability, and the theory of evolution obviously suggests why. From an evolutionary perspective, the significance of perception is that it informs action. Our perceptual neural processes are subconsciously constructing models of the environment. At some point, a judgment must be reached that the perceptual apparatus has obtained a model that is suitable as a basis for action. That the processes that yield this imprimatur operate for the most part beneath the conscious level helps to explain why we so uncritically accept the truth of our perceptual models of macroscopic objects in our everyday environment.

Kuhn correctly emphasized the relevance of cognitive psychology, and he was on to something important with his use of Gestalt-switch experiences as an analogue to theory choice. Gestalt-switch diagrams—in which, for example, an intersection of lines may be perceived alternately as an indentation and as a protrusion—reveal the continuous operation of subconscious modeling processes and indicate that they are primed to affirm one model or another as representing the actual state of affairs, somewhat independently of analysis at the conscious level.[5] (Presumably, our distant ancestors did not have the luxury of sedentarily questioning and debating the foundations of their inferences.) Is it not likely that our very concept of "truth" as thing-itself knowledge of the external world has its origins in the certainty that a model will be reliable as a basis for action? Our sense that there must *be* a "truth of the matter" must likewise be rooted in the action-oriented nature of subconscious modeling processes. *Conscious* construction and testing of models may be an extension of processes already in operation subconsciously.

We are also taking into account that scientists desire resources to carry out their research, and if science generates practical benefits, it will attract investment from the broader community. Concern is often raised that the operation of these incentives, particularly in a laissez-faire market, can lead to overemphasis on applied research, thus distorting scientific priorities and inhibiting progress; the favored remedy is no-strings funding of pure science via research grants based on peer review. Whatever the merits of this remedy as government (or corporate) policy, in such discussions one generally senses an implicit assumption that the direction of rationality points entirely from science to technology. The methods of science draw it toward truth about nature, much as gravity draws water downhill; in turn, truth about nature finds application in technology. Any diversion of science from its free course is likely therefore to slow the acquisition of knowledge and inhibit technological advances in the long run.

If the truth of science follows from the methods of science, however, what ensures that the methods are correct? We cannot appeal to the truth of science itself without falling into circularity. Either the correctness of the methods is independently established and whatever those methods yield is to be regarded as truth, or we establish the truth independently of the methods and then adjust our methods until they yield this independently established truth. Given the inadequacies of logical methodology and our lack of direct access to the "truth," neither approach looks promising. Economics takes us out of the circle. An individual's judgment of his or her satisfaction is absolute. It is the bedrock of economic rationality. Fulfillment of individual goals reflects back on choice of action, on method.

5. Video games provide a more recent and striking example of how our brains create a reality out of sensory signals. Electric currents in a computer cause changing patterns of light to be displayed on a screen in response to internal programming and inputs from a hand-held controller. But ask a player what he is doing, and he might respond, "I'm trying to find the magic sword, so I can slay the dragon and rescue the princess."

Thus, in disciplining the methods of science, the technological connection takes its place alongside the desire of scientists for understanding and for peer recognition. Because of the interconnectedness of overlapping research specialties, the disciplining influence propagates even to realms of science somewhat removed from practical application. The technological connection thereby links the goals of scientists with the goals of people in the wider community, providing incentives for each in his own way to serve the interests of the other. Technology serves as the ultimate argument against those who challenge the authority of science and champion "other ways of knowing." Show us the shaman who can conjure up vaccines and lasers!

From a self-described "Marxish" perspective, Railton ([1984] 1991) arrives at the strikingly similar conclusion that science acquires both impetus and discipline from its economic connection to society in a market economy. In his words,

> the rise of capitalism gives enormous impetus and scope to the pursuit of inquiry in ways that increase the possibility of receiving and responding to causal feedback from natural phenomena.
>
> Thus, even if one accepts Marx's view that modern science serves the interests of capital and owes much of its shape and success to the development of modern commerce and industry, science can be regarded as attaining substantial objectivity in virtue of its particular position and function within the capitalist division of labor. (769)

Such congeniality of view may be surprising, given that Marxism and Austrianism (my own starting point) are widely considered to lie at opposite poles of economic thought. But Marx himself acknowledged the productivity and dynamic character of capitalism; his polemic was directed against the exploitation and alienation of workers and the inequality of incomes. Let Railton's words therefore give pause to any readers who might be inclined to dismiss the economic perspective of the present work as some figment of laissez-faire ideology.

Suppose that science were freed from the technological connection. The economic model suggests that scientific inquiry might then undergo something metaphorically akin to genetic drift, evolving into practices that demonstrate less propensity to contribute, via technological applications, to the fulfillment of goals in the wider community—practices more closely attuned to peer recognition and a sense of understanding. There are intellectual realms that, from the vantage point of science as we know it, lie in that direction: philosophy, theology, and literary criticism come to mind.

To conclude in a speculative vein, I suggest that a classical realm of truth arises out of modeling in much the same way that classical mechanics arises out of quantum mechanics. Both classical and quantum mechanics are models of nature, but physicists consider the latter to be more fundamental. Where the deBroglie wavelengths of particles are insufficiently small compared to the relevant scales of interaction or of confinement, we are obliged to use quantum mechanics and to forgo attaching any meaning to certain classical "facts of the matter." All we have are the wave function and the experimental results that can be calculated from it. For macroscopic objects in our everyday environment, however, it would be tedious beyond reason, counterproductive, and perhaps even misleading to insist on employing quantum formalism and to deny that, say, a projectile follows a definite trajectory in obedience to Newton's laws.

Similarly, if all we have are our perceptual and consciously constructed models of the world around us, these models nevertheless serve as the basis of our actions, and we must act. Our perceptual models are ordinarily quite reliable, and action is facilitated because we take them as constituting the actual state of affairs. (Interpersonal cooperation relies on this classical realm of truth, in which it can be established, for example, that you assisted me yesterday, and I, in return, promised to assist you today.) As science probes aspects of nature far removed from those to which human common sense has been attuned, our intuitions about what is going on may mislead us. We seek the truth, but what we come up with is an evolving array of models. We are good scientists because we are by nature expert modelers and possess the flexibility to change our allegiance from one model to another, removing the label "truth" from the former and affixing it to the latter. To be equally good *philosophers* of science, however, requires that we confront our classical-realm intuitions and take into account their profound influence on our thinking. Science succeeds even if scientists do not quite understand what they are doing, but such understanding is itself the object of philosophy.

Summary

In order to treat science as a market process, I adopted two major premises about economics. First, it is high time to discard artificial limitations on its scope. Economics is simply what the Austrians have called praxeology: the study of human action. Individuals make choices as they pursue their goals; economics is the discipline that traces the consequences of that fact. Second, it is time to adopt a broader understanding of the market to go along with a broad understanding of economics. In society, individuals pursue their goals at least in part through interaction with others. A term is needed to refer to the entire array of options for noncoercive exchange, whether mediated by money or not. *Market* is the appropriate term.

In applying economics, we construct models (scenarios) based on simplifying assumptions. The first-approximation model I presented here takes for granted that scientists have independent means with which to pursue their research and assumes that scientists are motivated primarily by a desire for peer recognition of their research accomplishments. Adjustments to these assumptions can be (and are, in this article) incorporated as needed.

In the context of this model, I explored a number of similar or common features of science and traditional economic activity, such as specialization, exchange, investment, entrepreneurship, and self-regulation. In particular, I interpreted the observed practice of citation as part of a system of exchange, making good on the characterization of science as a market process. Nevertheless, I rebutted attempts to draw a parallel between citation and money and to interpret citation as a property right.

Next, I employed economics as a critical perspective from which to examine classic approaches to understanding scientific inquiry: logical methodology, evolutionary epistemology, Mertonian norms, and Kuhnian revolutions. An economic approach arguably covers much the same ground and does so more aptly. Finally, I utilized economic thinking, together with insights from the modeling approach to philosophy of science, to examine the nature of scientific change and scientific rationality.

If science is a market process, then the conceptual toolkit of economics (in particular, in my view, the Austrian toolkit) has far more to offer to the study of scientific inquiry than has been widely appreciated or exploited. The upshot is that the economic point of view remains highly underdeveloped and offers the prospect of a most favorable return on cognitive investments at this time.

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