Nobel Lecture: The Process and Progress of Economics

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Nobel Lecture: The Process and Progress of Economics

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The lecture focuses on the reasons that new ideas are accepted or rejected by a science. A distinction is drawn between prescientific and scientific stages of a discipline. The diverse fates of new ideas are illustrated by a variety of episodes in the history of economics, including the economics of information and the theory of economic regulation.

In the work on the economics of information which I began 20 some years ago, I started with an example: How does one find the seller of automobiles who is offering a given model at the lowest price? Does it pay to search more, the more frequently one purchases an automobile, and does it ever pay to search out a large number of potential sellers? The study of the search for trading partners and prices and qualities has now been deepened and widened by the work of scores of skilled economic theorists.

I propose on this occasion to address the same kinds of questions to an entirely different market: the market for new ideas in economic science. Most economists enter this market in new ideas, let me emphasize, in order to obtain ideas and methods for the applications they are making of economics to the thousand problems with which they are occupied: these economists are not the suppliers of new ideas but only demanders. Their problem is comparable to that of the automobile buyer: to find a reliable vehicle. Indeed, they usually end up by buying a used, and therefore tested, idea.

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Those economists who seek to engage in research on the new ideas of the science—to refute or confirm or develop or displace them—are in a sense both buyers and sellers of new ideas. They seek to develop new ideas and persuade the science to accept them, but they also are following clues and promises and explorations in the current or preceding ideas of the science. It is very costly to enter this market: it takes a good deal of time and thought to explore a new idea far enough to discover its promise or its lack of promise. The history of economics, and I assume of every science, is strewn with costly errors: of ideas, so to speak, that wouldn't run far or carry many passengers. How have economists dealt with this problem? That is my subject.

I begin by distinguishing the prescientific stage of a discipline from its scientific stage. A science is an integrated body of knowledge, and it is pursued and developed by a group of interacting practitioners called scientists. The validation and extension of that body of knowledge is the intellectual goal of the scientists, although of course the pursuit of that goal in turn serves whatever personal goals—such as prestige, reputation, and income—the scientists seek. These are only definitions, but I hope they are not strained or unnatural ones.

The prescientific stage is characterized in part by the incompleteness of the body of knowledge, but that is only a relative matter since no science is ever complete. This prescientific stage is also characterized by absence of a set of interacting practitioners who are devoting a large part of their lives to the accumulation of knowledge, and hence it is characterized by the absence of cumulative progress.

I. Prescientific Economics: Mercantilism

We will find it useful to spend a short time with the large body of writing called mercantilism. This literature ranges over several centuries, and over England and western Europe. The literature comprises hundreds of pamphlets and books and includes participants of the stature of John Locke and William Petty. I must confess at once that I have little direct knowledge of that literature, for I have concentrated my historical work upon the period which followed. However, three major studies of mercantilism are reassuringly agreed upon the characteristics I wish to discuss. The studies are Edgar Furniss's book, *The Position of the Laborer in a System of Nationalism* (1920), Jacob Viner's famous essay, "English Theories of Foreign Trade before Adam Smith" (1930), and Eli Heckscher's masterly treatise, *Mercantilism* (1935).

A first characteristic of all three surveys of mercantilism is that they almost totally lack a time dimension. Furniss will document a statement by references to two tracts written more than a century apart.

With the very first doctrine of mercantilism—that it was vitally important to have an excess of exports over imports—Viner begins a sequence of illustrative quotations with Richard Leicester, who wrote in 1381. (Of course, if one were allowed to go out of economics it would be easy to continue the sequence of praises of an export balance a full 6 centuries through 1981 and probably another 6 centuries through the year 2581!) Heckscher also seldom finds it necessary to notice the temporal sequence of two writers.

A second characteristic is that most mercantilists propose their own views without any attempt to utilize or improve upon the work of other mercantilists. There were sharp controversies, of course, but no regular pattern of sequences of criticisms and responses. These writings, one may note, were almost always briefs for special interests.

The third characteristic is almost a corollary of the first two: there was no cumulative improvement in the doctrines being propounded. I quote Viner:

> In many respects, indeed, as the mercantilist argument became more elaborate and involved, it became more objectionable from the point of view of modern doctrine, and, except with reference to the bullionist doctrines, a strong argument could be presented in defense of the thesis that the mass of ordinary tracts on trade of the first half of the eighteenth century showed a more extreme and confused adherence to the fallacies of mercantilism than did the writings of the sixteenth and early seventeenth centuries. . . . In so far as trade theory was concerned, such progress as occurred was due almost solely to a small group of capable writers, able to analyze economic problems more acutely and logically than their predecessors, but not able to make a marked impression upon their contemporaries or even to attract their attention. [Viner 1937, p. 109]

The process of analysis simply was not cumulative: there was little advantage in studying foreign trade if one were born in 1680 instead of 1580.

I am now prepared to come to the rescue of an economist who needs little rescuing: Adam Smith. A considerable number of economists, and a few considerable economists, have emphasized the fact that Smith had many gifted predecessors and almost all or perhaps exactly all of his ideas are to be found expressed, and sometimes well expressed, by these predecessors. Some economists therefore wish to give the title of founder of economics to earlier writers such as Cantillon. This line of argument, in my view, misses the point.

It was Smith who provided so broad and authoritative an account of the known economic doctrine that henceforth it was no longer permissible for any subsequent writer on economics to advance his own ideas while ignoring the state of general knowledge. A science consists of interacting practitioners, and henceforth no one could decently ignore Smith's own work and in due time the work of Malthus, Ricardo, and the galaxy of economists who populated the first half of the nineteenth century.

The change came fast. Smith himself did not interact with any writers on economics after 1776, and of course even in his treatise he coolly ignored his leading rival, Sir James Steuart. Five years after the first edition of the *Essay on Population* (1798), by contrast, Malthus was making fundamental concessions in response to Godwin and other critics. The age of economic science had begun.

How complete the transformation of economics has become may be illustrated by an episode earlier in this century. A. C. Pigou, holding the chair in economics that his predecessor, Alfred Marshall, had made the most prestigious in the world, committed an error in stating the theory of external diseconomies. He asserted that when a firm contemplated entry into a competitive industry which is subject to rising supply prices of its inputs, that firm would make a socially inefficient decision because it would ignore the effect of its entrance into the industry in raising the prices other firms would have to pay for inputs. The error involves a confusion of transfer payments with social costs. The error appeared in his famous treatise, *Wealth and Welfare*, in 1912.

Allowing for the distractions created by the First World War, major economists soon devoted themselves to the problem. The two most famous refutations, by Dennis Robertson and Frank Knight, came in 1924, but the essential point had been made earlier by J. M. Clark (1913) and Allyn A. Young (1913). Under these attacks even Pigou, the most remote of scholars, capitulated. The era had already begun when only the detected errors of unimportant economists are spared a prompt refutation.

II. Economic Science: The Environmental View

The politics and economics of mercantilistic policy were the determinants of the issues in the mercantilist literature. Indeed the prescientific age of any discipline is dominated by the practical concerns of the society in which it is cultivated. It is an easy step to the view that the main problems of a discipline, even after it becomes an organized science, are posed directly by the paramount problems and policies of the society in which it is pursued.

Wesley Clair Mitchell went so far as to attempt to present a systematic history of economic thought in terms of the responses of each generation to its environment: "One of the results of any survey of the development of economic doctrines is to show that in large measure the important departures in economic theory have been intellectual responses to changing current problems; that is, the economic theorists who have counted most in the development of thought have been men who have been deeply concerned with problems that troubled their generation" (Mitchell 1967, 1:13). As examples, he told us: "Malthus's problem of population was as obviously an intellectual reflection of current events as was Adam Smith's 'obvious and simple system of natural liberty' " (1:235). "The description of the course of English politics in Parliament shows that Ricardo got this problem [how to determine the way in which the produce of the country was divided]—his appreciation of its importance—not in his study, but by following current events. It should also be noticed that Ricardo got his peculiar conception of what the problem of distribution is directly from the Parliamentary struggle" (1:286). Yet when Mitchell reached the 1870s and the rise of the marginal utility theory, he abandoned the attempt to find environmental changes to which economic theory was responding. He attributed the abandonment to the difficulty of achieving understanding of and detachment toward more recent work, not the failure of his hypothesis (Mitchell 1967, 2:2).

The central task of an empirical science such as economics is to provide general understanding of events in the real world, and ultimately all of its theories and techniques must be instrumental to that task. That is very different from saying, however, that it must be responsive to the contemporaneous conditions and problems of the society in which it is situated.

If the problems of economic life changed frequently and radically and lacked a large measure of continuity in their essential nature, there could not be a science of economics. An essential element of a science is the cumulative growth of knowledge, and that cumulative character could not arise if each generation of economists faced fundamentally new problems calling for entirely new methods of analysis. The change of problems and methods would also undermine the training of economists: if the young studied under the old, the young could be confident that they were learning things that were rapidly becoming obsolete. A science requires for its very existence a set of fundamental and durable problems.

In economics the most fundamental of these central problems is the theory of value. The theory of value must explain how the comparative values of different goods and services are established. Until that problem is solved, it is not possible to analyze for scientific purposes what will be produced and in what quantities, how the resources will be employed in producing the menu of outputs, and how the resources will be valued. Without a theory of value the economist can have no theory of international trade nor, possibly, a theory of money. This central problem of value does not change in its essential content if one seeks to explain values in rural or urban societies, or in agricultural or industrial societies. Indeed, if the problem of value were so chameleon-like as to alter its nature whenever the economic or political system altered, each epoch in economic life would require its own theory, and short epochs would get short-lived theories.

If an empirical science requires for its very existence a set of fundamental and persistent phenomena, that is not the only kind of phenomena with which it will deal. It will continuously be confronted with new circumstances which call for more than a routine application of standard knowledge. Thus the energy crisis of the 1970s has provided much employment to economists, but it has not called for important changes in economic science.

An empirical science has a second, and vastly more important, interest in and responsiveness to contemporary problems: its received theory will at times be incapable of dealing with these problems. When England began the long-term importation of grain at the time of the Napoleonic wars and pressed hard upon its domestic production capacities, the economists introduced the law of diminishing returns in dealing with the price of grain. It would be difficult to deny a role to the environment in the appearance of this law. So much for the origin of that theory: it would not help us one whit in understanding Edgeworth's famous analysis of this law in 1911 to look at his economic environment. The important place that diminishing returns has achieved in economics is due precisely to the fact that its usefulness was not limited to Ricardo's analysis of agriculture in Great Britain.

The responsiveness of economics to environmental problems will naturally be more complete and more prompt, the more urgent the problems of the day. The response will also be more complete, the less developed the relevant body of economic analysis. The responsiveness of macroeconomics to contemporary events is notorious. Keynes's conquest in the 1930s was due to the fact that the neoclassical theory could not account for the persistent unemployment of that decade. A generation later, persistent inflation even with less than full employment was equally decisive in ending Keynes's supremacy. If and when macroeconomics produces a good theory of the business cycle, its responsiveness to environmental changes will diminish sharply.

A viable and healthy science requires both the persistent and almost timeless theories that naturally ignore the changing conditions of their society and the unsettled theories that encounter much difficulty in attempting to explain current events. Without the base of persistent theory, there would be no body of slowly evolving knowledge to constitute the science. Without the challenges of unsolved, important problems, the science would become sterile.

One final observation: there is no simple or known relationship between environmental changes and changes in economic analysis. During the Industrial Revolution, economists adopted the law of diminishing returns but ignored the most sustained and widespread growth of output that the world had yet observed. The vast governmental income redistribution programs of the last hundred years have only recently attracted the attention of economic theorists. The scholars who create economic theory do not read the newspapers regularly or carefully during working hours.

III. The Omniscient Scholar?

Once a science becomes well populated, has achieved a secure academic base, and is equipped with the machinery of intellectual exchange—journals, learned societies, and conferences—it is presented with a stream of proposals for new directions or new methods for research. Indeed, the science itself carefully fosters the output of new ideas. Robert K. Merton has shown in his fundamental studies of the reward structure of science that immense value is attached to priority in the development of successful new ideas (Merton 1973, esp. chap. 14).

And yet ideas will be proposed which are ignored at the time but at some later date are accepted (almost invariably after an independent rediscovery) as important to the science. This phenomenon repeatedly called forth the rebukes of Schumpeter in his great *History of Economic Analysis*. Here are examples of men who, Schumpeter believed, quite correctly, were "writing above their time": "Longfield's merits may be summed up by saying that he overhauled the whole of economic theory and produced a system that would have stood up well in 1890" (Schumpeter 1954, p. 465). John Stuart Mill "even went so far as to compare [John] Rae's performance on accumulation with Malthus' performance on population. And all this, written in what was to be for forty years the most influential textbook of economics, was insufficient to introduce Rae to the profession or to rouse any curiosity concerning the rest of his book!" (p. 469). Of course Schumpeter, than whom no economist was more sophisticated, gave some sensible reasons for these acts of neglect of genius, but he failed to give the most important reason of all.

In every period of the active pursuit of a science, new ideas are continually being proposed. Any new idea—a new conceptualization of an existing problem, a new methodology, or the investigation of a new area—cannot be fully mastered, developed into the stage of a tentatively acceptable hypothesis, and possibly exposed to some empirical tests without a large expenditure of time, intelligence, and research resources. That is fact 1. Fact 2 is that the overwhelming majority of these new ideas will prove to be sterile—in fact, quite possibly all the new ideas of a period of years will prove to be sterile. Only afterward, with the fullness of knowledge that history sometimes provides, can we identify the truly fertile ideas of a period.

Some men have superb instincts as to which of the new ideas of the time will repay intensive exploration, but no one is infallible. Even the greatest of economists pursue some problems that take them nowhere. In the last months of his life, Ricardo was still attempting to fashion a precise measure of value and not advancing one inch. John Stuart Mill and Léon Walras devoted much energy to the propagation of the proposal of nationalization of unanticipated future increments of land values—not the first time or the last that someone proposed nationalizing a sum with an expected value no larger than zero. Jevons could not get over the idea that cycles in sunspots left their tracks on commercial cycles. The great Pareto took a detour through the question of the order in which people consumed various products, out of a belief that this was related to the order of integration of a partial differential equation.

Not only great economists, but all economists who pursue anything, pursue will-o'-the-wisps for periods of time that are painful to consider in retrospect. In the 1930s, the area variously known as industrial organization and microeconomics-with-evidence was offered the following major research hypotheses:

1. The ownership and the effective control of large corporations have become separated.
2. The phenomenon of product differentiation calls for fundamental changes in the theory of the firm and the industry (the theory of monopolistic competition).
3. Prices do not respond downward to changes in supply and demand, perhaps because a particular expectation with respect to rivals' behavior creates a kink in the firm's demand curve.
4. The economist is able to construct criteria of the satisfactory or, alternatively, the unsatisfactory performance of an industry, where the satisfaction of the economist should be shared by society (the theory of workable competition).

These were not the only new research proposals: the annual output of new theories of oligopoly was supplemented by searches for truth through the feeding and wining of business leaders.

Each of the four rescarch proposals I have listed received a good deal of attention: none lost its fashionable appeal to at least some highly competent economists for at least 5 or 10 years, and indeed not one is a cold corpse today. But it is also true that not one of them has been absorbed into the mainstream of price theory as a regular and significant part of the analysis of the workings of markets and industries. Quite possibly one could find that Schumpeter followed several of these detours for at least a short distance. Of course at the same time some important new ideas (such as that of Hotelling on exhaustible resources and Ramsey on optimal pricing) were being neglected. To err is not only human but also scientific.

IV. The Continuity of Scientific Change

"Nature does not move in jumps," says the proverb, and a science also progresses through time without making large jumps. This continuity is often illustrated by two kinds of evidence.

One evidence of scientific continuity that has been adduced by Robert Merton is the existence of multiple and nearly simultaneous independent discoveries of a theory by several scientists. The popular examples in economics are the discovery of the theory of rent by Edward West and Thomas Robert Malthus in 1815 and the publication of the theory of utility in the early 1870s by Jevons, Menger, and Walras. In each case, the new idea was presumably appropriate to the development of economics at the time: the rent theory allowed the construction of a theory of the distribution of income; and the utility theory led naturally to the marginal productivity theory and the generalization of the theory of utility-maximizing behavior.[1]

This continuity is also used to explain the not uncommon phenomenon of the failure of a man of genius to get acceptance of his ideas from his contemporaries, even though later generations will applaud the performance. Augustin Cournot, for example, was an important scholar in one of the leading intellectual centers of Europe, but he could not persuade economists in 1838 that the mathematical theory of maxima and minima was a useful tool for economic analysis.

I would find it more persuasive to establish the continuity of scientific development by a close examination of the evolution of important concepts in economics, but that route does not seem appropriate to the occasion.[2] Candor compels me to note that the route of close historical study would not be easy to follow because it would

[1] I have presented elsewhere an alternative interpretation of Merton's theory of multiple discoveries which emphasized even more than he does how essential it is that science be "ready" for a new idea (see Stigler 1980).

[2] For a fascinating case study in another discipline, see Fisher (1982).

require definite answers to the questions: What is a large change in a science? What is a rapid change in a science?

Gary Becker has suggested that a substantial resistance to the acceptance of new ideas by scientists can be explained by two familiar economic concepts. One is the concept of specific human capital: the established scholar possesses a valuable capital asset in his command over a particular body of knowledge. That capital would be reduced if his knowledge were made obsolete by the general acceptance of a new theory. Hence, established scholars should, in their own self-interest, attack new theories, possibly even more than they do in the absence of joint action. The second concept is risk aversion, which leads young scholars to prefer mastery of established theories to seeking radically different theories. Scientific innovators, like adventurers in general, are probably not averse to risk, but for the mass of scholars in a discipline, risk aversion is a strong basis for scientific conservatism. We will find the specific human capital theory illustrated in the episodes to which I shall soon turn.

No one can describe the precise characteristics or content of a new piece of scientific work that will find ready and eager reception from the scientists of a period. Indeed, if knowledge sufficient to identify the theories that will succeed were possessed, it would be of immense value in finding and developing those theories and therefore would be the key to scientific fame. To the scientist such knowledge would be much more valuable than an accurate method of predicting stock prices! Even without such a priceless key to the understanding of scientific innovation, it is interesting to examine several routes by which a scientific idea makes its way into the work of economists. I illustrate two of these routes by subjects on which I have worked.

Acceptance without Struggle: The Economics of Information

Economists have always known that the extent and accuracy of the knowledge of the economic actor had influence, and often a decisive influence, on his behavior and therefore on the behavior of markets.

One striking example of this critical role of information is provided by the theory of oligopoly. The first formulation of the problem of oligopoly as a specific problem in economic theory was made by Cournot, whose long failure to get acceptance I have already mentioned. It was essential, in explaining how each of two rivals in a market would behave, to attribute to each some belief about the behavioral pattern of the other. Cournot made the assumption that each assumed that the rival did nothing in response to his own actions. The later theories of oligopoly all rest upon different assumptions concerning patterns of behavior which each seller attributes to his rivals. A dozen other areas of economic analysis, such as the workings of the labor market and the role of advertising, also rest squarely on assumptions about information of the economic actors. In this tradition, the amount of information possessed by individuals in any market was arbitrarily postulated rather than derived from economic principles. The consensus was that consumers knew little, traders on organized exchanges a great deal; investors were either gullible or omniscient. Even the powerful and luminous essay by Friedrich von Hayek on "The Use of Knowledge in Society" (1945) had not addressed the principles of acquisition of knowledge.

I proposed (in 1961) the use of the standard economic theory of utility-maximizing behavior to determine how much information people would acquire with special attention to the prices at which they would buy and sell, and a year later made an application of the analysis to labor markets. There is one interesting feature of the subsequent history of the reception of this work by economists to which I wish to call attention.

The proposal to study the economics of information was promptly and widely accepted, and without even a respectable minimum of controversy. Within a decade and a half, the literature had become so extensive and the theorists working in the field so prominent that the subject was given a separate classification in the *Index of Economic Articles,* and more than a hundred articles a year are now devoted to this subject.

The absence of controversy certainly was no tribute to the definitiveness of my exposition. I had chosen fixed sample rather than sequential analysis, which a majority of later economists prefer. I had not presented a general equilibrium solution in which the behavior of both sides of a market is analyzed, and that step proved difficult to take. I had done little with information on quality and other variables, in contrast to price, although I soon extended the approach to a different kind of information in the theory of oligopoly. I had not applied the theory to the problem of unemployment, a literature initiated by an important paper by Armen Alchian (1970). All I had done was to open a door to a room that contained many fascinating and important problems.

The absence of controversy was due instead to the fact that no established scientific theory was being challenged by this work: in fact, all I was challenging was the neglect of a promising subject. Moreover, the economics of information was susceptible to study by quite standard techniques of economic analysis. The theory immediately yielded results which were intuitively or observationally plausible. Here was a Chicago theory that didn't even annoy socialists!

Acceptance by Necessity: The Economics of Regulation

The work on the economics of regulation has entered economics by a different route.

The modern era of economists' interest in the economic workings of the state may be dated from the influential work of Anthony Downs, *An Economic Theory of Democracy* (1957), and James Buchanan and Gordon Tullock, *The Calculus of Consent* (1962). Although I had read these works with deep interest and admiration, my own work on regulation at first followed a different, more empirical route.

An examination of the economic literature had revealed no serious professional attempt to measure the impact of public regulation in areas with long histories: the regulation of rates of electrical utilities; the review of new issues by the Securities and Exchange Commission; and the antitrust policy of the United States. The investigations of these problems (with the indispensable assistance of Claire Friedland), strongly reinforced by related work of colleagues and students, gradually forced me to confront a question that should have been obtrusively obvious at once: Why does the state engage in its regulatory activities?

The answer (at least for an economist) seemed to lie much less in the theorems of welfare economics or the prescriptions of traditional political science than in the systematic examination of the self-interest of the various participants in political life. These participants, to be sure, operated under different rules and constraints than the traders in markets, but that did not argue against using that powerful tool of economic analysis, the theory of utility-maximizing behavior. Once the economist can identify the costs and returns from various actions, this theory allows him to make predictions of behavior that have been reasonably successful.

This approach proved to be highly uncongenial to many economists. My teacher, Frank Knight, had often expressed the belief that many economists still share, that the actors (and especially the voters) in political life are ignorant, emotional, and usually irrational. In a famous, unpublished speech he ended a parable with the words: "Truth in society is like strychnine in the individual body, medicinal in special conditions and minute doses; otherwise and in general, a deadly poison." These economists believe that voters are myopic and forgetful, and that political institutions are designed or perverted to allow the public servants to pursue chiefly their own interests. Another and perhaps larger group of economists are critical of the utility-maximizing approach for the opposite reason: that it appears to be an attack on the chief instrument for purposive social improvement that a society possesses: the state.

Nevertheless the economic theory of regulation is achieving a substantial scientific prosperity. Its findings with respect to both the operation and the origins of regulatory policies directed to particular industries (such as the securities markets, transportation, and occupational licensing) command a substantial support. To be sure, the explanatory triumphs have not been overwhelming, and indeed the theory itself is still relatively primitive. The main reason for the considerable acceptance of the approach is that fundamental rule of scientific combat: it takes a theory to beat a theory. No amount of skepticism about the fertility of a theory can deter its use unless the skeptic can point to another route by which the scientific problem of regulation can be studied successfully.

There is an interesting asymmetry in the success of this literature in dealing with the two problems into which the theory is commonly divided: Why are regulatory policies adopted and abandoned? What are their effects? Economists have been much more successful in measuring effects of policies than in explaining their adoption. The explanation is that one can choose the effects of a policy to study, and usually more easily measured effects are chosen for study. One has no such options when addressed with the question, Why did the United States adopt an antitrust policy in 1890?

Thus studies of effects of regulatory policies have usually been concerned with the effects upon prices and outputs, although the effects desired by the supporters of these policies have probably been upon the distribution of income. The panoply of regulatory measures can be used to effect vast income redistributions, and these redistributions of income do not appear explicitly in the budget of the state. The frequent exclusion of new entrants from a field, for example, leads to smaller outputs, higher prices, and higher profits for the protected enterprises and allows these benefits to increase with the growth of the protected area. If these income transfers are as large as fragmentary evidence suggests, the theory of regulation may well become a full partner of tax and expenditure theory in public finance.

Acceptance by Trial by Combat?

Is it exceptional of the theories I have been discussing that neither was subjected to direct trial by combat with an alternative theory? We speak so often of the competition of ideas. How is that competition conducted?

The direct confrontation of two alternative theories, each seeking to explain the same body of observable phenomena, is not common in economics.[3] (It is perhaps encountered more often in macroeconom-

[3] I once made such a direct confrontation of the theory of the kinked oligopoly demand curve and more traditional theories, finding no evidence to support the exis-

ics than in microeconomics.) Two modern examples from microeconomics will illustrate the proposition that economists seldom choose between directly rival theories on the basis of critical empirical tests.

1. The doctrine of limit pricing by oligopolists asserted that the firms in an industry would set prices at such a level as to discourage or prevent entry of additional firms into an industry. The theory had a long prehistory under the name of potential competition, but it was given an explicit formulation by Sylos-Labini (1962), Joe Bain (1949), and Franco Modigliani (1958). This version gave rise to a substantial literature, but at no time was a direct empirical test made of this theory as against explicit alternative theories of oligopoly behavior.

2. The Pigovian theory of external economies was challenged directly by Ronald Coase (1960), who in effect argued that the Pigovian theory had assumed noneconomic behavior on the part of the economic actors in a wide class of phenomena. This challenge was met for a time by a considerable number of counterarguments, but these arguments were addressed to the logic of what has come to be known as the Coase theorem. No explicit comparison of the explanatory powers of the Coasian and Pigovian approaches has been undertaken.

Why did not the profession seek directly to test these theories and, for that matter, the four theories of the 1930s that I characterized as largely unsuccessful innovations? Some part of the answer may lie in the fact that formal empirical tests of economic theories have historically been scarce, although they are increasing in frequency, but I would not press this answer. Instead, the testing procedure—the trial by combat—takes a different form.

It is seldom that a theory in economics has a well-defined domain of applicability. It may have been created to explain a specific class of events—the pricing by oligopolists when entry is possible, in the first illustration above—but it always has a wider domain of possible applicability. The specification of a critical test which, if conducted correctly on a sufficient scale, will decide the combat between two alternative theories is seldom possible over the whole range of the domains of the two theories.

Economists have therefore generally chosen to decide between the alternative theories by the process of using each to explore a variety of problems. How does the limit theory of oligopoly pricing, for example, handle the process of growth of an industry or the phenomenon of vertical integration? How does the Coasian theory illuminate the structure of the law of torts or the economics of professional sports? These explorations are a form of testing of the theories: they

tence of a kink. The theory has disappeared from professional work but lives in every textbook (see Stigler 1978).

test the fertility of the theories (or at least the intellectual fertility of economists), and the varied applications are partial empirical tests of the theories. Gradually a consensus emerges among the economists working on the subject: the theory becomes a part of the standard analytical corpus or it dies of neglect.

V. Conclusion

Our list of factors which influence the receptivity of a science to new ideas could easily be extended.

In particular, it would be useful to examine the question of whether the attractiveness of the public policy positions associated with a theory has an effect upon the acceptability of the theory. The textbooks on methodology lecture us on the need to separate positive and normative theories. The study of economics tells us that few if any theories lead unequivocally to one set of policy implications. So science and policy should be separated. Are they? I believe that the separation has been far from complete, especially in the short run, but this is not the occasion to undertake the substantial study necessary to support the belief.

Again, the institutional organization of economic research is a potential influence upon the receptiveness of a science to new ideas. The powerful institutional position of Schmoller and the German Historical School no doubt played a role in the slow development of economic science in Germany after 1870. The dominant role of Cambridge University in economics from Marshall to Keynes surely was not favorable to the receptiveness of new ideas from outsiders. I believe that the shift of the center of economics to the United States was due in some part to the failure of the English economists to share fully in the quantitative empirical study of economics.

Even if I extended this list of potential determinants of scientific choice and documented each more fully than I have, I would still have kept my promise not to tell you the detailed characteristics of the successful new theories in economic science. I do not lament this failure.

The fascination of scientific work does not lie in the craftsman-like utilization of the tools of a science. It is admirable for the gymnast to put his splendidly disciplined body through intricate maneuvers, and it is no doubt equally admirable for the scientist to put his disciplined mind through a sequence of complex analytical or experimental maneuvers. The great fascination of scientific endeavor, however, is precisely in the speculative pursuit of new ideas that will widen the horizon of our understanding of the world. This endeavor is not that of a graceful, intellectual gymnast: on the contrary, the scientist is stumbling about in a jungle of ideas or facts that seem to defy system or logic, and usually he fails to emerge with anything but scratches. The dangers of the search include the chance that a gifted rival will reach the goal, and the danger is not reduced by the fact that the rivalry is conducted under what for able and ambitious competitors are unusually chivalrous rules. Still, learning more about how this search for new knowledge proceeds is itself a worthy search for new knowledge, and we shall not abandon it.

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