

economists. It was, as a colleague remarked, a case of supply meeting demand. The proceedings of this conference were published in a special issue of the *Journal of Law and Economics*.⁹

Another development with which I am associated is the establishment of the Center for Research on Contracts and the Structure of Enterprise at the Business School of the University of Pittsburgh. This center will make large-scale collections of business contracts and will prepare databases which will be made available to all researchers, whatever their institution. Nor should we forget the work now getting started at the Center for Economic Studies of the Bureau of the Census.

This greater availability of data and the encouragement given to all researchers working on the institutional structure of production by the award to me of the Nobel prize should result in a reduction in that elegant but sterile theorizing so commonly found in the economics literature on industrial organisation and should lead to studies which increase our understanding of how the real economic system works.

My remarks have sometimes been interpreted as implying that I am hostile to the mathematisation of economic theory. This is untrue. Indeed, once we begin to uncover the real factors affecting the performance of the economic system, the complicated interrelations among them will clearly necessitate a mathematical treatment, as in the natural sciences, and economists like myself, who write in prose, will take their bow. May this period soon come.

I am very much aware that many economists whom I respect and admire will not agree with the opinions I have expressed, and some may even be offended by them. But a scholar must be content with the knowledge that what is false in what he says will soon be exposed and as for what is true, he can count on ultimately seeing it accepted, if only he lives long enough.

9. *Journal of Law and Economics* 34(2), pt. 2 (October 1991).

TWO

How Should Economists Choose?

I had a close relationship with Warren Nutter at the University of Virginia. I came to admire him for the thoroughness with which he carried out his researches, for the conscientiousness with which he performed his academic duties, and for the courage he displayed in doing what he believed to be right. Warren Nutter was an excellent economist, which is rare, but he was something rarer still, a truly moral man. Frank Knight, who was so much admired by Warren Nutter, tells us that the "basic principle of science—truth or objectivity—is essentially a moral principle, in opposition to any form of self-interest. The pre-suppositions of objectivity are integrity, competence and humility."¹ Integrity, competence, and humility—these three qualities sum up Warren Nutter's character. He knew that in economic affairs people are mainly motivated by self-interest, but he did not believe that this was their sole motivation and certainly he thought it should not be. In his own actions, Warren Nutter cared as much for others as he did for himself. As a colleague and friend, I knew him to be utterly reliable. It is our good fortune that he devoted himself to the service of economics. We are all in his debt.

To have been asked to deliver one of the Warren Nutter memorial lectures is a great privilege. But it is not easy to prepare a lecture of a standard that will truly honour Warren Nutter's memory. There is also the problem of choosing a topic appropriate to the occasion. On this

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1. Frank H. Knight, *Freedom and Reform* (New York: Harper & Brothers, 1947), 244.

score, however, I believe I have succeeded and that Warren Nutter would have found the questions I will be discussing of great interest and would have treated my point of view with sympathy.

Many economists, perhaps most, think of economics as the science of human choice, and it seems only proper that we should examine how economists themselves choose the theories they espouse. The best-known treatment of this question is that of Milton Friedman, who, in "The Methodology of Positive Economics," his most popular paper (in itself a somewhat suspicious circumstance), tells us "how to decide whether a suggested hypothesis or theory should be tentatively accepted as part of" the positive science of economics. As you all know, the answer he gives is that the worth of a theory "is to be judged by the precision, scope, and conformity with experience of the predictions it yields. . . . The ultimate goal of a positive science is the development of a 'theory' or 'hypothesis' that yields valid and meaningful . . . predictions about phenomena not yet observed."²

I should say at once that I do not consider Milton Friedman's answer satisfactory. At this point, I fear that many in this audience will be inclined to regard this statement as lese majesty. But I hasten to reassure them by saying that it is my belief that my way of looking at this question is more consonant with Friedman's general position as expressed in *Capitalism and Freedom* or *Freedom to Choose* than with that found in "The Methodology of Positive Economics." I should add that I am in no sense well informed in the philosophy of science. Words like epistemology do not come tripping from my tongue. What I have to say consists of reflections based on what I have observed about the actual practice of economists.

The view that the worth of a theory is to be judged solely by the extent and accuracy of its predictions seems to me wrong. Of course, any theory has implications. It tells us that if something happens, something else will follow, and it is true that most of us would not value the theory if we did not think these implications corresponded to happenings in the real economic system. But a theory is not like an airline or bus timetable. We are not interested simply in the accuracy of its predictions. A theory also serves as a base for thinking. It helps us to understand what is going on by enabling us to organise our

2. Milton Friedman, "The Methodology of Positive Economics," in *Essays in Positive Economics* (Chicago: University of Chicago Press, 1953), 3-4, 7.

thoughts. Faced with a choice between a theory which predicts well but gives us little insight into how the system works and one which gives us this insight but predicts badly, I would choose the latter, and I am inclined to think that most economists would do the same. No doubt it would be their belief that ultimately this theory would enable us to make predictions about what would happen in the real world; but since these predictions would emerge at a later date (and probably would also be about different things), to assert that the choice between theories depends on their predictive powers becomes completely ambiguous.

Friedman enlarges his argument by maintaining that theories are not to be judged by whether their assumptions are realistic. Let me quote what he says:

Consider the density of leaves around a tree. I suggest the hypothesis that the leaves are positioned as if each leaf deliberately sought to maximize the amount of sunlight it receives, given the position of its neighbors, as if it knew the physical laws determining the amount of sunlight that would be received in various positions and could move rapidly or instantaneously from any one position to any other desired and unoccupied position. . . . Despite the apparent falsity of the "assumptions" of the hypothesis, it has great plausibility because of the conformity of its implications with observation.³

Let us suppose that it is true that the assumption that a leaf subscribes to *Scientific American* and the *Journal of Molecular Biology* and that it understands what is contained therein enables us to predict what the distribution of leaves around a tree will be. Such a theory nonetheless provides a very poor basis for thinking about leaves (or trees). Our problem is to explain how leaves come to be distributed on a tree given that a leaf does not have a brain. Similarly, to take an example in economics, we could have predicted over the last few years what the American government's policies on oil and natural gas would be if we had assumed that the aim of the American government was to increase the power and income of the OPEC countries and to reduce the standard of living in the United States. But I am sure that we would prefer a theory that explains why the American government, which

3. *Ibid.*, 19-20.

presumably did not want to bring about these results, was led to adopt policies which harmed American interests. Testable predictions are not all that matters. And realism in our assumptions is needed if our theories are ever to help us understand why the system works in the way it does. Realism in assumptions forces us to analyse the world that exists, not some imaginary world that does not.

It is, of course, true that our assumptions should not be completely realistic. There are factors we leave out because we do not know how to handle them. There are others we exclude because we do not feel the benefits of a more complete theory would be worth the costs involved in including them. Their inclusion might, for example, greatly complicate the analysis without giving us greater understanding about what is going on. Again, assumptions about other factors do not need to be realistic because they are completely irrelevant. If we wish to show that enforcement of a minimum wage will lead to unemployment among less productive workers, it is unnecessary to be accurate about the exact way in which capital gains are taxed. There are good reasons why the assumptions of our theories should not be completely realistic, but this does not mean that we should lose touch with reality.

I now turn to what is, from my point of view, the strangest aspect of "The Methodology of Positive Economics." It is that what we are given is not a positive theory at all. It is, I believe, best interpreted as a normative theory. What we are given is not a theory of how economists, in fact, choose between competing theories but, unless I am completely mistaken, how they ought to choose. When Friedman says that the "ultimate goal of a positive science is the development of a 'theory' or 'hypothesis' that yields valid and meaningful . . . predictions about phenomena not yet observed," I cannot help mentioning that a science has no goals, only individuals have goals. What has to be shown if Friedman's criteria are to be accepted as a positive theory is that individual economists actually choose among competing theories according to these criteria. I will show the difficulty of interpreting Friedman's argument in this way by considering three episodes, all of which occurred in my youth and, unlike more recent events, I remember vividly. These are episodes in the 1930s in which economists changed their views, that is, changed the theories they espoused. I will mainly be discussing what happened in economics in England, but

these were times when, to a very considerable extent, this was what happened in economics.

The first episode I will discuss is local, but the economists involved were among the best in the world. In February 1931, Friedrich Hayek gave a series of public lectures entitled "Prices and Production" at the London School of Economics, and in September 1931 these lectures were published as a book. They were undoubtedly the most successful set of public lectures given at LSE during my time there, even surpassing the brilliant lectures Jacob Viner gave on international trade theory. The audience, notwithstanding the difficulties of understanding Hayek, was enthralled. What was said seemed to us of great importance and made us see things of which we had previously been unaware. After hearing these lectures, we knew why there was a depression.

Most students of economics at LSE and many members of the staff became Hayekians or, at any rate, incorporated elements of Hayek's approach in their own thinking. With the arrogance of youth, I myself expounded the Hayekian analysis to the faculty and students at Columbia University in the fall of 1931. What now strikes me as odd is the ease with which Hayek conquered LSE. I think this was in part the result of a lack of precision in the existing analysis or, at any rate, in our grasp of it, so that Hayek's analysis seemed to give a well-organised and fruitful way of thinking about the working of the economic system as a whole. As far as I can see, the Hayekian analysis did not make predictions except in the sense that it explained why there was a depression. What can be said is that the analysis seemed to be consistent with everything we observed. To show that this was so, Lionel Robbins published in 1934 *The Great Depression*, the only one of his works, as he tells us, that he wishes he had not written.⁴

The next episode I will consider was by no means local, although I viewed it from the London School of Economics. It was a worldwide phenomenon. This was the Keynesian revolution. I will not labour its importance—that is conceded by the great majority of economists. I need only quote the statement of John Hicks: "The Keynesian revolution is the obvious example of a big revolution [in economics]; there

4. Lord Robbins, *Autobiography of an Economist* (London: Macmillan, 1971), 154, 160.

are not more than two or three others which might conceivably be compared to it."⁵

While in the case of Hayek I thought (incorrectly) that I understood what was going on, I was never under such an illusion in the case of Keynes. By that time, I was wholly absorbed in what is now called microeconomics. What I mainly remember from this period is that everything I said on the subject was wrong because savings equaled investment. Fortunately I am not concerned so much with the substance of Keynes's *General Theory* as with the circumstances of its acceptance by the economics profession. For there can be no question that Keynes triumphed. Nor did it take very long. The *General Theory* was published in February 1936. Although some of the early reviews were hostile or lukewarm, it was soon apparent that the economics profession was, for the most part, going to adopt the Keynesian approach. Abba Lerner, for example, published his influential account of the Keynesian system in the *International Labour Review* in October 1936. As Paul Samuelson has said:

The *General Theory* caught most economists under the age of thirty-five with the unexpected virulence of a disease first attacking and decimating an isolated tribe of South Sea islanders. Economists beyond fifty turned out to be quite immune to the ailment. With time, most economists in between began to run the fever, often without knowing or admitting their condition.⁶

I cannot vouch for the accuracy of Samuelson's account of the difference in the response of economists in the United States to Keynes's *General Theory* according to their age, but it has very little relevance to events in England; there were, in fact, very few economists there who were older than fifty in 1936. Among those who were at Cambridge or were associated with Keynes when the *General Theory* appeared, apart from Keynes himself, who was fifty-two, only A. C. Pigou was over fifty, and he proved not to be immune to the Keynesian

5. Sir John Hicks, "'Revolutions' in Economics," in Spiro Latsis, ed., *Method and Appraisal in Economics* (Cambridge: Cambridge University Press, 1976), 208.

6. Paul A. Samuelson, "The General Theory," in Robert Lekachman, ed., *Keynes' General Theory: Reports of Three Decades* (New York: St. Martin's Press, 1964), 315-16.

"disease," as Samuelson describes it. D. H. Robertson was then forty-five, R. F. Harrod thirty-six, Joan Robinson thirty-two, Richard Kahn thirty, J. E. Meade twenty-eight. The economists at LSE were even younger. Robbins was thirty-seven, Hayek thirty-six, Hicks thirty-one, Lerner thirty-two, and Nicky Kaldor twenty-seven at the time the *General Theory* was published.

Whether the acceptance of Keynes's system of analysis was or was not affected by the age distribution of economists in Britain, its success was such that by the outbreak of war in 1939, it could be said to be the orthodox approach among British economists. In fact, Robbins, as director of the Economics Section of the War Cabinet Office, enthusiastically supported the proposals in the White Paper on Employment Policy issued in 1944. And Sir William Beveridge, who had attacked the *General Theory* in 1937 as theory untested by facts, was to publish his *Full Employment in a Free Society*, also in 1944, assisted by a number of Keynesians, including Kaldor.

This swift adoption of the Keynesian system came about, I believe, because its analysis in terms of the determinants of effective demand seemed to get to the essence of what was going on in the economic system and was easier to understand (at least in its broad outlines) than alternative theories. That the Keynesian system offered a cure for unemployment without requiring any sacrifices, provided a clearly defined role for government, and a policy easy to carry out (as it then appeared) added to its attractiveness. It can hardly be maintained that the Keynesian analysis was adopted because it yielded accurate "predictions about phenomena not yet observed." It is true that Keynes claimed to demonstrate that the economic system could function in such a way as to bring about persistent mass unemployment. But mass unemployment could not be described in the 1930s as a phenomenon "not yet observed." And it is not without relevance that the alternative theory that was displaced, or at any rate displaced at LSE, was that of Hayek, a theory which also explained why the economic system could operate in such a way as to lead to mass unemployment. Keynes's analysis was adopted in the main because it seemed to make more sense to most economists. Or, as I put it earlier, it provided a better base for thinking about the problems of the working of the economic system as a whole. And to those economists who were less concerned about the niceties of the analysis, Keynes's policy recom-

mendations undoubtedly provided a sufficient reason for many of them to adopt his theory and to reject that of Hayek.

The third episode I will consider is concerned with the change in the way in which economists analysed the working of a competitive system following the publication in 1933 of Edward Chamberlin's *Theory of Monopolistic Competition* and Joan Robinson's *Economics of Imperfect Competition*. These books were, as George Stigler has said, "enthusiastically received."⁷ Robert L. Bishop exaggerated somewhat, but not perhaps a great deal, when he said, writing in 1964, that it was "the consensus of economists" that these two books "touched off, in 1933, a theoretical revolution whose relative importance in the microeconomic area was comparable to that of the Keynesian analysis in macroeconomics."⁸ These books were certainly an instant success, and their contents were quickly absorbed and used by economists interested in price theory. As an example, although these books appeared in 1933, I had completed by mid-1934 a paper in which I used the geometrical analysis of Mrs. Robinson to illuminate and extend Chamberlin's treatment of duopoly and had corresponded with both Chamberlin and Mrs. Robinson. This paper, "The Problem of Duopoly Reconsidered," was published in the *Review of Economic Studies* in 1935. At about the same time Kaldor wrote his article on "Market Imperfection and Excess Capacity," which was also published in 1935, in *Economica*. I have no doubt that there was similar activity in the United States among economists writing on price theory.

The speedy adoption of these new approaches was in large part due to the very unsatisfactory state of the existing price theory. That this was so had been demonstrated beyond doubt by the controversies in the *Economic Journal* in the 1920s and perhaps above all by Piero Sraffa's 1926 article. We were therefore looking for ways to solve the dilemmas these discussions revealed. These new books by Chamberlin and Mrs. Robinson, which started the analysis with the decisions of the individual firm and used new tools such as the marginal revenue schedule, seemed to offer the way out. They certainly gave us a lot to

7. George J. Stigler, "Monopolistic Competition in Retrospect," in *Five Lectures on Economic Problems* (London: London School of Economics, and Longmans, Green and Co., 1949), 12.

8. Robert L. Bishop, "The Theory of Imperfect Competition after Twenty Years: The Impact on General Theory," *American Economic Review* 54 (May 1964):33.

put on the blackboard and to explain to our students. They enlarged our analytical apparatus. They seemed to give us a better understanding of how a competitive system works, but whether this was really so is another matter.

My own view of the contribution of these books is not essentially different from that expressed by Stigler in his lecture "Monopolistic Competition in Retrospect" published in 1949. But what is particularly interesting and useful, given the questions I am discussing, is that in this lecture Stigler also appraised Chamberlin's theory of monopolistic competition using Friedman's methodological principles. He argued that Chamberlin's theory should be adopted "if it contains different or more accurate predictions (as tested by observation) than the theory of competition." His personal belief was that "the predictions of [the] standard model of monopolistic competition differ only in unimportant respects from those of the theory of competition." He added, however, that "this is a question of fact, and it must be resolved by empirical tests of the implications of the two theories (a task the supporters of the theory of monopolistic competition have not yet undertaken)."⁹

The fact that supporters of the theory of monopolistic competition had not made empirical tests comparing the predictions of the alternative theories of competition (and, I may add, do not appear to have made such tests in the years since Stigler wrote) lends support to the view that Friedman's methodology is not a positive but a normative theory. Certainly this is the way that Stigler used it. Stigler was not saying that supporters of the theory of monopolistic competition made such tests but did them badly and so came to the wrong conclusion. He was saying that they did not make them at all. Since they should have done so, this merits our disapproval.

If choosing theories in accordance with Friedman's criteria is to be treated as a positive theory, economists would need to adopt a procedure somewhat similar to the following. When a new theory is advanced, economists would compare the accuracy of its predictions, preferably about "phenomena not yet observed," with that of the predictions of the existing theory and would choose that theory which gave the best predictions. Nothing remotely resembling this procedure happened during the three episodes that I have discussed, two of which

9. Stigler, "Monopolistic Competition," 24.

are recognised as having involved very important changes indeed in economic theory. For one thing, in each case the new theory was adopted within a time period too short for such a procedure to be followed. I believe that these three cases will be found to be quite representative of the process by which one theory has displaced another in economics, in large part because I do not believe that the process could, in general, be otherwise. An insistence that the choice of theories be made in accordance with Friedman's criteria would paralyze scientific activity.

Except in the most exceptional circumstances, the data required to test the predictions of a new theory (statistics and other information) will not be available or, if available, will not be in the form required for the tests (and, even when put into this form, will need a good deal of manipulation of one sort or another before they can be made to yield the requisite predictions). And who would be willing to undertake these arduous investigations? Someone who believed in a new theory might be willing to make these tests to convince unbelievers that the theory yielded correct predictions. And someone who did not believe in a new theory might make these tests to convince believers that the theory did not yield correct predictions. But for the tests to be worthwhile, someone has to believe in the theory, at least to the extent of believing that it might well be true. There is little profit in undertaking an investigation that is expected to show that a theory in which no one believes yields incorrect predictions, and I doubt whether any editor of a professional journal could be found who would be willing to publish a paper giving the results of such an investigation. If all economists followed Friedman's principles in choosing theories, no economist could be found who believed in a theory until it had been tested, which would have the paradoxical result that no tests would be carried out. This is what I meant when I said that acceptance of Friedman's methodology would result in the paralysis of scientific activity. Work could certainly continue, but no new theories would emerge.

But the world is not like that. Economists, or at any rate enough of them, do not wait to discover whether a theory's predictions are accurate before making up their minds. Given that this is so, what part does testing a theory's predictions play in economics? First of all, it very often plays either no part or a very minor part. A great deal of economic theory, so-called pure theory (and this is most of economic theory), consists of logical constructions based on assumptions about

human nature so basic that they are difficult to question, assumptions such as that, faced with a choice between \$100 and \$10, very few people will choose \$10. The kind of prediction that results is that if the price of a commodity is reduced, more will be demanded, or if the price is increased, more will be supplied. But, of course, that this is so must have been known before economics existed as an academic study. Other parts of theory, and this applies particularly to monopoly theory, tell us that if something happens, the price will go up, go down, or remain the same, depending on demand and cost conditions. It goes without saying that its predictions are always accurate. It might be argued that what this theory does is to tell us, given the demand and cost conditions, whether the price will go up, go down, or remain the same, but it is not easy to discover in practice what demand and cost conditions really are, and they are commonly inferred from the result rather than the other way round.

Some of you may be inclined to think that, while what I have been saying no doubt applies very well to the economic theory of my youth, things are very different in present-day economics with its massive use of quantitative methods. No doubt things are different. But in what way? What I have to say is largely based on the quantitative articles published in the *Journal of Law and Economics* when I was editor, but I have no doubt that what they reveal is representative of other quantitative studies in economics. First of all, many of these papers cannot be said to test a theory at all. They are measurements of an effect, the nature of which was already well established but of which the magnitude was unknown. For example, economists would expect that governmental control of entry into banking would reduce the number of banks, but without a quantitative study we would be unable to estimate the extent of the reduction.¹⁰ Of course, later on, theories may be developed to explain why some magnitudes are greater than others, and then such studies could be used to test such theories. But, generally speaking, this does not appear to be where we are at present.

Other papers take the form of a test of the theory espoused by the author: there is a model, then regressions, followed by conclusions. In almost all cases it will be found that the statistical results confirm the theory. Sometimes it does happen that some of the expected relationships are not statistically significant, but they will usually be found to

10. See Sam Peltzman, "Entry in Commercial Banking," *Journal of Law and Economics* 8 (October 1965):11-50.

be in the right direction. And when results are obtained that do not square with the theory, which occasionally happens, these results are not usually treated as invalidating the theory but are left as something calling for further study. I would not claim that such studies have never led the investigators to modify their theories, but such cases appear to be rather uncommon.

Some articles, of course, involve the testing of alternative theories, and this means that some theories are bound to come out worse. But I doubt whether such studies have often led to a change in the views of the authors. My impression is that these quantitative studies are almost invariably guided by a theory and that they may most aptly be described as explorations with the aid of a theory. In almost all cases, the theory exists before the statistical investigation is made and is not derived from the investigation.

I do not believe that, for the most part, economists could act in any other way. I am bolstered in this view by the fact that quantitative methods do not appear to be used in the natural sciences in a way essentially different from the way they are used in economics. At this point, I should acknowledge my indebtedness to Thomas Kuhn.

I first heard Milton Friedman expound his views on the methodology of positive economics one evening in London in the company of Ralph Turvey, at a time before Friedman's essay had been published. My immediate response was unfavourable. I voiced various objections to Friedman's views. But Adam Smith's impartial spectator, asked to report on this debate, would have said that I lost every round. Whatever argument I put forward, Friedman had a more telling counterargument. And yet I was not convinced. It was not until 1958—59, when Kuhn and I were both fellows at the Center for Advanced Study in the Behavioral Sciences at Stanford, that I learned about Kuhn's views and came to see exactly what it was about Friedman's methodological position that I did not like. But what most influenced me was not so much the argument that was later to appear in Kuhn's famous book *The Structure of Scientific Revolutions* (although I am in general agreement with its main thrust) as what he said in an earlier paper, "The Function of Measurement in Modern Physical Science", published in 1961.¹¹ Among other things, this paper makes clear that

11. Thomas S. Kuhn, "The Function of Measurement in Modern Physical Science," reprinted in *The Essential Tension* (Chicago: University of Chicago Press, 1977), 178.

quantitative methods are used in economics in essentially the same way as in the natural sciences.

I said that quantitative studies in economics are explorations with the aid of a theory. Consider what Kuhn wrote:

*The road from scientific law to scientific measurement can rarely be traveled in the reverse direction. To discover quantitative regularity one must normally know what regularity one is seeking and one's instruments must be designed accordingly; even then nature may not yield consistent or generalizable results without a struggle.*¹²

I remarked earlier on the tendency of economists to get the result their theory tells them to expect. In a talk I gave at the University of Virginia in the early 1960s, at which Warren Nutter was, I think, present, I said that if you torture the data enough, nature will always confess, a saying which, in a somewhat altered form, has taken its place in the statistical literature. Kuhn puts the point more elegantly and makes the process sound like a seduction: "Nature undoubtedly responds to the theoretical predispositions with which she is approached by the measuring scientist."¹³

I also observed that a failure to get an exact fit between the theory and the quantitative results is not generally treated as calling for the abandonment of the theory but rather the discrepancies are put on one side as something calling for further study. Kuhn says this: "Isolated discrepancies . . . occur so regularly that no scientist could bring his research problems to an end if he paused for many of them. In any case, experience has repeatedly shown that in overwhelming proportion, these discrepancies disappear upon closer scrutiny."¹⁴ Because of this, Kuhn argues that "the efficient procedure" is to ignore them, a conclusion economists will find easy to accept. Furthermore, Kuhn says:

Anomalous observations . . . cannot tempt [a scientist] to abandon his theory until another one is suggested to replace it. . . . In scientific practice the real confirmation questions always involve the comparison of two theories with each other and with the world, not the comparison of a single the-

12. *Ibid.*, 219 (emphasis in original). 13. *Ibid.*, 200. 14. *Ibid.*, 202.

ory with the world. In these three-way comparisons, measurement has a particular advantage.¹⁵

This last statement of Kuhn's has a special significance for economists. Quantitative studies, or qualitative studies for that matter, may give someone who believes in a theory a better idea of what that theory implies. But such studies (normally quantitative in the natural sciences and increasingly so in economics) also play, as Kuhn indicates, another and very important role. The choice economists face is a choice between competing theories. These studies, whether quantitative or qualitative, perform a function similar to that of advertising and other promotional activities in the normal products market. They do not aim simply at enlarging the understanding of those who believe in the theory but also at attracting those who do not believe in it and at preventing the defection of existing believers. These studies demonstrate the power of the theory, and the definiteness of quantitative studies enables them to make their point in a particularly persuasive form. What we are dealing with is a competitive process in which purveyors of the various theories attempt to sell their wares.

Failure to realise that we are dealing with a competitive situation seems to have led astray even so accomplished an economist as Don Patinkin. Consider this remark of his:

What generates in me a great deal of skepticism about the state of our discipline is the high positive correlation between the policy views of a researcher (or, what is worse, of his thesis director) and his empirical findings. I will begin to believe in economics as a science when out of Yale there comes an empirical Ph.D. thesis demonstrating the supremacy of monetary policy in some historical period and out of Chicago, one demonstrating the supremacy of fiscal policy.¹⁶

I assume that Patinkin did not mean that the empirical findings are fabricated. If this were so, it would be a cause for disquiet. While there is, I suppose, some fraud in economics, it must be quite rare and is certainly not common at either Yale or Chicago. Patinkin expressed

15. *Ibid.*, 211.

16. Don Patinkin, "Keynesian Monetary Theory and the Cambridge School," *Banca Nazionale del Lavoro Quarterly Review* (June 1972):142.

concern about the high positive correlation between the policy views of a researcher and his empirical findings. But this is how it should be. I would be very worried by a negative correlation: if, for example, an economist at Yale advocated reliance on fiscal policy while his Ph.D. thesis demonstrated the superiority of monetary policy. The policy views of an economist should accord with the results of his empirical investigations. What I think really worried Patinkin is that, according to his observations, the empirical findings at Yale and Chicago are not the same. Such differences could come about because researchers in the two universities use different methods for estimating the magnitudes of important variables in spheres in which measurement is very difficult. But I do not think that this is what Patinkin had in mind.

Assuming that Patinkin is right and that the empirical findings of economists at Yale and Chicago are not the same, this undoubtedly reflects a difference in their view about how the economic system operates, a difference, that is, in the theories espoused at the two universities. As Kuhn explains, this will inevitably lead to differences in the empirical findings. A belief that the empirical findings by research workers in all economics departments should be the same might lead an arrogant and ignorant university administration to attempt to destroy an economics department that had a distinctive character and to attempt to remake it so as to be like Yale (few would want all economics departments to be like Chicago). But that would be the way to mediocrity for that university as well as impeding the search for truth by restraining the competitive process.

Some may think that I have treated somewhat too literally what Patinkin said and have therefore failed to deal with the serious issue that inspired it. This may well be right. Earlier I said that many, I thought most, economists would choose to employ one theory rather than another because it afforded them a better base for thinking. Economists who choose theories using this criterion will not necessarily choose the same theory. They may be interested in different problems or approach the same problem in rather different ways or use different techniques of analysis, and these factors may lead them to prefer one theory rather than another. This does not bother me. In such cases there is little that should be done other than to leave economists free to choose.

But there are some motives for selecting one theory rather than another that are more worrying than others, and I think it was this con-

cern that lay behind Patinkin's somewhat facetious remark. In public discussion, in the press, and in politics, theories and findings are adopted not to facilitate the search for truth but because they lead to certain policy conclusions. Theories and findings become weapons in a propaganda battle. In economics, whose subject matter has such a close connection with public policy, it would be surprising if some academic economists did not adopt the criteria of public discussion in selecting theories, that is, choose a theory because it lends support to a particular policy (perhaps the policy advocated by a particular political party). At the same time, they may belittle the work of other economists because it seems to have the wrong policy conclusions. Many of us will, I feel sure, be able to think of an instance of a scholar doing solid work who suffered because his policy conclusions were considered unacceptable at that time.

Yet, such instances notwithstanding, what is striking is how unimportant the influence of such behaviour is over the long period. As an example, consider what has happened to academic opinion on government regulation. Some fifteen or twenty years ago, economists, under the influence of Pigou and others, thought of the government as waiting beneficently to put things right whenever the invisible hand pointed in the wrong direction. The conclusions they drew for policy involved extensive government regulation. Studies made in the intervening years have shown that such regulation often has no effect or has effects opposite to those expected and was commonly introduced to serve the interests of politically influential groups. What has happened is that most economists have changed their views on policy to fit the new findings.

One might have expected, given the stakes involved, that the various groups active in the political arena could have procured economists to voice opinions which served their interests. There can be no question that the affiliation of economists with business or labour organisations or with political parties or even their engaging in consulting does threaten academic integrity. No doubt some economists have been corrupted. Yet my experience is that corruption of this sort, at any rate among economists of quality, is very uncommon or even nonexistent. As Stigler says: "I have seen silly people—public officials as well as private, by the way—try to buy opinions but I have not seen or even suspected any cases in which any important economist sold his profes-

sional convictions." Stigler is clearly troubled by the thought that this implies that economists are not maximising their money incomes, and so he adds:

When we strive to solve a scientific problem, is ambition for our own professional status completely overshadowed by our love of knowledge? . . . When we write an article to demonstrate the fallacies of someone else's work, is our hatred for error never mixed with a tiny bit of glee at the display of our own cleverness?¹⁷

So if we have to admit that we are not maximising our money incomes, we can at least console ourselves by claiming that we are maximising our self-esteem.

It is also true that we value the respect of our colleagues. As Samuelson has said: "In the long run, the economic scholar works for the only coin worth having—our own applause."¹⁸ The professional position of an economist depends on work that could not even be understood by the ordinary person. Samuelson does not owe his reputation to those of his writings that are read by the public but to papers that would be completely incomprehensible to them.

Just as is true for those working in the natural sciences, the activities of economists are regulated by, or at least much influenced by, professional organisations (universities or societies) in such matters as the design of courses, the requirements for degrees, the allocation of research funds, the standards for publication, and qualifications for employment. Respect and position are obtained by doing work which meets the standards of the economics profession. This regulation through professional organisations means that we are to a very considerable extent insulated from outside pressures. But we avoid that danger only by creating another. This danger is that the implementation of such standards, through its influence on courses, research funds, publication, and employment, none of which are necessarily completely

17. George J. Stigler, *The Intellectual and the Market Place* (New York: Free Press of Glencoe, 1963), 92.

18. Paul A. Samuelson, "Economists and the History of Ideas," *American Economic Review* 52 (March 1962):18.

unaffected by political considerations, may be so rigid as to impede the development of new approaches. If this happens, the likely response will be an attempt to form new professional groupings or to carry forward the work under other auspices. If professional organisation is sufficiently loose, as it tends to be in the United States, and the new approach has real promise, such efforts will probably succeed. It is not without significance that the new group of studies that has come to be known as "law and economics" has to a very considerable extent been carried forward in law schools rather than in economics departments, where the economists' somewhat narrow conception of the scope of their subject led them to be, at least initially, largely uninterested in the field.

For economists to be free to choose the theories that will be most helpful in guiding them in their work, and to invent new theories when the existing ones seem unsatisfactory, research has to be carried on within a relatively free educational structure, with universities, research institutes, and the foundations and other bodies that finance research all following independent policies and even within universities allowing a considerable degree of autonomy for schools and departments.

I started this talk by asking, How should economists choose? I have ended by discussing the organisation and finance of academic activities. I do not think that I have lost my way. Instead of confining ourselves to a discussion of the question of how economists ought to choose between theories, developing criteria, and relying on exhortation or perhaps regulation to induce them to use these criteria in making their choices, we should investigate the effect of alternative institutional arrangements for academic studies on the theories that are put into circulation and on the choices that are made. From these investigations we may hope to discover what arrangements governing the competition between theories are most likely to lead economists to make better choices. Paradoxically, the approach to the methodological problem in economics that is likely to be the most useful is to transform it into an economic problem.

In carrying out this task, we may draw inspiration from the example of Warren Nutter. As I said at the beginning of this talk, he possessed what Knight considered the essential attributes of a good scholar: integrity, competence, and humility. But Warren Nutter added

courage. Fearless in the defense of the causes in which he believed, he calls to mind that heroic figure in Bunyan's *Pilgrim's Progress*, Valiant-for-Truth. And it may surely be said of Warren Nutter, as it was of Valiant-for-Truth, that when "he passed over . . . all the trumpets sounded for him on the other side."