Are Economists Getting a Bum Rap?*

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There has been a lot of disparaging talk about economists lately. My good and respected friend, Irving Kristol, wrote an article in the *Wall Street Journal* entitled, "Most Economists Ignore Reality". A little later the same newspaper ran a front page story about the inadequacy of economists and the declining reliance that businessmen place on them. *TIME* magazine on August 27, 1984 carried a feature article headlined "The Forecasters Flunk". The subhead was "Poor predictions give once prestigious pundits a dismal reputation." A leading journalist for the *Washington Post*, most of whose writing consists of retelling what economists said to him, wrote an Op-Ed piece entitled, "The Economists Are Guessing Again". During his first debate with Walter Mondale, when he was asked why his 1980 promise of a balanced budget by 1983 had not come true the President blamed the failure on poor forecasts by economists. The President seems to like economics jokes—that is,—jokes about economists, not by them. Murray Weidenbaum, the first Chairman of President Reagan's Council of Economic Advisors quotes the President as wondering whether he needs a Council of Economic Advisers. Lester Thurow has said that "the public esteem of economists is lower than at any time since World War II." My statistical abstract contains no series on the public esteem of economists, and Professor Thurow is too young to remember back to World War II, so I wonder how he knows that, but I will take his word for it.

All of this gives one to think, as Hercule Poirot says. That is why I have taken the title for this talk, "Are Economists Getting a Bum Rap?" For the benefit of those who have to leave early to catch a plane I will summarize the answer. The answer is "Yes and no." That is the kind of answer economists are accused of always giving. In fact, they don't give it often enough.

I believe that I can evaluate these charges with objectivity, and not only because I am nearing the stage of retirement from the economics business. I have always been quite aware of the limitations of my own knowledge, limitations that I did not think were much more severe than those of the rest of the profession. A good deal of my career as an economist for twenty-two years at the Committee for Economic Development was devoted to developing and explaining a strategy for managing fiscal policy that had as its basic premise that neither economists nor politicians could forecast the movements of the economy very well. In 1969, when Paul McCracken came into office as Chairman of the Council of Economic Advisers, and Hendrik Houthakker and I were members, the *New York*

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Herbert Stein

Times said that the distinguishing feature of the new Council was humility. That was accurate. As compared with our predecessors, and probably as compared with most economists, we had a low estimate of what economists knew. As things turned out, we were probably not as humble as we should have been, but we were on the modest side of the profession. During my service on the Council of Economic Advisers I frequently found myself making forecasts of the short-run behavior of the economy. That was not because I had come to believe that I had a great ability to do that. It was only because I had learned that my ability in the field, when fortified by the Council staff, was not significantly less than the ability of other people who did it all the time. After I left the Council I returned to my stance of eschewing short-run forecasting and of emphasizing the limitations of the knowledge claimed by economists, which is a full-time occupation. Earlier this year I gave a lecture at the annual meeting of the Western Economic Association entitled "Bricks Without Straw," which was mainly a demonstration that economists do not know many of the things they say, together with some speculations on why they say them anyway and on how the economy nevertheless prospers.

So I do not feel personally aggrieved by all the recent criticism of the economics profession. But I do believe that much of this criticism rests on little information and less analysis.

For one thing, most of the criticism relates to errors in short term forecasts of economic aggregates like GNP, unemployment and inflation, As I shall explain at greater length below, that is too narrow a standard by which to evaluate the work of economists. But even on that standard the criticism rests on little information. In the articles about the errors of economic forecasts written by journalists and other non-economists, I never see any factual information on the record of economic forecasting. It is as if a sports reporter were to appraise Ted Williams as a batter without giving his batting average but only giving an anecdote about the day he struck out three times. In fact, there is a good deal of work on the record of economic forecasting, all of it done by economists rather than by their critics. I think of the work done by Victor Zarnowitz [7] and Stephen K. McNees [5].

In a survey of forecasts done a year ago, McNees reported the following results for the median forecasts of five prominent forecasters for the period 1971 to 1983 [5, 10]: the average absolute errors of the forecast of the change of nominal GNP made four quarters earlier was 2.2 percent. This was for a period in which the average annual change of nominal GNP was 9.7 percent. For the change in real GNP the average absolute error was 1.6 percent when the average actual change was 3.3 percent. The error in forecasting the change in the implicit price deflator for GNP was 1.4 percent when the average change was 6.8 percent, and the average error in forecasting the level of the unemployment rate was 0.7 percent when its average level was 7 percent. Forecasting the figure for a particular quarter four quarters ahead is harder than forecasting the figure for a year, which allows for compensating errors in the quarters. The average errors of the consensus of the Blue Chip panel in forecasting the next year's change of real GNP was 1.1 percent for the past seven years, when the actual changes averaged 3.6 percent [1, 8].

Are these errors large or small? I will turn to that question in a moment, but first I want to emphasize that the usual critics of economists' forecasts are not looking at this information at all, as one would expect from a serious appraisal.

One way in which economists have sought to measure the validity of these forecasts is to compare their results with the results of using some naïve hypothesis, such as the extrapolation of last year's change. The usual finding is that the sophisticated forecasts come out better than the naïve ones, although there may be some variables for which that is not true. But still this kind of comparison does not provide a satisfactory explanation of the contribution of economists to our knowledge of the future. For even a naïve process like extrapolation is the extrapolation of a quantity that was conceptualized and measured by economists, without whose work the naïve process would have been impossible. It's like criticizing Christopher Columbus for not knowing where Columbus, Ohio would be. It is relevant that two economists, Simon Kuznets and Richard Stone, have received the Nobel Prize for work in the definition and measurement of national income. That was considered a sufficient contribution without the further requirement of ability to forecast it. But now people who would never have imagined the GNP without the prior work of economists criticize economists for errors in forecasting it.

Another question that might be asked about economists' forecasts is whether they are inferior to the forecasts of the same variables made by non-economists. This is hard to answer with confidence, because as far as I know no systematic records have been kept of the forecasts of non-economists. I suppose that every year there is some non-economist who makes a better forecast than the median of economists. If there were some one non-economist who had a long-term batting average better than the median of economists, I am sure we would have heard of him, but we have not. If we had heard of him we would, of course, declare him to be an economist, because the requirements for that designation are quite flexible. In any case it is inconceivable that this unknown forecaster did not use information provided by economists. And I am sure that the average forecast of all non-economists is not as good as the average forecast of all economists. I do not think we need to feel depressed about the possibility that someone else is forecasting the economy better than we are.

It would be interesting to compare the accuracy with which economists forecast the near-term behavior of the economic aggregates with the accuracy of other scientists in estimating their variables. That is not, however, possible in a meaningful way, because the difficulty of the forecasts varies enormously among subjects. It is obviously easier to forecast the time of tomorrow's sunrise than to forecast the GNP for the next quarter. I see no way to discount the accuracy of the forecasts for the degree of difficulty, except by assuming that economists are as smart as astronomers and the relative accuracy of their forecasts measures the difficulty of the subject. But that would beg the question.

I tried to raise this issue with a friend who is the president of the National Academy of Sciences. When I was unable to make the question clear in general terms I asked him whether he could compare the accuracy of economists' forecasts with the accuracy of physicians' forecasts. He replied that he did not consider medicine a science. I came away from that conversation only slightly consoled by the knowledge that physicians are in great demand and earn high incomes. I have always been impressed by the fact that engineers do not know with great precision what is the load that a steel beam will bear. They guard against the collapse of bridges by using much more steel than their most probable estimate of the requirements. They are able to do that because the cost of steel is low compared to the probable cost of a bridge collapsing. That is much harder to do with economic policy, but it does point to the need to consider how we can most efficiently hedge against the consequences of the limitation of our knowledge.

Having found no better way to evaluate the degree of our ignorance I fall back on the market test. The premise of much writing about the decline of economists is that the users of economists have become disillusioned and no longer rely upon them or employ them. One aspect of that question was examined in a recent address on a subject like mine by A. Nicholas Filippello of the Monsanto Company and President of the National Association of Business Economists [3]. He pointed out that the number of members of the National Association of Business Economists increased from 322 in 1959 to 2200 in 1974 and to about 4000 in 1984. Moreover, according to salary surveys conducted by NABE, the compensation of business economists is still rising. And it should be noted that short-run economic predictions are a more important part of the job of business economists than of other economists.

The job market for college and university economists is not booming, but that is due to the levelling-out of the college population, not to disaffection with economists and economics. The demand for economists on the lecture circuit seems to continue strong, and what the audiences on this circuit most want to hear from an economist is a short-run prediction. I have pointed out elsewhere that the demand for economist lecturers is motivated by the desire of business organizations to make weekends at golf resorts tax deductible. But the recent reduction of marginal tax rates does not seem to have reduced the demand. Also, the employment of professional economists in writing for newspapers and magazines is flourishing. The number of best-seller books about economics, even excluding self-help books, is large. And hardly a day passes without some economist appearing on national television.

Even the present Administration, not notoriously enamored of economists, contains an impressive number. The Secretary of State, the Chairman of the Federal Reserve, the Undersecretaries of Treasury and Commerce, the Chairman of the Federal Trade Commission and undoubtedly other high officials of whom I do not now think, are economists. The White House keeps two of the three members it is authorized to have on the President's Council of Economic Advisers. We even have a president who was an economics major in college.

So I find little basis for criticizing the short-run forecasting of economists, except to say that it is not as good as we would like — but, then, few things are. Neither is there much evidence that, despite the complaints, the customers have stopped buying the product.

But the most important point, in my mind, is that economists should not be judged by the forecasts of next year's GNP or price level. I am prepared to stipulate that the value of economics should be judged by forecasts of some kind. I will confess that there are moments when I think that economics is a branch of literature, or entertainment, or show business, which can have value even if it cannot forecast. But economics would not rank very high in that category, and I will not press the matter. The main thing to say is that short-run aggregate forecasts are not the most significant forecasts that economists make or the ones they do best.

Economists have made a major contribution to the world's thinking by predicting some general consequences of the way the world is organized. The most important of these predictions was that an economy organized by voluntary exchange would be more satisfactory to its participants than one organized by command. That prediction has turned out to be correct. It was made a little over two hundred years ago, and one might ask whether that justifies the presence of about 20,000 economists in the United States today. But everyone does not yet know about that prediction, and most economists are still engaged in reminding people of it, just as most physicists are engaged in teaching about predictions that Isaac Newton made centuries ago.

There is a less grand level of prediction that is nevertheless very useful. Thus, in an illuminating essay on Frederich Hayek, Samuel Brittan said:

> . . . even before the fashion for forecasting models had fully developed, Hayek wrote a shrewd critique of bogus quantification based on the complexity of the phenomena studied. His essential point is that we cannot count on the good fortune of being able to discover by direct observation simple quantitative regularities between economic variables — but it is still possible and worthwhile to formulate general rules. For instance, economic theory can tell us that we cannot maintain a fixed rate of exchange and at the same time maintain an independent financial policy with a national price level objective; but this does not mean that we can predict where the exchange rate will go if the latter option is chosen [2, 58].

What Brittan here calls a general rule is a prediction — a prediction that if a country maintains an independent financial policy with a national price level objective its exchange rate will not remain fixed. That is a useful prediction, and one that would only have been made by economists.

Even in the area of quantitative forecasts, where we have been most criticized, we have allowed ourselves to be too much measured by unrealistic expectations to which we have contributed. I believe that one of the best-established findings of economics is the relation between the money supply and the price level. This relation is commonly belittled these days because we cannot tell how much difference it will make for next year's price level whether the money supply grows by 5 percent or 6 percent. But the prediction that if there is a very high rate of monetary growth for a very long time there will be much inflation is valid and useful. Such a prediction may seem so obvious today that one doesn't have to be an economist to make it. But it was not made, however, without the work of economists. And one has only to look at Israel and Argentina, for example, to see that the prediction has not been obvious to everyone.

Whether or not economists make predictions about the economy, many other people will do so. Economists serve a useful function as critics of these predictions, helping to cast light on their probable validity. As one example, for a long time there has been a popular prediction that a general reduction of tax rates would, within a short period, raise the revenues. This prediction has been mainly the property of old Republican war horses — Andrew Mellon, Harold Knutson, Daniel Reed — and others not so old. Although some economists have recently supported the proposition, on the whole economics has been skeptical of it. The basic contribution of economics to this issue has been to analyze the proposition into the parts that would have to be true if the conclusion were to be true. If the tax cut were to raise the revenue the elasticities of supply of labor and of savings and of tax shelters would have to be not only in the right direction but also of at least a certain magnitude. Then it would be possible to speculate about or try to measure the relevant magnitudes. The measurements, admittedly inconclusive, at least suggested that the prediction of more revenue from lower taxes was very doubtful.

There was another kind of valuable contribution to this question. That came from the attention economics pays to indirect effects usually overlooked by non-economists. In the case under consideration, if the tax cut did not raise the revenue, there would be, under reasonable assumptions, an increase in the deficit, and the effects of that would have to be considered. Thus there was at least a possibility that the tax cut would not only not raise the revenue but would actually depress rather than raise the long-run rate of growth through the adverse effect of the deficit.

These warnings of mainstream economics may have been unjustified although nothing that has happened in the past three years has suggested that. But whatever may yet turn out to be true in that case, consideration of the kinds of analysis that economists use will, on the average, yield better results than the intuition and wishful thinking commonly applied.

I was struck recently by a tribute to economics from what would be universally considered a "real scientist". Edward O. Wilson, a noted biogeographer, was discussing with a colleague, Robert MacArthur, the problem of determining how many species would exist in a certain piece of territory, such as an island. MacArthur said:

> Here's how a physicist or economist would represent the situation. As the island fills up, the rate of extinction goes up and the rate of immigration goes down, until the two processes reach the same level. So by definition you have dynamic equilibrium. When extinction equals immigration, the *number* of species stays the same, even though there may be a steady change in the particular species making up the fauna [6, 456].

Economists will recognize the economists' kind of reasoning in these remarks.

Economists can contribute to improving the general run of predictions on which the citizens base their ideas of public policy by dispensing even the simplest kind of information. I have recently participated in a number of radio and TV shows in which the audience calls in to express its thoughts. I have been staggered by the ignorance that is out there and that must influence what policymakers feel they can do. Callers insist that the budget could be balanced by cutting out foreign aid or by recapturing the interest the Federal Reserve earns on the Federal debt it holds. A recent poll showed that a large proportion of the population thinks that defense expenditures absorb about 40 percent of the GNP and only 6 percent of the respondents believe, correctly, that it is less than 10 percent. Can you imagine trying to make reasonable decisions about the defense budget in a country like that? Can you imagine trying to make reasonable decisions about tax policy in a country where the common belief is that corporate profits equal 40 percent of corporate sales? Economists know better than that.

At the man-in-the-street level, among callers-in to radio and TV shows, there is a mass of economic information that just isn't so. At a somewhat more sophisticated level, among politicians and journalists, there is a body of influential beliefs that if not certainly wrong are highly improbable and where facts readily available to economists will reveal their improbability. I think of the common prediction that the economy can or will grow by 5½ percent per annum for the next five years, instead of the 3½ percent projected by the Congressional Budget Office or the 4 percent projected by the Office of Management and Budget, and that this higher rate of growth will bring the budget into balance without other measures being taken. But I look at the 33 overlapping five-year periods since World War II and see that the average annual rate of growth in those periods was 3.4 percent, that the standard deviation around the average was 1.0 percent, that there was only one period in which the rate was over 5 percent and that the trend of the growth rates has been declining. None of this means that the economy cannot grow by 5½ percent per annum in the next five years. It does mean that a plausible forecast of such a growth rate requires a good reason for thinking that the economy has changed substantially. I bring this up to illustrate the point that the simple facts with which economists are all familiar can make a useful contribution to the consideration of economic policy that is carried on continuously by non-economists.

The claims that economists can legitimately make for themselves are modest ones. There are a great many decisions that people must make, or at least do make, in their private and public life, to which the information, analysis and way of thinking of economists can make a useful if not conclusive or infallible contribution. That should be enough.

I said at the outset of these remarks that I would answer the question, "Are Economists Getting a Bum Rap?" by "Yes and No." I have talked so far about the yes side. I will turn now to the other side.

Our problem is not so much that the world thinks too little of us. It is more that we have allowed the world to think too much of us. We have allowed the world to think that we know more than we do, or that we know what we know with more certainty than we do. We have *allowed* the world to think that; we have not *made* the world think that. We have not insisted on how much we know, but we have not insisted on telling the world about all the limitations of our knowledge.

We are led into this behavior because that is what the world seems to want, and we supply it. If they want to know what percent of the nation's poverty is due to President Reagan's budget cuts, we tell them—down to two decimal places. If they want to know how much the deficit will raise interest rates, we tell them, to at least one decimal place. If they want to know next year's inflation rate, we tell them. There are exceptions—economists who emphasize that they don't know or at least indicate the range of possible answers and what can be said about their probabilities. But, unfortunately, even when that happens only the unqualified guesses make the editorial pages of the newspapers.

Still, economists have a share of the responsibility. Partly it is because they seek the attention and other rewards that come from clear unqualified statements. But it is also partly because they are so fascinated by their econometric finger exercises that they mistake them for revelation. They are like pharmaceutical companies that rush to try their new medicines on humans before testing them on rabbits.

In his excellent book, *Knowledge and Ignorance in Economics*, T. W. Hutchinson delivers a forceful admonition to economists:

> No kind of ignorance can be more dangerous than ignorance regarding the limits and limitations of one's knowledge. Insofar as he may be able to combat and reduce this kind of ignorance, the student of the methodology and history of economics has a task which is of considerable social and political importance, as well as a worthy scholarly and philosophically interesting pursuit. In fact, to promote clarification of the extent and limits of economic knowledge and ignorance may well do much more to reduce dissatisfaction with current economic policies and their results, than do many or most of the contributions to confused and undisciplined wrangles and debates on particular policy problems [4, 5].

Hutchinson then goes on to quote Keynes saying that he looked forward to the day when economists would be regarded as modest, useful people, like dentists—which is ironic, because there has probably been no economist less like a dentist than Keynes was.

The exaggerated view of what we know leads to occasional spasms of public disappointment and annoyance when the exaggeration is discovered, as must happen from time to time. There is, however, a more serious consequence. Because we think, erroneously, that we know so much we fail to give the necessary attention to developing policies to cope with our ignorance. This takes me back to the work we were doing at the Committee for Economic Development almost forty years ago. Being greatly impressed with the unreliability of short-run forecasts, we tried to formulate a fiscal policy that did not depend on such forecasts. That was the origin of the idea of setting the budget to balance at high employment. Subsequently, especially after inflation became a major factor in our lives, inadequacies of this strategy appeared. But I believe that we were making a step in the necessary direction. Similarly, proposals for stable rules of monetary policy, like the rule of a constant rate of growth of the money supply, are efforts to cope with the policy consequences of our ignorance and inability to forecast changes in velocity. But on the whole the profession seems too confident of what it already knows, or is about to learn, to be much interested in policies that start from the proposition that we do not know very much.

We have immediately before us an example of neglect of the basic fact that we do not know what we pretend to know. Contending parties in the debate over tax and budget policy rest their case on projections of the economic growth rate for the next five years. But the basic fact is that neither Donald Regan, nor Rudoph Penner nor I knows what this growth rate will be. We should not simply insist on a policy that is consistent with what we now believe to be the most probable growth rate. Since the probability of making an error is high, we should be asking ourselves which error — assuming too high or too low a growth rate — will be less costly. We should be deciding what we will do if the initial assumption, whatever it is, turns out to be incorrect, and how we will recognize that it has been incorrect. We should be exploring what advance preparations we can make for the adaptation of policy when the error of the initial assumption appears. Those would be more useful reactions to the limited state of our knowledge than repeated assertions that growth will be 3½ percent or 5½ percent.

So I conclude where I began. We should not be bowed down by the complaints of our vulgar critics, but we should try to adapt intelligently to the fact of our ignorance.

I will close with one last reminiscence. When I was a student at Chicago almost fifty years ago, Professor Frank Knight used to ask medical doctors how many years they thought it would be before they cured more people than they killed. I used to think that was a terribly cynical remark, and I also thought that he had economics in the back of his mind as being even more primitive than medicine. But I later came to see the hopeful side of Knight's question. He was assuming that the time *would* come when medicine cured more people than it killed. Indeed, fifty years later it may have already come. The same may be true of economics. We *will* pass the cost / benefit test, and may already be doing so.

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