I

Research Leadership

Martin Feldstein became President of the National Bureau of Economic Research on April 22, 1977, upon election by the Board of Directors. Dr. Feldstein, professor of economics at Harvard University, succeeded John R. Meyer, who is also a professor at Harvard and who had advised the Board in April 1976 of his desire to step down from the NBER's presidency which he had taken over in 1967 from Arthur F. Burns.

Also in April 1977, James J. O'Leary was elected Chairman of NBER's Board. In the Bureau's chairmanship, Dr. O'Leary, who is Vice Chairman of the Board, United States Trust Company of New York, succeeded J. Wilson Newman, retired Chairman and Chief Executive Officer of Dun & Bradstreet, Inc., and current Chairman of its Finance Committee.

A MESSAGE FROM MARTIN FELDSTEIN

I am pleased and honored by the opportunity to serve as President of the National Bureau of Economic Research. During the past few months, I have thought a great deal about the Bureau and the unique and valuable contribution that it can make to economic research in this country. If I had to summarize in a single word what I regard as the Bureau's potential contribution, that word would be "interaction." The Bureau can provide a framework within which to achieve research goals that individual scholars could not reach alone or through other existing organizations.

Scientific research in economics is generally done by a single individual who works in a university department where he is the only expert on his subject. Contact with the other academic researchers who are concerned with the same problem is infrequent and often unsuited to careful collaboration or critical discussion. Contact with business management and labor leaders and with knowledgeable business economists is even more rare. All of this limits and impedes research.

First-rate scholars in particular fields and different universities can benefit by meeting regularly, having offices at the Bureau, participating in summer research groups, or attending frequent small workshops. Other well-trained economists can benefit from research leadership and technical advice by participating in projects and programs at the Bureau. Such interaction among scholars will both improve the quality of ongoing research in the Bureau's areas of emphasis and permit the development of research projects of a greater scope and effort than would otherwise be possible. The Bureau can also provide the vehicle through which academic economists can develop working relationships with economists outside the universities and with researchers in other countries that have economic problems similar to our own.

Interaction of the sort I envision need not mean a huge program. My own predisposition is to a small, concentrated, and high-quality research group that does not attempt to include all the important problems in economics or all the good economists in the profession! This brings me to the substantive focus of the Bureau's research. In the past fifteen years the Bureau has pioneered and developed a substantial program of research in various aspects of social economics: health, fertility, human capital, law, family structure, etc. I believe that these are important subjects for economic research, that the Bureau associates in this area are of very high quality, and that this work should remain a significant part of the Bureau's overall research effort.

There are other subjects that were once central concerns of Bureau research but that now receive relatively less attention. I have identified three such broad areas in which I plan to develop major long-run programs of research: (1) the aggregate economic behavior of the economy, including the long-term growth of the economy, the impact of inflation, and the problems of unemployment; (2) the public sector, including issues in taxation; and (3) the changing structure of labor and product markets. The lines separating these subjects are often ambiguous. Moreover there are important and closely related issues that do not fall neatly into any one of them and that should be significant in the Bureau's research, e.g., the problems of international trade and investment, the behavior of financial markets, and the distribution of income. The research programs in these areas will deal with economic questions of the greatest importance and will allow outstanding researchers to benefit from working within the Bureau framework.

Let me conclude with a brief word of thanks to John Meyer for his encouragement and for his efforts during the period of transition. I know that John can look back with substantial satisfaction at the achievements and innovations of the past decade. His long-standing plan to retire from the presidency at this time reflects his strong belief in the value of bringing new ideas and new people to the Bureau. I know I speak for everyone associated with the National Bureau of Economic Research when I thank John for his contribution to the development of the Bureau and I extend to him our best wishes for the future.

REPORT FROM JOHN R. MEYER

It was with considerable satisfaction that I approached the chore of drafting this, my final status report, to the Board of the National Bureau. My reason is simple: The Bureau has almost surely never been in better condition in the ten years that I have been at its helm than it is now. The Bureau's budget is currently in balance, even surplus,[1] and the research program continues to flourish and develop. This helps explain why, among other reasons, this seems a propitious moment for me to step aside and hand over my duties to someone else. In addition, I have now served just a few months less than ten years at the helm of the Bureau, and as I pointed out when I accepted the position back in 1967, I felt strongly that five to ten years was a proper range for one to serve in the Bureau's leadership. Above all else, that self-imposed time limit seems sensible because an organization dedicated to basic research, as the Bureau is, constantly needs new ideas, new departures, and innovations to properly fulfill its role. To be preoccupied with the past—to do again what has already been done successfully before—would be fatal!

New Research Undertakings

Several new research initiatives have, in fact, been launched at the Bureau recently. Most of these are in areas that might best be described as macroeconomic in character. First, we have undertaken an effort at modeling trade patterns of the Pacific rim; this work is under the direction of Bert G. Hickman and Lawrence J. Lau of Stanford University. We have also launched an experimental investigation of the costs associated with using housing policy as a major policy tool for stabilizing business fluctuations; this is a cooperative venture with the Harvard-MIT Joint Center for Urban

1. As of March 1, 1977, the Bureau operating surplus (unaudited) for the first eight months of this fiscal year was $58,000.

Studies and is being conducted under the direction of Carol Corrado of the Federal Reserve Board staff and Tom Cooley of the University of California, Santa Barbara.

Another new initiative in macro research at the Bureau involves the work of Benjamin Friedman of Harvard. He and a group of young associates are working on a very ambitious effort to model the major financial linkages of the American economy. Their immediate goal is to develop a more detailed monetary market sector for some of the standard econometric models now in wide use. Friedman has also agreed to think about where Bureau business cycle research might best develop and to prepare a report on this subject for future submission to and consideration by the Bureau's Board.

Another major research undertaking of the past few months, more microeconomic in character, has been the development by Robert Fogel of a very detailed and extensive scheme for investigating United States demographic patterns of the nineteenth century. This work will build on some rather unusual data bases, in particular the genealogical archives of the Mormon church.

We have also become tentatively involved in undertaking a study of the economics of library operations. This study was at the instigation of a long-time friend of the Bureau, Axel Rosin. At his suggestion, the director of the New York City Public Library and I have sat down on several occasions during the past few months to discuss what it was that economists did not know about libraries and, vice versa, what librarians did not know about economics. From these discussions a research design has emerged that could yield some interesting results, not only from the standpoint of bettering our understanding of libraries and local public financial problems, but also of extending our knowledge of the service industries and their characteristics. Malcolm Getz of Vanderbilt University and Robert Leone of Harvard have played the lead roles in this development.

There have been other new undertakings at the Bureau during these past few months, but I shall skip over them somewhat lightly on the grounds that most represent extensions or developments that have emerged from previous work. They are also reported extensively in the progress reports found elsewhere in this volume. My major point is, simply, that the Bureau has *not* stagnated intellectually in these past few months even though we have experienced an inhibiting budget situation as well as the uncertainties inevitably associated with a leadership transition. The fact that the organization has not stagnated in these circumstances is, I think, a strong tribute to its underlying strength.

Projects Terminated, Opportunities Forgone

A few words are also in order on what we have ceased doing, or avoided doing, at the Bureau during these past few months. Probably the biggest single "phase down" at the Bureau is that associated with the U.S.-U.S.S.R. exchange program. That program is now more or less completed in so far as the Bureau is concerned as we have handed the task on to others. Some minor tasks (e.g., completing certain publication and dissemination activities) remain to be done, but most should be finished by the end of the summer. In budgetary terms this program has dropped from an activity level of approximately $150,000 per year to about $20,000 to $30,000 per year as of April 1977. Needless to say, phasing this project down will help reduce the "leveraging" of our unrestricted funds—something which I have indicated to you in the past I have felt was necessary and a view which several Board members have shared with great vigor.

As an aside—perhaps the kind of an aside that one can be permitted in a final status report—I would observe that this U.S.-U.S.S.R. exchange activity represented more of a service than a research undertaking and therefore is of a type that the Bureau should probably not normally undertake. Of course, it was not always obvious that this project would continue to retain a heavy service emphasis. Initially, we had great hopes that the exchange program would lead to some truly substantive joint research undertakings between American and Soviet scholars. Unfortunately, and perhaps quite predictably for those who have had longer exposure to these activities than I have had, these hopes never came to fruition. The project, though, was in my view nevertheless worth the effort. It did lead to some improvement in communications and understanding between Soviet and U.S. scholars—a development which is of benefit even if it is not exactly appropriate or central to the activities of a research organization such as the Bureau.

A temptation almost always exists, incidentally, for the Bureau to become too involved in service activities. I recall that in the very first months after I assumed the Bureau presidency, the suggestion was made in a New York *Times* editorial that the Bureau assume the function of rating municipal bonds—an invitation I promptly declined. Since then, members of the staff and people inside and outside the Bureau often have suggested that the Bureau become involved in various data bank, computer utility, and similar service activities. I feel that the Bureau should eschew such undertakings except to the extent that they very directly complement its research function. For example, we have made the Bureau's time series business cycle data bank available to outsiders on a limited cost-recouping basis since we had to maintain the data bank for our own internal research purposes under any circumstances; it was also helpful to have others use the bank in order to give us feedback on its accuracy, utility, and other properties. Similarly, we have involved ourselves in operating a *very modest* computer utility activity as a means of obtaining feedback on the software developed at the Cambridge Computer Center; this activity also provides a means for initial dissemination and testing of our computer programs as these have been developed. Nevertheless, I feel very strongly that the Bureau should be *extremely* careful in extending these activities or undertaking any new ones of this type. Indeed, a good general management rule for the Bureau is to never do anything for itself that can possibly be hired from outside at a remotely reasonable price.

The Future

Having pontificated on what we should not do at the Bureau, I would also volunteer some speculations on where our research interests might develop in the future. As I said in my very first introduction to a National Bureau annual report, trying to identify research priorities for an organization like the Bureau really starts with attempting to identify which public policy issues of an economic character are likely to come to the fore and require administrative or legislative action over the next decade or so. This exercise, incidentally, is also likely to be a bit better focused if, in addition, we can identify some of the more likely or prospective solutions that will receive serious consideration. With these alternatives in mind, at least implicitly, attempts should then be made to identify those facts that are likely to be crucial in making choices between these alternatives and those that seem to be most uncertain or unknown. The Bureau function, quite clearly, is to attempt to fill any such factual void through research. That is what brought people together to create the Bureau in the first place and seems to be the major cement for continuing the association.

In the paragraphs that follow I have attempted to make a few such identifications, offering them primarily as a basis for further discussion.

1. Refinancing Less Developed Countries' Private Bank Debt

The major alternatives would appear to be some kind of government absorption or guarantee of these debts either via extension of the responsibilities of the International Monetary Fund or the Export/Import Bank or the creation of a new federal banking institution (perhaps in the mode of those backing up mortgage markets, such as Fannie Mae or Ginnie Mae). It is also possible that the need might be met by creating a new "window" at the Federal Reserve or through commodity price stabilization agreements. Among the many factual uncertainties that would seem to cloud this choice would be, to start, a determination of the extent of the existing exposure, prospective future demands for increasing that (private bank) exposure, and the risk premium, if any, built into present and prospective loan agreements between the private banks and the LDCs. It would also be well to have a firmer determination of the exact extent to which previous attempts at commodity price stabilization have really been effective in achieving their goals. Of course, there are also many uncertainties inherent in any future "third party" developments; for example, the possible impact on the LDCs of the Soviet bloc countries emerging from autarchy and becoming more competitive as sellers in international raw material and basic manufacturing export markets.

2. Social Security Financing

A much too often used word, "crisis," may actually apply to Social Security financing. Certainly, the escalation of taxes in recent years is well known and dramatic; it is almost as well known that Social Security taxes are inherently highly regressive and therefore counter to other widely advocated or accepted taxation goals. There is also a growing suspicion that the burden of Social Security taxation on the employer is having an adverse effect on employment levels and particularly on the absorption of younger, less skilled workers into the labor force. An often-suggested alternative for alleviating these problems is to transfer some of the financial burden now supported by Social Security taxes onto general tax revenues, e.g., the income tax. The recent proposal to absorb a small percent of Social Security taxation for corporate employers represents one small step in this direction. Other suggestions have been made that the specific responsibility for some of the more particularized and rapidly growing burdens now borne by Social Security (e.g., in medical and dependent child care), might be better supported from general taxes. Among the many factual uncertainties that becloud the policy choices in this area are such simple ones as determining the possible costs of the different alternatives and the effect of revised tax burdens on the general economy, effects which might be expected to be transmitted through savings, investment decisions, and related multipliers. A more intensive analysis might also be made to determine the probable effects of any such changes on labor force characteristics and participation. For example, what would be the impact on female participation in the labor force? On the age of retirement? On the nonprofit sector, which has the option, increasingly indulged, of leaving the Social Security system?

A good deal of research now going on at the Bureau relates fairly directly to these questions and issues. For example, our studies on health economics could help clarify, perhaps even answer, some of the questions regarding the probable financial cost of some of the alternatives. Much of the Bureau's work on income distribution and related questions of human capital is directly relevant. A recently inaugurated program to study intensively the American family and its changing status would be directly relevant to bettering our understanding of labor force participation and a whole host of related questions such as the role of the family itself as a source of old age security or insurance.

3. Protectionism versus Free Trade in International Economic Relationships

Only a few years ago, protectionism and related manifestations of economic nationalism seemed to be essentially dead issues in American politics. However, recent changes in international trade relationships coupled with a world recession in the industrial countries have led to a sharp revival of concern over import competition. It seems reasonably obvious that these concerns will be heard in Congress. The exact form that the policy responses might assume is difficult to determine. The most straightforward, of course, would be to retreat behind a more protective tariff, trade quotas, and similar restrictions. Another response might be to develop domestic policies for more fully compensating those dislocated by new trade patterns; for example, one might think of a greatly expanded government program of extended unemployment compensation or retraining for those who lose their jobs because of expanded imports. Still another, and probably more constructive, response would be to undertake a systematic overhaul of the international monetary and trade agreements and their related agencies, i.e., GATT, IMF, IBRD, etc. Among the many factual uncertainties is our scant knowledge of the potential impact of newly emerging trade patterns and relationships. For example, we really do not know very much about what the long-run impact might be of the various commodity agreements now being implemented or discussed. Similarly, as already noted, very little has been done to study the longer-run implications of Soviet and Eastern European emergence from autarchy, although it is reasonably clear that this emergence is likely to impact less developed countries rather more seriously than the industrialized West. Another uncertainty is determining what would be the *net* effect on U.S. employment of more protectionist policies on the part of the U.S. government. On a more general level, a firmer estimate is needed of the impact of protectionism on the world's overall economic growth performance. There is also the issue of what the imposition on multinationals of more nationalistic or regional governmental controls might do. Finally, there is the ever-present specter, especially now that world currencies have been disengaged from pegged levels, that competitive devaluation of currencies might recur, with all the attendant turmoil these could generate. Again, we have been studying some of these issues at the National Bureau, but without question a more concentrated effort would be desirable.

4. Establishing Environmental Targets and Goals

The issue of what constitutes sensible environmental goals for our society is one that is ever recurrent. The Clean Air Act Amendments of 1970, which ostensibly established effluent standards for automobiles, have been the subject of tense legislative discussion and debate for several years now and promise to be on the legislative agenda for years to come. Though not quite so dramatic or well known, water quality controls are almost as controversial. A major new element has been injected into the policy debate, moreover, by the recent introduction of bills that would achieve ambient air or water quality *not* through establishing standards as such, but rather by imposing taxes that provided incentives for people to pollute less; a major advantage of this tax approach would be more flexibility or choice, e.g., those who found it relatively inexpensive to "clean up" would do so, while those who found it difficult might be less prompt in compliance. There are several factual issues that confuse these environmental policy debates. For example, very little is known about the way in which various effluent levels translate into actual ambient air quality; that is, we really know very little about the physical processes by which poor ambient air or water is created. Obviously, it is difficult to assess the economic benefits and costs when the physical facts are little known. While a better start has been made on the economic than the physical aspects, little enough is known about the economics, especially macro, of pollution. In particular, we know little about the possible impact of environmental controls on longer-term economic growth and capital needs. One plausible hypothesis would be that the controls would slow growth at first and then speed it up (by forcing earlier retirement of some existing but highly polluting capital equipment). At present, moreover, we really do not have the tools to measure these macro effects adequately. For example, the present practice is to exclude most externalities and nonmarket activities from official national income or GNP estimates. This may badly bias our judgments in these matters, since environmental improvement would often normally show up as a nonmarket externality. Thus, it is at least possible that we are doing better than the official estimates indicate in growth of productivity and in terms of true economic and social well-being. It is also highly probable that our presently accepted estimates of the cost of environmental improvement may be badly misleading.

5. Welfare Reform

Several attempts have been made in recent years to simplify and improve the methods by which transfers are made from government to low-income groups in society. It is also a subject which has received a good deal of attention from economists. Indeed, one might go so far as to say that there is fairly strong consensus in economic circles, shared by both the right and left of the political spectrum, that some form of fairly direct transfer mechanism, such as that of a negative income tax, would be the best way to solve the so-called welfare problem. Some experimentation, in fact, has been undertaken with direct transfers to determine what the effect on economic incentives might be of going from the present system to more direct and graduated transfer systems. It is also clear, regardless of the consensus among economists and the growing body of factual material on which that consensus is based, that the issue of welfare reform is very far from resolved politically and will be under intense legislative and administrative scrutiny in years to come. I would not pretend to know where this debate will end or even which one of the many different possible solutions will receive major attention. At the Bureau, our principal contribution to the factual aspects of these policy debates has been work on making a better determination of income distribution. One useful extension of this work would be to focus somewhat more closely on what the distribution of income might look like if better account were taken of so-called income-in-kind. It is not clear, for example, that simple monetary measures of income distribution actually reflect the true realities; for example, it is at least arguable that with income-in-kind included, distribution of income is not as skewed or uneven as many argue, and there are not as many people in true poverty in the United States as many sometimes assert. A fairly firm grip could be obtained on some of these unsettled issues by analyzing some of the data that have recently been generated at the Bureau. One might also add that the whole question of evaluating welfare programs, as well as OASI, would be advanced greatly by having a firmer fix on the distribution of *wealth* as well as of income in our society. There is much evidence, for example, that while the aged have quite low incomes, they also have a fairly substantial wealth position as contrasted with the younger groups in society who are now being taxed to finance transfers to the elderly. Achieving better estimates of the distribution of wealth has, in fact, long been a major goal of Bureau research but one that has proved quite elusive for a number of reasons, e.g., the difficulties of tracing and measuring wealth in a complex society such as ours. The recent emergence of large-scale pension programs and the ever greater role of government have only compounded these difficulties.

6. General Observations

Obviously, this list of contemporary or potentially contemporary policy issues is hardly exhaustive. In some sense, the list represents my own personal biases and judgments. It is also clear that I have left out many areas of intense public concern. For example, I have mentioned nothing about important issues of industrial structure or regulation. This reflects a judgment on my part, possibly mistaken, that these issues are not likely to achieve a consensus or sense of focus sufficient to elevate them to immediate policy concern; e.g., I suspect there will continue to be a good deal of talk about these issues, but relatively little action on them. Nevertheless, we have been doing some research at the Bureau that relates to some of these concerns and have scheduled a major conference on regulatory issues for the near future.

I have also not discussed what really remains the basic economic problem of industrialized countries: easing their economies back into a sustainable steady growth pattern that is devoid of both inflation and gross environmental abuse. Rather, I have partitioned this overriding or key economic question into subparts relating to particular policy concerns that are likely to be actively under consideration in the years immediately ahead. Clearly, though, to achieve these goals of full employment and growth without inflation or environmental deterioration, we must understand many, many things better than we do now. For example, what is it that determines wage rate movements in our society? How do we better evaluate and design programs to put people to work? What is the role of capital and capacity creation in the inflation process?

In large measure, the basic approach of economists to understanding most of these "large" issues has been in the realm of macroeconomics, while most of the studies I outlined above are rather more micro or detailed in character. The basic method of macroanalysis today (and probably for some while in the future, too) has been to develop various empirical models. These models have included large-scale input-output matrices and reasonably complex econometric formulations. Actually, many of the factual voids listed above represent identification of areas in which these macro models provide insufficient detail to permit well-informed policy decisions.

This weakness or limitation of macroanalysis has, in turn, led many observers of the national political and economic scene to the view that the complexity of our problems is so great today that they verge on being unmanageable. Increased complexity and inadequacies of our factual bases have diverse origins, and it is foolish to think that there is a quick technological or research fix that will solve them. At the same time, a convergence of new technology in the guise of the modern computer and the advent of these macroeconomic models have made some contribution toward the resolution of these problems. The full potential of the models, though, has not been realized. Part of this inadequacy originates in the way models are developed and used in the policy process. In particular, a complex model is by its very nature only a partial and incomplete representation of reality. More attention needs to be paid to assessing model reliability. The failure to do so accounts for much of the widespread skepticism about the use of models—a skepticism for economic policymaking which is by no means totally unjustified.

Some work, to be sure, has been done on developing procedures to test the validity of large-scale economic models, and the National Bureau, in fact, is at the forefront of these efforts. While engineers can perform experiments, it is seldom possible to do so with economies, and hence economic models cannot be directly validated in this way. There are, however, alternative procedures analogous to experimental methods that our preliminary explorations have shown to have great promise.

Conclusion

This listing of future research possibilities, obviously fragmentary, is also merely a prologue since the actual decisions on future NBER directions must and will be a primary responsibility of my successor and the Bureau's Board. I am sure, too, that new research priorities will emerge with the passage of time and events. Sorting them out should be an interesting exercise for the Board and the new leadership.

With this report, I bring to a close almost ten years of association with the Bureau. I think these ten years have been marked by many achievements and advances in the Bureau's program. In simple quantitative terms, for example, the Bureau is four times as large today as when I assumed the presidency. This expansion, a quite conscious and deliberate decision, provided a means of accommodating at least a few of the many new research and policy interests emerging in our burgeoning profession.

Certainly, these last ten have not been years of complacency and maintenance of the status quo at the Bureau. Some may say that we have perhaps launched a few too many experiments during these years. Not all of our new endeavors have, of course, succeeded. I would argue, though, that that is inherent in a good research enterprise, as research is inherently a risky venture. The easiest way to avoid failures is to restrict the program to the tried and true. But, then, is it research?

Finally, I thank all of you for your tolerance and help during these years and wish you and my successor all the very best in the future.

MILTON FRIEDMAN NOBEL LAUREATE

The award of the 1976 Nobel Prize in Economics to Milton Friedman caps a distinguished career in which his long association with the National Bureau of Economic Research has been an important feature.

Milton Friedman first joined the National Bureau staff in 1937 to complete a study that Simon Kuznets—the 1971 Nobel laureate, who for many years was himself a member of the Bureau's research staff—had begun as part of his pioneer work in estimating national income. Their report was published in 1945 as *Income from Independent Professional Practice*. In that book, Friedman presented a theoretical model of the distribution of income based on the idea that differences in human capital investments in different occupations explain differences in incomes of doctors, dentists, and lawyers. In testing the theory with income data collected for the study, he distinguished between forces producing permanent income and those producing transitory income.

In *A Theory of the Consumption Function*, which the National Bureau issued in 1957, Friedman applied that distinction to the study of consumption. The theory he offered was that the fraction of their income people spent on consumption depended on permanent income, and that permanent consumption did not change because of transitory increases or decreases in income. He used the theory to show that the reason saving as a fraction of income was much higher at high than at low incomes in family budget data was that consumption and income as measured combine permanent and transitory components. On the other hand, time series data, which are closer approximations to permanent consumption and permanent income, showed that saving in the United States was a roughly constant fraction of national income, despite the rise in real income.

Friedman is perhaps best known for his monetary studies, which have been written mainly for the National Bureau. His 1959 Occasional Paper, *The Demand for Money: Some Theoretical and Empirical Results*, represented another application of the distinction between permanent and transitory income, this time to the demand for money. The theory is that the amount of money balances people choose to hold is determined by their permanent, not measured, income. Again, he used the theory to reconcile what seemed to be conflicting empirical findings. Monetary velocity—the ratio of measured income to money—rises during business expansions and falls during business contractions. However, monetary velocity tends to fall as income rises over the long run because income in this case is an approximation to permanent income.

Friedman's major monetary studies, written in collaboration with Anna J. Schwartz, are *A Monetary History of the United States, 1867–1960* (1963) and *Monetary Statistics of the United States* (1970). For the construction of the estimates of the U.S. money stock series that underlie both works, Friedman prepared a Technical Paper, *The Interpolation of Time Series by Related Series*, published in 1962. The historical study documented a consistent relationship between fluctuations in the money stock and national income in the United States and traced the duration and severity of the business contraction of 1929–1933 to the one-third decline in the money stock over that period. Friedman's 1971 Occasional Paper, *A Theoretical Framework for Monetary Analysis*, is a formal statement of the dynamics of the money-income relationship. Friedman and Schwartz are currently revising a manuscript on long-term trends in the United States and the United Kingdom over the past century that contains the empirical tests of the theory.

The Nobel award cited Friedman's work on the consumption function, on monetary theory and policy and on economic stabilization policy. His colleagues at the Bureau salute him for exemplifying the tradition of the National Bureau in pursuing empirical research to provide tested knowledge of important issues in economic science.