DISCUSSION

Includes comments by the chairman, Robert V. Roosa, of Brown Brothers Harriman; and by William J. Baumol, of Princeton University; Sidney Homer, of Salomon Brothers & Hutzler; and James J. O'Leary, of the United States Trust Company of New York, who were the discussants. Remarks made during the open discussion period are not included.

Introductory Remarks by Robert V. Roosa

I know that John Lintner's paper is going to occupy many of you long after these meetings are concluded. You will find that it is more than a remarkable survey of the staggering contribution in space, time, and pagination that the Bureau has contributed to the field of financial research over its lifetime. Not only has he read, digested, criticized, and summarized this vast volume of Bureau contributions, but he has gone on to make suggestions, to point out to the rest of us where he thinks additional Bureau research can go and the changes in dimension and focus that might now be possible on the basis of this massive body of accumulated information and analysis that the Bureau has already provided.

I think I should indicate at the beginning that not only has John Lintner provided splendid coverage of Bureau documentation in the broad area of financial research, he has introduced into his paper the great breadth of knowledge he has in the field as a whole. There are even one or two footnote references to his own contributions which I have always found scintillating and perceptive, and I know you will today in digesting this ambitious paper.

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William J. Baumol: It is a profound privilege to have the opportunity to pay tribute to the National Bureau of Economic Research on the occasion of its fiftieth anniversary and to acknowledge the profession's great debt to it for its contributions to our learning, most notably for leading us to face the quantitative facts, even when we might have been reluctant to do so.

Lintner has performed a substantial task, having undertaken to survey the enormous volume of writings in the field of finance that has

over the years been produced under the sponsorship of the Bureau. He has, with his usual thoroughness and insight, produced not only an overview but also a helpful evaluation and a discussion of programs for further research. The validity and comprehensiveness of his discussion suffer from only one disadvantage: They make it very difficult for a discussant who can find little to expand upon and nothing with which to disagree.

I believe Lintner is right in his evaluation of the National Bureau's work in the financial area and right also in the directions to which he points for further research. Though he calls for the use of a theoretical and analytical base, he is careful to avoid the tired discussion of "facts without theory." What makes his observations on this score timely is not primarily a matter of the Bureau's own orientation, but a change in the nature of the theoretical material that is available for use as a framework for empirical research.

For our discipline, the sixties was a decade which brought forth a number of important new ideas. Few of them, however, were contributions to pure theory. Rather, a preponderance of the major innovations were composed of applications of the theory to a number of fields which were formerly the exclusive province of those who provide descriptive and historical materials. For example, the field of public utility regulation, previously devoted heavily to the discussion of legal institutions and the course of their development, has suddenly been inundated by a variety of theoretical writings making heavy use of relatively sophisticated mathematical tools such as Kuhn-Tucker theory. Public finance is another applied field in which the role of formal theory has expanded significantly. In short, it would not be seriously misleading to characterize the ten years that have just passed as the decade of applied theory.

Probably no single field has felt the impact of this development more strongly than that of finance. The rash of literature on the term structure of interest rates does base itself on earlier work by Lutz and Hicks. But recent contributions by writers such as Meiselman and Malkiel have provided a basis and a wealth of hypothesis for systematic empirical research. Modigliani and Miller as well as Professor Lintner have given us a body of materials on the role of dividends and the valuation of stocks which has led to a protracted discussion of fundamental elements in the analysis of financial instruments, and has produced contentions that can only be settled empirically. The portfolio selection analysis contributed by Markowitz and Tobin is still another illustration of this sort of development.

All of this is to say that several decades ago, a call in the field of finance for the empiricists to base their work more heavily on theoretical analysis would not have been easy to heed. Today there seems to be no end to the work of this variety that still needs to be done. Now, therefore, such an admonition is much less an empty gesture because the program of work is there and hardly needs to be laid out.

However, if this is the direction we are now to propose for the work of the Bureau, it would seem that we come a little late. The fact is that many of the outstanding economists associated with the Bureau have already gone off in this direction, and are well along on the way. If one thumbs through the *50th Annual Report* of the National Bureau, one finds described in it a profusion of econometric studies based upon theoretical models, these in a variety of fields. The section devoted to finance is no exception. For example, one finds reported there Cagan's study of the monetary effects of interest rates, Kessel's study of the cyclical behavior of interest rates, Sargent's "Expectations at the Short End of the Yield Curve" and his study of the Gibson paradox,[1] all of them topics clearly having substantial theoretical implications. The fact is that the Bureau's work has already been turned toward theory both as a source for topics for research, and as an area to which its own work can contribute.

This is not meant to imply that the Bureau has abandoned the portions of its work which are devoted to descriptive studies and to data gathering. However, no one would wish for any slackening in the pace of these undertakings. This sort of effort has always produced valuable contributions, and one would hope that it will continue alongside the activities that are more analytical in their orientation.

Let me, in closing, mention one problem in the field of finance not emphasized by Lintner to which one might well ask for the devotion of further research effort. Despite the profusion of developments in the field, both empirical and analytical, the results have so far been fairly disappointing to those who are concerned with application, at least at the micro level. The reason is that so many of the conclusions that have been derived are essentially negative in character. The random walk literature

[1] Phillip Cagan, *The Channels of Monetary Effects on Interest Rates*, forthcoming; Reuben A. Kessel, *The Cyclical Behavior of the Term Structure of Interest Rates*, Occasional Paper 91, 1965; Thomas J. Sargent, "Expectations at the Short End of the Yield Curve: An Application of Macauley's Test," in Jack M. Guttentag, ed., *Essays on Interest Rates*, Vol. II, 1971; and Sargent, "A Study of the Gibson Paradox," *50th Annual Report of the National Bureau of Economic Research*, September 1970.

provides the outstanding illustration, for its implication is that, in the absence of inside information, the attempt to analyze securities and forecast their value may well be a waste of time. Similar in spirit in this respect is Little and Raynor's work indicating that past performance of a company in terms of the rate of growth of its earnings is virtually useless as a predictor of its prospective performance. A group working at Princeton, including Burton G. Malkiel, Richard Quandt, and Peggy Heim has found virtually no relationship between the resources a firm plows back into its capital stock and the rate of growth of its earnings.[2]

All of this has been extremely disquieting to those who have looked to the economists' writings on finance for guidance for their own activities. A bit of debunking is no doubt a good thing, but where does one go from there? All of this is to say that any results which are less negative in character will find a most receptive audience. The field, moreover, does not lack hypotheses, albeit most of them naive. Perhaps the random walk results and the other pieces of evidence mean that there exist in this area no positive results in search of a discoverer, but I think this has not yet been demonstrated. Perhaps someone among those whose work is sponsored by the National Bureau can help to show the way. Certainly, many interesting and challenging issues for research are to be found here.

Sidney Homer: There is only one real answer to the question "What should the National Bureau be doing in the future?" and that is, "Everything." I am sure that John Meyer would add, "If all our subscribers multiply their subscriptions by ten or twenty, we will." I have a little list here today, though, of a few things which would be especially useful from my selfish point of view, some of which are certainly already being worked on. I have in mind more listing of factual data, so that the reader can apply his own theories and perhaps disagree with the author.

Over the years perfectly magnificent work has been done by Raymond Goldsmith and others in flow-of-fund statistics, through the capital market, into and out of various forms of institutions, and all integrated together. At the bottom of the supply list, there is a residual called "miscellaneous and private investors," which in our analyses of

[2] This work almost suggests that companies might as well not plough back any of their funds for all the difference, overall, that it makes to their earnings! The rate of return on new debt seems to be somewhat higher, while the return on new equity turns out, generally, to be significantly more substantial. For the results of this study see William J. Baumol et al., "Earnings Retention, New Capital and the Growth of the Firm," *Review of Economics and Statistics*, November 1970, pp. 345–55.

credit flows we could have pretty well ignored in most of the postwar years. We are now in a period, as you all know, in which "miscellaneous and unaccounted for" are accounting for something like 60 per cent of the capital flows in the bond market. I have over the last few years been needling anyone who is available to be needled—the FED, the SEC, and the National Bureau—to look into this massive supply-of-credit field, which last year bought over $25 billion of bonds net, and to break it down into component parts that have some meaning, so that the flows can be traced insofar as possible from savings institutions, stocks, and so on into bonds; so that we may know something about this group—as much as we know about savings banks, for example, and life insurance companies.

Second would be further analysis of the government sector. Not enough has been done on the role of government in our capital markets. The proportion attributed to government in the GNP analysis I suspect strongly underestimates the role of government in our economy. More and more in our capital markets, the device of guaranteeing private obligations is becoming massive, and with tremendous implications for the future. I would suggest this be studied on a broader base than the GNP base, particularly as regards its effects on the capital market, its effects on the economy, and its effects on the distribution of resources.

Next, I would suggest a very lively topic which will not come as a surprise to most of you: the topic of the role of social priorities in the capital markets, the question of how resources are directed by pure open-market rationing as distinguished from national objectives and national priorities. It seems to me that this is not new but of greatly expanding importance, and I haven't seen it really covered in a complete way.

We have another topic that has become very lively this year, the question of liquidity: the quality of institutional portfolios, the liquidity of institutions, and the liquidity of industrial firms. We know this is not purely a statistical matter; so far as I know there is no set of ratios that answers the question, because liquidity is at least 50 per cent psychological.

But the whole liquidity question and the credit question and the credit quality question are vital. I do not know how the econometricians work it into their models. One week the lending policies of American institutions are among the most liberal in years; a week later, they are among the most conservative in years. Something has happened—I'm

referring now to the Penn Central, but this is only a symbol—there are plenty of other situations that have worried lending officers. If their credit policies change, money flows change, and there is a vital effect on the economy. No doubt in a broad sense this has been studied, but I would think that in a specific and practical sense it would be a big field for research work now.

That is far from the end of my list, but those are the high spots. I will stop here and make two general observations. A great many important statistical studies have been done, by the National Bureau and by others, that are extremely useful. It is regrettable that in many cases when they come out the latest statistics in them are four years old. I know this is a mechanical problem that is extremely difficult to solve, but the pertinence of the document is greatly impaired. Along the same lines, and again from my practical standpoint, I would like to see a great deal of the statistical work brought up to date. I know it is the policy of the Bureau to do the basic work in the hope that some government agency will keep it up to date. In the case of flow of funds this has been done, but there are other instances where this has not been done. I would personally find very useful some periodic publication that brings past statistical studies up to date, at least that type of study that can be brought up to date in a ready manner, so that the study becomes fresh all of a sudden, and useful.

James O'Leary: When I read John Lintner's paper I had the same feeling that Bill Baumol had. Lintner has done a monumental job. The books he has reviewed would occupy about twenty feet of shelving in any library. In effect, what he has done is a one-man exploratory committee job on what's going on in the financial research field and what needs to be done. I think the Bureau, which in the past has had exploratory committees, owes a big debt of gratitude to John Lintner for the job he has done.

As I read through that paper, I was a little taken aback because I hadn't realized quite what the nature of the paper would be. I expected this to be a paper oriented toward monetary issues. As I got into it, I kept meeting old friends all along the way, and it was a very heartwarming experience for me. I had the great fortune right after World War II when teaching at Duke to be offered the job of director of investment research for the Life Insurance Association of America. One part of my job was to help the life insurance business decide on financial research that it felt it could support.

In the period from 1947 through 1967, while I was involved in this, we advanced something like two million dollars in research funds to the National Bureau. I suppose today, in terms of what that will buy in research, it would be equivalent to maybe four or five million dollars in research funds. We got involved in this actually before I was on the scene in the very early projects that are described in John's paper where the original exploratory committee of the Bureau in 1937 came up with some projects. We were participants, for example, in the corporate bond project that Brad Hickman carried out. As you look through this list we did the financing of Ray Goldsmith's study of savings. It was one of the most exciting two years of my life to be closely associated with this, not actually in a research role but very closely associated with it. And that wasn't done, incidentally, as a National Bureau project. It was done under R. W. Goldsmith Associates, before Goldsmith became associated with the Bureau. The interesting thing there was that we clothed him with an advisory committee so that he'd be completely independent. That advisory committee had some notable names on it. It had, among the economists, Win Riefler as chairman. It also had on it Jack Viner of Princeton, Sumner Slichter of Harvard, Arthur F. Burns, Simon Kuznets, and Ted Yntema. In addition we financed another project called the Study of the Postwar Capital Markets, exploring the changes in the capital market. There were a number of participants in this. This is the project in which Saul Klaman did his great job on the residential mortgage market; and Roland Robinson, a great job on state and local government financing. Then we financed Kuznets's study of capital requirements. Incidentally, in many of these cases, the idea of the projects actually originated with the life insurance people. We financed a study of interest rates which the Bureau has done. Phil Cagan had a very important part in that. As I mentioned, we financed Hickman's corporate bond project and many others. We put money into the pension fund study conducted by Roger Murray. So I take a great deal of pride when I hear John talk about what has come out of this research; I agree with him. I think it has been a very, very important contribution, and a great deal is owed some of the men who helped obtain the financing of these projects—in particular, John Sinclair, who later headed the National Industrial Conference Board; F. W. Ecker, who later became the chief executive of Metropolitan Life; Don Slichter, president of Northwestern Mutual; George Conklin, president of the Guardian Life Company; and Robert B. Patrick, senior vice president of Bankers Life of Iowa.

I think we have to do more to get the commercial banks and other financial institutions interested in this sort of approach. As I see it, the amount of money that has to go into financial research, if it is to be effective with the Bureau and other places, is a large multiple of what's been done in the past.

Now let me just wind up by saying, I couldn't agree more with John Lintner's suggestions as to the direction that research should take. I think it is true that no revolutionary change is needed. The kind of research that the Bureau has proved itself so well in is what it should continue to do. I agree there should be some change in strategy in the direction of more analytical work rather than purely a collection of figures. But it will be awfully hard for the Bureau to do this because the financial system is changing so quickly that simply the gathering of data is going to continue to be a major part of this whole picture. I think the sort of issues and problems John has raised are right, and all I would like to do is to add one or two that occur to me. There is one big one, and then there are others that are more obvious. The big one is this: Since the latter part of 1965 there has been an escalation of inflation, and along with it there has been the sharp rise in interest rates, with interest rates now at record high levels. The likelihood is that interest rates, even though they will fluctuate cyclically, are going to continue to remain at high levels. What I would like to see the Bureau do is to study the question of how our financial system is affected by this sort of condition; how it is affected by Federal Reserve policies now as compared with a condition in which we had a lower level of interest rates and more in the way of price stability. I think that there are profound effects on the financial institutions if you analyze them in these terms. For example, how is this sort of condition affecting the cash flow of the financial institutions? How is it affecting their willingness and their ability to make forward investment commitments to buy bonds and mortgages? How is it affecting their preference for equities or equity kickers? How does monetary policy work differently today under these sets of conditions than would have been the case, say, even three or four years ago, or certainly ten years ago? If we should go through another cycle such as the one we had in 1966, or in 1969–70, I wonder whether some of our financial institutions could survive. I think that some are more threatened than others. Can our financial system as we have known it in the past continue to function successfully with a 4 per cent rate of inflation, which is what many people are assuming?

To take a few other areas, perhaps the Bureau could do more in the way of putting some meat and bones on the whole question of how expectation of inflation affects investment decisions. This is a tremendously important development. I don't see quite how the Bureau can get at it in a quantitative way, but, I think, through close contact with decision-makers in the financial markets, perhaps an awful lot can be done to help get a better understanding of the effects of the expectation of inflation on the bond and stock markets. Our financial system is changing rapidly. As you look at the catalogue of what the Bureau has done, you can see how far out of date it is in terms of what's going on today. For example, there is very little that the Bureau has done in the area of at least updating information on mortgage loans on income-type property. Very little in the way of data has been collected on the whole use of equity kickers on loans. I would say, finally, I think there is a great opportunity now to make another advance in the flow-of-funds area to get gross figures. The flow-of-funds data are still just net changes in outstandings, whereas, for example, in the life insurance business, there is now a rich body of data on the gross cash flow for investment. I think this sort of data probably is available in some of the other financial institutions as well.

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Closing Remarks by Roosa

It does seem to me that we have assembled a list here which is largely action oriented in terms of the implications of the array of suggestions for future research. I think this is in keeping with the approach that John Lintner has taken in his paper, indicating that broadly speaking so much has been done in describing the what of the financial system that we need now more on the interrelations and the why. All the same, I think we must agree with Jim O'Leary and Sidney Homer that much of the work that has already been done by the Bureau that stands as classic is still in need of updating—of continuous testing against new data. There does have to be a compromise between these two kinds of objectives, and I am sure will always continue to be. I'd like to sum up what I think are several of the action-oriented suggestions that have been made thus far, and then add a couple.

It does seem to me that John has indicated the need for getting inside the boxes of the whole flow-of-funds analysis, for updating and digging further into the Goldsmith and the Kuznets work, not merely in

terms of the data, vast as that is, but in terms of the causation. In addition, we have had some variants on that fundamental approach in Bill Baumol's recognition that the Bureau in its effort to move in new directions has had, as I think he put it, an increasing infusion of mathematical inspiration. I may not have accurately paraphrased him, but the implications there, in terms of the techniques of data manipulation and in developing the power of theoretical analysis, are clear enough. But he took this in another direction as well. He was a little mystified, if I am not unduly generalizing from his more precise statement, as to how the capital allocation mechanism really works and how it can be considered rational as it relates to the application of retained earnings in the total process of capital formation in the economy. He can't also quite understand how the returns that he finds on new equity and on the commitment of new investment funds in the established large and growing corporations explain the claim in relative terms that they are exerting on the total of our supply of available savings—the contribution to the capital stock that they do in fact carry through. I may be sharpening too invidiously what he said much more delicately, and I may have misunderstood. But I would suggest that this is one set of implications that probably does deserve additional thought and consideration.

Before we get too far with those, we have to come to one of the several suggestions that Sidney Homer made, because we are entering what probably is another mutation phase in the array of criteria that society is going to accept or impose on itself. Implications of this for the processes of capital allocation have yet to be determined. The point Sidney made was that we haven't yet figured it out, although some of us find that we are harassed or buffeted or moved or pressed every day or so by the criticism we are receiving that social priorities are not adequately represented in the processes of capital allocation. We have to find a clearer way of expressing what they are and how they can be quantified, how they can be represented in the investment process. I think to be more specific, Sidney also reminded us that the miscellaneous categories in some of our financial data have now become so large that we lack an explanation of the principal sources of investment in fixed-interest obligations which we can't penetrate further. Of course, we aren't going to get the answer satisfactorily there.

In addition, if we are going to find not only the who but the why, we must go on into some of the other questions that he and Jim O'Leary raised. The question, of course, that is going to have a pervasive significance, as long as any of us remember those two words Penn Central, is the

significance of liquidity not merely for the financial institutions but for the industrial firm. What does it mean and how are we usefully going to establish standards, both of evaluation and performance, that will fit with the criteria we want to have for rational capital allocation in the system? All of this can become almost revolutionarily terrifying if we let ourselves think too far. The implications in pure research are, I think, themselves sufficiently challenging. As Jim O'Leary brought us closer to the present day in another respect, he stressed that none of us really have satisfactorily thought through yet, as far as I know, even the purely abstract systematic implications of our kind of economy, operating in an environment in which it must be assumed that there will be a continuing pattern of inflation—I think he said at the 4 per cent level. We haven't really thought through what that kind of outside given element will do to so much of the pattern of interrelations that we would otherwise be measuring from past performance. Just within the past two years we've had a mutation or followed a path that won't be completely reversible, even if we do get lower rates of inflation in the future.

The significance of having had this bath of inflation—whether or not it continues—is certainly an area we want to be certain is explored and reflected in the many more detailed and specific projects that the Bureau will be carrying forward. As he said in concluding, and this is implied in many of the other things I've also mentioned, there is the more specific translation of this inflation mentality into expectations as they affect investment decisions, that is, the investment decisions of the firm as well as the commitment of the lender or the source of funds.

In all this we may have passed through a period that is so rich in new experience that we can well justify spending a major part of any research effort such as that of the Bureau in trying not to look at the longer time series that will, perhaps, over the decades ahead provide the more lasting evidences of the inner patterns of the economy. We may find that we are going to need and want to devote more attention to the study of the special phenomena that came with this, let's hope, short-lived experience of rapid inflation in this kind of economy. It is quite understandable and, I think, all of us have shared a bit of it, that during an inflation of this kind there should have been a revival of that search for the Holy Grail or the simple formula that always characterizes mankind in the face of crisis or new challenge. This time, we called back to Chicago and came out with the revival of the monetarist approach, which is probably likely to shift back into a position of more balanced perspective than might have been implied in some of the things that were

receiving widespread attention a year or so ago. Nonetheless, I think, what we are going to want to do is to study it in every way we can. This will involve the selective study of cases, as well as the analysis of data, and the extent to which, in this recent period when inflation has been the dominant characteristic of our economic performance, the monetary phenomenon as such has attained a special new significance and a new force, a force certainly greater than would have been attributed to it, as John Lintner points out in his paper, in any of the earlier model building. In this respect, he does go back as early as the first Klein models twenty years ago, indicating (in keeping with our Keynesian biases of those days) that very little recognition was then given to money as an independent influence.

Reaching outside this need for bringing up to date in the present context what deserves to survive of the monetarist view, there is another area of financial analysis which no one has mentioned today. It is a part of the Bureau's effort nonetheless, and since I am generally considered to be rather fanatic on this subject, I wouldn't dare turn the meeting over to you without mentioning it: We have had new developments in the balance-of-payments relations of this country and in the rest of the world as well.

Another new phenomenon to which the Bureau has already given some attention (as have members of the Bureau staff on their own) is the phenomenon of the Eurodollar market, both short and long. The relations between the supply of and demand for funds in that market inject another dimension of altogether different scale in magnitude and in implications for the conduct of our financial markets, an altogether different dimension from anything that existed during the period when most of the data to which John refers in his paper were being put together. I think we have here another important area, and not merely for debunking the significance of what I suppose this year will be an official settlements deficit of eight or nine billion dollars that will occur largely because a completely extraterritorial volume of forty billion odd dollars can move back and forth between private holders abroad and central banks abroad, and such movement happens to be defined as a deficit in the U.S. balance of payments. These are new phenomena, but these patterns of movement have their ricochets, with implications reaching back into the money market, and to some extent into the bond market, sometimes as a safety valve and sometimes as a source of new fear and concern, which I'm sure somewhere in this scale of projects deserves some mention.

I will close with one more comment. I have been a little struck, and I am not sure whether this is too naive to mention, but I'm a little struck with the alacrity with which the present Administration has adopted an approach that some of us like to be identified with from an earlier Administration; that is, the concept of the economy's potential as a guide both to fiscal policy and to the potential risks of inflation. My own thought, when we were giving a lot of attention in 1960–62 to this same concept (beginning from a low level of employment), was that if this potential were to be usable as the base on which to fill out additional spending—either through a government deficit or an easier monetary policy—it required a little more than just simply projecting a 4 per cent line across a piece of graph paper. There was also going to have to be an underlying capital base—a capital base which would be expanding, improving, or enlarging, or raising the productive quotient of the total mix of men and labor in the economy. Conversely, if we were to assume stagnation in the capital formation process, we wouldn't be able to draw that handsome line across the graph paper. What I suggest is, at least as I saw it in those days, we couldn't really draw that line unless we had also assured ourselves of a fairly sustained pattern of capital formation in the direction of intensifying the capital structure.

That's why, at least in my view, we began so aggressively and succeeded only after two years in introducing the investment credit as a part of this procedure. Whether or not you like that technique, it is a good one to study—for its effects as well. Every time people in my firm or other's write a story about any firm they are considering investing in these days, three or four lines have to be inserted about how, without the investment credit, this had happened or that had happened. People were inclined to say, and people in high places said a year or so ago, that the investment credit was a mere fiction, but the analysis of the individual cases now is certainly showing that, at least in those that I see, and there are quite a few, the elimination of the investment credit is doing something to impair the capital market. It is possible that here in the Bureau's action-oriented part of the program there may be something worth exploring; that is, whether we can safely assume that this projection of the full employment potential for the economy can exist regardless of what is going on or permitted or encouraged insofar as capital formation is concerned.