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DISCUSSION—MORNING SESSION

Includes comments by Paul Samuelson, of the Massachusetts Institute of Technology, who was chairman of this session; Otto Eckstein, of Harvard University, and Henry Wallich, of Yale University, who acted as program discussants; and Bert Hickman, of Stanford University, who offered additional observations; also, comments by Moore on the papers by Fabricant and Mintz and a reply by Mintz. The recorded oral presentations were edited by, or with the cooperation of, the speakers. Remarks made during the open discussion period are not included.

Introductory Remarks by Paul Samuelson

Now that the National Bureau is fifty years old, it has worked itself out of one of its first jobs, namely, the business cycle. I don't know when the American Cancer Society was founded, but by similar reasoning fifty years after that date some optimist could hope to cross cancer off his list. The Bureau was thus in danger of becoming just a museum of fossils; but nobody likes to work himself out of a job, so you naturally redefine the field of study. I predicted some time ago that this would happen, and Ilse Mintz this morning has confirmed my prediction.

I am not sure whether it is a healthy sign in a science when you have experts quoting what some expert said back in 1946, and some other expert said even earlier—however appropriate reminiscing may be for a fiftieth birthday party. The discussion then becomes very academic—almost that of the medieval schoolmen, almost talmudic. Substantively, I would like to make three brief observations. The first relates to the story told by our President Meyer about his being told on a visit to Japan that during what they call a recession real output does not decline but rather grows only by 5 and 12 per cent. I suggest that this is not a joke, but deserves really very serious consideration. Second, I'd like to add a similar observation: If our president would go to Latin America, he might hear some Brazilian or Chilean economists speak about a recession in the sense that the rate of growth of the price level has gone from 70 per cent per year down to only 30 per cent per year. That, too, suggests the importance of both the study of price growth cycles, Geoffrey Moore's subject, and the use of deflators. And finally, for the non-academic members of this group, I might mention that there is a third facet of life that some people consider important. I have in mind a recent interview with Sidney Homer, of Salomon Brothers and Hutzler. He said that this may seem like a mild recession to academics, but from

the standpoint of Wall Street and the capital markets it has been quite a serious situation. I invite you to recall 1953–54 from this viewpoint. Everybody agrees it represented both a growth cycle and an old-fashioned recession. But from the standpoint of the financial market it probably was hardly noticed at all, whereas the present experience which Homer made reference to has been very important in the financial area, as was the nonrecession of 1961–62.

* * *

Otto Eckstein: I am very pleased to be able to participate in this Fiftieth Anniversary celebration of the Bureau. My own ties to the Bureau are fairly tenuous; I am only a director. You know what that means!

I learned my National Bureau methodology not at the feet of Wesley Mitchell or Arthur Burns, but from Gottfried Haberler, when he was president of the Bureau. So if I say anything unsound you will have to blame it on Gottfried.

The fact that you asked outsiders to comment was an open invitation to be a bit broad-minded about the subject, to be a bit of a gadfly, and I will oblige. Let me start with the final question. Are we in a recession, or are we not? We really don't have trouble with the facts; we only have trouble with fitting them into the National Bureau definition. All through the postwar period, we have had genuine business cycles with all the proper absolute declines, and the slowdowns of '52 and '62. There is no ambiguity about these cases, and the growth cycle definition picks them up very easily. I don't think there are any close decisions. The current episode is on the margin of recession, and the growth recession clearly covers the case. So it appears to be useful.

How do we move from here? What do we do with the National Bureau cycle methodology now that we move to the growth cycle concept? First, we have to draw a clear distinction between measuring the economic process and measuring economic performance. Performance focuses on advancement of economic goals, full employment, growth, price stability, etc. The Bureau, on the other hand, measures process, which is the basic building block of the theory of Wesley Mitchell. It is the theory in Arthur Burns's more modern statement. It is what the leading indicators try to measure—some things happen before others. It is usually intuitively obvious what precedes what, and the National Bureau has made an enormous contribution in trying to develop this sequence of processes. This has really been the substructure of the sub-

sequent definitions of cycles. On the other hand, when people are interested whether there is a recession or not, the process definition can get in the way. One of the things that is odd about it, compared to the man-in-the street's notion, is that recession is defined to be the period of contraction; it begins at the peak, when nobody feels in a recession. It ends at the lower turning point, and yet three months after that turning point people are out of a job, profits are down; it still feels like recession. The reason is that the public thinks about performance, and the definition is in terms of process. What is it we wish to measure? Do we wish to continue to focus only on process, or do we also wish to measure performance? Your *Annual Report* shows various projects on measuring social indicators, and there the same problem appears. Are we going to measure process, or are we going to measure performance? Most people assume that there will be performance measures—performance in the sense of how well we are achieving our goal.

Let me make a few specific points on the papers. The paper by Ilse Mintz moves the subject forward in a very significant substantive way, and I think it contains in it the foundation for where one should go. Let me put out a few warning signs early in the game before it becomes official Bureau dogma. First, the seventeen indicators in her paper are still a hodgepodge of different things. There are real magnitudes, such as production, employment, and unemployment. There are money magnitudes, such as personal income and profits, among others. And there are interest rates. We are having an inflationary recession in which the price level has not slowed down as quickly as real activity, so a blend of money and real indicators becomes confused and measures nothing.

Sol Fabricant has faced up to that question very clearly and has come to a conclusion. I have come to the same conclusion. We don't want a single composite indicator. What, then, would we want? First, we should switch to the growth cycle idea because, given the values of society, the probability of a traditional recession with unemployment of 7 per cent is fairly low; and in every meaningful episode you do have a growth cycle.

Second, we don't want to lose the idea of process. However, once we accept it, we have to go back to the distinction of leading, coincident, and lagging indicators—but focused around growth rather than just the cycle.

Third, I would give up the idea of an all-in-one leading indicator (God forbid, weighted equally). At the very least—and here Julie Shiskin

and BCD [*Business Conditions Digest*] have led the way—we do need subindexes, and we need to emphasize that it is in the subindexes in which the truth resides, not in a composite. We definitely need a growth index which is a real measure based on production, payroll data, perhaps unemployment data, perhaps income data as well, but the income had better be deflated. We need another set of indexes—which Geoff Moore has started—on prices and costs; here, particularly, the process is important, and we hope that the BLS will move ahead and produce leading, coincident, and lagging indexes of prices and costs.

Fourth, we probably need some kind of a subindex on the financial sector, though I have not thought through its conceptual foundations.

Finally, I would hope that the Bureau would apply its expertise more explicitly to develop the social indicators. I would hope that the social indicator project will benefit from the long history of the Bureau on the traditional economic indicators, and that you will attempt to develop the same kind of work carefully. This might include such factors as income distribution, minority unemployment, etc. I am tempted to urge leading, coincident, and lagging social indicators. How does social progress happen? What are the early signs of social disintegration? There is a need for indicators of process and of performance.

Henry Wallich: The principal topic to which I would like to address myself is the shift in definitions, from an absolute cycle to a growth cycle.

As a general proposition it seems to me that when ancient definitions cease to reflect current reality it is a good thing to shift gears and get it over as quickly as possible. There is so much inertia in every system that whatever one can do to move things ahead will probably be to the good. I am also impressed by the usefulness of a change in definitions in terms of the self-preservation of the cyclical analyst. As Paul Samuelson pointed out—when by traditional measures the cycle seems to disappear, we have to redefine it so as to be able to earn our daily bread.

The change that is being proposed here is a particularly brilliant one because, while updating definitions, it also recaptures the cycle. As you will remember, the cycle has twice deserted the analysts. It deserted them first when it ceased to be periodic. Periodicity, after all, is implicit in the term "cycle"; but that aspect somehow got lost in the course of time. It deserted the analysts a second time when it refused to turn down in an absolute sense. Now, by shifting to a growth cycle, all is recov-

ered. Not only are there downturns. There is also again periodicity, because we have a rather plain pattern of, I think, four growth recessions during each of the postwar decades; the soaring sixties and the fumbling fifties suddenly look alike in the number of recessions, and they look alike in periodicity.

This much said in favor of a change in definition, I nevertheless become a little ill at ease when I observe the policy implications. Here I must apologize to the two speakers. I realize that they do not draw policy implications from their scholarly findings. Unfortunately, one cannot put fine print into headlines, and people sometimes do not read fine print. If they see a headline "National Bureau Declares Recession" it does not help a great deal to have it said somewhere in the paper that this really is no call for an easier monetary policy or a budget deficit.

It is my fear that, in going to a growth cycle definition for income and for prices, we are doing a bad thing in terms of policy making. In the area where we have done relatively well, the area of income stabilization, we are now raising our standards. In the area where we have done relatively badly, in the area of price stability, we seem to be lowering our standards. With apologies to Geoff Moore, one hardly can blame a reader of his excellent paper for failing to note the difference between reducing the rate of inflation and achieving stability. Thus we establish a lower performance test on inflation and a tougher performance test on income. Hence, as Mrs. Mintz points out at the beginning of her paper, there is a clear danger of inviting more inflationary policies.

My final caution on the definition of the cycle is this: Any definition of an economic pattern ought to allow for the possibility of its nonexistence. If we define the cycle in such a way that it must necessarily exist, because fluctuations in the rate of growth are inevitable, we have lost some of the interest inherent in the analysis of business cycles. These are the misgivings I have about the principle of the redefinition.

As for the timing of the proposed redefinition, clearly this must be done in the light of experience. Mrs. Mintz cites some of the elder statesmen of the National Bureau to the effect that we have to keep observing how the definitions stand up in the light of developments. On the other hand, there is a danger of tailoring new definitions too closely to a single event—acting too much under the impression, for instance, of the disappearance of the traditional cycle during the 1960's. We may be in danger of doing that now.

The conclusion that I would draw, as far as analysis and policy action are concerned, is this: The new definition, if it is to be accepted,

ought to be "defused." It should be defused in the sense that any current policy implications should be removed as clearly as possible. That means, for instance, to maintain the old definition along with the new. It might mean to differentiate among different degrees of severity of the new cycle. We could say, for instance, that a given move was a very slight one, or a more severe one, or that a growth cycle had changed into the old traditional cycle. It would be very unfortunate if publication of a monthly GNP index, as Geoff Moore suggests, were to give rise to demands for expansionary policies each time the rate of growth turned down, which could happen several times a year.

On the question of whether we have been in a recession, I agree with President Meyer that this is not the subject of today's session, and it is, in any event, an irrelevant question. By the narrowest of margins, if one takes a couple of billion very seriously, one could say that there were two successive quarters of downturn. I was impressed by the complete absence from today's discussion of this popular conception of National Bureau theology, namely, two successive quarters of downturn. But that is the popular conception, and seventeen indicators will not eradicate it. By that popular test, one can argue there was or that there was not a recession. I would not put enough weight on the fourth quarter of 1969 to say that there was. What I would stress is that, by this test, an expansion started, in the second quarter, because the trough of the movement evidently was in the first. The upturn, therefore, is substantially behind us. If the automobile strike causes a drop in the fourth quarter somebody will undoubtedly say that this was the year in which we managed to have two recessions.

In conclusion, we have on one side the "new cycle," which seems to argue for more hectic policy activity, while on the other side there is the New New Economics, which argues against fine tuning and instead for very stable policies, both monetary and fiscal. I would urge that we combine these two novelties and do the best we can flexibly but not hectically.

* * *

Bert Hickman: I am in the enviable position of having the opportunity without having had the responsibility—I think I really ought to take advantage of that. As I've been sitting here I've been thinking of some historical and doctrinal comments I want to make. I hope they're not particularly dull, and don't take us too far back into history. There are

some names I would have thought might have come up in this discussion but have not. These names, long associated with the National Bureau, include Kuznets and Abramovitz. And the reason I think of them is that they talked about something called growth cycles too—the long swings in the rate of economic growth. This concept was originally coined by Kuznets, and also by Arthur Burns, who worked on it in the 1930's. In more recent years Moses Abramovitz has spent a great deal of time and effort investigating, dating, and analyzing long swings in the rate of growth. There may be some awkwardness developed if we start talking about growth cycles in this new sense that Mrs. Mintz has proposed, because that terminology is also sometimes used in connection with the long swings in the rate of growth—sometimes called growth cycles, also sometimes called Kuznets cycles.

Another name which I thought might have come up is that of Ruth Mack, because Ruth Mack for many years was interested in something she called the subcycle.[1] This included the concept of a possible cycle consisting not of an absolute decline but a retardation in the rate of growth. Of course, for Ruth Mack this was primarily connected with the question of inventory phenomena; and the endogenous component, if any, of these subcycles, was thought to have something to do with the inventory investment process.

So, I would think, it might be very interesting to try to review some of that earlier work in the context of this present chronology. Given the fact that you have the long swings or long growth cycles, it might be useful to think in terms of a parallel to the major and minor business cycle concept, which was also popular for many years. The distinction between major contractions and minor contractions was primarily associated with Alvin Hansen's and R. A. Gordon's work on business cycles. We might have major and minor growth cycles now—that's a possibility, I suppose.

All this suggests a more basic point I want to stress: I would hope that to the extent that this new method of looking at changes in aggregate economic activity does become rooted in study, there will be an effort made to distinguish among different kinds of so-called growth cycles, instead of treating it as all one phenomenon, because I don't really think it is all one phenomenon. I think that's a very important point. The distinction between major and minor business cycles is con-

[1] As presented at the colloquium, the paper by Mrs. Mintz did not include the discussion of the subcycle concept which appears in the final version included in this volume. [Ed.]

cerned not only with amplitude characteristics, but also with the notion that there were different causal factors at work. Similarly, Ruth Mack's subcycle distinction was getting at the notion that there were different causal factors at work. So one would hope that this new chronology will not be used in such a way that everything becomes averaged into one set of turning point comparisons, one set of reference cycle and specific cycle patterns, and so forth, instead of taking the opportunity of perhaps thinking of some of these growth retardations (I would prefer that term to growth recession) as being due to different causes and having different implications than others.

I was struck by some of these differences in looking at some of the charts that have been presented, and one in particular strikes me as important and I would like to mention that specifically. (Oh, by the way, one more thing on terminology. One way around this terminological problem is to use Schumpeter's device. You remember he made the distinction between Kondratieff, Juglar, and Kitchin cycles. And later we had the Kuznets cycle, or major growth cycle. We could think of a Mintz or a Mack cycle, or perhaps Mack-Mintz might turn out to be the proper term; it depends on what correlation there is between Ruth Mack's work and Ilse Mintz's work, and I just don't know that at this point, but I suspect there may well be a high correlation in their chronologies.) The thing that strikes me is a chart that appeared in Sol Fabricant's paper. It's a chart on the relation of GNP in 1958 dollars to potential GNP. There's a corresponding chart on the inverted unemployment rate and the GNP gap as a percentage of potential GNP. These are panels B1 and B2 of Chart 1 in the paper. And the thing that strikes me is this, that the most recent growth cycle distinguished by Ilse Mintz before the present one was in 1962–64. And that looks to me very different from this present one in one particular respect. The retardation of growth in 1962–64 carried only to the potential output line if you regard that as the one drawn on this particular chart, and correspondingly, the unemployment rate stayed at slightly less than 4 per cent and was stable throughout that period. If you look at the recent period, according to this chart we've been carried a considerable distance below potential output and the unemployment rate has risen considerably above 4 per cent—in fact it's recently reached 5 per cent. Now you might want to call both of these things growth retardations or growth recessions, but I think it makes a lot of difference to have a deceleration that does not carry unemployment above that which is regarded as the full employment level and one which does. In somewhat the same vein,

there's the very old story that we might remind ourselves of—if you go back to the period between 1960 and 1965, there are certainly periods there of an upswing in the growth cycle, an increase in the rate of increase of GNP, but it's still not carried up to full employment. What I wish to do here is simply to reinforce those remarks that have already been made about the need to be careful in drawing policy implications from this kind of chronology. One has to look at what's happening in these different episodes to the performance of unemployment, to actual GNP in relation to potential, to rates of price increases, and so forth, so as to try to distinguish among these episodes to understand why some of them are mild and some severe.

* * *

Paul Samuelson: In connection with the last point made, I was a little disappointed, when Otto spoke about performance, that he immediately began to talk about social indicators, because that sounds as if it's a shift from the hard economics to a soft noneconomics. And he may have intended only that, although he did indicate inequality and so forth. But he didn't use the word "gap." Now the whole Keynesian thrust for two decades, which rather pointed away from an interest in National Bureau matters, was: *It's the level of unemployment and how well we do with respect to our potential that is the most important thing.* I remember as a student that I couldn't care less whether 1924 or 1927 were regarded as bona fide National Bureau cycles. There were some disputes among the scholastics of that day. Well, similarly, a decade from now nobody will be interested *really* in similar nice questions. Going back to how you create work for yourself, if we think of a man who is an expert in dinosaurs—let's say in a university or in a museum—and he runs out of dinosaurs, well, he can, of course, redefine a lizard to be a dinosaur and keep up his budget. But you can't really interest people in lizards—because they're really *not* dinosaurs, and they're not as interesting. The reason that we used to be so desperately interested in the Wesley Mitchell process was because in the bad old days that conventional business cycle process was unmistakably associated with very bad performance. And so a humanitarian like Wesley Mitchell could take the oath of pure science knowing that the things he did in pure science would be extremely interesting. However, what really has happened, it seems to me, is not that the cycle has disappeared but that its spectrum has shifted; namely, it used to be a cycle that went from high unemployment to low

unemployment at reasonably stable prices, and now it's a cycle that goes through a somewhat similar unemployment range but with prices moving cyclically around an average that represents creeping inflation. The amplitude of the movement, if you can define something that consists of different dimensions, might not be all that different. Also, in respect to Japanese and German cycles, I understand that the amplitude of their growth cycles in terms of GNP behavior was approximately the same as that of the American cycle only over a different spectrum of average real growth rates.

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Geoffrey H. Moore: Both Fabricant and Mintz agree, and I agree with them, that the recent economic slowdown has been an exceedingly mild affair compared with the business cycle contractions experienced in the past. Gross national product in constant dollars declined from the third quarter of 1969 to the first quarter of 1970 by about 1 per cent, which is much smaller than the declines in two of the mildest recessions in the National Bureau's hundred-year chronology, those of 1926–27 and 1960–61. Furthermore, one must always remember that GNP figures are revised, and that in each of the past four recessions, including 1960–61, the revised figures showed a smaller drop than the preliminary. Since the current figures have not yet been revised, the chances are that when they have been, the current slowdown will look even milder than it does now. Other measures of aggregate activity, however, confirm the finding that the recent dip has been exceptionally small.

What is more controversial in the two papers are the attempts at a new definition. Fabricant seems ready to adopt a definition that has not been thoroughly tested. Mintz tests rather thoroughly two types of definition, neither of which Fabricant accepts. In my paper, I use still another method of developing a cyclical chronology, which seems to me particularly appropriate in studying prices. So we have a variety of approaches, but no consensus. In view of this, I believe that Fabricant's conclusion, namely, that a new and untested method be adopted now, and used to characterize the current situation, is premature.

Fabricant does not tell us how the method he proposes, which depends upon trend-fitting, would have worked in the past—how reasonable or unreasonable its specific results would be. He concentrates most of his attention on how it would have worked in 1969–70.

Methods of cyclical analysis that depend on trend-fitting have been notoriously weak, because the cycles defined by a fitted trend depend significantly on the type of trend that is fitted, what period is used in fitting it, how it is extrapolated, and how deviations from it are measured and analyzed.

For example, the growth trend used in measuring potential GNP has been raised in steps from 3.5 per cent in 1952–62, to 3.75 per cent in 1962–65, to 4.0 per cent in 1965–69 and to 4.3 per cent in 1969–70. If, instead, the 3.75 percent rate had been used from the start and extrapolated through 1970, GNP would have been below "potential" ever since 1955. Cycles defined in terms of deviations from a trend line are highly sensitive to the particular trend that is used.

Fabricant suggests, however, that trends be used in a different manner. A recession would be defined as a "sustained and widely diffused decline in the rate of growth of real aggregate economic activity, relative to its long-term trend." If we adopt his definition, he says, 1966–67 and perhaps 1962 as well as other periods might be identified as recessions. In the case of 1966–67, if we take the 4 per cent rate of growth in potential GNP as the criterion, the quarterly rates fell below this from QII–1966 to QI–1968 with the exception of two isolated quarters. That is, there was a "recession" lasting almost two years. Would anyone accept this as a reasonable definition? Furthermore, since rates of growth of real GNP remained above 4 per cent only for the next three quarters, the implication is that since the beginning of 1966 we have been in a "recession" more than 80 per cent of the time. During most of this period, of course, the unemployment rate was well below 4 per cent, and the price level was climbing at a rapid rate. The term "recession," or even "growth recession" just does not seem to fit.

Fabricant indicates he would not be willing to depend upon a single series, say, real GNP. If not, what method of trend determination is to be applied to other series, and what would the results look like? Since such trend-adjusted series are not in common use today—and even the GNP gap measurement has suffered some diminution in attention, partly because it seems to require rather arbitrary shifts in the rate of trend growth—is it wise to adopt a method of defining the business cycle that depends upon data that are not widely used and accepted? This is my principal criticism of Ilse Mintz's trend-deviation method.

It is not quite so obvious as it may first appear that deflated aggregates should be the desideratum, as Fabricant argues. The price

level does reflect what is happening to the state of demand. Suppose the physical volume of sales has been declining, but begins to rise at a rate of 3 per cent per year. Suppose the price level has been falling, and continues to fall for a time, say, at a rate of 4 per cent per year. Has an expansion in economic activity begun, or not? That is, if sales are rising, but the aggregate *value* of what is being sold is falling, is "economic activity" rising or falling? To put it another way, should not the severity of a recession (or a possible recession) be judged in part by what is happening to demand, including prices? If one of our goals is stability in the price level, should not the price level be considered in judging the degree of instability? Exclusive emphasis on the physical volume of activity or the utilization of capacity in defining the business cycle, it seems to me, means paying less attention to the behavior of the price level. This does not, of course, mean that current-dollar aggregates or price indexes should be used exclusively either. I do think they have a bearing on how we should define a recession, and when it starts or ends.

Although Fabricant does pay some attention to the concept of diffusion, he does not deal with the idea that business cycle turning points have in the past been chosen to represent the consensus among a large number of economic activities. A peak in a broad aggregate can generally be depended upon to represent the consensus of peaks in its components. This concept of consensus has great advantages in analyzing a large number of different economic series, since comparisons of each with the consensus is a short-cut way of getting at direct comparisons of one series with another. If series A leads the business cycle and series B lags, we can usually infer that series A leads series B. It is not clear what happens to this idea when business cycles are defined in terms of growth rates. They will certainly no longer represent the consensus of up-and-down movements in individual series. Should all series be converted to growth rates? If not, how does one use the business cycle chronology in analyzing individual series?

Finally, a word about "step cycles." Mrs. Mintz's use of this idea for defining a cycle in rates of growth is ingenious, and carefully executed. The principal question I have about it is that the rates of growth to which the steps are fitted do not look much like steps. Without smoothing, as she points out, most rates of change are jagged, irregular, randomlike series. After smoothing, it seems to me, they look much like other series, and they do not proceed in steps. The step-fitting operation, therefore, seems to be imposing on the data something that is not obvi-

ously there. This is the reason why, in my chronology of the rate of change in prices, I simply identified peaks and troughs in rates of change measured over a long enough span so that most of the irregularities were erased and cyclical movements exposed.

To sum up, a number of empirical and theoretical matters need to be considered in adopting or changing the definition of business cycles. I'm all for considering them, but it seems to me premature to adopt them without further empirical testing. Ilse Mintz has begun this work, and I hope that others, including Fabricant, will take it up.

Reply by Ilse Mintz

Terminology

The terms that I use—growth cycles, speedups, slowdowns—leave much to be desired, and I would be glad to drop them if better ones were suggested. (Lempert suggests "high-rate phase," "low-rate phase," which is correct but very awkward.)

But however deficient they may be, using new terms is a great deal better than using old terms with a new meaning. Fabricant uses "recession" for periods which differ in dates, durations, amplitudes, etc., from traditional recessions. My objection to this has absolutely nothing to do with whether the new definition is or is not in the spirit of the old one. As long as measures of duration, amplitude, and so on, based on the new concept differ from their counterparts based on the old concept, separate terms are needed for clarity and simplicity. If both types of measures are described as measures of "recessions" it becomes necessary to add to each statement or table a note saying which concept of recession is being referred to.

Even now, Fabricant's use of "recession" has given the misleading impression that his and my conclusions about 1969–70 are different. In fact, they are very nearly the same, except that he calls "recession" what I call "slowdown."

The Alternative Revised Concepts

A decline in selected indicators: Moore has suggested (orally) that a slowdown could be defined as a decline in those indicators which still do show absolute falls.

My objection to this proposal is that most indicators of this type are leaders and thus not usable for a chronology. When we look at

1966–67 as a good example of the type of episode we want to define, we find that only the following coincident indicators declined: some (not all) measures of the labor market, interest rates, and the industrial production index. Determination of a cycle would thus rest on very thin evidence, which might easily dwindle further in the near future so that another revision might soon be required. The concept of a widely diffused decline in aggregate activity would, of course, be abandoned by this definition.

Peaks and troughs in rates of change: Moore's method for price cycles is to identify turns in the rate of change measured over six-month spans and centered in the middle month. This method is very useful for interprice comparisons, but it is not suitable (and has not been proposed by Moore) for reference cycle dating.

The main reason for not using growth rate turns is explained below. In addition it should be noted that the method is not as simple and familiar as one might think from inspection of Moore's charts. How many readers realize that a peak in, say, February 1970 has nothing to do with what happened in that month, but signifies that the rise from November to December 1969 was greater than the rise from May to June 1970? Also, the turning date depends on the choice of the span. For instance, the peak in the rate of change of the CPI is in March 1969, with a three-month span, and in February 1970 with a six-month span.

Deflated indicators: This method (Fabricant's) is discussed in Moore's comments. I will add only that the main difference between it and my deviation cycles is in the type of trend removed. While I remove the total trend, Fabricant removes only that part of the trend which is due to inflation. He classifies a period as expansion as long as real growth is positive. Alternation of a period of 10 per cent real growth with one of 1 per cent real growth is not regarded as a cycle. The years 1961–69 remain an unbroken expansion.

It will be very interesting to see the results obtained with this method, but I doubt that it goes far enough in adjusting the cycle concept to current needs. Since the absence of traditional recessions is not solely due to inflation, we will not obtain by deflation the distinction between cycle phases that is needed as an analytic tool and that corresponds to the general views on current fluctuations.

Cycles in deviations from trends: Moore's principal criticisms of this method are the arbitrariness of trend adjustment and the unfamiliarity of trend-adjusted data. Both objections are justified. The question

is whether they outweigh the objections to the use of data that are not trend-adjusted.

Since my reference cycle is based on a number of indicators, each adjusted by its own trend, the choice of the trend line is not as crucial as in the GNP-gap approach, cited by Moore, where *everything* depends on one trend. Furthermore, the deviation cycles are not accepted unless they are confirmed by the step-cycle method.

As to the unusual form of the data, I think the public will quickly get used to detrended series just as it got used to deseasonalized, deflated, and rates-of-change series.

Step cycles: Evidently the explanation of step cycles in my colloquium paper is insufficient and greatly in need of improvement. The step-cycle concept does not rest on the assumption that growth rates are a peculiar type of series moving stepwise. Moore is right in finding that growth rate curves, except for choppiness, look like curves of other series. The reason for the step-cycle treatment of rates of change is that they must be interpreted differently from other series. What is commonly regarded as a period of expansion is not a period of rising growth rates, but a period of high growth rates.

Growth rate peaks typically occur shortly after cyclical upturns, so that expansions of the economy are characterized largely by falling, not by rising, growth rates. In terms of growth rates, a business cycle is an alternation between high and low rates, not between rising and falling rates. In traditional business cycles "low" rates must be negative; in growth cycles they may be either positive or negative. The month in which high growth ends is the downturn. Usually this is also the month in which above-trend growth ends, the date of downturn of the deviation cycle.

The turning points are selected by our efficient computer program, which partitions the interval between two upturns into a high-rate and a low-rate period in such a way that the variance between the two steps is maximized. It enables us to distinguish between periods of above-average and below-average rates independently of any arbitrarily imposed trend line.

In answer to Moore's questions, near the end of his comment, there is high consensus among individual growth rate series. Except for one cycle, sixteen out of the seventeen indicators agree in some months of each cycle phase. All series are converted into growth rates or first differences.

The Urgency of Choosing a Revised Business Cycle Definition

Moore thinks that further testing is required. Juster believes it will be a long time for a decision to be made.

In my view the decision should be made right now. The need for a reference frame is so great that, in the absence of leadership from the NBER, every analyst is driven to devise his own method. This is what is happening today, and the various methods used are, of course, shortcuts and inferior to NBER methods. As the responses to my colloquium paper suggest, a tower-of-Babel confusion has replaced the former uniform NBER concepts.