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THE MONETARY STUDIES OF
THE NATIONAL BUREAU

PART II

Reports on

Selected

Bureau

Programs

To the theologian, money is the "root of all evil." To the economist, money had hardly less importance up to the early 1930's. It was then widely accepted that long-period changes in the quantity of money were the primary source of trends in the level of prices and that short-period fluctuations in the quantity of money played an important role in business cycles and might be the major explanation of them. For example, in his monumental book on business cycles published in 1913, Wesley C. Mitchell, while by no means promulgating or accepting an exclusively monetary theory of the cycle, gave much attention to monetary factors, constructing new estimates of various monetary components which are still part of the statistical underpinning of our present series on the stock of money.

The Keynesian revolution in economic thought in the mid-1930's produced a radical change in the attention paid by economists to money. The fact that the Federal Reserve System did not stem the Great Depression was interpreted as meaning that money was of secondary importance, at most a reflection of changes occurring elsewhere. Though this conclusion was a *non sequitur,* it was nonetheless potent. And it was all the more readily accepted because Keynes provided an intellectually appealing alternative explanation of the Great Depression. For nearly two decades thereafter, money became a minor matter in most academic economic writing and research, to be mentioned almost as an afterthought. And economic research on money was notable by its absence.

Recently there has been a revival of interest in money and a great increase in the amount

of economic research on money. Several causes combined to produce this revival of interest. One was dissatisfaction with the predictions yielded by the Keynesian analysis—the most dramatic being the failure of the much-predicted postwar depression to occur. A second was the emergence of inflation as a major problem in all countries that adopted the easy-money policy, widely regarded as called for by the Keynesian analysis. No country succeeded in stemming inflation until it replaced the easy-money policy by more "orthodox" monetary measures. A third was scholarly criticism and analysis of Keynes' theoretical structure, and the resulting attribution of an important theoretical role to the so-called "real-balance" effect. A fourth was the accumulation of empirical evidence bearing on the behavior of money and its relation to other economic magnitudes. The combined effect has been striking. Ten years ago, we at the National Bureau and an associated group at the University of Chicago were almost the only academic economists working intensively on money. Today, I am glad to say, we have a host of competitors.

1. THE STUDIES COVERED BY THIS REPORT

The National Bureau's monetary research has throughout been closely connected with its studies of business cycles. Wesley Mitchell's preliminary manuscript on business cycles contained a long chapter on the role of money and credit in the cycle. For that chapter, he had collected many series bearing on money and credit, which remain the backbone of the Bureau's collection of series in this area. The chapter was the starting point of the studies covered by this report, as other chapters were of so many of the major National Bureau studies.

This report covers only those monetary studies of the Bureau for which Anna J. Schwartz, Phillip Cagan, and I have had responsibility. The group of studies, begun well

over a decade ago, is now, I am glad to report, nearly completed. Hence, this report deals mostly with work already done or nearly done. Needless to say, just as our studies built on the earlier work of the Bureau and other investigators, so, I trust, they will in their turn open up new avenues of future research for the Bureau and for others. The test of success in any scientific research is dual: the questions it answers and, even more, the new questions it raises. Though I shall refer incidentally to some of the questions our work raises and on which further research is needed, I shall not attempt a comprehensive survey. Research must lead its own life. I am all too aware how much our own work departed from the lines we initially expected it to follow to want to peer too deeply into that clouded (and crowded) crystal ball.

As our work proceeded, we came to plan three monographs. One, *A Monetary History of the United States, 1867-1960*, by Anna J. Schwartz and myself, was published in 1963. A second, "Determinants and Effects of Changes in the Money Stock, 1875-1955," by Phillip Cagan, will soon go to press. The third, "Trends and Cycles in the Stock of Money in the United States," by Anna Schwartz and myself, is in first draft form. The major unfinished work is the substantial revision and expansion of the present draft, which was completed years ago and then put aside while we finished the *Monetary History*. We hope that by the next annual meeting we can report that this monograph too is ready or nearly ready for review by the Board of Directors.

In addition, four other Bureau publications have come from our studies. "Money and Business Cycles," by Friedman and Schwartz (Conference on the State of Monetary Economics, *Review of Economics and Statistics*, Feb. 1963 suppl.), is something of a preview and advance summary of one part of our projected volume, "Trends and Cycles." Friedman, *The Demand for Money* (O.P. 68, 1959), is a preliminary version of another chapter of that work, and Friedman, *The Interpolation of Time Series by Related Series* (T.P. 16, 1962), is a by-product of our monetary estimates.

Phillip Cagan's *The Demand for Currency Relative to Total Money Supply* (O.P. 62, 1958) is a preliminary version of part of his monograph.

2. THE MEANING OF "MONEY" AND OUR ESTIMATES OF THE QUANTITY OF MONEY

It will help put our work in proper perspective to distinguish at the outset between different senses in which the word "money" is used. In popular parlance, there are three main senses—as in pocket money, money market, and making money. In the first sense money refers to a class of assets of wealthholders; in the second, to credit; in the third, to income. Our work has been concerned with money in the first sense. We have of course had to consider both credit conditions and income: credit conditions as affecting the quantity of money, as being in turn affected by changes in the quantity of money, and as one of the channels through which changes in the quantity of money may affect income; similarly, income as perhaps the central total whose fluctuations constitute business cycles, as a source of changes in the quantity of money, and as itself affected by changes in the quantity of money. We have repeatedly been impressed in the course of our work with the importance of clearly distinguishing between money as an asset—as a stock at a point in time—and these other phenomena for which the word money is frequently used. Indeed, a key finding in our *Monetary History* is that the confusion of money and credit has been a primary source of difficulty in monetary policy. And recent experience indicates this is still so.

Credit conditions are affected by a much broader range of factors than those linked to the quantity of money and they require study in their own right. This is being done in the National Bureau studies of consumer credit, interest rates, and the quality of credit.

Our emphasis on money as an asset led us to take as our first major project the con-struction of a consistent and continuous set of estimates on the quantity of money for as long a period as possible. This turned out to be a more arduous task than anticipated, involving as it did piecing together numerous bits of data from a wide variety of sources. The final series starts in 1867, is for semi-annual or annual dates to 1907, and monthly thereafter. Though the series is now available (in an appendix to *A Monetary History*),a full description of sources and methods, and supplementary tables giving various components of the series and related series, are yet to be published. They will be included in our planned volume, "Trends and Cycles."

These estimates, as well as our subsequent work, brought to the fore the more specific question of precisely how to define money. Should it include only literal pocket money—that is, paper currency and coin? Or also demand deposits subject to transfer by check? Commercial bank time deposits? Mutual savings bank deposits? Savings and loan shares? Cash surrender values of life insurance policies? Series E bonds? And so on toward the outer bound defined by some of the broad concepts of liquidity; or, in a different and more appealing direction, toward weighted aggregates of the several elements.

Our statistical estimates, so far as feasible, give the components separately, so that each user can make his own choice within the limits of what we could estimate. In our work, we have generally found that the most useful single total is an intermediate one—currency held by the public, plus demand deposits adjusted of commercial banks, plus time deposits of commercial banks. Hence, we have termed this total "money" for our purposes and have used other expressions for other totals. The forthcoming volume on trends and cycles will discuss the question of definition in some detail and present the empirical evidence which led us to adopt this particular definition. So far as I can see, no issue of principle is involved in the choice of definition, but only a question of the empirical usefulness of one or another admittedly imperfect approximation to a theoretical construct. So far as I can see, no

important substantive issues are involved either. Judged by the criteria we used, alternative definitions are not much inferior to the one we adopted, so that a strong case against them cannot be made. Whenever possible, we have tried systematically to see whether any substantive conclusion is affected by substituting an alternative concept. Typically, none is, though some of the numerical relations may be different for one concept than for another. The occasional impression in the scientific literature that important substantive issues are involved generally turns out to be a result of the use of the word money to refer to different things.

All of our studies have been heavily dependent on the new estimates of the quantity of money we constructed. Our *Monetary History* "traces the changes in the stock of money . . . examines the factors that accounted for the changes, and analyzes the reflex influence that the stock of money exerted on the course of events."[1] In his monograph Cagan examines intensively the sources of changes in the stock of money and gives a detailed statistical analysis of the cyclical and secular behavior of each of the proximate determinants of the quantity of money, as we term them: high-powered money, the ratio of deposits at banks to their reserves, and the ratio of the public's holdings of deposits to its holdings of currency. The "Trends and Cycles" volume will, besides giving the basis for our new estimates, present a full statistical analysis of the secular and cyclical behavior of the stock of money and of monetary velocity in relation to other economic magnitudes. We shall rely heavily on the standard Bureau techniques to determine characteristic cyclical amplitude and timing. We plan also to supplement these techniques with both correlation techniques and—hopefully—spectral analysis, to see whether different techniques give consistent results.

The major scientific contribution of the studies probably will prove to be their quantitative findings about a host of specific magnitudes and relations. Most of our findings to date are summarized in the final chapter of *A Monetary History,* in the final chapter of Cagan's monograph, and in "Money and Business Cycles." They constitute building blocks to be incorporated in that general theory of the cycle which is the ultimate aim of scholars in the field.

Rather than try to summarize those findings here again, I should like instead to give something of the flavor of our work by considering an important specific issue, outlining the kind of evidence that is available from our published work on it, and giving some additional evidence from our unpublished work. I shall then summarize the general qualitative conclusions we have reached, with special stress on their limitations, and, finally, illustrate the applicability of some of our results to the interpretation of recent economic changes.

3. THE DIRECTION OF INFLUENCE BETWEEN MONEY AND BUSINESS

The specific issue I propose to consider is in some ways the central issue in dispute about the role of money in business cycles, namely, whether the cyclical behavior of money is to be regarded as a major factor explaining business fluctuations or as simply a reflection of business fluctuations produced by other forces. In Irving Fisher's words, the issue is whether the cycle is largely a "dance of the dollar" or, conversely, the dollar is largely a dance of the cycle. Stated still differently, the issue is whether the major direction of influence is from money to business or from business to money.

In each of these statements of the issue, I have used an adjective like "major" or "largely." One reason is that the alternatives contrasted are not mutually exclusive. Undoubtedly there can be and are influences running both ways. Indeed, insofar as the cycle is in any measure self-generating and not simply a response to external shocks, and insofar

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[1] *A Monetary History,* p. 3.

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as money plays any systematic role in producing the cycle, the influences must run both ways, the changes in the stock of money producing changes in business that produce changes in the stock of money that continue the cycle.

A second reason for the qualifying words is that there can be and almost certainly are factors other than money that contribute to the cycle, whatever may be the role of money. The question at issue is, therefore, whether money exerts an important independent influence, not whether it is the only source of business fluctuations and itself wholly independent of them.

What kind of evidence can be cited on this issue?

(1) QUALITATIVE HISTORICAL CIRCUMSTANCES

Perhaps the most directly relevant kind of evidence emerges from an examination of the historical circumstances surrounding changes in the quantity of money. They often have decisive bearing on whether the changes could have been an immediate or necessary consequence of contemporary changes in business conditions. This is particularly true about policy changes deliberately instituted by monetary authorities, which is why, as we say in *A Monetary History*, "the establishment of the Federal Reserve System provides the student of money a closer substitute for the controlled experiment to determine the direction of influence than the social scientist can generally obtain."[2]

From such evidence, it is possible to identify a number of occasions on which monetary changes have clearly been independent of contemporaneous changes in business conditions. On those occasions, the monetary changes have been accompanied by economic changes in the same direction, monetary contractions (or more precisely, reductions in the rate of change in the stock of money) being accompanied by contractions in money income,

[2] *A Monetary History*, p. 687.

prices, and output; and monetary expansions, by the opposite. The relation between monetary and economic change at those times also has been very much the same as on other occasions when historical circumstances were less decisive about the source of the monetary change. We ended our summary of this evidence in the final chapter of *A Monetary History* as follows: "Mutual interaction, but with money rather clearly the senior partner in longer-run movements and in major cyclical movements, and more nearly an equal partner with money income and prices in shorter-run and milder movements—this is the generalization suggested by our evidence."

(2) THE BEHAVIOR OF THE DETERMINANTS OF THE MONEY STOCK

In his monograph, Cagan provides a rather different kind of evidence. Any change in the money stock can be attributed to changes in the three proximate determinants, mentioned earlier: high-powered money, the deposit-reserve ratio, and the deposit-currency ratio. Any influence of business conditions on money must operate through one or more of these determinants. If this is the major direction of influence, the determinants separately should be more closely related to business conditions than the money stock as a whole is; moreover, the observed relation should be consistent with what we know about the character of the monetary institutions regarded as producing it. Hence, examination of the relation of money and each determinant separately to business conditions provides evidence on the direction of influence.

For secular movements, Cagan finds that high-powered money. is the major source of changes in the stock of money. During most of the period studied, increases in prices would be expected to have reduced the quantity of high-powered money by discouraging gold output and encouraging gold exports. Conversely, decreases in prices would have encouraged gold output and stimulated gold inflows. Yet the actual relation is the other way: price increases are associated with a higher than

average rate of rise in high-powered money; price decreases, with a lower than average rate of rise. Moreover, there is a closer relation between income and changes in the total money stock than between income and the separate determinants. Cagan concludes that, for secular movements, the predominant direction of influence must run from money to income. "To explain secular movements in prices," he writes, "we should look primarily to the supply of money and then secondarily to nonmonetary factors that may also have been important."

For cyclical fluctuations, Cagan finds the evidence more mixed. It is clearest for the severe business contractions. For these, he does not find it possible to attribute the changes in the stock of money to the effect of business on the determinants of the stock of money. Hence, the uniform coincidence of severe monetary contraction and severe economic contraction seems persuasive evidence for an influence running from money to business. As Cagan writes, "a monetary explanation of why some business contractions become severe, whatever may have started them, is hardly novel, but the supporting evidence is much stronger than is generally recognized." Incidentally, this explanation of severe business contractions is not necessarily inconsistent with an alternative explanation suggested by Moses Abramovitz in his work on long cycles. The relation between the two explanations will be examined in our "Trends and Cycles" volume.

For business cycles not containing severe contractions, Cagan finds clear evidence of the influence of business on money operating through the determinants. The deposit-currency ratio was the most important single source of cyclical fluctuations in the rate of change in the money stock. Cagan attributes most of the fluctuations in the deposit-currency ratio to the effect of the contemporaneous cyclical movements in economic activity. Similarly, he regards the fluctuations in the reserve ratio as reflecting cyclical movements in credit demands. For mild cycles, there is therefore clear evidence of a feedback effect of business on money. But Cagan also finds evidence of the same kind of effect of money on business

which is so clearly present in secular movements and severe contractions. That evidence is the fact that the relation between money and business during mild cycles remains the same over a long period despite substantial changes in the institutional structure connecting business and the separate determinants.

(3) Consistency of Timing on Positive and Inverted Basis

A third type of evidence is provided by the cyclical timing of monetary changes. However, to explain the relevance of this evidence, I shall have to digress briefly to describe our measures of the cyclical timing of money.

In studying the cyclical timing of money, we have found it more useful to examine the rate of change in the money stock than its absolute level. The reason is that the upward secular trend in the quantity of money has been so strong that the quantity of money has frequently tended to rise during both cyclical expansions and cyclical contractions. Cyclical forces show up much more clearly in the rate at which the stock of money rises than in whether it rises; or, alternatively, cyclical forces show up more clearly in the deviations of the stock of money from a secular trend.

We have used two alternative methods to describe the timing of the cyclical fluctuations in the rate of change in money. One is the standard Bureau specific cycle analysis: we date the months in which the series reaches peaks and troughs, and designate the resulting dates, the peaks and troughs in the rate of change. However, we have been hesitant to rely on this method alone. The major reason is purely statistical. Rate-of-change series are very erratic and jagged, having a characteristic saw-tooth appearance. This often makes it difficult to choose a particular month as the peak or trough. Several months, sometimes separated by a long interval, often seem about equally plausible. A subsidiary reason we have been hesitant to rely on the rate-of-change peak and trough dates alone is analytical. What feature of the money series is most relevant

to the cycle is by no means clear; whether the rate of change alone, or some cumulative total such as the deviation from a trend.[3]

Accordingly, we have used a second method of dating suggested by the empirical observation that the rate-of-change series often seemed to move around the same level for a time and then shift abruptly to a new level. This suggested approximating the rate-of-change series by a set of horizontal steps, which turn out typically to alternate between high and low steps. We designate as a "step peak" the month in which a high step ends and is succeeded by a low step, and as a "step trough" the month in which a low step ends and is succeeded by a high step. It turns out that these dates approximate the dates at which the deviation from a trend would reach a peak or trough. Their use obviates the necessity of actually fitting a trend.

We had hoped that one of these methods would yield dates bearing a more consistent relation to the timing of reference cycles than the other, giving us a basis for choosing between the two methods. So far, this hope has not been realized (see Table II-1); the two yield about equally consistent timing measures. Hence, we have continued to use both, regarding this as a way both to average out errors and to take account of different characteristics of the money series.

Both the rate-of-change peak and the step peak in the money series tend regularly to come earlier than the peak in general business (the reference peak) to which we match them, and both the rate-of-change trough and the step trough to come earlier than the matched reference trough. The interval is somewhat longer at peaks than at troughs, and decidedly longer for the rate-of-change turning points than for the step turning points. On the average of twenty-one matched cycles (from 1870 to 1961) the rate-of-change peak comes 17 months earlier than the reference peak, and

the step peak, 6 months earlier; the rate-of-change trough comes 13 months earlier than the reference trough, the step trough, 4 months earlier. As to consistency, the rate-of-change turning point comes earlier than the reference turning point at every one of the 42 turning points included in the above averages; the step turning point does so in 29 out of the 42.

These regular and sizable leads of the money series are themselves suggestive of an influence running from money to business but they are by no means decisive. One reason is that both the monetary changes and the business changes might be the common consequence of some other influences which have their effect on money more promptly than on business. A second is that the characteristics of business change affecting money may not be those that are dated by the Bureau reference dates.

The most important reason, however, why the consistent leads of the money series are not decisive is that, given a recurrent cyclical process, these leads may be simply the reflection of an earlier influence of business on money; they may be a statistical artifact resulting from our matching the turning points in money with the wrong turning points in business. Instead of matching a peak in the money series with the subsequent reference peak, we could match it with the prior reference trough; similarly, we could match the rate of change trough with the prior reference peak. This procedure yields shorter average timing differences for the rate-of-change dates—an average lag of 6 months at reference peaks and 13 months at reference troughs—and longer average timing differences for the step dates—an average lag of 16 months at reference peaks and 19 months at reference troughs.[4]

The question whether it is preferable to interpret the money series as mainly conform-

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[3]For a fuller discussion of this point and also some of the other points considered in this subsection see Friedman, "The Lag in Effect of Monetary Policy," *Journal of Political Economy,* Oct. 1961, pp. 447-466.

[4]Of course, given a recurrent cycle, a money peak could be matched with a prior reference peak as well, and similarly for the trough, implying a long-delayed positive effect of business on money; or a money peak and trough, with a succeeding reference trough and peak, implying a long-delayed inverted effect of money on business, and so on. We have restricted the discussion to the simplest alternative interpretations.

ing positively to the cycle with a lead or invertedly with a lag is therefore relevant to the more general question whether the predominant direction of influence is from money to business. All theoretical analysis I know of which would explain how money can play an independent role in the cyclical process also implies that the connection is positive, that is, that unusually high rates of rise in money promote business expansion, unusually low rates, business contraction. Hence, inverted conformity, whether with a lag or a lead, would sharply contradict the existence of a strong influence from money to business, and positive conformity, especially with a lead, would be consistent with such an influence. On the other hand, many of the links between business and money, as Cagan has shown, may be expected to produce an inverted response; the clearest example is the tendency of business expansion to produce gold outflows and hence downward pressure on high-powered money. Inverted conformity with a lag would therefore be entirely consistent with an influence running from business to money. Positive conformity could be, too, since some of the effects of business on money are in a positive direction, for example, the effect of business expansion on bank reserve ratios. However, it is not easy to rationalize positive conformity with a lead as reflecting supply response.

The nub of these considerations is that inverted conformity would clearly contradict a predominant influence of money on business; positive conformity would be consistent with such an influence and, especially with a lead, would constitute evidence in favor of it but would not rule out an influence of business on money. And, of course, as with the more general question, positive and inverted conformity are not mutually exclusive; both exist, and both are plausible. The question is, which is dominant.

How can our timing measures help us choose between positive and inverted conformity? One obvious answer is by seeing which interpretation yields more consistent timing measures. Are the leads or lags more nearly the same from cycle to cycle on one interpretation than

on the other?

Table II-1, which comes from our unfinished manuscript, "Trends and Cycles," contains the relevant evidence. It gives, for all cycles from 1870 to 1961, the dispersion (as measured by the standard deviation) of the leads and lags as computed under the two interpretations and as determined both from rate-of-change and step dates. The dispersion is uniformly lower when the money series is treated as conforming positively, and the difference is substantial.[5] So far as this evidence goes, it clearly supports positive conformity.

(4) Serial Correlation of Amplitudes of Cycle Phases

A very different kind of evidence on positive versus inverted conformity is provided by the size of cyclical movements in money. In order to explain what this evidence is, I shall again have to digress, this time to describe a most interesting feature of business cycle behavior which has implications for many problems besides the one under discussion.

The feature in question is the relation between successive phases of business cycles. Is the magnitude of an expansion related systematically to the magnitude of the succeeding contraction? Does a boom tend on the average to be followed by a large contraction? A mild expansion, by a mild contraction? To find out, we have used two different measures of the amplitude of cyclical phases: one, the Moore index,[6] as an indicator of the change in the physical volume of activity; the other, the vol-

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[5]If the standard deviations on the two interpretations could be regarded as statistically independent of one another and each based on independent observations, the ratio of the larger to the smaller that would be exceeded by chance less than one time in twenty would be 1.46, and less than one time in 100, 1.73. For three of the four comparisons in Table II-1, the ratio considerably exceeds the latter level, and for the fourth, the former. The specified conditions are not satisfied by these data but it is not clear in which direction the comparison is biased.

[6]The Moore index is our designation of an average of three trend-adjusted indexes of general business used by Burns and Mitchell (*Measuring Business Cycles*, p. 403) as a broad indicator of the amplitude

TABLE II-1

COMPARISON OF TIMING MEASUREMENTS OF RATE OF CHANGE IN
MONEY STOCK ON POSITIVE AND INVERTED BASIS, 1870-1961

	KIND OF SPECIFIC CYCLE TURN IN RATE OF CHANGE IN MONEY STOCK			
	Last Month of Step at:		*Trough or Peak in Rate of Change at:*	
	Reference Troughs	Reference Peaks	Reference Troughs	Reference Peaks
Mean lead (–) or lag (+), in months				
Positive basis	–4.0	–6.1	–13.2	–16.9
Inverted basis	19.5	15.6	12.8	6.4
Standard deviation of lead or lag, in months				
Positive basis	5.6	7.1	6.0	7.6
Inverted basis	11.7	15.8	15.1	12.3
Number of observations	21	21	21	21

NOTES Matching with reference turns follows Arthur F. Burns and Wesley C. Mitchell, *Measuring Business Cycles,* New York, NBER, 1946, pp. 115-128, with a few exceptions. Strict adherence to the Burns and Metchell procedure would not reverse the finding that the standard deviations are larger on the inverted basis than on the positive basis.

SOURCE: Money stock: 1870-1946, from *A Monetary History,* Table A-1, col. 8; 1947-61, *Supplement to Banking and Monetary Statistics,* Sect. 1, Board of Governors of the Federal Reserve System, Oct. 1962, pp. 20-22.

ume of bank clearings or debits, as an indicator of the change in money values. Lines 2 and 3 of Table II-2 (which, like Table II-1, is taken from the present draft of "Trends and Cycles") show that, when the amplitude of an expansion is correlated with the amplitude of the succeeding contraction, the resulting correlation is negligible for both measures. Surprisingly, perhaps, there appears to be no systematic connection between the size of an expansion and of the succeeding contraction, whether size

is measured by physical volume or by dollar value.

Let us now ask the same question, except that we start with a contraction and ask how its amplitude is related to that of the succeeding expansion. As lines 5 and 6 of Table II-2 show, the results are very different for the physical-volume measure though much the same for the dollar-value measure. A large contraction in output tends to be followed on the average by a large business expansion; a mild contraction, by a mild expansion.

This phenomenon, if it should be confirmed by a fuller analysis of data for the United States and other countries, would have important implications for the analysis of business cycles in general, not solely for our monetary

of cycles, and revised and extended by Geoffrey H. Moore (*Business Cycle Indicators,* G. H. Moore, ed., Princeton for NBER, 1961, Vol. I, p. 104; and an unpublished memorandum).

TABLE II-2

RANK DIFFERENCE CORRELATION BETWEEN CHANGE IN ONE CYCLE PHASE AND
CHANGE IN NEXT SUCCEEDING CYCLE PHASE, RATE OF CHANGE IN MONEY AND
TWO INDICATORS OF GENERAL BUSINESS, 1879-1961, EXCLUDING WAR CYCLES
AND 1945-49

Series Correlated with Itself	Annual and Semiannual Data 1879-1908	Monthly Data 1908-61	Whole Period 1879-1961
Expansion in Indicated Series and Succeeding Contraction in Same Series			
1. Rate of change in money stock, per cent per month in specific cycles	−.02	.33	.24
2. Moore index, in specific cycle relatives (indicator of physical change in general business)	−.07	.10	.10
3. Clearings-debits, in reference cycle relatives (indicator of dollar-value change in general business)	−.05	−.39	.15
Number of pairs	8	10	18
Contraction in Indicated Series and Succeeding Expansion in Same Series			
4. Rate of change in money stock, per cent per month in specific cycles	.83	.68 ·	.74
5. Moore index, in specific cycle relatives	.71	.85	.86
6. Clearings-debits, in reference cycle relatives	−.17	.46	.26
Number of pairs	8	7	15

NOTE: War cycles 1914-19 and 1938-45 are omitted because of their special charac-
teristics. The 1945-49 cycle is omitted because the expansion is skipped by the rate-of-change
in money series. Specific cycles are those matched with reference cycles in the column
headings. There was a one-to-one correspondence between specific and reference cycles.

SOURCE: Money stock: see Table II-1. Specific cycle analysis follows Burns and
Mitchell, *Measuring Business Cycles*, pp. 115-141.

Moore index: Unpublished memorandum by Geoffrey H. Moore, extending table
in *Measuring Business Cycles*, p. 403, and revising and updating table in *Business Cycle
Indicators*, Vol. I, p. 104. An average of three trend-adjusted indexes of business activity—
A.T.&T., Persons-Barrons, and Ayres—each of which was analyzed for specific cycles,
suppressing specific cycle turns not corresponding to reference cycle turns.

Clearings-debits: Bank clearings outside New York City, monthly,
1879-1919: bank debits outside New York City, monthly, 1919-61.

1879-1942: Seasonally adjusted from *Historical Statistics of the United States, 1789-1945*,
Bureau of the Census, 1949, pp. 324-325, 337-338.

1943-61: Board of Governors of the Federal Reserve System, Division of Bank Operations,
mimeographed table, "Bank Debits and Rates of Turnover" (C.5, Revised Series, 1943-52),
Dec. 23, 1953; thereafter *Federal Reserve Bulletin*, adjusted for seasonal variation by
NBER. Reference cycle analysis follows Burns and Mitchell *Measuring*, pp. 160-170.
Values of the rank-difference correlation coefficient that would be exceeded in absolute
value by chance in the indicated proportion P of independent samples are:

Value of P	Number of observations				
	7	8	10	15	18
.10	.71	.64	.56	.44	.40
.05	.79	.74	.65	.52	.48
.01	.93	.88	.79	.69	.63

studies. For one thing, it would cast grave doubt on those theories that see as the source of a deep depression the excesses of the prior expansion.[7] For another, it would raise serious questions about both the analytical models, in terms of which most of us have come to approach the analysis of cycles, and the statistical methods we use to analyze them.

Our analytical models generally involve a conception of a self-generating cycle, in which each phase gives rise to the next, and which may be kept going by a sequence of random shocks each giving rise to a series of damped perturbations. The corresponding physical analogy is of an electrical network in which responses are described by sine waves. The asymmetric serial correlation pattern suggests that this analogy may be misleading, that a better one is what can be termed a plucking model. Consider an elastic string stretched taut between two points on the underside of a rigid horizontal board and glued lightly to the board. Let the string be plucked at a number of points chosen more or less at random with a force that varies at random, and then held down at the lowest point reached. The result will be to produce a succession of apparent cycles in the string whose amplitudes depend on the force used in plucking the string. The cycles are symmetrical about their troughs; each contraction is of the same amplitude as the succeeding expansion. But there is no necessary connection between the amplitude of an expansion and the amplitude of the succeeding contraction. Correlations between the amplitudes of successive phases would be asymmetric in the same way the correlations in lines 2 and 5 of Table II-2 are. Expansions would be uncorrelated with succeeding contractions, but contractions would be correlated with succeeding expansions. Up to this point, the peaks in the series would all be at the same level. To complete the analogy, we can suppose the board to be tilted to allow for trend and the underside of the board to be irregular to generate variability in the peaks, which would also introduce something less than perfect correlation between the size of contractions and subsequent expansions.

In this analogy, the irregular underside of the rigid board corresponds to the upper limit to output set by the available resources and methods of organizing them. Output is viewed as bumping along the ceiling of maximum feasible output except that every now and then it is plucked down by a cyclical contraction. Given institutional rigidities in prices, the contraction takes in considerable measure the form of a decline in output. Since there is no physical limit to the decline short of zero output, the size of the decline in output can vary widely. When subsequent recovery sets in, it tends to return output to the ceiling; it cannot go beyond, so there is an upper limit to output and the amplitude of the expansion tends to be correlated with the amplitude of the contraction.

For series on prices and money values, the situation is different. The very rigidity in prices invoked to explain the decline in output may mean that the declines in prices vary less in size than the declines in output. More important, there is no physical ceiling, so that there is nothing on this level of analysis to prevent the string from being plucked up as well as down. These differences make it plausible that the asymmetric correlation would be much less marked in money-value series than in output and perhaps entirely absent in price series. This is so for the correlations in Table II-2, which are small for clearings-debits. The same conclusion is suggested also by graphic inspection of a wide variety of physical volume and price series. A symmetric pattern of downward pluckings can be clearly seen in many of the physical volume series; such a pattern is much less clear in the price series; and, in some price series, symmetric upward pluckings seem about as numerous.

[7]The major qualification that must be attached to our result for this purpose is the definitions of the cycle and of expansion and contraction phases on which it rests. Proponents of the view cited might well argue that what matters is the cumulative effect of several expansions, as we define them, and that the relevant concept of expansion is of a "major" expansion or a phase of a long cycle.

The contrast between the physical-volume and dollar-value or price series can be put somewhat differently. The indicated pattern in physical-volume series is readily understandable regardless of the reason for the cyclical fluctuations in the series—of the source of the pluckings, as it were. A similar pattern in value or price series would have to be explained by some similar pattern or asymmetry in the source of the cyclical fluctuations, some factor that prevents upward plucking from being as important as downward plucking.

Let us now return to our major theme and see how we can use this feature of business cycles to get additional evidence on the appropriate interpretation of the money series. If positive conformity is dominant, and if the monetary changes are linked with physical-volume changes, then the serial correlations for money should be the same as for the Moore index. On the other hand, if inverted conformity is dominant, and changes in business produce later changes in the opposite direction in money, then the correlations for money should be the opposite of those for the Moore index, that is, the amplitude of an expansion should be correlated with that of the succeeding contraction; and the amplitude of a contraction should be uncorrelated with that of the succeeding expansion.

The relevant correlations for the specific cycle amplitudes of the rate of change in money are given in lines 1 and 4 of Table II-2. We have as yet no parallel analysis for step amplitudes, though we plan one. The correlations we have for money are roughly the same as for the Moore index. The simplest interpretation of this result is that the pattern for business is a reflection of the pattern for money. In terms of our analogy, every now and then the money string is plucked downward. That produces, after some lag, a downward movement in economic activity related in magnitude to the downward movement in money. The money string then rebounds, and that in turn produces, after some lag, an upward movement in economic activity, again related in magnitude to the upward movement in money. Since the downward and subsequent upward movements in money are correlated in amplitude with one another, so are downward and subsequent upward movements in economic activity. Since the upward and subsequent downward movements in money are not correlated in amplitude, neither are the upward and subsequent downward movements in economic activity.

Personally, I find this bit of evidence in favor of dominant positive conformity particularly persuasive for two reasons. The first is that I have been unable to construct an explanation of how the observed asymmetric correlation pattern for money could be produced by an inverted response of money to business cycles. The second is that our historical studies have uncovered a number of episodes that correspond precisely to the notion of downward pluckings of the money string.

(5) Evidence from Foreign Countries

All the evidence so far cited is for the United States. In addition, there is much evidence of a similar kind for other countries.[8] Cagan's earlier work on hyperinflations provides some striking results of a positive relationship for rather extreme monetary episodes.[9] Several studies on Chile, done by students or faculty members of the University of Chicago, provide persuasive evidence of a more mod-

[8] I exclude the well-known studies which deal chiefly with long-period secular rather than short-period cyclical relations, such as Earl J. Hamilton's classic work on the price revolution in the sixteenth century as a result of the inflow of specie from the New World, or J. E. Cairnes' "Essays Toward A Solution of the Gold Question" (*Essays in Political Economy,* London, Macmillan, 1873, pp. 1-165), in which he analyzed in advance the effects to be expected from the gold discoveries in Australia and California and then after the event added postscripts checking his predictions with the actual outcome—one of the earliest and still one of the best applications of the scientific method in economics.

[9] Phillip Cagan, "The Monetary Dynamics of Hyperinflation," in *Studies in the Quantity Theory of Money,* Milton Friedman, ed., University of Chicago Press, 1956.

erate though still substantial inflation.[10] Some unpublished work on Canada by George Macesich demonstrates that the timing relations between monetary and economic change there are very similar to the relations in the United States.

In order to expand the range of evidence on this and related issues, I went on something of a fishing expedition last year (on leave from both the University of Chicago and the National Bureau) to explore the data available for foreign countries differing as widely as possible from the United States, and to learn something about their monetary arrangements. The countries I studied in some detail were Yugoslavia, Greece, Israel, India, and Japan. For each, I collected data on the quantity of money, income, prices, indexes of industrial production, interest rates, and the like. There is no doubt that sufficient data are available to make comparative studies feasible.

So far, I have been able to do little analysis of the data I gathered. But even that superficial analysis has uncovered some interesting bits of additional evidence on the direction of influence. For Yugoslavia, for example, there happens to be an episode for which the direction of relation is hardly doubtful: the stock of currency (which seems the appropriate measure of "money" for such a country) and income in current prices both have been rising rather rapidly in the past decade, with one marked exception in both. There is one year in each series in which the upward trend is replaced by a horizontal movement. That year comes one year earlier in the currency series than in the money income series! For Israel, the data, which are carefully compiled, show roughly the same relation between rates of change as for the United States, with rates of change in currency leading rates of change in income by about a year. For Japan, cyclical fluctuations

of the past ten years or so seem readily interpreted as a strictly self-generating monetary cycle in response to changes in the rate of change in the money stock. The contractionary monetary changes are produced by the reactions of the monetary authorities to recurrent balance of payments difficulties, which are a response to prior expansionary monetary changes that occur when the balance of payments eases. The Japanese data show about a three- six-months' lead of the rate of change in the money supply over the rate of change in production and prices. We have as yet no conceptually similar timing comparisons for the United States, though we are in the process of making them. Perhaps the closest are the timing comparisons between the step dates and reference turns. Those show a roughly similar lead.

(6) THE COMBINED WEIGHT OF
THE EVIDENCE

In a scientific problem, the final verdict is never in. Any conclusion must always be subject to revision in the light of new evidence. Yet I believe that the available evidence of the five kinds listed justifies considerable confidence in the conclusion that the money series is dominated by positive conformity, which reflects in some measure an independent influence of money on business. The feedback effect of business on money, which undoubtedy also exists, may contribute to the positive conformity and may also introduce a measure of inverted conformity.

In the "Trends and Cycles" volume, we hope to carry farther our analysis of the evidence based on the timing and amplitude of fluctuations in the money series (subsections 3 and 4). We have no present plans for doing any further work on the qualitative historical evidence or on that provided by the determinants of the money stock (subsections 1 and 2). Data for foreign countries (subsection 5) merit much fuller analysis, and I have interested a number of students in research for doctoral dissertations which will make a start in that direction. However, this is not part of

    [10]John Deaver, "The Chilean Inflation and the Demand for Money," unpublished Ph.D. dissertation, University of Chicago, 1960; Arnold C. Harberger, "The Dynamics of Inflation in Chile," in Carl Christ et al., Measurement in Economics, Stanford University Press, 1963, pp. 219-250.

the Bureau's program, though it is obviously relevant to our common intellectual interests.

4. OUR CENTRAL QUALITATIVE CONCLUSIONS AND THEIR LIMITATIONS

The central conclusion we have reached in our studies is of a piece with that reached on the specific issue considered in the preceding section, and like that, though still tentative, in our opinion justifies much confidence. Stated simply, it is that money does matter and matters very much. Changes in the quantity of money have important, and broadly predictable, economic effects. Long-period changes in the quantity of money relative to output determine the secular behavior of prices. Substantial expansions in the quantity of money over short periods have been a major proximate source of the accompanying inflation in prices. Substantial contractions in the quantity of money over short periods have been a major factor in producing severe economic contractions. And cyclical variations in the quantity of money may well be an important element in the ordinary mild business cycle.

These qualitative conclusions, and even more specific quantitative findings, are important. But they are also limited. Because they go sharply counter to what has been so widely believed for nearly two decades, there has been some tendency to interpret our claims as being far more than they are. For example, one newspaper story referring to similar views interpreted them as asserting that "the growth of the money supply is the single most important factor affecting the nation's economy"—which is very far indeed from what we are saying. To avoid misunderstanding, let me state explicitly some of the limitations of our conclusions.

One limitation is linked to the distinction between "real" magnitudes—relative prices, quantities of output, levels of employment, efficiency of production, accumulation of capital, and the like—and "nominal" magnitudes—

absolute prices, quantity of money, nominal money income, and so on. The quantity of money in general appears not to be an important factor affecting secular changes in the real magnitudes. They are determined primarily by such basic phenomena as the kind of economic system, the qualities of the people, the state of technology, the availability of natural resources, and so on. These, not monetary institutions or policy, are the critical factors that ultimately determine the "wealth of nations" and of their citizens. In general, the major long-run impact of the quantity of money is on nominal magnitudes, and especially on the absolute level of prices. Our conclusions are in no way inconsistent with that celebrated—and much misunderstood—statement of John Stuart Mill, "There cannot, in short, be intrinsically a more insignificant thing, in the economy of society, than money; except in the character of a contrivance for sparing time and labor. It is a machine for doing quickly and commodiously, what could be done, though less quickly and commodiously, without it; and like many other kinds of machinery, it only exerts a distinct and independent influence of its own when it gets out of order."[11]

What we can now add to this is a much more explicit specification of what it means for the machinery of money to "get out of order." It gets out of order, we have tentatively concluded, when the quantity of money behaves erratically, when either its rate of increase is sharply stepped up—which will mean price inflation—or sharply contracted—which will mean economic depression—and especially when such erratic movements succeed one another. One of our major findings is that, over periods spanning several cycles, the average rate of growth of the stock of money—so long as it is relatively stable and within moderate limits—has no discernible effect on the rate of growth of real output. Differences in monetary growth are reflected instead in prices. Our findings give no support to the view, now widely popular, that long-run inflation is favorable to

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[11]*Principles of Political Economy* (1848), Ashley ed., Longmans, Green, 1929, p. 488.

economic growth. Deviations from the average rate of growth of the stock of money, if sharp, account for the inflations or severe contractions already referred to. If mild, the deviations are linked to the usual business cycle, and appear to be reflected partly in prices and partly in quantity, though we know little as yet about what determines how much of the effect is on prices and how much on quantity. The general subject of the division of changes in money income between prices and quantity badly needs more investigation. None of our leading economic theories has much to say about it. Yet knowledge about it is needed for better understanding of the impact not only of monetary changes but also of other factors significant in the business cycle.

A second limitation is linked to the distinction between average behavior and behavior in a particular episode. The fact that we can predict within fairly narrow limits the number of heads that will come up in a thousand tosses of a fair coin does not enable us to predict what will come up the next time. As students of business cycles, we are concerned largely with average behavior. The data for any particular episode are bound to be subject to considerable errors of measurment and to be affected by casual events peculiar to that episode. We can largely compensate for both bad data and erratic behavior by constructing averages for a number of episodes. The results may be well established, on the average, yet not reliable for predicting an individual case. Our earlier discussion of cyclical timing is an excellent example. As noted above, data on the month-to-month changes in the quantity of money are highly erratic and irregular, and there is often much uncertainty for an individual cycle about which month shows the highest rate of change (rate-of-change peak), or which month is followed by a shift in the rate of change to a lower level (step peak). Hence there is also much uncertainty about the difference in time between the rate-of-change peak and the reference peak or between the step peak and the reference peak—a date which is itself subject to error. But such errors may be expected to cancel out, so the average

timing may be well determined. For example, in the course of 21 matched cycles from 1870 to 1961, the estimated difference in timing between the step peak and the reference peak varied from a lag of 4 months to a lead of 17 months with a standard deviation of 7 months. These estimated differences average out to a lead of 6 months, and this average is rather accurately determined. The standard error of the *average* is only 1.6 months, which means that the odds are 2 to 1 that the error in the average time is less than 1.6 months and 20 to 1 that it is less than 3.2 months.

Looked at another way, the fact that, on the average, the step peak comes 6 months before the estimated reference peak does not enable us to say very much about any particular occasion. Even if we could know that an observed shift to a lower rate of growth of the money stock is one that we would later regard as a step peak—much easier to know by hindsight than at the time—about the most we could say would be that there was roughly a 50-50 chance that a turn in business that we could later regard as a reference peak would occur between 1 and 11 months later. Our inability to be more precise may reflect our inability to measure the various magnitudes very accurately, or it may reflect inherent variability in the economic response to monetary stimuli. At the present stage of our knowledge, we do not know which.

Our assertion that money matters is therefore very far indeed from an assertion that we know enough about the role it plays and can measure sufficiently accurately the relevant magnitudes to predict precisely what effect an observed change in the quantity of money will have in a particular case. Needless to say, the aim of further research is to improve the precision of such predictions.

A third limitation, and the last one I shall mention, is that we are still a long way from having a detailed and tested theory of the mechanism that links money with other economic magnitudes. For long-period secular changes, for short-period rapid inflations, and for severe contractions, there exist reasonably well-formulated theories and a good deal of

empirical evidence on transmission mechanisms. But for the ordinary business cycle, we are in a much less satisfactory position. In "Money and Business Cycles," we sketched very broadly some of the possible lines of connection between monetary changes and economic changes "in order," as we wrote, "to provide a plausible rationalization of our empirical findings . . . to show that a monetary theory of cyclical fluctuations can accommodate a wide variety of other empirical findings about cyclical regularities, and . . . to stimulate others to elaborate the theory and render it more specific."[12] We shall try to improve and elaborate this sketch in our "Trends and Cycles" volume, but I am not sure just how far we can get within the limits we have imposed for ourselves. Identification of the channels through which short-run monetary changes work their effects, and specification in quantitative terms of the characteristics of the channels and of the effects exerted through them, remain major tasks for future research.

5. THE STOCK OF MONEY AND RECENT ECONOMIC CHANGES

A look at recent history will enable us to illustrate many of the points made in the preceding sections and to show the relevance of some of our findings to current problems.

The upper panel of Chart II-1 shows for the past seven years three series: (1) the money stock, as we define it, which is to say currency plus all commercial bank deposits adjusted; (2) currency plus demand deposits adjusted only, an alternative concept which is often referred to as the money supply; (3) the Federal Reserve index of industrial production, as a single index of the physical volume of general economic activity. The vertical scale is logarithmic, to show relative not absolute changes.

[12]Friedman and Schwartz, "Money and Business Cycles," p. 59.

The two money series illustrate why the total stock of money is not of itself a very useful magnitude for studying cyclical movements. The series are smooth and dominated by their trends. Cyclical fluctuations show up in the form of waves about the trend and only occasionally in the form of absolute ups and downs. For this period, there is only one absolute decline in the money stock series (from 1959 to 1960). The trends of the two series differ much more for that period than for most, reflecting the recent rapid rise in the time deposits of commercial banks, apparently largely in response to the successive rises in the rates of interest banks have been permitted, and have been willing, to pay on them. But aside from the trend, it is perhaps obvious even from these series that the two show very much the same movements.

The series on industrial production is much less smooth. It shows three decided declines: the first, a reflection of the 1957-58 recession; the second, of the steel strike—this one, we would be inclined to smooth out as a random movement; and the third, of the 1960-61 recession. The letters T and P at the bottom of each panel on the chart correspond to the months designated by the Bureau as reference troughs and peaks, respectively. The first trough coincides with the upturn in the production index; the succeeding peak comes three months after the downturn in the production index; and the second trough, one month after the upturn in the production index.

Whereas the money series represent stocks at successive points of time — like the stock of housing or the level of inventories—the index of industrial production represents a flow—like new construction or additions to inventories. This is a major reason the production index is so much more variable than the money series are.

In the lower panel of Chart II-1, we have converted the money series into flow series, also, by plotting the month-to-month percentage changes in them. They show the cyclical fluctuations much more clearly. The characteristic saw-tooth pattern in first-difference series is obvious, and so is the frequent difficulty of

CHART II-1

Comparison of Changes in Money and Production, 1957-63
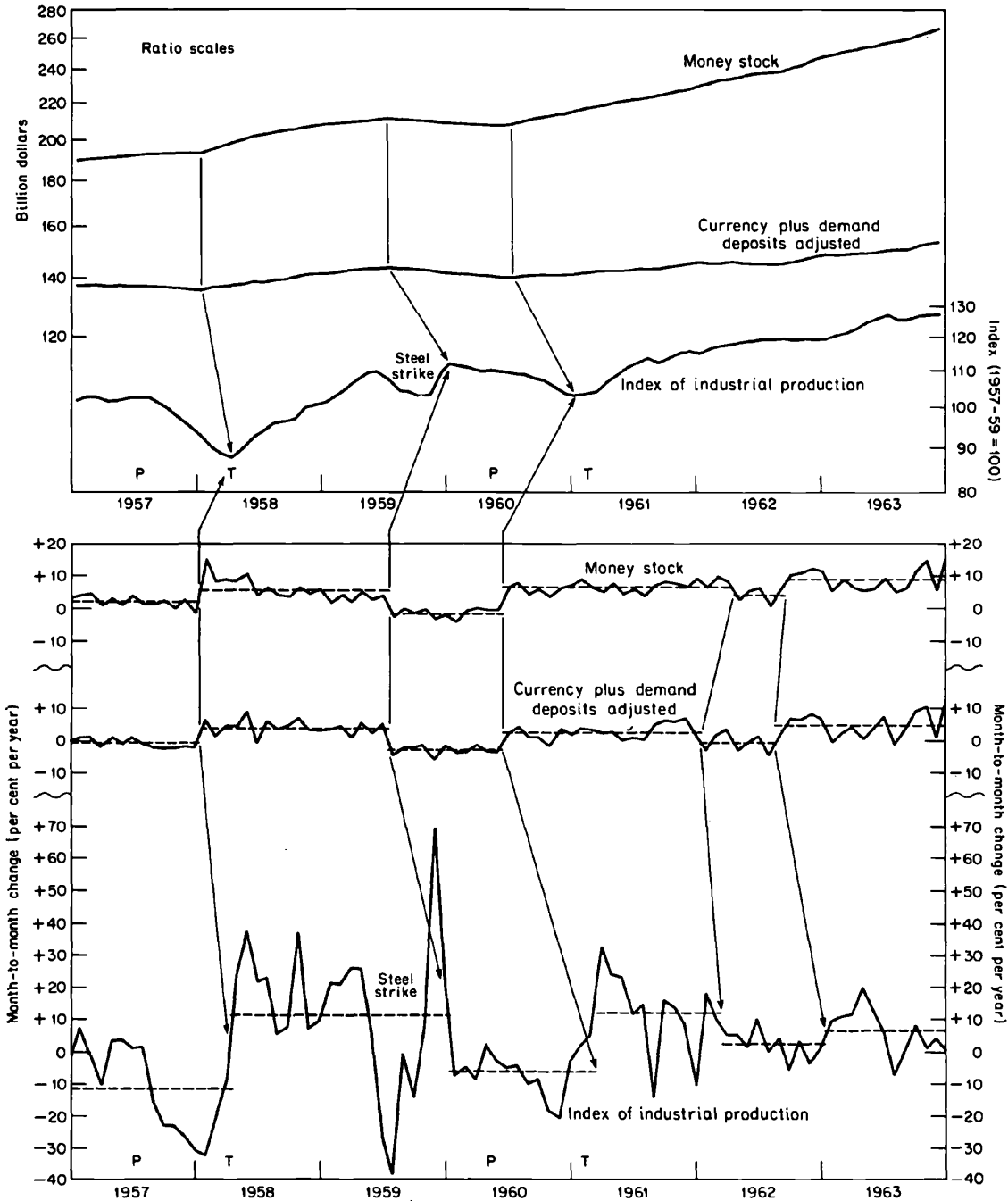
The horizontal broken lines represent high and low steps in the rate of change.
T and P show reference cycle turning points.

picking single months to represent the peaks and troughs. This segment of time, 1957-63, also shows clearly the tendency—noted above for much earlier periods—of the rate of change to move around a rather constant level and then shift to a new level. The horizontal lines are the "steps" with which we have approximated the series, and the ends of the steps are our step peaks and step troughs. For this segment, the step dates seem less ambiguous than the specific cycle dates, but for other segments the opposite is true.

Comparison of the two money series in the lower panel illustrates our general finding that the substantive results do not depend on which particular definition is used. The two series are obviously closely parallel. The only appreciable differences are in early 1958 and in early 1962, when the rate of change of the broader series is higher relative to its level before and after than is the rate of change of the narrower series. The reason for the first difference is not clear. The second comes immediately after the Board of Governors raised the rates of interest that commercial banks were permitted to pay on time deposits. The dates we have chosen for the ends of our steps are identical for both definitions, except the low step in early 1962. We date that step as beginning February 1962 and ending August 1962, for the narrower concept, and as beginning May 1962 and ending September 1962, for the broader. Because of the disturbances introduced by the change in the rates of interest on time deposits, we are inclined to prefer the date derived from the narrower concept—but clearly, no great error will be introduced, whichever is used.

Comparison of the money series with the production index illustrates the positive conformity and the lead that we have found so characteristic, as well as the variability of the lead. To bring this out arrows have been drawn from the ends of the steps in the rate-of-change money series in the lower panel and from the corresponding dates on the stock series in the upper panel to the turning points of the production index. For the step dates, the leads at the two troughs are 3 months and 7 months

(8 months to the terminal reference trough) and at the intervening peak 6 months (10 months to the reference peak). These are certainly very much in line with the average timing over the past 90 years, which is 4 months at the trough and 6 months at the peak (see Table II-1). So this segment illustrates very well the stability we have found in monetary relations.

The reason for drawing the arrows from the stock series as well as from the rate-of-change series is to show how the movements which show up so clearly in the rate-of-change series can be seen also in the stock series, once one looks for them.

The money series show a low step in 1962 that we have so far not matched with any corresponding movement in the upper panel for the production index. However, though the production index has risen since early 1961 except for an occasional month, it is clear that there was a distinct retardation in late 1962. The retardation was the source of much concern at the time and was associated with the lower level of national income attained than had been forecast early in the year. To bring that movement into sharper relief, we have used the same technique for the production index as for the money series, namely, plotted month-to-month percentage changes. This series is even more erratic than the money series, but there is clearly a low step in 1962 to correspond with the low step in the money series. Its onset, as we have dated it, comes 2 months after the beginning of the low step in currency and demand deposits, and 1 month before that in the broader money total. The shift to a new higher level comes 5 months after the shift to a higher level in the rate of change in money.

This minor perturbation in industrial production will not and should not be classified by the National Bureau as a reference cycle; hence, neither its occurrence, its correspondence to the shift in money, nor the timing of the two movements would be revealed in a standard Bureau cyclical analysis. This is one of that species of subcycles that Ruth Mack has brought to our attention. The existence of such episodes is one of the reasons we plan to supplement the standard cycle analysis in

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our "Trends and Cycles" volume with correlation analysis of at least quarterly series.

The chart shows very much wider fluctuations in industrial production than in the rate of change in money series. If instead of industrial production a measure of aggregate output had been used, the contrast would have been narrower but still present. The contrast is even greater for aggregate money income than for output. We reported in "Money and Business Cycles" that, on the average, the percentage fluctuations in income were twice as large as those in the rate of change in money and offered a hypothesis to explain why this should be so.

So far, I have used the recent period to illustrate some of our technical problems and some of our descriptive findings. But it can also serve to illustrate the problems of interpretation. I have described Chart II-1 entirely in terms of a positive conformity of the money series; trying to describe it in terms of inverted conformity will perhaps suggest some of the difficulties we have found with such an interpretation and some of the reasons we have rejected it. The still more important question is whether we should interpret the positive conformity as reflecting the influence of money on business, or of business on money. If these were the only alternatives, I would find the former much more appealing for this segment of time in particular. There have been in this period five rather clear-cut shifts in monetary action—as judged by the rate of change in the stock of money. Each has been followed after some months (with one possible exception, early 1962, if the link is made with the broader money series) by a shift in the same direction in the rate of growth of economic activity, as judged by the production index. Perhaps this pattern reflects the common effect of some third force; it is hard to explain it by any direct influence of business on money.

MILTON FRIEDMAN

THE BEHAVIOR AND DETERMINANTS OF INTEREST RATES: A PROGRESS REPORT

The interest-rate project is the most recent of a number of major studies of financial markets made possible by grants from the Life Insurance Association of America. Our work has benefited substantially from the advice and assistance of an Advisory Committee under the chairmanship of W. Braddock Hickman, President of the Federal Reserve Bank of Cleveland. The research is being conducted by Jack Guttentag (University of Pennsylvania), Morris Beck (Rutgers University), Avery Cohan (University of North Carolina), William H. Brown, Jr. (Swarthmore College), Phillip Cagan (Brown University), Reuben Kessel (University of Chicago), and myself.

The present project consists of six studies. In two of them, we examine important but neglected sectors of the capital market: mortgages and direct placements. In two, we study interest-rate behavior through time: seasonal and cyclical movements. In two, we study specific aspects of the linkage of markets: the

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term structure of rates and the new-outstanding yield spread on corporates. I shall report on each of these in turn.

1. THE MORTGAGE MARKET

This study is being directed by Jack Guttentag, with a major segment of the work, the task of data gathering, under the direction of Morris Beck. Mortgages are frequently overlooked in discussion of interest rates and their determination. One reason is that we know far too little about this market, and another is that the quantitative importance of mortgages is often inadequately recognized. In fact, the volume of mortgage debt and its rate of growth far exceed that of any other segment of the capital market.

Mortgage debt in the United States amounted to $252 billion at the end of 1962, compared with less than $95 billion in total corporate long-term bonds and notes outstanding. Furthermore, mortgage debt had risen 348 per cent since 1948 compared with a rise of 192 per cent in corporate bonds. Chart II-2 compares annual increases in mortgage debt with those in three other major categories of debt between 1954 and 1962.

The mortgage market is of especial interest not only because of its size but also for other reasons. It is one of the sectors affording some of the clearest evidence of an influence of interest rates on economic activity. It is a sector for which direct controls have been repeatedly suggested as an adjunct to monetary policy. In this market government activity has become extensive, including the FHA and the VA programs as well as FNMA and other agencies. In it, we can examine the effects of partial interest-rate ceilings, as in the FHA and VA programs.

Despite its importance, we know disturbingly little about the mortgage market. Our present study includes two major parts. The first, a group of cross-section studies, is intended chiefly to find the major yield-determining characteristics of mortgages other than forces influencing interest rates generally. The second will analyze the way mortgage rates have

behaved over time and compare their behavior

CHART II-2

Annual Changes in Debt Outstanding, 1954-62
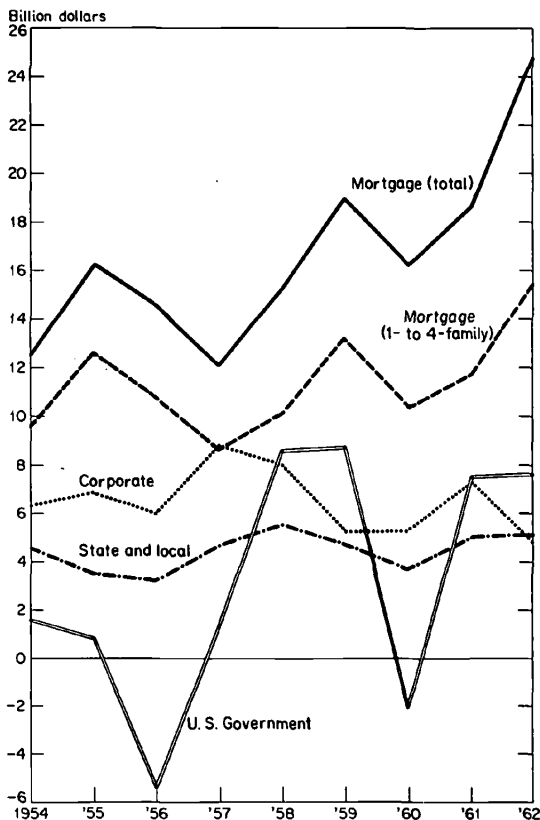
with that of yields on other securities. We present here a sketch of findings to date.

Yield-Determining Characteristics

The most important yield-determining characteristics of residential mortgages appear to be location of property, type of lender, size of loan, and, under some circumstances, loan-to-value ratios. The role of maturity cannot be fully determined until further study is completed.

Location is one of the most important variables determining mortgage yields. We have not yet been able to examine the important question of differences in yield between rural and urban

loans, but we have some information on yield differences between regions. In 1953, conventional mortgages authorized by large life insurance companies in the three Pacific coast states yielded an average of 4.86 per cent as compared with 4.55 per cent in the Middle Atlantic states. Similarly, in 1960, Pacific coast yields were 6.13 per cent on the average, compared with 5.95 per cent in the Middle Atlantic states. The regional yield differentials are probably larger among local lenders than among outside lenders like the life insurance companies just described.

That there is a significant difference in mortgage yields between different types of lenders is well known. What has not been known is whether this can be wholly explained by the difference in type of loan made. Guttentag's study of the Chicago data reveals that differences persist even when essentially identical mortgages are compared. Among the three types of institution for which adequate data were available to permit comparisons, savings and loan mortgage yields were highest, mortgage company yields next, and commercial bank yields lowest. Yields on loans of both savings and loan associations and commercial banks tend to be inversely related to size of lending institution (after taking account of other yield-determining factors), but the tendency is not uniform.

Size of loan affects yields through two channels, and the effects are opposite in direction. First, the size of loan and the size of monthly payments are highly correlated. Presumably, high monthly payments (income held constant) carry greater risk and result therefore in high yields. Hence a positive correlation would tend to be established between size of loan and yield. However, when we hold monthly payment constant, we find that size of loan is negatively correlated with yield. This suggests that cost of processing per dollar of loan is appreciably smaller on larger loans, so that large loans generally yield significantly less than small ones, when risk is held constant.

Differences in risk explain in important measure observed differences between yields on mortgage loans. One important risk variable is size of loan relative to property value. As one might expect, changes in the ratio of loan to value make little difference in the yield when the ratio is very low to begin with. The ratios on conventional mortgages made by commercial banks, for example, fall generally within a range of 50-65 per cent and no relationship to yield is evident. At higher ratios, however, the relationship to yield is quite pronounced. Even on VA mortgages, which carry a guarantee, the loan-value ratio has a statistically significant (although quantitatively modest) effect on yield because the ratios range generally over 90 per cent.

The influence on yield of term to maturity is one of the most difficult to unravel because maturity is related to many other factors that bear on yield. Longer-maturity mortgages, for example, typically carry higher loan-value ratios, which work toward higher yields. On the other hand, longer mortgages also tend to be larger, which works toward lower yields (after account is taken of the monthly payment). Even abstracting from cost and risk considerations, moreover, lenders have different attitudes toward maturity because of different investment objectives. On conventional mortgages on new homes made by savings and loan associations in Chicago, there is a pronounced positive relationship between yield and maturity, after account is taken of other factors. On loans covering existing properties, however, which carry lower loan-value ratios and higher income-loan ratios, no relationship is evident. Generalization regarding the influence of maturity would be precarious until our analysis is pushed further.

OTHER OBSERVATIONS

The cyclical timing of mortgage rates as compared with that of other security yields looks interesting, and we hope it may shed further light on the determinants of interest rates generally. On the basis of incomplete data, the

well-known tendency of mortgage rates to lag behind bond yields appears to be appreciably reduced by recording mortgage rates on authorization dates instead of disbursement dates, but the tendency is not entirely eliminated. The amplitude of the yield series for all mortgages authorized by life insurance companies appears to be somewhat less than that of government bonds.

2. DIRECT PLACEMENTS

The second sector of the capital market that seemed to us especially in need of study is the one in which corporate bonds are placed directly through negotiation between borrower and lender. As Chart II-3 shows, this sector of the market is quantitatively extremely important. The volume of newly issued direct placements has averaged very close to that of public offerings during the entire period shown. In every year of the 1950's, substantially larger amounts of new money were secured through this avenue than through all new equity issues including both common and preferred shares.

. The major growth of direct placements has come in the last thirty years. Between 1900 and 1933 corporate direct placements were only 3 per cent of all corporate debt offerings. By 1935, this ratio had risen to 29 per cent for industrial, financial, and service industries, but it still remained relatively low for rails and public utilities. As the chart shows, the importance of this financial procedure had grown greatly by 1950. Between that year and the end of 1961, $42.4 billion was borrowed through this channel, an amount representing 46 per cent of total corporate debt issues. The economic contribution of the financial innovation was clearly of tremendous importance. As Avery Cohan, who is directing our study, has stated, this procedure "made long range funds available to a whole new range of borrowers— especially those who were relatively small or

CHART II-3

New Long-Term Corporate Security Issues, 1950-61
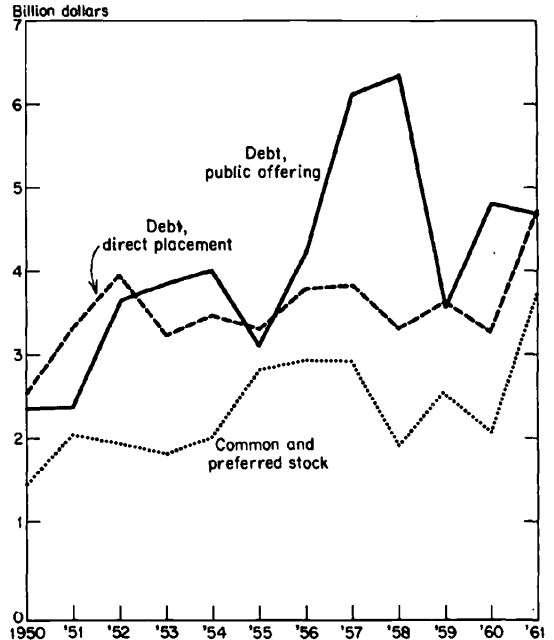
unknown or who had reasonable but unconventional arrangements in mind," so that a public bond offering would not have been feasible.

As in the mortgage market, we wanted to evaluate the yield-determining characteristics of direct placements and to utilize this information in constructing time series of yields for homogeneous classifications of issues. Since data permitting such a study are not publicly available, it was necessary to go directly to major lenders and copy the needed information, item by item, from their records.

YIELD-DETERMINING VARIABLES

On the basis of interviews with practitioners in the market for direct placements, Cohan

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selected 18 variables which might prove influential in determining yields. The variables included financial characteristics of the company issuing the debt and characteristics of the security. These were used as independent variables in 22 cross-section regressions, one regression for each six-month period from 1951 through 1961.

The accompanying figures show the average influence of each of the six most important variables on industrials. The left-hand column gives the percentage by which the yield of direct placements would vary, if the variable measured moved within its entire range as experienced during the 11-year period, other variables held constant. The right-hand column gives similar results in basis points if a 4 per cent yield level is used as base.

Independent Variable	Maximum Influence on Yield of Direct Placements	
	In % of Yield	In Basis Points
1. Total capitalization	31.8	127
2. Times interest earned (5-yr. average)	10.5	42
3. Earnings before interest and taxes	8.9	35
4. Size of issue	7.5	30
5. Maturity	6.1	24
6. Average term (including effects of amortization)	5.4	22

Three of the variables (1, 3, and 4) are measures of size of borrower or are commonly correlated with size. The second most influential item, like size of borrower, is a measure having direct bearing on the risk of the loan: times interest earned. It is not highly correlated with any other variable showing importance as a yield determinant, and hence the influence indicated above does not reflect the force of other variables among the 18 we examined. Finally, variables 5 and 6 both measure duration of loan. As would be expected, they are highly correlated with one another.

With the possible exception of the variables measuring duration of loan, all the above variables influenced yield in the direction to be expected. As for the exception: anyone familiar with yield curves on outstanding publicly offered bonds in recent times might expect yield

to rise with length of term. In fact, the opposite effect is shown by our statistics, as a number of experts in this market had anticipated.

TIME SERIES

Time series have not yet been thoroughly analyzed, though they have been plotted for four categories of direct placement (see Chart IV-5, page 114). Two observations are suggested by these graphs. First, general yield movements of direct placements since 1950 seem quite similar to those on Moody averages of high-grade securities. Second, yields on new publicly issued bonds appear to have been rising relative to the return on direct placements. This means that if direct placement yields are higher than yields on other bonds of comparable quality, as is generally assumed, the differential has been getting smaller over the decade. Whether such a differential exists, and if so, how large it is cannot now be determined from our data because we have not yet derived a link between Moody ratings and our own quality classifications.

3. SEASONAL BEHAVIOR

Just as we decided upon two projects dealing with important but neglected sectors of the capital market, so we also decided to undertake studies of two aspects of the movement of yields over time. The first concerned the seasonal behavior of interest rates. Toward the end of the 1950's Frank Morris of the Investment Bankers Association and others were

publishing evidence of a seasonal movement in certain interest rates. We decided to explore that behavior with three major objectives in mind. One was to determine whether there really are identifiable seasonal patterns in interest rate series, and if so to measure them. The second was to make seasonal adjustments in those interest-rate series requiring them in order that adjusted series may be available for cyclical analysis and other purposes. The third was to learn what seem to be the causes of seasonal patterns and what this information might contribute to an understanding of interest rates generally. William H. Brown, Jr., is carrying on this study.

The seasonal behavior of interest rates is largely influenced by man-made seasonal phenomena which prove to be highly unstable over any extended period. Thus, tax dates have had a dominant role in determining seasonal interest rate movements, but the amount of taxes collected in each of the four quarters changed substantially over the decade studied. The decision of the government to issue or not to issue tax-anticipation bills or certificates can greatly influence "seasonal" interest-rate patterns. Correction for such seasonal movements is just as important for business cycle study as any other seasonal behavior is, but identification of shifting seasonal patterns is extremely difficult.

A major characteristic of any seasonal adjustment under these conditions is that the adjustment will commonly vary from year to year. Yet the process of identifying a seasonal pattern is ordinarily one of averaging several years together in order to weed out entirely irregular movements. In consequence, seasonal adjustment when the seasonal itself has shifted must be an uneasy compromise. A major June decline that might have resulted from a truly irregular cause or even a cyclical one can influence averages that include several previous and succeeding Junes, giving the appearance of a seasonal low extending over several years. At the same time, a change in the pattern of seasonal movement may be fairly sudden, in fact, but it will be spread out and softened by averaging in the adjustment procedure.

CHART II-4

Weekly Seasonal Adjustment Factors for Three-Month Treasury Bills, 1950, 1955, 1960
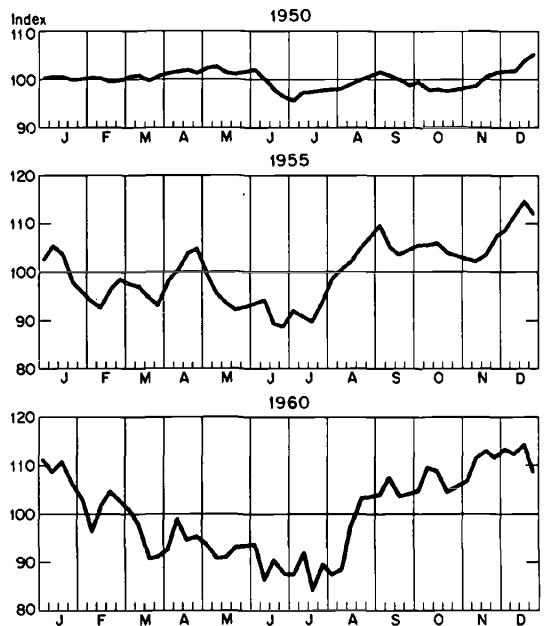
After considerable experimentation, our final adjustments were made by the Bureau of the Census Method II developed by Julius Shiskin. This is a highly sophisticated procedure and we know of none better, but our findings will be badly misread if the limitations referred to above are not kept in mind.

The clearest evidence of a seasonal pattern is for short-term securities, especially three-month Treasury bills. The seasonal adjustment factors show a high in December during all ten years 1951-60. They show a low in June or July nine of the ten years. Not only was this pattern extremely stable, but also its amplitude widened with a remarkably consistent trend over the decade. Chart II-4 depicts this trend and other regularities in the pattern of adjustment factors for Treasury bills.

It should be emphasized that this record provides no basis for future prediction, unless the study of causation suggests that the under-lying reasons for the pattern will probably per-

sist. As we show below, such prediction would be highly inappropriate in this case.

The significance of these seasonal movements may be more fully understood if they are compared with other types of change. The movements of the original series during 1951-61 can be divided into three components: seasonal, cyclical-secular, and irregular. The ratio of the average monthly amplitude of one type movement to that of another gives a measure of their importance in terms of size. Thus the ratio of the seasonal to the cyclical for Treasury bills equals .94. The average monthly "irregular" movement was slightly higher than the seasonal, the ratio of seasonal to irregular being .99. The ratio of the seasonal movement to the average monthly change of the original series was .55. Clearly, the seasonal movements were highly significant not only in their regularity, but also in their quantitative importance. Similar data for other issues are provided in Table II-3.

Two short-term rates in addition to Treasury bills were examined. The over-all pattern of behavior of yields on bankers' acceptances was remarkably similar to that of Treasury bills, both in timing and amplitude. Commercial paper seasonals reveal some interesting differences. Seasonal highs are not in December but October every year from 1952 to 1959; in 1960, the high moved to December. This pattern is much closer to that of long-term corporates than to that of bills. The lows on commercial paper seasonals were in March from 1953 through 1956, again similar to the pattern for corporate bonds. After 1956, low points shifted to July, matching the behavior of Treasury bills. For further characteristics see Table II-3.

I have already commented in passing on long-term corporates. The seasonal highs of Moody Aaa corporates were in September 1954 through 1958, and then successively in October and December. After 1953, bond seasonals move almost uninterruptedly and sharply from their lows in February or March to their high in September, after which a sharp decline contrasts severely with the "leveling but still rising" movement of Treasury bill seasonals as seen in Chart II-4.

The timing of seasonal yield movements on state and local issues and on governments follows the same pattern as that on highest grade corporates. The evidence of a true seasonal in highest grade long-term corporates and in governments is weak, as indicated by low amplitude, by a fundamental change in timing over the decade, and by low statistical significance.

Analysis of the causes of seasonal movement in interest rates is not yet completed, but a few

TABLE II-3

SEASONAL ADJUSTMENT FACTORS, 1951 TO 1960

| Type of Security | Levels and Ratios of Average Monthly Amplitudes | | | | |
	Original Series[a]	Ratio: Seasonal to Original	Ratio: Seasonal to Cyclical	Ratio: Seasonal to Irregular	Level of Significance by F-Test
Treasury bills	8.45	.55	0.94	.99	.01
Bankers' acceptances	3.81	.61	0.70	1.04	.01
Commercial paper	3.77	.50	0.65	.75	.05
Aaa corporates	1.47	.47	0.75	.81	.05
U.S. long terms	2.00	.35	0.62	.53	ns
Aaa state and local	3.10	.51	1.00	.84	.05

[a]Measured in basis points.   ns = not significant.

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general comments may be made. There is a variety of convincing evidence indicating that a major cause of the seasonal movement in Treasury bill yields is the seasonal variation in government securities of less than one year maturity held by the public. These fluctuations in turn reflect the seasonal pattern of Treasury receipts and expenditures. Confirming this presumption is the important fact that during the years 1961 and 1962 two parallel changes may be noted: (1) the seasonal in bill yields was sharply reduced in 1961 and virtually eliminated in 1962; (2) in 1961, the variation in the supply of bills made available to the public was reduced, and a much sharper reduction occurred in 1962. Also, the variation in total net cash borrowing from the public in 1962 was substantially smaller than in immediately preceding years.

4. CYCLICAL BEHAVIOR

Cyclical movements of interest rates, under study by Phillip Cagan, are important both because of their possible influence on other economic variables and as a source of clues to understanding the determinants of interest rates.

One indication of the importance of the cyclical movements is their amplitude. The average cyclical rise and fall on twenty-year governments between October 1949 and February 1961 was about three-quarters of one percentage point, representing a price difference of about 11 points. The amplitude of yields on three-month Treasury bills during the same period averaged 1.86 percentage points. The size of the yield changes may be better grasped if we note that the average peak yield on longs was 127 per cent of the average of yields at the preceding and following troughs; the corresponding figure for bills was 233 per cent.

Two major characteristics of cyclical fluctuations require special attention—timing and amplitude. Each shows a wide variety of behavior, as we compare different cycles with one another, and as we compare different

interest-rate series during the same cycle. Despite this diversity, certain generalizations can be made. Let us begin with timing. During the two closing decades of the nineteenth century, the few series available lagged the business cycle markedly. Lags on long terms were by far the greatest, generally exceeding eight months; but shorts also generally turned after business with median lags of three to seven months. Later, and especially after World War II, there has been a persistent tendency for closer and closer synchronization between cycles in business activity and the movements of interest rates, both long term and short term.

At peaks the lag in long rates has not only diminished but in a slight majority of cases was replaced by leads after World War II.

As for shorts, Treasury bills have consistently led at peaks since 1923, and even at troughs they led more often than any other series did. Call money rate is the only other short term with a substantial number of leads at either troughs or peaks. Bank rates on business loans have continued to lag.

In a somewhat different analysis of these data, Cagan shows that with one exception turning points of interest-rate series are very much closer to one another in time than to the business cycle. The one exception is with shorts at peaks, where all turns come close to being synchronous with each other and with the business cycle peak.

The amplitude of cyclical movement on call money rates and commercial paper rates was very wide between 1885 and 1913, averaging between 11 and 31 basis points per month. The amplitude declined during that period, and it continued at generally much lower levels after 1919. In sharp contrast, the amplitude of movement in long terms was extraordinarily low in the early period, averaging between 1.5 and 2.1 basis points per month between 1885 and 1913 on the two series available; these amplitudes were substantially higher after World War I than before. Thus, the secular change in amplitude on longs from the late nineteenth century to the 1920's was exactly opposite to the change in amplitude on shorts,

and both changes were very marked. Between the 1920's and the 1950's, however, amplitudes on shorts seem to have risen somewhat, whereas there was no clear trend in amplitudes on longs. A similar generalization can be made regarding the relative size of interest-rate amplitudes as compared with the amplitudes of business cycles.

For many of us, the most important reason for study of the cyclical behavior of interest rates is to help find the causes of this behavior. A systematic exploration of these questions will not be possible in the present project, but Cagan has opened the door to two varieties of study that are suggestive both for further research and for possible hypotheses regarding the determination of interest rates.

There are good reasons to believe that two of the important elements which may help explain the cyclical behavior of interest rates are (1) movements in the rate at which the money supply changes, and (2) the behavior of banks with respect to loans and investments.

For each cycle from 1904-08 through 1958-61 Cagan superimposed the reference cycle patterns for commercial paper rates and those for the percentage change in the money supply (see Chart IV-4). The inverse conformity between the two series is striking, especially in view of the fact that many other elements must influence interest rates. Indeed, the conformity is marked in every cycle except the decline following 1929. The causal implications of this observation are now being explored.

Cagan compared the cyclical patterns from 1919-21 through 1958-61 on short-term interest rates, long-term interest rates, bank investments, bank loans, and a number of other series. As is well known, bank loans and bank investments commonly move inversely to each other. Banks attempt to accommodate their customers and adjust their portfolios to meet this need as required. If necessary, and sometimes when not necessary, banks may borrow from Federal Reserve Banks instead of selling securities, but this does not prevent the opposing movements of loans and investments. The cyclical patterns reveal the inverse conformity clearly, loans moving cyclically in agreement with business expansion and contraction. This means that, typically, banks throw securities on the market during the expansion of business. The cyclical pattern makes plausible the hypothesis that one of the important factors causing interest rates on bills and bonds to rise during business expansions may well be the action of banks, whereby a changed demand for bank loans is transmitted directly to the capital market through the securities in which banks trade—primarily Treasury bills but also long-term government bonds. When the demand for bank loans increases, the banks' sale of securities causes their prices to fall and their yields to rise. At the same time, bank rates on loans also increase.

Important evidence in support of this thesis is revealed by study of cases where bank investments do not move inversely to the business cycle. Inverse conformity between interest rates and bank investments remains strong and distinct in almost all such cases, as well as when bank investment behavior conforms inversely to the business cycle pattern.

5. THE TERM STRUCTURE OF RATES

Our third pair of projects consists of two studies of the linkage of markets. The first, on the term structure of interest rates, is important partly because it is central to any interest theory, and partly because of its direct relevance for government policy and for portfolio management. Its relevance for portfolio management is self-evident. Its relevance for government policy is seen in relation to any program in which debt management is proposed in order to influence the level of economic activity, or in which monetary policy is conducted by trading in securities with different term to maturity. An important contemporary objective directly involving term structure has been the government's desire for low long rates to encourage business at home, together with high short rates in order to pre-

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vent the balance of payments consequences of short-term capital exports.

An analysis of term structure is fundamental to any theory of interest rates, because major contending theories about the term structure imply entirely different determinants of long-term rates. Among academic scholars, by far the most widely accepted explanation of the term structure of interest rates is based on the expectations hypothesis or a modified version of it. Briefly summarized, the unmodified version of this theory runs essentially as follows. If one-year rates are now 1 per cent and if one-year rates are expected to be 3 per cent next year, then two-year rates today will have to be in the neighborhood of their average, 2 per cent. Only such a relationship can equalize returns for a two-year investment by the two avenues available: the purchase of a two-year security, or investment in a one-year obligation followed by reinvestment in another. Though individuals may not all be able to invest for the full two-year period, speculators will force the approximate equality.

This example may be generalized by stating that long rates will tend to be an "average" of expected short-term rates over the intervening period. Because of compounding, this is a complex kind of weighted average. It should be noted from the derivation of our generalization that it implies expected yields over any given holding period, including capital gains or losses, must be equal on all securities, long or short. If expectations are uniform and held with perfect confidence, securities of different term become perfect substitutes for one another. One consequence is that under this assumption a change in the mixture of outstandings between longs and shorts will not affect the term structure, unless it changes expectations. Another implication of this theory is that from the yield curve at a given time it is possible to derive the expected future short-term rates up to the maturity of the longest security on the yield curve.

Reuben Kessel conducted our major study of the term structure of interest rates. Kessel's work consisted chiefly of testing a modified version of the expectations hypothesis. The modification takes account of the view, expressed by Hicks and others, that investors are not indifferent to the risk of holding long-term securities and hence may demand (or offer) a premium in the form of higher (or lower) yields on longs. The risk exists because the capital values of longs (in contrast to shorts) may change in unexpected, drastic amounts if interest rates change.
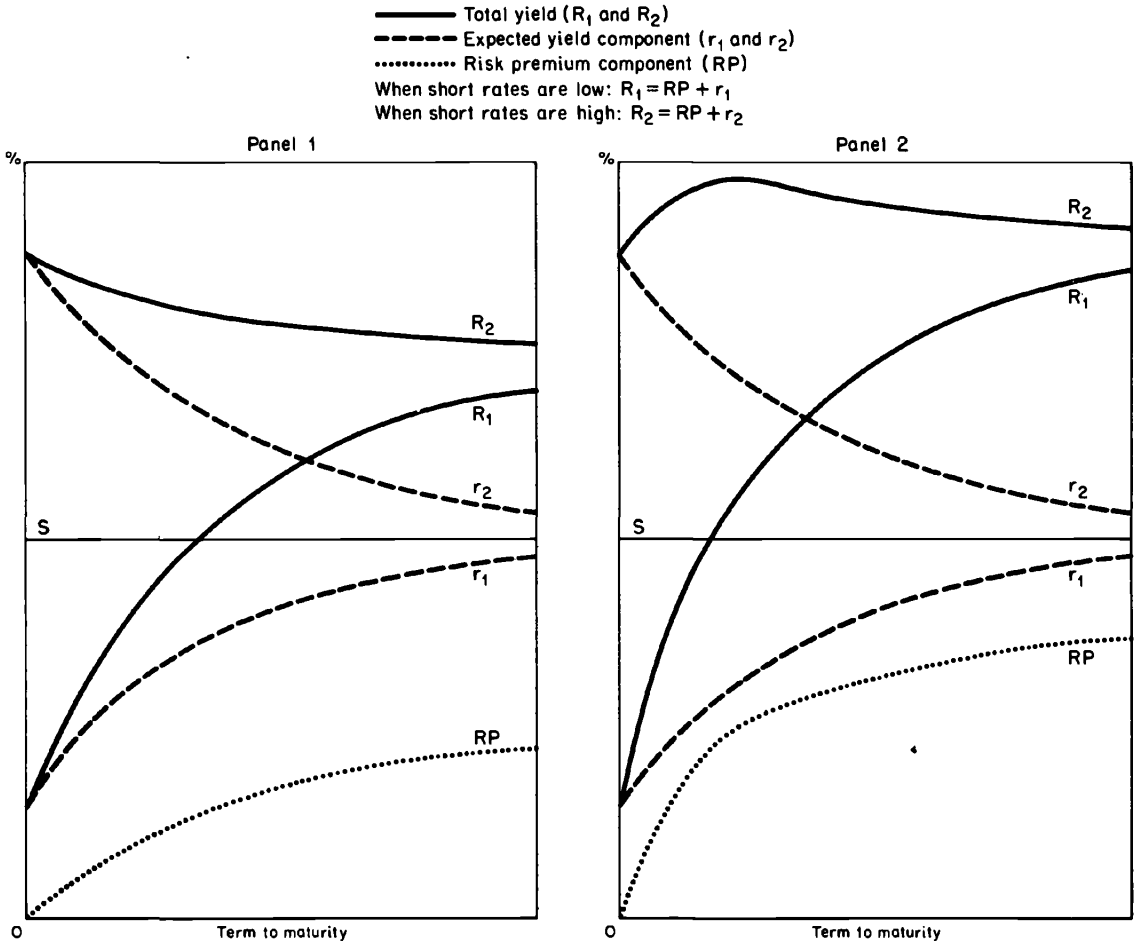
Kessel undertook to explore whether such a modified expectations hypothesis is consistent with the observed facts and, if so, whether the risk premium on longs is positive or negative. His studies support the view that without the introduction of positive risk premiums the expectations hypothesis seems untenable: it is contradicted by a persistent tendency of yield curves to slope positively and by other evidence of implied market predictions that seem too wide of the mark to persist in the real world. The introduction of risk premiums provides a theory in which these objections are greatly softened, and one which explains much of observed interest rate phenomena.

Chart II-5 illustrates the way interest rates might reflect the combined forces of expectations and risk premiums. RP is the risk premium curve, these premiums rising monotonically with term to maturity because of the rising risk of capital loss as term lengthens. According to Kessel's findings, this component of observed interest rates will always be positively sloped, but it may grow steeper when interest rates rise. The r_1 and r_2 curves represent the yield curve as it would be under an unmodified expectations hypothesis, the first when current short rates are low and expected to rise, and the other when they are high and expected to fall. As portrayed, this curve would respond to changing levels of short rates by taking positive and negative slopes so that an over-all composite would show rough symmetry about a horizontal line drawn at S.

Observed yields would be the sum of the appropriate RP and r curves. The result of the configuration in Panel 1 is that a majority of yield curves are positively sloped, but that in times when rates are very high relative to historical standards the dominance of the ex-

CHART II-5

Components of Hypothetical Yield Curves,
Expectations and Risk-Premium Hypotheses Combined

—————— Total yield (R_1 and R_2)
— — — — Expected yield component (r_1 and r_2)
············· Risk premium component (RP)
When short rates are low: $R_1 = RP + r_1$
When short rates are high: $R_2 = RP + r_2$

pectations component may produce negatively sloped curves like R_2.

Panel 2 represents an alternative possibility. If the RP curve is much more sharply curved at an early point and then flattens rapidly, and if the r_2 curve resembles the one shown here, the combination may well produce a hump in the intermediate short-term range. Such curves have been observed at times of high rates in the post-World War II period.

A composite theory of this kind is consistent with Kessel's empirical studies. It is supported by both Meiselman's and Kessel's studies of the implications of combining the expectations hypothesis with Meiselman's hypothesis on the determination of expectations. It is given some support from Kessel's tests of the forecasting ability of the market, which looks much more reasonable when risk premiums are admitted to the theory. Finally, it is consistent with the logic of equilibrium relations on which the expectations hypothesis was initially founded.

A general objection to this logic is raised by financial practitioners, who rightly point out

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that they make no attempt to predict short-term rates over extended periods into the future, and who therefore dismiss as nonsense a theory which says the market makes long rates equal to an average of such expected short rates. Expectations theorists would make the following rejoinder. It is not required by the theory that investors consciously think in terms of present and future short-term rates. It is only necessary that the market observe when past movements in the term structure make it obviously absurd to have held one term instead of another. If the market only attempts to bring expected holding-period yields into some kind of conformity, it will be doing what the expectations hypothesis asserts. History makes clear that holding-period yields on longs and shorts have varied widely, but it is difficult to believe that there are not elements in the market attempting to take advantage of such potential discrepancies to the extent that their best forecasting ability permits. If this is true, then the expectations hypothesis provides a partial explanation of the term structure.

Admitting risk premiums to the expectations hypothesis destroys the view that securities of different term are perfect substitutes and provides instead the picture of a partly segmented market. The resulting implication that supply can alter the term structure is consistent with market observations and may make the combined theory more acceptable to financial practitioners than the unmodified expectations hypothesis.

At this point, it will be useful to note an alternate interpretation of admitting risk premiums to the expectations hypothesis. We have talked of risk premiums as if somehow "true" interest rates were those on shorts, and additional payments had to be made to holders of longs. It would be equally legitimate to assume that long rates provide a base, and that holders of shorts earn a "liquidity premium"—a non-pecuniary return—in addition to their pecuniary return. This terminology has the advantage of providing an easy bridge to Keynesian liquidity preference theory. Just as Keynes said that liquidity preference would give an advantage to cash over bonds (especially in times of low

rates), so Kessel expands this concept to say that liquidity preference provides an advantage to short-term securities over long-term, as a result of which holders demand a smaller pecuniary reward for holding shorts. Whereas we have suggested above that the hypothesis supported by Kessel's studies is a modified expectations hypothesis in which risk premiums are admitted, we may now express the same idea by stating that these studies indicate the need to marry the liquidity preference theory to the expectations hypothesis.

6. THE SPREAD BETWEEN NEW AND SEASONED YIELDS ON CORPORATES

The second study of linkage in markets is an examination of the yield difference between newly issued securities and seasoned issues of the same character. The size of this spread, as observed from published series such as those of Moody, seems to give this question importance both for investment policy and because of the problems it presents regarding the efficiency of the capital market. If new and seasoned issues are essentially identical it would seem that any difference between their yields implies imperfections in the capital market. Yet according to the published record the spread is great. In June 1957, it equaled 90 basis points according to Moody data. In every month from April 1956 through December 1957, the investor could have obtained a higher yield on new Aa's than he could have received on Seasoned A's, and his average gain by securing the higher-grade bond would have been 33.5 basis points. Less dramatic but sizable new-outstanding yield spreads appear to have persisted most of the time through 1960. The purpose of this study was partly to find whether the yield spread is genuine, and partly to explain the determinants of any part found to be real.

Our work is focused chiefly on the first decade after the Treasury–Federal Reserve Accord of 1951, whereby the pegging of long-term government rates was discontinued. The primary analysis was based on two series:

Moody's series of Aa corporates, and a series of Aa utilities compiled by Sidney Homer of Salomon Brothers and Hutzler. In each case we derived regressions in which the dependent variable was the spread in yield between new and seasoned issues. The regressions are essentially similar, including as independent variables the volume of recently issued securities and four change-in-yield variables covering different time periods during the year preceding the date of observation of the yield spread. The equations for the Moody series include also, as an independent variable, the difference between the average coupon of the new and the seasoned issues. The Homer series is corrected for coupon differences and therefore does not require such a variable. However, in the study of this series the bill rate was included as an independent variable.

In both equations each of the independent variables had a statistically significant influence on the yield spread and, together, they accounted for about 87 per cent of the variation in the Moody series and for about 92 per cent in the Homer series.

The most important implication of these regressions is that a very great deal of the published new-seasoned yield spread and its variation is essentially spurious. The apparent spread results to a large extent from the fact that published series of new and seasoned issues are not homogeneous with respect to coupon. High-coupon issues are less attractive than otherwise similar low-coupon issues, and consequently the market demands a higher yield on them. The most important reason for the greater attractiveness of low-coupon bonds is that high-coupon issues are more likely to be called than others; when they are called, this requires reinvestment of funds just when rates are relatively low.

When correction is made in the Moody series on seasoned issues in order to give yields of bonds with coupon similar to that of new issues, the average spread for the decade is reduced from 23.7 basis points to 13.7 basis points. The Homer series show even greater influence of coupon differences on the level of spread. The average spread between yields on new issues and yields on 2¾-2⅞ per cent utilities was 26.5 basis points for the decade, whereas the average spread where outstandings are corrected for coupon is only 9.4 basis points.

Although correction for coupon removes a significant part of the apparent spread between yields on new and seasoned issues, we still want to explain what remains. The following hypothesis was partly developed early in our study and then tested by the data; in part it grew out of modifications in our earlier thought, which the data forced upon us.

There are two major elements in our explanation of new-seasoned yield spreads. In the first place, since dealers are in the business of selling securities they wish to make new offerings attractive enough to sell promptly. This leads to "sweetening" the yield on new issues under many circumstances, especially when there is reason to fear that failure to clear shelves promptly may bring losses through falling security prices. One part of the explanation of yield spreads, then, should be found in an examination of the conditions that might lead dealers to want to encourage rapid security sales.

A second element is suggested by the question: Why does the market ever let a spread of this kind develop? So long as new issues are available, why would anybody buy an equivalent seasoned issue at a price that provides a lower yield? Put otherwise, why is the market price of outstandings not forced down to match yields available on new offerings?

With respect to the first of these elements (sweetening by dealers) it seems reasonable that rising yields in the immediate past might encourage the fear of further decline in security prices. Observation of the data strongly supports this hypothesis and, indeed, indicates that the direction of yield movements over a fairly extended past period also influences expectations. Experimentation resulted in the introduction of the four change-in-yield variables referred to above.

A second consideration which might be expected to influence the amount of sweetening expected from dealers is the volume of new

issues thrown on the market in the immediate past and current periods. The most influential feasible variable we found for volume was the two-month volume of all new corporate issues, including direct placements and equities.

Tightness of the money market, as indicated by the Treasury bill rate, appears to be a significant explanatory variable. Because of very high correlation with coupon difference between new and seasoned issues, this variable does not appear in the Moody equation, but it is statistically significant in the Homer equation. Two closely related reasons for this relation suggest themselves. One is that dealers may be concerned about financing their position in new securities when the money market is tight. A similar reason is that the cost of carry will be higher (or the gain on the carry less) under these conditions.

In all these respects our armchair hypothesis received confirmation from statistical tests.

We turn now to the second type of consideration that might cause yield spreads to exist. These spreads depend, we have indicated, upon the failure of the market yields on seasoned issues to move up rapidly to whatever yields are provided on new securities. We have sought without success for variables that might logically be expected to influence the degree of friction in the market, and that test out successfully with the data. But the extraordinarily great influence of one variable already included may be attributable in part to the presence of such a lag. If the various influences that determine interest rates operate directly on the new issue market, then new issue rates should respond promptly to such forces. If there is sluggishness in the market for seasoned issues, then a spread should be created when rates generally rise, and the spread should be much greater when rates rise rapidly. This has been shown to be so. The change in yield from the preceding month is by all odds the most powerful and the most significant variable in all the equations we have run (assuming securities homogeneous with respect to coupon). It is entirely possible that this variable performs double duty, influencing the sweetening by dealers, and reflecting the influence of lags in seasoned yields on the yield spread.

The preceding paragraph raises some important issues regarding the way the securities markets behave: Do the forces that determine interest rates operate primarily and directly on new issue rates, to which seasoned issues respond with a lag? Or can we think of the market for seasoned issues as the major market, where the determinants of interest rates operate directly, and then add that new issue yields will reflect these rates subject to the modification imposed by dealer desires to sweeten yields? We conducted a study of leads and lags which led us to the following conclusions: Market forces operate directly on the yields of both new and seasoned issues, though certainly more rapidly and probably more strongly on the former. Movements in new issue yields are a compromise between forces in the market for money and credit and forces leading to different degrees of sweetening by dealers. Movements in seasoned yields are a similar compromise between forces emanating from the market for money and credit and forces tending to eliminate the yield spread between new and outstanding issues.

One of the important questions of our entire interest-rate project is to discover how perfect are the markets for money and credit. The finding of this study is that the market is much more nearly perfect than would appear from observation of yield spreads uncorrected for coupon. Furthermore, the imperfection that remains after coupon correction is eliminated fairly soon. Our studies show that the yield spread between a new issue and similar outstandings tends to disappear within about three months from date of issue.

One final observation suggests general support for the kind of explanation of yield spreads given here. Data have been plotted for the period 1920-40, and spreads for those years follow a similar pattern. They were usually positive, especially in times of rising rates. But they fell to low or even negative values at the end of periods of sustained declines in interest rates.

7. FUTURE STUDY

Further research on interest rates is clearly in

order. Major contenders for study, in my view, include the following.

1. The mortgage market. With the aid of data collected by Guttentag and Beck it should be possible to learn more about the relationship between interest rates and other dimensions of credit availability. Also much remains to be learned about geographic differentials in interest rates on mortgages, both interregional and intraregional. The nonresidential mortgage market provides a third important area now largely unexplored, and one that may shed new light on the effects of interest rates on the level and nature of economic activity.

2. The term structure of rates. We should test other hypotheses besides those examined by Kessel for the present project, compare the outcomes, and attempt to demonstrate either the superiority of one theory over others or to construct an eclectic theory which draws upon valid elements of several approaches.

3. Cyclical movements. Following Cagan's work on the nature of cyclical movements in interest-rate series, we should examine cyclical movements of related series and discover what clues are suggested regarding the determination of interest rates. These hypotheses should then be tested by various empirical techniques.

4. Effects of interest rates. There is a growing conviction that research has gone far enough now to justify further intensive work on the effects of interest rates on economic activity, in contrast to the focus of our present study, which has been to find the determinants of interest rates and their movements. At various institutions, a number of such studies have been recently completed or are in process, but there is room for much further work on this knotty problem.

Many other topics than those listed above should be considered. Further work on direct placements could include default experience and the relation between commitments and take-downs. The relations between interest rates and return on equities present a difficult group of problems on which some economists are already at work, but further explorations here might be feasible. The role of interest rates in international economic relations, including especially capital movements, requires more extensive empirical examination than has yet been carried on. A description and explanation of secular movements in interest rates would be an important candidate for further study. Consideration might also be given to an investigation concentrating on the influence on interest rates and the capital market generally of new instruments, institutions, and knowledge developed in the period since World War II.

<div align="right">JOSEPH W. CONARD</div>

THE NATIONAL BUREAU'S STUDIES IN INTERNATIONAL ECONOMIC RELATIONS

Developments during the past several years demonstrate the relevance of our basic research to contemporary international economic problems. Consider first the relation between business cycles and foreign trade. Ever since it became clear several years ago that this country faced a balance-of-payments problem of serious dimensions, we have had the problem of judging how much of the deterioration—or at times how much of the improvement—in our position was due to cyclical or other short-run influences and how much to more funda-

mental factors. Some who failed to make this distinction tended to regard a bad situation as hopeless in early 1959 when our imports were moving up swiftly and our exports, after a steep fall from the 1957 peak, were still pointing down.

Contrariwise, the subsequent sharp recovery of our exports and of our export surplus produced exaggerated hopes, if allowance was not made for the temporary influence of the 1960-61 recession in curtailing our imports. Then, as we moved out of that recession, the opposite effect was produced once more as our imports rose by 11 per cent in 1962 while our exports, chiefly because of business conditions in Japan and the United Kingdom, rose by less than 3 per cent. It is one of the encouraging aspects of our balance-of-payments development last year that, with domestic business expansion continuing, our imports rose by only about 5 per cent while our exports moved up by 7 per cent. (Table II-4).

Clearly, therefore, any attempt to appraise trends in our foreign trade and their meaning for the balance of payments must take due account of cyclical influences at home and abroad. Thanks to Ilse Mintz's research, we should be better able to understand the nature and timing of these influences and to make appropriate allowance for them. In her work on the behavior of U.S. exports during business cycles she noted, for instance, that the latter part of our export decline from 1957 to 1959 had come during a period of "counterexpansion"—that is, business activity had turned up in the United States after the second quarter of 1958 but was still declining abroad. She further reported: "Our analysis discloses that U.S. exports have invariably declined during counterexpansions. The latest drop was one of the two mildest among six periods of this type."[1]

Mintz's note in Part IV of the present report will be helpful in judging the likely effects of the current business expansion on our foreign trade. If the expansion in domestic activity continues, imports will doubtless also continue to rise, probably at a faster rate than last year. But foreign demand affecting our exports also seems to be growing strongly. Herein may indeed lie the chief threat of the moment to the smooth development of our international trade and payments. In some countries expansion has taken on an inflationary character such as to require them to take countermeasures in the interest of price stability and the balance of payments. It remains to see whether they can execute this sometimes delicate operation without provoking a downturn in economic activity and without resort to measures directly aimed at curtailing imports.

Beyond these cyclical aspects, there is need to develop a better understanding of the forces affecting our international competitive position over the longer run. It is common knowledge that the share of world exports of manufactures supplied by the United States has shrunk from 26 per cent in 1953 to about 20 per cent last year. It is also well known that, in some key products, our prices have risen a good deal more than those of our leading competitors, or at least that they apparently did so until around 1959. In that year, the machinery and motive products component of our wholesale price index was 24 per cent higher than in 1953, and the metals and metal products component was 21 per cent higher. Other leading countries reported much smaller rates of increase for these groups. It is logical to suppose that the fall in our share of the export market for manufactures is related to the increase in our prices compared with those of other countries. More empirical research is needed, however, before we can judge the breadth and force of this influence on our international competitive position.

A major difficulty in this regard is that the information available for making price comparisons is so deficient in quantity and quality. This problem is given some attention in my volume, *Problems of the United States as World Trader and Banker*,[2] published a year ago. As observed there, one may particularly question

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[1]*American Exports During Business Cycles, 1879-1958*, Occasional Paper 76, NBER 1961, p. 5.

[2]Pp. 56-68.

TABLE II-4

U. S. Merchandise Exports and Imports, 1956-63

	Value ($ millions)		Percentage Change from Preceding Year	
	Exports	Imports	Exports	Imports
1956	17,379	12,804	+21.7	+11.1
1957	19,390	13,291	+11.6	+ 3.8
1958	16,264	12,952	− 16.1	− 2.6
1959	16,282	15,310	+ .1	+18.2
1960	19,459	14,723	+19.5	− 3.8
1961	19,915	14,514	+ 2.3	− 1.4
1962	20,479	16,145	+ 2.8	+11.2
1963	21,902	16,962	+ 7.0	+ 5.1

Note: The figures are given as adjusted for balance of payments purposes in the sources indicated.

Source: *Balance of Payments, Statistical Supplement* (revised ed.), Supplement to *Survey of Current Business*, 1963, pp. 3-4; *Survey of Current Business*, March 1964, page 16.

the reliance frequently placed on so-called "unit value" indexes of exports and imports. By their nature, these indexes tend to be most inadequate in their coverage of advanced manufactures—that is, the very products in which we are most interested for purposes of considering changes in competitive positions.

In this connection, Robert Marjolin, a vice-president of the European Economic Commission, in a recent address largely devoted to inflationary tendencies in the Common Market, limited his international comparisons to consumer prices and wage costs per unit of output.[3] The figures he cited are interesting enough to bear repetition.

Percentage Increase, 1959-63

	Wage Costs per Unit of Output in Industry	Consumer Prices
The Netherlands	17	10
Federal Republic of Germany	20	11
France	21	19
Italy	28	16

These figures, in contrast with the relative stability observed for the United States over the same period, led Marjolin to formulate a stern warning over the deterioration in the international competitive position of the Common Market countries. At the same time, use of these data is a reminder of how far we are from being able to generalize about relative prices, and changes in relative prices, of goods actually moving in international trade.

We expect to know more about the problems and the possibilities of making such international price comparisons as the result of the study now being conducted by Kravis, Lipsey, and Bourque. Their study covers a particularly difficult area (machinery and other metal products) and should be of value not only in providing new price information but also in developing methodology and in providing insights into the influence of price differences

[3]Address given on Jan. 21, 1964, to the European Parliament in Strasbourg, "The Economic Situation of the European Economic Community in 1963 and the Outlook for 1964."

on trade flows. The information needed for this study is being obtained directly from business and government organizations operating internationally. It reflects the experience of these organizations as sellers and, more frequently, as buyers. The investigators expect that, with the excellent cooperation they are receiving, they will be able to make both comprehensive and detailed place-to-place price comparisons that have never been possible before. They expect also to be able to construct more comparable and more relevant indexes of price changes through time than are now available. Finally, they hope to develop information on nonprice factors to aid in evaluating the influence of prices on trade.

Another empirical study focused on this country's international competitive position is Georgiadis' work on the performance of the United States in international trade. The basic purpose here is to go beyond the generalizations that can readily be made about the decline in the over-all share of the United States in world exports of manufactures and to see more specifically in what products and in what countries this decline has occurred and how the United States has fared in other areas of competition. It is hoped that his analysis will provide useful clues to the nature of the causal influences at work—e.g., whether they are general and pervasive or specific to certain products and markets.

Much of the detailed information on trade, production, and other variables needed for this study has been assembled and processed, and the analysis will soon begin to take shape. Meanwhile, the selection of data given in Table IV-17 of Georgiadis' report in Part IV provides food for thought. It will be seen that the U.S. loss in export market shares for manufactures from 1953-55 to 1960-61 was largely accounted for by Canada. This was the result of, first, a sharp relative decline in Canada's total imports of virtually all items compared with the imports of other leading foreign countries and, second, some loss in the share of Canada's imports supplied by the United States. A contrary development may be seen in Germany and some other countries with the notable exception of their imports of automobiles and steel. The stronger performance of our exports to overseas markets was, however, far from sufficient to offset the adverse developments in our major Canadian market.

Georgiadis tentatively suggests that there may be a connection between the large size of our share in the Canadian market for most manufactures and the general tendency for that share to decline and, similarly, a connection between the much smaller size of our shares in most other countries and the fairly widespread tendency for those shares to rise. However that may be, even his preliminary results do suggest the need to consider not only price changes but also other factors influencing our share of world markets.

Another illustration of the qualifications I have in mind concerning the role of price changes is given by my brief note in Part IV concerning the Occasional Paper in preparation on imports of manufactures by the United States and Western Europe from less developed countries and Japan. A point of major interest is the great contrast between the United States and Continental Western Europe (the United Kingdom falling somewhere between in terms of these comparisons). Taking labor-oriented manufactures other than food, we find that United States imports in 1962 from less developed countries and Japan totaled close to $1.7 billion, or more than three times as much as Western Continental Europe imported from these sources. In our case, these imports made up about 30 per cent of our total imports of the products in question in 1962. For most European countries other than the United Kingdom, the corresponding percentage was only some 1.5 to 3 per cent, with a high of 4.5 per cent for West Germany and a low of 1.2 per cent for Austria.

It is possible that these striking differences are attributable to relative price changes during the period. This would mean that the rise in costs and prices was great enough in the United States, but not in Continental Europe, to provide large market opportunities for imports of manufactures from the less developed countries and from Japan. I suspect, however,

that basic cost relations were already favorable to the development of this trade with the United States, and that the important new factor has been the imaginative and energetic organization of production, frequently at the initiative of, or in close collaboration with, American importers. I suspect also that, in Continental European countries, it is not so much smaller price differences as greater import impediments of one kind or another that explain the failure of their imports from the less developed countries and Japan to become more important. These impediments include not only tariffs and quotas at the official level but also restrictive business practices and attitudes. If such imports had developed, they would have helped to relieve inflationary pressures in European countries both by the direct effect on prices and by the release of scarce manpower to more productive employment. They would have contributed, at the same time, to economic growth in some of the less privileged parts of the world.

The importance of being able to examine not only merchandise trade but also other international-payments relations in their regional aspects is stressed in Herbert B. Woolley's study, "Measuring Transactions Between World Areas," which we expect to send to press soon. His regionally elaborated balance-of-payments statistics for 1950-54 will help other economists to form a judgment as to the utility of this kind of information and also as to the feasibility of regularly preparing such a record by integrating, reconciling, and supplementing the separate balance-of-payments estimates of individual countries. Walther Michael's more detailed work on statistics of international capital movements between major world areas will also help in weighing the usefulness and the practicability of compiling such information.

In regard to this question it would seem that, in principle, our interest in geographic detail should not stop with merchandise trade but should extend to all items in the balance of payments, if we are to have a more adequate statistical basis for analyzing international economic and financial relations. These relations

seem likely to concern us more in the future than in past years, to judge from our recent experience and current preoccupations.

Let me illustrate by reference to the regional balance-of-payments data for the United States cited by Woolley in his last chapter. On the basis of these figures, he notes that this country's direct bilateral balance on recorded transactions of all kinds with Western Europe shifted from a deficit of $1.2 billion in 1950-54 to a surplus of $900 million in 1961-62 (annual averages). Despite this shift, Western Europe continued to acquire gold and liquid dollar funds from the United States at an annual rate of more than $1 billion. At the same time, the United States has been making net payments on recorded transactions with non-European countries far greater than can be accounted for by their acquisitions of gold and liquid dollars from us. We are thus able to conclude, on the basis of United States data alone, that there must have developed in recent years large net transfers of dollars to Western Europe from other areas. But we cannot explain the origins or nature of these dollar transfers in the existing state of knowledge about international transactions between foreign areas. It is impossible to say, for instance, to what extent the transfers are due to increases in payments by third countries to Western Europe for goods and services or to increases in capital movements or, if the latter, how much arises from debt repayment and how much from the flight of capital from Latin America, the Far East, and other areas. I scarcely need belabor the importance of such information for the correct appraisal of problems and the development of appropriate policies.

This experience suggests a need for the kind of regional payments accounts developed by Woolley, though views will differ on the best way of consolidating the data by areas and by types of transactions and on other methodological points. Views will also differ concerning the practicability of preparing such regional accounts as a recurrent statistical operation. For one thing, even if organized on a regular basis, the preparation of such information is

likely to be costly in terms of skilled manpower required, and the results could be available only with a delay determined by the most laggard among the reporting countries. Another difficulty is that, at best, the consolidated estimates will still be subject to the famous "errors and omissions" which beset national balance-of-payments statisticians. For this reason, and perhaps also on conceptual grounds, there may be questions concerning the significance of Woolley's residual calculation of net multilateral settlements.

It may be that these or other problems will be considered too serious to permit the systematic elaboration of payments accounts between major areas in the manner attempted by Woolley. If so, we hope the difficulties will, by the same token, increase interest in the short-cut methods with which he has experimented for computing goods and services transactions, both net and gross, in regional detail. Subject to further experimentation and testing, we have here a methodology which could, with a minimum of cost and time, add to the flow of current information on international trade and payments relations. Though limited to goods and services, this information would be an advance over what we now have and would furnish a basis to which efforts being made elsewhere to improve information on international capital flows could be related.

Woolley's work will, we believe, help others to decide whether continuing work should be organized along either the broader or the narrower lines which he has explored. The National Bureau does not, of course, undertake such recurrent statistical operations, and the publication of Woolley's results and, subsequently, of Michael's supporting study will conclude this investigation. Several exploratory investigations of other topics are under way. Some of these may be developed into full-fledged research projects. The summary analysis of imports of manufactures from the less developed countries and Japan, to which I have already referred, will be developed into a short occasional paper. It should cast light into some dark corners and also enable us to decide whether to undertake further work on the ob-

stacles to this trade and its potentialities.

Another topic we are considering is the relation between technology and international trade. It has often been suggested that a reduction of our technological lead over other countries has contributed importantly to our international payments difficulties of recent years. We should like to subject this hypothesis to empirical test. It is, however, an area in which facts are particularly elusive, and it remains to be seen if a promising approach can be developed.

Still another subject for investigation concerns the causal forces determining international capital movements—a subject of great topical interest, because of the sharp differences of views which have developed about it and because of its bearing on monetary policy. The rise in the rate of U.S. private investment abroad has been an outstanding feature of our balance of payments during the past decade. In the first half of the 1950's that flow, including both short-term and long-term U.S. private capital net of repayments, averaged just over $1 billion annually. It then rose to more than $3 billion in 1956 and remained generally within the range of $3 to $4 billion through 1962. In the first half of 1963 these investments, especially in the form of new foreign security flotations, seemed to take another big jump, rising from $900 million in the first quarter (seasonally adjusted) to over $1.7 billion in the second—i.e., to an annual rate of close to $7 billion. Therewith, the balance-of-payments deficit for the second quarter moved up abruptly to a rate of more than $5 billion. The quarterly estimates frequently show wide variations, of course, and there were some distinctly temporary elements in the second quarter, especially in Canadian borrowings. But, according to the United States authorities, there was also clear indication of further increases in European security flotations in this market.

To deal with this difficulty, the Administration proposed, it will be recalled, an interest-equalization tax which, though not yet passed by Congress, seems to have had the effect of greatly reducing the flow of U.S. capital into

foreign securities since the middle of last year. Some think, indeed, that the measure is more effective while still pending, and while the legal situation is not yet clear, than it will be when and if enacted.

If the Administration's proposal becomes law, its effects will be studied for any light they may cast on the relation between interest rates and international capital flows. The proposal itself implies a view that relative rates of interest—or relative rates of net return on capital in any form—are important in this regard and hence that, if capital outflows are not to become excessive, rates here need to be kept at an appropriate adjustment to rates in other financial centers. This view has been questioned on the ground that little correlation can be found in the available data on interest rates and capital flows. These data relate in the main to rather recent United States experience, and they need to be supplemented in various ways and, in any case, studied more carefully. It may not be possible to reach firm conclusions even when that is done, but the questions involved—the magnitude, fluctuations, direction, and determinants of international capital flows, and the policies to deal with them—are surely among the most important for study in relation to both national and international financial policy.

<div align="right">HAL B. LARY</div>

BUSINESS CYCLES IN PERSPECTIVE

<div align="center">I</div>

The distinction between the short run and the long run is not new to economists, but it may be well to remind ourselves and others of it occasionally. Particularly is it necessary when the daily economic news deals almost exclusively with the very short run. Current statistics reported in the press compare last week's retail sales with that of the week before or with the same week a year ago. Industrial production is headlined as advancing if the latest month's figure exceeds the previous month's. Not only do the figures pertain to very short intervals of time—which they must do if they are to be "current" and therefore newsworthy —but the comparisons also generally relate to rather short spans, a week, a month, or a year.

For the purpose of analyzing business cycles historically and keeping one's appraisal of the business cycle up to date, short time units and short time spans are essential. Such appraisals are crucial for many decisions. The four business recessions in the United States since World War II each lasted about a year, or less. Inventories accumulated at the peak of the cycle may prove seriously burdensome during the ensuing twelve months. Prompt and accurate appraisal of short-run shifts in demand may make the difference between success and failure in buying materials, scheduling production, operating on overtime, raising working capital, and so on. Similarly, on the level of national economic policy there are numerous decisions and actions of a temporary, reversible sort that require a minimum of lag between the recording of an event and the reaction to it. While promptly available quarterly data will sometimes suffice for this purpose, monthly data are usually necessary and, at critical junctures of the business cycle, weekly data can be helpful. It would be unthinkable to base monetary

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policy, for example, on monetary statistics that become available only once a year, say, and referred to the year as a whole.

On the other hand, there are decisions of a longer-run and more nearly irreversible kind that require a different view of the past and the future. A commitment to build a new plant, to erect an office building or apartment house, or to float a bond issue depends heavily upon an appraisal of prospects for growth. Decisions of this type must be justified in terms of the long-run developments in demand, competitive position, and profits that will either affect or be affected by them. The short-run outlook is relevant, however, to the *timing* of such decisions. The bond market may be temporarily unfavorable, or cost conditions may dictate postponement of a modernization project. For small business firms, appropriate timing may make the difference between a profitable outcome and bankruptcy. Yet even in small firms the pros and cons of major investment decisions must be weighed in terms of long-run prospects.

II

All of us tend to become specialists, and to emphasize our specialties. I should like to illustrate, with some simple charts, how some of our business cycle indicators can be viewed in a longer-run perspective. The function of these series that has been emphasized in our studies of indicators is to throw light on short-run, cyclical developments in the economy. But the same materials can be used in other ways, and for some purposes it is important to do so, as already noted.

The first set of charts, II-6 to II-10, illustrates the effect of lengthening the time unit in which the data are expressed. In quarterly data (Chart II-6), and even more in monthly data (not shown), the business cycle is plainly evident. The mild recessions during the middle twenties and since World War II, the severe though brief contractions of 1920-21 and 1937-38, and the prolonged contraction and recovery of 1929-37—all this varied cyclical experience during the past four and a half
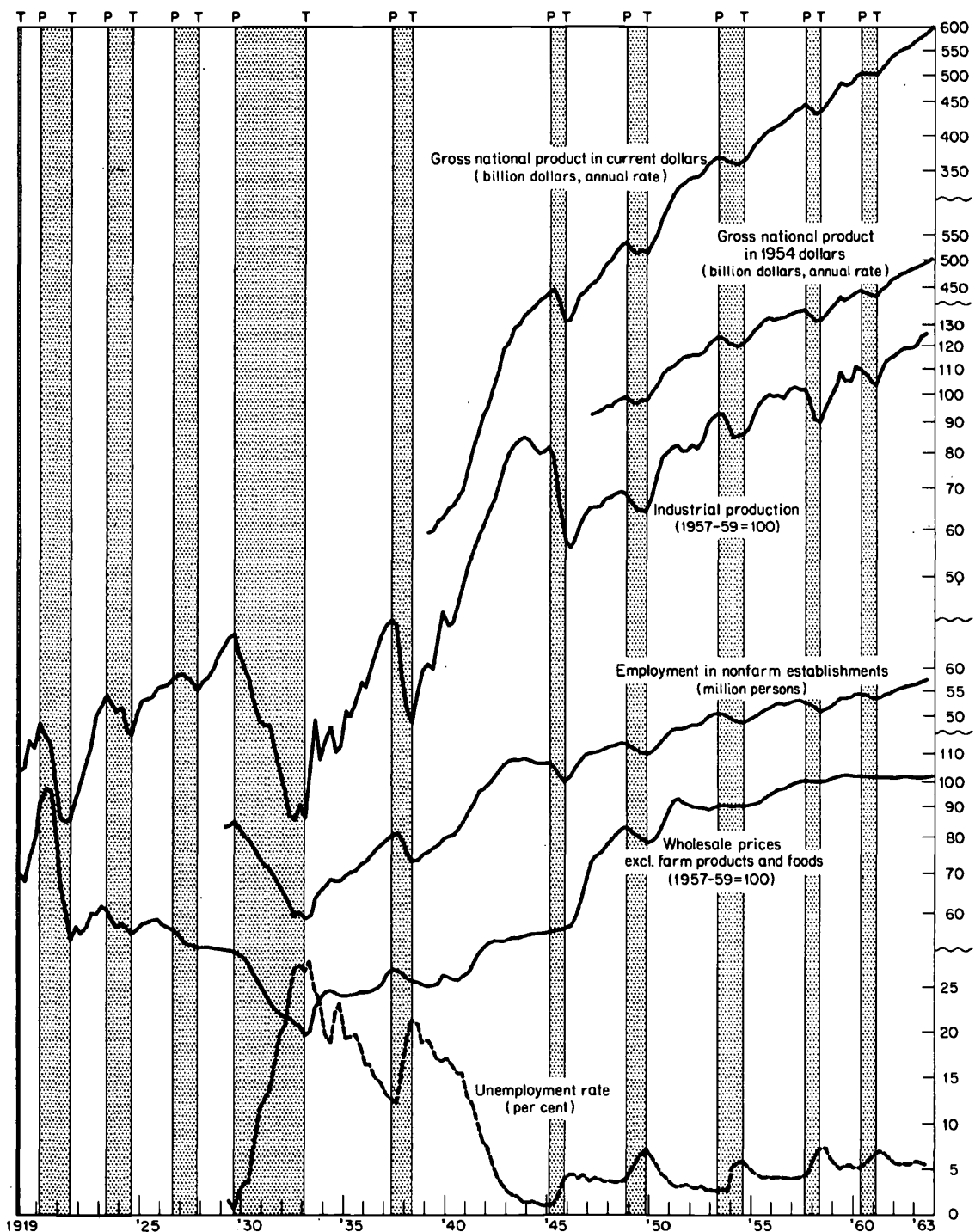
decades is traced out faithfully in the quarterly record. When the quarters are consolidated into years (Chart II-7), the cyclical picture is blurred but not suppressed. In terms of gross national product the mild recessions often appear simply as a retardation or cessation of growth in the annual data. Industrial production, nonfarm employment, and unemployment are more sensitive. Here, even the mild recessions are marked by declines in production and employment and by increases in the unemployment rate.

When the view encompasses three years at a time, or five years, or ten years, however, the business cycle virtually disappears (Charts II-8, II-9, II-10). Only the Great Depression, which affected the economy for a decade, remains. Every other recession is wiped out (with the exception, in the three-year figures alone, of the 1945-46 reconversion period). What remains in the charts is a representation of the long-run growth in the nation's output, the upward sweep in the number of persons employed in producing that output, the relatively steady proportion of the labor force that is unemployed, and the great deflation of prices in the twenties and thirties and the subsequent inflation.

It should not be supposed, of course, that this means that those whose policies or decisions depend upon the prospects for growth or inflation can ignore the business cycle. The business cycle, as I remarked above, may be a critical factor in the timing of such policies or decisions. Furthermore, no one can offer an absolute guarantee that a severe depression, with effects lasting several years, will not occur again, although much has been done to reduce the probability of such an occurrence. Federal insurance of bank deposits has reduced this probability, as the monetary studies by Friendman, Schwartz, and Cagan have shown. Unemployment insurance has reduced the probability, as Creamer's study of personal incomes showed some years ago and as Philip Klein's recent work has confirmed. Federal mortgage loan guarantee and insurance has reduced it, as the investigation by Saulnier, Jacoby, and Halcrow indicated. The regulation of securities
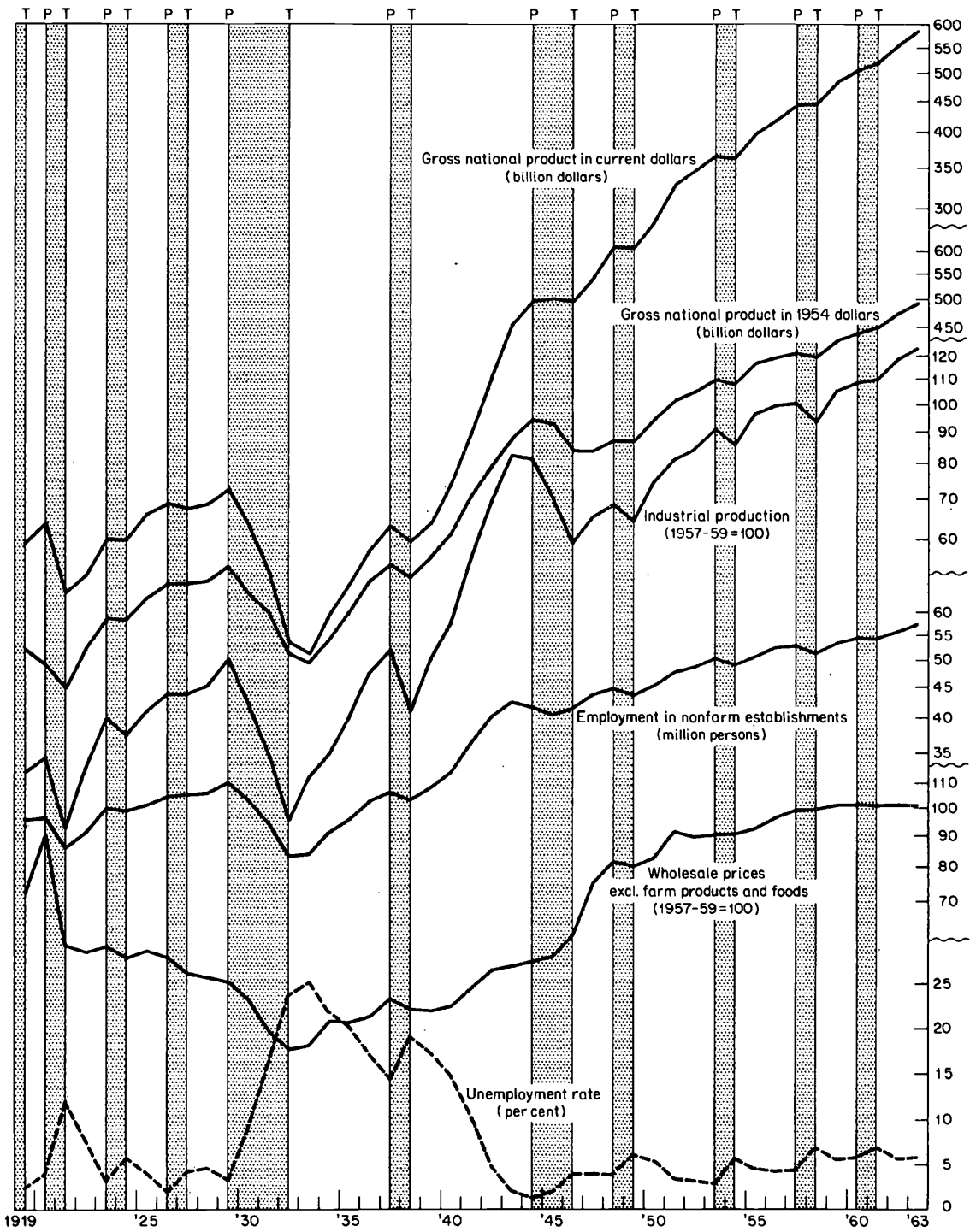
CHART II-6

Business Cycles in Perspective, Quarterly Data

Gross national product in current dollars
(billion dollars, annual rate)

Gross national product
in 1954 dollars
(billion dollars, annual rate)

Industrial production
(1957-59=100)

Employment in nonfarm establishments
(million persons)

Wholesale prices
excl. farm products and foods
(1957-59=100)

Unemployment rate
(per cent)

Shaded areas represent business cycle contractions; unshaded areas, expansions.
All series are plotted on ratio scale except unemployment rate.

CHART II-7

Business Cycles in Perspective, Annual Data

Gross national product in current dollars
(billion dollars)

Gross national product in 1954 dollars
(billion dollars)

Industrial production
(1957-59=100)

Employment in nonfarm establishments
(million persons)

Wholesale prices
excl. farm products and foods
(1957-59=100)

Unemployment rate
(per cent)

Shaded areas represent business cycle contractions; unshaded areas, expansions.
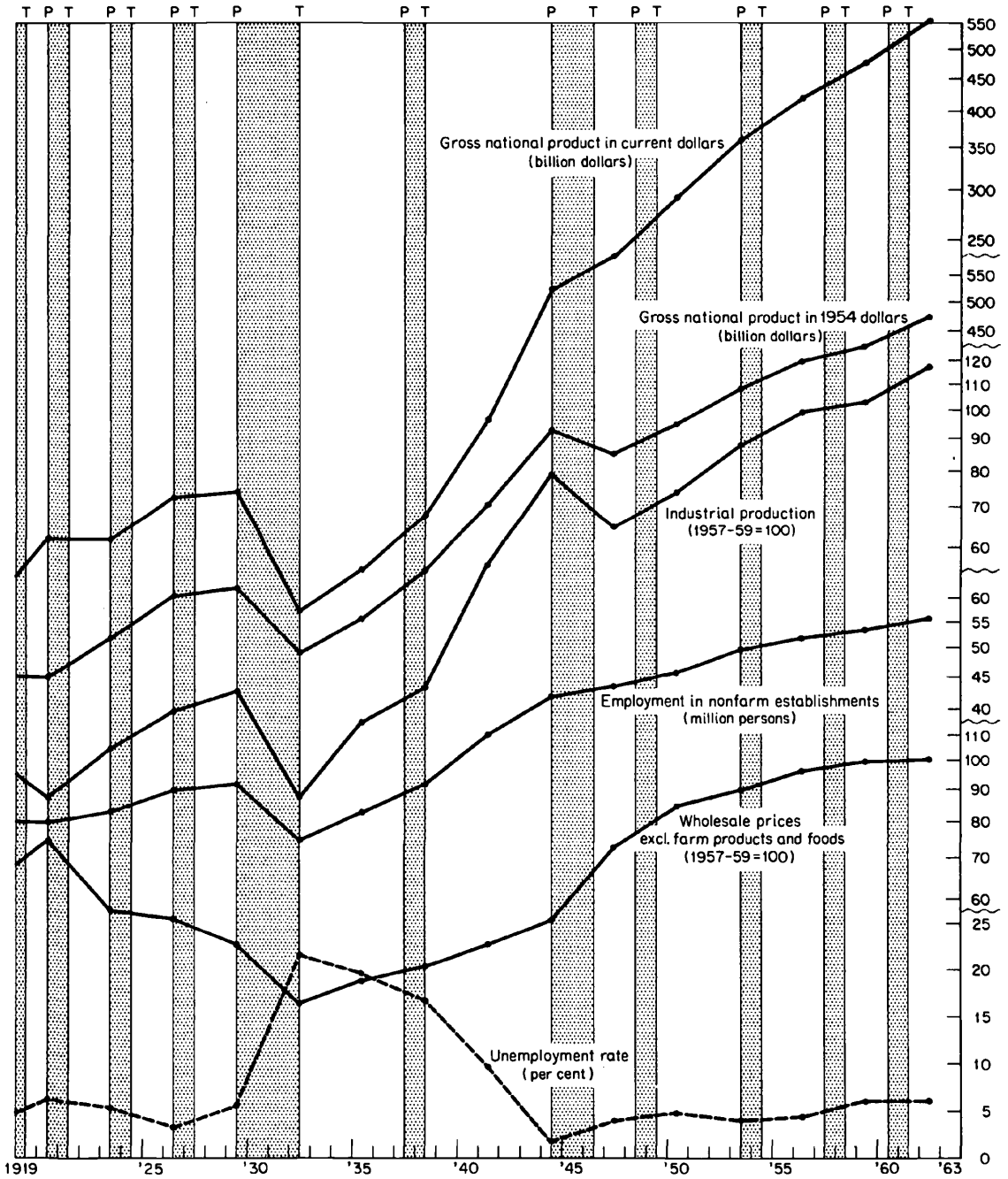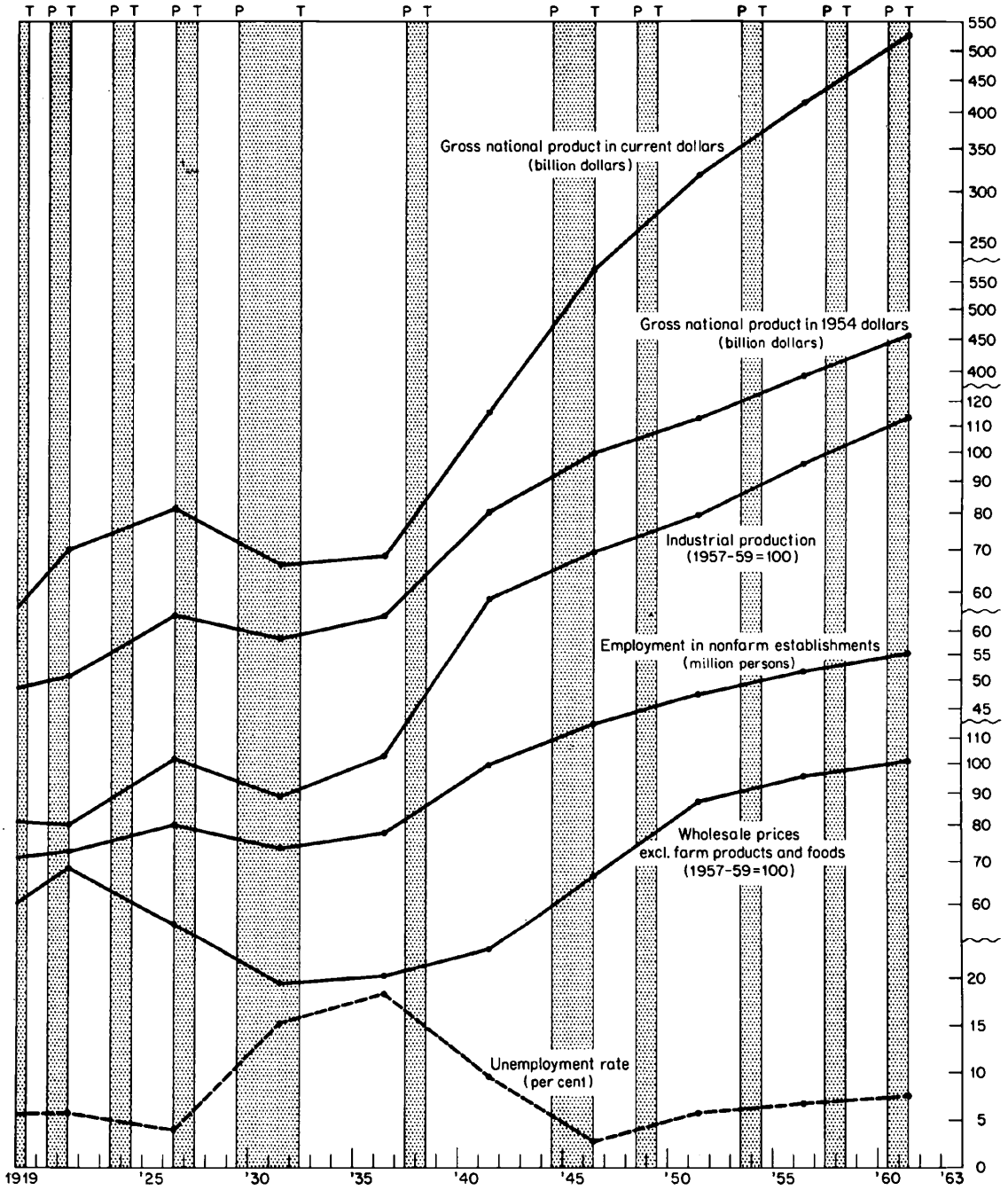All series are plotted on ratio scale except unemployment rate.

CHART II-8

Business Cycles in Perspective, Three-Year Averages (centered)

Shaded areas represent business cycle contractions; unshaded areas, expansions.
All series are plotted on ratio scale except unemployment rate.

CHART II-9

Business Cycles in Perspective, Five-Year Averages (centered)

Gross national product in current dollars
(billion dollars)

Gross national product in 1954 dollars
(billion dollars)

Industrial production
(1957-59=100)

Employment in nonfarm establishments
(million persons)

Wholesale prices
excl. farm products and foods
(1957-59=100)

Unemployment rate
(per cent)

Shaded areas represent business cycle contractions; unshaded areas, expansions.
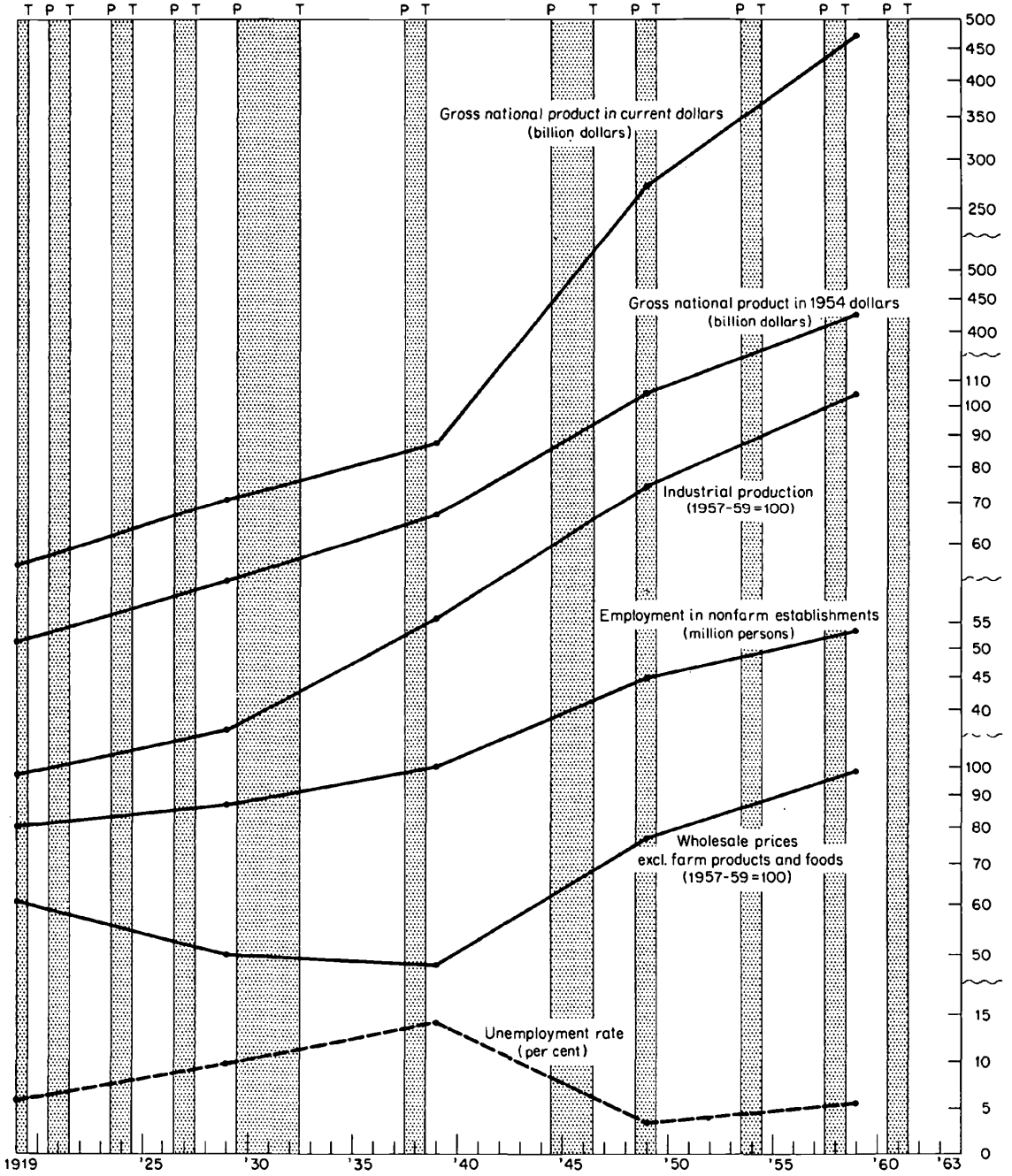All series are plotted on ratio scale except unemployment rate.

CHART II-10

Business Cycles in Perspective, Ten-Year Averages (centered)

T P T   P T   P T   P   T   P T   P   T   P T   P T   P T   P T

Gross national product in current dollars
(billion dollars)

Gross national product in 1954 dollars
(billion dollars)

Industrial production
(1957–59=100)

Employment in nonfarm establishments
(million persons)

Wholesale prices
excl. farm products and foods
(1957–59=100)

Unemployment rate
(per cent)

500
450
400
350
300
250

500
450
400

110
100
90
80
70
60

55
50
45
40

100
90
80
70
60
50

15
10
5
0

1919   '25   '30   '35   '40   '45   '50   '55   '60 '63

Shaded areas represent business cycle contractions; unshaded areas, expansions.
All series are plotted on ratio scale except unemployment rate.

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and commodities markets, insurance of savings and loan accounts, farm price supports, the graduated income tax, and a host of other developments have tended to reduce the chance of severe depression. The general appraisal which Arthur Burns offered a few years ago indicated that this country had definitely made progress toward economic stability, as he put it.[1]

Government policy, though, remains in the hands of men. Their policy goals may conflict, they may not be able to determine what action will produce the desired effect, and action itself may be long delayed. Consider the history of the recently enacted tax cut legislation. Tax reduction conflicted with tax reform and with the goal of a balanced budget; the nation's ability to produce *without* the tax cut in 1963 proved stronger than it had been estimated to be *with* the tax cut; and action recommended in January 1963 (indeed, considered seriously in the summer of 1962) was taken only in March 1964.

Since 1938, we have enjoyed the longest period in our history without a severe business contraction. This encouraging record is owing in no small part to the structural changes noted above. But the experience does not make certain that we have the means to prevent the occurrence of a speculative boom, or that we could prevent its collapse with the attendant repercussions on confidence. It is no more than prudent, therefore, to keep an eye on the business cycle, even when one's primary concern is with the long pull. The important thing is to avoid becoming so concerned with every wiggle in the curve that the perspective of the longer drift is lost.

III

The second set of charts, II-11 and II-12, makes the same point as the first set but in a different way. Here the time unit in which the data are recorded remains the same, but the

rates of change in the data are computed over longer and longer spans.

Chart II-11 shows annual rates of change in the monthly index of industrial production, first from one month to the next, then over spans of 3 months, 6 months, and 12 months. The bottom line is the familiar comparison of the current month with the same month a year ago. What the chart demonstrates is that over short spans of a month or so there are great irregularities, even in an index as broadly based as the production index. Then, as the span increases, the business cycle emerges more clearly.

Chart II-12 takes this procedure a step further, using annual data on gross national product. Year-to-year rates of change are shown at the top, then rates of change over spans of 2, 3, 5, and 10 years. Since all the rates are expressed as annual rates, they all run at about the same average level, roughly 3 or 4 per cent per year. But the variation around that level is very different, diminishing sharply as the span increases. Rates of growth over 5- or 10-year spans remain relatively steady for many years at a stretch; over shorter spans they are far more variable. The short-span rates are, of course, importantly affected by the business cycle; the long-span rates are dominated by growth.

Thus we see that the span over which change is measured may be too short to reveal the business cycle clearly, and that spans long enough to accomplish it may be too short to disclose growth trends. An appropriate choice of span is therefore essential, for the choice can vitally affect the results. If growth is the problem, spans must be long enough to bridge the business cycle.

IV

The charts illustrate two ways in which the growth trends in "business cycle indicators" can be revealed: first, by enlarging the time unit in which the data are expressed, and next, by increasing the span over which change is measured. There are, of course, other ways to

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[1]*American Economic Review,* March 1960.

CHART II-11

Industrial Production,
Percentage Rates of Change per Year Based on Monthly Data, 1947-63
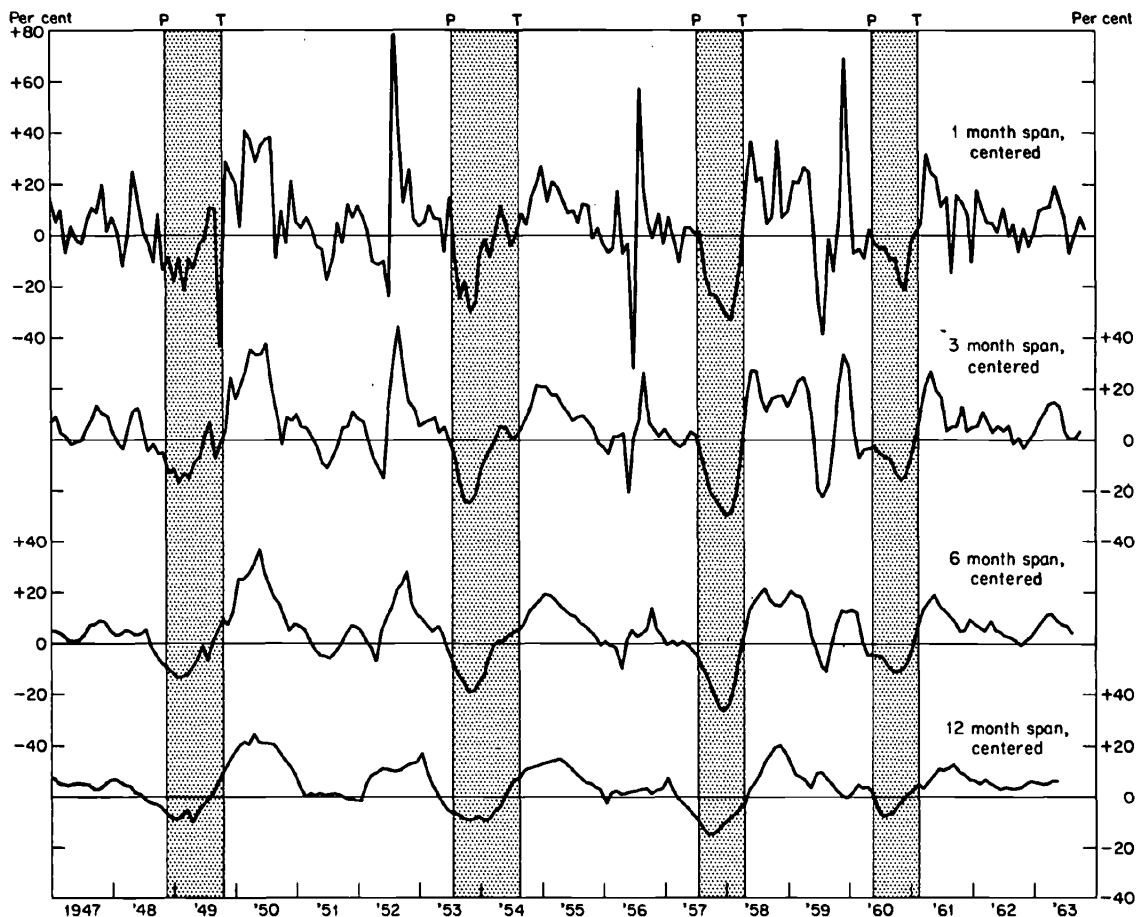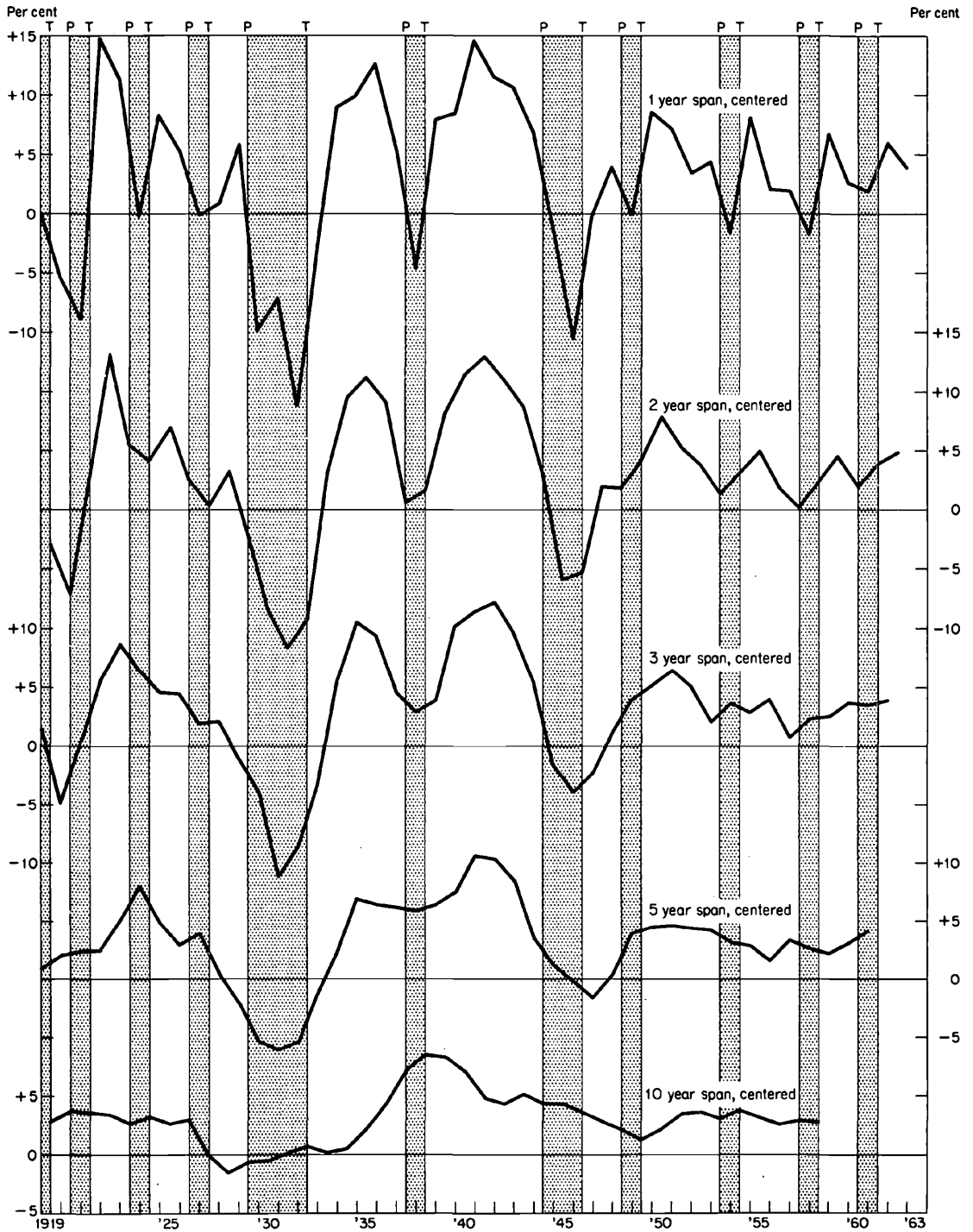

Shaded areas represent business cycle contractions; unshaded areas, expansions.

CHART II-12

**Gross National Product in Constant (1954) Dollars,
Percentage Rates of Change per Year Based on Annual Data**

Shaded areas represent business cycle contractions; unshaded areas, expansions.

accomplish this result. Mathematical trend lines can be fitted, data can be averaged over the period covered by each successive business cycle, rates of change can be calculated from the peak of one cycle to the peak of the next, or the effects of cyclical fluctuations in the utilization of resources can be eliminated by regression procedures. These various techniques have different merits and deficiencies. But my illustrations are sufficient, I think, to call attention to the importance of keeping long-run trends in view, and to demonstrate some simple ways of doing so.

The trends revealed are important in their own right, and they are important for their effects on the nature of business cycles. They depict a type of economic movement which persists over periods much longer than most business cycles, and they indicate that business cycles, too, are subject to persistent changes. I need not elaborate. Many of the National Bureau's studies reviewed in this year's report are making interesting contributions to an understanding of these trends. For example, the new study of the service industries is turning our attention to a sector that has both more growth potential and less cyclical instability than most. The study of consumer finance is revealing some little-known facts about another "growth industry," one that has been altering the nature of capital formation in this country by putting a larger share of it into the hands of the consumer, with far-reaching effects. The study of labor force trends is developing information on the factors that make for long-run shifts in the size and character of the working population, dimensions basic to any analysis of the nation's capacity to maintain full employment. These are but a few of the investigations that, while focusing on trends, have a bearing on business cycles. In addition, a number of studies reported on below are concerned directly with business cycle phenomena. With the aid of all these investigations, we shall be better equipped to keep business cycles in perspective.

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