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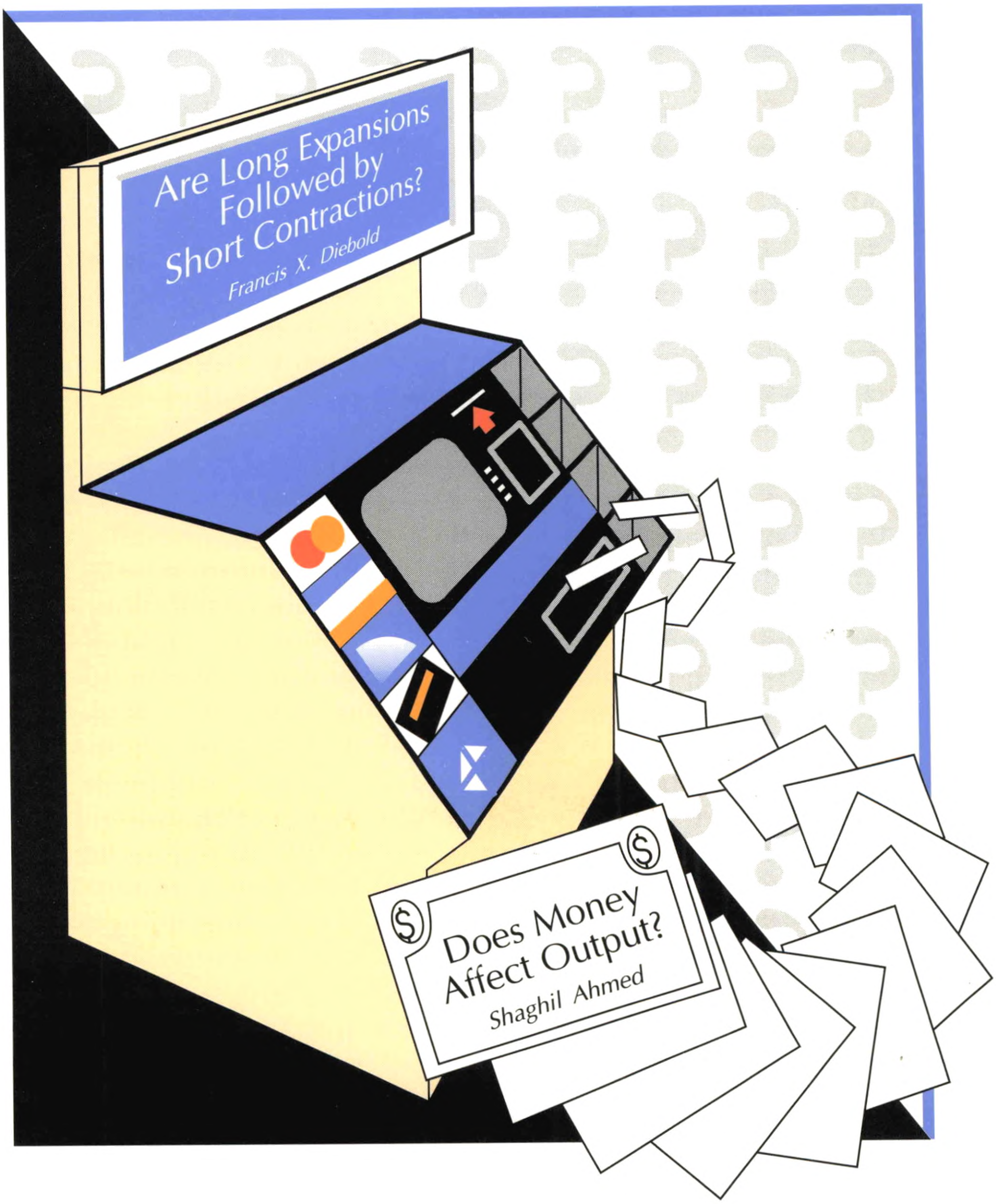

Business Review

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JULY/AUGUST 1993

Are Long Expansions Followed by Short Contractions?

*Francis X. Diebold**

The great expansion of the 1980s is the longest peacetime expansion on record. Does knowing how long an economy grows during an expansion reveal anything about how long the following contraction is going to last? More generally, is there any relationship between the lengths of neighboring expansions and contractions?

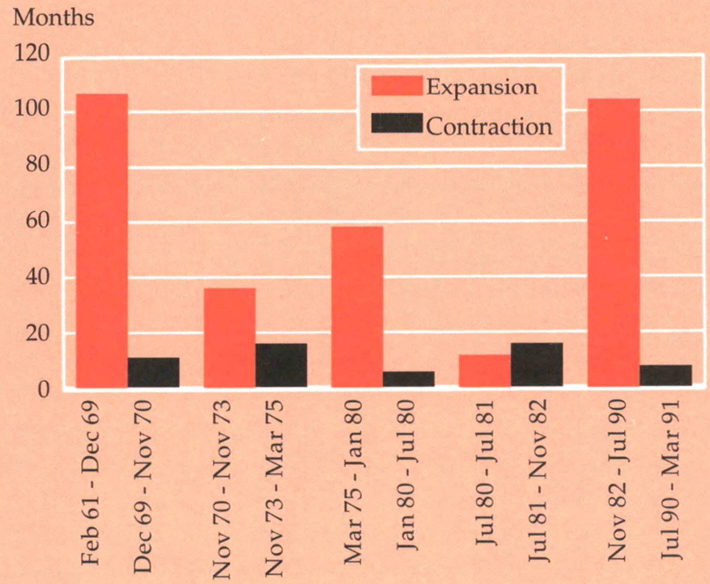
Economists have occasionally asserted the existence of such relationships. As Arnold Zellner of the University of Chicago noted in a research paper published in 1990:[1]

> *". . . in much of the literature . . . the hypothesis is made, implicitly or explicitly, that there exists some relationship between what occurs in the expansion phase of a business cycle and what happens in the following contraction phase. . . The hypothesis tentatively offered in explanation of this negative relation is that long expansion phases may be indicative of strong growth forces at work in the economy. The influences of these trend forces may persist in such a way as to allow the adjustments of the contraction phase to take place in a shorter time than if the growth forces had not been operating or were operating with diminished efficiency." (p. 1).*

* Diebold is associate professor of economics at the University of Pennsylvania. When this article was written, he was a visiting scholar in the Research Department of the Philadelphia Fed. He gratefully acknowledges support from the National Science Foundation, the Sloan Foundation, and the University of Pennsylvania Research Foundation. He thanks Tom Stark for outstanding research assistance.

[1]Complete references to papers cited in this article may be found in the "References" section.

FIGURE 1
Lengths of Last Five Expansions and Contractions



Using data from 1854 to shortly after World War II, Zellner found evidence that long expansions tend to be followed by short contractions. Based upon statistical analysis, he argued that for each additional month that the economy expands, we can expect a reduction of one-half month in the length of the following contraction. The economy has changed significantly since World War II, however, so we'd like to see if Zellner's findings are still applicable to today's economy.

We know, as a byproduct of recent research examining business-cycle lengths, that expansion lengths are approximately unrelated to lengths of previous and subsequent expansions.[2] Similarly, contraction lengths are approximately unrelated to the lengths of previous and subsequent contractions. However, this recent work doesn't look at the relationship between the length of an expansion and the length of the contraction that follows, leaving open the possibility that the length of a contraction *does* depend on the length of the previous expansion, as suggested above.

Simple graphical analysis seems to indicate that the relationship has remained intact over the last five business cycles (Figure 1). The long expansion (106 months) of the 1960s was followed by a short contraction of only 11 months. The shorter expansion (36 months) in the early 1970s was followed by a somewhat longer contraction of 16 months. The next expansion in the late 1970s (58 months) was longer, and the following contraction was shorter (6 months). Then there was a very short expansion from July 1980 to July 1981 (12 months) followed by a long contraction (16 months). Finally, the great expansion of November 1982 to July 1990 was followed by a brief contraction that ended in March 1991. However, we don't want to rely too heavily on casual evidence gleaned from graphical analysis. Therefore, in the remainder of this article, we provide a replication of Zellner's prewar results, examine their validity in the postwar period, and provide a somewhat critical assessment of the overall methodology.

[2]See the articles by Diebold and Rudebusch (1990, 1991, 1992) and Diebold et al. (forthcoming).

BUSINESS CYCLES BEFORE WORLD WAR II

The National Bureau of Economic Research (NBER), a nonprofit, nongovernmental research institute, determines when business cycles begin and end in the United States. A business cycle is defined as beginning when an expansion begins and ending (after a contraction) when the next expansion begins. The NBER has established a list of dates of when business cycles began and ended; the list is called a business-cycle chronology (Table). An expansion begins when business activity has bottomed out and is beginning to rise; a contraction begins at the peak of the business cycle, when business activity starts declining.

Zellner used the prewar NBER business-cycle chronology from December 1854 to October 1949 (the first 23 cycles in the Table) in his research.[3] Zellner tested the data and used statistical techniques that related the length of a contraction to the length of the preceding expansion.[4] Following

[3] I use the terms "prewar" and "postwar" rather loosely; the last contraction used by Zellner in fact ends in 1949, but I will refer to this as a prewar contraction.

[4] Zellner used data for the U.S. and Great Britain; in this article, I focus only on the U.S. data.

TABLE
The NBER Business-Cycle Chronology

Cycle Number	Expansion Begins	Length (months)	Contraction Begins	Length (months)
PREWAR				
1	Dec. 1854	30	June 1857	18
2 *	Dec. 1858	22	Oct. 1860	8
3 *	June 1861	46	Apr. 1865	32
4	Dec. 1867	18	June 1869	18
5 *	Dec. 1870	34	Oct. 1873	65
6 *	Mar. 1879	36	Mar. 1882	38
7	May 1885	22	Mar. 1887	13
8	Apr. 1888	27	July 1890	10
9	May 1891	20	Jan. 1893	17
10	June 1894	18	Dec. 1895	18
11	June 1897	24	June 1899	18
12	Dec. 1900	21	Sep. 1902	23
13	Aug. 1904	33	May 1907	13
14	June 1908	19	Jan. 1910	24
15	Jan. 1912	12	Jan. 1913	23
16	Dec. 1914	44	Aug. 1918	7
17 *	Mar. 1919	10	Jan. 1920	18
18	July 1921	22	May 1923	14
19	July 1924	27	Oct. 1926	13
20 *	Nov. 1927	21	Aug. 1929	43
21 *	Mar. 1933	50	May 1937	13
22 *	June 1938	80	Feb. 1945	8
23	Oct. 1945	37	Nov. 1948	11
POSTWAR				
24	Oct. 1949	45	July 1953	10
25	May 1954	39	Aug. 1957	8
26	Apr. 1958	24	Apr. 1960	10
27 *	Feb. 1961	106	Dec. 1969	11
28	Nov. 1970	36	Nov. 1973	16
29	Mar. 1975	58	Jan. 1980	6
30	July 1980	12	July 1981	16
31**	Nov. 1982	104	July 1990	8
32**	Mar. 1991			

Note: An asterisk (*) indicates that the cycle is a "major" cycle. Double asterisks (**) on the last two cycles indicate that we have yet to determine whether these cycles are major or minor.

the lead of early economists like Hansen (1951) and Gordon (1952), Zellner classified cycles as "major" or "minor," based on both duration and amplitude. Minor cycles are of shorter duration and smaller amplitude.

Looking just at minor cycles, Zellner argued that, on average, an additional month of expansion tends to be associated with roughly a half-month reduction in the duration of the following contraction. Frequently, major cycles contain wars. The economy behaves differently in wartime than in peacetime, so I focus on minor cycles. Using data that correspond roughly to the data used by Zellner, but which have been revised somewhat, I was able to replicate Zellner's results closely.[5] The replication shows that, for minor cycles, an additional month of expansion is associated with a half-month shorter contraction, as did Zellner's study.

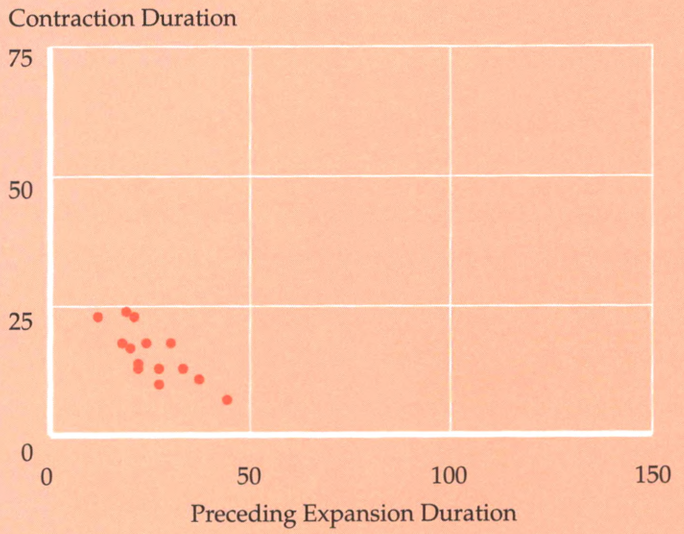
The average minor contraction in the prewar period lasted 13 months. To forecast the length of a contraction, rather than assuming that the contraction will last 13 months, the results imply that a better forecast can be formed by taking the length of the preceding expansion into account. A contraction can be expected to last 28 months minus one-half times the length of the previous expansion.[6] So if an expansion were average and lasted 30 months, a forecast of the length of the following contraction would be 28 - (½ x 30) = 13 months, which is the average length of a contraction. But if an expansion were longer, like the 37-month expansion from 1945 to 1948 (cycle number 22 in the Table), the forecast would be 28 - (½ x 37) = 9½ months; in fact, the contraction lasted 11 months, so the prediction was fairly accurate. If the expansion were shorter than 30 months, like the 20-month expansion of 1891 to 1893 (cycle number 9), the forecast would be 28 - (½ x 20) = 18 months, which is very close to the actual length of 17 months.

INCORPORATING BUSINESS CYCLES AFTER WORLD WAR II

What happens when we examine minor business cycles since World War II?[7] The most

FIGURE 2

Prewar Relationship for Minor Cycles



[5]For those interested in the technical details, the results discussed in this article are reported in detail in the Appendix.

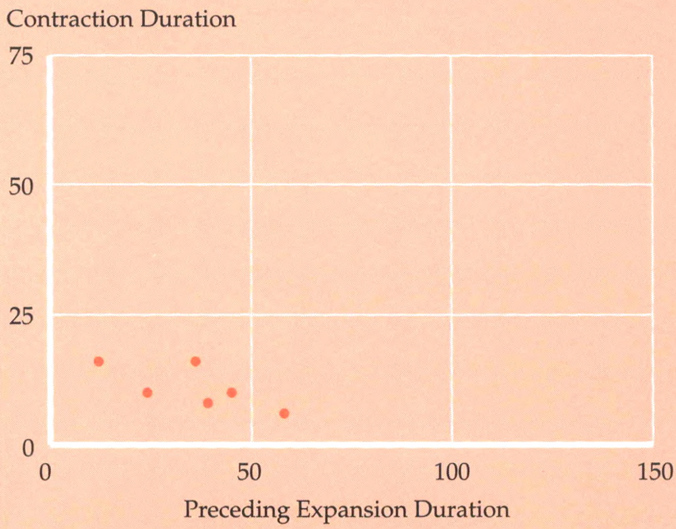
[6]The numbers in this formula come from using statistical (regression) techniques on the data, as reported in the Appendix.

obvious feature of the postwar results is that the negative prewar relationship between the length of an expansion and the length of the following contraction seems to remain intact. There are a number of apparent differences, however. First, an additional month of expansion is now associated with only a one-fifth-month-shorter contraction. Second, the "fit" of the postwar relationship is poorer than that of its prewar counterpart; that is, the association of long expansions with short contractions is not as reliable. Graphs showing the length of an expansion plotted against the length of the following contraction in the prewar (Figure 2) and postwar (Figure 3) periods demonstrate how this relationship has deteriorated. There is still a negative relationship between the length of an expansion and the length of the following contraction, but less confidence should be attached to the postwar relationship because of both the greater dispersion of the data points around the fitted line and the smaller number of data points. To the extent that there are differences between the prewar and postwar relationships, they are likely due to the same factors that caused the postwar lengthening of expansions relative to contractions, as documented in Diebold and Rudebusch (1992). These factors include different patterns of postwar supply shocks and postwar policy and nonpolicy structural changes (for example, "automatic stabilizers" and the shift away from agriculture).

Although there appears to be some change in the magnitude of the relationship between the length of an expansion and the length of the following contraction, it may nevertheless be of interest to examine the results obtained from pooling the prewar and postwar data. This is because we can't be certain that a postwar shift occurred, particularly in light of the fact that the qualitative nature of the Zellner relationship appears to remain intact. Moreover, because there have been so few business cycles since World War II, it is hard to draw any reliable statistical conclusions from the postwar data alone.

Pooling the prewar

FIGURE 3
Postwar Relationship for Minor Cycles



[7] The 1982-1991 cycle is omitted because it probably would not be judged a minor cycle using the criteria of Hansen (1951) and Gordon (1952), an important element of which is the overall duration of the cycle.

and postwar data, we estimate that a one-month-longer expansion yields a one-third-month-shorter contraction. Plotting the length of an expansion against the length of the following contraction for the pooled data (Figure 4) shows this relationship very clearly. As expected, the pooled estimate lies between the separate prewar and postwar estimates.

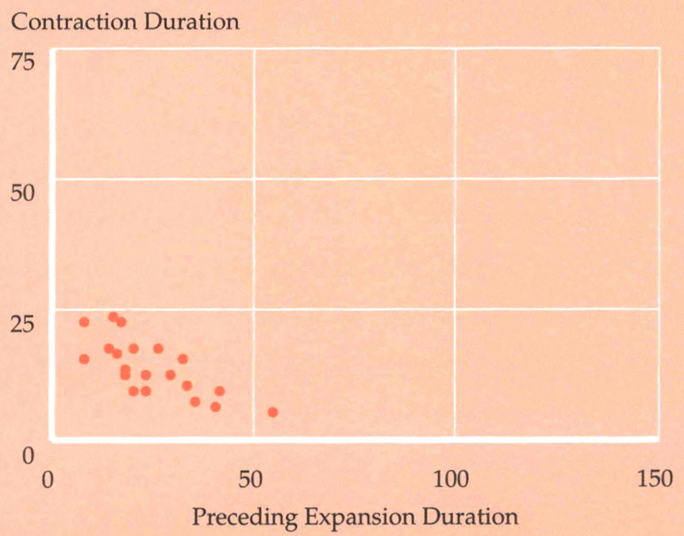
Looking at all the minor cycles since 1854, the average contraction lasted just under 13 months. As before, rather than assuming that a contraction will last 13 months, the results indicate that a better forecast could be formed by taking account of the length of each expansion. A contraction can be expected to last 24 months minus one-third times the length of the previous expansion. So if an expansion lasted 33 months, the contraction would be expected to last 24 - (1/3 x 33) = 13 months, which is the average length of a contraction. But if an expansion were longer, like the 58-month expansion from 1975 to 1980 (cycle number 29 in the Table), a shorter contraction of 24 - (1/3 x 58) = 5 months would be expected; in fact, the contraction lasted six months—a fairly accurate prediction. If the expansion were shorter than 33 months, like the 12-month expansion of 1980 to 1981 (cycle number 30), the forecast would be for a contraction of 24 - (1/3 x 12) = 20 months; that's not too far from the actual length of 16 months.

CAVEATS

Several sobering facts should temper one's degree of belief in the findings reported here. First, substantial uncertainty exists regarding the business-cycle chronology itself. In particular, the prewar business-cycle chronology is subject to much greater uncertainty, stemming from the inferior quality and quantity of prewar source data;[8] that is, the NBER business-cycle chronology is only an estimate, or best guess, of the "true" business-cycle chronology, and the confidence we have in our guess is lower in the prewar period.[9]

FIGURE 4

Pooled Relationship for Minor Cycles



[8]See the work by Romer (1991) and Watson (1992).

[9]Indeed, some economists, such as Christina Romer of the University of California at Berkeley, have produced business-cycle chronologies that are different from the NBER's—they show contractions and expansions starting and ending on different dates than those in the NBER

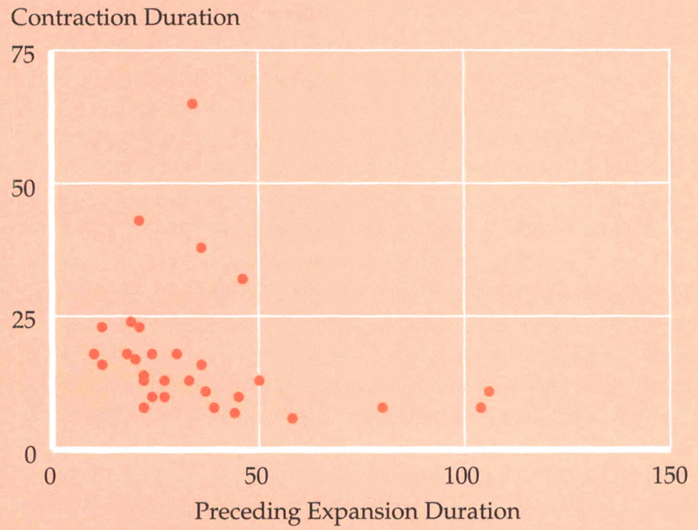
Second, we have used data corresponding only to minor cycles. Minor business-cycle data seem to show a negative relationship between the length of an expansion and the length of the following contraction. But what about major cycles? The data show no relationship between the length of an expansion and the length of the subsequent contraction when we consider major and minor cycles together, as shown in Figure 5. So splitting business cycles into major and minor categories is important to finding a relationship between the lengths of neighboring expansions and contractions.

chronology. Although there are some real problems with the Romer chronology (see Zarnowitz, 1992), which is why we don't examine it here, it does represent a serious reassessment by a knowledgeable expert and serves to highlight the uncertainty inherent in any business-cycle chronology, particularly in the prewar era.

If the relationship holds only for minor cycles and we want to use it for forecasting purposes, we need to be able to classify cycles as major or minor. Ultimately, however, it's clear that the methods used by Zellner and others to separate minor and major cycles are incompletely specified and highly subjective. The situation is not hopeless, however; at least part of the implicit algorithm used to identify major cycles can be readily inferred. Typically, for example, cycles containing wars are designated as major. And it makes sense that these cycles be excluded from the analysis, since the economy behaves much differently during wartime than during peacetime—the influence of a war on economic activity often dominates any other features of the economy. Wartime major cycles are numbers 3, 22, and 27 (from the Table).

In addition to wars, other major events in the economy have led economists such as Hansen (1951) and Gordon (1952) to label certain peacetime cycles as major cycles. Major cycles typically represent larger, longer-term changes in the economy than do minor cycles. The downturns in a major cycle are longer and more severe than those of a minor cycle because they involve large structural changes in the economy. Generally, a major upswing in the economy is a time when there are powerful forces causing economic growth to occur, perhaps due to profitable long-term investment opportunities. Cycles 2, 5, 6, 17, 20, and 21 are the peacetime major cycles.

FIGURE 5
Pooled Relationship: Major and Minor Cycles



CONCLUDING REMARKS

I have reported on recent research indicating that, for prewar U.S. minor business cycles, there exists a negative relationship between the length of an expansion and the length of the following contraction. Moreover, it seems that the relationship has stood the test of time—a qualitatively similar, if somewhat less pronounced, relationship holds in the postwar period.

I indicated how such a relationship could be used to forecast a contraction's length based upon knowledge of the previous expansion's length. But I also pointed out—and I hasten to do so again—that potential pitfalls abound. The pitfalls concern primarily the uncertainty inherent in any business-cycle chronology, the lack of precise definitions of major and minor cycles and the associated difficulty of distinguishing them, and the possibility of secular change in the nature of the relationship. Thus, further research is needed to determine the real usefulness of the ideas discussed here.

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APPENDIX

Regression Results
Using the NBER Business Cycle Chronology

Dependent Variable: Contraction Length

	(1) Prewar (Zellner)	(2) Prewar	(3) Postwar	(4) Pooled
Intercept	27.2 (p=.00)	27.6 (p=.00)	17.5 (p=.01)	23.9 (p=.00)
Expansion Length	-.45 (p=.00)	-.47 (p=.00)	-.18 (p=.12)	-.33 (p=.00)
Obs.	15	15	6	21
$\bar{R}^2$	.54	.57	.38	.54

This table shows the results of an ordinary least squares regression of the length of minor contractions on an intercept and the length of the previous expansion.

Column (1) reports the results of Zellner (1990), where the sample consists of prewar minor cycles.

Column (2) reports the results of our replication of Zellner's regression.

Column (3) reports the results for the postwar minor cycles.

Column (4) reports the results for the pooled prewar/postwar sample.

P-values, or marginal significance levels relative to the t-distribution, are given in parentheses below the coefficient estimates. A small p-value indicates high statistical significance.

$\bar{R}^2$ denotes the percentage of variation in minor contraction durations explained by variation in preceding expansion durations, after correcting for the degrees of freedom used in estimation.

Does Money Affect Output?

*Shaghil Ahmed**

Economists studying business cycles often focus on the relationship between money growth and output growth. Examination of the data reveals two key features: (1) when output grows at an above-average pace, so does the money supply; (2) changes in money growth occur prior to changes in output growth.

Two very different theories explain these features. The first, monetary-business-cycle theory, assigns a causal role to money supply in influencing real economic activity: changes in growth of the money supply cause changes in output growth (money causes output). The second, real-business-cycle theory, challenges traditional monetary-business-cycle theory. Real-business-cycle models explain the relationship between money and output by appealing to the idea of *reverse causation.* Developments in the real sectors of the economy affect people's financial decisions, which, in turn, influence the quantity of money demanded. So long as the financial system responds to the change in money demand, changes in output

* When he wrote this article, Shaghil Ahmed was a visiting scholar in the Research Department of the Philadelphia Fed. He is an assistant professor of economics, The Pennsylvania State University. Shaghil thanks Paul Calem, Dean Croushore, Steve Meyer, and Carlos Zarazaga for helpful comments, and Sally Burke for extremely valuable editorial help.

growth generate changes in money growth (output causes money), not vice versa. The two theories thus differ primarily in the direction of causation between money growth and output growth.

The key difference between the two theories lies in their implications for the role of monetary policy. In monetary-business-cycle models, active and discretionary monetary policy can stabilize the economy. In some versions of these models, stabilization policy is highly desirable. But, according to real-business-cycle models, stabilization policy doesn't work.

The controversy over these theories is far from settled. In the past, economists have typically focused on aggregate data in their analysis of the relationship between money growth and output growth. But, using aggregate data, it is impossible to infer the direction of causation between money and output; only correlation, not causation, can be observed.

The first purpose of this article is to review the basic structure of each of these two types of theories and to highlight the different policy implications to show why monetary policymakers need to know which of these two views is more in accord with the data. We focus on simple versions of representative models from each theory to make clear the distinctions between the theories. Second, we evaluate some empirical evidence, based on the division of money into its broad components, that attempts to provide information on the relative merits of these two competing theories.

MONETARY BUSINESS CYCLES

Why does money affect output? Classical economics leads us to expect that changes in money will have no effect on real economic variables. To see this, consider a hypothetical experiment. Suppose the Federal Reserve announces that the public can exchange every dollar of currency it holds for two dollars. Logically, we would expect the public to take the Fed up on its offer, and all prices and wages in the economy would quickly double, the nominal value of the dollar relative to other currencies would halve, and the economy would demand and supply the same amount of goods and services as before. Thus, none of the real economic variables would change in response to the change in money. When this occurs, we say that money is *neutral.*

Monetary-business-cycle theorists must explain why money is not neutral, and explanations have evolved along two very different lines. One appeals to failure of markets to clear, and models in this category are known as new Keynesian models.[1] One widely used new Keynesian model is the sticky-wage model, which views wage contracts as a central feature of the economy. Workers and firms sign long-term wage contracts that fix workers' money wage over the length of the contract. If money supply grows at a faster rate than was foreseen at the time the contracts were signed, inflation will be higher than expected, so workers' real wages will fall. The fall in real wages induces firms to hire more workers, which raises the economy's output. As a result, there is a positive correlation between the growth rates of money and output.

Not all changes in money growth lead to changes in output growth, however. Changes in money growth that were expected at the time of contract negotiation would just lead to workers' demanding, and getting, proportionally higher wages, with no change in real wages, employment, and output. So only changes in money unanticipated at the signing of contracts affect output. Given uncertainty and the fact that contracts are negotiated less often than the money supply is changed, the sticky-wage

[1]One important defining characteristic of new Keynesian models, as opposed to old-style Keynesian theories, is the concept of rational expectations. According to this concept, agents use all available information efficiently in forming expectations of future values of economic variables.

theory explains the positive relationship between money and output observed in the data.[2]

A second explanation by monetary-business-cycle theorists for the nonneutrality of money is based on a class of models known as imperfect-information models.[3] These models eschew any notion of the importance of nonrenegotiable labor-market contracts. Prices and wages are assumed perfectly flexible in these models, so that, in the absence of uncertainty, the economy is always at full employment, and monetary changes have no real effects. However, monetary changes can have real effects because people have limited information and thus may misperceive aggregate and relative changes. Suppose that people do not observe the prices of all goods, but observe only the prices of goods in the sector of the economy in which they work and produce. In this model, as in classical economics, if the money supply goes up, prices will tend to rise throughout the economy. But people observe only their own sector's price rising, so they attribute part of the price increase to a shift in demand toward their own product and away from the goods produced by other sectors (that is, a change in relative demand). Based on this misperception of an increase in relative demand, they work and produce more. Later, when people realize that the economywide price level went up and there was no change in relative demand, they regret such decisions. Nevertheless, given the information available at the time, their response was perfectly rational.

Policy Implications of Monetary-Business-Cycle Models. In the sticky-wage model, monetary policy can stabilize the economy. If government policymakers don't react properly, the economy can suffer a recession. To see this, suppose private demand for goods falls for some reason (for example, foreign demand for U.S. goods falls). This lowers prices and raises real wages, since money wages are contractually fixed. Since firms have to pay workers higher real wages, while demand for their products has fallen, they will lay off workers, and there will be a recession. Given that people in the private sector have tied their hands in the setting of wages and that it is too costly to renegotiate contracts, the appropriate policy response is to counter the fall in private demand by increasing the money supply. This raises prices, drives real wages back down, and increases employment and output, offsetting the effects that created the recession. The upshot is that countercyclical monetary policy, defined as expanding the money supply to increase output when private demand is temporarily low, can be used to fine-tune the economy and promote stability.

The above role for policy does not carry over to the imperfect-information model. In this model, monetary policy can influence output only by creating misperceptions. Suppose, as above, that foreign demand for U.S. goods falls. So long as people correctly perceive the change in demand, prices and wages will adjust, demand will go back up, and the economy will remain at full employment.

But suppose that policymakers recognize the drop in foreign demand, while the public does not. Then, in principle, policymakers can respond by creating changes in money not anticipated by the private sector and, thus, can influence output. However, such an action is questionable from the viewpoint of stabilizing output for two reasons. First, if policymakers have some information about the economy that the public does not, they can stabilize output by disseminating that information; they do not need to change the money supply. Changing

[2]Economists associated with new Keynesian sticky-wage models include Stanley Fischer (1977), Jo Anna Gray (1976), and John Taylor (1980).

[3]These models were formalized by Robert Lucas (1972, 1975) and Robert Barro (1976), building on the earlier ideas of Milton Friedman (1968) and Edmund Phelps (1972).

the money supply affects output only because people are fooled into doing something they wouldn't do if they had full information. Second, changing the money supply has a short-lived effect on output. It lasts only until people realize the full extent to which policymakers have changed the money supply.

Based on this argument, advocates of the imperfect-information model believe in the "monetary-policy-irrelevance proposition." This proposition consists of two parts: (1) well-understood changes in money supply growth are neutral (they have no effect on real economic variables); and (2) fixed rules are preferred to discretion in the conduct of monetary policy, since they minimize the potential for policymakers to create misperceptions about changes in relative demand.[4] In these models, creating such misperceptions would undermine the central role that prices play in free-market-oriented economies in allocating resources to sectors in which they are most needed.

REAL BUSINESS CYCLES

Real-business-cycle theorists believe in the neutrality of money. They argue that the path of the economy over time is determined by people's responses to changes in their real opportunities and has nothing at all to do with the growth of the money supply. The real opportunities referred to here are supplies of real resources and relative prices that people expect to face over time. Unanticipated changes (shocks) that can influence these real opportunities include technological innovations, other sources of productivity changes, environmental conditions (particularly the weather in agriculture-oriented economies), the world price of energy, developments in the labor market (demographic factors, rising rates of labor force participation for women, and unemployment insurance), and government spending and taxes. So in real-business-cycle theory, output growth depends on real shocks, not money growth.[5]

If the real-business-cycle view is correct, what explains the positive relationship between money and output that we observe? It comes about as a result of both money and output reacting to the same real shocks. Money is neutral and has no effect on real variables, but money is related to output because it responds to the same real shocks that output does. This influence of real shocks on money has been labeled *reverse causation.*

But why should we expect money to respond to real shocks? In answering this question, real-business-cycle theorists divide money into two components: one directly under the control of policymakers and one determined in the banking sector, largely by the financial decisions of private agents. The first component is the monetary base, defined as the sum of currency in circulation and bank reserves, because actions of the monetary authority ultimately determine the supply of currency and bank reserves. The monetary base is referred to as "outside money" (quantity is determined outside the realm of the private sector). The second component is bank deposits, referred to as "inside money" (quantity is determined within the banking sector).

[4]Thomas Sargent and Neil Wallace (1975) provide a formal analysis of the "monetary-policy-irrelevance proposition." This proposition can be stated another way. If the policymakers and the private sector have the same information, the positive relationship between money growth and output growth cannot be exploited for stabilization purposes. If policymakers have superior information, they can influence output by creating misperceptions, but such a policy is inefficient.

[5]Early versions of formal real-business-cycle models, pioneered by Finn Kydland and Edward Prescott (1982) and John Long and Charles Plosser (1983), focused exclusively on technological disturbances as the sources of economic fluctuations. Later models have extended the real-business-cycle framework to allow for a rich variety of real shocks.

Given this distinction between inside and outside money, real-business-cycle theorists offer two reasons why money responds to real shocks. The first reason is based on the idea that the banking sector produces transaction services, which are inputs into private production just like capital and labor. Suppose there is a real shock that makes it worthwhile for people to produce more output in the near future. To produce this output, firms increase their demand for transaction services, since these are inputs into production. Real-business-cycle theorists assume that the flow of transaction services is directly related to the stock of deposits produced by the banking sector. Hence, the banking sector will react to the increased demand for transaction services by soliciting additional funds to increase deposits, which increases the quantity of inside money. In this case, therefore, reverse causation reflects the increased transaction services produced in anticipation of the increase in output. We call this the transaction-services explanation of reverse causation.

The second reason rests on the assumption that people have information about future economic activity that cannot be quantified. (An example is the election of Bill Clinton to the presidency, which apparently led to a rise in consumer confidence and a perception of better prospects for the economy.) Such information influences people's decisions and is quickly reflected in asset prices and interest rates. For instance, higher expected output might increase the demand for money and credit. If monetary policymakers are targeting interest rates, policymakers will let the money supply rise to accommodate the rise in money demand so that interest rates do not change.[6] In this view of reverse causation, money growth is a signal of future real opportunities that are not easily quantified. We call this the signaling explanation of reverse causation. This signaling explanation is distinct from the transaction-services explanation in that it rationalizes why even the monetary base (the measure of outside money), the supply of which is determined by the monetary authority, responds to real shocks in a real-business-cycle framework.

Policy Implications of Real-Business-Cycle Models. The notion that people determine output growth solely by their reactions to real shocks leads to a negative view of the effectiveness of monetary policy in stabilizing output. Fine-tuning the economy is not only undesirable; it is self-defeating. The correlation of broader measures of money (including bank deposits) with output has nothing to do with monetary policy. If the monetary authority attempts to change the money supply by changing the growth of outside money, output is unaffected.

The theories discussed above thus have very different policy implications: in sticky-wage monetary-business-cycle models, money causes output and countercyclical monetary policy is desirable. In imperfect-information monetary-business-cycle models, unanticipated money causes output, but countercyclical policy is not efficient. In real-business-cycle models, output causes money, and monetary policy is completely irrelevant for output behavior.[7] Yet these models share the common empirical im-

[6]For an early reference, see Tobin (1970). Tobin showed that the money-output correlation could be a result of a particular operating procedure for implementing monetary policy, rather than a causal influence of money on output.

[7]Another view of the money-output correlation that we have not explicitly discussed here is the credit view, which emphasizes that there is something special about bank loans that causes them, and hence money, to be closely related to output. (See Ben Bernanke and Alan Blinder, 1988, for an exposition of the credit view.) The credit view is close in spirit to the monetary-business-cycle view in the sense that the direction of causation is from money to output; however, the policy implication that the monetary authority can stabilize output follows from this only if monetary policy is able to affect the quantity of bank loans.

plication that the growth rate of money and the growth rate of output are positively related.

EMPIRICAL EVIDENCE ON MONEY AND OUTPUT GROWTH

Not only is there a positive relationship between money growth and output growth, but changes in money tend to lead (that is, occur prior to) changes in output growth. This feature of the data is illustrated in the figure, which plots the quarterly growth rate of output against the growth rate of money (using the M2 measure) one quarter and two quarters earlier, respectively. The relationship is clearly positive, although far from perfect.

Given that this relationship between money growth and output growth is a feature of several theories, how can economists test the different theories about the business cycle?

Evidence Based on Aggregate Data. Two ideas motivated early empirical work: (1) the distinction between unanticipated and anticipated changes in money is important for testing imperfect-information models; and (2) a careful analysis of the exact timing of the money-output relationship is the key to pinpointing the direction of causation. In this subsection, we summarize this early evidence and give arguments for why it does not distinguish between the different theories, given what we know now. In the second subsection, we discuss some new evidence, based on disaggregating money into its inside and outside components, that is likely to prove more fruitful in distinguishing between the different theories.

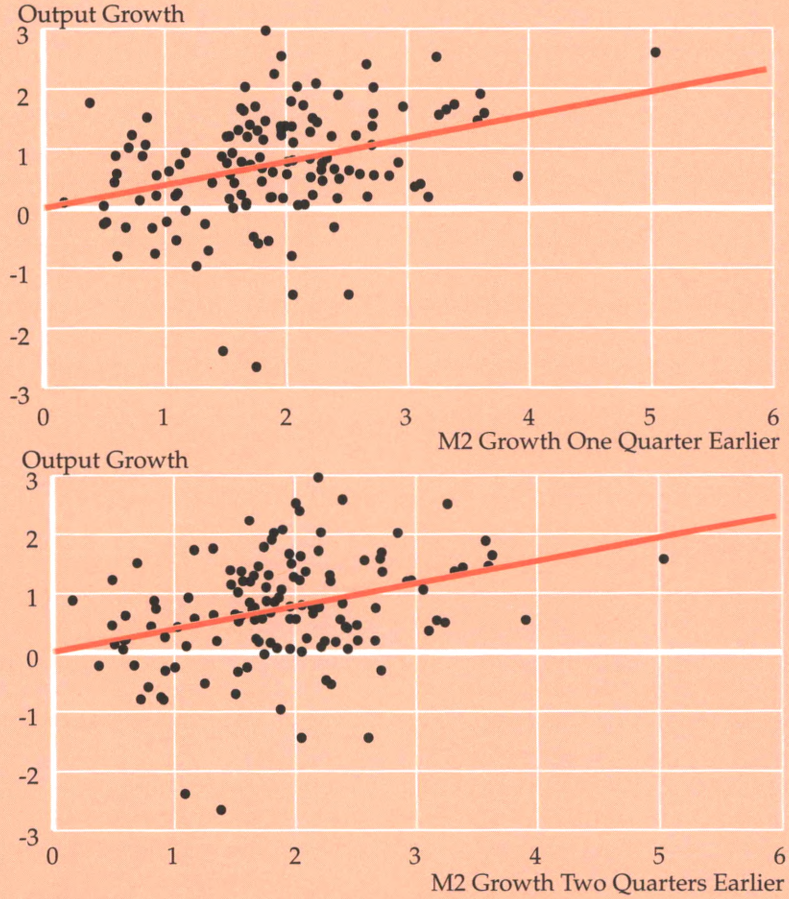
Anticipated Versus Unanticipated Money Growth. In a series of papers, Robert Barro (1977, 1978, 1981) investigated whether anticipated and unanticipated changes in money growth have different effects on the economy. He also tested to see if anticipated changes in money growth have *any* effects on real variables, a test of the neutrality of anticipated money growth. His findings suggest that *unanticipated* changes in money growth have effects on output that continue for about two years, but the effects peak after one year. By contrast, his results supported the hypothesis that *anticipated* changes in money growth have no effect on output. Barro's results have received much attention—and with good reason, since the findings have led to much progress in the business-cycle literature.

Initially, Barro's results were regarded as good news for the imperfect-information view of monetary-business-cycle theory. However, since his original work, many issues have cropped up that have made this conclusion less tenable. First, if the imperfect-information view is true, the effects of unanticipated money growth should disappear once people realize that money is growing faster than they initially thought. Why, then, does unanticipated money growth continue to affect output for up to two years? The usual answer is that the direct effects of monetary changes on output are short-lived, but they cause other things to happen, such as irreversible changes in investment, that have long-term effects on output. Second, Frederic Mishkin (1982) and others have argued that Barro's conclusions do not hold up under reasonable alternative ways of testing. In particular, the results depend on the exact specification of the money-supply forecasting equation used to derive anticipated and unanticipated money growth. Third, though Barro's method was designed to test the imperfect-information model, his results may also be consistent with the sticky-wage model, as pointed out by Stanley Fischer (1980). For example, suppose that the values of unanticipated money growth in the past six months affect output. If wage contracts were signed one year ago, unanticipated money growth over the past six months may just reflect surprise changes that have occurred since the signing of contracts. These surprise changes influence real wages, and hence output, so that the sticky-wage model is consistent with Barro's results.

FIGURE

Relationship Between Money Growth and Output Growth

(1960 - 1992)



These graphs depict a scatterplot of output growth (in percentage terms) on the vertical axis against money growth (in percentage terms) one quarter and two quarters earlier, respectively, on the horizontal axis. The upward sloping lines represent the average relationship between the two variables over the period 1960 to 1992. These upward sloping lines indicate that, on average, current output growth is positively related to money growth one quarter and two quarters earlier. More formal statistical analysis (not reported here) shows that about 15 percent of current output growth can be explained by growth in the M2 measure of money over the current and previous four quarters in the period 1960-92. When subperiods are considered separately this number goes up to 20 percent for the periods from the first quarter of 1960 to the third quarter of 1979 and from the fourth quarter of 1979 to the second quarter of 1992, and to 30 percent for the period from the fourth quarter of 1982 to the second quarter of 1992.

My own sense of the current interpretation of Barro's results by the economics profession is as follows. The results establish the important fact that the correlation between unanticipated money growth and output is stronger than that between anticipated money growth and output.[8] Nevertheless, the results do *not distinguish* between the sticky-wage model and the imperfect-information model or tell us the *direction of causation* between unanticipated changes in money and output. Barro's original empirical work took as given that the direction of causation is from unanticipated money growth to output growth. The question he focused on was whether anticipated money growth *also* affects output growth.[9]

Causality Tests. To investigate the direction of causation between money and output, a number of empirical studies look more closely at the idea of whether changes in money growth precede changes in output growth or vice versa. Statistical tests based on this idea can be thought of as formal tests of the "post hoc, ergo propter hoc" ("after it, therefore because of it") proposition.[10] These tests show that recessions tend to be preceded by tight monetary policy and expansions by loose monetary policy. Economists originally believed that the only way to explain these results was to assign a *causal* role to money in influencing economic activity, thereby lending support to the monetary-business-cycle position.

However, this conclusion from statistical causality tests may not be valid. Statistical causality and *economic* causality are not the same thing.[11] To give the flavor of why this is so, consider two examples. First, suppose that changes in money growth *do* cause changes in output growth (as in the sticky-wage model). Suppose monetary policymakers can forecast changes in private demand for goods, and they try to offset changes in private demand to stabilize output. If they are successful at stabilizing output growth, money growth moves around a lot over time in response to changes in private demand. Since money is moving around but output is relatively stable, we will find no statistical evidence of money growth affecting output growth. Yet, by the assumptions of this example, money growth causes changes in output growth in the economic sense.[12] While the assumptions of this example are extreme, it shows that the observed magnitude of the effect of past changes in money growth on output growth may understate the true causal influence of money growth on output growth.

A second example illustrates how the relationship between statistical causality and economic causality can break down when expectations of the future feed into current decisions. Suppose that expected changes in future economic activity cause current changes in monetary aggregates, as in real-business-cycle theory. For example, suppose the expectation of a recession leads to a perception that less transaction services will be needed, so fewer transaction services—and hence less deposits and money—are produced in anticipation of the fall in output. The fall in money growth

[8]Even though Mishkin questioned Barro's result that anticipated money growth had no effect on output growth, he did find that the effect of unanticipated money growth was greater than that of anticipated money growth.

[9]The popularity of real business cycles and reverse causation as a plausible alternative explanation of the money-output correlation came several years after Barro's research was done.

[10]This notion of statistical causality is attributed to Clive Granger (1969) and Christopher Sims (1972).

[11]Thomas Cooley and Stephen Le Roy (1985) provide an excellent, though quite technical, exposition of this point, with detailed examples. For a more intuitive discussion, see Carlstrom and Gamber (1989).

[12]This example is from Gregory Mankiw (1986).

occurs in *anticipation* of the fall in output growth because transaction services and, hence, bank deposits, are intermediate goods that act as inputs into production, and therefore, their quantity has to change before the change in final output. In this example we find statistical causality from money to output, but there is no economic causality. A simpler example, which is not from economics, has sometimes been used to drive home the point: we all know that it is the expectation of Christmas that causes Christmas cards to be sent; yet, if we did statistical causality tests on the timing, we would conclude that Christmas cards cause Christmas!

Based on the different possible economic interpretations of the results, we conclude that statistical causality tests failed to live up to their early promise. They do provide evidence on the timing of the money-output correlation and indicate that changes in money growth that took place up to two years ago have substantial predictive power for output growth movements. Under particular assumptions, they may also shed light on the direction of economic causality. In general, however, they fail to provide strong evidence on the direction of causation in the money-output relationship.

Evidence Based on Components of Money. The relationship between aggregate money growth and output growth is unable to tell us the direction of causation. But real-business-cycle theory makes a distinction between inside money (bank deposits) and outside money (monetary base) that can be used in empirical tests.

Inside Money and Outside Money Growth. Research by Robert King and Charles Plosser in 1984 found that inside money growth is more highly correlated with output growth than is outside money growth. King and Plosser view the above result as at least prima facie evidence supporting the real-business-cycle transaction-services view of reverse causation.

King and Plosser used annual data from 1953 to 1978. We extend their results to quarterly data and update them using data up to 1992. (See *Updating the King/Plosser Study*.) In updating their results we look at the correlations between money and output separately for the period before the fourth quarter of 1979 and the period since then. The motivation for doing this is that many economists hold the view that the change in the Federal Reserve's operating procedure in 1979 caused a change in the nature of the money-output relationship.[13]

We find results similar to King and Plosser's for the period before the fourth quarter of 1979. However, there is little evidence in the period since then that the relationship between the growth rates of inside money and output is stronger than the relationship between the growth rates of outside money and output. Unless a plausible reason can be found for why the particular measure of inside money used for the period since the fourth quarter of 1979 has ceased to be a good proxy for transaction services since 1979, these results are not consistent with the real-business-cycle transaction-services theory.

An argument can be made that the results since the fourth quarter of 1979 differ because the period from this particular quarter to the third quarter of 1982 was special owing to the strong disinflationary stance of monetary policy. In that case, we should obtain results similar to King and Plosser's when data only since the fourth quarter of 1982 are used. However, this

[13]For example, when the Federal Reserve is targeting interest rates, it is more likely that the effects of real shocks, which might affect interest rates, would show up in outside money (the monetary base). Therefore, we would expect the relationship between outside money and output to be different in periods when the Fed is more serious about targeting interest rates (as it was prior to the fourth quarter of 1979) from periods when it is not (as from the fourth quarter of 1979 to the third quarter of 1982). For some empirical evidence that shows that the money-output relationship is, in fact, different in the periods before and after the fourth quarter of 1979, see my working paper.

Updating the King/Plosser Study

King and Plosser (1984) studied the correlation between output growth and current and past changes in inside money (deposits) growth and outside money (the monetary base) growth for the period 1953 to 1978 using annual data. They found that while the correlation between outside money growth and output growth is statistically significant, it is not as strong as the correlation between inside money growth and output growth.

We extend the King and Plosser study with updated quarterly data. For reasons given in the text, we consider the correlations between money growth and output growth separately for the period from the second quarter of 1959 to the third quarter of 1979 and the period from the fourth quarter of 1979 to the second quarter of 1992. The measure of "outside" money is the monetary base. For "inside" money we use the sum of demand deposits and other checkable deposits, savings deposits, small time deposits, money market mutual funds, and money market deposit accounts.[a] However, similar conclusions are obtained when the measure of inside money is changed to the sum of demand deposits and other checkable deposits (the noncurrency component of M1).

For the period from the second quarter of 1959 to the third quarter of 1979, the results are very similar to those of King and Plosser. Expressing output growth as a linear function of outside money growth and inside money growth in the same quarter, we find no statistically significant relationship between outside money growth and output growth. By contrast, a one-percentage-point increase in inside money growth is accompanied, within the quarter, by a .30 percentage point increase in output growth, and this effect is statistically significant. This number goes up to .45 when the noncurrency component of M1 is used as inside money.[b] Statistical tests support the conclusion that the cumulative change in output (over the current and the next three quarters) following a percentage point increase in inside money growth is larger than that following a percentage point increase in outside money growth.

But these results are reversed for the period since the fourth quarter of 1979. For this period, the cumulative output changes following an increase in outside money growth are bigger in magnitude than those following an increase in the growth of inside money. However, they are not very precisely determined, so that there is no statistically significant difference in the cumulative effects for the two types of money.

For reasons noted in the text, the results from only the post-1982 period are also of special interest. They lead to exactly the same conclusions as above (although the magnitudes of the correlations are different) with one notable exception: when real deposits (the dollar amount of deposits divided by the price level) are used, there is a fairly strong correlation between inside money growth and output growth for the post-1982 period. However, even when real deposits are used, the results from the post-1979 period are still the same as before.

[a]Thus, inside money is defined as the noncurrency component of M2.

[b]These results hold up when we change the output growth equation in various ways, such as including past money growth rates in the output equation.

turned out not to be so: even restricting the sample to this subperiod, there is no evidence of a strong relationship between the growth of output and the growth of deposits. We should emphasize, though, that the results for the post-1982 data change when the real, rather than the nominal, quantity of deposits is used.

Taking the results as a whole, we still conclude that there is no overwhelming evidence from the post-1979 data in favor of the transaction-services view of reverse causation.

Anticipated and Unanticipated Changes in Money Growth. The distinction between unanticipated and anticipated money growth may also be important in real-business-cycle models. According to the signaling view of reverse causation, only the unanticipated component of outside money growth provides new information about future output and thus leads to subsequent changes in output growth. In light of this, we extend the Barro study, which emphasizes the distinction between anticipated and unanticipated money growth, to incorporate the distinction between inside and outside money introduced by King and Plosser. In doing so, we use quarterly data up to 1992. (See *Extension of the Barro Study*.) We find that the correlation between unanticipated changes in outside money growth and output growth is stronger than that between unanticipated changes in inside money growth and output growth in the period before the fourth quarter of 1979. The reverse is true for the period since then. Once again, these results create a problem for explanations that attribute the entire money-output correlation to reverse causation. To reconcile these findings with the real-business-cycle view that there is no direct causation from various components of money growth to output growth, one would have to argue that the signaling view of reverse causation *primarily* applied in the period before the fourth quarter of 1979, while the transaction-services view of reverse causation *primarily* applied in the period since then. It is also surprising that distinguishing between anticipated and unanticipated money growth seems to have reversed the findings of King and Plosser.[14]

The above conclusions are based on using the noncurrency component of M2 as the measure of inside money. As to whether unanticipated changes in outside or inside money growth primarily influence output growth in the pre-1979 period (when we use the noncurrency component of M1 to measure inside money), the statistical results depend on how many previous quarters of unanticipated money growth are allowed to influence current output growth. There is no evidence that unanticipated changes in outside money growth are important for output growth when only the post-1982 sample period is used. (See *Extension of the Barro Study* for details.)

Using Real Shocks to Test Causation. The statistical tests we have looked at so far assume that we cannot directly observe real shocks to the economy. This may be true of some important real disturbances, such as technological innovations and information about expected future economic activity that cannot be quantified easily. Nevertheless, at least some of the shocks to real opportunities that lie at the heart of real-business-cycle theory can be observed. Researchers have begun to exploit this fact in distinguishing between the monetary-business-cycle and real-business-cycle views. For instance, John Boschen and Leonard Mills (1988) examined how much of short-run output growth could be explained by real shocks such as changes in oil prices, in government spending and tax rates, in population and the labor force, and in real exports (reflecting short-run real demand shocks originating in the rest of the world).[15] Then they examined if different components of money, such as currency, bank reserves, and bank deposits, affected output growth after the effects of the real shocks had been accounted for.

Boschen and Mills found that up to 50 percent of output growth can be explained by real shocks.[16] They concluded that while the real

[14]For further evidence on the transaction-services and signaling views of reverse causation and on the "money causes output" view, see my working paper.

[15]In 1983, James Hamilton argued that oil-price shocks have had a significant effect on real economic activity in the postwar United States. This applies even to the period before the large OPEC oil-price increases of 1974 and 1979.

[16]This figure is obtained by adding up the highest numbers in their Table 3.

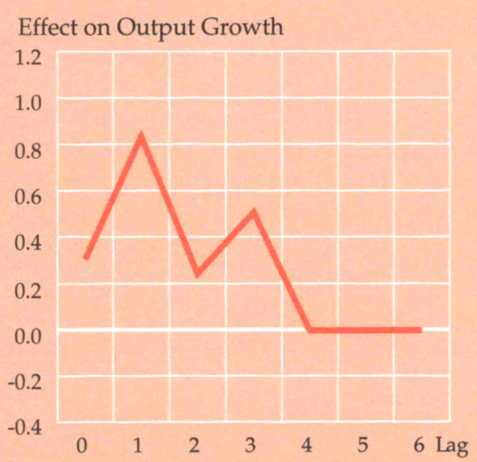
Extension of the Barro Study

Barro (1978) distinguished between unanticipated and anticipated money growth and tested for the neutrality of anticipated changes in money growth. Testing whether anticipated changes in money growth are neutral is not easy because we cannot observe people's expectations and, therefore, cannot obtain direct data on anticipated money growth. Barro followed a statistical procedure to handle this problem. He estimated a money-supply equation in which current money growth is a function of past growth rates of money (to capture the observed momentum in money growth rates), the past unemployment rate (to capture countercyclical monetary policy), and a current fiscal variable (to capture the revenue-creating motive of money growth). He took the forecasted values from this equation as a measure of anticipated money growth, and the difference between actual money growth and anticipated money growth as a measure of unanticipated money growth.

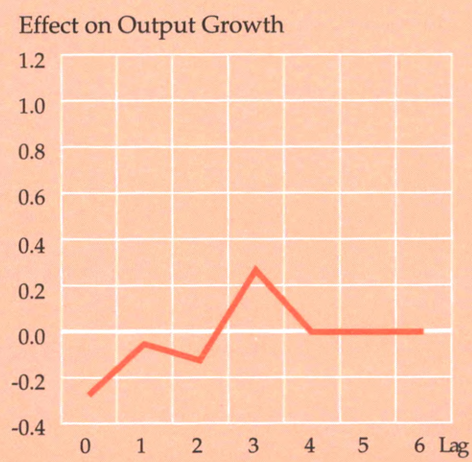
We extend the Barro study to distinguish between inside and outside money, using the same definitions of these components of money as in the update of the King/Plosser study. Once again, the periods before the fourth quarter of 1979 and since then are analyzed separately. The Barro strategy is implemented by first constructing anticipated changes in outside money and inside money growth from forecasts of each of these variables based on past growth rates of output, outside money, and inside money. As in Barro's work, the deviations of the actual growth rates from the forecasted values are taken to be the unanticipated growth for the different measures of money. We then examine the relationship of these surprise components of money with output growth.

The results are presented in the figures below. Each plot traces, for a specific time period and a specific measure of money, a statistical estimate of the average change in output growth over time following a one-percentage-point unanticipated increase in money growth.[a] The length of time after which the response of output growth is assumed to become zero is based on statistical criteria. Before 1979, unanticipated changes in outside-money

Effects of Unanticipated Outside Money Growth: Pre-1979



Effects of Unanticipated Inside Money Growth: Pre-1979



growth led changes in output growth for four quarters (the current quarter is labeled 0), with the response of the output growth rate going up over time at first. On the other hand, unanticipated changes in inside money growth do not induce such a strong effect on output growth. The effect is negative and statistically insignificant. This should be contrasted with the results after 1979 in which there is a strong, long-lasting, and hump-shaped response of output growth subsequent to an unanticipated rise in the growth rate of inside money.[b] Unlike the earlier period, the response of output growth to unanticipated changes in the growth rate of outside money is not strong; in fact, it is negative, but statistically insignificant.

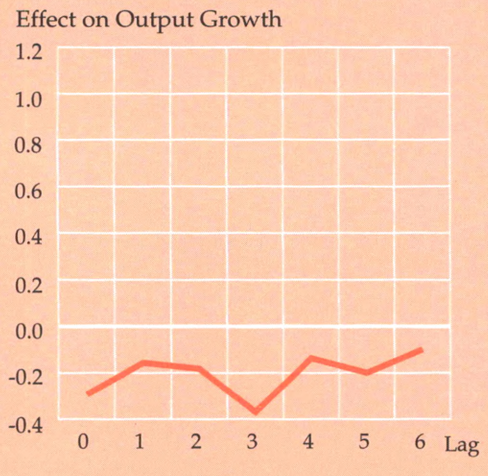
The results shown in the figure are based on the noncurrency component of M2 as the measure of inside money. When the noncurrency component of M1 is used as the measure of inside money, the results are less clear-cut for the pre-1979 period. When changes in unanticipated money growth from up to two years ago are allowed to influence output, the results support unanticipated inside money growth being primarily important for output growth. However, if only unanticipated money growth up to one year ago is allowed to influence output—a premise that formal statistical tests support—it is primarily unanticipated outside money growth that influences output growth in the pre-1979 period, which is the same conclusion that we reached with the M2 results above.

When only the post-1982 period is used, no evidence is found that unanticipated changes in the monetary base have a significant effect on output growth. However, this result should be interpreted with caution, since, owing to the lags involved, the number of usable observations that the post-1982 period provides is unsatisfactory from a statistical viewpoint.

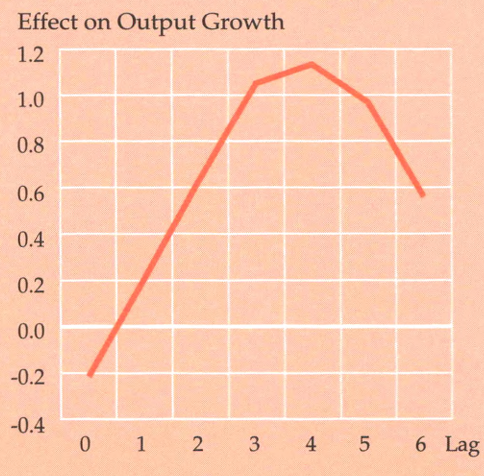
[a]The effects shown are on output growth rates over time. The cumulative effect on the level of output can be obtained by summing up the effects on the growth rates.

[b]The exact sample periods vary depending on the number of lags involved in the money-forecasting and output-growth equations.

Effects of Unanticipated Outside Money Growth: Post-1979



Effects of Unanticipated Inside Money Growth: Post-1979



shocks explain a lot, they also leave a lot unexplained. According to Boschen and Mills, monetary variables explain a significant portion of the variation in output growth not explained by real shocks. However, when they empirically accounted for the possibility that the monetary variables might be signaling other unobserved real changes, they found that money growth has no additional explanatory power for changes in output growth. These results seem to lend strong support to the reverse-causation view, but alternative interpretations are possible. For instance, unobserved real shocks may result from unobserved unanticipated changes in money growth.

Whether the Boschen and Mills results provide strong support for the real-business-cycle view over the monetary-business-cycle view or not, at the very least they serve to remind those of us who study business cycles of an important fact: the simple correlation between observed real shocks and short-run variations in output growth is higher than that between measures of money growth and output growth. This reminder is important because, for a long time, economists paid much more attention to the correlation between money growth and output growth than to the correlation between observed real shocks and output growth. This was not entirely unjustified, however. Even though smaller in magnitude, the money-output correlation is less obvious and, therefore, more intriguing.

CONCLUSION

This article posed the question: do changes in money cause changes in output? This question is important, but difficult to answer. It is not easy to isolate the direction of causation from the observed facts about the relationship between money growth and output growth. Yet, doing so can be crucial for formulating appropriate monetary policy. According to real-business-cycle theory, countercyclical monetary policy is a costly exercise with no reward. By contrast, according to sticky-wage monetary-business-cycle theory, not pursuing countercyclical policy leads to the economy's operating, for significant lengths of time, substantially below the level of employment and output of which the economy is capable.

Evidence based on the growth rates of components of money (as opposed to an aggregate measure of money growth), which is potentially more useful in isolating the direction of causation, is mixed. It seems to indicate that each view—monetary changes are responsible for output changes or output changes are responsible for monetary changes—is correct some of the time. Both directions of causation appear important, and much more research is needed to pin down more precisely the size of the effects in each direction. In particular, richer economic models of reverse causation are needed to see if this hypothesis can better explain the co-movements between components of money and real variables.

While the jury is still out on exactly where between the two extremes the truth lies, two conclusions can be made. First, even if money growth is not the prime factor in influencing output growth, it does not necessarily mean that monetary policy is completely impotent. In principle, as long as there is significant cause and effect operating in the direction from money growth to output growth, active monetary policy has a role in reducing the severity of the (inevitable) business cycles. Whether in practice, however, such a policy is efficient and desirable is still a controversial issue. Second, in looking for the sources of output fluctuations, empirical evidence indicates that money is, in fact, unlikely to be the major factor. Real shocks, such as technology shocks, government tax and spending changes, and factors originating in the labor market, are likely more important. Therefore, it is not possible, and thus too much to hope for, that good monetary policy can rid us of business cycles altogether.

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