

Review

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In This Issue . . .

This issue of the *Review* contains three articles that investigate the influence of changes in money growth and monetary policy actions on diverse economic behavior.

In the first article, "Why Do Food Prices Increase?" Michael Belongia discusses the various explanations that have been offered to account for increases in food prices. Many popular explanations (for example, unionization, price supports and "middlemen") fail to distinguish between *relative* prices and *nominal* (or money) prices. Taking this distinction into account, the author analyzes graphically the different patterns of price behavior that would be observed under each type of price change. Plots of actual data suggest that most of the recent changes in food prices have followed a path similar to that for changes in the nominal prices of other goods. Therefore, models that explain isolated changes in relative prices are of limited use, at best, in explaining ongoing changes in nominal food prices.

A statistical analysis of food prices from 1960 through 1982 shows that the primary cause of changes in the food component of the Consumer Price Index (CPI) has been the past growth of the money stock. Belongia's analysis thus indicates that, while many of the current explanations are inconsistent with the actual behavior of food prices, the rate of increase in the food component of the CPI in the current quarter shares an approximate one-to-one correspondence with the rate of growth of the money stock over the previous four quarters.

In the second article, "Polynomial Distributed Lags and the Estimation of the St. Louis Equation," Dallas S. Batten and Daniel L. Thornton engage in a detailed re-estimation of the nature of the impact of money growth and government expenditures in the well-known St. Louis equation.

The major purpose of the study is to determine whether the conclusions drawn from previous estimations of this equation depend on the selection of lag length or the imposition of polynomial restrictions. In conducting this examination, the authors generalize a procedure for selecting the lag length and polynomial degree that is both convenient and computationally efficient.

They find that the St. Louis equation's policy conclusions are unaffected by the lag length selected or the polynomial restrictions imposed. In particular, the long-run effectiveness of money growth on nominal spending growth and the long-run ineffectiveness of the growth in government spending are substantiated.

Their investigation also identifies a different specification of the equation that outperforms the currently used St. Louis equation in terms of both in-sample and out-of-sample criteria. This new specification has substantially longer lags for both money and government spending growth and more polynomial restrictions than the currently specified St. Louis equation.

In the third article, R. W. Hafer focuses on the predictions of weekly money growth that financial analysts use in attempting to anticipate Federal Reserve policy actions. Although several studies have shown the weekly M1 numbers to be

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unreliable predictors of long-term policy trends, weekly predictions of M1 frequently are used to determine short-term financial market strategies. In "Weekly Money Forecasts," Hafer examines whether the October 6, 1979, change in the Federal Reserve's procedures to control the money supply affected the forecasters' abilities to predict the change in M1. More specifically, he addresses the issue of whether the change in operating procedures affected the unbiased and efficiency characteristics of these M1 forecasts.

To answer this question, the author assesses the money supply forecasts from a survey of actively participating money market analysts. Using the average forecast as the "market's" prediction, he finds that the change in monetary control procedures significantly altered the characteristics of the weekly money supply forecasts. Prior to October 1979, forecasts of the weekly change in M1 generally were unbiased and efficient estimates of the actual change; since October 1979, these forecasts have been biased and inefficient. These findings, along with those presented in studies that analyze the effects of unanticipated weekly money changes on interest rates, "suggest that a more predictable [monetary policy] control procedure would contribute to a more stable financial market."

Why Do Food Prices Increase?

MICHAEL T. BELONGIA

OVER the past decade economists have devoted much research effort to identifying factors that influence the direction and magnitude of changes in food prices. Under the widely-accepted belief that “food prices rose faster than nonfood prices during the 1970s,” many have attempted to identify the unique characteristics of food products and their marketing system that have caused food prices to rise faster than the general rate of inflation.¹ These studies typically concluded that market concentration and increases in the costs of assorted inputs were the chief causes of increases in retail food prices.

Not all analysts share these views, however. First, there is some disagreement concerning whether food has, in fact, become relatively more expensive in recent years. Second, recent empirical research has found that increases in food prices are more directly related to the monetary policy of the Federal Reserve than they are related to unique marketing practices of firms in the food industry. Thus, contrary to the predominant view, these arguments contend that increases in food prices, on average, share the same path as that followed by other prices.

The following discussion attempts to clarify some of these issues. After several basic economic concepts are defined, a statistical analysis of the data is conducted. The evidence suggests that virtually all of the long-run increases in food prices can be explained by past rates of growth of the money stock. Conversely, the discussion in the article’s final section indicates that predic-

tions of competing theories often are contradicted by actual events.

RELATIVE VS. NOMINAL PRICES

The first step necessary in a discussion of price changes draws the distinction between *relative* and *nominal* prices. Put most simply, nominal (or money) prices are the actual, dollar-denominated prices at which goods are exchanged; for example, a newspaper’s nominal price is 25 cents. A relative price, however, expresses the cost of a good in terms of other goods, not in terms of money. That is, if a book’s nominal price is \$2, the relative price of a newspaper — relative to a book — is $\frac{1}{8}$ ($\$0.25 \div \$2.00 = \frac{1}{8}$). This shows that the newspaper is “worth” one-eighth of a book.

The importance of this distinction is more than numerical in nature. There is a crucial *economic* distinction between nominal and relative prices. Changes in relative prices reflect changes in the rate of exchange between goods caused by relative changes in the supply and/or demand for goods; changes in nominal prices reflect changes in the rate of exchange between goods and money associated with changes in the supply and/or demand for money. For example, under a neutral inflation, in which all nominal (money) prices increase at the same rate, a 20 percent increase in the price of newspapers to 30 cents would be matched by a 20 percent increase in the price of a book to \$2.40 ($1.20 \times \$2.00 = \2.40). This equal percentage increase in all money prices is neutral because relative prices are unaffected; that is, with a neutral 20 percent inflation, the relative price of a newspaper is still $\frac{1}{8}$ ($\$0.30 \div \$2.40 = \frac{1}{8}$) of the book.

The distinguishing feature of an equal percentage change in all nominal prices is that it has no long-run impact on economic activity; that is, it does not change

¹See, for example, R. McFall Lamm, “Prices and Concentration in the Food Retailing Industry,” *Journal of Industrial Economics* (September 1981), pp. 67–78; Larry E. Salathe and William T. Boehm, *Food Prices in Perspective: A Summary Analysis*, Economics, Statistics and Cooperatives Service (U.S. Department of Agriculture 1978); and R. McFall Lamm and Paul C. Wescott, “The Effects of Changing Input Costs on Food Prices,” *American Journal of Agricultural Economics* (May 1981), pp. 187–96.

the allocation of resources between newspapers and books.² In other words, when all prices — including incomes — are rising at equal rates, relative prices remain unchanged. In this instance, an individual who allocates fixed proportions of his income to newspapers, books, food and housing is unaffected by a neutral inflation: even though all prices rise by 10 percent, these changes are offset by a 10 percent increase in income. Nominal price changes of this nature share a one-to-one correspondence with past rates of growth of the money stock.³

Conversely, relative price changes for individual products both result from, and contribute to, changes in economic relationships. For example, if an increase in demand doubled the price of newspapers from 25 cents to 50 cents, an individual who purchased newspapers would adjust his spending patterns to reflect this increase. That is, if one person previously had purchased four newspapers per week for \$1 ($4 \times \0.25) out of a \$100 weekly income, there would be \$99 per week to spend on other items. When the newspaper price rises to 50 cents, the four newspapers cost \$2 and only \$98 remains for other purchases. The change in the relative price of newspapers forces this individual to reallocate the \$100 of weekly income: either the purchase of newspapers or other goods must be reduced by \$1.

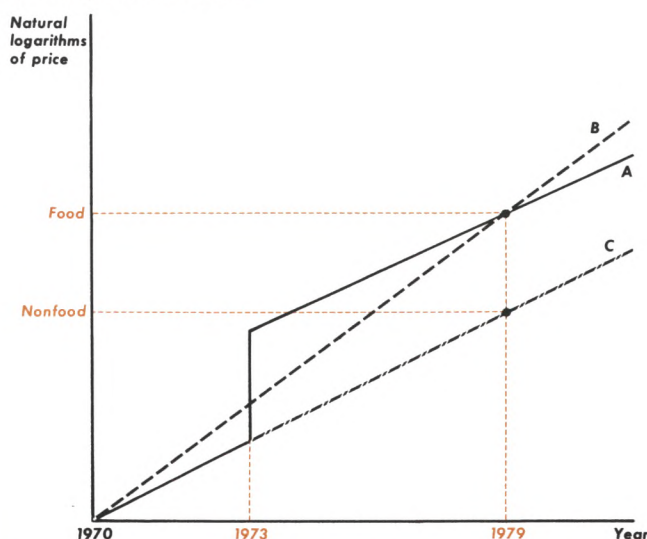
The issue of changes in food prices also can be reduced to this simple dichotomy between movements in relative and nominal prices. Analysts who believe

²Rational expectations theorists may argue that real economic activity will be affected in the short run unless price changes are forecast perfectly, e.g., Robert E. Lucas, Jr., "Expectations and the Neutrality of Money," *Journal of Economic Theory* (April 1972), pp. 103–24. The present analysis also ignores the effects of factors like a progressive tax structure, usury laws and other impediments that prevent or complicate a complete indexation of this type of price change. For purposes of illustration, however, this simple example is intended only to draw a distinction between relative and nominal prices.

³The linkage between past growth rates of the money stock and the current rate of inflation has been established in a number of studies. Among these are: Peter I. Berman, *Inflation and the Money Supply in the United States, 1956–1977* (Lexington Books, 1978); Yash P. Mehra, "An Empirical Note on Some Monetarist Propositions," *Southern Economic Journal* (July 1978), pp. 154–67; Robert E. Lucas, "Two Illustrations of the Quantity Theory of Money," *American Economic Review* (December 1980), pp. 1005–14; Denis S. Karnosky, "The Link Between Money and Prices," this *Review* (June 1976), pp. 17–23; Keith M. Carlson, "The Lag from Money to Prices," this *Review* (October 1980), pp. 3–10; and John A. Tatom, "Energy Prices and Short-Run Economic Performance," this *Review* (January 1981), pp. 3–17.

Further discussion of the distinction between inflation and changes in relative prices can be found in Lawrence S. Davidson, "Inflation Misinformation and Monetary Policy," this *Review* (June/July 1982), pp. 15–26.

Figure 1
Theoretical Differences Between Rates of Price Change
and Changes in Price Levels



food prices have risen faster than nonfood prices are arguing that shifts in the relative supply and demand conditions for both food and nonfood products have resulted in a net increase in the relative price of food. Conversely, those who argue food prices grew at the same rate as other prices believe that most of the recent changes in food prices can be linked directly to the high rate of money growth that existed over this period. The distinction between these views is illustrated in the graphical analysis that follows.

ALTERNATIVE INTERPRETATIONS OF HISTORICAL DATA

Those who argue that food prices increased at a relatively faster rate than nonfood prices in the 1970s (see footnote 1) base their conclusion on the observation that, over this period, the food component of the Consumer Price Index (CPIF) increased by 87 percent compared to a 66 percent increase for the nonfood component (CPINF). Although these statistics are correct technically, they are based on *total* increases for the 10-year period. That is, the 87 percent increase for CPIF is determined by constructing the simple difference of index values for December 1969 and December 1979. This simple calculation of price change, however, fails to distinguish between changes in price levels and average rates of price change.

To see the problem with this type of calculation, consider figure 1. Lines A, B and C represent different growth paths for the food and nonfood components of

the CPI. The horizontal lines drawn at levels denoted by Food and Nonfood indicate, respectively, the 87 and 66 percent increases these indices registered during the 1970s.

Although lines A and B both are consistent with the actual 87 percent increase in food prices that occurred during the 1970s, the differences in their slopes imply very distinct economic interpretations of this statistic. On one hand, lines B and C are compatible with the popular view that food prices increased at a relatively faster rate over this 10-year interval. That is, since 1970, the slope of line B, which represents a constant rate of growth for food prices, has been greater than the slope of line C, which depicts the growth rate of nonfood prices. This suggests that fundamental differences in production and marketing processes established different long-run growth rates for food and nonfood prices in the 1970s. Or, because the difference in slopes appears to be a permanent structural difference, lines B and C also carry the implicit hypothesis that food will continue to increase in value, relative to nonfood products.

Lines A and C also are consistent with the historical data but do not imply any fundamental changes in the relative growth rates of food and nonfood prices. Instead, line A illustrates the effect of certain events in 1973 on the *relative level* of food prices. But, aside from this isolated change caused by relative shifts in world food supply and demand relationships, lines A and C have the same slope. That is, with the exception of 1973's adjustment in relative prices, both food and nonfood prices, on average, have grown at the same rate both before and since 1973. Therefore, lines A and C are consistent with the nominal price changes that occur during a neutral inflation. Or, stated differently, the slopes of lines A and C depict the shared increases in all nominal prices that are associated commonly with past rates of growth of the money stock.

These theoretical relationships can be compared to plots of actual price changes shown in chart 1. In general, these plotted lines reflect the same qualitative results suggested by lines A and C in figure 1. The *level* of food prices did increase, relative to nonfood prices, in 1973 but, after the effects of this relative price change dissipated, food and nonfood prices tended to follow the same trend rate of growth. In fact, declines in the relative price of food in every year since 1978 have caused the food price and the nonfood price lines to converge. Or, rather, the large increase in the relative price of food during 1973–74 has been offset by five consecutive declines in relative food prices since 1978.

Food Prices and Money Growth

The distinctions of the two preceding sections suggest that the problem for an analysis of food prices is to specify a statistical model that can distinguish between changes in relative and nominal prices or, alternatively, between the types of change depicted by lines A and B in figure 1. One such model can be specified as:

$$(1) \quad \dot{CPIF} = a + \sum_{i=0}^4 b_i \times \dot{M}_{t-i} + \sum_{j=0}^1 d_j \times \dot{y}_{t-j} + \sum_{k=0}^1 g_k \times \dot{RP}_{t-k} + h \times Z_1 + q \times Z_2 + e_t,$$

where CPIF is the CPI for food; M is the narrowly defined money stock, M1; y is real GNP; RP is the ratio of the Producer Price Indexes for the "food" and "nonfood" groups;⁴ Z₁ and Z₂ are 0/1 dummy variables for phases I–II and phases III–IV, respectively, of Nixon administration price controls; b, d, g, h and q are estimated coefficients; t indicates time (quarterly intervals, 1960–82); and e_t is a model error term. Dots over variable names indicate data measured in growth rates. All data are seasonally adjusted.

The reasoning behind this model of food price behavior derives from the basic considerations of figure 1 and the discussion of relative versus nominal prices.⁵ Because we know any observed change in food prices is likely to be apportioned in some manner between changes in relative and nominal values, a model of price change must include variables associated with general inflation and with changes in product supply-demand relationships. Therefore, the model includes past growth rates of the money stock to account for that portion of changes in food prices that is associated with general inflation. Changes in the growth rate of real GNP are included to represent a cyclical effect on prices not captured by money growth. That is, if the equation of exchange is rewritten as: $\dot{P} = \dot{M} + \dot{V} - \dot{y}$, then, for a given rate of increase in money and a given M1 velocity, a higher rate of real income growth will tend to be associated with a slower rate of nominal price increase. Therefore, the signs on coefficients d_j

⁴The actual commodity groups are the Producer Price Indexes for "all farm foods and feed" and "all industrial commodities," respectively; these groups represent, essentially, a "food" and "nonfood" division of the PPI.

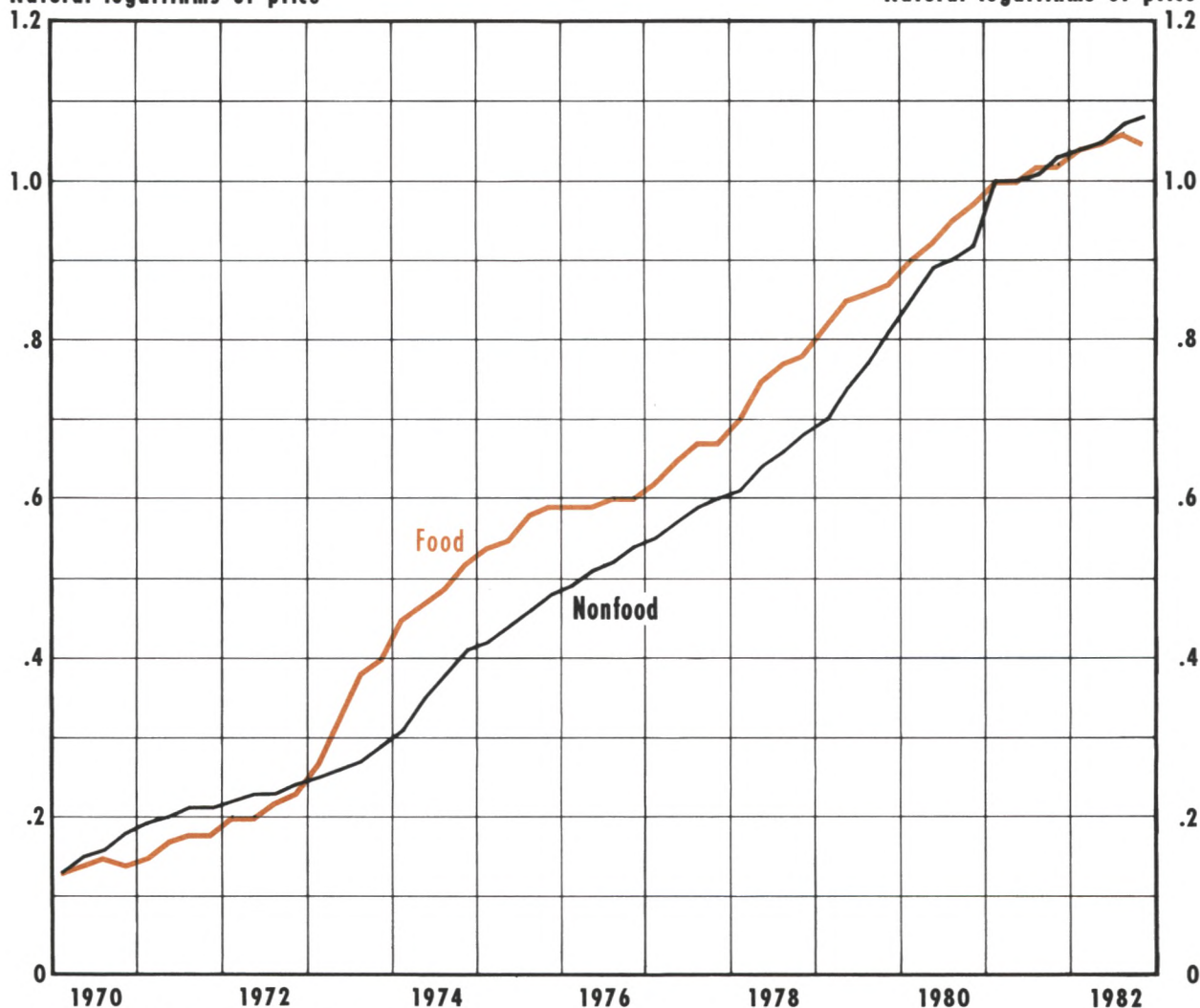
⁵This same basic model, estimated with monthly data, and a more detailed explanation of its theoretical support is found in Michael T. Belongia and Richard A. King, "A Monetary Analysis of Food Price Determination," *American Journal of Agricultural Economics* (February 1983), pp. 131–35.

Chart 1

Actual Movements in Food and Nonfood Prices ¹

Natural logarithms of price

Natural logarithms of price

¹ Data are from Consumer Price Indices.

are expected to be negative. Changes in basic food supplies are represented by a proxy of changes in the growth rate of relative food prices at the producer, or wholesale, level. The effects of official price controls from August 1971 through January 1974 are represented by variables Z_1 and Z_2 . Together, these variables encompass the sources and types of price changes discussed earlier.

This model implies several specific hypotheses. First, a one-to-one relationship between past rates of money growth and nominal prices would be supported by a test of the full impact of all current and past values of M on CPIF; the specific hypothesis to be tested is:

$$(2) \quad \sum_{i=0}^4 b_i = 1,$$

or that an X percent increase in the rate of money growth over the most recent five quarters will cause a similar X percent change in the current growth rate of nominal food prices.⁶

⁶The postulated lag length is considerably shorter than the 20-quarter lag between money and prices reported in other studies. The reason for this difference is the choice of price index for the model's dependent variable. Because supply and demand functions for food products tend to be more inelastic than those associated with other goods, changes in the supply of, or demand for food will tend to affect prices more quickly than is typical in other markets.

Another hypothesis concerns changes in relative prices. Here, the concern is the *net* impact of a change in the growth rate of real income and a change in relative producer prices. In addition to the effect of real activity on nominal price growth shown via the equation of exchange, a change in product supplies also could affect CPIF by changing the relative price of food. Because these effects are expected to be offsetting, the hypothesis test takes the form:

$$(3) \sum_{j=0}^1 d_j + \sum_{k=0}^1 g_k = 0.$$

Finally, it is interesting to know whether general price controls during the 1971–74 period had significant effects on food prices, which were treated differently than other controlled commodities. If controls were effective, the coefficient on Z_1 should be negative and the coefficient on Z_2 , when controls were gradually relaxed, should be positive.

The ordinary least squares results in table 1 support these propositions. The hypothesis test for equation 2 suggests that the net impact of money growth is not significantly different from one; the rate of money growth over the current and past four quarters causes an equal change in the subsequent growth rate of retail food prices.⁷ Therefore, except for transitory short-run deviations, the observed changes in retail food prices have been changes in their nominal values, not in their relative prices. Changes in food prices are related most closely to changes in the growth rate of the money stock.⁸

This result is supported by the tests of other a priori hypotheses. The net effect of changes in the growth rates of real income and relative producer prices is shown to be zero, indicating that relative food prices have not changed significantly over this sample period. This provides further support for the notion that food prices have increased, on average, in a fashion similar to general inflation. Therefore, as the discussion in the next section indicates, studies based only on factors affecting supply and demand conditions are in substantial disagreement with the historical data: if relative prices have not changed appreciably, studies based on factors that shift supply and demand functions will not

⁷Although the coefficients on the third and fourth lags of money growth are nonsignificant individually, an F-test on their joint significance suggests these terms should be retained in the model.

⁸This relationship also appears to be stable over time. The model also was estimated over 1960–72, 1970–82 and 1973–82 sub-samples and, in each case, the growth rates of the money stock and food prices shared an approximate one-to-one correspondence.

Table 1
Estimated Results for Equation 1

Variable	Coefficient estimates	t-statistics
α	0.165	0.58
\dot{M}_t	0.097	0.66
\dot{M}_{t-1}	0.345	2.25
\dot{M}_{t-2}	0.300	2.07
\dot{M}_{t-3}	0.155	1.02
\dot{M}_{t-4}	0.238	1.60
\dot{Y}_t	-0.218	-1.90
\dot{Y}_{t-1}	-0.156	-1.42
\dot{RP}_t	0.168	5.03
\dot{RP}_{t-1}	0.058	1.74
Z_1	-0.838	-2.06
Z_2	1.770	3.58

Hypothesis tests

$$4 \quad \sum_{i=0}^4 b_i = 1 \quad F = 0.43$$

$$1 \quad \sum_{j=0}^1 d_j + \sum_{k=0}^1 g_k = 0 \quad F = 1.37$$

Critical value for $F_{1,79} = 3.97$ ($\alpha = 0.05$)

$$\bar{R}^2 = 0.55$$

$$DW = 1.66$$

present accurate descriptions of observed price changes.

Finally, the coefficients on price control variables are of the expected sign. From August 1971 through the end of 1972, when controls were applied most stringently, they apparently did reduce the rate of increase in reported food prices.⁹ Then, from 1973 through 1974, controls were relaxed gradually and food

⁹This does *not* imply, however, that controls were an effective anti-inflationary policy. In fact, although there is an observed statistical effect on food prices in these results, controls themselves were abandoned, in large part, because of the resource allocation problems they caused. That is, controls masked changes in relative prices that give signals to producers concerning their output decisions. Consider, for example, that higher food prices are caused by product shortages. Higher prices, however, will tend to encourage increased production and, in the longer run, increased production will cause lower prices. Therefore, if price controls limit or forbid price increases, their negative impact on production incentives will exacerbate the shortage-high price conditions.

prices began to increase at a faster rate. These results again support expected price behavior during this period.

The general conclusion of this analysis might be seen more clearly by constructing a comparison of the effects of \bar{M} , \bar{y} and $\bar{R}P$ on the growth rate of retail food prices. After adjusting CPIF for the effects of the model's intercept, Z_1 and Z_2 , it is possible to write:

$$(1') \overline{CPIF} \simeq \sum_{i=0}^4 b_i \times \bar{M} + \sum_{j=0}^1 d_j \times \bar{y} + \sum_{k=0}^1 g_k \times \bar{R}P$$

where the bars over variable names indicate their average, or mean, values. By summing the coefficient estimates as indicated and inserting the data means, equation 1' can be rewritten as:

$$(4) \quad 1.280 \simeq (1.136 \times 1.32) + (-0.374 \times 0.77) \\ + (0.226 \times (-0.23))$$

or,

$$(5) \quad 1.280 \simeq 1.500 - 0.288 - 0.052 \simeq 1.160.$$

In this form, an evaluation of the model's results at the data means indicates that \bar{M} and CPIF share an approximate one-to-one correspondence, whereas changes in real activity — over this sample period — tend to *decrease* the relative price of food. Contrary to the popular belief, food price increases would have been larger had it not been for the mitigating effects of real income growth and shifts in relative producer prices.

NONMONETARY EXPLANATIONS FOR FOOD PRICE INCREASES: A CRITIQUE

A number of studies have offered alternative explanations for why food prices increase and, further, why they have increased relative to other prices. These explanations include increasing prices for farm products,¹⁰ farm price support programs,¹¹ unioniza-

tion of food sector employees¹² and increased concentration of the food industry.¹³ The following discussion indicates that these explanations either are unrelated to the *trend* growth rate of food prices or predict results contrary to observed events.

Rising Input Costs

One alleged cause of increased food prices attributes observed increases in the CPIs for various food groups to increases in the prices of inputs used to produce finished retail food products. Specifically, some previous studies have found that increases in the nominal costs of raw farm products have led to subsequent increases in the retail prices of foods purchased by consumers. The logic behind this explanation is, essentially, that if the prices of the inputs used to produce food items are increased, those processors and retailers who produce and sell food products also must raise their prices to maintain previous profit margins or avoid losses.

The explanation that rising input costs have caused increases in retail food prices is flawed on an empirical basis, if for no other reason. That is, because the relative prices of major food groups at the producer level declined during most years of the 1970s, these inputs actually became relatively less expensive for food manufacturers. These declines in relative prices for raw farm products should have put downward pressure on both producers' costs and output prices. Or, other things being equal, these data suggest that food manufacturers should have been able to produce a given quantity of food at lower — and declining — costs. This is an unlikely explanation for increasing retail food prices.

Concentration Ratios and Prices

Higher concentration ratios for the food industry or relatively higher union membership among workers in the food industry might explain why food prices are at a higher level than their values under perfect competition. But these structural characteristics of the industry could only cause food prices to rise continuously if it is shown that these monopolistic elements also strengthened continuously over the same period. Institutional arrangements — like union bargaining power and pricing strategies among a few relatively large

¹⁰See, for example, Don Paarlberg, *Farm and Food Policy* (University of Nebraska Press, 1980); Albert Eckstein and Dale Heien, "The 1973 Food Price Inflation," *American Journal of Agricultural Economics* (May 1978), pp. 186–96; Rodney C. Kite and Joseph M. Roop, "Changing Agricultural Prices and Their Impact on Food Prices Under Inflation," *American Journal of Agricultural Economics* (December 1981), pp. 956–61; and Lamm and Wescott, "The Effects of Changing Input Costs"

¹¹J. B. Penn, "Commodity Programs and Inflation," *American Journal of Agricultural Economics* (December 1979), pp. 889–95.

¹²R. McFall Lamm, "Unionism and Prices in the Food Retailing Industry," *Journal of Labor Research* (Winter 1982), pp. 69–79.

¹³Lamm, "Prices and Concentration"

firms — usually act in a manner similar to price support programs. That is, some degree of control over pricing decisions — such as a union's ability to secure higher nominal wages for union workers — can act like a price support which raises a commodity's price above its competitive market value. The ability of a union or a highly-concentrated food industry to raise wages or prices to higher levels, however, is not the same as an ability to raise relative wages or prices continuously. Again, there is a necessary distinction between *rates* of price change and changes in *relative price levels*.

There are at least two reasons why neither type of market power is likely to explain ongoing price changes. On the one hand, a producer facing a downward-sloping linear demand curve will have an incentive to raise prices until profits are more affected by declining sales than by higher prices. If a firm starts at a position where raising prices is profitable and decides to raise its product's price, the firm will benefit in two ways. The increased price will, *ceteris paribus*, reduce the quantity sold, which will reduce costs. At the same time, total revenue will increase because the percentage reduction in the quantity sold will be less than the percentage increase in the output price. At some point, where the product's price elasticity is equal to -1 , total revenue will be maximized. At prices above this level, total costs will continue to decline but total revenue also will fall. Therefore, as Batten has explained, price increases beyond some level will result in reductions in marginal revenue (from a smaller quantity sold) larger than the associated decreases in marginal costs (from producing less).¹⁴ In this case, the price increases will reduce profits and, if other firms do not follow the price increases — as traditional oligopoly theory suggests — the firm's market share also will be diminished.

A second counterargument to the alleged relationship between increasing concentration ratios and inflation is found in the reason why an industry becomes more concentrated. Eckard, who found no relationship between concentration ratios and price increases, argues that industries become more concentrated because firms are able to produce at *lower* cost.¹⁵ The sequence of events begins with gains in productivity (most notably, labor productivity) that reduce a firm's input costs and allow it to price its

output below the level charged by competitors. Consequently, more efficient production and lower prices provide an opportunity for this firm to increase sales which, in turn, tends to make its industry more concentrated. This sequence of events — increased productivity and lower input costs ultimately resulting in increased industry concentration — is supported by empirical evidence provided by Peltzman.¹⁶ The concentration ratio-inflation hypothesis also suffers from its own predictions, however: if these models were correct, actual *declines* in the relative price of food must imply that the food industry has become *less* concentrated over this period.

Union Power and Prices

Similarly, the existence of union bargaining power might explain a higher level of costs for a firm purchasing this type of labor. And, a higher level of costs might be used to explain a higher price level for the products produced by a firm using union labor. For the same reasons used in the previous argument, however, the existence of bargaining power in wage negotiations is unlikely to explain why nominal or relative food prices would rise continuously.

One extension of the sequence by which union power causes higher prices through increased wages is presented explicitly in a model by Moore and implicitly in some food price studies.¹⁷ The argument presented is that union wage negotiations and their wage contracts are ongoing processes that result in continuous upward adjustments in nominal wage levels. Further, it is recognized that because wages are just one price among all prices, an increase in the relative price of labor necessarily must be offset by a decline in the relative price of one or more other goods unless the money stock is increased. So, instead of an adjustment of relative prices and wages, the models argue that the Federal Reserve will monitor nominal wage increases and "ratify" them by increasing the money supply. Increases in the growth rate of the money stock will cause inflation, however, and therefore will reduce the purchasing power of wages as product prices increase. This reduction in purchasing power will, it is alleged, set off another round of wage increases to re-establish purchasing power. But, the effort is futile as the money

¹⁴Dallas S. Batten, "The Cost-Push Myth," this *Review* (June/July 1981), pp. 20–25.

¹⁵E. Woodrow Eckard, Jr., "Concentration Changes and Inflation: Some Evidence," *Journal of Political Economy* (October 1981), pp. 1044–51.

¹⁶Sam Peltzman, "The Gains and Losses from Industrial Concentration," *Journal of Law and Economics* (October 1977), pp. 229–63.

¹⁷Basil J. Moore, "Monetary Factors," in Alfred S. Eichner, ed., *A Guide to Post-Keynesian Economics* (M. E. Sharpe and Co., 1979), pp. 120–38.

stock grows again and the rate of inflation increases further.

Although a plausible explanation for ongoing increases in food prices, this type of model rests on the assumptions that (a) wage increases established by union power cause increases in product prices, and (b) the Federal Reserve will ratify nominal wage increases with an expansion of the money stock. These are testable hypotheses of real-world behavior. But, an empirical investigation of these relationships rejected the notions that wage increases cause increases in food prices and that the growth rate of the money stock responds to changes in nominal wages.¹⁸ Therefore, in the one case when unions and food prices might be related, the statistical evidence does not support any direct linkage between wage rates and food prices.

¹⁸M. Belongia, "A Note on the Specification of Wage Rates in Cost-Push Models of Food Price Determination," *Southern Journal of Agricultural Economics* (December 1981), pp. 119-24.

CONCLUSIONS

Changes in food prices since 1970 have been attributed to a variety of sources. These explanations, however, often are based on some confusion over the basic distinction between isolated changes in relative prices and ongoing changes in nominal price levels. After accounting for this distinction, statistical analysis of the data suggest that the recent increases in food prices are increases in nominal price levels that share an approximate one-to-one relationship with past rates of money growth. Competing explanations of food price behavior — unionization, oligopoly power and rising input prices, among others — actually predict results that are contrary to the observed data over this period. Specifically, competing models are based on theories that predict *increases* in the relative price of food; in fact, the relative price of food has *declined* over much of the sample period. Relating money growth to food prices appears to offer a better explanation of what actually produced the food price increases during the 1970s, and what is likely to do the same in the 1980s.



Polynomial Distributed Lags and the Estimation of the St. Louis Equation

DALLAS S. BATTEN and DANIEL L. THORNTON

SINCE its introduction in 1968 to investigate the relative impact of monetary and fiscal actions on economic activity, the St. Louis equation has been the focus of considerable criticism.¹ Much of this criticism stemmed from the fact that Andersen and Jordan's conclusions were substantially different from those of the larger econometric models. In particular, they found that changes in the money stock have a significant, lasting impact on nominal income, while changes in high-employment government expenditures and revenues, although having a short-run impact, have no significant, lasting effect.

Criticism of the St. Louis equation generally has fallen into two categories: the specification of the equation and the use of the polynomial distributed lag (PDL) estimation technique.² The second category has

received far less attention in the literature, and investigations of it have been conducted in a far less systematic manner than investigations of the other category. Consequently, we have undertaken a thorough examination of the use of the PDL estimation technique to determine whether the conclusions of the St. Louis equation are sensitive to either the lag structure employed or the polynomial restrictions imposed.

A BRIEF SURVEY OF THE ST. LOUIS EQUATION

The St. Louis equation has not changed substantially since its introduction. The original specification was:

$$(1) \Delta Y_t = \alpha + \sum_{i=0}^3 \beta_i \Delta M_{t-i} + \sum_{i=0}^3 \gamma_i \Delta G_{t-i} + \sum_{i=0}^3 \delta_i \Delta R_{t-i} + \varepsilon_t$$

where Y = nominal GNP,

M = a monetary aggregate (either M1 or the monetary base),

G = high-employment federal government expenditures,

The authors would like to thank R. Carter Hill and Thomas B. Fomby for their suggestions and comments.

¹The St. Louis equation first appeared in Leonall C. Andersen and Jerry L. Jordan, "Monetary and Fiscal Actions: A Test of Their Relative Importance In Economic Stabilization," this *Review* (November 1968), pp. 11–24.

²There have been three major criticisms of the specification of the St. Louis equation. First, since the equation is not derived explicitly from a structural macroeconomic model, relevant exogenous, right-hand-side variables may be excluded, and, as a result, the equation may be misspecified. See, for example, Franco Modigliani and Albert Ando, "Impacts of Fiscal Actions on Aggregate Income and the Monetarist Controversy: Theory and Evidence," in Jerome L. Stein, ed., *Monetarism*, vol. 1, Studies in Monetary Economics (North-Holland, 1976), pp. 17–42; and Robert J. Gordon, "Comments on Modigliani and Ando," in *Monetarism*, pp. 52–66.

Second, failure to specify the appropriate indicators of monetary and fiscal actions may distort their exhibited relative importance. See Frank De Leeuw and John Kalchbrenner, "Monetary and Fiscal Actions: A Test of Their Relative Importance in Economic Stabilization — Comment," this *Review* (April 1969), pp. 6–11; Edward M. Gramlich, "The Usefulness of Monetary and Fiscal Policy as Discretionary Stabilization Tools," *Journal of Money, Credit, and Banking* (May 1971), pp. 506–32; and E. Gerald Corrigan, "The Measurement and Importance of Fiscal Policy Changes," Federal Reserve Bank of New York *Monthly Review* (June 1970), pp. 133–45.

Finally, ordinary least squares (OLS) estimates of the parameters will exhibit simultaneous equation bias if the right-hand-side variables are not exogenous with respect to nominal income. See Stephen M. Goldfeld and Alan S. Blinder, "Some Implications of Endogenous Stabilization Policy," *Brookings Papers on Economic Activity* (3: 1972), pp. 585–640; Robert J. Gordon, "Notes on Money, Income, and Gramlich," *Journal of Money, Credit, and Banking* (May 1971), pp. 533–45; De Leeuw and Kalchbrenner, "Monetary and Fiscal Actions: Comment," J. W. Elliott, "The Influence of Monetary and Fiscal Actions on Total Spending," *Journal of Money, Credit, and Banking* (May 1975), pp. 181–92; Keith M. Carlson and Scott E. Hein, "Monetary Aggregates as Monetary Indicators," this *Review* (November 1980), pp. 12–21; and R. W. Hafer, "The Role of Fiscal Policy in the St. Louis Equation," this *Review* (January 1982), pp. 17–22.

R = high-employment federal government revenues and

ε = error term.³

The Δ s indicate that all variables are first differences (i.e., $\Delta Y_t = Y_t - Y_{t-1}$). The coefficients of each lagged variable were constrained to lie on a fourth degree polynomial with both endpoint coefficients for each variable constrained to equal zero.⁴ In the original article, longer lag lengths were estimated but, since no coefficient past the third lag was statistically significant, these lags were excluded. None of the reported results indicated any investigation of different lag lengths or different polynomial degrees for each variable individually.⁵ In addition, equation 1 also was estimated in a modified form by combining the high-employment government spending and revenue terms into the high-employment surplus/deficit (i.e., $R-G$).

When Andersen and Carlson made the St. Louis equation the cornerstone of the St. Louis model, it contained the contemporaneous value and four lags of ΔM and ΔG ; ΔR , however, was excluded from the equation.⁶ The same degree polynomial was employed, and the endpoint constraints were imposed.

Many studies of the estimation of the St. Louis equation, both critical and supportive, appeared during the 1968–1975 period. These studies investigated, among other things, the sensitivity of the original results to the choice of lag structure and, indirectly, the appropriateness of the restrictions imposed by the use of a PDL model.⁷ Frequently, however, these studies

made several changes simultaneously (e.g., employing different measures of monetary and/or fiscal policy actions and imposing a different polynomial degree and/or a different lag structure), so that it is difficult to identify the marginal impact of any individual change.⁸ Moreover, with one exception, the polynomial restrictions were never examined directly.⁹

Schmidt and Waud were the first to investigate the lag lengths for the individual variables of the St. Louis equation. They did so, however, within the framework of a fourth degree polynomial.¹⁰ They refrained from using endpoint constraints, arguing that the behavior of the polynomial outside of the range defined by the parameters is irrelevant. Using the minimum standard error as their criterion, they determined the appropriate lag structure for the original equation to be six lags of ΔM , five lags of ΔG and seven lags of ΔR . Despite these changes, their results were not qualitatively different from those of Andersen and Jordan.

Elliott attempted to examine systematically the sensitivity of the results to the choice of lag structure and the impact of the polynomial restrictions. Using a fourth degree PDL procedure, he estimated the equation as modified by Andersen and Carlson with four, eight and twelve lags for each variable. He also employed both ordinary least squares (OLS) and Shiller's method of fitting lags with smoothness priors. His results indicated that the conclusions drawn from the estimation of the St. Louis equation do not depend importantly upon the lag structure chosen or the restrictions imposed by using a fourth degree PDL. Elliott did not conduct statistical tests of these propositions. Instead, he based his conclusions on a casual comparison of the results. Furthermore, he consid-

³Andersen and Jordan, "Monetary and Fiscal Actions."

⁴Without these constraints, the use of a PDL model would have been erroneous, as each variable in the original equation had only four coefficients in its lag structure while five parameters are needed to construct a fourth degree polynomial; the imposition of the endpoint constraints reduces the number of parameters to three. Thus, the use of a PDL model in the original St. Louis equation conserves three degrees of freedom.

⁵Andersen, in a subsequent paper, did investigate longer lag lengths (again with the same lag length specified for each variable) using the minimum standard error of the regression as the criterion for choosing the appropriate lag structure. He concluded that, based on the above criterion, the appropriate lag structure was longer than the one chosen originally, but that the qualitative results were not sensitive to the lag structure chosen. See Leonall C. Andersen, "An Evaluation of the Impacts of Monetary and Fiscal Policy on Economic Activity," *Proceedings of the Business and Economic Statistics Section* (American Statistical Association, 1969), pp. 233–40.

⁶Leonall C. Andersen and Keith M. Carlson, "A Monetarist Model for Economic Stabilization," this *Review* (April 1970), pp. 7–25.

⁷Peter Schmidt and Roger N. Waud, "The Almon Lag Technique and the Monetary Versus Fiscal Policy Debate," *Journal of the American Statistical Association* (March 1973), pp. 11–19; Elliott, "The Influence of Monetary and Fiscal Actions;" Leonall C.

Andersen, "An Evaluation of the Impacts of Monetary and Fiscal Policy on Economic Activity;" Corrigan, "The Measurement and Importance of Fiscal Policy Changes;" De Leeuw and Kalchbrenner, "Monetary and Fiscal Actions: Comment;" William L. Silber, "The St. Louis Equation: 'Democratic' and 'Republican' Versions and Other Experiments," *The Review of Economics and Statistics* (November 1971), pp. 362–67; Gramlich, "The Usefulness of Monetary and Fiscal Policy;" and Leonall C. Andersen and Denis S. Karnosky, "The Appropriate Time Frame for Controlling Monetary Aggregates: The St. Louis Evidence," in *Controlling Monetary Aggregates II: The Implementation*, Proceedings of a Conference Sponsored by the Federal Reserve Bank of Boston (Series No. 9, 1972), pp. 147–77.

⁸For example, see Corrigan, "The Measurement and Importance of Fiscal Policy Changes;" Silber, "The St. Louis Equation: 'Democratic' and 'Republican' Versions;" Gramlich, "The Usefulness of Monetary and Fiscal Policy;" and De Leeuw and Kalchbrenner, "Monetary and Fiscal Actions: Comment."

⁹The one exception is Elliott, "The Influence of Monetary and Fiscal Actions."

¹⁰Schmidt and Waud, "The Almon Lag Technique."

ered only three possible lag structures (which were assumed to be the same for each distributed lag variable) and only a fourth degree polynomial.

After the Andersen-Carlson modifications of the original Andersen-Jordan equation, the only substantive change in the equation took place as a result of an exchange between Friedman and Carlson in the late 1970s.¹¹ In updating the sample period over which the equation had been estimated, Friedman noticed that the cumulative effect of government spending became statistically significant. In his response Carlson pointed out that when the original sample was expanded, the standard error of the regression nearly doubled. This indicated that these errors were heteroscedastic.¹² Using annual rates of change in place of the original first differences of the variables, Carlson respecified the equation.¹³ In this form, the errors were homoscedastic and the cumulative effect of government spending was no longer statistically significant. Since the Friedman-Carlson exchange, the growth rate specification (or an approximately equivalent alternative, first differences in natural logarithms) has been the widely accepted one.¹⁴

In summary, even though a number of studies have attempted to investigate the effects of the lag length and PDL specification of the St. Louis equation, relatively little work has been directed at investigating

and testing the propriety of the polynomial constraints or the lag structure employed. Furthermore, most previous investigations have been conducted using the first difference specification of the equation. Thus, whether the policy conclusions drawn from the estimation of the equation (especially for the growth rate specification) are influenced significantly by the choice of lag length and polynomial restrictions employed remains unresolved.

POLYNOMIAL DISTRIBUTED LAGS

The PDL estimation technique forces the coefficients of each lagged variable of an equation to lie on a polynomial of degree p . In the presence of a high degree of multicollinearity, OLS estimates are not precise. Thus, the rationale for the use of the PDL technique is that it increases the precision of the estimates. Estimates of the individual lag weights, however, will be biased generally unless the correct lag length and degree of polynomial are specified.¹⁵ Therefore, it is important that the appropriate specification be determined.

There are a number of procedures and criteria for determining the appropriate lag length and polynomial degree.¹⁶ We use a computationally efficient procedure outlined recently by Pagano and Hartley (hereafter PH).¹⁷ Details of the PH technique and other relevant considerations are presented in the appendix.

When Almon first introduced PDL models, she suggested that endpoint constraints always be employed.

¹¹Benjamin M. Friedman, "Even the St. Louis Model Now Believes in Fiscal Policy," *Journal of Money, Credit, and Banking* (May 1977), pp. 365-67; and Keith M. Carlson, "Does the St. Louis Equation Now Believe in Fiscal Policy?" this *Review* (February 1978), pp. 13-19.

¹²When the variance-covariance matrix is misspecified, the estimated t-ratios are biased, and neither the direction nor extent of the bias can be determined a priori. See G. S. Watson, "Serial Correlation in Regression Analysis. I," *Biometrika* (December 1955), pp. 327-41.

¹³This re-specification was proffered as an alternative to first differences in the original Andersen-Jordan article. John Vrooman, "Does the St. Louis Equation Even Believe in Itself?" *Journal of Money, Credit, and Banking* (February 1979), pp. 111-17, attempts to correct for heteroscedasticity in the first difference specification. He does so by dividing the observation matrix by the square-root of ΔY_t . This transformation, however, creates correlation between the error term and the right-hand-side variables — a violation of one of the classical assumptions of ordinary least squares estimation.

¹⁴See, for example, Keith M. Carlson, "Money, Inflation, and Economic Growth: Some Updated Reduced Form Results and Their Implications," this *Review* (April 1980), pp. 13-19; Carlson and Hein, "Monetary Aggregates as Monetary Indicators;" John A. Tatom, "Energy Prices and Short-Run Economic Performance," this *Review* (January 1981), pp. 3-17; Laurence H. Meyer and Chris Varvares, "A Comparison of the St. Louis Model and Two Variations: Predictive Performance and Policy Implications," this *Review* (December 1981), pp. 13-25; and Hafer, "The Role of Fiscal Policy in the St. Louis Equation."

¹⁵Let ℓ , p and ℓ^* , p^* denote the assumed and correct lag length and degree of polynomial, respectively. Estimates of the parameter vector will be biased if (a) $\ell = \ell^*$ and $p < p^*$, (b) $\ell < \ell^*$ and $p = p^*$ or (c) $\ell > \ell^*$, $p = p^*$ and $\ell - \ell^* > p^*$. In the instance where $\ell - \ell^* \leq p^*$, the polynomial distributed lag estimates may be biased, but need not be. That is, there are restrictions that may or may not be satisfied by the data. Furthermore, PDL estimators will be inefficient if $\ell = \ell^*$ and $p > p^*$. See P. K. Trivedi and A. R. Pagan, "Polynomial Distributed Lags: A Unified Treatment," *Economic Studies Quarterly* (April 1979), pp. 37-49.

¹⁶See Trivedi and Pagan, "Polynomial Distributed Lags: A Unified Treatment;" D. F. Hendry and A. R. Pagan, "Distributed Lags: A Survey of Some Recent Developments," unpublished manuscript; Robert J. Shiller, "A Distributed Lag Estimator Derived from Smoothness Priors," *Econometrica* (July 1973), pp. 775-88; J. D. Sargan, "The Consumer Price Equation in the Post War British Economy: An Exercise in Equation Specification Testing," *The Review of Economic Studies* (January 1980), pp. 113-35; and George G. Judge and others, *The Theory and Practice of Econometrics* (John Wiley and Sons, Inc., 1980), chap. 11.

¹⁷See Marcello Pagano and Michael J. Hartley, "On Fitting Distributed Lag Models Subject to Polynomial Restrictions," *Journal of Econometrics* (June 1981), pp. 171-98.

The suggested endpoint constraints take the form

$$\beta_{\ell+1} = \beta_{-1} = 0,$$

where ℓ is the chosen lag length. Although the endpoint constraints put explicit restrictions on the distributed lag weights outside of their relevant range, they also imply homogeneous restrictions on the lag weights inside the range via homogeneous restrictions on the polynomial coefficients.¹⁸ Thus, the endpoint constraints add two additional homogeneous restrictions for each PDL variable to those already implied by the PDL model. The problem is that endpoint constraints have no basis in either economic or econometric theory, as Schmidt and Waud have pointed out.¹⁹ As a result, they represent a set of ad hoc restrictions whose sole purpose is to increase the efficiency of estimation. Nevertheless, their validity can be tested.

APPLICATION TO THE ST. LOUIS EQUATION

To investigate the appropriate lag lengths and polynomial degrees for the St. Louis equation, we employ the growth rate specification²⁰

$$\dot{Y}_t = \alpha + \sum_{i=0}^J \beta_i \dot{M}_{t-i} + \sum_{i=0}^K \gamma_i \dot{G}_{t-i} + \varepsilon_t.$$

The dots over each variable represent quarter-to-quarter annualized rates of change, and Y , M and G represent nominal GNP, money (the M1 definition) and high-employment government expenditures, respectively. The estimation period considered is II/1962 to III/1982.

Lag Length Selection

The first step of the PH technique is to select

appropriate lag lengths (J , K) for money and government expenditure growth. Once these lag lengths are selected, a re-application of the technique results in the selection of the polynomial degrees.²¹ The PH procedure is somewhat complicated when appropriate lag lengths and polynomial degrees must be selected for *two* variables.²²

The use of the PH technique, like other procedures for specifying a distributed lag model, requires the choice of a maximum lag length (L). We considered two choices of L : 12 and 16.²³

An application of the PH technique to the St. Louis equation results in a choice of 10 lags on \dot{M} and 9 on \dot{G} . This selection is basically consistent with the results of a standard F-test.²⁴ Ordinary least squares estimates of this lag specification, as well as the usual specification with four lags on both \dot{M} and \dot{G} , are presented in table 1. Note that the standard error of the regression is reduced substantially and the adjusted R^2 is increased substantially by including the additional distributed lag variables. Furthermore, the coefficients on the longest lag terms are significant in the longer lag specification. These results suggest that this specification is preferable. Indeed, a likelihood ratio test of the restrictions implied by the current specification rejects them at the 5 percent level.²⁵

Nevertheless, it is interesting to note that the conclusions about the long-run efficacy of monetary and fiscal policy are unaffected by the choice of lag structure. The hypothesis of the long-run ineffectiveness of money can be rejected for both lag specifications; the

²¹Standard statistical procedures cannot be used to select the lag length if the polynomial degree is specified first. See footnote 6 of the appendix for further details.

²²The choice of lag length and polynomial degree also involves sequential hypothesis testing. As we note in the appendix, care must be taken in conducting sequential tests. Given the problems with sequential tests (and those of preliminary test estimation), we initially chose a relatively low significance level of 15 percent, opting to guard against incorrectly excluding relevant components of the distributed lag. As a general rule, one would have expected the chosen lag length to be shorter had we used a more common significance level, such as 5 percent. In our case, the lag specification would have been the same had we selected a 5 percent significance level.

²³The results for $L = 16$ were identical to those for $L = 12$. Thus, the PH technique seems to be relatively insensitive to the choice of L .

²⁴With $L = 12$ for both \dot{M} and \dot{G} , the F-statistic calculated to test the hypothesis that the 10th lag on \dot{M} is significant was 2.45*. The F-statistic calculated for the same test for the 8th and 9th lags on \dot{G} were 2.55* and 1.77, respectively. (The * indicates significance at the 10 percent level.)

²⁵The likelihood ratio statistic was 32.13, which compares with a critical value of $\chi^2(11)$ of 19.68 at the 5 percent level.

¹⁸This can be seen by noting that the endpoint constraints require

$$\delta_0 + \delta_1(-1) + \delta_2(-1)^2 + \dots + \delta_p(-1)^p = 0 \text{ and}$$

$$\delta_0 + \delta_1(\ell+1) + \delta_2(\ell+1)^2 + \dots + \delta_p(\ell+1)^p = 0.$$

These restrictions can be written as $R\hat{\delta} = 0$, because for a PDL model, $\hat{\beta} = H\hat{\delta}$, so that $\hat{\delta} = H^+\hat{\beta}$, where $H^+ = (H'H)^{-1}H'$. Therefore, $R\hat{\delta} = RH^+\hat{\beta} = R^*\hat{\beta} = 0$. Thus, the endpoint constraints impose a set of homogeneous restrictions R^* on $\hat{\beta}$. See Daniel L. Thornton and Dallas S. Batten, "Endpoint Constraints and the St. Louis Equation: A Clarification," Federal Reserve Bank of St. Louis Research Paper No. 83-001 (1983).

¹⁹See Schmidt and Waud, "The Almon Lag Technique," p. 12.

²⁰We chose to employ this specification because it is the one included in the St. Louis model. For a complete specification of the St. Louis model, see the appendix to Keith M. Carlson, "A Monetary Analysis of the Administration's Budget and Economic Projections," this *Review* (May 1982), pp. 3-14.

Table 1

**Ordinary Least Squares Estimates of
Alternative Lag Length Specifications
of the St. Louis Equation,
II/1962–III/1982**

Variable	Estimated Coefficients	
	PH Specification	Current Specification
Constant	2.342 (1.56)	1.643 (1.07)
M ₀	0.767* (4.61)	0.474* (3.37)
M ₁	0.635* (3.66)	0.441* (3.09)
M ₂	0.295 (1.80)	0.356* (2.51)
M ₃	-0.377* (2.36)	-0.179 (1.22)
M ₄	0.233 (1.38)	0.022 (0.15)
M ₅	-0.127 (0.68)	
M ₆	-0.134 (0.79)	
M ₇	-0.126 (0.74)	
M ₈	0.297 (1.69)	
M ₉	0.230 (1.15)	
M ₁₀	-0.530* (2.77)	
ΣM	1.163* (4.50)	1.114* (4.69)
G ₀	0.110* (2.34)	0.108* (2.21)
G ₁	0.056 (1.24)	0.034 (0.71)
G ₂	-0.095* (2.11)	-0.096* (2.04)
G ₃	0.028 (0.61)	0.040 (0.84)
G ₄	-0.001 (0.03)	-0.004 (0.09)
G ₅	-0.042 (0.90)	
G ₆	0.095 (1.93)	
G ₇	0.047 (0.92)	
G ₈	-0.116* (2.32)	
G ₉	-0.116* (2.33)	
ΣG	-0.034 (0.26)	0.082 (0.82)
	SE = 3.21	SE = 3.58
	R ² = 0.47	R ² = 0.33
	DW = 2.17	DW = 2.01

*Indicates significance at the 5 percent level. Absolute value of t-statistics in parentheses.

same hypothesis about government expenditures cannot be rejected.

Polynomial Degree Selection

The chosen lag structure is used in the selection of the appropriate polynomial degree. The appropriate polynomial degree is selected by re-parameterizing the model and applying the same technique used to select the lag length.

A direct application of the PH technique to the question of polynomial degree selection results in selecting a ninth degree polynomial on \dot{M} and a

seventh degree polynomial on \dot{G} . The results of conventional F-tests, however, indicate that there are more restrictive specifications that cannot be rejected at the 5 percent level. Given that the polynomial restrictions tends to smooth out the distributed lag weights and, thus, might result in more accurate out-of-sample forecasts, we decided to present the results of both the PDL specification resulting from a strict application of the PH technique and the one determined by employing the greatest number of polynomial constraints that satisfy a conventional F-test at the 5 percent level. The latter specification has a sixth degree polynomial on \dot{M} and a third degree polynomial on \dot{G} . The results of the estimation of these specifications (denoted A and B, respectively) and the PDL specification presently used (denoted C) are given in table 2. These equations were estimated with restricted least squares (RLS).²⁶ We believe RLS is preferable to the standard PDL method because it makes the parameter restrictions explicit and permits ease in testing the individual and joint PDL restrictions.

It is clear from these results that each of the two longer lag PDL specifications performs better than the current one. Each has a smaller standard error and a larger adjusted R². Nevertheless, it is interesting to note that the tests of the long-run efficacy of the monetary and fiscal policy variables also are insensitive to the PDL specification. The long-run effect of money is not significantly different from one, while the long-run effect of government expenditures is not significantly different from zero, for all three specifications.²⁷ The short-run distributed lag response patterns, however, differ significantly.

Tests of the Endpoint Constraints

As we noted earlier, endpoint constraints represent ad hoc restrictions and, thus, should not be employed routinely. Nevertheless, since the current specification of the St. Louis equation employs polynomial restrictions only in the form of endpoint constraints, we decided to test these constraints for all three specifications. The results of these tests for the relevant joint and individual restrictions are presented in table

²⁶For a discussion of the equivalence between standard PDL estimation and RLS, see Judge and others, *The Theory and Practice of Econometrics*, pp. 640–42.

²⁷Estimates of two other PDL specifications yielded the same conclusions regarding the efficacy of monetary and fiscal policy. See the appendix for details of these specifications.

Table 2

Estimates of Various PDL Specifications of the St. Louis Equation, II/1962–III/1982

Variable	Estimated Coefficients					
	A		B		C	
Constant	2.366	(1.56)	2.608	(1.63)	1.799	(1.16)
M ₀	0.642*	(4.14)	0.557*	(3.90)	0.461*	(3.87)
M ₁	0.771*	(5.01)	0.677*	(5.01)	0.458*	(5.62)
M ₂	0.236	(1.56)	0.198*	(2.27)	0.244*	(2.46)
M ₃	-0.312*	(2.15)	-0.053	(0.57)	0.015	(0.19)
M ₄	0.075	(0.57)	-0.061	(0.78)	-0.092	(0.76)
M ₅	0.080	(0.63)	-0.037	(0.42)		
M ₆	-0.243	(1.85)	-0.081	(1.05)		
M ₇	-0.080	(0.51)	-0.087	(0.96)		
M ₈	0.209	(1.27)	0.114	(1.20)		
M ₉	0.410*	(2.30)	0.355*	(2.19)		
M ₁₀	-0.645*	(3.50)	-0.501*	(2.64)		
ΣM	1.143*	(4.38)	1.081*	(3.96)	1.086*	(4.52)
G ₀	0.118*	(2.52)	0.106*	(2.32)	0.094*	(2.18)
G ₁	0.039	(0.88)	0.022	(0.80)	0.022	(0.65)
G ₂	-0.068	(1.64)	-0.016	(0.58)	-0.041	(1.12)
G ₃	-0.002	(0.06)	-0.021	(0.82)	-0.026	(0.77)
G ₄	0.011	(0.31)	-0.008	(0.35)	0.034	(0.78)
G ₅	-0.016	(0.43)	0.012	(0.54)		
G ₆	0.041	(1.10)	0.024	(0.94)		
G ₇	0.096*	(2.18)	0.016	(0.60)		
G ₈	-0.125*	(2.54)	-0.027	(1.07)		
G ₉	-0.120*	(2.42)	-0.116*	(2.53)		
ΣG	-0.026	(0.19)	-0.008	(0.07)	0.110	(0.82)
	SE = 3.24		SE = 3.42		SE = 3.65	
	$\bar{R}^2 = 0.46$		$\bar{R}^2 = 0.39$		$\bar{R}^2 = 0.31$	
	DW = 2.27		DW = 2.41		DW = 2.17	

*Indicates significance at the 5 percent level. Absolute value of t-statistics in parentheses. Specification A has ninth degree and seventh degree polynomials on M and G, respectively. Specification B has sixth and third degree polynomials on M and G, respectively. Specification C is the current specification with four lags on both M and G and endpoint constraints.

3. The test of all four endpoint constraints rejects these constraints for both specifications A and B, but not for the current specification. The head constraint on \dot{M} , however, is never rejected by the F-test, and the tail constraint is rejected only for specification B. Nevertheless, in general, the endpoint constraints do not fare well when applied to the longer lag specifications.

Out-of-Sample Forecast Comparisons

While it is clear that the alternative PDL representations of the St. Louis equation perform better on an in-sample comparison, it is interesting to see how

well they perform on the basis of out-of-sample forecasts. To this end, we estimated these specifications from II/1962 to a terminal period and forecasted out-of-sample for four quarters. We then added four quarters to our estimation period, re-estimated the equation and repeated the process. We did this for six periods beginning with a terminal date of III/1976, generating 24 out-of-sample forecasts of the growth of nominal GNP. The root mean square errors (RMSEs) of these forecasts are summarized in table 4. Both the PH specification and the current specification do about equally well by a RMSE criterion over the entire period; there are significant differences, however, in

Table 3

Tests of Endpoint Constraints for Various PDL Specifications of the St. Louis Equation

Specification/ Variable	F-Statistics for Constraints		
	Head	Tail	Head and tail
Specification A			
M	3.22	1.99	1.61
G	3.66	8.42*	4.21*
M and G			3.15*
Specification B			
M	2.40	7.09*	3.59*
G	6.46*	6.86*	4.72*
M and G			3.74*
Specification C			
M	0.81	1.84	1.13
G	1.83	4.11*	2.18
M and G			1.68

*Indicates significance at the 5 percent level.

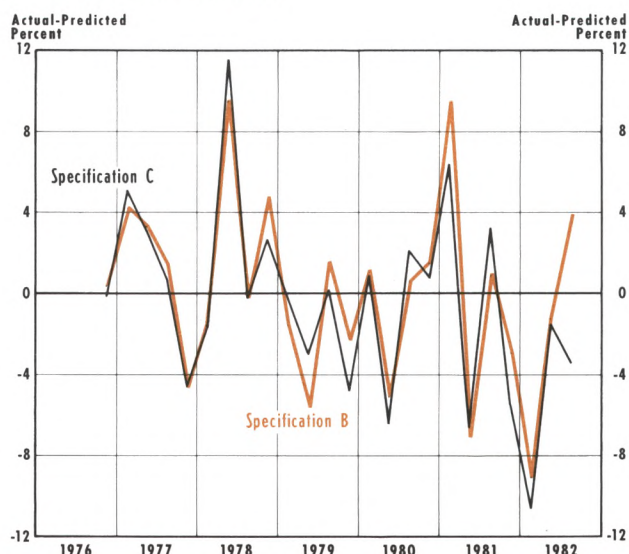
Table 4

Root Mean Square Error of the Forecast for Various Specifications of the St. Louis Equation

Period	A	B	C
IV/1976–III/1982	4.77	4.49	4.70
IV/1976–III/1977	4.13	2.77	2.98
IV/1977–III/1978	3.42	5.31	6.28
IV/1978–III/1979	5.35	3.81	2.02
IV/1979–III/1980	4.17	2.89	4.17
IV/1980–III/1981	6.29	5.96	4.87
IV/1981–III/1982	4.72	5.16	6.25

Chart 1

Forecast Errors of Alternative Specifications of the St. Louis Equation



their subperiod forecast performances.²⁸ The most restricted PDL specification shows an improvement over the current specification, reducing the out-of-sample RMSE by nearly 5 percent over the period and producing a smaller RMSE of the forecast in four of the six subperiods. A graph of the out-of-sample forecast errors for specifications B and C is presented in chart 1. It is clear from chart 1 that both specifications produce similar patterns of forecast errors over the period. The only significant exception occurs in the third quarter of 1982, when specification B underpredicts nominal GNP growth by about as much as specification C overpredicts it.

SUMMARY AND CONCLUSIONS

This paper has investigated the lag length and polynomial degree specifications of the St. Louis equation.

²⁸One could argue that the result may be biased in favor of our PDL specification because the lag structure was chosen over the entire period. Indeed, the lag structure appears to lengthen during the latter part of the sample. The estimated lag structure for the period ending III/1976 was four on \hat{M} and six on \hat{G} . Thus, the lag structure chosen was nearly that of the current specification. The PDL specification was a first degree polynomial on \hat{M} and a sixth degree on \hat{G} . When this specification was used to forecast out-of-sample, it performed somewhat worse than the current specification, with a RMSE of 4.89. Our estimates indicate that the lag structure lengthened when the terminal date of the sample period was extended to III/1979. If the shorter lag structure were used over the first three subperiods and the longer lag structure (specification B) used over the last three, the RMSE for the entire period would be 4.39, somewhat better than either specification alone.

tion to determine whether its conclusions about the long-run efficacy of monetary policy and inefficacy of fiscal policy are affected by the lag length employed or its polynomial distributed lag specification. In so doing, we have employed a computationally efficient method for determining the appropriate lag length and polynomial degree of a general polynomial distributed lag model.

Our results indicate that the important policy conclusions of the St. Louis equation are insensitive to the lag length specified and to the polynomial restrictions imposed. In particular, the long-run effectiveness of money growth and the long-run ineffectiveness of growth in high-employment government expenditures are substantiated by ordinary least squares estimates of model parameters using both the Pagano-Hartley-

determined lag length and the current lag length specifications, as well as by estimates of several PDL specifications. Thus, there is no evidence that the conclusion of the St. Louis equation can be traced to these types of econometric misspecification.

We did find a PDL specification that outperforms the current specification by both in-sample and out-of-sample criteria. This specification has considerably longer lags on both the monetary and expenditure variables and more polynomial restrictions.

Finally, we found that the Pagano-Hartley technique, used in conjunction with standard F-tests, is a convenient and computationally efficient tool for selecting the lag length and polynomial degree of a PDL model.

APPENDIX

Pagano and Hartley have recently developed a methodology for determining the appropriate lag length and degree of polynomial which is computationally efficient.¹ In order to illustrate the use of the Pagano-Hartley (PH) technique, consider the general distributed lag model

$$(A.1) \quad Y_t = \sum_{k=1}^K \mu_k Z_{kt} + \sum_{j=0}^{\ell^*} \beta_j X_{t-j} + \varepsilon_t, \quad t=1, 2, \dots, T,$$

where $\varepsilon_t \sim \text{NID}(0, \sigma^2)$, and where Z_{kt} is the k^{th} independent variable and X_t is an independent variable which affects Y_t with a lag of length ℓ^* .

The polynomial distributed lag (PDL) model involves imposing restrictions on the β coefficients such that

$$\beta_j = \delta_0 + \delta_1 j + \delta_2 j^2 + \dots + \delta_{p^*} j^{p^*}.$$

That is, each of the individual lag weights falls on a polynomial of degree p^* , where $p^* < \ell^*$.² These restrictions can be written more compactly in matrix notation as

$$\underline{\beta} = H \underline{\delta},$$

where $\underline{\beta} = (\beta_0 \beta_1 \dots \beta_{\ell^*})'$, $\underline{\delta} = (\delta_0 \delta_1 \dots \delta_{p^*})'$, and H is

a $(\ell^* + 1)$ by $(p^* + 1)$ matrix of coefficients.³ Substituting the above restrictions into the model, we get

$$(A.1') \quad Y_t = \sum_{k=1}^K \mu_k Z_{kt} + \sum_{q=0}^{p^*} \delta_q X_{qt}^*,$$

where $X_{qt}^* = \sum_{j=0}^{\ell^*} (X_{t-j} h_{j+1, q+1})$ and where $h_{j+1, q+1}$

is the $(j+1)$ th, $(q+1)$ th element of H , $j = 0, 1, 2, \dots, \ell^*$ and $q = 0, 1, 2, \dots, p^*$. It is clear that imposing the polynomial restrictions reduces the number of parameters by $\ell^* - p^*$ and, thus, imposes $\ell^* - p^*$ homogeneous restrictions on the parameter vector $\underline{\beta}$. Thus, estimating equation A.1' is tantamount to estimating equation A.1 subject to homogeneous restrictions of the form $R \underline{\beta} = 0$, where R is a $(\ell^* - p^*)$ by $(\ell^* + 1)$ matrix.⁴ It should be apparent that the validity of the

³Specifically, H takes the general form

$$H = \begin{bmatrix} 1 & 0 & 0 & 0 & \dots & 0 \\ 1 & 1 & 1 & 1 & \dots & 1 \\ 1 & 2 & 4 & 8 & \dots & 2^{p^*} \\ \vdots & & & & & \\ \vdots & & & & & \\ 1 & \ell^* & \ell^{*2} & \ell^{*3} & \dots & \ell^{*p^*} \end{bmatrix}.$$

¹Pagano and Hartley, "On Fitting Distributed Lag Models."

²Strictly speaking, p^* could equal ℓ^* ; however, there would be no polynomial restrictions. Thus, it is doubtful that one would describe a model as a PDL if $p^* = \ell^*$.

⁴There are a number of ways of generating the restriction matrix, R . See Shiller, "A Distributed Lag Estimator," and Judge and others, *The Theory and Practice of Econometrics* (John Wiley and Sons, Inc., 1980), pp. 642-44.

polynomial restrictions, including the endpoint constraints, can be tested easily.⁵

Of course, the correct values of the lag length and degree of the polynomial are generally unknown. Since the selection of an improper lag length or polynomial degree generally leads to biased coefficient estimates, the selection of ℓ and p is extremely important. The selection process, however, is not easy. For one thing, the appropriate lag length cannot be determined using standard procedures if the degree of the polynomial has been selected.⁶ Even though a number of techniques have been suggested for selecting ℓ and p , the PH method was chosen, in part for its computational convenience.⁷

The PH method proceeds by determining the lag length and then the degree of the polynomial. The PH technique can best be illustrated by rewriting equation A.1 in matrix form as

$$(A.2) \quad \underline{Y} = \underline{Z}\underline{\mu} + \underline{X}\underline{\beta} + \underline{\varepsilon},$$

where \underline{Z} and \underline{X} are T by K and T by $(\ell^* + 1)$ matrices of observations on the independent variables, and $\underline{\mu}$ and $\underline{\beta}$ are K by 1 and $(\ell^* + 1)$ by 1 vectors of parameters. The procedure begins by choosing a maximum lag length L . Equation A.2 with the maximum lag length can be rewritten as

$$(A.3) \quad \underline{Y}_L = \underline{W}_L \underline{\psi}_L + \underline{\varepsilon}_L,$$

where $\underline{W}_L = [\underline{Z}; \underline{X}_L]$, and $\underline{\psi}_L = [\underline{\mu}; \underline{\beta}_L]'$. The observation matrix \underline{W}_L is then decomposed to

⁵There are a number of alternative norms for testing these restrictions. See Judge and others, *The Theory and Practice of Econometrics*, p. 646.

⁶This is seen by noting that, once the polynomial degree is selected, alternative lag specifications amount to imposing the polynomial restrictions on different parameter spaces. Thus, restrictions on the lag length are non-nested when p is specified. See Peter Schmidt, "A Modification of the Almon Distributed Lag," *Journal of the American Statistical Association* (September 1974), pp. 679-81; and Hendry and Pagan, "Distributed Lags: A Survey of Some Recent Developments." In this regard, it would be appropriate to use the maximum \bar{R}^2 criterion as Schmidt and Waud do; however, this procedure may lack power. A more useful procedure has been suggested by Pesaran. Neither procedure, however, provides information concerning the degree of polynomial. See Schmidt and Waud, "The Almon Lag Technique"; and M. H. Pesaran, "On the General Problem of Model Selection," *Review of Economic Studies* (April 1974), pp. 153-71.

⁷One attractive method has been suggested by Hendry and Pagan, "Distributed Lags: A Survey of Some Recent Developments." This procedure involves a sequence of hypothesis tests commencing with an initial arbitrary choice of a lag length. While this procedure has potential merit, it is not without its difficulties. Furthermore, it may involve an extremely laborious test procedure when there are two PDL variables, as in the St. Louis equation. For another procedure, see Sargan, "The Consumer Price Equation in the Post War British Economy."

$$\underline{W}_L = \underline{Q}_L \underline{N}_L$$

by the Gram-Schmidt decomposition. Here \underline{Q}_L is a matrix whose columns form an orthonormal basis for the column space of \underline{W}_L , and \underline{N}_L is an upper triangular matrix with positive diagonal elements.⁸ Equation A.3 now can be rewritten as

$$\underline{Y}_L = \underline{Q}_L \underline{\lambda}_L + \underline{\varepsilon}_L,$$

where

$$\underline{\lambda}_L = [\underline{\lambda}^\mu; \underline{\lambda}^\beta]' = \underline{N}_L \underline{\psi}_L.$$

Given that \underline{Q}_L is orthonormal, the least squares estimate of $\underline{\lambda}_L$ is given by

$$\hat{\underline{\lambda}}_L = [\hat{\underline{\lambda}}^\mu; \hat{\underline{\lambda}}^\beta]' = \underline{Q}_L' \underline{Y}_L,$$

and the structural parameters can be obtained from

$$\underline{N}_L \hat{\underline{\psi}}_L = \hat{\underline{\lambda}}_L.$$

An advantage of the PH method comes in noting that the elements of $\hat{\underline{\lambda}}_L$ are mutually independent random variables. In particular,

$$\hat{\lambda}_i^\beta \sim \text{NID}(\lambda_i, \sigma^2), \quad i=0, 1, 2, \dots, \ell^*$$

$$\hat{\lambda}_i^\beta \sim \text{NID}(0, \sigma^2), \quad i=\ell^*+1, \ell^*+2, \dots, L.$$

Pagano and Hartley note that there is a one-to-one correspondence between the null hypothesis involving the β s and the λ s. Given this and the orthogonality of the PH procedure, the following sets of hypotheses are equivalent:

$$H_{L-j}: \beta_L = \beta_{L-1} = \dots = \beta_{L-j} = 0 \quad j=0, 1, 2, \dots, L$$

$$H_{L-j}^*: \lambda_L^\beta = \lambda_{L-1}^\beta = \dots = \lambda_{L-j}^\beta = 0 \quad j=0, 1, 2, \dots, L.$$

Hence, the Gram-Schmidt decomposition provides a convenient basis for testing the null hypothesis that there exists a lag length, ℓ , such that the null hypothesis $\beta_\ell = 0$ can be rejected. If no such ℓ can be found, then there is no distributed lag of X .

The test of the simple hypothesis $\lambda_{L-j}^\beta = 0$ can be carried out by a t -test of the form

$$t_{L-j} = \lambda_{L-j}^\beta / s \quad j=0, 1, 2, \dots, L,$$

where

$$s^2 = \frac{\underline{\varepsilon}_L' \underline{\varepsilon}_L}{T-K-L-1}, \quad \text{and}$$

$$\underline{\varepsilon}_L = \underline{Y}_L - \underline{Q}_L \hat{\underline{\lambda}}_L.$$

⁸The Gram-Schmidt procedure is often used when the observation matrix is ill-conditioned. If the diagonal elements are chosen to be positive, as they are in our case, \underline{Q}_L and \underline{N}_L are unique; see G. A. F. Seber, *Linear Regression Analysis* (John Wiley and Sons, Inc., 1977), chapter 11.

Because of their common divisor, these t-statistics are not independent; however, they are uncorrelated.⁹

Pagano and Hartley also suggest that the above hypotheses are equivalent to

$$H'_{L-j} \lambda_{L-j}^{\beta} = 0 \quad j=0, 1, \dots, L,$$

due to the orthogonality of their procedure. These hypotheses, however, are not equivalent in any direct sense. To see this, recall that

$$\underline{\lambda}_L = N_L \underline{\psi}_L,$$

where N_L is an upper-triangular matrix with positive diagonal elements. The i th row of N_L can be represented as

$$N_L^i = (0, \dots, 0, \eta_{ii}, \eta_{ii+1}, \dots, \eta_{iL}),$$

where η_{ij} is the i th- j th element of N_L . Thus, the hypothesis test that $\lambda_L^{\beta} = 0$ is given by

$$\lambda_L^{\beta} = \eta_L \beta_L = 0.$$

Likewise, the test that $\lambda_{L-1}^{\beta} = 0$ is given by

$$\lambda_{L-1}^{\beta} = \eta_{L-1} \beta_{L-1} + \eta_L \beta_L = 0,$$

and so on. Thus, the hypotheses of H'_{L-j} are really tests of linear combinations of the distributed lag weights, where the particular linear combination is determined by the elements of rows of N_L . In practice we found that the absolute value of the diagonal elements of N_L tended to be somewhat large relative to the off-diagonal elements for the lag length selection and very small relative to the off-diagonal elements in the polynomial selection. In the former case, therefore, testing the hypothesis that $\lambda_j^{\beta} = 0$ was very near testing the hypothesis that $\beta_j = 0$, while in the later case it was closer to the null hypothesis H_{L-j}^* .

Given this, we decided to supplement the use of t-tests on the λ s with conventional F-tests of the equivalent hypotheses of H and H^* . We recommend that one investigate the N_L matrix to identify the nature of the hypotheses being tested when using the PH t-statistics.

We should note also that the use of the PH method is complicated somewhat by the presence of two distributed lag variables on the right-hand side. One can readily see that, in view of the upper-triangular form of N_L , hypothesis tests involving a second distributed lag will not be consistent with H_{L-j}^* unless the Gram-Schmidt procedure is applied to each set of distributed lag regressors separately. Unfortunately, the resulting

sets of jointly orthogonal regressors will not themselves be orthogonal to each other. As an alternative, we ran two separate Gram-Schmidt regressions with each distributed lag variable entered last. Furthermore, we did this by reducing by one the lag length or polynomial degree for one variable and holding the maximum lag length or polynomial degree for the other variable (which was entered last) constant. In this way, we determined whether the lag length chosen for one variable was affected by the lag length specified for the other. Of course, we were particularly concerned that the lag length selected for one be the same if the chosen lag length of the other was used instead of L . The procedure had the added advantage of allowing us to calculate an L by L matrix of F-statistics for all possible combinations of lag structures (or in the case of PDL selection, degrees of polynomials) from L orthogonal regressions.¹⁰

Hypothesis Testing Considerations

When determining the "correct" lag length using either the t-tests or the F-test, care must be taken in choosing a critical value on which to test the null hypothesis. Two considerations are important. First, the null hypotheses

$$H_{L-j}^*: \lambda_{L-j}^{\beta} = 0 \quad j = 0, 1, 2, \dots, L$$

represent a set of sequential hypotheses. It is usually assumed that these hypotheses are nested so that if any one is true, the preceding hypotheses must be true also and, if any one is false, so must be the succeeding ones. Thus, the null hypothesis becomes more restricted as each successive test is conducted, and the probability of committing a Type I error increases. If we let ζ_j denote the significance level of the j th test, it can be shown that the probability of committing a Type I error for the j th test, α_j , is

$$\alpha_j = \begin{cases} \zeta_1 & \text{if } j = 1 \\ \zeta_j(1 - \alpha_{j-1}) + \alpha_{j-1} & \text{if } j \geq 2. \end{cases}$$

Thus, the probability of rejecting the null hypothesis when it is true will rise as the length of the lag is reduced. Anderson suggested that one would like to balance the desirability of not overestimating the lag length with the sensitivity to non-zero coefficients.¹¹ He recommends setting L fairly large, but letting ζ_j be

¹⁰This can be seen by noting that the RSS when j lags are omitted is given by

$$RSS_{L-j} = \underline{Y}_L' \underline{Y}_L - \sum_{k=1}^K (\hat{\lambda}_k^{\beta})^2 - \sum_{k=0}^{L-j-1} (\hat{\lambda}_k^{\beta})^2.$$

¹¹Anderson also provides a test procedure for orthogonal regressors which have some optimal properties; however, the test is somewhat cumbersome. See T. W. Anderson, *The Statistical Analysis of Time Series* (John Wiley and Sons, Inc., 1971), pp. 30-43.

⁹This permits the use of t-tables from Seber. See Seber, *Linear Regression Analysis*, pp. 404-5.

small for j near L . While no optimal rules exist, Anderson suggests

$$(A.4) \quad \zeta_j = \frac{\zeta(L+1-j)}{L}, \quad j = 1, 2, 3, \dots, L$$

for subsequent tests. An alternative would be to use the t -tables from Seber.

In addition to the above problem, we have the problem that an estimator based on a prior test is a preliminary test estimator. While nothing is known about such estimators when the sequence of tests is greater than one, it is known that, in the case of one pre-test, the estimator has a risk function which may exceed that of OLS.¹² Furthermore, the difference between the risk of the preliminary test estimator and OLS increases as the significance level is reduced. While the optimal critical value will vary with the particular choice of loss function, the evidence suggests that standard significance levels of 5 or 10 percent may be below the optimal level for one pre-test.¹³ These considerations, coupled with the fact that overestimates of the lag length are less likely to result in bias than underestimates, suggest that one may want to consider an initial value of the significance level that is fairly large.¹⁴

POLYNOMIAL DEGREE SELECTION

Having selected a lag length, ℓ , the next step is to determine a polynomial degree, p . This can be accom-

¹²The risk function is $E[(\varphi^* - \varphi)'X'X(\varphi^* - \varphi)]$, where φ^* is the pre-test estimator of φ .

¹³For example, Sawa and Hiromatsu have shown that the standard critical values of the t -statistic are substantially above the optimal critical values in the case of a mini-max regret loss function with one restriction. On the other hand, Toyoda and Wallace have shown that OLS should always be chosen when the number of linearly independent restrictions are less than five if one wishes to minimize the average regret. See Takamitsu Sawa and Takeshi Hiromatsu, "Minimax Regret Significance Points for a Preliminary Test in Regression Analysis," *Econometrica* (November 1973), pp. 1093-1101; and T. Toyoda and T. D. Wallace, "Optimal Critical Values for Pre-Testing in Regression," *Econometrica* (March 1976), pp. 365-75.

¹⁴To guard against incorrectly excluding components of the distributed lag or imposing invalid polynomial restrictions, an initial significance level of 15 percent was chosen. The critical t -values for testing each successive hypothesis are as follows:

j	t -value
1	1.46
2	1.51
3	1.56
4	1.61
5	1.67
6	1.74
7	1.81
8	1.90
9	2.00
10	2.12
11	2.30
12	2.57

plished by simply re-applying all of the procedures outlined above to the PDL model with lag length ℓ . To see this, write the model with the selected lag length as

$$(A.5) \quad \underline{Y}_\ell = \underline{Z}_\ell \underline{\mu} + \underline{X}_\ell \underline{\beta}_\ell + \underline{\varepsilon}_\ell.$$

Recall that $\underline{\beta} = H\underline{\delta}$ where H is $(\ell+1)$ by (p^*+1) and $\underline{\delta}$ is (p^*+1) by 1. Thus, this equation can be rewritten as

$$(A.6) \quad \underline{Y}_\ell = \underline{Z}_\ell \underline{\mu} + \underline{X}_\ell H \underline{\delta} + \underline{\varepsilon}_\ell$$

or

$$(A.6') \quad \underline{Y}^* = \underline{Z}_\ell \underline{\mu} = \underline{X}^* \underline{\delta} + \underline{\varepsilon}^*.$$

It is clear from this expression that the choice of a polynomial degree p is completely analogous to the choice of the lag length above, where the maximum degree of the polynomial considered, p , initially is set equal to ℓ .¹⁵

EMPIRICAL RESULTS

In applying the PH technique, we initially chose a maximum lag length of 12; however, we also considered $L = 16$. The PH t -statistics for those runs with both \dot{M} and \dot{G} last are given in table A.1. This procedure chose 10 lags on \dot{M} and 9 on \dot{G} for $L = 12$ and 16. We then chose these lags for one variable and let the other be set at $L = 12$. The results were unchanged. These results also appear in table A.1. Furthermore, F -tests of the restrictions implied by this section were basically consistent with the PH results, when L was set at 12 (see footnote 24 of the text). This was not true, however, for $L = 16$. In this instance, the presence of a number of insignificant coefficients prior to the first significant one diluted the calculated F -statistic so that a very short lag would have been chosen by an F -test. Thus, the PH t -statistics appear to be less sensitive to the choice of L than the standard F -test.

Letting the maximum degree polynomial be 10 for \dot{M} and 9 for \dot{G} , we then re-applied the PH technique to

¹⁵Pagano and Hartley offer an equivalent two-step procedure, which is not discussed here. See Pagano and Hartley, "On Fitting Distributed Lag Models Subject to Polynomial Restrictions." As an efficient alternative to either of these approaches, one could employ the stochastic information from the lag length selection process with the nonstochastic information in the design matrix in a Theil-Goldberger mixed estimation procedure similar to Schiller's Bayesian method. Fomby has shown that such stochastic restrictions can be tested under a generalized mean square error norm. See H. Theil and A. S. Goldberger, "On Pure and Mixed Statistical Estimation in Economics," *International Economic Review* (January 1961), pp. 65-78; Thomas B. Fomby, "MSE Evaluation of Shiller's Smoothness Priors," *International Economic Review* (February 1979), pp. 203-15; and Judge and others, *The Theory and Practice of Econometrics*, pp. 652-53.

Table A.1

Pagano-Hartley t-statistics for Lag Length Selection

Lag	\dot{M} with ℓ on \dot{G} equal to			\dot{G} with ℓ on \dot{M} equal to		
	16	12	9	16	12	10
0	4.84	5.45	5.42	2.68	2.67	2.72
1	4.49	4.33	4.61	1.04	1.13	1.16
2	2.51	2.36	2.24	-1.84	-1.89	-1.90
3	-2.20	-1.73	-1.71	0.97	0.96	1.01
4	0.28	0.09	0.60	0.23	0.17	0.19
5	-1.96	-2.05	-2.11	-0.89	-1.21	-1.22
6	-0.42	-0.01	-0.47	1.34	1.37	1.41
7	-0.42	-0.61	-0.43	0.58	0.44	0.44
8	0.77	0.88	1.22	-2.30	-2.38	-2.34
9	-0.50	0.10	-0.13	-2.22*	-2.22*	-2.32*
10	-2.58*	-2.70*	-2.72*	-0.30	-0.58	-0.65
11	0.09	-0.13	0.19	0.93	1.18	1.20
12	-0.10	0.17	0.31	0.98	0.64	0.68
13	-0.57			1.15		
14	0.41			1.01		
15	-0.82			-1.24		
16	0.19			1.28		

*First significant t-statistic

Table A.2

Pagano-Hartley t-statistics for Polynomial Degree Selection

Polynomial Degree	\dot{M} with p on \dot{G} equal to 9	\dot{G} with p on \dot{M} equal to 10
0	3.44	-0.27
1	-5.73	-2.85
2	2.84	-0.17
3	-2.17	-2.77
4	-2.34	0.73
5	-0.48	1.05
6	-2.32	1.12
7	0.44	2.55
8	1.11	-1.65*
9	-1.85*	-0.47
10	1.26	

*First significant t-statistic.

determine the polynomial degree. The PH t-statistics are presented in table A.2. The PH technique selected a ninth degree polynomial on money and an eighth degree polynomial on government expenditures for the same significance level as used before. When we re-estimated the equation on the lower degree polyno-

mials, however, the coefficient of the eighth degree on \dot{G} failed to be significant. The seventh was significant, regardless of the lag length on \dot{M} . Thus, the PH technique suggests a ninth degree polynomial on \dot{M} and a seventh degree on \dot{G} . This implies only one polynomial restriction on \dot{M} and two on \dot{G} . (An F-test of these restrictions could not reject the null hypothesis. The calculated F-statistic was 1.43.)

Furthermore, the matrix of F-statistics of all possible polynomial restrictions on a PDL model with 10 lags on \dot{M} and 9 on \dot{G} , given in table A.3, suggests that even more restricted models could pass an F-test. Clearly, a number of different polynomial degree specifications satisfy an F-test at the 5 percent level. We can see, for example, that had we chosen the polynomial degree on \dot{M} first and then selected the polynomial degree on \dot{G} , we would have chosen a fourth degree polynomial on \dot{M} and an eighth degree polynomial on \dot{G} .

Alternatively, had we investigated \dot{G} first, we would have chosen a seventh degree polynomial on \dot{G} and a sixth on \dot{M} . These are circled in table A.3. We could also choose the polynomial degree by selecting the most restricted model that passes an F-test at, say, the 5 percent level. This criterion would select a sixth degree polynomial on \dot{M} and a third degree on \dot{G} . This F-statistic is bracketed in table A.3. All four of these

Table A.3

F-statistics for Testing Polynomial Restrictions on \dot{M} and \dot{G}

Degrees for \dot{M}	Degrees for \dot{G}									
	0	1	2	3	4	5	6	7	8	9
0	4.09	4.13	4.38	4.53	4.75	5.08	5.47	5.62	5.76	6.32
1	3.00	2.64	2.80	2.82	2.92	3.10	3.32	3.20	3.05	3.37
2	2.78	2.46	2.61	2.58	2.65	2.79	2.99	2.87	2.50	2.79
3	2.80	2.46	2.63	2.54	2.57	2.64	2.82	2.68	2.24	2.51
4	2.49	2.13	2.30	2.10	2.21	2.26	2.43	2.13	(1.76)	2.02
5	2.61	2.26	2.45	2.28	2.41	2.49	2.69	2.40	2.02	2.37
6	2.58	2.17	2.37	[1.96]	2.10	2.21	2.37	(1.46)	1.33	1.62
7	2.77	2.35	2.59	2.14	2.33	2.51	2.77	1.74	1.63	2.09
8	3.02	2.56	2.84	2.27	2.54	2.83	3.20	1.82	1.75	2.52
9	3.03	2.48	2.79	2.09	2.37	2.63	2.94	1.43	0.87	1.60
10	3.13	2.51	2.86	2.06	2.37	2.69	3.16	1.48	0.22	—

PDL specifications — the one selected by the PH technique and the three indicated in table A.3 — were estimated; however, only the results for the one selected by the PH technique and the most restricted specification are presented in this paper. The results of the other specifications were similar to those of the

most restricted PDL specification and, hence, are not reported here.¹⁶

¹⁶The hypothesis tests concerning the effects of monetary and fiscal policy yielded conclusions identical to those reported here. The out-of-sample RMSEs of the forecast for the period III/1976–III/1982 were smaller than the RMSEs of specifications A or C.



Weekly Money Supply Forecasts: Effects of the October 1979 Change in Monetary Control Procedures

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THE activity of most financial market participants on Friday afternoons can be predicted with great accuracy: they anxiously will be awaiting the 4:15 p.m. EST announcement of the new weekly money stock data. Despite the fact that the weekly data are contaminated by a great deal of "noise," a fact that greatly reduces the data's usefulness in revealing any policy trend, market participants still wager large sums and reputations on correctly anticipating the elusive weekly money figure.¹

The impact of unanticipated changes in the weekly money supply on short-term interest rates has been investigated extensively. In general, the evidence shows a positive relationship between unanticipated changes in money and movements in market rates.² Although this empirical relationship existed through-

out the 1970s, the relative impact of weekly money "surprises" on short-term interest rates has been greater since the October 1979 change in monetary control procedures. In fact, over 25 percent of the volatility of the 3-month Treasury bill rate during the time period of the money supply announcement can be attributed directly to the increased volatility of unanticipated weekly changes in money since October 1979.³ Moreover, unanticipated money supply changes that lie outside the Federal Reserve's announced money growth range appear to have a relatively greater effect on interest rates than money surprises falling within the announced growth range.⁴

The evidence clearly indicates that unanticipated changes in the money stock have an important effect on interest rates. Consequently, examining the characteristics of the money supply forecasts that give rise to such behavior is important. Several studies have examined the weekly money supply forecasts for the period prior to October 1979; but little has been done on comparing the forecasts across the announced change in monetary control procedures.⁵ The purpose of this article is to analyze the effects of the October

¹See David A. Pierce, "Trend and Noise in the Monetary Aggregates," in Federal Reserve Staff Study, *New Monetary Control Procedures, vol. II* (February 1981), especially pp. 19–22. Pierce estimates that the noise in weekly money data is around \$3 billion dollars, assuming an aggregate level of \$400 billion. As he notes, "In general, these results are further evidence that very little can be inferred from any but the most atypical movements in weekly data" (p. 22).

²See, for example, Jacob Grossman, "The 'Rationality' of Money Supply Expectations and the Short-Run Response of Interest Rates to Monetary Surprises," *Journal of Money, Credit and Banking* (November 1981), pp. 409–24; V. Vance Roley, "The Response of Short-Term Interest Rates to Weekly Money Announcements," Working Paper No. 82-06, Federal Reserve Bank of Kansas City (September 1982); Thomas Ulrich, "The Information Content of Weekly Money Supply Announcements," *Journal of Monetary Economics* (July 1982), pp. 73–88; and Thomas J. Ulrich and Paul Wachtel, "Market Response to the Weekly Money Supply Announcements in the 1970s," *Journal of Finance* (December 1981), pp. 1063–72. For another interpretation, see Bradford Cornell, "Money Supply Announcements and Interest Rates: Another View," *Journal of Business* (January 1983), pp. 1–23.

³Roley, "The Response of Short-Term Interest Rates."

⁴Ibid. See also, Neil G. Berkman, "On the Significance of Weekly Changes in M1," *New England Economic Review* (May–June 1978), pp. 5–22.

⁵Studies investigating the forecasts prior to the October 1979 policy shift are Grossman, "The 'Rationality' of Money Supply Expectations," and Thomas Ulrich and Paul Wachtel, "The Structure of Expectations of the Weekly Money Supply Announcement," (New York University, February 1982; processed). Roley, "The Response of Short-Term Interest Rates," provides some evidence on this issue for the period February 1980 to November 1981.

1979 change in monetary control on the weekly money supply forecasts. Under the assumption of rational expectations, a change from one recognized monetary control procedure to another should have no effect on the forecast characteristics.⁶ In other words, a change from one monetary control procedure to another should not affect the unbiased and efficiency aspects of the forecasts. If, however, the new procedure is not “well-defined” — that is, the rules of the game are changing constantly — then weekly money supply forecasts may appear biased and inefficient.⁷

WHAT DOES “RATIONALITY” IMPLY?

The theory of rational expectations is based on the premise that market participants construct forecasts of the future in a manner that fully reflects the relevant information available to them. Because wealth-maximizing individuals will not make forecasts that are continually wrong *in the same direction*, the rational expectations approach suggests that forecasts of economic phenomena should be unbiased. Moreover, if the forecast errors could not have been reduced by using other available information, then forecasters have efficiently utilized the relevant data at their disposal.

The issue investigated here is whether the weekly forecasts of the M1 money stock change have been affected noticeably by the October 1979 change in monetary control procedures. More specifically, the question asked is: assuming rational expectations, has

the change in monetary control procedures affected the unbiased and efficiency characteristics of the weekly money supply forecasts? If the forecasts from the post-October 1979 period are not different than those from before, we then would conclude that the forecasters have adapted to the new policy regime. If they differ, however, the evidence would not reject the hypothesis that they have been unable to ascertain the policymaker's behavioral rule.⁸

Three sample periods are used in the following analysis. The full period is from the week ending January 11, 1978, to the week ending June 16, 1982. Given the change in operating procedures in late 1979, the relevant subperiods are from the week ending January 11, 1978, to the week ending October 3, 1979, and from the week ending October 10, 1979, to the week ending June 16, 1982.⁹ With these sample periods, the unbiased and efficiency characteristics of the weekly money supply forecasts across the change in monetary control procedures can be investigated.

Weekly Money Supply Data

The money data series used in this article are the actual and expected, initially announced week-to-

⁶The concept of rational expectations is based on the belief that economic agents are utility maximizers. Thus, market participants form expectations that fully reflect all available information. More formally, rational expectations imply that individuals' subjective probability distribution of possible outcomes is identical to the objective probability distributions that actually occur. Consequently, the only way policymakers can affect behavior is to “fool” the people in an inconsistent manner. This concept is developed more fully in John F. Muth, “Rational Expectations and the Theory of Price Movements,” *Econometrica* (July 1961), pp. 315–35; Robert E. Lucas, Jr., “Expectations and the Neutrality of Money,” *Journal of Economic Theory* (April 1972), pp. 103–24; Robert J. Barro, “Rational Expectations and the Role of Monetary Policy,” *Journal of Monetary Economics* (January 1976), pp. 1–32; and Thomas J. Sargent and Neil Wallace, “Rational Expectations, the Optimal Monetary Instrument, and the Optimal Money Supply Rule,” *Journal of Political Economy* (April 1975), pp. 241–54.

⁷Implicit in this is the presumption that market participants will expend resources to decipher the new policy procedures and adapt their forecast formation process accordingly. This does not seem unreasonable given the sophistication of financial market analysts in gauging actual Federal Reserve behavior. For a discussion on the transition from one policy to another and the implications for rational expectations, see Benjamin M. Friedman, “Optimal Expectations and the Extreme Information Assumptions of ‘Rational Expectations’ Macromodels,” *Journal of Monetary Economics* (January 1979), pp. 23–41.

⁸The dilemma facing market participants is known as the “Lucas problem.” Essentially, even though individuals act rationally in making their forecasts — that is, use all of the information thought to be relevant — failure to account for a procedural shift will lead to incorrect forecasts. Thus, forecasting guidelines used under one procedure may not apply under another. For the specific problem tested here, it may be the case that the announced policy differs from that actually followed. If policy actions are not characterized easily, that is, if policy is unpredictable, then forecasts may be biased and inefficient simply because agents have not determined the structure of the model. For a discussion of this concept, see Robert E. Lucas, Jr., “Econometric Policy Evaluation: A Critique,” in Karl Brunner and Allan H. Meltzer, eds., *The Phillips Curve and Labor Markets*, The Carnegie-Rochester Conference Series on Public Policy (vol. 1, 1976), pp. 19–46.

Bradford Cornell recently has argued that apparent irrational behavior on the part of market participants evidenced by biased and inefficient forecasts, may very well be due to the change from a predictable policy regime to one that continues to be unpredictable. As he states, “On October 6 [1979], market participants suddenly discovered that even the rules of the game were subject to change. As a result, they began studying weekly money supply figures not only with the goal of determining what the current policy was, but also with the goal of determining how the rules of the game might be changed.” In this sense, market participants face a perpetual “Lucas problem.” See Cornell, “Money Supply Announcements and Interest Rates: Another View,” p. 21.

⁹Note that the post-October 1979 period includes the period of credit controls, essentially the second quarter of 1980. This period is included because an examination of the error pattern from weekly money forecasts indicated no difference between this period and any other. Moreover, market participants continued to forecast weekly money changes throughout the control period.

week changes in the narrowly defined money stock (M1). Figures for the actual changes in M1 are taken from the Federal Reserve's H.6 weekly statistical release. Because the sample covers a period of changing definitions, the following guideline is used: From January 11, 1978, to January 31, 1980, the weekly money supply changes are based on the old definition of M1. From February 8, 1980, to November 20, 1981, the money stock is defined as the actual M1B measure, *not* the M1B figure that was adjusted for NOW account movements. Finally, from November 27, 1981, to June 16, 1982, the data are based on the then-current definition of M1.

The data used as a measure of the market's forecasts were obtained from Money Market Services, Inc.¹⁰ Since 1977 this firm has conducted a weekly telephone survey of 50 to 60 government securities dealers to get their expectations of the impending change in money. Prior to early 1980, the poll was conducted twice a week, on Tuesdays and Thursdays. Since then, however, only the Thursday survey has been conducted consistently, because of the shift in the Federal Reserve's announcement of the weekly money supply figures from Thursday to Friday afternoon. For our purposes, therefore, we employ the mean of the Thursday survey responses.¹¹

Are Weekly Money Forecasts Unbiased?

Forecasts of weekly changes in the money stock are unbiased predictors of the actual change if the actual and forecasted values differ only by some random term. Mathematically, this requirement can be stated as

$$(1) \Delta M_t = {}_{t-1}\Delta M_t^E + \varepsilon_t$$

where ΔM_t is the actual change in the money stock, ${}_{t-1}\Delta M_t^E$ is the expectation held in period $t-1$ for the change in the money stock in period t , and ε_t is a random error term with zero mean and variance σ_ε^2 .

¹⁰It has been argued that survey data are not good measures of the market's expectations of some macroeconomic variable. This argument is founded on the belief that most survey respondents are not actual market participants. In other words, their responses to the survey are not based on some profit-maximizing behavior that has generated the forecast. The weekly money forecasts used here are taken from dealers actively participating in the financial market, thus reducing the force of this criticism. See Edward J. Kane and Burton G. Malkiel, "Autoregressive and Nonautoregressive Elements in Cross-Section Forecasts of Inflation," *Econometrica* (January 1976), pp. 1-16.

¹¹For an analysis of the Tuesday and Thursday forecasts, see Grossman, "The 'Rationality' of Money Supply Expectations." This analysis covers only the period 1977 to 1979.

To test for the absence of bias, equation 1 is rewritten and estimated as

$$(2) \Delta M_t = \alpha_0 + \beta_1 {}_{t-1}\Delta M_t^E + \varepsilon_t$$

where α_0 and β_1 are the parameters to be estimated.¹² In this form, the weekly money forecasts are unbiased predictors of actual money supply changes if the *joint* hypothesis that $\hat{\alpha}_0 = 0$ and $\hat{\beta}_1 = 1$ cannot be rejected. Moreover, the estimated residuals from this regression ($\hat{\varepsilon}_t$) should not exhibit serial correlation if the forecasts are unbiased predictions of the actual change in money.

Table 1 presents the regression results from estimating equation 2 using the expected and actual money stock changes. The full-period results suggest that the forecasts of weekly changes in the money stock are unbiased predictors of the actual changes. The calculated F-statistic does not exceed the critical value of 3.04 at the 5 percent significance level. Consequently, the null joint hypothesis that $\hat{\alpha}_0 = 0$ and $\hat{\beta}_1 = 1$ is not rejected. Moreover, the residuals of the equation show no indication of first-order serial correlation, as evidenced by the Durbin-Watson statistic. Thus, the weekly money supply forecasts appear to be unbiased across the full sample.

To see if the forecasts are unbiased before and after the October 1979 change in monetary control procedures, equation 2 was re-estimated for the two periods January 11, 1978, to October 3, 1979, and October 10, 1979, to June 16, 1982. These regression results also are reported in table 1.¹³

The estimates from the pre-October 1979 period again indicate that the forecasts are unbiased. The calculated F-statistic is not statistically significant, and the Durbin-Watson statistic again indicates no first-order serial correlation among the residuals. In contrast, the post-October 1979 regression results permit us to reject the hypothesis that the forecasts are unbiased predictors of the actual changes. Although the estimated constant term is statistically insignificant, the hypothesis that the estimated slope term ($\hat{\beta}_1$) does not differ from unity is rejected easily ($t = 2.33$). Consequently, the joint hypothesis underlying this

¹²This type of test is used widely in studies of expectations data. For studies examining money stock forecasts, see, for example, Grossman, "The 'Rationality' of Money Supply Expectations;" Urich and Wachtell, "The Structure of Expectations;" and Roley, "The Response of Short-Term Interest Rates."

¹³This dichotomization of the sample is supported statistically by Chow-test results: the calculated F-value is $F(2,228) = 3.93$, which exceeds the critical 5 percent level.

Table 1

Test Results for Bias

Equation Estimated: $\Delta M_t = \alpha_0 + \beta_1 \Delta M_{t-1}^E + \varepsilon_t$

Period	Estimated coefficients ¹		Summary statistics ²		
	α_0	β_1	\bar{R}^2	DW	F ³
1/11/78–6/16/82	–0.044 (0.30)	1.207 (10.43)	0.32	1.87	1.65
1/11/78–10/3/79	–0.352 (1.65)	1.060 (6.54)	0.32	1.89	1.60
10/10/79–6/16/82	0.181 (0.91)	1.373 (8.60)	0.35	1.86	3.70

¹Absolute value of t-statistics appear in parentheses.² \bar{R}^2 is the adjusted coefficient of determination; DW represents the Durbin-Watson test statistic. The reported F-statistic is used to test the null hypothesis that $(\alpha_0, \beta_1) = (0, 1)$.³The relevant 5 percent critical F-values are: January 11, 1978, to June 16, 1982 — 3.04; January 11, 1978, to October 3, 1979 — 3.10; and October 10, 1979, to June 16, 1982 — 3.07.

test also is rejected; the calculated F-statistic of 3.70 exceeds the 5 percent critical value of 3.07. Thus, the evidence suggests that forecasts of weekly money supply changes have been biased since the October 1979 change in implementing monetary policy.

Are Weekly Money Forecasts Efficient?

The efficiency condition requires that forecasts fully reflect all pertinent and readily available information.¹⁴ Since the information available to individuals includes the past history of the series being forecast, it is possible to test the hypothesis that the forecasts are “weakly” efficient; that is, at least the information contained in the history of weekly money supply changes is used efficiently. This concept of efficiency requires that the process actually generating observed changes in weekly money and the process generating the forecasts of these changes are the same. The simplest process to assume is an autoregressive one, where observed and expected changes are generated solely by the past history of the series itself. Mathematically, this concept of efficiency can be stated as

$$(3) \Delta M_t = \sum_{i=1}^n \beta_i \Delta M_{t-i} + \mu_{1t},$$

¹⁴Of course, additional information will be acquired only if the marginal benefits are at least as large as the marginal costs of acquisition. A useful discussion of this point is provided in Armen A. Alchian, “Information Costs, Pricing, and Resource Unemployment,” in Edmund S. Phelps, and others, *Microeconomic Foundations of Employment and Inflation Theory* (W. W. Norton & Company, Inc., 1970), pp. 27–52.

$$(4) \Delta M_t - \Delta M_t^E = \sum_{i=1}^n \beta_i' \Delta M_{t-i} + \mu_{2t},$$

where μ_{1t} and μ_{2t} are random error terms. In this format, weak-form efficiency requires that $\beta_i = \beta_i'$ for all i ; $i = 1, 2, \dots, n$.¹⁵

To determine if survey respondents efficiently utilized the information contained in past weekly money supply changes, equation 4 is subtracted from equation 3, yielding the estimated equation

$$(5) \Delta M_t - \Delta M_t^E = b_0 + \sum_{i=1}^n b_i \Delta M_{t-i} + \phi_t,$$

where the dependent variable $\Delta M_t - \Delta M_t^E$ represents the forecasters' errors in predicting weekly money changes, and the independent variables, ΔM_{t-i} , are the actual changes in money.¹⁶ The equation permits a constant term (b_0) to be estimated instead of subsuming it into the error structure, which is represented by the term $\phi_t (= \mu_{1t} - \mu_{2t})$. The null hypothesis to be tested is that the estimated $b_i (= \beta_i -$

¹⁵This form of the efficiency test was proposed in James E. Pesando, “A Note on the Rationality of the Livingston Price Expectations Data,” *Journal of Political Economy* (August 1975), pp. 849–58.

¹⁶The lagged values of data used in the efficiency test are the one-week revised numbers, not the initially reported weekly figures. Since the revised figures contain more information than the originally released data — the data contained in the revision itself — using original data would deprive forecasters of some information. It should be noted, however, that the conclusions reached were not affected when originally reported data was used to generate lagged changes in the money stock.

Table 2
Test Results for Weak-Form Efficiency

$$\text{Equation Estimated: } \Delta M_t - {}_{t-1}\Delta M_t^E = b_0 + \sum_{i=1}^4 b_i \Delta M_{t-i} + \phi_t$$

Period	Estimated coefficients ¹					Summary statistics ²		
	b ₀	b ₁	b ₂	b ₃	b ₄	\bar{R}^2	DW	F ³
1/11/78–6/16/82	0.139 (0.87)	–0.042 (0.69)	–0.067 (1.14)	–0.087 (1.48)	0.005 (0.09)	0.01	1.92	0.81
1/11/78–10/3/79	–0.259 (1.24)	0.026 (0.28)	–0.077 (0.84)	–0.021 (0.23)	–0.017 (0.20)	0.01	1.95	0.28
10/10/79–6/16/82	0.398 (1.77)	–0.071 (0.90)	–0.068 (0.90)	–0.112 (1.48)	0.007 (0.10)	0.02	1.93	0.82

¹See notes accompanying table 1.

²See notes accompanying table 1. The reported F-statistic is used to test the null hypothesis that \hat{b}_i ($i = 1, 2, 3, 4$) = 0.

³The relevant 5 percent critical F-values are: January 11, 1978, to June 16, 1982 — 2.41; January 11, 1978, to October 3, 1979 — 2.48; and October 10, 1979, to June 16, 1982 — 2.44.

β'_i) are not statistically different from zero for all i ($i = 1, 2, \dots, n$) as a group. Moreover, the estimated error structure should not exhibit serial correlation.¹⁷

Table 2 presents the results of estimating equation 5 for the period January 11, 1978, to June 16, 1982. Four lags were chosen to capture the informational content of past changes in weekly money. The regression results indicate that past changes in the money supply do not explain any significant portion of the forecast error. The calculated F-statistic (0.81) is far below acceptable critical values. The Durbin-Watson statistic also indicates that serial correlation is not present among the residuals. Thus, for the full period, we cannot reject the hypothesis that forecasters efficiently used the information contained in past changes in the money stock in forming their predictions.

We next test the efficiency hypothesis for the pre- and post-October 1979 periods; these empirical results also are found in table 2. In both instances, we again cannot reject the hypothesis that past information about weekly money changes was used efficiently. Neither F-statistic is significant at the 5 percent level. Based on these results, therefore, the weak-form efficiency hypothesis is not rejected by the data, regardless of the sample used.

Tests of Stronger-Form Efficiency

The above evidence suggests that forecasts of weekly money stock changes are weakly efficient. Efficiency, however, also may be considered in a broader sense. This broader efficiency criterion requires that forecasts incorporate all of the relevant and available information. Thus, similar to the previous hypothesis, efficiency in the broad sense requires that the forecast errors be orthogonal, or systematically unrelated to all relevant available information sets.¹⁸

To test this concept of efficiency, we estimate the equation

$$(6) \quad \Delta M_t - {}_{t-1}\Delta M_t^E = c_0 + \sum_{i=0}^n c_i I_{t-i} + w_t,$$

where I_{t-i} refers to lagged values ($i = 0, 1, \dots, n$) of information that are not incorporated in past money stock changes, and w_t is another random error term. The analysis is intended to determine whether the survey respondent's weekly errors in forecasting money supply changes can be explained by some set(s) of information that are readily available. If the esti-

¹⁷See Donald J. Mullineaux, "On Testing for Rationality: Another Look at the Livingston Price Expectations Data," *Journal of Political Economy* (April 1978), pp. 329–36 for a discussion of this test.

¹⁸Tests using this stronger form of efficiency are presented in Grossman, "The 'Rationality' of Money Supply Expectations," and, using interest rate expectations data, in Benjamin M. Friedman, "Survey Evidence on the 'Rationality' of Interest Rate Expectations," *Journal of Monetary Economics* (October 1980), pp. 453–65, where the phrase "information orthogonality" was coined.

Table 3
Test Results for Stronger-Form Efficiency

$$\text{Equation Estimated: } \Delta M_t - {}_{t-1}\Delta M_t^E = c_0 + \sum_{i=0}^n c_i I_{t-i} + w_t$$

Information set	Calculated F-statistics		
	Period		
	1/11/78–6/16/82	1/11/78–10/3/79	10/10/79–6/16/82
Consumer and industrial loans	3.55 ¹	1.74	2.94 ¹
Demand deposits at large weekly reporting banks	4.25 ¹	0.51	3.57 ¹
Float	0.60	0.55	0.61
Adjusted base	3.29 ¹	0.55	3.65 ¹

¹Significant at the 5 percent level of confidence. The relevant critical F-values are: January 11, 1978, to June 16, 1982 — 2.26; January 11, 1978, to October 3, 1979 — 2.32; and October 10, 1979, to June 16, 1982 — 2.28.

mated c_i coefficients are not significantly different from zero as a group, then we cannot reject the stronger-form hypothesis of efficiency. If contrary evidence is found, then the results would suggest that forecasters could have reduced their prediction errors by using the information sets investigated here.

It is, of course, impossible to account for every imaginable information set that each forecaster could have used. Consequently, we analyze several sets of information that are available on a timely basis and are potentially useful in estimating future money stock developments. The information sets used are consumer and industrial loans, demand deposits at large weekly reporting banks, float and the adjusted monetary base as defined by the Federal Reserve Bank of St. Louis. In all cases, the data used are taken from original Federal Reserve statistical releases that were available to forecasters prior to the weekly money stock announcements.¹⁹ Although we realize that the series

chosen do not exhaust the set of possible information sources, they are sufficiently broad to test the hypothesis at hand.

Table 3 reports the calculated F-statistics from estimating equation 6 using the different information sets. In each test, the information set contains contemporaneous and four lagged terms. The outcome for the full period suggests that forecasters efficiently utilized the information contained in the float information set: the reported F-statistic is not large enough to reject the null hypothesis. The results for the other information sets — consumer and industrial loans, demand deposits at large weekly reporting banks and the adjusted base — reject the efficiency hypothesis. For these, the F-statistics exceed the 5 percent critical value (2.26), implying that forecast errors could have been lessened if the information contained in these data had been used.

Equation 6 was re-estimated for the pre- and post-October 1979 periods; these results also are found in table 3. The full-period results are dominated by the post-October 1979 period. Prior to the shift in control procedures, forecasters' predictions of weekly money supply changes appear to have efficiently incorporated the information sets tested here: all the F-statistics are less than the 5 percent critical value (2.32). In contrast,

¹⁹All data are in terms of level changes from the previous week. Data sources are the Federal Reserve H4.1 and H4.2 statistical releases, and the Federal Reserve Bank of St. Louis.

This procedure may impart some measurement error since only the initially released data are used. Given the short time horizon used and the observation that the weekly data revisions are not severe, the approach used seems sufficient. It also should be noted that, since February 1980, data on consumer and industrial loans and demand deposits at weekly reporting banks have been released concurrently with the money supply numbers. Thus, these two series offer no *prior* information during the post-

February 1980 period. They do, however, provide more information that forecasters may use in generating their expected money numbers.

the post-October 1979 results reveal that, except for float, the forecasters could have improved upon their ability to predict changes in the money stock by incorporating the information contained in the series on loans, demand deposits and the adjusted base. Thus, over the recent period, the forecasts do not meet the broader efficiency criterion tested here.

CONCLUSION

Previous examinations of survey data on weekly money supply forecasts have focused primarily on the effects of unanticipated money changes on market interest rates. Although several studies have examined the forecasts' rationality, there has been no systematic investigation into the effect of the change in monetary control procedures on the unbiased and efficiency characteristics of the forecasts.

The evidence presented here indicates that the change in control procedures has had a significant effect on the characteristics of weekly money supply forecasts. Prior to October 1979, the forecasts of the change in the weekly money stock were unbiased and efficient. In contrast, weekly money forecasts since October 1979 have been biased and inefficient.

The results of this investigation lend support to the recently suggested hypothesis that, since October 1979, "market participants [have] concluded that the rules under which monetary policy is conducted could no longer be considered constant."²⁰ If this indeed is true, then the combined evidence from this study and those dealing with the interest rate effects of unanticipated money supply changes suggests that a more predictable control procedure would contribute to a more stable financial market.

²⁰Cornell, "Money Supply Announcements," p. 22.