THE IRRELEVANCE OF TESTS FOR BIAS IN SERIES OF MACROECONOMIC FORECASTS

Roy H. Webb

Many economists have recently examined time series of macroeconomic forecasts or surveys of expectations for statistical bias.[1] A partial listing of prominent writings includes articles by Brown and Maital [1981], de Leeuw and McKelvey [1981], Figlewski and Wachtel [1981], Friedman [1980], Gramlich [1983], Hafer [1985], Holden and Peel [1985], Lakonishok [1980], McNees [1978], Pearce [1984], Urich and Wachtel [1984], and Zarnowitz [1985].

The standard test for bias in a series of forecasts begins by estimating coefficients in the following equation:

$$(1) \quad A_t = \alpha + \beta\ {}_{t-1}P_t + \epsilon_t$$

where A_t is the actual value at time t of the variable predicted, ${}_{t-1}P_t$ is the prediction made at time t-1 for the value at time t, α and β are coefficients estimated by least squares, and ϵ_t is an error term that is assumed to be from a series of independent and identically distributed normal random variables with zero mean. An F-test can then be used to test the joint hypothesis that $\alpha = 0$ and $\beta = 1$. If that hypothesis is rejected, the standard interpretation is that the series of forecasts is biased.

Most of the authors apparently believe that by examining the statistical bias of those time series, they are testing an important component of the new classical economics, the hypothesis of rational expectations. As Hafer put it, "Because [wealth-maximizing] agents presumably will not make forecasts that are continually wrong in the same direction, rational forecasts should be statistically unbiased."[2]

The assertion that bias is not consistent with rational expectations is examined below. First, two meanings of the term rational expectations will be presented. Several difficulties with interpreting the test for bias are discussed next; several limit the relevance of the test for the more important definition of rational expectations. Even if that test is interpreted as applying to the less important definition (a technical requirement adopted for analytical convenience), it is argued that the authors cited above have failed to consider several possible explanations for their results that do not contradict rational expectations. It is concluded that tests of macroeconomic predictions for statistical bias have not yielded useful information about the rationality of expectations.

Rational Expectations

The term rational expectations has become widely used; different authors, however, may attach different meanings to the term. This paper will focus on two ideas, one that is a general principle and the other a highly restricted form of the first. The general principle is that the actions of optimizing individuals lead to an absence of rents in equilibrium (in other words, profitable opportunities will be exploited). An important implication is that costly information will be used efficiently. This "informational efficiency" idea is crucial to economists often labeled as rational-expectations analysts. The restricted form of informational efficiency that is often used is "certainty equivalence," which implies that a representative individual's optimally predicted value of an economic magnitude can be identified with the mathematical expectation of a specific linear function that correctly describes the operation of the economy. The assumption of certainty equivalence helps economists

[1] An even larger literature exists in finance, where tests for bias have been stimulated by the efficient markets hypothesis.

[2] Hafer, page 3.

build theoretical models that are mathematically tractable; as the passages below indicate, however, it is not a critical idea for economists who have pioneered the use of rational expectations.

> The [rational expectations] hypothesis asserts three things: (1) Information is scarce, and the economic system generally does not waste it. (2) The way expectations are formed depends specifically on the structure of the relevant system describing the economy. (3) A "public prediction," in the sense of Grunberg and Modigliani, will have no substantial effect on the operation of the economic system (unless it is based on inside information). . . . *For purposes of analysis, we shall use a specialized form of the hypothesis.* In particular, we assume: (1) The random disturbances are normally distributed. (2) Certainty equivalents exist for the variables to be predicted. (3) The equations of the system, including the expectations formulation, are linear. Muth [1961], p. 348. (Emphasis added.)

> But it has been only a matter of analytical convenience and not of necessity that equilibrium models have used . . . the assumption that agents have already learned the probability distributions they face. [It] can be abandoned, albeit at a cost in terms of the simplicity of the model. Lucas and Sargent [1979], p. 13.

> [The rational expectations] approach says that if people do not observe something directly—such as the current price level—then they form the best possible estimate of this variable, given the information that they possess. In other words people make efficient use of their limited data, so as not to commit avoidable errors. Barro [1984], pp. 468-69.

As Muth noted, the strong requirement of certainty equivalence was adopted for analytical convenience. Without certainty equivalence it is not necessary that optimal predictions are mathematical expectations. Also, note that the authors stress that individuals make the best possible use of information they possess—not that individuals have perfect information.

SEVEN REASONS WHY OPTIMAL FORECASTS CAN SHOW BIAS

This section explains why a series of predictions could appear biased even though they were originally prepared optimally. Many of the explanations have the common thread of asymmetric information. In some cases, the ex post reviewer uses more information than was actually available to forecasters when the forecasts were made. In others, the process of reviewing forecasts ignores relevant data that was available to forecasters. Failure to properly account for either of those informational asymmetries limits

the relevance of tests for bias. The first four reasons below question the relevance of tests for bias as a test for both informational efficiency and certainty equivalence; the last three only apply to the strict requirements of certainty equivalence.

1. Unequal Data Availability: Real-Time Forecasts versus Ex Post Evaluation

In many cases economists have tested for biased predictions by comparing recorded forecasts with the latest available data. The data on which the forecasts were based, however, have often been revised substantially by the agencies that compile and report the data. In fact, it is possible that the ex post bias found in forecasts could be due to the reviewer having access to data revisions that were unavailable to real-time forecasters (that is, those who actually issued forecasts before the fact).[3]

Lupoletti and Webb [1986] noted that preliminary data on the rate of change of the GNP implicit price deflator were at one time biased predictors of the final data released. Since most findings of biased fore-casts or surveys of expectations refer to the inflation rate, the biased original inflation data could explain many biased forecasts without contradicting their rationality.

To see whether early reports of the percentage change in the implicit price deflator were biased, consider:

$$(2) \quad A_t = \alpha + \beta P_t + \epsilon_t$$

where A_t is the actual value[4] at time t of the percentage change in the implicit price deflator from the previous quarter, P_t is the first data officially released for that percentage change[5], α and β are

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[3] It is implicitly assumed that forecasters attempt to predict the true value that is estimated in official reports, and that successive revisions are usually closer to the true value than initial reports. The first assumption may not always be valid; consider a bond trader who is concerned about market changes in the first few minutes following a preliminary report.

[4] The actual data are index numbers, based on 1982 = 100. They reflect all revisions through early 1987.

[5] Approximately fifteen days after the beginning of each calendar quarter (t + 1) the Commerce Department released its preliminary estimate of the implicit price deflator for the previous quarter (t). At that time a forecaster would also have had a value for the deflator two quarters earlier (t – 1), which would have been revised twice since its preliminary release. At the beginning of quarter t + 1, therefore, the first official estimate of the change in the deflator between quarter t and quarter t – 1 becomes available. It is that first official estimate that is used as the early series P_t in this section.

Due to the benchmark revision of 1976, which changed the base year of the index from 1958 to 1972, there is a discontinuity in late 1975 for the original data. To adjust for the base period change, data before the revision were multiplied by

coefficients estimated by least squares, and ϵ_t is an error term that is assumed to be from a series of independent and identically distributed normal random variables with zero mean. If the preliminary value P_t is an unbiased predictor of the latest revised value A_t, then the estimate of the coefficient α should be 0 and the estimate of β should be 1.

Table I contains regression results for equation (2) over the 1970s. The hypothesis of no bias is decisively rejected by a conventional F-test. Forecasters in the 1970s, therefore, should not be assumed to have had unbiased data on which to base their forecasts, given the subsequent revisions in the implicit deflator.

The implicit deflator is not the only measure of prices that has been studied. Leonard and Solt [1986] noted that the consumer price index diverged from other measures of consumer prices before 1983 due to the CPI's treatment of mortgage interest payments (which has been criticized by many analysts). They found that survey data which other authors had found to be biased were unbiased when compared against a better estimate of consumer prices.

The problem of biased initial data that is later revised is not confined to prices. Mork [1987] found that early releases of real GNP growth from 1968 through 1984 were biased. Since it is widely believed that real GNP is the best single statistic for describing the economy's performance, Mork's finding is particularly disturbing. Certainly many

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0.68187, the ratio of the deflator for the last two quarters of 1975 with 1972 = 100 to the deflator for those two quarters with 1958 = 100. Visual inspection of the series, before and after rebasing the earlier data, did not reveal any distortion. Also, residuals from the regression reported in Table I were not unusual in late 1975.

Table I

REGRESSION RESULTS: TESTS FOR BIAS IN PRELIMINARY DATA

$$A_t = 2.95 + 0.67 \, P_t$$
$$(.65) \quad (.09)$$

Time span: 70:1 to 79:4
$R^2 = .57$
DW = 2.18
F-statistic (for $\alpha = 0$ and $\beta = 1$) = 11.9
$F_{.01,2,38} = 5.26$

Notes: A is the actual inflation rate, measured with latest data.
P is the inflation rate, based on the preliminary data release.
Standard errors are in parentheses.

economists' expectations of other variables would be affected by the reported growth rate of real GNP.

Zarnowitz [1982] has not only found evidence of biased initial data releases for many time series, but also found "extraordinary divergences" among various data series describing real economic activity in 1973-74. Since most studies of forecasts or expectations include that period, confusion at that time could have a strong impact on the results of ex post studies.

None of the studies cited in the introduction attempt to determine the extent to which their results might be due to bias in the data available to forecasters at the time forecasts were prepared. Pearce [1984] and Zarnowitz [1985], however, do mention the problem of data available to forecasters.

2. Difficulty of Improving Real-Time Forecasts

It may seem that a biased series of forecasts would indicate that forecasters did ignore an easy method of improving forecasts: simply removing that bias. That, at least, is apparently the assumption of most of the articles cited.

Now suppose that a series of forecasts was found to be biased—that is, after the coefficients in equation (1) were estimated, the joint hypothesis of $\alpha = 0$ and $\beta = 1$ was rejected. As Theil [1966] has noted, a more accurate series of forecasts P^* could then be constructed by adjusting the series P:

$$(3) \quad P_t^* = \hat{\alpha} + \hat{\beta} P_t$$

where $\hat{\alpha}$ and $\hat{\beta}$ are estimates of the coefficients α and β from equation (1). For example, if the predicted series was expressed in percentage points and a forecaster was on average one percentage point too high ($\hat{\alpha} = -1$), then the adjusted forecast would subtract one percentage point from that forecaster's prediction. This would be an almost costless way of improving forecasts. Failing to use it would therefore seem to waste information.

The flaw in that argument is that it assumes that the coefficients of equation (3) were known to forecasters at the time of forecast. In fact, those coefficients could have been estimated only after the forecasts were issued. Now if the coefficients were stable over time, one could reasonably impute their knowledge to a forecaster, since after a few years the forecaster could have recognized the bias and estimated the coefficients. But if the coefficients were to change over time, then using historic data to estimate them would not necessarily improve forecasts, since estimates of α and β would no longer be relevant.

In other words, the analyst studying a sample of forecasts has more information than did the forecaster when the forecasts were prepared. Just because using some of the additional information could improve forecasts does not show that forecasters ignored potentially valuable data.

For example, Table II contains regression results for equation (1), using quarterly growth rates of the implicit price deflator from 1970 to 1984 as the actual series and the published forecast of Wharton Econometric Forecasting Associates as the forecast series. An F-test shows statistical bias; moreover, the residuals were significantly autocorrelated. Could Wharton have produced more accurate forecasts by using the Theil adjustment mechanism shown by equation (3)?

To answer that question, the first 25 observations were used to estimate equation (1), and the estimated coefficients were used to adjust the forecast as in equation (3). Next, one observation was added, equation (1) was reestimated, and the next quarter's forecast was adjusted. That process was repeated until 32 adjusted forecasts were obtained.[6] The adjusted forecasts had a slightly larger root mean squared error (1.565) than did Wharton's published series (1.538). It therefore appears that Wharton did not waste the information from their past forecast errors even though a sample of its historic forecasts now appear biased.

Wharton's inflation forecasts therefore provide a counterexample to the idea that a retrospective finding of bias *proves* that information was wasted.[7] Of course, an author might still be able to show that other information—that was available to Wharton when its forecasts were prepared—could have improved its forecasts. The point is, that author would have to specifically identify the useful information that was wasted. A simple test for bias does not iden

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[6] Although serially correlated residuals were apparent in the whole data range, standard tests revealed no significant serial correlation in the partial ranges that ended before 1981. The coefficients in the earlier ranges were therefore estimated by OLS. When serial correlation became significant, a first-order autoregressive process was assumed and maximum likelihood estimates were made for the coefficients in (1) and for rho. The estimated rho value was then also used to adjust the Wharton forecasts.

[7] The general usefulness of the Theil-type bias correction is an unresolved issue. It is possible that the Wharton counterexample is simply a small-sample happenstance. It is also possible that findings of bias themselves are often small-sample happenstances with little predictive value. In any particular case, actually testing the bias correction method gives an indication of its ability to improve forecast accuracy; unfortunately, such tests are rarely performed.

Table II

REGRESSION RESULTS: INFLATION FORECASTS FROM WHARTON ECONOMETRICS

$$A_t = 3.23 + 0.60 \,_{t-1}P_t + 0.37 \, u_{t-1}$$
$$\quad\;\; (.94) \quad (.14) \quad\quad (.13)$$

Time span: 70:1 to 84:1
$R^2 = .52$
DW = 2.01
F-statistic (for $\alpha = 0$ and $\beta = 1$) = 6.29
$F_{.01,2,54} = 5.04$

Notes: A is the actual inflation rate, measured with latest data.
P is the Wharton Econometrics forecast for the inflation rate, prepared at the end of the previous quarter.
Standard errors are in parentheses.

tify that information; moreover, any process that identified wasted information would probably make a test for bias superfluous. None of the authors cited in the introduction specifically identify the wasted information that could account for findings of bias.

3. Average versus Marginal

In many cases it is marginal behavior that determines economic outcomes. Studies of surveys of expectations, however, often focus on average behavior. The relevance of such studies was questioned by Mishkin [1981, p.295]:

Not all market participants have to be rational in order for a market to display rational expectations. The behavior of a market is not necessarily the same as the behavior of the average individual. As long as unexploited profit opportunities are eliminated by some participants in a market . . . then the market will behave as though expectations are rational despite irrational participants in that market.

Mishkin tested the same survey data that were found to be inconsistent with rationality by Friedman. By focusing on marginal behavior, he found the data to be consistent with rationality. To explain Friedman's results, he suggested that the survey data did not accurately describe actual behavior in the bond market.

Other studies of expectations also focus on average behavior. None of the authors cited in the introduction who study surveys of expectations examine whether their conclusions would change if they examined marginal behavior.

4. Uninformed Opinions of Irrelevant Aggregates

Although most economists could state an opinion for the future time path of macroeconomic variables, not all would be willing to bet money on their predictions. Equivalently, it is a trivial matter to put a number on a survey form; if an important decision were to be based on the data, however, careful and thoughtful analysis would probably precede any forecast.

Surveys of expectations do not necessarily measure solid analyses or even informed opinions that affect real decisions. Instead, it is quite possible that they contain relatively uninformed opinions of persons who will not make important decisions based on their expectations and accordingly have little incentive to acquire costly information. The relevance of any findings of bias in such surveys is questionable.

5. Nonstationary Data

Suppose that a data series z is generated by the following random walk:

$$(4) \quad z_t = z_{t-1} + e_t$$

where e_t is from a series of independent and identically distributed normal random variables with zero mean. The mathematical expectation of z_t at time $t-1$ is therefore z_{t-1}. But a sample of such forecasts could be found to be statistically biased if the coefficients of equation (1) were estimated, assuming that $A_t = z_t$ and $_{t-1}P_t = z_{t-1}$. That is, equation (1) is misspecified if the actual data-generating process is given by (4)[8]. Significance tests from a misspecified equation can of course be misleading.

A Monte Carlo study illustrates that point. Equation (4) was used to generate 101 observations of z_t, where $z_1 = 0$ and values of e_t were randomly drawn from a normal distribution with a zero mean and a unit variance. Using z_{t-1} as the forecast for z_t, since $E_{t-1}[z_t] = z_{t-1}$, equation (1) was estimated and an F-test performed for bias. The procedure was then repeated 999 times, thereby testing 1000 random walks of 100 observations each for bias. By construction there was no bias; yet in 189 cases, the

hypothesis of no bias was rejected at the 5 percent level, and in an additional 139 cases it was rejected at the 10 percent level. That is, investigators would have found bias in many instances due to the inappropriate choice of a test statistic.

Once the possibility of nonstationary data is recognized, the burden of proof should be on the author to demonstrate that F-tests are valid.[9] For example, Schwert [1987] discusses procedures that could be used to test time series for stationarity. In addition, Nelson and Plosser [1982] and Schwert have presented evidence that many macroeconomic time series appear to be empirically indistinguishable from random walks. Yet none of the authors cited in the introduction test for stationarity in the actual data series employed. That is especially troubling for those authors that examined predicted stock prices, since stock indexes are widely believed to follow random walks.

6. Peso Problems

If an unlikely event would make a dramatic impact on predicted outcomes, that event's likelihood can affect optimal forecasts, even if the event did not occur during a particular interval[10]. In effect, the forecast contains a risk premium for the unlikely yet dramatic event. For example, if Russian investors in 1916 assigned a positive probability to a Bolshevik Revolution, stock prices of Russian firms in 1916 might appear lower than could be explained by observable factors such as earnings, dividends, and interest rates. In hindsight, such a forecast appears eminently rational. Krasker [1980] has noted that such peso problems can invalidate usual tests of efficiency in the foreign exchange market.

In studying macroeconomic forecasts, a particularly important event to consider is the possibility of a major policy regime change. The acknowledged possibility of a regime change could account for statistical bias over almost any specific interval. For example, downward-biased forecasts of inflation could be due to a positive probability placed on the Federal Reserve's adopting a monetary policy emphasizing price stability. Even if such a policy were not adopted during a particular time period, a forecaster's subjective probability of such a policy being adopted may have been correct.

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[8] To see why equation (1) would be misspecified, note that it assumes the existence of a fixed constant term, which is not consistent with the assumed random walk. A random walk can often drift far from the origin without ever crossing the origin in a fixed sample. In that case, a regression equation such as (1) will find a significant constant term and slope coefficient different from unity. Those findings, however, have no meaning for the future behavior of the random walk.

[9] Indeed, even with stationary data that is highly autocorrelated, Mankiw and Shapiro [1986] have shown that conventional F-tests will reject true models too frequently.

[10] This is labeled a peso problem due to a lengthy period when the Mexican peso traded in forward markets at a rate below the fixed spot rate, due to the widespread belief that a devaluation of the peso would eventually occur.

Although it is not possible to completely rule out peso problems, it is feasible to see whether plausible anticipated policy changes could account for findings of statistical bias. None of the authors cited in the introduction makes the attempt.

7. Representative Individual's Utility Function

Zellner [1986] has noted that, for many utility functions, an individual's optimal forecast can be biased. In particular, by accepting some bias it may be possible to lower the standard error of a point predictor and thereby lower the mean squared error of a series of forecasts. Also, if the loss of utility from an overprediction does not precisely equal the loss of utility from an underprediction of the same magnitude, then an unbiased forecast may not maximize utility. Zellner provides a specific example to illustrate the latter point. Stockman [1987] derives a loss function for forecast errors from an agent's exact decision problem, finding that in general such loss functions will not value over- and underpredictions equally.

None of the studies cited in the introduction provide evidence that a representative individual's utility function is maximized by an unbiased forecast. That key point is simply assumed.

CONCLUSION

Many authors have tested for bias in surveys of macroeconomic expectations or time series of forecasts. Although the authors believed they were testing the rationality of expectations, there are many reasons why they could have found bias. Seven reasons are listed above that are seldom examined, that are likely to affect the results of conventional tests, and that have little relevance to important economic questions. Some of the reasons are due to the reviewer using information that was not available to forecasters. Others are due to the reviewer not using relevant information that was available to forecasters. Since any finding of bias could be due to at least one of the reasons given above, the relevance of such tests is questionable.

The convenient assumption of certainty equivalence can be appropriately tested, once careful attention is given to data available to real-time forecasters. The fundamental idea of informational efficiency is much harder to test. It has not been, and almost certainly cannot be, properly examined by simple tests for biased expectations or forecasts.

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MONEY GROWTH VOLATILITY AND HIGH NOMINAL INTEREST RATES

Yash Mehra [*]

On October 6, 1979, the Federal Reserve announced a change in its operating procedures aimed at improving control of the monetary aggregate M1. Since then, however, M1 growth has become highly volatile.[1] Furthermore, the increased volatility of money growth was accompanied early by increased levels as well as volatility of nominal interest rates. Nominal interest rates rose and remained at significantly high levels through mid-1984, despite the sharp reduction in actual inflation which occurred between 1979 and 1982. Since mid-1984, however, nominal interest rates have declined significantly.

Some analysts contend that the high nominal interest rates of the 1979-86 period were due to increased volatility of money growth[2] caused by the Federal Reserve's new operating procedures. The main argument is that the increased money growth volatility induced by policy raised uncertainty about the direction of Federal Reserve monetary policy. A rise in uncertainty resulted in an increased demand for money which—in the absence of an accommodative Federal Reserve policy—caused nominal interest rates to rise. Nominal interest rates have declined sharply since mid-1984, even though M1 growth continues to be highly variable. This argument, if correct, attributes such a decline to a more accommodative Federal Reserve monetary policy stance adopted since then.

This article reexamines the foregoing hypothesis. The period since 1979 has been marked by a new round of financial deregulation including the introduction nationwide of interest-bearing NOWs in 1981 and Super NOWs in 1983. It is now widely recognized[3] that these developments may have played an important role in causing shifts in money demand and in raising the volatility of money growth over the 1980s. Hence, when testing the validity of the hypothesis of money growth volatility, it is essential to control for such effects of financial deregulation.

The empirical work reported here suggests that not all of the increase in the volatility of M1 growth should be attributed to Federal Reserve operating procedures. The recent round of financial deregulation has caused shifts between the types of assets the public wants to hold, shifts manifested by movements into and out of M1 that made M1 growth more volatile. Evidence supports this conclusion; for while the volatility of M1 did increase significantly after the change in procedures, the volatility of a broad monetary aggregate, M2 or M3, was not significantly greater than before. If all of the increased volatility of M1 growth was policy-induced, the volatility of M2 and M3 also should have increased, ceteris paribus.

It is also concluded that money growth volatility does not exert an independent influence on the public's demand for real money balances, so that the increased volatility of M1 growth did not contribute to high nominal interest rates through the money demand channel. Nevertheless, M1 demand has shifted upward during the 1980s, and the major source of this shift appears to be financial deregulation. Hence, high nominal interest rates observed early in the 1979 to 1986 period could have been caused in part by an increase in the demand for money. Apparently, too, deregulation has raised the magnitude of the response of the nominal interest rate to expected inflation, which could explain part of the high levels of nominal interest rates observed in recent years.

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[1] In the late fall of 1982, the Federal Reserve again modified its operating procedures. The volatility of M1 growth has declined somewhat since then. However, M1 growth has remained quite variable during the 1982 to 1986 period.

[2] Mascaro and Meltzer (1983) and Hall and Noble (1987).

[3] Simpson (1984), Mehra (1986), Kretzmer and Porter (1986), Wenninger (1986), Trehan and Walsh (1987), and Hetzel and Mehra (1987).

The remainder of the article contains an evalua-
tion of the money growth volatility hypothesis and
the empirical results that underlie the conclusions
reached here.

I.
Money Growth Volatility Hypothesis

This section presents and evaluates the money
growth volatility hypothesis.

Background

Analysts who contend that the Federal Reserve's
new operating procedures caused money growth to
be highly volatile usually point to a sharp increase
in the variability of M1 growth since 1979. As shown
in Chart 1a, the variability increased sharply between
1979Q4 and 1982Q2, declined somewhat thereafter,
and has remained high since then.[4] Chart 2 depicts
the behavior of the nominal interest rate over the
same period. As shown in Chart 2, the nominal in-
terest rate, measured here by the yield on one-year
Treasury bills, rose to high levels between 1979 and
1982 when the new monetary control procedures
were in force. The nominal interest rate persisted
at fairly high levels through the first half of 1984 and
since then it has trended downward. It is a widely
held view that the behavior of the nominal interest
rate since 1979 could not be readily predicted from
its past relationship with inflation, money growth,
cyclical pressure, and fiscal policy variables.[5]

Money Growth Volatility Hypothesis Stated

Mascaro and Meltzer (1983) instead have attrib-
uted the above noted behavior of nominal rates to
an increase in the degree of monetary instability,
which, they allege, was caused by the Federal
Reserve's new monetary control procedures. They
reason that in a less stable, more variable environ-
ment, people choose to hold more money and less
of other assets such that there is a positive associ-

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[4] Variability is here defined as the eight-quarter moving average
of the standard deviation of quarterly growth rates of actual
money stock. This measure is similar in spirit to measures used
in other studies although some of those deal with unexpected
portions of the growth rate of the money stock.

[5] For example, Clarida and Friedman (1984) using a vector
autoregression model reach the conclusion that short-term
interest rates in the United States have been "too high" since
October 1979. The standard estimated Fisher-type interest rate
regressions used in several interest rate studies, including Wilcox
(1983), Peek (1982), Tanzi (1980), and Makin (1983), tend to
underpredict the nominal interest rate in the post-1979 period.

Chart 1a
VARIABILITY OF M1 GROWTH*
Percent   1963-1986
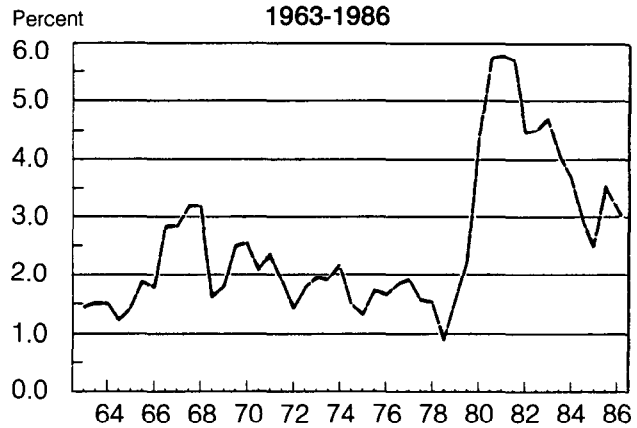
Chart 1b
VARIABILITY OF M2 GROWTH*
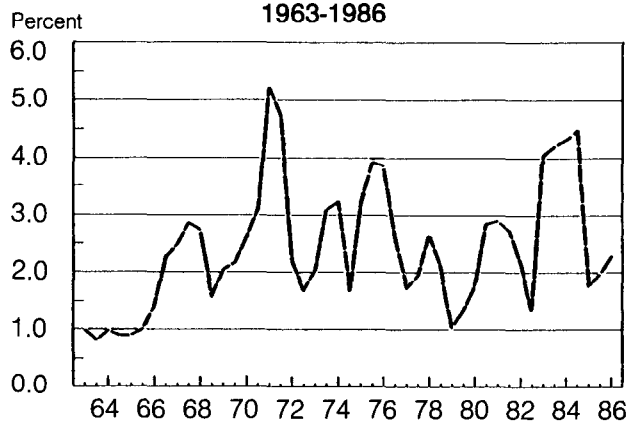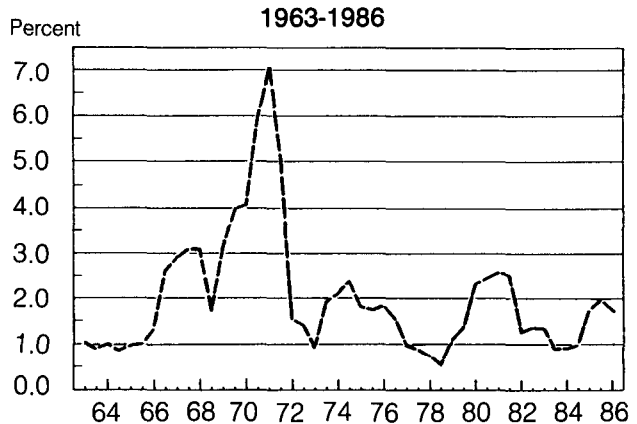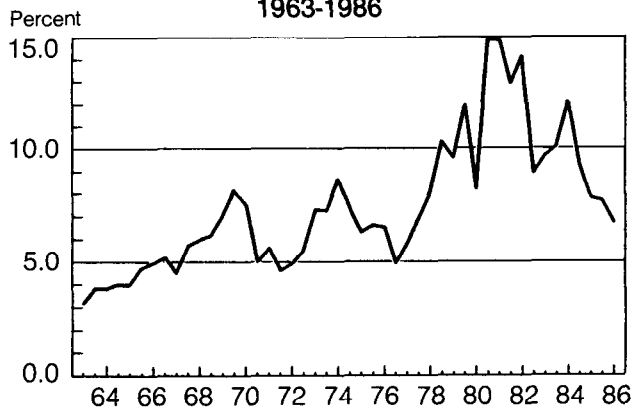Percent   1963-1986

Chart 1c
VARIABILITY OF M3 GROWTH*
Percent   1963-1986

* Variability of money growth is measured as the eight-quarter
moving average of the standard deviation of quarterly growth rates
of actual money stock.

Chart 2

NOMINAL INTEREST RATE
ONE-YEAR TREASURY BILL RATE
1963-1986

Percent

ation between variability and the demand for money. Assuming further that an increase in the demand for money raises short-term nominal interest rates, it follows that there is also a positive association between monetary variability and the level of the nominal interest rate.

The assumptions which underlie their hypothesis can be clarified further using the IS-LM model. Consider first the simple IS-LM model which contains two assets—money and capital—and in which the demand for money depends positively upon the degree of monetary instability. In such a model an increase in the degree of monetary instability raises money demand which, in turn, results in a higher short-term nominal interest rate, provided the real supply of money remains fixed. The latter assumption implies either that the price level does not adjust or that the Federal Reserve does not accommodate the increase in money demand.

In case there are more than two assets, the effect of an increase in the degree of monetary instability on short-term interest rates is ambiguous. Mascaro and Meltzer (1983) consider the IS-LM model in which there are three assets—money, short-term bonds, and capital. If one continues to assume that the demand for money depends positively on the degree of monetary instability, then increased instability could cause people to hold more of both money and short-term bonds that are close substitutes for money. In that case, even though an increase in the demand for money raises the short-term interest rate, higher demand for substitute short-term bonds depresses the nominal interest rate, so that the net impact of an increase in the degree of monetary instability on the short-term rate is uncertain.

The empirical work reported in Mascaro and Meltzer, however, shows that the short-term nominal rate does rise in response to an increase in the variability of M1 growth. Moreover, they also report evidence suggesting a positive association between the variability of M1 growth and the demand for money.

Criticism of the Money Growth Volatility Hypothesis

The major objection to the money growth volatility hypothesis questions the empirical validity of the underlying assumptions of (1) a positive association between degree of monetary instability and demand for money, and (2) the supposition that the increased monetary instability was due entirely to the Federal Reserve's monetary policy operating procedures. The empirical evidence supporting these assumptions is not very persuasive because it does not control for the potential effects on money demand of financial deregulation.

An alternative hypothesis receiving considerable support in several recent money demand studies, including Simpson (1984), Mehra (1986), Kretzmer and Porter (1986), Trehan and Walsh (1987), and Hetzel and Mehra (1987), is that M1 demand has been stronger and more volatile in the 1980s than before because M1 now contains interest-bearing assets such as NOWs and Super NOWs. The inclusion in M1 of NOWs and Super NOWs has reduced the opportunity cost of holding money, thereby inducing the public to hold more of it. Moreover, the public has also been willing to substitute more than before between the interest-bearing deposits included in M1 on the one hand and the substitute, savings-type deposits included in M2 and M3 on the other.

A bit of evidence that supports the above mentioned shifts in money demand is reproduced below. It consists of out-of-sample prediction errors of the conventional money demand regression that uses alternative measures of money—M1, M2, and M3. A standard money demand regression that uses these measures is estimated over the common sample period 1963Q1 to 1979Q3 and simulated out-of-sample over 1979Q4 to 1986Q4. The resulting errors are reported in Table I. The percentage error in predicting the level of nominal money demand is reported in columns A1, and the error in predicting its quarterly growth rate is reported in columns A2 (only second- and fourth-quarter observations are reported). RMSE statistics are also reported in Table I. For M1, prediction errors are large and positive and the RMSE value is high, implying that M1 demand had been strong and highly variable in

Simulation Results, Percentage Error in Predicting Nominal Money Demand
Quarterly Data 1979Q4 to 1986Q4

Measure of Money Used in the Money Demand Regression

Year/Quarter	M1		M2		M3	
	A1 Level	A2 Quarterly Growth Rate	A1 Level	A2 Quarterly Growth Rate	A1 Level	A2 Quarterly Growth Rate
1979Q4	−.4	−1.5	−.0	−.0	.2	.8
1980Q2	−2.9	−10.6	−.0	−4.1	−.9	−5.1
1980Q4	−1.5	1.1	−.6	−1.5	−1.0	−.1
1981Q2	−1.9	1.9	.6	5.4	.5	2.9
1981Q4	−3.5	−.8	.0	.7	.8	1.2
1982Q2	−2.5	1.3	1.6	2.7	2.5	3.9
1982Q4	.7	10.9	2.9	1.0	3.9	.7
1983Q2	3.5	6.4	5.7	.9	4.1	−.6
1983Q4	4.7	.0	4.9	−1.8	3.5	−1.8
1984Q2	4.4	1.1	3.5	−1.1	3.2	.8
1984Q4	4.4	−.0	3.9	1.6	3.9	2.0
1985Q2	6.6	4.9	3.3	−3.9	2.6	−4.6
1985Q4	10.6	5.6	3.0	−2.3	1.9	−1.8
1986Q2	15.4	11.9	3.2	2.4	2.3	1.3
1986Q4	22.6	14.2	4.0	2.5	2.5	1.4
RMSE	7.6	5.9	3.1	3.0	2.6	2.3

Notes: The values reported in columns A1 above are the percentage errors in predicting the level of the nominal money demand, whereas those reported in columns A2 are the (annualized) quarterly growth rate errors (only second- and fourth-quarter observations are reported). The predicted values are from the money demand regressions estimated over 1963Q1 to 1979Q3. The underlying money demand regression is of the form

$$\ln(M_t/P_t) = a + \sum_{s=0}^{3} b_s \ln y_{t-s} + \sum_{s=0}^{3} c_s \ln R_{t-s} + D74$$

where M is either M1 or M2 or M3, y is real GNP, R is the 4-6 month commercial paper rate, and P is the implicit GNP deflator. Estimation is by Hildreth-Lu procedure, and simple distributed lags are used. D74 is the zero-one dummy variable, taking values 1 in 1974Q2-1976Q4 and zero otherwise. RMSE is the root mean squared error, calculated using the errors over the 1979Q4 to 1986Q4 period.

the 1980s. However, there is a sharp reduction in the magnitudes of prediction errors if M2 or M3 is used. For example, the RMSE values of the quarterly growth rate prediction errors are *5.9, 3.0,* and *2.3* percent for M1, M2, and M3, respectively. These estimates, therefore, support the presence of increased substitutions by the public between assets included in M1 on the one hand and M2 and M3 on the other.

The alternative explanation of M1 demand behavior has several implications for the validity of the money growth volatility hypothesis. First, one may find divergence in the volatility of monetary aggregates. Deregulation-induced substitutions could produce an increase in the volatility of M1 growth accompanied by little or no change in the volatility of broad aggregates.[6] This is confirmed further by

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[6] Formally, this point can be explained as follows. Consider the following expressions for the variance of the broad aggregates

Var M2 = Var M1 + Var (M2−M1) + 2 COV (M1, M2−M1)   (a)

Var M3 = Var M1 + Var (M3−M1) + 2 COV (M1, M3−M1)   (b)

where (M2−M1) is the non-M1 component of M2; (M3−M1), the non-M1 component of M3; Var, the variance; and COV, the covariance of the relevant variables. If the increase observed in the variance of M1 is policy-induced, then variances

the evidence reported in Charts 1b and 1c and Table II. Charts 1b and 1c display the variability of broad aggregates, M2 and M3, and Table II reports mean standard deviation values of money growth computed over the two sample periods, 1961Q1 to 1979Q3 and 1979Q4 to 1986Q4. As can be seen by comparing Charts 1a through 1c, the broad measures of money—M2 and M3—have not since 1979 displayed the variability of the M1 measure. The same is true if one compares the mean standard deviation values reported in Table II.[7]

The implication of all this is that not all of the increase observed in the variability of M1 growth should be attributed to the adoption by the Federal Reserve of new monetary control procedures. A part has been due to an increase in the variability of M1 demand.

A second implication is that the money growth volatility hypothesis should be reexamined using broad measures of money. The broad measures of money are likely to internalize the above mentioned deregulation-induced substitutions. Hence they should provide a sharper test of the joint hypothesis that the increased volatility of money growth was policy-induced and contributed to high nominal interest rates via raising money demand.

If financial deregulation is at the source of the observed strength in money demand, then another important consequence is a potential increase in the magnitude of the response of the nominal interest rate to inflation. The basic argument here is as follows. Most empirical studies of interest rate determination have found that fully anticipated inflation has less than a one-for-one effect on the nominal interest rate. Thus, expected inflation reduces real rates of return on financial assets (bonds). Some analysts attribute this result to the existence of legal restrictions on the payment of explicit interest on money.[8] According to their argument, optimizing individuals tend to hold money and financial assets to the point

Table II

Volatility of Monetary Aggregates
Mean Values of the Standard Deviation
of Quarterly Growth Rates

| | Monetary Aggregate | | |
Sample Period	M1	M2	M3
1961Q1-1979Q3	1.88	2.18	2.10
1979Q4-1986Q4	4.15	2.73	1.67

Notes: Volatility is defined as the eight-quarter moving average of the standard deviation of quarterly growth rates of actual money stock. The values reported above are mean values of the standard deviation of monetary aggregates.

at which their (after-tax) yields are equal. Inflation reduces the equilibrium real rate of return on money, which, in the presence of the prohibition of the payments of explicit interest on money, is just the negative of the rate of inflation. If one assumes further that financial assets are closer substitutes for money than for capital,[9] then an inflation-induced fall in the equilibrium real rate of return on money forces a corresponding fall in the real rate of return on financial assets as investors substitute out of money into financial assets.

The introduction nationwide since 1981 of interest-bearing NOWs, Super NOWs, and Money Market Deposit Accounts together with the gradual lifting in recent years of the remaining regulatory interest rate restrictions on several components of money and money substitutes[10] means that a rise in anticipated inflation does not reduce equilibrium real rates of return on money and money substitutes as much as it did before. The presence of this effect tends to enhance the response of the nominal interest rate to expected inflation.

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of the broad measures of money should also increase. However, if part of the increase observed in the variance of M1 is due to increased substitution by the public between assets included in M1 on the one hand and assets included in (M2−M1) or (M3−M1) on the other, then the broad measures—M2 and M3—should be relatively less variable, as higher variance terms in (a) and (b) above are offset by large, negative covariance terms.

[7] Two aspects of this data, reported in Table II, warrant underscoring. First, whereas the mean values of the standard deviation of broad measures of money exceeded that of the narrowly defined measure M1 in the early sample period, 1961Q1-1979Q3, this ordering is reversed in the later sample period, 1979Q4-1986Q4. Second, while the variability of M1 growth increased most over the 1979Q4 to 1986Q4 period, that of M2 growth showed only a modest rise, and that of M3 fell.

[8] Carmichael and Stebbing (1983) and Fried and Howitt (1983).

[9] Several analysts including Carmichael and Stebbing (1983) and Fried and Howitt (1983) have emphasized that money and financial assets are likely to be highly substitutable at the margin. This is so because, apart from the medium of exchange function, money and financial assets are almost identical; they are both nominal stores of value, they have very similar liquidity and risk characteristics, and so on.

[10] The maximum rate payable on NOWs was initially set at 5¼ percent. As of January 1986 this restriction has been removed. The maximum rates payable on passbook savings accounts and several time deposits have also been completely deregulated. However, the explicit nominal rate payable on demand deposits held by businesses is still fixed at zero.

II.

Empirical Evidence

In this section I present empirical evidence on the relative roles of money growth volatility and financial deregulation in explaining the recent behavior of the nominal interest rate.

Specification of the Testable Hypothesis

The major thrust of the money growth volatility hypothesis is that the degree of monetary instability is an important determinant of nominal interest rates. A policy-induced increase in monetary instability tends to raise the level of the nominal interest rate, because such an increase raises uncertainty which, in turn, raises money demand. A simple way to test these implications is to estimate the following interest rate and money demand regressions:

$$i_t = a_0 + a_1\Pi_t - a_2MG_t + a_3X_t + a_4VOL_t \tag{1}$$

$$(M_t/P_t) = b_0 + b_1y_t - b_2R_t + b_3(M_{t-1}/P_t) + b_4VOL_t. \tag{2}$$

Equation (1) is the Fisher type interest rate regression in which i is the nominal interest rate, Π is expected inflation, MG is money growth, X is the variable measuring shift in exogenous aggregate demand, and VOL is the variable that measures the degree of monetary instability. Equation (2) is the standard money demand regression that includes real income (y), the short-term nominal interest rate (R_t), lagged real money balances (M_{t-1}/P_t), and money growth volatility (VOL) as the explanatory variables. The money growth volatility hypothesis posits that coefficients a_4 and b_4 attached to the VOL measure in regressions (1) and (2) are positive and significantly different from zero.

The alternative hypothesis is that financial deregulation is at the source of the strength in M1 demand. Furthermore, as a result of financial deregulation, the magnitude of the response of the nominal interest rate to expected inflation should have increased over the 1980s. In order to control for these effects of financial deregulation, consider the following expanded interest rate and money demand regressions

$$i_t = a_0 + a_1\Pi_t - a_2MG_t + a_3X_t + a_4VOL_t + a_5D81\cdot\Pi_t \tag{3}$$

$$(M_t/P_t) = b_0 + b_1y_t - b_2R_t + b_3(M_{t-1}/P_t) + b_4VOL_t + b_5SHIFT_t \tag{4}$$

where all variables except D81 and SHIFT are as defined before. D81 is the zero-one dummy variable that takes values unity in the post-1981 period and zero otherwise. D81·Π is formed by taking the product of D81 and expected inflation Π. SHIFT is a variable that captures the effect of financial deregulation on money demand. In empirical work this effect is captured by broadening the measure used in defining money. If the money growth volatility hypothesis is valid, then coefficients a_4 and b_4 should continue to be significant in (3) and (4).

It should, however, be pointed out that the aforementioned interest rate and money demand regressions (3) and (4) provide a test of the joint hypothesis that money growth volatility affects the nominal interest rate through the money demand channel. But money growth volatility could affect the nominal interest rate through other channels as well. In particular, increased money growth volatility also generates inflation uncertainty, which could directly change the real rate by influencing saving, investment, and real output. In general, the impact of inflation uncertainty on the equilibrium real rate is indeterminate. But, as shown in Makin (1983), inflation uncertainty could directly raise the real rate if it depresses saving more than investment.[11] With respect to the empirical tests proposed above, this point implies that the money growth volatility variable could be significant in the nominal interest rate regression (3) but not necessarily so in the money demand regression (4). Hence one must be careful in interpreting results from the tests conducted in this article.

Empirical Results

This section presents estimates of the interest rate and money demand regressions (3) and (4). All regressions are estimated over the common sample period, 1963-86. The interest rate regression[12] is estimated by the instrumental variable estimation

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[11] This impact may be enhanced if higher inflation uncertainty also depresses real output.

[12] Since the measure of expected inflation used in the interest rate regressions is based on the Livingston Survey inflation forecasts, the regressions are estimated using semiannual observations that correspond to the survey data collected each June and December. The variables included in the regressions are measured as follows: i is the average market yield on a one-year Treasury bill (June and December observations), Π is the Livingston Survey forecast of inflation over the 14-month horizon, and MG is the annualized growth rate of the nominal money stock over the last six months minus its annualized growth rate over the last three years (second- and fourth-quarter observations are used). The interest rate regression estimated here is in essence similar to the ones given in Carlson (1979), Peek (1982), Wilcox (1983), and Mehra (1985).

procedure, correcting estimated standard errors for the presence of any heteroscedastic disturbance term.[13] The money demand regression is estimated by the Hatanaka two-step estimation procedure that corrects for the presence of first order serial correlation.[14,15] The interest rate regressions are presented first, followed by money demand regressions.

Evidence from the Interest Rate Equation. Table III reports interest rate regressions that include money growth volatility and inflation interaction dummy variables. Regressions 3.1 and 3.2 indicate that the volatility variable based on the M1 measure of money is highly significant in explaining the nominal interest rate. The M1 volatility variable continues to appear statistically significant in the interest rate regressions that also included the inflation interaction dummy variable (see equation 3.2 in Table III).

Contrariwise, volatility variables based on broad measures of money do not do as well in these interest rate regressions (see equations 3.3, 3.4, 3.5, and 3.6 in Table III). In these the coefficient on the volatility variable remains positive but generally not

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[13] Estimation treats MG endogenous with the instruments used including the contemporaneous and lagged values of the expected inflation rate, volatility, and inflation-interaction dummy variables and the lagged values of the nominal interest rate and money growth. The standard errors of the regression estimates were corrected for the presence of heteroscedasticity, using estimated covariance matrix as outlined in White (1980). Actual estimation was carried out by using the procedure outlined in *RATS*, Chapter 18, Section 3. It should be pointed out that the interest rate regressions reported in Table III of the text were also estimated by using ordinary least squares. Except for the coefficient on the variable MG, ordinary least squares estimates of other parameters are not different from the ones generated by the instrumental variable estimation procedure. Hence, the inference regarding the impact of volatility on the nominal interest rate is not sensitive to the estimation procedure chosen here.

[14] The money demand regressions are estimated using quarterly observations. The measures of nominal money used are M1, M2, and M3. The scale variable used is real GNP, and the opportunity cost of holding money is measured by the commercial paper rate (R). The degree of monetary instability is measured by the eight-quarter moving average standard deviation of the quarterly growth rates of actual money stock.

[15] The use of the lagged dependent variable in the money demand regression is subject to several well-known criticisms, as reviewed recently in Mehra (1986). Since this is the specification originally reported in Mascaro and Meltzer (1983) I chose the same, so that results could be compared. In view of the presence of a lagged dependent variable as well as serially correlated errors in the money demand regressions, the Hatanaka two-step estimation procedure, rather than the commonly employed Cochrane-Orcutt procedure, is used. The use of the Cochrane-Orcutt procedure can result in biased estimates of the parameters (Hatanaka (1974)). Nevertheless, the money demand regressions were also estimated by ordinary least squares, correcting standard errors for the presence of first-order serial correlation in the residuals. These estimates imply results similar to the ones based on the Hatanaka procedure.

statistically significant. Instead, the inflation interaction dummy variable usually appears significant in these regressions.

Nominal interest rate regressions that included volatility variables were also estimated over the 1963 to 1979 period. None of these volatility variables were significant however (these regressions are not reported).

At best, these estimates provide only a mixed support for the money growth volatility hypothesis. True, it appears that the heightened volatility of M1 growth is highly correlated with the nominal interest rate in the 1963 to 1986 period. But this correlation does not appear to indicate the presence of a systematic relation between the two variables. Money growth volatility variables are never significant in interest rate regressions estimated over the period excluding the 1980s. Furthermore, even over the sample period 1963-86 volatility variables based on broad measures of money are generally not significant in explaining the behavior of the nominal interest rate.[16,17]

The evidence from the money demand regression reviewed below further casts doubt on the validity of the money growth volatility hypothesis.

Evidence from the Money Demand Equation. An important assumption implicit in the volatility hypothesis is that an increase in monetary instability raises the demand for money. The money demand regressions reported in Table IV provide a direct test of this contention. Equation 4.1 (Table IV) is the standard money demand regression that includes the M1 volatility measure estimated over the period 1963-86. The coefficient on the volatility variable is of the hypothesized sign and statistically significant. This regression indicates that the degree of monetary instability is an important determinant of money demand.

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[16] Kantor and O'Brien (1985) report a similar conclusion.

[17] As noted in the text, the interest rate regressions reported here included, in addition to volatility measures, other variables intended to capture the effects on the interest rate which are due to changes in expected inflation and monetary accelerations. Rises in expected inflation (Π) are found to raise interest rates while accelerations in money growth (MG) lower them (see Table III). Other variables such as supply shocks, lagged real income growth and changes in the exogenous components of aggregate demand were also tried and found generally insignificant in these interest rate regressions. In particular, I found no significant effect of the fiscal deficit on the level of the nominal interest rate. The interest rate regressions similar to those reported in Table III were reestimated including, in addition, the fiscal deficit variable (measured by the ratio of federal deficits to GNP). The coefficient that appears on this fiscal deficit variable is negative and generally insignificant.

Table III

Estimates of the Interest Rate Equation
Semiannual Data, 1963.06-1986.12

Independent Variables	Dependent Variable: One-Year Treasury Bill Rate											
	Eq. 3.1		Eq. 3.2		Eq. 3.3		Eq. 3.4		Eq. 3.5		Eq. 3.6	
constant	1.0	(2.6)	1.4	(3.1)	2.0	(3.0)	2.5	(3.7)	2.5	(3.7)	2.1	(3.1)
Π	.8	(10.7)	.8	(10.8)	.9	(8.5)	.8	(7.6)	1.0	(9.7)	.8	(7.6)
MG	−38.7	(3.9)	−33.0	(3.2)	−33.8	(2.6)	−24.0	(1.8)	−31.2	(2.0)	−32.2	(2.1)
VOL($\dot{M}$1)	1.3	(9.8)	1.0	(4.6)								
VOL($\dot{M}$2)					.4	(1.8)	.1	(.5)				
VOL($\dot{M}$3)									.1	(.6)	.3	(1.3)
D81*Π			.2	(1.7)			.5	(5.2)			.6	(5.5)
CC	−8.2	(6.6)	−7.1	(5.1)	−.8	(5.5)	−3.6	(4.2)	−6.1	(5.9)	−4.6	(4.7)
$\bar{R}^2$	.82		.84		.59		.80		.58		.78	
SER	1.24		1.18		1.87		1.13		1.89		1.37	
DW	1.62		1.70		.59		1.51		.61		1.50	

Notes: The nominal interest rate equation is estimated by instrumental variable procedure, and t values (absolute values reported in parentheses) have been corrected for the presence of heteroscedacity (see footnote 13). Π is the expected inflation proxy (measured here by the Livingston Survey forecast of inflation), MG is the annualized growth rate of the nominal money stock (M1 or M2 or M3) over the last six months minus its annualized growth rate over the last three years, and VOL ($\dot{M}$) is the moving-average standard deviation of quarterly changes in the money stock. VOL($\dot{M}$1) is based on M1 measure of money; VOL($\dot{M}$2), on M2; and VOL($\dot{M}$3), on M3. D81*Π is formed by taking the product of Π and a dummy variable that takes values 1 in 1981-86 and zero otherwise. CC is the credit control dummy taking value unity in 1980.06 and zero otherwise. Estimation treats the variable MG endogenous, and the instruments used included the contemporaneous and lagged values of the expected inflation rate, volatility, and inflation-interaction dummy variables and the lagged values of the nominal interest rate and money growth.

But this conclusion is quite fragile. The regression 4.1 (Table IV) is the standard money demand regression estimated in level form and including a lagged dependent variable. When this regression is estimated either in the first difference form or with simple distributed lags, the volatility variable (VOL($\dot{M}$1)) becomes insignificant. Likewise, that variable is no longer significant in the money demand regression estimated over the early sample period, 1963-79 (these regressions are not reported).

The money demand regression 4.1 (Table IV) does not allow for evaluation of the alternative hypothesis that M1 demand during the 1980s was affected by the inclusion in M1 of interest-bearing NOWs and Super NOWs. In particular, as explained before, broad measures of money, since they internalize deregulation-induced substitutions from components of M1 to components of M2 and M3, provide a more stringent test of the money demand channel of the volatility hypothesis. Hence the regression 4.1 (Table IV) is reestimated using instead the M2 and M3 measures of money as the left-hand-side dependent variable. The result (see equations 4.2 and 4.3 in Table IV) is that M1 volatility variable is no longer statistically significant.[18] On balance, these results do not support the contention that money growth volatility is a significant determinant of the public's demand for real money balances.[19]

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[18] It should also be pointed out that money growth volatility variables based on broad measures of money are not statistically significant in such regressions.

[19] Hall and Noble (1987) use Granger-causality tests to show that volatility influences velocity. However, this causality result is also shown not to be very robust (Mehra (1987)).

Table IV

Estimates of the Money Demand Equation
Quarterly Data, 1963Q1-1986Q4

Independent Variables	Dependent Variable					
	Equation 4.1 M1		Equation 4.2 M2		Equation 4.3 M3	
constant	−.38	(4.2)	−1.01	(4.4)	−.81	(2.7)
y_t	.05	(3.4)	.19	(4.6)	.14	(2.9)
R_t	−.02	(4.9)	−.03	(7.1)	−.01	(3.0)
M_{t-1}/P_t	.99	(36.8)	.84	(22.5)	.90	(25.2)
$VOL(\dot{M}1)$	.002	(2.1)	.001	(1.5)	−.000	(.1)
D74	−.002	(1.4)	.001	(1.1)	.000	(.2)
D742	.0001	(1.2)	−.000	(1.1)	.000	(.1)
$\bar{R}^2$	.99		.99		.99	
SER	.00615		.00487		.00477	
DW	2.0		2.2		1.70	
Rho	.11	(1.2)	.43	(5.1)	.75	(10.3)

Notes: The money demand regression estimated by the Hatanaka procedure is of the following form:

$$\ln(M_t/P_t) = b_0 + b_1 \ln y_t - b_2 \ln R_t + b_3 \ln(M_{t-1}/P_t) + b_4 VOL(\dot{M}1)$$

where y is real GNP, R is the commercial paper rate, P is the implicit GNP deflator, and $VOL(\dot{M}1)$ is the measure of M1 volatility. D74 is the dummy variable that takes value 0 through 1974Q1, 1 in 1974Q1, incrementing by ones until it reaches 11 in 1976Q4 and remaining at 11 thereafter. D742 is the square of D74. ln is the natural logarithm. Parentheses contain absolute values of t statistics. The regression is estimated using M1, M2, and M3 as the dependent variable, but the VOL variable used is based on M1.

III.
Concluding Remarks

If one focuses primarily on the behavior of M1—the narrowly defined measure of money—then the evidence reviewed here supports the contention that the volatility of money growth did increase during the period that followed the change in monetary control procedures. Since then M1 demand has also been stronger than predicted from its past relationship with real income, the price level, and the nominal interest rate. Furthermore, some specifications of interest rate and money demand equations suggest that the increased volatility of money stock raised money demand and thus contributed to high levels of nominal interest rates in the 1979-86 period.

An entirely different set of inferences emerges if one focuses on the behavior of broad monetary aggregates. The error in predicting money demand over this period is sharply lower when a broad definition of money is used in the money demand regression, suggesting that M1 demand had in fact been affected by changing asset preferences of the public. If one controls for this effect and uses the broad definition of money in measuring volatility, then the evidence reported here does not support the hypothesized causal link between the degree of monetary instability and the level of the nominal interest rate. Money stock volatility does not exert an independent influence on the public's demand for real money balances.

An increase in the demand for money not caused by increased money stock volatility could have contributed to high nominal interest rates in the 1979 to 1986 period. Also the nominal interest rate now moves more in line with expected inflation than before. This consideration too could explain part of the high levels of interest rates observed in recent years.

What do the results presented imply for monetary policy? An important issue raised by changes in Federal Reserve operating procedure is whether

money stock volatility matters enough to receive an independent weight in policy decisions. Some analysts contend it does matter enough, because it induces interest rate volatility and generates uncertainty about monetary policy. This uncertainty then supposedly raises the general level of interest rates, with adverse consequences for the performance of the economy.[20] If so, the Federal Reserve should pay serious attention to the volatility of the growth path of the money stock, in addition to focusing on money's growth rate.

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[20] Evans (1984) and Tatom (1985).

The empirical work reported in this article, however, provides mixed support for the above view. A policy-induced rise in the volatility of money growth is likely to be associated with a rise in the volatility of the interest rate since accelerations or decelerations in the growth rate of the money stock influence nominal interest rates in the short run. This result is due to the so-called liquidity effect of money on interest. However, the evidence presented to support the hypothesis that money stock volatility exerts an independent influence on the level of the nominal interest rate is not robust. Thus it is not clear that money stock volatility ought to be given an independent weight in monetary policy decisions.

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