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U.S. Money Demand: Surprising Cross-sectional Estimates

THE SPECIFICATION of the money demand function has important implications for a number of macroeconomic issues. First, if policymakers are to be responsible for achieving price stability, they need reliable quantitative estimates of money demand.[1] In particular, if the money demand function is stable, the income elasticity yields the rate of money growth that is consistent with long-run price stability.

Second, macroeconomic theorists need quantitative estimates of the money demand function in order to determine the exact predictions of their models. In Keynesian models, for instance, the relative ability of monetary and fiscal policy to affect the real economy depends on the elasticities of the demand for money. For a given interest elasticity, a larger income elasticity implies a more vertical LM curve; as a result, monetary policy is relatively more potent than fiscal policy. In fact, part of the debate between monetarists and fiscalists in the 1950s and 1960s was over the "slope" of the LM curve. Such issues are especially important to many economists of the 1990s, who are called upon to assess

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1. That economic research promotes "prosperity and price stability" has always been a primary goal of the *Brookings Papers on Economic Activity*. Readers of *BPEA* find a formal statement of this goal on the first page of every volume.

the macroeconomic consequences of deficit reduction. For other economists who are inclined to think in terms of the general-equilibrium, real business-cycle model (which recently has begun to emphasize monetary aspects of the economy), money demand elasticities are among those figures that need to be replicated by the equilibrium conditions of their models. Furthermore, in such models, money demand elasticities matter in determining the aggregate price level and the inflation rate, given the growth rate of money. Classical economists may also argue that the elasticities are important in determining the optimal seigniorage policy.

Finally, both classical economists and Keynesians need to worry about the reasons why people hold money. That is, they need a theory of money demand that can be tested against the data. One of the predictions of such a theory will be the elasticity of money demand with respect to income and the interest rate. Some models will further predict that the elasticities are structural and, therefore, stable. Hence the size and stability of the money demand elasticities can be seen as tests of the implications of different theories.

Economists disagree about the size of the income elasticity of money demand. At the theoretical level, the predicted elasticities range between one-third and one: a strict interpretation of the Baumol-Tobin model[2] of the transactions demand for money predicts an income elasticity of one-half. This is true if transaction costs are thought to be independent of income. This assumption, however, is not completely realistic. For instance, if transaction costs are related to the time needed to go to the bank, then the cost is related to the wage rate, which, in turn, will be positively correlated with the aggregate level of income. The overall income elasticity would in this case be greater than one-half.[3] The stochastic version of the model (developed by Merton Miller and Daniel Orr) reduces the prediction to about one-third.[4] The elasticity predicted by the popular "cash-in-advance" model is unity.[5]

At the empirical level, the elasticity estimates are even more erratic.[6]

2. See Baumol (1952) and Tobin (1956).
3. Karni (1973).
4. Miller and Orr (1966).
5. See Barro and Fischer (1976) for a survey of theories of money demand.
6. Many empirical studies of money demand exist; their estimates vary widely. An illustrative, but not exhaustive, list would include Friedman (1959), Meltzer (1963a, 1963b), Laidler (1985), Goldfeld (1973, 1976), Judd and Scadding (1982), Lucas (1988), and Braun and Christiano (1992).

They seem sensitive to the choice of sample period, to the exact functional form and number of lags, and to the inclusion and precise definition of the interest rate variable. Typical problems arise from the potential simultaneity bias of money supply and money demand; from the correlation of income and transaction technologies over time;[7] from the potential instability of the coefficients; and from possible nonstationarities (which would dictate whether regressions should be run using first differences, using levels with time trends, or using trends with a number of breaks).

That the money demand function is stable over time is a standard identifying assumption, yet there is no shortage of evidence to the contrary. Benjamin Friedman and Kenneth Kuttner insist that for 1970–90, time-series data reveal no "close or reliable relationship between money and nonfinancial economic activity."[8]

In this paper, we argue that these and other problems are avoided when money demand is estimated cross-sectionally. We estimate money demand functions using cross sections of U.S. states from 1929 to 1990 and arrive at a number of interesting conclusions. First, from our preferred equations, we find that the income elasticity of both demand deposits and a broader measure of money lies between 1.3 and 1.5 for the entire period of 1929–90. Second, year-by-year cross-sectional estimates of the income elasticity for these two measures are almost always well above 1.0 during this long period—which includes both the Depression and World War II—and do not differ individually from the estimates for the sample period as a whole. Third, we conclude that income per capita is a better scale variable than consumption, although the empirical estimates do not depend significantly on the choice of the scale variable. Finally, during some time periods, we find that agricultural regions have demanded more money than would be predicted given their incomes.

The paper is organized as follows. The first section describes our data set, which measures various bank deposits for 48 U.S. states from 1929 to 1990. The deposit data are used to construct a narrow measure of

7. Because of its unobservable nature, financial technology is commonly thrown into the error term. To the extent that income is positively correlated with technology (which affects the demand for money negatively), the estimates of the income elasticity are biased downward.

8. Friedman and Kuttner (1992, p. 490).

money (which we call MX1), as well as a broader measure (MX2). The second section argues that time-series estimation of money demand encounters a number of problems that can be successfully solved using cross-sectional analysis. In the third section, we summarize previous findings in the literature. The fourth section presents the empirical estimates. In the fifth section, we discuss the "shifts" in the money demand function. The final section summarizes our main findings, discusses the relevance of our estimates to macroeconomic policy, and offers conjectures about the reasons for income elasticities above one.

Data: Sources and Definitions

We have compiled data on two concepts of money—which we call MX1 and MX2—for 48 states. Our sample period is 1929–90. In this first section, we review conventional definitions of money for the United States as a whole. We then explain our state money data. We conclude the section by describing other variables included in the empirical analysis.

U.S. Money Aggregates

For the United States, four aggregate definitions of money are common: M0 (the monetary base), M1, M2, and M3. Currency, together with reserves, constitutes the monetary base. M1 is the sum of currency, traveler's checks, demand deposits and, after the 1980s, other checkable deposits. M1, savings deposits, small time deposits, overnight repurchase agreements, overnight Eurodollars and money market mutual funds (excluding institution-only funds) constitute M2. M3 is M2 plus other "less liquid" financial assets.[9]

State Money Aggregates

Because currency data are not available by state, it is very difficult to measure the monetary base at the state level. However, aggregate U.S. data suggest that broader aggregates can be approximated by deposit data: in 1987, for instance, currency constituted only 26 percent of M1.[10] Hence we collect and analyze deposit data by state.

9. Barro (1990, pp. 427–29).
10. Barro (1990, p. 428).

In each year since 1950, the Federal Deposit Insurance Corporation (FDIC) has surveyed all banks. Each bank has reported the composition of its deposits, as well as a profile of its depositors. Thus a bank's "call report" reveals amounts owed in the form of demand deposits, savings deposits, and time deposits. The reports also show the importance of various depositor groups: individuals, partnerships, and corporations; federal government agencies; state and local governments; and other banking institutions. Before 1950, similar surveys were conducted by state governments or by the Federal Reserve.[11]

The FDIC summed various subsets of banks and reported state aggregates for various types of deposits. For 1950–57, all operating banks were included in the aggregates. After 1957, only FDIC-insured commercial banks were included; mutual savings banks or uninsured banks were excluded.[12] Demand deposit measures by state were compiled by the Federal Reserve for 1929–49, often using individual state government sources. The Federal Reserve totals included all banks.

Today, a deposit is considered to be in a state if the banking branch at which the deposit is made is located in that state, regardless of the location of the main office.[13] Before 1981, what mattered was the location of the main office.

For 1929–87, our narrow measure of money, MX1, is demand deposits held at banks by individuals, partnerships, and corporations.[14] After

11. For FDIC surveys, see FDIC, *Banks and Branches Data Book; Data Book, Operating Banks and Branches; Bank Operating Statistics; Statistics on Banking;* and *Assets, Liabilities, and Capital Accounts of Commercial and Mutual Savings Banks*. For Federal Reserve surveys, see Board of Governors of the Federal Reserve System (1959).

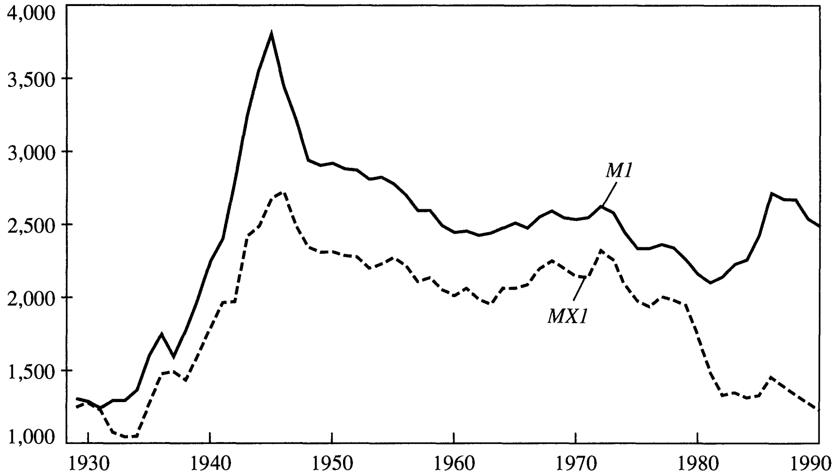
12. In 1973, 99.1 percent of demand deposits in the United States were liabilities of FDIC-insured commercial banks. FDIC (1973, table 6, p. 132). In 1983, the fraction had fallen to 98.6 percent. FDIC, *Data Book, Operating, Banks and Branches: June 30, 1983*, table 1.

13. According to the FDIC, a branch is "any office or facility of a bank, including its main office, at which deposits are received, checks paid, or money lent, even though some of these may not be defined as branches by State laws. A branch includes, but is not limited to all of the following: drive-in facilities, seasonal offices on military bases or government installations; paying/receiving stations or units, and non-deposit offices." (FDIC *Banks and Branches Data Book, June 30, 1984*). Branches do not include electronic fund transfer units and customer bank communication terminals.

14. Not included as individuals, partnership, or corporations (IPCs) are federal government agencies and other banks. State and local governments also are not included, except in the 1929–49 period. Deposits held by mutual savings banks at FDIC-insured commercial banks are sometimes included in the IPC total during the 1958–87 period. As noted above, state aggregates include deposits owed by all banks for 1929–57, but only those owed by FDIC-insured commercial banks for 1958–90.

Figure 1. Comparing M1 and State-aggregated Demand Deposits (MX1), 1929–90

Real balances per capita in 1982 dollars



Source: Authors' calculations based on Friedman and Schwartz (1963); *Statistical Abstract of the United States 1991*, p. 7; and other sources listed in appendix 2. MX1 is state-aggregated demand deposits as described in the text. MX1 and M1 are deflated using 1982 as the base year and converted into per capita values to remove the common trend in population. MX1 and the population variable are based on aggregated data from 48 states, excluding Alaska and Hawaii.

1987, we use "non-interest-bearing deposits," regardless of the depositor. Our broad measure, MX2, includes all deposits held at insured commercial banks. MX2 includes savings and time deposits, including those held by public entities. Inconsistencies in the types of banks surveyed for MX1 also apply to MX2.

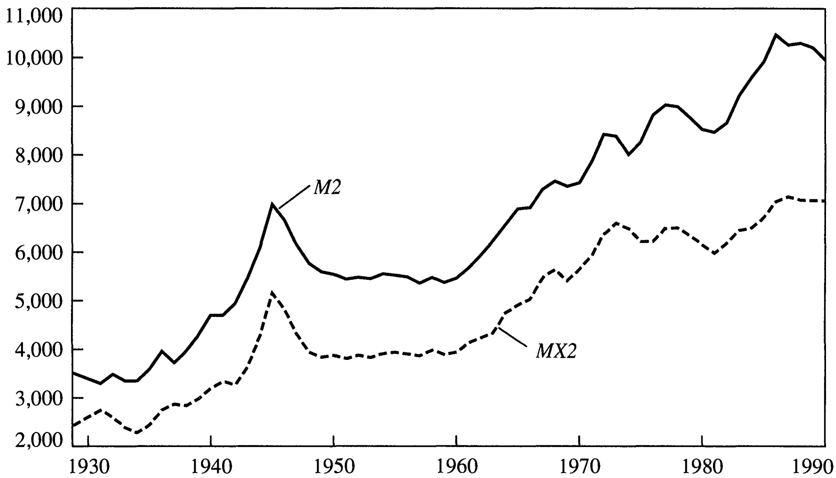
We noted above some minor inconsistencies in the definitions of MX1 and MX2 over time. Mutual savings banks may or may not be counted, government bank deposits are sometimes counted, and surveys vary between June and December. In every instance of a definitional change (which occurred in 1950, 1958, 1984, and 1988), we had overlapping data. We adjusted levels of four series accordingly. Most importantly, we kept definitions consistent cross-sectionally.

Figures 1 and 2 sum our measures of money—MX1 and MX2—for all 48 states and compares them with two similar Federal Reserve concepts, M1 and M2.[15] We deflate the four series and divide by the U.S. population

15. MX1 and MX2 exclude Alaska and Hawaii.

Figure 2. Comparing M2 and State-aggregated Total Deposits (MX2), 1929–90

Real balances per capita in 1982 dollars



Source: Authors' calculations based on Friedman and Schwartz (1963); *Statistical Abstract of the United States 1991*, p. 7; and other sources listed in appendix 2. MX2 is state-aggregated demand deposits as described in the text. MX2 and M2 are deflated using 1982 as the base year and converted into per capita values to remove the common trend in population. MX2 and the population variable are based on aggregated data from 48 states, excluding Alaska and Hawaii.

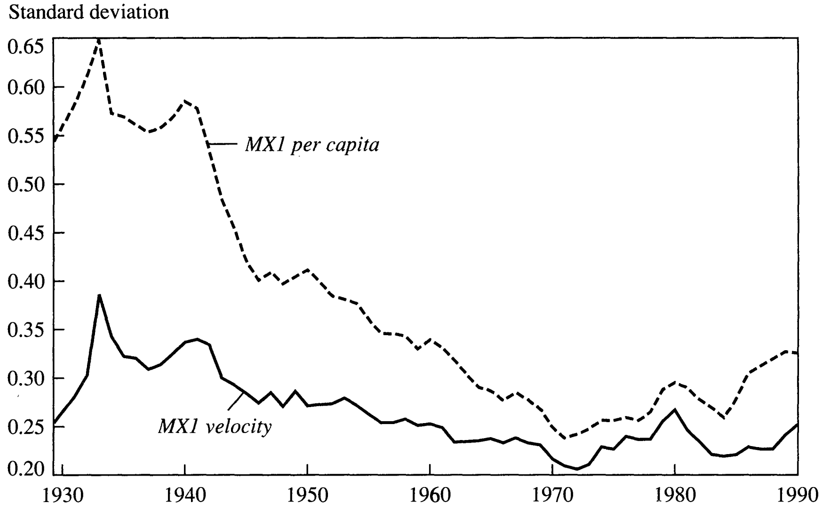
to remove the common trend.[16] Of course, because M1 and M2 include currency, the levels of M1 and M2 are greater than the levels of MX1 and MX2, respectively. The figure shows that year-to-year variations are fairly similar when M1 is compared with MX1 and when M2 is compared with MX2. The main exception to this observation occurs in the early 1980s, when demand deposits dropped more sharply than M1. MX1 and M1 have a correlation of 0.80 for the full sample and one of 0.97 when the 1980s are excluded. The main difference between M1 and MX1 during the 1980s was probably caused by the introduction of NOW accounts and other checkable deposits that are part of M1 but not part of MX1. MX2 and M2 have a correlation over the full sample of 0.99.

Dispersion of Velocity and Money per Capita

In this section, we argue that the cross-sectional variation of money is sufficiently large to justify cross-sectional econometric analysis. In

16. For the U.S. population and for MX1 and MX2, we use the sum over the 48 states, excluding Alaska and Hawaii.

Figure 3. Dispersion of State Demand Deposits, 1929–90



Source: Authors' calculations using data listed in appendix 2. Standard deviations are from cross-sectional estimates of the log of state demand deposits (MX1) per capita and log MX1 velocity.

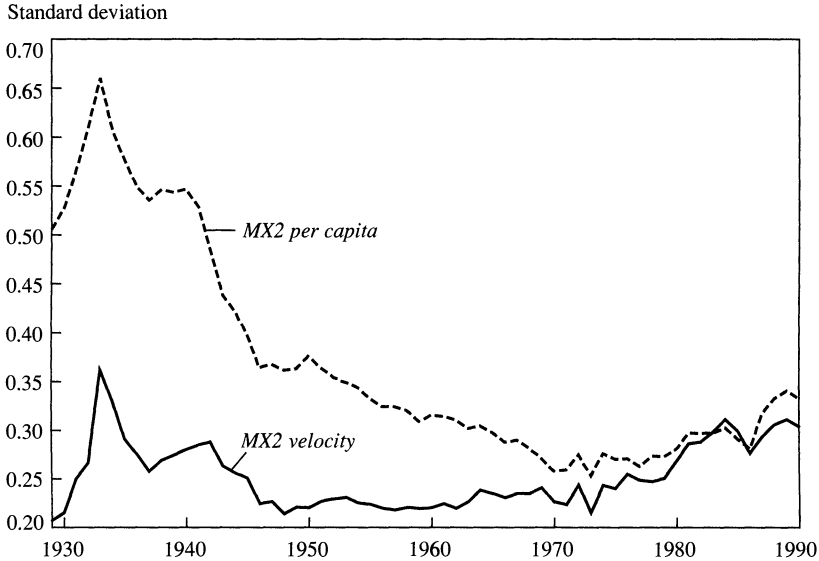
other words, states are not simply miniature replicas of the United States as a whole.

Figure 3 graphs the dispersion of state demand deposits for 1929–90. The dashed line plots the unweighted, cross-sectional standard deviation of log MX1 per capita for 48 states. This measure is very high during the Depression and through World War II (denoted hereafter as the Depression-War period), peaking at nearly 0.65. After World War II, dispersion diminishes steadily until the early 1970s. From 1973 to 1980, it steadily increases. Note that dispersion is never below 0.23. The solid line in figure 3 presents dispersion of the log of MX1 velocity (also calculated as the unweighted, cross-sectional standard deviation of the log MX1 velocity for the 48 states). The pattern is quite similar to that of MX1 per capita, and the measure never falls below 0.20.

Figure 4 presents the cross-sectional dispersion of our broader measure of money, MX2.[17] The dispersion of MX2 per capita follows a similar pattern to that of MX1; it is always above 0.25. The dispersion of the log of velocity (shown as a solid line) is a bit smaller; it has a lower bound of 0.21.

17. One notable outlier occurs in our MX2 data. Delaware experiences an unusually rapid expansion of MX2 during the 1980s.

Figure 4. Dispersion of State Total Demand Deposits, 1929–90



Source: Authors' calculations using data listed in appendix 2. Standard deviations are from cross-sectional estimates of the log of total state demand deposits (MX2) per capita and log MX1 velocity.

As a comparison, the U.S. aggregate time series for log MX1 per capita at constant prices has a standard deviation of 0.26 from 1929 to 1990. When the Depression-War period is excluded, the figure is 0.215.[18] Hence the cross-sectional variation of log MX1 is quite comparable to the U.S. time-series variation.[19] The time-series dispersion of aggregate U.S. (log) MX2 velocity is a mere 0.14 for 1929–90 and is 0.08 for 1947–90.[20] Hence the cross-sectional variation in our data is large.

18. The corresponding figures for U.S. log MX1 velocity are 0.49 for 1929–90 and 0.48 for 1947–90.

19. Time-series variation for individual states is similar to that for the United States as a whole. Constant-price, standard deviations of log MX1 per capita range from 0.18 for New Jersey to 0.60 for Arizona and North Dakota. On average, they are 0.38. The corresponding figures for log MX1 velocity average 0.45 and range from 0.36 for Florida, Vermont, and West Virginia to 0.59 for Delaware.

20. Like the U.S. aggregate, time-series variation of MX2 for individual states is low. Excluding Delaware's standard deviation of 0.40, standard deviations of (log) MX2 velocity range from 0.09 for Indiana, Louisiana, and Tennessee to 0.28 for Maryland and South Dakota. On average, they are 0.16. The corresponding figures for log MX2 per capita, using constant prices, average 0.47. They range from 0.16 for California to 0.78 for South Dakota.

Other Variables

Our primary scale or transactions concept will be personal income, although in part of our analysis we will also consider "consumption," as measured by retail sales. The data set includes annual observations for 48 states compiled by the Bureau of Economic Analysis (BEA).[21] The retail sales series differs from a broad measure of consumption because it excludes services. However, it includes consumer durables and other forms of nonfood consumption that are excluded from bodies of data such as the University of Michigan's *Panel Study of Income Dynamics*, which other researchers have used.

Population density and agriculture's share of personal income are used to capture other state-specific determinants of money demand. We include an agricultural variable as an attempt to capture regional differences in prices or transaction technologies. In particular, we would like to allow for the possibility that new transaction technologies may slowly diffuse from urban to more rural areas. Hence at a given point in time, different states may be undergoing different degrees of technological progress. However, our annual agricultural income series has too much high-frequency variation to capture our notion of technological diffusion. We therefore compute five-year averages of agriculture's share of personal income. Population and area are taken from the Bureau of the Census' *Current Population Report* and the *Statistical Abstract of the United States*, respectively.

For our time-series analysis, MX1, MX2, personal income, and retail sales are expressed in constant prices. We deflated using the U.S. implicit price deflator for personal consumption expenditure taken from the *Economic Report of the President*.

Time-Series Problems and Cross-Sectional Solutions

Traditionally, money demand equations have been estimated with time-series data.[22] The constant elasticity money demand equation given below is typical of those used in time-series analyses:

$$\log m_t = \alpha + \beta \log y_t - \delta \log R_t + \epsilon_t. \tag{1}$$

21. For retail sales the available years are: 1929, 1935, 1939, 1948, 1954, 1958, 1963, 1967, 1972, 1977, 1982, 1984, 1987, and 1989. A *Census of Retail Trade* was not conducted every year.

22. See, for example, the references listed in footnote 6.

In this specification, y is real per-capita output, R is an interest rate, and m is real money balances per capita.

Time-Series Problems

Several difficult issues arise in examining this type of specification. Most of these can be overcome by estimating money demand with cross-sectional data.

First, what is the relevant interest rate? Inventory models of money demand such as those by Baumol and Tobin predict that the interest rate relevant for money demand is the return on an alternative, less liquid asset.[23] For demand deposits, the appropriate asset might be Treasury bills. For a broader concept of money, corporate bonds or equities might be appropriate.

Time-series estimates are somewhat sensitive to the choice of an interest rate. In table 1, we display some time-series regressions of money demand (using U.S. aggregates) for 1932–90. When the Treasury bill rate is used and the equation is expressed in first differences, the estimated income elasticity is 1.32. However, when the Treasury bill rate is replaced by Moody's Aaa corporate bond rate, the elasticity falls to 0.94.[24]

Second, it is difficult to measure "money" consistently over time. It can be persuasively argued that 50 years ago, M1 was a good definition of money, but with technological advances and financial deregulation, a broader concept of money is more appropriate today. Although some attempts have been made to construct a consistent time series for the United States, a cross-sectional analysis can make use of a more consistent definition for money.

Third, how can the analysis deal with both serial correlation of the error term and with nonstationarity? Time-series estimates of equation 1 yield serially correlated errors. According to Robert Lucas, various

23. Baumol (1952) and Tobin (1956).

24. The first three parts of table 1 use the sum of all states' MX1 for every year. As a comparison, the last part of table 1 runs the same regressions over the same time period as the others, but uses M1. For 1959–90, M1 is taken from the *Economic Report of the President*, and for 1929–58, M1 is taken from Friedman and Schwartz (1963). Note that the estimated elasticities for M1 are similar. In particular, note that the point estimates seem quite sensitive to the choice of interest rate. In all sections of table 1, substituting the level of interest rates for the log of the interest rate does not seem to have much impact on the estimated income elasticity.

Table 1. Time-series Estimates with MX1 and M1 Aggregates

Aggregate and period	*Equation form*	*Type of interest rate*	*Income elasticity*	*Interest elasticity*
MX1,	Differenced	None	1.04	...
1932–90			(0.18)	...
		Tbill	1.32	−0.04
			(0.18)	(0.01)
		Commercial paper	1.11	−0.09
			(0.16)	(0.03)
		Corporate bond	0.94	−0.38
			(0.14)	(0.07)
	Trend	None	2.71	...
			(0.24)	...
		Tbill	2.66	0.03
			(0.03)	(0.02)
		Commercial paper	2.80	0.06
			(0.25)	(0.05)
		Corporate bond	2.11	−0.34
			(0.23)	(0.07)
	Level	None	−0.01	...
			(0.08)	...
		Tbill	0.02	−0.01
			(0.15)	(0.04)
		Commercial paper	0.42	−0.22
			(0.15)	(0.06)
		Corporate bond	0.47	−0.65
			(0.12)	(0.06)
MX1,	Differenced	None	0.97	...
1932–79			(0.16)	...
		Tbill	1.21	−0.04
			(0.16)	(0.01)
		Commercial paper	1.03	−0.08
			(0.15)	(0.03)
		Corporate bond	0.88	−0.38
			(0.14)	(0.08)
	Trend	None	2.12	...
			(0.20)	...
		Tbill	2.13	−0.01
			(0.20)	(0.02)
		Commercial paper	1.88	−0.09
			(0.23)	(0.05)
		Corporate bond	1.35	−0.43
			(0.12)	(0.04)

Table 1. *(continued)*

Aggregate and period	*Equation form*	*Type of interest rate*	*Income elasticity*	*Interest elasticity*
MX1,	Differenced	None	1.25	. . .
1947–90			(0.35)	. . .
		Tbill	1.68	−0.08
			(0.33)	(0.02)
		Commercial paper	1.73	−0.10
			(0.33)	(0.03)
		Corporate bond	1.34	−0.31
			(0.28)	(0.06)
	Trend	None	2.76	. . .
			(0.31)	. . .
		Tbill	2.66	0.03
			(0.33)	(0.03)
		Commercial paper	2.69	0.02
			(0.34)	(0.04)
		Corporate bond	2.83	−0.03
			(0.34)	(0.07)
M1,	Differenced	None	0.91	. . .
1932–90			(0.20)	. . .
		Tbill	1.42	−0.07
			(0.16)	(0.01)
		Commercial paper	1.04	−0.11
			(0.17)	(0.02)
		Corporate bond	0.84	−0.35
			(0.16)	(0.07)
	Trend	None	2.32	. . .
			(0.21)	. . .
		Tbill	2.38	−0.04
			(0.02)	(0.02)
		Commercial paper	2.08	−0.15
			(0.19)	(0.04)
		Corporate bond	1.50	−0.46
			(0.13)	(0.04)

Source: Authors' calculations using data listed in appendix 2. The basic regression follows equation 1 in the text and takes the form $\log m_t = \alpha + \beta \log y_t - \delta \log R_t + \epsilon_t$. All variables draw on annual data and are expressed in logarithms. The dependent variable is our measure of demand deposits summed over the 48 states; it is differenced when applicable. The income variable is personal income summed over the 48 states. Both are deflated by the personal consumpation deflator. Standard errors are shown in parentheses. Results for up to three equation forms are reported. "Trend" means that a time trend was included as an explanatory variable. "Differenced" means that equation 1 was estimated in first differences. "Level" means no trend was used.

correction procedures obtain "wildly erratic elasticity estimates."[25] A related problem is the potential nonstationarity of various series.

Table 1 illustrates some of the problems. When a differenced money demand equation is estimated in a time series with U.S. aggregates, the income elasticity is fairly near unity.[26] Elasticities fall to less than one-half when the differenced specification is replaced by a level specification. Adding a time trend variable to the level specification delivers estimated elasticities of nearly three!

Fourth, is the money demand function stable? Many econometricians have argued that U.S. money demand is not stable, meaning that either the intercept or the slope coefficients in the money demand equation, or both, change over time.[27] The next two time periods in table 1 suggest some instability in money demand. When the 1980s are dropped from the 1932–90 sample period, income elasticity estimates fall from about 2.7 (as seen in the entire period) to about 2.0 when the trend specification is used. Dropping the Depression-War period tends to increase income elasticity estimates under a differenced specification; the income elasticities here are about 1.5.

Of course, time-series estimates of money demand assume that the money demand coefficients are constant over time. A cross-sectional approach would instead assume geographical similarities in money demand, at least once certain conditioning variables were held constant. The individual cross-sectional estimates can be used to test the stability of the coefficients over time.

Fifth, if the level of technology is increasing with the level of income, how can the analysis deal with the bias that the omission of technology introduces into the estimated income elasticities? Correlation of the money demand disturbance with real income could very well be the reason behind the apparent instability of the time-series estimates of the income elasticity. The correlation between financial innovation and income growth may vary over time; this will introduce different degrees of bias in different time periods. Observing the instability of the estimated

25. Lucas (1988, p. 140, footnote 2).

26. In table 1, we made no attempt to replicate any of the previous time-series studies of money demand. In particular, we did not correct for serial correlation, we did not include lagged money (except for the case when we used first differences), and we did not perform any of the sophisticated econometric techniques usually used in this literature.

27. See Friedman and Kuttner (1992) and Braun and Christiano (1992).

elasticities, the time-series researcher may be led to think that the true elasticities are unstable when they are not.

Sixth, money demand may be a function of transactions or of some other variable that can be only partially approximated by income (or consumption). It may be that, in the long run, the true scale variable and income move very closely together, while in the short run, income is polluted by all kinds of noise that have nothing to do with money demand. This suggests using very long-run time-series data (as Milton Friedman did in his 1959 study) or using cross-sectional state data.[28] The slow process of convergence documented by Robert Barro and Xavier Sala-i-Martin indicates that cross-state income differentials are quite permanent.[29] As a result, they are likely to be a much better measure of the true transactions-scale variable.

Finally, as Lucas has pointed out, "shifts" in the money demand function can be associated with changes in the stochastic environment.[30] In principle, it is difficult to explain why optimizing agents living in different states should use drastically different forecasting rules, which in turn makes it difficult to pinpoint important geographical differences in the stochastic environment. On the other hand, optimizing agents would probably not apply the same forecasting rules in the 1980s as they did in the 1930s. This is another reason why the underlying parameters of the money demand function could, in principle, be best estimated using cross-sectional data.

Three Plausible Assumptions

To avoid having to solve the difficult problems posed above, the income elasticity of money demand can be estimated using cross-sectional data, if three plausible assumptions are made about regions of the United States.[31]

28. Friedman (1959).
29. Barro and Sala-i-Martin (1991).
30. Lucas (1988).
31. Some of the problems are solved by the mere use of state data. For instance, because states do not print their own money, the demand/supply simultaneity problem arising from monetary policy at the federal level disappears. As we argued above, our data set contains measures of money that are consistent across states for every year. Finally, using a cross section eliminates the worry about the stationarity, integration, and cointegration properties of our series.

The first assumption is that the interest rate relevant for money demand—whether it is the Treasury bill rate, the Aaa corporate bond rate, or the return on some other asset—is the same for every state, or at least uncorrelated with income. Hence for every cross section, the interest rate effect is subsumed in the constant term. This assumption is plausible if it is thought that everybody in the United States has access to the same capital market.

It could be argued that this assumption is not plausible and that low-income regions tend to have higher interest rates because some fraction of their capital stock cannot be used as collateral in nationwide credit transactions. If true, this would tend to bias the income elasticity upward. However, in the appendix we show that the magnitude of such a bias is likely to be smaller than 0.125. Note that the existence of different tax treatments of interest income will tend to introduce different after-tax interest rates across states, even if everybody in the country can buy the same assets (say, Treasury bills). However, to introduce an important bias into our estimates, tax differentials in interest income would have to be highly correlated with income. We know of no evidence supporting such a hypothesis.

The second assumption is that the price level is the same in every state, or at least that it is uncorrelated with the level of income. Again, a U.S.-wide price level is subsumed in the constant term for every cross section.

We make this assumption because data on state price levels are not available. One reasonable conjecture is that richer states tend to have higher price levels. If this were the case, our assumption of constant regional prices would introduce a term $(1 - \beta)p_i$ in the error term (where β is the true income elasticity of money demand and p is the price level of state i). The correlation between the explanatory variable and the error term would introduce a bias in our estimates. If the coefficient of a regression of state prices on state real income is denoted by s, the estimated income elasticity of money demand would be $\beta = \beta + [(1 - \beta)s/(1 + s)]$. Note that the bias introduced by the omission is positive when $\beta < 1$ and negative when $\beta > 1$. Furthermore, the omission of state price levels biases the estimates of the income elasticity of money demand toward one, but it never biases it so much that it overshoots one.[32]

32. In the empirical section, we show that our estimated elasticities are larger than one. The reasoning above suggests that the omission of a state price variable is *not* inducing this result. Omitting state price variables probably yields estimates that are too small.

Our third assumption is that at any particular date, the money demand functions are the same in every state. In the empirical section, we allow for the possibility of different states having different levels of transactions technology (and, therefore, different constant terms) at a given moment in time. However, we impose the same income elasticity for all states at a point in time (although we allow for these income elasticities to differ over time).

This assumption is critical, and there are reasons to believe that it may not be realistic. States do not have uniform banking laws. Some states permit nonbank entities to provide transaction services, while other states have discouraged savings and loan institutions and credit unions from providing transaction accounts. Some states—most notably New York—specialize in providing banking services to out-of-state residents. Such geographical banking differences can be interpreted as violations of assumption 3: there are cross-sectional differences in the level of the money demand function. Because we cannot say a priori how the differences are correlated with income, we cannot place a sign on the bias of our income elasticity estimates. For example, it is possible that the richer states, because they have more professional workers, tend to dominate the banking industry. Our data set would therefore show more deposits in a rich state than its residents would demand; thus our income elasticity estimates would be biased upward. On the other hand, it can be argued that richer states can more readily implement the newest transaction technologies, which allow agents to economize on their cash balances. This second effect would tend to bias our income elasticities downward.

Our regression analysis attempts to assess the quantitative importance of these three assumptions. In some specifications, we try to capture the differing degrees of financial sophistication by introducing the share of income originating in the agricultural sector as an explanatory variable. This is meant to capture the possibility that technology diffuses slowly from urban to rural areas. Some of our other specifications use a state's population density as an alternative to the agricultural variable.

To the extent that geographical differences in price levels, financial sophistication, banking industries, and banking laws persist over time, state fixed-effects estimated in a pooled regression should mitigate the bias of our income elasticity estimates. A comparison of income elasticity estimates obtained with and without state fixed-effects will therefore provide an indicator of the qualitative impact of assumptions 2 and 3.

Under our assumptions, a constant elasticity money demand equation for period t is

$$\log M_{it} = \alpha_t + \mu_i + \beta_t \log Y_{it} + \gamma Z_{it} + \epsilon_{it}, \tag{2}$$

for i = AL, AZ, . . . WY and t = 1929, 1930 . . . 1990. In this equation, M is nominal money per capita, Y is per capita nominal income, and Z is a vector of state variables such as population,[33] population density, agricultural sector's share of income, and regional dummies. Nominal money is appropriate if all states have the same price level since, as we have already argued, the price level is subsumed by the constant term α_t. Some of our empirical analysis will allow for state fixed-effects. All of our regressions will allow for time effects.

In conclusion, it may be preferable to estimate money demand functions using cross-sectional data, rather than time-series data. First, interest rates do not appear in a cross-sectional regression, so the econometrician can estimate the income elasticity without settling on a particular interest rate series. Second, it is easier to consistently define money in a cross section. Third, the cross-sectional approach conveniently sidesteps some difficult time-series questions such as, is money demand stable? or what are the time-series properties of the money demand errors? While avoiding these issues, a cross-sectional analysis permits us to estimate the income elasticity of the demand for money and to examine the stability of those estimates over time.

Our cross-sectional approach, however, also has drawbacks. First, currency is excluded. Second, the census data upon which our series are based in some instances count only a subpopulation, such as FDIC-insured commercial banks. Third, we rely on the geographical similarity of money demand functions, although we permit money demand to change over time. Fourth, to the extent that state income differentials vanish slowly over time (as Barro and Sala-i-Martin demonstrate they do),[34] our estimates are closer to what time-series analysts call "long-run elasticities." Hence our analysis is silent as far as short-run elasticities are concerned. As we argued in the previous section, however, this may be more of an advantage than a disadvantage.

There are some criteria for which time-series and cross-sectional approaches cannot be ranked a priori. For instance, income can have tran-

33. We also include population in an attempt to correct for any aggregation bias.

34. Barro and Sala-i-Martin (1991).

sitory components in both a time-series and a cross-sectional sense. If permanent income determines money demand, then the income elasticity obtained with actual income will be biased downward. Actual income is a noisy proxy for permanent income.[35] An opposing upward bias would result if money is a store of value during periods of high transitory income. Based on some of our results, we will conclude that transitory components of income are not quantitatively problematic for our cross-sectional estimates of money demand.

A Literature Review

A number of studies in the 1960s examined money demand with cross-sectional data. Allan Meltzer's 1963 study of cross sections of business firms in different sectors for different years contains 126 regressions. Meltzer's point estimates of the sales elasticity of money demand range from 0.88 to 1.27, with the bulk of them (100 of 126) above one.[36]

Edgar Feige's 1963 doctoral thesis utilizes state deposit data spanning eleven years (1949–59). Although the focus of his work is on the cross-price elasticities of the demand for commercial bank deposits, he does offer some estimates of the income elasticity, and they are close to one.[37]

There are two reasons to believe that his estimates are too low. First, he does not allow for time effects. Thus the time-series problems and biases we highlighted in the previous section apply to his analysis. This is particularly true when he introduces a large number of regional dummies, which remove the cross-sectional variation and leave only the time-series variation. Second, as we will note in the next section, the 1950s are somewhat unusual, perhaps because they were an era of increasing financial sophistication and financial innovations diffused from urban (richer) areas to rural (poorer) areas. This tends to bias the estimates of the income elasticity downward. Once we introduce some conditioning variables to proxy for this phenomenon—which Feige does not do—our estimates for the 1950s coincide with those for the other decades.

35. See Friedman (1959).
36. Meltzer (1963b, table 1, p. 411).
37. Feige (1963, p. 34).

Philip B. Hartley, Tong Hun Lee, and Bruce C. Cohen run similar regressions for the same time period as Feige.[38] When they do not introduce large numbers of regional dummies (so the cross-sectional variation is left intact), the estimated income elasticities are significantly larger than one. When they introduce a large number of regional dummies, the estimates fall below one. None of the studies includes time effects.

In a 1974 study, Feige introduces time effects in one regression for the period 1949–65.[39] The problem is that he also introduces state effects. (He never runs a regression with time effects and no state effects.)

We think that income elasticities that are estimated together with state effects in such a short sample are troublesome for two reasons. First, as Feige notes in his thesis, the BEA's personal income estimates and population figures are obtained by interpolating between benchmark years.[40] The state dummies remove the cross-sectional variation of the income variable. Because of the interpolation procedure, the remaining time-series variation is just noise; as a result, the estimates are not reliable.[41]

Second, as we argued in the previous section, income in the short run is polluted with noise that has little to do with its role as a scale variable (intended, presumably, to measure transactions). The removal of the cross-sectional information (which measures long-run variation of income much more closely) leaves a close-to-meaningless measure of transactions as the single explanatory variable. (Note that these two criticisms apply only when the fixed-effects model is used with a very short sample period.)

In 1974, Feige and P.A.V.B. Swamy also estimated a similar model with random effects. Unfortunately, we do not think that their elegant

38. Hartley (1966); Lee (1966); and Cohen (1967).

39. Feige (1974).

40. After 1965, the BEA yearly estimates are not based on interpolation, but instead on quarterly reports from the State Employment Security Agencies. (Only dividends, which represent less than 3 percent of personal income, are based on interpolation of benchmark years.) See the "Sources and Methods" section of Bureau of Economic Analysis (1986).

41. This point is not a very significant criticism of Feige's excellent study because he was mainly interested in cross-price elasticities. Writing his thesis in the early 1960s, he also faced more data and computational limitations. In fact, his computing power was so limited that he had to divide his model with fixed effects and time effects into five-year periods because he could not handle the entire sample at the same time. This smaller time span of his actual regressions reinforces the problems we mentioned above.

random-effect model is the correct model to use in estimating the income elasticity from panel data because it assumes that the expected value of the constants is time-invariant. As we argued in the previous section, we think that the error term of the money demand equation, which embodies the unobservable financial technology, is both changing over time and correlated with income over time. The assumptions of Feige's and Swamy's random-effect model do not allow for the removal of such correlation. Thus Feige and Swamy's estimates of the income elasticity will be biased downward for the same reason as the time-series estimates are.[42]

Finally, Arthur Gandolfi and James Lothian ran panel regressions of money on income and interest rates on demand and other deposits for the period 1929–68. They used total deposits in commercial and mutual savings banks as their concept of money and found that the slope of the money demand function was about 1.3.[43] They made no attempt to allow for time or geographical differences in price levels, banking laws, or financial sophistication, nor did they consider alternative concepts of money.

The results described in this literature highlight three systematic relationships across studies. First, when the econometric specifications allow for the cross-sectional variation to dominate, the estimated income elasticities are significantly larger than one (and always close to 1.3–1.4). Second, when the cross-sectional variation is removed from the data (with the introduction of a large number of regional dummies, with state fixed-effects, or with any other procedure), the estimated income elasticity falls below one and is always close to 0.9.[44] Third, no matter what or how many explanatory variables are included, the hypothesis that the estimated income elasticity is stable over time is never rejected.

42. Another reason not to trust Feige's and Swamy's estimates is that the estimated variances associated with some of the (random) coefficients are negative. Feige and Swamy (1974, p. 249).

43. Gandolfi and Lothian (1976, p. 48).

44. As we argued before, the reason these estimates are lower is that the number of years used in the analyses is rather small. We think that the income elasticity needs to be estimated using long-run variation of income. This can be achieved by using very long-run time-series data or by using cross sections in which income differentials are quite persistent. Because the authors in the 1960s used short time-series, every time they got rid of the cross-sectional variation by introducing large amounts of regional dummies, they got low estimates.

Regression Results

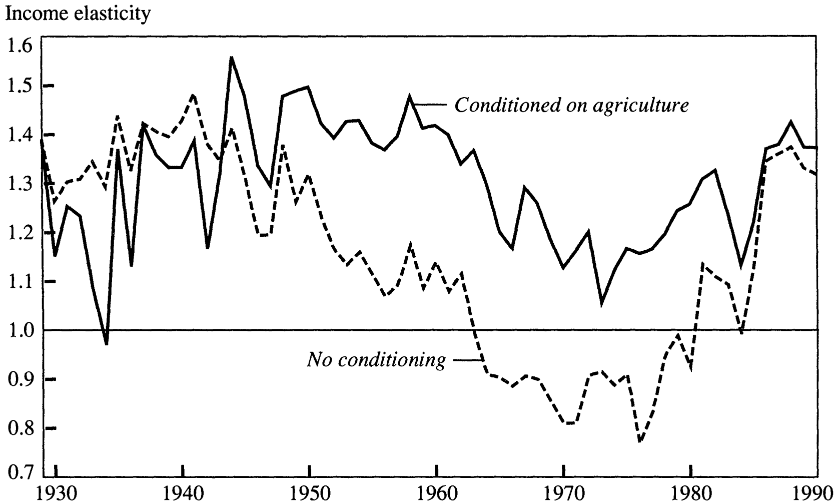
In this section, we present our cross-sectional money demand estimates. We begin by showing that the income elasticity estimated from a pooled sample without controlling for any other variables is significantly larger than one. However, we find substantial year-to-year variations in cross-sectional estimates. In the next section, we add conditioning variables and find sharper results. With this equation—our preferred version—we cannot reject stability of the income elasticity; the estimates from individual cross sections do not differ significantly from the estimate for the pooled sample. Estimates for the first and second half of the sample are not significantly different. Consumption elasticities are remarkably similar, although it is personal income that has the most explanatory power for money demand. Conclusions for MX1 carry over to our broader money concept MX2, at least when conditioning variables are included.

Cross-sectional Estimates of Univariate Regressions

Table 2 shows regression estimates of the income elasticities of money demand for five-year intervals. The dependent variable in all regressions is the log of the stock of demand deposits (MX1) per capita in year t. The first two columns of results report the log of per capita income as the only regressor in each of the years. Figure 5 plots the annual cross-state income elasticity of money demand (MX1) from 1929 to 1990 for this case. The dashed line plots the annual income elasticities corresponding to the first column of results in table 2. The solid line plots the annual income elasticities when we allow for special characteristics of agricultural states, as discussed below.

The top entry in the first column of results of table 2 shows that the cross-sectional money demand elasticity for 1930 was 1.26 (s.e. = 0.10). Hence not only is the coefficient significantly positive, but it is also significantly larger than one. The number to the right of the elasticity is the $\bar{R}^2$, which in this case is 0.78. The standard error of the regression is 0.26, shown in parentheses below $\bar{R}^2$. This good fit can also be seen in figure

Figure 5. Cross-state Income Elasticity of Money (MX1) Demand, 1929–90



Source: See appendix 2 for detailed source notes. The figure plots the annual income elasticities based on the regression results obtained in table 1. The dashed line shows the annual income elasticities with no added conditioning variables. The solid line plots the income elasticities when the log of the share of income originating in the agricultural sector is added as a regressor.

6, which presents scatter diagrams of (log) personal income per capita versus the (log) of MX1 per capita in 1930, 1950, 1970, and 1990.

As can be seen from the $\bar{R}^2$ reported in table 2, the fit is not as good for 1990 as it was for 1930, but it is better than for 1970.[45] Figure 7 is a scatter diagram of the log of MX1 per capita and the log of income per capita in all 62 cross sections (from 1929 to 1990) at the same time. Time and state fixed-effects are extracted from each data point to yield an impressive picture that clearly presents the goodness of fit of these state money demand equations. The slope of the regression line in figure 7 is 1.45.

For all the years before 1963, the point estimates are above one. For the period between 1963 and 1980, the point estimate falls below one,

45. Although the $\bar{R}^2$ statistics of the cross-sectional regressions changed dramatically with time, the standard errors of the regressions do not change as much. Hence, we do not report weighted least squares estimates; the weighted least squares (WLS) elasticities will be very close to the ordinary least squares (OLS) estimates. For example, the restricted WLS income elasticity estimate for the second column of table 2 is 1.30, compared with 1.31 for OLS.

Table 2. Cross-state Regression Estimates of the Income Elasticity of Money (MX1) Demand

			With agriculture's share		
Year	*Income elasticity*	$\bar{R}^2$ [$\hat{\sigma}$]	*Income elasticity*	*Agriculture coefficient*	$\bar{R}^2$ [$\hat{\sigma}$]
1930	1.26	0.78	1.15	−0.07	0.79
	(0.10)	[0.26]	(0.14)	(0.06)	[0.26]
1935	1.44	0.74	1.37	−0.04	0.74
	(0.12)	[0.29]	(0.20)	(0.09)	[0.29]
1940	1.42	0.73	1.33	−0.05	0.73
	(0.13)	[0.31]	(0.19)	(0.08)	[0.31]
1945	1.31	0.57	1.48	0.08	0.58
	(0.17)	[0.28]	(0.20)	(0.05)	[0.05]
1950	1.32	0.59	1.50	0.10	0.62
	(0.16)	[0.26]	(0.18)	(0.05)	[0.25]
1955	1.11	0.45	1.37	0.11	0.49
	(0.18)	[0.27]	(0.22)	(0.05)	[0.26]
1960	1.14	0.44	1.42	0.11	0.49
	(0.19)	[0.25]	(0.21)	(0.05)	[0.24]
1965	0.90	0.29	1.20	0.10	0.36
	(0.20)	[0.24]	(0.36)	(0.04)	[0.23]
1970	0.81	0.24	1.12	0.09	0.31
	(0.20)	[0.22]	(0.24)	(0.04)	[0.21]
1975	0.91	0.20	1.16	0.10	0.36
	(0.25)	[0.23]	(0.24)	(0.03)	[0.20]
1980	0.92	0.16	1.26	0.11	0.25
	(0.30)	[0.27]	(0.31)	(0.04)	[0.25]
1985	1.15	0.38	1.24	0.03	0.37
	(0.21)	[0.22]	(0.24)	(0.04)	[0.22]
1990	1.31	0.40	1.37	0.02	0.39
	(0.23)	[0.25]	(0.26)	(0.04)	[0.25]

Source: See appendix 2 for detailed source notes. The dependent variable is the log of nominal money (MX1) per capita. Data are annual and by state. Standard errors for the explanatory variables are shown in parentheses below the point estimates. The standard errors for the regressions are shown in brackets below $\bar{R}^2$. A constant for each year (not shown in the table) is estimated in all regressions.

but the standard error of these estimates increases substantially. In the first row of the addendum to table 2, after the five-year intervals of the first column, we report the income elasticity when it is constrained to be equal across all 62 years. The constrained result is 1.25 (s.e. = 0.02). The low elasticities in the 1960s and 1970s are reflected in pooled regressions

Table 2. (*Addendum*)

	Period	*Income elasticity*	*Income elasticity w/ ag. share*
Income elasticity constrained over period:	1929–90	1.25 (0.02) F = 1.17[a]	1.31 (0.03) F = 0.42[a]
	1929–59	1.32 (0.03)	1.33 (0.03)
	1960–90	1.03 (0.04) F = 32.84[b]	1.27 (0.04) F = 3.15[b]
Income elasticity constrained and data pooled over five-year intervals:	1930–90	1.26 (0.04)	1.32 (0.06)
	1930–55	1.33 (0.05)	1.34 (0.07)
	1960–90	1.03 (0.08) F = 8.57[c]	1.27 (0.09) F = 0.32[c]
Income elasticity constrained and state effects removed:[d]	1929–90	1.45 (0.02)	1.20 (0.02)
	1947–90	1.36 (0.03)	1.34 (0.03)
	1960–90	1.59 (0.05)	1.52 (0.05)

a. The income elasticities are constrained to be the same over the periods shown. The F-test is based on the null hypothesis that the coefficients on income are the same across all 62 years. The 0.05 critical value with 61 degrees of freedom for the numerator and more than 1000 degrees for the denominator is 1.32.

b. The F-test is based on the null hypothesis that the coefficients on income are the same in the two subperiods (each subperiod includes thirty-one years). The 0.05 critical value with 1 degree of freedom for the numerator and more than 1000 for the denominator is 3.84.

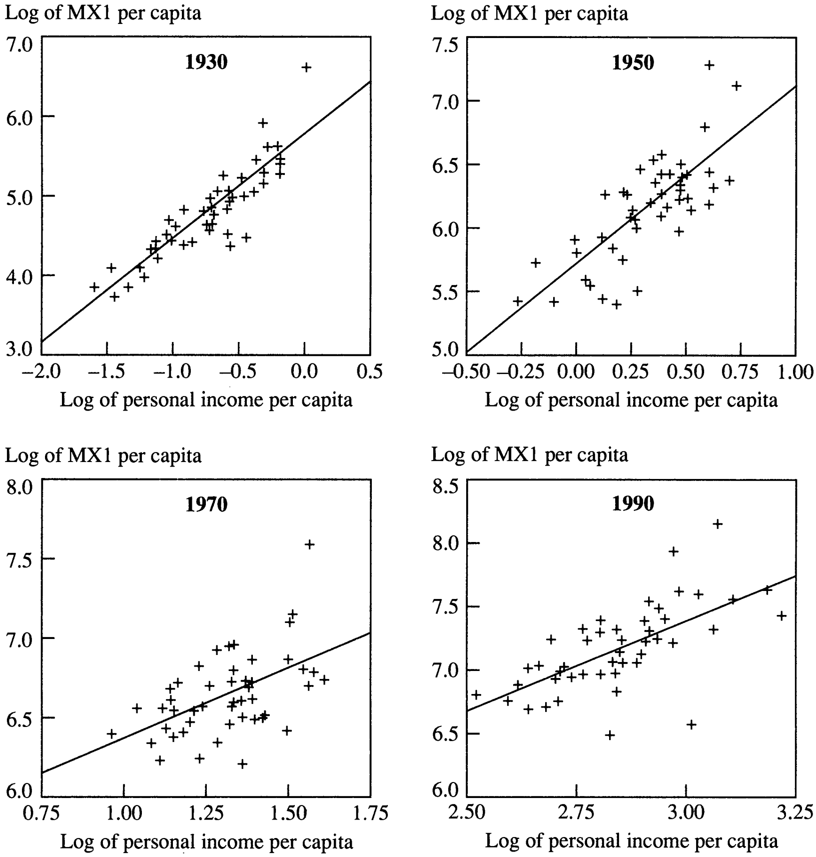
c. The F-test is based on the null hypothesis that the coefficients on income are the same for the two subperiods (the first subperiod includes seven years at five-year intervals and the second includes six years at five-year intervals). The 0.05 critical value for 1 degree of freedom in the numerator and more than 400 for the denominator is 3.86.

d. The rows next to the label "Income elasticity constrained and state effects removed" report income elasticities and their standard errors when a constant is estimated for each state, as well as for each year, while a single income elasticity is estimated. As above, coefficients on agriculture's share are not restricted over time.

that exclude the Depression-War period. For the period 1947–90 (not shown in table 2), our restricted estimate is 1.10 (s.e. = 0.03).

One of the key questions asked in the money demand literature is whether the elasticity of money demand is stable over time. Here we address part of that question: the stability of the income elasticity of money demand. Testing the null hypothesis that the constrained coefficients on income are constant across the 62 years, we find that the F-statistic is

Figure 6. State Cross Sections of Money (MX1) and Personal Income per Capita, Various Years



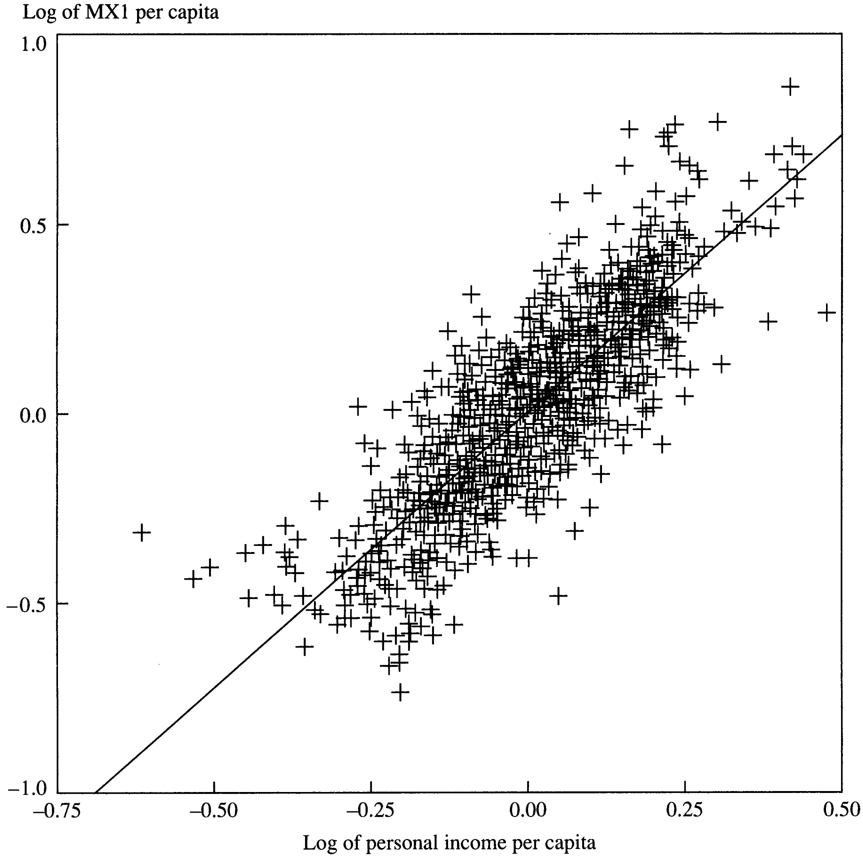
Source: See appendix 2 for detailed source notes.

1.17.[46] We cannot reject the hypothesis that the income elasticity has been stable over the long sample period 1929–90.[47] If we restrict the first 31 years of the sample to have the same coefficient on income, the estimate is 1.32 (s.e. = 0.03). The corresponding constrained coefficient for the second 31-year subperiod is 1.03 (s.e. = 0.04). A test of the hypothe-

46. The restricted estimates of the elasticity and standard errors when we use information over five-year intervals (13 time-series data points) coincide up to the third decimal point with those we get when we use all the available information. This suggests that serial correlation is not likely to be a problem.

47. The 10 percent critical value is 1.24. The null hypothesis cannot be rejected even at this significance level.

Figure 7. State Cross Sections of Money (MX1) and Personal Income per Capita, 1929–90



Source: See appendix 2 for detailed source notes. The slope of the regression line for 62 cross sections over 48 states is 1.45. Both series are corrected for state effects and time effects.

sis that the two subperiods have identical income elasticities is rejected at the 5 percent level of confidence. (The F-statistic is 32.84 and the 5 percent critical value is 3.84.)

We noted above that the income data before 1965 were constructed by interpolating estimates at approximately five-year intervals. This suggests that the yearly observations do not provide independent information on the money demand function. We reestimated our pooled regressions using data at five-year intervals only. The restricted estimate for the entire period is 1.26 (s.e. = 0.04). The estimate for the first half of the sample period is 1.33 (s.e. = 0.05) and the estimate for the second half is 1.03 (s.e. = 0.08). The hypothesis that the two coefficients are the same is rejected. (The F-statistic is 8.57 and 5 percent critical value is 3.86.) Our main result is that the estimates of income elasticities with no conditioning appear to be unstable.

Adding Some Conditioning Variables

Consider the individual elasticities in the first two columns of results in table 2. Notice that the point estimates for the 1960s and 1970s are smaller than the rest. This is the main reason for rejecting the stability hypothesis. One reason why the elasticities in the 1960s and 1970s are smaller could be that the introduction of financial technologies follows a slow process of regional diffusion. Hence in any given year, different states may enjoy different degrees of financial sophistication. To the extent that the high-income states tend to implement those technologies faster (perhaps because they are urban states where it pays banks to introduce the technological innovations more quickly, or perhaps because, when wages are higher, it is more costly for people to go to the bank), the coefficients on income would tend to be biased downward.

AGRICULTURAL SHARES. To assess this possibility, we introduce an additional regressor: the log of the share of income originating in the agricultural sector. We believe that the process of diffusion of financial technologies is likely to start in urban areas and slowly extend to rural areas. We expect to find, therefore, a positive association between the agricultural shares (which we call AGRY) and demand deposits.

Time-series studies have traditionally dealt with changes in the degree of financial sophistication by positing an inverse relationship between the degree of financial sophistication in a given region and its amount of agricultural activity. James Tobin has argued that as the United States moved out of agriculture in the late 19th and early 20th centuries, the nation became more financially sophisticated and that this trend caused a steady shift in money demand.[48] However, Tobin and others have argued that the omission of a financial sophistication variable would bias income elasticity estimates upward, because in poor agricultural areas, the "money economy" was limited in scope.[49]

The last set of regressions in table 2 report estimates of income elas-

48. Tobin (1965).

49. Bordo and Jonung (1990, p. 167).

ticity when we have introduced as an explanatory variable a state's agricultural activity as a proxy for its financial sophistication. We find that *only one* of 62 estimates of the income elasticity is now below one—the point estimate for 1934—while only three fall below 1.1. We also find that the agricultural share is statistically insignificant between 1929 and 1950. In 1950, it starts having a significantly positive effect on the demand for money. The positive association between AGRY and MX1 disappears in the 1980s. Under the hypothesis of slow technological diffusion, this would suggest that between 1950 and 1980, a process of financial innovation occurred that moved slowly from urban to rural areas. Hence other things being equal, rural states tended to demand relatively more money over this period.

The introduction of AGRY, however, increases the point estimates of the income elasticity for the periods when the elasticity was previously below one. (Compare with the results from the first two columns of table 2.) This, again, is consistent with the concept of slow regional diffusion of technology as a source of bias in the univariate regressions. Once we correct for this bias, the estimates of the income elasticity move up to their true values.

In the addendum to table 2, the restricted elasticity when AGRY is included is 1.31 (s.e. = 0.03), significantly larger than one. The F-statistic is now 0.42. Thus the hypothesis of a stable income elasticity cannot be rejected, even at the 10 percent significance level. Dividing the sample period into two subsets of equal size and restricting the elasticities to be the same within subsamples yields an estimate of 1.33 (s.e. = 0.03) for the period 1929–59 and 1.27 (s.e. = 0.04) for 1960–90. The hypothesis that the elasticities are the same across the two 31-year periods cannot be rejected. (The F-statistic is 3.15 and the 5 percent critical value is 3.84.)

Reestimating the pooled regressions at five-year intervals yields a restricted elasticity of 1.32 (s.e. = 0.06). The elasticities for the two subperiods are 1.34 (s.e. = 0.07) for 1930–55 and 1.27 (s.e. = 0.09) for 1960–90. Again, an F-test of the hypothesis that the elasticities are the same across the two thirty-year subperiods cannot be rejected at the 5 percent level of confidence. (The F-statistic is 0.32 and the 5 percent critical value is 3.86.)

Thus the main result is that once we hold constant what we think is a proxy for the degree of financial development, the income elasticities appear to be very stable over the entire sample period of 1929–90.

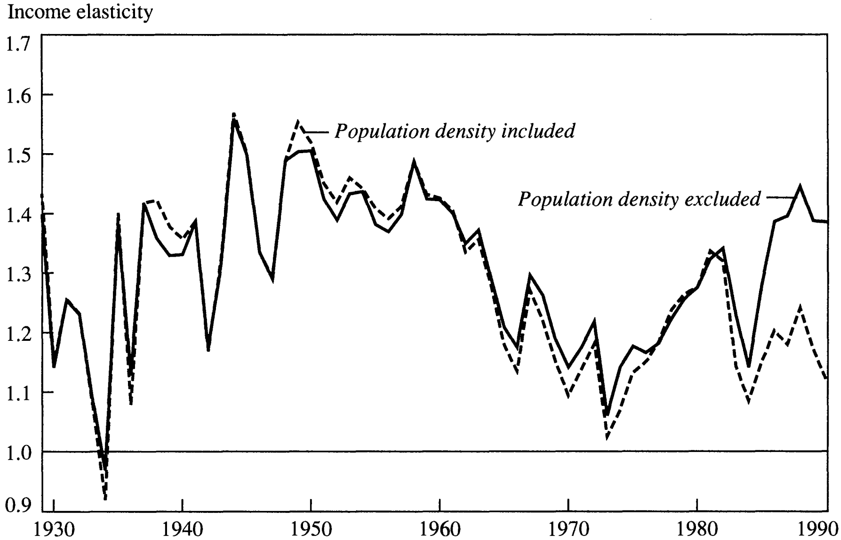
POPULATION DENSITY. Another possible measure of urbanization is the population density of a state. When we looked at population density as an explanatory variable, it did not seem to have an important effect on the estimates we reported in table 2. For instance, when we included it along with agriculture's share of income, we found it to be not significantly different from zero in most of the 62 years.[50] The restricted income elasticity was 1.36 (s.e. = 0.03), compared with 1.36 (s.e. = 0.03) when density is excluded. Furthermore, our previous results for agricultural shares do not change when density is included: agricultural shares have statistically positive coefficients in the 1950s, 1960s, and 1970s.

The only individual years for which the density variable seems to make a substantial difference are the last five years of the sample, 1986–90. Density has smaller but perceivable effects in 1938–40 and 1970–74. The solid line in figure 8 plots the yearly income elasticities when population density is excluded. The dashed line represents the estimates when density is included. The two lines follow each other in an almost perfect fashion until 1985. After that, the elasticities we found when density was included were substantially smaller and not significantly different from one. The maximum difference between the two estimates occurs in 1990. Hence running a single cross section for 1990 will reveal that the income elasticity is not different from 1.0. This, however, does not happen for any other year before 1985. A cross-state regression for the early 1970s might also yield a point estimate between 1.0 and 1.1. However, figure 8 shows that such a result does *not* depend on the inclusion of population density.

This finding is significant in the light of Julio Rotemberg's comments on this paper. Rotemberg runs a single cross-section regression for 1990 and includes a measure of urbanization as an explanatory variable in the money demand regression. He finds that, by including urbanization, the estimated income elasticity falls below one. We do not have his measure of urbanization, but we suspect that it is highly correlated with our measure of population density. As a result, we suspect that if he tried to run his regression for other years, he would also find that the income elasticity is unchanged. Furthermore, if he constrained the elasticity to be the same for all periods, we suspect that he would find an estimate of 1.36 and would be unable to reject stability of the income elasticity over time.

50. The null hypothesis that all the coefficients on population density are zero can be rejected in a regression of MX1 on time effects, personal income, and population density. (For personal income, a single income elasticity is estimated.)

Figure 8. Cross-state Income Elasticities, for MX1 with and without Population Density, 1929–90



Source: See appendix 2 for detailed source notes. The figure plots the annual income elasticities using the authors' regression results. The solid line plots the log of nominal money (MX1) per capita as the dependent variable and the log of income per capita and agriculture's share of income as explanatory variables. The dashed line is similarly specified, but includes population density as a third explanatory variable.

STOCK OF POPULATION. As we argued above, the aggregation of families and firms into states implies that state population is a relevant variable. When we included state population in the money demand equation with agriculture's share of income, we found that the estimated income elasticities and their stability tests were not substantially different from those reported in table 2 when agriculture's share of income was included. We also found that the coefficient on the log of population was fairly stable over time: close to 0.1. We have not displayed these results because they are similar to those shown in table 2.

Other Geographical Differences in Financial Sophistication and Institutional Arrangements

It could be argued that New York City, a major world financial center, may be distorting our estimates of the income elasticity. It is true that New York State has relatively more demand deposits per capita (a large fraction of which are owned by non–New Yorkers) than the other

47 states. It is also true that for some of the years included in our sample, New York was also among those states with the highest per capita income in the country. Hence, New York could be considered an outlier that biases our point estimates.

We reestimated all the regressions in table 2, excluding New York, and found little difference in our original estimated elasticity or the stability of the coefficients over time. For instance, the restricted point estimate when we include the agricultural share (excluding New York) is 1.26 (s.e. = 0.02), while with New York, we found a value of 1.31 (s.e. = 0.03). Here the F-statistic is 0.96, well below the 5 percent critical value of 1.32. The F-statistic in this case is 1.20, again below the 5 percent critical value. Thus our conclusions about the magnitude and stability of the income elasticity are not driven by the influence of New York State.

STATE FIXED-EFFECTS. New York may not be the only state whose deposits could be considered "unusual" given its income. Other states besides New York may specialize in banking. Others may have peculiar banking laws. To the extent that such phenomena persist, their influence on our estimated income elasticity can be removed by estimating state fixed- effects in a pooled regression. Even if the phenomena are not perfectly constant over time, a comparison of estimated income elasticities in pooled regressions with and without state effects should indicate how our estimated elasticities compare qualitatively to the true elasticity.

The last part of the addendum to table 2 labeled "income elasticity constrained and state effects removed" reports the restricted income elasticities and their standard errors when state dummies are added to a pooled regression of MX1 on personal income and time dummies. The income elasticity is 1.45 (s.e. = 0.02) in the full sample. Notice that this is an increase over the corresponding estimate without state effects, 1.25 (s.e. = 0.02). The same is true for later samples. The 1947–90 pooled regressions (not reported) also exhibit an increase in the income elasticity from 1.10 to 1.36 when state effects are added. Furthermore, the postwar period is more like the full sample period when the state effects are estimated.[51] The introduction of state effects also produces a higher income elasticity in the 1960–90 period, 1.59 (s.e. = 0.05).

51. The 1980s are infamous in monetary economics, but they do not drive our finding of an income elasticity above 1.0. We ran, but did not report, a pooled regression that includes only the 1950s, 1960s, and 1970s and estimates state and time effects. The income elasticity is 1.34 (s.e. = 0.03).

We also introduced state effects into the pooled regressions, which include agriculture's share of income as an explanatory variable. In the full sample period, the introduction of fixed state effects reduced the estimated income elasticity from 1.31 to 1.20. This small reduction appears to reflect a reduction in the first half of the sample period. For the 1947–90 period, the estimated income elasticity increases slightly (1.34 with state effects, and 1.33 without them). For the 1960–90 period, the estimates with state effects are much higher (1.52) than those without state effects (1.27).

The inclusion of state effects can be seen as a test of the claim that our estimates reflect supply rather than demand for deposits. Some analysts have claimed that states specializing in banking hold large amounts of deposits for out-of-state agents (firms and families). To the extent that these states tend to have higher incomes, our income elasticities would be biased upward. Under this view, the introduction of state dummies will tend to correct for the initial omission, so the income elasticities estimated with fixed-effects will be closer to the true elasticities. That is, they will tend to be lower. In five of the six cases shown in table 2, the introduction of state effects increases our elasticity estimates. Thus we conclude that ignoring geographical differences in financial sophistication, institutional arrangements, or price levels leads to income elasticities that are too small.

The Choice of a Scale Variable: Consumption or Income?

We now use retail sales as a measure of consumption to analyze an alternative scale variable. N. Gregory Mankiw and Lawrence Summers estimate time-series money demand equations and argue that consumption is a better scale variable than income because it more accurately reflects permanent income.[52]

As we mentioned in the first section, the *Census of Retail Trade* is not conducted every year. In order to achieve comparability, the first set of regressions reported in table 3 uses the log of personal income per capita as the scale variable for the years for which retail sales are available. As we found in table 2, the coefficient is stable over the entire sample, with a restricted estimate of 1.28 (s.e. = 0.04). The F-statistic is 1.4, below the 5 percent critical value of 1.79. The second set of regressions in

52. Mankiw and Summers (1986).

Table 3. Comparing Income and Consumption Elasticities for MX1

Year	*Income elasticity*	$\bar{R}^2$ [$\hat{\sigma}$]	*Consumption elasticity*	$\bar{R}^2$ [$\hat{\sigma}$]
1929	1.37	0.83	1.46	0.69
	(0.09)	[0.22]	(0.14)	[0.30]
1935	1.44	0.74	1.25	0.55
	(0.12)	[0.29]	(0.17)	[0.39]
1939	1.39	0.72	1.28	0.53
	(0.13)	[0.30]	(0.17)	[0.39]
1948	1.38	0.57	1.24	0.50
	(0.17)	[0.26]	(0.18)	[0.29]
1954	1.16	0.47	1.39	0.48
	(0.18)	[0.27]	(0.21)	[0.27]
1958	1.19	0.44	1.39	0.43
	(0.19)	[0.26]	(0.23)	[0.26]
1963	1.00	0.36	1.28	0.36
	(0.19)	[0.24]	(0.25)	[0.24]
1967	0.90	0.28	1.21	0.36
	(0.21)	[0.24]	(0.29)	[0.23]
1972	0.90	0.26	0.81	0.08
	(0.21)	[0.21]	(0.35)	[0.23]
1977	0.82	0.15	0.41	0.01
	(0.27)	[0.24]	(0.36)	[0.26]
1982	1.11	0.28	0.65	0.27
	(0.25)	[0.23]	(0.34)	[0.05]
1989	1.33	0.46	0.86	0.12
	(0.21)	[0.24]	(0.32)	[0.31]
Addendum				
Elasticities	1.28	. . .	1.26	. . .
constrained	(0.04)	. . .	(0.06)	. . .
	F = 1.40	. . .	F = 1.14	. . .

Source: See appendix 2 for detailed source notes. The dependent variable is the log of nominal money (MX1) per capita. Data are annual and by state. Standard errors for the income and consumption elasticities are shown in parentheses below the point estimates, while the standard errors for the regressions are shown in brackets below $\bar{R}^2$. A constant for each year is estimated, but not reported. The first set of regressions includes the log of personal income per capita as the only regressor. This regression differs from the first regression of table 2 because here we include only the years for which retail sales are available, to achieve comparability. The second set of regressions uses retail sales as its only regressor and as its proxy for consumption. The F-test is based on the null hypothesis that the coefficients on income and consumption are the same across the 12 subperiods. It follows an F-distribution. The 0.5 percent critical value with eleven degrees of freedom is 1.79. The 10 percent critical value is 1.58.

table 3 substitutes consumption (using the log of retail sales per capita as a proxy) for personal income as the scale variable. The consumption elasticity is estimated to be above one for all periods before 1972. The point estimate falls and the standard error increases after that date. The restricted point estimate over the entire sample period is 1.26 (s.e. = .06). A test of the stability of the coefficients fails to reject the hypothesis that they are stable over time. Hence consumption also seems to be a good scale variable.

A key question posed by Mankiw and Summers is which of the two scale variables fits the data better.[53] We can answer this question by putting both explanatory variables in the same regression. If we restrict the coefficients of both consumption and income over time (not shown), we find that the coefficient on income is 1.30 (s.e. = 0.08) and the one on consumption is not significant (−0.03, s.e. = 0.10). Mankiw and Summers found the opposite result.

A Broader Definition of Money

Table 4 reproduces table 2, except that the dependent variable is MX2 (total deposits), rather than MX1 (demand deposits). The first two columns of results in the main part of table 4 show the univariate relations between the log of total deposits per capita and the log of personal income per capita. Figure 9 plots the cross-state income elasticity for total deposits (MX2) from 1929 to 1990. The coefficients, plotted as the dashed line in the figure, fluctuate between 1.51 for 1935 and 0.34 for 1983. The restricted point estimate is 1.24 (s.e. = 0.02), which is statistically different from one. The F-statistic for the test of stability of coefficients is 2.52, higher than the 5 percent critical value of 1.32.

As shown in the addendum to table 4, a restricted estimate over the subperiod 1929–59 yields an elasticity of 1.34 (s.e. = 0.02). The corresponding number for the subperiod 1960–90 is 0.87 (s.e. = 0.04). An F-test of the hypothesis that the elasticities are the same over the two subperiods is rejected at all sensible levels of confidence. (The F-statistic is 99.28 and the 5 percent critical value is 3.84.)

Reestimating the elasticities using only data at five-year intervals yields a restricted value of 1.24 (s.e. = 0.04) for the entire sample period. Dividing the sample into two subperiods of 30 years yields an estimate of 1.35 (s.e. = 0.04) for the subperiod 1930–55 and 0.89 (s.e. = 0.09) for the subperiod 1960–90. An F-test of the equality of elasticities across the two subperiods is clearly rejected. (The F-statistic is 22.29 and the 5 percent critical value is 3.86.) Thus the univariate cross-sectional regressions from MX2 are *not* stable over time.

53. Mankiw and Summers (1986).

Table 4. Cross-state Regression Estimates of the Income Elasticity of Money (MX2) Demand

			With agriculture's share		
Year	*Income elasticity*	$\bar{R}^2$ [$\hat{\sigma}$]	*Income elasticity*	*Agriculture coefficient*	$\bar{R}^2$ [$\hat{\sigma}$]
1930	1.27	0.88	1.27	−0.00	0.87
	(0.07)	[0.19]	(0.10)	(0.05)	[0.26]
1935	1.51	0.83	1.39	−0.06	0.83
	(0.10)	[0.24]	(0.16)	(0.07)	[0.24]
1940	1.39	0.80	1.27	−0.06	0.80
	(0.10)	[0.25]	(0.15)	(0.06)	[0.25]
1945	1.32	0.63	1.43	0.05	0.63
	(0.15)	[0.24]	(0.17)	(0.05)	[0.24]
1950	1.31	0.69	1.45	0.08	0.71
	(0.13)	[0.21]	(0.14)	(0.04)	[0.20]
1955	1.12	0.54	1.40	0.11	0.59
	(0.15)	[0.23]	(0.18)	(0.05)	[0.22]
1960	1.16	0.52	1.42	0.11	0.57
	(0.16)	[0.22]	(0.18)	(0.04)	[0.21]
1965	1.06	0.37	1.37	0.10	0.44
	(0.20)	[0.24]	(0.22)	(0.04)	[0.23]
1970	0.82	0.23	1.29	0.13	0.38
	(0.21)	[0.23]	(0.23)	(0.04)	[0.21]
1975	0.97	0.20	1.21	0.09	0.32
	(0.27)	[0.24]	(0.26)	(0.04)	[0.23]
1980	0.66	0.08	1.05	0.12	0.22
	(0.30)	[0.27]	(0.30)	(0.04)	[0.25]
1985	0.40	0.02	0.83	0.14	0.17
	(0.28)	[0.29]	(0.30)	(0.05)	[0.27]
1990	0.85	0.14	1.11	0.08	0.18
	(0.29)	[0.31]	(0.32)	(0.05)	[0.31]

Source: See appendix 2 for detailed source notes. The dependent variable is the log of nominal total deposits (MX2) per capita. Data are annual and by state. Standard errors for the explanatory variables are shown in parentheses below the point estimate. The standard errors for the regressions are shown in brackets below $\bar{R}^2$. A constant for each date is estimated in all regressions, but is not reported.

The second set of regressions in table 4 includes the agricultural share (AGRY) as an explanatory variable. All the point estimates for income elasticity with AGRY lie above one, except for five (1983–87). As shown in the addendum to table 4, the restricted coefficient is 1.30 (s.e. = 0.02)

Table 4. (***Addendum***)

	Period	*Income elasticity*	*Income elasticity w/ ag. share*
Income elasticity constrained over period:	1929–90	1.24	1.30
		(0.02)	(0.02)
		F = 2.52[a]	F = 0.63[a]
	1929–59	1.34	1.34
		(0.02)	(0.03)
	1960–90	0.87	1.21
		(0.04)	(0.05)
		F = 99.28[b]	F = 6.50[b]
Income elasticity constrained and data pooled over five-year intervals:	1930–90	1.24	1.36
		(0.04)	(0.06)
	1930–55	1.35	1.21
		(0.04)	(0.10)
	1960–90	0.89	1.31
		(0.09)	(0.05)
		F = 22.29[c]	F = 1.64[c]
Income elasticity constrained and state effects removed:[d]	1929–90	1.45	1.14
		(0.02)	(0.03)
	1947–90	1.14	1.10
		(0.04)	(0.04)
	1960–90	1.30	1.32
		(0.06)	(0.07)

a. The income elasticities are constrained to be the same over the periods shown. The F-test is based on the null hypothesis that the coefficients on income are the same across all 62 years. The 0.05 critical value with 61 degrees of freedom for the numerator and more than 1000 for the denominator is 1.32.

b. The F-test is based on the null hypothesis that the coefficients on income are the same in the two subperiods (the first subperiod includes 31 years and the second includes 30 years). The 0.05 critical value with 1 degree of freedom for the numerator and more than 1000 for the denominator is 3.84.

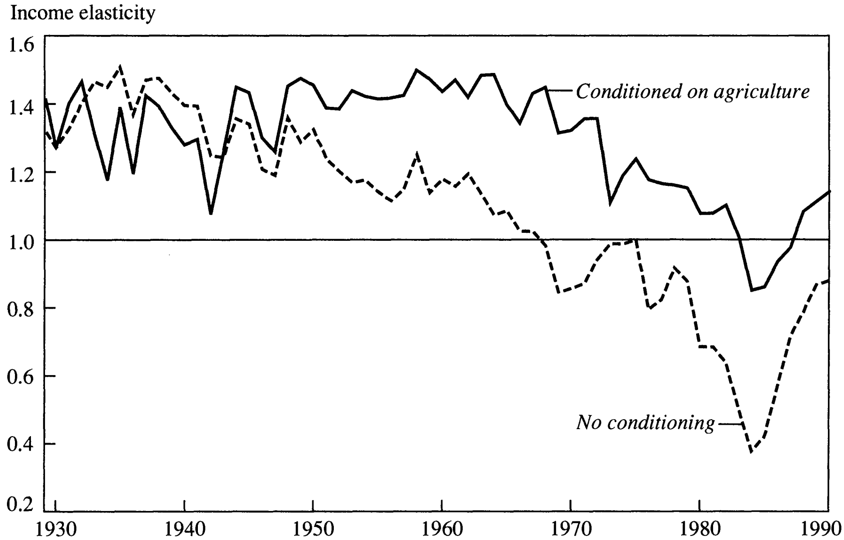
c. The F-test is based on the null hypothesis that the coefficients on income are the same for the two subperiods (the first sub-period includes seven years at five-year intervals and the second includes six years at five-year intervals). The 0.05 critical value for 1 degree of freedom in the numerator and more than 400 for the denominator is 3.86.

d. The rows next to the label "income elasticity constrained and state effects removed" report income elasticities and their standard errors when a constant is estimated for each state, as well as for each year, while a single income elasticity is estimated. As above, coefficients on agriculture's share are not restricted over time.

and the F-statistic is 0.63. Hence the hypothesis that the MX2 elasticity is 1.30 for the years 1929–90 cannot be rejected.

The pooled estimate of the income elasticity over the first 31 years is 1.34 (s.e. = 0.03) and the estimate over the second 31 years is 1.21 (s.e. = 0.05). An F-test of the equality of these two estimates is rejected

Figure 9. Cross-state Income Elasticity for Total Deposits (MX2), 1929–90



Source: See appendix 2 for detailed source notes. The figure plots the annual income elasticities using the regression results from table 4, where the dependent variable is the log of nominal total deposits per capita. The dashed line shows the income elasticities with no added conditioning variables. The solid line plots the income elasticities when the log of the share of income originating in the agricultural share is added as a regressor.

at the 5 percent level (the F-statistic is 6.50 and the 5 percent critical value is 3.84). When data at five-year intervals are used, the pooled estimates are 1.36 (s.e. = 0.06) for the entire sample; 1.21 (s.e. = 0.10) for the 1930–55 period; and 1.31 (s.e. = 0.05) for the 1960–90 period. An F-test of the equality of the elasticities across the two subperiods cannot be rejected at the 5 percent (or 1 percent) level. (The F-statistic is 1.64 and the 5 percent critical value is 3.86.)

As was the case for MX1, the income elasticity for our broad measure of money appears to be remarkably stable over the entire sample period of 1929–90.

The inclusion of state effects does not have a systematic effect on the estimated MX2 income elasticity. The final rows of the addendum to table 4, labeled "income elasticity constrained and state effects removed," display the estimated income elasticities and their standard errors when state effects are included in addition to time effects. In the full 1929–90 sample, the elasticity increases from 1.24 to 1.45 when state dummies are added to the pooled regression. However, the elasticity decreases

Table 5. Comparing Income and Consumption Elasticities for MX2

Year	*Income elasticity*	$\bar{R}^2$ [$\hat{\sigma}$]	*Consumption elasticity*	$\bar{R}^2$ [$\hat{\sigma}$]
1929	1.47	0.89	1.22	0.83
	(0.11)	[0.17]	(0.13)	[0.21]
1935	1.52	0.86	1.09	0.78
	(0.15)	[0.22]	(0.16)	[0.27]
1939	1.49	0.85	1.21	0.79
	(0.14)	[0.21]	(0.15)	[0.25]
1948	1.49	0.73	1.47	0.76
	(0.13)	[0.19]	(0.12)	[0.18]
1954	1.47	0.67	1.56	0.69
	(0.15)	[0.20]	(0.16)	[0.19]
1958	1.52	0.62	1.63	0.65
	(0.18)	[0.20]	(0.18)	[0.19]
1963	1.48	0.55	1.63	0.54
	(0.20)	[0.21]	(0.22)	[0.21]
1967	1.36	0.45	1.52	0.44
	(0.23)	[0.22]	(0.26)	[0.22]
1972	1.29	0.36	1.07	0.14
	(0.26)	[0.22]	(0.40)	[0.26]
1977	1.10	0.25	0.58	0.06
	(0.29)	[0.23]	(0.38)	[0.26]
1982	1.07	0.24	0.52	0.07
	(0.32)	[0.26]	(0.38)	[0.29]
1989	1.17	0.19	0.99	0.07
	(0.32)	[0.31]	(0.39)	[0.33]
Addendum				
Elasticities				
constrained	1.42	...	1.27	...
	(0.04)	...	(0.08)	...
	F = 0.53	...	F = 1.91	...

Source: See appendix 2 for detailed source notes. The dependent variable is the log of nominal total deposits (MX2) per capita. Data are annual and by state. Standard errors for the income and consumption elasticities are below the point estimates, while the standard errors for the regressions are shown in brackets below $\bar{R}^2$. A constant for each year is estimated, but not reported. The first set of regressions includes the log of personal income per capita as the only regressor. The second set of regressions uses retail sales as its regressor and as a proxy for consumption. All regressions in this table use agriculture shares, the log of population levels, and a constant as additional explanatory variables; however, these results are not reported. The F-test is based on the null hypothesis that the coefficients on income and consumption are the same across the twelve subperiods. It follows an F distribution. The 0.05 critical value with eleven degrees of freedom is 1.79. The 10 percent critical value is 1.58.

from 1.30 to 1.14 when the dummies are added to the second set of regressions. A similar pattern occurs in the postwar period. State effects increase the estimated elasticity in both sets of regressions for the 1960–90 period.

Table 5 replicates table 3 to incorporate consumption into the analysis of MX2. The first two columns of results report the individual cross-sectional regressions when personal income is used as a scale variable. The years used correspond to the years for which the consumption variable is available. Because we rejected the hypothesis of stability of the income coefficients in the univariate case (the first set of regressions in table 4), we estimated each regression with the agricultural shares and the population variables. All the point estimates of the elasticity of money with respect to income are larger than one. The restricted coefficient is 1.42 (s.e. = 0.04). The hypothesis that the elasticities are stable over time cannot be rejected at the 5 percent level.

The second set of regressions in table 5 repeats the exercise with consumption as the relevant scale variable. The conditioning variables and the time periods are otherwise identical to those in the first set of regressions. The point estimates for the consumption elasticity are significantly larger than one for every year until 1977. The point estimate drops below one in 1977 and remains below one throughout the 1980s. The restricted estimate is 1.27 (s.e. = 0.08), but the F-statistic is 1.91, slightly above the 5 percent critical value of 1.79. Hence the stability of the consumption elasticity for MX2 (total deposits) is rejected.

When both consumption and income are introduced in the same panel set, the restricted point estimate for income is 1.37 (s.e. = 0.16) and the one for consumption is −0.03 (s.e. = 0.17). Hence income, not consumption, fits the data better as the scale variable in the MX2 demand equation.

Policy Conclusions and Directions for Future Research

We found four main empirical results. First, the income elasticity of both MX1 and MX2 has been surprisingly stable for an impressive period that includes the Great Depression, World War II, the oil shocks of the 1970s, and the Reagan-Volcker years. Second, the estimates of elasticity for the entire period are substantially higher than unity (between 1.3 and 1.4) for both measures of money. Given the small size of the standard errors, these elasticities are significantly larger than one. Third, insofar as we can determine, the relevant scale variable is income, not consumption. Our estimated consumption elasticities, however, do not differ greatly from those for personal income. Finally, the inclusion of state effects in addition to time effects did not change our finding of an income elasticity that is larger than one. Thus we are skeptical that geographical differences in the level of the money demand function, which could arise because some states have peculiar banking laws or because some states specialize in banking, are responsible for our reported high income elasticities.

Our finding of such a high income elasticity is not new. In 1959, Milton Friedman argued that U.S. secular trends in real balances and income during the 1870–1954 period suggested an income elasticity of 1.8.[54] In his discussion of his somewhat unconventional estimate, Friedman noted that an elasticity estimated using higher frequency data (which tend to yield estimates close to one) would suffer from two biases. First, to the extent that money demand depends on permanent income, income elasticities are biased downward. Our results suggest that the quantitative importance of this bias is likely to be minimal because "income elasticities" and "consumption elasticities" are quite similar. Even if the bias is important, our conclusion that the income elasticity is greater than one is only strengthened.

Second, to the extent that money balances absorb transitory income fluctuations, elasticities estimated with high-frequency data will be biased upward. Friedman offered the conjecture that such effects would be important only at very high frequencies, not at annual frequencies.[55] Our results provide two more pieces of supporting evidence. First, demand deposits may have been an important "shock absorber" 50 years ago, but by the 1980s, technological advances should have motivated people to absorb shocks with other assets, such as savings accounts or money market funds. If the shock absorber bias were ever important, technological advances should have the effect of reducing the bias and introducing a downward trend in the income elasticity. However, we

54. Friedman (1959). It is interesting that Friedman (1959, p. 208) and Friedman and Schwartz (1963, p. 639) cite Feige's cross-state regressions as evidence in favor of a high income elasticity (greater than 1.0). They also mention that studies for other countries have also found high income elasticities. Tobin (1965) criticized the 1.8 estimate on the grounds that it is driven by a downward trend in velocity during the 1867–1903 period, which resulted from changes in U.S. financial structure. Friedman and Schwartz (1982, p. 243) remove that trend and obtain an income elasticity of 1.2. However, because the revised estimate was not based on any data on "financial structure," the revision may have been too severe.

55. Friedman (1959) also noticed that a "shock absorber" explanation is difficult to reconcile with the business-cycle behavior of income and real balances.

found the income elasticity remarkably stable over the 1929–90 period. If anything, after taking account of state effects, it appears higher in the postwar period. Second, the tendency to absorb transitory income shocks should be even greater for broader definitions of money; the upward shock absorber bias should be stronger for MX2 than for MX1. However, we estimated very similar income elasticities for MX1 and MX2. If anything, the MX2 elasticity was lower! Thus we do not think that our high income elasticities are a statistical artifact.

Policy Implications

Our results suggest a number of implications for rules for conducting economic policy. First, some economists, such as Benjamin Friedman and Kenneth Kuttner, have cited instability of time-series equations in order to argue the "money demand" is not a structural relationship that can be relied upon by the monetary authorities.[56] They use these findings to argue against targeting of monetary aggregates. While our results do not address the overall stability of the demand for money, they do suggest that the unstable income elasticities reported by Friedman and Kuttner may well be statistical artifacts arising from the use of time-series data and the omission of measures of the financial technology.

Second, our results have implications for those who would pursue money growth rules. For example, Milton Friedman's constant money growth rule would not achieve price stability if based on the unit income elasticity most economists believe to be true. If per capita output growth proceeds at 2 percent a year, a 0.5 underestimate of the income elasticity would result in 1 percent a year deflation instead of the intended price stability—an outcome that could have undesirable political and economic consequences.

Third, our finding that income and not consumption is the relevant scale variable may have implications for Keynesian fiscal policy analysis. Mankiw and Summers argue that if consumption is the relevant scale variable, then a tax hike could have expansionary effects if the consumption elasticity of money demand were large enough.[57]

If we compared our large estimates of the consumption elasticity of money demand with the rest of the parameters of the IS-LM model used

56. Friedman and Kuttner (1992).
57. Mankiw and Summers (1986).

by Mankiw and Summers, we would conclude that, in fact, tax increases are expansionary. The problem is that our results also suggest that income—not consumption—is the relevant scale variable. In the framework used by Mankiw and Summers, this implies that tax increases are unambiguously contractionary. The quantitative effects of such tax increases will also be altered by our empirical findings: high elasticity of money demand suggests that the LM curve is steeper than previously thought. This means that—in a Keynesian world—fiscal policy is less potent.

A Research Challenge

Our high estimates of the income elasticity of money demand pose a challenge for economists. Milton Friedman insisted that an income elasticity greater than unity was difficult to reconcile with transactions theories of money demand. He proclaimed that "it is dubious that there has been any secular increase in the ratio of transactions to income."[58] If Friedman is correct, then we need a theory of money demand that predicts that real balances are highly sensitive to the volume of transactions.[59]

We conclude with two conjectures for explaining the high elasticities we have estimated. Our first explanation depends on demographic changes at the household level.[60] At the family level, economies of scale exist in the use of money: larger families tend to use less money per person than smaller ones. Hence, if children are an inferior good while divorce is a luxury good (as seems to be the case), higher income is associated with smaller families and larger demand for money. It follows that the income elasticity is larger than one.

Second, the process of economic development is associated with a larger number of vertically disintegrated firms (using more complicated technologies with more varieties of inputs and interacting with a larger number of suppliers). To the extent that firms need money to transact with other firms, but not for internal transactions, a higher level of income will be associated with a more than proportionally higher level of money demand.

58. Friedman (1959, p. 136).
59. Friedman's story was that monetary services are a luxury good.
60. See Becker (1991) for discussions of demographics, economics, and the family.

APPENDIX 1

Bias Introduced by Differences in Regional Interest Rates

BARRO, MANKIW, AND SALA-I-MARTIN (1992) argue that perfect capital mobility is consistent with regional differences in income if some assets cannot be used as collateral. In particular, they identify human capital as a possible noncollateralizable asset.

Econometric theory allows an upper bound on the bias to be computed. First, suppose that a state's income, y_i, is a Cobb-Douglas function of its capital stock, k_i, and a productivity parameter, A_i:

$$y_i = A_i k_i^{\alpha}.$$

Interpret capital k_i broadly to include not only physical capital, but human capital. Using standard analysis of omitted variable bias, the formula for the bias is $\delta(1-\alpha)/\alpha$ when the productivity parameter A is cross-sectionally uncorrelated with income. If productivities and incomes are positively correlated, $\delta(1-\alpha)/\alpha$ is an upper bound. Using a cross section of the 48 states, Barro and Sala-i-Martin (1991) argue that in order to explain the slow speed of convergence across states, the capital share cannot be smaller than 0.8. (Of course, this would include human capital and other kinds of inputs that can be purposefully accumulated.) If open-economy considerations are taken into account, the capital share needs to be closer to 0.9; see Barro, Mankiw, and Sala-i-Martin (1992).

If an interest rate elasticity of $\delta = 0.5$ and a capital share of $\alpha = 0.8$ is chosen, the implied bias is 0.125. Lower interest rate elasticities or higher capital shares reduce the bias. (A higher capital share allows for less cross-sectional variation of interest rates.) Finally, an offsetting effect that tends to reduce the bias includes the productivity level, A_i.

If, following Barro, Mankiw, and Sala-i-Martin, we think that this is why we should not assume a constant interest rate across states, then we should keep in mind that the underlying theory of money demand may be along the following lines: people go to the bank and exchange human capital (which, admittedly, is not a very liquid asset) for money so they can purchase other goods.

APPENDIX 2

Detailed Source Notes for Figures and Tables

Figures 1 and 2. Authors' calculations based on U.S. Department of Commerce (1975, pp. 1002–03); *Economic Report of the President 1991*, pp. 302 and 373; *Economic Report of the President 1981*, p. 236; Friedman and Schwartz (1963, pp. 712–22); Bureau of Economic Analysis (1986); *Survey of Current Business* (various issues); FDIC, *Bank Operating Statistics* (various issues); FDIC, *Assets, Liabilities and Capital Accounts of Commercial and Mutual Savings Banks* (various issues); FDIC, *Banks and Branches Data Book* (various issues); FDIC, *Data Book, Operating Banks and Branches* (various issues); FDIC, *Statistics on Banking* (various issues); Board of Governors of the Federal Reserve System (1959).

Figures 3–7 and 9. Authors' calculations based on U.S. Department of Commerce (1975, pp. 1002–03); Friedman and Schwartz (1963, pp. 712–22); Bureau of Economic Analysis (1986); *Survey of Current Business* (various issues); FDIC, *Bank Operating Statistics* (various issues); FDIC, *Assets, Liabilities and Capital Accounts of Commercial and Mutual Savings Banks* (various issues); FDIC, *Banks and Branches Data Book* (various issues); FDIC, *Data Book, Operating Banks and Branches* (various issues); FDIC, *Statistics on Banking* (various issues); Board of Governors of the Federal Reserve System (1959).

Figure 8. Same as figures 3–7 and 9, but also including *Statistical Abstract of the United States 1990*, p. 195; U.S. Bureau of the Census, *Current Population Reports* (various issues).

Table 1. Same as figures 1 and 2, but also including p. 378 of the *Economic Report of the President, 1991*.

Tables 2 and 4. Authors' calculations based on U.S. Department of Commerce (1975, pp. 1002–03); Friedman and Schwartz (1963, pp. 712–22); U.S. Bureau of Economic Analysis (1986); *Survey of Current Business* (various issues); FDIC, *Bank Operating Statistics* (various issues); FDIC, *Assets, Liabilities and Capital Accounts of Commercial and Mutual Savings Banks* (various issues); FDIC, *Data Book, Operating Banks and Branches* (various issues); FDIC, *Banks and Branches Data Book* (various issues); FDIC, *Statistics on Banking* (various issues); Board of Governors of the Federal Reserve System (1959).

Tables 3 and 5. Same as tables 2 and 4, but also including *Statistical Abstract of the United States* (various issues).

Comments and Discussion

N. Gregory Mankiw: Casey Mulligan and Xavier Sala-i-Martin have written an intriguing report. Examining an age-old question with a novel data set, they reach a surprising conclusion: the income elasticity of money demand is greater than one. In my comments, I will address three questions. First, assuming that their conclusion is correct, what are the implications for policy? Second, assuming that their conclusion is correct, can it be reconciled with standard theories of money demand? Third, is their conclusion correct?

IMPLICATIONS FOR POLICY. When we are told that the income elasticity of money demand is greater than one, should we care? Should it change our view about the conduct of monetary or fiscal policy?

For most practical purposes, the answer is no: the income elasticity of money demand is not a pressing issue for macroeconomic policy. Of course, as all good undergraduates know, the income elasticity of money demand does affect the slope of the LM curve, which in turn affects the impact of fiscal policy, holding the money supply constant. Yet this exercise has limited practical significance. We ask our students about such hypothetical policy experiments so they can develop facility with the models we teach. But in the world, in contrast to the textbooks, the money supply is almost never held constant.

To put the point bluntly, I doubt that Federal Reserve Chairman Alan Greenspan loses much sleep over the parameters of the money-demand function. The Federal Reserve can conduct a reasonably good monetary policy without thinking much about what determines the demand for money. For example, it can use the interest rate or the monetary base as the short-term instrument and nominal GDP as the medium-term target. By adjusting the instrument as the economy deviates from the target, the

Federal Reserve can avoid major recessions or inflations, without knowing the deep parameters governing money demand.

Although real-world policymakers do not care much about parameters such as the income elasticity of money demand, these parameters are of some interest to academics. We use these estimates to calibrate models in order to consider alternative, hypothetical rules for monetary policy. And we use them to evaluate our theories of money demand.

RECONCILIATION WITH THEORY. Probably the best theory of money demand we have is the Baumol-Tobin model. This model implies an elasticity of money demand with respect to expenditure of one-half, holding other variables constant. The findings in Mulligan's and Sala-i-Martin's report might be interpreted as decisive rejections of the Baumol-Tobin model.

Yet the implications of the Baumol-Tobin model are more flexible than is often suggested. In the world, one variable that is not constant over time or across states is the fixed cost of making a trip to the bank. Rather than being a fixed dollar cost, as is usually assumed, it is plausibly a fixed time cost. That is, a trip to the bank may require a certain amount of time, so the dollar cost of a trip depends on the wage, which in the long run is roughly proportional to income and expenditure. In this case, the Baumol-Tobin model implies an income elasticity of one, rather than one-half.

It is even possible to modify this argument to reconcile the Baumol-Tobin model with the findings in this report. If the labor-supply curve is backward-bending—as it may be in the long run—then income and expenditure move less than proportionately with the wage. Conversely, the wage, and thus the cost of a trip to the bank, move more than proportionately with income and expenditure. In this case, the Baumol-Tobin model yields an income elasticity of money demand greater than one.

One can also raise the income elasticity in the Baumol-Tobin model by incorporating capital income taxation. Under a progressive income tax, higher income leads to a higher marginal tax rate, which in turn leads to a lower after-tax interest rate. Since money demand is interest-elastic, the result is an increase in the effective income elasticity of money demand.

Finally, a larger income elasticity could arise because not all households face the same before-tax interest rate. Low-income households are less likely to hold Treasury bills and are more likely to be indebted to credit-card companies or the local loan shark. Thus the return on the alternative asset to demand deposits might vary across poor and rich households. Because these differences are not measured, they may be reflected in a larger estimated income elasticity.

The bottom line is that the Baumol-Tobin model can yield a larger income elasticity than is generally supposed. From the standpoint of theory, the results presented in this report are not as surprising as they first seem.

OTHER EVIDENCE. Let me now turn to the central issue: is the income elasticity of money demand in fact larger than one, as this paper argues?

Most of the past empirical work on money demand has used aggregate time-series data. It is easy to be skeptical of this work, however, for much of it does not take the identification problem seriously. To the extent that the Federal Reserve has ever targeted the money supply, shifts in money demand are correlated with income. For example, positive residuals in the money-demand function, such as those in the early 1980s, lead to contractionary shifts in the LM curve and thus reductions in income. The induced correlation between the residual and income tends to bias estimates of the income elasticity.

One of the best attempts to address this identification problem is in a paper by Miquel Faig published several years ago.[1] Faig uses the identifying assumption that the money-demand function does not shift over the seasons. Thus the seasonal fluctuations in income can be used to identify the income elasticity. Faig finds that money balances fluctuate much less than income over the seasonal cycle, implying an income elasticity much smaller than one.

Faig also finds that consumption is a better scale variable than income in the money-demand function. In some countries, such as Germany, consumption and income have quite different seasonal patterns. Faig shows that the seasonal pattern of money balances more closely matches that of consumption.

Mulligan and Sala-i-Martin, like Faig, use a plausible assumption to solve the identification problem. They assume that cross-state differences in income are not correlated with state-specific shifts in the money-demand function. Yet they reach the opposite conclusion: they find an income elasticity significantly greater than one.

1. Faig (1989).

Table 1. Cross-sectional Money Demand Estimates

Specification and result	*Regression 1*	*Regression 2*	*Regression 3*	*Regression 4*
Estimation	OLS	IV	IV	OLS
Instruments	. . .	Education	State dummies	. . .
Number of observations	2,865	2,865	2,865	2,762
Constant	0.52	−0.52	3.31	0.61
	(0.32)	(0.83)	(1.26)	(0.30)
Log(income)	0.58	0.68	0.30	0.19
	(0.03)	(0.08)	(0.13)	(0.03)
Log(wealth)	. . .	. . .	. . .	0.36
				(0.02)

Source: Estimates are based on the 1983 *Survey of Consumer Finances* conducted by the Survey Research Center of the University of Michigan. The variables used are B3401 for demand deposits, B3201 for income, B3324 for wealth, B4505 for education, and B3121 for state. Standard errors are shown in parentheses. The dependent variable is the log of demand deposits. OLS stands for ordinary least squares. IV stands for instrumental variables.

So who is right? After my first reading of this report, I thought that it might be possible to reconcile these disparate pieces of evidence. I conjectured that wealth was the key missing variable. Suppose, for example, that money demand is positively related to both income and wealth, and that the sum of the elasticities equals one. Consider what happens if we incorrectly leave out wealth and estimate only an income elasticity. Clearly, the estimated income elasticity is biased upward because of the positive correlation between income and wealth. But the extent of the bias depends on the data set. Over the business cycle or the seasonal cycle, wealth moves less than proportionately with income, so the estimated income elasticity would be less than one. Yet, because wealth is more concentrated than income, wealth could move more than proportionately with income in cross-state data. This could explain an estimated income elasticity larger than one.

To address this possibility, I turned to yet another data set, the 1983 *Survey of Consumer Finances*.[2] My goal in looking at cross-sectional household data was to estimate separate wealth and income elasticities. What I found was very different from what I had expected.

I began by trying to confirm the finding in this paper. In regression 1 in table 1, I report a regression of the log of the total checking account balance on the log of total income. To my surprise, I found an income elasticity only slightly larger than one-half. Moreover, because of the

2. University of Michigan (1983).

large number of observations, the standard error is small, so an elasticity of one or higher is decisively rejected.

At first, I thought the problem was measurement error. Perhaps my income data are contaminated with so much error that my coefficient is substantially biased toward zero. So in regression 2, I instrumented income with years of schooling. The estimated coefficient does rise, as the measurement-error hypothesis predicts, but the estimate of 0.68 is still significantly below one.

Next, I thought that perhaps there was something special about grouping people into states. So in regression 3, I used state dummies as instruments. This regression should, I thought, be close to those reported by Mulligan and Sala-i-Martin. But again, the estimated income elasticity is significantly below one.

Why are these results from household data so different from the authors' results from aggregate state data? I do not know for sure. One possibility is that the discrepancy comes from the treatment of out-of-state bank accounts. If I hold a checking account in a New York bank, my money is considered New York money in the authors' data, even though I live, earn my living, and spend my money in Massachusetts. If out-of-state banking were random, then it would merely add noise to the left-hand-side variable, without biasing the estimates. Yet if out-of-state bank accounts tend to be in high-income states, then money will appear to more closely associated with income than it really is.

The magnitude of this problem is hard to judge. The problem might be severe for business holdings of demand deposits. Business banking might be concentrated near corporate headquarters, which tend to be in large cities, which tend to be in high-income states. If so, this would bias upward the estimated income elasticity in the authors' aggregate state data.

For completeness, I report in the last column of table 1 the regression that originally drew me to these data. Here we find that money holdings are significantly related to both income and wealth. One interpretation of this regression is that both income and wealth are proxies for permanent income or consumption.

To sum up, I am not yet ready to accept the conclusion that the income elasticity of money demand is larger than one. I would first like to see all the conflicting evidence resolved. Fortunately, the Federal Reserve need not wait for the resolution. The size of the income elasticity of money demand is, literally, an academic question.

Julio J. Rotemberg: The estimation of the empirical relationship between the amount of liquidity that people want to hold and its determinants using time-series data is subject to a serious endogeneity problem. If the money supply is held constant and money demand rises, standard models imply that output should fall and interest rates should increase. Thus a correlation exists between the residual in the money-demand equation and the two right-hand-side variables. This problem could be held in abeyance if the money-demand relation were miraculously exempt from the instability that plagues most stochastic relations among aggregate variables. Of course, it is not.

This instability is to be expected. Suppose that, as many models predict, increasing money demand while holding the money stock constant raises interest rates and lowers output. It then seems very hard to imagine that the stochastic relation among these three variables would be constant. The reason is that Federal Reserve operating procedures vary over time. Changes in Federal Reserve operating procedures affect the degree to which an increase in money demand is matched by either an increase in money or an increase in interest rates. Therefore, changes in these procedures affect the degree to which the residual in traditional money-demand equations is correlated with the right-hand-side variables.

The resulting lack of believable estimates of the parameters of money demand is a terrible loss for macroeconomics. It means that we have no credible empirical model in which nominal magnitudes matter. Thus I want to applaud Casey Mulligan and Xavier Sala-i-Martin for making an attempt to uncover money-demand parameters from some other source.

I also think that, more specifically, looking at the cross-sectional relation between money and income makes sense. Cross-sectional evidence cannot be used to estimate the effect of exogenous changes in interest rates on money demand; however, the relationship can be observed with income. The big advantage of this relationship is that, cross-sectionally, there is no a priori reason to expect any correlation between income and the residual in the money-demand equation. By contrast, in the aggregate, we expect money-demand increases that are not fully accommodated by increases in the money stock to lower income. This negative correlation between the residual and income might well bias the estimated income elasticities downward. From this perspective, one would expect the correct income elasticity to be larger than the one obtained in typical time-series studies—and this paper does indeed estimate larger elasticities.

The authors estimate this elasticity to be around 1.3. The extent to which this is significantly higher than one is a bit oversold. It is significantly higher than one for very few individual years. Pooling all the years is a bit problematic because it is not clear that one is getting independent observations. In other words, serial correlation exists in the state-specific error term. Nonetheless, the fact that this coefficient is bigger than one is interesting and challenging to the conventional wisdom. In fact, I regard the coefficient as so high as to be implausible.

Money is rate-of-return-dominated and no safer than interest-paying government obligations, so that it is probably held only for the transactions services it provides. It seems unlikely that the volume of transactions rises more than proportionately to income. Nor does it seem likely that more than twice the money is needed to carry out two times as many transactions. Rather, the reverse is almost certainly true. One reason, stressed by William Baumol and James Tobin, is that one can avoid holding twice the money balances by carrying out financial transactions more often. Similarly, on large transactions, it is beneficial to find ways of carrying out the transaction that may have larger fixed costs, but which avoid the need to hold money. The costs of holding money are proportional to the amount of money held, whereas the costs of financial transactions are not as sensitive to the size of the transaction. So, as more funds are involved, incurring fixed costs of carrying out more and more complex transactions becomes more attractive. Along the same lines—and this is of particular relevance cross-sectionally—individuals and firms that carry out many transactions find it advantageous to pay fixed costs to hold credit cards and special types of accounts that allow them to hold fewer demand deposits.

Mulligan's and Sala-i-Martin's estimates are implausible as estimates of money demand elasticities, which prompts the question of what they might be estimating. My instinct is that, to a large degree, the estimates reflect the elasticity of the supply of banking services with respect to the income of the state. To see what I have in mind, start with a parable. Consider a town surrounded by a fertile agricultural region. Income per capita will probably be measured as being higher in town. In part, this is because the agricultural produce that is consumed by producers is not counted as income. But mainly the difference in per capita income reflects the fact that skilled professionals live in town. Also, it seems reasonable to expect all the banking and all the deposits to take place in town. Why this is so is an interesting question. It must mean that there are some sort of increasing returns to banking or that there are externalities from other factors that are located in town. Whatever the case, if these two conditions are met, then a regression of per capita deposits on per capita income will have a huge coefficient.

The problem is that banks are agglomerated in cities. The authors would probably reply that they are looking at a coarser level of aggregation and that each state has at least one bank. This does not fully resolve the problem because banks provide many different services; certain customers require services that can be provided only by large banks. That is why we talk of financial centers. The authors were clearly aware of this problem because they did two things. First, they redid their regressions, taking out New York. This does indeed lower their estimated coefficient—but not by enough to make it plausible. Second, the authors ran regressions controlling for population density in the state. They report that this does not change the results. The problem is that population density is a very crude measure of the degree to which a state is a financial center for its surrounding area. Several poor southeastern states, such as Kentucky (with 94 persons per square mile in 1990) and West Virginia (with 74) are denser than Texas (with 64); nonetheless, Texas has far more important cities and financial centers.

Using the data that the authors have kindly supplied, I have also considered two exercises along these lines. The first is to rerun their regressions, but only for those states that, in 1991, did not include cities whose size equals or exceeds that of Boston, a financial center that I know well. In particular, I have used the 33 states that do not contain a city whose population is larger than Boston. (Texas has three such cities.)[1] The coefficient falls substantially. Using M to denote the log of per capital money and Y to denote the log of per capita income, the regression for the 1990 data is

$$\underset{(0.78)}{M = 4.13} + \underset{(0.277)}{1.038}\, Y.$$

The other thing I have done is to add an additional explanatory variable: the fraction of the state's population that lives in a "metropolitan area," as defined by the Census Bureau. This fraction varies between

1. Data on city population and state densities come from the *World Almanac* (1992).

100 percent for New Jersey and 22 percent for Vermont.[2] This has an even more dramatic effect on the results. Using 1990 data for money and income and letting $U86$ be the 1986 data for urbanization yields

$$\underset{(0.78)}{M = 4.65} + \underset{(0.286)}{0.739\ Y} + \underset{(0.002)}{0.006\ U86}.$$

One interesting aspect of these regressions is that my measure of urbanization and income are not very highly correlated; the standard error on income does not rise very much when urbanization is included. Both variables are statistically significant, although the urbanization variable is more strongly correlated with money than the income variable.

These results are not confined to 1990. I have rerun these equations with the authors' 1970 data, including their agricultural share variable. (Without this variable, the income coefficient is only 0.9.) This yields

$$\underset{(0.26)}{M = 5.44} + \underset{(0.236)}{1.122\ Y} + \underset{(0.04)}{0.009\ AGR}.$$

Excluding those states that include at least one major city in 1991, this regression becomes

$$\underset{(0.24)}{M = 5.68} + \underset{(0.216)}{1.014\ Y} + \underset{(0.03)}{0.12\ AGR}.$$

Using the data for all states, but adding the urbanization variable yields

$$\underset{(0.33)}{M = 5.54} + \underset{(0.259)}{1.035\ Y} + \underset{(0.002)}{0.003\ U72} + \underset{(0.04)}{0.12\ AGR}.$$

So, once again, the exclusion of urban states or the inclusion of a crude measure of urbanization reduces the coefficient on income.

I am not claiming that either of these regressions adequately controls for the supply effect of financial agglomeration on deposits. In fact, neither variable makes Delaware an important financial center; Wilmington is a small city and only 66 percent of Delaware's population is in a metropolitan area. On the other hand, Delaware is a place where deposits are large. In both 1940 and 1990, its level of deposits per capita was second only to that of New York. Perhaps Delaware has such an unusually high level of deposits because so many companies are incorporated in this

2. Data on percent of the state's population in a metropolitan area comes from the *Statistical Abstract of the United States* (1988, p. 27).

state. However, this is a source of agglomeration for which my variables do not control.

One piece of evidence that deposits are held by out-of-state residents (and thus correspond to out-of-state income) comes from firms' financial statements. The Standard and Poor's *Register of Corporations, Directors, and Executives* lists the main bank of many companies. According to this publication, Ben and Jerry's, a fairly large ice-cream company located in Vermont, used Marine Midland Bank of New York in 1991. Demonstrating that I, too, live in a financial center, the *Register* shows that First Mutual of Boston was the main bank of Tom's of Maine, which makes new-age toiletries and is located in Kennebunk. Out-of-state banking is not the exclusive province of small and new companies. The Boise Cascade corporation, which is a Fortune 500 company and is located in Boise, Idaho, uses Bank of America of California as its main bank. Thus the income earned by this company's employees, which is classified as Idaho income, corresponds to banking that takes place in California. It is thus not surprising that deposits are a larger fraction of California's income than they are of Idaho's income.

In conclusion, I find the authors' basic result interesting. However, I would emphasize its relationship with the supply of financial services, instead of emphasizing its connection with the demand for money.

General Discussion

Several participants suggested that although cross-sectional data avoid some of the simultaneity problems present in time-series analysis, cross-sectional analysis has its own difficulties. Christopher Sims noted, as an example, that business activity and hence demand for money may vary with income across states in a different way than it varies with income over time. Hence the cross-sectional elasticities may carry very little information about the relationship between income and the demand for money over time. He also argued that an identification problem arises because of the differences in money supply schedules across states. Historically, bank regulations have differed substantially across states, leading to significant differences in regional interest rates. Moreover, these differences have changed over time.

Sims also believed that the evidence that the income elasticity of money demand is significantly greater than 1.0 is weaker than suggested in the paper. Even in the authors' preferred equation, the coefficient is not significantly greater than 1.0 in 10 of the 13 cross-sectional equations reported. Furthermore, he argued that the standard errors in the pooled regressions are understated. The theory used to conclude that the large standard errors for individual cross sections become much smaller in pooled regressions assumes no correlation of error terms across years. This implies that the estimated elasticities for cross sections are independent across years. The figures show that this is very far from being true.

Robert Hall questioned the validity of focusing on demand deposits alone, noting that many alternative ways of holding liquid assets exist and that innovations in the financial sector have increased the number and quality of those alternatives over time. William Brainard agreed, observing that the relationship between income and transactions has changed dramatically over time. As an example, he cited the fact that the ratio of bank debits to GDP has tripled since 1975. While an increased volume of financial transactions, at a given level of income, may have increased the demand for money, innovations such as the use of sweep accounts by large firms that maintain zero checking balances at the end of each day have reduced the demand. Hall suggested making further use of available data by distinguishing between personal and corporate deposits. He pointed out that while most people hold very little cash, the national average of currency per capita is $2000. This implies that relatively few people hold large idle cash balances. The same may be true with demand deposit accounts. Hence Hall argued that the finding of the paper is actually that those who happen to hold large balances live disproportionately in states with high per capita incomes.

William Nordhaus questioned whether the inclusion of farm income provided a suitable representation of the importance of financial sophistication. He suggested that it would have been desirable to include variables that might serve as proxies for the volume of financial market transactions or that measure the volume of sales, rather than value added.

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