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Bank Pricing of Retail Deposit Accounts and "The California Rate Mystery"

Jonathan A. Neuberger and Gary C. Zimmerman

Economists, Federal Reserve Bank of San Francisco.

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In this paper, we examine apparent interest rate discrepancies on retail deposit accounts between banks in California and those in the rest of the country. Some have suggested that California banks pay below-market rates on their deposits. We investigate these claims for both transaction accounts and certificates of deposit. We find that the discrepancies are primarily limited to transaction-based accounts. Using a microeconomic model of deposit interest rate setting, we show that the interest rate discrepancies can be partially explained by the unique characteristics of California bank markets and by different responses of California banks to interest rate determinants. However, a substantial portion of the interest rate differentials for transaction accounts persists even after accounting for these effects.

On several occasions during the past few years, the financial press and a number of consumer groups in California have suggested that there is a "deposit rate mystery" in the state, noting that California banks have been paying lower interest rates on deposit accounts and charging higher rates on loans than banks in other parts of the country. These groups have suggested that California banks have sufficient market power to pursue anticompetitive pricing policies. If such claims are true, then consumer welfare could be enhanced by policies that encourage greater competition and reduce market power in the California market for bank services.

Although these claims have provoked a heated debate on the nature of banking in California, they have not produced much rigorous analysis of the issue. Rigorous analysis is needed to establish first, whether statistically significant interest rate differentials between California and the rest of the country do, in fact, exist and second, what factors account for the differences in interest rates.

In this paper, we conduct such an analysis. In Section I we examine interest rates paid by banks in California and the rest of the U.S. and find that differentials do exist for at least some types of retail deposit accounts. We then consider the price-setting behavior of banks in Section II to determine why such differentials persist. Broadly speaking, there are two possible explanations. First, interest rate disparities may arise because the characteristics of bank markets in California differ from those in the rest of the country. Alternatively, California banks may respond to the determinants of deposit rates differently than their counterparts do elsewhere. In the final sections of this paper, we conduct an empirical analysis of bank price-setting behavior using explanatory factors suggested by economic theory. We employ a pooled time-series, cross-section data base of over 400 banks, including 29 California banking institutions, to estimate interest rate equations for four types of bank deposit accounts. Our analysis indicates that both explanations of the origins of the interest rate differentials are valid, and each helps to explain at least a portion of the observed interest rate discrepancies.

I. Is There a Rate Mystery?

In this section we look for evidence of a California deposit rate mystery. We examine interest rate differentials on four of the most popular retail deposit accounts in the U.S. As of December 30, 1987, these accounts had a combined total of $782.3 billion in deposits, comprising approximately 39 percent of total bank deposits nationally.

We consider two accounts with transaction features and two categories of small-denomination time certificates of deposit (CDs). The transaction accounts are Negotiable Order of Withdrawal (NOW) accounts (an interest-bearing, unlimited transaction checking account with $174.8 billion in deposits) and money market deposit accounts (MMDAs), a limited-checking transaction account with $353.8 billion in deposits.

The two retail CD accounts are both small denomination (less than $100,000). The first account includes CDs issued with three- to six-month original maturities. These accounts had $132.4 billion in deposits nationally as of December 30, 1987. The other account is a long-term CD, including deposits with original maturities of 2½ years or more and had $121.3 billion in deposits. These were the two most popular of the six retail time certificate maturity categories reported during the 1984-1987 period.

Charts 1 and 2 show the differences between average deposit interest rates paid by a sample of 435 banks nationwide during the 1984-1987 period and average rates paid on comparable accounts by the 29 California banks in the sample. The data are taken from Federal Reserve Board surveys of interest rates paid by banks on retail deposits.[1] These surveys provide the most common rate paid on retail accounts for each bank. Most common interest rates are adjusted for differences in compounding and then converted to basis points. As Chart 1 indicates, we observe a substantial differential between bank rates in the U.S. and those in California for both MMDAs and NOWs. The positive numbers graphed in the chart indicate that, on average, interest rates on both NOWs and MMDAs were lower in California than elsewhere. Over the two-year period ending in December 1987, the NOW differential averaged 37 basis points. The average differential for MMDAs measured 28 basis points over the 1984-87 period. Both differences are statistically significant at the five percent level.[2] At no time during this period did average rates on these deposits in California exceed the national average. Moreover, the rates in major California markets were below the average for several other major markets, including New York, Chicago, Philadelphia, and Boston. This direct interest rate comparison confirms that the California rate mystery has indeed existed for both NOW accounts and MMDAs.[3]

Chart 1
MMDA and NOW Accounts
Rate Differentials: U.S - California

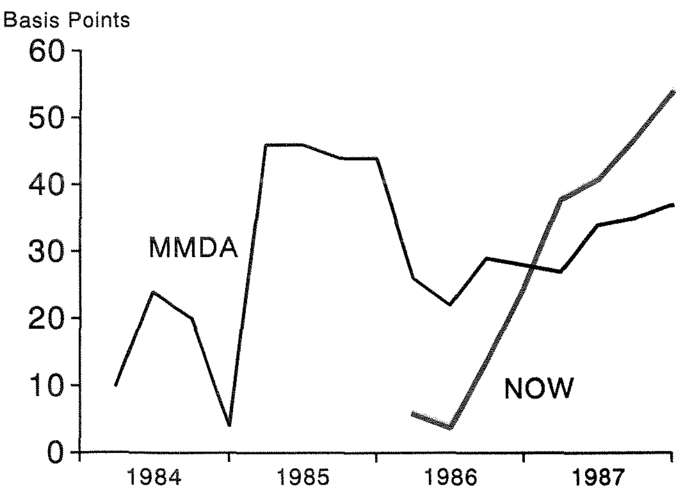
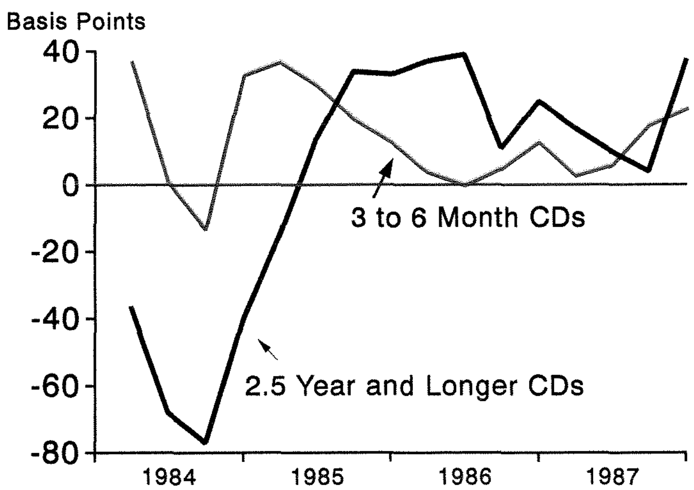


Chart 2
3 to 6 Month and 2.5 Year and Longer CDs
Rate Differentials: U.S. - California



The evidence for the two retail CD accounts, presented in Chart 2, is less dramatic, with considerably smaller interest rate differentials. The three- to six-month CDs averaged 20 basis points lower in California over the 1984-1987 period, while the 2½ year and over CDs averaged only 11 basis points less in California. During the early part of the sample period, rates on the long-term CDs in California regularly exceeded the U.S. average. In both instances, the observed differentials are not statistically significant at the five percent level. There is thus less clear-cut evidence that a persistent rate differential has existed for the two CD accounts.

II. Determinants of Retail Deposit Interest Rates

Our comparison of retail deposit pricing in the U.S. and California indicates that banks in California have priced some, but perhaps not all, of their retail deposits differently than banks in other states. To determine whether these disparities arise because the characteristics of bank markets in California are different from those elsewhere or because California banks respond differently to interest rate determinants, we consider a model of bank deposit pricing that takes into account many of the factors that may influence interest rates on bank deposits. If variation in these factors explains the observed interest rate differentials, then it is the unique characteristics of bank markets in California that give rise to the disparities in rates. On the other hand, if these influences cannot account for the disparities in rates, then California banks must be responding to these influences differently than their counterparts do elsewhere.

For purposes of modelling bank deposit pricing, we can envision a bank as a "financial factory," combining inputs via a production technology to yield a set of outputs. The bank's outputs are the various lending, intermediary, and transaction services it provides. The bank's inputs are its deposits. When a depositor puts funds in a bank account, the bank can use these funds to make loans or other investments. In return, the depositor receives a direct payment for providing the input, namely interest, as well as the ability to consume some of the bank's outputs, namely bank services associated with the deposit account (in this way, bank deposits play a dual role in this model). These services are a form of "implicit interest" received by depositors.

It is essential to incorporate the service aspects of bank deposits into the analysis since such services may be a significant component of the total return to depositors. Explicit interest rates, by themselves, may not adequately measure this total return. We can treat the direct price the bank pays for its inputs (that is, the interest rates it pays on its deposits) like any other input price. Assuming the bank acts to maximize profits, it is possible to determine the price of the input as a function of output prices (that is, the value of bank services, such as transaction services or convenience), relevant characteristics of the production technology, and the price of near-substitutes (in this case, a market interest rate). This simple microeconomic framework suggests a number of variables that should help to explain the interest rates that banks pay on deposits. Thus, in general terms, we can model deposit interest rates as

$$r = f(x) \qquad (1)$$

where r is the interest rate paid by a bank on a particular deposit account, x is a vector consisting of variables like measures of bank costs of providing services to depositors, measures of the availability of bank services, market interest rates, etc., and $f(.)$ is a functional form that links deposit rates to the variables in vector x.

One issue that arises in modeling deposit rates is the relevant structure of bank deposit markets. Banks that enjoy some degree of market power as purchasers of deposit funds may be able to exercise this market power by acting as price setters rather than price takers in deposit markets. If evidence of bank market power does exist, this would lend credence to the complaints of California consumer groups regarding bank behavior in the state. Equation (1), therefore, may need to be modified in the following way:

$$r = f(x, MP) \qquad (2)$$

where MP is a variable that measures market power in the relevant deposit market.

This formulation suggests that empirical analysis of deposit rates should include some measure of market power. Following the market structure-performance framework from the economics of industrial organization, a number of studies of bank behavior have used measures of market structure as proxies for market power. According to this framework, there is a positive relationship between market concentration and firm profitability. This is due to the hypothesis that a high degree of market concentration endows firms with significant market power and makes it easier for them to collude or engage in other forms of non-competitive behavior.

There is an extensive literature on the empirical relationship between measures of market structure, on the one hand, and bank profits and prices, on the other. In these studies, measures of market concentration, such as n-firm concentration ratios or the Herfindahl-Hirschman Index (HHI), and/or market share statistics are used as proxies for market power. Although many of these studies have identified a positive relationship between market concentration and bank profitability, the results vary considerably as to the size of the estimated effect. In addition, a number of studies have failed to identify such a relationship between market structure and bank performance. The findings from this research thus are not conclusive.[4]

Demsetz (1973), Peltzman (1977), and others have offered an alternative interpretation of the positive relationship between structure and profits. According to this "efficient structure" hypothesis, particular industries may exhibit firm-specific efficiencies that lead naturally to relatively concentrated markets. These efficiencies enable leading firms in such markets to capture large market shares and to enjoy higher profits than less efficient firms in less concentrated settings. In effect, concentration is the result of the operational efficiency of firms rather than an exogenous characteristic of the market that firms exploit.[5] One important implication of this hypothesis for bank markets is that efficient banks in concentrated markets offer rates on deposits that are *more* favorable to depositors. This prediction contrasts with the structure-performance framework described above.

In a recent study, Berger and Hannan (1989) develop an empirical framework that enables them to differentiate between the structure-performance hypothesis and the efficient structure hypothesis. Both of these models imply a positive relationship between market concentration and bank profitability. They suggest opposite effects of concentration on *prices*, however. Berger and Hannan investigate the relationship between bank prices (that is, interest rates on retail deposit account products) and measures of market concentration using a cross section of individual banks in the U.S. They find a statistically significant negative relationship between interest rates on money market deposit accounts and market concentration. This means that bank customers face less favorable rates on MMDAs in markets that are more concentrated. This finding supports the structure-performance hypothesis and rejects the efficient structure hypothesis.

In contrast to their findings regarding MMDAs, Berger and Hannan find no evidence of any price-concentration relationship for several categories of certificates of deposit. They argue that such instruments are traded in broader geographic markets that are less likely to be influenced by local market conditions. One implication of these findings is that bank pricing strategies differ across account types. Alternatively, banks may not have the same market power for all retail deposit products. Berger and Hannan find no evidence to support the efficient structure hypothesis.

Another important factor that may affect the structure of bank markets is the regulatory environment in which banks operate. A number of bank regulations, such as state branching restrictions or unit banking laws, limit the geographic scope of bank markets. Just as market power can act as a hindrance to competitive behavior, regulatory restrictions can erect barriers to entry that shield banks from the influence of unrestricted competition. Any characterization of bank market structure should thus include the effects of bank regulations as well as measures of market power.

The microeconomic framework discussed above predicts that a number of explanatory variables should be included in a properly specified interest rate equation. A bank-specific model, developed by Hannan (1989), provides some guidance about likely candidates to include in empirical interest rate equations. For purposes of the current study, we divide these factors into three general categories: measures of market conditions, indicators of state-specific regulatory restrictions on banking, and cost and balance sheet data on individual banks. This last group of variables acts as proxies for service levels provided by banks and controls for other relevant effects.

Measures of Market Conditions

One issue that arises in deriving measures of market conditions is the proper definition of the relevant market in which the bank operates. Some bank deposits can be considered primarily local products, for example, accounts with transaction features. Since local checks are easier to cash, and clear faster than out-of-town checks, local providers of transaction accounts have a competitive advantage over out-of-town providers. Competition for transaction accounts therefore may be geographically limited by the need to provide local check-clearing services.[6] In contrast, certificates of deposit are pure savings vehicles that may trade in broader geographic markets. As a result of this ambiguity regarding the appropriate market definition, we provide measures of both local and statewide market conditions in order to capture influences of the varying geographic scope of bank markets.

As a measure of market power, we use local market three-firm concentration ratios, with local markets defined as Metropolitan Statistical Areas (MSAs) and non-MSA counties. As an additional measure of local market conditions, we include the growth rate of deposits in these markets. We expect that higher deposit growth rates reflect greater demand for bank deposits by banks. If this is true, higher growth rates will be associated with higher deposit interest rates and we thus predict a positive coefficient on this explanatory variable. We also recognize, however, that the influence of deposit growth rates on deposit interest rates could represent supply effects in the market for bank deposits, implying a different relationship between these variables. Even though this variable could reflect both demand and supply conditions in the market for bank deposits, we believe it is important to include it in the regression equations in this paper. One reason is that California bank markets exhibited among the slowest deposit growth rates in the sample. This variation from the rest of the sample should be included in the empirical analysis. There are also a number of precedents for this variable in deposit interest rate studies, such as Berger and Hannan (1989) and Keeley and Zimmerman (1985).

At the state level, we include a number of variables that capture important aspects of the broader geographic market for bank services. One of these variables is the total per capita bank offices in the state. This measure controls for differences across states in the relative availability of bank offices. One interpretation of this variable is that it represents the level of competition in the state banking market. If more bank offices per capita mean greater competition, then this variable may be associated with more favorable interest rates for bank depositors. The expected sign of the estimated coefficient on this variable would then be positive. Alternatively, more banking offices in a state may increase banks' ability to deliver services on a per capita basis. This variable may proxy, therefore, for convenience and service differentials that exist at state levels. In this case, the estimated coefficient should be negative.

As a measure of general market conditions, our regressions include the money market mutual fund rate as a proxy for the "market" interest rate. The money market fund rate varies over time with other market interest rates and captures the return to a near-substitute for many bank deposits. Banks must compete with money market funds in order to continue attracting deposits.[7] We expect that there is a very strong positive correlation between the money market fund rate and the rates paid on retail deposits that serve as savings vehicles since these accounts are close substitutes. The relationship between transaction account rates and money fund rates is not likely to be as strong. Transaction services are an essential component of these accounts. They are therefore less obvious substitutes for money market mutual funds.

Measures of State-Specific Restrictions on Banking

Our regressions measure state-level regulatory restrictions on banking and bank branching. We include dummy variables for states that have limited branching and unit banking laws. Limited branching laws represent a regulatory barrier to entry into local markets within a state. Such barriers are likely to restrict the degree of competition across local banking markets. Within their designated markets, banks in limited branching states may offer services that are roughly comparable to those of banks in states that allow unlimited branching. The primary difference between banks in limited and statewide branching states may therefore be this regulatory barrier to entry. We expect that the effect of limited branching on deposit interest rates is negative.

Likewise, banks in states with unit banking laws face regulatory barriers to entry. Thus, the relationship between unit banking laws and deposit rates should be negative. However, banks in unit-banking states cannot offer the same kinds of services that banks in unlimited (or even limited) branching states can. In effect, unit banks are forbidden from competing for deposits in many of the nonprice dimensions, such as offering more convenience through a branch network. Banks in these states essentially are forced to compete primarily through the explicit interest rates on their deposit accounts. This will have a positive effect on deposit rates. Thus, the estimated coefficient on the unit banking dummy could be either positive or negative, depending on which influence dominates.

Cost and Balance Sheet Measures of Individual Banks

The last group of explanatory variables contains factors specific to individual banks. This group includes variables intended to capture service and convenience aspects of bank deposit accounts. Some of these variables, such as the number of bank branches, attempt to measure implicit interest on bank accounts directly, while others infer the value of implicit interest from measures of the costs that arise from providing these services.

The first factor, then, is the number of bank branches.[8] This variable may help to capture the service and convenience components of an individual bank's products. Banks may offset a lower explicit interest rate on certain kinds of deposits with the convenience and service of an extensive branch network. As a result, we would expect to observe a negative relationship between deposit rates and the number of branches.

The branches variable has several limitations. It is not a useful proxy for bank-specific implicit interest payments in the eight states that had unit banking laws during the sample period. It also may not fully capture the service dimension of bank deposits. For example, longer hours and additional days open, ATMs, free or underpriced services, and promotions are not captured by the number of branches. But since these factors do entail higher operating costs, we include two cost variables as proxies: overhead (non-interest) expenses per dollar of assets and average bank salaries (total payroll expenses including benefits divided by the number of employees). Assuming that banks are profit maximizers, differences in overhead expenses and average salaries across banks should reflect either differences in the level of services provided (and, therefore, differences in implicit interest), or differences between high- and low-cost areas. To the extent that differences in operating costs reflect differences in implicit interest, we would predict a negative correlation between deposit rates and overhead expenses and salaries. Banks that offer higher compensation in the form of implicit interest may pay less explicit interest, with the net result that total compensation to the depositor is unchanged.

The bank-specific variables also include an asset-based measure of bank size as a control variable. Aside from the part size plays in determining market concentration measures, a bank's size may be important if depositors use it as an indicator of an institution's health and staying power, or its financial resources. This study includes a full range of banks, from money center institutions to small, single-office banks. Larger banks may have a wider range of alternatives to retail deposits than small institutions. Thus, at the margin, it is likely that bank size exhibits a negative influence on deposit rates.

Finally, we include a measure of the portfolio composition of each bank, as measured by the ratio of retail time deposits to total deposits. In general, the markets for large-denomination, wholesale CDs are more competitive than retail deposit markets. Banks that rely more heavily on retail core deposits, therefore, may be able to tap cheaper funding sources. The effect of this variable on deposit rates is thus likely to be negative.

By incorporating all of the above influences into an empirical pricing model of retail deposits, we hope to capture the key determinants of retail deposit rates. In this way, we can determine whether it is the unique characteristics of California banking markets that explain the deposit rate mystery.

III. Empirical Results: The Rate Mystery Thickens

Our discussion in the previous section describes a model of bank behavior and suggests a number of factors that should influence the interest rates paid on bank deposits. These include the local market concentration ratio as well as a number of other market, regulatory, and bank-specific cost factors. We estimate a version of this model on a time-series, cross-section sample of approximately 430 banks during the 1984-1987 period. With 16 quarterly values for each bank, we have almost 7,000 observations in our sample.[9]

These data suggest that California bank markets differ from markets elsewhere in a number of important respects. As Table 1 shows, California alone accounts for ten percent of U.S. bank deposits. In terms of the average asset size of the banks in our sample, California ranks second, at $6.8 billion, after New York. The sample average is only $2.6 billion in assets. California ranks third in terms of the number of branches per bank, at 121, well above the sample average of only 40 branches. Despite the large branch systems designed to attract retail deposits, California banks rank relatively low in the proportion of retail time deposits to total deposits. Moreover, California banks were among the slowest growing banks during the 1984 through 1987 period.[10] Indeed, in terms of the growth rate of local market deposits over this period, California banks ranked 47th out of the 48 states and District of Columbia included in our data sample.

California banks not only grew more slowly, they also incurred higher costs than the average. California ranked fourth highest in terms of average salary costs per employee and second in terms of overhead expenses per dollar of assets. The high costs may reflect additional expenses associated with staffing and operating the large retail branch systems common in the state. They may also be due to higher land and labor costs in California. Alternatively, these higher costs may reflect inefficiencies associated with a lack of competition arising from geographic barriers or monopoly power.

While California banks display some unique characteristics, a notable exception is the level of market concentration. Our measure of concentration is the three-firm deposit concentration ratio for the local market. This is defined as the combined market share of deposits held by the three largest banks in the market, divided by total bank deposits in the market. In terms of a weighted average state 3-firm concentration ratio (where local markets are weighted by deposit shares), California ranks near the middle, 25th out of 48 states and the District of

Columbia. It would appear at first glance that concentration alone cannot explain the lower deposit interest rates paid by California banks.[11]

These observations suggest that differences between bank markets in California and those elsewhere may help to explain deposit rate disparities. To test this hypothesis, we estimate the following equation:

$$r_{ijt} = a + b_1 CR3_{jt} + b_2 x_{ijt} + b_3 y_{ijt} + b_4 z_{ijt} + cCA_i + e_{ijt} \quad (3)$$

where r_{ijt} is the interest rate paid on one type of retail account by bank i in local market j at time period t. $CR3_{jt}$ is the 3-firm concentration ratio in local market j at time t, x_{ijt} is a vector of the market-specific variables included in the model, y_{ijt} represents a vector of the regulatory variables that may be important for bank i's pricing decisions in market j, z_{ijt} is a vector of bank-specific variables relevant to deposit pricing, CA_i is a dummy variable for banks located in California, and e_{ijt} is the error term. The parameters a and c and the vectors b_1 through b_4 are coefficients to be estimated.

Table 1

Average Sample Characteristics of U.S. vs. California Banks and Bank Markets

	U.S.	California	CA Rank
Total Deposits of Insured Commercial Banks (as of 12/31/87; $ billions)	$2,324	$231	2
3-Firm Concentration Ratio (wtd. avg. of local markets in state)	53.9%	63.2%	25
Market Deposit Growth Rate (wtd. avg. of local markets)	9.2%	2.5%	47
Per Capita Bank Offices in State	2.5	2.0	48
Average Bank Assets ($ millions)	$2,627	$6,811	2
Number of Branches per Bank	40	121	3
Retail Time Deposits as % of Total Deposits	55.1%	48.9%	43
Overhead Expenses per Dollar of Assets (cents)	2.1	2.9	2
Average Salary per Employee ($ thousands)	$15.6	$18.1	4

We estimate the model for the full sample of banks over the entire sample period for each deposit category. If the California dummy variable is statistically significant, then there are differences in interest rates between banks in California and those elsewhere that cannot be attributed merely to differences between the characteristics of bank markets in California and those elsewhere. Rather, a statistically significant coefficient on this dummy variable suggests that the explanation for the California deposit rate mystery lies elsewhere.

In Table 2, we present regression results for the four categories of deposits included in this study.[12] The first two columns contain the regression results for the two transactions-oriented accounts: NOWs and MMDAs. As described above, NOWs are interest-bearing checking accounts with transaction features that tie them predominantly to local bank markets. MMDAs provide a combination of features, including limited transactions and short-term market rate savings services designed to make them competitive with money market mutual fund shares. The transaction features may also tie MMDAs to local bank markets to some extent, as well.

Looking first at columns (1) and (2), we observe that local market concentration exerts a significant effect on deposit interest rates for both NOWs and MMDAs. These results suggest that local market power (as measured by the concentration ratio) is associated with lower deposit interest rates for these transaction-based accounts. The estimated coefficients are of similar magnitude, and predict that a 10-percentage point increase in market concentration (say, from 50 percent of deposits controlled by the top three firms in the local market to 60 percent) reduces deposit rates 1.3 to 1.7 basis points on MMDAs and NOWs, respectively.[13,14]

Several other factors are also significant in determining the deposit interest rates on these two accounts. The estimated coefficients on local market deposit growth rates are positive and significant in both regressions. These estimates imply that this variable captures demand factors in the local market, as we hypothesized above. Bank assets also are positively correlated with deposit interest rates, indicating that larger banks tend to pay higher rates. The limited branching dummy variable has the expected negative sign, suggesting that state branching restrictions do indeed represent market barriers to entry. Unit banking laws, in contrast, appear to exert an upward influence on deposit rates. This finding suggests that such laws force banks to compete through the explicit interest rates they pay on retail deposits. The more transactions-oriented NOW accounts are only loosely related to market interest rates, as indicated by the 0.26 coefficient on the money market fund rate. The more savings-oriented MMDAs follow market interest rates more closely, with an estimated coefficient of 0.83.

It is noteworthy that a number of the variables that proxy for implicit interest payments also are significant in these two regressions. The estimated coefficients on the number of bank branches are significant and negative for both transaction accounts. In addition, the average salary variable also displays a significantly negative coefficient. This term represents some of the costs associated with maintaining branches and providing implicit interest. Overhead expenses per dollar of assets are not significant in these two regressions.

The final explanatory variable included in these regressions is the dummy variable for California banks. The results described here indicate that our interest rate model has suggested a number of variables that are important determinants of deposit interest rates. On top of these determinants, however, we observe significant coefficients for the California dummy variable. Thus, rates paid by California banks on NOWs and MMDAs differ from the rest of the banks in the sample in a way that cannot be explained by the model. This means that, after taking account of the effects of the explanatory variables included in the model, deposit rates on these two accounts were consistently lower in California by an average of 26 basis points for NOWs during the 1986-87 sample period and by 19 basis points for MMDAs from 1984 to 1987. In view of the average differentials observed in Chart 1 of 37 and 28 basis points for NOWs and MMDAs, respectively, these

Table 2

Deposit Rate Regressions, 1984–1987

	Dependent Variable (rate in basis points)			
Independent Variable	(1) NOW (1986–87)	(2) MMDA	(3) 3–6 mo. CD	(4) 2.5 yr. CD
Constant	390.97**	101.06**	70.45**	169.75**
	(44.82)	(16.73)	(10.08)	(17.42)
3-Firm Concentration Ratio	−17.20**	−13.26**	−8.93*	−11.55
	(−4.54)	(−3.45)	(−2.00)	(−1.90)
Market Deposit Growth Rate	22.48**	16.97**	17.93**	35.61**
	(4.48)	(3.37)	(3.14)	(4.55)
Per Capita Bank Offices in State	0.60	8.27**	2.18	−0.33
	(0.49)	(6.48)	(1.47)	(−0.16)
Bank Assets	0.81**	0.74**	1.06**	0.66**
	(5.28)	(4.67)	(5.70)	(2.48)
Number of Branches	−0.10**	−0.06**	−0.09**	−0.01
	(−8.23)	(−4.92)	(−6.34)	(−0.57)
Retail Time as % of Total Deposits	−0.07	−0.46**	−0.18**	−0.05
	(−1.49)	(−9.68)	(−3.37)	(−0.71)
Overhead Expenses per $ Assets	−0.77	−1.25	−3.37**	−3.42**
	(−1.22)	(−1.87)	(−4.29)	(−3.05)
Average Salary	−0.84**	−1.79**	−1.39**	−1.15**
	(−7.78)	(−15.35)	(−10.28)	(−5.90)
Limited Branching Dummy	−6.62**	−3.73*	−7.00**	−5.18*
	(−4.50)	(−2.46)	(−3.97)	(−2.14)
Unit Banking Dummy	5.33*	19.88**	0.93	−3.02
	(2.28)	(8.62)	(0.35)	(−0.81)
Money Market Mutual Fund Rate	0.26**	0.83**	1.00**	1.00**
	(22.82)	(196.10)	(202.77)	(146.50)
California Dummy	−25.67**	−19.39**	−7.73*	−1.72
	(−8.20)	(−6.05)	(−2.11)	(−0.33)
R-Bar Squared	0.243	0.866	0.869	0.780
No. of Observations	3415	6573	6637	6475

Notes: *(**) indicates coefficient significantly different from zero at the 5 (1) percent level; t-statistics in parentheses.

coefficients suggest that variations in the model's explanatory variables account for approximately one-third of the observed differentials on both NOWs and MMDAs.

As a final observation on these regressions, we note that the deposit rate model performs considerably better in explaining the variation in MMDA rates than it does for NOW account rates. The R-bar squared statistic is .866 for the MMDA regression, and only .243 for NOWs. We thus explain only about one quarter of the variation in NOW rates. In fact, NOW account interest rates move infrequently while the explanatory variables exhibit considerable variation during the sample period. Our deposit rate model clearly does not capture the reasons for the sluggish movement in NOW rates, as reflected by the low explanatory power of this regression.

The last two columns of Table 2 contain comparable regression results for the two categories of retail certificates of deposit. Looking first at the estimated coefficients on the concentration ratio, we find that the estimates are negative, but are smaller and less statistically significant than for the two transaction accounts. The coefficient on the three-firm concentration ratio is significant at the five percent level for the short-term CD, although the absolute value of the point estimate is substantially smaller than for either transaction account. The estimated coefficient on market concentration is not significantly different from zero for the long-term certificate. The relationship between local market concentration and deposit interest rates for these CDs is thus less important than it is for the two transaction accounts.[15]

One explanation for this finding is that these retail certificates of deposit are more strictly savings vehicles and, the longer the certificate, the less important local bank services are likely to be to the depositor. Therefore, markets for these CDs may encompass a much broader geographic scope. Moreover, CD rates are frequently published and made available on a regional or national basis, allowing funds to be deposited outside the local market area by mail or through deposit brokers. At the margin, competition may serve to minimize differentials across markets. As a result, CD rates are less likely to be affected by local market conditions and, thus, we would expect to observe a smaller effect of local market concentration on the interest rates on CDs than on transaction accounts.

The regression results for the two CDs also differ from those for NOWs and MMDAs in other ways, for example, with respect to the significance of the variables that measure implicit interest. Aside from average salaries, which have a negative impact on all four rates, we find that short-term CDs appear to have some degree of a service component, as indicated by the significant negative coefficient on the number of branches. The long-term certificate is the only account category of the four for which this variable is not statistically significant. Overhead expenses also appear to exert some downward pressure on CD rates, in contrast to a lack of any observed effect on transaction accounts. This finding is difficult to explain, especially if we believe that these non-interest expenses measure the cost of providing implicit interest.[16]

The dummy variable for unit banking states has no statistically significant effect on deposit rates for either CD category, in contrast to the positive estimated coefficients for the two transaction accounts. Although unit banking laws appear to induce banks to compete primarily on the basis of interest rates for transaction accounts, these laws have no such identifiable effect for certificates. This result is consistent with the notion that CDs trade in geographic markets that are not confined by state borders. In the market for strict savings vehicles, one bank may look like any other, regardless of its ability to offer branches.

Finally, we expect that both of these savings certificates should follow market interest rates closely to maintain their attractiveness relative to competing instruments. The estimated results confirm this prediction, as shown by the 1.0 estimated coefficients on the money market fund rate.

The estimated coefficients on the California dummy variable also indicate some important differences between transaction accounts and certificates of deposit. While the point estimates for these dummy variable coefficients are negative for both account maturities, the California dummy variable is not statistically significant for long-term CDs and is significant only at the five percent level for the short-term certificates. The latter results suggest that, after taking other factors into account, short-term CD rates were eight basis points less in California than elsewhere during the 1984-1987 sample period. This is substantially smaller than the observed differentials for the transaction accounts.[17] For the long-term CDs, rates in California are statistically indistinguishable from those paid by banks in other states.[18,19]

IV. Explaining the Mystery

The estimated coefficients on the California dummy variables in Table 2 provide evidence that, although standard determinants of bank deposit rates help to explain a portion of the disparity in rates between California and the U.S., a sizable proportion of this disparity apparently is the result of other factors. Specifically, with respect to NOWs and MMDAs, California banks may respond differently to the factors included in our model than do banks elsewhere.[20] There is less evidence, however, that different pricing strategies prevail for the two certificates of deposit. Given these findings, we focus in the remainder of this paper on explaining the sources of the interest rate differentials for NOWs and MMDAs only.

One way to interpret the significant California dummy coefficients for the transaction accounts is that they indicate an inappropriate restriction on the estimated model. The full-sample regressions impose the restriction that the estimated coefficients for all banks (regardless of location) are identical. If California banks respond differently to the determinants of deposit interest rates than banks elsewhere, then this restriction is incorrect. F-statistics constructed from separate regressions for the California and non-California banks in our sample support the notion that California banks respond differently to interest rate determinants than banks elsewhere. These tests confirm that the sets of estimated coefficients for NOW accounts are statistically different between California and non-California banks. We find weaker evidence of differential responses for MMDAs.

If California banks respond differently to interest rate determinants than banks elsewhere, as the above tests suggest, then we wish to find how much of the observed discrepancies can be attributed to these different responses. To accomplish this, we re-estimate the regressions in Table 2, including dummy variables for California banks interacted with the other explanatory variables. The estimated coefficients on these interacted variables represent the marginal effects of the explanatory variables for California banks, over and above their effects for the sample as a whole. The results from these estimates are presented in Table 3.

Looking first at the results from the NOW account regression, we find estimated coefficients for the non-interacted variables that are extremely close to the estimates in Table 2, with the exception of the coefficient on overhead expenses. Among the interacted variables, we observe a large negative intercept term and a large positive coefficient on concentration, both of which are statistically significant. Among the remaining interacted variables, the two cost measures are both statistically significant (with opposite signs), and we observe a large positive coefficient on market deposit growth.

Using this new set of estimated coefficients, we can calculate the implied deposit interest rates paid by a bank with average sample characteristics for a non-California bank, operating in an average sample, non-California market. For purposes of this exercise, we exclude the effects of unit banking and limited branching restrictions. With these same non-California average sample values, we can then determine the interest rate this bank would charge if it were to respond to the explanatory variables as the California banks do. The difference between these two estimates provides an indication of the extent to which California banks respond differently to rate determinants. This difference is then compared to the observed differentials.[21]

Using this approach to generate the NOW account deposit interest rate implied by the average non-California sample values of the explanatory variables, we obtain a rate of 5.21 percent. By inducing this bank to act like its California counterpart, we get an interest rate of 5.31 percent *using the same average sample values*. In effect, the marginal influence of the different response of California banks is to raise the NOW rate *above* that for the rest of the nation. Allowing for a different response of California banks to the determinants of deposit interest rates suggests that California banks ought to pay *higher* deposit interest rates on NOWs, not lower ones. These differential responses thus provide no explanation for the interest rate disparity we observe on NOW accounts.

Pursuing this same exercise for MMDA deposit rates, we find fewer significant interacted explanatory variables. Only two of these variables, a negative intercept and a positive coefficient on overhead expenses, are significant at the one percent level. A positive coefficient on concentration is also significant at the five percent level. The marginal effects of the different responses of California banks to the model's explanatory variables are thus smaller for MMDAs than for NOWs. Calculating the interest rates implied by these estimated coefficients, we obtain 6.63 percent for the average non-California bank, and 6.44 percent for an average bank that acts like a California bank. This differential of 19 basis points suggests that differences in the behavioral response of California banks to the determinants of deposit interest rates explain approximately two-thirds of the observed interest rate discrepancy in MMDA rates of 28 basis points from 1984-87. The remaining discrepancy is due either to the unique charac-

Table 3

Deposit Rate Regressions, 1984–87: Variables Interacted with CA Dummy

Independent Variable	Dependent Variable (rate in basis points) NOW (1986–87)	MMDA
Constant	393.79**	102.69**
	(44.43)	(16.43)
3-Firm Concentration Ratio	−17.19**	−13.93**
	(−4.58)	(−3.59)
Market Deposit Growth Rate	19.71**	16.65**
	(3.98)	(3.28)
Per Capita Bank Offices in State	0.43	8.26**
	(0.39)	(6.47)
Bank Assets	0.72**	0.71**
	(4.68)	(4.42)
Number of Branches	−0.11**	−0.05**
	(−7.40)	(−3.31)
Retail Time as % of Total Deposits	−0.07	−0.45**
	(−1.49)	(−9.18)
Overhead Expenses per $ Assets	−2.46**	−1.88**
	(−3.66)	(−2.61)
Average Salary	−0.59**	−1.73**
	(−5.22)	(−14.04)
Limited Branching Dummy	−7.60**	−3.57*
	(−5.14)	(2.30)
Unit Banking Dummy	3.70	20.12**
	(1.57)	(8.41)
Money Market Fund Rate	0.26**	0.83**
	(22.37)	(190.05)
CA*Constant	−138.75**	−97.85**
	(−3.71)	(−3.51)
CA*3-Firm Concentration Ratio	158.39**	72.80*
	(4.88)	(2.00)
CA*Market Deposit Growth Rate	461.47**	45.69
	(8.14)	(0.99)
CA*Bank Assets	2.39*	0.14
	(1.96)	(0.13)
CA*Number of Branches	−0.11	−0.02
	(−1.42)	(−0.31)
CA*Retail Time as % of Total Deposits	−0.07	0.14
	(−0.36)	(0.64)
CA*Overhead Expenses per $ Assets	13.87**	5.75**
	(7.75)	(2.84)
CA*Average Salary	−2.48**	−0.53
	(−6.89)	(−1.34)
CA*Money Market Fund Rate	0.02	0.02
	(0.34)	(1.29)
R-Bar Squared	0.269	0.866
No. of Observations	3415	6573

Notes: *(**) indicates coefficient significantly different from zero at the 5 (1) percent level; t-statistics in parentheses.

teristics of California banks and the state's bank markets, or to a misspecified deposit interest rate model.

The results of these tests indicate that relaxing the constraint that all banks in the sample act the same "explains" a considerable proportion of the rate mystery for MMDAs. Allowing for differences in bank behavior, however, explains virtually none of the interest rate discrepancies for NOWs. Of course, finding evidence that banks in California are different from those elsewhere begs the more fundamental question why this should be so.

It is also difficult to attribute portions of the differentials to the explanatory variables. The Table 3 regression for NOWs, for example, shows a large negative interacted constant, suggesting a shift in the level of rates by California banks. This level shift is offset by a large positive coefficient on interacted market concentration, a result that is contrary to the theoretical predictions of the structure-performance hypothesis and with the empirical results for the rest of the sample. While the positive coefficient on interacted concentration is consistent with the efficient structure hypothesis discussed in Section II above, the magnitude of the implied price effect makes it seem unlikely that California banks are that much more efficient than those elsewhere. We therefore put little credence in this interpretation of the results. These findings are thus difficult to explain and contribute little to identifying the sources of the NOW rate mystery.

An alternative avenue of research is to investigate other ways in which market power may manifest itself. There is reason to believe that local market concentration ratios may not be adequate measures of market power in California banking. Banks in the state have large, statewide branching networks and appear to price their deposits on a statewide basis. Market power in California, therefore, may be exercised by banks in a way that is not well captured by this traditional measure of local market concentration. One suggestion by Neumark and Sharpe (1989) is that market power may manifest itself in the rate at which deposit rates adjust to changes in market interest rates. Specifically, banks that exercise market power may adjust deposit rates more slowly in an upward direction than in a downward direction. If this is true, then in markets where banks have market power, we would observe deposit rates lagging market interest rates when rates are rising, but declining in concert with market rates when rates are falling.

In an attempt to address this issue, we estimated different regressions for periods when rates were rising and falling, and found some evidence that the California dummy variable was larger in periods of rising rates than in periods of falling rates for both transaction accounts. While this finding is consistent with California banks exercising market power in adjusting their deposit interest rates, an extensive analysis of this dynamic adjustment model is beyond the scope of this paper. It does suggest, however, that additional research in this area may prove fruitful.

V. Summary and Conclusion

In this paper, we explore the so-called California deposit rate mystery. We confirm that California banks paid lower deposit rates on two kinds of retail transactions accounts than non-California banks during the 1984-1987 sample period. For two maturities of time certificates of deposit, the estimated interest rate differentials are smaller and less distinctive. There is less evidence, therefore, that the deposit rate mystery extends to time CDs. Our results suggest that the discrepancies are primarily a phenomenon associated with transaction-based accounts. We also find that the unique characteristics of banking markets in California account for approximately one-third of the observed differentials.

We then estimate regression equations for these transaction-based accounts that permit the behavior of California and non-California banks to vary and find significant differences in the responses of the two samples of banks to the model's explanatory variables. Allowing for these different responses is sufficient to eliminate two-thirds of the predicted interest rate discrepancies for MMDAs, but "explains" very little of the differential for NOWs.

Despite these positive findings, the California deposit rate mystery remains an interesting puzzle. For example, we cannot explain why California banks act differently from banks elsewhere. It appears that state borders have shielded banks in California from the influences affecting banks in other states. The importance of these borders will decline in 1991 when California allows full interstate banking. Will the different pricing behavior of California banks continue after 1991, or will banks in the state come to resemble those elsewhere? Perhaps even more interesting, will non-California bank holding companies acquiring banks in California continue to behave as they previously did outside the state or will they act like their California counterparts in setting deposit interest rates? The answers to these questions, and the final resolution to the rate mystery itself, likely will have to wait until after 1991.

NOTES

1. Data are from the Monthly Survey of Select Deposits (FR 2042 Report). The most common interest rates paid on several retail deposit accounts are collected as of the close of business on the last Wednesday of the month from a sample of approximately 435 banks nationwide. The sample includes institutions of all size categories. We use the observation for the last month in the quarter to obtain a quarterly time series on deposit rates.

2. To determine the statistical significance of these differentials, we regressed the average rate paid by California banks in the sample against a constant term and the average rate paid by all banks in the sample. A significant estimated constant term indicates that the differences in the interest rates are statistically significant.

3. See Neuberger and Zimmerman (1989) for additional discussion.

4. See the extensive surveys by Gilbert (1984) and Rhoades (1977,1982) for discussion and analysis of these studies.

5. Smirlock (1985) tests this hypothesis on a sample of unit banks during the 1970s. His analysis shows that once market share is accounted for, concentration has no explanatory power for bank profitability. In contrast, market share is positively and significantly related to bank profitability even after controlling for concentration. Smirlock interprets these results as contrary to the structure-performance hypothesis and supportive of the efficient structure hypothesis. He argues that market concentration is indicative not of collusive market power but of the superior efficiency of leading firms.

6. Although limited transaction MMDA deposits generally are drawn from a bank's local market area, Keeley and Zimmerman (1985) found no evidence to support the hypothesis that MMDA markets in California were local. However, their analysis did find evidence of local markets for the Super NOW account, which pays market rates and provides full transaction services, and is similar to the NOW accounts studied here.

7. An alternative specification of the model, using quarterly time dummies in place of the money market mutual fund interest rate produced similar regression results. The interest rate on money market funds moves closely with open market interest rates. In addition, money market funds compete directly with retail deposit products offered by banking institutions.

8. In our empirical estimates, we use the number of branches owned and operated by each bank. A reasonable case can be made that this number should be normalized, for example, by dividing by market population or market size. However, it is not clear which is the appropriate standard for the normalization. We tried several normalization techniques and obtained similar results to the estimates using only the number of branches. We thus chose to use the number of branches.

9. The most common interest rates paid during the month for each deposit category are reported for each bank in the survey. We define the bank's local market as the MSA or non-MSA county in which the home office is located. In this way, we apply the most common interest rate to a local market. This means we have one observation per time period for each bank. Bank characteristic data were obtained from call reports, which are available on a quarterly basis. The interest rate data are monthly time series. We used the last monthly observation in each quarter. Note that data for NOW accounts are available starting in 1986. The NOW regressions were thus estimated on approximately 3500 observations from 1986 to 1987.

10. This was also a period when California banks lost a significant share of the deposit market to thrifts. Thrift institutions in the state accounted for more than half of total domestic deposits by the end of the period, far higher than the national average.

11. In 1987, the 3-firm concentration ratio of 55.0 for the entire state (including all banks and all markets) ranked 15th out of the 50 states, although California was not statistically significantly different from the mean across all states of 46.6 percent.

12. These estimates are analogous to those reported in Berger and Hannan (1989), with the sample updated to include quarterly data for 1986 and 1987.

13. These results are consistent with the findings of Berger and Hannan (1989), although our estimated coefficients are considerably smaller than theirs. There are a number of potential reasons for the different estimated coefficient on the concentration ratio between our study and that of Berger and Hannan. First, our specification contains several variables that they do not include in their estimated equations. The results of the two studies, therefore, are not perfectly comparable. More importantly, there is evidence that the relationship between market concentration and bank deposit pricing decisions is changing over time. To test this hypothesis, we split the sample in half and ran the same regressions over the two intervals. In the MMDA regressions, the coefficient on concentration was twice as large in the 1984-85 regression as it was in the 1986-87 estimates. These estimated coefficients were significantly different from one another at the 5 percent level. While the 1984-85 results are closer to the findings of Berger and Hannan than the whole-sample regressions, we still estimate significantly smaller concentration coefficients than they do.

14. This result conflicts somewhat with the results reported by Keeley and Zimmerman (1985). Using a limited sample of nine western states that allowed statewide branching, those authors found a significant, negative relationship between interest rates on MMDAs and a state-level market concentration measure, but no significant relationship between MMDA rates and local market concentration.

15. We examined other maturities of retail CDs and generally found similar results. The estimates for the various CDs were generally quite consistent and significantly different from the transactions accounts.

16. It is plausible that banks may allocate some overhead costs to these savings instruments. However, most banks generate little fee income from these accounts to offset this overhead. In contrast, banks charge for many services associated with transaction accounts, i.e., monthly charges and per item fees to name a few, and this fee income may reduce the strength of any relationship between gross overhead costs and interest rates on these accounts.

17. The estimated coefficient on the California dummy variable for the three- to six-month CD may give some indication of the costs associated with switching bank accounts. Flannery (1982) has suggested that bank accounts involve quasi-fixed costs that prevent a complete adjustment of deposit interest rates to closely competitive instruments, such as other market rates or rates at competing depository institutions. Over a year, an eight basis point difference in deposit rates translates to a loss of only eight dollars on a $10,000 account. This may be too little to induce many CD holders to find alternative investments.

18. The results presented in Table 2 are consistent with those reported in Berger and Hannan's paper: market concentration is associated with lower deposit interest rates for MMDAs but not with lower longer-term CD rates. The addition of two years of data to the sample has not altered the basic findings of their study. Moreover, the additional period allows us to include NOW accounts in our analysis, confirming the results for transaction-oriented accounts.

19. The rate mystery may also extend to other major states. One version of the model included dummy variables for several major banking markets, including New York, Illinois, Pennsylvania, Michigan, Texas, and California. The results indicate that rates in a number of these states also differed from the sample average by statistically significant amounts.

20. We know from observing the pricing behavior of California banks that many of these institutions employ a statewide policy of setting deposit interest rates. That is, an account at a major California bank receives the same interest rate whether it is at a branch in a remote rural area of the state or in a densely populated urban center. California banks have thus chosen to ignore to some degree local market conditions in setting interest rates on their deposit accounts, a decision which may not apply to other markets.

21. In order to generate the "average sample" interest rates presented below, we use average sample values of the explanatory variables for the non-California sample of banks, and multiply them by the estimated coefficients (excluding the interacted variables) in Table 3. Sample periods are 1986-87 for NOWs and 1984-87 for MMDAs. We then add to these estimates the same non-California average sample values multiplied by the corresponding interacted coefficients, including the interacted constant term. The result is the interest rate the average non-California bank would pay if it were to act like the California banks in our sample.

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Economic Integration and Fiscal Policy Transmission: Implications for Europe in 1992 and Beyond

Reuven Glick and Michael Hutchison

Senior Economist, Federal Reserve Bank of San Francisco, and Economist, Bank for International Settlements. The major part of Michael Hutchison's contribution to this paper was done while at the University of California, Santa Cruz, and working as a visiting scholar at the Federal Reserve Bank of San Francisco. The authors appreciate helpful comments received from seminar participants at Gothenburg University, the Federal Reserve Bank of San Francisco, and the Bank for International Settlements. Members of the editorial committee were Gary Zimmerman, John Judd, and Ramon Moreno.

Increased financial integration within the European Community has implications for the conduct of fiscal policy by member nations. This paper shows that with greater integration, a fiscal policy shift within a given country will tend to have a diminished local impact and a correspondingly greater external impact on other member countries. This suggests that some convergence of fiscal policies may be necessary within the region as integration proceeds.

Recent plans by the member countries within the European Community (EC) to create a single integrated market by 1992 have raised questions concerning the appropriate conduct of fiscal policy in interdependent, open economies. There is little disagreement that this increased integration will necessitate greater coordination of monetary policies if the European countries are to move closer to their longer-run goal of a full monetary union, possibly with a common currency. However, the possible need to establish a community-wide fiscal policy stance either through fiscal policy "harmonization" or coordination has only recently received much attention. A report sponsored by the EC and issued in April 1989 suggested that although the level and composition of government spending as well as many revenue measures should remain the preserve of member states even in the final stage of economic union, closer coordination of national budgetary policies may be necessary.[1]

An important concern underlying these policy recommendations is the presumption that the EC's moves to liberalize capital flows will magnify the domestic and international transmission of economic disturbances, particularly divergent fiscal policies. In the absence of controls, some believe fiscal policy shifts and other disturbances could lead to greater macroeconomic instability. Large divergences in budgetary positions and marked differences in external balance among EC members have reinforced this concern.

This paper addresses the question whether liberalization of capital controls in the EC will make greater harmonization or coordination of fiscal policies more desirable. We analyze how the effects of policy changes and disturbances are likely to change in response to greater interest rate linkage associated with increasing financial integration within the EC. In particular, we investigate the merits of the view that a higher degree of capital mobility is likely to cause divergent fiscal policies to have greatly magnified—and potentially destabilizing—real effects on the EC economies.

Our analytical framework highlights the role of intertemporal budget constraints and private sector behavior in the context of a two-period, two-country framework. In this framework, private and public sector spending decisions are not independent events with a one-time outcome, but are multiperiod decisions linked across time through borrowing and lending. Moreover, our framework considers the effects of government policies in a general equilibrium setting with rational, forward-looking households. This allows us to focus on the interactions between financial liberalization and fiscal policy in the two countries in a well-defined way that is not possible in a small open economy setting.

The framework is designed to analyze the effects of financial liberalization and fiscal policy on real consumption, saving, trade balances, and real interest rates in the two countries. In concentrating on the real side of the economy, however, we abstract from some other important issues. In particular, we do not attempt to assess the effects of financial liberalization on the operation of a monetary union and the maintenance of fixed nominal exchange rates. A number of other recent papers have addressed these issues (for example, Lane and Rojas-Suarez, 1989).

A major conclusion of our analysis is that greater financial liberalization creates an environment in which fiscal disturbances originating at home tend to have smaller consequences for the domestic economy and larger consequences for the foreign economy. In particular, a home fiscal expansion places less upward pressure on domestic interest rates and more upward pressure on foreign interest rates as financial integration grows. Correspondingly, domestic consumption is "crowded out" less, and foreign consumption declines more. From this perspective, the call for greater fiscal policy harmonization or coordination may be viewed as an effort by individual EC nations to limit the increased exposure to disturbances emanating from other European economies that accompanies greater financial integration.

This paper is organized as follows. Section I presents an overview of the major financial liberalization measures and the process of financial integration that have taken place over the past several years among the EC member countries. It also presents some summary historical statistics covering the fiscal stances and debt positions of the EC nations. In Section II we formally analyze the economic effects of increased financial integration. Section III concludes the paper with a number of policy implications.

I. Financial Integration and Fiscal Policy in the EC

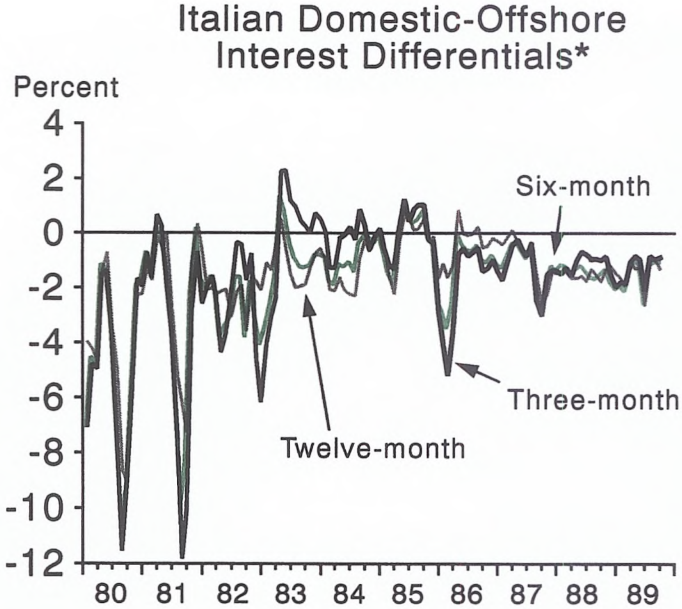
Removing barriers to capital movements is a central part of the EC plan for financial integration, as it lays the foundation for the integration of financial markets and provision of financial services.[2] As mentioned above, however, many are concerned that divergent fiscal stances among the member states of the EC could have adverse consequences in a deregulated financial environment. This section provides a brief overview of financial integration in the EC and presents summary statistics demonstrating the existing divergences in fiscal positions.

Financial Integration

European countries traditionally have imposed a wide variety of restrictions and taxes on international financial transactions, most with the intent of limiting net capital outflows.[3] In some cases these controls have taken the form of limits on the extent domestic residents can invest abroad either through the imposition of quantitative quotas, as in France after 1981 and the United Kingdom until 1979, or prohibitive taxes, as in Italy. Likewise, dual exchange rate systems, as in Belgium and Luxembourg, often work to limit capital outflows when particular international financial transactions are restricted to being conducted at a less advantageous exchange rate than other transactions. In other cases, capital controls have taken the form of restrictions on foreigners' borrowing in domestic capital markets. This has been practiced by France, Italy, and Denmark.

The effect of such restrictions has been to discourage active arbitrage between domestic and international financial markets and to reduce the linkage between interest rates at home and abroad. Giavazzi and Giovannini (1986) show that capital controls limited arbitrage between domestic and offshore market interest rates in France and Italy between 1979 and 1985. This effect is particularly pronounced for Italy, but also has been apparent in France during periods of turbulence in the European Monetary System. Frankel and MacArthur (1988) use data on covered interest differentials for domestic securities over the period from 1982 to 1987, and find that France and most of the small European countries effectively limited capital market arbitrage, thereby maintaining domestic interest rates at lower levels than otherwise would have been the case.[4] Giavazzi and Pagano (1985) and Barone, *et. al.* (1989) present evidence that Italian capital controls effectively limited capital outflows during the early 1980s.

The chart reproduces evidence presented by Barone, *et. al.* (1989) showing that domestic Italian rates (Treasury Bill rates) have been lower than offshore Lira rates (Eurolira deposit rates). Effective restrictions on capital outflows, particularly in the early 1980s, presumably limited arbitrage possibilities and the ability of domestic residents



* Italian Treasury bill rates minus interest rates on lira deposits in the Euro-market. Interest rate levels are monthly averages of daily rates.

to take advantage of higher Euromarket rates. Barone, *et. al.* suggest that the narrowing of the domestic-offshore interest differential in recent years and its reduced volatility provide evidence of the progress already achieved in liberalizing international capital movements in Italy.[5]

In fact, the gradual relaxation of restrictions on international capital flows in most EC nations has been a general phenomenon since the early 1960s. Although there were several notable setbacks in the 1970s, as a number of countries reimposed controls in the face of balance of payments problems, momentum was regained in the early 1980s. Moreover, in 1986 the European Community agreed in principle to remove capital controls directly related to trade and investment, and in 1988 to remove all remaining controls.

At present, the United Kingdom, Germany, the Netherlands, and Denmark have fully eliminated capital controls. Belgium, Luxembourg, France, and Italy still have a few remaining barriers, but are scheduled for complete liberalization by July 1990.[6] (The dual rate system of Belgium-Luxembourg is scheduled to be eliminated by the end of 1992.) The few restrictions that do remain for these nations include French and Italian restrictions on accounts held abroad by residents and the Italian restrictions associated with the foreign exchange monopoly of the central bank. In Italy, residents still have an obligation to surrender all foreign exchange earnings and are not allowed to hold foreign deposits. Banks, likewise, are restricted in their holdings of foreign exchange and net open positions. These remaining restrictions continue to limit capital outflows.[7]

Divergent Fiscal Positions

It is clear that the EC member states have pursued widely varied budgetary policies over the past decade with no recent moves toward convergence. Table 1 presents some summary fiscal statistics on general government financial balances and debt for the EC nations. The table shows wide variation in budgetary positions in 1987: the general government financial balance of Denmark was in surplus, while Germany, France, and the United Kingdom displayed small deficit positions of two percent of Gross Domestic Product (GDP) or less. The remaining six EC nations posted significantly larger financial deficits. Moreover, although the financial position of these countries has changed in the past decade, the magnitude of the divergences in government financial positions has remained approximately constant: similar cross-country variation in government financial balances was in evidence a decade ago.

The outstanding public debt positions of the EC nations reflect the diversity of their budgetary positions. Net public debt positions in 1987, for example, ranged from a low of 22.6 percent of GDP for Germany to a high of 121.8 percent for Belgium. Moreover, the figures in Table 1 also show that the diversity in debt positions among the EC nations at present is roughly the same as that prevailing at the beginning of the decade. No moves toward fiscal convergence are apparent in the data.

Table 1

General Government Fiscal Indicators in the European Community
(Percent of GDP)

European Community Members	Fiscal Balance		Gross Public Debt		Net Public Debt	
	1978	1987	1980	1987	1980	1987
Germany	−2.4	−1.8	32.5	43.6	14.3	22.6
France	−2.1	−2.0	37.3	43.9	14.3	25.8
U.K.	−4.4	−1.5	54.6	50.0	47.5	43.4
Italy	−10.3	−10.5	58.5	92.7	53.6	90.6
Belgium	−6.0	−7.2	79.9	132.5	69.3	121.8
Netherlands	−3.1	−6.1	45.9	76.9	24.9	52.1
Denmark	−0.3	2.0	33.5	57.2	7.3	25.3
Greece	−1.7	−11.1	27.7	63.3	NA	NA
Spain	−1.8	−3.6	18.5	48.1	7.8	31.0
Ireland	−8.7	−9.9	78.0	137.2	NA	NA

Source: OECD *Economic Outlook,* June 1989, Tables R-15, 33 and 34.

The calls for greater convergence and coordination of fiscal policy are in part based on the strong economic linkages that already exist within the EC, particularly among the nations participating in the European Monetary System, and on the expectation that deregulation of capital controls will strengthen these ties.

Simulations of a number of large econometric models illustrate these strong linkages, not least on the fiscal side. Representative results from these exercises, presented in Table 2, show the effects of independent fiscal expansions in each of the largest EC countries. The experiment shown is the effect of a sustained rise in real government expenditure equal to one percent of GNP on the level of domestic and foreign real GNP. One year following a one percent fiscal expansion in Germany, French real GNP is estimated to rise by .44 percent, Italian GNP by .45 percent, and U.K. GNP by .07 percent. The multipliers for France and Italy are smaller by a fraction corresponding to the size of their economies, but nonetheless are significant. Clearly, the degree of linkage among EC nations—even with the existing degree of international capital mobility—is sufficiently large as to transmit fiscal shocks from one EC nation to another.

Nonetheless, it is not entirely clear from a theoretical perspective that this will present a particular problem for these economies after the complete removal of capital controls within the EC. In particular, it is not obvious that the disruptive effects of fiscal divergences ("shocks") need increase as the degree of capital mobility increases. We address this issue below within the context of a simple theoretical framework.

In the following analysis, we refer to EC nations that are in the process of removing the existing controls and restrictions on capital outflows as "Italy." Those that already have removed restrictions on international capital movements, but are likely to be affected by the liberalization process in other EC nations, are referred to as "Germany." The starting point for our analysis is that both the "Italian" private and government sectors are net borrowers abroad.[8] In addition, it is assumed that controls on capital outflows effectively limit the extent to which "Italian" private residents purchase foreign assets, while encouraging them to borrow more abroad. This implies that private net foreign lending (borrowing) is less (more) than in the absence of controls. Since one of the motivations for the introduction of capital outflow controls presumably is the desire to finance government debt domestically at relatively favorable rates,[9] the level of foreign government debt financing may be interpreted as being less than would otherwise be the case in the absence of controls.

Table 2

European Community Real Linkages: Effects of Sustained Increase in Real Government Expenditure Equal to 1% of Real GNP

Fiscal Expansion in:	Change in Real GNP (Percent)			
	Germany	France	U.K.	Italy
Germany	1.10	.44	.07	.45
France	.20	1.54	.03	.26
U.K.	.13	.18	1.17	.15
Italy	.23	.41	.05	1.92

Source: Economic Research Institute, Economic Planning Agency, Japan

Note: The numbers represent the percentage increase in real GNP (for the country listed in the column headings) one year after the beginning of a sustained fiscal stimulus (by the country listed in each row).

II. Analytical Framework

This section develops a simple model to explore the effects of increased financial integration on the countries within an economic union. In particular, we analyze the way greater financial integration influences the impact of changes in fiscal policy on real macroeconomic variables such as real interest rates, consumption levels, and the trade balance.

The model highlights the role of intertemporal budget constraints and private sector behavior. This intertemporal perspective is crucial in analyzing the general equilibrium effects of particular policies in a framework with rational, forward-looking households.[10]

We capture these effects within a real, two-period, two-country framework. The two-period assumption allows us to capture the flavor of intertemporal decision making and relationships within the simplest possible setting. The general results we obtain are invariant to a multiperiod setting. The two-country framework allows us to focus on the direct and indirect interactions between two economies in a well-defined way that is not possible with a small open economy model. In this analysis we focus on the "real" aspects of integration and abstract from monetary issues.

The Model

Consider a world of two countries, each of which has a household sector and government. The home country will be referred to as Italy, and the foreign country as Germany. In each period t ($t=1,2$), the home country produces a given quantity of output Y_t of a single good; the foreign country produces Y_t^* of the same good. Out of these quantities, residents in each country pay T_t and T_t^* lump-sum units of taxes to their respective governments. They also invest in local government bonds and borrow (or lend) abroad. What is left over is consumed.

Specifically, the home (Italian) households' first and second period budget constraints are:

$$C_1 = Y_1 - T_1 - B + F^p \tag{1}$$

$$C_2 = Y_2 - T_2 + (1+r_b)B - (1+r_f)F^p \tag{2}$$

where C_t denotes consumption in period t, B denotes lending by home country households to the home country (Italian) government, and F^p denotes borrowing from foreign (German) households in period 1. It is assumed that the associated interest rates on these activities are r_b and r_f, respectively. Equation (1) defines home country consumption in the first period as output plus foreign borrowing net of taxes and domestic lending. Consumption in the second period is given by output, net of taxes, plus the return earned on first-period lending, net of foreign debt repayment. The two-period horizon of the model implies that all borrowing undertaken in period 1 is repaid in period 2, and no new debts are incurred. For simplicity we have assumed that there is no real investment and that output is exogenous.

Households in the foreign country, Germany, are assumed to lend both to their own government and to the Italian government, while also lending to Italian households. The period budget constraints for the German households are:

$$C_1^* = Y_1^* - T_1^* - B^* - F^p - F^g \tag{3}$$

$$C_2^* = Y_2^* - T_2^* + (1+r_b^*)B^* + (1+r_f^*)(F^p+F^g) \tag{4}$$

where foreign variables are denoted by asterisks and defined analogously to the home variables. For example, B^* denotes lending by foreign (German) households to the foreign country (German) government, and r_b^* denotes the associated interest rate. F^p represents net borrowing by Italian households from German households, as defined above (negative levels of F^p denote net foreign lending by Italian households); F^g denotes borrowing by the Italian government from German households; and the associated interest rate from the perspective of German households, r_f^*, is assumed identical for these two cross-border financial activities. Note that in this two-country setup foreign borrowing by the Italian private sector from German households, F^p, represents lending by the German private sector, $-F^p$.

With perfect capital markets and no tax differentials, international capital arbitrage implies that the relevant interest rates faced by residents in the home and foreign countries will be equalized; that is, $r_b = r_f = r_f^* = r_b^*$. International capital controls, however, drive a wedge between these rates from the point of view of Italian households. In particular, we assume that

$$(1+r_f) = (1+r_f^*)/(1+u), \quad 0<u<1, \tag{5}$$

where u reflects the reduction in the return to Italian residents on lending abroad that arises from controls on capital *outflows*. These controls *reduce* the interest rate received by Italians on foreign lending below that paid by the German borrowers; that is, $r_f < r_f^*$. The reduction in the return to home households may be interpreted as arising from a combination of deadweight losses and taxes associated with the controls.[11] Such controls correspondingly imply that the interest rate *paid* by Italian residents on foreign borrowing will be below that received by German lenders.

We assume that controls affect only *international* capital flows, and that arbitrage continues to operate in domestic markets. Thus, interest rates within each country are equalized; $r_b = r_f$ and $r_b^* = r_f^*$, implying $r_b = r_f < r_f^* = r_b^*$. To simplify the notation in our subsequent analysis, we define $r = r_b = r_f$ and $r^* = r_f^* = r_b^*$.

The intertemporal consolidated present-value budget constraint for the household sector in each country may be obtained by dividing (2) and (4) by $1+r$ and $1+r^*$, respectively, and adding the resultant equations to (1) and (3), respectively:

$$C_1 + RC_2 = Y_1 + RY_2 - (T_1 + RT_2) \tag{6}$$

$$C_1^* + R^*C_2^* = Y_1^* + R^*Y_2^* - (T_1^* + R^*T_2^*) \tag{7}$$

where $R = 1/(1+r)$ and $R^* = 1/(1+r^*)$ are the period 1 present value factors. The intertemporal budget constraints in each country limit consumption by the difference between the discounted present value of output and taxes. Note that this specification implies that as long as the discounted sums of taxes, T_1+RT_2 and $T_1^*+R^*T_2^*$, remain unchanged, the timing of taxes does not influence private sector behavior.[12]

While government spending and taxes are given from the point of view of households, they are linked together through the requirement that the government be solvent. The government budget constraint requires that in each period government outlays be financed by taxes or by domestic foreign debt issue and that in the last period all debt be repaid without issuing new liabilities. Thus for the domestic country government,

$$G_1 = T_1 + B + F^g \tag{8}$$

$$G_2 = T_2 - (1+r)B - (1+r^*)F^g \tag{9}$$

where F^g denotes (Italian) government foreign borrowing in period 1. (Negative levels of F^g denote foreign public lending.) The Italian government borrows domestically at the domestic interest rate r. When borrowing abroad, however, it is assumed that it does not face the capital controls imposed on the private sector, and the relevant interest rate for domestic government borrowing abroad is the foreign interest rate r^*. Moreover, to simplify the analysis, it is assumed that the government is unable to extract by taxes any of the interest differential r^*-r associated with the domestic household sector's foreign borrowing and lending.

The domestic government's present-value budget constraint may be obtained analogously to that for the private sector. Dividing (9) by $1+r$ and adding the result to (8) gives:

$$G_1 + RG_2 = T_1 + RT_2 - uF^g \tag{10}$$

where use has been made of the result that (5) implies $R/R^* - 1 = u$. Although government spending, taxing, and financing decisions are all assumed exogenous, equation (10) makes clear that they are not independent of one another since the government's lifetime budget constraint must be satisfied. In the presence of controls on private sector capital outflows ($u > 0$), (10) implies that the present value of government revenue (discounted at the private home discount rate r) is reduced the greater is the extent of public foreign borrowing F^g. Intuitively, the existence of controls on capital outflows bottles up domestic funds and pushes down the home interest rate to a level below the foreign interest rate. Government borrowing (lending) abroad then implies a loss (gain) in revenue relative to borrowing (lending) domestically.

In the case of the foreign government, it is assumed that it borrows only from its local residents. Thus the single-period and present-value budget constraints abroad can be written as:

$$G_1^* = T_1^* + B^* \tag{11}$$

$$G_2^* = T_2^* - (1+r^*)B \tag{12}$$

$$G_1^* + R^*G_2^* = T_1^* + R^*T_2^* \tag{13}$$

Fully-informed, rational agents "see through" the government budget constraints, and recognize that the levels of government spending generate (implied) tax liabilities. Hence they incorporate the implications of the government budget constraints into their own budget constraints. The resulting consolidated budget constraint for the home country is obtained by substituting (10) into (6) and noting that (5) implies $R = R^*(1+u)$:

$$C_1 + RC_2 = Y_1 + RY_2 - (G_1+RG_2) - uF^g \equiv W \tag{14}$$

The righthand side of (14) may be interpreted as a measure of household wealth W, defined as the difference between the present value of output and taxes, discounted by the domestic interest rate, plus a term associated with the (exogenous) foreign financing actions of the home government.[13] The corresponding constraint for the foreign country is obtained analogously by substituting (13) into (7):

$$C_1^* + R^*C_2^* = Y_1^* + R^*Y_2^* - (G_1^*+R^*G_2^*) \equiv W^* \tag{15}$$

Several observations may be drawn from (14) and (15) concerning the effects of government policies on household wealth. First, observe from (14) that the home government's foreign financing actions, F^g, affect home household wealth because capital outflow controls reduce the foreign interest rate faced by households (r) below that faced by the government (r^*). This implies that borrowing abroad by the home government reduces its discounted revenue, increases its need for domestic financing of given public spending levels, and thereby lowers private sector wealth. Foreign lending by the Italian government has the reverse effect. This may be interpreted as an example in which capital controls break down the Ricardian equivalence between lump-sum taxes and foreign financing.[14] We shall see below that through this wealth effect government financing will influence household behavior and the real economic equilibrium of the home and foreign countries.[15]

Second, observe that given the pattern of government spending, Ricardian equivalence still holds between lump-sum taxes and domestically-issued public debt, neither of which enters into (14). Thus households do not perceive *domestic* public debt as affecting private wealth.[16] This implies that there is a distinction between government expenditures financed by taxes or domestically-issued public debt, on the one hand, and government expenditures financed by reduced (increased) foreign public lending (borrowing), on the other. Thus a switch from domestically-financed government expenditures to foreign-financed government expenditures will have real effects as long as capital controls exist.[17] Observe from (15) that in the foreign country where there are no such controls, only the present value of government expenditures matters.

Optimal Household Behavior and Equilibrium

The households in each country are assumed to maximize lifetime utility subject to the intertemporal budget constraints above, where lifetime utility is defined as:[18]

$$V = \ln C_1 + D\ln C_2 \tag{16}$$

$$V^* = \ln C_1^* + D^*\ln C_2^* \tag{17}$$

where $D = 1/(1+d)$, $D^* = 1/(1+d^*)$ denote subjective discount factors, and d, d^* denote the corresponding subjective rates of time preference.[19] The solution to this problem implies that the households in the two countries will choose intertemporal patterns of consumption which satisfy:

$$C_1/C_2 = (R^*/D)\,(1+u) \tag{18}$$

$$C_1^*/C_2^* = R^*/D^* \tag{19}$$

Thus the lower is the interest rate relative to the rate of social time preference (that is, the higher is R^*/D or R^*/D^*), the less is the incentive to lend, and the greater is the level of first period consumption relative to that in the second period. Note that capital controls, by restricting outflows and foreign lending by Italian households, also work to increase current relative consumption for the home country.

Optimization also requires that the economy-wide intertemporal budget constraints (14) and (15) be satisfied. Use of (18) and (19) along with these equations allows us to obtain

$$C_1 = \frac{W}{1+D}\,;\; C_2 = \left[\frac{D}{R^*(1+u)}\right]\left[\frac{W}{1+D}\right] \tag{20}$$

$$C_1^* = \frac{W^*}{1+D^*}\,;\; C_2^* = \left[\frac{D^*}{R^*}\right]\left[\frac{W^*}{1+D^*}\right] \tag{21}$$

The wealth coefficients represent the marginal (and average) propensities to consume out of wealth in each period. Observe that these propensities are all less than 1.

In equilibrium, the world supply of the single good is equal to the demand in each period. Thus, in period 1,

$$Y_1 + Y_1^* = \frac{W}{1+D} + \frac{W^*}{1+D^*} + G_1 + G_1^* \tag{22}$$

where W and W^* are given by the righthand sides of (14) and (15), respectively. An analogous condition defines equilibrium in period 2. It can be shown, however, that this condition is redundant.

By substituting the definitions of W and W^* into (22), we obtain an equation that relates the equilibrium foreign interest rate factor, R^*, to the government spending levels, G_t and G_t^*; home country foreign financing, F^g; output levels, Y_t and Y_t^*; the degree of home country capital controls, u; and the subjective time preference factors, D and D^*:

$$R^* = \frac{(Y_1-G_1)D(1+D^*) + (Y_1^*-G_1^*)D^*(1+D) + uF^g(1+D^*)}{(Y_2-G_2)(1+D^*)(1+u) + (Y_2^*-G_2^*)(1+D)} \tag{23}$$

The home interest rate factor follows immediately from (5) which implies $R = R^*(1+u)$.

We will discuss the determinants of interest rates below. Before doing so, we note that the home country's trade balance surplus in period 1, TB_1, is given by the difference between its output and absorption, $TB_1 = Y_1 - G_1 - C_1$.[20] Substituting with (14), (5), (20), and (23) yields the following expression:

$$TB_1 = \frac{(Y_2^*-G_2^*)[(Y_1-G_1)D+uF^g]-(1+u)(Y_1^*-G_1^*)D^*(Y_2-G_2)}{(Y_2-G_2)(1+D^*)(1+u)+(Y_2^*-G_2^*)(1+D)} \tag{24}$$

Observe that in the special case of balanced growth and fiscal spending across countries and time (that is, $Y_1-G_1 = Y_2-G_2 = Y_1^*-G_1^* = Y_2^*-G_2^* = Y-G$) and no capital controls ($u = 0$), equation (24) reduces to $TB_1 = (D-D^*)(Y-G)/(2+D+D^*)$ which is negative if $D < D^*$; that is, if $d > d^*$.[21] Thus the home country runs a trade deficit in the first period if it has a higher rate of time preference and is more "impatient" than the foreign country.

Effects of Reduced Capital Controls

We are now able to investigate how financial liberalization may change the impact of fiscal policy on key macroeconomic variables, particularly interest rates, consumption levels, and the trade balance. We will consider three ways in which fiscal policy might change: (a) a domestically-financed increase in government expenditures; that is, $dG_1 > 0$, $dF^g = 0$; (b) a change from domestic to international financing of a given level of government expenditure; that is, $dG_1 = 0$, $dF^g = -(dT_1 + dB) > 0$; and (c) an internationally-financed increase in government expenditures; that is, $dG_1 = dF^g > 0$, $dT_1 + dB = 0$. The results of these exercises are summarized in Table 3. Inspection of (23) and (24) indicates that the effects of exogenous domestic supply shocks ($dY_1 > 0$) are symmetrical to case (a). The analysis of the effects of foreign and future fiscal policy changes is similar, but is not presented.

a. Domestically-financed government expenditures

The multiplier effects of temporary fiscal policy changes on the equilibrium foreign interest rate $1+r^* = 1/R^*$ may be determined from (23). An increase in first-period government spending financed out of either taxes or domestically-issued bonds ($dG_1 = dT_1 + dB, dF^g = 0$) leads to a fall in R^* and a rise in r^*.[22] Intuitively, the increase in fiscal spending leads to an excess demand in the first-period goods market. To eliminate this excess demand, the relative price of first-period goods in terms of future goods, that is, the interest rate, must rise. Given u, the level of r rises as well.[23]

Observe from (20) and (21) that the resulting increase in r^* and corresponding decline in R^* imply substitution away from current consumption and towards future consumption in *both* countries. Thus an increase in first-period home government spending crowds out not only current domestic consumption, but also current foreign consumption. Part of the rise in domestic government spending is "financed" through the crowding out of foreign consumption. In an interdependent world, increased fiscal spending in one country is financed by higher interest rates and the crowding out of private spending in both countries. From (24), it may be discerned that even though home consumption is crowded out, on balance, the home country's trade balance worsens in response to the fiscal stimulus.

A decline in controls on capital outflows in the domestic country *diminishes* the effect of fiscal policy on the home country's interest rate r and *magnifies* the effect on the foreign interest rate r^*. Intuitively, diminished capital controls allow Italian residents greater access to the higher interest rates available abroad. This lessens the bottling up of domestic funds and allows domestic policies to have a smaller effect locally and a larger effect abroad. This implies that current domestic consumption will be crowded out less and foreign consumption more in response to domestic fiscal stimulus as capital controls are lowered. Note also that because the home country's current level of consumption falls less, the fiscal expansion leads to a greater decline in the trade balance.

Table 3

Fiscal Policy Multipliers and Effects of Increasing Financial Liberalization on:[1]

	r^*	r	C_1^*/C_2^*	C_1/C_2	C_1^*	C_1	TB_1
a. Domestically-Financed Government Expenditure Increase	+	+	−	−	−	−	−
Effect of Increased Liberalization	+	−	+	−	+	−	+
b. Shift from Domestic to International Public Financing	−	−	+	+	+	−	+
Effect of Increased Liberalization	−	−	−	−	−	−	−
c. Internationally-Financed Government Expenditure Increase	+[2]	+[2]	−	−	−	−	−
Effect of Increased Liberalization	+	−	+	−	+	−	+[2]

Notes: [1]The first line of each case gives the sign of the fiscal policy multiplier. The second line gives the effect of a decline in u on the absolute value of the corresponding multiplier. These signs assume, where necessary, $Y_1 - G_1 = Y_2 - G_2 = Y_1^* - G_1^* = Y_2^* - G_2^* = Y - G, F^g > 0$, and $(Y - G)R^* - F^g > 0$.
[2]Assumes $D > u$.

b. Shift from domestic to international public financing

Next, we consider the effect of a switch in the financing of given levels of current fiscal spending from domestic to foreign sources ($dG_1 = 0, dF^g = -(dT_1 + dB) > 0$); that is, an increase (decrease) in public borrowing (lending) abroad. Because the government faces a higher interest rate abroad than do domestic residents, such a shift in financing will have an effect on real behavior.

In particular, it may be shown that increased public foreign borrowing leads to a decline in r^* and r. The reason is that the increase in borrowing creates a negative domestic wealth effect since the private sector perceives that the government will need to raise taxes to offset the greater cost associated with borrowing at the relatively higher foreign interest rate. (See equation (14).) The fall in wealth, in turn, implies households will reduce their current consumption, borrow less and/or lend more abroad, thereby pushing down interest rates in both countries. Hence increased government foreign borrowing crowds out private foreign borrowing.

With declining interest rates, both countries will increase first period consumption relative to second period consumption. However, because domestic households experience a wealth loss directly proportional to the extent of government foreign financing, the absolute level of consumption falls in the home country, and its trade balance improves correspondingly.

Increased integration diminishes the loss in wealth, the channel through which government foreign financing actions affect private behavior. Correspondingly, the declines in r^* and r and the effects on both domestic and foreign macro aggregates are reduced. In the absence of capital controls, Ricardian equivalence between foreign and domestic financing holds, and there is no effect at all.[24]

c. Internationally-financed government expenditures

An internationally-financed increase in government expenditures, that is, $dG_1 = dF^g > 0$, $dT_1 + dB = 0$, represents the combination of the previous two cases. The rise in government expenditures causes r^* and r to rise. The increases in interest rates are dampened, however, by the adverse wealth effects of public foreign financing. The net effects of the fiscal stimulus are qualitatively the same as with domestically-financed government expenditures when the wealth effects associated with the decline (rise) in foreign lending (borrowing) are not too large.[25] Thus, with the existence of capital controls ($u > 0$) the domestic and foreign interest rate are both *less* sensitive to internationally-financed temporary changes in home country government spending. That is, an internationally-financed increase in home country government spending leads to smaller rises in r^* and r than for a domestically-financed increase. The reason is that, as noted above, when government spending is financed by more foreign public borrowing, home households perceive a fall in wealth. The resulting decline in consumption lessens the pressure on interest rates.

Thus the larger the share of home country government spending that is financed internationally, the *less* is the effect on foreign activity. This dampening of the transmission effect to foreign economies associated with the method of financing fiscal spending is dependent on the presence of capital controls. As integration increases, then, the transmission of changes in home country fiscal policy increases.[26]

The analysis indicates that reduced restrictions on capital flows increase the transmission of disturbances, such as fiscal policy shifts, across national borders. Domestic disturbances have smaller effects domestically, and larger effects on foreign economies. Conversely, foreign shocks have larger impacts on the domestic economy.

III. Conclusions

A major finding of our analysis is that, with greater financial integration, a given domestic fiscal expansion (or adverse supply shock) will place less upward pressure on domestic interest rates and more upward pressure on foreign interest rates. Correspondingly, current domestic consumption will be crowded out by less and foreign consumption by more in response to domestic fiscal stimulus as capital controls are lowered. The reason here is that the closer linkage of the foreign and the domestic financial markets in effect "spreads" more of the effect of fiscal stimulus internationally.

Our analysis thus sheds light on the theoretical circumstances under which divergent fiscal policies may have larger disruptive effects as international capital mobility increases. Up to this point, the rise in the degree of capital mobility in Europe over the past decade seemingly has not contributed to real instability associated with divergences in fiscal policy.[27] However, it is possible that further liberalization measures combined with a different pattern of fiscal disturbances within the EC could generate greater instability than has been observed so far. In particular, eliminating controls on capital movements will further increase the degree of linkage among the EC economies. Fiscal actions in one EC nation will be felt by its neighbors more than before. A given fiscal stimulus or contraction will have larger international repercussions in this new environment, and recent proposals to limit the magnitude of budgetary divergences may be viewed as an attempt to limit these transmission effects.

NOTES

1. The so-called Delors report, released in April 1989, outlines the specific steps required to achieve the "final stage" of economic and monetary union in Europe. With respect to macroeconomic policy coordination, the final stage of economic and monetary union envisioned involves permanently fixed exchange rates and possibly, though not necessarily, a single EC currency. It also recommends the setting of a Community-wide fiscal policy position and close coordination of national budgetary policies. Specifically, the report recommends "binding rules" be adopted: (i) to impose effective upper limits on budget deficits of individual member countries, (ii) to strictly limit monetary finance of budget deficits, and (iii) to limit external borrowing in non-EC member country currencies.

2. Key (1989) provides an excellent summary of the institutional features of the international liberalization of capital movements in Europe and the changes planned. Our descriptive material in this section draws on his article.

3. See Giavazzi and Giovannini (1986) for the role of capital controls in the workings of the European Monetary System.

4. Frankel and MacArthur (1988) find a bias against capital outflows in Italy until 1983, a neutral effect in 1984, and a bias effectively working to limit capital inflows in 1985 (i.e., Italian covered interest differentials were significantly positive). The latter may be attributable to the temporary constraint on capital inflows during this time: banks in Italy were not allowed to increase their net debtor foreign position from the second half of 1984 to December 1985. Barone, *et. al.* (1989) provide a useful description of the capital controls in effect in Italy during the 1980s.

5. Although a new foreign exchange law in Italy enacted in October 1988 liberalized the controls on capital flows, restrictions still remain. (See Barone, *et. al.,* 1989.) For example, Italian residents still are not allowed to hold funds in bank accounts abroad, and non-bank residents are not permitted to extend credit lines to non-residents or purchase money market instruments abroad with a maturity of less six months. These residual controls should be eliminated by the middle of 1990 with the implementation of the EC Directive on the liberalization of short-term capital flows.

6. For Spain, Ireland, Portugal, and Greece the deadline for removal of restrictions on capital movements is 1992, with a possible extension for Portugal and Greece.

7. Several "safeguard" measures have been adopted in tandem with the process of liberalization of capital movements. These include a loan facility for member countries with balance-of-payments difficulties and a clause allowing the reimposition of capital controls in the event of exchange crises. These measures have been put into place not so much because of a concern that surges in capital outflows would follow as a consequence of lifting the few remaining restrictions, but primarily because it is believed that exogenous shocks—one of which may be divergent fiscal positions—will lead to balance-of-payments and exchange crises in a fully deregulated environment.

8. According to national sources, in 1987 the Italian private and Italian public sectors (excluding the monetary authorities) had net foreign liability positions of \$37.6 billion and \$35.9 billion, respectively.

9. Another motivation is the desire to maintain domestic monetary control simultaneously with fixed exchange rates. Large government debt issues may place pressure for monetization on the central bank, which in turn may be forced to impose capital controls to maintain the exchange rate objective.

10. A number of papers have examined international aspects of fiscal policies in models in which agents' intertemporal objectives and constraints are explicitly modelled. See Frenkel and Razin (1985, 1987), Djajic (1985), and Greenwood and Kimbrough (1985), among others.

11. In the case of controls on capital inflows, the analogous condition to (5) would be $(1+r_f^*)(1+u_b) = 1 + r_f$, where u_b reflects the added cost to Italian residents from borrowing abroad.

12. Also, the intertemporal budget constraint implies $TB_1 + RTB_2 = 0$, that is, the discounted sum of trade balance surpluses must equal the sum of the inherited initial debt, which is zero in this model. Thus a trade deficit in the first period must be followed by a surplus in the second.

13. Note that the definition of home country household wealth in (14) discounts future output and government expenditures at the domestic interest rate. Use of the relation $R = R^*(1+u)$ allows equation (14) to be rewritten as $C_1 + RC_2 = Y_1 + R^*Y_2 - (G_1 + R^*G_2) + u[R^*(Y_2-G_2)-F^g] \equiv W$. This representation of the consolidated home country budget constraint separates household wealth into a component that discounts future output and government expenditures at the foreign interest rate—what may be interpreted as the "true" or shadow interest rate for home households—and a component associated with the direct effects of capital controls on household wealth.

14. Greenwood and Kimbrough (1985) obtain a similar nonequivalence result, although in their model capital controls take the form of quantitative restrictions on capital flows.

15. One possible extension of the model is to assume that capital controls take the form of an explicit tax on capital outflows and that the government can extract at least a fraction of the interest differential r^*-r associated with household foreign lending, F^p. It can be shown that if the home government fully extracts this differential without any deadweight losses and if rational households fully internalize the effect of the controls on their wealth, F^g will not matter.

16. A number of papers have modelled the circumstances under which the nonequivalence between taxa-

tion and domestic bonds breaks down in an international setting. For example, Frenkel and Razin (1987, Chapter 11) develop a two-country version of Blanchard's (1985) uncertain-lifetime setup in which the relevant household discount rate is below that of the infinitely-lived government. Obstfeld (1989) analyzes the long-term dynamics of fiscal policy in a model with economic growth. In his paper, nonequivalence between domestic debt and taxation arises because new households are assumed to be unconnected with existing households. Since current debt holders do not value the consumption of unborn taxpayers, a fraction of public debt is perceived as net wealth by existing households.

17. Greenwood and Kimbrough (1985) make a similar point.

18. We do not directly analyze government optimization decisions; hence we ignore the problem of the time inconsistency of government policies.

19. The results would not be affected by including government spending levels in these utility functions as long as preferences for the privately- and publicly-provided goods were separable.

20. This relation is consistent with the summing of equations (1) and (8), which implies $Y_1 - C_1 - G_1 = -(F^p + F^g) = TB_1$, that is, national saving equals the capital account deficit, which, in turn, equals the current account surplus.

21. With capital controls, (24) reduces to $TB_1 = [(D-D^*)(Y-G) - u((Y-G)D^* - F^g)]/[2+D+D^* + u(1+D^*)]$. In this case the condition for $TB_1 < 0$ is $(Y-G)[D-D^*(1+u)] + uF^g < 0$.

22. An increase in current foreign fiscal expenditures such that $dG_1^* = dT_1^* + dB^*$ has the same effect on r^*. An increase in second period fiscal spending in either country has the opposite effect.

23. In our benchmark model, output levels in the two periods are assumed fixed and given by endowments. Extending the model to allow real investment provides a richer "supply side" to the model by causing output growth to become endogenous. This would focus attention on production opportunities of each economy, as government policies influence private investment decisions and hence the future capital stock and output potential. This supply mechanism generally dampens the effects of such exogenous changes as stimulatory fiscal policy on interest rates. In addition, it implies that the net impact of fiscal stimulus on aggregate income and consumption could be positive, as suggested by typical Keynesian models.

24. In this analysis the level of government financing (F^g) is treated as an exogenous variable. In addition, it has been assumed that the domestic government is unable to extract any of the interest differential between domestic and foreign interest rates through taxes. Relaxing this assumption could create an incentive for the government to exploit the corresponding arbitrage opportunity by borrowing less abroad, where interest rates are higher, and more at home. Such an analysis would necessitate extending the model by specifying a government objective function and determining optimal government behavior.

25. A sufficient condition is $D > u$.

26. Our basic model focuses on the intertemporal terms of trade—the real interest rate—as a central component in the transmission mechanism of fiscal policy. Introducing non-tradable goods focuses attention on the intratemporal terms of trade, that is the real exchange rate, defined as the inverse of the relative price of non-tradable goods to tradable goods. In this case the effects of government spending depend on the commodity composition and time pattern of the spending. See Chapter 9 of Frenkel and Razin (1987) for a detailed exposition of the effects of fiscal policy in a two-country, two-period model with tradable and non-tradable goods.

27. Tanzi and Ter-Minassian (1987) discuss in detail the extent to which monetary and fiscal policies in the EC members of the European Monetary System (EMS) have tended to converge. Tanzi and Ter-Minassian argue that the discipline associated with nearly fixing exchange rates (despite the fact that there have been eight EMS realignments since its inception resulting in a 27 percent cumulative appreciation of the DM against other EMS currencies) has been partly responsible for a convergence in monetary policies and hence inflation rates. The convergence of monetary policies has not been matched by a convergence of fiscal policies, however.

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Capital Market Efficiency: An Update*

Stephen F. LeRoy

Professor, University of California, Santa Barbara, and Visiting Scholar, Federal Reserve Bank of San Francisco. Members of the editorial committee were Adrian Throop, Michael Keeley, and Jonathan Neuberger.

Statistical evidence accumulated in the 20 years following Eugene Fama's (1970) survey raises questions about his conclusion that capital markets are efficient. Stock price volatility has been shown to exceed the volatility consistent with capital market efficiency. Other evidence —for example, the small-firm effect, the January effect, and other calendar-based anomalies of stock prices—points in the same direction. Finally, analysts find it difficult to explain stock prices even after the fact using realized values of variables which, according to efficient capital markets theory, should account for stock price changes.

Economist 1: *"That looks like a $100 bill over there on the sidewalk."*

Economist 2: *"Don't bother going over to check it out. If it were genuine, someone would have picked it up already."*

The theory of efficient capital markets says, most simply, that the prices of financial assets equal the discounted value of the expected cash flows these assets generate. In the context of the stock market, efficiency implies that stock prices equal the discounted value of expected future dividends. Investors are not assumed to form perfectly accurate forecasts of future dividends, but they are assumed to make effective use of whatever information they have. If capital markets are efficient in this sense, changes in stock prices should be associated exclusively with new information leading to revisions in expected future dividends: when dividend prospects improve, stock prices rise, and conversely.

Moreover, since all relevant, publicly available information is discounted in asset prices as soon as it becomes available, investors cannot construct systematically profitable trading rules based only on this information. Thus, in an efficient market there is no motive to buy stock based on favorable information; if the information is in fact favorable, the market already has discounted it. In other words, the $100 bill above could not be genuine; otherwise, it would have been picked up already.

These observations suggest that factors not identifiable with future profitability—fads, nonrational speculative bubbles, investor psychology—should not affect stock prices. In this regard, the stock market selloff on October 19, 1987, offers dramatic evidence that capital markets may not be efficient. On that single day, stock values declined by approximately a half trillion dollars, a magnitude unprecedented in absolute terms. In relative terms the selloff was comparable only to the stock market panic of October 1929 which heralded the Great Depression.

According to the efficient markets theory, the selloff could have been caused only by information made available that day (or over the preceding weekend since October 19, 1987, was a Monday) that justified a downward revision on the order of 22 percent in the present discounted value of expected future dividends. However, no economic information of an even mildly unusual nature was made public that day, let alone information that would drastically increase investors' estimates of the probability of an impending economic cataclysm. It is true that investors were worried about recession, but no more than they usually are. In any event, whatever fears of recession investors had subsequently proved unfounded, as the economy showed virtually no ill effects following the stock market collapse.

Moreover, the partial recovery of stock prices in the days following the selloff can only be reconciled with the efficient markets model if the recovery could be associated with economic news inducing investors to believe that the impending recession would, after all, not be as severe as the news that led to the selloff had indicated. Again, however, no economic news of the requisite importance was reported during the week of October 19.

This is not to say that stock price changes on the order of ten or twenty percent, even over a period as short as several days, are never associated with changes of commensurate magnitude in fundamentals. Following the June 1989 suppression of student protests in China, stock prices in Hong Kong dropped by a magnitude comparable in relative terms to the U.S. selloff in October 1987. The connection between political conditions in China and the role of Hong Kong firms in the Chinese economy is so strong that a stock price change on the order of twenty percent is not an obviously disproportionate response to the news that the Chinese government opted to suppress rather than accommodate the liberalization that the students were advocating. Therefore, there is no clear conflict between market efficiency and the selloff that occurred on the Hong Kong exchange in June 1989.

A single dramatic event like the October 19, 1987, selloff, however, does not invalidate the most important prediction of the efficient markets theory, which is that there should not exist trading rules that allow investors systematically to outperform the market. Research conducted in the 1960s and reported in Fama (1970) generally supported this implication, leading financial economists to conclude that capital market efficiency was corroborated empirically.

The more recent evidence, however, does not substantiate Fama's verdict. Detailed analysis using financial data bases developed in the 1970s, and drawing on a more extensive understanding of the empirical implications of market efficiency than was available in 1970, suggests that the October 19, 1987, selloff was not an isolated episode (although, of course, it was virtually unprecedented in magnitude). Instead, the evidence now suggests that most fluctuations in stock prices cannot be traced to changes in rational forecasts of future dividends, contrary to the prediction of the efficient markets model.

The new evidence arises from two areas of research which developed largely independently. First, analysts realized about fifteen years ago that market efficiency implied an upper bound on the volatility of stock prices. Empirical tests suggest that this bound is violated, indicating that stock prices are more variable than is consistent with market efficiency. Second, beginning about the same time analysts came to realize that stock returns display a variety of systematic patterns that are difficult to explain within a framework of rational optimization. The "variance-bounds" and "anomalies" literatures are surveyed in this paper.

Some economists view the updated evidence on market efficiency as demonstrating that the theory of efficient capital markets is wrong, and that investors are simply not as rational as efficient markets theory assumes. If so, it follows that capital markets are probably not doing a good job of resource allocation. Most economists, however, start out with a strong commitment to the assumption that people act rationally, and these economists will not reject the efficient markets model—and with it, the presumption that capital markets are doing a reasonably good job of allocating capital—unless confronted with absolutely airtight evidence against efficiency. None of the evidence reported in this paper meets such an exacting standard. Therefore those who start out with a strong predisposition in favor of capital market efficiency interpret the recent evidence as perhaps raising questions about the theory and suggesting topics for future research, but not as justifying definitive rejection.

I. The Efficient Markets Model

Contrary to the impression given above, the efficient markets model does not start out assuming that asset values equal the present value of expected future cash flows. Rather, the present-value representation is derived from the more primitive assumption that the rate of return r_{jt} on the j-th stock (more generally, the j-th asset) satisfies:

$$E(r_{jt} \mid I_t) = \rho \tag{1}$$

Here I_t comprises investors' information at t; $E(.|I_t)$ denotes the mathematical expectation of (.) conditional on I_t; ρ, the expected rate of return on stock, is a positive constant, on the assumption that capital markets are perfect and investors are risk-neutral. Equation (1) says that an investor with information I_t will predict an expected rate of return equal to ρ for any asset. Since this is the same prediction that an uninformed investor would make, the efficient markets model implies that the information set I_t is useless in predicting expected rates of return. In this sense information I_t is "fully reflected" in securities prices.

For example, suppose that I_t contains the history of dividends, earnings, sales, advertising outlay, and costs for firm j up to date t, and possibly also macroeconomic variables like GNP, interest rates, commodity prices, and the money stock. Equation (1) says that no matter what values the variables in I_t take on, asset prices will depend on these values in such a way that the expected rate of return on the j-th asset is always ρ. If so, an investor who knows dividends, earnings, and so on is no better off than an investor who does not know the past history of these variables since the uninformed investor can always predict an expected rate of return of ρ without knowing I_t and is assured that his prediction will coincide with that of the informed investor, who predicts an expected rate of return of ρ for all values of I_t.

If at each date the expected rate of return on each asset is ρ, it follows that the expected rate of return on any portfolio is also ρ, since the expected rate of return on a portfolio is just a weighted average of the expected rates of return on its component securities. Accordingly, no trading rules based on information I_t can generate an expected rate of return greater than ρ. Of course, an investor in possession of information better than "the market's" information I_t could use this information to detect differentials in expected rates of return among the various assets, and consequently could construct profitable trading rules. However, efficient markets theory postulates that there do not exist investors with information better than the market's information, or more realistically, that if such investors exist, they do not affect prices.

Fama (1970) distinguished three versions of market efficiency depending on the specification of the information set I_t. Markets are "weak-form efficient" if I_t comprises past returns alone, "semi-strong-form efficient" if I_t comprises all publicly available information, and "strong-form efficient" if I_t includes insider information as well as publicly-available information.[1] It is clear that strong-form efficiency implies semi-strong form efficiency, which in turn implies weak-form efficiency, since expected returns that cannot be predicted based on a large information set surely cannot be predicted based on a small information set that is contained in the large information set. However, the reverse implications do not follow; a capital market easily could be weak-form efficient but not semi-strong-form efficient, or semi-strong-form efficient but not strong-form efficient.

The efficient markets model (1) says that rates of return on stock are unpredictable. It might appear to follow that the efficient markets model implies that stock prices are completely without structure, but that is not the case. In fact, the efficient markets model turns out to be exactly the same model as the present-value relation with which the efficient capital markets model was identified in the introduction. The derivation of this equivalence follows. Because (one plus) the rate of return is by definition equal to the sum of the dividend yield (d_t/p_t) and the rate of capital gain (p_{t+1}/p_t), (1) can be rewritten as:

$$p_t = \frac{E(d_{t+1} + p_{t+1} \mid I_t)}{1 + \rho} \tag{2}$$

Substituting $t+1$ for t, (2) becomes:

$$p_{t+1} = \frac{E(d_{t+2} + p_{t+2} \mid I_{t+1})}{1 + \rho} \tag{3}$$

Using (3) to eliminate p_{t+1} in (2), the price of stock can be written:[2]

$$p_t = \frac{E_t(d_{t+1})}{1 + \rho} + \frac{E_t(d_{t+2} + p_{t+2})}{(1 + \rho)^2} \tag{4}$$

Here $E_t(.)$ is used as an abbreviated notation for $E(.|I_t)$. Proceeding similarly $n-1$ times, there results:

$$p_t = \frac{E_t(d_{t+1})}{1 + \rho} + \frac{E_t(d_{t+2})}{(1+\rho)^2} + \ldots + \frac{E_t(d_{t+n-1})}{(1 + \rho)^{n-1}} + \frac{E_t(p_{t+n} + d_{t+n})}{(1 + \rho)^n} \tag{5}$$

Assuming that $(1 + \rho)^{-n} E_t(p_{t+n})$ converges to zero as n approaches infinity, (5) becomes the familiar present-value equation:

$$p_t = \frac{E_t(d_{t+1})}{1 + \rho} + \frac{E_t(d_{t+2})}{(1 + \rho)^2} + \frac{E_t(d_{t+3})}{(1 + \rho)^3} + \dots . \tag{6}$$

Further, the proof is completely reversible, implying that if the present-value relation (6) is satisfied, so is the efficient markets model (1). Samuelson (1965, 1973) and Mandelbrot (1966) were the first to state this result and to point out its relevance to efficient-markets theory.

What is striking here is that even though dividend changes in (6) can be partly forecast, the generating equation (1) implies that rates of return cannot be forecast. For example, if "the market" expects dividends to rise, the price of stock will be high relative to dividends now, so that when dividends do rise, no extra-normal return will be generated. Stockholders will earn extra-normal (sub-normal) returns only if dividends increase more (less) than had been expected. Thus if capital markets are efficient, a general expectation of a dividend increase does not imply that stocks should be bought (or, for that matter, sold), since the expected increase is already reflected in market prices.

This similarity between the efficient markets model and the "fundamentalist" model means that the much-publicized feud between Wall Streeters, who analyze stocks by computing discounted cash flows, and efficient marketers, who believe that rates of return cannot be forecast, is largely based on misunderstanding. The fundamentalist model focuses on the predictable part of prices, whereas the efficient markets model focuses on unpredictable returns, but the mathematical equivalence between the two models guarantees that there is no inconsistency.

However, the dispute is not entirely without substance: fundamentalists do not assert that prices are exactly equal to the discounted value of future dividends, but rather that prices fluctuate around the discounted value of future dividends. This apparently trivial difference is essential, since only in the latter case can profits be made by buying stocks that are priced lower than fundamentals justify, and selling stocks that appear to be overpriced. If underpriced and overpriced securities do not exist, as advocates of the efficient markets model maintain, then such trading strategies cannot succeed.

In deriving the expected present-value equation (6) from the efficient capital markets model (1), it was necessary to assume that $(1 + \rho)^{-n} E_t(p_{t+n})$ converges to zero as n approaches infinity. This convergence assumption means that price is expected to grow more slowly than the rate at which future returns are discounted. Violation of the convergence assumption would mean that there exist speculative bubbles: even though price exceeds the discounted value of expected dividends, investors are willing to hold stocks because they anticipate that price will exceed expected dividends by an even wider margin in the future.

It is known that, in theory, speculative bubbles can exist even in simple models in which agents are assumed to be rational and to have identical preferences and endowments, and in which there is no uncertainty (Gilles-LeRoy 1989). In such countries as Japan, where stocks routinely trade at prices 50 times earnings (although such figures are difficult to interpret because accounting practices are different in Japan from those in the U.S.), it is plausible that speculative bubbles are an important determinant of stock prices. However, the same is probably not true of the U.S., where stocks trade at price-earnings multiples on the order of 10 or 15. It is not easy to devise empirical tests which can reliably detect the presence of bubbles. However, one particularly simple kind of bubble would, if it occurred, result in a sustained downward trend in the dividend-price ratio as stock prices rose without limit. Data for the dividend-price ratio in the U.S. do not display any downward trend. The absence of trend in the dividend-price ratio led West (1988a), for example, to conclude that speculative bubbles are probably not an important component of U.S. stock values.

The expected present-value model often strikes people as highly implausible. Many investors do not even consider dividend levels in their investment decisions. Instead they buy stocks that are believed likely to appreciate. Further, the stocks of many firms which do not pay, and have never paid, dividends command high prices. The proposition that rates of return cannot be forecast, on the other hand, is very appealing: the negation of (1) has the unattractive implication that there exists some information variable known to investors which they can use to construct systematically profitable trading rules. Yet the mathematical equivalence of (1) and (6) (granted the convergence condition just discussed) means that it is logically inconsistent to reject the expected present-value model while at the same time accepting the unpredictability of rates of return.

If the reasonableness of (1) is accepted, it follows that the objections to the logically equivalent (6) cannot be as compelling as they appear at first. It is perfectly natural that investors might exhibit greater awareness of capital gains than dividends, given the greater variability and unpredictability of capital gains. Although most investors do not think much about dividend yields, the hypothesis that capital gains reflect changes in dividend prospects nonetheless still holds. Also, whether a given firm has paid dividends in the past is irrelevant. What is relevant is the firm's capacity to pay dividends in the future, which is governed by the firm's earnings prospects. The expected present-value equation (6) says only that the value of a firm that investors were absolutely certain would never pay dividends in the future (even a liquidating dividend if the firm were to disband or merge into another firm) would be zero.

II. Market Efficiency and Its Implications for Volatility

The October 19, 1987, episode was not the first time stock prices had dropped sharply in the apparent absence of news of commensurate importance bearing on dividend prospects. October 19 was typical of major stock price changes in this respect, not exceptional: most stock price changes, major or minor, cannot convincingly be associated with contemporaneous changes in investors' expectations of future corporate profits (Cutler, Poterba, and Summers, 1987). To the extent that stock prices frequently fluctuate in response to variables unrelated to dividend prospects, stock prices in some sense should be more volatile than is consistent with market efficiency. This consideration led analysts to ask whether market efficiency could be shown formally to have the implication that stock price volatility should be lower than the volatility of dividends, and if so how this prediction could be tested.

Proponents of market efficiency were skeptical of this approach. They argued that since efficiency implies that prices respond instantaneously to new information, stock price volatility cannot be deemed in any sense "excessive." However, because market efficiency has been shown to imply that stock prices equal the discounted sum of expected future dividends, stock prices will behave like a weighted average of dividends over time, and an average is always less volatile than its components.[3] There is no contradiction, then, between the requirement that stock prices respond quickly to new information and the implication that the volatility of prices is related to that of the underlying dividends stream.

Results of tests of the implications of market efficiency for stock price volatility were circulated in 1975 in my paper with Richard Porter (published in 1981). The timing, incidentally, was not coincidental—our thinking on this topic was prompted by the 1974-1975 stock market drop, the most pronounced in the postwar U.S. economy up to that time. Robert Shiller reported similar volatility results in his 1979 and 1981 papers. These papers used different analytical methods, but the results were the same: stock price volatility is too great to be consistent with market efficiency.

These papers alleging excess volatility of asset prices were well-received by economists sympathetic to the idea that asset price changes are not closely linked to changes in the expected discounted value of the cash flows to which these assets give title. However, defenders of the efficient markets model were motivated to search for statistical problems with the specific econometric procedures used in the initial papers. They found several serious biases, all of which predisposed the tests to reject market efficiency. The most important papers here are Flavin (1983) and Kleidon (1986). At the same time, new volatility tests were being devised which were free of the biases that attended the initial tests (West, 1988; Mankiw, Romer, and Shapiro, 1985; Campbell and Shiller, 1988a, 1988b; and LeRoy and Parke, 1990). These new tests continued to indicate that asset prices are excessively volatile, although perhaps not by as great a margin as the initial tests suggested.

Lawrence Summers has likened the findings of the volatility tests to that of the statistical tests for a link between smoking and lung disease. Early tests indicating the presence of such a link were found to be contaminated by statistical problems which biased the outcome toward that finding. Nevertheless, subsequent tests, which were free of statistical bias, continued to support the original conclusion of a statistically significant link, although the link was shown not to be as strong as had first been thought.

The volatility test reported below, which is very simple and yet appears econometrically sound, is drawn from LeRoy and Parke (1990). Recall that the efficient markets model says that stock price equals the discounted value of expected dividends:

$$p_t = \frac{E_t(d_{t+1})}{1+\rho} + \frac{E_t(d_{t+2})}{(1+\rho)^2} + \frac{E_t(d_{t+3})}{(1+\rho)^3} + \dots \quad (6)$$

Because there is no direct way to measure investors' information, direct observation of $E_t(d_{t+1})$, $E_t(d_{t+2})$, . . . , is not possible. This greatly complicates the derivation of the implications of market efficiency for price volatility. However, it is possible to show that the less information investors have, the higher will be the variance of the rate of return (LeRoy, 1989). Consequently, assuming markets are at least weak-form efficient, so that investors' information includes at least past returns, puts a lower bound on the amount of information investors have, therefore implying an upper bound on the variance of the rate of return.[4]

To derive the upper bound on the variance of the rate of return, it is necessary to evaluate this variance when investors predict future dividends using no information other than past returns. It is assumed that dividends follow a geometric random walk:

$$d_{t+1} = d_t \epsilon_{t+1} \tag{7}$$

where the ϵs are constant-mean random variables distributed independently over time. Analysts disagree about the accuracy of the geometric random walk specification. Some evidence shows it to be surprisingly accurate for such a simple specification, while other evidence suggests that in some contexts the geometric random walk specification can be misleading. For the present purpose the most attractive feature of the geometric random walk is its simplicity, which allows a very intuitive development of the variance-bounds relations. More complex characterization of dividend behavior, while allowing greater accuracy, would necessarily complicate the discussion by requiring use of more general analytical methods (Campbell-Shiller, 1988, 1988a).

When markets are at least weak-form efficient the upper bound on the variance of the rate of return on stock is the variance that would occur if investors based their dividend forecasts on past dividend behavior and nothing else. In this case the geometric random walk model implies that the best guess about future dividends is that they equal current dividends, multiplied by a trend term which depends on the mean value of ϵ. Therefore price will be given by a constant markup applied to current dividends:

$$p_t = k\, d_t \tag{8}$$

If price is proportional to dividends, the rate of return will equal the dividend growth rate multiplied by a constant which is very near one. To see this, recall the definition of the rate of return r_t as the dividend yield plus the rate of capital gain:

$$r_t = \frac{d_{t+1} + p_{t+1}}{p_t}\ . \tag{9}$$

Substituting $p = k\, d_t$ and $p_{t+1} = kd_{t+1}$ into (9) and using (7), we have

$$r_t = \left(\frac{k+1}{k}\right)(1 + \epsilon_{t+1}) - 1. \tag{10}$$

Because k, the price-dividend ratio, is on the order of 25, the multiplicative constant $(k+1)/k$ is not far from one, and therefore can be ignored. Thus the rate of return approximately equals the dividend growth rate, and the variances of these variables are approximately equal also.

In sum, this decreasing relation between investors' information and return volatility implies that if capital markets are at least weak-form efficient (and if dividends follow a random walk) the variance of the rate of return on stock cannot be greater than the variance of the dividend growth rate.

III. Empirical Results

Chart 1 shows the Standard & Poor's stock price index from 1926 to 1985, adjusted for inflation in commodity prices using the producers' price index. As expected, real stock prices display a pronounced upward trend over time, reflecting corporate retained earnings and, to a lesser extent, new equity issues. A very striking observation from Chart 1 is that stock price volatility has decreased between the 1930s and the 1980s. The decline from 1929 to 1932, the rise in the mid-1930s, and the decline in the years just before World War II were much more pronounced than any change occurring between World War II and the mid-1970s. This decreasing volatility of stock prices goes contrary to a common impression that stock market volatility has increased in recent decades. Another observation is that the October 19, 1987, selloff appears in Chart 1 as only a minor drop at the end of the period, rather than as the cataclysm it in fact was. The reason is that it came after nine months of rapid gains in stock prices, so that annual data show only a small drop from 1986 to 1987.

Chart 1
Standard And Poor's Stock Price Index

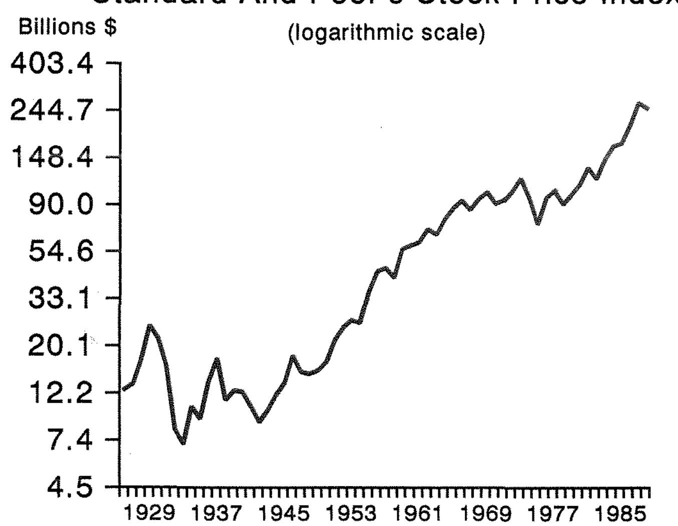


Chart 2 displays a simulated rate of return series that is representative of the pattern that would be expected under weak-form market efficiency. To generate the artificial stock prices on which the returns in Chart 2 were based, investors were arbitrarily assumed to be able to forecast dividends with perfect accuracy five years into the future. Beyond that horizon, however, they were assumed to have no information at all. Therefore they were assumed to extrapolate dividends using a constant growth rate, as implied by the geometric random walk. As would be expected in an efficient market, rates of return were higher than normal in years preceding dividend growth that was higher than normal, and lower than normal in years preceding low dividend growth. However, the relevant observation is that the rate of return has lower volatility than the dividend growth rate, conforming to the implication of market efficiency outlined above.

Chart 3 is similar to Chart 2 except that the actual rate of return on stock, rather than the simulated return based on market efficiency, is shown. Several aspects of this diagram are surprising. Most striking is the decrease in the volatility of both the rate of return on stock and the dividend growth rate from the 1930s to the 1980s. This decline in stock price volatility was noted in the discussion of Chart 1. Chart 3 makes clear that the decline in the volatility of dividend growth is even more pronounced than that in return volatility. However, for the purpose of testing the volatility implications of market efficiency, the relevant observation is that over the postwar period the rate of return on stock was much more variable than the dividend growth rate (in the prewar period the difference is not nearly as great). This result is inconsistent with the stock market being weak-form efficient.

The volatility test just presented was chosen because it is easy to motivate intuitively. Because the test depends on strong simplifying assumptions, it may be that the finding of excess volatility arises from a violation of these assumptions rather than of market inefficiency. For example, without the simplifying random walk assumption, it is not necessarily true that the variance of the growth rate of dividends is an upper bound for the variance of the rate of return. Equally important, the version of the expected present-value model used to derive the volatility test incorporated the assumption that the discount rate is constant at ρ. Changing real interest rates over time are therefore a conceivable alternative to market inefficiency as a cause of the apparent excess volatility. However, both of these possibilities have been explored extensively in the variance-bounds literature, and so far, it appears that allowing for these more general specifications does not help explain the excess volatility. Thus, the conclusion that volatility is excessive can be justified in much more general settings than assumed here. The volatility test just reported then should be regarded as a sample from the volatility literature in which simplicity of exposition is purchased at the expense of restrictive specifications.

There are two possible sources of excess volatility in stock prices. First, investors could be overreacting to relevant information; second, they could be reacting to information which is irrelevant according to the efficient markets model. Although there do not appear to exist studies which attempt formally to apportion the excess

Chart 2
Simulated Rate Of Return
And Dividend Growth Rate

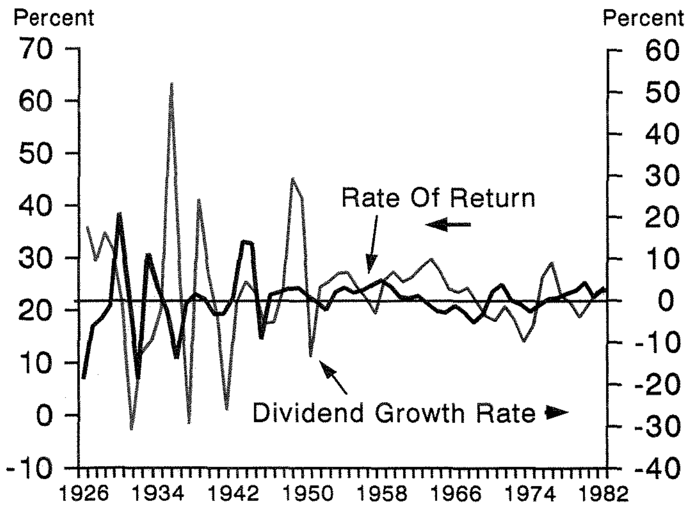
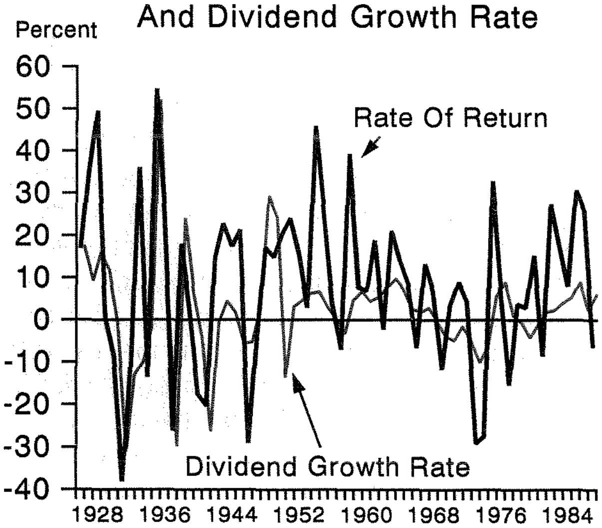


Chart 3
Actual Rate Of Return
And Dividend Growth Rate



volatility between these two sources, it seems likely that both are important.

That investors react to irrelevant information, at least, has been well established. For example, Roll (1984) documented the importance of irrelevant information in determining orange juice futures prices. Efficient markets theory implies that changes in the futures price of orange juice concentrate will reflect changes in the spot price which market participants expect will prevail at the date of the expiration of the futures contract. Roll argued persuasively that the only variable that can plausibly be viewed as giving relevant information about spot prices is weather—specifically, weather forecasts leading market participants to change their estimates of the probability of a freeze in Florida, since a freeze would adversely affect the orange crop.

Other variables which could in principle be relevant, Roll argued, would be expected to have only minor effect in the context of orange juice futures prices since current changes in supply induced by factors other than weather are of secondary importance, inasmuch as these factors do not change abruptly. For example, the number of trees bearing oranges at any time reflects planting decisions made several years earlier. Similarly, it appears unlikely that consumers' income and the prices of such substitutes as apple juice or tomato juice figure in an important way. Thus the efficient markets model predicts that weather should exert a dominant influence on futures prices. Roll verified that low temperatures in Florida were in fact associated with increases in orange juice futures prices, as expected. However, only a few percent of the total variation in futures prices can be explained in this way. In fact, Roll was unable to find any variable at all which correlated significantly with futures prices.

In his Presidential address to the American Finance Association, Roll (1988) reported the results of tests of whether the efficient markets model provides accurate *ex post* explanations for stock prices. He found that, again, irrelevant information appears to be of dominant importance. Even using such data as industry average prices and aggregate stock market indexes, Roll was able to explain *ex post* only a small fraction of the variation in prices of individual stocks.

IV. Asset Pricing Anomalies

There has always existed evidence at odds with the simplest models incorporating market efficiency. Prior to the 1970s, this conflict between theory and evidence usually was dismissed on the grounds that with relatively minor modifications, the efficient markets model could accommodate the contrary observations. For example, analysts identified trading rules that apparently could generate systematic profits, contrary to the efficient markets model. However, when these analysts allowed for brokerage charges, the profits usually evaporated.

More recently, however, analysts have recognized that there exists evidence that is not easy to square with the efficient markets model, even after making reasonable allowance for brokerage charges and other transactions costs. The "P-E anomaly" (Basu 1977, 1983) is the most prominent. It refers to the finding that stocks with low price-earnings ratios generate systematically higher rates of return than do stocks with high P-E ratios. This pattern is difficult to square with any recognizable version of the efficient markets model. In an efficient market, the stock price of successful firms should rise, but only by as much as is consistent with the firms earning normal returns in the future, and similarly with unsuccessful firms.

In contrast, it is easy to relate the P-E anomaly to the excess volatility of stock prices, at least informally. If investors overreact to news, then the stocks of successful firms will be bid to a higher multiple over earnings than is justified by the objective probability of this success continuing in the future. Subsequently the euphoria will wear off, generating low or even negative returns on average. Similarly, investors may be overeager to unburden their portfolios of losers, to the point where these stocks are discounted more than the facts justify. Subsequently such stocks on average generate higher returns than normal as their prospects improve. Correspondingly, this pattern of systematic overreaction to news would be expected to lead to price volatility in excess of that predicted by the efficient markets model. Therefore it is possible that the excess volatility of stock prices is the same thing as the P-E anomaly.

DeBondt and Thaler (1985, 1987) recently have documented a pattern similar to the P-E anomaly. They compared fictional portfolios of "winners"— stocks that had appreciated significantly in the recent past—with similar portfolios of "losers." They found that the losers strongly outperformed the market generally in subsequent years, while winners earned lower returns than the market averages. This result also suggests a pattern of overreaction, although the relation between DeBondt-Thaler's result and the P-E anomaly remains unclear.

Development of large data bases suitable for computerized study of stock prices have led to new anomalies.

Of these, the most striking is the "January effect" (Rozeff and Kinney, 1976; see Thaler, 1987, for a survey). Rozeff and Kinney found that rates of return on stock averaged 3.5 percent in January, whereas in other months returns averaged only 0.5 per cent. Several explanations involving tax-related purchases and sales of stocks have been investigated, but these explanations are not entirely convincing.

Another anomaly is the "small-firm" effect (Banz, 1981) in which small firms appear to earn higher returns than large firms, even when allowance is made for differences in riskiness. A subsequent study (Keim, 1983) showed that the January effect and the small-firm effect may be the same thing: the January effect appears only in samples that give equal weight to large and small firms. Value-weighted samples, in which small firms have much less importance relative to their role in equal-weighted samples, show little evidence of a January effect. This is exactly the pattern that would be expected if small firms account for the January effect.

Still other calendar-based anomalies have surfaced in recent years. Cross (1973), French (1980), and Keim and Stambaugh (1984), among others, have analyzed the "weekend effect," which refers to the observation that stock returns are on average negative from the close of trading on Fridays to the opening of trading on Mondays. Gibbons and Hess (1981) showed that a similar effect exists for bonds. Further, we have the "Wednesday effect": in 1968 the New York Stock Exchange was closed on Wednesdays in order to allow the back offices of brokerage houses to catch up with paperwork. Roll (1986) found that the volatility of stock prices was lower from Tuesday to Thursday when the market was closed on Wednesdays than over two-day periods over which the Exchange was not closed. This puzzle is difficult (although not impossible; see Slezak, 1988) to reconcile with market efficiency, given that as much news about corporate dividends presumably was arriving when the market was closed on Wednesdays as on other weekdays. The implication is that to some extent the trading process itself generates price volatility, a phenomenon clearly inconsistent with market efficiency. Finally, there exists a day-of-the-month effect: stock returns are positive in the days surrounding the turn of the month, but are zero on average for the rest of the month (Ariel, 1985).

Finally, Tinic and West (1984) investigated the seasonal pattern in the risk-return tradeoff. Fama and MacBeth's (1973) paper earlier had verified the prediction from finance theory that high-risk firms earn higher average rates of return than low-risk firms. Motivated by the results on the January effect, Tinic and West investigated the seasonal pattern in the correlation between risk and return which Fama-MacBeth had estimated. They found that this correlation is due entirely to the data for January. Given Keim's result that small firms earn high returns in January, and given the obvious fact that small firms are riskier than large firms, it is not surprising that the correlation between risk and expected return is strongest in January. What is surprising, however, is that the correlation between risk and return is essentially zero for the other eleven months of the year. Inasmuch as investors are risk-averse, this lack of compensation for risk in eleven of the twelve months of the year is not easy to reconcile with market efficiency.

V. Conclusions

Several essentially unrelated types of evidence that capital markets are inefficient have been discussed in this paper. Since it is not easy to think of non-trivial predictions of the efficient markets model that are borne out empirically, the burden of the evidence is negative. (Of course, trivial predictions are borne out. For example, it is true that the sustained upward trend in dividends that has occurred in the U.S. economy is associated with sustained price appreciation, as the efficient markets model predicts.)

How important this conclusion is depends on what lies behind the contrary evidence. The version of capital market efficiency adopted in the variance-bounds test reported above is grossly over-simplified (for example, equation (1) does not allow that investors are risk-averse, and therefore will demand a higher rate of return on high-risk securities than on low-risk securities). If it were to turn out that minor modification of the efficient markets model were sufficient to dispose of the contrary evidence, then the violations of market efficiency would not be important. However, most of the obvious extensions of the efficient markets model have been tried already, largely without success so far. Although it is possible that these extensions of the efficient markets model will succeed in the future, it may at some point be necessary for economists to face the uncongenial task of thinking about a world in which asset prices do not behave according to the precepts of finance and economic theory.

Economists are accustomed to thinking of prices not simply as measuring the amount of wealth that is transferred from one person to another when goods change hands, but also as guiding resource allocation. This is true as much for asset prices as for the prices of consumption goods. To see how this works in the context of asset prices, think of the petroleum market. There exists a large but far from infinite supply of oil reserves in the Middle East and other parts of the world. Other sources of energy exist, but they are at present more expensive than petroleum, at least for such purposes as automobile transportation and heating. However, when the petroleum runs out at some point in the future, the price of petroleum must be high enough to induce energy-users to shift to other energy sources. In the simplest idealized case, the price of petroleum will rise to equality with the alternate energy source just as the last gallon of oil is extracted, so that energy users are induced to shift sources at exactly the right time. Before that day of reckoning, petroleum prices must be rising to guarantee to holders of petroleum reserves a competitive return.[5] In this stylized account, the price of petroleum gives exactly the right signals to users of petroleum: they have adequate incentive to conserve, but are not induced irrationally to squander other resources so as to save petroleum. It follows that a massive program to encourage conservation or reliance on alternative sources is likely to do more harm than good, inasmuch as such a program amounts to fixing a social mechanism that is not broken.

Evidence of capital market inefficiency means that it cannot be taken for granted that asset prices are doing as good a job of rationing resources among alternative users as the foregoing account implies. The existing price of petroleum may not, after all, fully reflect the best information about petroleum reserves, alternative energy technologies, and so forth. Accordingly, the price of petroleum may not be providing the right incentives for conservation and development of alternative technologies.

It is apparent that an extreme interpretation of the evidence against capital market efficiency has the effect of opening the door to a variety of schemes to alter economic institutions. Inasmuch as such schemes generally have met with various degrees of failure in the past, we should not be too quick to jettison capital market efficiency, and with it the idea that prices determined in competitive markets do a reasonably good job of allocating resources. The evidence reviewed here suggests, rather, that economists ought to be aware that the evidence in favor of their way of thinking about the economy is far from clear-cut.

NOTES

* A more detailed version of this paper is found in LeRoy (1989).

1. Although these verbal characterizations of market efficiency are drawn directly from Fama (1970), it is not unambiguously clear that Fama identified market efficiency with the fair-game model (1); see LeRoy (1976, 1989) for discussion.

2. Used here is the rule of iterated expectations, which says that $E(E(d_{t+2} \mid I_{t+1}) \mid I_t) = E(d_{t+2} \mid I_t)$, and similarly for p_{t+2}.

3. Even though future dividends are weighted differently from current dividends because of discounting, and future dividends are not known with certainty, price behaves like an average of dividends over time.

4. The test to be described is known as the "West test" (West, 1988), although the original version of the West test is formally equivalent to one of the volatility tests derived by LeRoy-Porter (1981). (See Gilles-LeRoy, 1988.) West's derivation was independent, and he was the first actually to conduct the test. Also, West was the first to realize that the return volatility test has certain econometric advantages over price volatility tests, particularly for diagrammatic presentation. These advantages justify adoption of the West test here.

In one respect the test reported here differs from that derived by LeRoy-Porter and West. The formal derivation of the West test assumes constant-variance linear processes, which is an unsatisfactory specification in light of the upward trend in stock prices over the past fifty years. In order to correct for scale, Chart 3 instead compares the rate of return with the dividends growth rate. Formal derivation of the validity of this comparison, which is based on the linearization procedure of Campbell-Shiller (1988), is found in LeRoy-Parke (1990).

5. The implication that the prices of exhaustible resources should rise at a rate approximately equal to the real interest rate has been studied by Schmidt (1988). Schmidt found no evidence of rising prices over time, implying that holders of wealth in the form of exhaustible resources earned a zero real rate of return.

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