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Settlement Delays and Stock Prices

by Ramon P. DeGennaro

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Introduction

The typical stockbroker requires only about two minutes to execute and confirm a market order. During that time, the order is routed electronically either to the specialist or to the Intermarket Trading System, which connects eight regional markets including the New York Stock Exchange and the National Association of Securities Dealers. These agents then pair the order with another buy or sell order.[1] Thanks to modern technology, the process of executing a trade and producing a confirmed order is quick and efficient.

Although this confirmed order represents a binding contract between the buyer and seller, neither the security nor payment for the security changes hands at the time the trade is confirmed. Instead, payment for the stock occurs five business days later, when the buyer delivers a bank check to the seller and the seller delivers the promised securities.[2] Until final payment is made, the stock trade remains conditional, and official title remains with the seller, who cannot use the proceeds of the sale.

The equity markets have no provision to compensate the seller for the opportunity cost he bears while waiting for the trade to clear. In contrast, bond-market procedures call for explicit adjustment of the cost of the bond for interest accrued since the most recent coupon date. Interest is calculated using the number of days from the last coupon payment until the date of delivery, not the date of the trade. If the terms of the trade call for delivery tomorrow instead of today, the buyer must pay an extra day's worth of interest. Another important market, residential real estate, while not explicitly adjusting the purchase price for the date of closing, does prorate taxes and rents for the date of occupancy.

Although the stock markets make no explicit adjustment for the opportunity cost of settlement delays, rational investors do not ignore the fact that they lose several days' worth of interest. Indeed, much empirical work has assumed that investors consider delivery procedures in pricing assets, although few studies have tested this theory.

This paper studies whether investors do, in fact, consider settlement delays in determining stock prices. We construct two models of stock

1 For a further discussion of trading details, see Jakus and Chandy (1989).

2 In practice, these transactions usually are executed by brokers acting as agents.

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returns. The first expresses returns as a function of changes in the settlement delay. The second models returns as a function of changes in the length of the delay and in the federal funds rates during the delay. The first model controls for variation in the length of the delay, while the second controls for both the opportunity cost and the length of the delay. We then conduct regression tests of the significance of these variables. Both models show that in the full sample and all subperiods, investors apparently do consider the settlement delay; the variables controlling for it are statistically significant and correctly signed.

Section I reviews previous research regarding payment delays, and section II develops our model of the return-generating process. In section III we describe the data, conduct preliminary tests, and report the results. A summary concludes the paper.

I. Previous Research and lhe Impact of Delivery Procedures

Lakonishok and Levi (1982) speculate that settlement and check-clearing delays might explain the "weekend effect" in stock prices. The weekend effect refers to the well-documented tendency of stock prices to decline on Monday.[3] Lakonishok and Levi note that, in addition to the settlement delay, the check presented at settlement requires another business day to clear. They claim this makes the total payment delay six business days. For their empirical work, they add and subtract interest based on the prime rate, but, more important for our purposes, they conduct no tests to determine if buyers actually do compensate sellers in the manner they suggest.

DeGennaro (1990, forthcoming) tests the conjecture that the combined settlement and check-clearing delays explain the weekend effect. He concludes that, while the combined delay fails to explain the weekly return pattern, it does appear to influence measured stock returns. However, he also reports that the estimated rate of compensation for the combined delay varies substantially, suggesting that further work is necessary.

Another example is Choi and Strong (1983), who study "when-issued common stock. Firms announce stock issues well in advance of the time the new securities are issued; investors trade these securities on a "when-issued basis. Choi and Strong attempt to determine why this when-issued stock commands a premium over the corresponding stock that is currently outstanding. They speculate that when-issued stock represents the existing share plus a zero-interest loan. They find that adjusting prices for the interest savings is insufficient to explain the discrepancy, but again, they do not test to see if investors price the zero-interest loan.

More recently, Flannery and Protopapadakis (1988) assume that settlement and clearing delays are priced in their test of the generality of the weekend effect. They study three stock indexes and seven Treasury bond maturities to learn if intraweek seasonality is the same across these assets. Following the suggestion of Lakonishok and Levi, they adjust the returns on the 10 assets to control for the financing costs incurred during the payment delays. They find that the returns on these assets do not vary in a similar manner during the week, but again, the authors do not test if the delay is actually priced.

DeGennaro (1988) shows such payment delays can have important implications for interest rates. If delays exist in the Treasury bill market, but are not explicitly incorporated into pricing equations, certain common estimators of term premiums are biased in favor of finding positive premiums. He shows that this bias is sufficiently large to explain the results of McCulloch (1975). However, he does not test if investors do, indeed, consider these delays.

The results of the present paper are important for several reasons. First, if the delays have no impact on observed prices, then the aforementioned studies must be flawed: theoretical work begins with inappropriate assumptions, and empirical studies are misspecified. Second, if investors do consider settlement delays in determining equity prices, then observed prices diverge from true prices. This has implications for the event-study methodology commonly used in empirical tests (see, for example, Hite and Owers [1983]). To conduct an event study, the researcher first estimates the parameters of a model using time-series data prior to the event in question. He then calculates abnormal returns, defined as realized returns less the returns predicted by the model. Significance tests can be conducted using the cumulative sum of these residuals.

To date, all event studies known to the author have ignored the possibility that payment delays

3 The weekend effect was first identified by Cross (1973). An important paper by French (1980) reexamined this apparent anomaly, demonstrating that returns on Monday are so persistently negative that rational investors must expect to suffer losses on Mondays. Lakonishok and Smidt (1988) extend the evidence of negative Monday returns to a 90-year sample. Gibbons and Hess (1981) show that Treasury bills also earn below-average returns on Mondays, although returns are not negative for bills.

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may influence the measured stock price and return. If these delays do affect stock prices, events that may seem to be economically significant may in fact be negligible once proper accounting for the delays is made. Conversely, events judged to be insignificant may be important.

Consider, for example, an event that the researcher expects to generate positive returns, but which in fact does not. The total compensation for the settlement delay, capitalized in the observed price, may be higher than usual on the event date (due to a holiday that lengthens the delay, or perhaps simply to an increase in interest rates). This would make the observed price higher than usual, biasing the significance of test statistics. The reverse might also be true. The economic effect of an event may be positive and significant, but if the number of calendar days in the delay is lower than usual, or if the opportunity cost on a daily basis is less, then the impact of a true economic event might be negated and appear insignificant.

Other important results might also be affected. For example, French and Roll (1986) document a large decrease in volatility when markets are closed. The variance of stock returns from Friday's close to Monday's close is only about 10 or 15 percent higher than during a one-day holding period. If the opportunity cost of the settlement delay varies systematically—for example, if interest rates or the delay varies according to the day of the week—French and Roll's variance ratio measures both the true volatility and the variance in the opportunity cost. While this is unlikely to be sufficient to overturn their results, divergences from true prices are especially important in studies of variance, which is a particularly sensitive measure due to the squaring of deviations from the mean.

Perhaps the most important reason for studying whether delivery procedures are important and whether settlement delays are priced is their implication for market efficiency. If the settlement delay does not affect prices, then researchers must not only reinterpret research that presumes it does, but they must also explain why rational investors ignore the fact that the present value of the purchase price is reduced because of these delays.

The choice of delivery procedures may become an increasingly important policy issue for the securities industry. For example, the present five-day delivery terms trace to the inability of technology to handle heavy trading volume during the late 1960s. Prior to February 9, 1968, the settlement period was only four business days; extending it to five ensures that brokers have a weekend between the trade date and the delivery date to complete the necessary paperwork. Conceivably, further increases in volume could force another extension, while technological advances might permit a reduction.

A reduction in the time between the trade date and the delivery date may be important in preventing defaults on trades. For example, although the buyer and seller commit to trade at the confirmation of their order, large price changes create incentives for one side of the transaction to renege. For example, equity purchasers during the week of October 12, 1987, expected to receive stock worth a given amount; instead, they received stock worth about 20 percent less. Although the safeguards against such defaults proved adequate in this case, the increasing volatility of financial markets observed in recent years means larger losses can be sustained between the time of the trade and the date that the trade becomes final, increasing the likelihood that the buyer will default.

II. The Model

If investors consider delivery procedures in pricing stocks, then observed prices contain the true value of the underlying asset plus an adjustment for the settlement delay. Observed prices misstate true values. Since empirical work must use observed prices, we must devise a model that removes any adjustment the market incorporates for the delay. To do this, we first define the true stock price, P^*, as the price observed in the absence of delays. The expected true price at time t as a function of the true price at the beginning of the holding period (time $t-1$) is

$$(1) \quad E_{t-1}(P_t^*) = P_{t-1}^* \exp\left[E(R^*) - E_{t-1}(d_t)\right],$$

where E_{t-1} is the expectations operator conditioned on information available at time $t-1$, P_t^* is the unobservable true price at time t, $E(R^*)$ is the unobservable (constant) expected continuously compounded daily rate of return on the stock in the absence of delays, d_t is the dividend yield, and exp is the base for natural logarithms. Equation (1) states that if no dividends are expected to be paid, the expected price at t is the price at $t-1$ adjusted for the expected continuously compounded rate of price appreciation. If dividends are expected to be paid, the expected price is adjusted downward accordingly.

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To incorporate the settlement delay, the observed price P is written as

$$(2) \quad P_t = P^*_t exp\left(\sum_{j=1}^{s_t} c_{jt}\right),$$

where s_t is the number of days in the settlement delay and c_{jt} is the continuously compounded rate of compensation on day j for trades made at t. If investors ignore delivery procedures, c equals zero and the true price equals the observed price. If sellers demand and receive compensation for the settlement delay, c is positive. In equation (2), $\sum_{j=1}^{s_t} c_{jt}$ represents the total compensation to the seller for financing the position until he receives the proceeds of the sale at settlement.

Equation *(2)* is also true at $t-1$, so

$$(3) \quad P_{t-1} = P^*_{t-1} exp\left(\sum_{j=1}^{s_{t-1}} c_{j(t-1)}\right).$$

Solving equations *(2)* and **(3)** for P^*, substituting into equation (1), and assuming that the P and c are uncorrelated, we can rearrange equation (1) to obtain

$$(4) \quad \log[E_{t-1}(P_t)/P_{t-1}] + E_{t-1}(d_t) = E(R^*) + \sum_{j=1}^{s_t} c_{jt} - \sum_{j=1}^{s_{t-1}} c_{j(t-1)}.$$

Letting c be constant and defining As, as the change in s at time t, we obtain

$$(5) \quad E_{t-1}(R_t) = E(R^*) + c \times \Delta s_t,$$

where the total expected return on the stock—capital gains plus dividends—is written as R_t. Intuitively, equation (5) says the observed expected return equals the expected return in the absence of delays plus changes in the impact of delays.

To proxy for the dependent variable R_t, we use the return on the value-weighted portfolio, including dividends, provided by the Center for Research in Security Prices (CRSP). Although we have derived our model in terms of an individual stock, if the settlement delay affects any stock, it must affect all stocks. Further, this effect is not diversifiable: any settlement effects must appear in the observed return on a portfolio.

Substituting ex post values, we obtain our test equation:

$$(6) \quad R_t = b_0 + b_1 \Delta s_t + e_t.$$

In this model, b_0 estimates the unobservable expected continuously compounded daily rate of return on the stock in the absence of delays, and b_1 estimates c, the rate sellers receive as compensation for the settlement delay. Theory suggests that both coefficients should be positive. This is because risk-averse investors require a premium to compensate for the nondiversifiable risk contained in stocks, and increases in the financing costs during the settlement delay require buyers to raise their bids to compensate the sellers. Therefore, one-tailed tests are appropriate.

One potential problem with this specification is that As varies relatively little. To circumvent this, we also estimate a second specification. Rather than letting the settlement cost per day (c) be constant in equation (4), we use the federal funds rate as a proxy for c. The federal funds rate is both readily available and responsive to changes in the economic environment. Formally, we write

$$(7) \quad \gamma f_{jt} = c_{jt},$$

where f_{jt} is the federal funds rate on day j of the settlement delay for trades at t, and y is a constant. For notational convenience, we define $\sum S_t$ as $\sum_{j=1}^{s_t} f_{jt}$, so that y X $\sum S_t = \sum_{j=1}^{s_t} c_{jt}$. Substituting $\sum S$ into equation *(4)* and combining terms yields

$$(8) \quad E_{t-1}(R_t) = E(R^*) + \gamma \times \Delta\Sigma S_t,$$

where ACS, is the change in CS at t, and the total expected return on the stock is again written as R_t. Substituting ex post values, we obtain

$$(9) \quad R_t = b'_0 + b'_1 \Delta\Sigma S_t + e'_t.$$

As in equation (6), b'_0 estimates the unobservable expected continuously compounded daily rate of return on the stock in the absence of delays, but in this model, b'_1 estimates y, the proportion of the federal funds rate sellers receive as compensation for the settlement delay. One-tailed tests are again appropriate.

Equation *(9)* offers both advantages and disadvantages relative to equation *(6)*. In equation (9), the independent variable is a function of the federal funds rate, and therefore may be simultaneously determined with the stock return. However, it controls for both the length of the settlement delay and the opportunity cost during that delay rather than for simply the number of

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TABLE 1

Regression Results

Equation (10), Full Sample

Estimates obtained by regressing the rate of return on the CRSP value-weighted index, including dividends (R_t), on the change in the settlement delay (As,), corrected for heteroscedasticity:

$$(10)\quad R_t = b_0 + b_1 \Delta s_t + u_t,$$

$$u_t = e_t - \theta e_{t-1}.$$

Full sample period: January 1, 1970-December 31, 1986 (4,296 observations)

Variable	Parameter estimate (t-statistic)
b_0	4.68×10^{-4} (3.03)[a]
b_1	7.27×10^{-4} (4.07)[a]
θ	0.23 (15.5)[a]

a. Significant at the 1 percent level.
NOTE: Significance levels are for one-tailed tests on b_0 and b_1.
SOURCE: Author's computations.

days in the delay. It is also much more variable than As in equation (6).

Which economic or institutional forces could cause the slope coefficients in equations (6) and (9) to be not significantly different from zero? First, s_t is the *promised* settlement delay. Although the exchanges alter s_t only rarely, brokerage firms may not credit and debit accounts as accurately as the exchanges. For example, they may err and credit a customer's account later than promised. Such mistakes may not always be discovered. Even if the customer does detect the error, he must take the time to complain. Investors may, therefore, base compensation on the expected value of the delay rather than on the promised delay. If so, the independent variables in equations (6) and (9) are incorrect proxies for the true values, and the estimated coefficients could be insignificant.

In addition, some investors face different values of s_t because of the procedures of their agents. For example, some brokers debit accounts for purchases on the trade date, but credit accounts for purchases only on delivery. This asymmetric treatment permits the brokerage firm to use the funds between the two dates. The firm generates revenue by imposing an added cost of trading on its customers. If these investors are the marginal traders, neither As, nor $\Delta\Sigma S_t$ measures the true cost of the delays these investors face. Again, the estimated slope coefficients could be insignificant.

III. Data and Results

Data

The stock-return measure is the return on the CRSP value-weighted index, with dividends. We use 4,296 observations from January 1, 1970, through December 31, 1986. Federal funds rates used to compute the opportunity cost of the settlement delay are from the Federal Reserve Board. We estimate equation (6) in the full Sample and in three subperiods partitioned at October 6, 1979 and October 9, 1982, the dates of important changes in the Federal Reserve's operating procedures. On the former date, the central bank began focusing on the level of nonborrowed reserves rather than on the level of the federal funds rate. On the latter date, it began attempting to stabilize interest rates.

Preliminary Tests

The ordinary least squares residuals from equation (6) exhibit positive first-order serial correlation, while higher-order autocorrelations are small. This is consistent with the use of an index as the dependent variable and with the results of Scholes and Williams (1977). To see this intuitively, note that some securities composing the index do not trade at the closing bell. The most recent prices for these securities are "stale." If the market moves up or down since the last trade, these stale prices tend to move in the same direction when the securities subsequently do trade, inducing serial correlation at lag one. Therefore, we fit a first-order moving average to equation (6) and estimate

$$(10)\quad R_t = b_0 + b_1 \Delta s_t + u_t,$$

$$u_t = e_t - Be_{t-1}.$$

To formally investigate the possibility that the parameters in equation (10) may not be stable across subperiods, we conduct the test according to Chow (1960) for each subperiod partition. These tests show that both break points are

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TABLE 2

Regression Results

Equation (10), Subperiods

Estimates obtained by regressing the rate of return on the CRSP value-weighted index, including dividends (R_t), on the change in the settlement delay (Δs_t), corrected for heteroscedasticity:

$$(10)\quad R_t = b_0 + b_1 \Delta s_t + u_t,$$

$$u_t = e_t - \theta e_{t-1}$$

First sample period: January *1, 1970* **- October** *6, 1979* *(2,467* **observations)**

Variable	Parameter estimate (t-statistic)
b_0	3.10×10^{-4} (1.47)[a]
b_1	7.20×10^{-4} (3.43)[b]
θ	0.31 (16.3)[b]

Second sample period: October 7, *1979* **- October** *8, 1982* *(760* **observations)**

Variable	Parameter estimate (t-statistic)
b_0	5.60×10^{-4} (1.41)[a]
b_1	8.73×10^{-4} (1.76)[c]
θ	0.17 (4.86)[b]

Third sample period: October *9, 1982* **- December** *31, 1986* **(***1,069* **observations)**

Variable	Parameter estimate (t-statistic)
b_0	7.61×10^{-4} (2.92)[b]
b_1	5.98×10^{-4} (1.60)[a]
8	0.10 (3.42)[b]

a. Significant at the 10 percent level.
b. Significant at the 1 percent level.
c. Significant at the 5 percent level.
NOTE: Significance levels are for one-tailed tests on b_0 and b_1.
SOURCE: Author's computations.

necessary. For the first partition, the F-value is *7.64*, which exceeds the *1* percent critical value of *3.78*. For the second, the F-value is *5.59*, which again is significant at the *1* percent level. In addition, the test rejects the conjecture that the first and third subsamples can be combined.

Because of weekends and holidays, holding periods range from one to four days. Given the results of French and Roll (1986), we would expect heteroscedasticity to be present, depending on the holding period for the observations. This proves to be the case. In the full sample, for example, the F-ratio using the variance of the three-day holding period and the one-day holding period is *1.31*, which exceeds the critical *1* percent value of *1.15*. Similar results are found for both subperiods. Therefore, we weight observations by the inverse standard deviation of the residuals for the holding period in all reported results.

Results Using the Change in the Length of the Delay

Table *1* contains the results obtained by estimating equation *(10)* using the full sample. Given the results of the Chow tests reported above, these estimates must be interpreted with caution, but we report them for completeness. All parameters have their expected signs and are statistically significant. The intercept, which estimates the expected daily stock return in the absence of delays, implies an annual rate of about *11.80* percent. This is quite close to the actual realized value of *10.97* percent. The parameter b_1 estimates c, the rate of compensation for the settlement delay. This parameter is also significant, with a *t*-statistic of *4.07*.

Table *2* contains the results from the subperiods, which are broadly consistent with the full sample. For the first subperiod, the intercept is positive and significant at the *10* percent level, and is almost exactly the correct magnitude. The estimated value of .000310 implies an annual rate of about *7.81* percent; the actual value was *7.13* percent. The estimate of b_1 is reliably different from zero, with a *t*-ratio of *3.43*.

After the first change in Federal Reserve operating policy, the results are somewhat different. The intercept is still marginally significant and again about the correct size (it implies a daily rate of *14.10* percent versus the actua1.12.08 percent). Despite being larger in magnitude, however, the significance of the slope coefficient is smaller. The *t*-ratio is *1.76*. The larger standard error is consistent with the smaller sample size and with the increased volatility during this

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TABLE

Regression Results

Equation (11), Full Sample

Estimates obtained by regressing the rate of return on the CRSP value-weighted index, including dividends (R_t), on the change in total return from an investment in federal funds during the settlement delay (ACS_t), corrected for heteroscedasticity:

$$(11)\quad R_t = b'_0 + b'_1 \Delta\Sigma S_t + u'_t,$$

$$u'_t = e'_t - \theta' e'_{t-1}.$$

Full sample period: January 1, 1970–December 31, 1986 (4,296 observations)

Variable	Parameter estimate (t-statistic)
b'_0	4.69×10^{-4} (3.03)[a]
b'_1	2.73 (4.02)[a]
θ'	0.23 (15.6)[a]

a. Significant at the 1 percent level.
NOTE: Significance levels are for one-tailed tests on b'_0 and b'_1.
SOURCE: Author's computations.

period, when the Federal Reserve did not attempt to stabilize interest rates.

The third sample begins on October 9, 1982. The results of this subsample are similar to those of the second subsample. The estimate of b_0 implies a stock return of 19.17 percent; the actual value was 19.07 percent. The t-value of 2.92 is significant at the 1 percent level. The estimated slope coefficient is 0.000598, which differs from zero at the 10 percent level.

Results Using the Change in the Opportunity Cost During the Delay

The preliminary tests using equation (9) yield results similar to those of equation (6). Chow tests confirm that the subperiods are best estimated separately. Heteroscedasticity is again present, and a first-order moving average is required. We estimate

$$(11)\quad R_t = b'_0 + b'_1 \Delta\Sigma S_t + u'_t,$$

$$u'_t = e'_t - \theta' e'_{t-1}.$$

For completeness, table 3 contains the results obtained by estimating equation (11) using the full sample. Again, all parameters have their expected signs and are statistically significant. The intercept, which estimates the expected daily stock return in the absence of delays, is very close to the value in table 1. The parameter b'_1 estimates y, the proportion of the federal funds rate that buyers receive as compensation for the settlement delay. This parameter is also significant, with a t-statistic of 4.02. The coefficient of 2.73 is also reliably different from unity. A t-ratio testing the hypothesis that the estimated value equals one is 2.55, which rejects the null hypothesis at the 1 percent level. Thus, we reject the conjecture that the rate of compensation is the federal funds rate. The federal funds rate is too low or too stable to serve as the rate of compensation.

Table 4 contains the estimates from the subperiods, which are again similar to those from equation (10). For the first subperiod, the intercept is the same size and is equally significant as in table 2. The estimate of the slope coefficient, b'_1, is 3.80. As is the case for the full sample, this is reliably different both from zero and from unity. The t-ratios are 3.64 and 2.69, respectively.

After the first change in Federal Reserve operating policy, the intercept is still significant and again about the correct size, but the slope coefficient is much smaller. The estimated value is 1.84. This differs from zero at the 10 percent level, but unlike the case in the first subsample, it does not differ from unity. The t-statistic is only 0.71. We cannot reject the hypothesis that the rate of compensation for settlement delays equals the average realized federal funds rate during the sample.

The third sample begins on October 9, 1982. The results are similar to the second subsample and comparable to equation (10). The estimate of b'_0 is significant and implies a stock return of 19.18 percent, compared to the actual value of 19.07 percent. The estimated slope coefficient, b'_1, is 2.59, which differs from zero at the 5 percent level, but does not differ from unity. The t-statistic is only 1.03.

The results suggest that during the first sample, the Federal Reserve's intervention in the federal funds market prevented the federal funds rate from tracking market conditions as well as it did during periods when the Federal Reserve concentrated on other policy vehicles. When the federal

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TABLE 4

Regression Results

Equation (11), Subperiods

Estimates obtained by regressing the rate of return on the CRSP value-weighted index, including dividends (R_t), on the change in total return from an investment in federal funds during the settlement delay ($\Delta\Sigma S_t$), corrected for heteroscedasticity:

$$(11)\quad R_t = b'_0 + b'_1 \Delta\Sigma S_t + u'_t,$$

$$u'_t = e'_t - \theta' e'_{t-1}.$$

First sample period: January 1, 1970 - October 6, 1979 (2,467 observations)

Variable	Parameter estimate (t-statistic)
b'_0	3.10×10^{-4} (1.46)[a]
b'_1	3.80 (3.64)[b]
θ'	0.31 (16.2)[b]

Second sample period: October 7, 1979 - October 8, 1982 (760 observations)

Variable	Parameter estimate (t-statistic)
b'_0	5.62×10^{-4} (1.41)[a]
b'_1	1.84 (1.55)[a]
θ'	0.17 (4.85)[b]

Third sample period: October 9, 1982 - December 31, 1986 (1,069 observations)

Variable	Parameter estimate (t-statistic)
b'_0	7.61×10^{-4} (2.92)[b]
b'_1	2.59 (1.68)[c]
θ'	0.10 (3.44)[b]

a. Significant at the 10 percent level.
b. Significant at the 1 percent level.
c. Significant at the 5 percent level.
NOTE: Significance levels are for one-tailed tests on b'_0 and b'_1.
SOURCE: Author's computations.

funds rate is permitted to float freely, we cannot reject the notion that stock purchasers compensate sellers for the settlement delay at the federal funds rate. However, when the central bank intervenes, the federal funds rate appears to be too stable to serve as the rate of compensation.

Since the estimates of b'_1 exceed unity, they are higher than predicted by Iakonishok and Levi (1982), who argue that delays should be compensated at the riskless rate. To the extent that the overnight federal funds rate is riskless, the coefficient should be one if Iakonishok and Levi are correct. The results in table 4 are, however, consistent with their empirical results. Iakonishok and Levi assume that settlement and check-clearing delays are priced at the prime rate and test to see if the prime rate is large enough to explain the weekend effect. Although a strict interpretation of their story requires that sellers be compensated at the riskless rate, they report that the prime rate is too low to eliminate these effects completely. This suggests that if the settlement and check-clearing delays were in fact the sole reason for the weekly pattern, rates of compensation during these delays must be larger than the riskless rate. Since our results apply only to the settlement delay and not to the check-clearing delay, they do not directly relate to those of Iakonishok and Levi. However, they do suggest the possibility that rates of compensation are larger than the riskless rate.

Conceivably, though, the rate of compensation should *not* be the riskless rate: errors in posting to brokerage or bank accounts do occur. While restitution is always made if the error is caught, the seller may not notice it. Even if he does, complaining is time-consuming. The seller may therefore require a premium over the riskless rate. In addition, the buyer may very well be willing to pay this premium. If he monitors his account, it cannot be debited early, but through bank or brokerage error, it may be debited late. Since the buyer can only win, he is willing to pay extra for this possibility.

Using the brokers' call money rate as the interest rate proxy would probably produce smaller values of b'_1. This rate tends to be higher than the federal funds rate, so smaller proportions of the call money rate imply the same levels of compensation. If the call money rate is as variable as the federal funds rate, t-tests would be less likely to reject the notion that the rate of compensation is the call money rate.

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IV. Conclusion

This paper shows that investors consider delivery procedures in pricing stocks. We model stock returns in two ways. The first uses a function of the length of the settlement delay, while the second uses a function of both the length of the delay and interest rates during the delay. We find that the coefficient on this variable is always correctly signed and statistically significant. This means that observed prices diverge from the prices that would be observed in the absence of this trading mechanism. This, in turn, means that measured returns diverge from true returns.

While this result is comforting to researchers who have assumed that settlement delays are priced, it does have implications for empirical studies using daily stock-return data. Since the observed price equals the true price plus a premium to compensate for financing costs, measured returns diverge from true returns if the premium changes during the holding period. This could, for example, affect event studies either by masking the impact of a true economic event or by lending statistical significance to "events" which result only from changes in the premium and not from any underlying economic force.

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The Effect of Bank Structure and Profitability on Firm Openings

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Introduction

The banking industry has undergone significant changes in recent years. Much attention has been given to the effect of financial deregulation and interstate banking on the structure of the banking industry. Attention has also been directed at the systematic effects of financial structure on the national economy.

However, bank structure can also affect local economic development.[1] The availability and the cost of financing potentially varies across regions due to differences in bank structure and in the health of the local banking sector. Since bank credit is an important source of financing for new firms, differences in bank structure can affect regional growth.

This paper examines the effects of bank structure and profitability on the birth of new firms, an important component of economic development. Specifically, we enter measures of profitability, concentration, size, and entry of a region's banking sector (as well as an overall measure of lending activity) into a standard model of firm location. This enables us to test for independent effects of bank structure and profitability on regional growth, as measured by business openings.

Our results suggest that bank structure and profitability have significant effects on firm openings. A profitable and competitive banking market is associated with a higher rate of firm births. In particular, firm births are found to be associated with higher bank profits, higher numbers of bank employees, lower levels of concentration, higher proportions of small banks, and freer entry of new banks into the region. These results support the position that bank structure and profitability influence economic development.

Section I briefly reviews previous work relating banking and economic activity and discusses the implications of bank structure for regional growth. Section II presents a standard model of firm location and extends it to include measures of bank structure and profitability. Section III describes the data, and section IV provides results on the impact of banking on firm location. Finally, section V presents conclusions.

■ 1 We use the term "bank structure" to refer to both the organization of banks themselves (number of branches, employees per bank, etc.) and the market structure of the banking sector (concentration, ease of entry, etc.).

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I. Bank Structure and Regional Growth

With the advent of deregulation and interstate banking, the banking industry has changed significantly in recent years. Much attention has been given to the effects of these developments on the structure of the banking industry itself.[2] Attention has also been directed at the systematic effects of bank failures and financial structure on aggregate economic activity.[3] The effect of changes in bank structure on regional economies, however, remains an open question?

For example, Eisenbeis (1985), in a recent article on interstate banking, comments that:

> The most controversial issues surrounding consideration of modifying interstate banking laws deal with the implications of proposed changes for competition and concentration of resources. There is little doubt that restrictions on geographic expansion have, in the past, insulated many local markets from competition and have restricted economic growth. While casual inspection of the data suggest that states with more liberalized policies toward intrastate banking have generally had higher economic growth rates than unit banking states, empirical studies show no convincing relationship between banking structure and economic development. More detailed study would have to be done to determine whether this is just a matter of correlation or causation. (p. 231-32)

We approach this issue by studying the effect of bank structure on business openings. If bank structure and the health of the local banking sector affect the cost and availability of credit for new firms, changes in bank structure will potentially affect regional growth.

Financial institutions, especially banks, are the primary supplier of external funds to new businesses, which are typically small, independent enterprises. Unlike medium-sized (100 to 500 employees) or large corporations, small businesses have limited access to organized open markets for stocks, bonds, and commercial paper. Approximately three of every four existing small businesses have borrowed from banks.5

The availability of credit at affordable rates for the start-up and the continued operation of new firms is not necessarily a given.[6] For small start-up firms (typically "mom and pop" operations), financing comes mostly from private sources, such as personal savings, home equity loans, and loans from friends or relatives. For larger small businesses, capital for start-ups comes from financial institutions and organized venture capital firms, as well as from friends, relatives, and informal investors. Even after being established, firms may require financing when cash inflow lags behind cash outflow due to a rise in receivables or an inventory buildup.

When external financing is used, it is received primarily from commercial banks. The rates charged for small start-up firms are typically 2 to 3 percentage points above that charged for larger firms. This is due in part to the high-risk nature of new small businesses, which lack collateral and a credit history and suffer high rates of failure.

Some researchers and many policymakers argue that banks do not meet the needs of various types of businesses, particularly small businesses. They contend that due to high monitoring costs and a lack of adequate information about risk, a market failure exists—popularly referred to as the "credit gap." It has been argued that the price of credit, especially working capital, provided to small and middle-sized firms is too high after controlling for appropriate risk factors. The

■ 2 For example, Lee and Schweitzer (1989) use event-study analysis to determine the effect on stock prices of decisions by bank holding companies (BHCs) to establish subsidiaries within Delaware and find no evidence of long-term stock price changes during the postannouncement period. Trifts and Scanlon (1987) use a sample of interstate mergers to provide early evidence of the effects of interstate bank mergers on shareholder wealth. Bom, Eisenbeis, and Hanis (1988) provide evidence on the market evaluation of financial firms entering into interstate banking when restrictions are relaxed and find no significant effect of an announced geographic interstate expansion on shareholder values.

■ 3 Gertler (1988) provides an overall review. Bemanke (1983) argues that extensive bank runs and defaults in the 1930-1933 financial crisis reduced the efficiency of the financial sector in performing its intermediation function, causing adverse effects on real output, other than through monetary channels. Samolyk (1988) conducts a similar test on British data, using corporate and noncorporate insolvencies as proxies for the health of the financial sector, and also finds that credit factors matter empirically on output. Gilbert and Kochin (1990, forthcoming) provide additional tests of the hypothesis that bank failures have adverse effects on economic activity using rural county-level data and find that closing banks has adverse effects on local sales and nonagricultural employment.

■ 4 As discussed in Gertler (1988), the literature on financial structure and economic development has principally focused on variations across countries. Gurley and Shaw (1955) emphasize the role of intermediaries in the credit supply process. They note that in developed countries there typically exists a highly organized system of financial intermediation facilitating the flow of funds between savers and investors. They argue that the role intermediaries play in improving the efficiency of intertemporal trade is an important factor governing general economic activity. The correlation between economic development and financial sophistication across time and across countries has often been noted. See Goldsmith (1969) and Cameron (1972) for examples of such studies.

■ 5 Small Business Administration (1985), p. 206.

6 Current information is not available on the sources of internal financing to small firms. For historical data, see Small Business Administration (1984).

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credit gap is aggravated in times of tight credit, during which banks ration funds, with larger firms receiving a disproportionately large share.

This perception of market failure is reflected in how public-sector development agencies lower the cost of credit by providing access to sheltered pools of money (such as public pension funds), by passing on the favorable tax treatment of funds (through tax abatement and public bonds), or by accepting risks greater than private institutions are willing to bear (such as the loan guarantee program of the Small Business Administration)?

While there are no direct measures of the price and availability of credit for small businesses across regions, they are likely to vary with bank structure.[8] Concentrated banking markets with large banks and high barriers to entry may be unresponsive to the credit needs of small businesses and new firms. Lending to new firms entails higher risks than lending to established firms, since a large proportion of new firms fail in the first few years.

Heggestad (1979), Rhoades and Rutz (1982), Clark (1986), and Liang (1987) argue that banks in highly concentrated markets trade potential monopoly profits for lower risk. Alternatively, a highly competitive bank market, characterized by large numbers of smaller banks and easy entry, may result in a greater availability of credit at lower prices for small businesses. Finally, a profitable banking sector is expected to result in less credit rationing and a greater supply of credit for small firms. Even if most start-ups do not rely directly upon commercial banks for their initial financing, the expectation of ample credit for future expansion at low cost potentially affects the decisions of entrepreneurs to start a firm.[9]

An understanding of the impact of bank structure on firm location and regional growth is important because of the significant changes occurring due to deregulation and interstate banking. By the end of 1988, all but three states permitted some form of interstate acquisition of their banks, 14,600 offices of banking organizations existed outside the organizations' home state, and more than half of these were permitted to offer all banking services.[10] To the extent that this results in freer entry and increased competition among banks, the availability of capital for small businesses and new firms could increase. In the Southeast and New England, however, these developments have increased the number of extremely large banks, called "super-regionals," at the expense of regional banks. Increased concentration could reduce the supply of credit for small businesses.

A recent survey of state bank regulators by Hill and Thompson (1988) found that advancing economic development is an important goal of state bank regulators.[11] If changes in bank structure do indeed affect regional growth, however, policymakers may be misjudging the costs and benefits of deregulation and interstate banking. We now turn to an empirical analysis of this issue.

II. A Model of Firm Location

To study the effect of bank structure and profitability on local economic activity, we concentrate on firm openings because they are driven by current and expected economic conditions, as opposed to expansions, contractions, and deaths, which will be greatly affected by the large fixed costs associated with changing locations. The model estimated here was originally developed by Carlton (1979), though we more closely follow Eberts and Stone (1987).[12]

The number of new establishments in a city is assumed to depend on the number of potential entrepreneurs in the city and on the probability that a given entrepreneur will start a new firm. The higher the level of economic activity in a city, the greater the number of potential entrepreneurs. Also, the higher the expected profitability of new firms, the larger the probability that they will actually emerge.

■ 7 See Hill and Shelley (1990, forthcoming).

■ 8 This would not be true if banks were perfectly contestable; the actual number and size distribution of competitors would not affect the price or the availability of credit. Whalen (1988) found that there is evidence that bank performance is systematically related to proxies designed to measure the intensity of actual and potential competition in rural banking markets in Ohio and concludes that these non-SMSA banking markets are contestable, since potential competition matters, but are not perfectly contestable. Our results suggest this may be true for SMSAs as well.

■ 9 Unfortunately, we do not have measures of sources of funds from non-bank entities, which potentially compete with commercial banks.

■ 10 These figures come from a recent comprehensive review of interstate banking by King et al. (1989). Earlier surveys include Whitehead (1983a, 1983b, and 1985), and Amel and Keane (1986).

■ 11 It ranked third, just behind ensuring the safety and soundness of depositors' funds and providing banking (depository) services throughout their states.

■ 12 For reviews of the firm-location literature, see Bartik (1985, 1988), Wasylenko (1988), and Wolkoff (1989).

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Carlton *(1979)* modeled this birth process as a Poisson probabilistic model, since the birth of new establishments is a discrete event. Let p_i be the probability that a potential entrepreneur will start an establishment in a given city; then let

$$(1) \quad \ln p_i = x_i b + e_i, \quad i = 1,..., M,$$

where x_i is a vector of independent variables affecting firm profitability, b is a vector of fixed coefficients, e_i is an error term composed of the variance of the Poisson process and a random error, and M is the number of cities in the sample. Consistent estimates of the mean and variance of p, are given by

$$(2) \quad E(p_i) = (N_i / BP_i),$$

$$(3) \quad Var(p_i) = (N_i / BP_i^2),$$

where N_i is the observed number of births and BP_i is the birth potential as proxied by employment in the standard metropolitan statistical area (SMSA).[13] Carlton shows that a consistent and asymptotically efficient estimate of b can be obtained by weighted least squares, with weights equal to the standard error of the Poisson process.

The independent variables typically used to measure expected profitability include wage rates, tax rates, unionization rates, and energy prices. We extend this list by including measures of bank structure and profitability. As discussed in the previous section, these measures determine, at least in part, the price and availability of credit and thus expected profitability and firm openings. Measures of bank structure and profitability are employed because direct measures of the price and the availability of credit are unavailable. To control for the effects of bank structure and the availability of credit on firm births, we include measures of the number and size distribution of banks as well as a measure of the financial health of banks.

III. Data

Data from *259* SMSAs across the country are employed to estimate the model. The dependent variable *(BIRTHRATE)* is the natural log of the ratio of new firm births (as reported for the years *1980* to *1982* in the USELM data) to existing employment in the SMSA.[14] *A* birth is defined as an establishment that did not exist in *1980* but did exist in *1982*. Births within this two-year period are treated as comparable.

We divide the independent variables into two types. The first are measures of local economic conditions, and the second are measures of bank structure and profitability. All data are measured at the *SMSA* level unless otherwise noted.

The measures of local economic activity are the natural logs of the wage rate (WAGE), the number of establishments *(FIRMS)*, the gross state product (*GSP*), the personal income (*PINC*), and the population *(POP)*. Also included is the effective state corporate tax rate (*TAX*).[15] We control for population by entering it directly into our equation rather than using per capita variables that would impose additional structure.

Bank data are obtained from the Federal Financial Institutions Examination Council's Reports on Condition and Income, known as call reports, for *1980*. (We assume that the lagged *1980* variables on banking are exogenous to firm births occurring between *1980* and *1982.)* Measures of bank structure and profitability are created by aggregating data from individual banks up to the *SMSA* level. The total amount of loans and leases *(LOANS)* is a measure of the level of bank intermediation. The average rate of return (RETURN), net income divided by assets, measures the amount of resources available for future lending and the health of the banking sector.[16] This variable may also be measuring the effects of bank structure and the general economic health of the region. The empirical analysis will thus explicitly control for these effects.

We employ standard measures of market structure such as the total number of banks *(HQS)* and branches *(BRANCH)*, the number of bank employees per bank (*BANKEMP*), and a Herfindahl index of the concentration of deposits (*HERF*).[17] We also include a measure of bank

13 Although policymakers concerned with economic development value the employment resulting from new firms, the firm location literature explicitly models the birth of the firm itself. Using job creation (instead of firm births) as the dependent variable, however, yielded similar results.

14 USELM stands for the U.S. Establishment and Longitudinal Microdata file constructed for the Small Business Administration by Dun and Bradstreet.

15 *WAGE* and *TAX* are 1977 variables from the Census of Manufactures. *GSP, PINC,* and *POP* are 1980 variables from the Census Bureau and the Department of Commerce. The number of establishments is a 1980 variable from the USELM data.

16 Specifications using income divided by equity capital yield similar results.

17 The Herfindahl index is defined as the sum of the square of each bank's share of deposits for a given SMSA. While we are interested in the effect of concentration in the lending market, we assume that deposits are subject to less geographic dispersion than loans, and thus provide a more accurate indicator of concentration in the local banking sector.

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TABLE 1

Descriptive Statistics

Variable	Mean	Standard Deviation
BIRTHRATE (firm birth/employment)	*0.008*	0.003
WAGE (manufacturing)	*5.986*	1.183
TAX (effective tax rate)	*0.403*	0.039
FIRMS (number of establishments)	*13,150*	24,713
POP (population, thousands)	*635.4*	1,060.2
LOANS (total loans and leases, millions)	*2,656.4*	9,411.5
RETURN (net income/assets)	*0.009*	0.003
HQS (number of banks)	*23*	39
BRANCHES (number of branches)	132	252
BANKEMP (employees/bank)	*196.8*	324.6
HERF (Herfindahl concentration index)	*2,499*	1,849
SIZE 1 (percent of banks with *$0-$25* million assets)	*0.456*	0.224
SIZE 2 (percent of banks with *$25-$50* million assets)	*0.180*	0.129
SIZE 3 (percent of banks with *$50-$75* million assets)	*0.084*	0.092
SIZE 4 (percent of banks with *$75-$100* million assets)	*0.058*	0.100
SIZE 5 (percent of banks with *$100-$250* million assets)	*0.042*	0.073
SIZE 6 (percent of banks with *$250-$400* million assets)	*0.028*	0.081
ENTRY (percentage change in the number of banks)	*-0.014*	0.156
PINC (personal income, millions)	*6,740.4*	12,413.0
GSP (gross state product, millions)	*100,680*	84,277

NOTE: Changes are measured as log differences.
SOURCE: Authors' calculations.

entry (ENTRY), the percentage net change in the number of banks from *1978* to 1980.[18]

Our last measures of bank structure are a set of variables (*SIZE 1–SIZE* 6) that control for the size of banks. *SIZE* 1 is the proportion of banks in an SMSA with assets less than *$25* million, *SIZE 2* is the proportion of banks with assets between *$25* and *$50* million, *SIZE* 3 is the proportion of banks with assets between *$50* and *$75* million, *SIZE 4* is the proportion of banks with assets between *$75* and *$100* million, *SIZE 5* is the proportion of banks with assets between *$100* and *$250* million, and *SIZE* 6 is the proportion of banks with assets of *$250* to *$400* million. The proportion of banks with assets greater than *$400* million is the omitted category in our estimations.[19] Summary statistics for these variables are presented in table *1*.

A pervasive problem with this data set for the purpose of looking at how banking activity affects the regional economy is that regions for which data are collected (SMSAs and states) and economic regions do not necessarily match. In addition, for some variables, such as LOANS, though the total dollar value of loans is known, it is not possible to determine where the loans were made. For example, loans made by an Ohio bank to firms in Florida and Ohio are counted in the same way.

With the banking data, there is an additional measurement problem in that a call report for a consolidated banking unit may include data for branches not located in the SMSA. In states that allow branch banking, activity at the branches may be reported solely in the SMSA headquarters. Thus, our measures of competition and concentration are potentially subject to errors. The sensitivity of our full sample results to this potential errors-in-variables problem is tested by running the model without SMSAs in states that have statewide branching, and then again without SMSAs in states that have limited branching (that is, only SMSAs in unit banking states).

IV. Estimation and Results

Full Sample Results

Estimates of variations of the above model for the full sample are presented in table 2. Equa-

18 Note that this measure treats entry and exit symmetrically.

19 Alternative measures of size were also tested. In general, only the measures of the smaller banks were statistically significant.

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TABLE 2

Estimation Results

Coefficient	(1)	(2)	(3)
WAGE	-0.6823[a]	-0.4426[a]	-0.5076"
	(0.1131)	(0.1023)	(0.1140)
TAX	-1.8368[a]	-1.7032[a]	-1.5193"
	(0.5694)	(0.5442)	(0.5490)
FIRMS	0.2825"	0.3453"	0.3046[a]
	(0.0940)	(0.0939)	(0.1090)
POP	-0.2412[a]	-0.1694[b]	-0.3532"
	(0.1015)	(0.1002)	(0.1692)
LOANS	—	-0.0393	-0.0602
		(0.0870)	(0.0872)
RETURN	—	31.7890[a]	31.2940"
		(6.8238)	(6.8055)
HQS	—	-0.0693	-0.0451
		(0.1294)	(0.1293)
BRANCHES	—	-0.2271[a]	-0.1945[a]
		(0.0555)	(0.0574)
BANKEMP	—	0.3192[a]	0.3191[a]
		(0.0942)	(0.0938)
HERF	—	-0.1987"	-0.1911[a]
		(0.0687)	(0.0684)
SIZE 1	—	0.8650"	0.8550[a]
		(0.2463)	(0.2450)
SIZE 2	—	0.3396	0.3168
		(0.2537)	(0.2525)
SIZE 3	—	0.4889[b]	0.4486
		(0.2746)	(0.2742)
SIZE 4	—	0.4387	0.4101
		(0.2688)	(0.2677)
SIZE 5	—	-0.0085	-0.0432
		(0.3159)	(0.3146)
SIZE 6	—	-0.0803	-0.0816
		(0.2784)	(0.2770)
ENTRY	—	0.4314[a]	0.4239[a]
		(0.1319)	(0.1312)
PINC	—	—	0.1838
			(0.1785)
GSP	—	—	0.0427[b]
			(0.0239)
CONSTANT	-4.0502[a]	-4.6572"	-6.3725"
	(0.4267)	(0.7856)	(1.5336)
Log likelihood function	-95.4467	-46.6358	-44.1093
R-square	0.2109	0.4579	0.4683
Mean of the dependent variable	-4.9267	-4.9267	-4.9267
No. of obs.	259	259	259

a. Significant at the 95 percent confidence level.
b. Significant at the 90 percent confidence level.
NOTE: Standard errors of the coefficients appear in parentheses.
SOURCE: Authors' calculations.

tion (1) is a basic, static model of firm location, where the probability that a birth will occur depends on the wages, taxes, number of establishments, and population. This set of variables differs somewhat from that employed by Carlton (1979), who also used the unionization rate and energy prices in his estimates for selected industries. Eberts and Stone (1987) found that energy prices do not matter when the model is estimated with aggregate manufacturing data, and it is even less likely that energy prices would matter since we are looking at all industries.

Because we are not concerned about differences across industries and are interested only in whether there are statistically significant effects on aggregate regional economic activity as a result of bank structure and profitability, energy prices can safely be omitted. The unionization rate was omitted due to lack of available data. We assume that unionization is not systematically related to the banking variables.

All the coefficients in equation (1) are statistically significant at the 95 percent confidence level. As expected, we find that higher wages and higher effective corporate tax rates reduce the probability of firm births in an SMSA. Also, the probability of firm births increases with a greater number of establishments (*FIRMS*) and a lower population. Though the coefficient on population is somewhat unexpected, this result suggests that given the similar magnitude and opposite signs of these two coefficients, perhaps the number of firms per capita is the appropriate regressor. We continue entering population as a separate regressor because this is the most general way of including population in the model.[20]

Equation (2) estimates the same model, only now the measures of bank structure and profitability are included. The results strongly support the view that bank structure and profitability have a statistically significant effect on firm births. The addition of the bank structure variables did not affect the estimates of the basic firm location variables. The basic firm location coefficients have roughly the same magnitude and remain statistically significant at the 90 percent confidence level or higher.

The measure of the total amount of financial intermediation (LOANS) is negative but not statistically significant. The *RETURN* variable has a positive and statistically significant coefficient,

20 More restrictive specifications using per capita variables yielded similar results.

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TABLE 3

Unit and Limited Branching States

Coefficient	(1)	(2)	(3)
WAGE	-0.7558[a]	-0.4559[a]	-0.4610[a]
	(0.1137)	(0.1075)	(0.1340)
TAX	-3.0484[a]	-1.5043[a]	-0.7901
	(0.6175)	(0.6943)	(0.8031)
FIRMS	0.4437[a]	0.4013[a]	0.4063[a]
	(0.1132)	(0.1392)	(0.1654)
POP	-0.4337[a]	-0.3001[a]	-0.3458[b]
	(0.1224)	(0.1367)	(0.2088)
LOANS	—	-0.1162	-0.1612
	—	(0.1352)	(0.1371)
RETURN	—	44.3430[a]	43.4040[a]
		(9.9812)	(9.9638)
HQS	—	0.1324	0.2018
	—	(0.2000)	(0.2031)
BRANCHES	—	-0.2778[a]	-0.2647[a]
	—	(0.0735)	(0.0736)
BANKEMP	—	0.5493[a]	0.5817[a]
	—	(0.1412)	(0.1419)
HERF	—	-0.2163[a]	-0.2104[a]
	—	(0.0863)	(0.0861)
SIZE 1	—	1.2428[a]	1.2287[a]
	—	(0.3579)	(0.3569)
SIZE 2	—	0.7064[a]	0.6672[b]
	—	(0.3454)	(0.3449)
SIZE 3	—	0.8670[a]	0.8677[a]
	—	(0.3380)	(0.3370)
SIZE 4	—	0.9456[a]	0.9459[a]
	—	(0.3281)	(0.3270)
SIZE 5	—	0.7980[b]	0.7962[b]
	—	(0.4074)	(0.4068)
SIZE 6	—	0.0360	0.1004
	—	(0.4510)	(0.4527)
ENTRY	—	0.1757	0.1948
	—	(0.2295)	(0.2311)
PINC	—	—	0.0108
	—	—	(0.2472)
GSP	—	—	0.0661[b]
	—	—	(0.0372)
CONSTANT	-3.7568[a]	-5.1642[a]	-5.9276[a]
	(0.4690)	(1.0234)	(1.9894)
Log likelihood function	-53.0456	-19.2143	-17.4198
R-square	0.3675	0.5569	0.5652
Mean of the dependent variable	-4.9699	-4.9699	-4.9699
No. of obs.	190	190	190

a. Significant at the 95 percent confidence level.
b. Significant at the 90 percent confidence level.
NOTE: Standard errors of the coefficients appear in parentheses.
SOURCE: Authors' calculations.

suggesting that (controlling for structure) a profitable banking sector is associated with a higher probability of firm births. Profitable banks could have more opportunities for providing intermediation services and engage in less credit rationing, suggesting a positive relationship with firm births. Alternatively, high profits in the banking sector could merely be indicating profitable market conditions for other industries as well. (We will therefore control for regional economic activity in equation [3].)

The number of banks (*HQS*) is not statistically significant, but BRANCHES, *BANKEMP*, and *HERF* are, suggesting that the greater the number of branches and the more concentrated the banking market (at least as measured by HERF), the lower the probability of firm births. More branches could reflect more of a retail orientation of the banks. Also, the more employees per bank, the higher the probability of firm births.

The statistical significance and the magnitude of *SIZE* 1 suggest that smaller banks (those with less than $5 million in assets) are more involved in firm births than larger banks: the higher the proportion of small banks, the higher the probability of firm births. Finally, the coefficient on ENTRY is positive and statistically significant, implying that the more contestable the banking market (as indicated by a larger value for entry), the higher the probability of firm births.

In equation (3), two more measures of regional activity (*PINC* and GSP) are added to the model to see whether the bank structure and profitability effects are merely reflecting regional economic conditions. Of the added regressors, only *GSP* is statistically significant and only at the 90 percent confidence level. The bank-related coefficient estimates do not change appreciably with the addition of these regressors. In particular, *RETURN* retains its positive and statistically significant value even when we control as much as possible for local economic conditions, suggesting that this variable is doing more than just reflecting a robust local economy.[21]

Partial Sample Results

As previously discussed, the banking data are potentially subject to significant measurement

21 Specifications that included the complete set of economic variables but entered the various bank structure variables separately (instead of the full set) yielded similar results. An exception was our measure of concentration, *HERF*, which was statistically significant only when the *SIZE* variables were included as well.

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TABLE 4

Unit Banking States

Coefficient	(1)	(2)	(3)
WAGE	-0.8847[a]	-0.5494[a]	-0.3466[a]
	(0.1994)	(0.1951)	(0.2724)
TAX	-1.6874	-0.2816	-0.9859
	(1.0677)	(0.9922)	(1.7693)
FIRMS	0.5193[a]	0.3525	0.5890[b]
	(0.1778)	(0.2747)	(0.3543)
POP	0.5029[a]	0.0184	0.2364
	(0.1885)	(0.2915)	(0.3563)
LOANS	—	0.2934	0.1598
	—	(0.3359)	(0.3606)
RETURN	—	36.6800[b]	43.8810[b]
		(22.1410)	(23.4160)
HQS	—	-0.4136	-0.1035
	—	(0.6288)	(0.6956)
BRANCHES	—	-0.3807[b]	-0.4427[b]
	—	(0.2136)	(0.2367)
BANKEMP	—	0.0810[b]	0.1937
	—	(0.4796)	(0.5147)
HERF	—	-0.1543	-0.0565
	—	(0.2107)	(0.2396)
SIZE 1	—	2.7195[a]	2.5134[b]
	—	(1.3662)	(1.4066)
SIZE 2	—	1.9879	1.7754
	—	(1.2694)	(1.3086)
SIZE 3	—	2.3452[a]	2.2601[a]
	—	(0.9367)	(0.9560)
SIZE 4	—	0.7998	0.7543
	—	(1.1518)	(1.1646)
SIZE 5	—	2.0300[b]	1.7276
	—	(1.0934)	(1.1633)
SIZE 6	—	1.1386	1.1365
	—	(1.0377)	(1.0511)
ENTRY	—	1.5843[a]	1.3682[a]
	—	(0.6238)	(0.6601)
PINC	—	—	-0.4996
	—	—	(0.4562)
GSP	—	—	-0.0231
	—	—	(0.0741)
CONSTANT	-4.2875[a]	-10.0850[a]	-5.8005
	(0.6673)	(2.8175)	(4.9151)
Log likelihood function	-13.6582	12.8326	13.7363
R-Square	0.4021	0.7603	0.7677
Mean of the dependent variable	-4.7987	-4.7994	-4.7993
No. of obs.	58	58	58

a. Significant at the 95 percent confidence level.
b. Significant at the 90 percent confidence level.
NOTE: Standard errors of the coefficients appear in parentheses.
SOURCE: Authors' calculations.

error. In states that permit statewide branching, a call report for a consolidated banking unit may include data for branches not located in the SMSA. While the standard errors-in-variables problem in econometrics results in a bias toward zero in the estimated coefficients, we wanted to test whether our results were due to measurement error. We therefore estimate the model without SMSAs in states that have statewide branch banking, and then again without SMSAs in states that allow statewide or limited branching. These results are reported in tables 3 and 4.

In table 3, we reestimate the model omitting SMSAs in states with statewide branching.[22] Although the magnitude of the coefficients tends to be larger, there is no qualitative change in the results in equation (1). In equation (2), the results are again quite similar to those in table 1, except that more of the size variables are statistically significant, but ENTRY is no longer statistically significant. These differences carry over to the results for equation (3). Thus, omitting the SMSAs in the statewide branching states has little effect on our results.

Though we remove most of the measurement problems in the banking variables by omitting the SMSAs in the statewide branching states, the same problems hold to a much lesser degree for the SMSAs in the states with limited branching, which generally allow branches to operate only in contiguous counties.

In table 4, the model is reestimated with only the SMSAs in the unit banking states.[23] These statistical results are not as strong, but our sample has fallen from 259 in table 2, to 190 in table 3, to only 58 in table 4. Of the bank structure and profitability variables (reported in equation [2]), RETURN, *BRANCHES*, *SIZE* 1, *SIZE* 3, and *SIZE* 5 all remain statistically significant. *BANKEMP* and *HERF* lose their statistical significance, but ENTRY once again becomes statistically significant. When we add *PINC* and *GSP* in equation (3), *WAGE* is no longer statistically significant, but the number of establishments *(FIRMS)* is. Of the banking variables, *RETURNS*, *BRANCHES*,

22 Thus, we omit SMSAs in the following states: Alaska, Arizona, California, Connecticut, Delaware, Florida, Hawaii, Idaho, Maine, Maryland, Nevada, New Hampshire, New Jersey, New York, North Carolina, Oregon, Rhode Island, South Carolina, South Dakota, Utah, Vermont, Virginia, and Washington.

23 Thus, only SMSAs in the following states are included in this sample: Colorado, Illinois, Kansas, Missouri, Montana, Nebraska, North Dakota, Oklahoma, Texas, West Virginia, and Wyoming.

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SIZE 1, SIZE 3, and ENTRY all remain statistically significant. In the basic firm-location model (equation [1]), the coefficients retain the same signs and magnitudes, though the state corporate tax rate (TAX) is no longer statistically significant. When we add the bank variables, only WAGE retains its statistical significance.

Clearly, the model does not perform as well with this sample. Even the coefficients in the basic firm location model lose their statistical significance (except for FIRMS). Whether this is due to the small sample size or to possibly peculiar characteristics of the included SMSAs is unclear.[24] Yet even with this sample, bank structure (as measured by *RETURN*, BRANCHES, SIZE 1, SIZE 3, and ENTRY) retains a statistically significant effect on firm births.

In summary, the error-in-variables problem discussed in the previous section does not appear to severely bias our results. Estimates of the model using the full sample are very similar to the estimates obtained using only SMSAs in states with unit or limited branching. When the model is estimated with just the SMSAs in unit branch banking states, the estimates change much more, but the profitability of the banking sector, the number of branches, the proportion of small banks, and entry all have a statistically significant effect on the probability of firm births. Our measure of concentration (*HERF*) retains the same sign and magnitude but is not statistically significant. Banking structure and the availability of credit appear to have measurable effects on firm births.

V. Conclusion

This study presents evidence on the effects of bank structure and profitability on the births of new firms. The attraction of new firms is an important goal of local economic development policies, which often provide public-sector financial incentives. Private-sector financial structure, however, potentially influences firm location through the price and availability of credit from commercial banks.

The empirical analysis examines the relationship between banking activity and regional development from 1980 through 1982. Using bank-level data, we construct measures of lending, profitability, concentration, size, and entry in the banking sectors of 259 SMSAs. Measures of bank structure are included in a standard model of firm location in order to test for independent effects of banking on regional growth as measured by firm births.

As with other firm location studies, we find firm births to be positively associated with low wages, low taxes, and a large number of existing firms. Our analysis, however, also shows that the private banking sector appears to be systematically related to the probability of firm births. Higher rates of firm openings are associated with a healthy and competitive banking sector. Specifically, firm births are associated with higher rates of bank profits, higher numbers of bank employees, lower levels of concentration, higher proportions of small banks, and higher rates of entry of new banks into the SMSA. These results are robust across several specifications and samples and support the position that bank structure and profitability are significant factors in facilitating economic development.

■ **24** The remaining SMSAs in the sample tend to be in states with large energy and agricultural sectors.

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