THE FINANCE-GROWTH NEXUS: EVIDENCE FROM BANK BRANCH DEREGULATION*

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This paper provides evidence that financial markets can directly affect economic growth by studying the relaxation of bank branch restrictions in the United States. We find that the rates of real, per capita growth in income and output increase significantly following intrastate branch reform. We also argue that the observed changes in growth are the result of changes in the banking system. Improvements in the quality of bank lending, not increased volume of bank lending, appear to be responsible for faster growth.

I. INTRODUCTION

This paper provides new evidence that financial markets can directly affect economic growth by studying intrastate branch banking reform in the United States. Since the early 1970s, 35 states have relaxed restrictions on intrastate branching, both by allowing bank holding companies to consolidate bank subsidiaries into branches and by permitting de novo branching statewide. We estimate the change in economic growth rates before and after branch reform relative to a control group of states unaffected by reform using a generalized "difference-in-differences" method. Our results suggest that the rate of real, per capita growth increases significantly following intrastate branch reform. We also find evidence that bank lending quality is the main channel through which the financial sector reform considered here affects economic growth.

The debate on the relationship between growth and finance is an old one. Schumpeter [1969] argued that financial systems are important in promoting innovations; economies with more efficient financial systems grow faster. On the other hand, Robinson [1952] believed that the causality was reversed; economies with good growth prospects develop institutions to provide the

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funds necessary to support those good prospects. In other words, the economy leads, and finance follows.

Recent theoretical developments have fleshed out two likely linkages between financial systems and growth. Financial markets can matter either by affecting the volume of savings available to finance investment [Bencivenga and Smith 1991; Jappelli and Pagano 1993] or by increasing the productivity of that investment [Fernandez and Galetovic 1994; Greenwood and Jovanovic 1990; King and Levine 1993a]. These models show that an improvement in financial market efficiency can act as a lubricant to the engine of economic growth, allowing that engine to run faster.[1]

Empirical evidence linking growth and finance goes back to Goldsmith [1969], McKinnon [1973], and Shaw [1973], who showed that high growth economies tend to have well-developed financial markets, although this evidence did little to resolve the Schumpeter/Robinson debate. More recently, research has demonstrated that the size and depth of an economy's financial system is positively correlated with its future growth in per capita, real income [King and Levine 1993b; De Gregorio and Guidotti 1994]. The evidence from cross-country regressions, however, is plagued by omitted variables problems and must be viewed with some skepticism because the data have been used so intensively by so many researchers (Levine and Renelt [1992], for instance, demonstrate the instability of inference from cross-country regressions).

Despite the advances in the growth literature, the debate over whether financial systems play any causal role in economic growth remains unresolved. In particular, cross-country correlations between rates of economic growth and predetermined measures of financial market depth will not satisfy those predisposed to believe that high growth economies tend to demand large, privately funded financial systems. The observed correlation between financial markets and future growth may reflect causality flowing *from* growth *to* financial systems. High growth economies may gear up their financial systems prior to a growth spurt.

The particular policy change considered here allows us to make progress in resolving this problem. We provide evidence that states did *not* deregulate their banks in anticipation of future good growth prospects. The typical state did not deregulate

1. For a review of this literature see Galetovic [1994] and Pagano [1993].

during the upswing of its business cycle. Moreover, we find only weak evidence that bank lending increased following intrastate branch reform and no evidence that the rate of investment increased following reform. While one can never establish direct causal links beyond doubt with nonexperimental data, these findings clearly suggest that the policy change was not the result of increased growth; probably the reverse is true.

This conclusion is supported by changes in bank lending following branch deregulation. The *quality* of banks' loan portfolios improves significantly after reform. We argue that improvements in lending quality are therefore the key to the beneficial growth effects of branching reforms. Banks do not necessarily lend more, but they appear to lend better. Although the evidence on this is preliminary, they lend cautious support to the recent finding that the principal channel through which finance influences growth is through improved efficiency of investment and not through increased volumes of investment [De Gregorio and Guidotti 1994; King and Levine 1993b].

Previous research has shown that banking markets change substantially once branching is allowed. The banking industry consolidates after branch reform as large bank holding companies acquire banks and convert existing bank subsidiaries into branches [McLaughlin]. Small banks lose market share and previously sheltered banking markets experience significant entry by new banks [Calem 1994; Amel and Liang 1992].[2]

These changes in banking markets may be the source of improved loan monitoring and screening. Little has been done to discern the effects of branch deregulation on the quality of bank intermediation. We conjecture that the beneficial effects occur because the least efficient banks face competition through entry into local markets and management of those banks faces a less restricted market for corporate control. Thus, a more potent selection mechanism tends to improve the average quality of surviving banks. The increased threat of takeover may also improve management's incentive to operate surviving banks better [Berger, Kashyap, and Scalise 1995; Schranz 1993]. Furthermore, increases in the average size of banks improve efficiency because larger banking companies can take advantage of wide branch

2. There is also some international evidence suggesting that banking deregulation is beneficial to the real economy. Harris, Schiantarelli, and Siregar [1994] find that banking deregulation in Indonesia expanded the volume and quality of intermediated credit.

networks, better diversification, and lower costs of monitoring risky loans.

The remainder of the paper is organized into five sections. In Section II we describe the process of intrastate branch reform that has occurred over the past three decades, how the policy changes have affected banking markets, and how we define and date the deregulation. Section III describes the empirical method and presents our estimates of the growth increase following deregulation. In Section IV we consider and reject an alternative interpretation of the improved performance associated with intrastate banking. In particular, we reject the idea that the deregulation of intrastate branch restrictions occurred in anticipation of increased economic growth. Section V provides preliminary evidence on the channels through which banking reform affects growth. Section VI concludes the paper.

II. Intrastate Branch Deregulation

This section briefly describes the history of the changes in intrastate branching laws and the effects that these changes have had on banking markets. Our purpose here is to explain why we focus on this form of deregulation.

A. The Effects of Deregulation on Banking Markets

Banks and bank holding companies have faced restrictions on geographical expansion both within and across state borders. The Douglas amendment to the Bank Holding Company Act of 1956 prevented holding companies from acquiring out-of-state banks unless that state explicitly permitted such acquisitions by statute. Since no state allowed such acquisitions, holding companies were effectively prohibited from crossing state lines, although the Bank Holding Company Act grandfathered nineteen existing multistate holding companies. In 1975 states began introducing laws permitting out-of-state bank holding companies to acquire in-state banks. Furthermore, federal legislators amended the Bank Holding Company Act in 1982 under the Garn-St Germain Act to allow failed banks to be acquired by any holding company, regardless of state laws.

Prior to the 1970s most states also had laws restricting within-state branching, although in many cases a holding company could expand throughout a state by setting up multiple bank subsidiaries. From the middle of the 1970s to the present,

most of these states have deregulated the restrictions on intrastate branching. We focus here on the growth effects of intrastate branch reform. Our conjecture is that these changes reduced the average costs of intermediation by increasing the efficiency of the average bank and by improving the quality of intermediation. Since theories linking financial markets to growth imply that improved intermediation leads to faster economic growth, these theories imply that state economic growth rates will increase after intrastate branch restrictions are lifted. This is the basis of our empirical model.

Previous research indicates that branching reforms have had important effects on the structure of banking markets. Amel and Liang [1992] find significant entry into local markets after intrastate branching restrictions are lifted via de novo branching. Calem [1994] finds that many small banks are acquired and incorporated as branches into large bank holding companies after branching reform. McLaughlin [1994] finds that many multibank holding companies (MBHCs) convert existing and acquired bank subsidiaries into branches following deregulation. Moreover, Savage [1993] shows that over the 1980–1993 period the market share of large banks grew, while concentration at both the state and national level rose.

Overall, the evidence suggests that more efficient banks emerge post-deregulation. Entry and consolidation provide an important selection mechanism to remove less efficient banks. Calem argues that the formation of larger banking organizations allows better exploitation of economies associated with expansion of branch networks. Also, the fact that we see MBHCs convert subsidiaries into branches suggests that cost reductions can be achieved by lowering overhead associated with redundant layers of management, multiple boards of directors, examination of multiple bank subsidiaries, and so on. In addition, increases in size are associated with better diversification [Demsetz and Strahan 1995] and may lead to reduced costs associated with monitoring risky loans [Diamond 1984].

Whether the increased threat of takeover also improves the performance of surviving banks (by strengthening management's incentives to maximize the value of the firm) remains an open question. In Section V we discuss preliminary evidence that lending improves at banks that remain in operation following intrastate branch reform.

In contrast to intrastate branch reform, there is little to sug-

gest that the gradual reduction in barriers to *interstate* banking has had an important effect on the costs of intermediation. Both Calem [1994] and Amel and Liang [1992], for instance, find that banking market structure changed little in states which reformed interstate banking laws permitting MBHCs to expand across state lines by acquiring subsidiary banks. Consequently, this paper focuses on intrastate branch reform.[3]

B. Recent Changes in State Branching Restrictions

The unusual history of United States banking law provides a unique opportunity to study the questions at hand. Most states entered the 1970s with restrictions prohibiting or sharply limiting geographical expansion both within and across state borders. During the next two and a half decades, 35 of the 50 states substantially eliminated restrictions on intrastate branching. All but three states now allow some form of statewide branching.

Reform of restrictions on intrastate branching typically occurred in a two-step process. First, states permitted MBHCs to convert subsidiary banks (existing or acquired) into branches. MBHCs could then expand geographically by acquiring banks and converting them into branches. Second, states began permitting de novo branching, whereby banks could open new branches anywhere within state borders.

The process of branch deregulation varied from state to state. For instance, it appears that West Virginia's state legislature passed a bill lifting most branching restrictions to help an ailing economy. The legislature's actions were ". . . inspired by the state's need for industrial expansion and a greater job base. West Virginia leads the nation in unemployment" [*American Banker,* 04/17/84]. By contrast, the Pennsylvania legislature faced lobbying pressure from large banking companies such as Mellon Bancorp, which argued that "they [Mellon et al.] needed broader powers to meet challenges from national financial institutions

3. There is some evidence, however, that interstate banking did affect commercial banking. Hubbard and Palia [1995] find stronger links between CEO pay and bank performance in states that allow interstate banking, suggesting that managerial labor markets function better (and possibly banks perform better) in states allowing interstate banking. We did test for effects of interstate banking on growth in the analysis presented in the remainder of this paper. We found some increases in growth rates following interstate banking. However, the increases were neither statistically significant nor robust to model specification. To conserve space, these results are not reported in the tables that follow.

and to bolster themselves to compete in an anticipated era of interstate banking" [*Wall Street Journal,* 03/05/82].

In at least six states—Texas, Florida, Mississippi, Tennessee, Louisiana, New Mexico—the relaxation of branch restrictions was initiated by a *national* bank regulator, the Office of the Comptroller of Currency (OCC). The OCC began loosening branching restrictions when it allowed the Deposit Guaranty National Bank of Jackson, Mississippi, to open a branch in Gulfport, Mississippi. Gulfport is more than 100 miles from Jackson. At the time, state banks in Mississippi were allowed to branch only within the county where their principal office was located, or within a 100-mile radius.

The Deposit Guaranty National Bank, as its name suggests, had a national bank charter; and the OCC was—and continues to be—the regulator of national banks. The OCC exploited a provision of the National Bank Act (1864) which specified that a national bank may branch within the state of its location to the same extent that state banks could. The agency ruled that since state savings banks in Mississippi offered traditional banking services, and since such thrifts were allowed to branch freely within the state, the provisions of the National Bank Act allowed commercial banks with national charters to branch freely as well.[4] Commercial banks with national charters in Texas, Florida, Louisiana, Tennessee, and New Mexico soon followed suit in requesting and being granted permission to branch. Faced with this fait accompli, state chartered banks demanded and won similar rights.[5]

The most useful feature of this experience from a research standpoint is that the states deregulated *at different times* during the past 25 years. As a consequence, cross-sectional and time series variation in states' restrictions on geographical expansion permit the use of powerful econometric techniques applied to panel data sets. We use these techniques to reduce the extent of omitted variables, a problem that has plagued previous empirical research efforts, in our model of the determinants of long-run growth.

4. Savings banks are state-chartered institutions regulated by state authorities where they are located as well as by the Federal Deposit Insurance Corporation.

5. For descriptions of the OCC decision and its court challenges, see "Texas Gets Statewide Branching," *American Banker,* 06/27/88, and "National Banks Can Branch Statewide in Mississippi," *Banking Expansion Reporter,* 03/02/87.

Table I describes the history of the deregulation of branching restrictions of the 50 states plus the District of Columbia since 1972. The first column presents the year in which each state permitted branching via merger and acquisition (M&A) through the holding company structure. The second column presents the date at which each state first permitted banks to expand via de novo branching. The dates chosen in Table I reflect the time at which each state *finished* the deregulation process, as detailed in Amel [1993]. These choices in some cases require judgment, since some states deregulated gradually over time. In four cases we chose dates earlier than the literal end of the process of deregulation since we felt that the remaining restrictions no longer imposed a meaningful constraint on branching.[6]

We use these policy changes to determine empirically whether states grow faster once they allow statewide branching. As Table I makes clear, most of the states removed barriers to intrastate branching via M&A first and soon after removed restrictions on de novo branching. Unfortunately, since the dates of both types of intrastate branch reform are so highly correlated, we are unable to identify separately the effects of branching via M&A from the effects of de novo branching. In our empirical model, we use dates associated with deregulation of prohibitions on branching via M&A to construct a measure of intrastate branch reform.

III. The Growth Effects of Branch Reform

This section describes the empirical model, the data, and the definitions of the dependent and independent variables and presents the results of the basic growth model. We also present tests of model robustness and provide estimates of both the short- and long-run growth effects.

6. For instance, in 1982 Pennsylvania began permitting banks to branch in the home office county, in a contiguous county, in a bicontiguous county or in the counties of Allegheny, Delaware, Montgomery, and Philadelphia. In 1990 Pennsylvania permitted unrestricted branching statewide. In the results presented below, we assume that by 1982 Pennsylvania permitted intrastate branching, despite the fact that the process was not finished until eight years later, since the effect of the 1982 law brought Pennsylvania so close to complete intrastate branch freedom. We follow a similar practice for the states of Ohio, Virginia, and Washington. Our results are not sensitive to the alternative dating of deregulation in these four states.

TABLE I
DESCRIPTION OF CHANGES IN INTRASTATE BRANCHING RESTRICTIONS SINCE 1972

States deregulated by 1972	Year M&A branch restrictions lifted	Year branch restrictions lifted via de novo branching
Alaska		
Arizona		
California		
DC		
Delaware		
Idaho		
Maryland		
North Carolina		
Nevada		
Rhode Island		
South Carolina		
South Dakota		
Vermont		
States that deregulated after 1972		
Alabama	1981	1990
Colorado	1991	Still restricted
Connecticut	1980	1988
Florida	1988	1988
Georgia	1983	Still restricted
Hawaii	1986	1986
Illinois	1988	1993
Indiana	1989	1991
Kansas	1987	1990
Kentucky	1990	Still restricted
Louisiana	1988	1988
Massachusetts	1984	1984
Maine	1975	1975
Michigan	1987	1988
Missouri	1990	1990
Mississippi	1986	1989
Montana	1990	Still restricted
North Dakota	1987	Still restricted
Nebraska	1985	Still restricted
New Hampshire	1987	1987
New Jersey	1977	Still restricted
New Mexico	1991	1991
New York	1976	1976
Ohio	1979	1989
Oklahoma	1988	Still restricted
Oregon	1985	1985
Pennsylvania	1982	1990

TABLE I
(CONTINUED)

States that deregulated after 1972	Year M&A branch restrictions lifted	Year branch restrictions lifted via de novo branching
Tennessee	1985	1990
Texas	1988	1988
Utah	1981	1981
Virginia	1978	1987
Washington	1985	1985
Wisconsin	1990	1990
West Virginia	1987	1987
Wyoming	1988	Still restricted
States that have not deregulated		
Arkansas		
Iowa		
Minnesota		

A. An Empirical Model of Growth

We use the dates in column 1 of Table I to construct an indicator variable equal to one for states permitting branching via M&A and zero otherwise. The growth effects of the policy are estimated in a fixed-effects model, as follows:

(1)
$$Y_{t,i}/Y_{t-1,i} = \alpha_t + \beta_i + \gamma D_{t,i} + \epsilon_{t,i} \quad i = 1, \ldots, 50, t = 72, \ldots, 92,$$

where $Y_{t,i}$ equals a measure of real, per capita income (output) during year t in state i, and $D_{t,i}$ is a branching indicator equal to one for states without restrictions on branching via M&A. In this specification β_i measures the state-specific component of long-run economic growth; α_t measures the common, economywide shock to growth at time t; and γ measures the increase in per capita economic growth stemming from branch deregulation.

In constructing $D_{t,i}$, we drop the year in which the deregulation went into effect. We also drop Delaware from the analysis entirely.[7] Thus, we have 21 years times 50 states minus 35 state-

7. We drop Delaware because in 1982 a law was passed providing a tax incentive for credit card banks to operate there. As a result, the share of gross state product in Delaware attributed to the banking industry doubled during the mid-

years in which the deregulation occurred during the sample, leaving a total of 1015 observations.

The model described in equation (1) has a number of advantages. First, the state fixed effects control for time-invariant differences in long-run growth rates due to unexplained factors that differ across states. Examples include income and property tax rates, environmental regulations, public rates of investment, and so on. These fixed effects can also account for the convergence phenomenon documented by Barro and Sala-i-Martin [1992]. Second, the time fixed effects control for the business cycle. Third, this specification is a generalization of the difference-in-differences approach where the effect of deregulation is estimated as the difference between the change in growth before and after deregulation with the difference in growth for a control group not experiencing a change in their deregulation status. In this specification the control group is constructed from the average of all states in the sample, rather than from a different set of states not experiencing any change in their branching laws.

We estimate the model in equation (1) two ways. In the simplest version we use ordinary least squares (OLS). The model is also estimated by weighted least squares (WLS), with weights proportional to the size of the state economy. We use WLS because measurement error in state economic data is likely to be greater for smaller states. Measurement problems associated with interstate commerce are likely to be more pronounced in smaller states. Smaller states are also more likely to depend on a limited number of industries, leading to greater susceptibility to industry-specific shocks.[8] We weight by the size of the state economy at the beginning of the period. In all cases we report heteroskedasticity consistent standard errors [White 1980].

Table I shows that many states in the South and Midwest deregulated around the same time, leading to the possibility that regional business cycle effects could drive the estimate of the growth effect (γ). While there is no a priori reason to suspect that regional business cycles will introduce a bias, we also present es-

dle 1980s. Our growth results are not sensitive to the exclusion of Delaware. Some of the results on changes in bank lending presented in Sections IV and V are affected, since Delaware's banking market grew so fast after reform. This growth, however, occurred because of the entry of credit card banks.

8. In fact, the residual variance from equation (1) decreases with the size of a state's economy.

timates from an augmented version of the model in equation (1) allowing the time effects (i.e., the business cycle effects) to vary across four broad regions within the United States. This specification is included mainly as a robustness check. The model with regional effects follows:

$$Y_{t,i}/Y_{t-1,i} = \alpha_{t,j} + \beta_i + \gamma D_{t,i} + \epsilon_{t,i}, \tag{2}$$

where j indexes the four regions.[9] In this model, $\alpha_{t,j}$ controls for business cycle effects in region j at time t. This approach, which reduces the likelihood that our estimate of γ will be biased by a correlation between regional cycles and branch reform, comes at a high cost in terms of lost degrees of freedom. In the model of equation (2) we sacrifice 63 additional degrees of freedom in adding the four time-varying regional effects.

We use two measures of state economic activity, personal income and gross state product, to construct per capita growth rates. Each of these series is published annually by the U. S. Department of Commerce. Annual state population figures are from the U. S. Census Bureau. The two measures of economic activity differ primarily in their treatment of capital income. Personal income measures the income of state residents while gross state product measures the total incomes of factors of production located within the state. For personal income, capital income is allocated based on the state of residence of the owner of capital while for gross state product capital income is allocated based on the physical location of the capital itself.

We convert nominal personal income to constant dollars using a national price deflator, the Consumer Price Index. As a result, real personal income may be affected by changes in relative prices. For instance, real personal income in oil states will increase (decrease) as oil prices rise (fall). Gross state product, by contrast, is converted to constant dollars using industry-specific price deflators, so it is better insulated from changes in relative prices. The average (unweighted) rate of growth in real per capita personal income was 1.6 percent per year from 1972 to 1992.

9. We split the lower 48 states into four large regions of approximately equal size. Region 1 (Northeast) contains CT, MA, MD, ME, NH, NJ, NY, PA, RI, VT, and WV; Region 2 (South) contains AL, AR, DC, FL, GA, KY, LA, MS, NC, OK, SC, TN, TX, and VA; Region 3 (Midwest) contains IA, IL, IN, KS, MI, MN, MO, NE, ND, OH, SD, and WI; Region 4 (West) contains AZ, CA, CO, ID, MT, NM, NV, OR, UT, WA, and WY. Hawaii and Alaska are dropped from the model in equation (2).

Gross state product grew by 1.4 percent per year from 1978 to 1992.[10]

The results of the growth model outlined above appear in Table II. The first two rows present the OLS and WLS results for the basic model (equation (1)) using personal income to construct the dependent variable. The third and fourth rows present the OLS and WLS results for the model which includes time-varying regional effects (equation (2)). The last four rows repeat these specifications using gross state product to construct the dependent variable.

Overall, the results consistently show that real, per capita economic growth increases significantly following intrastate branch deregulation. The coefficient on the deregulation indicator variable is positive and statistically significant at the 5 percent level in each of the eight specifications. The point estimates are also economically large, indicating that annual growth rates increase by 0.51 to 1.19 percentage points following intrastate branch deregulation.

B. Robustness Tests

The first concern with robustness of our growth results is that they may be driven by a few states experiencing a particularly strong growth spurt following branch reform. To investigate this possibility, we look separately at the 35 deregulating states in Figure I. Here we compare the change in the average annual growth rate in personal income after deregulation with the change in growth rates for the control states over the same period. Each of the 35 deregulating states appears as a pair of points on the graph: the state's two-letter zip code name indicates the change in average annual growth rate of that state (the "treatment state") following deregulation; a triangle appears directly above (or below) the state name indicating the change in the mean growth rate over the same period for all states that did not alter their regulatory regime during the 1972–1992 period (the "control group"). For example, Wyoming (represented as "WY" in the top right corner) is recorded as having increased its mean annual growth rate following branching reform in 1988 by 1.7 percentage points. The associated triangle below WY indi-

10. Since the Department of Commerce changed the base-year for the industry price deflators in 1977, we could not construct a consistent growth series prior to 1978 using gross state product.

TABLE II
GROWTH REGRESSIONS: BASIC MODELS

Growth based on personal income:	Estimated percentage point change in growth	Adjusted R^2 (number of observations)
1. Basic model, OLS	0.94*	49%
	(0.26)	(1015)
2. Basic model, WLS	1.19*	70%
	(0.24)	(1015)
3. Regional effects, OLS	0.51*	62%
	(0.23)	(974)
4. Regional effects, WLS	0.59*	78%
	(0.18)	(974)
Growth based on gross state product:		
5. Basic model, OLS	1.03*	43%
	(0.36)	(668)
6. Basic model, WLS	1.08*	65%
	(0.30)	(668)
7. Regional effects, OLS	0.69*	54%
	(0.33)	(641)
8. Regional effects, WLS	0.84*	72%
	(0.24)	(641)

This table presents estimates of the increase in growth following relaxation of intrastate branching restrictions. Table I presents the dates at which each state relaxed its restrictions on branching. We present a regression of real, per capita growth based on personal income or gross state product on time and state fixed effects, and an indicator variable equal to one for states with no restrictions on branching via M&A. We also estimate a regional effects model in which the time effects are interacted with a set of four regional indicators. Delaware is dropped from all regressions, while Alaska and Hawaii are dropped from the regional effects model. Also, the year in which each state deregulated was dropped. Growth data for personal income are from 1972–1992 and for state product from 1978–1991. Standard errors appear below coefficients in parentheses. An asterisk indicates statistical significance at the 5 percent level; reported standard errors are heteroskedasticity-consistent [White 1980]. Coefficients are multiplied by 100.

cates that all states which did not change policy experienced a *decline* in growth of approximately 0.6 percentage points over the same period.

Figure I suggests that the growth regression results in Table II are not particularly dependent on any one state.[11] Of the 35

11. We have also checked the robustness of the growth effects of branch reform by estimating the model separately for small and large states, where a small state is defined as any state with total personal income (*not* per capita personal income) below the median at the beginning of the period. Overall, these results (not shown) provide no support for the hypothesis that the growth effects differ based on state size. The point estimate of the growth effect is larger for the large states when growth is constructed from personal income, but the growth effect is found to be smaller for the large states when growth is based on gross state product. In neither case can we reject the hypothesis that the growth effects are equal for large and small states. These results are available from the authors.

Change in Mean Growth Rates: Treatment versus Control States

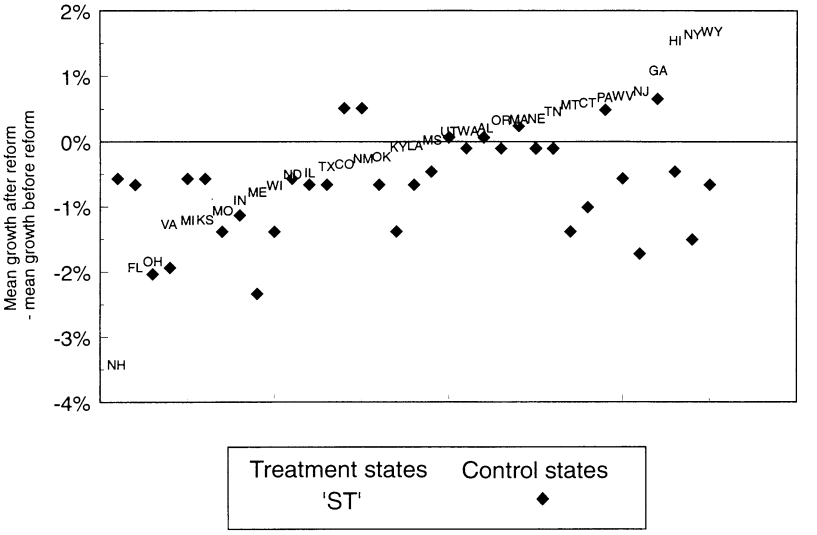

FIGURE I

states that deregulated since 1972, all but 6 states performed better than the corresponding control states. (The exceptions are New Hampshire, Florida, Michigan, Kansas, Colorado, and New Mexico). Even when the deregulating states experienced growth declines, the control states generally fared even worse.[12] The relative improvement in growth performance following intrastate branching appears to be a general phenomenon.[13]

All of the results presented to this point are, of course, also

12. Figure I suggests that New York and New Jersey fared particularly well following deregulation (relative to their controls). In order to test the influence of these two states, we reestimated the parameters in Table II without these two states. We find that results do not change significantly.

13. The regressions in Table II rely on those states that did not change their branching policy regime as a "control" to eliminate confounding effects such as the business cycle and to isolate the growth effects of intrastate branching. To further refine these controls, we estimated the growth model controlling for three sector-specific shocks to output (not shown). This model is expected to control for sector-specific shocks that may have affected states in the same region differently. The model includes interaction terms between each state's share of value added coming from the energy, agriculture, and government sectors and the time effects. We include these three sectors since we know that there were important disturbances to each in the 1980s, the decade during which most of the branch deregulation occurred. Nevertheless, the growth effects of branch deregulation remain positive and statistically significant in this model. These results are available from the authors on request.

subject to the criticism that we have omitted an important variable linked to growth. The fixed effects approach is less vulnerable to this problem than cross-sectional methodologies used in the extant evidence comparing growth across countries. Our approach could be biased, however, if many of the states in our sample experienced pro-growth changes in policy around the same time that the state deregulated its banking system. Such coincident policy shifts could occur following changes in the control of the state legislature or governorship, for example. Table III presents evidence that no such coincident policy shifts occurred in our sample. We augment our growth model with two variables measuring the fiscal policies of the state government, the ratio of public investment to income and the ratio of tax receipts by the state government to total income (lagged one period). We find no significant changes in our estimate of the growth effects of branch deregulation, even after controlling for these variables.[14]

Another plausible explanation for the observed increase in growth rates following branching reform is that states deregulated when their economies were doing poorly. Following reform, the economy may have nowhere to go but up if the policy change occurred in the trough of a recession. The timing of the policy change may create a spurious association between branching reform and growth.[15]

This possibility is suggested by the fact that 25 of the 35 states that deregulated during the sample period changed policy after 1984, the first of many years of dramatically increased bank failure rates.[16] It is possible that, confronted with a severe negative shock to the economy and to the banking system, small banks—the traditional constituency for branching restrictions—dropped their opposition to branching in order to find higher pur-

14. We have also controlled for concurrent changes in general economic policy by including three variables measuring the proportion of control of the state government by one of the two major parties: an indicator variable equal to one if the Governor is a member of that party, and two continuous variables between zero and one measuring the percentage of state senators and state assembly members in that party. These results provide no evidence that these variables affect the state growth rate (suggesting that the economy grows equally fast under either party) or that their omission has any effect on the estimate of the coefficient on the intrastate branch indicator.

15. This is a concern when there are state-specific business cycles since these cycles will not be controlled for by the year fixed effects and the time-varying regional fixed effects in the models estimated so far.

16. Over the nine-year interval between 1984 and 1992, 1296 banks were subject to FDIC intervention. In contrast, a mere 25 banks failed over the nine years prior to 1984 [FDIC 1993].

TABLE III
GROWTH REGRESSIONS: INCLUDING STATE FISCAL POLICY VARIABLES

Growth based on personal income:	Estimated percentage point change in growth	State capital expenditure/ income	State tax receipts/ income	Adjusted R^2 (number of observations)
1. Basic model, OLS	0.98*	0.012	−0.018	50%
	(0.25)	(0.019)	(0.014)	(994)
2. Basic model, WLS	1.20*	0.005	−0.012	71%
	(0.24)	(0.015)	(0.010)	(994)
3. Regional effects, OLS	0.61*	0.015	−0.013	63%
	(0.23)	(0.014)	(0.008)	(953)
4. Regional effects, WLS	0.63*	0.018	−0.006	78%
	(0.18)	(0.013)	(0.008)	(953)
Growth based on state product:				
5. Basic model, OLS	1.00*	−0.111*	0.028	44%
	(0.36)	(0.047)	(0.018)	(654)
6. Basic model, WLS	1.04*	−0.086*	0.029	65%
	(0.30)	(0.044)	(0.016)	(654)
7. Regional effects, OLS	0.70*	−0.107*	0.027	55%
	(0.33)	(0.041)	(0.014)	(627)
8. Regional effects, WLS	0.85*	−0.073	0.016	72%
	(0.25)	(0.039)	(0.013)	(627)

This table presents estimates of the increase in growth following relaxation of intrastate branching restrictions. Table I presents the dates at which each state relaxed its restrictions on branching. We present a regression of real, per capita growth based on personal income or gross state product on time and state fixed effects, an indicator variable equal to one for states with no restrictions on branching via M&A, the state government's capital expenditure per dollar of income (product), and the state government's tax receipts per dollar of income (product). We also estimate a regional effects model in which the time effects are interacted with a set of four regional indicators. Delaware and DC are dropped from all regressions, while Alaska and Hawaii are dropped from the regional effects model. Also, the year in which each state deregulated was dropped. Growth data for personal income are from 1972–1992 and for state product from 1978–1991. Standard errors appear below coefficients in parentheses. An asterisk indicates statistical significance at the 5 percent level; reported standard errors are heteroskedasticity-consistent [White 1980]. Coefficients are multiplied by 100.

chase prices when exiting the distressed banking industry. Regulators may have pushed for liberalized branching to increase bank consolidation and to wean out weaker banks.[17]

Figure I shows informally that there is no correlation between the business cycle and the timing of deregulation. All states whose zip code labels appear below the zero line experienced decreased average growth following deregulation. As is evi-

17. We are grateful to Charles Calomiris and to Stavros Peristiani for suggesting this possibility.

dent in the figure, state labels are evenly distributed above and below zero. Approximately half the states (18 of the 35) had *lower* growth after deregulation. Of the seventeen states that did experience growth spurts after intrastate branching was allowed, state-by-state *t*-tests fail to reject the hypothesis that the average growth rate did not increase following deregulation (even for those states that increased growth rates most: Wyoming, New York, and Hawaii). In addition, Figure I shows that states deregulating prior to the onset of problems in banking in 1984 enjoyed faster growth following reform than the control group of states. In fact, *all* of the early deregulators grow faster than the control states following reform.[18]

We deal more formally with the problem of the timing of deregulation relative to the local business cycle by introducing lags of the dependent variable into our basic growth model. This model removes state-specific business cycle effects as well as national and regional cycles, whereas the models of Table II remove only the national and regional cycles.[19] Table IV presents the growth model with three lags of the state per capita growth rate as additional explanatory variables. This approach removes the potential for the bias that would result from a correlation between the timing of deregulation and the state of the business cycle.

As shown in Table IV, we continue to estimate a large, positive, statistically significant increase in growth following bank branch deregulation. The level of significance, however, has weakened. Five of the eight regressions establish a growth-deregulation association that is significant at the 5 percent level, and two regressions establish the association at 10 percent. This suggests that there is no strong correlation between the local business cycle and the timing of deregulation.

C. Growth Dynamics

The point estimates in Tables II–IV may seem too large to reflect the long-run growth effects of branch bank reform. An in-

18. Ten states allowed intrastate branching prior to 1984: Alabama, Connecticut, Georgia, Maine, New Jersey, New York, Ohio, Pennsylvania, Utah, and Virginia.

19. OLS produces inconsistent estimates of autocorrelation coefficients in dynamic models with fixed effects [Hsiao 1986]. However, the asymptotic bias is probably limited here since we have long panels: 11 years for the gross state product series and 21 years for the personal income series.

TABLE IV
GROWTH MODEL WITH LAGGED DEPENDENT VARIABLES

Growth based on personal income:	Estimated percentage point change in growth	$Growth_{t-1}$	$Growth_{t-2}$	$Growth_{t-3}$	Adjusted R^2 (number of observations)
1. Basic model, OLS	0.88*	0.12	−0.05	−0.06	50%
	(0.27)	(0.09)	(0.06)	(0.08)	(1015)
2. Basic model, WLS	0.97*	0.18*	0.04	0.02	72%
	(0.23)	(0.05)	(0.04)	(0.04)	(1015)
3. Regional effects, OLS	0.49*	0.06	−0.04	0.01	62%
	(0.23)	(0.08)	(0.07)	(0.08)	(974)
4. Regional effects, WLS	0.52*	0.14*	0.02	0.05	78%
	(0.18)	(0.05)	(0.05)	(0.05)	(974)
Growth based on gross state product:					
5. Basic model, OLS	0.75	0.16*	0.05	−0.04	39%
	(0.45)	(0.06)	(0.05)	(0.08)	(521)
6. Basic model, WLS	0.85*	0.15*	0.08	−0.03	60%
	(0.37)	(0.05)	(0.06)	(0.07)	(521)
7. Regional effects, OLS	0.62	0.12*	0.03	0.02	48%
	(0.42)	(0.06)	(0.05)	(0.07)	(500)
8. Regional effects, WLS	0.52	0.18*	0.07	0.01	69%
	(0.30)	(0.05)	(0.05)	(0.07)	(500)

This table presents estimates of the increase in growth following relaxation of intrastate branching restrictions. Table I presents the dates at which each state relaxed its restrictions on branching. We present a regression of real, per capita growth based on personal income or gross state product on time and state fixed effects, three lags of the dependent variable, and an indicator variable equal to one for states with no restrictions on branching via M&A. We also estimate a regional effects model in which the time effects are interacted with a set of four regional indicators. Delaware is dropped from all regressions, while Alaska and Hawaii are dropped from the regional effects model. Also, the year in which each state deregulated was dropped. Growth data for personal income are from 1972–1992 and for state product from 1981–1991 (three years are lost with the addition of the lagged dependent variables). Standard errors appear below coefficients in parentheses. An asterisk indicates statistical significance at the 5 percent level; reported standard errors are heteroskedasticity-consistent [White 1980]. The coefficient on the branching indicator is multiplied by 100.

crease of 0.5 percentage points in annual growth rates is an increase of about one-third of the (unconditional) mean growth rate over the sample period. We argue that these estimates are indeed plausible because they come from relatively high frequency data that may be dominated by the years just after branch reform. In our sample, 24 of the 35 deregulating states did so after 1985. For these states we observe, at most, seven years of growth experience after reform. The coefficient estimates will reflect, in large part, the growth experience immediately after reform of branching laws.

How can the short-run effects of branch reform be so large? We know that a significant fraction of the economy's capital is held by the banking system. In fact, commercial banks' share of credit to all nonfinancial borrowers ranged from a high of just over 35 percent in 1974 to a low of about 22 percent in 1994 [Edwards and Mishkin 1995]. A better banking system can therefore influence growth in three ways: (1) by increasing the value of the existing stock of capital held within the banking system; (2) by lowering the costs of intermediation and thereby increasing the amount of savings and investment; and (3) by improving the quality of investment. The first effect, while not sustainable, can have large effects on economic growth immediately following reform because a small change in the value of the stock of existing capital will have a large effect on economic output.[20]

The long-run effects of branch restrictions on growth, of course, can depend only on the quantity and quality of *changes* to the capital stock (i.e., investment).[21] In order to estimate these long-run effects—to the extent allowed by our data—we split our deregulation indicator variable into three indicator variables, one for the first five-year window following deregulation, one for the second five-year window, and the third for ten years or more after deregulation. The last variable is intended to measure the longer run growth impact of branch deregulation, while the first two measure growth during the transition period.

For this analysis we want to extract information about the long-run growth effects of reform from states which allowed intrastate branching before 1970. (Since most states observed to relax branching restrictions during our sample period did so in

20. An example can illustrate how important changes in the value of the existing capital stock can be. Suppose that better monitoring of bank loans following branch deregulation leads to an increase in the market value of those loans of 20 percent. Assume that the aggregate production is a constant returns to scale, Cobb-Douglas function of capital and labor. In equilibrium the income shares of labor and capital equal the elasticity of output with respect to each of these two inputs (assuming competitive factor markets). With capital's share of income at about 25 percent and commercial banks holding about 25 percent of total credit to nonfinancial sectors, the assumed 20 percent increase in the (market) value of bank loans would increase per capita income by 1.25 percent. This 1.25 percent jump in income spread out over five years would increase the rate of economic growth by 0.25 percent per year, or about one-half of the measured growth effect following branch reform (based on the model with regional effects).

21. If growth is governed by exogenous technical change, financial innovations have no long-run consequences [Solow 1956]. In endogenous growth models, however, higher levels of savings and investment or higher quality investment can raise long-run growth [Grossman and Helpman 1991; Lucas 1988; Romer 1986].

the 1980s, they contribute little to estimating the long-run changes in growth following branch reform.) To do this, we drop the state fixed effects from the model. These fixed effects remove each state's mean growth rate from the data and thereby prevent the early deregulators (e.g., California) from contributing information toward estimating the long-run growth parameter in the model. In the absence of fixed effects, we need to control for the convergence phenomenon documented in Barro and Sala-i-Martin [1992]. We do this by adding the beginning of period per capita income to the model. This eliminates the potential for bias that would occur if high-income states also tended to deregulate early. We also include three lags of the dependent variable.

The results, presented in Table V, provide some evidence that the growth effects of branching diminish after ten years. The estimated growth increase after ten years is less than the increase in growth during the first ten years in six of the eight regressions. (The difference, however, is statistically significant in only two cases.) For example, in the model based on personal income, we estimate growth increases of 0.3 to 0.9 percentage points during the first ten years, slowing to 0.2 to 0.3 percentage points ten years after reform.

While we cannot reliably estimate changes in long-run growth rates, the available data do not rule out the possibility that the growth effects associated with branching restrictions extend beyond ten years. In Table V the growth increase following intrastate branching lasts more than ten years in four of eight regressions at a 5 percent significance level and in one more regression at a 10 percent significance level. The data, however, do not allow us to determine how far beyond ten years these growth effects extend.

IV. Does Bank Branch Deregulation Lead to Faster Growth?

So far we have argued that following branch reform, states' economic growth increased significantly. We have shown that the growth-deregulation association survives refinements to the control group, inclusion of other possible pro-growth policy changes coinciding with branching reform, and controlling for local business cycles.

The natural question of interest, then, is whether the change in intrastate branching policy and the associated changes in the banking industry contributed to improved economic perfor-

TABLE V

GROWTH DYNAMICS

Growth based on personal income	Growth effect: Years 1–5	Growth effect: Years 6–10	Growth effect: Years 10+	$Growth_{t-1}$	$Growth_{t-2}$	$Growth_{t-3}$	Lag of per capita income	Adjusted R^2 (number of observations)
1. Basic model, OLS	0.59*	0.86*	0.34	0.14	−0.03	−0.04	−0.38*	52%
	(0.23)	(0.23)	(0.22)	(0.08)	(0.06)	(0.08)	(0.13)	(1015)
2. Basic model, WLS	0.61*	0.86*	0.34*	0.20*	0.06	0.04	−0.29*	73%
	(0.21)	(0.22)	(0.16)	(0.05)	(0.04)	(0.04)	(0.08)	(1015)
3. Regional effects, OLS	0.35	0.37	0.17	0.08	−0.03	0.02	−0.29*	64%
	(0.20)	(0.20)	(0.19)	(0.08)	(0.07)	(0.08)	(0.11)	(974)
4. Regional effects, WLS	0.31*	0.38*	0.21	0.16*	0.04	0.07	−0.28*	79%
	(0.16)	(0.19)	(0.13)	(0.05)	(0.04)	(0.05)	(0.09)	(974)
Growth based on gross state product								
5. Basic model, OLS	0.77*	0.94*	0.63*	0.21*	0.09	0.03	−0.07*	41%
	(0.30)	(0.30)	(0.27)	(0.06)	(0.05)	(0.07)	(0.03)	(521)
6. Basic model, WLS	0.64*	0.83*	0.48	0.21*	0.13*	0.06	−0.09*	62%
	(0.26)	(0.33)	(0.26)	(0.05)	(0.06)	(0.07)	(0.03)	(521)
7. Regional effects, OLS	0.60*	0.65*	0.67*	0.15*	0.06	0.07	−0.04	50%
	(0.29)	(0.27)	(0.27)	(0.06)	(0.05)	(0.07)	(0.02)	(500)
8. Regional effects, WLS	0.43*	0.57*	0.59*	0.23*	0.11*	0.08	−0.08*	69%
	(0.21)	(0.24)	(0.24)	(0.04)	(0.04)	(0.07)	(0.03)	(500)

This table presents estimates of the increase in growth following relaxation of intrastate branching restrictions. Table I presents the dates at which each state relaxed its restrictions on branching. We present a regression of real, per capita growth based on personal income or gross state product on time fixed effects, state per capita income at time $t-1$, three lags of the dependent variable, and three nonoverlapping indicator variables for states with no restrictions on branching via M&A: one for years 1 to 5 after deregulation, one for years 6 to 10 after deregulation, and one for all years beyond 10. We also estimate a regional effects model in which the time effects are interacted with a set of four regional indicators. Delaware is dropped from all regressions, while Alaska and Hawaii are dropped from the regional effects model. Also, the year in which each state deregulated was dropped. Growth data for personal income are from 1972–1992 and for state product from 1981–1991 (three years are lost with addition of the lagged dependent variables). Standard errors appear below coefficients in parentheses. An asterisk indicates statistical significance at the 5 percent level; reported standard errors are heteroskedasticity-consistent [White 1980]. The coefficients on the branching indicators and the lag of income are multiplied by 100.

mance.[22] Or did causality flow from economic growth to branching reform? Perhaps state legislatures relaxed branching restrictions anticipating faster growth and the need to finance attractive projects.

For evidence on the motives of state legislatures, we again look to Figure I. As discussed before, Figure I suggests that half the deregulating states did not experience an increase in average growth rates following intrastate branching. This is not consistent with the notion that branching restrictions were relaxed in anticipation of an economic boom.

Stronger evidence on legislature motives is found in changes in bank lending following reform. If states deregulated branching rules anticipating the need to finance a future economic boom, then we should see bank lending increase following deregulation. The increased demand to finance high-yielding projects should be reflected in increased lending activity. Table VI presents estimates of the change in loan growth after states lifted branching restrictions. The change in loan growth in deregulating states is estimated relative to a control group of states that did not change policy using the same fixed effects model employed in the growth regressions (Table II).

We use two series of bank loan data in Table VI: total loans and commercial loans. The latter is the sum of commercial and industrial loans (C&I loans) and commercial real estate loans. The commercial loan category deserves special attention because it is likely to be closely linked to commercial investment and economic conditions. Only commercial bank loan data are used. Thrifts (an important source of home mortgage loans) and nonbank banks (which provide substantial volumes of consumer loans) are excluded since the branching policy changes considered here affect only commercial banks directly.

The loan variables in Table VI are based on all loans held by individual banks operating within each state as of the end of each calendar year.[23] Bank-level loan data are taken from end-of-year

22. To the extent that the political process leading to intrastate branching is not completely captive to economic influences, the evidence presented so far reduces the endogeneity problems inherent in the cross-country studies establishing simple correlations between (lagged) proxies of financial sector development and economic growth [King and Levine 1993b; De Gregorio and Guidotti 1994].

23. Loans recorded in a bank's balance sheets ("loans held") are not necessarily loans originated by that bank. Banks sell some of the loans originated by them in secondary loan markets; they also buy loans originated by others. For the purposes of this paper, we are interested in loans originated rather than loans held. Although we do not have origination data, loans held should serve as a reasonable

TABLE VI
BANK LOAN GROWTH FOLLOWING INTRASTATE BRANCH DEREGULATION

Real growth in loans (mean = 1.0%):	Estimated percentage point change in loan growth	Adjusted R^2 (number of observations)
1. Basic model, OLS	0.79	6%
	(3.60)	(1015)
2. Basic model, WLS	2.62*	21%
	(1.22)	(1015)
3. Regional effects, OLS	0.40	7%
	(2.78)	(974)
4. Regional effects, WLS	1.85*	23%
	(0.87)	(974)
Real growth in commercial loans (mean = 1.0%):		
5. Basic model, OLS	−0.47	4%
	(5.12)	(767)
6. Basic model, WLS	3.29	9%
	(1.70)	(767)
7. Regional effects, OLS	−1.20	5%
	(4.00)	(736)
8. Regional effects, WLS	2.34	9%
	(1.24)	(736)

This table presents estimates of the change in loan growth (aggregated to the state level) following relaxation of intrastate branching restrictions. Table I presents the dates at which each state relaxed its restrictions on branching. We present a regression of the growth rate of all loans and the growth rate of commercial loans (commercial and industrial loans plus commercial real estate loans) on state and time fixed effects and an indicator variable equal to one for states with no restrictions on branching via M&A. We also estimate a regional effects model in which the time effects are interacted with a set of four regional indicators. Delaware is dropped from all regressions, while Alaska and Hawaii are dropped from the regional effects model. Also, the year in which each state deregulated was dropped. Loan growth data are from 1972–1992; commercial loan data are from 1977–1992. Standard errors appear below coefficients in parentheses. An asterisk indicates statistical significance at the 5 percent level; reported standard errors are heteroskedasticity-consistent [White 1980]. Coefficients are multiplied by 100.

Quarterly Reports of Condition filed by all commercial banks over the 1972–1992 period. Data on C&I loans and commercial real estate loans are available over the 1976–1992 period. Loans held by all commercial banks in each state were summed to derive the total volume of loans in each state.

As shown in Table VI, we find little evidence that lending increased after intrastate branching was allowed. Of the eight regressions, only two record significant increases in loan growth (at the 5 percent level). Two regressions record decreases in com-

proxy. Most banks hold C&I and commercial real estate loans originated by them on their balance sheets.

mercial loan growth rates after the regime change (although the effect is not significant).

Although there is no strong evidence that lending increased at the time of the branching policy changes, we cannot entirely rule out this possibility. Two of the eight regressions in Table VI indicate significant growth in total loans. Two other regressions are marginally significant in this table (the two WLS commercial loan regressions). If lending did increase, and if states indeed deregulated in anticipation of good growth prospects, then branch deregulation should be observed to have little (or at least reduced) effect on growth once we account for that lending increase.

We have tested this possibility by reestimating the basic growth regressions in Table II, this time adding the growth rate of commercial and industrial (C&I) loans as a control variable. These results (not shown) indicate that C&I loan growth is positively correlated with the state economic growth rate. Seven of eight estimated regressions establish a significant positive correlation between these two variables. Despite controlling for this relation, the deregulation effect continues to be positive and significant. Moreover, the point estimates are almost identical to those in Table II (which does not control for loan growth). We conclude that even if lending did increase following branch reform, it does not account for the observed increase in growth following branch deregulation.[24]

To summarize, there is little to suggest that intrastate branching was prompted by the anticipated need to finance unusually good growth in states' economies.[25] This by no means rules out the possibility that some states acted for these reasons. However, we have little reason to believe that these were the dominant motives for most deregulating states or that they fully explain the observed growth-deregulation association. We have to look elsewhere to explain the observed increase in growth following branch reform.

24. These results are available on request.

25. This conclusion is further strengthened by evidence from investment data (not presented here, but available on request). We examined the ratio of capital expenditures in manufacturing to manufacturing value added between 1977 and 1991. (Source: Census of Manufactures and the Annual Survey of Manufactures as made available by DRI.) We found no increase in the rate of investment following branching reform. Like the bank lending results, this finding is inconsistent with the notion that intrastate branching was allowed in anticipation of an economic boom.

V. Transmitting Finance to Growth: Efficiency of Investment versus the Level of Investment

If bank branch reform had real effects, and this explains at least some of the observed improvement in economic performance once intrastate branching was allowed, what are the channels transmitting such effects? While cross-country studies often find a positive relation between growth and financial development, there is less evidence on the channels by which financial institutions affect the real economy.

The debate centers on the relative importance of two broad channels. One possible influence may be that improved intermediation increases the level of investment. This view was emphasized by McKinnon [1973] and Shaw [1973] when interpreting the early cross-country evidence. As the financial sector develops, it is better able to mobilize savings and translate them into investment. Financial markets insure individuals and firms against risk associated with their liquidity needs, thereby allowing them to invest in productive (but illiquid) assets and technologies [Bencivenga and Smith 1991; Levine 1992; Saint-Paul 1992].

An alternative interpretation of the finance-growth nexus is that better financial intermediation improves the *efficiency* of investment even if it does not increase the *level* of investment. Better screening and monitoring of investors by banks may improve the marginal productivity of capital [Goldsmith 1969; Greenwood and Jovanovic 1990; Fernandez and Galetovic 1994]. Evidence in support of this view is offered by De Gregorio and Guidotti [1994] who find that 75 percent of the positive growth-finance correlation remains even after accounting for cross-country variation in investment levels.

Our finding that loan growth does not change and that controlling for loan growth does not reduce the association between branch deregulation and growth is consistent with the De Gregorio and Guidotti conclusion. But branch liberalization by states has some unique advantages in answering questions about finance-growth links. Chief among them is that we can observe directly the behavior of banks after the policy change.

Did the quality of financial intermediation by banks improve following branching deregulation? To answer this question satisfactorily, we would like data on bank borrowers such as the productivity and longevity of the typical bank borrower (especially

among bank-dependent firms such as small businesses). But such data are conspicuous by their absence.[26] Without such borrower information, the only available evidence about bank lending quality comes from banks' balance sheets.

Table VII produces evidence of improved bank lending quality following deregulation using the same fixed effects model. These results provide preliminary evidence that banks' improved their screening and monitoring of borrowers after intrastate branching. The first loan quality indicator used in Table VII is the fraction of total loans classified as "nonperforming."[27] End-of-year nonperforming loan amounts for all commercial banks over the 1982 to 1992 period are taken from *Quarterly Reports of Condition*. A state-level aggregate nonperforming loan amount is derived by summing over all banks in each state. The final variable of interest is the ratio of nonperforming loans to total loans held by all banks in each state.

As Table VII indicates, nonperforming loans decline dramatically after branch restrictions are lifted. The ratio of nonperforming loans to total loans declines by 0.24 to 0.77 percentage points, depending on the model estimated. Since the mean nonperforming to total loan ratio for the entire sample is 2 percent, this decline represents a reduction of 12 to 38 percent relative to the unconditional mean.

A second loan quality indicator is the fraction of loans written off during the year. Net charge-offs (gross charge-offs less recoveries) for individual banks are again taken from end-of-year *Quarterly Reports of Condition* filed by all commercial banks over the 1976–1992 period. State-level total charge-offs are derived, and the dependent variable is the ratio of charge-offs to total loans. As shown in Table VII, we find that a significantly smaller share of loans is charged off following branch reform. The decline is significant for all four regressions, and the decrease is at least 35 percent of the unconditional mean.[28]

Another balance sheet indicator of better lending by banks is

26. Even the amount of bank lending to small business, let alone information on borrowers, is not readily available. The *Quarterly Reports of Condition* record information on the amount of small loans made, but this information dates back only to 1993.

27. All loans 90 days or more past due and nonaccrual loans are classified as nonperforming loans.

28. Shrinking nonperforming loans and reduced charge-offs need not reflect superior screening and monitoring of borrowers. Instead, they may reflect changes in the bank loan portfolio; banks may now be making fewer risky loans.

TABLE VII
BANK LOAN QUALITY REGRESSIONS

Nonperforming loans/loans (mean = 2%):	Estimated percentage point change	Adjusted R^2 (number of observations)
1. Basic model, OLS	−0.77*	46%
	(0.17)	(523)
2. Basic model, WLS	−0.24	59%
	(0.19)	(523)
3. Regional effects, OLS	−0.63*	60%
	(0.15)	(502)
4. Regional effects, WLS	−0.34*	73%
	(0.12)	(502)
Charge-offs/loans (mean = 0.76%):		
5. Basic model, OLS	−0.61*	38%
	(0.08)	(816)
6. Basic model, WLS	−0.46*	47%
	(0.07)	(816)
7. Regional effects, OLS	−0.46*	52%
	(0.07)	(783)
8. Regional effects, WLS	−0.27*	61%
	(0.05)	(783)
Loans to insiders/loans (mean = 0.46%):		
9. Basic model, OLS	−0.15*	66%
	(0.05)	(474)
10. Basic model, WLS	−0.13*	59%
	(0.04)	(474)
11. Regional effects, OLS	−0.20*	68%
	(0.05)	(455)
12. Regional effects, WLS	−0.11*	63%
	(0.04)	(455)

This table presents estimates of the change in bank loan quality (aggregated to the state level) following relaxation of intrastate branching restrictions. Table I presents the dates at which each state relaxed its restrictions on branching. We estimate a regression of the loan quality variable on time and state fixed effects and an indicator variable equal to one for states with no restrictions on branching via M&A. We include three measures of loan quality: nonperforming loans to total loans (loans more than 90 days past due plus nonaccrual loans), net charge-offs (gross charge-offs minus recoveries) on loans to total loans, and loans to insiders to total loans (executives and principal shareholders). Nonperforming loans are from 1982–1992, charge-offs are from 1976–1992, and insider loans are from 1983–1992. Delaware is dropped from all regressions, while Alaska and Hawaii are dropped from the regional effects model. Also, the year in which each state deregulated was dropped. Standard errors appear below coefficients in parentheses. An asterisk indicates statistical significance at the 5 percent level; reported standard errors are heteroskedasticity-consistent [White 1980]. Coefficients are multiplied by 100.

found in the decline in loans to insiders. "Insider loans" are defined as extensions of credit to executive officers and principal shareholders. We presume here that such loans are potentially less productive than standard loans. Insider loans are also likely

to be a proxy for the degree to which a bank is operated for the benefit of its management.

The bottom of Table VII presents the results using the state-level aggregate ratio of insider loans to total loans as the dependent variable. The results indicate that loans extended to insiders decline by 24 to 43 percent (relative to the unconditional mean) after branching reform.[29] Although such loans constitute only a small fraction of the total portfolio of the average bank, they may be indicative of broader trends.[30]

VI. Conclusions

This paper shows that economic growth accelerated following intrastate branching reform. We argue that changes in branching policy played an important role in the observed growth pickup. We find no other concurrent policy changes to explain the improved growth performance. Nor do we find any evidence that statewide branching was implemented in anticipation of future growth prospects. Moreover, we observe improvements in loan quality but no consistent increase in lending after branch reform, suggesting that bank monitoring and screening improvements are the key to the observed growth increases.

These findings are consistent with theoretical models which stress that economies with financial systems which channel savings into better projects grow faster. We do not find support for the idea that better financial markets can increase growth rates by increasing overall savings and investment.

This paper provides preliminary evidence on the channels by

We tested for this possibility by looking for changes in banks' loan portfolios after branch reform. We find that banks' loan portfolios show no movement away from the two riskiest loan categories, C&I loans and commercial real estate loans, following branch liberalization.

29. Recent data suggest that the extent of insider lending is larger than previously thought. Since 1993 banks have been required to add loans to directors, along with loans to executives and principal shareholders. As a result, the reported level of insider loans rose, on average, by a factor of four (to about 2 percent of total loans).

30. We recognize that increased economic growth following branch reform would lead to improvements in both nonperforming loans and loan charge-offs (although not insider loans). However, our estimated coefficients are much larger than would be expected from growth alone. Swamy, Barth, Chou, and Jahera [1995] estimate that a one percentage point decline in unemployment would, at most, reduce the charge-offs-to-assets ratio by 0.13 percentage points. The reductions in charge-offs in our paper are much larger than would be predicted by the Swamy et al. results.

which financial sector innovations affect the real economy, a topic that deserves more attention than it has received in the empirical literature in this area. The policy change considered here promises further insight into this issue. We are currently working to understand better why banks improve the quality of their loans after branching reform. Is it because weak banks that survived behind regulatory entry barriers failed (or lost market share) once those barriers were dismantled? Such a "selection mechanism" would improve the observed performance of the average surviving bank after branch deregulation. Alternatively, was greater management discipline in the face of a more active corporate takeover market responsible for the improved bank performance following branch liberalization? We expect to find answers to these questions upon closer scrutiny of changes in the banking industry following branch deregulation.

Passage of the Riegle-Neal Act of 1994 permitting interstate banking and branching has generated renewed debate about the effects of bank deregulation on economic performance. The law gives states the right to opt out of interstate branching by 1997. Texas has already availed itself of this provision and opted out of interstate branching. Several other states are currently considering following suit. Our results suggest that state governments would be well-advised to consider the impact of opting out on economic growth.

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