THE FINANCE-GROWTH NEXUS: EVIDENCE FROM BANK BRANCH DEREGULATION

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Evidence from Bank Branch Deregulation

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Abstract

This paper provides evidence that financial markets can directly affect economic growth by studying the relaxation of bank branch restrictions in the United States over the past 25 years. 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 reflect causality flowing *from* financial sector reform *to* improved growth performance. This argument is supported by evidence from the process of branching deregulation, from the timing of such policy changes, and from bank lending following branch reform. Moreover, the particular financial sector policy experiment studied here leads to faster growth by improving the quality of bank lending.

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I. Introduction

This paper provides 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" methodology. Our results suggest that the rate of real, per-capita growth increases significantly following intrastate branch reform. We also find 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 funds necessary to support those good prospects.

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 McKinnon (1973) and Shaw (1973), who showed that high growth economies tend to have well-developed financial markets,

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

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 has been used so intensively by so many researchers (Levine and Renelt (1992), for instance, demonstrates 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.

In resolving the causality problem, this paper makes its most significant contribution. States' economies are observed to perform better following branch deregulation. But this may be interpreted in either of the following two ways: (1) states relaxed branching restrictions because of anticipated good growth prospects and the need to finance future investments; or, (2) changes in banking after branch deregulation contributed directly to increased growth. We consider and reject the possibility that the expectation of future growth prospects led the state governments to effect these reforms.

We present three pieces of evidence consistent with this interpretation. First, states did not deregulate prior to a growth spurt. Second, the rate of investment following state branching reform remains unchanged. Third, there is little evidence that bank lending increases following intrastate branch reform. Taken together, these findings are inconsistent with the idea that the expectation of

high growth led to branching reforms, since one would expect that the political pressures for such reforms would come from prospective bank borrowers; we see no evidence that loan demand increases following intrastate branch deregulation.

We argue that improvements in lending *quality* are the key to the beneficial growth effects of branching reforms. Banks don't necessarily lend more but they appear to lend better. This finding contributes to the debate on the channels though which the financial sector influences growth. Do financial innovations increase the volume of investment, the productivity of investment, or both? Evidence from banks' balance sheets suggests that the particular financial sector policy experiment studied here leads to faster growth by improving the quality of bank lending. This is consistent with De Gregorio and Guidotti (1994), who find that 75 percent of the positive finance-growth correlation in cross-county data remains after accounting for variations in the level of investment, i.e. the bulk of the finance-growth relation is explained by the productivity of investment. Our results suggest that improved screening and monitoring of investments can lead to faster economic growth, even absent increases in the level of investment.

Liberalizing branch restrictions may affect the quality of banking in several ways. Previous research has shown that banking markets become more consolidated after branch reform as large bank holding companies acquire banks and convert existing bank subsidiaries into branches. This process, we argue, has beneficial effects on the efficiency of financial intermediation. 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. Furthermore, increases in the average size of banks improves efficiency because larger banking companies can take advantage of wide branch 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 methodology and presents our estimates of the growth effects of deregulation. Section IV presents evidence on the causal relation between branching reform and growth. We consider and 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 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 multi-state 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 levels 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 which remain in operation following intrastate branch reform.

In contrast to intrastate branch reform, there is little to suggest 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.

B. Recent Changes in State Branching Restrictions

The unusual history of U.S. 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. Currently, all but three states 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 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 which has plagued previous empirical research efforts, in our model of the determinants of long-run growth.

Table 1 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 1 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.[2]

We use these policy changes to determine empirically whether states grow faster once they allow statewide branching. As Table 1 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

[2] 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.

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.

A. An Empirical Model of Growth

We use the dates in Column (1) of Table 1 to construct an indicator variable equal to 1 for states permitting branching via M&A and 0 otherwise. The growth effects of the policy are estimated in a fixed-effects model, as follows:

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

where $Y_{t,i}$ equals a measure of real, per-capita income (output) during year t in state i; $D_{t,i}$ is a branching indicator equal to 1 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, economy-wide 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.[3] Thus, we have 21 years times 50 states minus 35 state-years in which the deregulation occurred during the sample, leaving a total of 1,015 observations.

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[3] 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 middle 1980s. Our growth results are not sensitive to the exclusion of Delaware. Some of the results on changes in bank lending presented in Section III are affected, since Delaware's banking market grew so fast after reform. This growth, however, occurred because of the entry of credit card banks.

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 which 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 methodology 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) with four specifications. 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.[4] 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 1 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 coefficient, γ. While there is no a priori reason to suspect that regional business cycles will introduce a bias, we also present estimates from an augmented version of the model in equation (1)

[4] In fact, the residual variance from equation (1) increases with the size of a state's economy.

allowing the time effects (i.e. the business cycle effects) to vary across four broad regions within the U.S. This specification is included mainly as a robustness check. The model with regional effects follows:

$$Y_{t,i} / Y_{t-1,i} = \alpha_{t,j} + \beta_i + \gamma D_{t,i} + \epsilon_{t,i} \quad i=1,...,48,\ j=1,..,4,\ t=72,...,92 \tag{2}$$

where j indexes the four regions.[5] 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. Commerce Department. 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

[5] We split the lower 48 states into four large regions of approximately equal size. These regions are described in an appendix available from the authors. Hawaii and Alaska are dropped from the model in equation (2).

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. Gross state product grew by 1.4 percent per year from 1978 to 1992.[6]

B. Results

The results of the growth model outlined above appear in Table 2. 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 1.18 percentage points following intrastate branch deregulation.[7]

The point estimates in Table 2 may seem too large to reflect the long-run growth effects of branch bank reform. An increase of 0.5 percentage points in annual growth rates is an increase of about one third. We argue that these estimates are indeed plausible because they come from relatively high frequency data which may be dominated by the years just after branch reform. In our sample,

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[6] Since the Commerce Department 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.

[7] We have 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 personal income below the median at the beginning of the period. Overall, these results 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; the point estimate of the growth effect is smaller for the large states when growth is constructed from gross state product. In neither case can we reject the hypothesis that the growth effects are equal. These results provide further support that the growth results are robust.

24 of the 35 deregulating states did so after 1985. For these states we have, at most, seven years of growth experience after reform. Thus, 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 much of the economy's capital is held by the banking system. 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. This first effect, while not sustainable, could have a large effect on 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.

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). Since capital's share of income is about 25 percent and commercial banks hold 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.[8] This 1.25 percent jump in income spread out over 5 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).

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[8] Commercial banks hold approximately 25 percent of total funds advanced in credit markets to nonfinancial sectors, see Federal Reserve Bulletin, Table 1.60. The estimate of capitals's share of income is borrowed from Lucas (1988).

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). In order to estimate these long-run effects, we compare states which deregulated early with the late deregulaters. We divide our sample into three groups of states: states which never deregulated (3), states which have been deregulated since 1972 (12), and states which deregulated after 1972 (35). We assume that the 12 states deregulated prior to 1972 have reached their long-run equilibrium growth rate by the beginning of our analysis, while the 35 states which deregulated during our sample period may be experiencing rapid growth above the long-run level during a transition period.

In order to estimate both the long-run growth effect and the growth effect during the transition, we introduce a second indicator variable into our empirical model equal to 1 for the 12 states deregulated prior to 1972. Since this indicator variable is constant over time for each state, however, we can no longer include state fixed-effects in the model. Instead, we replace the state fixed effects with regional fixed effects and estimate the following model:

$$Y_{t,j} / Y_{t-1,j} = \alpha_t + \beta_j + \gamma^{tr} D^{tr}_{t,i} + \gamma^{lr} D^{lr}_{i} + \epsilon_{t,j} \quad i=1,...,48,\ t=72,...,92,\ j=1,...,4 \tag{3}$$

where j indexes regions; $D^{tr}_{t,i} = 1$ if state i were deregulated via M&A during year t; $D^{lr}_{i} = 1$ if state i were deregulated prior to 1972.[9] As before, we estimate this model using both personal income and gross state product to construct the growth variable. In parallel with the eight specifications estimated for the basic growth results, we also estimate the model with time-varying regional effects.

The results, presented in Table 3, provide some evidence that the growth effects differ significantly comparing states which deregulated after 1972 with states which have had no restrictions on branching since 1972. The coefficient on D^{lr} is negative in all eight cases but statistically significantly so in only three of those cases. In addition, the point estimates of the long-run growth

[9] We drop Alaska and Hawaii from this analysis.

effect ($\gamma^{sr} + \gamma^{lr}$) is positive in all eight specifications but significantly so in only four of the eight specifications (those based on gross state product). We conclude that the long-run growth effects are probably smaller than the short-run effects.

All of the results presented to this point are, of course, 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 4 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.[10]

Another possible "omitted variable" misspecification is suggested by the fact that many states deregulated when the U.S. banking system was in distress. Of the 35 states which deregulated since 1972, 25 did so after 1984, the first of many years of dramatically increased bank failures.[11] This suggests the possibility that small banks, the traditional constituency for branching restrictions,

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[10] We have also tested for omitted variables bias by including three variables measuring the proportion of control of the state government by the Republican Party: an indicator variable equal to 1 if the Governor is Republican, a continuous variable between 0 and 1 measuring the percentage of Republican State Senators and another continuous variable between 0 and 1 measuring the percentage of Republican in the State Assembly. These results provide no evidence that these variables affect the state growth rate or that their omission has any effect on the estimate of the coefficient on the intrastate branch indicator.

[11] 1296 banks were subject to FDIC intervention over the nine year interval between 1984 and 1992. In contrast, a mere 20 banks failed over the nine years prior to 1984 (FDIC, 1993).

dropped their opposition to branching in order to find higher purchase prices when exiting the distressed banking industry. Regulators may have pushed for liberalized branching to increase bank consolidation and to wean out weaker banks. In this scenario, branch policy would likely be changed when the economy (or that section of the economy which supports unit banking) is in distress. If the estimated growth model does not control for such shocks effectively, then the observed growth pick up following deregulation may happen simply because the economy is in a slump at the time of deregulation. The economy has nowhere to go but up. The observed growth increase following branch liberalization is a spurious "correlation" produced by the timing of deregulation.[12]

Table 5 tests this possibility by estimating the basic growth model (of Table 2) on only those 10 states which deregulated between 1972 and 1984.[13] These states did not face banking crises prior to the policy change. Also included in the sample are the 12 states which had deregulated before 1972 and the 3 states which retained branching restrictions through 1992. Table 5 establishes that branch deregulation has a significant effect on growth rates even for those states which deregulated before the banking industry slid into distress. Moreover, the growth effect of the policy change for these states is not significantly less than the effect for the entire sample.[14] We conclude that the observed growth increase following branch deregulation is not an artifact of the policy change occurring when the economy is in a slump.

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[12] We are grateful to Charles Calomiris and to Stavros Peristiani for suggesting this possibility.

[13] These states are: Alabama, Connecticut, Georgia, Maine, New Jersey, New York, Ohio, Pennsylvania, Utah and Virginia.

[14] These results hold for the simple growth model. Deregulation is not significant in the models with regional fixed effects and are not presented here. But this is not surprising given the low power of the test when estimating the regional effects model. Only 10 states loosened branching rules between 1972 and 1984, the restricted sample used in the personal income-based regressions in Table 5. For the gross state product-based regressions, the problem is even more severe; only 6 states changed policy between 1978 (the first year real gross state product growth rates are available) and 1984.

Furthermore, the results in Table 5 support the notion that the growth effects of branch deregulation were not temporary. For each of the 10 states in the sample used in Table 5, we observe growth rates for 10 years or more after lifting branch restrictions. The effect of branching policy change on economic growth is significant even for these states, and the effect is not significantly smaller than that for the entire sample (which includes the 25 states which deregulated late–1984 and after).

IV. Why was Bank Branching Deregulated?

If relaxing branching regulations by states occurred at random, the effects of branch deregulation on economic growth would be all but established. But the proposed causal relation becomes more problematic when we consider the following possibility about the process leading to statewide branching: states may have relaxed branching restrictions because they anticipated increased economic growth and foresaw the need for increased funding of attractive projects; causality flows from growth to deregulation although growth is observed to follow deregulation.

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 (for example, King and Levine (1993b), and De Gregorio and Guidotti (1993)).

However, we can do better. In this section we present evidence from the process of deregulation, from the timing of deregulation, and from bank lending behavior following deregulation to establish more persuasively that intrastate branching was not prompted by policy makers anticipating increased growth.

A. The Process of Deregulation

One possible source of the deregulatory process has already been mentioned above: facing a drastic negative shock to the banking system, small banks may have dropped their traditional opposition to bank branching in order to expand the set of possible buyers. Moreover, regulators, faced with a distressed banking system, may have supported branching liberalization in order to encourage stronger banks to acquire weaker banks. This may well have been part of the story for states such as Texas and Oklahoma whose banking systems were seriously distressed immediately prior to branching deregulation.

Another, very different process of deregulation may be discerned in some states. 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.[15] Commercial banks with national charters in Texas,

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

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.

Significantly, this process of OCC-inspired deregulation was opposed by *state* banking authorities in several instances. In Mississippi and Texas, for example, state bank regulators challenged the OCC decision in court but lost.[16] This pattern of state-level opposition to branch liberalization is not consistent with the idea that the deregulation was prompted by anticipated future growth of states' economies.

Not all states lifted their branch restrictions in response to OCC pressure. Several years before the national bank regulator's Mississippi decision, West Virginia's state legislature passed a bill lifting most branching restrictions. 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). Such anecdotal evidence suggests that, at least in West Virginia, bank branch expansion was expected to stimulate economic growth (and not the reverse).

Anecdotal evidence sheds only partial light on the endogeneity problem. Our knowledge of the political and legislative process leading to the deregulation of intrastate branching is admittedly incomplete. The available evidence on some states is inconclusive. In Pennsylvania, for example, large banks such as Mellon Bancorp lobbied the state legislature into relaxing branch restrictions using the argument that "they [Mellon et al] needed broader powers to meet challenges from national financial institutions and to bolster themselves to compete in an anticipated era of interstate banking" (Wall Street Journal, 03/05/82).

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

It is entirely possible that states which most feared such entry by out-of-state banks were those states with good growth prospects. The possibility remains that, at least for these states, economic conditions drove deregulation. This suggests the need for other types of evidence to establish the direction of causality in the link between intrastate branching and growth.

B. The Timing of Deregulation

The timing of deregulation is a potentially useful source of information about the motivation behind such policy changes. If states were liberalizing in anticipation of future growth, we should see most states deregulating before or during the growth phase of the business cycle.[17]

Note that the growth results presented so far in Table 2 do not establish that liberalizing states enjoyed a growth spurt post-deregulation. All they establish is that *relative to the control group of non-deregulators*, the deregulators have higher growth. This may capture either of the following possibilities: (1) deregulating states were growing faster than the control states, or (2) the former were shrinking slower. (This "ambiguity" in the interpretation of the deregulation dummy is an artifact of the difference-in-differences model with its reliance on a control group.)

Figure I presents a histogram of changes in the average annual growth rate in personal income before and after deregulation for the 35 states which relaxed their branching restrictions during the 1972-1992 period. There is no discernible pattern of faster growth after deregulation. Half the states (18 of the 35) had *lower* growth after deregulation. State-by-state t-tests of the hypothesis that the average growth rate increased following deregulation show no significant difference even for those states which increased growth rates most (Wyoming, New York and Hawaii).

Such state-by-state comparison of means tests suffer from low power. For example, New York allowed intrastate branching in 1976; this means we have only 4 pre-deregulation years in the

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[17] Of course, it is possible that policy makers were mistaken in their optimism leading up to the branch deregulation; the anticipated boom never occurs, and our "test" here will not detect this possibility.

sample. In order to construct a more powerful test of the hypothesis that the average growth rate increased following intrastate branching reform, we pooled all states which changed regulatory regimes during the 1972-1992 period and tested for an average increase in growth rates. Table 6 presents these results, which are equivalent to those in Table 2 without time fixed effects. In the absence of fixed effects controlling for cyclical changes in growth rates, the coefficient on the deregulation indicator should be even more positive and significant if states deregulated before or during the growth phase of the business cycle.

The evidence in Table 6 suggest that, if anything, the average state deregulated during or before the *downswing* of the business cycle. The deregulation dummy coefficient is never significant in any of the four regressions in Table 6. In contrast, the results in Table 2 establish that controlling for the business cycle by including time fixed effects leads to a positive, significant coefficient on the deregulation dummy; lifting branch restrictions has a positive effect on growth. This suggests that the reason for the non-significant deregulation dummy coefficient in Table 6 is that, on average, states deregulated during or before a slowdown in economic growth.[18]

C. Evidence from Bank Loan Data

Perhaps the strongest evidence on the motives of state legislatures comes from bank lending data. 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. If such loan growth is not

[18] One potential problem with the test in Table 6 is that it pools states such as New York and New Jersey which deregulated in the mid-1970s with late deregulators such as Wisconsin (which allowed intrastate branching in 1990). It is possible that states experience a growth spurt immediately after deregulation but the long-run growth effect is minimal. Pooling early deregulators with late deregulators may bias the coefficient of deregulation dummy in Table 6 toward zero. In order to control for this possibility, we estimated the model in Table 6 with only those states which liberalized in 1985 or after (24 states). The results were unchanged.

observed in deregulating states, the likely explanation is that the branching policy change was not prompted by anticipated growth prospects.

In order to see whether loan demand increased at the time of branching deregulation, we look at the growth rate of bank lending and at the "prices" of bank intermediation. In the top two sections of Table 7, we estimate the change in loan growth rates after states lifted branching restrictions. The bottom section of Table 7 estimates the impact of branch deregulation on prices. Changes in loan growth and in price are estimated relative to a control group of states which did not change policy using the fixed effects model (difference-in-differences) employed in the growth regressions (Table 2). The results in Table 7 suggest that there is no consistent evidence that loan demand increased significantly at the time of branching liberalization.

We use two series of bank loan data in Table 7: 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 individual 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 non-bank 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 the top two sections in Table 7 were based on all loans held by individual banks operating within each state as of the end of each calendar year.[19] Bank-level loan data are taken from end-of-year *Quarterly Reports of Condition* filed by all commercial banks over

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

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.

The first two sections of Table 7 present the results from estimating the change in the growth rate of total loans and of commercial loans following deregulation. We find little evidence that lending increased after intrastate branching was allowed. Of the eight regressions, only four record significant increases in loan growth. Two regressions record decreases in commercial loan growth rates after the regime change (although the effect is not significant).

Moreover, if loan demand increased significantly, then we would expect the price of intermediation to increase (assuming marginal cost of intermediation is increasing). Table 7 also shows that prices remained unchanged after intrastate branching was allowed. The bottom section of Table 7 shows the estimated change in net interest margins (NIM) following deregulation. NIM measures the spread between the interest earnings by banks and the interest cost of funds to banks. This measure of price is constructed by taking the ratio of the difference between interest income and interest expense to the value of interest earning assets. The data was taken from end-of-year *Quarterly Reports of Condition* filed by individual banks between 1983 and 1992 (the longest period available). The NIM for a given state in a given year was calculated by summing interest expense and interest income across all banks in that state, then dividing the difference by the total value of interest bearing assets held by all banks in the state. The resulting variable was regressed on the standard fixed effects model used thus far.

Table 8 offers further evidence that the demand for bank loans did not increase at the time of branch deregulation. Here we look at the change in capital expenditures in manufacturing following branch liberalization. The dependent variable is the rate of investment in manufacturing between

1977 and 1991.[20] This variable is constructed by taking the ratio of total capital expenditure in each state to the value added by manufacturing in that state. Capital expenditure data are taken from DRI (which in turn relies on Census of Manufactures and the Annual Survey of Manufactures by the Bureau of the Census). The source of manufacturing value added data is the Commerce Department's Bureau of Economic Analysis.

Table 8 establishes that the rate of investment in manufacturing did not increase following branch deregulation. This is not consistent with the idea that increasing demand for bank loans (fueled by growing investments) drove state legislatures to lift restrictions on branching.

To summarize, there is little to suggest that intrastate branching was prompted by the anticipated need to finance unusually good growth in states' economies. This by no means rules out the possibility that some states acted for this reason. However, we have little reason to believe that this was the dominant motive for most deregulating states.

If bank lending did not increase after branch liberalization, just how did changes in banking sector promote economic growth? We turn to this question next.

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

If bank branch reform had real effects, 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

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[20] As with gross state product data, the series on manufacturing value added (in constant dollars) dates back only to 1977. Also, capital expenditures data for three years--1979,1980,1981--are missing in the DRI database used. These three years are omitted from the regressions in Table 8.

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 (1995)). 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.

Branch liberalization by states is an experiment with some unique advantages in answering this question. Chief among them is that we can observe the behavior of banks after the policy change. We have already seen that there is little increase in lending following branch deregulation, suggesting that increased investment funding by banks had little to do with the observed increase in growth rates.

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.[21] Without such

[21] Even the amount of bank lending to small business, let alone information on borrowers, is not readily available. The *Quarterly Reports of Condition*, filed by commercial banks with Federal regulators, records information on the amount of small loans made, but this information dates back only to 1993.

borrower information, the only available evidence about bank lending quality comes from banks' balance sheets.

Table 9 produces evidence of improved bank lending quality following deregulation. The first quality indicator used in this table is the fraction of total loans classified as "non-performing."[22] End-of-year non-performing loan amounts for all commercial banks over the 1982 to 1992 period are taken from *Quarterly Report of Condition*. A state-level aggregate non-performing loan amount is derived by summing over all banks in each state. The final variable of interest is the ratio of non-performing loans to total loans held by all banks in each state.

The change in non-performing loans (as a ratio of total loans) is estimated using the same fixed effects model employed in the growth regressions (Table 2). Table 9 indicates that non-performing loans decline dramatically after branch restrictions are lifted. The ratio of non-performing loans to total loans declines by 0.24 to 0.77 percentage points, depending on the model estimated. Since the mean non-performing to total loan ratio for the entire sample is 2 percent, this decline in non-performing loans represents a reduction of 12 to 38 percent relative to the unconditional mean.

Shrinking non-performing loans 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. This possibility is inconsistent with the results in the middle panel of Table 9. The two riskiest loan categories in banks' balance sheets are C&I loans and commercial real estate loans. Banks' loan portfolios show no indication of movement away from such loans following branch liberalization. (Furthermore, better economic conditions as the explanation for reduced non-performing loans is ruled out because of the presence of a control group of non-reforming states which are presumably enjoying the same benefits of any improved economic conditions).

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[22] All loans 90 days or more past due and nonaccrual loans are classified as non-performing loans.

Another balance sheet indicator of better lending by banks is found in the decline in loans to insiders. "Insider loans" are defined as extensions of credit to executive officers, directors 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 9 presents the results from estimating the fixed effects model with the ratio of insider loans to total loans as the dependent variable. This variable is constructed for each state by summing across individual banks' insider loans for all banks within that state and dividing by the total loan stock in the state. The results indicate that the fraction of total loans that is extended to insiders declines by 24 to 43 percent (relative to the unconditional mean of 0.46 percent) after branching reform. Although such loans constitute only a small fraction of the total portfolio of the average bank, they may be indicative of broader trends.

Thus far we have interpreted decreased non-performing loans and insider lending as evidence of improved lending practices following branch deregulation. There is, however, an alternative interpretation. The "bail out" story of the deregulatory process suggests that branch restrictions were lifted in order to salvage a bank system in distress. If this were true, we would expect weak banks with substantial amounts of non-performing loans on their balance sheets to be acquired and those bad loans to be written off once branching was liberalized. In that case, the reduced stock of non-performing loans in the state is not due to improved lending by survivor banks.

We find two pieces of evidence inconsistent with the "bail out" story. First, we find that loan charge-offs also decrease after branch liberalization. (These results are not presented here in order to conserve space.) If distressed banks are being bought out after the branching policy change, and their bad loans are written off, we would expect to see an *increase* in charge offs. Second, we examine the change in non-performing loans between 1988 and 1993 for banks which were not subject to a

takeover. (1988 and 1993 are selected because of the unusual number of states, twelve, changing branching policy in this window). We find that non-performing loans decline significantly more in these twelve states (the treatment group) than in states which had allowed intrastate branching prior to 1988 (the control group). This suggests that the decline in non-performing loans observed in Table 9 is at least partly driven by improved lending practices following deregulation.

VI. Conclusions

This paper has established that economic growth accelerates following intrastate branching reform. We argue that the policy change caused the growth spurt. We find no concurrent policy change to explain the growth pick-up. 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 or investment after branch reform. These findings suggest that bank monitoring and screening improvements are the key to the growth increases and support theoretical models which stress that 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.

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 growth. Banking markets function better in the absence of restrictions on geographical expansion and barriers to entry; states without such restrictions enjoy higher rates of income growth.

The channels by which financial sector reforms affects the real economy deserve further attention. In the context of the particular policy change investigated in this paper, more needs to be done to understand how branch deregulations affected economic performance. Why do banks improve the quality of their loans after branching was liberalized? Is it because weak banks which survived behind regulatory entry barriers failed once those barriers were dismantled? Such a "selection mechanism" would improve the observed performance of the average surviving banks 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? These questions await further investigation.

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Table 1
Description of Changes in Intrastate Branching Restrictions Since 1972

	Year M&A Branch Restrictions Lifted (1)	Year Branch Restrictions Lifted via *De Novo* Branching (2)
States Deregulated by 1972		
Alaska		
Arizona		
California		
DC		
Delaware		
Idaho		
Maryland		
North Carolina		
Nevada		
Rhode Island		
South Carolina		
South Dakota		
Vermont		
State Which 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
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
State Which have not Deregulated		
Arkansas		
Iowa		
Minnesota		

Table 2
Growth Regressions: Basic Model

This table presents the estimated increase in growth following relaxation of intrastate branching restrictions. Table 1 presents the dates at which each state relaxed its restrictions on branching. We estimate a basic model with time and state fixed effects and a model with time-varying regional effects, as follows:

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

and

$$Y_{t,i} / Y_{t-1,i} = \alpha_{t,j} + \beta_i + \gamma D_{t,i} + \epsilon_{t,i} \quad i=1,...,48,\ j=1,..,4,\ t=72,...,92$$

where $Y_{t,i}$ equals a measure of real, per-capita income based on personal income or gross state product during year t in state i; j indexes four regions used for time varying regional effects; $D_{t,i}$ is an indicator variable equal to 1 for states with no restrictions on branching via M&A. 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 based on state product are available from 1978-1991. A "*" indicates statistical significance at the 5% level; reported standard errors are heteroskedasticity-consistent (see White, 1980).

Growth Based on Personal Income:	Estimated Percentage Point Change in Growth Rate (Standard Error)	Adjusted R^2 (Number of Observations)
	(1)	(2)
Basic Model, OLS	0.94*	49%
	(0.26)	(1,015)
Basic Model, WLS	1.18*	70%
	(0.24)	(1,015)
Regional Effects, OLS	0.51*	62%
	(0.23)	(974)
Regional Effects, WLS	0.59*	77%
	(0.18)	(974)
Growth Based on Gross State Product:		
Basic Model, OLS	1.03*	43%
	(0.36)	(668)
Basic Model, WLS	1.08*	64%
	(0.30)	(668)
Regional Effects, OLS	0.69*	54%
	(0.33)	(641)
Regional Effects, OLS	0.84*	72%
	(0.24)	(641)

Table 3
Growth Regressions:
Long and Short Run Effects of Branch Deregulation

This table presents the estimated long- and short-run increase in growth following relaxation of intrastate branching restrictions. Table 1 presents the dates at which each state relaxed its restrictions on branching. We estimate a basic model with time and census-region fixed effects and a model with time-varying regional effects, as follows:

$$Y_{t,i} / Y_{t-1,i} = \alpha_t + \beta_j + \gamma^{sr} D^{sr}_{t,i} + \gamma^{lr} D^{lr}_i + \epsilon_{t,i} \quad i=1,...,50,\ j=1,...4,\ t=72,...,92,$$

and

$$Y_{t,i} / Y_{t-1,i} = \alpha_{t,j} + \beta_j + \gamma^{sr} D^{sr}_{t,i} + \gamma^{lr} D^{lr}_i + \epsilon_{t,i} \quad i=1,...,48,\ j=1,...,4,\ t=72,...,92$$

where $Y_{t,i}$ equals a measure of real, per-capita income based on personal income or gross state product during year t in state i; j indexes four regions used for time varying regional effects; $D^{sr}_{t,i}$ is an indicator variable equal to 1 for states with no restrictions on branching via M&A; D^{lr}_i is an indicator equal to 1 for states deregulated since 1972. Delaware, Alaska and Hawaii are dropped from all of the regressions, which include regional effects. Also, the year in which each state deregulated was dropped. Growth data based on state product are available from 1978-1991. A "*" indicates statistical significance at the 5% level; reported standard errors are heteroskedasticity-consistent (see White, 1980).

Growth Based on Personal Income:	Estimated Percentage Point Change in Growth Rate (Standard Error)	Early Deregulation Effect (Standard Error)	Adjusted R^2 (Number of Observations)
	(1)	(2)	(3)
Basic Model, OLS	0.62*	-0.44*	56%
	(0.20)	(0.20)	(974)
Basic Model, WLS	0.86*	-0.61*	72%
	(0.21)	(0.18)	(974)
Regional Effects, OLS	0.36*	-0.28	63%
	(0.18)	(0.17)	(974)
Regional Effects, WLS	0.39*	-0.32*	78%
	(0.15)	(0.14)	(974)
Growth Based on Gross State Product:			
Basic Model, OLS	0.70*	-0.10	49%
	(0.23)	(0.23)	(641)
Basic Model, WLS	0.80*	-0.11	65%
	(0.24)	(0.20)	(641)
Regional Effects, OLS	0.66*	-0.05	54%
	(0.23)	(0.20)	(641)
Regional Effects, WLS	0.69*	-0.05	71%
	(0.19)	(0.18)	(641)

Table 4
Growth Regressions:
Including State Fiscal Policy Variables

This table presents the estimated growth following relaxation of intrastate branching restrictions. Table 1 presents the dates at which each state relaxed its restrictions on branching. We augment the basic model by including two measures of state fiscal policy. The model is specified as follows:

$$Y_{t,i} / Y_{t-1,i} = \alpha_t + \beta_i + \gamma D_{t,i} + \gamma^K (K/Y)_{t-1,i} + \gamma^T (T/Y)_{t-1,i} + \epsilon_{t,i} \quad i=1,...,49,\ t=72,...,92$$

and

$$Y_{t,i} / Y_{t-1,i} = \alpha_{t,j} + \beta_i + \gamma D_{t,i} + \gamma^k (K/Y)_{t-1,i} + \gamma^t (T/Y)_{t-1,i} + \epsilon_{t,i} \quad i=1,...,47,\ j=1,...,4,\ t=72,...,92$$

where $Y_{t,i}$ equals a measure of real, per-capita income based on personal income or gross state product during year t in state i; j indexes four regions used for time varying regional effects; $D_{t,i}$ is an indicator variable equal to 1 for states with no restrictions on branching via M&A; K/Y is the state government's capital expenditure per dollar of income (product); and (T/Y) is the state government's tax receipts per dollar of income (product). 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 based on state product are available from 1978-1991. A "*" indicates statistical significance at the 5% level; reported standard errors are heteroskedasticity-consistent (see White, 1980).

Growth Based on Personal Income:	Estimated Percentage Point Change in Growth Rate (Standard Error)	State Capital Expenditure / Income (Standard Error)	State Tax Receipts / Income (Standard Error)	Adjusted R^2 (Number of Observations)
	(1)	(2)	(3)	(4)
Basic Model, OLS	0.98*	0.01	-0.02	50%
	(0.25)	(0.02)	(0.01)	(994)
Basis Model, WLS	1.20*	0.01	-0.01	71%
	(0.24)	(0.02)	(0.01)	(994)
Regional Effects,	0.60*	0.02	-0.01	63%
OLS	(0.23)	(0.01)	(0.01)	(953)
Regional Effects,	0.63*	0.02	-0.01	78%
WLS	(0.18)	(0.01)	(0.01)	(953)
Growth Based on State Product:				
Basic Model, OLS	1.00*	-0.11*	0.03	44%
	(0.36)	(0.05)	(0.02)	(654)
Basic Model, WLS	1.04*	-0.09*	0.03	65%
	(0.30)	(0.04)	(0.02)	(654)
Regional Effects,	0.70*	-0.11*	0.03	55%
OLS	(0.33)	(0.04)	(0.02)	(627)
Regional Effects,	0.85*	-0.07	0.02	72%
WLS	(0.25)	(0.04)	(0.01)	(627)

Table 5
Growth Regressions:
States Deregulating before 1984

This table presents the estimated increase in growth following relaxation of intrastate branching restrictions. We exclude all states which deregulated their branching laws after 1984, leaving 25 states. Table 1 presents the dates at which each state relaxed its restrictions on branching. We estimate a basic model with time and state fixed effects, as follows:

$$Y_{t,i} / Y_{t-1,i} = \alpha_t + \beta_i + \gamma D_{t,i} + \epsilon_{t,i} \quad i=1,...,25,\ t=72,...,92$$

where $Y_{t,i}$ equals a measure of real, per-capita income based on personal income or gross state product during year t in state i; $D_{t,i}$ is an indicator variable equal to 1 for states with no restrictions on branching via M&A. Delaware is dropped from all regressions. Also, the year in which each state deregulated was dropped. Growth data based on state product are available from 1978-1991. A "*" indicates statistical significance at the 10% level; reported standard errors are heteroskedasticity-consistent (see White, 1980).

Growth Based on Personal Income:	Estimated Percentage Point Change in Growth Rate (Standard Error)	Adjusted R^2 (Number of Observations)
	(1)	(2)
Basic Model, OLS	0.84*	52%
	(0.39)	(515)
Basic Model, WLS	0.96*	75%
	(0.31)	(515)
Growth Based on Gross State Product:		
Basic Model, OLS	0.84*	48%
	(0.46)	(343)
Basic Model, WLS	0.63*	74%
	(0.32)	(343)

FIGURE 1

Change in Mean State Per-Capita Growth

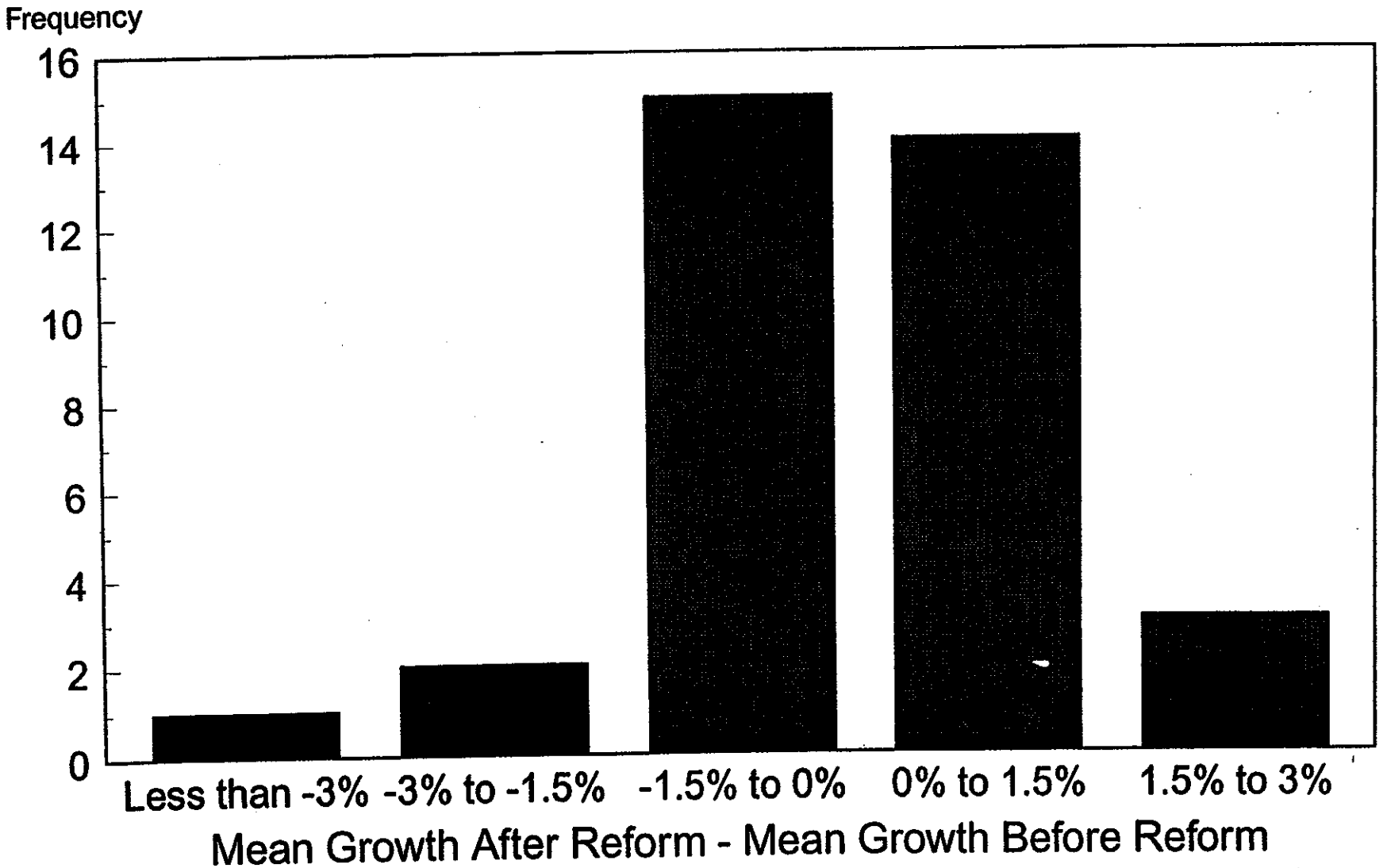

Table 6
Growth Regression without Time Fixed Effects:
Intrastate Branch Reform Timing is Unrelated to Future Growth

This table presents the estimated increase in growth following relaxation of intrastate branching restrictions. Table 1 presents the dates at which each state relaxed its restrictions on branching. We estimate the basic model without time fixed effects, as follows:

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

where $Y_{t,i}$ equals a measure of real, per-capita income based on personal income or gross state product during year t in state i; $D_{t,i}$ is an indicator variable equal to 1 for states with no restrictions on branching via M&A. Delaware is dropped from all regressions. Also, the year in which each state deregulated was dropped. Growth data based on state product are available from 1978-1991. A "*" indicates statistical significance at the 5% level; reported standard errors are heteroskedasticity-consistent (see White, 1980).

Growth Based on Personal Income:	Estimated Percentage Point Change in Growth Rate (Standard Error)	Adjusted R^2 (Number of Observations)
	(1)	(2)
Basic Model, OLS	-0.09	1%
	(0.27)	(1,015)
Basic Model, WLS	-0.05	1%
	(0.34)	(1,015)
Growth Based on Gross State Product:		
Basic Model, OLS	0.40	5%
	(0.37)	(668)
Basic Model, WLS	0.07	6%
	(0.42)	(668)

Table 7
Bank Lending and Pricing Regressions

This table presents the estimated change in bank lending and pricing following relaxation of intrastate branching restrictions. Table 1 presents the dates at which each state relaxed its restrictions on branching. We estimate a basic model with time and state fixed effects and a model with time-varying regional effects, as follows:

$$Y_{t,i} = \alpha_t + \beta_i + \gamma D_{t,i} + \epsilon_{t,i} \quad i=1,...,50,\ t=72,...,92$$

and

$$Y_{t,i} = \alpha_{t,j} + \beta_i + \gamma D_{t,i} + \epsilon_{t,i} \quad i=1,...,48,\ j=1,..,4,\ t=72,...,92$$

where $Y_{t,i}$ equals the growth in loans (all loans or commercial loans, which we define as the sum of C&I and commercial real estate loans) or the price of intermediation, as measured by net-interest margin (NIM); j indexes four regions used for time varying regional effects; $D_{t,i}$ is an indicator variable equal to 1 for states with no restrictions on branching via M&A. 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. NIM is only available from 1983 on while growth in commercial lending is available starting in 1977. A "*" indicates statistical significance at the 5% level; reported standard errors are heteroskedasticity-consistent (see White, 1980).

Growth in Loans (Mean = 1.0%):	Estimated Percent Change in Lending (Standard Error)	Adjusted R^2 (Number of Observations)
	(1)	(2)
Basic Model, OLS	0.8	6%
	(3.6)	(1,015)
Basic Model, WLS	2.6*	21%
	(1.2)	(1,015)
Regional Effects, OLS	0.4	7%
	(2.7)	(974)
Regional Effects, WLS	1.8*	23%
	(0.9)	(974)
Growth in Commercial Loans (Mean=1.0%):		
Basic Model, OLS	-0.5	4%
	(0.5)	(767)
Basic Model, WLS	3.3*	9%
	(1.7)	(767)
Regional Effects, OLS	-1.2	5%
	(4.0)	(736)
Regional Effects, WLS	2.3*	9%
	(1.2)	(736)
NIM (Mean=4.7%):		
Basic Model, OLS	-2.7	76%
	(5.0)	(425)
Basic Model, WLS	3.7	91%
	(5.0)	(425)
Regional Effect, OLS	-2.0	78%
	(6.0)	(408)
Regional Effect, WLS	-6.0	93%
	(5.0)	(408)

Table 8
Investment and Intrastate Branch Reform

This table presents the estimated change in the state investment rate (based on manufacturing output) following relaxation of intrastate branching restrictions. Table 1 presents the dates at which each state relaxed its restrictions on branching. We estimate a basic model with time and state fixed effects and a model with time-varying regional effects, as follows:

$$I_{t,i} / Y_{t,i} = \alpha_t + \beta_i + \gamma D_{t,i} + \epsilon_{t,i} \quad i=1,...,50,\ t=77,...,91$$

and

$$I_{t,i} / Y_{t,i} = \alpha_{t,j} + \beta_i + \gamma D_{t,i} + \epsilon_{t,i} \quad i=1,...,48,\ j=1,..,4,\ t=77,...,91$$

where $I_{t,i}/Y_{t,i}$ equals the ratio of capital expenditure in the manufacturing sector to total value added in that sector during year t in state i; $D_{t,i}$ is an indicator variable equal to 1 for states with no restrictions on branching via M&A. 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. Data on capital expenditures are not available for 1979-1981. A "*" indicates statistical significance at the 5% level; reported standard errors are heteroskedasticity-consistent (see White, 1980).

Investment Rate:	Estimated Percentage Point Change in Investment Rate (Standard Error)	Adjusted R^2 (Number of Observations)
	(1)	(2)
Basic Model, OLS	-0.28	46%
	(0.53)	(571)
Basic Model, WLS	0.11	62%
	(0.30)	(571)
Regional Effects, OLS	-0.81	56%
	(0.50)	(548)
Regional Effects, WLS	-0.06	71%
	(0.25)	(548)

Table 9
Bank Loan Quality Regressions

This table presents the estimated change in bank loan quality following relaxation of intrastate branching restrictions. Table 1 presents the dates at which each state relaxed its restrictions on branching. We estimate a basic model with time and state fixed effects and a model with time-varying regional effects, as follows:

$$Y_{t,i} = \alpha_t + \beta_i + \gamma D_{t,i} + \epsilon_{t,i} \quad i=1,...,50,\ t=t_0,...,92$$

and

$$Y_{t,i} = \alpha_{t,j} + \beta_i + \gamma D_{t,i} + \epsilon_{t,i} \quad i=1,...,48,\ j=1,..,4,\ t=t_0,...,92$$

where $Y_{t,i}$ equals a measure of average bank loan quality by all commercial banks operating in state i; j indexes four regions used for time varying regional effects; $D_{t,i}$ is an indicator variable equal to 1 for states with no restrictions on branching via M&A. We include three measures of loan quality: non-performing loans to total loans (loans more than 90 days past due plus nonaccrual loans), commercial loans to total loans (C&I loans plus commercial real estate loans) and loans to insiders to total loans (executives and principal shareholders). Non-performing loans are available from 1982, commercial loans from 1976 and insider loans from 1983. 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. A "*" indicates statistical significance at the 5% level; reported standard errors are heteroskedasticity-consistent (see White, 1980).

	Estimated Percentage Point Change (Standard Error)	Adjusted R^2 (Number of Observations)
Non-Performing Loans/Loans (Mean = 2%):	(1)	(2)
Basic Model, OLS	-0.77* (0.17)	46% (523)
Basic Model, WLS	-0.24 (0.19)	59% (523)
Regional Effects, OLS	-0.63* (0.15)	60% (502)
Regional Effects, WLS	-0.30* (0.12)	73% (502)
Commercial Loans/Loans (Mean=44%)		
Basic Model, OLS	0.01 (0.52)	79% (816)
Basic Model, WLS	-0.60 (0.52)	83% (816)
Regional Effects, OLS	-0.43 (0.46)	80% (783)
Regional Effects, WLS	-0.25 (0.48)	84% (783)
Loans to Insiders/Loans (Mean = 0.46%):		
Basic Model, OLS	-0.15* (0.05)	66% (474)
Basic Model, WLS	-0.13* (0.04)	59% (474)
Regional Effects, OLS	-0.20* (0.05)	68% (455)
Regional Effects, WLS	-0.11* (0.04)	63% (455)