# CHANGE-POINT ANALYSIS OF THE GROWTH EFFECTS OF STATE BANKING DEREGULATION

DONALD G. FREEMAN*

*This article uses recent developments in change-point analysis to demonstrate that stronger income growth rates previously associated with state bank branch deregulation were in all cases temporary rather than permanent. In addition, patterns of temporal ordering across regions suggest that in many states deregulation was a response to economic conditions rather than the reverse.* (*JEL* E44, G28)

## I. INTRODUCTION

Though most researchers agree that a healthy financial sector and robust economic growth go hand in hand, there remains some debate as to whether one is necessarily precedent to the other. One view, often identified with Schumpeter (1934), is that an efficient banking sector serves as the engine of growth by identifying and channeling financial capital to innovative entrepreneurs: financial development leads growth. The other view, as expressed by Robinson (1952), is that economic growth creates the conditions necessary for profitable financial intermediation: Growth leads financial development.

Goldsmith (1969) and McKinnon (1973) provide empirical evidence that high-growth economies tend to have well-developed financial markets, but issues of cause and effect are not addressed. More recent research, as summarized by Levine (2003), finds consistent results that countries with well-developed financial markets and institutions tend to grow faster than countries without and that financial developments tend to precede economic developments.

In the United States, the banking industry at the national level has undergone a series of major deregulatory moves over the past quarter century, beginning with the removal of some interest rate ceilings on deposits in 1978, followed closely by the Depository Institutions Deregulation and Monetary Control Act of 1980, running through the Riegle-Neal Interstate Banking and Branching Efficiency Act of 1994, and culminating in the Gramm-Leach-Bliley Act of 1999, which overturns most of the provisions of the Glass-Steagall Act of 1933 and allows banks to affiliate with securities and insurance firms.

Proponents of deregulation argue that reform will have positive real growth effects by enhancing bank efficiency and allowing better geographic and sectoral diversification of bank portfolios. If these arguments bear out, the many reforms of the past few decades should result in higher overall growth for the U.S. economy. Opponents express concerns that removing restrictions will limit competition, drive up the prices of banking services, and reduce accessibility as banks move away from less profitable lines of business and less desirable geographic regions. If these arguments bear out, the banking industry may benefit from deregulation, but the deadweight loss from increased monopoly power will reduce total welfare.

The difficulty in ascertaining which argument is correct is in isolating the effects of national bank deregulation among the many influences on aggregate economic growth. The U.S. economy is probably too large

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*Freeman:* Associate Professor, Department of Economics and International Business, Sam Houston State University, P.O. Box 2118, Huntsville, TX 77341-2118. Phone 1-936-294-1264, Fax 1-936-294-3488, E-mail freeman@shsu.edu

**ABBREVIATIONS**

BEA: Bureau of Economic Analysis
FDIC: Federal Deposit Insurance Corporation
SUR: Seemingly Unrelated Regressions



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and complex to be measurably affected by incremental developments in a single sector. As an alternative, some researchers have taken advantage of the state-chartering provisions of banking in the United States and the historical restrictions on interstate banking to focus on the effects of bank deregulation on economic growth at the state level. Because the states removed restrictions, especially those governing bank branching, at different times, a correlation of dates of changes in state growth rates with the dates of deregulation holds the possibility of identifying growth effects of bank deregulation.

For branching deregulation as a specific reform, papers by Jayaratne and Strahan (1996), Krol and Svorny (1996), and Strahan (2003), using fixed effects regressions of state panels of economic growth, find positive intercept shifts for states subsequent to years of deregulation. The effects of branch deregulation are found to be quite considerable, to the order of a permanent 0.5% increase in annual real state growth, or about $40 billion in additional income per year at current rates for the 37 states that have deregulated since 1970.

The difficulty in these types of exercises, however, is in separating the change in regulation from the existing economic environment. The 1980s, when many reforms were enacted, was a time of acute economic stress for many states, when banks and other financial institutions were struggling with bad loans in real estate and commodity sectors, and buyers were sought for the assets of failed institutions. If reform was in response to these conditions, a subsequent increase in economic activity may have been only a cyclical recovery, independent of any reform. In this case, estimates of the effects of bank branch deregulation in a simple regression framework will be overstated, a point argued in Freeman (2002).[1]

The common element in much of the previous work is the use of the date of the banking reform in each state as an exogenous variable in a model of structural change. The selected date effectively divides the sample into two periods, and growth rates are compared for the two periods for any significant changes, using the classical Student $t$ or $F$-tests of structural change as in Chow (1960). Christiano (1992), however, used simulations to show that standard critical values in tests of structural change with arbitrarily chosen change points yield test sizes that are far larger than the nominal 5% or 10%, resulting in a severe bias in favor of rejecting the no-structural-change hypothesis.

The bias stems from two problems: One is that the standard distribution used for the test is only asymptotically correct, even when the breakpoint is chosen independently of the data. The bias toward rejection of the null is especially large when testing breakpoints in the middle of relatively short time series, which is the case in the Jayaratne and Strahan (1996) and Strahan (2003), who analyze annual state growth rates over only a 20-year period. The second, and potentially more serious problem, is that the breakpoint is almost never chosen independently of the data, inducing endogeneity bias in the test for structural change. In this latter case, the true breakpoint may have occurred before or after the selected date, so that different researchers may reach quite different conclusions regarding the actual timing of the structural change, as shown by Hansen (2001).

Andrews (1993) works out the distribution theory of estimating unknown change point in the context of a general problem where a nuisance parameter (in this case the change date) exists in the alternative but not in the null hypothesis. Andrews calculates that the asymptotic 5% critical value for rejecting the null of no change when the change point is unknown is over twice that of the usual Chow-type test. More recently, a growing body of literature has further investigated the properties of endogenous change-point tests, with important extensions to the analyses of potential multiple change points by Bai (1997) and Bai and Perron (1998). Applications include tests of structural change in macroeconomic variability by McConnell and Perez-Quiros (2000), in Organisation of Economic Co-operation and Development growth rates by Ben-David and Papell (2000), and in pollution emissions by Fomby and Lin (2003).

These advances in structural change analysis suggest that models of the growth effects of

1. Abrams et al. (1999) also find that controls for branch banking status are not robust to the inclusion of other variables in tests of banking and fiscal policies on state economic growth. Wheelock (2003) argues that spatial correlation among states deregulating their banking sectors may have influenced findings of strong growth effects. Wall (2004) finds that the effects of deregulation on entrepreneurship differ across regions, with increases in some regions and decreases in others.

banking deregulation using preselected change points may have been biased either toward rejection of the null hypothesis of no change, or when change had occurred, toward misidentification of the true change point. That is, economically significant growth effects identified in previous research may have occurred, but the statistical significance and/or the timing of the effects, as well as their persistence, may have been mismeasured.[2] This last point is especially important in the present case, when the timing of policy changes may have been endogenous.

This article avoids the presumption of a known breakpoint at dates of bank deregulation when testing for structural change, opting instead to test for structural changes of unknown timing, and then comparing any dates identified using the Andrews (1993) critical values to dates of bank deregulation. The objective is to allow the data to "speak for itself" regarding structural changes. By estimating instead of preselecting change points, and by using accurately sized statistical tests, evidence can be adduced as to whether structural economic change led to change in regulatory conditions or the reverse. Although the case for deregulation per se will probably continue to stand on its merits, given the arbitrary and onerous nature of many of the individual state regulations,[3] a more accurate appraisal of its effects will be an important contribution to the literature on the role of financial sector development in studies of economic growth.

The sample period is extended in this article to the years 1959–2002, well before and after the period of intensive deregulation, to better ascertain whether any measured breaks are temporary or permanent departures from trends and/or levels. As noted, previous research used much shorter samples, in which the null hypothesis of no structural break is more likely to be rejected. In longer samples, there is a lesser likelihood that a cyclical movement would be interpreted as a structural change in trend.

The article proceeds as follows. Section II presents a discussion of the particulars of bank branch deregulation and a reproduction and robustness test of the Jayaratne and Strahan (1996) and Strahan (2003) results. Section III discusses the methodology of structural change tests and the approach to be taken with regard to the banking data. Section IV presents the empirical results, and section V concludes.

## II. BRANCH DEREGULATION AND ECONOMIC GROWTH

Prior to 1970, only 12 states allowed unlimited intrastate branching, and none allowed interstate branching. Kane (1996) notes that state banking laws went mostly unchanged from the 1930s until the 1970s due to a combination of historical aversion to the concentration of power in large financial institutions, the desire of states to protect a lucrative source of revenues from fees related to bank charters, and the political power of protected interests, especially in rural areas.

Beginning in 1975, however, the wall against bank branching began to crack. In that year Maine, followed a year later by New York and a year after that by New Jersey, relaxed some restrictions on intrastate branching. Berger et al. (1995) argue that the real impetus for widespread deregulation was the deregulation of small-deposit interest rates in 1980, followed almost immediately by the recessions of 1980 and 1981–82. The "perfect storm" that resulted from decontrolled costs at a time of financial stress was the occasion of many large bank failures and the passage, at the federal level, of legislation allowing out-of-state institutions to acquire failed banks and thrift institutions, regardless of state laws. Most states eventually responded with their own legislation, and by the early 1990s, only Iowa retained comprehensive restrictions on intra- and interstate branching. The Riegle-Neal Act of 1994 essentially eliminated any remaining federal restrictions on interstate branching.[4]

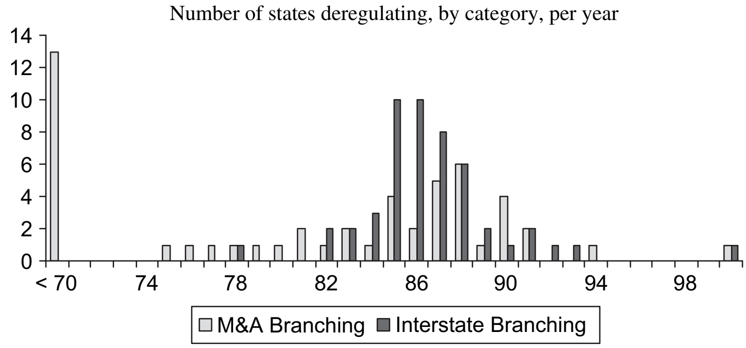
Figure 1 presents the distribution of state laws deregulating banking restrictions, by year, showing the concentration of this legislation in the 1980s. "M&A Branching" refers to banks acquiring branches either by merging

2. Hansen (2001) illustrates this point by showing that using standard chi-square critical values would have identified a structural break in labor productivity for any date chosen over a 24-year period.

3. Restrictive regulations in Illinois may account for the lack of any top 25 banks in the state, after the merger of Bank One with J.P. Morgan Chase, despite Chicago's history as a financial center (Schmeltzer 2003).

4. States were given the option of opting out of Riegle-Neal; only Montana and Texas chose to do so.

**FIGURE 1**
Frequency: Branching Deregulation




with other banks within the state or by buying individual branches from other banks, again within the same state. "Interstate Banking" refers to legislation allowing out-of-state banks to acquire in-state institutions as subsidiaries, but not branches. The flurry of deregulatory activity in the mid- to late 1980s is consistent with a response by the states to the rapid increase in their own bank failures after 1982 as noted by Jeon and Miller (2003) and to the competitive pressures of the deregulation trends in neighboring states as described in Garrett et al. (2003).

Banking deregulation clearly had a dramatic effect on the banking industry, especially in states with prior restrictions on branching. As shown in Figure 2, the Federal Deposit Insurance Corporation (FDIC 2004) reports that the number of institutions in Texas, a unit-banking state with the largest number of institutions of any state, fell from 1,854 to 669, more than 60 percent, whereas the number of branches increased by an order of magnitude, from 339 to 3,701. The vertical line in the figure is the year of Texas's deregulation of interstate mergers of state-chartered banks

For the total United States, the number of commercial banking institutions, which had fluctuated in a narrow range since the Great Depression, fell by almost one-half during the period 1984 to 2002, from 14,496 to 7,887. During the same period, the number of bank branches rose by more than 50%, from 41,799 to 66,185. This rate of increase in branches was actually somewhat less than in prior decades, as the increase in branching in states like Texas was partly offset by consolidations of branches in states like California and New York with long experience in branching.

As significant as the impact of financial deregulation was on the structure of the banking industry, the question remains as to the broader effects of branch deregulation on states' economies. As mentioned, Jayaratne and Strahan (1996), Strahan (2003), and Krol and Svorny (1996) have found quite large growth effects associated with branch deregulation, on the order of a 0.5% to 1% *permanent* increase in state real per capita income growth. These studies employ a fixed effects pooled time-series/cross section regression of the form:

$$(1) \qquad y_{it} = \mu_i + \tau_t + \gamma BR_{it} + \varphi X_{it} + \varepsilon_{it},$$

where $y_{it}$ is the annual growth rate in per capita income (calculated as log differences of income levels) for state $i$ in year $t$, $t = 1, \ldots, T$; $u_i$ is the state fixed effect; $\tau_t$ is the year time effect; $BR_{it}$ is an indicator variable with the value of 1 during the years subsequent to deregulation in state $i$ for time $t$; and $X_{it}$ is a vector of control variables, primarily employment shares by industry. In the regressions that follow, $BR_{it}$ refers to the earlier of the "M&A Branching" or "Interstate Banking" forms of deregulation presented in Figure 1.[5]

5. This specification is something of a hybrid of Jayaratne and Strahan (1996), who define the break date in terms of M&A Branching only, and Strahan (2003), who uses both M&A and Interstate Branching as separate dates (but finds that the coefficient of the latter is insignificant). All three specifications lead to identical qualitative and similar quantitative results.

**FIGURE 2**
Changes in Commercial Banking

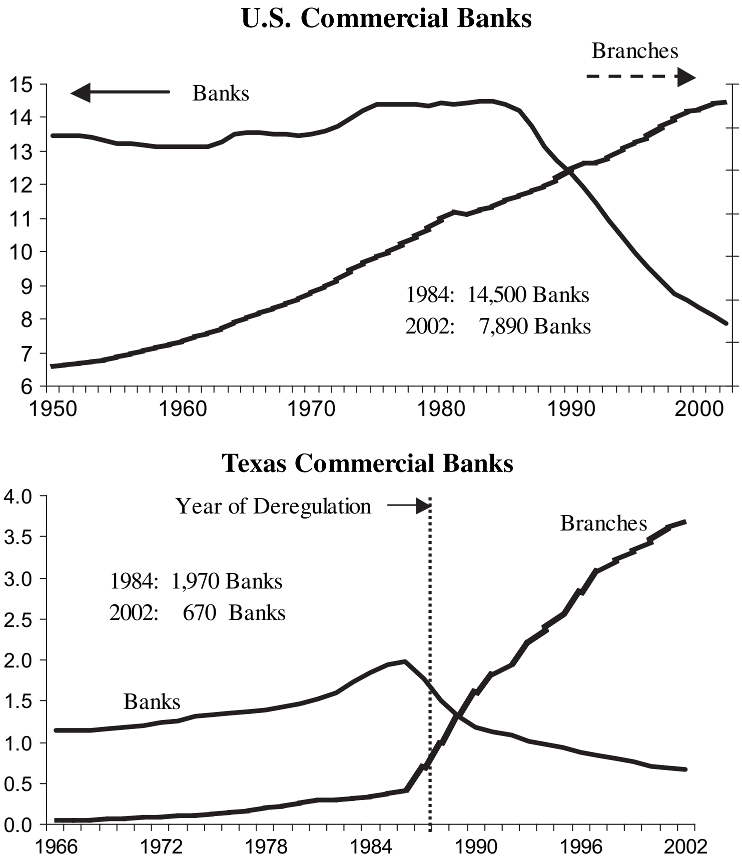



Table 1, model A reproduces the regression from Strahan (2003, Table 4), using the sample period 1976–96. Fixed and time effects are included in the regressions, but not shown; the control variables used by Strahan (the $X_{it}$) are omitted, as they did not materially affect the estimated coefficients of the branching variables. All coefficients in Table 1 are scaled to percentage points. The postbranching variable is shown to have a 0.67% positive effect on per capita income growth, quite similar to the range of results found in Strahan (2003).

Figure 1 and the foregoing discussion suggest, however, that states deregulated their banking sectors in response to economic events beyond their control, such as interest rate deregulation, competitive pressures, and the financial crises caused by the collapse of commodity prices and the double-dip recessions of 1980–82. If this is the case, the Strahan specification may be simply picking up the rebound from abnormally low growth rates *prior* to bank deregulation in each state, especially in such a short sample. In a test similar to that in Freeman (2002), I add a second dummy variable to equation (1), $PBR_{it}$, which equals 1 in each of the five years prior to the year of deregulation and 0 elsewhere.

I find, in model B, that the coefficient of $BR_{it}$ is still positive, but only about one-third its size in model A and no longer statistically significant at the usual levels. By contrast, $PBR_{it}$, the dummy for preregulation years is significant and is very close to the size of and of opposite sign to $BR_{it}$ in model A, indicating that the "rebound" hypothesis has

**TABLE 1**
Pooled Time-Series Cross-Section Tests of State per Capita Income Growth on Breakpoints for Banking Deregulation: 50 States, Annual Data, 1959–2002

| Variable | Model A Strahan Case (1976–96) | Model B Augmented Strahan (1976–96) | Model C Full Sample (1959–2002) |
|---|---|---|---|
| Postbranching | 0.67** (3.08) | 0.23 (0.88) | 0.41** (2.03) |
| Prebranching | — | −0.69** (3.22) | −0.65** (3.01) |
| Wald statistic | 9.498 (3.841) | 9.896 (3.841) | 21.01 (5.991) |

*Notes:* Absolute values of *t*-statistics for coefficients in parentheses; Wald statistic is distributed as $\chi^2$ with one degree of freedom in models A and B, two in model C. Upper 0.05 critical values for the $\chi^2$ distribution are in parentheses. Included in Tables 1 and 2 but not reported are fixed effects for states and years, as well as controls for employment share by industry, as in Strahan (2003). $^{**}p$-value $< 0.05$.

some empirical support and that the gains from branching deregulation may simply represent a return to trend growth rates.

As a further test, the sample is extended to include years prior to and after the Strahan (2003) time period. Model C includes the years 1959–2002. Extending the sample may provide a better test of what a departure from "normal" or trend growth rates would represent.[6] The year 1959 is chosen as the beginning date due to the high degree of variation within state growth rates prior to that year.

The results in model C appear to support both positions. Compared to the control period (all years prior to the deregulation year), the five-year period before is marked by below-trend growth and the postderegulation period by above-trend growth. The coefficient for the prebranching variable is virtually unchanged from model B, while the coefficient for the postbranching variable is about two-thirds its size in model A. And because model C is extended for six years after deregulation, the hypothesis of permanent growth effects would seem to receive further support.

This replication and extension of previous research using panel data methods suggests that the relationship between bank branch deregulation and state economic growth is more complex than one-way causality. One interpretation of the results of model C of Table 1 is that the states deregulated because of financial and economic distress, then saw the benefits in higher growth thereafter. To further test this interpretation, the analysis examines developments in individual states.

6. It should be recalled that the inclusion of time effects in equation (1) controls for national economic conditions, and therefore for the national business cycle.

### *Individual State Regressions*

One of the advantages of using pooled samples is the efficiency gained from the extra degrees of freedom, but the cost is potential bias if the heterogeneity of the sample is not adequately controlled. Extending the time dimension of the sample adds degrees of freedom and opens up the possibility of testing the hypothesis of deregulation's growth effects on the individual states while avoiding bias problems from mixing heterogeneous time series. Table 2 reports summary results for regressions of the 37 states in the sample that deregulated branch banking for the 1976–96 Strahan (2003) sample and 1959–2002, the extended sample. For each period, the basic model, containing only the postbranching variable, and the augmented model, containing both the pre- and postbranching variables, are estimated. The dependent variable is now the difference between state and national per capita income growth, which is comparable to controlling for national effects in the pooled models with time effects summarized in Table 1.[7]

The results for individual states in model D agree with those from the pooled sample in model A, with an average coefficient across states within 20% of the pooled coefficient of model A in Table 1. Thirty states have positive coefficients (seven of which are significant) and seven states having negative coefficients (one of which is significant). If we make the assumption that the 37 individual

7. One difference is that the time effect results in the unweighted average of state growth rates is controlled for, rather than the weighted average (national growth), but this difference is small.

**TABLE 2**
Individual State Regressions of Relative State per Capita Income on Breakpoints for Banking Deregulation: 37 States with Branching Restrictions, Annual Data, 1959–2002

| Variable | Model D Strahan Sample (1976–96) | Model E Augmented Strahan (1976–96) | Model F Full Sample (1959–2002) | Model G Augmented Full Sample (1959–2002) |
|---|---|---|---|---|
| Postbranching ($BR_{it}$) | | | | |
| Mean for all states | +0.51 | +0.49 | +0.18 | +0.04 |
| Number of states > 0 | 30 (7) | 25 (4) | 23 (5) | 23 (4) |
| Number of states < 0 | 7 (1) | 4 (1) | 14 (0) | 14 (0) |
| Prebranching ($PBR_{it}$) | | | | |
| Mean for all states | — | −0.39 | — | −0.89 |
| Number of states > 0 | | 14 (0) | | 8 (2) |
| Number of states < 0 | | 15 (5) | | 29 (13) |
| Fisher's $P_\lambda$ | 132.98 (99.62) | 74.27 (85.96) | 76.14 (99.62) | 211.5 (99.62) |

*Notes:* Number of coefficients with $p < 0.10$ in parentheses. Fisher's $P_\lambda$ is given by $-2\sum_{i=1}^{n} \ln(p_i)$, where $p_i$ is the $p$-value of the $t$-statistic for the coefficient of the dummy variable, and $n$ is the number of states. In models D and F, the postbranching coefficients are tested; in models E and G, only the prebranching coefficients are tested. Fisher's $P_\lambda$ is distributed as $\chi^2_{2n}$ The upper 0.05 critical values for the $\chi^2$ distribution are in parentheses.

regressions are independent tests, we can use a statistic originally attributed to Fisher and described by Maddala (1977) to conduct a meta-analysis test of the null hypothesis that branching deregulation had no effect on state income growth.[8] Fisher's $P_\lambda$ is defined as $-2\sum_{i=1}^{n} \ln p_i$, where $p_i$ is the $p$-value of the $t$-test of the $i$th state's coefficient of the branching variable in equation (1). Fisher's $P_\lambda$ is distributed as $\chi^2_{2n}$, where $n$ is the number of states. The results of the $P_\lambda$ test for model A support rejection of the null, with a test statistic of 132.98 and a 0.05 critical value of 99.62, again in agreement with the pooled sample of model A, Table 1.

In model E, I add the prebranching variable and test its significance using the Fisher test. The value of the test for model E is not significant at the 0.05 level, however, indicating that the prebranching variable is not significant for the short sample.[9] Most of the coefficients of the prebranching variable are negative, as is the average, but only five are significant. The coefficient of the postbranch variable is relatively unchanged from model D. The combined information from models D and E tends to support the position that branch banking had positive one-way effects on growth for the time period in question.

Extending the sample to the 1959–2002 period for the individual state regressions, though, yields different results. The average coefficient in the single variable postbranching case of model F is much reduced, at +0.18, with only 23 coefficients positive and only 5 of those significant at the 0.10 level; Fisher's $P_\lambda$ is less than the 0.05 critical value as well. In the augmented model G, the average coefficient of the postbranching variable is even lower, and the prebranching variable is negative in 29 states, with a relatively large average value of −0.89. Fisher's $P_\lambda$, which in model G considers only the prebranching coefficients, is quite large and well exceeds the 0.05 critical value for the test. In the extended sample, therefore, the evidence supporting the arrow of direction from economic events to deregulation is much stronger than the evidence for

8. Clearly the regressions do *not* constitute independent tests. As shown by Garrett et al. (2003), there is strong evidence of spatial correlation across neighboring states in the deregulation decision. The use of seemingly unrelated regressions (SUR) was considered, but in the restricted time period Strahan case, the number of states exceeds the number of time periods, so SUR is not feasible. In the full sample case, SUR is feasible, but the number of states (37) is so close to the number of time periods (44) that the determinant of the cross-correlation matrix is very close to zero, very likely biasing downward the resulting estimates of the standard errors of the coefficients, as in Beck and Katz (1995). The actual size of Fisher's test in the event of cross-correlation tends to exceed the nominal size from the standard $\chi^2$ distribution. However, the use of Fisher's $P_\lambda$ in the present case is meant to be only suggestive.

9. There are only 29 states in model E, as the early deregulation year prevented using a prebranching variable in the remaining states.

the reverse arrow of deregulation causing economic events.

The foregoing analysis suggests that the case for significant and permanent economic effects from bank branch deregulation is far from ironclad. If anything, the evidence appears to be at least as strong that states deregulated because of economic conditions and that the stronger growth following the years of deregulation is better explained by a simple return to trend growth. In the following sections, I take an agnostic view of the cause–effect relationship between deregulation and economic growth and allow the data to inform us of any significant changes in the growth paths of state and regional economies. If any changes are thus identified, I shall then compare them with known dates of deregulation, providing the evidence that I seek.

## III. METHODOLOGY: CHANGE-POINT TESTS

For testing structural changes in the mean growth rate, a variation of the following equation will be estimated for each of the 50 states and for regions and other combinations of states:

$$(2) \qquad y_{it} = u_i + \beta_i y_t + \sum_{i=1}^{p} \gamma_i DU_{\tau i} + \sum_{i=1}^{p} \delta_i DU_{\tau i} y_{\tau i} + \varepsilon_{it},$$

where $y_{it}$ is the annual growth rate in per capita income for state $i$ in year $t$, $t = 1, \ldots, T$; $u_i$ is the state-specific intercept; $y_t$ is growth rate in national per capita income; $DU_{\tau i} = 1$ for years $t > \tau_i$, where $\tau \in ([0.15T],[0.85T])$ and the parentheses denote an interval and square brackets denote the greatest integer function; and $u_i$, $\beta_i$, $\gamma_i$, and $\delta_i$ are parameters to be estimated. This specification controls for the effects of changes in national income on state income growth, while testing for any structural change in that relationship. Any $\gamma_i \neq 0$ and/or $\delta_i \neq 0$ will constitute evidence of structural change.[10]

The procedure is to estimate (1) first with $\gamma_i = \delta_i = 0$, the null hypothesis, then over the range of $\tau$ for the alternative(s). Let $S_R$ denote the sum of squared residuals under the null hypothesis, and $S(\tau)$ the sum of squared residuals under the alternative. The candidate for a change point is the estimate, $\hat{\tau}$, that minimizes the quantity $S(\tau)$. Equivalently, because $S_R$ is a fixed value, $\hat{\tau}$ is the estimate that maximizes the Wald statistic:

$$(3) \qquad W(\tau) = (S_R - S[\tau])/\hat{\sigma}^2(\tau),$$

where $\hat{\sigma}^2(\tau) = S(\tau)/(T - k)$ is the estimated error variance of the unrestricted model, and $k$ is the total number of (unrestricted) parameters. In classical testing with known change point, $W(\hat{\tau})$ is distributed $\chi^2_R$, where $R$ is the number of restrictions under the null. The critical values of this statistic for unknown change points have been tabulated by Andrews (1993), and will be used to test whether (1) has undergone a structural change.[11]

If $W(\hat{\tau})$ exceeds the critical value for the test, the sample will be divided and tested for further breaks within the subsamples using the sequential methodology of Bai and Perron (1998) to identify multiple breaks. In this article, I use the refinement technique of Bai (1997), whereby if a second breakpoint is found, it is used to reestimate the first breakpoint.[12] Bai (1997) also works out the asymptotic distribution of the estimated change point, $\hat{\tau}$, which in turn provides the method for computing confidence intervals for the estimate. The confidence interval is given by:

$$(4) \qquad [\hat{\tau} - [c/\hat{L}] - 1, \hat{\tau} + [c/\hat{L}] + 1],$$

where $c$ is the $(1 - \alpha/2)$th quantile of the limiting distribution, $\hat{L}$ is a scaling factor that depends on the data, and $[c/\hat{L}]$ is the integer part of $c/\hat{L}$. Because the errors appear to be homoscedastic

10. In one version of the model, $\delta_i$ is restricted to be 0; in another, $\delta_i = 0$, and $\beta_i = 1$.

11. Given that the uncertainty of the change point is likely to lead to an increased number of rejections of the null hypothesis over repeated samples, the critical values computed by Andrews (1993) are considerably larger than if $\hat{\tau}$ is known. I compute $p$-values for the *sup-W* statistic using the method of Hansen (1997).

12. It is mainly for the refinement and sequential tests for multiple breaks that I use the asymptotic values reported in Andrews (1993), rather than simulating the values for the present sample via the bootstrap; this is also the approach followed by Bai (1997). In simulations of a single break in mean growth using the individual states (5,000 replications), the 5% and 10% critical values for the *sup-W* statistic (using a 15% trim) were 9.32 and 7.83, respectively, versus the 8.85 and 7.17 reported by Andrews (1993). I also use the Andrews values because I prefer to be somewhat biased toward identifying breakpoints.

across regimes and serially uncorrelated, the formula above is case 1 in Bai (1997).[13]

As noted, the aim of the analysis is to identify any structural changes in the time series of state growth rates and attempt to relate them to the known dates of state regulatory changes. It is important to note that not all states passed deregulation measures during the sample; some, for example, had no restrictions on branching to begin with. These states comprise an important control to ascertain whether states that did pass legislation actually experienced more structural changes during the sample than the "control" states.

In the following section, change-point tests are applied to individual states, and then, because the state results are ambiguous, to states grouped according to the regional designations of the Bureau of Economic Analysis (BEA).

## IV. RESULTS OF CHANGE-POINT TESTS

Results of applying the change-point tests of equation (2) for individual states were inconclusive. For space considerations, the findings will be summarized; complete results are available on request.

Of the 50 states tested, there were 18 with significant change points, 7 with a single breakpoint and 11 with two, using either models with restricted (i.e., $\beta_i \equiv 1.00$) or unrestricted coefficients on national income. Of these, 13, or 72%, were states that deregulated after 1970, almost exactly the same percentage as this category in the total population. For the deregulating states, five had positive first breaks and two negative prior to deregulation, and three had positive first breaks and three negative after deregulation. Thus there does not appear to be a preponderance of evidence for either direction of causality between bank deregulation and economic growth. If anything, the majority of positive breaks prior to deregulation argue against either of the competing hypotheses.

Of special note in this regard is the complete reversal of sign changes in the 11 states with second breaks. In effect, the "structural breaks" in this set of states were temporary deviations from trend growth, later partially or completely eliminated. This behavior is not consistent with research that found large and permanent differences in state growth rates following bank deregulation.[14] Thus it is difficult to draw conclusions regarding the causes or effects of bank deregulation and economic growth using change-point analyses of state-level data. There is no easily discernible pattern between breakpoints and deregulation; most breaks appear to be temporary departures from long-term trends.

As noted by Wheelock (2003), however, and explored in a model of spatial autocorrelation of banking deregulation by Garrett et al. (2003), there is strong evidence of regional patterns in both economic events and in states' decisions to deregulate during the years 1975–90, when most deregulation events occurred. States in the West were most likely to permit unlimited branching prior to 1970. States in the Northeast were the first to deregulate, often in regional compacts that allowed holding companies in one member state to locate subsidiary banks in other member states. States in the Midwest and South were next to deregulate, mainly in response to the banking crises and failures associated with falling oil and commodity prices and the recessions of 1980–82. States in the Plains region were among the last to deregulate, and then only in advance of national legislation (the Riegle-Neal Act of 1994).

These considerations suggest that examining the data using regional aggregation may be useful. The states are grouped by BEA classification into eight regions: New England, Middle Atlantic, Great Lakes, Plains, Southeast, Southwest, Rocky Mountain, and Far West.[15]

13. See section II.D and appendix B in Bai (1997) for more details on constructing the confidence intervals for the estimated breakpoints.

14. Baier et al. (2004) point out that a financial innovation can increase output growth permanently only in the case that the innovation causes a permanent increase in input and/or total factor productivity growth. In the present case, it is not obvious how branch deregulation would lead to the continuous increases in human capital or investments in new knowledge necessary to support permanently higher growth rates. However, as Baier and colleagues note, an increase in the output *level* that is large relative to the variance of the growth rate may cause average growth rates to be higher for quite long intervals.

15. The states comprising these regions are: New England: Connecticut, Maine, Massachusetts, New Hampshire, Rhode Island, and Vermont; Middle Atlantic: Delaware, District of Columbia, Maryland, New Jersey, New York, and Pennsylvania; Great Lakes: Illinois, Indiana, Michigan, Ohio, and Wisconsin; Plains: Iowa, Kansas, Minnesota, Missouri, Nebraska, and North and South Dakota; Southeast: Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Mississippi, North and South Carolina, Tennessee, Virginia, and West Virginia; Southwest: Arizona, New Mexico, Oklahoma, and Texas; Rocky Mountain: Colorado, Idaho, Montana, Utah, and Wyoming; Far West: Alaska, California, Hawaii, Nevada, Oregon, and Washington.

**TABLE 3**
Tests of structural change, regional growth rates relative to national average, 1959–2002

| | | With National Income as Dependent Variable | | | | | |
|---|---|---|---|---|---|---|---|
| Region[a] | Regional Average[b] | First Break | Second Break | Relative Growth Prebreak | Relative Growth Post–First Break | Relative Growth Post–Second Break | $\beta_i$ |
| New England; CT: 80, ME: 75, MA: 83, NH: 87, RI: 70, VT: 70 | 81.3 | 79 (77, 81)[c] | 89 (86, 92) | 0.45 | 2.45 | 0.53 | 0.890 |
| Middle Atlantic; DE: 70, DC: 70, MD: 70, NJ: 77, NY: 76, PA: 82 | 78.3 | 80 (76, 84) | 91 (88, 94) | 0.66 | 1.57 | 0.44 | 0.874 |
| Southeast; AL: 81, AR: 89, FL: 85, GA: 83, KY: 84, LA: 87, MS: 86, NC: 70, SC: 70, TN: 85, VA: 78, WV: 87 | 84.5 | 75 (71, 79) | 90 (85, 95) | 1.85 | 1.09 | 0.46 | 0.902 |
| Southwest; AZ: 70, NM: 89, OK: 87, TX: 87 | 87.7 | 82 (80, 84) | 89 (87, 91) | −0.13 | −2.60 | −0.37 | 1.130 |
| Mountain; CO: 88, ID: 70, MT: 90, UT: 81, WY: 87 | 86.5 | 83 (79, 87) | 90 (87, 93) | 0.09 | −1.81 | 0.43 | 1.018 |
| Average | | | | 0.58 | 0.14 | 0.30 | 0.963 |
| Avg Abs Δ | | | | | 1.61 | 1.66 | |

[a]Regions with no structural breaks include Great Lakes (average year of deregulation: 84.8), IL: 86, IN: 86, MI: 86, OH: 79, WI: 87; Plains (87), IA: 91, KN: 87, MN: 86, MO: 86, NE: 85, ND: 87, SD: 70; Far West (85.3), AK: 70, CA: 70, HI: 86, NV: 70, OR: 85, WA: 85.

[b]Refers only to states in the region that deregulated after 1970.

[c]95% confidence intervals for the change points; see text.

These regions contain neighboring states, reflecting shared economic characteristics and the high likelihood of cross-state banking relationships, either through interstate lending or correspondent banking.

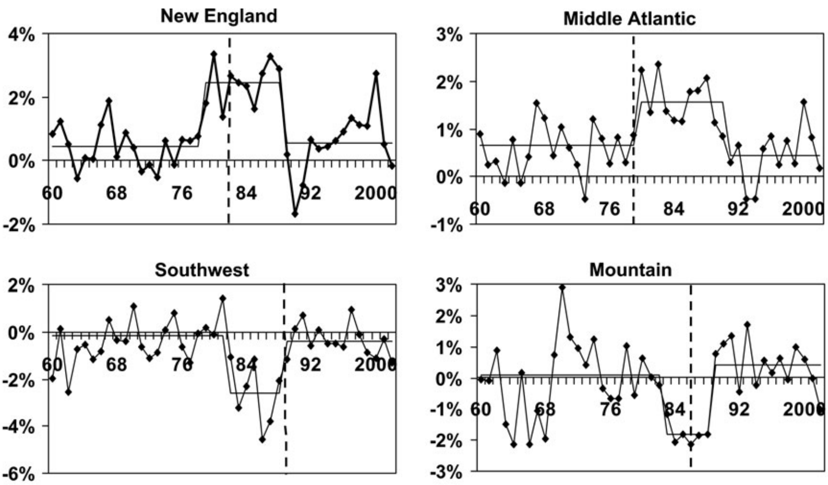
The regions are subjected to the same analysis as the states, with the results reported in Table 3. The use of region-level data makes clear that the 1980s were an unusual decade for regional performance relative to the national economy. Five of the eight regions, or 62.5%, experienced structural breaks during the sample period versus only 35% of individual states. There were no regions with single breaks. All first breaks occur during 1975–83; all second breaks occur during 1989–91.[16] The range of estimated intercepts for the five regions is 1.88 in the prebreak periods, 4.60 in between the first and second breaks, and 0.70 after the second break. That the growth paths of the five regions should become so similar during the 1990s suggests a convergence of economic growth rates (if not of levels) among the regions.

The five regions with structural breaks can be segmented into three groups. The first group, New England and the Middle Atlantic, experienced a positive shift in relative growth rates from 1979–80 until 1989–91, then a reverse negative shift, after which relative growth was close to its prebreak average. States in these regions were “early adopters” of bank deregulation, with the average date of deregulation corresponding roughly to the date of the first structural break. The confidence intervals for the estimated first breakpoints both have upper limits less than or equal to 1984. Only one state for this group, New Hampshire (1987), has a deregulation date later than 1984.

The second group has a single region, the Southeast, which experienced two negative shifts in relative growth, the first in 1975 from 1.85 points above the national average, to 1.09

16. The regions without significant breaks, the Great Lakes, the Plains, and the Far West, had $p$-values for two structural break tests of 0.27, 0.86, and 0.98, respectively, with corresponding first break–second break years of 1978–1984, 1976–1989, and 1981–1991.

**FIGURE 3**
Regions with Structural Change in Growth Rate




*Note:* Lines with markers are the quantities $y_{it} - \beta_i y_t$; straight lines are estimated intercepts using stuctural breaks. Dashed vertical lines are average years of bank deregulation for the regions.

points above the national average; and in 1990, from 1.09 points above the national average to 0.46 points above the national average. States in this region were "middle adopters" of bank deregulation, with the average date equal to the middle of 1984. The principal cause for the structural breaks in the Southeast appears to be the convergence of incomes in the relatively poor Southeast with the rest of the country. In the 1960s and 1970s the Southeast was in a "catch-up" period when income was growing faster than the national average. Per capita income in the Southeast was 75% that of the national average in 1960, 81% in 1970, 86% in 1980, 89% in 1990, and 89% in 2000. Thus this convergence process was mostly completed by the date of the second structural break.

The third group, the Southwest and Mountain, experienced a negative shift in relative growth rates in 1982–83, then a reverse positive shift and return to approximate prebreak growth rates in 1989–90. States in these regions are "late adopters" with an average date of deregulation for the two regions of 1987, very close to the end of the post–first break period. Only one state in this group, Utah (1981), deregulated before 1987, the lower limit of the confidence intervals of the second breakpoints for each region. The pattern of growth in this group is practically the mirror image of the pattern in the first group. Figure 3 illustrates the shifts in growth rates for the four regions comprising the first and third groups. The jagged line in each chart is the quantity $y_{it} - \hat{\beta}_i y_t$, estimated regional growth relative to national growth. The solid line is the quantity $\hat{\mu}_i + \hat{\gamma}_1 DU_{\hat{\tau}1} + \hat{\gamma}_2 DU_{\hat{\tau}2}$, trend growth with the national influence removed. The vertical, dashed line is the average year of bank branch deregulation for the region.

In group one, in the top half of the figure, and three, in the bottom half, bank deregulation precedes increases in trend growth, but the circumstances are much different. The Northeast and Middle Atlantic deregulated at the start of a regional boom, which lasted throughout the 1980s until a slowdown to more normal growth in the 1990s. The Southwest and Mountain deregulated toward the end of a regional bust, which began several years before and ended with a return to more normal growth in the 1990s. In all the regions in Figure 3, therefore, the decade of the 1980s was something of an aberration. The changes in trend growth were temporary in nature. Only in the Southeast, not shown in Figure 3, was the structural change a permanent one, and that to growth rates more consistent with the national average.

In summary, the regional results reflect a high degree of economic turbulence during period 1975–90, but provide no evidence of a systematic relationship between financial deregulation occurring during the period and the ups and downs of regional growth. In four regions, growth following deregulation was higher than that contemporaneous with deregulation, but the growth increase was either temporary or a recovery to former levels. In a fifth region, the Southeast, growth decreased following deregulation, but the decrease appears to be part of a long-term adjustment.

## V. SUMMARY AND CONCLUSIONS

The structure and regulation of financial markets and institutions are the focus of much policy attention for good reason. The view that a well-functioning financial system exerts a powerful influence on a country's economic growth has strong support in recent empirical research. Financial institutions play the key role in transmitting monetary and credit policies to the economy, and to the extent that deregulation of the sector can result in a more efficient flow of funds and information, the fewer the regulatory burdens, the better.

However, care should be taken to ensure that policy effects are measured as accurately as possible. Proposed changes to current regulations, even when demonstrably beneficial, may be met with skepticism if experience suggests that earlier proposals were "oversold." This article amends existing research on the growth effects of bank branch deregulation by demonstrating that previous findings overstated the incremental growth effects of deregulation. Using recent developments in the estimation of structural change in economic time series, this research suggests that stronger growth rates following deregulation had no consistent ordering of cause and effect and were in all cases temporary rather than permanent. The preemptive deregulation of the financial system in the Northeast and the Middle Atlantic appears to have had little to do with the reactive deregulation in the Southwest and Rocky Mountain states.

This research remains consistent with the proposition that deregulation is efficiency-enhancing. The reaction of the banking industry was to deregulation was swift, with rapid consolidation and an expansion of branching networks, and so revealed preference suggests that producer surplus is higher than it otherwise would be. Whether consumers are better off under bank deregulation is an open question and beyond the scope of this study. A closer examination of the causes and the effects of deregulation is in order, with more attention paid to the transmission mechanism of changes in financial institutions on the economy and to the feedback of economic events on the political economy of regulation.

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