# Credit Be Dammed: The Impact of Banking Deregulation on Economic Growth

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**Abstract**

This paper examines channels through which state-level financial deregulation in the United States causes economic growth. We find that states that deregulated bank branching laws relatively early experienced a significant increase in per capita income compared to their controls, but states that deregulated later experienced no abnormal economic growth from deregulation. We use this heterogeneity to examine channels—the bank efficiency channel, the borrower quality channel, and/or the dammed credit channel—through which deregulation may cause economic growth. Our results point to the dammed credit channel as the causal link between financial deregulation and economic growth.

## I. Introduction

This paper explores three channels through which financial development may cause economic growth. We exploit the staggered, state-by-state deregulations of bank branching restrictions in the United States where, between 1970 and 1996, 35 states deregulated their intrastate bank branching restrictions at various times. Previous research using this setting, such as Jayaratne and Strahan (1996), finds that branching deregulation causes economic growth. We use the rich heterogeneity of this quasi-experimental setting to study the channels through which deregulation causes growth.

Our identification of these channels relies on the variation in the treatment effect of bank branching deregulation on growth. The traditional method of inference, differences-in-differences, loses much of the sample heterogeneity by pooling states into one treatment group and comparing it to one control group. Instead, we use an empirical technique called the synthetic controls method. Synthetic counterfactuals control for not only observable covariates that might relate to a state's economic growth, but also unobservable factors (see Abadie and Gardeazebal, 2003, and Abadie, Diamond, and Hainmueller, 2010). The method constructs a synthetic control unit for each deregulating state which provides a credible counterfactual for studying the impact of deregulation and avoids important shortcomings of the differences-in-differences method in this setting.

The synthetic controls method is similar in spirit to the tracking portfolio approach of Lamont (2001). For each deregulating state we construct a "portfolio" of non-deregulating states as a synthetic control, designed to match the deregulating state as closely as possible. The main benefit of this approach is that we can construct a data-driven counterfactual for each event state, and, importantly, examine time-series and cross-sectional heterogeneity in the economic impact of deregulation.

Our results paint a different picture of bank branching deregulation and its role in economic growth than the prior literature suggests. In the full sample of deregulating states, we find that the average effect of deregulation on economic growth is indistinguishable from zero. Our findings suggest that if regulators were to assign deregulation events randomly—the setting that an empirical study might try to reproduce via truly exogenous variation in financial development—there would be no statistically significant economic growth effect. However, the data also show that individual state-level economic growth following deregulation exhibits significant heterogeneity.

Prior research suggests that the treatment effect of deregulation may be different for early and late deregulators. For instance, DeLong and DeYoung (2007) and Huang (2008) suggest that the staggered nature of bank branching deregulation allowed states that deregulated later in the sample to learn from the early deregulators and hence to benefit more from the deregulation. In contrast, Kroszner and Strahan (1999) argue that states that deregulated early had strong economic incentives to do so. Consistent with Kroszner and Strahan (1999), bank branching deregulation only results in branching activity in early deregulating states and not late deregulating states. We find that early deregulating states in our sample experience large growth effects from bank branching deregulation while late deregulating states experience no growth effects. We interpret our results as evidence that early deregulators had stronger economic motives to deregulate, used deregulation to expand branching networks, and subsequently experienced economic growth.

We examine three channels through which deregulation may cause economic growth in the early deregulating states: a bank efficiency channel, a borrower quality channel, and a dammed credit channel. The *bank efficiency channel* suggests that deregulation changed the operation of banks: deregulation spurred competition among banks, causing banks to improve the efficiency of their operations, thereby making better, more efficient loans with less waste of resources. The *borrower quality channel* suggests that deregulation changed the pool of borrowers, by facilitating access to financing for small businesses and therefore spurring innovation. Strahan and Weston (1998) and Black and Strahan (2001) argue that branching deregulation increased risk-sharing, allowing banks to make more loans to small businesses which may have been unable to obtain financing previously. Hence, bank branching deregulation may lead to more innovative and productive utilization of loans. Finally, the dammed *credit channel* suggests that regulatory frictions impeded the flow of capital because banks could not freely move capital between branches to satisfy local demand. Deregulation would have enabled banks with extensive branch networks to allocate capital more efficiently by turning deposits from one geographic region into loans in another geographic region.

We utilize the fact that only early deregulating states have abnormal post-deregulation growth to identify the channels that lead to economic growth. If the bank efficiency channel is the reason deregulation causes growth, we would expect to see banks becoming more efficient following deregulation in the early deregulating states, but not in the late deregulating states where there is no abnormal growth. We find that bank efficiency improves in both the early and late deregulating samples. These findings suggest that bank efficiency is not sufficient to explain why deregulation causes economic growth.

If the borrower quality channel is the source of economic growth, we would expect to see a change in measures of borrower or project quality. One proxy for changing project quality that has been recently used in the literature is patent activity. We examine whether there is an increase in innovation (patent activity) after deregulation in the early deregulating states, but no abnormal innovation after deregulation in the late deregulating states. Instead, we find that abnormal patent activity decreases in both the early and late deregulating states. These results are inconsistent with the borrower quality channel linking financial deregulation to economic growth.

Our results are consistent with the dammed credit channel underlying the causal link between bank branching deregulation and economic growth. We find evidence that bank branching deregulation resolved excess demand for loans in early deregulating states—but not late deregulating states—which led to significant economic growth in those early deregulating states. We find that borrowers in early deregulating states faced borrowing costs 50 basis points higher compared to borrowers in a control group of non-deregulating states. Within two years of deregulation, banks in early deregulating states began to lend significantly more than their controls, at which point abnormal demand and abnormal borrowing costs returned to levels indistinguishable from zero. Specifically, we find a statistically significant increase in per capita loans of 35% relative to pre-deregulation loans per capita among early deregulating states. We also document an increase in per capita income of about $790 within 5 years of deregulation. We conclude that resolving the dammed credit problem is a major contributory channel through which deregulation causes economic growth.

Finally, we make a methodological contribution by showing how the synthetic controls method from Abadie, Diamond, and Hainmueller (2010) can be applied to a setting with multiple treated units. The synthetic controls method provides a way of advancing the finance-growth nexus literature beyond simple differences-in-differences estimation.[1] The method allows us to form strong counterfactuals, while maintaining external validity by capturing whole state economies. In addition, having individual state-level counterfactuals allows us to explore the heterogeneous impact of deregulation on economic outcomes and to identify cleanly the likely channels through which financial deregulation causes economic growth.

[1] Besley and Case (2000), and Bertrand, Duflo, and Mullainathan (2004) all note various econometric challenges to the differences-in-differences framework.

The remainder of the paper is organized as follows. Section II provides a brief historical account of bank branching deregulation in the United States and the use of this natural experiment in the literature. Section III introduces the synthetic controls methodology and its application to the bank branching deregulation setting. Section IV presents our findings on the channels through which deregulation causes economic growth. Section V presents robustness tests and Section VI concludes the paper.

## II. Bank Branch Deregulation and Economic Growth

This section reviews the history of bank regulation in the United States and discusses the series of staggered state-level deregulations. We highlight the rich cross-sectional and time-series variation inherent in the history of US bank deregulation. Finally, we review the ways in which the previous literature has exploited this variation to link financial development to economic growth.

### A. History of State-Level Bank Regulation

The history of state-level bank regulation and subsequent deregulation provides a framework for exploring the effects of deregulation on economic growth. Until 40 years ago, banking in the United States was heavily regulated at the state level. State laws prevented interstate and intrastate bank branching. These laws restricted banks from opening new branches throughout the state and they restricted bank holding companies from consolidating subsidiaries into branches.

Like other researchers, we focus on 35 states that relaxed their intrastate branching laws during our sample period, 1970 to 1996. Several key pieces of legislation pushed states to deregulate throughout the sample period. In 1975, Oregon and Tennessee began to allow out of state bank holding companies to own in-state banks. The federal Bank Holding Company Act of 1982 allowed failed banks to be acquired by any holding company regardless of state location, bypassing state-level restrictions of these acquisitions. In 1994 the Riegle-Neal Interstate Banking and Branching Efficiency Act led all remaining states to deregulate intrastate branching (Sherman, 2009).

Deregulation typically occurred in two steps. The first step was allowing merger and acquisition (M&A) branching, which permitted bank holding companies to convert subsidiaries into branches or to purchase other banks and convert them into branches. The second step allowed banks to branch via de novo branching, meaning that banks could originate and locate new branches anywhere in the state. So that our results will be comparable to prior literature, we use the M&A deregulation date. Table 1 provides the details of bank deregulation by state and year.

[Insert Table 1 here]

**B. Data**

We collect data on economic conditions, population, and banking sector characteristics for each state. In this section we outline the sources of our data. Our sample spans the period from 1970 to 1996 and covers 35 intrastate deregulations that occurred between 1975 and 1991. Our analysis requires 5 years of data before and after each deregulation event. As in Jayaratne and Strahan (1996), we exclude Delaware and South Dakota from our sample due to the presence of unique tax incentives that eliminated usury ceilings in order to attract credit card banks. Thirteen bank deregulations occurred prior to our sample period. The 13 deregulation events include 12 states and the District of Columbia. For the remainder of the paper we refer to these deregulations as the 13 states that deregulated prior to the beginning of our sample and assign them a deregulation year of 1971. For a detailed history of bank branching deregulation see Sherman (2009).

To conduct our empirical analysis we gather state-level data from the Bureau of Economic Analysis (BEA), the Bureau of Labor Statistics (BLS), the US Census Bureau, the Federal Depository Insurance Corporation (FDIC), and the National Bureau of Economic Research (NBER). We use personal income per capita from the BEA to measure state-level economic growth. We measure personal income per capita in 2005 US dollars using the consumer price index (CPI) deflator from the BLS and scale personal income by the annual population per state to generate income per capita. We obtain state-level annual population data from the US Census Bureau. In order to control for the health of a state's economy prior to deregulation, we gather data on the size of the labor force and the level of unemployment from the BLS. We calculate the population density of a state as the ratio of the total state population and the total area of the state, measured in square miles. The dataset includes average housing prices in each state.

We also collect information on bank characteristics that we hypothesize will influence a state's choice to deregulate and the economic impact following deregulation. We measure the average size of an institution's branch network as the number of branches divided by the number of institutions. Using bank balance sheet data from the FDIC, we compute the ratio of non-interest expenses to assets as a measure of bank lending inefficiency. We compute average loan prices as the ratio of total income from loans and leases to total loans and leases minus the ratio of total interest paid on deposits to total liabilities. We include a measure of loans growth as the year over year growth in the dollar amount of loans in each state. We measure bank profits as net income scaled by deposits.

Following the recent literature, we proxy for innovation in a state with the number of successful patent applications (Amore, Schneider, and Zaldokas, 2013; Chava, Oettl, Subramanian, and Subramanian, 2013; Cornaggia, Mao, Tian, and Wolfe, 2013). We use the National Bureau of Economic Research (NBER) patent data which includes data on all the patents awarded by the US Patent and Trademark Office (USPTO). To measure patent growth, we sum all of the patents in each year for each state and scale state-level annual patents by the state's 1970 patent level.

Table 2 presents summary statistics for our sample states. The mean per capita income over the 27 year period is $21,928, measured in 2005 US dollars. Average income growth over our sample period is 2.29%, which is consistent with average income growth over the period. The population density of states in our sample is 343 individuals per square mile. The patent data exhibit a steady increase in aggregate state-level patenting activity between 1970 and 1996. On average, the number of patents is 20% higher than its 1970 level. . The ratio of non-interest expenses to assets aggregated at the state level ranges from 1.81% to 6.63% with a mean of 3.12%. The average interest rate spread (loan rate – deposit rate) is 6.11%, which is consistent with an average loan rate of 9.92% and an average deposit rate of 3.86%.

[Insert Table 2 here]

**C. Replication and Literature Review**

Bank branching deregulation has provided a source of exogenous variation in access to banking in numerous academic studies. Jayaratne and Strahan (1996) establishes a causal link between deregulation and economic growth. Related literature suggests banks became more efficient after deregulation (Jayaratne and Strahan, 1998). Calem (1994) shows that banking markets consolidate after deregulation. Clarke (2004) finds that bank deregulation enhances short-run economic growth.

Studies subsequent to Jayaratne and Strahan (1996) find heterogeneity in the effect of deregulation on economic growth. Specifically, Wall (2004) controls for regional effects and finds that the deregulation effect varies across regions. In addition, Freeman (2002) compares the economic growth in each deregulating state to the economic growth of the national economy and finds that states deregulate when their economy has underperformed persistently relative to the national economy.

To address the contradictory developments in the empirical literature, Huang (2008) highlights the need for a valid counterfactual to control for factors that potentially confound state-to-state comparisons. He compares the economic performance of contiguous counties on either side of state borders. He calls this technique "geographic matching." He finds that only five of the 23 deregulation events in his sample lead to positive and statistically significant economic growth. Moreover, the five events that are associated with positive economic growth occur in the latter half of the study period. He concludes that deregulation does not, in general, cause economic growth. The myriad contrasting results in the literature leave the debate open for further investigation.

Because we are revisiting the setting from Jayaratne and Strahan (1996) with a different empirical technique, we begin by replicating their main result. Table 3 presents regression results for growth in personal income per capita due to bank branching deregulation. The basic model is a difference-in-differences model with deregulation serving as the differencing dimension with state and year fixed effects:

$$\frac{Y_{i.t}}{Y_{i,t-1}} = \alpha_t + \beta_i + \gamma \times D_{i,t} + \varepsilon_{i,t} \qquad (1)$$

where $D_{i,t}$ is an indicator variable that takes the value 1 if state $i$ deregulated by date $t$ and 0 otherwise. This analysis is the same analysis presented in the first panel of Tables II and IV in Jayaratne and Strahan (1996). We find that bank branching deregulation has a positive and significant effect on economic growth based on this specification. Deregulation increases personal income growth by 0.93% annually (our result; 0.94% in Jayaratne and Strahan, 1996) and is economically and statistically consistent with their results. This result is robust to the presence of lagged growth and to estimation by weighted least squares (WLS).[2]

[Insert Table 3 here]

**D. Counterfactuals**

To conclude that the results from Table 3 indicate that deregulation causes growth, one must rely on the assumption that the average non-deregulating state provides a good counterfactual unit for the average deregulating state. However, Freeman (2002) demonstrates that bank deregulations, in general, took place during times of state-level economic nadirs. Hence, deregulating states are systematically different from non-deregulating states, which raises the question of whether a traditional differences-in-differences approach to studying the real effects of bank branching deregulation is appropriate.

This issue applies to many studies exploring the real effects of deregulation on state-level outcomes using a differences-in-differences design. Huang (2008) proposes that researchers get a closer comparison unit by studying the *county*-level differences for counties that are on either side of a deregulating state's border, rather than the state-level differences. The tradeoff in his research design is between valid counterfactuals and external validity. While event counties and their cross-border control counties are probably economically similar, it is not clear that the results extend to the state level, especially given that few state border counties, especially those in Huang's (2008) sample, are hubs of economic activity.

The synthetic controls method of Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010) provides a solution to this inference problem by

[2] Jayaratne and Strahan also control for regional effects by assigning states to one of four geographic regions of the United States. In untabulated results we replicate their findings for the regional specifications and for the specifications using growth in gross state product as the dependent variable.

producing a better-constructed counterfactual comparison unit for each deregulating state. The purpose of synthetic matching is to build a valid counterfactual, as in Huang (2008), while maintaining external validity by capturing the entire state economy instead of just border counties.

There are several advantages to using a synthetic control as a counterfactual. First, when units of analysis are large aggregate entities, such as states or regions, a combination of comparison units (a “synthetic control") often does a better job reproducing the characteristics of the treated unit than any single comparison unit alone. Second, if the number of pre-intervention periods in the data is large, matching on pre-intervention outcomes produces a match along both observable and unobservable characteristics. Thus, the synthetic control mitigates concerns over unobservable characteristics, which typically plague comparative case studies. Third, because we are matching on pre-intervention data, the method does not require access to post-treatment outcomes to construct the synthetic control.

The intuition behind the method is that only units that are alike along both observed and unobserved determinants of the outcome variable should produce similar trajectories of the outcome variable over extended periods prior to treatment. Thus, a good synthetic control contains all information about the deregulating state up to the point of deregulation. The method directly addresses the potentially poor quality of control groups in the difference-in-differences approach, while maintaining external validity by capturing the entire state economy. Therefore, we are able to provide new insights into and evidence of the real effects of bank branching deregulation.

## III. Synthetic Controls Methodology

This section describes the construction of our synthetic control matches. Our discussion and analysis in this section focus on economic growth following bank branching deregulation. In the remainder of the paper, we use the synthetic control method to construct a control group for the deregulating states to identify the channels through which bank branching deregulation causes economic growth.

### A. Synthetic Controls: Theoretical Construct

In order to illustrate the synthetic controls methodology, we use the example of Abadie, Diamond, and Hainmueller (2010). Much of the following discussion draws heavily from their paper. Assume that our data involve $J$+1 states for $T$ periods. State 1 (the treatment unit) is exposed to an intervention at time $T_0$ ($1<T_0<T$) and the remaining $J$ states serve as potential controls (the "donor pool"). Consider the following variables:

$Y_{i,t}^{I}$ : The outcome for state *i* at time *t* if state *i* were exposed to the intervention.

$Y_{i,t}^{N}$ : The outcome for state *i* at time *t* if state *i* were not exposed to the intervention.

$D_{i,t}$ : An indicator variable that equals 1 if state *i* were exposed to the intervention on or before period *t*, and 0 otherwise.

Then, $Y_{i,t}^{N} = Y_{i,t}^{I}\ \forall \mathrm{i},\ \forall \mathrm{t} \in \{1,2,...,\mathrm{T}_0\}$.

Also define the effect of the intervention on state *i* at time *t*, $\alpha_{i,t} = Y_{i,t}^{I} - Y_{i,t}^{N}$.

Then $Y_{i,t} = Y_{i,t}^{N} + \alpha_{i,t} D_{i,t}$ and the aim of the analysis is to estimate $(\alpha_{i,T_0+1}, \alpha_{i,T_0+2}, ..., \alpha_{i,T})$. We assume that the outcome variable can be characterized by:

$$\mathrm{Y}_{\mathrm{i.t}}^{\mathrm{N}} = \delta_{\mathrm{t}} + \theta_{\mathrm{t}} \mathrm{Z}_{\mathrm{i}} + \lambda_{\mathrm{t}} \mu_{\mathrm{i}} + \varepsilon_{\mathrm{i,t}} \qquad (2)$$

where $\delta_t$ is an unobserved common factor, $\theta_t$ is a vector of parameters, $Z_i$ is a vector of observed covariates, $\lambda_t$ is a vector of unobserved common factors, $\mu_i$ is a vector of unobserved common factor loadings, and $\varepsilon_{i,t}$ is a transitory shock.

Suppose that a set of weights $W^* = (w^*_2,...,w^*_{J+1})$ exists that satisfies:

$\sum_{j=2}^{J+1} w^*_j Y_{j,1} = Y_{1,1},..., \sum_{j=2}^{J+1} w^*_j Y_{j,T_0} = Y_{1,T_0}$ and $\sum_{j=2}^{J+1} w^*_j Z_j = Z_1$, i.e. the set of weights $W^*$ defines a synthetic observation whose outcome variable and observable covariates match those of the treatment unit during the pre-treatment window.

Under some regularity conditions, the synthetic state defined by $W^*$ provides a perfect counterfactual for the treatment unit as the pre-treatment window gets large, i.e. $Y^N_{i,t} - \sum_{j=2}^{J+1} w^*_j Y_{j,t}$ approaches 0 almost surely as $T_0$ approaches infinity.[3] As a practical matter, the value $\sum_{j=2}^{J+1} w^*_j Y_{j,t}$ provides a good approximation for $Y^N_{i,t}$ and we can obtain an approximation of $\alpha_{i,t}$:

$$\hat{\alpha}_{1,t} = Y_{i,t} - \sum_{j=2}^{J+1} w^*_j Y_{j,t} \tag{3}$$

In practice, an exact $W^*$ does not exist and we use a $\hat{W}$ that minimizes the distance between the outcome variable and covariates of the synthetic unit and of the treatment unit during the pre-treatment window. Formally, define:

$$X_i = (Z'_i, Y_{i,1}, Y_{i,2},..., Y_{i,T_0}) \text{ and } X_{-1} = \left(X_2 \quad ... \quad X_J\right)'$$

[3] The regularity conditions and the proof of the proposition are outlined in Abadie, Diamond, and Hainmueller (2010).

Then $\hat{W} \in \underset{W}{\operatorname{argmin}} \|X_1 - X_{-1}W\| = \sqrt{(X_1 - X_{-1}W)'V(X_1 - X_{-1}W)}$ for some symmetric, positive semi-definite matrix *V*.

It is important to note that the value $\sum_{t=1}^{T_0}(Y_{1,t} - \sum_{j=2}^{J+1}\hat{W}_j Y_{j,t})^2$ provides a measure of the goodness of fit of the synthetic control and is referred to as the Mean Square Prediction Error (MSPE). Similarly, the Root Mean Square Prediction Error (RMSPE), defined as the square root of the MSPE, can be used to assess the goodness of fit.

The creation of a synthetic match as a linear combination of other states is comparable to standard regression analysis (OLS). To demonstrate this relationship, the next example shows how an OLS forecast for one state's outcome is a linear combination of other states' outcomes. Consider the case where we want to predict an outcome variable $Y_T$ for (treated) state T using a collection of covariates $X_T$. We have observations of the outcome variable $Y_C$ and the covariates $X_C$ for control states C. Using an OLS regression we estimate coefficient $\hat{\beta} = (X_C^{'} X_C)^{-1} X_C^{'} Y_C$, and use these estimates to form our forecast $\hat{Y}_T = \hat{\beta}^{'} X_T$. If we re-arrange the terms we can write the forecast as: $\hat{Y}_T = Y_C^{'} X_C (X_C^{'} X_C)^{-1} X_T$. Noting that the quantity $W = X_C (X_C^{'} X_C)^{-1} X_T$ could be viewed as a weight, we can see that the forecast $\hat{Y}_T = Y_C^{'} W$ is indeed a linear combination of the control states' outcomes.

**B. Matching**

We use the synthetic controls method to determine if bank branching deregulation significantly affects average economic growth. Our analysis begins by matching each state's level of per capita income over time. We create a synthetic match for each event state from the beginning of our sample to its deregulation year based on the following covariates: per capita income, personal income growth, log of population density, scaled patenting activity, change in bank efficiency, loan rate, deposit rate, and the spread between the loan and deposit rates. These covariates permit us to replicate the per capita income trajectory, but also control for the state-level banking environment, which we hypothesize will be important in a state's response to branching deregulation. In addition, by including income growth and population density, we alleviate concerns that differences in state population or the economic growth trajectory of a state might lead to differences in a state's response to deregulation.

During our sample, 35 states deregulate at different points. To maximize the number of states in the donor pool, we assume that the economic effects of deregulation are negligible after a window of time. This assumption is corroborated by Jayaratne and Strahan (1996); they find that deregulation effects diminish after a 10-year period. For every event state, we first use a five-year exclusion window where only states that did not deregulate during that window are allowed in the donor pool. We create this exclusion window to avoid potential confounding effects and correlations among states deregulating within a short time of each other.

We then extend this exclusion window in the robustness tests reported in Section V, where we use a 10-year exclusion window, a "no re-entry" exclusion window to restrict reentry into the donor pool, a "no-deregulators" exclusion window that restricts the donor pool to include only the states that were deregulated prior to 1975, and a "no border state" exclusion window that restricts the donor pool to exclude states that border the deregulating states. Our results are robust to the choice of exclusion window. Using these criteria we are able to construct counterfactuals for the 35 deregulation events, denoted in Table 1 with asterisks, that occurred from 1975-1991.

It is helpful to think of the synthetic controls optimization routine as an analogue to the tracking portfolio approach in asset pricing. The tracking portfolio approach allows us to find a weighted combination of non-deregulating states that optimally mimics the characteristics of each deregulating state. Researchers can then draw statistical inference about the economic indicator based on the projected performance of the tracking portfolio. Therefore, synthetic states can be thought of as portfolios of states. Each synthetic state is a combination or portfolio of states from the donor pool with the closest possible average pre-deregulation characteristics, where the characteristics are defined as per capita income, personal income growth, log of population density, scaled patenting activity, change in bank efficiency, loan rate, deposit rate, and the spread between the loan and deposit rates.

The purpose of this analysis is to measure the treatment effect of deregulation on economic growth (per capita income). Using a limited donor pool and parsimonious matching characteristics we construct high quality matches that reflect the per capita income trajectory in the deregulating state. We evaluate the quality of our matches using the Root Mean Square Prediction Error (RMSPE), which measures the distance between the event state and its synthetic counterpart prior to deregulation. RMSPE is analogous to the "tracking error" of a portfolio where a low RMSPE denotes a good match. Overall, our matches have low RMSPE which indicates that the states in our donor pool can closely mimic the income trajectory of each of our treatment states.

In Table 4 we show the composition of nine such portfolios for three of our best, median, and worst matches, ranked by RMSPE. The average synthetic state is composed of about 4 states, with an average "portfolio weight" of 30%. For example, synthetic Connecticut is a portfolio of 9.1% California, 11.9% Hawaii, 22.7% D.C., and 56.3% Nevada. On the other hand, synthetic Virginia is composed of a much more diverse portfolio of states. Mississippi matches to only one state (100% Arkansas) and is one of our worst matches, yet its RMSPE is only 1.36%. Mississippi has the highest poverty and lowest income, and is difficult to match as the convex combination of multiple states.

Nevertheless most state matches appear to provide a good fit, both visually and based on the RMSPE.[4]

[Insert Table 4]

In Figure 1 we show the real (solid line) and synthetic (dashed line) per capita income trajectories from 1970 to 1996. Figure 1 reveals that the synthetic control units are good matches for our event states. In the pre-deregulation years (i.e. matching window) we are able to get near-exact tracking for our best and median matches. Even for our worst matches (ranked by RMSPE), the economic growth trajectory of synthetic portfolios follows the true state's trajectory quite well.

[Insert Figure 1]

Table 5 presents the summary statistics for the real states and their synthetic controls during the matching period. The table includes the covariates that we use to construct the synthetic match: per capita income, personal income growth, log of population density, scaled patenting activity, change in bank efficiency, loan rate, deposit rate, and the spread between the loan and deposit rates. In addition, we include the unemployment rate, growth in bank loans, the number of branches, average state housing prices, and bank profitability to verify that the matches are able to control for variables not directly included in the match.

[Insert Table 5]

Column 5 reports the normalized differences of the means in characteristics between the real and the synthetic control states. The normalized differences indicate that there is no significant difference between our real states and their synthetic matches,

[4] The synthetic control method bounds the matching weights to be between 0 and 1 to prevent extrapolation. However, this restriction can be relaxed to allow negative weights—the analog to short selling a matching state. When we explore the effect of allowing negative weights, the resulting matches have a lower RMSPE and the average synthetic state is composed of over a dozen states compared to four states in the main analysis. Relaxing the no extrapolation restriction does not change our conclusions.

with the exception of the population density. The population density measure is influenced by the inclusion of the exceptionally dense District of Columbia in the matching process.

The treatment effect (deregulation effect) is the difference between the per capita income of each deregulated state compared to the per capita income of its synthetic control in the post-deregulation period. The synthetic controls method permits a state-by-state assessment of the deregulation effect because the method constructs an individual counterfactual for each treated unit. A visual inspection of Figure 1 reveals that there is heterogeneity among state-level responses to deregulation. For example, Virginia exhibits a positive deregulation effect while Wyoming experiences a negative deregulation effect.

**C. Average Treatment Effect**

In order to assess the average effect of deregulation on economic growth we calculate an average treatment effect across all deregulating states. The average treatment effect is the difference in per capita income of all treated units compared to per capita income of all control units following the treatment. For each year in event time, the treatment effect is the average of all states' treatment effects.

Abadie, Diamond, and Hainmueller (2010) and Abadie and Gardeazabal (2003) develop the synthetic controls method for comparative case-studies in which one unit is treated and rely mostly on "placebo studies" to assess the impact of the treatment. Acemoglu, et al. (2010) is the first paper to apply synthetic controls to a sample with multiple treated units. In our analysis, with multiple treated units, we measure the average treatment effect of deregulation on state-level economic growth. To evaluate the average treatment effect, we extend the "placebo study" approach proposed in Abadie and Gardeazabal (2003) to multiple treated units.

We validate the statistical significance of the average treatment effect by applying synthetic controls to a set of "placebo" deregulation events. The idea is to compare the economic growth of the same states during periods in which they did not deregulate. This procedure allows us to assess whether the gap observed for the deregulating sample can simply be attributed to sampling variation.[5] In other words, we assess the likelihood that any combination of states and event years could produce the same upward sloping figure, if we randomly assigned deregulation.

**D. Placebo Study**

To construct placebo average treatment effects, we perform the following process for each of our deregulating states. First, we assign the state a false deregulation year, or a placebo deregulation. The placebo deregulation cannot be assigned to the state's true deregulation year or to a year within five years of the state's true deregulation. For this placebo state/deregulation year pair we repeat the method outlined in Section III.B to generate a synthetic match. We then calculate the placebo deregulation effect as the difference between the per capita income of the placebo state and its synthetic match. The event time year 0 coincides with the state's placebo deregulation year. We apply the synthetic control method to each state with each eligible placebo deregulation year to obtain a distribution of placebo treatment effects.

To assess statistical significance, we construct confidence intervals from the sample of simulated average treatment effects. We draw a random sample of 35 placebo deregulation events, which equals the number of deregulating states in our true sample. Using our sample of 35 placebo deregulations, we calculate an average treatment effect for each event year from -10 to 10. We repeat this procedure 1000 times to obtain a distribution of average treatment effects for each event year. We use the distribution of

[5] We thank Alberto Abadie for suggesting a placebo study method to assess the statistical significance of our average treatment effect across multiple treated units.

simulated placebo treatment effects to calculate confidence intervals at the 95% and 99% levels for each year.[6]

## IV. Results

### A. Deregulation Does Not Cause Growth, on Average.

Panel A of Figure 2 reveals that the average treatment effect of deregulation is statistically significant only after nine years following deregulation. The solid line depicts the average treatment effect and the dashed lines denote confidence intervals at the 95% and 99% levels. Note that prior to deregulation, there is no statistically significant difference between the per capita income growth in event states compared to their synthetic matches. Over time, the per capita income trajectory of event states accumulates and becomes increasingly positive, such that in the ninth and tenth years following deregulation, event states have higher per capita income than control states. This effect after ten years is statistically significant at the 1% level.

[Insert Figure 2]

At first glance, the results of Figure 2, Panel A suggest that a positive deregulation effect exists in a longer time series. However, over ten years, factors beyond the effects of deregulation may begin to influence the evolution of a state's economy. Overall we cannot conclude that the positive deregulation effect in Figure 2, Panel A is driven systematically by financial deregulation.

### B. Deregulation Causes Growth for Early States.

Prior research suggests that the treatment effect of deregulation may be significantly different for early and late deregulators. DeLong and DeYoung (2007) document a learning-by-observing phenomenon among banks undertaking M&A

[6] In our robustness section, we form synthetic matches using more restrictive donor pools and our confidence intervals are qualitatively consistent across changes in the donor pool.

ventures. They find that banks that engage in M&A activity later in time have the opportunity to learn from prior bank M&A activity. Huang (2008) suggests that the staggered nature of bank branching deregulation allowed states that deregulated later in the sample to learn from the early deregulators and hence benefit more from deregulation. Specifically, he hypothesizes that banks in later deregulating states exploited opportunities from deregulation more successfully as time went by through a learning-by-observing process, while the earlier deregulators may not have realized the extent of the new opportunities caused by deregulation.

In contrast, Kroszner and Strahan (1999) show that certain states deregulated early because the public and private economic costs of the branching restrictions created stronger incentives to deregulate. They use state-level characteristics of the banking sector to predict the timing of bank branching deregulation and find that states with smaller, financially weaker banks tended to deregulate earlier. Therefore the timing of deregulation may be an important factor in determining the economic gains from deregulation.

Our sample spans the period between 1970 and 1996. We divide the sample into two groups based on whether the state deregulated prior to 1985 (early) or in or after 1985 (late) consistent with Huang (2008). We find a dramatic difference in the average deregulation effect for early compared to late deregulators. Consistent with Kroszner and Strahan (1999), we find that early deregulating states in our sample experience larger growth effects from bank branching deregulation than states that deregulate later in the sample. In fact, branching deregulation has a positive effect on economic growth only for the early deregulating states. The late deregulating states experience no growth effect from deregulation. Figure 2, Panels B and C summarize the results. States that deregulated early (pre-1985) experienced positive and statistically significant economic growth of $780 in per capita income five years following deregulation (Panel B). But states that deregulated late (1985 and later) experienced no significant deregulation effect (Panel C).

These results reveal that the timing of deregulation is associated with significant cross-sectional heterogeneity in the economic responses. In the following sections we rely on the sample heterogeneity to identify the channel through which bank branching deregulation caused economic growth. Because only early deregulating states experienced significant economic growth in our sample, the causal channel should be most prominent in the early subsample. We consider three channels in turn: bank efficiency, borrower quality, and dammed credit.

**C. Does Bank Branching Deregulation Lead to Increased Bank Branching?**

If bank branching regulation is a binding constraint, then deregulation should lead to increased bank branching activity. We use the time-series evolution of the ratio of branches to institutions as a proxy for M&A branching activity. As new branches open, or in the case of M&A branching, as the number of institutions declines due to consolidation, the ratio of bank branches to institutions increases.

Figure 3, Panel A shows that, overall, M&A banking activity does not change after deregulation. For early deregulators, in Panel B, the average institution gained an additional 2.36 branches following deregulation. In contrast, late deregulators, in Panel C, show no change in M&A branching activity. These results confirm the hypothesis that early deregulating states had stronger economic incentives to deregulate. Figures 2 and 3 exhibit strong similarities in the trajectories of economic growth and M&A branch network size. These similarities are consistent with a link between changes in branch network size and economic growth following deregulation.

[Insert Figure 3]

**D. Improved Bank Efficiency is Not Sufficient for Economic Growth**

The bank efficiency channel suggests that bank branching deregulation allowed for new entry into local markets, resulting in more competition among banks and consequently more efficient lending practices. Jayaratne and Strahan (1996, 1998) find evidence that banks became more efficient after deregulation, and that deregulation led to economic growth. Following Jayaratne and Strahan (1998), we measure bank efficiency as the ratio of non-interest expenses to total assets. This measure captures the amount of money spent on non-banking activities. As bank lending efficiency increases, the amount of resources wasted on non-interest expenses should decrease. Hence the ratio is a measure of bank *inefficiency*.

Figure 4, Panel A shows that on average non-interest expenses decline (efficiency improves) after deregulation for the full sample, consistent with Jayaratne and Strahan (1996, 1998). However, bank efficiency improves for both early and late deregulator subsamples (Panels B and C). Banks in late deregulating states reduce non-interest expenses by 20 basis points relative to their synthetic control counterparts whereas banks in early deregulating states reduce non-interest expenses by only 4 basis points. Given that economic growth occurs only in the early deregulating states (Panel B), and that the change in efficiency for the early states is economically small, we interpret these results as evidence that more efficient lending is not sufficient for economic growth.

[Insert Figure 4]

**E. Borrower Quality Does Not Facilitate Economic Growth**

The borrower quality channel suggests that bank branching deregulation helped banks improve their risk-sharing abilities, which may have allowed them to make loans to more innovative businesses. Recent literature suggests that bank financing permits businesses to innovate.[7] Other research shows that innovation leads to economic growth.[8] For instance, entrepreneurs and small businesses play an important role in innovation and economic growth, but face high costs of bank capital (see Berger and Udell, 1995; Petersen and Rajan, 1994; Berger, Miller, Petersen, Rajan, and Stein, 2005). Weston and Strahan (1998) and Black and Strahan (2001) find that better risk sharing leads to more bank financing for entrepreneurs after bank branching deregulation.

We follow the recent literature and examine the impact of innovation, as measured by patent growth, on economic growth. The state-level patent filings data comes from the National Bureau of Economic Research (NBER) which includes all the patents awarded by the US Patent and Trademark Office (USPTO). For comparability across states, we measure patent growth as the number of patents scaled by the per-state 1970 level.

Figure 5, Panel A shows that patenting activity actually declines following bank branching deregulation. Figure 5, Panels B and C show that the drop in patenting activity occurs for both early and late deregulating states. The decline in patents following deregulation in the full sample as well as the two subsamples is inconsistent with the hypothesis that an increase in innovation, measured by patent filings, spurred economic growth.

[Insert Figure 5]

**F. The Dammed Credit Channel is the Causal Link.**

The basic consequence of a dammed credit market is that qualified borrowers may not be able to obtain loans either because the bank refuses outright to make them a

[7] See for example Amore, Schneider, and Zaldokas (2013); Chava, Oettl, Subramaniam, and Subramaniam (2013); Cornaggia, Mao, Tian, and Wolfe (2013).

[8] Greenwood and Jovanovic (1990), and Kalemli-Ozcan, Sorenson, and Yosha (2003) argue that risk sharing leads to innovation and growth. Solow (1957), King and Levine (1993), and Schumpeter (1912) link innovation directly to economic growth.

loan, or because the cost of borrowing is too high. This leads to an inefficient allocation of resources, and at the macroeconomic level, results in under-investment and low economic growth. We look for evidence of the dammed credit channel by examining the supply of loans, as well as the price of loans.

Although we are not able to observe the state-level demand for loans, we can observe the actual amount of loans made. Our measure of lending is the dollar amount of total loans and leases per capita. The dammed credit channel implies that regulation prevents an efficient equilibrium quantity of lending, i.e. lending activity is too low. Deregulation should correct this imbalance, resulting in an increase in the amount of loans provided by banks.

Figure 6 shows that lending increases in the early deregulating states (Panel B) by a statistically significant $20 per capita following deregulation, but does not increase in the late deregulating states (Panel C). Prior to deregulation, early deregulators had an average per capita loan amount of $60. An increase of $20 per capita in lending following deregulation means that the average per capita loans increased by about one-third for the early deregulators. These results support the hypothesis that regulation resulted in lending below the optimal level in some states. After the relaxation of branching regulation banks were able to lend more.

[Insert Figure 6]

Next we examine the average price of loans. Intuitively, if the supply of loans is restricted by a credit market imperfection such as bank branching regulation, then the quantity of loans will be “too low” and the price for loans will be “too high” compared to a setting in which such imperfections are absent. We construct state-level measures for the average price of loans (the loan interest rate) relative to the cost of lending (the deposit interest rate). We proxy for the average loan interest rate in each state by dividing the total income on loans and leases by the total amount of loans and leases as in

Jayaratne and Strahan (1998). We construct a proxy for the average deposit interest rate in each state as the total interest on deposits divided by the total liabilities at the state level. Figure 7, Panel B shows that prior to deregulation the loan-to-deposit interest spread is much higher for early deregulating states. Within two years of deregulation the loan-to-deposit interest spread returns to normal levels as the supply of loans increases.[9] For late deregulating states the loan-to-deposit interest spread is indistinguishable from that of the control states.

[Insert Figure 7]

Taken together, the evidence on loan activity and price provides strong support for our hypothesis that early deregulating states were able to resolve the dammed credit problem, which led to economic growth.

## V. Robustness

### A. Changes in the Donor Pool

We explore whether our economic growth results are robust to changes in the construction of the donor pool. In our main results section, we impose a five-year exclusion period to construct the donor pool for our synthetic matching. In Figure 8, we report the average treatment effect when we use synthetic matches constructed from different donor pools. We find that the positive average treatment effect is robust to a variety of control groups constructed from donor pools that exclude states according to the criteria presented in this section. Using a narrower donor pool reduces the potential quality of the matches, but at a benefit of more aggressively excluding donor pool observations that may be contaminated with persistent deregulation effects. For instance, the average RMSPE when we impose a 10-year exclusion window for the donor pool is

[9] We also use the average loan interest rate by itself, and the loan interest rate minus the Federal Funds rate and find the same results. Our results are not driven by the average deposit interest rate.

0.62 compared to an average RMSPE of 0.41 for the matches based on the five-year exclusion window. In our main results, we use a less restrictive donor pool (five-year exclusion), which biases against finding a deregulation effect, if the effect persists beyond five years. When we restrict the donor pool, we find that the positive average treatment effect becomes more pronounced.

[Insert Figure 8]

First, we impose a 10-year exclusion window before and after the deregulation event in order to construct our synthetic matches. This exclusion requires that the control group contain states that have not experienced a deregulation event ten years before or ten years after the deregulation of the treatment state. To construct the placebo sample, we assign placebo deregulation years that are outside of the ten year exclusion window. This procedure is analogous to our five year exclusion window, but is more restrictive in order to mitigate residual deregulation effects that may persist in a deregulated state. Figure 8, Panel A depicts the average treatment effect of the true deregulations (solid black line) and the 95% and 99% confidence intervals, constructed from the placebo sample. The figure shows that after five years the per capita income of deregulating states still has not exceeded the per capita income of control states. By year ten, the positive treatment effect is statistically significant at the 5% level.

Next, we allow the control group to contain only states that have never deregulated. The motivation behind this "no re-entry" restriction is that after a state has deregulated, the impact of deregulation on economic growth might be permanent. This restriction automatically excludes the 13 states that deregulated prior to 1975 because those states may have a permanent deregulation effect embedded in their per capita income. Over the sample period, this restriction reduces the match quality for late deregulating states. Figure 8, Panel B depicts the average treatment effect using the "no re-entry" restriction. Starting in year four the positive average treatment effect of deregulation becomes statistically significant at the 5% level.

In another test, we consider only the 13 states that deregulated prior to the beginning of our sample as potential control units. This control group corresponds to the control group that Jayaratne and Strahan (1996) use in their Figure I. Figure 8, Panel C shows the average treatment effect using this restriction. The deregulation effect accumulates over the 10-year period. In years eight through ten, the deregulation effect is positive and statistically significant.

Lastly, we control for potential geographical spillover effects by eliminating states that border the deregulating state from the potential control group for each deregulating state. For each deregulating state, we exclude bordering states from the potential control group and we impose a five-year exclusion window. Figure 8, Panel D depicts the results from this robustness test. The figure shows that after nine years the per capita income of deregulating states begins to exceed the per capita income of control states. This effect is statistically significant at the 5% level. In year ten, the positive treatment effect is statistically significant at the 1% level.

**B. Unit Banking and Limited Branching Laws**

Two forms of bank branching regulation existed during the sample period: unit banking laws, which were more restrictive and limited branching laws, which were less restrictive. One hypothesis is that more restrictive bank branching laws prior to deregulation explain economic growth following deregulation.[10] Perhaps the severity of branching laws explains our results.

We explore the deregulation effect of unit banking states compared to that of limited branching states and do not find evidence that unit banking states experienced higher economic growth following deregulation. Flannery (1984) questions the degree to

[10] We thank Philip Strahan for suggesting this line of inquiry.

which the legal environment in a state materially restricted unit banking states, noting that of the thirteen unit-banking states in our sample, only Wyoming enforced strict no-branching laws. In fact, the thirteen unit banking states all appear in our late deregulating sample, where we find no treatment effect of deregulation.

### C. De novo branching deregulation

De novo branching permitted banks to open new branches anywhere within state borders. De novo branching deregulation may have effects on economic growth that confound the economic growth that we document from M&A deregulation. We repeat our main analysis using the de novo branching dates as the event dates for branching deregulation to test this hypothesis.

The M&A and de novo branching dates reported in Table 1 of Jayaratne and Strahan (1996) show that in general, states removed restrictions on M&A branching first and subsequently permitted de novo branching. There are no instances in which de novo deregulation occurs prior to M&A deregulation. Furthermore, some states relaxed M&A branching restrictions but did not permit de novo branching during the sample period.

In unreported tests, we verify that the de novo branching does not lead to economic growth in the full, early, or late deregulating samples. In the early sample, this result suggests that the economic growth that we document in our main analysis is due to M&A branching, rather than de novo branching deregulation. Neither M&A branching nor de novo branching causes economic growth among late deregulating states. These results support the conclusion that the M&A branching deregulation is the economically relevant event to use in this setting.

## VI. Conclusion and Discussion

This paper explores three channels through which financial development may cause economic growth: a bank efficiency channel, a borrower quality channel, and a dammed credit channel. Using the synthetic control method, we are able to examine the significant heterogeneity in the deregulation effect in order to identify which channel is responsible for economic growth.

We do not find evidence that economic growth was caused by more efficient lending from banks. Banks became more efficient in the full sample, but only early deregulators experienced economic growth, which suggests better lending was not sufficient for economic growth. We also examine whether borrowers became more innovative by examining the number of patents filed after deregulation. We find that on average the number of patents filed decreased, which suggests innovation did not lead to economic growth.

Our results support the hypothesis that dammed credit is the link between deregulation and economic growth. Bank branching restrictions stifled lending activity in the early deregulating states, while these restrictions did not bind in the later sample. We conclude that financial development leads to economic growth, but only where financial development resolves the dammed credit problem.

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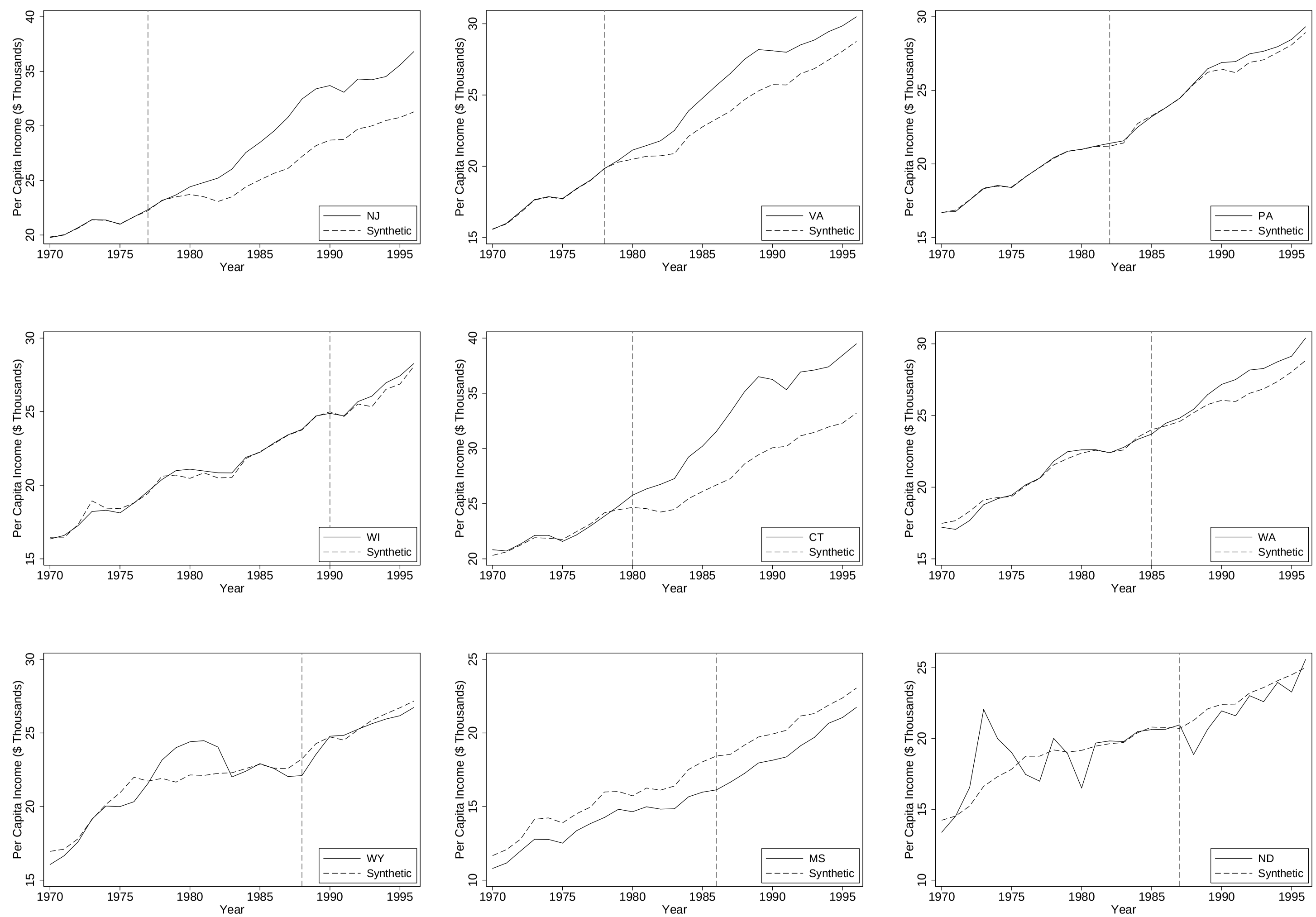
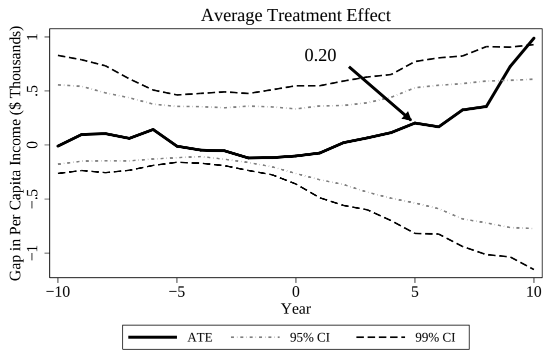
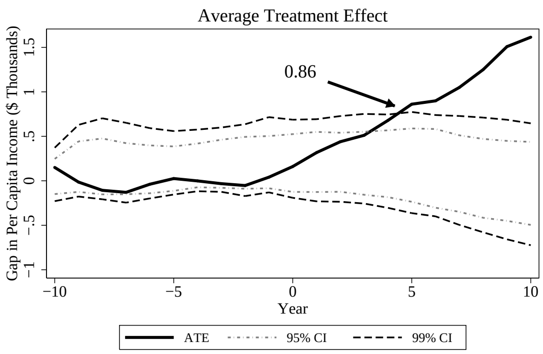


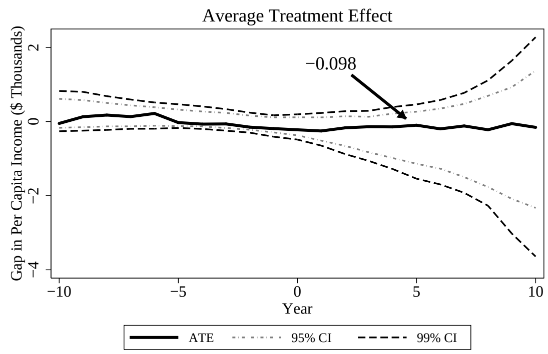

Figure 1: Per Capita Income Trajectories with Synthetic Controls
These figures present the per capita income trajectories for deregulating states and their 'synthetic state' matches. New Jersey (NJ), Virginia (VA), and Pennsylvania (PA) constitute the three best matches (top panel). Wisconsin (WI), Connecticut (CT), and Washington (WA) constitute the three median matches (middle panel). Wyoming (WY), Mississippi (MS), and North Dakota (ND) constitute the three worst matches (bottom panel). Control states are constructed by the data-driven synthetic controls method in which deregulating (event) states are matched to a portfolio of non-deregulating states that best replicate a selection of covariates during the pre-event period. The outcome variable is the level of per capita income in each state. The solid line represents the per capita income in each event state and the dashed line represents the per capita income in the synthetic control state. The vertical lines denote the year in which the state deregulated.




(a) Full Sample

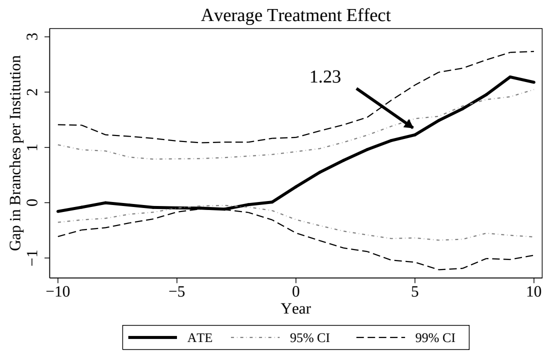



(b) Early Sample

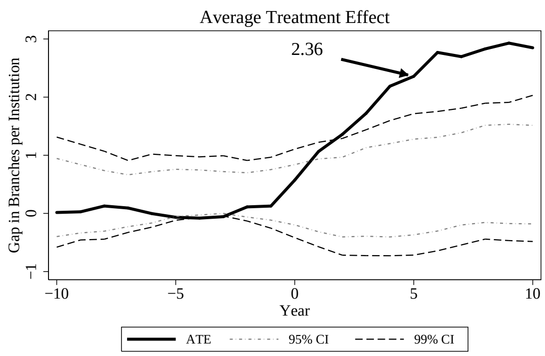



(c) Late Sample

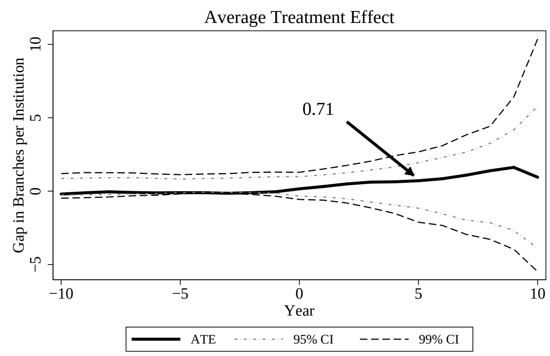
Figure 2: Average Treatment Effect (Per Capita Income)
Figure 2 depicts the average treatment effect of deregulation on a state's per capita individual income. To calculate the average treatment effect, we match each deregulating state to a synthetic state based on per capita income and a selection of covariates prior to deregulation. In each year subsequent to deregulation the treatment effect is the difference between the per capita income of the deregulating state and the per capita income of the synthetic match for that state for that year. The average treatment effect is the average of the deregulating state treatment effects calculated in event time. The confidence bounds are calculated by randomly sampling a set of states and assigning an arbitrary deregulation year (placebo deregulations). We calculate an average treatment effect for this group of state/deregulation year combinations. The procedure is repeated 1,000 times and for each event year the 95% and 99% values of the treatment effect from the placebo samples are used as confidence intervals. Panel 2a shows the average treatment effect for all states in our sample. Panels 2b and 2c show the average treatment effect for two sub-samples: states that deregulate early (Panel 2b) and states that deregulate late (Panel 2c). Early deregulators consist of states that deregulated prior to 1985, while late deregulators consist of states that deregulated in or after 1985. For the early (late) subperiod placebo deregulations are constrained to be within the early (late) period as well.




(a) Full Sample

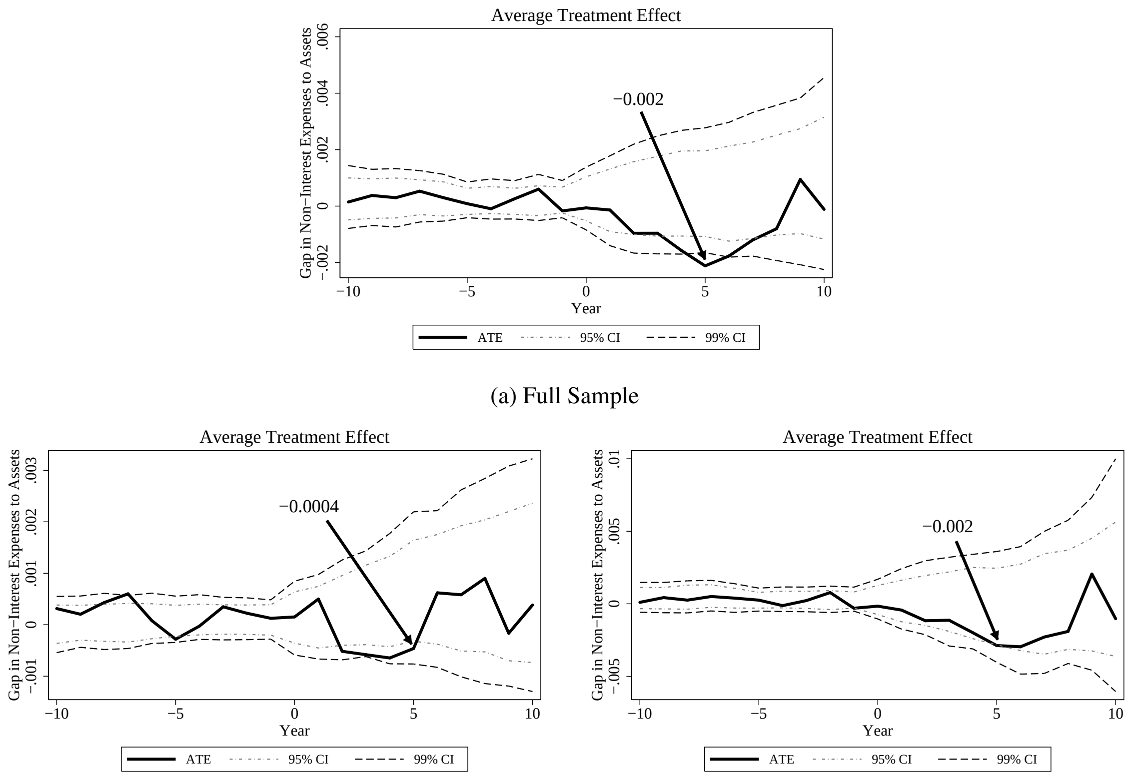



(b) Early Sample




(c) Late Sample

Figure 3: Branch Networks
Figure 3 depicts the average treatment effect of deregulation on a state's bank branching network size. To calculate the average treatment effect, we match each deregulating state to a synthetic state based on branches per institution and a selection of covariates prior to deregulation. In each year subsequent to deregulation the treatment effect is the difference between the branches per institution of the deregulating state and the branches per institution of the synthetic match for that state for that year. The average treatment effect is the average of the deregulating state treatment effects calculated in event time. The confidence bounds are calculated by randomly sampling a set of states and assigning an arbitrary deregulation year (placebo deregulations). We calculate an average treatment effect for this group of state/deregulation year combinations. The procedure is repeated 1,000 times and for each event year the 95% and 99% values of the treatment effect from the placebo samples are used as confidence intervals. Panel 3a shows the average treatment effect for all states in our sample. Panels 3b and 3c show the average treatment effect for two subsamples: states that deregulate early (Panel 3b) and states that deregulate late (Panel 3c). Early deregulators consist of states that deregulated prior to 1985, while late deregulators consist of states that deregulated in or after 1985. For the early (late) subperiod placebo deregulations are constrained to be within the early (late) period as well.




(a) Full Sample

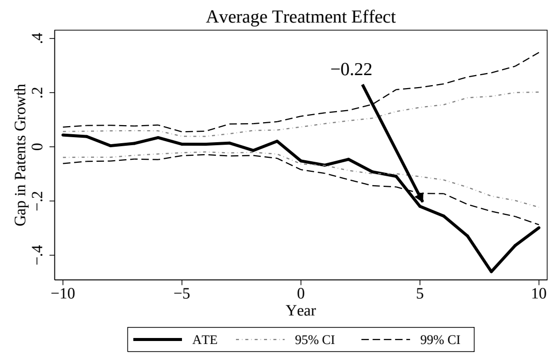


(b) Early Deregulators

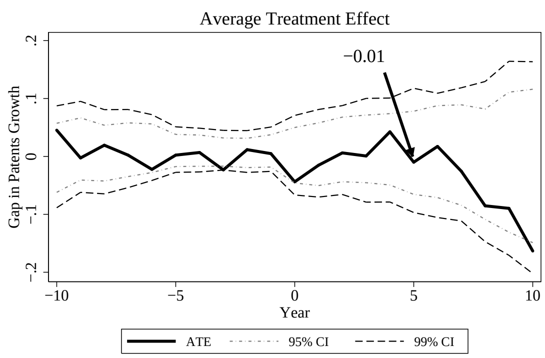


(c) Late Deregulators

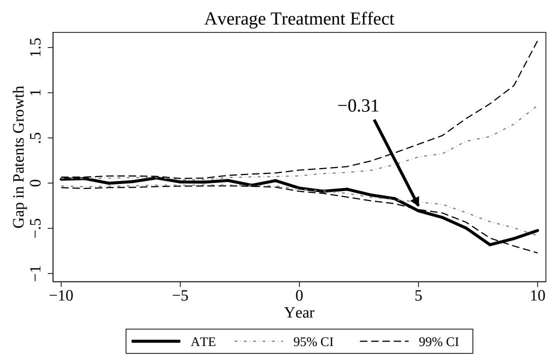
Figure 4: Bank Inefficiency
Figure 4 depicts the average effect of deregulation on a measure of the inefficiency of banks in a state. The measure of inefficiency is the aggregate non-interest expenses relative to assets of banks in a given state based on the income statements provided by the FDIC. We match each deregulating state to a synthetic state based on observations of non-interest expenses to assets and a selection of covariates prior to deregulation. In each year subsequent to deregulation the treatment effect is the difference between the ratio of non-interest expenses to assets of the banks in the deregulating state and the non-interest expenses to assets of the synthetic match's banks. The average treatment effect is the average of the deregulating state treatment effects calculated in event time. The donor pool in this specification is restricted to include only those states that did not deregulate within 5 years of the deregulation of the state to be matched. Panel 4a consists of all states in our sample. The last two panels represent subsamples based on the deregulation year. Panel 4b represents the subsample of states that deregulated prior to 1985, while Panel 4c uses states that deregulated after 1985. The confidence bounds are calculated by randomly sampling a set of states and assigning an arbitrary deregulation year (placebo deregulations). We calculate an average treatment effect for this group of state/deregulation year combinations. The procedure is repeated 1,000 times and for each event year the 95% and 99% values of the treatment effect from the placebo samples are used as confidence intervals.




(a) Full Sample

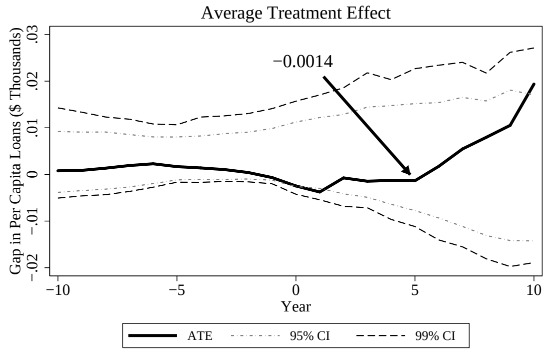



(b) Early Deregulators

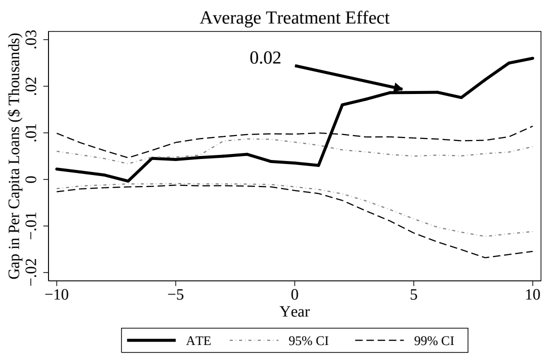



(c) Late Deregulators

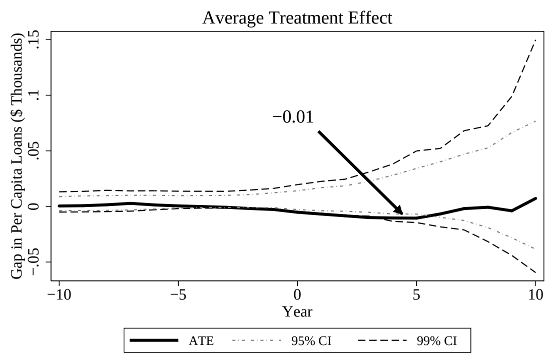
Figure 5: Patent Growth
Figure 5 depicts the average effect of deregulation on a state's patent growth. The reported patent numbers are relative to the each state's total number of patents in the first year in the sample 1970. We match each deregulating state to a synthetic state based on observations of patents and a selection of covariates prior to deregulation. In each year subsequent to deregulation the treatment effect is the difference between the patents of the deregulating state and the patents of the synthetic match for that state for that year. The average treatment effect is the average of the deregulating state treatment effects calculated in event time. The donor pool in this specification is restricted to include only those states that did not deregulate within 5 years of the deregulation of the state to be matched. Panel 5a reflects data for all states in our sample. The other two panels represent subsamples based on the deregulation year. Panel 5b represents the subsample of states that deregulated prior to 1985, while Panel 5c uses states that deregulated after 1985. The confidence bounds are calculated by randomly sampling a set of states and assigning an arbitrary deregulation year (placebo deregulations). We calculate an average treatment effect for this group of state/deregulation year combinations. The procedure is repeated 1,000 times and for each event year the 95% and 99% values of the treatment effect from the placebo samples are used as confidence intervals.




(a) Full Sample

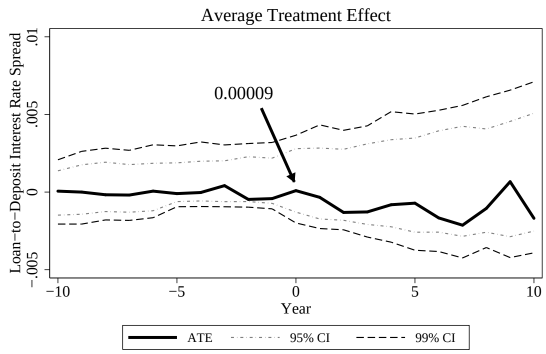



(b) Early Sample: Per Capita Bank Loans

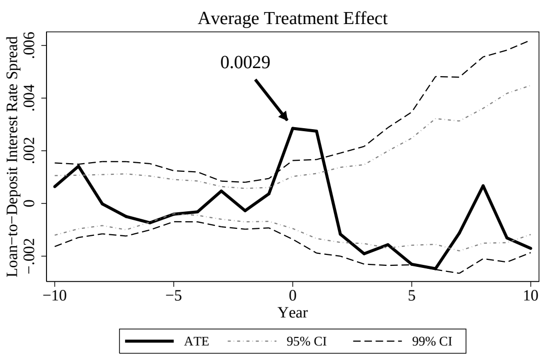



(c) Late Sample: Per Capita Bank Loans

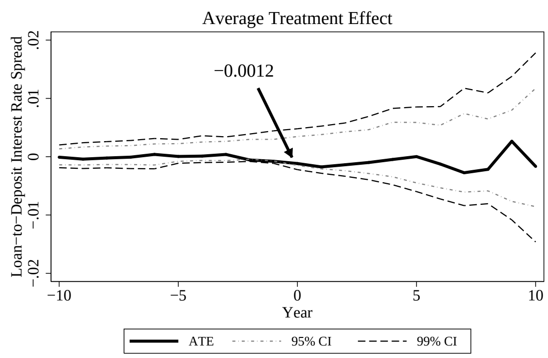
Figure 6: Loan Activity
Figure 6 depicts the average effect of deregulation on a state's level of bank loans. We match each deregulating state to a synthetic state based on observations of bank loans per capita and a selection of covariates prior to deregulation. In each year subsequent to deregulation the treatment effect is the difference between the bank loans of the deregulating state and the bank loans of the synthetic match for that state for that year. The average treatment effect is the average of the deregulating state treatment effects calculated in event time. The donor pool in this specification is restricted to include only those states that did not deregulate within 5 years of the deregulation of the state to be matched. Panel 6a reflects data for all states in our sample. The other two panels represent subsamples based on the deregulation year. Panel 6b represents the subsample of states that deregulated prior to 1985, while Panel 6c uses states that deregulated after 1985. The confidence bounds are calculated by randomly sampling a set of states and assigning an arbitrary deregulation year (placebo deregulations). We calculate an average treatment effect for this group of state/deregulation year combinations. The procedure is repeated 1,000 times and for each event year the 95% and 99% values of the treatment effect from the placebo samples are used as confidence intervals.




(a) Full Sample

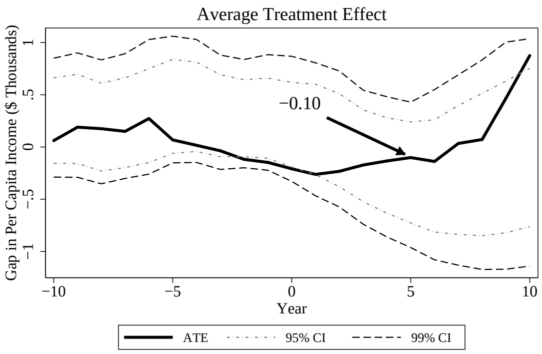



(b) Early Deregulators

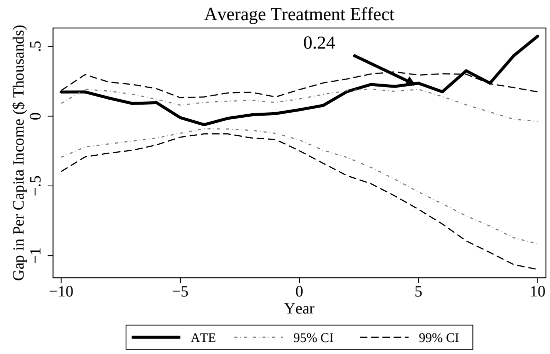



(c) Late Deregulators

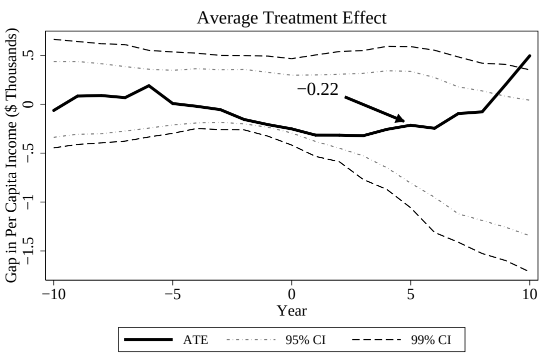
Figure 7: Loan Price
Figure 7 depicts the average effect of deregulation on a state's average loan prices. Average loan prices are computed as the ratio of interest on total loans and leases divided by total loans and leases (average loan interest rate) minus interest on deposits divided by total liabilities (average deposit interest rate). We match each deregulating state to a synthetic state based on observations of loan prices and a selection of covariates prior to deregulation. In each year subsequent to deregulation the treatment effect is the difference between the loan prices of the deregulating state and the loan prices of the synthetic match for that state for that year. The average treatment effect is the average of the deregulating state treatment effects calculated in event time. The donor pool in this specification is restricted to include only those states that did not deregulate within 5 years of the deregulation of the state to be matched. The first two panels represent subsample based on the deregulation year. Panel 7a represents a sample that consists of all states in our sample. Panels 7b and 7c use subsamples of states that deregulated before and after 1985 respectively. The confidence bounds are calculated by randomly sampling a set of states and assigning an arbitrary deregulation year (placebo deregulations). We calculate an average treatment effect for this group of state/deregulation year combinations. The procedure is repeated 1,000 times and for each event year the 95% and 99% values of the treatment effect from the placebo samples are used as confidence intervals.



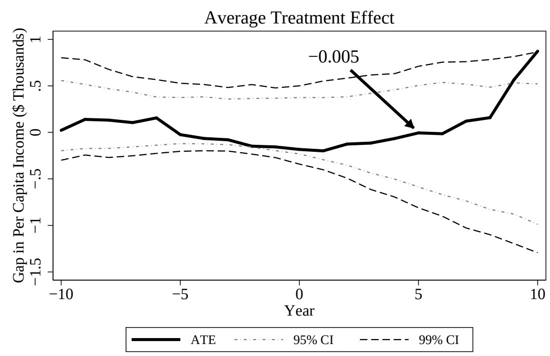

(a) 10 Year Exclusion




(b) No Re-entry




(c) Original 13 States




(d) No Border States

Figure 8: Average Treatment Effects Under Alternate Specifications
Figure 8 depicts the average effect of deregulation on a state's per capita individual income under different specifications for the synthetic match of a deregulation state. We match each deregulating state to a synthetic state based on observations of per capita income and a selection of covariates prior to deregulation. In each year subsequent to deregulation the treatment effect is the difference between the per capita income of the deregulating state and the per capita income of the synthetic match for that state for that year. The average treatment effect is the average of the deregulating state treatment effects calculated in event time. The different panels represent different donor pools for the construction of the synthetic match. Panel 8a represents the average treatment effect when the donor pool is restricted to include only those states that did not deregulate within 10 years of the deregulation of the state to be matched. In Panel 8b, the donor pool consists of only those states that have not deregulated prior to the state in question and do not deregulate less than 5 years thereafter. The donor pool used for Panel 8c includes only states that deregulatd prior to our sample.The donor pool used for Panel 8d consists of the base case donor pool with a 5 year exclusion window, but in addition all bordering states are also excluded from the donor pool to avoid geographical contamination. The confidence bounds are calculated by randomly sampling a set of states and assigning an arbitrary deregulation year (placebo deregulations). We calculate an average treatment effect for this group of state/deregulation year combinations. The procedure is repeated 1,000 times and for each event year the 95% and 99% values of the treatment effect from the placebo samples are used as confidence intervals.

Table 1: Bank Branching Deregulation
The following table reports each state in our sample with its corresponding deregulation year. We use the year in which a state completed deregulation of M&A branching as the year of deregulation. In our analysis, we analyze the effects of deregulations that occur after 1972, resulting in 35 deregulations. The remaining states are included in the sample as potential control states in our synthetic controls procedure. * denotes an event state in our sample.

| State | Deregulation Year | State | Deregulation Year |
|---|---|---|---|
| Alabama* | 1981 | Montana* | 1990 |
| Alaska | 1971 | Nebraska* | 1985 |
| Arizona | 1971 | Nevada | 1971 |
| Arkansas | 1994 | New Hampshire* | 1987 |
| California | 1971 | New Jersey* | 1977 |
| Colorado* | 1991 | New Mexico* | 1991 |
| Connecticut* | 1980 | New York* | 1976 |
| District of Columbia | 1971 | North Carolina | 1971 |
| Florida* | 1988 | North Dakota* | 1987 |
| Georgia* | 1983 | Ohio* | 1979 |
| Hawaii* | 1986 | Oklahoma* | 1988 |
| Idaho | 1971 | Oregon* | 1985 |
| Illinois* | 1988 | Pennsylvania* | 1982 |
| Indiana* | 1989 | Rhode Island | 1971 |
| Iowa | 2001 | South Carolina | 1971 |
| Kansas* | 1987 | Tennessee* | 1985 |
| Kentucky* | 1990 | Texas* | 1988 |
| Louisiana* | 1988 | Utah* | 1981 |
| Maine* | 1975 | Vermont | 1971 |
| Maryland | 1971 | Virginia* | 1978 |
| Massachusetts* | 1984 | Washington* | 1985 |
| Michigan* | 1987 | West Virginia* | 1987 |
| Minnesota | 1993 | Wisconsin* | 1990 |
| Mississippi* | 1986 | Wyoming* | 1988 |
| Missouri* | 1990 | | |

Table 2: Summary Statistics

Table 2 presents the summary statistics for our sample across states and over time. Our data set spans the years 1970-1996. We construct our measure of per capita income as personal income per state per year. Using the national Consumer Price Index (CPI), we measure state income in 2005 US dollars. We divide the CPI adjusted annual state income by the annual state population. We use state population figures from the Bureau of Labor Statistics annual report. Income growth is defined as $\frac{Y_{t,i}}{Y_{t-1,i}}$, where $Y$ is the per capita income for each state ($i$) in each year ($t$). The population density of a state is the ratio of the total state population and the total area of the state, measured in square miles. Patent growth is the growth in awarded patents in the state relative to the number of patents awarded in 1970. The yearly change in Non-interest Expenses to Assets is calculated using aggregate banking data for each state. The Loan and Deposit rates are estimates of the average rate charged/payed by banks for loans and deposits. The interest rate spread is the difference between the loans rate and the deposit rate. We exclude Delaware and South Dakota from our analysis due to the presence of unique tax incentives that eliminated usury ceilings in order to attract credit card banks. The data reported are for the 49 remaining states (including the District of Columbia).

| | N | Mean | SD | Min | Max |
|---|---|---|---|---|---|
| **Per Capita Income** | 945 | 21,504 | 4,307 | 11,665 | 33,205 |
| **Income Growth (%)** | 910 | 2.27 | 1.92 | -3.00 | 10.55 |
| **log(Population Density)** | 945 | 3.97 | 0.93 | 1.36 | 5.73 |
| **Patent Growth** | 945 | 1.29 | 0.56 | 0.55 | 4.89 |
| **Change in Non-Interest Expenses to Assets (%)** | 910 | 0.04 | 0.16 | -0.61 | 1.19 |
| **Loan Rate (%)** | 945 | 10.11 | 1.89 | 6.51 | 15.25 |
| **Deposit Rate (%)** | 945 | 3.80 | 1.37 | 1.33 | 7.40 |
| **Interest Rate Spread (%)** | 945 | 6.31 | 0.88 | 4.46 | 10.51 |

Table 3: Growth Regressions: Replication and Original Results

We replicate the main results from Jayaratne & Strahan (1996) using their sample period from 1972-1992 (Panel A). These results correspond to Tables 2 and 4 in Jayaratne & Strahan (1996) which we present in Panel B. We include estimates from their differences-in-differences specification $\frac{Y_{t,i}}{Y_{t-1,i}} = \alpha_t + \beta_i + \gamma * D_{t,i} + \varepsilon_{t,i}$ and the differences-in-differences specification that includes three lags of growth. Deregulation is a dummy variable equal to 1 in the years following deregulation and zero in the years prior to deregulation. The variables $\text{Income}_{t-1}$, $\text{Income}_{t-2}$, and $\text{Income}_{t-3}$ represent per capita income measured in 2005 US dollars at 1, 2, and 3 year lags. In all regressions we include year and state fixed effects, indicated by 'Controls'. We report heteroskedasticity robust standard errors in parentheses. *, **, and *** indicate significance at the 5%, 1% and .1% levels, respectively.

| | Panel A: Replication | | | | Panel B: Original | | | |
|---|---|---|---|---|---|---|---|---|
| | OLS | WLS | OLS | WLS | OLS | WLS | OLS | WLS |
| Deregulation | 0.932** | 1.070*** | 0.767* | 0.742* | 0.94** | 1.19*** | 0.88** | 0.97** |
| | (0.31) | (0.30) | (0.32) | (0.28) | (0.26) | (0.24) | (0.27) | (0.23) |
| $\text{Income}_{t-1}$ | | | 0.0084 | 0.1187 | | | 0.12 | 0.18** |
| | | | (0.12) | (0.08) | | | (0.09) | (0.05) |
| $\text{Income}_{t-2}$ | | | -0.06179 | 0.02406 | | | -0.05 | 0.04 |
| | | | (0.07) | (0.05) | | | (0.06) | (0.04) |
| $\text{Income}_{t-3}$ | | | -0.0416 | -0.0000138 | | | -0.06 | 0.02 |
| | | | (0.06) | (0.04) | | | (0.08) | (0.04) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| $R^2$ | 49.9% | 71.2% | 52.3% | 74.0% | 49.0% | 70.0% | 50.0% | 72.0% |
| N | 1,015 | 1,015 | 915 | 915 | 1,015 | 1,015 | 1,015 | 1,015 |

Table 4: Synthetic Controls for Selected States
This table provides the weights of control states that most closely match a selection of 9 deregulating states. We create a "synthetic" match for each state in our sample using the synthetic controls methodology. The synthetic matches are weighted averages of potential control states. We match states during a pre-deregulation window of at least 5 years. The synthetic controls method minimizes the mean-squared prediction error (goodness-of-fit) between the true state and a portfolio of potential matching states during the pre-deregulation window for a specified set of covariates. The covariates that determine the match are per capita income, personal income growth, log of population density, scaled patenting activity, change in bank efficiency, loan rate, deposit rate, and the spread between the loan and deposit rates. The weights are rounded to the nearest third decimal place.

| State | VA | NJ | PA | WI | CT | WA | WY | MS | ND |
|---|---|---|---|---|---|---|---|---|---|
| Alaska | 0.009 | - | 0.017 | - | - | 0.036 | 0.256 | - | 0.179 |
| Alabama | - | - | - | - | - | - | - | - | - |
| Arkansas | - | - | - | - | - | - | - | 1.000 | 0.544 |
| Arizona | - | - | - | - | - | - | - | - | - |
| California | 0.162 | - | 0.169 | - | 0.091 | 0.280 | - | - | - |
| Colorado | 0.011 | - | - | - | - | 0.146 | - | - | - |
| Connecticut | - | - | - | - | - | - | - | - | - |
| District of Columbia | 0.059 | 0.070 | 0.069 | 0.026 | 0.227 | - | - | - | - |
| Florida | - | 0.025 | - | - | - | - | - | - | - |
| Georgia | - | - | - | - | - | - | - | - | - |
| Hawaii | - | 0.301 | - | - | 0.119 | - | - | - | - |
| Iowa | - | - | 0.034 | 0.524 | - | - | - | - | - |
| Idaho | - | - | - | 0.031 | - | 0.064 | 0.631 | - | 0.277 |
| Illinois | - | - | 0.061 | - | - | - | - | - | - |
| Indiana | - | - | 0.106 | - | - | - | - | - | - |
| Kansas | - | - | - | - | - | - | - | - | - |
| Kentucky | - | - | 0.228 | - | - | - | - | - | - |
| Louisiana | 0.042 | - | - | - | - | - | - | - | - |
| Massachusetts | 0.092 | - | - | - | - | - | - | - | - |
| Maryland | - | - | - | - | - | - | - | - | - |
| Maine | - | - | - | - | - | - | - | - | - |
| Michigan | 0.044 | 0.158 | - | - | - | - | - | - | - |
| Minnesota | - | - | - | - | - | - | - | - | - |
| Missouri | - | - | - | - | - | - | - | - | - |
| Mississippi | - | - | - | - | - | - | - | - | - |
| Montana | - | - | - | - | - | - | - | - | - |
| North Carolina | - | - | - | - | - | - | - | - | - |
| North Dakota | - | 0.007 | - | - | - | - | - | - | - |
| Nebraska | - | - | - | - | - | - | - | - | - |
| New Hampshire | - | - | - | - | - | - | - | - | - |
| New Jersey | - | - | - | - | - | - | - | - | - |
| New Mexico | 0.057 | - | - | - | - | 0.248 | - | - | - |
| Nevada | - | 0.439 | - | 0.077 | 0.563 | 0.226 | 0.112 | - | - |
| New York | - | - | - | - | - | - | - | - | - |
| Ohio | - | - | - | 0.249 | - | - | - | - | - |
| Oklahoma | - | - | - | - | - | - | - | - | - |
| Oregon | - | - | - | - | - | - | - | - | - |
| Pennsylvania | - | - | - | 0.001 | - | - | - | - | - |
| Rhode Island | - | - | 0.231 | - | - | - | - | - | - |
| South Carolina | - | - | - | - | - | - | - | - | - |
| Tennessee | 0.426 | - | - | - | - | - | - | - | - |
| Texas | - | - | - | - | - | - | - | - | - |
| Utah | - | - | - | - | - | - | - | - | - |
| Virginia | - | - | - | - | - | - | - | - | - |
| Vermont | - | - | - | 0.091 | - | - | - | - | - |
| Washington | - | - | - | - | - | - | - | - | - |
| Wisconsin | - | - | 0.085 | - | - | - | - | - | - |
| West Virginia | - | - | - | - | - | - | - | - | - |
| Wyoming | 0.096 | - | - | - | - | - | - | - | - |
| RMSPE | 0.03 | 0.03 | 0.04 | 0.27 | 0.27 | 0.31 | 1.20 | 1.36 | 1.77 |

Table 5: Matching Period Characteristics
Table 5 presents a summary of the covariates used in the construction of synthetic matches as well as additional state-level characteristics during the matching period. For each state in our sample we construct a synthetic match using the synthetic controls method. We then calculate the characteristics of the synthetic match as a weighted average of the constituents of the match. All variables for the synthetic match are constructed in the same manner as our original sample. We include a measure of loans growth as the change in loans scaled by prior year loans. We measure bank profits as net income scaled by deposits. In addition, the table includes the number of bank branches and average housing prices in each state. We report the average and standard deviation of all variables for each real and synthetic state during the pre-deregulation period. The last column reports the normalized difference in means between the real and synthetic samples during the matching period (i.e. prior to deregulation).

| | **Real** | | **Synthetic** | | |
|---|---|---|---|---|---|
| | **Mean** | **SD** | **Mean** | **SD** | **Norm. Diff.** |
| **Per Capita Income** | 18,916 | 3,071 | 18,960 | 2,934 | -0.015 |
| **Income Growth (%)** | 2.349 | 3.308 | 2.364 | 2.230 | -0.006 |
| **Population Density** | 4.108 | 1.238 | 3.782 | 0.932 | 0.298 |
| **Patent Growth** | 1.066 | 0.303 | 1.063 | 0.219 | 0.010 |
| **Change in Non-Interest Expenses to Assets (%)** | 0.000 | 0.001 | 0.039 | 0.113 | 0.029 |
| **Loan Rate (%)** | 9.973 | 2.263 | 10.012 | 2.116 | -0.018 |
| **Deposit Rate (%)** | 3.915 | 1.560 | 3.815 | 1.427 | 0.067 |
| **Interest Rate Spread (%)** | 6.058 | 1.007 | 6.197 | 0.867 | -0.148 |
| **Unemployment (%)** | 7.113 | 2.381 | 7.110 | 1.358 | -0.172 |
| **Loans Growth (%)** | 10.613 | 7.642 | 11.879 | 7.031 | 0.000 |
| **Branches** | 565 | 592 | 642 | 418 | -0.150 |
| **Housing Prices** | 94,569 | 23,098 | 96,341 | 23,577 | -0.076 |
| **Bank Profit (%)** | 0.963 | 0.328 | 1.001 | 0.194 | -0.145 |