Did U.S. banking deregulation strengthen economic growth? The importance of spatial spillovers

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Abstract

This paper analyzes the effect of deregulation of the banking industry on economic growth in the United States. In contrast to earlier studies, we explicitly allow for spillover effects, through which deregulation of the banking sector in one state may affect economic growth in neighboring states. We find robust evidence in favor of a positive effect running form interstate banking deregulation to growth, whereas no evidence is found for an effect of intrastate branching deregulation. In addition, we find that there are indeed strong spatial spillover effects of interstate banking deregulation. Hence, interstate banking deregulation is not only beneficial for the state undergoing the deregulation, but also for neighboring states.

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1. Introduction

Liberalization and deregulation of the banking industry have traditionally been seen as important drivers of economic growth. By fostering efficiency and competition, these measures were believed to lead to improved lending conditions for borrowers and a better allocation of savings to profitable investment opportunities. These improvements, in turn, would have a positive effect on the efficiency and growth of the real sector of the economy (Besanko and Thakor, 1992; Smith, 1998).

More recently, however, the potential downsides of liberalization and deregulation have received more attention. By facilitating expansion across state borders, these measures have allowed some banks to grow so large that they are considered too-big-to-fail (Mishkin, 1999). The resulting increase in risk-taking by these large banks can be very disruptive to the economy, as we have observed during the recent financial crisis. Some also argue that an increase in the competitiveness of the banking industry, to which deregulation is supposed to contribute, might not necessarily foster economic growth. The argument is that banks which operate in a highly competitive environment might be inhibited from forming long-term lending relationships with small and medium-sized enterprises (SMEs). Since SMEs are important drivers of innovation but are typically dependent on bank credit, a highly competitive banking industry might thus be detrimental to economic growth (Petersen and Rajan, 1995; Cetorelli and Peretto, 2012).

This paper analyzes the effect of deregulation of the banking industry on economic growth in the United States. Following much of the existing literature (Jayaratne and Strahan, 1996; Black and Strahan, 2002; Freeman, 2002; Strahan, 2003; Dick, 2006; Rice and Strahan, 2010; Koetter et al., 2012; Amore et al., 2013; Chava et al., 2013), we use the incremental relaxation by state legislatures of intrastate branching and interstate banking restrictions in the 1970s, 80s and 90s as a natural experiment. Since different states deregulated their banking industries at different points in time, the resulting combination of cross-sectional and temporal variation allows for a clear identification of the effects of deregulation. The contribution of our study to the existing literature is twofold. First, we take into account the possibility that the effect of deregulation on growth may produce *spillovers* to neighboring states. In the context of the relationship between banking sector deregulation and economic growth, spillover effects can be expected because (1) firms may be able to borrow funds from banks in neighboring states and (2) the economies of neighboring states are typically connected by trade linkages and commuters. Controlling for potential spillovers is important, because if spillovers are present, ignoring them will lead to biased estimates of the effect of deregulation on economic growth. Second, we critically analyze the robustness of our findings by comparing local growth rates in a matched-pairs setting. In this part of the study, we build upon Huang (2008), who analyzes differenes in local growth rates across state borders with regulatory differences. Since the decision to deregulate the banking sector is taken at the state level, a local analysis is necessary to rule out the possiblity that the observed relationship between deregulation and economic growth is due to simultaneity, i.e. due to a change in state-level economic growth leading to deregulation.

The rest of the paper is structured as follows. Section 2 summarizes the existing theoretical and empirical litature on the relationship between banking sector deregulation and economic growth. Our empirical strategy is elaborated upon in Section 3. A description of the data then follows in Section 4, after which Section 5 reports the results of our main analysis. Some concluding thoughts follow in Section 6.

2. Related literature

Theory. The theoretical literature that analyzes the real effects of banking deregulation took off with the seminal paper by Besanko and Thakor (1992), who build a spatial model to illustrate the effects of a relaxation of entry barriers into banking. Their model shows that banking deregulation raises competition and thus improves the welfare of borrowers and savers by lowering loan rates and increasing deposit rates. Both savings and investments would be expected to increase, with beneficial effects for economic growth. Petersen and Rajan (1995), on the other hand, argue that a more competitive banking sector does not necessarily lead to higher growth rates because competition might hamper relationship lending. Young and innovative firms are typically not profitable in their early years, but might become so when they mature. When banks have market power, relationship lending allows them to extract rents from such firms once they become profitable. In a competitive banking industry, however, borrowers can turn to a competing bank once they are profitable, so that the initial lender cannot expect to share in the future surplus of the borrower. As a result, young firms may not be able to obtain a loan in the first place. Another reason why a more competitive banking sector might hamper economic growth is that it may lead to less efficient screening by banks. As a result, lending rates might actually be pushed *up* rather than down (Marquez, 2002). In addition, investments in information acquisitions might become less worthwile and therefore fall, resulting in less efficient lending decisions (Hauswald and Marquez, 2006). The potentially ambiguous effect of banking competition on growth is confirmed by Cetorelli and Peretto (2012), who build a model in which banks can choose between lending at arm's length and relationship lending. They show that an increase in competition lowers banks' incentive to engage in relationship lending, which lowers the *quality* of investments. However, competition also lowers interest rate spreads, which positively affects the *quantity* of lending. As a result, the overall effect of a change in banking competition on growth is theoretically ambiguous.

Empirics. Given the ambiguity of the theoretical literature, we now turn to empirical studies of the relationship between banking sector deregulation and economic growth. This literature kicks off with a study by Jayaratne and Strahan (1996), who study the growth effects of the relaxation of intrastate bank branch restrictions in the United States in the 1970s and 80s. They find that these deregulations had a positive and large, significant effect on growth rates. Moreover, their study suggests that this posivite effect cannot be explained by increases in saving and lending following deregulation. Instead, Jayaratne and Strahan (1996) find that this finding can be explained by the fact that better banks grow at the expense of their less efficient rivals after deregulation has taken place. As a result, the performance of the banking sector as a whole improves. The results of Jayaratne and Strahan are corroborated by a number of studies. Black and Strahan (2002) find that the rate of new incorporations increases after states relax branching restrictions. Strahan (2003) also finds an increase in entrepreneurial activity as well as growth rates, after deregulation. Moreover, studies by Dick (2006) and Rice and Strahan (2010) indicate that interest rate spreads fall after deregulation. Finally, Koetter et al. (2012) find that banks become more efficient after deregulation, while the results of Amore et al. (2013) and Chava et al. (2013) indicate that interstate banking deregulation spurred innovation by public and private firms. These findings, and especially the earlier studies, have received a fair amount of criticism, however, with the main point being that deregulation might be endogenous to state-level economic conditions. For example, Freeman (2002) uses an event study methodology to argue that states have tended to deregulate their banking system during times of econonomic distress. Hence, the increase in growth rates observed after deregulation could be attributed to a recovery from a recession rather than to a causal effect. Wall (2004) finds that the positive relationship between deregulation and entrepreneurship becomes ambiguous once regional effects are taken into account. Finally, Huang (2008) compares the growth rates in counties on opposite sides of state borders and concludes that the evidence for a causal effect running from deregulation to growth is weak. He argues that the observed correlation between deregulation events and subsequent growth spurts at the state level could instead by explained by expectations of future growth opportunities inducing state legislatures to deregulate their banking sectors.

The argument of Huang (2008) fits well in an old debate on the relationship between growth and finance. In this debate one side is of the Schumpeterian viewpoint that financial development causes economic growth (Schumpeter, 1934), whereas the other side argues that 'where the economy leads, finance follows' (Robinson, 1952). In the banking deregulation literature, an important study which analyzed the determinants of deregulation has been conducted by Kroszner and Strahan (1999). Their findings indicate that the relative strength of potential winners (large banks and small firms) and losers (small banks and insurance firms) can explain the timing of intrastate branching deregulation across states. A spatial analysis by Garrett et al. (2005), which takes into account the fact that that state-level banking deregulations are highly spatially correlated (i.e. states tend to deregulate when their neighbours have recently done so), largely confirms these findings. Since the idea that the strength of these interest groups is determined by growth rates seems far-fetched, this would suggest that deregulations can safely be assumed to be exogenous when analyzing their effect on economic growth. Nevertheless, given the findings of Freeman (2002) and Huang (2008), causality running from economic growth to relaxations of banking restrictions cannot entirely be ruled out. Hence, we have to take this possibility into account in our analysis.

3. Empirical strategy

This section discusses the empirical strategy used to analyze the effect of banking sector deregulation on economic growth. As was mentioned in the introduction, we study the incremental relaxation of restrictions on intrastate branching and interstate banking in the U.S. in the 1970s, 80s and 90s as a natural experiment. Intrastate branching restrictions refer to state-level regulations which prohibit or restrict banks from expanding *within* a state by acuiqiring branches of existing banks or by establishing new branches. In 1970, only a handful of states allowed banks to freely expand within their borders. Most states restricted intrastate branching in some way, with some states going so far as to only allow *unit banking*, which means that banks we/re allowed to have only one branch. Interstate banking restrictions, on the other hand, refer to regulations that prevent out-of-state banks from expanding *across* borders into the regulated state. Interstate banking was even more restricted in 1970, when not a single state allowed out-of-state banks to freely enter its market. In the period between 1970 and 1997, both intrastate branching and interstate banking restrictions were gradually relaxed, however, until the passage of the Riegle-Neal Interstate Banking and Branching Efficiency Act (IBBEA) removed the remaining barriers to intrastate branching and interstate banking in 1997.

In assessing the effect of the above-mentioned deregulations on economic growth, the empirical challenge is the fact that there might be a two-way causality between banking deregulation and economic growth. That is, deregulation might not only affect growth, but (expectations of future) growth might also induce deregulation. Since it is difficult to convincingly rule out simultaneity with only a state-level analysis, we need additional evidence from an analysis at a more local level to determine the causal relationship of state-level banking deregulation on economic growth. A complicating factor is that economic growth, deregulations and the relationship between them can be expected to be spatially correlated. An increase in economic growth in a certain area is likely to have a positive spillover effect on growth in neighboring areas. Furthermore, it has been shown by Garrett et al. (2005) that states tend to deregulate when their neighbors have recently done so. Finally, deregulations can have spillover effects in the sense that they might not only affect growth in the deregulated state itself, but also growth in neighboring states. These spillover effects could occur either because deregulation in one state directly affects growth in neighboring states, or because a change in a state's growth rate following deregulation spills over to neighboring states. The former type of spillover is typically referred to as a *local spillover*, whereas the second type is referred to as a *global spillover*. If the growth effects of deregulation indeed spill over to neighboring states, it is not surprising that the study by Huang (2008), which focuses on differences in growth rates between counties on opposite sides of state borders, does not find strong evidence in favor of a causal effect running from deregulation to growth. The reason for this is that, while the county in the deregulated state is expected to experience higher growth due to the direct effect of deregulation, the county on the opposite of side of the border is expected to experience higher growth as well, due to the spillover effect of deregulation on growth. Hence, a higher rate of economic growth would be expected on *both* sides of the border. We therefore need a different strategy to obtain evidence on the deregulation-growth nexus at the local level if the data suggest that spillovers are present.

To tackle the above-mentioned issues, we procede as follows. First, we study the relationship between deregulation and growth at the state level, and analyze whether or not the data suggest the presence of spillover effects. Next, we analyze the relationship between deregulation and growth at the local level, using a matched-pairs setting, in which growth rates are compared within pairs of counties that are located in states which deregulated their banking sectors at different points in time. The approach of Huang (2008) is appealing in this respect, since contiguous counties on opposite sides of state borders are likely to be similar in terms of unobservable characteristics. However, in the presence of spillovers, identifying an effect of deregulation on growth might be difficult in this setup, as explained above. In the matching of local areas, there thus appears to be a tradeoff between the *comparability* of areas and the *identifiability* of an effect of deregulation on growth. Matching areas that are located further away from one another should make it easier to identify a relationship between deregulation and growth, since spillovers effects can be expected to decrease with distance. At the same time, this would make it more difficult to convincingly argue that the identified relationship represents a causal effect, since the two areas can be expected to be less comparable with respect to unobservable characteristics. We therefore try to strike a balance between compariablity and identifiability by matching areas based on observable characteristics instead of geographic location, but we require matched areas to be located in the same geographic region, with the regions being the West, Midwest, South and Northeast of the U.S.[1]

[1]We follow Jayaratne and Strahan (1996) in the grouping of states.

Below, we elaborate upon the state-level component of our empirical analysis. In the next subsection, we provide more details about the matching procedure used in the local-level component of our study.

3.1. State-level analysis

We begin our state-level analysis by estimating the basic model of Jayaratne and Strahan (1996):

$$\boldsymbol{y}_t = \boldsymbol{\alpha} + \beta_t + \boldsymbol{X}_t\boldsymbol{\gamma} + \boldsymbol{\varepsilon}_t, \tag{1}$$

where y_t is a vector of per capita income growth rates at time t, α is a vector of state-specific constants included to capture unobserved state heterogeneity, and β_t is a time-specific constant included to control for country-wide business cycle effects.[2] Furthermore, $\boldsymbol{X}_t$ is a matrix that includes two vectors of deregulation dummies with a value of 1 in the years following intrastate branching or interstate banking deregulation, and a value of 0 otherwies. The parameters of interest are included in the vector $\boldsymbol{\gamma} = [\gamma_1, \gamma_2]$, which captures the effects of intrastate branching and interstate banking deregulation on growth. Finally, ε_{it} is an error term with mean zero, which is assumed to be uncorrelated with the explanatory variables. As explained above, we suspect that deregulations might have spillover effects. If this is the case, the estimates of Equation (1) will be inconsistent due to omitted variable bias. Furthermore, we expect the spillover effects to be captured by the error term since they are not accounted for by the model, in which case the error terms will be spatially correlated. That is, we expect a positive correlation between the error in one state in a particular period and the errors in neighboring states in the same period. As a first test of the presene of spillovers, we estimate a so-called Spatial Error Model (SEM) (Anselin, 1988; Anselin et al., 1996), which captures the presence of spatial correlation in the error term:

$$\boldsymbol{y}_t = \boldsymbol{\alpha} + \beta_t + \boldsymbol{X}_t\boldsymbol{\gamma} + \boldsymbol{u}_t \tag{2}$$

$$\boldsymbol{u}_t = \lambda \boldsymbol{W} \boldsymbol{u}_t + \boldsymbol{\varepsilon}_t. \tag{3}$$

Here, $\boldsymbol{u}_t$ is a vector of (potentially) correlated error terms and $\boldsymbol{W}$ is an N-dimensional *spatial weight matrix* which describes the spatial structure of the states in our analysis.[3] We use a so-called *binary contiguity (BC) matrix*, with entry (i, j) equal to the inverse of the number of neighbors of state i if

[2] In fact, this constant captures any time-varying variable that is constant over all states. For instance, it captures the total number of states which have deregulated their banking sector at any point in time.

[3] Note that the model in Equation (2) and (3) can be written as: $\boldsymbol{y}_t = \boldsymbol{\alpha} + \beta_t + \boldsymbol{X}_t\boldsymbol{\gamma} + (\boldsymbol{I}_N - \lambda\boldsymbol{W})^{-1}\boldsymbol{\varepsilon}_t$. Since this is a non-linear model, we estimate it by means of Maximum Likelihood estimation using Stata's *xsmle* package.

states i and j share a border and 0 otherwise.[4] Intuitively, this means that it is assumed that the error of a particular state in year t depends on the average error of its neighbors in the same period. Note that the expression of the error term in Equation (3) is similar to that of the error in an autoregressive model, with the difference that it includes the term $\boldsymbol{W u}_t$ (a spatial lag) rather than $\boldsymbol{u}_{t-1}$ (a temporal lag). Indeed, the SEM model with a BC matrix can be interpreted as the spatial counterpart of an AR(1) model. Where the AR(1) model assumes that the error in one period is only directly affected by the error in the previous period, the SEM with a BC matrix assumes that the error in one state is only directly affected by the errors in its immediate neighbors. We test for the presence of spillovers by testing the significance of λ, which would indicate spatial correlation in the error term, and by comparing the estimates of the SEM with those of the base model using a spatial Hausman test based onPace and LeSage (2008). In the presence of spillovers, we expect a significant difference between the estimates of the two models and a positive and significant estimate of λ.

If the results of the models above suggest that spillover effects are present, these spillovers can be modelled in different ways. First, deregulation in one state could directly affect growth in neighboring states. This is called a *local* spillover, because the spillover effect crosses only one border in any direction. This type of spillover may occur if firms from neighboring states are able to borrow from banks in a deregulating state, so that this deregulation affects the funding of firms in neighboring states. In contrast, a *global* spillover would occur if changes in growth itself spill over to neighboring states. This type of spillover may occur if states are economically dependent on one another, for instance due to trade linkages or commuting. If this is the case, the change in the growth rate of neighboring states will in turn spill over the neighbors of those neighbors, and so on, which is why the process is referred to as a global spillover. Obviously, local and global spillover effects are not mutually exclusive and may occur simultaneously. Since we want to take into account the potential occurrence of both local and global spillovers, we estimate the so-called Spatial Durbin Model (SDM) (Anselin, 1988; LeSage and Pace, 2009).[5] This model allows for spillovers of both types of spillovers and is specified as follows:

$$\boldsymbol{y}_t = \boldsymbol{\alpha} + \beta_t + \rho \boldsymbol{W y}_t + \boldsymbol{X}_t \boldsymbol{\gamma} + \boldsymbol{W}\mathbf{X}_t \boldsymbol{\theta} + \boldsymbol{\varepsilon}_t. \tag{4}$$

Here, the inclusion of the term $\boldsymbol{W y}_t$ captures the idea that an increase in the growth rate of a given

[4] We also considered a so-called *inverse distance matrix*, in which entry (i, j) equals the inverse of the geographical distance between the centroids of states i and j. However, Bayesian posterior model probabilities clearly indicate that a binary contiguity matrix better describes the spatial structure of the data.

[5] We also considered models which only allow for local spillovers. However, Bayesian posterior model probabilities suggest that the SDM best describes our data.

state may effect growth in other states. Furthermore, the inclusion of the term $\boldsymbol{W}\mathbf{X}_t$, captures the idea that intastate branching and interstate banking deregulation in one state may effect growth in other states.[6] Estimation of the SDM allows for distinguishing between a direct effect and a spillover effect of deregulation on growth. The direct effect refers to the effect of deregulation on growth in the deregulating itself, whereas the spillover effect refers to the cumulative effect of deregulation on growth in all other states. It should be noted, however, that the estimated coefficients of Equation (4) do not correspond directly with the marginal effects of deregulation. In the SDM, marginal effects typically vary by state and depend in a complicated way on the spatial structure of the data.[7] The interested reader is referred to (LeSage and Pace, 2009) and Elhorst (2014) for a more thorough discussion of the SDM model.

3.2. Local-level analysis

As Huang (2008) correctly points out, a state-level analysis has the drawback that it can never entirely rule out reverse causality running from (expectations of future) growth to deregulation, since the *decision* to deregulate is taken at the state level as well. For this reason, we continue our study by following Huang (2008) and analyzing the relationship between deregulation and growth at the local level. In accordance with the existing literature, we define a local banking market as either a county (for non-metropolitican counties) or a metropolitan statistical area (MSA). For simplicity, we refer to local banking markets as counties in the remainder of this paper.

We perform our analysis by matching counties from different states into pairs and analyzing how differences in economic growth within these pairs are related to differences in the timing of bank deregulation in the respective counties' states. We do this by estimating the following model:

$$y_{ipt} = \alpha_{ip} + \beta_{pt} + \gamma_1 intra_{ipt} + \gamma_2 inter_{ipt} + \boldsymbol{\delta}^T \boldsymbol{z}_{itp} + \varepsilon_{ipt}, \tag{5}$$

where y_{ipt} is the rate of per capita GDP growth in county i ($i = 1, 2$) of county-pair p in year t, α_{ip} is a county-specific constant and β_{pt} is a pair-year-specific constant. By controlling for county and pair-year fixed effects, we only use the within-pair variation in growth rates to identify the effect of deregulation on growth. Our analysis thus takes only the other county in a given pair as the control county, whereas in a traditional regression analysis, *all* other counties are used as controls. Implicitly,

[6]The model in Equation 4 is non-linear, and can be written as $\boldsymbol{y}_t = (\boldsymbol{I}_N - \rho\boldsymbol{W})^{-1}(\boldsymbol{\alpha} + \beta_t + \boldsymbol{X}_t\boldsymbol{\gamma} + \boldsymbol{W}\mathbf{X}_t\boldsymbol{\theta} + (\boldsymbol{I}_N - \rho\boldsymbol{W})^{-1}\boldsymbol{\varepsilon}_t$. We estimate it by means of Maximum Likelihood estimation using Stata's *xsmle* package.

[7]More specifically, whereas the (constant) marginal direct effects of deregulation correspond with the estimates in $\boldsymbol{\gamma}$ for the OLS and SEM, the (state-specific) marginal direct effects are represented by the diagonal elements of the N-dimensional matrix $[(\boldsymbol{I}_N - \rho W)^{-1}\gamma_k]$ for k $= 1, 2$ in the SDM. In a similar sense, the (state-to-state-specific) marginal spillover effects are equal to the off-diagonal elements of the N-dimensional matrix $[(\boldsymbol{I}_N - \rho W)^{-1}(\gamma_k + \boldsymbol{W}\theta_k)]$. See Vega and Elhorst (2013) for a proof.

a traditional regression thus assumes that one county in the U.S. is as good a control as any other, whereas we specifically match counties to obtain appropriate controls.

An important issue in our setup is the way in which counties are matched. As was explained above, we have to use a matching procedure which results in matched counties that are comparable with each other, without losing the ability to identify an effect of deregulation on growth in the presence of spillover effects. Since matching counties purely on the basis of geography (as is done by Huang (2008)) gives high comparability but low identifiablity, we pursue a different approach. More specifically, our matching procedure is as follows. First, we collect data on the population, average level of education and per capita income of each county in 1970. We then do a principal component analysis, using these three variables and a dummy variable which indicates whether or not the county represents a metropolitical statistical area (MSA). We order the counties on the basis of their value on the resulting first principal component and match on the basis of that ordering. That is, the first county is matched with the second, the third with the fourth, and so forth. The idea behind this procedure is that we match counties which are relatively similar in terms of population, education, income per capita and degree of urbanization. Given these observable variables, two counties in the same pair would therefore be expected to have undergone a deregulation of their banking sector at approximately the same time. In this sense, our procedure resembles that of propensity score matching (PSM), a procedure that is often used to estimate treatment effects in a cross-sectional setting. In our case, however, *all* counties are eventually treated, but there is variation in the *timing* of the treatments. By comparing growth rates of counties that are expected to have been deregulated in the *same* year (given the data of 1970), but in reality were deregulated in *different* years, we are able to identify a causal effect of deregulation on growth. To check the robustness of our results, we repeat the above-mentioned procedure using OLS instead of PCA. Here, the intrastate branching and interstate banking dummies are used as the dependent variable, whereas the variables used in the PCA are included as explanatory variables. Using a similar reasoning as before, counties are then matched on the basis of the predicted timing of their deregulations rather than the score on their first principal component.

Note that we sort the counties by four main geographic regions in the U.S. before matching them, so that matched counties are always from the same region.[8] This ensures that counties are similar with respect to unobservable variables that are constant within these four major regions. We prefer to account for geography in this way rather than by including a geographic variable in the PCA, since the latter strategy would be likely to result in county pairs consisting of neighboring or otherwise very approximate counties. As argued above, comparing counties that are geograpically very close to one

[8]These regions are the West, Midwest, Northeast and South. We follow Jayaratne and Strahan (1996) in this respect.

another is problematic, due to the potential spillover effects that may result from deregulation.

4. Data

Our sample includes the 48 states of the contiguous United States and runs from 1970 to 2000. We collect state-level and county-level income data from the Bureau of Economic Analysis. Our dependent variable, economic growth, is calculated as the annual percentage change in the level of per capita personal income expressed in 1983 U.S. dollars. Nominal income figures are deflated using a national consumer price index taken from the Bureau of Labor Statistics. The average state-level growth rate in the sample period is 1.76%. As shown in Figure 1a, growth rates at the state level are typically between -10 and 10%, although there are a few outliers.

[Figure 1 about here.]

On the county level, the variation in growth rates around the mean of 1.93% is significantly larger and outliers pose a larger problem, as can be seen in Figure 2a. However, as Figures 1b and 2b illustrate, censoring the data at the 1% level gets rid of all outliers. For this reason, we estimate the models on both the original data and on the data that results after having winsorized the growth rates at the 1% level. Winsorizing the data ensures that our results are not be driven by outliers, without throwing away information. We apply the Harris-Tzavalis unit-root test and reject the null hypothesis of a unit root in both state-level and local growth rates. Hence, we can safely conclude that income growth is stationary, irrespective of the level of analysis.

[Figure 2 about here.]

The timing of intrastate branching and interstate banking deregulations are taken from Demyanyk et al. (2007). In the case of intrastrate branching restrictions, a distinction can be made between the year in which a state relaxed restrictions on branching through mergers and acquisitions (M&As) and the year in which it allowed branching through the establishment of new branches (de novo branching). We follow most of the literature by choosing the year in which states allowed branching through M&As as the deregulation year. As shown in Figure 3, intrastate branching restrictions had already been relaxed in 11 states at the beginning of our sample period. After 1970, the number of states that allowed intrastate branching without any restrictions gradually increased, until all 48 states did so in 1997. On the contrary, not a single state allowed out-of-state banks to enter its market in 1970, but most states relaxed interstate banking restrictions in the 1980s. Note that since we do not know the exact date at which states deregulated their banking sectors and since we expect that it will take some time before these deregulations affect economic growth, we construct our deregulation dummies in such a way that they have a value of 1 in the years *after* deregulation has taken place, and a value of 0 in the years before and the year of the deregulation.

[Figure 3 about here.]

Data on education in 1970 is taken from the U.S. Census Bureau, which distinguishes between four levels educational attainment. These four levels correspond to people with (i) less than a high school diploma, (ii) a high school diploma, (iii) some college, and (iv) four years of college or higher. For every county, we calculate an *educational attainment index* by giving 1 point to each person with less than a high school diploma, 2 points to persons with only a high school diploma, and so on, and then taking the average number of points per inhabitant. Data on GDP per capita and the number of inhabitants per county in 1970 are taken from the Bureau of Economic Analysis. Finally, geographic data used to construct the spatial-weight matrices are obtained from Merryman (2005).

5. Results

We describe the results of our state-level estimations below. The results of the matched county-pairs analysis follow in the second subsection.

5.1. State-level analysis

Main analysis. The estimated coefficients of our state-level models are reported in Table 1. The estimates of the base model suggest that both intrastrate branching deregulation (intra) and interstate banking deregulation (inter) have a significant effect on growth (column 1). This finding is in line with earlier studiess in the literature that use a state-level model to assess the effects of banking deregulation (Jayaratne and Strahan, 1996; Strahan, 2003). Once we allow for spatial autocorrelation in the error term by estimating the SEM model, however, the significance of both coefficients disappears (column 2). Moreover, the change from significant to non-significant coefficients does not result from an increase in the standard errors, but from a drop in the estimates. As explained in Section 3, this suggests that both the base model and the SEM are misspecified. To formally test for misspecification, we conduct a Hausman test based on Pace and LeSage (2008). The idea behind this test is the following: if the models are correctly specified, which means that the true data-generating process (DGP) is correctly described by either Equation (1) or Equation (2), the OLS estimates will be consistent, while the SEM estimates will be consistent and efficient. This implies that the estimated coefficients of the two models should be approximately the same. A significant difference between the estimates of the two models thus suggests that they are both misspecified. The Hausman test gives a chi-square statistic of 14.5, which is significantly different from zero at any reasonable significance level. We thus reject the null hypothesis that the two models have equal coefficients and conclude that they are misspecified. Since the estimated spatial correlation coefficient (lambda) is positive and highly sigifnicant, we interpret this finding as an indication that spillover effects may be present.

The estimates of the SDM confirm this interpretation (column 3). They suggest that interstate banking deregulation has had a significant effect on economic growth in both the state itself as well as neighboring states. The point estimates suggest that interstate banking deregulatoin resulted in an increase in growth of around 0.5 percentage points, whereas the spillover effect on other states is found to be approximately 2.3 percentage points. Both effects are found to be statistically significant at the 1% level of signifance. The spillover effect may seem unrealistically large, but it should be pointed out that the estimated spillover effect refers to the *cumulative* effect on *all* other states. This makes it difficult to compare the size of the spillover effect with the direct effect. One way in which this could be done is by dividing the point estimate of the cumulative spillover effect by 47, which gives an average spillover effect of approximately 0.05 percentage points on the growth rate of a random other state. Clearly, the estimated spillover effect is larger for neighboring states than for states located furher away, since (1) neighboring states are affected by both local and global spillover effects, whereas states located further away only experience global spillovers, and (2) neighboring states experience first-order spilover effects, whereas other states are only affected by second-order or higher-order spillovers. As such, we believe that the statistical significance of the spillover effect is more relevant than its precise point estimate. In contrast to effects of interstate banking deregulation, we find only weak evidence in favor of a direct effect of intrastrate branching deregulation on economic growth, and no evidence for a spillover effect of intrastate branching deregulation.

Since the data indicate that there is a small probability that the Spatial Durbin Error model (SDEM) (LeSage and Pace, 2009) provides a better description of the data, we present its estimates in the final column.[9] The estimates confirm our findings, as we again find that significant direct effects and spillover effects of interstate banking deregulation on growth, but no significant effect of intrastate branching deregulation. Tihs result is in line with Strahan (2003) and Stiroh and Strahan (2003), who find stronger effects of interstate banking deregulation than of intrastate branching deregulation on the number of acquisitions and market share reallocation, respectively, in the banking industry.

[Table 1 about here.]

[9]The SDEM can be written as: $\boldsymbol{y}_t = \boldsymbol{\alpha} + \beta_t + \boldsymbol{X}_t\boldsymbol{\gamma} + \boldsymbol{W}\mathbf{X}_t\boldsymbol{\theta} + (\boldsymbol{I}_N - \lambda\boldsymbol{W})^{-1}\boldsymbol{\varepsilon}_t$. Hence, it captures local spillover effects and a spatially correlated error term, but no global spillover effects. Our Bayesian posterior model probabilities indicate that the probability that the SDM gives the best description of the data is about about 4 times as that of the SDEM, which is why our focus is on the SDM.

Robustness checks. We perform a wide range of robustness checks. The results are reported in Table 2 for the (SDM) and Table 3 (for the SDEM), which have the same structure. In column (1), we report the results after having dropped Delaware from the sample. As explained by Jayaratne and Strahan (1996), Delaware passed a law in 1982 which provided a tax incentive for credit card banks to locate there. As a result, Delaware's banking industry grew extremely fast in the years following the passage of this law.[10] Column (2) gives the results when we use winsorized growth data, where the data are winsorized at the 1st and 99th percentile of the distribution. In column (3), we have changed the timing of the deregulation dummies so that they change to 1 in the year in which the deregulation event took place. In column (4), we have included a lagged dependent variable, while we have included lagged real income and its square in column (5). The columns (6) through (8) repeat the pattern of columns (2) through (4), but with lagged real income and its square included. Finally, we include the second lag of real income and its square in column (9), and additionally include a lagged dependent variable in the final column. a model with only *local* spillover effects Table 3 These robustness checks confirm our main findings: we find a significant effect at the 5% level of significance in all specifications. The effect of interstate banking deregulation on economic growth is found to be somwhere in the range of 0.4 to 0.7 percentage points. The estimated spillover effect of interstate banking deregulation is statistically significant at the 1% level of significance in most instances, and at the 5% level in the remaining cases. In line with our baseline model, we do not find robust evidence in favor of an effect of intrastate branching deregulation on growth. The coefficients of lagged real income and its square are significant and have the expected sign. Consistent with the convergence hypothesis (J.B. and Sala-i-Martin, 1995), we find that states with a high level of initial income grow slower, but that the marginal effect becomes less pronounced the higher is the level of income. This finding continues to hold when we include the second lag of real income and its square instead of the first lag. Finally, the coefficient of lagged growth is insignificant and quite close to zero, which suggests that a static model appears to be appropriate.

[Table 2 about here.]

[Table 3 about here.]

As a further check, we store the residuals of our baseline models and regress them on a constant and two indicator variables that are equal to one in the three years before a state deregulated intrastate

[10] Note that the exclusion of Delaware requires a new spatial weight matrix, with dimension 47 rather than 48. However, since Delaware is a coastal state with only three neighbors, the effect of this change in the spatial weight matrix on the results should be modest.

branching and interstate banking, respectively. If Freeman (2002) is correct in arguing that states typically deregulated their banking industry during a recession in an attempt to stimulate growth, we would expect growth rates in the years prior to deregulation to be significantly lower than predicted on the basis of our model. This implies that we should find negative coefficients when we regress the residuals on the two indicator variables. In reality, however, we obtain coefficients that are not significantly different from zero.[11] Hence, there is no evidence that states tended to deregulate their banking industries during economic downturns. Finally, since we apply a differences-in-differences estimator to panel data, the standard errors might be biased downward due to serial correlation, as illustrated by Bertrand et al. (2004). We therefore estimate the standard errors of the base model and the SLX models using a wild boostrap procedure with 10,000 replications. The resulting standard errors are actually slightly smaller than the clustered standard errors reported in Table 1.[12] We thus conclude that the significance of our results does not appear to be driven by a downward bias in the standard errors.

Overall, we conclude that in order to obtain robust results, it is important to control for spillover effects when estimating the effect of banking deregulation on state-level economic growth. Moreover, we find a strong effect of interstate banking deregulation on growth, both in the deregulating state itself and through spillovers on neighboring states. However, we do not find robust evidence in favor of an intrastate branching deregulation on growth. In the next subsection, we report the results of our county-level analysis, in which we further investigate whether the relationship between interstate banking deregulation and economic growth can be attributed to a causal effect of deregulation on growth.

5.2. Local-level analysis

We now turn to the results of our analysis at the local level. As explained in Section 3, we conduct a matched-pairs analysis at the local level to rule out the possibility that the positive relationship between bank deregulation and economic growth might be the result of reverse causality running from (expectations of) growth to deregulation. The main idea behind the procedure is to match counties with similar observable characteristics, so that they would be expected to deregulate at approximately the same time given these observables. We are thus looking at the relationship between differences in growth rates and differences in the timing of bank deregulation of two counties, which would have been expected to have deregulated at the same time given their observable characteristics. Concep-

[11]The results are available upon request.

[12]Note that we can only apply the wild bootstrap to the base model and SLX model, since this procedure destroys the spatial strucuture of the dependent variable that is exploited to estimate the SEM, SDEM and SDEM models. Results are available upon request.

tually, the analysis is quite similar to that of propensity score matching (PSM) in an experimental setting. Whereas PSM compares treated and non-treated subjects with an a priori equal *probability* of having been treated, we compare subjects with a different *timing* of the treatment and with an a priori equal *expected* timing of the treatment. We consider three different matching procedures.

The first procedure is based on a PCA, with the following variables: income per capita in 1970, the population in 1970, the average level of education in 1970 and a dummy indicating that the county is a metropolitan statistical area. The first principal component of these four variables explains more than half of the variation in these variables and has positive factor loadings an all variables. We match counties based on this first principal component. All four variables are positively correlated with the first principal component. The correlations are 0.82, 0.53, 0.82 and 0.67, respectively. Hence, on one end of the spectrum we compare urban, high-income, high-education counties with each other, whereas on the other side of the spectrum we match rural, low-income, low-education counties. The second and third matching procedure are based on OLS, where we regress the timing of intrastate branching (2) and interstate banking (3) in the county on the above-mentioned observables. Counties are matched on the basis of the predicted timing of intrastate branching and interstate banking deregulation in the county's state. The results of the regressions are reported in Table 4. Since we are not interested in the estimates themselves, but only in the predicted timing of deregulations, we report conventional standard errors. The significance of the estimates should thus be interpreted with caution.

[Table 4 about here.]

After having matched local banking markets on the basis of the three above-mentioned procedures, we estimate the effects of banking market deregulation by estimating Equation (5) with a wide range of alternative specifications. As can be seen in the equation, we include pair-specific time fixed effects in the model. The identification of the coefficients associated with intrastate branching and interstate banking deregulation is thus purely based on the variation in the timing of deregulations within each county pair. The results are reported in Tables 5, 6 and 7, where every table corresponds to one of the matching procedures. The three tables have the same structure. Column (1) gives the results of our baseline model, after which a wide range of robustness checks folllow. The results in column (2) arise once we omit all counties from Delaware from the sample. Column (3) gives the results when the timing of the deregulation dummies is such that they have a value of 1 in the deregulation year. In column (4), we have included a lagged dependent variable in the set of regressors. In columns (5) through (8) we repeat the pattern of columns (1) to (4) after having included the lag of real income and its square to the model. Finally, columns (9) through (12) repeat the pattern again, but now we have included the *second* lag of real income and its square instead of the first lag.

[Table 5 about here.]

[Table 6 about here.]

[Table 7 about here.]

The picture that emegers from the three tables is that there is robust evidence in favor of the view that interstate banking deregulation has a positive effect on growth, which confirms the results of our state-level analysis. The estimates of the effect of interstate banking deregulation is statistically significant at at least the 5% level of significance in 33 of the 36 specifications. In the remaining specifications, it is significant at the 10% level of significance. The effect is also economically important: with a few exceptions, the estimates of the coefficient associated with the interstate banking deregulation dummy suggests an effect of deregulation on growth of 0.6 to 0.8 percentage points. Consistent with the results of the state-level model, we find that the intrastate branching deregulation dummy has a coefficient which is not significantly different from zero in the majority of cases. This finding is in line with the results of Huang (2008), who also obtains a statistically non-significant relationship between intrastate branching deregulation and economic growth in the majority of cases.

As expected, we find that counties with an initially high level of income per capita grow more slowly compared with counties with a low initial level of income per capita. Consistent with the convergence hypothesis, we also find the strenght of this relationship decreases with higher levels of (initial) income per capita. Most importantly, the estimated coefficient of interstate banking deregulation remains significant once we control for lagged income per capita. This suggests that our results are not explained by differences in growth opportunities between regulated and deregulated areas. The negative estimates of the coefficient associated with lagged economic growth could also be explained by a convergence effect: if a county grows relativey fast compared with the county with which it is paired, we might expect the other county to catch up in the next period, given that the two counties are relatively similar with respect to the characteristics on which they are matched.

Overall, we conclude that there is robust evidence in favor of a positive effect of interstate banking deregulation on economic growth. This effect is estimated to be somewhere in the range of 0.6 to 1 percentage point and remains significant after we control for spatial autocorrelation, local and global spillover effects, growth opportunities and potential reverse causality. This result also continues to hold when we use (panel-)bootstrapped instead of conventional (clustered) standard errors. We do not obtain robust evidence in favor of an effect of intrastate branching deregulation on growth, however.

This latter finding is in line with earlier work by Huang (2008), who compares ccontiguous counties across state borders to identify the effect of intrastate branching deregulations and also fails to find a robust effect on growth.

6. Conclusion

This paper has analyzed the effect of state-level deregulations of competitive restrictions in the banking industry on economic growth in the U.S. Since these deregulations occured in a staggered way, with different states relaxing restrictions at different points in time, we were able to identify the effect of deregulation on growth. The evidence suggests positive growth effect of relaxing restrictions interstate banking, but no effect of relaxing restrictions on within-state branching. We additionally find interstate banking deregulation produces spillover effects on neighboring states. A more detailed analysis at the local level confirms these findings. We find that counties in deregulated states experience higher growth compared with counties with similar charactersistics that are from states which have not yet been deregulated. Our results suggest that, even when *activity restrictions* on banks may have beneficial effects, *geographical restrictions* in the banking sector appear to be detrimental to the real economy. This finding is also relevant for regions such as Europe, where official geographic restrictions in banking are no longer present, but where in practice banks find it difficult to enter other markets due to for instance regulatory or cultural differences. Our study suggests that taking away such barriers may have beneficial growth effects.

Although this study has shown that deregulation of the banking sector spurred economic growth in the US, it leaves open the question of which mechanism is responsible for this connection between deregulation and growth. One potential channel could be that deregulation fostered liquidity creation by large banks, which could more easily operate at a larger scale after deregulation. Indeed, Berger and Sedunov (2017) find that liquidity creation is an important driver of economic growth and the results of Jiang et al. (2018) suggest that interstate banking deregulation spurred liquidity creation by banks. Another potential channel might be an increase in banking competition following deregulation, with local banks facing more competition both from domestic banks within their state as well as from out-of-state banks. Finally, deregulation may have increased the efficiency of the banking sector, so that savings were channeled more effectively into the best investment opportunities. We leave an investigation into these potential mechanisms open for future research.

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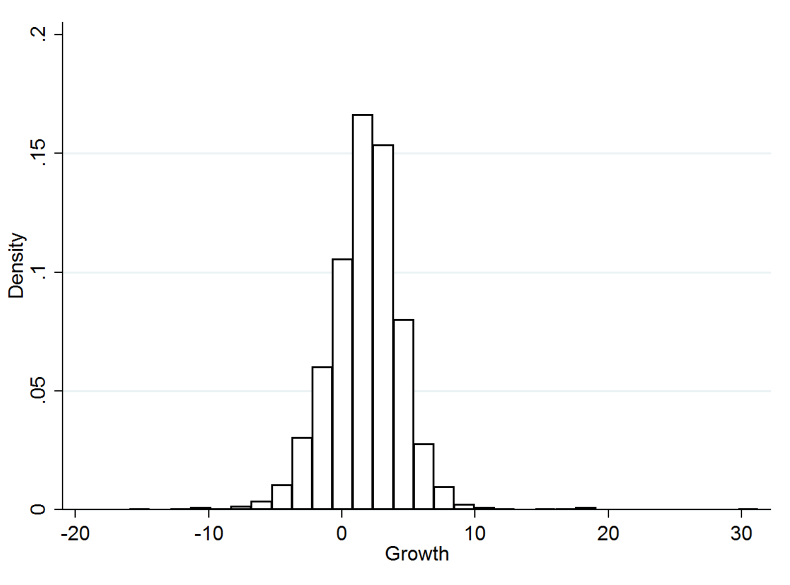
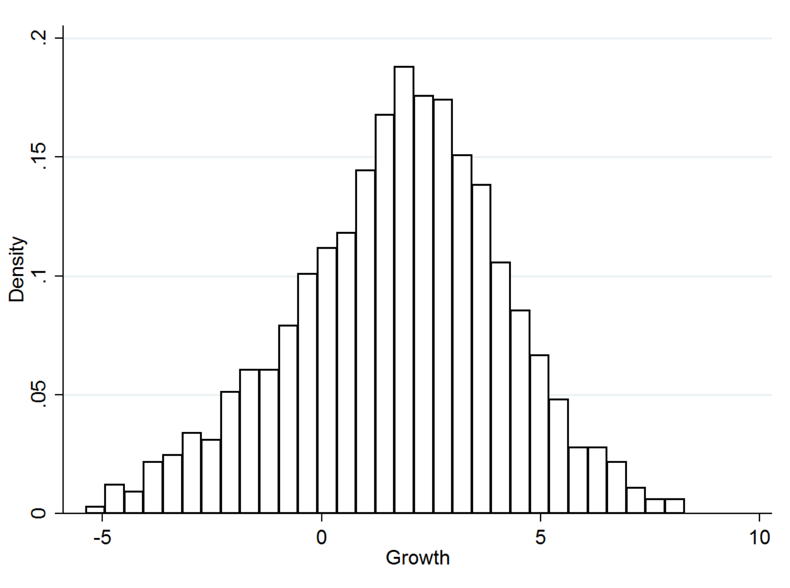
List of Figures

Figure 1: Distribution of state-level growth rates including (a) and excluding (b) the 1st and 99th percentile of the distribution. Growth refers to the annual percentage change in the level of per capita income in constant U.S. dollars.

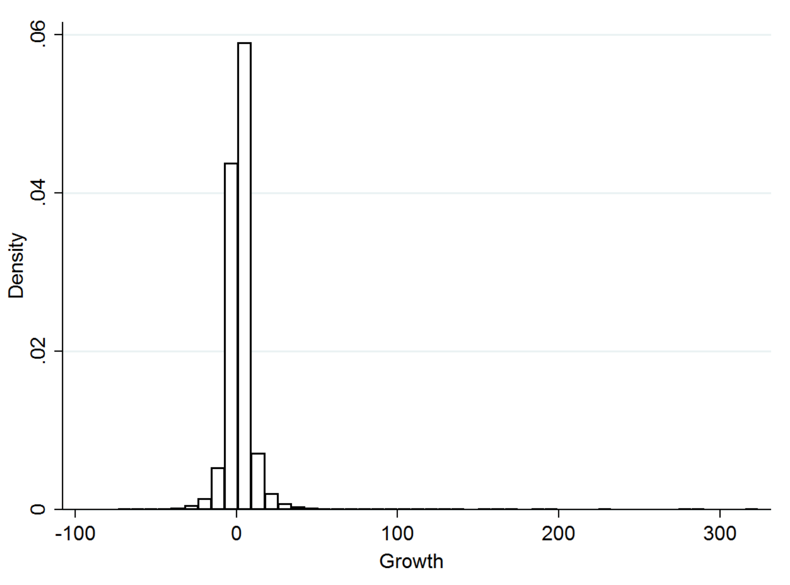


(a)

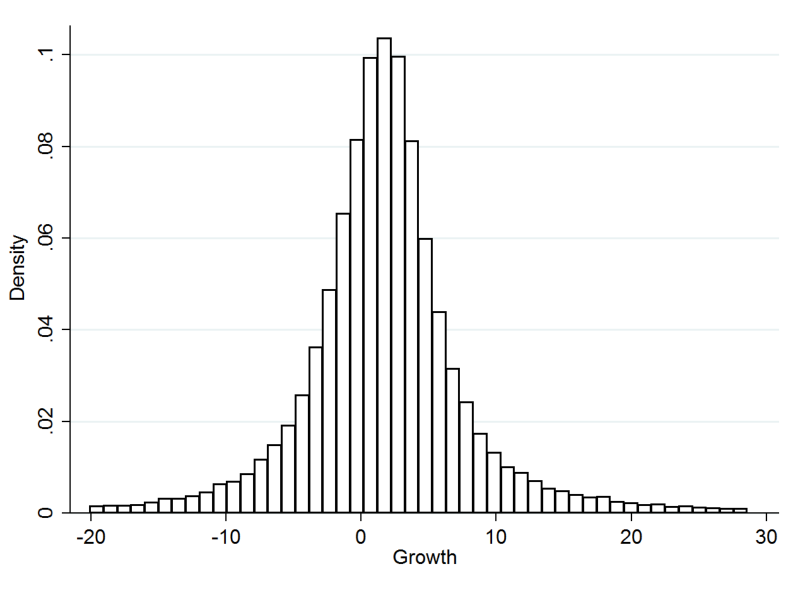


(b)

Figure 2: Distribution of local growth rates including (a) and excluding (b) the 1st and 99th percentile of the distribution. Growth refers to the annual percentage chagne in the level of per capita income in constant U.S. dollars.



(a)



(b)

Figure 3: Number of states that have relaxed restrictions with respect to intrastate branching and interstate banking.

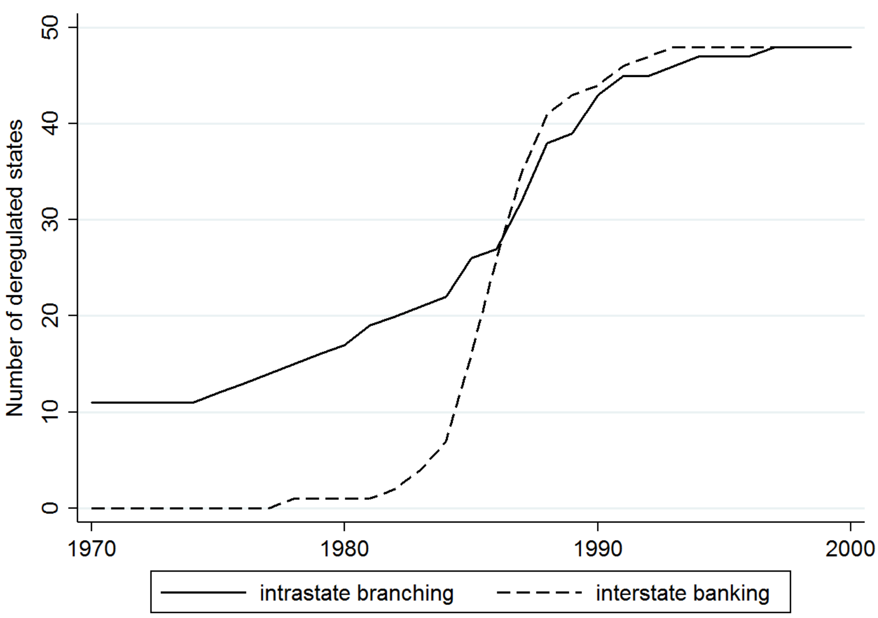


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Table 1: Estimated marginal effects of the base model, SEM and SDM.

Dependent variable: growth	(1) OLS	(2) SEM	(3) SDM	(4) SDEM
intra (direct effect)	0.410**	0.178	0.243*	0.247*
	(0.165)	(0.152)	(0.134)	(0.134)
intra (spillover effect)			0.597	0.365
			(0.492)	(0.315)
inter (direct effect)	0.894***	0.285	0.520***	0.544**
	(0.285)	(0.196)	(0.201)	(0.231)
inter (spillover effect)			2.354***	1.524***
			(0.802)	(0.562)
lambda		0.532***		0.523***
		(0.051)		(0.051)
rho			0.522***	
			(0.051)	
Observations	1,488	1,488	1,488	1,488
Number of states	48	48	48	48
R-squared	0.543	0.537	0.551	0.546
Log-likelihood		-2963.9	-2957.7	-2958.9

Standard errors are clustered by state: *** p<0.01, ** p<0.05, * p<0.1. For the direct effect, the marginal effect of deregulation on growth in the state itself is reported. For the spillover effect, the cumulative marginal effect of deregulation of on growth in all other states is reported. Lambda and rho refer to the spatial correlation coefficient of the error term and of the dependent variable, respectively. The r-squared is calculated as the square of the correlation between actual growth and predicted growth, including the state fixed effect.

Table 2: Robustness checks of the SDM model.

Dependent variable: growth	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
intra (direct effect)	0.252*	0.292**	0.204*	0.256**	0.267	0.313**	0.191	0.255	0.225	0.257
	(0.137)	(0.128)	(0.119)	(0.125)	(0.171)	(0.150)	(0.160)	(0.168)	(0.156)	(0.165)
intra (global spillover)	0.536	0.388	0.647	0.680	1.548**	1.224**	1.470*	1.503**	1.290**	1.478**
	(0.491)	(0.497)	(0.443)	(0.534)	(0.720)	(0.515)	(0.773)	(0.734)	(0.570)	(0.736)
inter (direct effect)	0.536***	0.404**	0.559***	0.551***	0.659***	0.526***	0.612***	0.651***	0.563***	0.661***
	(0.206)	(0.187)	(0.177)	(0.210)	(0.220)	(0.168)	(0.186)	(0.215)	(0.194)	(0.217)
inter (global spillover)	2.438***	2.119***	2.476***	2.566**	2.579**	2.285***	2.405***	2.619***	2.217**	2.662***
	(0.835)	(0.666)	(0.848)	(1.016)	(1.007)	(0.717)	(0.775)	(0.936)	(0.880)	(0.946)
income (t-1)					-2.263***	-1.941***	-1.945**	-2.306***		
					(0.594)	(0.374)	(0.828)	(0.563)		
income-squared (t-1)					0.055***	0.047***	0.048**	0.056***		
					(0.014)	(0.009)	(0.020)	(0.014)		
income (t-2)									-2.068***	-2.283***
									(0.377)	(0.560)
income-squared (t-2)									0.051***	0.056***
									(0.010)	(0.014)
growth (t-1)								0.027		-0.078
								(0.045)		(0.065)
Observations	1,457	1,488	1,488	1,488	1,488	1,488	1,488	1,488	1,488	1,488
R-squared	0.551	0.631	0.553	0.625	0.580	0.577	0.579	0.581	0.579	0.579
Log-likelihood	-2903.6	-2624.9	-2957.1	-2957.4	-2908.8	-2568.6	-2909.8	-2908.1	-2916.3	-2910.3
Number of fips	47	48	48	48	48	48	48	48	48	48

Standard errors are clustered by state: *** p<0.01, ** p<0.05, * p<0.1. For the direct effects, the marginal effect of deregulation on growth in the state itself is reported. For the spillover effect, the cumulative marginal effect of deregulation of on growth in all other states is reported. For the control variables, the estimated coefficients rather than the marginal effects are reported. The R-squared is calculated as the square of the correlation between actual growth and predicted growth, including the state fixed effect.

Table 3: Robustness checks of the SDEM model.

Dependent variable: growth	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
intra (direct effect)	0.270**	0.304**	0.206*	0.254*	0.197	0.262*	0.122	0.187	0.177	0.193
	(0.137)	(0.129)	(0.119)	(0.133)	(0.183)	(0.149)	(0.172)	(0.179)	(0.163)	(0.177)
intra (local spillover)	0.382	0.279	0.387	0.384	0.794**	0.644*	0.748*	0.778**	0.672**	0.762**
	(0.316)	(0.333)	(0.302)	(0.327)	(0.377)	(0.341)	(0.403)	(0.384)	(0.324)	(0.383)
inter (direct effect)	0.563**	0.424**	0.588***	0.564**	0.677**	0.539***	0.648***	0.660**	0.595**	0.667**
	(0.236)	(0.214)	(0.190)	(0.237)	(0.267)	(0.202)	(0.227)	(0.269)	(0.233)	(0.271)
inter (local spillover)	1.601***	1.370***	1.647***	1.575***	1.735***	1.555***	1.654***	1.686**	1.523**	1.697**
	(0.582)	(0.447)	(0.518)	(0.589)	(0.662)	(0.505)	(0.543)	(0.663)	(0.591)	(0.666)
growth (t-1)				-0.034				0.037		-0.113
				(0.063)				(0.049)		(0.076)
income (t-1)					-3.020***	-2.564***	-2.983***	-3.110***		
					(0.782)	(0.475)	(0.793)	(0.734)		
income-squared (t-1)					0.071***	0.060***	0.071***	0.073***		
					(0.018)	(0.011)	(0.019)	(0.017)		
income (t-2)									-2.736***	-3.084***
									(0.455)	(0.715)
income-squared (t-2)									0.065***	0.073***
Observations	1,457	1,488	1,488	1,488	1,488	1,488	1,488	1,488	1,488	1,488
R-squared	0.547	0.625	0.547	0.546	0.579	0.657	0.577	0.579	0.573	0.577
Number of fips	47	48	48	48	48	48	48	48	48	48
Log-likelihood	-2904.5	-2626.2	-2958.2	-2958.0	-2900.0	-2558.8	-2901.1	-2898.8	-2911.0	-2901.3
Number of fips	47	48	48	48	48	48	48	48	48	48

Standard errors are clustered by state: *** p<0.01, ** p<0.05, * p<0.1. For the direct effects, the marginal effect of deregulation on growth in the state itself is reported. For the spillover effect, the cumulative marginal effect of deregulation of on growth in neighboring states is reported. For the control variables, the estimated coefficients rather than the marginal effects are reported. The R-squared is calculated as the square of the correlation between actual growth and predicted growth, including the state fixed effect.

Table 4: Estimated coefficients of the two regressions performed to match local markets.

	(1)	(2)
Dependent variable:	intra	inter
income	-0.850**	0.059
	(0.384)	(0.089)
population	-1.755**	-0.417**
	(0.817)	(0.189)
education	0.736	3.541***
	(1.400)	(0.325)
metro	-2.791***	-1.561***
	(0.665)	(0.154)
constant	1,985.7***	1,981.1***
	(1.761)	(0.408)
Observations	2,271	2,271
R-squared	0.023	0.110

Standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 5: Results of local-level model when the matching of local markets is done on the basis of Principal Component Analysis.

Dependent variable: growth	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
intra	0.324	0.326	0.109	0.291	-0.024	-0.018	-0.293	-0.020	0.128	0.131	-0.118	-0.029
	(0.224)	(0.224)	(0.167)	(0.258)	(0.288)	(0.289)	(0.280)	(0.287)	(0.213)	(0.213)	(0.174)	(0.283)
inter	0.627**	0.627**	0.601**	0.724**	0.793**	0.793**	0.615***	0.817**	0.607**	0.607**	0.580***	0.818**
	(0.292)	(0.292)	(0.264)	(0.339)	(0.336)	(0.336)	(0.225)	(0.351)	(0.282)	(0.282)	(0.213)	(0.341)
growth (t-1)				-0.188***				-0.085***				-0.290***
				(0.013)				(0.010)				(0.020)
income (t-1)					-4.703***	-4.703***	-4.702***	-4.278***				
					(0.381)	(0.381)	(0.380)	(0.357)				
income-squared (t-1)					0.093***	0.093***	0.093***	0.087***				
					(0.011)	(0.011)	(0.011)	(0.010)				
income (t-2)									-2.045***	-2.045***	-2.047***	-3.770***
									(0.142)	(0.141)	(0.141)	(0.329)
income-squared (t-2)									0.040***	0.040***	0.040***	0.071***
									(0.005)	(0.005)	(0.005)	(0.010)
Pair-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Area fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	70,308	70,246	70,308	68,040	70,308	70,246	70,308	68,040	68,040	67,980	68,040	68,040
R-squared	0.634	0.634	0.634	0.659	0.691	0.691	0.691	0.697	0.648	0.648	0.648	0.692

Standard errors are clustered by state: *** p<0.01, ** p<0.05, * p<0.1. Estimates of spillover effects refer to average cumulative spillover effect. For the control variables, the estimated coefficients rather than the marginal effects are reported. The R-squared is calculated as the square of the correlation between actual growth and predicted growth, including the state fixed effect.

Table 6: Results of local-level model when the matching of local markets is done on the basis of OLS with the timing of intrastate branching deregulation as the dependent variable.

Dependent variable: growth	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
intra	0.443**	0.445**	0.176	0.437**	-0.048	-0.049	-0.380	-0.009	0.208	0.209	-0.088	0.003
	(0.184)	(0.184)	(0.182)	(0.216)	(0.324)	(0.324)	(0.346)	(0.305)	(0.184)	(0.184)	(0.191)	(0.287)
inter	0.737**	0.740**	0.688**	0.870**	0.734*	0.735*	0.483*	0.798**	0.642**	0.643**	0.556**	0.805**
	(0.342)	(0.342)	(0.272)	(0.381)	(0.382)	(0.382)	(0.274)	(0.395)	(0.315)	(0.315)	(0.233)	(0.379)
growth (t-1)				-0.191***				-0.085***				-0.296***
				(0.012)				(0.010)				(0.022)
income (t-1)					-5.040***	-5.041***	-5.049***	-4.498***				
					(0.351)	(0.351)	(0.347)	(0.333)				
income-squared (t-1)					0.103***	0.103***	0.103***	0.093***				
					(0.009)	(0.009)	(0.009)	(0.008)				
income (t-2)									-2.058***	-2.057***	-2.066***	-4.038***
									(0.182)	(0.182)	(0.178)	(0.409)
income-squared (t-2)									0.041***	0.041***	0.041***	0.080***
									(0.004)	(0.004)	(0.004)	(0.011)
Pair-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Area fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	70,308	70,246	70,308	68,040	70,308	70,246	70,308	68,040	68,040	67,980	68,040	68,040
R-squared	0.634	0.634	0.634	0.660	0.693	0.693	0.693	0.699	0.647	0.647	0.647	0.693

Standard errors are clustered by state: *** p<0.01, ** p<0.05, * p<0.1. Estimates of spillover effects refer to average cumulative spillover effect. For the control variables, the estimated coefficients rather than the marginal effects are reported. The R-squared is calculated as the square of the correlation between actual growth and predicted growth, including the state fixed effect.

Table 7: Results of local-level model when the matching of local markets is done on the basis of OLS with the timing of interstate banking deregulation as the dependent variable.

Dependent variable: growth	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
intra	0.324	0.326	0.109	0.291	-0.024	-0.018	-0.293	-0.020	0.128	0.131	-0.118	-0.029
	(0.224)	(0.224)	(0.167)	(0.258)	(0.288)	(0.289)	(0.280)	(0.287)	(0.213)	(0.213)	(0.174)	(0.283)
inter	0.627**	0.627**	0.601**	0.724**	0.793**	0.793**	0.615***	0.817**	0.607**	0.607**	0.580***	0.818**
	(0.292)	(0.292)	(0.264)	(0.339)	(0.336)	(0.336)	(0.225)	(0.351)	(0.282)	(0.282)	(0.213)	(0.341)
growth (t-1)				-0.188***				-0.085***				-0.290***
				(0.013)				(0.010)				(0.020)
income (t-1)					-4.703***	-4.703***	-4.702***	-4.278***				
					(0.381)	(0.381)	(0.380)	(0.357)				
income-squared (t-1)					0.093***	0.093***	0.093***	0.087***				
					(0.011)	(0.011)	(0.011)	(0.010)				
income (t-2)									-2.045***	-2.045***	-2.047***	-3.770***
									(0.142)	(0.141)	(0.141)	(0.329)
income-squared (t-2)									0.040***	0.040***	0.040***	0.071***
									(0.005)	(0.005)	(0.005)	(0.010)
Pair-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Area fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	70,308	70,246	70,308	68,040	70,308	70,246	70,308	68,040	68,040	67,980	68,040	68,040
R-squared	0.634	0.634	0.634	0.659	0.691	0.691	0.691	0.697	0.648	0.648	0.648	0.692

Standard errors are clustered by state: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Estimates of spillover effects refer to average cumulative spillover effect. For the control variables, the estimated coefficients rather than the marginal effects are reported. The R-squared is calculated as the square of the correlation between actual growth and predicted growth, including the state fixed effect.