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U.S. Banking Deregulation and Local Economic Growth: A Spatial Analysis

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Abstract

The economic literature has largely ignored the existence of global common factors and local spatial dependence in the assessment of the real effects of U.S. banking deregulation. Motivated by consistency concerns, this study uses spatial econometric models with common factors to analyze the impact of U.S. banking deregulation on county-level economic growth during the 1970–2000 period. We estimate the direct effects of banking deregulation, as well as the size, geographic scope and source of any spatial spillovers. Statistically and economically significant growth effects were experienced by counties in states that deregulated intrastate branching, but only after an initial period without any growth effects. We find no significant growth effects of interstate banking deregulation. During the later half of the sample, intrastate branching deregulation increased the average expected annual growth rates of counties in the deregulated state by about 0.5 p.p. in the long run. Local spatial dependence turns out to be a crucial feature of county-level economic growth, even after common factors are accounted for. As a result, significant spatial spillovers of intrastate branching deregulation were experienced by counties in states surrounding the deregulated state during the later half of the sample. Intrastate branching deregulation increased the average expected annual growth rates of counties adjacent to the deregulated state by about 0.2 p.p. in the long run, while the spillovers to hinterland counties in adjacent states were still about 0.05–0.1 p.p. A comparison to models that ignore common factors or local spatial dependence substantiates our consistency concerns.

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U.S. Banking Deregulation and Local Economic Growth: A Spatial Analysis

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Abstract

The economic literature has largely ignored the existence of global common factors and local spatial dependence in the assessment of the real effects of U.S. banking deregulation. Motivated by consistency concerns, this study uses spatial econometric models with common factors to analyze the impact of U.S. banking deregulation on county-level economic growth during the 1970–2000 period. We estimate the direct effects of banking deregulation, as well as the size, geographic scope and source of any spatial spillovers. Statistically and economically significant growth effects were experienced by counties in states that deregulated intrastate branching, but only after an initial period without any growth effects. We find no significant growth effects of interstate banking deregulation. During the later half of the sample, intrastate branching deregulation increased the average expected annual growth rates of counties in the deregulated state by about 0.5 p.p. in the long run. Local spatial dependence turns out to be a crucial feature of county-level economic growth, even after common factors are accounted for. As a result, significant spatial spillovers of intrastate branching deregulation were experienced by counties in states surrounding the deregulated state during the later half of the sample. Intrastate branching deregulation increased the average expected annual growth rates of counties adjacent to the deregulated state by about 0.2 p.p. in the long run, while the spillovers to hinterland counties in adjacent states were still about 0.05–0.1 p.p. A comparison to models that ignore common factors or local spatial dependence substantiates our consistency concerns.

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1. Introduction

Economic theory predicts that reducing entry barriers into banking markets will foster banking competition, resulting in improved lending conditions for borrowers and a better allocation of savings to profitable investment opportunities (e.g., Besanko and Thakor, 1992). Some studies associate these improvements with an increase in the efficiency and growth of the real economy (e.g., Smith, 1998). Other studies are less positive about the real effects of increased banking competition by showing that banks operating in a highly competitive environment could be inhibited from forming long-term lending relationships with small and medium-sized enterprises (e.g., Petersen and Rajan, 1995). Since such enterprises are important drivers of innovation but are typically dependent on bank credit, a highly competitive banking industry could thus be detrimental to economic growth (Cetorelli and Peretto, 2012).

Given the inconclusiveness of the theoretical literature, the impact of banking deregulation on economic growth is ultimately an empirical matter. The U.S. provides a particularly interesting setting for an empirical investigation of the real effects of intrastate branching and interstate banking deregulation. Intrastate branching restrictions refer to state-level regulations which prohibit or restrict banks from expanding *within* a state by acquiring branches of existing banks or by establishing new branches. Interstate banking restrictions, on the other hand, refer to regulations that prevent out-of-state banks from expanding *across* borders into the regulated state. Until the 1970s, most U.S. states restricted intrastate bank branching in some way. Some states went so far as to prohibit banks from having more than one office ('unit banking'). Interstate banking was even more restricted, with not a single state that allowed out-of-state banks to freely enter its market until the mid 1970s. In the period between 1970 and 1994, both intrastate branching and interstate banking restrictions were relaxed gradually by state legislatures. The passage of the Riegle-Neal Interstate Banking and Branching Efficiency Act in 1994 removed the remaining barriers to interstate banking by 1995 and interstate branching by 1997, although the new law offered states the possibility to opt-in early or to opt-out of the interstate branching provisions. For a detailed historical overview of U.S. banking regulation, we refer to Felsenfeld and Glass (2011).

Jayaratne and Strahan (1996) use the geographically staggered relaxation of U.S. intrastate branching restrictions in the 1970s and 80s as a natural experiment. Their difference-in-difference (DiD) analysis establishes a statistically and economically significant increase in growth rates following deregulation, which cannot be explained by increases in saving and lending. They argue that better banks grew at the expense of their less efficient rivals after deregulation. As a result, the performance of the banking sector as a whole improved; see also Jayaratne and Strahan (1998).

The above findings have been corroborated and extended by various other studies (e.g., Krol and

Svorny, 1996; Black and Strahan, 2002; Freeman, 2002; Strahan, 2003; Wheelock, 2003; Wall, 2004; Dick, 2006; Huang, 2008; Kerr and Nanda, 2009; Rice and Strahan, 2010; Koetter et al., 2012; Amore et al., 2013; Chava et al., 2013; Tewari, 2014; Krishnan et al., 2014; Krishnamurthy, 2015; Goetz et al., 2013, 2016; Dao Bui and Ume, 2020). A related strand of literature has investigated the real effects of banking deregulation in European countries such as Spain and Italy (e.g., Carbó Valverde et al., 2007; Pastor et al., 2017; Bernini and Brighi, 2018).

Due to the existence of global common factors, such as business cycle effects and other aggregate shocks, counties' growth rates are expected to be correlated across space. Furthermore, because nearby economies are typically connected by flows of goods, services, technology, production factors and payments, we also expect to find local spatial dependencies among counties' growth rates (Fingleton and López-Bazo, 2006; Park et al., 2009; McKenzie, 2013). The literature refers to the resulting cross-sectional dependence as 'strong' (global common factors) and 'weak' (local spatial dependence) (Chudik et al., 2011). Ignoring common factors or local spatial dependence in growth rates will result in inconsistent estimates of the impact of banking deregulation on economic performance (Anselin, 1988; Wheelock, 2003; Pesaran, 2015).

In the presence of local linkages among counties' real and financial economies, state-specific economic shocks such as banking deregulation will spill over to the growth rates of counties in surrounding states (Wheelock, 2003; Garrett et al., 2007). As a result, the spatial spillovers of banking deregulation are also relevant from an economic perspective, because their presence would provide support to studies that plead for welfare-enhancing cross-border policy coordination for financial regulation (e.g., Agénor and Pereira da Silva, 2018; VanHoose, 2016). Studies in other fields have already found spatial spillovers for U.S. policies affecting infrastructure investments, local decentralization, defense spending, state deficits and taxation (Cohen and Morisson Paul, 2004; Baicker, 2005; Hammond and Tosun, 2011; Carlino and Inman, 2013; Isen, 2014; Dupor and Guerrero, 2017; Coen-Pirani and Sieg, 2019).

Existing deregulation studies typically use a DiD approach, comparing the growth differences before and after deregulation in a 'treatment' group of deregulated counties (or states) to the growth differences before and after deregulation in a 'control' group of still regulated counties. These studies include time fixed-effects in their DiD regressions, which is a widely accepted approach in the panel data literature to account for global common factors. This approach assumes that the time effects are homogenous across all cross-sectional units. In practice, this may turn out a restrictive assumption. Common factors, by contrast, are allowed to have heterogeneous effects across units (e.g., Bailey et al., 2016a; Shi and Lee, 2017, 2018; Yang, 2021; Bai and Li, 2021). Also local spatial dependence has been largely ignored in the deregulation literature, with exception of Huang (2008). His estimates of the growth effects of U.S. banking deregulation are robust to spatial spillovers to the extent that they only affect the growth rates of counties that border the deregulated state.

The stringent assumptions about cross-sectional dependence cast doubt on the consistency of the reported growth effects of U.S. banking deregulation in the economic literature. Motivated by consistency concerns, the goal of this study is twofold. First, we aim to re-estimate the effects of U.S. banking deregulation on county-level economic growth during the 1970–2000 period while allowing for and distinguishing between global common factors and local spatial dependence in growth rates. Second, if any local spatial dependencies turn out to be present, we will estimate the size, geographic scope and source of the resulting spillover effects.

We use spatial econometric models that do not require a priori assumptions about the existence and the geographic scope of the spillover effects, but permit us to estimate the spillover effects of U.S. banking deregulation in a data-driven way (e.g., Elhorst, 2014). Furthermore, instead of assuming that spillovers are entirely due to spatial autocorrelation in growth rates, our approach also allows banking deregulation in one state to directly impact on the growth rates of counties in nearby states. Our starting point is the dynamic spatial Durbin model (DSD) model with global common factors, which we estimate using the quasi-maximum likelihood (QML) approach of Shi and Lee (2017, 2018). The unobserved 'interactive' effects of the common time factors and their individual loadings induce global (strong) cross-sectional dependence in addition to the local spatial dependence. These interactive effects are viewed as nuisance parameters that may correlate with the observed regressors and are concentrated out in the QML estimation using principal component theory. The DSD model with common factors encompasses several well-known specifications, including the DSD model with standard individual and time fixed-effects and the standard dynamic fixed-effects panel regression model without local spatial dependence. For the sake of comparison, we also estimate these restricted models, for which we use the QML approach of Lee and Yu (2010a). Our county-level analysis has the advantage of minimizing the risk that banking deregulation is driven by expectations of future growth opportunities, thus avoiding the endogeneity issues that are encountered at the state-level (Huang, 2008).

Our spatial models with common factors reveal that statistically and economically significant growth effects were experienced by counties in states that relaxed intrastate branching, but only after an initial period without any growth effects. By contrast, we do not find any significant growth effects of interstate banking deregulation. During the later half of the sample, intrastate branching deregulation increased the average expected annual growth rate of counties in the deregulated state by about 0.5 p.p. in the long run. We show that local spatial dependence is a crucial feature of county-level economic growth, even after common factors are accounted for. As a result, significant spatial spillovers of intrastate branching deregulation were experienced by counties in states surrounding the deregulated state during the later half of the sample. We find no evidence of spatial spillovers outside the channel of spatial autocorrelation. On average, intrastate branching deregulation increased the expected annual growth rates of counties adjacent to the deregulated state by about 0.2 p.p. in the long run. The growth spillovers to hinterland counties in adjacent states were still about 0.05–0.1 p.p., while they turned out economically minor for more remote counties. We confirm the aforementioned consistency concerns by means of a comparison to models that ignore common factors or local spatial dependence.

The rest of this study is structured as follows. Section 2 provides an overview of the relevant literature and formulates the hypotheses that will be tested in the empirical analysis. A description of the data is given in Section 3. Section 4 outlines the econometric framework, as well as our model specification search. The estimation results can be found in Section 5. Lastly, Section 6 concludes. An appendix with additional material is available.

2. Literature review and hypotheses

Based on the literature, this section formulates testable hypotheses about common factors, spatial autocorrelation and the direct and spillover effects of U.S. banking deregulation.

Common factors. Several studies have observed that certain states deregulated their banking sectors around the same time and recognized the possible influence of common business cycle factors (Jayaratne and Strahan, 1996; Freeman, 2002; Wall, 2004; Huang, 2008). This leads to our first hypothesis, stating that counties' growth rates exhibit strong cross-sectional dependence (**H1**).

Spatial autocorrelation. At the U.S. state level, Garrett et al. (2007) find that economic growth rates are spatially autocorrelated. That is, a ceteris paribus increase in a state's growth rate results in an increase in the expected growth rates of surrounding states. We are not aware of comparable county-level studies, but also at the local level we expect the linkages in the real and financial economy to be reflected in spatial autocorrelation. This leads to the hypothesis that there is spatial autocorrelation in local growth rates (**H2**).

Direct and spillover effects. As mentioned in the introduction, many studies use a DiD approach to assess the impact of banking deregulation on economic growth. The significantly positive marginal effects of banking deregulation on the growth rates of the deregulated state found by these studies are consistent with the traditional view that banking deregulation fosters economic growth in the deregulated state through increased competition and efficiency of the banking sector. However, these studies make restrictive assumptions regarding cross-sectional dependence in growth rates, implying that the estimated growth effects are possibly inconsistent (Anselin, 1988; Wheelock, 2003; Pesaran, 2015).

After controlling for common factors and local spatial dependence, we still expect to find significantly positive marginal effects of U.S. banking deregulation on economic growth in the deregulated state (**H3**). We also hypothesize significant growth spillovers of banking deregulation (**H4**) that affect both *adjacent* and *hinterland* counties in states adjacent to the deregulated state (**H5**). Hinterland counties are located in a state adjacent to the deregulated state, but do not border the deregulated state.

Several studies noted that certain states deregulated their banking sectors around the same time and recognized the possible influence of common business cycle factors. They warned that a spurious positive correlation between U.S. banking deregulation and economic performance may arise if business cycle effects are not properly accounted for (Jayaratne and Strahan, 1996; Freeman, 2002; Wall, 2004; Huang, 2008). Hence, the economic relevance of the growth effects of banking deregulation after controlling for common factors remains an empirical matter.

Source of the spillover effects. If banks in a deregulated state offer improved lending conditions to borrowers, this could attract firms and consumers from adjacent counties in still regulated states. In this way, banking deregulation in one state could *directly* affect the growth rates of counties in other states, even in the absence of spatial autocorrelation. However, the literature suggests that the geographic range of such spillover effects will be limited, due to the local nature of U.S. banking markets (e.g., Brevoort and Hannan, 2006; Ho and Ishii, 2011; Knyazeva and Knyazeva, 2012; Bellucci et al., 2013) and the information asymmetries between banks and customers that increase with distance (e.g., Degryse and Ongena, 2004; Chu, 2018). This leads to the hypothesis that any spatial spillovers of banking deregulation on local economic growth are caused by spatial autocorrelation instead of cross-border lending (**H6**).

3. Data

Our sample consists of 48 states of the contiguous United States, excluding Alaska, Hawaii and the District of Columbia. This results in a total of $n = 3,050$ counties observed over a period of $T = 31$ years. The data sources that we used are listed in Table A.1 of the appendix.

3.1. Description

Timing of deregulation. The timing of intrastate branching and interstate banking deregulations during the 1970–2000 period is taken from Demyanyk et al. (2007) and is shown in Table 1. For intrastate branching restrictions, we distinguish between the year that a state (i) permitted the formation of multi-bank holding companies (MBHC) that were allowed to own multiple banks but had to run them separately, (ii) relaxed the restrictions on branching through mergers and acquisitions (M&A), and (iii) allowed branching through the establishment of new branches ('de novo branching'). MBHC were typically allowed in a relatively early stage, often even prior to the start of the sample, while M&A and de novo branching were typically permitted later and often with only a limited time gap between the two. As shown by Strahan (2003), most banks preferred entering new markets by buying existing banks or branches instead of creating new ones. We therefore follow Strahan (2003) and others by choosing the year in which states allowed branching through M&A as 'the' year of intrastate branching deregulation. We also follow this literature by defining the year of interstate banking as the year in which each state first entered into interstate banking agreements with other states.

The passage of the Riegle-Neal Interstate Banking and Branching Efficiency Act in 1994 removed the remaining barriers to interstate banking by 1995 and to interstate branching by 1997, while states were offered the possibility to opt-in early or to opt-out of interstate branching provisions. To identify the long-run effects of interstate banking deregulation, our sample includes several years after 1994 and ends in the year 2000. As part of our empirical analysis, we will run a rolling-window analysis to explore the robustness of our results with respect to the choice of the sample period.

Growth rates. We collect county-level personal income data from the Bureau of Economic Analysis (BEA). These data are available since 1969 and we obtain the data for the period 1969–2000. Our dependent variable is economic growth, which is calculated as the annual logarithmic change in the level of per capita county-level personal income. Nominal income figures are deflated using a national consumer price index taken from the Bureau of Labor Statistics, resulting in the level of per capita county-level personal income expressed in 1983 U.S. dollars. Because the calculation of growth rates leads to a loss of one year of data for each county, our final sample covers the 1970–2000 period. We will later verify the robustness of the deflation procedure by using regional price indices taken from the U.S. Census Bureau.

Sectoral composition. From the BEA we obtain data for the 1970–2000 period on the number of jobs in the industries listed in Table 2. We use these data to calculate county-level employment shares for each industry. In line with Stiroh and Strahan (2003), these shares will be used as control variables to deal with differences in sectoral composition across counties. A formal economic motivation for including employment shares as control variables in growth regressions can be found in Echevarria (1997).

3.2. Sample statistics

Timing of deregulation. Intrastate branching restrictions had already been relaxed in 11 states prior to or as of 1970, 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. Table 1 shows the years of deregulation for each state in our sample, while the upper graph in Figure 1 displays the cumulative percentage of deregulated states over time. In most states, interstate and intrastate deregulation took place in different years. The gap between the two varies across the states, which allows us to disentangle the effects of these two forms of deregulation.

Growth rates. The lower graph in Figure 1 displays income growth over time. To avoid the possibility that the observed outliers distort our estimation results, we winsorize the growth rates at the 99.5% level. We investigate the presence of common factors in local growth rates and consider the exponent of cross-sectional dependence as discussed by Bailey et al. (2016b). This exponent is denoted by a and characterizes the degree of cross-sectional dependence by providing the rate at which the average pair-wise correlation coefficient over all n counties varies with n, for $n \to \infty$. This exponent can be estimated consistently if $1/2 < a \leq 1$. We therefore first test whether $a < 1/2$ using the cross-sectional dependence (CD) test of Pesaran (2015). For $T = O(n^{\epsilon})$, the null hypothesis underlying the CD test is $0 \leq a < (2 - \epsilon)/4$. Hence, for ϵ close to zero (i.e., for T almost fixed for $n \to \infty$), the null hypothesis is $0 \leq a < 1/2$. This range of values corresponds to weak cross-sectional dependence, comparable to the local spatial dependence induced by a binary contiguity matrix.

For our sample with n large relative to T, the pairwise correlation coefficient is 0.31 and the test statistic equals 3666.1 (asymptotic p-value 0.000). Hence, the CD test strongly rejects the null hypothesis of weak cross-sectional dependence in local growth rates. As a next step, we apply the approach of Bailey et al. (2016b) to estimate a and provide both a bias-corrected estimate of a and a robust estimate correcting for serial correlation in the factors and weak cross-sectional dependence in the error term.[1] Values $1/2 \leq a < 3/4$ indicate moderate cross-sectional dependence, consistent with the local spatial dependence induced by an inverse-distance matrix. Values $3/4 < a < 1$ reveal quite strong cross-sectional dependence, while $a = 1$ indicates strong cross-sectional dependence (Elhorst et al., 2021). The latter two cases point to the presence of common factors. Both our estimates of a are close to 1 and thus indicate strong cross-sectional dependence in county-level growth rates. In particular, the robust estimate of a equals 0.97, with 95% asymptotic confidence interval [0.933, 1.004]. Hence, this estimate of a does not significantly differ from 1 and provides strong evidence for the presence of common factors in local growth rates.

[1] Our estimates of a are based on Equation (13) in Bailey et al. (2016b). The robust estimate of a is obtained by estimating (30), where we use four principal components.

Sectoral composition. The average employment shares in the sample are shown in Table 2. Because the industries *Agricultural Services, Forestry and Fishing* and *Mining* contain many missing entries, we only provide their joint employment share (calculated as the residual employment share left by the other industries). For the remaining industries, the number of missing entries is at most a few percent. We impute these values using the state-level employment data, which have no missing values.

Other data. To obtain our final data sample, we match the available growth and deregulation data with a county-level adjacency (or 'first-order binary contiguity') matrix. We also consider a broader definition of 'adjacency' and define two counties to be adjacent if they either share a border *or* belong to the same Metropolitan Statistical Area (MSA). We also construct two county-level inverse-distance matrices, based on cut-off distances of 100 and 150 miles.[2] We thus obtain a balanced county-level sample of 3,050 counties in 48 states, observed during 31 years (1970–2000).

4. Econometric approach

Our starting point is a dynamic spatial Durbin (DSD) model with common factors (Shi and Lee, 2017, 2018; Elhorst et al., 2020). This model allows us to control for both local spatial dependence and common factors and to quantify the size, geographic scope and source of any spatial spillovers in a data-driven way. At the same time, this choice of model permits us to estimate the direct effects of banking deregulation.

4.1. Spatial Durbin model

The DSD model with common factors writes as

$$\boldsymbol{y}_t = \rho \boldsymbol{W} \boldsymbol{y}_t + \tau \boldsymbol{W} \boldsymbol{y}_{t-1} + \boldsymbol{X}_t \boldsymbol{\gamma} + \boldsymbol{D}_t \boldsymbol{\theta} + \delta \boldsymbol{y}_{t-1} + \boldsymbol{Z}_t \boldsymbol{\lambda} + \boldsymbol{\Gamma} \boldsymbol{f}_t + \boldsymbol{\varepsilon}_t \qquad [t = 1, \ldots, T]. \tag{1}$$

for $i = 1, \ldots, n$ and $t = 1, \ldots, T$. Here $\boldsymbol{y}_t = (y_{1t}, y_{2t}, \ldots, y_{nt})'$ contains the per capita real economic growth rates in county $i = 1, \ldots, n$ at time t and $\boldsymbol{\varepsilon}_t = (\varepsilon_{1t}, \varepsilon_{2t}, \ldots, \varepsilon_{nt})'$ is a vector of error terms. We assume that ε_{it} is i.i.d. across i and t, with zero mean and variance σ^2. The remaining terms are explained below.

Spatial, temporal and spatio-temporal lags. Spatial correlation in local growth rates enters the model via the spatial lag component $\rho \boldsymbol{W} \boldsymbol{y}_t$, where $\boldsymbol{W}$ is a spatial weight matrix and ρ the spatial autocorrelation coefficient. By including the term $\rho \boldsymbol{W} \boldsymbol{y}_t$ in the model, we induce local spatial dependence since each county's growth rate will depend on the weighted average growth rate of surrounding counties. If $\boldsymbol{W}$ is an adjacency matrix, an increase in a county's growth rate will be transmitted via adjacent

[2] We only consider cut-off distances of at least 100 miles to ensure that each county has at least one neighbor.

counties' growth rates to more distant hinterland counties in adjacent states, and from there to counties in hinterland states. Counties that are closer neighbors of the deregulated state will receive larger spatial spillovers than more distant counties, ceteris paribus. This creates a realistic setting, given that e.g. commuting flows to adjacent counties are relatively large in comparison to flows to non-adjacent ones. We will run several robustness checks with respect to the choice of the weight matrix $\boldsymbol{W}$, thus allowing for alternative patterns in the way growth is transmitted from one county to another. In particular, we will consider inverse-distance weight matrices with different cut-off distances.

The DSD model in (1) includes a temporal lag of $\boldsymbol{y}_t$ with coefficient δ and a spatio-temporal lag $\boldsymbol{W}\boldsymbol{y}_{t-1}$ with coefficient τ. With $\boldsymbol{W}$ an adjacency or inverse-distance weight matrix, each county's growth rate thus depends on its own lagged growth rate and on the average lagged growth rate of surrounding counties. In this way, we allow for mean reversion effects in local growth rates. Not controlling for county-level business cycle effects could bias the estimated impact of banking deregulation on economic performance (Jayaratne and Strahan, 1996; Freeman, 2002; Wall, 2004; Huang, 2008). Furthermore, the dynamic effects allow the short-run impact of banking deregulation to differ from the long-run impact. This is a convenient property, since several studies have found differences in the transitory and permanent effects of banking deregulation (DeYoung et al., 1998; Stiroh and Strahan, 2003; Freeman, 2005; Chava et al., 2013). Stiroh and Strahan (2003) explain these differences by observing that, over time, more states entered into reciprocal interstate banking agreements, resulting in an increasingly open interstate banking market. More generally, it may take a few years before deregulation starts to affect economic growth.

Deregulation dummies. While most U.S. banking deregulation studies are restricted to the real effects of a single type of banking deregulation, only few studies have analyzed the impact of intrastate branching and interstate banking deregulation jointly. We control for both types of deregulation to make sure that we account for all observable differences between counties. Hence, $\boldsymbol{X}_t$ represents an $n\times 2$ regressor matrix, which includes dummy variables for intrastate branching and interstate banking deregulation. These indicator variables have a value of 1 in the years after deregulation in the county's state – including the year of deregulation itself – and a value of 0 in the years before deregulation. The corresponding coefficient vector is $\boldsymbol{\gamma} = (\gamma_{intra}, \gamma_{inter})'$. In the DSD model, the term $\boldsymbol{X}_t\boldsymbol{\gamma}$ determines the impact of a state's banking deregulation on the growth rates of the counties in that state.

We also permit a state's banking deregulation to directly impact on the growth rates of counties adjacent to the deregulated state. We already mentioned cross-border banking as one of the economic motivations for allowing for such effects in the literature review. We therefore define two dummy variables, contained in the $n \times 2$ matrix $\boldsymbol{D}_t$, indicating whether or not the county is contiguous to a state that already deregulated intrastate branching or interstate banking, respectively. We include the term $\boldsymbol{D}_t\boldsymbol{\theta}$ in the DSD model, where $\boldsymbol{\theta} = (\theta_{intra}, \theta_{inter})'$ represents the coefficient vector. These terms will capture all direct effects from banking deregulation in one state on the growth rates of adjacent counties in other states and not just the effects due to cross-border banking.

The term $\boldsymbol{D}_t\boldsymbol{\theta}$ reflects the 'local' spillovers on economic growth, as opposed to the 'global' spillovers caused by spatial autocorrelation. While global spillovers have the potential to affect a large geographic area, local spillovers only affect adjacent counties' growth rates. In the presence of spatial autocorrelation, however, local spillovers become global spillovers, making it impossible to separate the two. We will nevertheless continue to refer to $\boldsymbol{D}_t\boldsymbol{\theta}$ as the local spillovers. We also note that $\boldsymbol{D}_t\boldsymbol{\theta}$ acts as an exogenous spatial interaction term, which explains why we refer to (1) as a (Dynamic) Spatial Durbin Model.

According to Huang (2008), causality from economic growth to state legislature is unlikely at the county level, because state legislature has to accommodate the interests of all constituencies and not just those of a small group of border counties. Because our dependent variable is county-level instead of state-level economic growth, we thus avoid endogeneity problems with respect to $\boldsymbol{X}_t$ and $\boldsymbol{D}_t$.

Control variables. The $n \times k$ matrix $\boldsymbol{Z}_t$ contains any remaining control variables that are expected to affect economic growth via the coefficient vector $\boldsymbol{\lambda}$, including the county-level employment shares mentioned in Section 3.

Common factors. We assume that the number of unobserved common time factors is a fixed constant r that is much smaller than both n and T (Shi and Lee, 2018). The $r\times 1$ vector $\boldsymbol{f}_t$ contains the r common factors, while $\boldsymbol{\Gamma}$ represents an $n \times r$ matrix of factor loadings. The unobserved interactive effects of the county-specific loadings and the common time factors induce strong cross-sectional dependence. The common factors reflect external factors such as business cycle effects, common shocks or changes in national regulation and may correlate with the observed regressors.

We note that the standard DSD model with county-specific fixed effects $\boldsymbol{\alpha} = (\alpha_1, \alpha_2, \ldots, \alpha_n)'$ and time fixed-effects (FE) $\beta_t\boldsymbol{\iota}_n$ (with $\boldsymbol{\iota}_n$ an n-dimensional column vector of ones) is a special case of the DSD model with common factors. It is obtained by setting $r = 2$, $\boldsymbol{\Gamma} = (\boldsymbol{\alpha} \quad \boldsymbol{\iota}_n)$ and $\boldsymbol{f}_t = (1 \quad \beta_t)'$. Because the interactive effects in (1) include individual and time FE as a special case, explanatory variables that are county-specific but time-invariant variables and time-specific but county-invariant will not be included in the DSD model.

4.2. Model estimation

We consider three different versions of the DSD model with common factors in (1). First, we estimate a standard dynamic FE panel regression that ignores local spatial dependence and restricts the common factors to county and time FE; i.e. we set $\rho = \tau = 0$, $r = 2$, $\boldsymbol{\Gamma} = (\boldsymbol{\alpha} \quad \boldsymbol{\iota}_n)$ and $\boldsymbol{f}_t = (1 \quad \beta_t)'$ in

(2). Second, we leave ρ and τ unrestricted, but continue to restrict the common factors to county and time FE. This yields the standard DSD-FE model. We estimate these two restricted versions of the DSD model by means of Quasi-Maximum Likelihood (QML) using the transformation approach of Lee and Yu (2010b), followed by their bias correction. The asymptotic properties of this QML estimator rely on the assumption that $n/T^3 \to 0$. The bias correction ensures that the asymptotic distribution is properly centered. Throughout, we use the sandwich estimator for the asymptotic covariance matrix, which provides robustness to deviations from normality of the model errors.

The third version of (1) that we estimate is the unrestricted DSD model with common factors. To estimate this model, Ciccarelli and Elhorst (2018) extend the approach of Bailey et al. (2016a) to include both serial dynamics and explanatory variables. Their approach consists of adding cross-sectional averages of the dependent and explanatory variables as control variables to the DSD model, with heterogeneous coefficients across cross-sectional units. This approach is not computationally feasible in our case, since the number of explanatory variables and the cross-sectional dimension are large. We therefore use the QML approach of Shi and Lee (2017, 2018). This method views the common factors and their loadings as nuisance parameters, which are concentrated out in the QML estimation procedure using principal component theory. The asymptotic properties of the resulting estimator and the associated bias correction rely on $T/n \to \kappa^2 > 0$.

To estimate the DSD model with common factors, we have to determine the number of factors r. Several methods have been proposed in the literature, including information criteria (e.g., Shi and Lee, 2017). Because Shi and Lee (2017) find that different criteria may yield different estimates for the number of factors, we report estimation results for different values of r for the sake of robustness.

We perform a specification search to determine the most appropriate spatial weight matrix and the explanatory variables to include. Following Stakhovych and Bijmolt (2009), we will use the Bayesian Information Criterion (BIC) for the model selection procedure. As an additional measure of the goodness-of-fit of the estimated models, we will report the (pseudo) R^2 discussed by (Elhorst, 2014, p. 59). This R^2 has a similar meaning as the R^2 in an OLS regression.

4.3. Marginal effects

In the DSD model of (1), the marginal effects of intrastate branching and interstate banking deregulation vary by county and state: counties closer to the deregulated state are expected to receive more of the growth spillovers of adjacent states' banking deregulation. Furthermore, the model allows for short-run (or immediate) and long-run (or permanent) marginal effects due to the presence of a temporal and spatio-temporal lag in the DSD model. More specifically, the $n \times 1$ vector of short-run marginal effects of banking deregulation in state $j = 1, \ldots, N$ on the growth rates of county $i = 1, \ldots, n$ is given by

$$(\boldsymbol{I}_n - \rho \boldsymbol{W})^{-1}[\gamma_\ell \boldsymbol{e}_j + \theta_\ell \widetilde{\boldsymbol{e}}_j] \qquad [\ell = intra, inter].$$

Here $\boldsymbol{I}_n$ denotes the $n \times n$ identity matrix and $\boldsymbol{e}_j$ the $n \times 1$ vector with a value 1 for the elements corresponding to the counties in state j and 0 otherwise. Similarly, $\widetilde{\boldsymbol{e}}_j$ is the $n \times 1$ vector with a value 1 for the elements corresponding to the counties that share a border with state j and 0 otherwise. We calculate the short-run marginal effects for all $j = 1, \ldots, N$ and proceed in a similar way for the long-run marginal effects. This yields the $n \times N$ matrices of short-run and long-run marginal effects, for $\ell = intra, inter$:

$$\boldsymbol{E}_\ell^{sr} = (\boldsymbol{I}_n - \rho \boldsymbol{W})^{-1}[\gamma_\ell \boldsymbol{I}_{n\times N} + \theta_\ell \widetilde{\boldsymbol{I}}_{n\times N}]; \qquad \boldsymbol{E}_\ell^{lr} = \{(1-\delta)\boldsymbol{I}_N - (\rho + \tau)\boldsymbol{W}\}^{-1}[\gamma_\ell \boldsymbol{I}_{n\times N} + \theta_\ell \widetilde{\boldsymbol{I}}_{n\times N}],$$

with $\boldsymbol{I}_{n\times N} = [\boldsymbol{e}_1\, \boldsymbol{e}_2 \ldots \boldsymbol{e}_N]$ and $\widetilde{\boldsymbol{I}}_{n\times N} = [\widetilde{\boldsymbol{e}}_1\, \widetilde{\boldsymbol{e}}_2 \ldots \widetilde{\boldsymbol{e}}_N]$. The columns of the matrices $\boldsymbol{E}_\ell^{sr}$ and $\boldsymbol{E}_\ell^{lr}$ refer to the state where deregulation is adopted and the rows to the counties whose growth rates are potentially affected by it. Element (i, j) of the marginal effects matrices refers to the ceteris paribus (short-run or long-run) impact of (intrastate branching or interstate banking) deregulation in state j on county i.

In line with Elhorst (2014, Sec. 2.7), we use summary measures to capture the marginal direct and spillover effects of banking deregulation on local economic growth. To obtain the summary measures, we first calculate the average marginal direct effect of deregulation experienced by the counties in the deregulated state. Subsequently, the average is taken over all states. In a similar way, we construct summary measures for the marginal spillover effects experienced by counties adjacent to a deregulated state. We also report summary measures for the marginal spillover effects experienced by hinterland counties in adjacent states and by counties in hinterland states.[3] To obtain standard errors and p-values for the summary measures, we obtain 1,000 draws from the QML estimates' asymptotic distribution, calculate the implied summary measures and obtain the implied asymptotic standard errors and p-values (Elhorst, 2014, p. 24). Appendix A.2 provides formal expressions for each of the summary measures.

4.4. Model selection

We will now present the final model specification that emerged from a model selection procedure applied to the DSD model in (1). We already mentioned that several studies distinguish between the short-run and long-run effects of banking deregulation (DeYoung et al., 1998; Stiroh and Strahan, 2003; Freeman, 2005; Chava et al., 2013). Instead of using a single matrix $\boldsymbol{X}_t$ with deregulation dum-

[3]Hinterland states are defined as states that do not share a border with the deregulated state.

mies in (1), we therefore include two matrices of county-level deregulation indicators: $\boldsymbol{X}_t^{sr}$ (where 'sr' stands for 'short run') and $\boldsymbol{X}_t^{lr}$ ('long run'). As before, the first column of these matrices corresponds to intrastate branching deregulation and the second to interstate banking deregulation. With $\ell = 1, 2$ (where 1 refers to intrastate branching and 2 to interstate banking deregulation), the matrices' elements are defined as $\boldsymbol{X}_t^{sr}(j, \ell) = 1$ if county j's state had type ℓ deregulation in year t, $t-1$ or $t-2$ and $\boldsymbol{X}_t^{sr}(j, \ell) = 0$ otherwise. Similarly, we define $\boldsymbol{X}^{lr}(j, \ell) = 1$ if county j's state had type ℓ deregulation in year $t - 3$ or earlier and $\boldsymbol{X}^{lr}(j, \ell) = 0$ otherwise. Hence, we allow the impact of deregulation on growth during the first three years to differ from the impact after four years and later. In a similar way, we define the matrices $\boldsymbol{D}_t^{sr}$ and $\boldsymbol{D}_t^{lr}$. The choice of the threshold level involves a trade-off between estimation accuracy and the ability to distinguish short-run and long-run effects. A period of three years turned out to strike a proper balance between the two.

The terms $\boldsymbol{D}_t^{sr}\boldsymbol{\theta}^{sr}$ and $\boldsymbol{D}_t^{lr}\boldsymbol{\theta}^{lr}$ represent the short-run and long-run local spillover effects of deregulation as discussed in Section 4. A specification search based on the BIC shows that adding these terms increases the value of the BIC while leaving the (pseudo) R^2 virtually unaffected. We therefore only report estimation results for the more parsimonious DSD model that does not contain the local spillover terms. The resulting model is known as a Dynamic Spatial Autoregressive (DSAR) model.

In our final specification, the matrix of control variables $\boldsymbol{Z}_t$ contains the employment share variables for the industries mentioned in Section 3, thus controlling for differences in sectoral composition across counties. We will run a robustness check by including a county-level educational attainment index as an additional control variable.

The models with the lowest BIC value turn out to be the ones that use the inverse-distance matrix with a 100-mile cut-off distance instead of the two adjacency matrices described in Section 3. For cut-off distances larger than 100 miles, the estimated parameters do not fall in the set of stationary solutions as described by Lee and Yu (2013, p. 371).[4]

Our modelling choices result in the following DSAR model and associated matrices of short-run and long-run marginal effects:

$$\boldsymbol{y}_t = \boldsymbol{X}_t^{sr}\boldsymbol{\gamma}^{\boldsymbol{sr}} + \mathbf{X}_t^{lr}\boldsymbol{\gamma}^{lr} + \rho\boldsymbol{W}\boldsymbol{y}_t + \tau\boldsymbol{W}\boldsymbol{y}_{t-1} + \delta\boldsymbol{y}_{t-1} + \boldsymbol{Z}_t\boldsymbol{\lambda} + \boldsymbol{\Gamma}\boldsymbol{f}_t + \boldsymbol{\varepsilon}_t; \tag{2}$$

$$\boldsymbol{E}_\ell^{sr} = \gamma_\ell^{sr}(\boldsymbol{I}_n - \rho\boldsymbol{W})^{-1}\boldsymbol{I}_{n\times N}; \quad \boldsymbol{E}_\ell^{lr} = \gamma_\ell^{lr}[(1-\delta)\boldsymbol{I}_n - \rho\boldsymbol{W}]^{-1}\boldsymbol{I}_{n\times N}, \tag{3}$$

with $t = 1, \ldots, T$ and $\ell = intra, inter$ and $\boldsymbol{W}$ an inverse-distance matrix with a 100-mile cut-off distance.

[4] We also find non-stationary outcomes for models based on inverse-distance matrices with no cut-off point, even if we square the inverse distance.

5. Estimation results

This section discusses the estimation results based on the DSAR model in (2), followed by several robustness checks.

5.1. Standard dynamic fixed-effects panel regression

We start with the full-sample estimation results for the standard dynamic FE panel regression that restricts the common factors to county and time FE and ignores any local spatial dependence; i.e., we set $\rho = \tau = 0$, $r = 2$, $\boldsymbol{\Gamma} = (\boldsymbol{\alpha} \quad \boldsymbol{\iota}_n)$ and $\boldsymbol{f}_t = (1 \quad \beta_t)'$ in (2). We estimate the resulting model using the QML procedure with bias correction of Lee and Yu (2010b); i.e., we estimate the standard dynamic FE panel regression as a restricted DSAR model.

The first column of Table 3 (captioned 'FE') reports the estimated marginal direct effects ('marginal DE'), with the short-run (SR) effects in the first horizontal panel and long-run (LR) effects in the second horizontal panel. Because the model ignores any local spatial dependencies, there are only direct effects of banking deregulation on the counties in the deregulated state and no spillovers to counties in other states. Furthermore, the marginal direct effects of banking deregulation are the same across states and counties. On average, intrastate branching deregulation increases the annual expected growth rate of counties in the deregulated state by 0.16 p.p. in the SR and by 0.32 p.p. in the LR, ceteris paribus. These effects are significant at the 10% and the 1% level, respectively. For interstate banking deregulation, these SR and LR average marginal direct effects are more substantial in magnitude and equal 0.65 p.p and 1.15 p.p., respectively. Both estimates are significant at the 1% level.

The third horizontal panel of Table 3 reports the coefficient estimate of the temporal lag, $\hat{\delta}$. Because $|\hat{\delta}| < 1$, the growth process is stationary. The fourth horizontal panel reports goodness-of-fit measures, as well as test results for the model residuals. The panel regression's (pseudo) R^2 equals 0.076, indicating that the model explains a relatively modest fraction of the total variation in local growth rates. The null hypothesis of weak cross-sectional dependence in the model residuals is not rejected by the CD test, while the estimates of the exponent of cross-sectional dependence are both less than 0.5. These outcomes suggest that the residuals of the panel regression are only subject to weak cross-sectional dependence, but this does not necessarily imply that the model is correctly specified. If common factors and local spatial dependence are not properly accounted for, the coefficient estimates will typically suffer from omitted-variables bias, which will also affect the properties of the model residuals.

5.2. *Local spatial dependence*

We now leave ρ and τ unrestricted to allow for local spatial dependence, but continue to restrict the common factors in (2) to county and time FE. Again we estimate this model using the QML approach with bias correction of Lee and Yu (2010b). The second column of Table 3 (captioned 'DSAR-FE') shows the full-sample estimation results for this model, including the summary measures for the marginal direct effects ('marginal DE') and the marginal spillover effects ('marginal SE').

The DSAR-FE model does not find any significant SR effects of banking deregulation. In the LR, the average marginal direct effect of intrastate branching on counties in the deregulated state is significant and equal to 0.31 p.p. The average marginal effect on neighboring counties in adjacent states is 0.12 p.p. in the LR, while this effect equals 0.03 p.p. for hinterland counties in adjacent states. The average marginal effect on counties in hinterland states is economically minor, although statistically significant. We find no significant LR growth effects of interstate banking deregulation.

The spatial and spatio-temporal lag coefficients ρ and τ are significant at the 1% level, both individually and jointly. Hence, the standard dynamic FE panel regression model that ignores these terms is misspecified. The estimated parameters fall the set of stationary solutions as described by (Lee and Yu, 2013). With $\hat{\rho} = 0.84$ ($\hat{\tau} = 0.18$), a county's expected growth rate in a particular year increases by 0.84 p.p. (0.18 p.p.) following a ceteris paribus increase of 1 p.p. in the same-year (previous-year) weighted-average growth rate of the counties that are located within a range of 100 miles. The estimate of the temporal lag coefficient is $\hat{\delta} = -0.23$, showing that high growth in one year is followed by lower expected growth in the next year, ceteris paribus.

The null hypothesis of weak cross-sectional dependence of the model residuals is not rejected by the CD test, while the bias-corrected and robust estimates of the exponent of cross-sectional dependence are both less than 0.5. The DSAR-FE's (pseudo) R^2 of 0.40 is much higher than the 0.076 we found for the standard dynamic FE panel regression, indicating that a substantially larger part of the variance is explained if local spatial dependence is included in the model.

Table 3 is limited to summary measures, which are averages calculated over different counties and states. To gain insight in the variation across both counties and states, we also provide information about the size and significance of the *individual* marginal effects for the counties used in each of the summary measures. These additional results are displayed in the first panel of Table 4 ('DSAR-FE').

For each of the four groups of counties used in the summary measures, the first panel of Table 4 ('DSAR-FE') reports the 2.5% and 97.5% quantiles of each type of marginal effect. We obtain these quantiles over the counties for which a statistically significant individual marginal effect is found. Furthermore, we also provide the percentage of counties for which a significant marginal effect is found. Throughout, we use a significance level of 10%.

Only the LR marginal effects of intrastate branching deregulation turn out significant for individual counties. For all counties in a deregulated state, the experienced LR marginal direct effects of intrastate branching deregulation are significant at the 10% level ('100%'), with an interval bounded by 0.25 p.p. (2.5% quantile) and 0.54 p.p. (97.5% quantile). The experienced spillover effects are significant for all counties adjacent to a deregulated state and range between 0.06 p.p. (2.5% quantile) and 0.23 p.p. (97.5% quantile). For 88.3% of the hinterland counties in adjacent states, the experienced spillover effects are significant, with an effect that ranges between 0.00 p.p. (2.5% quantile) and 0.08 p.p. (97.5% quantile). Hence, for hinterland counties the experienced spillover effect is economically small. The spillover effect is statistically significant for only 6.2% of the counties in hinterland states and its magnitude is economically negligible. We conclude that the summary measures give an accurate indication of the individual marginal direct and spillover effects for each group of counties.

5.3. Common factors

We continue to leave ρ and τ unrestricted and estimate the DSAR model in (2) with $r = 2, 3, 4, 5$. We estimate these models using the QML approach of Shi and Lee (2017, 2018), which means that the interactive effects are concentrated out in the QML estimation using principal component theory. The third and fourth column of Table 3 (captioned 'DSAR-CF2' and 'DSAR-CF4') show the full-sample estimation results for the models with two and four common factors, including the summary measures for the marginal direct effects ('marginal DE') and the marginal spillover effects ('marginal SE'). Because both models yield similar outcomes, our discussion of the results focuses on the DSAR-CF2 model.[5]

The DSAR-CF2 model does not find any significant SR effects of banking deregulation. In the LR, only the effects of interstate banking deregulation are significant at the 5% level. These effects are all negative. The LR average marginal direct effect of interstate banking deregulation on counties in the deregulated state is significant and equal to −0.24 p.p. The average marginal effect on neighboring counties in adjacent states is −0.09 p.p. in the LR, while this effect is −0.02 p.p. for hinterland counties in adjacent states. The LR average marginal spillover effect is economically negligible for counties in hinterland states. We will later see that the LR negative effects of interstate banking deregulation are not robust over time.

The temporal, spatial and spatio-temporal lag coefficients are of a similar magnitude and significance as in the DSAR-FE model. This result is robust to the inclusion of more common factors. Hence, local spatial dependence is a crucial feature of county-level economic growth, even if common factors have been accounted for.

[5]The DSAR models with two to five common factors yield similar estimation results. To save space, we only report the estimation results for two and four common factors.

The pairwise correlation coefficient corresponding to the residuals in the DSAR-CF2 model is 0.011, which results in a rejection of the null hypothesis of weak cross-sectional dependence by the CD test at the 1% level. The bias-corrected and robust estimates of the exponent of cross-sectional dependence a are less than 0.75, suggesting that (virtually) no common factors are left. The DSAR-CF2 model's (pseudo) R^2 of 0.60 indicates that a considerably larger part of the variance is explained if common factors are included in the model in addition to local spatial dependence.

We consider two extensions. We use county-level data on education from the U.S. Census Bureau for the years 1970, 1980 and 1990 as an additional control variable. These data distinguish between four levels of educational attainment. These four levels correspond to inhabitants 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 use the percentage of inhabitants with at least four years of college as a measure for county-level human capital, which is considered a determinant of economic growth (e.g., Galor and Tsiddon, 1997). We use the percentages in 1970 to construct the county-level control variable for education for the years 1970–1980, the percentages in 1980 for the years 1980–1990 and the percentages in 1990 for the years 1990–2000. As expected, the resulting control variable turns out significantly positive in the DSAR model. However, adding this additional control variable leads to an increase in the BIC (reflecting a poorer fit), while leaving the (pseudo) R^2 virtually unaffected. Furthermore, a regional consumer price index is available from the Bureau of Labor Statistics for four main regions as defined by the U.S. Census Bureau.[6] We use these four indices to deflate the county-level personal income data (replacing the national index) and re-estimate all models. Also this change hardly affects the estimation results and we therefore do not report the estimation results.

The last model extension we consider allows for spatial autocorrelation in the error term of the DSAR-CF2 model. This error term is assumed to be of the form

$$\boldsymbol{\varepsilon}_t = \zeta \boldsymbol{W} \boldsymbol{\varepsilon}_t + \boldsymbol{u}_t, \tag{4}$$

Here u_{it} is i.i.d. across i and t, with zero mean and variance σ^2. Together with (2), this specification of the error term yields a so-called General Nesting Spatial (GNS) model (Elhorst, 2014). The estimation results for the GNS-CF2 model, which continue to be based on the approach of Shi and Lee (2018), are given in the fourth column of Table 3. The estimate of ζ equals -0.3719 with p-value 0.0000. The estimated effects of interstate banking are a bit larger in magnitude and more significant than before, but the estimation results are still similar to what we found for the DSAR-CF2 and DSAR-

[6]To our best knowledge, a state-level consumer price index is not available at the website of the Bureau of Labor Statistics.

CF4 models. The pairwise correlation coefficient corresponding to the residuals in the GNS-CF2 model drops to 0.007 after controlling for spatial autocorrelation in the error term, while the bias-corrected and robust estimates of the exponent of cross-sectional dependence a become 0.66 and 0.70, respectively. The latter two estimates are significantly smaller than 0.75, indicating that the model residuals no longer contain common factors.

To gain insight into the variation across both counties and states, Table 3 provides information about the size and significance of the *individual* marginal effects for the counties used in each of the summary measures. These additional results are displayed in Table 4 ('DSAR-CF2', 'DSAR-CF4', 'GNS-CF2'). Again we conclude that the summary measures give an accurate indication of the individual marginal direct and spillover effects for each group of counties.

We conclude that both the standard dynamic FE panel regression and the DSAR-FE model overestimate the size and the significance of the direct effects of U.S. banking deregulation in comparison to the GNS-CF2 model and the other two models with common factors. If we interpret the spatial terms and common factors as omitted variables, we can view the overestimation as the consequence of an omitted variables bias. We chose the GNS-CF2 model as our final specification.

5.4. Robustness over time

We estimate the GNS-CF2 model over time by means of a rolling-window analysis. Hence, we estimate the model over the years $1970 + k, \ldots, 1970 + k + \ell$ for a fixed value of ℓ (the rolling-window with) and $k = 0, \ldots, 30 - \ell$. Too small a rolling-window width will result in erratic patterns in the estimates over time, while too large a window width will increase the risk of structural instability. Furthermore, the effects of banking deregulation can only be identified if there is sufficient variation in the years of deregulation across states. Eyeballing makes clear that a window width of $\ell = 20$ years strikes a good balance, yielding 11 subsequent sample periods over which the GNS-CF2 model is estimated.

Prior to running the rolling-window estimations, we apply the CD test to the subsample growth rates and also obtain the two estimates of the exponent of cross-sectional dependence. The resulting outcomes confirm our findings for the full-sample period and provide very strong evidence for the presence of common factors in growth rates.

The summary measures of the direct and spillover effects based on the rolling-window estimations are visualized in Figure 2. In each figure, the horizontal axis displays the mid-year of the 20-year rolling-window interval. We show both point estimates (solid lines) and pointwise 95% confidence intervals based on simulation from the asymptotic distribution of the QML estimator. To assess the significance of the estimated effects, we use the horizontal zero line. If this line is outside the 95% confidence interval for a certain rolling-window period, the estimated effects are significant at the 5%

level; otherwise they are not significantly different from 0.

The rolling-window estimates in Figure 2 reveal substantial temporal variation in the estimated summary measures of the direct and spillover effects. Yet this variation is relatively small in comparison to the estimation uncertainty as reflected by the confidence intervals. In particular, we observe that the significance of the direct and spillover effects of banking deregulation varies over time. In both the SR and the LR, the effects of intrastate branching deregulation become significant as of mid-year 1987. Hence, we establish significant direct and spillover effects of intrastate branching deregulation for samples as of 1977. Apparently, some time was needed before intrastate branching deregulation started to have an effect.

For interstate banking, the SR and LR effects are never significant, with exception of a single, early period during which the LR effects of interstate banking were significantly negative. Such negative effects were also found for the full sample, but are not robust over time.

The rolling-window estimates reveal some time variation in the estimates of δ, ρ, τ and ζ, but all estimates keep the same sign and remain significant over time.[7] We note that the rolling-window estimates of the coefficients fall in the stationary range as specified by Lee and Yu (2013). Furthermore, the rolling-window outcomes of the CD test applied to the model residuals are similar to what we found for the full-sample period. The same holds for the rolling-window estimates of the exponent of cross-sectional dependence.

To substantiate our consistency concerns, we compare the rolling-window estimation results for the GNS-CF2 model to those of the DSAR-FE model and the standard dynamic FE panel regression. The rolling-window estimates for the latter two models are shown in Appendix A.3. We conclude that the DSAR-FE model, which allows for local spatial dependence but restricts the common factors to county and time FE, overestimates the magnitude and significance of the direct and spillover effects of intrastate branching early in the sample, especially in the long run. The two models agree that interstate banking deregulation had no significant growth effects. For the standard dynamic FE panel regression model, which ignores local spatial dependence and restricts the common factors to county and time FE, the tendency to overestimate the magnitude and significance of the direct effects is even larger. The additional overestimation of the magnitude and significance of the direct effects of interstate banking is particularly apparent, especially in the short run. Both comparisons emphasize the importance of accounting for both common factors and local spatial dependence.

[7] These rolling-window estimates are not shown to save space.

5.5. *Hypothesis testing*

The outcomes of the CD test and the estimates of the exponent of cross-sectional dependence reveal strong cross-sectional dependence in county-level growth rates, confirming hypothesis **H1**. The significant spatial autocorrelation in local growth rates over all sample periods confirms hypothesis **H2**. For sample periods after 1977, we find significantly positive marginal effects of intrastate branching deregulation on economic growth in the deregulated state, both in the short run and the long run. Hence, we confirm hypothesis **H3** for intrastate branching but not for interstate banking deregulation. For the same period, we also find that counties bordering the deregulated state experienced statistically and economically significant growth spillovers of intrastate branching deregulation, confirming hypothesis **H4**, but only for intrastate branching. For hinterland counties in adjacent states the spillover effects of intrastate branching deregulation were statistically significant, but economically modest. This means that we confirm hypothesis **H5** for intrastate branching, but mostly in a statistical sense. We find no evidence that banking deregulation in one state affected the growth rates of counties in other states outside the channel of spatial autocorrelation. That is, the model that allows for local spillovers has a higher BIC than the model without such effects (and virtually the same (pseudo) R^2), indicating a poorer fit. The lack of evidence for local spillovers confirms hypothesis **H6** about the source of the spatial spillovers.

6. Conclusion

Our study is the first to estimate the real effects of U.S. banking deregulation after controlling for global common factors and local spatial dependence in county-level growth rates. We have shown that intrastate branching deregulation started to have statistically and economically significant growth effects on county-level growth rates after an initial period without such effects. During this period, intrastate branching deregulation increased the average expected annual growth rate of counties in the deregulated state by about 0.5 p.p. in the long run.

Spatial autocorrelation in growth rates turned out to be a crucial feature of county-level economic growth, even after common factors were accounted for. As a result, significant spatial spillovers of intrastate branching deregulation were experienced by counties in states surrounding the deregulated state during the later half of the sample. On average, intrastate branching deregulation increased the expected annual growth rates of counties adjacent to the deregulated state by about 0.2 p.p. in the long run. The growth spillovers to hinterland counties in adjacent states were still about 0.05–0.1 p.p., while they turned out economically minor for more remote counties. We have found no evidence of spatial spillovers outside the channel of spatial autocorrelation.

Virtually no significant growth effects were found for interstate banking deregulation, which is consistent with several other empirical studies (e.g., Jayaratne and Strahan, 1996; Beck et al., 2010).

Econometrically speaking, the presence of both common factors and local spatial dependence implies that models ignoring these forms of cross-sectional dependence will usually produce inconsistent estimates of the real effects of banking deregulation. A comparison of our estimation results to the outcomes of models that ignore common factors or local spatial dependence has substantiated these consistency concerns. From an economic perspective, our finding of significant spatial spillovers of banking deregulation lends support to studies that plead for welfare-enhancing cross-border policy coordination for financial regulation (e.g., Agénor and Pereira da Silva, 2018; VanHoose, 2016).

We have estimated the spatial spillovers of U.S. banking deregulation using models where local spatial dependence and unobserved common factors capture the real and financial linkages among counties' economies. Alternatively, one could explicitly specify the nature of the linkages among regional banking sectors and analyze the role of these linkages in the geographic transmission of the effects of banking deregulation. Such an approach would extend the strand of literature that analyzes the role of cross-border linkages between banking systems in transmitting local developments across borders (e.g., Tonzer, 2015). We leave this as a topic for future research.

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Figure 1: Percentage of deregulated states (upper graph) and income growth (lower graph) during the years 1970–2000

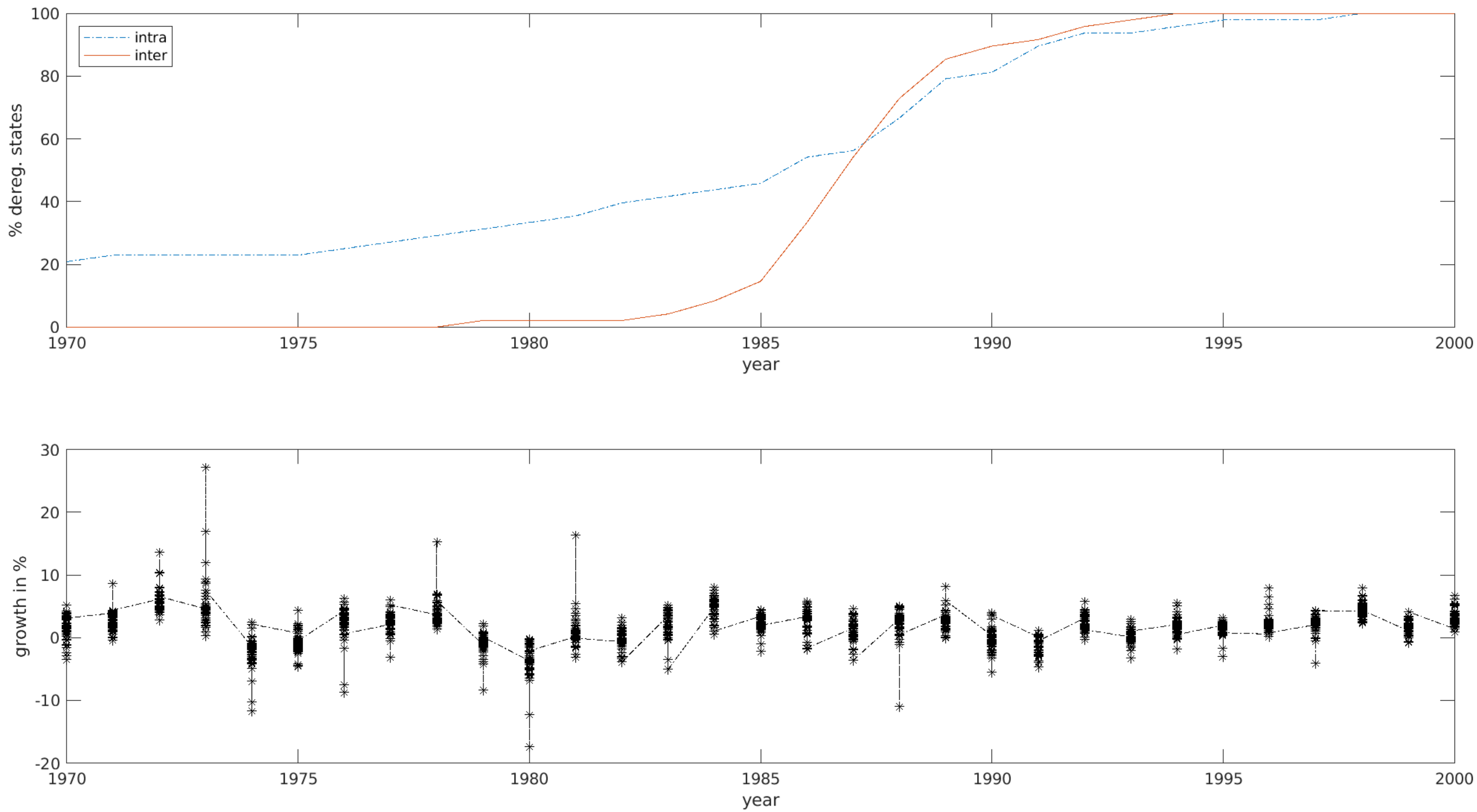
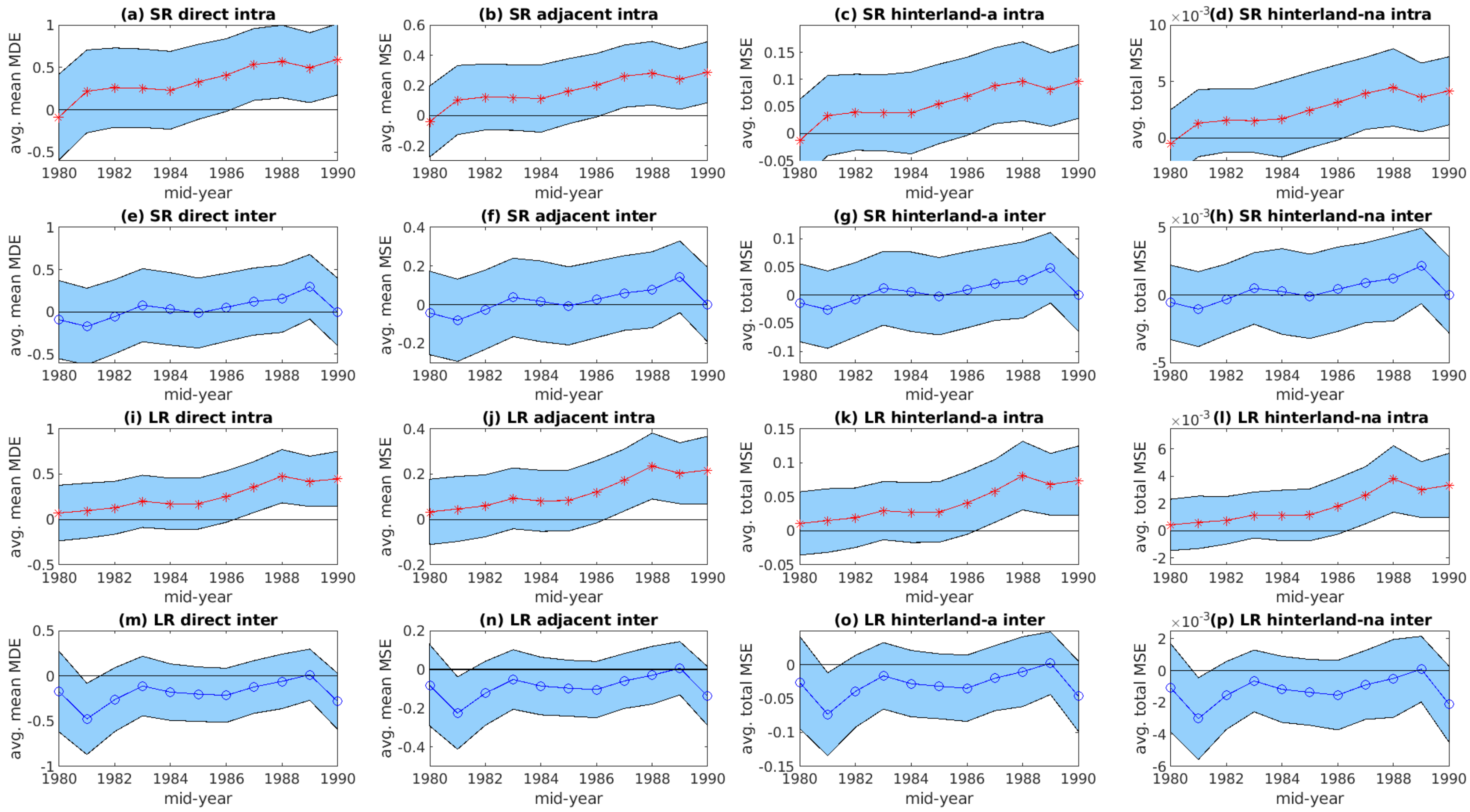


Figure 2: Rolling-window estimates for the GNS-CF2 model



Notes: This figure visualizes the summary measures for the direct effects of banking deregulation on the counties in the deregulated state ('direct'), the spillovers to counties adjacent to the deregulated state ('adjacent'), the spillovers to hinterland counties in adjacent states ('hinterland-a') and the spillovers to counties in non-adjacent states ('hinterland-na'). Abbreviations used in the graphs: SR = short run, LR = long run, MDE = marginal direct effect, MSE = marginal spillover effect. For all effects, the solid lines (with a circle or star as a marker) represent the point estimates, while the intervals in light blue constitute a pointwise 95% asymptotic confidence interval.

Table 1: Timing of deregulation

State	*Intra M&A*	*Inter*	*State*	*Intra M&A*	*Inter*	*State*	*Intra M&A*	*Inter*	*State*	*Intra M&A*	*Inter*
Alabama	1981	1987	Iowa	1997	1991	Nebraska	1985	1990	Rhode Island	1960	1984
Arizona	1960	1986	Kansas	1987	1992	Nevada	1960	1985	South Carolina	1960	1986
Arkansas	1994	1989	Kentucky	1990	1984	New Hampshire	1987	1987	South Dakota	1960	1988
California	1960	1987	Louisiana	1988	1987	New Jersey	1977	1986	Tennessee	1985	1985
Colorado	1991	1988	Maine	1975	1978	New Mexico	1991	1989	Texas	1988	1987
Connecticut	1980	1983	Maryland	1960	1985	New York	1976	1982	Utah	1981	1984
Delaware	1960	1988	Massachusetts	1984	1983	North Carolina	1960	1985	Vermont	1970	1988
Florida	1988	1985	Michigan	1987	1986	North Dakota	1987	1991	Virginia	1978	1985
Georgia	1983	1985	Minnesota	1993	1986	Ohio	1979	1985	Washington	1985	1987
Idaho	1960	1985	Mississippi	1986	1988	Oklahoma	1988	1987	West Virginia	1987	1988
Illinois	1988	1986	Missouri	1990	1986	Oregon	1985	1986	Wisconsin	1990	1987
Indiana	1989	1986	Montana	1990	1993	Pennsylvania	1982	1986	Wyoming	1988	1987

Notes: This table shows the year in which each state permitted intrastate branching through M&A and the year in which each state first entered into interstate banking agreements with other states. This information has been taken from Demyanyk et al. (2007).

Table 2: Average employment shares (1970–2000)

Industry	%
Construction	5.3
Manufacturing	15.3
Transportation and public utilities	4.1
Trade (wholesale + retail)	18.4
Finance, insurance and real estate	5.0
Services	19.0
Government and government enterprises	17.0
Agricultural services, forestry and fishing + Mining	15.9

Notes: The average employment shares have been calculated on the basis of the county-level data from the Bureau of Economic Analysis. As explained in the notes accompanying the data, the missing entries are usually due to disclosure restrictions. The missing numbers are included in the higher-order totals, though. The missing entries were obtained by regressing the available county-level employment shares for a specific industry on the corresponding state-level employment shares and a collection of year dummies. Subsequently, we used the resulting coefficient estimates to predict the missing entries at the county level. Because the data for the industries *Agricultural Services, Forestry and Fishing* and *Mining* contain too many missing entries for sensible imputation, we only provide their total employment shares. This total has been calculated as the residual employment share (i.e., 100% minus the sum of the other industries' employment shares). We use the employment shares of these two combined industries as the benchmark category in our estimations.

Table 3: Estimation results for the full sample (1970–2000)

short-run effects	FE	DSAR-FE	DSAR-CF2	DSAR-CF4	GNS-CF2
marginal DE intra	0.1561	0.3058	−0.0558	−0.0418	0.0198
	0.0551	*0.1866*	*0.7603*	*0.8075*	*0.9249*
marginal SE adjacent intra		0.1206	−0.0214	−0.0161	0.0093
		0.1866	*0.7602*	*0.8075*	*0.9249*
marginal SE hinterland-a intra		0.0316	−0.0055	−0.0041	0.0030
		0.1866	*0.7601*	*0.8074*	*0.9248*
marginal SE hinterland-na intra		0.0008	−0.0001	−0.0001	0.0001
		0.1870	*0.7601*	*0.8074*	*0.9247*
marginal DE inter	0.6469	0.1244	−0.2524	−0.2160	−0.2611
	0.0000	*0.6790*	*0.1577*	*0.1939*	*0.1940*
marginal SE adjacent inter		0.0490	−0.0969	−0.0832	−0.1221
		0.6792	*0.1579*	*0.1941*	*0.1943*
marginal SE hinterland-a inter		0.0129	−0.0247	−0.0213	−0.0391
		0.6794	*0.1581*	*0.1944*	*0.1949*
marginal SE hinterland-na inter		0.0003	−0.0006	−0.0005	−0.0016
		0.6800	*0.1590*	*0.1952*	*0.1968*
long-run effects	FE	DSAR-FE	DSAR-CF2	DSAR-CF4	GNS-CF2
marginal DE intra	0.3229	0.3093	0.0428	0.0604	0.0442
	0.0000	*0.0845*	*0.7007*	*0.5637*	*0.7271*
marginal SE adjacent intra		0.1184	0.0164	0.0235	0.0206
		0.0845	*0.7005*	*0.5636*	*0.7269*
marginal SE hinterland-a intra		0.0301	0.0042	0.0061	0.0066
		0.0847	*0.7004*	*0.5635*	*0.7268*
marginal SE hinterland-na intra		0.0007	0.0001	0.0001	0.0003
		0.0858	*0.7002*	*0.5634*	*0.7266*
marginal DE inter	1.1503	0.3262	−0.2385	−0.2290	−0.3162
	0.0000	*0.2714*	*0.0472*	*0.0422*	*0.0195*
marginal SE adjacent inter		0.1249	−0.0916	−0.0890	−0.1475
		0.2716	*0.0474*	*0.0425*	*0.0197*
marginal SE hinterland-a inter		0.0318	−0.0234	−0.0230	−0.0471
		0.2721	*0.0478*	*0.0429*	*0.0203*
marginal SE hinterland-na inter		0.0008	−0.0006	−0.0006	−0.0019
		0.2735	*0.0491*	*0.0442*	*0.0222*
spatio-temporal coeff.	FE	DSAR-FE	DSAR-CF2	DSAR-CF4	GNS-CF2
spatial lag		0.8441	0.8326	0.8341	0.9103
		0.0000	*0.0000*	*0.0000*	*0.0000*
temporal lag	−0.2431	−0.2292	−0.2446	−0.2503	−0.2455
	0.0000	*0.0000*	*0.0000*	*0.0000*	*0.0000*
spatial-temporal lag		0.1777	0.2036	0.2133	0.2224
		0.0000	*0.0000*	*0.0000*	*0.0000*
other results	FE	DSAR-FE	DSAR-CF2	DSAR-CF4	GNS-CF2
CD test (pairwise correlation)	0.000	0.000	0.011	0.012	0.007
CD test (test statistic)	0.508	0.040	130.172	140.003	81.286
CD test (p-value)	*0.612*	*0.968*	*0.000*	*0.000*	*0.000*
exponent of CD ($\hat{a}$, bias corrected)	0.456	0.443	0.697	0.709	0.660
95% conf. int.	[0.43, 0.48]	[0.42,0.47]	[0.66, 0.73]	[0.68, 0.74]	[0.62, 0.70]
exponent of CD ($\hat{a}$, robust)	0.467	0.443	0.741	0.755	0.699
95% conf. int.	[0.44, 0.49]	[0.42, 0.47]	[0.71, 0.78]	[0.72, 0.79]	[0.66, 0.73]
BIC	509,672	474,417	457,451	442,184	457,006
(pseudo) R^2	0.076	0.404	0.601	0.663	0.613
# counties	3,050	3,050	3,050	3,050	3,050
# years	31	31	31	31	31

Notes: The first and second horizontal panel report point estimates (normal font) and associated p-values (italic) for several summary measures corresponding to the estimated models. Summary measures are reported for the following marginal effects: the marginal direct effect of deregulation on the counties in the deregulated state ('marginal DE'), the marginal spillover effect on counties adjacent to the deregulated state ('marginal SE adjacent'), the marginal spillover effect on hinterland counties in adjacent states ('marginal SE hinterland-a'), and the marginal spillover effect on counties in non-adjacent states ('marginal SE hinterland-na'). The third panel reports the estimates of the spatio-temporal coefficients, while the fourth panel contains test results and goodness-of-fit measures. The estimated models are all based on (2). The GNS model additionally uses (4).

Table 4: Size and significance of individual marginal effects (1970–2000)

	DSAR-FE		DSAR-CF2		GNS-CF2	
	short run	*long run*	*short run*	*long run*	*short run*	*long run*
marginal DE intra	0	100.0	0	0	0	0
		[0.25, 0.54]				
marginal SE adjacent intra	0	100.0	0	0	0	0
		[0.06, 0.23]				
marginal SE hinterland-a intra	0	88.3	0	0	0	0
		[0.00, 0.08]				
marginal SE hinterland-na intra	0	6.2	0	0	0	0
		[0.00, 0.01]				
marginal DE inter	0	0	0	100.0	0	100.0
				[−0.42, −0.19]		[−0.69, −0.25]
marginal SE adjacent inter	0	0	0	100.0	0	100.0
				[−0.18, −0.04]		[−0.31, −0.07]
marginal SE hinterland-a inter	0	0	0	98.7	0	98.8
				[−0.06, −0.00]		[−0.13, −0.00]
marginal SE hinterland-na inter	0	0	0	27.6	0	35.3
				[−0.00, −0.00]		[−0.01, −0.00]

Notes: The columns captioned 'short run' apply to the short-run marginal effects and reports two figures for each group of counties: the percentage of counties for which the estimated short-run effect is significant at the 90% level and an interval representing the 2.5% and 97.5% quantiles of the estimated marginal effects for the individual counties with a significant effect. The columns captioned 'long run' do the same for the long-run effects. If the percentage of counties with a significant effect is 0, no interval is reported. The following marginal effects are considered: the marginal direct effect of deregulation on the counties in the deregulated state ('marginal DE'), the marginal spillover effect on adjacent counties ('marginal SE adjacent'), the marginal spillover effect on hinterland counties in adjacent states ('marginal SE hinterland-a'), and the marginal spillover effect on counties in non-adjacent states ('marginal SE hinterland-na').

A. Appendix with supplementary material

A.1. Data sources and code

Table A.1 on the next page contains a list of data sources used to collect the data for this study. Regarding the source of county personal income, we note that direct sources of real personal income are only available at the county level for 2007–2016.[1] Furthermore, county-level GDP data is only available for 2012–2015.[2]

The adjacency matrix we use in our specification search is a first-order queen contiguity matrix taken from Merryman (2005). We also extended this matrix to the broader definition of 'adjacency' using the list of MSAs taken from the source given in Table A.1. Regarding the NBER county-distance database we used for the inverse-distance weight matrix, we note that we used the data for the year 2000 from the 'SF1' source.

A.2. Formal expressions for summary measures

We denote element (i, j) of a matrix E by $E(i, j)$. The summary measures for the marginal direct effects (DE) of deregulation on economic growth are defined as

$$\text{avg. mean marginal DE} = \frac{1}{N}\sum_{j=1}^{N}\frac{1}{\#(A_j)}\sum_{i\in A_j}E_{\ell}^{k}(i, j) \qquad [k = sr, lr; \ell = intra, inter]; \tag{A.1}$$

$$A_j = \{m : \text{county } m \text{ is located in state } j\}. \tag{A.2}$$

For a given state j that adopts banking deregulation, the above summary measures first calculate the mean marginal direct effect of deregulation on growth over all counties in that state (contained in the set A_j). Subsequently, the average is taken over states $j = 1, \ldots, N$.

In a similar way, we construct summary measures for the marginal spillover effects (SE) to counties adjacent to a deregulated state, for $k = sr, lr$ and $\ell = intra, inter$:

$$\text{avg. mean marginal SE on adjacent counties} = \frac{1}{N}\sum_{j=1}^{N}\frac{1}{\#(B_j)}\sum_{i\in B_j}E_{\ell}^{k}(i, j); \tag{A.3}$$

$$B_j = \{m : \text{county } m \text{ is adjacent to state } j\}. \tag{A.4}$$

The measures in (A.3) reflect the average mean marginal spillover effect of banking deregulation on the economic growth of the counties adjacent to the deregulated state. In a similar way, we construct

[1] See https://www.bea.gov/data/prices-inflation/regional-price-parities-state-and-metro-area.
[2] See https://www.bea.gov/news/2018/prototype-gross-domestic-product-county-2012-2015.

Table A.1: Data sources

Type of data	Data source
Main analysis	
Deregulation data	Demyanyk et al. (2007, Table 1)
County personal income	https://www.bea.gov/data/income-saving/personal-income-county-metro-and-other-areas
All Urban Consumers Price Index, not seasonally adjusted	https://www.bls.gov/cpi/
Employment data	https://www.bea.gov/data/employment/employment-county-metro-and-other-areas
Stata module USSWM (state-level and county-level adjacency matrices)	https://econpapers.repec.org/software/bocbocode/s448405.htm and Merryman (2005)
List of Metropolitan Statistical Areas	https://apps.bea.gov/regional/docs/msalist.cfm
Distances between counties	https://www.nber.org/data/county-distance-database.html
Robustness checks	
Regional Price Indices (South, West, Midwest, Northeast)	https://www.bls.gov/cpi/regional-resources.htm
Education data	https://www.ers.usda.gov/data-products/county-level-data-sets/download-data/

summary measures for the spillovers to hinterland counties in adjacent states:

$$\text{avg. total marginal SE on hinterland counties} = \frac{1}{N}\sum_{j=1}^{N}\frac{1}{\#(C_j)}\sum_{i\in C_j}E_\ell^k(i,j); \tag{A.5}$$

$$C_j = \{m : \text{county } m \text{ is not adjacent to state } j \text{ but its state is adjacent to state } j\}. \tag{A.6}$$

We do the same for the spillovers to counties in hinterland states:

$$\text{avg. total marginal SE on counties in hinterland states} = \frac{1}{N}\sum_{j=1}^{N}\frac{1}{\#(D_j)}\sum_{i\in D_j}E_\ell^k(i,j); \tag{A.7}$$

$$D_j = \{m : \text{county } m \text{ is located in a state that is not adjacent to state } j\}. \tag{A.8}$$

Table A.2 provides the minimum, maximum and average number of elements in the sets A_j, B_j, C_j and D_j as defined in (A.2), (A.4), (A.6) and (A.8). For example, the number of counties adjacent to a state ranges between 3 and 55, with an average of 27.

Table A.2: Number of elements in sets

	min	max	mean
# counties in state (A_j)	3	254	64
# counties adjacent to state (B_j)	3	55	27
# hinterland counties in adjacent states (C_j)	7	750	279
# counties in non-adjacent states (D_j)	2,155	3,024	2,681

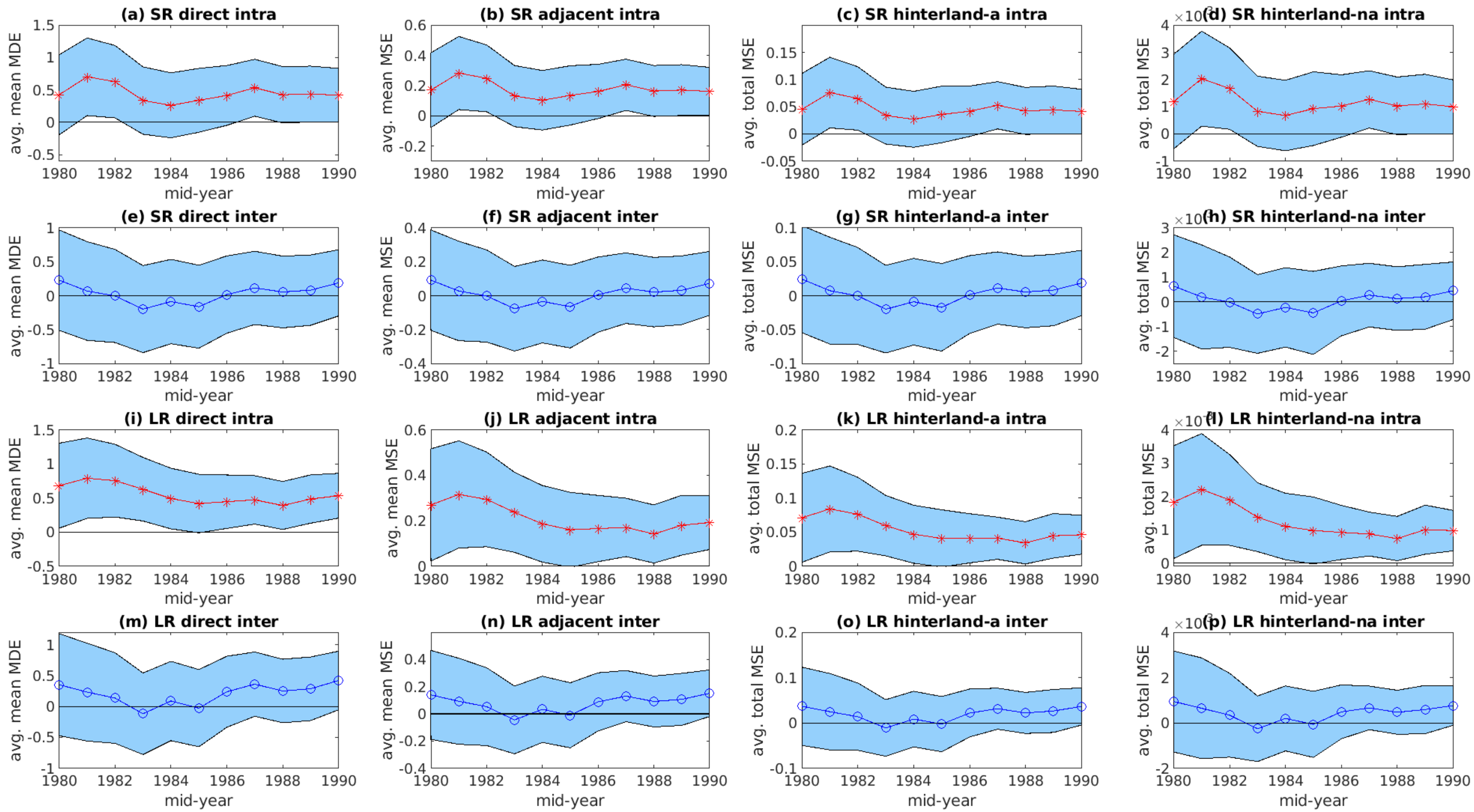
Notes: For each region, this table shows the minimum, maximum and average number of elements in the sets A_j, B_j, C_j and D_j as defined in (A.2), (A.4), (A.6) and (A.8). These sets are used in the summary measures for the marginal effects given by (A.1), (A.3), (A.5) and (A.7).

Remark: All estimations for our study have been done using the Matlab code provided at https://spatial-panels.com. We have adjusted this code to the requirements of our specific application, so any errors remain our own.

A.3. Rolling-window estimates for the DSAR-FE model

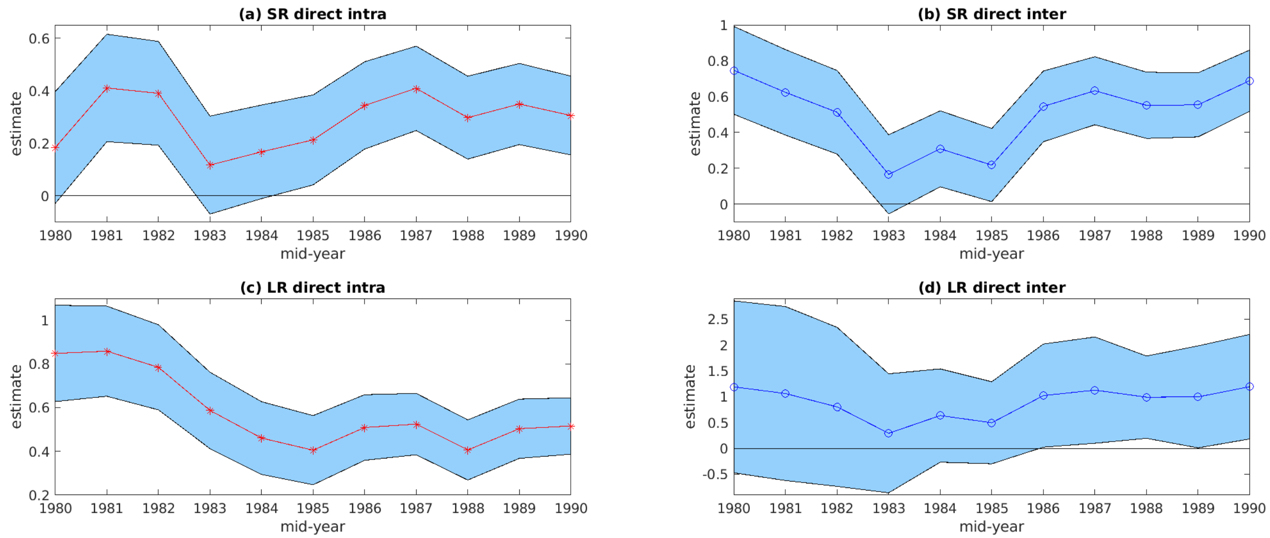
See Figures A.1 and A.2.

Figure A.1: Rolling-window estimates for the DSAR-FE model



Notes: This figure visualizes the summary measures for the direct effects of banking deregulation on the counties in the deregulated state ('direct'), the spillovers to counties adjacent to the deregulated state ('adjacent'), the spillovers to hinterland counties in adjacent states ('hinterland-a') and the spillovers to counties in non-adjacent states ('hinterland-na'). Abbreviations used in the graphs: SR = short run, LR = long run, MDE = marginal direct effect, MSE = marginal spillover effect. For all effects, the solid lines (with a circle or star as a marker) represent the point estimates, while the intervals in light blue constitute a pointwise 95% asymptotic confidence interval.

Figure A.2: Rolling-window estimates for the standard dynamic FE panel regression model



Notes: This figure visualizes the short-run (SR) and long-run (LR) marginal direct effects of intrastate branching and interstate banking deregulation according to the standard dynamic FE panel regression model. Because this model ignores any spatial effects, there are only direct effects of banking deregulation on the counties in the deregulated state and no spillovers to counties in other states. Furthermore, the marginal direct effects of banking deregulation are the same across states and counties. For all effects, the solid lines (with a circle or star as a marker) represent the point estimates, while the intervals in light blue constitute a pointwise 95% asymptotic confidence interval.