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JEL Classification

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U.S. Banking Deregulation and Local Economic Growth: Direct Effects and Externalities

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Abstract

This study investigates the effects of banking deregulation on county-level economic growth in the U.S. during the 1970–2000 period. Our main contribution to the literature is that we analyze both the direct and external effects of banking deregulation on local economic growth. For the regions South, West and Northeast, we find significantly positive long-run direct effects of intrastate branching deregulation on the expected growth rates of counties in the deregulated state itself, up to several percentage points. We also establish significantly positive long-run external effects on the expected growth rates of counties adjacent to the deregulated state, up to several tenths of percentage points. We do not find such robust effects for interstate banking deregulation.

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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, the literature has also considered potential downsides of liberalization and deregulation. 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). 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 that 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).

Given the inconclusiveness of the theoretical literature, the impact of banking deregulation on economic growth is ultimately an empirical matter. The U.S. provide 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 only allow unit banking, implying that banks were prohibited from having more than one office. 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 gradually relaxed. The passage of the Riegle-Neal Interstate Banking and Branching Efficiency Act (IBBEA) in 1994 removed the remaining barriers to interstate branching as of the 1st of June 1997, although this act offered states the possibility to opt out before this date. For a detailed overview of the history of U.S. banking regulation; see e.g. Felsenfeld and Glass (2011).

Jayaratne and Strahan (1996) use the incremental relaxation by state legislatures of intrastate branching restrictions in the 1970s and 80s as a natural experiment. They establish 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 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 positive effects of intrastate branching deregulation on economic performance found by Jayaratne and Strahan (1996) have been confirmed and extended by various studies (e.g., Kerr and Nanda, 2009; Rice and Strahan, 2010; Koetter et al., 2012; Amore et al., 2013; Chava et al., 2013; Krishnan et al., 2014; Tewari, 2014; Krishnamurthy, 2015). In addition to the literature on U.S. banking deregulation, several studies have investigated the effects of bank branch expansion and banking deregulation in European countries such as Spain and Italy (e.g., Carbó Valverde et al., 2003, 2007; Pastor et al., 2017; Bernini and Brighi, 2018).

A largely neglected issue in the literature is the existence of growth externalities of banking deregulation. Such externalities occur if banking deregulation in one state affects the growth rates of counties in states surrounding the deregulated state. One reason to expect such externalities is the presence of spatial correlation in counties' growth rates. Spatial dependencies among local growth rates arise because of interregional technology diffusion, factor mobility and transfer payments (Fingleton and López-Bazo, 2006). If counties' growth rates are spatially correlated, the impact of banking deregulation on the economic growth of counties in the deregulated state will spill over to surrounding counties.

To obtain consistent estimates of the impact of banking deregulation on local economic growth, spatial correlation and other mechanisms causing spatial externalities must be controlled for (Wheelock, 2003). Externalities do not only matter from an econometric point of view, though. By analyzing both the direct and the external effects of banking deregulation on economic growth, we obtain a more complete picture of the real effects of banking deregulation (Anselin, 2003). For example, ignoring any positive externalities might lead to systematic understatement of the welfare implications of banking deregulation.

The goal of this study is to analyze the effects of deregulation of the U.S. banking industry on county-level economic growth during the 1970–2000 period. Our main contribution to the literature is that we do not only investigate the impact of banking deregulation on the economic growth of the counties in the deregulated state itself ('direct' effects), but also on the growth rates of counties in states surrounding the deregulated state ('spatial externalities').

Our focus on county-level economic growth offers the advantage of minimizing the risk of endogeneity of banking deregulation relative to economic growth (Huang, 2008). Furthermore, our county-level sample is large enough to allow for both regional and temporal heterogeneity in the spatial correlation between counties' growth rates and in the effects of banking deregulation on economic growth.

Several studies have shown or argued that it is important to allow for such heterogeneity, for instance because of the regional variation in the timing of deregulation in relation to the business cycle (Wall, 2004; Freeman, 2005; Garrett et al., 2007).

One of our empirical findings stands out in terms of robustness over time and across the regions South, West and Northeast. This is the positive long-run effect of intrastate branching deregulation on the expected growth of counties in the deregulated state itself (up to several percentage points) and on the expected growth of counties adjacent to the deregulated state (up to several tenths of percentage points). We do not find such robust effects for interstate banking deregulation, which has significantly positive long-run direct and external effects in the South only. For the Midwest we find virtually no significant real effects of banking deregulation.

The differential effects between intrastate branching and interstate banking deregulation and the regional variations therein are consistent with a scenario in which the timing of banking deregulation played an important role. That is, in all regions apart from the South, intrastate branching took generally place prior to interstate banking deregulation. If no or limited additional efficiency gains were to be realized by the time that interstate banking restrictions were relaxed, this type of deregulation would have no significant effects in the regions West, Midwest and North.

The externalities that we find are entirely due to spatial correlation in adjacent counties' growth rates. According to our estimations, a county's expected growth rate increases by 0.5–0.7 percentage points following an increase of 1 percentage point in the average growth rate of adjacent counties. We find no evidence for externalities caused by mechanisms other than spatial correlation. As a result, the estimated external effects have the same sign as the direct effects and are economically relevant for counties adjacent to the deregulated state. Our finding of significantly positive external effects of deregulation in three out of four regions underlines that the welfare-enhancing effects of banking deregulation in these regions would be structurally understated if such externalities were ignored.

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 empirically tested in the empirical analysis. Section 3 outlines the econometric framework, while a description of the data is given in Section 4. Details about the specification search are given in Section 5. Our empirical results follow in Section 6, with a discussion in Section 7. Lastly, Section 8 concludes.

2. Literature review and hypotheses

In their seminal study, Jayaratne and Strahan (1996) establish significantly positive real effects of U.S. intrastate branching deregulation. Their research has been extended by many other studies. This section starts with a reviews of the literature that is related to spatial externalities, regional and temporal variation in the effects of banking deregulation, and the differential real effects of intrastate branching and interstate banking deregulation. Subsequently, we will formulate the hypotheses that will be tested in our empirical analysis.

2.1. *Spatial externalities*

Existing studies typically focus on the impact of banking deregulation on the economic performance of the deregulated state itself and ignore any external effects on other states. There are a few exceptions, though. Wheelock (2003) observes regional patterns in states' growth rates and their decisions to deregulate. He emphasizes the importance of controlling for spatial correlation in growth rates. Garrett et al. (2005) model the spatial dependence in states' choice of intrastate branching and interstate banking and show that states' deregulation decisions were influenced by adjacent states' decisions. In his matched-pairs analysis, Huang (2008) accounts for spatial externalities by performing a robustness check that uses more distant 'hinterland' counties instead of adjacent counties as the control group.[1]

In other strands of literature, spatial externalities have been found at various geographic levels in the U.S. in response to infrastructure investments, local decentralization and an increase in taxation and spending (Cohen and Morisson Paul, 2004; Baicker, 2005; Hammond and Tosun, 2011; Isen, 2014).

One channel that leads to spatial externalities is spatial correlation in counties' growth rates. Such spatial linkages in local growth rates arise because nearby economies are connected by flows of goods, technology, production factors and payments (Fingleton and López-Bazo, 2006). In the presence of spatial correlation, the impact of banking deregulation on the economic growth of the counties in the deregulated state spill over to the growth rates of counties in adjacent states. From there, the effects of deregulation spread to other counties via 'feedback' effects.[2]

Another channel that may lead to spatial externalities is cross-state lending, which refers to the situation that firms and consumers in regulated states borrow funds from banks in adjacent states that have already been deregulated. Huang (2008) acknowledges the existence of external effects on growth caused by cross-state lending, but expects the economic relevance of these effects to be small due to the local nature of banking markets (e.g., Brevoort and Hannan, 2006; Ho and Ishii, 2011; Knyazeva and Knyazeva, 2012; Bellucci et al., 2013). He emphasizes that the information asymmetries for banks increase with distance, due to communication and transport costs (e.g., Degryse and

[1] The term 'hinterland' counties has been borrowed from Huang (2008) and refers to counties that are not adjacent to the deregulated state, but are located in a state adjacent to the deregulated state.

[2] For estimates of state-to-state commodity trade flows and county-to-county commuting flows in the U.S., see for instance Park et al. (2009) and McKenzie (2013).

Ongena, 2004). A similar point is made by Chu (2018).

Even if the spatial externalities coming from cross-state lending are indeed negligible, externalities will still arise in the presence of spatial correlation in counties' growth rates. This point is also made by Garrett et al. (2007) in a general (non-banking) context. They argue that state-specific exogenous shocks that affect economic conditions are likely to influence the economic growth of adjacent states in the presence of spatial correlation.

2.2. *Regional and temporal differences*

In order to model the direct and external effects of U.S. banking deregulation, we must know to what extent we can expect regional and temporal variation in these effects.

Most studies that analyze the real effects of U.S. banking deregulation assume that there is no regional and temporal heterogeneity in the way U.S. banking deregulation affected economic growth. There are a few exceptions, though. Wheelock (2003) observes strong regional patterns in states' growth rates and their decisions to deregulate. For instance, states in the South and New England tended to deregulate earlier than Midwestern states, while several of the former states featured relatively high average growth rates during the 1976–1996 period. Wall (2004) allows for regional heterogeneity in the impact of U.S. banking deregulation on entrepreneurship. After controlling for such differences, the favorable effects of banking deregulation become substantially weaker. Freeman (2005) controls for regional heterogeneity in the impact of intrastate deregulation on economic growth and does no longer find any significant effects. In addition to regional heterogeneity, Freeman (2005) also establishes significant structural change in the direct effects of intrastate branching deregulation on economic growth. Evidence for regional differences is also provided by Garrett et al. (2007), who show that there is substantial regional variation in the spatial correlation of state-level income growth during the 1977–2002 period.

The aforementioned studies use the regional patterns in economic events and the timing of states' deregulation decisions as the rationale for allowing for regional and temporal variation in the way U.S. banking deregulation affected economic growth. The underlying idea is that the regional timing of deregulation in relation to the business cycle affected the impact of deregulation on economic growth. It seems likely that also unobserved and observed regional differences played a role here, including e.g. sectoral composition of regional economies, access to external financing, patterns of population settlement and the existence of regional interstate banking compacts.

Freeman (2005) finds that the effects of intrastate branching deregulation on economic growth were only temporary instead of permanent, suggesting that also the time horizon mattered. Also DeYoung et al. (1998), Stiroh and Strahan (2003) and Chava et al. (2013) distinguish between temporary (short-run) and permanent (long-run) effects of banking deregulation and find evidence for both of them. Stiroh and Strahan (2003) explain the difference between short-run and long-run effects by noting 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.

2.3. Intrastate branching vs. interstate banking deregulation

While many studies about U.S. banking deregulation focus on the impact of intrastate branching deregulation only, some studies analyze the effects of intrastate branching and interstate banking deregulation jointly. These studies provide rather ambiguous empirical evidence on the joint real effects of relaxing intrastate branching and interstate banking restrictions.

Jayaratne and Strahan (1996), Kroszner and Strahan (1999) and Beck et al. (2010) find that interstate banking, in contrast to intrastate branching deregulation, did not have a significant effect on the real economy, but do not provide detailed explanations for the differential effects. The studies by Kerr and Nanda (2009) and Amore et al. (2013) on the impact of banking deregulation on entrepreneurship, business closures and innovation – which are all related to economic growth according to the literature – suggest that interstate banking did have positive real effects. Also the work of Chava et al. (2013) on the effects of banking deregulation on innovation by young, private firms and on industry-level economic growth finds evidence for positive real effects of interstate banking deregulation.

Kerr and Nanda (2009) find that entrepreneurship increased significantly after interstate banking deregulation, while the number of business closures – especially among new start-ups – increased as well. Also the number of firms with long-term entry increased after relaxation of interstate banking restrictions. They find only limited effects after intrastate branching deregulation, though. Kerr and Nanda (2009) provide several explanations for the differential impact of the two forms of deregulation. They conjecture that both types had some impact of the competitive climate in the banking sector, but that only interstate banking had a substantive effect on the real economy. As a possible explanation, they suggest that the market for corporate should may be larger than an individual state in order to be effective. They also hypothesize that multi-state banks were more likely to invest in technology that would serve start-ups. Kerr and Nanda (2009, p. 143) conclude that *"the differential effect of these reforms remains a puzzle, however, and further work on untangling these differences is critical to understand the mechanisms connecting financial sector reforms to changes in the real economy"*.

Chava et al. (2013) find that intrastate branching deregulation decreased the level and risk of innovation by young, private firms, while interstate banking deregulation increased both of them. At the same time, they find that interstate banking deregulation had a positive effect on economic growth due to its positive effect on innovation by young, private firms, while the effects of intrastate branching deregulation on economic growth were negative due to the negative impact on innovation by such firms. Using existing results from the literature, they explain the differential effects of intrastate branching and interstate banking deregulation from the increase in banks' bargaining power with young, private firms caused by the former type of deregulation and the decrease in bargaining power caused by the latter (Strahan, 2003; Stiroh and Strahan, 2003; Berger et al., 1995; Cole et al., 2004; Berger et al., 2005).

Amore et al. (2013) find that interstate banking deregulation improved the quantity and quality of activities related to innovation, especially for firms that were highly dependent on external capital and more closely located to entering banks. They show that their results are driven by improved availability of deregulated banks to geographically diversify credit risk. Their results are robust to controlling for intrastate branching deregulation, but they do not examine the differential impact of the two types of deregulation.

2.4. Hypotheses

Based on the results from the theoretical and empirical literature on the real effects of U.S. banking deregulation, we formulate several empirically testable hypotheses.

According to the traditional view that banking deregulation fosters economic growth through increased competition and efficiency of the banking sector, banking deregulation in one state is expected to have a positive long-run impact on the economic growth of the counties in the deregulated state itself ('direct effects'). Because we expect the growth rates of nearby counties to be positively correlated (positive 'spatial correlation'), we also expect the positive effects of banking deregulation to spill over to the growth rates of counties in surrounding states ('spatial externalities').[3]

On the basis of Wall (2004) and Freeman (2005), however, we expect to find differences across regions and over time in the direct effects of banking deregulation on economic growth. We also expect to find regional heterogeneity in spatial correlations – and thereby in spatial externalities – because of the findings of Garrett et al. (2007). Furthermore, the results of Freeman (2005), Stiroh and Strahan (2003) and Chava et al. (2013) suggest that we may find discrepancies between the short- and long run effects of banking deregulation. It is difficult to extrapolate these results into more detailed hypotheses, though. The issue here is that the setup of the aforementioned studies differs from ours in terms of, for instance, the choice of the dependent variable, the type of banking deregulation considered and the level of analysis (state-level versus county-level). Moreover, due to the diverging

[3] In the literature, spatial 'externalities' are often referred to as spatial 'spillovers'. We will use both words interchangeably (Anselin, 2003).

results in the existing empirical literature regarding the differential impact of intrastate branching and interstate banking deregulation, we consider the differences in the effects of the two types of deregulation as an empirical matter.

3. Econometric approach

Following Wall (2004) and Freeman (2005), we assume that the impact of banking deregulation on county-level economic growth varies across regions due to both observed and unobserved regional differences. We assume that there are R regions, consisting of adjacent states with shared economic characteristics and possibly an increased likelihood of cross-state banking. For regions $r = 1, \ldots, R$, we specify a dynamic county-level fixed-effects Spatial Durbin Model (SD) model (Anselin, 1988; LeSage and Pace, 2009; Elhorst, 2014). We choose this model because it allows for the presence of spatial correlation and other external effects of banking deregulation on economic growth. Furthermore, the standard regression model that lacks any spatial externalities is a special case of the SD model.

3.1. Spatial Durbin model

For simplicity of notation, we omit the subscript indicating the region r from all notation. In its most basic form, the SD model then writes as

$$\boldsymbol{y}_t \quad = \quad \boldsymbol{\alpha} + \beta_t \boldsymbol{\iota}_M + \boldsymbol{X}_t \boldsymbol{\gamma} + \rho \boldsymbol{W}_c \boldsymbol{y}_t + \widetilde{\boldsymbol{W}}_c \mathbf{X}_t \boldsymbol{\theta} + \delta \boldsymbol{y}_{t-1} + \boldsymbol{Z}_t \boldsymbol{\lambda} + \boldsymbol{\varepsilon}_t \qquad [t = 1, \ldots, T]. \tag{1}$$

The vector $\boldsymbol{y}_t = (y_{1t}, y_{2t}, \ldots, y_{Mt})'$ contains the per capita real economic growth rates in county $i = 1, \ldots, M$ at time t, $\boldsymbol{\alpha} = (\alpha_1, \alpha_2, \ldots, \alpha_M)'$ a vector of county-specific fixed effects, β_t a time-specific fixed effect and $\boldsymbol{\varepsilon}_t = (\varepsilon_{1t}, \varepsilon_{2t}, \ldots, \varepsilon_{Mt})'$ is a vector of error terms, where ε_{it} is assumed to be i.i.d. across i and t with zero mean and variance σ^2.

Fixed effects. The fixed effects α_i reflect the county-specific component of long-run economic growth. They control for unobserved, time-invariant factors that differ across counties or states.[4] The county-specific fixed effects also control for initial differences in income level across counties that may lead to convergence effects in growth rates, as well as for other differences in initial conditions. The possibility to control for unobserved heterogeneity at a very narrow geographical level is one of the advantages of a county-level analysis.

[4]Examples of such factors include institutional features (e.g., tax rates, public rates of investment and environmental regulations), environmental characteristics (e.g., availability of natural resources and access to cheap water transportation) and many other characteristics related to e.g infrastructure, population settlement, sectoral composition, geographical location, urbanization, and human capital.

The time fixed-effects β_t reflect common shocks to the growth rates of the regional economy at time t, caused by e.g. the national or regional business cycle and technological innovation. They also control for the total number of states in the region that have deregulated up to a certain point in time.

Direct effects. $\boldsymbol{X}_t$ is an $M \times 2$ regressor matrix, which includes indicator variables for intrastate branching deregulation and for 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.[5] The corresponding coefficient vector is $\boldsymbol{\gamma} = (\gamma_{intra}, \gamma_{inter})'$.

Spatial correlation. Spatially speaking, we would like an increase in the growth rate of one county to spread via adjacent counties to more distant hinterland counties in adjacent states and from there to counties in hinterland states. Such feedback effects are known as 'global' externalities or spillover effects and enter the model via the spatial lag component $\rho \boldsymbol{W}_c \boldsymbol{y}_t$. The matrix $\boldsymbol{W}_c$ denotes a row-normalized adjacency matrix, with element (i, j) equal to the inverse of the number of counties adjacent to county i if county i and j share a border and 0 otherwise.[6] The parameter ρ denotes the spatial autocorrelation coefficient. Its sign reflects the sign of the spatial autocorrelation among adjacent counties' growth rates.[7]

In terms of spatial externalities, inclusion of the term $\rho \boldsymbol{W}_c \boldsymbol{y}_t$ means that counties that are closer neighbors of the deregulated state will receive larger spatial externalities than more distant hinterland counties. This seems a realistic assumption, given that e.g. commuting flows to adjacent counties are relatively large in comparison to flows to non-adjacent ones. We will later run robustness checks with respect to the choice of the matrix $\boldsymbol{W}_c$ and also consider an 'inverse-distance' matrix instead of an adjacency matrix.

Local externalities. We also want to permit 'local' externalities from state-level deregulation to the growth rates of counties adjacent to the deregulated state. This is achieved by inclusion of the term $\widetilde{\boldsymbol{W}}_c \mathbf{X}_t \boldsymbol{\theta}$. Here $\boldsymbol{\theta} = (\theta_{intra}, \theta_{inter})'$ is a vector of coefficients, while $\widetilde{\boldsymbol{W}}_c$ denotes another row-normalized adjacency matrix. Element (i, j) of this matrix is equal to the inverse of county i's number of adjacent counties in adjacent states if county i and county j share a border and are located in adjacent states, and 0 otherwise. If $\boldsymbol{\theta} \neq 0$ and $\rho \neq 0$, both local and global externalities are present. In this case,

[5] We note that including the interaction between the intrastate branching and interstate banking deregulation dummies does not make sense economically. Doing so would imply that the impact of intrastate branching deregulation on economic growth depends on whether or not interstate banking deregulation has taken place, and vice versa. However, only the deregulation that takes place first can depend on whether or not the other type has already occurred or not. The one that takes place last will always be conditional on the occurrence of the other.

[6] An adjacency matrix is also called a 'first-order binary-contiguity' matrix in the literature.

[7] We note that ρ is not a conventional correlation between $\boldsymbol{y}_t$ and $\boldsymbol{W}_c \boldsymbol{y}_t$, since its value does generally not lie in the interval $[-1, 1]$ (Elhorst, 2014, Ch. 4).

the local externalities $\widetilde{\boldsymbol{W}}_c \mathbf{X}_t \boldsymbol{\theta}$ become global externalities due to the feedback effects caused by the term $\rho \boldsymbol{W}_c \boldsymbol{y}_t$. We note that, for $\boldsymbol{\theta} = 0$ and $\rho = 0$, the SD model reduces to the standard dynamic panel regression model with only direct effects of deregulation on growth and no spatial externalities.

Business-cycle effects and control variables. Not controlling for counties' business cycle could bias the estimates of the impact of banking deregulation on economic growth (Jayaratne and Strahan, 1996; Freeman, 2002; Huang, 2008). For this reason, the SD model in (1) also includes a temporal lag of $\boldsymbol{y}_t$, with serial autocorrelation coefficient δ. The sign of δ reflects the sign of the serial autocorrelation in a county's growth rates.[8] By adding lagged growth to the model, we control for county-level business cycles and mean-reversion effects in growth rates. Furthermore, the dynamic SD model allows the short-run effects of banking deregulation to differ from the long-run effects on economic growth. This is a convenient property because several studies have found differences in the transitory and permanent effects of banking deregulation on economic growth (DeYoung et al., 1998; Stiroh and Strahan, 2003; Freeman, 2005; Chava et al., 2013). A sufficient condition for stationarity in the dynamic SAR model is $\rho + \delta < 1$ (Lee and Yu, 2010).

The $M \times K$ matrix $\boldsymbol{Z}_t$ contains various control variables, which affect economic growth via the coefficient vector $\boldsymbol{\lambda}$.

3.2. Marginal effects

In the SD model, the estimated coefficients of (1) do not reflect the marginal effects of banking deregulation on economic growth. Within a region, the marginal effects vary by county and state: counties close to state borders receive most of the spatial externalities of adjacent states' banking deregulation. More precisely, the $M \times 1$ vector of short-run (or immediate) marginal effects of deregulation in state j on the growth rates of county $i = 1, \ldots, M$ is given by

$$\mathbb{E}(\boldsymbol{y}_t \mid \boldsymbol{X}_t = [\widetilde{\boldsymbol{e}}_j \; \mathbf{0}]) - \mathbb{E}(\boldsymbol{y}_t \mid \boldsymbol{X}_t = [\mathbf{0} \; \mathbf{0}]) = (\boldsymbol{I}_M - \rho \boldsymbol{W}_c)^{-1} [\gamma_\ell \widetilde{\boldsymbol{e}}_j + \theta_\ell \widetilde{\boldsymbol{W}}_c \widetilde{\boldsymbol{e}}_j] \qquad [\ell = intra, inter].$$

Here $\widetilde{\boldsymbol{e}}_j$ is the $M \times 1$ vector with the elements that correspond to the counties in state j equal to 1 and the other elements equal to 0. Again we omit the regional subscripts for simplicity of notation.

In each region, we calculate these marginal effects for all $j = 1, \ldots, N$, yielding the $M \times N$ matrices of short-run marginal effects

$$\boldsymbol{E}_\ell^{sr} = (\boldsymbol{I}_M - \rho \boldsymbol{W}_c)^{-1} [\gamma_\ell \boldsymbol{I}_{M \times N} + \theta_\ell \widetilde{\boldsymbol{W}}_c \boldsymbol{I}_{M \times N}] \qquad [\ell = intra, inter], \tag{2}$$

[8] We note that δ is not a conventional correlation between $\boldsymbol{y}_t$ and $\boldsymbol{y}_{t-1}$, since its value does generally not lie in the interval $[-1, 1]$ (Elhorst, 2014, Ch. 4).

with $\boldsymbol{I}_{M\times N} = [\widetilde{\boldsymbol{e}}_1\ \widetilde{\boldsymbol{e}}_2 \dots \widetilde{\boldsymbol{e}}_N]$. In the matrices $\boldsymbol{E}^{sr}_{\ell}$, the columns refer to the state where deregulation is adopted and the rows to the counties whose growth rates are affected by it. The matrices of long-run (or permanent) marginal effects are given by

$$\boldsymbol{E}^{lr}_{\ell} = \{(1-\delta)\boldsymbol{I}_N - \rho\boldsymbol{W}_c\}^{-1}[\gamma_{\ell}\boldsymbol{I}_{M\times N} + \theta_{\ell}\widetilde{\boldsymbol{W}}_c\boldsymbol{I}_{M\times N}] \qquad [\ell = intra, inter]. \tag{3}$$

3.3. Summary measures of marginal effects

In line with Elhorst (2014, Sec. 2.7), we will report summary measures of the matrices of marginal effects in our empirical analysis. For each region, we will provide summary measures that capture the direct effects of banking deregulation on the growth of the counties in the deregulated state, as well as summary measures that reflect the external effects on counties in other states.

To construct the summary measures for the direct effects, we need the sets

$$A_j = \{m : \text{county } m \text{ is located in state } j\} \qquad [j = 1, \dots, N].$$

We use these sets to calculate the summary measures for the marginal Direct Effect (DE) of deregulation on economic growth:

$$\frac{1}{N}\sum_{j}\frac{1}{\#(A_j)}\sum_{i\in A_j} E^{k}_{\ell}(i,j) = \text{avg. mean marginal DE} \qquad [k = sr, lr;\ \ell = intra, inter]. \tag{4}$$

For a given state j that adopts banking deregulation, the above summary measure first calculates 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 $j = 1, \dots, N$.

To obtain the summary measures for the marginal External Effect (EE) of banking deregulation on growth, we need the sets

$$B_j = \{m : \text{county } m \text{ is adjacent to state } j\};$$

$$C_j = \{m : \text{county } m \text{ is not adjacent to state } j \text{ but its state is adjacent to state } j\};$$

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

On the basis of these sets, we construct three summary measures for the external effects, for $k = sr, lr$

and $\ell = intra, inter$:

$$\frac{1}{N}\sum_{j}\frac{1}{\#(B_j)}\sum_{i\in B_j}E_{\ell}^{k}(i,j) = \text{avg. mean marginal EE on adjacent counties;} \tag{5}$$

$$\frac{1}{N}\sum_{j}\sum_{i\in C_j}E_{\ell}^{k}(i,j) = \text{avg. total marginal EE on hinterland counties;} \tag{6}$$

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

The measures in (5) reflect the average mean marginal external effect of banking deregulation on the economic growth of the counties adjacent to the deregulated state. Similar to the summary measures for the direct effect, these measures first calculate the mean marginal external effect over the counties adjacent to the deregulated state j (contained in the set B_j) and then take the average over all states j in the region. The measures in (6) and (7) reflect the average *total* (or *cumulative*) marginal external effect of banking deregulation on the economic growth of hinterland counties and counties in hinterland states, respectively. Hence, these summary measures capture total instead of mean effects. The marginal external effect to hinterland counties and states exhibit a relatively high variability over the counties involved. This is due to the fact that counties with only a distant neighborship to the deregulated state receive only minor spatial externalities. Reporting a mean effect would therefore not be very informative.

The county-level SD model with $\rho \neq 0$ technically allows for global external effects, but the economic relevance and geographical outreach of the marginal external effects on the growth rates of counties in other states remains an empirical matter. The summary measures (5), (6) and (7) will prove particularly useful to assess the relevance and outreach of the external effects.

3.4. Spatial model estimation

We estimate the dynamic fixed-effects SD model by means of Quasi-Maximum Likelihood (QML) using the 'transformation approach' of Lee and Yu (2010), followed by a bias correction. The asymptotic distribution of this QML estimator relies on the assumption that $n/T^3 \to 0$, where n denotes the cross-sectional dimensions and T the time dimension. 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 error. To obtain standard errors and p-values for the summary measures, we obtain 1000 draws from the QML estimates' asymptotic distribution, calculate the implied summary measures and obtain the asymptotic standard errors and p-values (Elhorst, 2014, p. 24).[9]

3.5. Hypothesis testing

We will assess the relevance of the direct effects of banking deregulation by testing whether the summary measures for the direct effects are zero. Similarly, we will test the significance of the three external summary measures (5), (6) and (7), respectively. Furthermore, we will compare the summary measures associated with intrastate branching to those corresponding to interstate banking deregulation as a way to compare the real effects of both types of deregulation.

In a subsequent analysis, we will also look beyond the summary measures by investigating the direct and external effects of both types of deregulation for individual counties. To investigate the presence of spatial autocorrelation, we will test whether ρ – the coefficient of the spatial lag – is zero.

We will assess regional and temporal heterogeneity by comparing the estimation results across regions, over time and between time horizons (short run versus long run).

4. Data

Our sample includes 48 states of the contiguous United States, excluding Alaska, Hawaii and the District of Columbia. We collect county-level personal income data from the Bureau of Economic Analysis. This data is available since 1969 and we obtain the data for the period 1969–2000.[10]

4.1. Description

Following Jayaratne and Strahan (1996), we distinguish four regions: South, West, Midwest and Northeast. These regions are specified in Table 1. The number of counties in each subsample equals 1,288 (South), 411 (West), 1,052 (Midwest), and 299 (Northeast). The choice of the number of regions involves a trade-off between capturing regional heterogeneity and the accuracy of the QML estimation. We feel that a proper balance is struck by distiguishing four regions. Because there is no consensus about the definition of the four regions, we will later run robustness checks based on alternative definitions from the U.S. Census Bureau and the U.S. Bureau of Economic Analysis.

Our dependent variable, economic growth, 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. We use a national price index to obtain

[9] All SD models will be estimated using the Matlab code provided at https://spatial-panels.com. We have adjusted this code for to the requirements of our specific application, so any errors remain our own.

[10] We note that county-level GDP data is only available for 2012–2015; see https://www.bea.gov/news/2018/prototype-gross-domestic-product-county-2012-2015.

real county-level personal income, because other sources of real personal income are only available at the state level during the 2007–2016 period.[11] We will later run a robustness check using regional price indices taken from the U.S. Census Bureau. Because the calculation of growth rates leads to a loss of one year of data for each county, our final sample effectively covers the 1970–2000 period.

The timing of intrastate branching and interstate banking deregulations is taken from Demyanyk et al. (2007) and is shown in Table 1. For intrastate branching restrictions, a distinction can be made between the year that (i) a state permitted the formation of multibank holding companies (MBHC) that were allowed to own multiple banks but had to operate them separately, (ii) a state relaxed the restrictions on branching through mergers and acquisitions (M&A), and (iii) a stated 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 many others by choosing the year in which states allowed branching through M&A as 'the' deregulation year. In the sequel, we will simply refer to 'intrastate branching', meaning intrastate branching through M&A.

4.2. Sample statistics

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. The timing of banking deregulation varies substantially over the regions, as shown by Table 1.

We use the deregulation data for intrastate branching and interstate banking to calculate the percentage of states in each region that deregulated up to a certain year. For each region, the upper panel of Figure 1 displays the cumulative percentage of deregulated states over time. We make a couple of observations here. We see that the West was an 'early mover' in terms of intrastate branching deregulation, while the Midwest was 'late mover' and the South and Northeast were 'medium-early movers'. In all regions, interstate banking deregulation was adopted relatively late in comparison to intrastate branching. At the same time, the process of interstate banking deregulation was completed during a relatively short time period in comparison to intrastate branching. In terms of interstate banking

[11]For real county-level personal income during the 2007–2016 period; see https://www.bea.gov/data/prices-inflation/regional-price-parities-state-and-metro-area.

deregulation, the Northeast was an ‘early mover’, while the other regions all started with this type of deregulation in the mid 1980s. In the Northeast, intrastate branching deregulation took place in the same year or strictly before interstate banking deregulation in all but one state. Consequently, we can effectively only assess the impact of interstate banking deregulation conditional on intrastate branching deregulation for the Northeast. To a lesser extent, this also holds true for the West. Here only three states relaxed interstate banking restrictions prior to intrastate branching, with a time gap of at most three years.

In all regions deregulation occurred gradually and states deregulated at different point in time. We can therefore adopt a difference-in-differences approach and use the differences in counties’ growth rates before and after deregulation in comparison to the growth differences for counties that did not adopt this type of deregulation during the same period. Furthermore, in most states interstate and intrastate deregulation took place in different years, with a gap of a number of years between the two of them. This gap varies across the states in each region, allowing us to disentangle the effects of these two forms of deregulation.

The lower panel of Figure 1 displays income growth over time in each of the four regions, where we observe some outliers especially for the Midwest. To avoid the possibility that these outliers distort our estimation results, we winsorize the growth rates at the 99.5% level for each region.

To obtain our final data sample, we match the available growth and deregulation data with the county-level adjacency matrix available in the Stata module USSWM. We thus obtain a balanced county-level sample of $3,050$ counties in 48 states, observed during 31 years. We use this full-sample data to obtain the subsample data for each region.

Although we expect the four regions to share certain economic characteristics, we will control for county-level differences in sectoral composition. We therefore collect data from the Bureau of Economic Analysis for the 1970–2000 period on the number of jobs in the single-digit industries Agricultural services, forestry and fishing, Mining, Construction, Manufacturing, Transportation and public utilities, Trade (wholesale + retail), Finance, insurance and real estate, Services and Government and government enterprises. We use this data to calculate county-level employment shares for each industry. These shares will be used in our analysis as control variables to deal with differences in sectoral composition across counties. A formal economic motivation for including employment shares as control variables can be found in Echevarria (1997). Another empirical study that includes employment shares as control variables in deregulation regressions is Stiroh and Strahan (2003), whose analysis is at the state level.

For each of the four regions, we provide sample statistics for the employment shares in each industry; see Table 2. Because the industries Agricultural services, forestry and fishing and Mining contain many missing entries, we only provide their joint employment share.[12] This figure has been calculated as the residual employment share left by the other industries (i.e., as 100% minus the sum of the other industries' employment shares). For the other industries, the number of missing entries is at most a few percent. We impute the missing values using the state-level employment data, for which there are no missings. For this purpose, we regress the available county-level employment shares for a specific industry on the corresponding state-level employment shares and a collection of year dummies. We use the resulting coefficient estimates to predict the missing entries at the county level. Table 2 shows that there are relatively large differences across regions in terms of the average employment shares of Manufacturing, Services and Government and government enterprises and the combined industries of Agricultural services, forestry and fishing and Mining.

All data sources that we used are listed in Table A.1 in Appendix A.

5. Econometric implementation

Instead of estimating the SD model in (1), we estimate a slightly different specification based on a preliminary model selection phase.

5.1. Short- and long-run effects of deregulation

Instead of a single matrix $\boldsymbol{X}_t$, we include two matrices of county-level deregulation indicators: $\boldsymbol{X}_t^{sr}$ (where 'sr' stands for 'short run') and $\boldsymbol{X}_t^{lr}$ (where 'lr' stands for 'long run'). In these matrices, the first column corresponds to intrastate branching deregulation and the second to interstate banking deregulation. Their elements are defined as

$$\boldsymbol{X}_t^{sr}(j,1) = \begin{cases} 1 & \text{if county } j\text{'s state deregulated intrastate branching in year } t,\ t-1 \text{ or } t-2; \\ 0 & \text{otherwise;} \end{cases}$$

and

$$\boldsymbol{X}_t^{sr}(j,2) = \begin{cases} 1 & \text{if county } j\text{'s state deregulated interstate banking in year } t,\ t-1 \text{ or } t-2; \\ 0 & \text{otherwise;} \end{cases}$$

Similarly, we define

$$\boldsymbol{X}^{lr}(j,1) = \begin{cases} 1 & \text{if county } j\text{'s state deregulated intrastate branching in year } t-3 \text{ or earlier;} \\ 0 & \text{otherwise;} \end{cases}$$

[12] As explained in the notes accompanying the data from the Bureau of Economic Analysis, the missing entries are usually due to disclosure restrictions. The missing numbers are included in the higher-order totals, though.

and

$$\boldsymbol{X}^{lr}(j,2) = \begin{cases} 1 & \text{if county } j\text{'s state deregulated interstate banking in year } t-3 \text{ or earlier;} \\ 0 & \text{otherwise} \end{cases}$$

In this way, we allow the impact of deregulation on growth during the first three years to differ from the impact after four years and later. The reason that we do so is because 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. By allowing the impact of deregulation to depend on the number of years since deregulation we proceed in a similar hasfion as Stiroh and Strahan (2003) and Chava et al. (2013).

We only distinguish between short- and long-run effects for the regions South, Midwest and Northeast. In the region West, four out of eleven states implemented intrastate branching deregulation already in 1960, so prior to the start of the sample. Therefore, seven states are left for the identification of the short-run effects of intrastate branching deregulation. These states adopted interstate deregulation within three years before or after intrastate branching deregulation, as shown in Table 1. For this region, we therefore do not allow for additional short-run and long-run effects of intrastate branching deregulation besides those caused by the inclusion of a temporal lag. We do allow such additional short-run and long-run effects for interstate banking, though.

Throughout, the threshold level of three years was the outcome of a specification search. Because we estimate separate models for each region, the amount of variation in the deregulation data to identify short-run and long-run effects is more limited than in the nationwide sample. Our choice of the threshold level therefore involves a trade-off between the ability to capture short-run and long-run effects and the estimation accuracy. A period of three years seems to strike a proper balance between the two.

5.2. Local externalities

A specification search based on the Akaike Information Criterion (AIC) shows that adding the terms $\widetilde{\boldsymbol{W}}_c \mathbf{X}_t^{sr} \boldsymbol{\theta}^{sr}$ and $\widetilde{\boldsymbol{W}}_c \mathbf{X}_t^{lr} \boldsymbol{\theta}^{lr}$ – representing the local external effects of deregulation as discussed in Section 3 – does never decrease the value of the AIC. A possible explanation for the lack of significance of the local spillovers is related to the fact that cross-state lending – which we mentioned in Section 2.1 – can occur at both sides of state borders. This would imply that border counties may both gain and lose growth due to this phenomenon. If the gains and losses cancel out, the overall effect would be zero.

Because of the lack of significance of the local spillovers, we only report estimation results for the more parsimonious SD model in (1) that does not contain any local spillover terms. The resulting model is known as a dynamic Spatial Autoregressive (SAR) model.

In our SAR models, the matrix of control variables $\boldsymbol{Z}_t$ contains the employment share variables for the industries mentioned in Section 4, thus controlling for differences in sectoral composition across counties. As explained in Section 4, we use Agricultural services, forestry and fishing and Mining as the combined benchmark category in our analysis because of the missing entries in these categories.

These modelling choices result in the following final specification:

$$\boldsymbol{y}_t \quad = \quad \boldsymbol{\alpha} + \beta_t \boldsymbol{\iota}_M + \rho \boldsymbol{W}_c \boldsymbol{y}_t + \boldsymbol{X}_t^{sr} \boldsymbol{\gamma}^{sr} + \mathbf{X}_t^{lr} \boldsymbol{\gamma}^{lr} + \delta \boldsymbol{y}_{t-1} + \boldsymbol{Z}_t \boldsymbol{\lambda} + \boldsymbol{\varepsilon}_t \qquad [t = 1, \ldots, T], \tag{8}$$

where again we omit the index denoting the region for notational simplicity. The corresponding matrices of short-run and long-run marginal effects are given by

$$\boldsymbol{E}_\ell^{sr} = \gamma_\ell^{sr} (\boldsymbol{I}_M - \rho \boldsymbol{W}_c)^{-1} \boldsymbol{I}_{M \times N}; \quad \boldsymbol{E}_\ell^{lr} = \gamma_\ell^{lr} [(1-\delta) \boldsymbol{I}_M - \rho \boldsymbol{W}_c]^{-1} \boldsymbol{I}_{M \times N} \qquad [\ell = intra, inter]. \tag{9}$$

For the SAR model, we will report the summary measures (4)–(7) corresponding to the above marginal-effects matrices.

6. Estimation results

We start by using the full sample period 1970–2000 to estimate the SAR models. To investigate the structural stability of the estimated SAR models, we will later also run estimations over a range of subperiods.

6.1. Full sample

For each region, the summary measures for the marginal direct effects (‘marginal DE’) and the marginal external effects (‘marginal EE’) are displayed in Table 3. The short-run effects are summarized in the first panel and the long-run effects in the second panel. The third panel of Table 3 reports some additional estimation results, including the estimated coefficients and corresponding p-values of the temporal and spatial lags and the (pseudo) R^2 of the SAR model.

South. For the region South, both the short- and long-run effects of intrastate branching and interstate banking deregulation are significantly positive. This holds for both the direct and the spillover effects. On average, intrastate branching deregulation increased the annual expected growth rate of counties in the deregulated state by 0.99 percentage points during the first three years after deregulation and by 0.32 percentage points during subsequent years. The average marginal external effect to counties adjacent to the deregulated state are smaller, but still significant: 0.17 percentage points during the first three years and 0.05 percentage points during later years. Also hinterland counties in states adjacent to the deregulated state receive significant externalities from intrastate branching deregulation. In the short run, the average total spillover effect received by these counties equal 1.5 percentage points. In the long run, this total spillover effect reduces to 0.31 percentage points. The total spillovers to non-adjacent states are statistically significant, but economically minor.

For interstate banking deregulation, the magnitude of the effects is even more substantial. The annual average marginal direct effect on growth is 1.59 percentage points during the first three years and 2.27 during later years. The average marginal spillover effect on adjacent counties equals 0.28 percentage points in the short run and 0.33 percentage points in the long run. There are also economically relevant total spillover effects to hinterland counties in adjacent states, equal to 2.4 percentage points in the short run. In the long run, these spillover effects equal 2.20 percentage points. Although there are significant total spillovers to hinterland states, these effects are economically negligible.

The results for the South are consistent with the traditional view that banking deregulation fosters economic growth through increased competition and efficiency of the banking sector. To our best knowledge, we are the first to show that the beneficial effects of banking deregulation on economic growth are not limited to the deregulated state, but also include statistically and economically significant externalities to the growth rates of counties in surrounding states.

West and Midwest. We do not find any significant effects of deregulation for the region Midwest. For the West, the short-run effects of intrastate branching deregulation are significant at the 10% level. Both the direct and the spillover effects to adjacent and hinterland counties during the first three years after deregulation are positive and of an economically relevant magnitude. The annual average marginal direct effect on counties in the deregulated state is 0.61 percentage points, while the average spillover effect to adjacent counties equals 0.10 percentage points. The total spillover effect received by hinterland counties equals 0.54 percentage points. The total spillover effect to hinterland states is statistically significant, but economically minor. In the long-run, the marginal effects of intrastate branching deregulation are slightly smaller, but they remain significant at the 10% level.

The marginal effects of interstate banking deregulation are only significant during the first three years after deregulation, in which case they are negative. The short-run average marginal direct effect on counties in the deregulated state is -1.03 percentage points, while the average spillover effect to adjacent counties equals -0.16 percentage points. The total spillover effect received by hinterland counties equals -0.91 percentage points. Again the significant total spillovers to hinterland states are economically negligible.

The insignificant results for the Midwest are not consistent with the traditional view on the beneficial real effects of banking deregulation. The same holds for the negative effects of interstate banking deregulation on economic growth in the West; yet we note that these effects are only transitory. For intrastate branching deregulation in the West the significantly positive effects in both the short run and the long run are consistent with the traditional view, though.

Northeast. For the region Northeast, we find significantly negative average marginal effects for intrastate branching deregulation and significantly positive marginal effects for interstate banking deregulation in the short run. On average, intrastate branching deregulation decreases the annual expected growth rate of counties in the deregulated state by 0.48 percentage points during the first three years after deregulation, while interstate banking deregulation leads to a short-run average increase in expected growth of 0.64 percentage points. The short-run average spillovers to counties adjacent to the deregulated state are statistically significant but economically less important: −0.09 percentage points during for intrastate branching deregulation and 0.11 percentage points for interstate banking deregulation. Hinterland counties in adjacent states receive significant total spillovers from both types of banking deregulation equal to −0.28 and 0.31 percentage points, respectively. In the long run, only the marginal effects of intrastate branching are significantly positive, at the 10% level. The long-run average marginal effects to the deregulated state and to adjacent counties equal 0.31 and 0.06 percentage points, respectively. The total spillover effect to counties in hinterland states equals 0.21 percentage points. Again the significant total spillovers to hinterland states are economically negligible.

The results found for the Northeast are partially consistent with the traditional view on the positive real effects of banking deregulation.

Remaining estimation results. The third panel of Table 3 displays the estimated coefficients of the temporal lag (ρ) and the spatial lag (δ). Forth the Northeast, the estimate of ρ is significantly positive, while it is significantly negative for the other regions. In all cases, $\rho + \delta < 1$, revealing a stationary growth process. The estimate of δ is 0.68 for the Midwest, while it is about 0.5 for the other regions. Hence, county i's expected growth rate increases by 0.5–0.7 percentage points following an increase of 1 percentage point in the average growth rate of adjacent counties. As mentioned before, we do not find evidence for local spillover effects. Consequently, the significantly positive spatial autocorrelation in counties' growth rates fully accounts for the presence of spillover effects. This also implies that the sign of the spillover effects is always the same as that of the direct effects. The regional variation in the estimated values of ρ that we find extends the state-level results of Garrett et al. (2007) to the county level.

The third panel of Table 3 also reports the (pseudo) R^2. This R^2 is based on Elhorst (2014, p. 59) and has similar properties as the R^2 in an OLS regression. The R^2 ranges between 0.23–0.48 and is highest for the SAR model of the Midwest, the region for which the spatial autocorrelation is also highest.

Variation around the mean. Table 3 is limited to the average mean and average total marginal effects, as well as their significance. The reported summary measures in this table are averages or aggregates of the individual marginal effects calculated over two dimensions. Given deregulation in state j, the mean or total effect over a specific group of counties is calculated. Subsequently, the resulting mean effect is averaged over all states j in the region. To gain insight into the variation across both counties and states, we also provide information about the significance and magnitude of the *individual* marginal effects following banking deregulation for the counties used in each of the summary measures (4), (5), (6) and (7). For each these four groups of counties, we calculate the 2.5% and 97.5% quantiles of each type of marginal effect. We obtain the quantiles over the counties for which a significant individual marginal effect is found. Furthermore, we also calculate the percentage of counties for which a significant marginal effect is found. Throughout, we use a significance level of 10%. These additional results are displayed in Table 4. For each type of marginal effect, the first number reflects the percentage of counties with a significant marginal effect. The second figure is an interval, where the first number represents the 2.5% quantile and the second number the 97.5% quantile of the marginal effects in the group of counties under consideration.

As an example, we consider the results in Table 4 for the region South. For all counties in a deregulated state, the marginal effect is significant at the 10% level ('100%'). The 2.5% quantile of the direct effect is 0.78 percentage points, while the 97.5% quantile is 1.07 percentage points. Hence, there is considerable variation around the average mean effect of 0.99 as reported in Table 3. For all counties adjacent to a deregulated state, the spillover effects are significant ('100%'). The 2.5% quantile of these effects is 0.08 percentage points and the 97.5% quantile is 0.30 percentage points. For hinterland counties and counties in hinterland states the percentage of counties with a significant marginal effect is still high, but the range of the marginal effects for individual counties indicates that these effects are economically minor. The figures in Table 3 show that the *total* (or cumulative) amount of spillover effects received by these counties is substantial, though. We therefore conclude that the total effect is only substantial because *many* of such counties receive a small spillover.

Table 4 also reveals a strong positive relation between the percentage of counties with a significant individual marginal effect and the magnitude of the marginal effects. The weak effects for hinterland counties and states are accompanied by significance percentages of less than 100%.

Additional estimation results. We also estimate a nationwide SAR model that ignores any regional heterogeneity; see Table C.2. A comparison of the estimation results in Tables 3 and C.2 in Appendix C.1 makes clear that the conventional aggregate estimation approach masks important regional differences. This finding confirms Wall (2004) and Freeman (2005).

We also estimate a restricted version of the SAR model in (8), obtained by excluding the spatial lag ($\delta = 0$). This is the same estimator that we would obtain if we applied OLS to the restricted SAR model, after elimination of the county and time fixed-effects by means of the usual data transformation. The estimation results for the restricted SAR model are shown in Table C.3. A comparison of the estimation results in Tables 3 and Table C.3 shows that the traditional approach that ignores spatial autocorrelation in counties' growth rates leads to substantially different conclusions about the direct effects and spatial externalities of banking deregulation on economic growth. In particular, OLS ignores spatial externalities of banking deregulation and assumes that all counties in a state are affected in the same way by banking deregulation.

6.2. *Rolling-window approach*

We assess the structural stability of the SAR models by a rolling-window estimation approach. A point of attention here is the fact that each region contains an early subperiod during which the effect of interstate banking deregulation is not identifiable because none of the states in the region had yet been deregulated; see the upper panel of Figure 1. Furthermore, too small a rolling-window width will result in erratic patterns in the estimates over time and the inability to capture the effects of deregulation on economic growth, while too large a window width will yield too little variation over time and increases the risk of structural instability. Eyeballing makes clear that a window width of 20 years works well. Furthermore, the effects of both intrastate and interstate deregulation are identifiable during all resulting rolling-window subperiods, in all four regions. Additionally, the 20-year period ensures proper asymptotic behavior and sufficient accuracy of the QML estimator (Lee and Yu, 2010).

The summary measures of the direct and external effects are visualized in Figures 2 (South), 3 (West), 4 (Midwest) and 5 (Northeast). For each region, the rolling-window estimates of δ (coefficient of the temporal lag) and ρ (coefficient of the spatial lag) are displayed in Figure 6. 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 the aforementioned simulation from the asymptotic distribution of the QML estimates. To visually assess the significance of the estimated effects, we can 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. We have made similar figures including the 90% and 99%

confidence intervals, but do not show them to save space. Instead, Table 5 provides a qualitative summary of the rolling-window results, where we have added detailed information about the significance level. For completeness, Appendix C.3 provides tables in a format similar to Table 3 for the three rolling-window subperiods 1970–1990, 1975–1995, 1980–2000.

The rolling-window estimates reveal substantial temporal variation. Yet this variation is relatively small in comparison to the estimation uncertainty as captured by the confidence intervals. In particular, we observe that the significance of the direct and spillover effects varies substantially over time for especially the West and to a lesser extent for the Northeast. For the South, all effects are significant during all rolling-window periods. For the Midwest, only three rolling-periods provide significant results.

One of our findings stands out in terms of robustness over time and across the regions South, West and Northeast. This is the statistically and economically significant, positive long-run effect of intrastate branching deregulation on economic growth. We note that, with exception of two rolling-window periods for the West, also the other significant long-run effects that we find are positive. In particular, we find statistically and economically significant, positive long-run effects of interstate banking in the South. The positive long-run effects confirm the traditional view on the beneficial real impact of banking deregulation.

Figure 6 reveals some some time variation in the estimates of δ (coefficient of the temporal lag) and ρ (coefficient of the spatial lag), but the estimates remain significant during all subperiods and their sign does not switch.

6.3. Robustness checks

We have run various additional models and robustness checks. We do not present them here for the sake of brevity and clarity in presentation, but discuss their main results.[13]

We have used two alternative definitions of the four regions of the U.S.: one from the U.S. Census Bureau and one from the U.S. Bureau of Economic Analysis. These alternative definitions are also given in Appendix B. The estimated models based on these alternative definitions yield similar direct and spillover effects as before. For the Northeast, however, the significance of the results drops. This seems due to the substantially lower number of counties that the alternative definitions assign to the Northeastern region (244 and 217, respectively, instead of 299).

For the four regions defined by the U.S. Census Bureau, a regional consumer price index is available; see Table A.1 in Appendix A for more information. We have used these four indices to deflate

[13]Detailed estimation results are available upon request.

the county-level personal income data (replacing the national index) and re-estimated all models. This change hardly affects the estimation results.

For the Midwest, we find mostly insignificant effects of deregulation. We have also estimated a more parsimonious model for this region, which only includes a deregulation indicator based on the type deregulation that occurred first. As before, the latter model also distinguishes between the short- and long-run effects of deregulation. Again we find no significant real effects of deregulation in the Midwest. For completeness' sake, we have also estimated this reduces model for the other regions. For the South and Northeast, we then continue to establish significantly positive effects of deregulation, while no significant effects are found for the West.

Among others, the fixed effects in the SAR model of (8) control for time-invariant differences in expected growth between counties that are part of a Metropolitan Statistical Area (MSA) and those that are not. However, the direct and external effects may also differ between MSA and non-MSA counties. To verify whether this is the case, we have re-estimated all models after inclusion of interaction terms between the deregulation indicators and an indicator variable for MSA counties. These additional estimation results show the the effects of deregulation do not significantly differ between MSA and non-MSA counties. As an additional robustness check, we have also adjusted the definition of adjacency. According to the alternative definition, two counties are adjacent if they either share a border or belong to the same MSA. The resulting change in $\boldsymbol{W}_c$ hardly affects the estimation results.

As an additional control variable, we have collected county-level data on education from the U.S. Census Bureau for the years 1970, 1980 and 1990. This data distinguishes between four levels 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 the 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 a 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. In each region, the resulting control variable turns out insignificant in the SAR model. This may be due to the relatively limited amount of time variation in educational attainment, in combination with the inclusion of fixed effects in the SAR model that already control for time-invariant information.

The use of the adjacency matrix $\boldsymbol{W}_c$ in our SAR models hinges on the assumptions that adjacent counties have equal influence on a county's growth rate and that spatial autocorrelations beyond these adjacent counties do not matter. As a robustness check, we also estimated our models using a (row-normalized) inverse-distance instead of an adjacency matrix.[14] Following Stakhovych and Bijmolt (2009), we compare the goodness-of-fit of the models with different spatial-weight matrices in terms of their log-likelihood values. Stakhovych and Bijmolt (2009) show that the choice of the spatial-weight matrix affects the probability of detecting the true model and of accurately estimating its parameters in terms of mean squared error. They recommend a model-selection procedure based on a comparison of log-likelihood values or information criteria. In our case, the dynamic SAR models based on the adjacency matrix produce the best fit.

7. Discussion of the results

This section will provide explanations for the differential effects found for intrastate branching and interstate banking deregulation, as well as for the regional differences in the direct and external effects.

7.1. Intrastate branching versus interstate banking deregulation

The sign, significance and magnitude of the direct and external effects of banking deregulation exhibits substantial heterogeneity across counties and regions, over time, with the number of years passed since deregulation and the type of deregulation. Yet one of our empirical findings stands out in terms of robustness over time and across the regions South, West and Northeast. This is the positive long-run effect of intrastate branching deregulation on the expected growth of counties in the deregulated state itself and on the expected growth of counties adjacent to the deregulated state. We do not find such robust effects for interstate banking deregulation. The latter type of deregulation only has significantly positive long-run direct and external effects in the South.

A possible explanation for the stronger effect of intrastate branching deregulation is provided by Madura and Wiant (1994), who argue that interstate banking deregulation could be less efficiency enhancing than intrastate branching deregulation. Interstate acquisitions typically involve an expansion into markets where the acquiring banks are not well established, resulting in less potential to enhance efficiency because of little redundancy between the two entities, such as overlap in branches.

Also the timing of intrastate branching and interstate banking deregulation may play a role. In the regions Northeast and West, most states permitted intrastate branching prior to interstate banking; see Table 1. Only a few states in these regions permitted intrastate branching in the same year or very shortly after interstate banking. We can therefore view intrastate branching deregulation in the Northeast and West as ‘first-stage’ banking deregulation and interstate banking as ‘second-stage’

[14]We refer to Table A.1 in Appendix A for the source of the distance data.

deregulation. If many small banks were relatively size-inefficient, as argued by e.g. Calem (1994), the first-stage deregulation would already open the door to permit such banks to reach an efficient size or to achieve economies of scale by choosing among many merger partners or acquirers. At that point, the second-stage deregulation would face no or limited additional gains to be realized, as all the 'low-hanging fruit' had already been picked in response to intrastate branching deregulation. To the extent that increased bank efficiency resulted in e.g. lower loan rates, local economic growth would benefit from the improved banking efficiency achieved by relaxing intrastate branching restrictions.

In the South, the timing of intrastate branching and interstate banking deregulation was different, as shown by Table 1. In this region, interstate banking preceded intrastate branching deregulation in 6 out of 13 states, with a time difference of 1–6 years. The different timing of deregulation in the South difference could explain why both types of deregulation had a significantly positive long-run effects on economic growth in this region. However, the timing argument does not explain why, in the South, the direct and external effects of interstate banking deregulation on economic growth are significantly larger than those of intrastate branching deregulation; see Figure 2. An important factor seems the relatively early and long-lasting presence of the Southern regional reciprocal interstate banking compact from the mid-1980s until the implementation of the IBBEA. This compact allowed allowed banks headquartered within the region to acquire other banks headquartered within the same region but, at the same time, prevented banks headquartered outside of the region from making such acquisitions; see Swamy et al. (1996) and Hills (2007). Although other regional compacts existed, the conditions in the South were such that this region's compact could make a substantial contribution to the real economy (Hills, 2007).

7.2. Regional differences

In the Midwest, we find no significant real effects of either type of banking deregulation. A possible explanation for the lack of effect is the timing of intrastate branching and interstate banking deregulation in this region. The Midwest was a 'late mover' with respect to intrastate branching deregulation. The theory of Kane (1996) may therefore apply to a larger extent to the Midwest than to the other regions. According to this theory, deregulation is typically postponed until loopholes have been fashioned that are able to avoid the intended regulatory effects. Furthermore, in the Midwest both types of deregulation occurred relatively closely in time; see Table 1. As a result of this timing, our estimates may only reflect the aggregate effects of both types of deregulation. If these effects offset each other for some reason, the estimated aggregate effect will turn out zero.

For the Northeast we find significantly positive long-run effects of intrastate branching deregulation. By contrast, Huang (2008) finds hardly any significant effects for this type of banking deregulation during the 1970–1990 period using a matched-pairs approach. We think the main reason for the observed difference in results is our distinction between short-run and long-run effects of deregulation. In the short-run, we find significantly negative effects of deregulation for this period; see Figure 5. Some additional estimates confirm that ignoring the difference between the short-run and long-run effects leads to an 'overall' effect of intrastate branching deregulation that is not significant.

Our estimates of the impact of banking deregulation on economic growth are more significant than those of Freeman (2005), who also allows for regional and temporal differences in his state-level analysis of the impact of banking deregulation on economic growth. We expect that the difference in significance is due to the difference in the level of analysis. We use county-level instead of state-level data, which results in a much larger sample and more accurate estimation results at the regional level; especially if the sample is further reduced to study structural change. Also endogeneity of banking deregulation, which is minimized by using county-level data, may play a role here.

8. Conclusion

The goal of this study is to analyze the direct effects and spatial externalities of U.S. banking deregulation on county-level economic growth during the 1970–2000 period, while accounting for substantial regional and temporal heterogeneity in these effects.

For the regions South, West and Northeast, we consistently establish significantly positive long-run effects of intrastate branching deregulation on the expected growth of counties in the deregulated state itself (up to several percentage points) and on the expected growth of counties adjacent to the deregulated state (up to several tenths of percentage points each year). We do not find such robust effects for interstate banking deregulation, which turns out to have significantly positive long-run direct and external effects in the region South only. We establish virtually no significant real effects of banking deregulation for the Midwest.

The externalities that we find are entirely due to spatial correlation in adjacent counties' growth rates. According to our estimations, a county's expected growth rate increases by 0.5–0.7 percentage points following an increase of 1 percentage point in the average growth rate of adjacent counties. We find no evidence for other mechanisms that cause externalities. As a result, the external effects have the same sign as the direct effects and are economically relevant for counties adjacent to the deregulated state.

According to economic theory, externalities will lead to suboptimal allocations in the absence of a mechanism to internalize them (e.g., Hall, 2006). In particular, if geographic deregulation has positive externalities that are not internalized, this will result in under-provision of deregulation. Attaining the socially optimal level of deregulation would therefore require a coordinated action, which is exactly what we eventually observed for interstate banking. The emergence of various regional compacts could therefore be interpreted as an early attempt to internalize the expected externalities of interstate banking deregulation within the affected region. At the same time, the adoption of the Riegle-Neal act could be viewed as the completion of this internalization process.

We recommend policymakers and regulators to take into account these spatial and regional effects in their future decisions. In particular, we recommend them to recognize that the welfare implications of banking deregulation depend on their direct and external effects, and that optimal deregulation outcomes require any externalities to be internalized.

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Figure 1: Percentage of deregulated states and income growth in the four regions

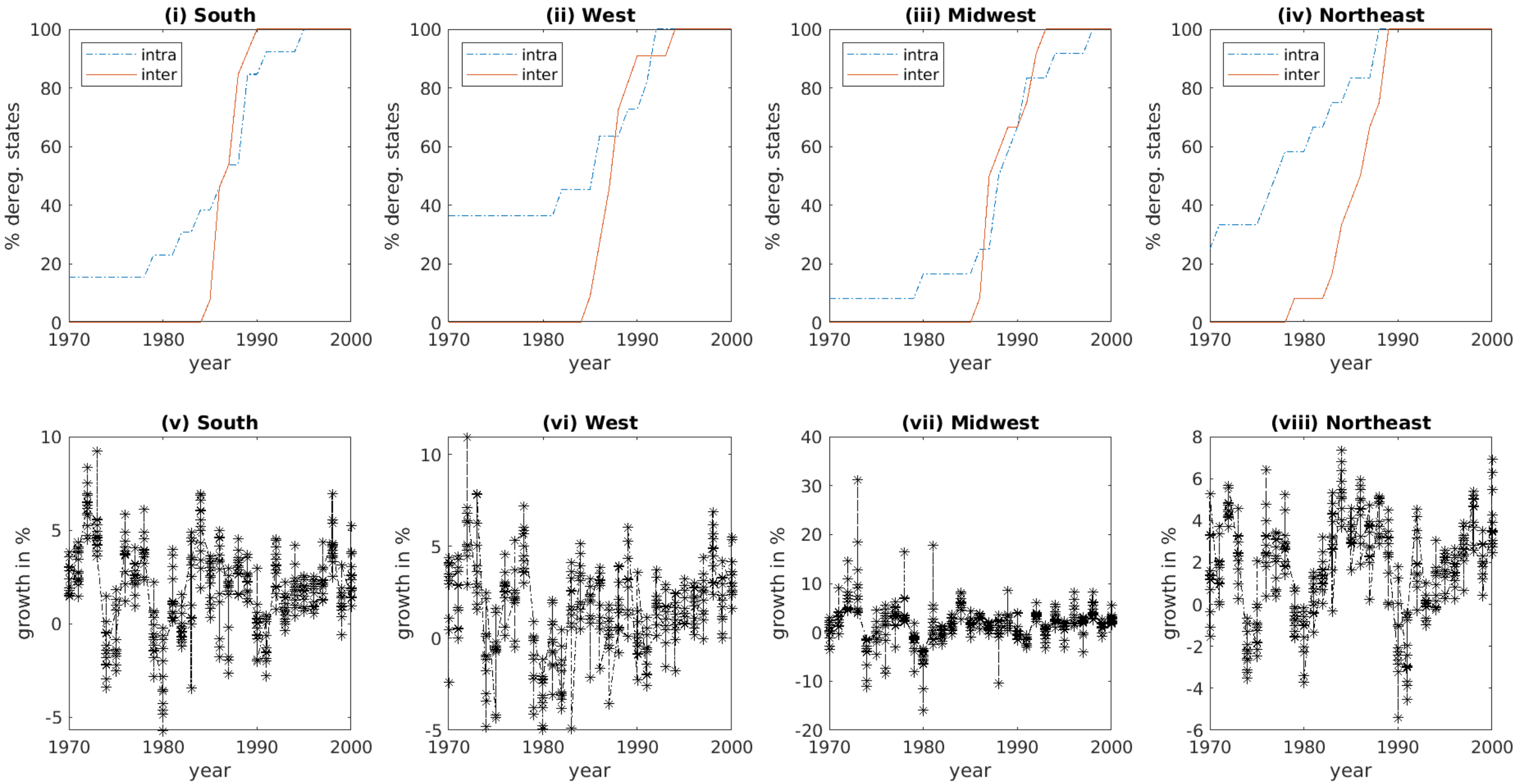
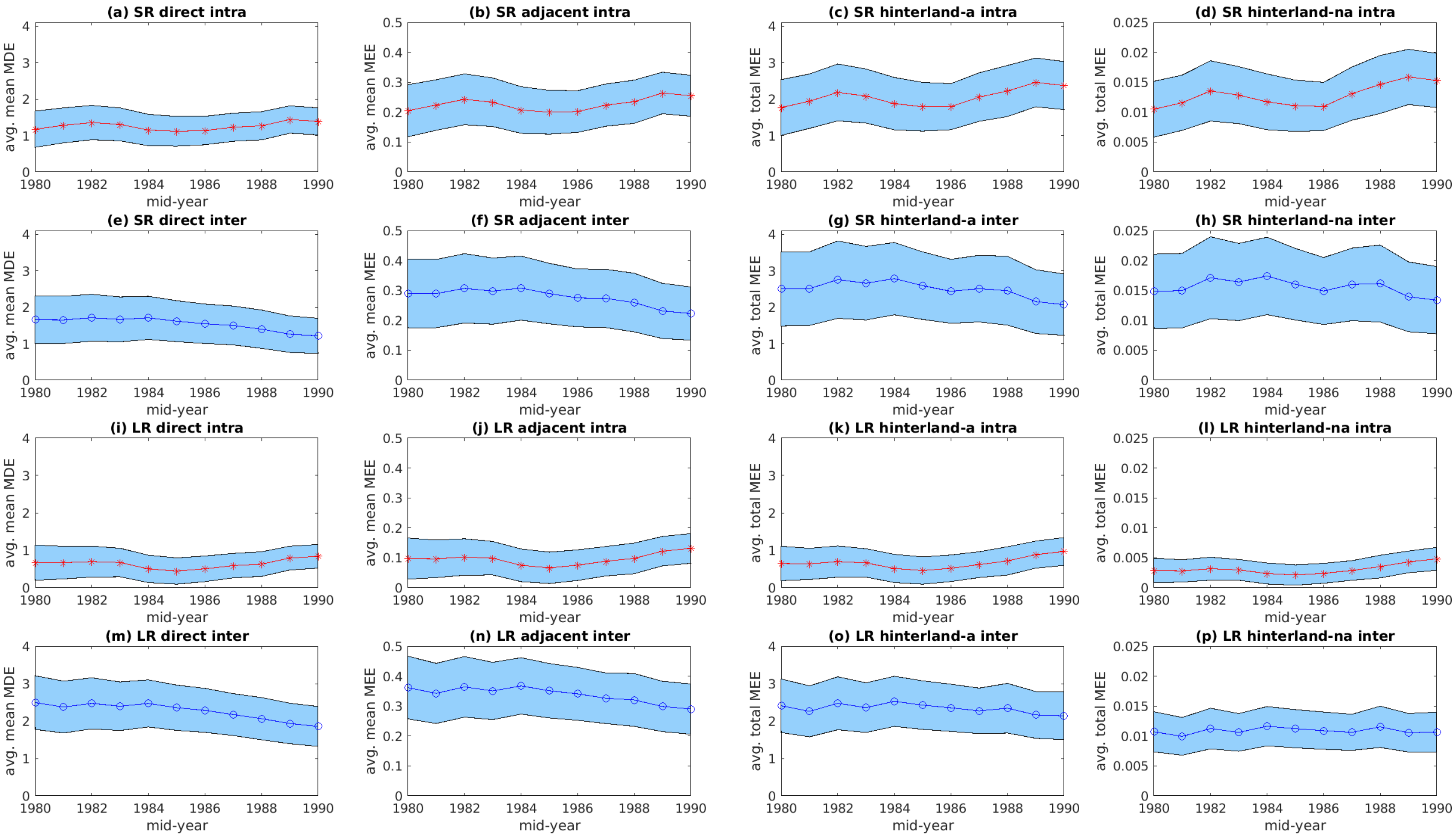
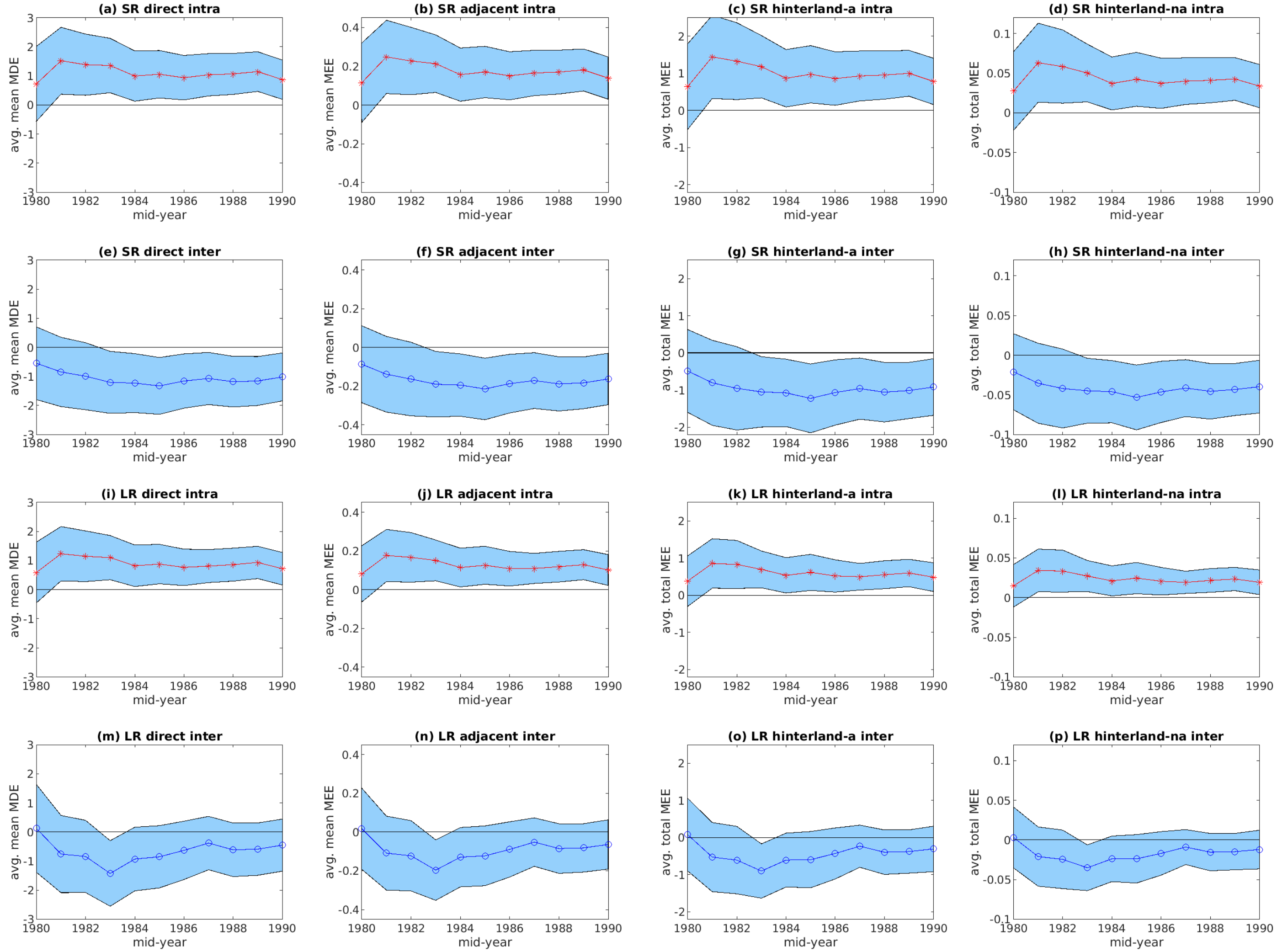


Figure 2: Rolling-window estimates for the South



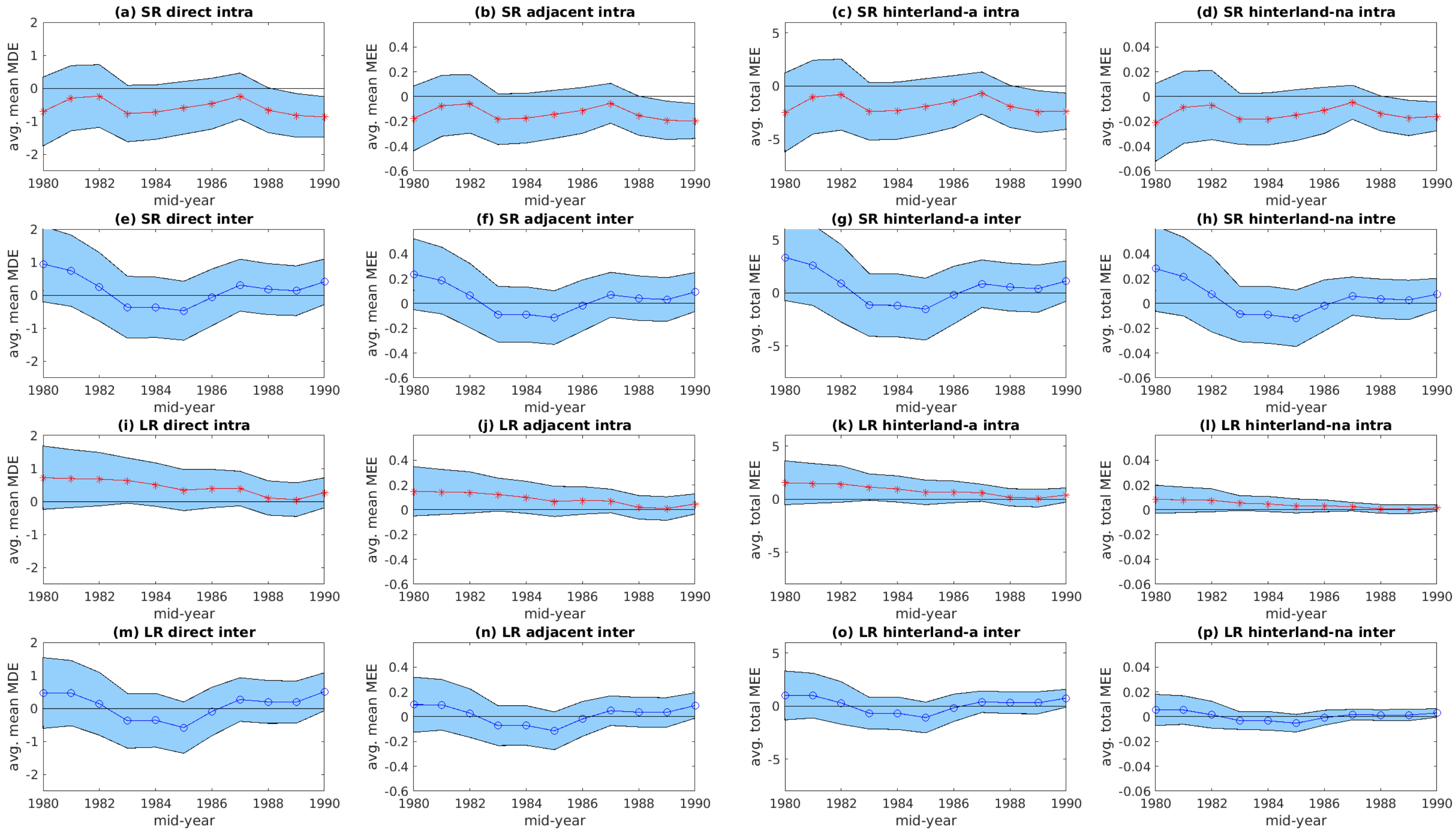
Notes: This figure visualizes the direct effects of deregulation on the counties in the deregulated state ('direct'), the effects on counties adjacent to the deregulated state ('adjacent'), the effects on hinterland counties in adjacent states ('hinterland-a') and the effects on counties in non-adjacent states ('hinterland-na'). Abbreviations used on the graphs: SR = short run, LR = long run, MDE = marginal direct effect, MEE = marginal external 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 3: Rolling-window estimates for the West



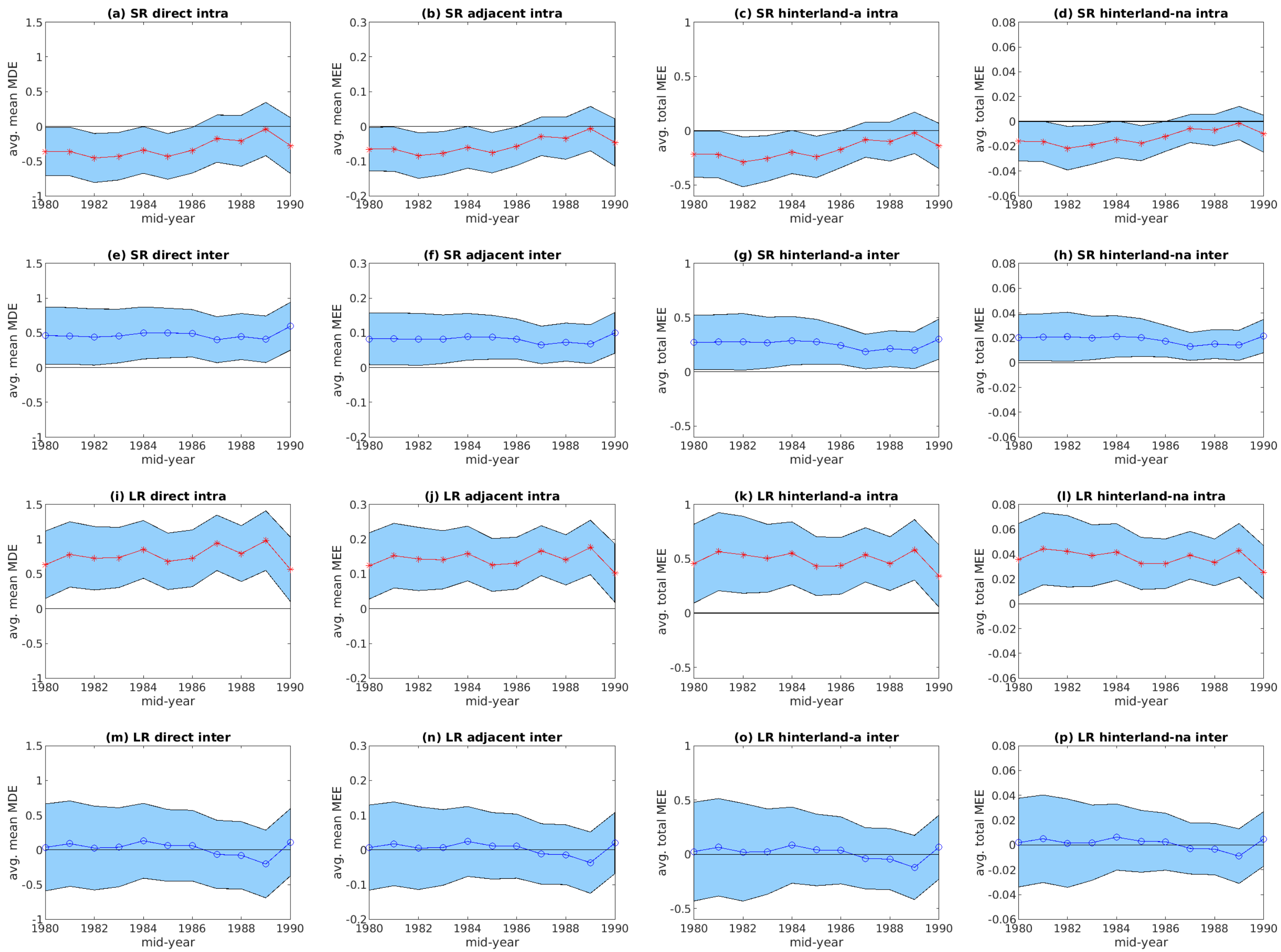
Notes: This figure visualizes the direct effects of deregulation on the counties in the deregulated state ('direct'), the effects on counties adjacent to the deregulated state ('adjacent'), the effects on hinterland counties in adjacent states ('hinterland-a') and the effects on counties in non-adjacent states ('hinterland-na'). Abbreviations used in the graphs: SR = short run, LR=long run, MDE = marginal direct effect, MEE = marginal external 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 4: Rolling-window estimates for the Midwest



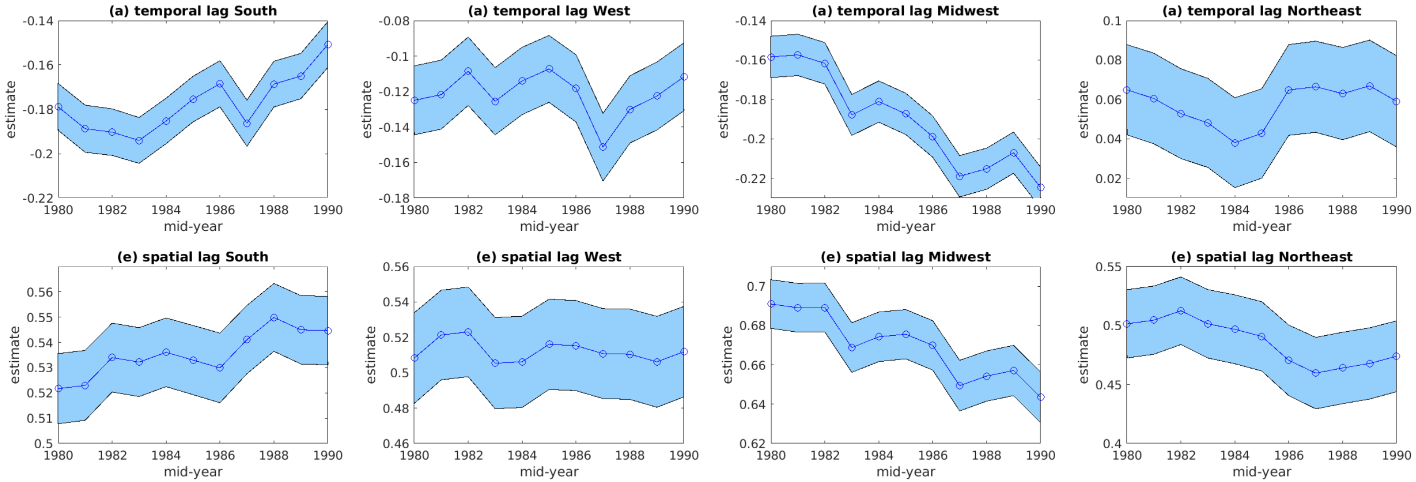
Notes: This figure visualizes the direct effects of deregulation on the counties in the deregulated state ('direct'), the effects on counties adjacent to the deregulated state ('adjacent'), the effects on hinterland counties in adjacent states ('hinterland-a') and the effects on counties in non-adjacent states ('hinterland-na'). Abbreviations used in the graphs: SR = short run, LR = long run, MDE = marginal direct effect, MEE = marginal external 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 5: Rolling-window estimates for the Northeast



Notes: This figure visualizes the direct effects of deregulation on the counties in the deregulated state ('direct'), the effects on counties adjacent to the deregulated state ('adjacent'), the effects on hinterland counties in adjacent states ('hinterland-a') and the effects on counties in non-adjacent states ('hinterland-na'). Abbreviations used in the graphs: SR = short run, LR = long run, MDE = marginal direct effect, MEE = marginal external 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 6: Rolling-window estimates for the coefficients of the temporal and spatial lag



Notes: For all coefficients, 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. The intervals in light blue represents a pointwise 95% asymptotic confidence interval.

Table 1: Timing of deregulation per region

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

Notes: For each state in each region, this table shows the year as of which intrastate branching through M&A was permitted and the year as of which interstate banking was permitted. This information has been taken from Demyanyk et al. (2007). As explained in Jayaratne and Strahan (1996), 'the' year of a particular type of deregulation refers to the year in which a state completed the process of that type of deregulation.

Table 2: Average employment shares per region(1970–2000)

Industry	South	West	Midwest	North
Construction	5.5	5.5	4.8	5.6
Manufacturing	17.9	8.6	14.0	17.8
Transportation and public utilities	3.9	4.2	4.2	4.6
Trade (wholesale + retail)	17.2	19.0	19.3	19.7
Finance, insurance and real estate	4.5	5.6	5.1	5.6
Services	17.8	20.5	18.7	23.2
Government and government enterprises	17.1	20.0	16.0	16.5
Agricultural services, forestry and fishing + Mining	21.7	22.0	22.7	12.6

Notes: The average employment shares have been calculated on the basis of the county-level data from the Bureau of Economic Analysis; see Table A.1. The missing entries have been imputed as described in Section 6. Because the industries Agricultural services, forestry and fishing and Mining contain many missing entries, we only provide their total employment share. This total has been calculated as the residual employment share (i.e., 100% minus the sum of the other industries' employment shares).

Table 3: Estimation results for the SAR model (1970–2000)

short-run effects	South	West	Midwest	Northeast
avg. mean marginal DE intra	0.99	0.61	−0.58	−0.48
	0.00	*0.08*	*0.12*	*0.00*
avg. mean marginal EE adjacent intra	0.17	0.10	−0.14	−0.09
	0.00	*0.08*	*0.12*	*0.00*
avg. total marginal EE hinterland-a intra	1.50	0.54	−1.96	−0.28
	0.00	*0.08*	*0.12*	*0.00*
avg. total marginal EE hinterland-na intra	0.01	0.02	−0.02	−0.02
	0.00	*0.08*	*0.12*	*0.00*
avg. mean marginal DE inter	1.59	−1.03	0.26	0.64
	0.00	*0.03*	*0.54*	*0.00*
avg. mean marginal EE adjacent inter	0.28	−0.16	0.07	0.11
	0.00	*0.03*	*0.54*	*0.00*
avg. total marginal EE hinterland-a inter	2.40	−0.91	0.90	0.37
	0.00	*0.04*	*0.54*	*0.00*
avg. total marginal EE hinterland-na inter	0.01	−0.04	0.01	0.03
	0.00	*0.04*	*0.54*	*0.00*
long-run effects	South	West	Midwest	Northeast
avg. mean marginal DE intra	0.32	0.49	0.21	0.31
	0.03	*0.08*	*0.41*	*0.08*
avg. mean marginal EE adjacent intra	0.05	0.07	0.04	0.06
	0.03	*0.08*	*0.41*	*0.08*
avg. total marginal EE hinterland-a intra	0.31	0.31	0.41	0.21
	0.03	*0.08*	*0.42*	*0.09*
avg. total marginal EE hinterland-na intra	0.00	0.01	0.00	0.02
	0.03	*0.08*	*0.42*	*0.09*
avg. mean marginal DE inter	2.27	−0.40	0.32	0.28
	0.00	*0.42*	*0.37*	*0.27*
avg. mean marginal EE adjacent inter	0.33	−0.05	0.06	0.05
	0.00	*0.42*	*0.37*	*0.28*
avg. total marginal EE hinterland-a inter	2.20	−0.25	0.62	0.19
	0.00	*0.42*	*0.37*	*0.28*
avg. total marginal EE hinterland-na inter	0.01	−0.01	0.00	0.01
	0.00	*0.42*	*0.37*	*0.28*
other results	South	West	Midwest	Northeast
spatial autocorr. coeff. (ρ)	0.52	0.51	0.68	0.50
	0.00	*0.00*	*0.00*	*0.00*
serial autocorr. coeff. (δ)	−0.18	−0.13	−0.18	0.06
	0.00	*0.00*	*0.00*	*0.00*
(pseudo) R^2	0.28	0.23	0.48	0.23
# counties	1,288	411	1,052	299
# years	31	31	31	31

Notes: The first two panels of this table report point estimates (normal font) and associated p-values (italic), where the latter are based on a simulation from the asymptotic distribution of the QML estimators. 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 external effect on adjacent counties ('marginal EE adjacent'), the marginal external effect on hinterland counties in adjacent states ('marginal EE hinterland-a'), and the marginal external effect on counties in non-adjacent states ('marginal EE hinterland-na'). The third panel reports some additional information, including the estimated coefficients of the temporal and spatial lags (with the corresponding p-values in italic) and the (pseudo) R^2.

Table 4: Additional estimation results for the SAR model (1970–2000)

short-run effects	South	West	Midwest	Northeast
marginal DE intra	100.0%	100.0%	0.0%	100.0%
	[0.78 1.07]	[0.47 0.68]		[−0.58 −0.37]
marginal EE adjacent intra	100.0%	100.0%	0.0%	100.0%
	[0.08 0.30]	[0.04 0.16]		[−0.17 −0.03]
marginal EE hinterland-a intra	99.3%	82.8%	0.0%	95.6%
	[0.00 0.04]	[0.00 0.03]		[−0.03 −0.00]
marginal EE hinterland-na intra	89.1%	13.6%	0.0%	66.7%
	[0.00 0.00]	[0.00 0.01]		[−0.00 −0.00]
marginal DE inter	100.0%	100.0%	0.0%	100.0%
	[1.25 1.71]	[−1.15 −0.79]		[0.48 0.76]
marginal EE adjacent inter	100.0%	100.0%	0.0%	100.0%
	[0.12 0.48]	[−0.28 −0.07]		[0.05 0.22]
marginal EE hinterland-a inter	99.3%	99.9%	0.0%	95.6%
	[0.00 0.07]	[−0.05 −0.00]		[0.00 0.03]
marginal EE hinterland-na inter	89.1%	79.5%	0.0%	72.9%
	[0.00 0.00]	[−0.00 −0.00]		[0.00 0.00]
long-run effects	South	West	Midwest	Northeast
marginal DE intra	100.0%	100.0%	0.0%	100.0%
	[0.27 0.34]	[0.39 0.54]		[0.23 0.38]
marginal EE adjacent intra	100.0%	100.0%	0.0%	100.0%
	[0.02 0.08]	[0.03 0.12]		[0.02 0.12]
marginal EE hinterland-a intra	99.2%	78.4%	0.0%	51.1%
	[0.00 0.01]	[0.00 0.02]		[0.00 0.02]
marginal EE hinterland-na intra	53.8%	10.6%	0.0%	3.9%
	[0.00 0.00]	[0.00 0.00]		[0.00 0.01]
marginal DE inter	100.0%	0.0%	0.0%	0.0%
	[1.86 2.40]			
marginal EE adjacent inter	100.0%	0.0%	0.0%	0.0%
	[0.15 0.58]			
marginal EE hinterland-a inter	99.3%	0.0%	0.0%	0.0%
	[0.00 0.07]			
marginal EE hinterland-na inter	89.8%	0.0%	0.0%	0.0%
	[0.00 0.00]			

Notes: The first panel of this table applies to the short-run marginal effects and reports two figures for each group of counties: the percentage of counties in the group under consideration for which the estimated short-run effects is significant at the 90% level and an interval representing the 2.5% and 97.5% quantiles of the estimated effects that are significant at the 90% level. The second panel does the same for the long-run effects. If the percentage of estimated effects 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 external effect on adjacent counties (‘marginal EE adjacent’), the marginal external effect on hinterland counties in adjacent states (‘marginal EE hinterland-a’), and the marginal external effect on counties in non-adjacent states (‘marginal EE hinterland-na’).

Table 5: Summary of rolling-window estimation results (sign and significance)

	South	West
short-run intra	always sign. at 1% level (+)	sign. at 1-10% level as of mid-year 1981 (+)
short-run inter	always sign. at 1% level (+)	sign. at 1-10% level as of mid-year 1982 (–)
long-run intra	always sign. at 1% level (+)	sign. at 1-10% level as of mid-year 1981 (+)
long-run inter	always sign. at 1% level (+)	sign. at 1-10% level for mid-years 1983 and 1984 (–)
spatial corr.	always sign. at 1% level (+)	always sign. at 1% level (+)
	Midwest	Northeast
short-run intra	sign. at 1-10% level as of mid-year 1988 (–)	sign. at 1-5% level up to mid-year 1986 (–)
short-run inter	never sign.	always sign. at 1-5% level (+)
long-run intra	never sign.	always sign. at 1-10% level (+)
long-run inter	sign. at 10% level for mid-year 1990 (+)	never sign.
spatial corr.	always sign. at 1% level (+)	always sign. at 1% level (+)

Notes: This tables summarizes the rolling-window estimation results. The reported significance applies to both the direct and external effects.

APPENDIX

A. Data sources

Table A.1 on the next page contains a list of data sources used to collect the data for this study.

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`
Stata package (state-level and county-level adjacency matrices)	`https://econpapers.repec.org/software/bocbocode/s448405.htm`
All Urban Consumers Price Index, not seasonally adjusted	`https://beta.bls.gov/dataQuery/find?fq=survey:[cu]&s=popularity:D`
Employment data	`https://www.bea.gov/data/employment/employment-county-metro-and-other-areas`
Robustness checks	
Regional Price Indices (South, West, Midwest, Northeast)	`https://www.bls.gov/cpi/regional-resources.htm`
List of Statistical Metropolitan Areas	`http://www.bea.gov/regional/docs/msalist.cfm`
Distances between counties	`https://www.nber.org/data/county-distance-database.html`

B. Regions and divisions of the U.S.

This appendix explains how we defined the four main regions. Throughout, we exclude the states Alaska, Hawai, and the District of Columbia from our analysis, as explained in the main text. We do not exclude the states Delaware and South Dakota, which is sometimes done in the existing literature. In line with Jayaratne and Strahan (1996), we find that our growth regressions are not substantially affected by the inclusion or exclusion of Delaware. Furthermore, we find the same for South Dakota.

The four regions defined by Jayaratne and Strahan (1996) are:

- Region South: Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia.
- Region West: Arizona, California, Colorado, Idaho, Nevada, New Mexico, Montana, Oregon, Utah, Washington, Wyoming.
- Region Midwest: Illinois, Indiana, Iowa, Kansas, Michigan, Minnesota, Missouri, Nebraska, North Dakota, Ohio, South Dakota, and Wisconsin.
- Region Northeast: Connecticut, Delaware, Maine, Maryland, Massachusetts, New Hampshire, Rhode Island, Vermont, New Jersey, New York, Pennsylvania, West Virginia.

As an alternative, we use the following definitions from the U.S. Bureau of Economic Analysis[15]:

- New England: Connecticut, Maine, Massachusetts, New Hampshire, Rhode Island, and Vermont.
- Mideast: Delaware, New Jersey, Maryland, New York, and Pennsylvania.
- Great Lakes: Illinois, Indiana, Michigan, Ohio, and Wisconsin.
- Plains: Iowa, Kansas, Minnesota, Missouri, Nebraska, North Dakota and South Dakota.
- Southeast: Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Virginia and West Virginia.
- Southwest: Arizona, New Mexico, Oklahoma, and Texas.
- Rocky Mountain: Colorado, Idaho, Montana, Utah, Wyoming.
- Far West: California, Nevada,Oregon, and Washington.

[15]See https://apps.bea.gov/regional/docs/regions.cfm.

We group these into four main regions:

- Region South: Southeast and Southwest.
- Region West: Rocky Mountain and Far West.
- Region Midwest: Great Lakes and Plains.
- Region Northeast: New England and Mideast.

The resulting number of counties assigned to each of the four regions equals 1389, 365, 1052 and 244, respectively.

Alternatively, we use the following divisions from the U.S. Census Bureau[16]:

- New England: Connecticut, Maine, Massachusetts, New Hampshire, Rhode Island, and Vermont.
- Mid-Atlantic: New Jersey, New York, and Pennsylvania.
- South Atlantic: Delaware, Florida, Georgia, Maryland, North Carolina, South Carolina, Virginia and West Virginia.
- East North Central: Illinois, Indiana, Michigan, Ohio, and Wisconsin.
- West North Central: Iowa, Kansas, Minnesota, Missouri, Nebraska, North Dakota, South Dakota.
- East South Central: Alabama, Kentucky, Mississippi, and Tennessee.
- West South Central: Arkansas, Louisiana, Oklahoma, and Texas.
- Mountain: Arizona, Colorado, Idaho, Montana, Nevada, New Mexico, Utah, and Wyoming.
- Pacific: Alaska, California, Oregon, and Washington.

On the basis of these divisions, the U.S. Census Bureau defines the four main regions as:

- Region South: South Atlantic, East South Central and West South Central.
- Region West: Mountain and Pacific.
- Region Midwest: East North Central and West North Central.
- Region Northeast: New England and Mid-Atlantic.

The resulting number of counties assigned to each region equals 1370, 411, 1052 and 217, respectively.

[16]See https://www2.census.gov/geo/pdfs/maps-data/maps/reference/us_regdiv.pdf.

C. Additional estimation results

This section provides thee types of additional estimation results: (1) nationwide estimates for the SAR models (ignoring regional differences), (2) estimation results for the restricted SAR model that ignores spatial autocorrelation in growth rates (which is equivalent to OLS estimation) and (3) estimation results for three subperiods.

C.1. Comparison to nationwide estimates

Table C.2 reports the estimation results for the nationwide SAR model that ignores any regional differences. By comparing the estimation results in Tables 3 and C.2, we observe that aggregate estimation masks important regional differences. First, the aggregate estimate of the spatial autocorrelation is 0.64, which is close to that of the Midwest but substantially different from the spatial autocorrelations of about 0.5 found for the other three regions. The aggregate estimates for the effects of intrastate branching deregulation are insignificant in the short run. So also here the Midwest seems to dominate the aggregate results. The aggregate estimates of the long-run effects of intrastate branching deregulation are significantly positive. The long-run average mean direct effect of intrastate branching deregulation equals 0.33 percentage points, while the spillover effect to adjacent counties equals 0.06 percentage point. The latter figures are relatively close to the significant effects that the regional SAR models found for the South and Northeast. Hence, again the aggregate estimates mask the regional differences in the significance of the long-run effects of intrastate branching deregulation that our regional SAR models found. Also the long-run aggregate estimates of the spillover effects of intrastate branching deregulation to hinterland counties and states mask such regional differences.

The masking problem is even worse for the aggregate estimates of the effects of interstate banking deregulation. Table 3 does not only show regional differences in the significance but also in the magnitude of the estimated effects. At the aggregate level, both the direct and spillover effects of interstate banking deregulation do not capture the substantial effects our regional models found in the short run for the South and West and in the long run for the South.

C.2. Comparison to OLS

We compare the SAR estimation results to the traditional approach that ignores spatial autocorrelation in counties' growth rates. Table C.3 reports the estimation results for the restricted version of the SAR model in (8), obtained by excluding the spatial lag ($\delta = 0$). This is the same estimator that we would obtain if we applied OLS to the restricted SAR model, after elimination of the county and time fixed-effects by means of the usual data transformation. That is, the ML estimator reduces to the OLS fixed-effects estimator in the absence of a spatial lag.

In the restricted SAR model, the short-run direct effects of banking deregulation are given by γ^{sr} and the long-run direct effects are equal to $\gamma^{lr}/(1-\rho)$. Hence, according to the restricted SAR model, the direct effects of banking deregulation do not vary across either counties or states. As shown by Table 4, there is such variation according to the unrestricted SAR model. Furthermore, due to the absence of a spatial lag in the restricted SAR model, there are no spillover effects.

A comparison of Tables 3 (unrestricted) and C.3 (restricted) reveals differences between the two models in terms of both the significance and magnitude of the direct effects. For example, for the region Midwest, the short-run average mean direct effect of intrastate branching deregulation and the long-run average mean direct effect of interstate banking deregulation are insignificant. According to the restricted SAR model, both effects are significant, though. Furthermore, Table 4 illustrates that the unrestricted SAR model allow for state- and county-specific marginal direct and spillover effects of deregulation on economic growth. In the restricted model, by contrast, there is no such variation; the estimated marginal effects are the same across all counties and each state. In the South, West and Northeast the unrestricted SAR model establishes statistically and economically significant spillover effects to adjacent counties as shown in Tables 3 and 4. The restricted model a priori assumes that no such externalities exist. On the basis of the restricted model, we would therefore conclude that banking deregulation only affects the economic growth of counties in the deregulated state itself.

C.3. Subperiod estimations

Tables C.4, C.5 and C.6 on the next pages contain the estimation results for the subperiods 1970–1990, 1975–1995 and 1980–2000.

Table C.2: Estimation results for the nationwide model (1970–2000)

	short run	long run
avg. mean marginal DE intra	0.22	0.33
	0.16	*0.00*
avg. mean marginal EE adjacent intra	0.05	0.06
	0.16	*0.00*
avg. total marginal EE hinterland-a intra	0.58	0.53
	0.16	*0.00*
avg. total marginal EE hinterland-na intra	0.02	0.02
	0.16	*0.00*
avg. mean marginal DE inter	0.45	0.79
	0.02	*0.00*
avg. mean marginal EE adjacent inter	0.10	0.15
	0.02	*0.00*
avg. total marginal EE hinterland-a inter	1.19	1.28
	0.02	*0.00*
avg. total marginal EE hinterland-na inter	0.05	0.04
	0.02	*0.00*
other results		
spatial autocorr. coeff. (ρ)	0.64	
	0.00	
serial autocorr. coeff. (δ)	−0.17	
	0.00	
(pseudo) R^2	0.39	
# counties	3,050	
# years	31	

Notes: This table reports the estimated short-run and long-run direct and marginal external effects (ME). The point estimates are in normal font, while the associated p-values are in italic. The lower panel reports some additional information, including the estimated coefficients of the spatial and temporal lags (with the corresponding p-values in italic) and the (pseudo) R^2.

Table C.3: Estimation results for the OLS model (1970–2000)

	South	West	Midwest	Northeast
short-run marginal DE intra	0.97	0.47	−0.67	−0.50
	0.00	*0.02*	*0.00*	*0.00*
short-run marginal DE inter	1.71	−0.78	0.25	0.76
	0.00	*0.01*	*0.21*	*0.00*
long-run marginal DE intra	0.33	0.40	0.10	0.45
	0.00	*0.02*	*0.45*	*0.00*
long-run marginal DE inter	2.55	−0.20	0.50	0.45
	0.00	*0.53*	*0.01*	*0.01*
ρ (serial autocorr.)	−0.25	−0.18	−0.31	0.07
	0.00	*0.00*	*0.00*	*0.00*
log lik OLS model	-203,371	-70,301	-185,577	-32,262
log lik SAR model	-195,967	-68,213	-171,796	-30,755
AIC OLS model	203,381	70,311	185,587	32,272
AIC SAR model	195,979	68,225	171,808	30,767
pseudo R^2 OLS model	0.07	0.04	0.10	0.04
pseudo R^2 SAR model	0.28	0.23	0.48	0.23
# counties	1,288	411	1,052	299
# years	31	31	31	31

Notes: The first two panels of this table report point estimates (normal font) and associated p-values (italic) for the short-run and long-run marginal direct effects (DE) based on the restricted SAR model with no spatial lag ($\rho = 0$). The estimation results of the restricted SAR model are equal to those based on OLS. The third panel reports some additional information including the estimated coefficients of the temporal lag (with corresponding p-values in italic), the values of the log likelihood, the AIC and the (pseudo) R^2.

Table C.4: Estimation results for the SAR model: 1970–1990

short-run effects	South	West	Midwest	Northeast
avg. mean marginal DE intra	1.17	0.73	−0.70	−0.37
	0.00	*0.27*	*0.19*	*0.04*
avg. mean marginal EE adjacent intra	0.20	0.12	−0.18	−0.07
	0.00	*0.27*	*0.19*	*0.04*
avg. total marginal EE hinterland-a intra	1.77	0.64	-2.49	−0.22
	0.00	*0.27*	*0.19*	*0.05*
avg. total marginal EE hinterland-na intra	0.01	0.03	−0.02	−0.02
	0.00	*0.28*	*0.19*	*0.05*
avg. mean marginal DE inter	1.66	−0.55	0.94	0.46
	0.00	*0.39*	*0.11*	*0.03*
avg. mean marginal EE adjacent inter	0.29	−0.09	0.24	0.08
	0.00	*0.39*	*0.11*	*0.03*
avg. total marginal EE hinterland-a inter	2.50	−0.49	3.34	0.27
	0.00	*0.39*	*0.11*	*0.03*
avg. total marginal EE hinterland-na inter	0.01	−0.02	0.03	0.02
	0.00	*0.39*	*0.11*	*0.04*
long-run effects	South	West	Midwest	Northeast
avg. mean marginal DE intra	0.67	0.59	0.72	0.63
	0.01	*0.27*	*0.14*	*0.01*
avg. mean marginal EE adjacent intra	0.10	0.08	0.15	0.12
	0.01	*0.27*	*0.14*	*0.01*
avg. total marginal EE hinterland-a intra	0.65	0.38	1.54	0.46
	0.01	*0.27*	*0.14*	*0.01*
avg. total marginal EE hinterland-na intra	0.00	0.01	0.01	0.04
	0.01	*0.28*	*0.14*	*0.02*
avg. mean marginal DE inter	2.49	0.12	0.47	0.04
	0.00	*0.87*	*0.39*	*0.91*
avg. mean marginal EE adjacent inter	0.36	0.02	0.10	0.01
	0.00	*0.87*	*0.39*	*0.91*
avg. total marginal EE hinterland-a inter	2.41	0.08	1.00	0.02
	0.00	*0.88*	*0.39*	*0.92*
avg. total marginal EE hinterland-na inter	0.01	0.00	0.01	0.00
	0.00	*0.88*	*0.40*	*0.92*
other results	South	West	Midwest	Northeast
spatial autocorr. coeff. (ρ)	0.52	0.51	0.69	0.50
	0.00	*0.00*	*0.00*	*0.00*
serial autocorr. coeff. (δ)	−0.18	−0.12	−0.16	0.06
	0.00	*0.00*	*0.00*	*0.00*
(pseudo) R^2	0.29	0.23	0.47	0.25
# counties	1,288	411	1,052	299
# years	21	21	21	21

Notes: The first two panels of this table report point estimates (normal font) and associated p-values (italic), where the latter are based on a simulation from the asymptotic distribution of the QML estimators. 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 external effect on counties adjacent to the deregulated state ('marginal EE adjacent'), the marginal external effect on hinterland counties in adjacent states ('marginal EE hinterland-a'), and the marginal EE on counties in non-adjacent states ('marginal EE hinterland-na'). The third panel reports some additional information, including the estimated coefficients of the temporal and spatial lag (with corresponding p-values in italic) and the (pseudo) R^2.

Table C.5: Estimation results for the SAR model: 1975–1995

short-run effects	South	West	Midwest	Northeast
avg. mean marginal DE intra	1.12	1.06	−0.59	−0.43
	0.00	0.01	0.15	0.01
avg. mean marginal EE adjacent intra	0.20	0.17	−0.14	−0.08
	0.00	0.01	0.15	0.01
avg. total marginal EE hinterland-a intra	1.79	0.98	−1.92	−0.24
	0.00	0.01	0.15	0.01
avg. total marginal EE hinterland-na intra	0.01	0.04	−0.01	−0.02
	0.00	0.01	0.15	0.01
avg. mean marginal DE inter	1.62	−1.33	−0.47	0.50
	0.00	0.01	0.30	0.01
avg. mean marginal EE adjacent inter	0.29	−0.21	−0.11	0.09
	0.00	0.01	0.30	0.01
avg. total marginal EE hinterland-a inter	2.59	−1.23	−1.53	0.28
	0.00	0.01	0.30	0.01
avg. total marginal EE hinterland-na inter	0.02	−0.05	−0.01	0.02
	0.00	0.01	0.31	0.01
long-run effects	South	West	Midwest	Northeast
avg. mean marginal DE intra	0.44	0.88	0.35	0.68
	0.01	0.01	0.28	0.00
avg. mean marginal EE adjacent intra	0.07	0.13	0.07	0.13
	0.01	0.01	0.28	0.00
avg. total marginal EE hinterland-a intra	0.46	0.61	0.64	0.43
	0.01	0.01	0.28	0.00
avg. total marginal EE hinterland-na intra	0.00	0.02	0.00	0.03
	0.01	0.01	0.28	0.00
avg. mean marginal DE inter	2.36	−0.85	−0.58	0.06
	0.00	0.12	0.14	0.81
avg. mean marginal EE adjacent inter	0.35	−0.12	−0.11	0.01
	0.00	0.12	0.14	0.81
avg. total marginal EE hinterland-a inter	2.43	−0.60	−1.08	0.04
	0.00	0.12	0.15	0.81
avg. total marginal EE hinterland-na inter	0.01	−0.02	−0.01	0.00
	0.00	0.13	0.15	0.82
other results	South	West	Midwest	Northeast
spatial autocorr. coeff. (ρ)	0.53	0.52	0.68	0.49
	0.00	0.00	0.00	0.00
serial autocorr. coeff. (δ)	−0.18	−0.11	−0.19	0.04
	0.30	0.24	0.47	0.24
(pseudo) R^2	0.00	0.00	0.00	0.00
# counties	1,288	411	1,052	299
# years	21	21	21	21

Notes: The first two panels of this table report point estimates (normal font) and associated p-values (italic), where the latter are based on a simulation from the asymptotic distribution of the QML estimators. 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 external effect on counties adjacent to the deregulated state (‘marginal EE adjacent’), the marginal external effect on hinterland counties in adjacent states (‘marginal EE hinterland-a’), and the marginal EE on counties in non-adjacent states (‘marginal EE hinterland-na’). The third panel reports some additional information, including the estimated coefficients of the temporal and spatial lag (with corresponding p-values in italic) and the (pseudo) R^2.

Table C.6: Estimation results for the SAR model: 1980–2000

short-run effects	South	West	Midwest	Northeast
avg. mean marginal DE intra	1.39	0.87	−0.87	−0.28
	0.00	*0.01*	*0.01*	*0.18*
avg. mean marginal EE adjacent intra	0.25	0.14	−0.20	−0.05
	0.00	*0.01*	*0.01*	*0.18*
avg. total marginal EE hinterland-a intra	2.37	0.78	-2.38	−0.14
	0.00	*0.01*	*0.01*	*0.19*
avg. total marginal EE hinterland-na intra	0.02	0.03	−0.02	−0.01
	0.00	*0.02*	*0.01*	*0.19*
avg. mean marginal DE inter	1.21	−1.02	0.40	0.59
	0.00	*0.02*	*0.25*	*0.00*
avg. mean marginal EE adjacent inter	0.22	−0.16	0.09	0.10
	0.00	*0.02*	*0.25*	*0.00*
avg. total marginal EE hinterland-a inter	2.07	−0.92	1.11	0.30
	0.00	*0.02*	*0.25*	*0.00*
avg. total marginal EE hinterland-na inter	0.01	−0.04	0.01	0.02
	0.00	*0.02*	*0.25*	*0.00*
long-run effects	South	West	Midwest	Northeast
avg. mean marginal DE intra	0.84	0.72	0.26	0.57
	0.00	*0.01*	*0.25*	*0.02*
avg. mean marginal EE adjacent intra	0.13	0.10	0.05	0.10
	0.00	*0.01*	*0.25*	*0.02*
avg. total marginal EE hinterland-a intra	0.97	0.49	0.39	0.34
	0.00	*0.01*	*0.26*	*0.02*
avg. total marginal EE hinterland-na intra	0.00	0.02	0.00	0.03
	0.00	*0.02*	*0.26*	*0.02*
avg. mean marginal DE inter	1.86	−0.45	0.51	0.11
	0.00	*0.32*	*0.08*	*0.66*
avg. mean marginal EE adjacent inter	0.29	−0.06	0.09	0.02
	0.00	*0.32*	*0.08*	*0.66*
avg. total marginal EE hinterland-a inter	2.14	−0.31	0.74	0.06
	0.00	*0.33*	*0.09*	*0.67*
avg. total marginal EE hinterland-na inter	0.01	−0.01	0.00	0.00
	0.00	*0.33*	*0.09*	*0.67*
other results	South	West	Midwest	Northeast
spatial autocorr. coeff. (ρ)	0.54	0.51	0.64	0.47
	0.00	*0.00*	*0.00*	*0.00*
serial autocorr. coeff. (δ)	−0.15	−0.11	−0.22	0.06
	0.30	*0.23*	*0.47*	*0.22*
(pseudo) R^2	0.00	0.00	0.00	0.00
# counties	1,288	411	1,052	299
# years	21	21	21	21

Notes: The first two panels of this table report point estimates (normal font) and associated p-values (italic), where the latter are based on a simulation from the asymptotic distribution of the QML estimators. 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 external effect on counties adjacent to the deregulated state ('marginal EE adjacent'), the marginal external effect on hinterland counties in adjacent states ('marginal EE hinterland-a'), and the marginal EE on counties in non-adjacent states ('marginal EE hinterland-na'). The third panel reports some additional information, including the estimated coefficients of the temporal and spatial lag (with corresponding p-values in italic) and the (pseudo) R^2.