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Evaluating the real effect of bank branching deregulation: Comparing contiguous counties across US state borders ☆

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Abstract

This paper proposes a new methodology to evaluate the economic effect of state-specific policy changes, using bank-branching deregulations in the US as an example. The new method compares economic performance of pairs of contiguous counties separated by state borders, where on one side restrictions on statewide branching were removed relatively earlier, to create a natural "regression discontinuity" setup. The study uses a total of 285 pairs of contiguous counties along 38 segments of such regulation change borders to estimate treatment effects for 23 separate deregulation events taking place between 1975 and 1990. To distinguish real treatment effects from those created by data-snooping and spatial correlations, fictitious placebo deregulations are randomized (permutated) on another 32 segments of non-event borders to establish empirically a statistical table of critical values for the estimator. The method determines that statistically significant growth accelerations can be established at a $>90\%$ confidence level in five (and none prior to 1985) out of the 23 deregulation events examined. "Hinterland counties" within the still-regulated states, but farther away from the state borders, are used as a second control group to consider and reject the possibility that cross-border spillover of deregulation effects may invalidate the empirical design.

JEL classification: G21; G28; O43

Keywords: Banking deregulation; Economic growth; Regression discontinuity

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1. Introduction

Liberalization of the banking sector is generally shown to have had a positive impact on local economic growth (Levine, 2005, provides a review of the related literature). In the United States, intrastate branching regulations imposed by state legislatures used to restrict a bank from making statewide branching expansions, and a bank holding company from folding its subsidiaries in different counties into a single operation entity. Beginning in the mid-1970s, individual states started to lift these restrictions at different times in a piecemeal fashion. The staggered nature of the deregulation timings has been exploited by researchers to study the effects of banking deregulation on the local economy, as the restriction on *inter*state branching (removed only much later) essentially produced 50 segregated banking systems within the United States, one for each state.[1]

Theoretically, removal of restrictions on bank entry and expansion could facilitate mergers and acquisitions, promote competition, and increase bank efficiency, thereby helping local economic growth. Jayaratne and Strahan (1997) find that the relaxing of restrictions on bank expansion led to greater bank efficiency, although they find no increase in credit supply. Using state-level data, Jayaratne and Strahan (1996) provide well-cited evidence that deregulation was typically associated with faster local economic growth. Strahan (2003) provides a good review of the available evidence in favor of the positive effects of such deregulation.

Notwithstanding, we believe that it remains an open empirical question whether regulation of commercial banks' expansion was a binding constraint on the growth of the local economy, and whether removal of this restriction created immediate and significant economic benefits for the local economy. Existing studies tend to find a significant positive effect from the deregulation of branching on the local economy, but most of these studies use a state as the unit of analysis. We argue that this practice is open to a number of econometric problems. Individual states deregulated branching in waves; in very few cases (which are the subject of our study) did states in the same region deregulate at very different times. To increase the degrees of freedom in regressions, previous studies typically have to use very diverse states from different regions to form the treatment and the control groups, comparing, for example, Texas to Michigan, although the two states are not synchronized in their business cycles. After controlling for regional effects, Freeman (2002) and Wall (2004) find that the positive effect of banking deregulation on the real economy is not an unambiguous result; in some regions the effect is positive, whereas in many more others, it is actually negative. Furthermore, banking deregulation could be induced by an expectation of future growth opportunities (unobservable to econometricians), which could create a spurious correlation between banking deregulation and future growth accelerations. Therefore, it is possible that the episodes of growth accelerations identified by previous studies could be a manifestation of heterogeneity in different regions' growth paths (Garrett, Wagner, and Wheelock, 2004), or difference of expected future growth opportunities across states, independent of and not caused by, changes in state-level banking regulations.

This study uses a novel procedure to establish whether a branching deregulation event produces a significantly positive treatment effect or not. The new method compares economic performance of contiguous counties separated by state borders in cases in which one state deregulated intrastate branching earlier than the other. Because these counties are immediately adjacent neighbors, they are expected to be similar in both observable, and more importantly, unobservable conditions, and to follow similar growth paths in the absence of regulation or policy changes. This study is not the first to use this geographic-matching methodology to conduct policy evaluations,[2] but it adopts an even more precise method in that it carefully matches and

[1]For a long period of time in the United States, an otherwise unified nation, banks from other states were viewed as "foreign." Interstate banking regulations used to strictly forbid out-of-state banks from acquiring a state's incumbent banks, let alone opening new in-state branches. Until 1994, even if a state amended its law and started to allow interstate banking, newly acquired banking assets could not be folded into the acquirer's banking operations outside the state.

[2]Fox (1986) finds that sales tax differences between neighboring states affect retail sales in border counties. Card and Kruger (1994) look at the New Jersey–Pennsylvania border area to examine the effects of an increase in the minimum wage. Black (1999) examines the price of houses located on school district boundaries and finds that parents are willing to pay 2.5% more for a 5% increase in test scores. Using a similar methodology, Holmes (1998) finds that as a group, counties on the pro-business side of state borders experience faster manufacturing growth.

compares each "treated" county with only its own paired neighbor across the border, instead of roughly comparing two strips of land on opposite sides of a long border.

Using a county as the unit of analysis can minimize endogeneity problems. Kroszner and Strahan (1999) find that the relative strength of winners (large banks and small, bank-dependent firms) and losers (small banks and the rival insurance firms) in bank deregulation can explain the timing of branching deregulation *across states*. In this study, however, it is unlikely that economic conditions and the financial sector's structure in a county can influence regulatory decisions made by the state legislature, which has to accommodate the interests of all constituencies, not only a small group of border counties. Furthermore, the lack of commuting labor movement across most state borders (according to the "Journal to Work" census) ensures that a regulatory change that affects only one side of the border should translate into perceivable *short-term* difference in local incomes relatively easily observable to econometricians.[3] Note that the New York–New Jersey border is not included in our sample.

Using state-level intrastate branching deregulation events as quasi-experiments, this study focuses on how the removal of restrictions on statewide branching affects growth by comparing the growth rate of per capita income on opposite sides of *regulation change borders*, after adjusting for both an income gap and a growth opportunity gap that could potentially bias the point estimate of treatment effects. Fictitious *placebo deregulations* are randomized on out-of-sample non-event borders, where dramatic cross-border regulatory differences associated with regulation change borders did not exist, to empirically obtain a statistical table of critical values that we use to statistically distinguish real treatment effects from the results of potential data-snooping. The same method also helps us adjust the critical values for spatial correlations of treatment effects within a chain of neighboring county pairs (which could bias the standard errors downward).

Of the 23 deregulation events that took place during the 15-year period from 1975 to 1990, this study finds statistically significant growth accelerations after deregulation in only five of them, and none of these events took place prior to 1985. These results call for further research on why regulation and deregulation of commercial banks' geographic expansion had little measurable effect on the local economy. In this paper we provide several plausible explanations for the findings.

First, the US economy is much less dependent on banks than are continental European economies, and thus, burdensome restrictions imposed on banks could have limited real effects. Bank lending may not be special or critical, for example, because other sources of financing can easily replace lending by commercial banks (Marquis, 2001). Further, empirical evidence suggests that bank loans have no significant impact on economic outputs.[4] Considering the important role that nonbank financial institutions (which have not been geographically restricted) and capital markets already played vis-à-vis commercial banks before deregulation, it is not clear whether commercial banks provided any credit service to the economy that could not have been replaced by nonbank financial institutions.[5] Moreover, post-

[3]Note that this divide does not make the contiguous-county economies on opposite sides of state borders isolated from each other in the long run. In responding to branching regulations that had been in place since the Great Depression, no frictions were great enough to hold off necessary economic adjustment for such a long period of time. By the time a deregulation event took place in the 1980s, the two contiguous counties in question were likely to be in an equilibrium state already with respect to observable and unobservable local factors, including the then-prevailing banking regulation arrangements. After a banking market liberalization on one side of the state border, the adjustment taking place in the newly deregulated county (presumably toward a higher income level than its neighbor if deregulation should have positive effects) and the cross-border growth rate difference generated by this unilateral adjustment should be perceivable in the short term (e.g., five years) until a new equilibrium is reached through for example migration.

[4]Using a panel of state-level data Driscoll (2004) finds that bank loans have small, often negative and statistically insignificant, effects on output. Ashcraft (2006) estimates that the elasticity of real state income to bank loan supply is close to zero and is definitely no larger than 10%. Ashcraft and Campello (2003) show that bank lending is demand-driven and influenced by local economic conditions.

[5]According to Berger, Kashyap, and Scalise (1995), only 20% of nonfarm and nonfinancial corporate debts were provided by these commercial banks in 1980; this ratio continued to drop through the 1980s. Finance companies, in contrast, facing few geographic expansion restrictions, provided nearly 10% of loans to nonfarm and nonfinancial firms. Many finance companies specialize in the factoring of trade account receivables, equipment loans, or leases, which are particularly relevant to small businesses that traditionally depend on banks. More importantly, a large number of entrepreneurs finance their ventures by taking second mortgages on their houses or using the generous limits on their personal credit cards or home equity lines of credit. None of these nonbank credit institutions (or products) is geographically restricted by branching regulations.

deregulation consolidations of banks could negatively impact smaller and newer firms that are the most dependent on banks.[6]

Second, the economic impact of the regulation or deregulation of US commercial banks could well be overstated. Kane (1996) and Kroszner and Strahan (1999) point out the irony that the cost of regulation is usually at its lowest by the time it is removed. In the history of the US financial service industry, most of the effects targeted by a rescission will have already been tolerated by the enforcement system for years before an exclusionary statute comes to be formally rescinded, and more importantly, will have been constantly subject to erosions by market players through legal loopholes, contractual and information processing innovations, regulatory/structural arbitrage,[7] and interpretive changes in the statutes that regulatory bodies actually enforce. As Kane (1981, p. 359) asserts, "In the 1970s, loophole mining and fabrication became the main business of modern depository institutions."

The rest of the paper is organized as follows. Section 2 the procedure of identifying regulation change borders and contiguous counties is introduced. In Section 3, the empirical strategies are introduced and several econometric difficulties are addressed, namely, how should the difference-in-differences treatment effect be defined to avoid potentially understating standard errors, how can we correct biases in the point estimate of treatment effects, and how can we establish correct standard errors and statistical significance of the estimates, through randomization-type fictitious "placebo deregulation events." In Sections 4–6, the proposed strategy is implemented. In particular, in Section 4, a statistical table of critical values is empirically created through a randomization procedure, taking into account the influence of spatial dependences. In Section 5, the economic effects of each of the 23 deregulation events are assessed based on the critical values. In Section 6, using hinterland counties as a second control group, we consider and reject the possibility of cross-border spillover of deregulation effects influencing the results. Finally, in Section 7 several plausible explanations are discussed for why regulation and deregulation of commercial banks' geographic expansions, in most cases, appear not to have substantially affected the local economies.

2. Matching of contiguous counties across regulation change borders

To assess the real effects of a deregulation event by comparing the economic performance of a treatment group vs. a control group, one first needs to look for pairs of neighboring states separated by so-called *regulation change borders*. To be included in the study, we require that, for a pair of states sharing a border, bank branching expansions in the second state must remain restricted for at least three years after restrictions in the first state are removed. These borders are called *regulation change borders*. In the resulting research sample, the average gap between two states' deregulation timings is approximately six years, which we believe is sufficiently long for the economic effects of regulatory differences across state borders to be observed, if they exist at all.

2.1. Identifying contiguous counties

We identify 38 regulation change border segments meeting the above requirements. Borders of Western states (i.e., Montana, Wyoming, Colorado, New Mexico, and all states to the west of them) are excluded from the sample.[8] These regulation change borders are listed in Table 1 and highlighted in the map in Fig. 1. Using

[6]Berger, Miller, Petersen, Rajan, and Stein (2005) provide evidence consistent with the belief that small banks are better able to collect and act on soft information than large banks. In particular, large banks are less willing than small banks to lend to those whose credit is "difficult" from the information standpoint, such as firms with no financial records. Brickley, Linck, and Smith (2003) also supply evidence that small, locally owned banks have a comparative advantage over large banks within specific environments.

[7]The U.S. Banking Act of 1971 defines a bank as an institution that offers demand deposits and originates commercial and industrial loans. A money market mutual fund is not a bank because it does not originate loans, and a finance company is not a bank because it does not accept demand deposits.

[8]It is much more difficult to identify good match of contiguous counties in the western states. In the eastern states, border counties on opposite sides of state borders are typically of fairly uniform width, nicely trace out the regulation change borders, forming strips of land on opposite sides of borders. In contrast, border counties in the western states are much larger in size, irregular in shape and less densely populated. This exclusion requirement does not reduce the sample size significantly, because most of the western states deregulated bank branching much earlier than the rest of the U.S., and thus, there are few cross-border regulatory differences in the west for us to exploit.

Table 1
Paired states and regulation change borders

Early deregulator	Deregulation year	Deregulation year	Late deregulator
Maine	1975	1987	New Hampshire
New York	1976	1980	Connecticut
New York	1976	1982	Pennsylvania
New York	1976	1984	Massachusetts
New Jersey	1977	1982	Pennsylvania
Virginia	1978	1985	Tennessee
Virginia	1978	1987	West Virginia
Virginia	1978	1990	Kentucky
Ohio	1979	1982	Pennsylvania
Ohio	1979	1987	Michigan
Ohio	1979	1987	West Virginia
Ohio	1979	1989	Indiana
Ohio	1979	1990	Kentucky
Connecticut	1980	1984	Massachusetts
Alabama	1981	1985	Tennessee
Alabama	1981	1986	Mississippi
Alabama	1981	1988	Florida
Pennsylvania	1982	1987	West Virginia
Georgia	1983	1988	Florida
Massachusetts	1984	1987	New Hampshire
Nebraska	1985	1990	Missouri
Nebraska	1985	1994	Iowa
Tennessee	1985	1990	Kentucky
Tennessee	1985	1990	Missouri
Tennessee	1985	1994	Arkansas
Mississippi	1986	1994	Arkansas
Kansas	1987	1990	Missouri
Michigan	1987	1990	Wisconsin
North Dakota	1987	1993	Minnesota
West Virginia	1987	1990	Kentucky
Illinois	1988	1994	Iowa
Louisiana	1988	1994	Arkansas
Oklahoma	1988	1994	Arkansas
Texas	1988	1994	Arkansas
Missouri	1990	1994	Arkansas
Missouri	1990	1994	Iowa
Wisconsin	1990	1993	Minnesota
Wisconsin	1990	1994	Iowa

Pairs of states that bilaterally form the 38 regulation change border segments are listed in the table, sorted by the year when the first state in each pair removed restrictions on statewide branching. The year when each state removed restrictions on statewide branching is also indicated in the table (*original source*: Amel, 1993).

these borders, 23 state-level branching deregulation events throughout the United States spanning from the 1970s to the 1980s can be evaluated for their impacts on the local economy. These deregulation events include (year of event): Maine (1975), New York (1976), New Jersey (1977), Virginia (1978), Ohio (1979), Connecticut (1980), Alabama (1981), Pennsylvania (1982), Georgia (1983), Massachusetts (1984), Nebraska (1985), Tennessee (1985), Mississippi (1986), Kansas (1987), Michigan (1987), North Dakota (1987), West Virginia (1987), Illinois (1988), Louisiana (1988), Oklahoma (1988), Texas (1988), Missouri (1990), and Wisconsin (1990).

One then needs to match pairs of contiguous counties across these so-called regulation change borders. We use *the National Atlas of the United States* (http://www.nationalatlas.gov/) to identify 285 pairs of contiguous counties. A list of the county pairs is available from the author upon request. In the study, the counties located in states that deregulated earlier than their neighbors form the treatment group, while those located in states

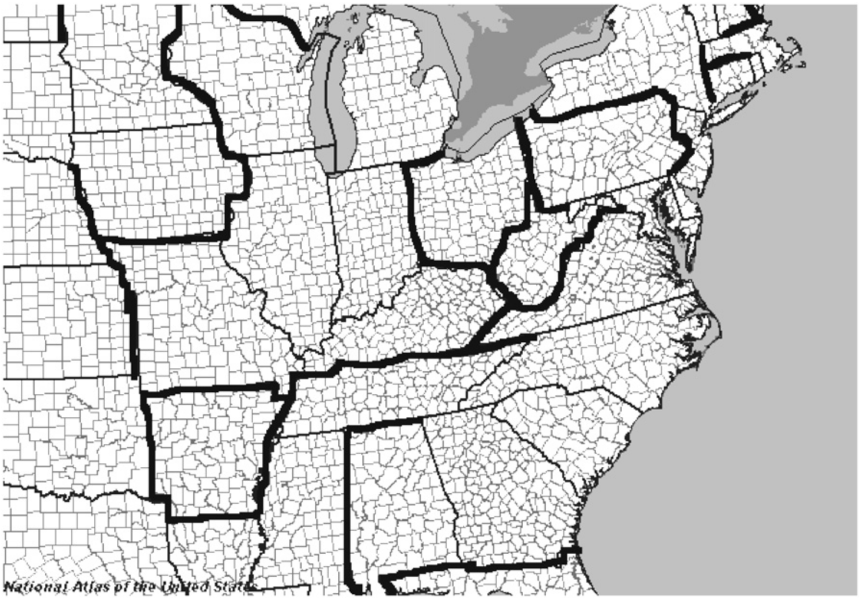

Fig. 1. Regulation change borders. This study identifies 38 *regulation change borders* segments, which are highlighted in the map above. For at least three years, and on average six years, there were regulatory differences across these regulation change borders: banks on one side of the borders were relieved from restrictions on statewide branching, while on the other side, restrictions were eventually removed at least three years later. See Section 2.1 for further details.

where restrictions are removed at least three years later form the control group. About one-third of the sample counties are located in metropolitan areas.

It has been a convention in the literature to use a county as the unit of analysis when studying local banking markets (e.g., Berger, Demsetz, and Strahan, 1999; Black and Strahan, 2002; Prager and Hannan, 1998; Rhoades, 2000). For example, many researchers study the effect of bank activities on economic outputs at the county level (e.g., Ashcraft, 2005; Calomiris and Mason, 2003; Clair, O'Driscoll, and Yeats, 1994; Gilbert and Kochin, 1989).Further, according to the Federal Reserve's definition of local banking markets (DiSalvo, 1999), which takes into account commuting patterns as well as other factors, a rural county is typically also regarded as a local banking market.

2.2. Contiguous counties are similar in observable characteristics

The geographic matching produces higher homogeneity between the treatment and control groups, potentially reducing background noise and standard errors when we estimate treatment effects, and increasing the power of the tests. Contiguous counties are arguably similar in many unobservable factors, but it is difficult for econometricians to formally verify such factors (otherwise, they would be observable in the first place). Nevertheless, here we can still analyze some observable characteristics.

To give readers a better understanding of how geographic matching yields, relative to previous studies, improves identification of the control group (at least in observable characteristics), we conduct the following counterfactual experiment. In the year before deregulation, we calculate each deregulated (treated) county's average absolute difference (in terms of income per capita and manufacturing income share) from all counties nationwide that deregulated at least three years later. This alternative way of forming the control group is equivalent to the practice of Jayaratne and Strahan (1996) and other typical studies in the literature, whereby a point estimate of the treatment effect is obtained by comparing at certain points in time deregulated states with all other states nationwide that had yet to deregulate. For a specific deregulation event, and letting counties in the control group be drawn nationwide from states that deregulated at least three years later, the

Table 2
How the use of contiguous counties helps reduce observable differences between treatment and control groups

	Absolute log difference (%) in income per capita		Absolute difference in manufacturing income ratio	
	Between treated counties and …			
	Contiguous border counties	Regulated counties nationwide	Contiguous border counties	Regulated counties nationwide
Alabama	15.33	23.70	0.17	0.21
Connecticut	15.66	35.24	0.07	0.20
Georgia	16.23	24.26	0.22	0.20
Illinois	8.36	18.56	0.18	0.16
Kansas	12.91	21.56	0.07	0.14
Louisiana	16.32	24.86	0.14	0.15
Maine	10.56	19.01	0.15	0.20
Massachusetts	7.86	37.86	0.08	0.21
Michigan	10.54	18.36	0.11	0.14
Mississippi	11.73	35.20	0.08	0.14
Missouri	13.07	25.86	0.17	0.16
Nebraska	12.11	21.65	0.08	0.15
New Jersey	6.36	31.18	0.10	0.19
New York	12.28	22.90	0.14	0.20
North Dakota	11.76	24.58	0.12	0.14
Ohio	12.18	21.26	0.15	0.21
Oklahoma	21.13	28.15	0.16	0.16
Pennsylvania	6.61	18.64	0.19	0.18
Tennessee	14.16	26.38	0.14	0.21
Texas	14.89	14.35	0.31	0.12
Virginia	19.20	22.29	0.14	0.19
West Virginia	14.71	23.47	0.08	0.15
Wisconsin	10.28	15.88	0.12	0.13
Total	13.45	23.44	0.14	0.18

To give readers a sense of how geographic matching improves on previous studies in identifying a better-matched control group *at least in some observable characteristics*, we conduct a counterfactual experiment. For each deregulated (treated) county, at the time of deregulation we also calculate its average difference (in terms of income per capita and manufacturing income ratio) from all counties nationwide that deregulated at least three years later. This alternative way of forming the control group is equivalent to the practice of Jayarante and Strahan (1996) and other typical studies in the literature, that produce point estimates of treatment effects by comparing at certain points in time deregulated states against all other states nationwide that had yet to deregulate. The numbers can tell us, for a specific treatment county, if its controls are drawn nationwide from states that deregulated at least three years later, as opposed to from contiguous counties, what the average differences between the treatment group and the control group counties will be in terms of the two observable characteristics. In the table, we present and compare the observed absolute differences between the treatment group and the control group averaged by deregulation event, achieved through the two control-group sampling approaches. It is clear that in almost all cases, geographic matching produces a smaller absolute difference between treatment and control groups than what can be achieved in pooled regressions a la Jayarante and Strahan (1996). See Section 2.2 for further details.

numbers tell us what the average absolute difference will be between the treatment group and control group counties, in terms of both income per capita and manufacturing share.

In Table 2, the average differences between the treatment and the control groups, achieved through the two different approaches of control-group sampling are compared based on income per capita and manufacturing income share, respectively, and are reported by individual deregulation event. It is clear that in most deregulation events, geographic matching produces a much smaller absolute difference between treatment and control groups, using these two observed characteristics, than what can be achieved in pooled regressions a la Jayaratne and Strahan (1996).

Certainly, if we scan the whole national sample, consider also counties that are not necessarily contiguous to the treatment counties, and retain only the best-matched counties in these two observable characteristics (per capita income and manufacturing income share), we could form an even better-matched control group. The

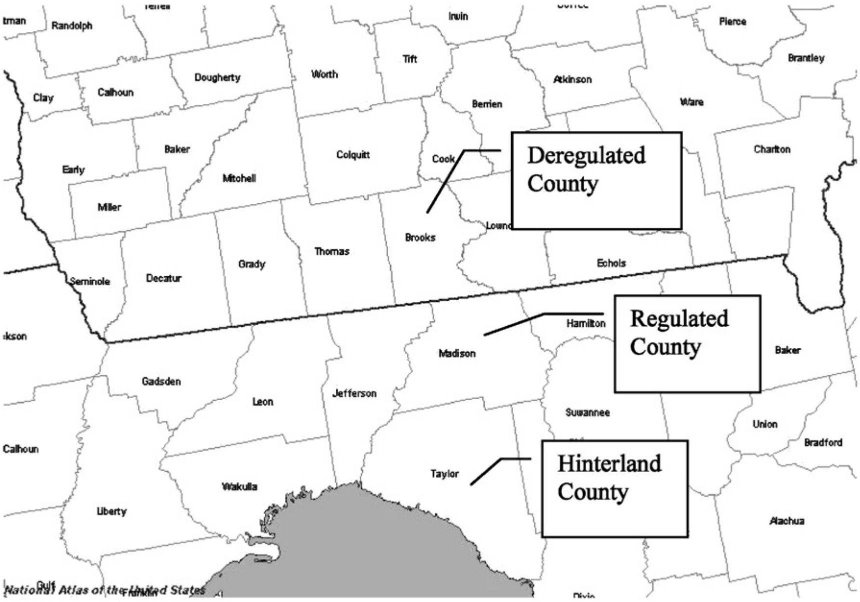

Fig. 2. Deregulated county, regulated county, and hinterland county. This map covers the area around the Georgia-Florida border. Georgia removed restrictions on statewide branching in 1983, whereas Florida removed them in 1988. Thus, there were regulatory differences across the Georgia-Florida border during the 1983–1988 period. An example is given in the map: Brooks county in Georgia is the so-called *deregulated county*, Madison county in Florida is the so-called *regulated county*, and Taylor county also in Florida is the so-called *hinterland county*. See Section 2 for further details.

reason for not doing so is that observable differences can be easily controlled for, do not pose a large challenge to econometricians, and hence are not a major problem in this study. In contrast, unobservable characteristics, in which contiguous counties are less likely to differ from each other, are what usually trouble econometricians because there is no way an econometrician can explicitly adjust for unobservable growth opportunities. Furthermore, there are many factors that are observable but difficult to exhaust, quantify, or control for, e.g., climate, access to transport, and agglomeration economy. However, these factors are less likely to differ or matter within a pair of contiguous counties. Therefore, the strategy adopted in the study, which takes into account such a tradeoff, is to use contiguous counties to minimize the difference in unobservable factors, and then to explicitly adjust for the remaining observable differences.

2.3. Hinterland counties

A second control group of paired counties is also identified, which we name *hinterland counties*. These counties are located on the same side of the regulated counties, and therefore were also regulated for a longer period of time than the deregulated counties on the opposite side of state borders. The hinterland counties are farther away from the regulation change borders but are contiguous with the regulated border counties on the same side of the border. In other words, hinterland counties are *co-contiguous* with the deregulated counties, with the border deregulated counties located in-between them. We identify 249 such hinterland counties. The list is available from the author upon request. For some deregulated counties, proper hinterland counties cannot be found for geographic reasons.[9] In the study, the hinterland counties are used as a second control group to consider potential spillovers of deregulation effects across state borders, which could disqualify border counties as valid controls in the event of treatment. The rationale for this robustness check is explained in detail in Section 6.

[9]There are several reasons why hinterland counties cannot be found for some county pairs. For example, in some cases, the hinterland is the Gulf of Mexico. Another common reason is that the candidate hinterland county borders another state that had deregulated earlier, which creates a new source of spillover potential.

To help readers better understand the geographic terms introduced above, Fig. 2 provides a graphical example: Georgia lifted its branching regulations in 1983, whereas Florida remained regulated until five years later in 1988. In this case, Brooks County in Georgia is a deregulated county, Madison County in Florida is a regulated county, and Taylor County in Florida is a hinterland county.

3. Methodology: estimating the treatment effects

3.1. Collapsing of information into pre- and post-treatment period

A difference-in-differences methodology compares outcomes in the treatment group and the control group in the pre- and post-treatment periods to identify the treatment effects. This study defines the two periods as follows:

(1) Pre-treatment period: In this period, both states restricted intrastate branching. The pre-treatment period is defined as the 10-year period before one of the two states first removed the restrictions. Thus, there were no treatments during this period. For states that deregulated before 1979, this period is shorter than 10 years, as county-level income growth data are available only from 1969.
(2) Post-treatment period: In this period, one of the two states was deregulated, but the other state remained regulated until much later. In this period, there were regulatory differences across state borders, and thus one state received treatment while the other did not. When Iowa is used in the comparisons as the regulated state, we end the post- period in 1994.[10]

To estimate the economic effect of deregulation, Jayaratne and Strahan (1996) use a panel data set pooling yearly time-series information. However, Bertrand, Duflo, and Mullainathan (2004) show that difference-in-differences estimation that uses many years of data and focuses on serially correlated outcomes does *not* produce consistent standard errors. Bertrand et al. find an effect significant at the 5% level for up to 45% of the placebo treatments, which clearly rejects the validity of the standard errors. Furthermore, Bertrand et al. do not find econometric corrections that place a specific parametric form on the time-series process to be able to correct the problem. Nevertheless, they do show that collapsing the time-series information into a pre- and post-treatment period works well.

As a basic but first and necessary step to avoid potentially inflating the statistical significance of the treatment effects, we follow exactly this prescription and study a treatment effect that is defined as difference-in-differences of average annual growth rates (%) between the pre- and post- periods, and between treatment and control counties on opposite sides of regulation change borders. The treatment effect (TE), i.e., ‘‘growth acceleration gap,’’ between two contiguous counties is thus measured by:

$$TE = (g_{1,\text{post}} - g_{1,\text{pre}}) - (g_{0,\text{post}} - g_{0,\text{pre}}), \tag{1}$$

where g_1 (g_0) is the average annual growth rate of real per capita income in the county that removed branching restrictions *earlier* (*later*), while subscripts *pre* and *post* denote the pre- and post- periods, respectively. County-level per capita personal income data are obtained from the Regional Economic Information System (REIS) of the Bureau of Economic Analysis (BEA). Real growth rates are obtained by deflating the nominal income data with the national consumer price index obtained from the Bureau of Labor Statistics (BLS).

The hypothesis of the study is as follows: if a certain bank branching deregulation event has any positive effect on the local economy, one should observe that deregulated counties should experience greater growth acceleration in the several years after the deregulation compared to their neighbors across the regulation

[10]Iowa eventually removed statewide branching restrictions completely in 2001. We end the comparison in 1994 when the Interstate Banking and Branching Efficiency Act (IBBEA) was passed because, by then, all of the other states had already permitted intrastate branching, and 1994 is generally regarded as the completion date of geographic banking deregulations in the United States.

change borders, or in other words, we should find the treatment effect (i.e., the growth acceleration gap) to be significantly positive, both economically and statistically.

There are two steps we need to go through before we can establish whether growth acceleration actually takes place after a specific deregulation event. First, we need to correct any bias in the point estimate of the treatment effects. Second, we need to know the estimation procedure's correct standard errors in order to establish statistical significance of the treatment effects. The second step is the most important and challenging part of the exercise. We start with the easier one first.

3.2. Correcting any bias in the point estimate of the treatment effects (TE)

To correct any bias in the point estimate of the treatment effects, we need to control for factors that could be correlated with both the deregulation event and future growth. These factors could be observable or unobservable, time-invariant or time-varying.

The use of contiguous counties helps minimize the influence of unobservable (to econometricians) factors, because contiguous counties are arguably similar in a lot of unobservable factors, although it is difficult for econometricians to verify such factors formally—otherwise they are observable in the first place.

Furthermore, any observable or unobservable factors that affect growth, if they are time-invariant, should not bias the point estimate of the difference-in-differences treatment effects; in the definition of a treatment effect, $TE = (g_{1,\text{post}} - g_{1,\text{pre}}) - (g_{0,\text{post}} - g_{0,\text{pre}})$, if a certain time-invariant, county-specific factor affects growth, it should affect $g_{1,\text{ pre}}$ ($g_{0,\text{ pre}}$) as much as it affects $g_{1,\text{ post}}$ ($g_{0,\text{ post}}$), and thus should be canceled out already.

Therefore, what remains is for us to adjust those factors that are both observable and time-varying. We control for two the most obvious factors that are likely to affect growth. Below, we first discuss how these two factors affect growth in general. We then elaborate on how the time-varying components can be incorporated in the estimation of treatment effects.

The first factor that is likely to affect the grow difference is the income gap, which affects the growth difference through the convergence effect. It is defined as the log difference (%) between two counties' per capita income. If a county that deregulates earlier is poorer compared to its neighbors at the beginning of a period, then it tends to grow faster in the subsequent years, even absent any deregulation effects. Not taking this factor into account would lead us to overestimate the treatment effects. Nevertheless, the income gap at the start of the post-treatment period alone does not matter to the treatment effects, because if the income gap is as large as it was 10 years before deregulation (i.e., the beginning of the pre-treatment period), then the convergence effect would be the same for both periods and should be canceled out in the difference-in-differences estimate. If the gap has changed during the 10-year period, however, the effect needs to be explicitly controlled for. Thus, the first factor we control for is the *change* in income gap between 10 years before the deregulation event and the time of the deregulation event.

The second important factor that likely affects the growth difference is the growth opportunity gap, which is determined by sector-specific shocks at the regional level (Barro and Sala-i-Martin, 1992). Sector-specific shocks at the regional level, i.e., regional sectoral growth patterns, affect local growth differentially depending on the local industrial structure. If in a certain region manufacturing grows slower than nonmanufacturing over a period, then a county with less manufacturing share than its neighbors at the beginning of the period tends to grow faster, even absent deregulation events. Not taking into account this factor would lead us to overestimate the treatment effects.

Within a county pair, the growth opportunity gap between two counties over a certain period is defined as the difference in manufacturing income share between the two counties at the beginning of the period, multiplied by the regional-level growth rate difference between the manufacturing sector and the nonmanufacturing sector, that is,

$$Growth\_Opportunity\_Gap = (M_1 - M_0)(G_\text{M} - G_\text{S}), \tag{2}$$

where M_1 is the manufacturing share (ratio) of county 1, and M_0 that of county 0; G_M is the annual growth rate (%) of manufacturing in the region, and G_S that of nonmanufacturing in the region. The derivation of the formula is explained in the footnote.[11] Sectoral growth data are obtained from the Bureau of Economic Analysis (BEA)'s database. The regional growth rate is defined as the average of the two states' economies in question, and thus growth opportunity gaps differ for every county pair. Again, this factor does not matter if (a) industrial structures remain constant between 10 years before and the time of the deregulation event, *and* (b) regional growth patterns are the same in the two periods. What we need to control for, instead, is the change (difference) in the growth opportunity gap between the post- and pre-treatment periods, as time-invariant components are already mechanically removed from the difference-in-differences treatment effects.

3.3. Establishing correct standard errors of the estimation procedure

The relatively more difficult part of the exercise is to establish the correct standard errors of a treatment effect, or in other words, to find out how large a treatment effect needs to be to qualify as a statistically significant growth acceleration. This is a challenging task. OLS standard errors obtained in-sample could be biased downward, because neither the research question we study nor the research sample we select are randomly drawn from the population of ideas; or in other words, (purposeful or collective) data-snooping could have been practiced to obtain the significant results. Indeed, in deciding to study one particular type of policy change in this paper, i.e., branching deregulations, as opposed to many other numerous potential candidates, we already make a nonrandom choice that is potentially guilty of data-snooping. This problem is particularly severe here because the outcome variable, income growth of US county economies, is widely studied, and the possibility of collective data-snooping cannot be easily ruled out. The presence of spatial correlation within a chain of neighboring county pairs along the same segment of a border further exacerbates the problem because a positive correlation of shocks and treatment effects within a border county chain greatly increases the chance of finding a large mean for the treatment effects in a data-mining process. Furthermore, the United States is a collection of diverse regional economies with heterogeneous levels and variance of growth rates, and the branching deregulation events spanned a 15-year period of unprecedented and volatile changes in both the banking sector and the economy. These factors greatly increase the probability of finding large treatment effects through data-mining.

To address the above concerns, we adopt a nonparametric strategy that is used rather routinely in clinical trial studies to establish statistical significance, namely, the randomization (or permutation) test. To implement this method, we utilize information from the out-of-sample "non-event borders." Other than the 38 regulation change border segments used to obtain treatment effects of actual deregulation events, we further identify 32 "nonevent border" segments (and 266 pairs of contiguous counties), for which there are no dramatic cross-border policy differences such as those observed for the "regulation change border." (i.e., counties on one side of the border deregulate earlier, but counties on the other side do not follow immediately within three years).

Next, we randomize (permutate) fictitious placebo deregulation treatments on the non-event borders and calculate the "treatment effects" for these placebo events based on actual growth rate outcomes as if real deregulations had actually taken place. As a result of these simulations, we are able to obtain an empirical distribution of the "treatment effects when there are no treatments" by exhausting all of the possible fictitious scenarios. Each placebo deregulation is specified as a different combination of the following three parameters: (a) any one county pair from the non-event borders, (b) any one deregulation year, and (c) which side of the border receives the deregulation earlier (i.e., which side will be assigned to the treatment group and which one to the control group). Therefore, the universe of placebo deregulations can be known by exhausting all of the possible combinations.

[11]The predicted growth rate of county 1, based on a region-wide sectoral-specific shock and the local industrial structure, is $M_1G_M + (1 - M_1)G_s$, and that for county 0 is $M_0G_M + (1 - M_0)G_s$.

The "growth opportunity gap" between county 1 and county 0 is therefore the difference between the between the two predicted rates:

$$\begin{aligned}
&[M_1G_M + (1 - M_1)G_s] - [M_0G_M + (1 - M_0)G_s] \\
&\quad = M_1G_M - M_1G_S - M_0G_M + M_0G_s \\
&\quad = (M_1 - M_0)(G_M - G_s).
\end{aligned}$$

Note that in our preferred procedure (see Section 4.4 for details), to remove the influence of positive spatial correlation of treatment effects within a chain of neighboring county pairs, in constructing a scenario, instead of a single county pair we draw a chain of a certain number of neighboring county pairs from a border and administer the placebo deregulation to all counties on one side of the border chain. Then, the *mean* treatment effect for these neighboring county pairs is calculated and retained to form an empirical distribution that by construction has taken into account the spatial correlations of treatment effects among neighboring county pairs.

Because the placebo deregulation events are completely fictitious, the distribution of their "treatment effects" can inform us intuitively: given a certain probability how large (extreme) a treatment effect can be obtained by examining county pairs randomly drawn from borders where no real treatments are applied in reality. Let's assume the 95th percentile of the distribution was a treatment effect of $+2\%$ per year, and you, a researcher examining the data set, are given 20 draws from the universe of possibilities in designing a study and producing an empirical result. Then 5% of the time you could expect to find growth acceleration of such magnitude in one of the 20 draws. Similarly, if 20 researchers are mining the same data set, by chance one of them could identify significant growth accelerations of such magnitude although no real treatments are actually applied. In this case, only when the treatment effect of an actual event is greater than $+2\%$ can you firmly acquit the result of the data-snooping charge and establish the statistical significance at the 95% level.

Based on the empirical distribution of treatment effects derived from the randomized simulations, a statistical table of critical values at various confidence levels can be created. Treatment effects estimated from actual deregulation events then can be compared against the corresponding critical values, and exact statistical significance can be established. This statistical table should be useful not only for this particular study, but also for future studies that utilize the same empirical setup to examine the economic impacts of many other financial regulations that used to exhibit cross-state differences at certain points in time, e.g., personal bankruptcy law, foreclosure law (judicial vs. power-of-sale), predatory lending law (modern version of usury law), depositor preference law, and antitakeover law, to name just a few obvious subjects of interest to financial economists.

3.4. Using the non-event sample to correct biases in the point estimate of actual treatment effects

The non-event borders sample also helps correct any bias in the point estimate of treatment effects. In Section 3.2, we establish that the income gap and the growth opportunity gap can affect the growth rate difference, and hence that they need to be controlled for to correct any bias in the point estimate of treatment effects. To do so, we need to run a regression of the raw treatment effects against changes in the income gap and the growth opportunity gap, and then retain the residuals of the regression as the *adjusted* treatment effects. However, is still not an unbiased point estimate, unless it is estimated on the non-event border sample where deregulations did not actually happen. The reason is that when one runs such a regression on the sample in which deregulation actually took place, what one is studying is *not* how the income gap *normally* affects growth, i.e., whether lower-income counties should grow faster than higher-income counties holding other factors constant. Instead, the coefficient on the income gap will reflect whether deregulation helps lower-income *deregulated* counties more than higher-income *deregulated* counties, *conditional on* a deregulation event having taken place and having produced positive effects. Such an interaction effect between the actual occurrence of a deregulation event and an initial income gap is implicitly installed in the regression model by the sample-selection itself, if the model is estimated in-sample (i.e., in which deregulations events are real.)

Our solution to this problem is to conduct a "dry run" on the out-of-sample non-event borders to obtain the coefficients that truly capture how changes in the income gap and the growth opportunity gap unconditionally predict treatment effects. The regression is specified as follows (*see* Section 3.2 *for definitions*):

$$\begin{aligned} \textit{Raw Treatment Effect} = \beta_1 \times \textit{Change in income gap} + \beta_2 \\ \times \textit{Change in growth opportunity gap} + \varepsilon. \end{aligned} \tag{3}$$

We apply the fitted coefficients of Eq. (3) to the actual regulation change borders to correct any bias in raw treatment effects. The formula is specified as follows, where $\bar{\beta}_1$ and $\bar{\beta}_2$ are the two fitted coefficients obtained from the regression specified in Eq. (3):

$$\begin{aligned} \textit{Adjusted Treatment Effect} &= \textit{Raw Treatment Effects} - \bar{\beta}_1 \times \textit{Change in income gap} \\ &\quad - \bar{\beta}_2 \times \textit{Change in growth opportunity gap}. \end{aligned} \tag{4}$$

4. Randomizing placebo deregulations on non-event borders

In this section, we implement the empirical strategies introduced in Section 3. Before working on the regulation change borders and assessing the actual deregulation events, we first need to conduct randomized simulations on the non-event borders to obtain an empirical distribution of the treatment effect estimator, as well as the coefficients of Eq. (3), which will be used to correct any bias in the point estimates.

4.1. Conducting simulations and obtaining estimates of ''treatment effects''

In the eastern United States (i.e., states to the west of Montana, Wyoming, Colorado, and New Mexico), there are 60 state border segments that can potentially be utilized for the study, of which 38 are *regulation change borders* according to our definition (i.e., one side of the border deregulated branching at least three years earlier than the other side). These regulation change borders are used to assess the real effects of actual deregulation events. The remaining 32 border segments are defined as *non-event borders*, for which dramatic events such as those observed in the regulation change borders did not take place. In Fig. 3, the 32 non-event border segments are highlighted in the map, and in Table 3, the states forming the bilateral borders are listed. Along these non-event borders, 266 pairs of contiguous counties are identified.

We simulate fictitious placebo deregulations on these borders to find out what magnitude of ''treatment effects'' we could obtain through data-snooping on these borders where differential treatments are not real.

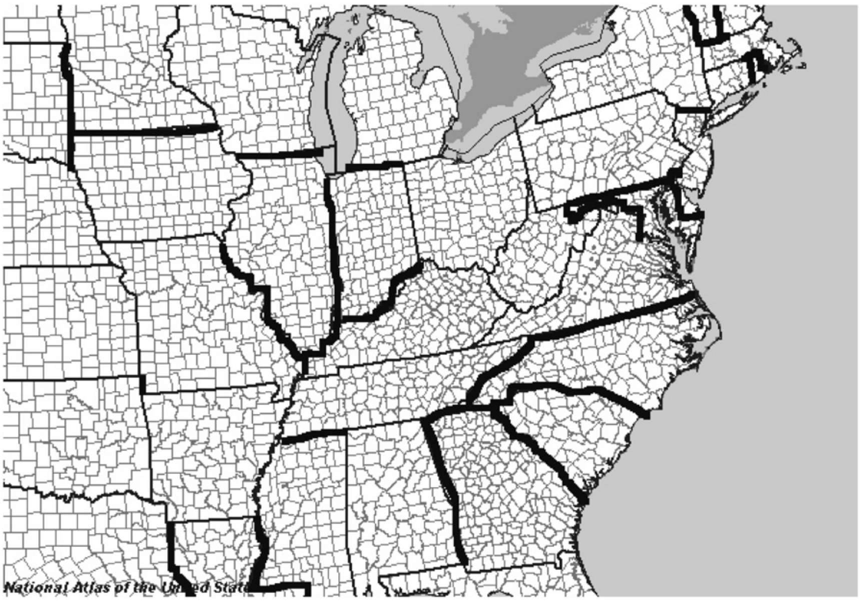

Fig. 3. Non-event borders. This study identifies 32 *non-event border* segments, which are highlighted in the map above. Across the so-called *regulation change borders*, for at least three years there were regulatory differences, with commercial banks on only one side of the borders free from restrictions on statewide branching. Across these *non-event borders*, however, there were no such differences. In the study, *fictitious placebo deregulation events* are randomly simulated on these borders where treatments do not exist in reality, to obtain an empirical distribution of the fictitious events' ''treatment effects,'' which can help us distinguish real treatment effects of actual deregulation events from the potential results of data-snooping. The critical values of the distribution at various confidence levels can tell us how easily we can obtain certain large treatment effects through data-snooping on borders where real treatments do not occur in reality. See Sections 3 and 4 for further details.

Table 3
Non-event states used for simulations

State name	Number of county pairs	Share in the sample (%)
Alabama	17	3.20
Connecticut	3	0.56
Delaware	11	2.07
Georgia	41	7.71
Illinois	40	7.52
Indiana	38	7.14
Iowa	14	2.63
Kentucky	27	5.08
Louisiana	23	4.32
Maryland	30	5.64
Massachusetts	7	1.32
Michigan	5	0.94
Minnesota	18	3.38
Mississippi	18	3.38
Missouri	19	3.57
New Hampshire	5	0.94
New Jersey	7	1.32
New York	12	2.26
North Carolina	50	9.40
Oklahoma	2	0.38
Pennsylvania	13	2.44
Rhode Island	8	1.50
South Carolina	31	5.83
South Dakota	10	1.88
Tennessee	23	4.32
Texas	11	2.07
Vermont	13	2.44
Virginia	20	3.76
West Virginia	9	1.69
Wisconsin	7	1.32
Total	532	100

Thirty-two *non-event border* segments are identified for the study. Placebo deregulations are randomly applied to these borders to obtain fictitious treatment effects. Placebo deregulations can be scheduled to take place earlier on either side of the border. Thirty states are eligible to receive a placebo deregulation shock earlier than their neighboring states, and thus, form the treatment group (similarly, in separate scenarios they can be scheduled to receive the treatments later than their neighbors, and thus, form the control group). The names of the states are listed in the table. The second column records the number of county pairs that can be studied if the corresponding state is slated for an earlier placebo deregulation. Note that the numbers add up to twice the number of county pairs along the non-event borders, because the deregulations can take place earlier in either side of the border, or in other words, a state can belong to both the treatment group and the control group in separate scenarios.

This helps us create a benchmark against which to statistically distinguish real deregulation effects from what can be obtained by data-snooping. In constructing a placebo deregulation, we randomly draw a county pair from these borders, choose the year for the placebo deregulation, and apply this year to one side of the border. We then calculate the "treatment effect" of this placebo deregulation using actual realized growth rate data. As a result, we are able to form an empirical distribution of the "treatment effects when treatments are not real" by exhausting all of the possible fictitious scenarios. A placebo deregulation can be produced from the random combination of the following three parameters: (a) any one of the 266 county pairs; (b) any one of the 11 years (1979–1989)[12]; and (c) either side of the border (for the deregulation to take place earlier). Therefore, the total number of possible combinations is 5,852 (i.e., $266 \times 11 \times 2$).

[12]The pre-treatment period is 10 years long, and county-level income data are available only after 1969; thus, the placebo deregulation can only take place in or after 1979. Similarly, as the sample period ends in 1994, the last year possible for a placebo deregulation with a five-year post-treatment period has to be 1989.

The schedules of placebo deregulation events are standardized so that the post-treatment period lasts for five years, i.e., there is a five-year waiting period before the second state also deregulates branching. This is representative of the actual deregulation schedule in our real sample, in which the median gap is exactly five years. The length of the post-treatment period is also similar to that in Jayaratne and Strahan's (1996) sample, which makes the point estimates somewhat comparable across studies, although they use a state as the unit of analysis.

4.2. Adjusting for the income gap and the growth opportunity gap

As discussed in Section 3.2, the income gap and the growth opportunity gap between the treatment and control groups, if not controlled for, would bias the point estimate of the treatment effects. Thus, after each simulation, we calculate not only the raw treatment effects, but also the changes in the income gap and the growth opportunity gap between the pre- and post-treatment periods. We then pool together the information of all of the 5,852 simulations, and estimate an OLS regression of the *raw* treatment effects against changes in the income gap and the growth opportunity gap, as specified in Eq. (3). The residuals of the regression are then retained as the *adjusted* treatment effects.

The regression results are reported as follows, with the estimation standard errors of the coefficients indicated within parentheses:[13]

$$\begin{aligned}\textit{Raw Treatment Effect} = & -0.1293932(0.0018873) \times \textit{Change in income gap} \\ & + 0.3815534(0.0558755) \\ & \times \textit{Change in growth opportunity gap}\ (\textit{adjusted}\ R^2 = 0.45). \end{aligned} \tag{5}$$

Note that the standard errors of these OLS coefficients are clearly underestimated, because a county is used in separate scenarios many times, and thus is included multiple times in the regression sample. We do not attempt to correct the standard errors, as only the point estimates of the coefficients, which are not contaminated, are used in this paper.

The negative coefficient on the income gap confirms that if the income gap between two contiguous counties widens (assuming that the deregulated county is initially poorer) during the 10-year period before the deregulation event, then the raw treatment effect will be biased upward because the convergence effect becomes greater and the deregulated counties will naturally tend to speed up. Without adjusting for this factor, we could identify a positive treatment effect for the deregulated county, even when the placebo deregulation has no effect.

The positive coefficient on the growth opportunity gap confirms that a change in either the local industrial structure or regional growth pattern/trend has an important impact on future growth. If county A has lower manufacturing share than its neighbor, and this remains unchanged over the 10 years preceding a deregulation event, but regional manufacturing grows more slowly than non-manufacturing in the post-treatment period than in the pre-treatment period, then county A will naturally tend to grow faster even in the absence of deregulation. Similarly, if the regional growth pattern remains unchanged across the pre- and post-treatment periods (and manufacturing grows more slowly than non-manufacturing), but county A's manufacturing share drops even further during the 10-year pre-period, then after deregulation county A will naturally accelerate further even absent the deregulation effect, as its growth opportunity is getting better.

4.3. Creating the statistical table of critical values for the treatment effect estimator

The residuals obtained from regression (5) are used as the *adjusted* treatment effects of the placebo deregulation events. Each residual value is linked to an individual placebo treatment. In Fig. 4, the whole

[13] By construction of the simulations, i.e., a county is used in both treatment and control groups (in separate simulation scenarios), the coefficient on the constant of the regression will always be zero when it is estimated based on the population of all 5,582 possible scenarios. For the same reason, both of the control variables, the change in the income gap and the change in the growth opportunity gap, have zero mean. The standard deviation of "change in income gap" is 10.8%, while that for "change in growth opportunity gap" is 0.366.

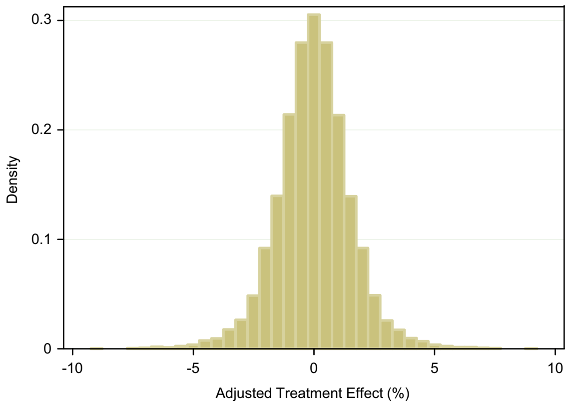

Fig. 4. Empirical distribution of fictitious treatment effects obtained from the placebo deregulation events. In the study, fictitious placebo deregulations are randomized on the non-event borders, and then the adjusted treatment effect is calculated based on actual growth realization data, for each of the 5,852 fictitious deregulation events. The distribution of the fictitious treatment effects is presented in the form of a histogram. Because the placebo deregulations are completely fictitious, the distribution depicted here can reveal the likelihood with which a fictitious treatment effect can be obtained by randomly selecting a county pair from the non-event borders and calculating the treatment effect based on actual growth rates data as if the treatment were real. See Section 4.3 for further details. The bin size of the histogram is 0.5%.

distribution of the residuals is presented in a histogram. As the placebo deregulations are completely fictitious, the reference distribution can tell us, in the absence of real treatments, how easily we will encounter a certain large (extreme) treatment effect when there are actually no treatments at all. Note that by construction a placebo deregulation could occur earlier in either side of the border in separate simulation scenarios, and this two-tails distribution of the fictitious treatment effects obtained from the population of placebo simulations is always symmetrical with zero as the mean.

The distribution in Fig. 4 tells us that when studying a non-event border along which there is only one county pair, there is a 10% chance that we could find treatment effects (growth acceleration) greater than 1.82%, even in the absence of any actual occurrence of a treatment on one side of the border. This means that when evaluating an actual deregulation event, if there is only one county pair along the regulation change border, in which case the point estimate of the treatment effect is 1.81%, we still cannot establish at the 90% confidence level that statistically significant growth acceleration actually occurs in this particular deregulation event, because even in the non-event borders where there are no real treatments there is more than a 10% chance that we can randomly run into treatment effects of such a magnitude. In Table 4, a table of critical values for various confidence levels is created based on the empirical distribution of fictitious treatment effects. According to the empirical distribution, when the treatment effect is estimated based on a single county pair, the critical value of treatment effects for a p-value of 0.10, 0.05, and 0.01 is $K_{0.10,1} = 1.82\%$, $K_{0.05,1} = 2.45\%$, and $K_{0.01,1} = 4.20\%$, respectively.

However, in actual deregulation events, along a regulation change border we usually use more than one county pair to form the *mean* treatment effect. Thus, we also need to obtain the critical values for the *mean treatment effect* of an N-observations sample when $N>1$. Assuming that the treatment effect of each individual county pair along a regulation change border is independent of its neighboring county pairs along the same border segment, the critical values for the mean treatment effect of an N-observations sample can be analytically extrapolated from the case of one single county pair. Specifically, the critical values for the mean treatment effect based on N observations of county pairs would be $K_{p,n} = K_{p,1}/\sqrt{N}$, where p indicates the p-value. It is easy to see that the critical values for mean treatment effects drop as the number of county pairs increases for a specific deregulation event.

The critical values for $N = 1,2, \ldots ,50$ presented in Table 4 are calculated this way. The values suggest that, for example, if there are 10 county pairs along a specific regulation change border, then we require the mean treatment effect of the 10 county pairs to be greater than $2.45\%/\sqrt{10} = 0.78\%$ to be statistically significant at

Table 4
Statistical table of critical values for the mean treatment effects (*not yet adjusted for spatial correlations*)

Number of county pairs used to form the mean	Statistical confidence level (*p*-value)		
	90% (0.10)	95% (0.05)	99% (0.01)
1	1.82	2.45	4.20
2	1.28	1.73	2.97
3	1.05	1.42	2.42
4	0.91	1.23	2.10
5	0.81	1.10	1.88
6	0.74	1.00	1.71
7	0.69	0.93	1.59
8	0.64	0.87	1.48
9	0.61	0.82	1.40
10	0.57	0.78	1.33
11	0.55	0.74	1.26
12	0.52	0.71	1.21
13	0.50	0.68	1.16
14	0.49	0.66	1.12
15	0.47	0.63	1.08
16	0.45	0.61	1.05
17	0.44	0.59	1.02
18	0.43	0.58	0.99
19	0.42	0.56	0.96
20	0.41	0.55	0.94
25	0.36	0.49	0.84
30	0.33	0.45	0.77
35	0.31	0.41	0.71
40	0.29	0.39	0.66
45	0.27	0.37	0.63
50	0.26	0.35	0.59

Along the 32 non-event border segments, randomized simulations let fictitious placebo deregulations take place on any of the 266 pairs of contiguous border counties, in any one year between 1979 and 1989. Once the state to be scheduled for an earlier placebo deregulation is selected (either side of the border can be selected), counties on the other side of the state border are scheduled to deregulate five years later. Then, the raw treatment effects are calculated based on the difference-in-differences of average annual growth rate between post- and pre-treatment periods and between the two contiguous counties. The ‘‘adjusted treatment effect’’ is then obtained by taking the residuals from a regression of the raw treatment effect on the change in the income gap and growth opportunity gap between the post- and pre-treatment periods.
An empirical distribution of the placebo deregulations' treatment effects is obtained based on all 5,852 possible scenarios. As the placebo deregulations are completely fictitious, the distribution can inform us the likelihood with which a certain large ‘‘treatment effect’’ could obtain through randomly selecting a county pair from borders where cross-border differential treatments did not occur in reality. In actual deregulation events, along a border there are multiple pairs of contiguous counties. Assuming no spatial correlations of treatment effects within a chain of neighboring county pairs along the same segment of a border, the critical values for the *mean* treatment effects can be extrapolated from the single county pair case by the formula: $K_N = K_I/\mathrm{SQRT}(N)$, where N is the number of county pairs used to form the mean, and K is the critical value. To save space, for $N>20$, critical values are reported in the table only for N's at the multiples of 5s.
Let's take an actual deregulation event to illustrate how the table is used to distinguish real treatment effects from the results of data-snooping. In the case of Illinois, there are nine pairs of contiguous border counties, and the mean adjusted treatment effect of this deregulation event turns out to be 0.46. Checking the table of critical values, in the row corresponding to the case of ‘‘9 county pairs,’’ we find three critical values, 0.61 for the 90%, 0.82 for the 95%, and 1.40 for the 99% confidence level. Since the actual treatment effect, 0.46, is smaller than 0.61, it is established that in the case of Illinois, a significant treatment effect cannot be established statistically in the years surrounding the deregulation event. The reason is that even by data-snooping, more than 10% of the time you can find a mean treatment effect greater than 0.61 if nine *independent* county pairs are drawn from borders where such differential treatments did not occur in reality. See Section 4.3 for further details.

the 95% level. If there are 20 county pairs, however, the threshold critical value will be lowered to $2.45\%/\sqrt{20} = 0.55\%$.

In Section 4.4, we drop the assumption of spatial independence and analyze how this effect would change the critical values.

4.4. Taking into account spatial correlations of treatment effects

The critical values produced in the last section for $N>1$ samples are unbiased only when we can assume that there are no correlations among the treatment effects within a chain of neighboring county pairs along the same segment of a border. If this were the case, then treatment effects obtained from each of the N county pairs would be independent, and it would be valid to apply the extrapolated critical values produced in the last section to *the mean* of the N treatment effects.

However, spatial correlation is typically present in the empirical setting of this study. Treatment effects for two pairs of counties next to each other are likely to be positively correlated, as counties on the same side of the regulation change border receive (or delay to receive) the same state-specific policy shocks. Not accounting for this factor would lead us to underestimate the standard errors of the mean treatment effects.

Again, we use randomized simulations to empirically solve the problem. In the last section, we randomly draw one single county pair in each simulated scenario, form the reference distribution of treatment effects, obtain critical values for the $N=1$ case, and then using the formula $K_n = K_1/\sqrt{N}$ calculate the mean treatment effects for the $N>1$ cases, assuming that treatment effects for neighboring county pairs along a border are independent of each other. In the new series of placebo deregulations, for each placebo deregulation event, we draw a chain of $N>1$ neighboring county pairs along a border, rather than an individual county pair, and as usual we choose the year of deregulation and the side of the border that is to be deregulated first. The treatment effect is calculated in the same way as in the case of a single county pair. What differs is that now we calculate and retain the mean of the treatment effects of the N neighboring county pairs (when $N>1$).

We simulate all possible combinations (scenarios) and repeat the procedure for different N values (the length of the chains of neighboring county pairs). As N increases, the number of possible combinations (and thus, draws of simulations) is reduced, because there are fewer non-event borders where longer chains of neighboring counties can be sampled.

As the product of the simulations, we obtain 50 empirical distributions of *mean* treatment effects, for $N=1$, 2, … ,50, respectively. Based on these distributions, we can empirically establish a table of critical values that are free from the influence of spatial correlations of treatment effects within a chain of neighboring county pairs without knowing the precise model of the spatial dependence. The table of critical values is presented in Table 5, for sample sizes from 1 to 50. To illustrate the changes in critical values after taking into account spatial correlations, Fig. 5 plots two curves based on two groups of critical values, with only one taking into account spatial correlations. The comparison clearly reveals the severe downward bias of standard errors when positive spatial correlations are not taken into account.

We take $N=10$ as an example to illustrate the difference between the two tables of critical values (one does not adjust for spatial dependence; the other does), and how the table of critical values can be used to assess treatment effects of actual deregulations. Not considering spatial correlation of treatment effects, when 10 independent county pairs are randomly drawn from the non-event borders, it is expected that 5% of the time the mean treatment effects will be greater than $2.45\%/\sqrt{10} = 0.78\%$, according to the first table of critical values (Table 4) produced in Section 4.3. However, when a chain of 10 neighboring counties along a border is drawn (which is what happens when we evaluate actual deregulations), according to the second table of critical values (Table 5), 5% of the time a mean treatment effect greater than 1.16% may exist. The comparison shows that positive spatial correlations of treatment effects, if not taken into account, would substantially bias the standard errors downwards. In the rest of the paper, we mainly use Table 5 to evaluate statistical significance of the estimated treatment effects.

5. Evaluating twenty-three actual deregulations events

After obtaining a statistical table (Table 5) of critical values that are robust to spatial correlation of treatment effects, we are ready to perform assessments of each of the 23 actual branching deregulation events identified in Section 2. The critical values indicate that, for a treatment effect of an actual deregulation event to be statistically significant at the 95% level, the magnitude of the effect must be greater than the fictitious treatment effects obtained in 95% of the placebo deregulations described in Section 4.

Table 5
Statistical table of critical values for the mean treatment effects (*robust to spatial correlations*)

Number of county pairs used to form the mean	Number of simulations conducted	Statistical confidence level (p-value)		
		90% (0.10)	95% (0.05)	99% (0.01)
1	5,852	1.82	2.45	4.20
2	5,423	1.51	2.02	3.32
3	5,005	1.36	1.82	2.94
4	4,631	1.25	1.63	2.80
5	4,268	1.16	1.51	2.50
6	3,905	1.12	1.43	2.44
7	3,575	1.05	1.34	2.34
8	3,300	1.01	1.26	2.22
9	3,047	0.94	1.23	2.08
10	2,805	0.89	1.16	1.91
11	2,574	0.85	1.11	1.68
12	2,354	0.82	1.06	1.59
13	2,167	0.81	1.03	1.51
14	2,002	0.80	1.03	1.45
15	1,859	0.77	1.02	1.40
16	1,727	0.75	1.01	1.35
17	1,595	0.72	0.99	1.30
18	1,463	0.70	0.96	1.24
19	1,364	0.69	0.92	1.19
20	1,287	0.67	0.91	1.13
25	902	0.65	0.78	0.97
30	539	0.65	0.74	0.95
35	297	0.63	0.73	0.89
40	143	0.64	0.69	0.80
45	66	0.51	0.54	0.65
50	11	0.50	0.55	0.55

Along the 32 non-event border segments, randomized simulations let fictitious placebo deregulations take place on any of the 266 pairs of contiguous border counties, in any one year between 1979 and 1989. Once the state to be scheduled for an earlier placebo deregulation is selected (either side of the border can be selected), counties on the other side of the state border are scheduled to deregulate five years later. Raw treatment effects are calculated based on the difference-in-differences of average annual growth rate between the post- and pre-treatment periods and between the two contiguous counties. The "adjusted treatment effect" is then obtained by taking the residuals from a regression of the raw treatment effect on the change in the income gap and the growth opportunity gap between the post- and pre-treatment periods.

Spatial correlation of treatment effects exists within a chain of neighboring county pairs along the same segment of a border. To make the procedure robust to such spatial dependences, we draw at each simulation a *chain* of N neighboring county pairs instead of a single individual county pair. Simulations are conducted for N-observation chains ($N = 1,2,...,50$, respectively). After simulating all possible scenarios (the number of scenarios varies depending on N, the length of the chain), an empirical distribution of the mean treatment effects can be obtained. Fifty such distributions are obtained, for $N = 1,2,...,50$, respectively. As the placebo deregulations are completely fictitious, the 50 empirical distributions can inform us the likelihood with which a certain large *mean* treatment effect could obtain through randomly selecting a chain of N ($N = 1,2,...,50$) county pairs from borders where cross-border differential treatment did not occur in reality. To save space, for $N > 20$, critical values are reported in the table only for the N's at the multiples of 5s.

Let's take an actual deregulation event to illustrate how the table is used to distinguish real treatment effects from the results of data-snooping. In the case of Illinois, there are nine pairs of contiguous border counties, and the mean adjusted treatment effect of this deregulation event turns out to be 0.46. Checking the table of critical values, in the row corresponding to the case of 9 observations we find three critical values, 0.94 for the 90%, 1.23 for the 95%, and 2.08 for the 99% confidence level. Since the actual treatment effect, 0.46, is smaller then 0.94, in the case of Illinois, a significant treatment effect cannot be established statistically in the years surrounding the deregulation. The reason is that even by data-snooping, more than 10% of the time a mean treatment effect greater than 0.61 can occur if a chain of nine neighboring county pairs is drawn from borders where treatments did not actually occur in reality. See Section 4.4 for further details.

In Table 6, some descriptive statistics are presented for the treatment group (deregulated counties), the first control group (border regulated counties), and the second control group (hinterland counties), on several variables of interest, including the means and medians of growth rates, income per capita, and manufacturing

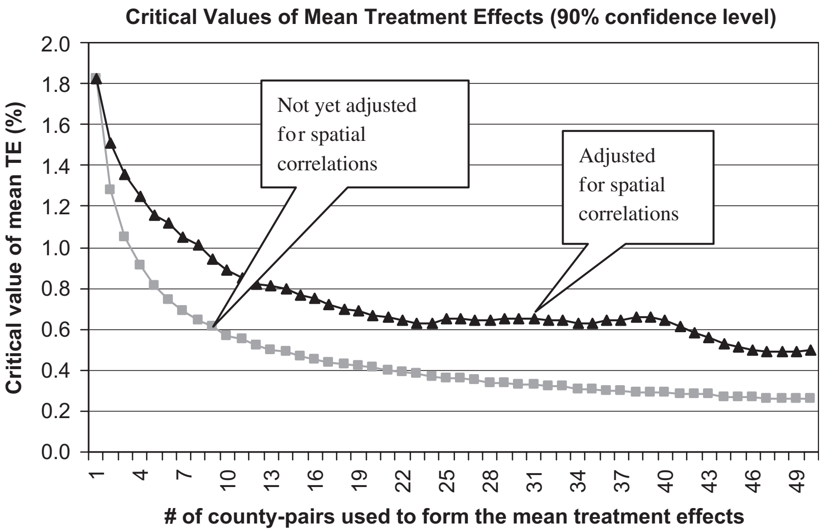

Fig. 5. Empirical critical values of mean treatment effects, with and without spatial correlation adjustments. In the study, we use placebo deregulations to create critical values for the mean treatment effects, to distinguish real treatment effects from the results of data-snooping. The critical values at various confidence levels can tell us the likelihood with which a mean treatment effect could obtain from data-snooping on the non-event borders where deregulations do not take place in reality; thus when one obtains a mean treatment effect from an actual deregulation event that actually occurs, the critical values indicate how likely the effect is the result of data-snooping vs. genuine treatment effects. When the number of county pairs used to evaluate a deregulation event is greater than one, the standard errors of mean treatment effects could be biased downwards by positive spatial correlations of treatment effects among neighboring county pairs within a regulation change border. We rely on randomized simulations to adjust for spatial dependence. In the simulations, we apply placebo deregulations to a chain of N neighboring county pairs instead of to an individual county pair. The empirical distribution of mean treatment effects obtained from such simulations is therefore robust to the influence of spatial correlations. In the chart, we present the critical values of treatment effect estimates before and after they have been adjusted for spatial correlations. It is clear from the chart that we would understate the estimation standard errors if we do not adjust for positive spatial correlations. See Section 4.4 for further details.

share. The averages and medians are calculated by pooling all county pairs used in the study from all 23 deregulation events, and serve to help readers gain an overall picture of the range of the average growth rate in the pre- and post-treatment periods. Separate assessments, however, will be conducted for each individual deregulation event, as pooling obscures the important idiosyncratic information of each individual event. Indeed, Wall (2004) shows that the deregulation effects are quite heterogeneous across individual events, which spanned two decades of radical changes in the banking sector, and took place in different locations under different circumstances. Unlike previous studies, we have the luxury of studying individual events separately because the use of a county as the unit of analysis increases the degrees of freedom in our estimations.

5.1. Obtaining point estimates of treatment effects

We first need to obtain a correct point estimate of the mean treatment effect for each actual deregulation event, adjusted for biases potentially created by a change in the income gap and a change in the growth opportunity gap between the pre- and post-deregulation periods (see Section 3.2). As discussed in Section 3.4, for the adjustment to truly reflect effects unrelated to the deregulation itself, we apply the fitted coefficients obtained from the non-event sample.

A narrowed income gap or widened growth opportunity gap over the 10-year period before deregulation, if not adjusted, could bias the point estimate of the treatment effect upward. The following formula based on coefficients obtained from Eq. (5) in Section 4.2 can help us correct for such biases:

$$\begin{aligned} \textit{Adjusted Treatment Effect}\,(TE) = \textit{Raw TE} &+ 0.1293932 \times \textit{change in income gap} \\ &- 0.3815534 \times \textit{change in growth opportunity gap}. \end{aligned} \tag{6}$$

Table 6
Summary statistics of the county economies (treatment vs. control group)

	Treatment group: deregulated counties		First control group: border-regulated counties		Second control group: hinterland- regulated counties	
	Mean	Median	Mean	Median	Mean	Median
Number of observations	285	285	285	285	249	249
Average growth rate in pre-treatment period (%)	1.74	1.69	1.75	1.67	1.66	1.49
Average growth rate in post-treatment period (%)	1.40	1.34	0.99	1.00	1.07	0.92
Time-series acceleration (%)	−0.34	−0.06	−0.76	−0.57	−0.59	−0.56
Standard deviation of the acceleration	(2.50)		(2.30)		(2.36)	
Income per capita at the time of deregulation (1980 USD)	8,538	8,398	8,529	8,486	8,306	8,126
Manufacturing income share at the time of deregulation (ratio)	0.26	0.25	0.28	0.28	0.27	0.27

For the actual deregulation events, 285 pairs of contiguous border counties can be examined. Using hinterland counties as the second control group, the number of pairs is reduced to 249, because no proper hinterland counties can be found for some treatment counties. The table presents the average (and median) growth rates, in the pre-deregulation period and in the post-deregulation period, and the difference between the two, i.e., the "within" treatment effect (not difference-in-differences treatment effect), for the treatment group (deregulated counties), first control group (border-regulated counties), and second control group (hinterland-regulated counties). The table also presents the mean (median) income per capita and manufacturing income share at the time of deregulation. All of the 23 deregulation events are pooled together to produce the summary statistics in the table for the purpose of helping readers gain an overall understanding of the characteristics of the county economies examined in the study. The state-level deregulation events spanned two decades of radical changes in the banking sector and their effects were heterogeneous across events; therefore, whether a significant growth acceleration actually occurred after a specific deregulation event must be evaluated separately, in light of the heterogeneity of results among deregulation events taking place in different years during a nearly two-decade period. The detailed assessment results are presented in Tables 7 and 8.

The coefficients are obtained from the non-event sample. Note that had we estimated and used the coefficients based on the in-sample observations, i.e., where actual deregulations took place, the coefficients would be contaminated by the sample selection problem discussed in Section 3.4.

The mean treatment effect of a deregulation event is estimated based on 12 county pairs on average, which raises the degrees of freedom in estimation and reduces standard errors of the point estimates. In Jayaratne and Strahan (1996) and similar studies that use a state as the unit of analysis, only one treated subject (state) can be evaluated for each deregulation event. To nominally raise the degrees of freedom and reduce estimation standard errors of OLS coefficients, these studies typically pool together all times-series information and all deregulation events. However, this strategy has a potential problem: Bertrand, Duflo, and Mullainathan (2004) show that by pooling serially correlated time-series information, the standard errors are likely to be understated, even after autocorrelation is explicitly modeled.

Furthermore, Wall (2004) points out that the pooling of different deregulation events assumes homogeneity of the treatment effects, which he shows to be actually quite heterogeneous across events. Nevertheless, Jayaratne and Strahan do stress that, based on their estimation methodology, the phenomenon they discover, namely, that deregulated states grew relatively faster after deregulation as compared to control states that at first had not yet deregulated, is not driven by isolated cases. They show that of the 35 states that deregulated since 1972, all but six states performed better (although not necessarily statistically significantly) than the corresponding control states. The six exceptions are New Hampshire, Florida, Michigan, Kansas, Colorado, and New Mexico.

In Table 7, for each of the 23 actual deregulation events, we report the mean treatment effects (both raw and adjusted), the number of observations (i.e., number of county pairs) used to form the mean treatment effects, and the average growth rate of the deregulated counties in the pre-treatment period. According to the results, the point estimates of the treatment effects are quite heterogeneous across individual deregulation events, which confirms Freeman (2002) and Wall's (2004) findings. In seven out of the 23 events examined in the

Table 7
Evaluating the actual deregulations events using the *contiguous counties* as the control group

Deregulation state	First year of deregulation	Number of county pairs	Mean average growth rate in pre-treatment period (%)	Mean raw treatment effect (%)	Mean adjusted treatment effects (%)	Statistical significance
Maine	1975	4	1.07	−1.81	−1.35	Negative
New York	1976	15	0.85	0.37	0.01	Insignificant
New Jersey	1977	8	1.98	0.65	0.59	Insignificant
Virginia	1978	27	4.14	0.16	0.64	Insignificant
Ohio	1979	41	2.10	0.20	0.01	Insignificant
Connecticut	1980	4	1.44	−0.46	−0.24	Negative
Alabama	1981	27	2.00	0.20	0.40	Insignificant
Pennsylvania	1982	6	1.77	−1.94	−0.70	Negative
Georgia	1983	12	0.60	−1.11	−0.85	Negative
Massachusetts	1984	3	2.32	0.54	0.32	Insignificant
Nebraska	1985	11	0.88	0.38	0.23	Insignificant
Tennessee	1985	25	1.65	1.52	1.31	1%
Mississippi	1986	5	0.56	−0.33	0.60	Insignificant
Kansas	1987	11	1.15	0.07	−0.09	Negative
Michigan	1987	5	0.51	2.71	1.94	5%
North Dakota	1987	6	2.82	0.34	0.61	Insignificant
West Virginia	1987	4	0.41	0.88	0.76	Insignificant
Illinois	1988	9	0.26	0.09	0.46	Insignificant
Louisiana	1988	8	0.80	0.72	1.15	5%
Oklahoma	1988	8	1.73	1.50	1.65	1%
Texas	1988	2	0.75	−0.90	−1.05	Negative
Missouri	1990	28	1.94	0.83	1.09	1%
Wisconsin	1990	16	0.93	1.43	−0.14	Negative

Each of the 23 bank-branching deregulation events is assessed separately to establish the statistical significance of its mean treatment effect. A different number of county pairs is identified and used in each deregulation event, depending on geography and the deregulation schedule of its neighboring states. The results of the assessments are presented in this table. The raw treatment effect is simply the difference-in-differences of the average growth rate in the pre- and post-treatment periods between the treatment counties and the control counties. Adjusted treatment effects control for the change in the income gap and the growth opportunity gap between the pre- and post-treatment periods, which if not taken into account can bias the point estimate. The mean treatment effect is obtained by averaging the treatment effects of all county pairs associated with a deregulation event. On average, evaluation of a deregulation event is based on 12 county pairs. The critical values of mean treatment effects are empirically obtained through applying placebo deregulations to non-event borders. We use the critical values tabulated in Table 5, which already adjust for the downward bias created by positive spatial correlation of treatment effects within a chain of neighboring county pairs. We assess the statistical significance of mean treatment effects only when they are positive.

study, the treatment effects are actually negative (Maine, Connecticut, Pennsylvania, Georgia, Kansas, Texas, and Wisconsin). In another two cases (New York and Ohio), the positive treatment effects are as small as 0.01%. Among these nine cases, only Kansas is indicated by Jayaratne and Strahan (1996) to have grown more slowly after deregulation, compared to the control states.

5.2. Establishing statistical significance

Comparing the values of the point estimates to the critical values at various confidence levels (obtained from the fictitious placebo deregulation events) clearly indicates that most of the positive treatment effects are *not* statistically distinguishable from what can be obtained in fictitious treatments.

The evaluation results of statistical significance are also presented in Table 7. Of the 23 actual branching deregulation events, in seven of them the mean treatment effects are negative. Therefore, they are immediately excluded from further examination. In the remaining 16 events, the point estimates are at least positive. We compare these point estimates against the two tables of critical values. Recall that Table 4 ignores spatial dependence within a chain of neighboring county pairs and is biased downward, whereas Table 5 adjusts for such dependence. Using the critical values from Table 4, which assumes no spatial dependence and hence underestimates the standard errors, there are only seven events where we can establish statistical significance at higher than the 90% level.

After adjusting for downward-biased standard errors due to positive spatial correlations, two more events are dropped and only five are left that are statistically significant at the 90% (or higher) confidence level. These five states are (in alphabetical order): Louisiana (8, 1.15%, >95%), Michigan (5, 1.95%, >95%), Missouri (28, 1.09%, >99%), Oklahoma (8, 1.65%, >99%), and Tennessee (25, 1.31%, >99%). Numbers in the parentheses are, respectively, number of county pairs used to calculate the mean treatment effect, point estimate of the mean treatment effect (per annum), and statistical confidence level.

The methodology of this study establishes that in these five states, growth accelerations indeed occurred in the years surrounding the state-level deregulation events. These five growth accelerations are economically quite sizable considering that the average (unconditional) annual growth rate in the pre-treatment period is only about 1.7%. This magnitude is nevertheless plausible in the several years immediately after deregulation because a small change in the value of stock of existing capital can have a large effect on economic output if the benefits are realized in a short period of time (Jayaratne and Strahan, 1996, p. 658).

Nevertheless, these five cases are out of the 23 events examined. In the vast majority (18 cases, or 80%) of the state-level branching deregulations we examine, significant economic growth accelerations cannot be statistically established in the years surrounding the deregulation events. However, the results can still be reconciled with those of the Jayaratne and Strahan (1996) study. On the one hand, our results indeed shows that the bank branching deregulations that took place prior to 1985 were generally not followed by faster economic growth. In fact, these early liberalizers grew *more slowly* by averagely 0.12% per annum compared with their regulated neighbors. On the other hand, the deregulation events that took place after 1985 (i.e., the second half of the sample) were in general associated with very large, positive growth effects (averagely 0.66% per annum). Note also that all of the five significant growth accelerations took place after (and including) 1985. Prior to 1985, there was no single case of *significant* growth acceleration. DeLong and DeYoung (2007) show that the earlier pioneers of mergers and acquisitions were typically less successful than the later followers, and that the so-called "learning-by-observing" effect exists in the banking industry consolidation process. The same logic can apply to the trend documented in our study, that the expansion opportunities enabled by the deregulations were exploited more and more successfully as time went by and experiences accumulated through a learning-by-observing process, while the earlier liberalizers may not necessarily benefit immediately.

Another noticeable difference between the five significant growth acceleration cases and the earlier deregulation events is that in all five cases the *interstate* banking deregulation took place before or during the same year of the *intrastate* branching deregulation, and thus much stronger competitive pressure was introduced by the deregulation by allowing not only typically small, out-of-county players, but also large, out-of-state (e.g., from New York or North Carolina) competitors to participate in the liberalized statewide banking market. These deregulations were thus stronger deregulations than others.

So far we have established that, in five out of 23 cases, *local economic growth appeared to significantly accelerate in the years surrounding the deregulation events* although this is not the same as saying that the deregulation events *caused* the accelerated growth rates. With respect to the other 18 events, no significant *correlation* between deregulation events and growth accelerations can be statistically established.

The main goal of this paper is to provide a generalized methodology and evaluation framework to assess the economic effect of many types of state-specific regulatory changes, of which branching deregulation is but one example. Thus, we do not intend to provide rigorous evidence to explain why we find what we find, although we do offer some plausible explanations later in Section 7. Hopefully, future research can explore further. Before turning to our view of potential explanations of the results, we first establish the robustness of our methodology and results in Section 6.

6. Robustness check: geographic spillover of deregulation effects?

If local residents can easily obtain access to credit from commercial banks on opposite sides of state borders, then the results of no deregulation effects can be easily explained by direct or indirect spillover of lending from the newly deregulated states to their neighbors across state borders. If border counties on both sides of the regulation change border benefit from the deregulation, then it is not surprising that we cannot find differences between them.

Cross-border lending by local commercial banks, however, should be minimal. In banking antitrust analysis conducted by the Federal Reserve Banks, the local market outside metropolitan areas is usually defined as a single county.[14] There are many reasons why banking markets are local, although the lending distance of nonbank financial institutions and credit card-type lending in particular has been increasing over time.

First, information asymmetry increases in distance as a result of communication and transport costs (Degryse and Ongena, 2004). Petersen and Rajan (2002), and Kwast Starr-McCluer, and Wolken (1997) both find that in the 1980s, when most of the branching deregulations took place, the *median* distance between banks and borrowers was four miles (and the 75th percentile 12 miles), which is well within county boundaries. Petersen and Rajan (2002) also find that 67% of the communications between banks and borrowers were carried out in a face-to-face personal meeting. Garmaise & Moskowitz's (2004, 2006) data on commercial real estate loans also suggest localized lending with a maximum radius of 15 miles. Many believe that the recent adoption of credit scoring models could increase lending distance. However, using Community Reinvestment Act data, Brevoort and Hannan (2006) show that, if anything, distance is becoming a more important factor even within a local market.

Second, state borders can create contract-enforcing barriers greater than those created by county borders. When defaults or disputes arise, in order for banks to recover loans from out-of-state debtors, banks may incur substantial costs in the process of going through the court system in a different state because their own in-house legal specialists may not have accumulated sufficient experience in the neighboring state's bankruptcy and foreclosure laws.

To summarize, even if borrowers are willing to travel across state borders, bankers could find it costly to lend to them due to information asymmetry reasons. Nevertheless, there could be some sort of indirect spillover of lending across state borders that could invalidate the comparison made in this study. For instance, residents in newly deregulated states could have more disposable cash on hand, which could be lent to their friends or relatives on opposite sides of state borders.

To consider this possibility, we collect a second group of counties as an alternative control, and then repeat the difference-in-differences analysis. The members of the treatment group remain the same. Here we compare the deregulated (treated) counties not to their immediate neighbors, but to their paired hinterland counties (as defined in Section 2.3) on the opposite side of the border. The hinterland counties are located within the still-regulated states, but farther away from the state borders. In other words, now the counties in the treatment group and the control group are *co-contiguous* with the border-deregulated counties located in-between them (see Fig. 2 for an example). The "Journey to Work" Census shows that although there still is a small number of people commuting between contiguous counties, the number is sharply reduced to trivial if the flow is between two *co-contiguous* (not directly contiguous) counties.

If spillovers effects of deregulation affected our previous results, which use border counties as a control, the use of hinterland counties as a control should reduce such influence, and the same difference-in-differences tests should signal many more cases of significant growth accelerations. Put differently, if there is any geographic spillover of lending across state borders, the hinterland counties that are farther away from state borders should not benefit as much as the border counties, if it is assumed that it takes lenders more effort to do business with more distant borrowers and that people have more friends in immediately adjacent counties. Note that the empirical design does *not* rely on assumptions about particular types of cross-border spillovers.

The results of the robustness test using hinterland counties as a second control group are presented in Table 8. The use of an alternative control group does not alter the main evaluation results. In only one more deregulation event (1986 in Mississippi) a statistically significant treatment effect is identified. In this event, using hinterland counties as a control group would signal marginally significant growth acceleration at the 90% confidence level. Furthermore, the statistical significance levels of the original five growth acceleration

[14]The Fed's definition of a local banking market is mainly based on the commuting pattern information obtained from the "Journey to Work" Census, which assumes that if people do cross borders in mass scale on a regular basis, then such borders do not effectively stop banks from competing to provide services to residents on the other side of the border and the two counties should belong to the same local market. The definition is designed for antitrust analysis, but it is also helpful in supporting the empirical design of this study.

Table 8
Robustness check: evaluating the actual deregulation events using the *hinterland counties* as the control group

Deregulation state	First year of deregulation	Number of county pairs	Mean average growth rate in pre-treatment period (%)	Mean raw treatment effect (%)	Mean adjusted treatment effect (%)	Statistical significance
Maine	1975	3	1.07	−0.44	−0.63	Negative
New York	1976	14	0.85	0.79	0.28	Insignificant
New Jersey	1977	6	1.98	0.17	0.41	Insignificant
Virginia	1978	16	4.14	−0.09	0.29	Insignificant
Ohio	1979	35	2.10	−0.05	−0.26	Negative
Connecticut	1980	2	1.44	−1.63	−0.73	Negative
Alabama	1981	22	2.00	0.09	0.37	Insignificant
Pennsylvania	1982	2	1.77	0.59	1.16	Insignificant
Georgia	1983	12	0.60	−0.44	0.18	Insignificant
Massachusetts	1984	3	2.32	−0.33	−0.06	Negative
Nebraska	1985	11	0.88	0.16	−0.11	Negative
Tennessee	1985	25	1.65	0.86	0.77	10%
Mississippi	1986	5	0.56	1.28	1.55	5%
Kansas	1987	11	1.15	0.56	0.49	Insignificant
Michigan	1987	5	0.51	2.68	1.99	5%
North Dakota	1987	6	2.82	−3.63	−2.26	Negative
West Virginia	1987	4	0.41	−0.24	0.20	Insignificant
Illinois	1988	9	0.26	−0.80	0.11	Insignificant
Louisiana	1988	8	0.80	0.57	1.05	10%
Oklahoma	1988	8	1.73	0.51	1.27	5%
Texas	1988	2	0.75	−1.65	−1.33	Negative
Missouri	1990	24	1.94	0.66	1.06	1%
Wisconsin	1990	16	0.93	2.21	0.53	Insignificant

To consider the possibility that cross-border spillover of deregulation effects may invalidate the empirical design, "hinterland counties" are identified and used as a second control group to conduct a robustness check on the main results. Each of the 23 bank-branching deregulation events is assessed separately to establish the statistical significance of its mean treatment effect. A different number of county pairs is identified in each deregulation event, depending on geography and the deregulation schedule of its neighboring states. The results of the assessments are presented in this table. The raw treatment effect is simply the difference-in-differences of the average growth rate in the pre- and post-treatment periods between the treatment counties and the control counties (in this case, the second control group of "hinterland counties"). Adjusted treatment effects control for the change in the income gap and the growth opportunity gap between the pre- and post-treatment periods, which if not taken into account can bias the point estimate. The mean treatment effect is obtained by averaging the treatment effects of all county pairs associated with a deregulation event. Evaluation of a deregulation event is based on 11 county pairs on average. The critical values of mean treatment effects are empirically obtained through simulating placebo deregulations on non-event borders. We use the critical values tabulated in Table 5, which already adjust for the downward bias created by positive spatial correlation of treatment effects within a chain of neighboring county pairs. We assess the statistical significance of treatment effects only when they are positive.

cases are higher when border counties as opposed to hinterland counties are used as control group, which goes against the hypothesis that cross-border spillover of deregulation effects bias against finding significant deregulation effects. These results suggest that cross-border spillover of deregulation effects should not have first-order influence on our previous results.

7. Discussions

Did the removal of restrictions on statewide branch-banking create significant growth acceleration in deregulated US states? Previous empirical literature finds that liberalization of statewide branching widely and significantly accelerated local economic growth. This study provides a more precise test by comparing border counties in deregulated states with their contiguous neighbors on opposite sides of state borders where intrastate branching was still prohibited. The comparisons reveal that significant growth acceleration in the years surrounding the deregulation events is not a general phenomenon as suggested by Jayaratne and Strahan

(1996). Specifically, statistically significant growth acceleration can be firmly established at a $>90\%$ confidence level in only five out of 23 of the deregulation events examined.

The endogeneity problem could be one of the reasons previous studies tend to find a correlation between deregulation and growth acceleration. Kroszner and Strahan (1999), for instance, find that the relative strength of winners (large banks and small, bank-dependent firms) and losers (small banks and the rival insurance firms) of deregulation can explain the timing of branching deregulation *across states*. Also, when state-level economic growth is studied, it is possible that the correlation found is created by deregulation events being *induced* by an expectation of growth opportunities that are *not observed by econometricians*. State-level deregulation events occurred in waves, usually clustered by region, and correlations identified in the existing literature could be picking up regional growth trends. The advantage of studying county-level growth is that it is unlikely that economic conditions of a county influenced regulatory decisions at the state level made by state legislatures, which had to accommodate interests of all constituencies, not only the border counties.

Moreover, Wheelock (2003) points out that states in the South and New England tended to deregulate earlier than Midwestern states, and several of these had among the highest average annual growth rates. It is possible that the growth accelerations are region-wide phenomena independent of banking regulations in individual states. When previous researchers compare earlier deregulated states in these regions with states in other regions, it is possible that they pick up the region-wide growth acceleration trend as evidence for the impact of banking deregulation at the state level. Our analysis at the finer geographic level is relatively free from the influence of such *cross-region* heterogeneity.

In financing economic growth, there could be a substitution effect between commercial banks (which were subject to branching regulation) and nonbank financial institutions (which have been free from such geographic restrictions). One explanation for the results of this study could be that local entrepreneurs are able to substitute other sources of financing (e.g., credit from nonbank financial institutions that lend at a longer distance) for bank financing. In the United States, long before the deregulation events considered in this paper, nonbank financial institutions developed to meet the demands frustrated by geographically restricted *commercial banks*. In the long term, the financing constraints created by branching regulation became less binding as nonbank financial institutions and capital markets (including the commercial paper market) reduced firms' dependence on banks. The negative effect of bank regulation on the local economy could have been overstated by not taking into account these substitution effects.

Furthermore, Kane (1996) and Kroszner and Strahan (1999) point out the irony that the cost of regulation is usually the lowest around the time it is removed. In the history of the US financial service industry, before an exclusionary statute comes to be formally rescinded, most of the effects targeted by the rescission will have already been tolerated by the enforcement system for years. Usually, statutory change does not occur until circumventive activity has driven the protective value of existing rules to their proponents below the amount opponents are willing to pay for their removal. Prior to the deregulations, the value of geographic exclusion had been eroded by technological innovations in lending. According to Petersen and Rajan (2002), the lending distance of nonbank finance companies grew rapidly in the 1970s and 1980s.[15] The increased ability of finance companies to lend to distant borrowers without setting up local branches clearly made branching regulations less effective over time in protecting the rents of local banks, which could explain why branching deregulation effects, when they took place, usually had already lost relevance to the local economy.

In the short term, it is still possible that, in the past, regulations and geographic restrictions on banks' expansions had inflicted large costs on the US economy, in particular at the early stage of industrialization when the absence of big banks posed constraints on financial needs of growing industrial corporations.[16] In the long term, such constraints have been greatly relieved because the development of accessible capital

[15]The median lending distance of nonblank institutions increased from 15.5 miles in the 1970s to 42 miles in the 1980s, and the share of in-person communication between borrowers and finance companies dropped from 27% to 12% among all types of communications, including phone-call and mail, whereas for banks, it only dropped from 77% to 67%.

[16]Giedeman (2005) finds that, during 1911–1922, restrictions on branch banking cause the severity of external finance constraints to *increase* with firm size. Rousseau and Wachtel (2005) find that the positive relation between finance and growth exists only for economies at per-capita income level between \$3,000 and \$12,000 (in 1995 constant USD), which may suggest that branching restrictions were more harmful in the past than now.

market and unregulated nonbank financial institutions has turned the US economy into one that is less bank-dependent than its European counterparts. Furthermore, to meet the frustrated demand and to exploit profit opportunities, market players have been constantly circumventing and eroding the burdensome regulations via legal loopholes, contractual and information-processing innovations, regulatory/structural arbitrage, and interpretive changes in the statutes that regulatory bodies actually enforce (Kane, 1981, 1984, 1996, provides detailed analyses). As Kane (1981, p. 359) asserts, "In the 1970s, loophole mining and fabrication became the main business of modern depository institutions." The development of all of these substitutes, however, required significant time, talent, and money. In the past, banking regulation could have inflicted costs on the economy in an endless race of loophole-mining and re-regulation between market players and regulators. Despite its long-term relative irrelevance, branching restrictions in the US could still be bad because it may have inflicted costs in the short term, which could amount to several decades.

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