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Evaluating the Real Effect of Bank Branching Deregulation[★]

Comparing contiguous counties across U.S. state borders

Rocco R. Huang[*]

The University of Amsterdam

ABSTRACT

This paper proposes a new methodology to evaluate the economic effect of state-specific policy changes, using bank-branching deregulations in the U.S. as an example. The new method compares economic performance of contiguous counties on opposite sides of 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. 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 only 5 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.

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[*] E-mail address: rhuang@tinbergen.nl

1. Introduction

Liberalization of the banking sector is, in general, shown to have had a positive impact on local economic growth (Levine [2004] 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 lifted 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, because the restriction on *inter*state branching (removed only much later) had essentially produced 50 segregated banking systems within the United States, one for each state.[1]

Potentially, removal of restrictions on bank entry and expansion could facilitate mergers and acquisitions, promote competition, increase bank efficiency, and thus, could help local economic growth. For example, Jayaratne and Strahan (1997) find that the relaxing of restrictions on bank expansion led to greater efficiency of banks, although they find no increase of credit supply. Using state-level data, Jayaratne and Strahan (1996) provide well-cited evidence that the deregulations were in general associated with faster local economic growth. Strahan (2003) provides a good review of the available evidence in favor of the positive effects of the deregulations.

However, 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 had created immediate and significant economic benefits for the local economy. Studies in existing literature tend to find a significant positive effect from the

[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.

deregulation of branching on the local economy, but most of them use a state as the unit of analysis. This practice we argue 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 degree of freedom in regressions, previous studies typically have had to use very diverse states from different regions to form the treatment and the control group; they were forced, for example, to compare Texas with 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 they are positive, whereas in many more others, they are 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 the manifestation of heterogeneity of growth paths in different regions (Garrett et al., 2004), or difference of expected future growth opportunities across states, independent of, or 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 only by state borders in cases in which one state deregulated intrastate branching earlier than did the other. Because these counties are immediately adjacent neighbors, they are expected to be similar in both observable, and more importantly, unobservable conditions, and will tend 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

[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

carefully matches and 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 a 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 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 shock that affects the local economy should translate into perceivable *short-term* changes in local incomes observable to econometricians.[3] Note that 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 removals of restrictions on statewide branching affect growth by comparing growth rate of per capita income on opposite sides of *regulation change borders*, after adjusting for income gap and 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 such dramatic cross-border regulatory difference as that seen in regulation change

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.

[3] This divide, however, 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 were more likely to be in an equilibrium state already, with respect to observable and unobservable local factors, including the then prevailing banking regulation arrangements. In the wake of a deregulation shock, however, adjustment taking place in the newly deregulated county (presumably toward a higher income level than its neighbor if deregulation should have positive effects) and cross-border growth rate difference created by this unilateral adjustment, should be perceivable in the short term (e.g., five years) as the pair slowly finds its way to a new equilibrium.

borders did not exist, to empirically obtain a statistical table of critical values, which helps us to statistically distinguish real treatment effects from the results of potential data-snooping. The same method also helps adjust the critical values for spatial correlations of treatment effects within a chain of neighboring county-pairs (which could bias the standard errors downward).

Among the 23 events of deregulations taking place during a 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. The new results call for further research on why regulation and deregulation of commercial banks' geographic expansions did not seem to substantially affect the local economy. We provide several plausible explanations.

The economic impacts of regulation or deregulation of U.S. commercial banks could well be overstated. Kane (1996) and Kroszner and Strahan (1999) have pointed out the irony that the cost of regulation is usually the lowest by the time it is removed. In the history of the U.S. financial service industry, most of the effects targeted by the 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 from the beginning constantly subject to erosions by market players through legal loopholes, contractual and information processing innovations, regulatory/structural arbitrage[4], and interpretive changes in statute-implementing regulations 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."

Also, the U.S. economy is much less dependent on banks than are continental European economies, and thus, burdensome regulations imposed on banks could have but limited real effects. Bank lending may not be critical, because other sources of financing can easily replace lending by commercial banks (Marquis, 2001). There is already empirical evidence suggesting

[4] US Banking Act of 1971 defines a bank as an institution that offeres demand deposits and originate 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.

that bank loans have no significant impact on economic outputs.[5] Considering the important role already played by *nonbank* financial institutions (which have not been geographically restricted) and capital markets already played vis-à-vis commercial banks before the deregulation, it is not clear whether commercial banks provided any credit service to the economy that could not be replaced by nonbank financial institutions.[6] Furthermore, post-deregulation consolidations of banks could negatively impact smaller and newer firms that are the most dependent on banks.[7]

Below we provide a roadmap for the rest of the paper. Section 2 introduces the procedure of identifying regulation change borders and contiguous counties. In Section 3, the empirical strategies are introduced. There are several econometric difficulties that need to be addressed: First, how should the difference-in-differences treatment effect be defined to avoid potentially understating standard errors? Second, how can we correct biases in the point estimate of treatment effects? Third, how can we establish correct standard errors, and thus, statistical significance of the estimates, through randomization-type fictitious “placebo deregulation events”? In Sections 4, 5, and 6, the proposed strategy is implemented. In Section 4, a statistical table of critical values is empirically created through a randomization procedure, also taking into account the influence of spatial dependences. In Section 5, the economic effects of each of the 23 events of branching deregulations are assessed, based on the critical values. In Section 6, using

[5] Driscoll (2004) uses a panel of state-level data to find 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.

[6] 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 the branching regulations.

[7] Berger et al. (2005) provide evidence consistent with the belief that small banks are better able to collect and act on soft information than large banks are. 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.

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 we discuss several plausible explanations about why regulation and deregulation of commercial banks' geographic expansions, in most cases, appeared not to have substantially affected the local economy.

2. Matching of Contiguous Counties across Regulation Change Borders

To assess the real effects of deregulations by comparing the economic performance of the treatment group vs. the control group, one first needs to look for pairs of neighboring states separated by the so-called *regulation change borders*. To be included in the study, we require that, for a pair of states, and thus their bilateral border, bank branching expansions in the second state must remain restricted for at least three years after restriction in the first state was removed. These borders are called *regulation change borders*. In the research sample we eventually composed, the average gap between the two states' deregulation timings reaches nearly 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

Thirty-eight segments of such regulation change borders meeting the above requirements are identified. 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 Figure 1. Using these borders, 23 events of state-level branching deregulations throughout the United States spanning from the 1970s to the 1980s

[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, becaue 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.

can be evaluated, regarding their impacts on the local economy. These deregulation events include (in chronological order): Maine (75), New York (76), New Jersey (77), Virginia (78), Ohio (79), Connecticut (80), Alabama (81), Pennsylvania (82), Georgia (83), Massachusetts (84), Nebraska (85), Tennessee (85), Mississippi (86), Kansas (87), Michigan (87), North Dakota (87), West Virginia (87), Illinois (88), Louisiana (88), Oklahoma (88), Texas (88), Missouri (90), and Wisconsin (90).

[Insert Table 1 and Figure 1 about here]

One then needs to match pairs of contiguous counties across these so-called regulation change borders. *The National Atlas of the United States* (http://www.nationalatlas.gov/) was used to identify 285 pairs of contiguous counties. The 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 will form the treatment group, while those located in states where restrictions were removed at least three years later will 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 local banking market (e.g., Berger, Demsetz, & Strahan, 1999; Black & Strahan, 2002; Prager & Hannan, 1998; and Rhoades, 2000). Many researchers have used a county as the unit of the local economy to study the effect of bank activities on economic outputs[9] (e.g., Ashcraft, 2005; Calomiris & Mason, 2003; Clair et al., 1994; and Gilbert & Kochin, 1989). In 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 a local banking market.

[9] because Forni and Reichlin (1997) show that, in the United States, county-specific components of output fluctuations are 1.35 times greater than state-specific components.

2.2. Contiguous counties are similar in observable characteristics

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

To give readers a better understanding of how geographic matching has improved from previous studies in identifying the control group at least in observable characteristics, we conduct a 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, respectively) from all counties nationwide that deregulated at least three years later. This alternative way of forming the control group is equivalent to the practices of Jayarante and Strahan (1996) and other typical studies in the literature, which obtain point estimate of treatment effect by comparing at certain points in time deregulated states with all other states nationwide that had yet to deregulate. For a specific deregulation event, the numbers tell us if counties in the control group are drawn nationwide from states that deregulated at least three years later, what the average absolute difference will be between the treatment group and control group counties, in terms of income per capita and manufacturing share, respectively.

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

[insert Table 2 about here]

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 reason for not doing so is that observable differences can be easily controlled for, do not pose a large challenge to econometricians, and thus, is not a major problem in this study. On the contrary, unobservable characteristics, in which contiguous counties are less likely to differ from each other, are what usually trouble econometricians because there is no way econometricians can explicitly adjust for unobservable growth opportunities, otherwise they are observable in the first place. 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 explicitly adjust for the remaining observable differences, which is less difficult for econometricians.

2.3. Hinterland counties

A second control group of paired counties is also identified, which we name *hinterland counties*. They are located on the same side of the regulated counties, and therefore were also kept regulated longer than the deregulated counties on the opposite side of state borders. The hinterland counties, however, are farther away from the regulation change borders, and are not directly contiguous with the deregulated counties. Nevertheless, they remain contiguous to the border regulated 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.[10] 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 of this robustness check will be explained in detail in Section 6.

To help readers better understand the geographic terms we mention above, Figure 2 provides a graphical example: Georgia lifted the branching regulation 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 farther within Florida is a hinterland county.

[Insert Figure 2 about here]

3. Methodology: Estimating the Treatment Effects

3.1. Collapsing of information into "pre-" and "post-" 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-" Period: In this period, both states restricted intrastate branching. The "pre-" treatment period is defined as a ten-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 ten years, as county-level income growth data are available only from 1969.

[10] There are several reasons why hinterland counties cannot be found for some county-pairs. One of the simple reasons is that 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.

(2) "Post-" 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.[11]

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-" 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:

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

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

[11] 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 the year is generally regarded as the completion date of geographic banking deregulations in the United States.

"pre-" and "post-" periods, respectively. Per capita personal income data at county level were 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: If a certain bank branching deregulation has any positive effect on the local economy, one should observe that deregulated counties experience a greater growth acceleration in the several years after the deregulation compared to their neighbors across the regulation 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 event of deregulation. First, we need to correct bias in the point estimate of treatment effect. 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. But let's start from the easier one first.

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

To correct bias in the point estimate of 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 has helped us 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 it 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;

because in the treatment effect's definition $TE = \left(g_{1,post} - g_{1,pre}\right) - \left(g_{0,post} - g_{0,pre}\right)$, if a certain time-invariant, county-specific factor affects growth, it should have affected $g_{1,\ pre}$ ($g_{0,\ pre}$) as much as it had affected $g_{1,\ post}$ ($g_{0,\ post}$) , and should have been canceled out already.

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

The first one is income gap, which affects 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 next years, even absent of any deregulation effects. Not taking into account this factor would lead us to overestimate the treatment effects. Nevertheless, income gap at the start of the "post-" 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-" period), then the convergence effect would be the same for both periods and should have been 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 10 years before and at the time of the deregulation.

The second important factor that affects growth difference is the growth opportunity gap, which is determined by sector-specific shocks at the regional level (Barro & Sala-i-Martin, 1992). Sector-specific shocks at the regional level, i.e., regional sectoral growth pattern, affect local growth differentially depending on the local industrial structure. If in a certain region manufacturing grows slower than non-manufacturing over a period, then a county with less manufacturing share than its neighbors at the beginning of the period tends to grow faster subsequently, even absent of 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 regional-level growth rate difference between the manufacturing sector and the non-manufacturing sector, i.e.,

$$Growth\_Opportunity\_Gap = (M_1 - M_0)(G_M - G_S) \qquad (2)$$

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 non-manufacturing in the region. The derivation of the formula is explained in the footnote.[12] 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-state economies in question, and thus, growth opportunity gaps differ for every county-pair. Again, this factor does not matter if (a) industrial structures remain the same 10 years before and at the time of the deregulation; *and* (b) regional growth patterns are the same in the two periods. What we need to control for, instead, is the change (difference) in growth opportunity gap between the "post-" and "pre-" 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 error of a treatment effect, or in other words, to find out how large a treatment effect needs to be to qualify as statistically significant growth acceleration. This is a challenging task. OLS standard errors obtained from the in-sample could be biased downward, because neither the research

[12] The predicted growth rate of county 1, based on region-wide sectoral-specific shock and local industrial structure, is $M_1 G_M + (1 - M_1) G_s$, and that for county 0 is $M_0 G_M + (1 - M_0) G_s$. The "growth opportunity gap" between county 1 and county 0, thus, is the difference between the two predicted rates:

$$[M_1 G_M + (1 - M_1) G_s] - [M_0 G_M + (1 - M_0) G_s]$$
$$= M_1 G_M - M_1 G_S - M_0 G_M + M_0 G_s = (M_1 - M_0)(G_M - G_S)$$

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. As a matter of fact, when we decide 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 non-random choice 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 exacerbate the problem because a positive correlation of shocks and treatment effects within a border county chain greatly increases the chance of finding large mean of 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 the banking sector and in the economy. These factors greatly increase the probability of finding large treatment effects through data-mining.

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

We then randomize (also known as "permutate") fictitious placebo deregulation treatments on these non-event borders and calculate the "treatment effects" for these placebo events based on actual growth rates 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 year for the deregulation to take place; and (c) either side of the border to receive the deregulation earlier (i.e., which side will be assigned to treatment group and the other to control group). Therefore, the universe of the placebo deregulations can be known by exhausting all of the possible combinations.

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 choose to draw a chain of 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 effects of these neighboring county-pairs is calculated and retained to form an empirical distribution that has by construction taken into account the spatial correlations of treatment effects among neighboring county-pairs.

Because the placebo deregulations are completely fictitious, the distribution of their "treatment effects" can inform us intuitively: by certain percentage of chance how large (extreme) a treatment effect can be obtained by examining a county-pair randomly drawn from borders where no real treatments are applied in reality. Let's assume the 95th percentile of the distribution is a treatment effect of +2% per year, and you, a researcher of the data set, are given 20 draws from the universe of possibilities in designing a study and producing an empirical result. Then simply by a five-percentage chance, you could find growth acceleration of such magnitude in 1 of the 20 draws. Similarly, if 20 researchers are mining the same dataset, one of them could by chance 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 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 will be useful not only for this particular study, but 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, which may include personal bankruptcy law, foreclosure law (judicial vs. power-of-sale), predatory lending law (modern version of usury law), depositor preference law, and anti-takeover 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 correcting bias in the point estimate of treatment effect. In Section 3.2, we have established that income gap and growth opportunity gap can affect growth rate difference, and they need to be controlled for to correct bias in the point estimate of treatment effects. To do this, we will need to run a regression of the raw treatment effects against changes in income gap and growth opportunity gap, and then the residuals of the regression are retained as the *adjusted* treatment effects. This, however, is yet to be 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 where deregulation actually took place, what one is studying is *not* how 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 deregulations help lower-income *deregulated* counties more than they help higher-income *deregulated* counties, *conditional on* deregulations having taken place and having produced positive effects. Such an interaction effect between the actual occurrence of deregulation event and initial income gap is implicitly installed in the regression model by the sample-selection itself, if the model is estimated on the in-sample, i.e., where deregulations actually happened.

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 income gap and growth opportunity gap unconditionally predict treatment effects. The regression is specified as follows (*see Section 3.2 for definitions*):

$$\textit{Raw Treatment Effect} = \beta_1 \times \textit{Change in income gap} + \beta_2 \times \textit{Change in growth opportunity gap} + \varepsilon \qquad (3)$$

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

$$\textit{Adjusted Treatment Effect} = \textit{Raw Treatment Effects} - \overline{\beta}_1 \times \textit{Change in income gap} - \overline{\beta}_2 \times \textit{Change in growth opportunity gap} \qquad (4)$$

4. Randomizing Placebo Deregulations on Non-Event Borders

In this section, we will implement the empirical strategies introduced in the 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 empirical distribution of the treatment effect estimator, as well as the coefficients of Eq.(3), which will be used to correct bias in 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 segments of bilateral state borders that can potentially be utilized for the study, of which 38 are so-called *regulation change borders* according to our definition (i.e., one side of the border deregulated branching earlier, while the other side had not followed within three years). These regulation change borders will be used to assess the real effects of actual deregulation events. The remaining 32 segments of borders are defined as the *non-event borders*, where such dramatic events as those observed in the regulation change borders did not take place. In Figure 3, the 32 segments of non-event borders 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.

[insert Figure 3 and Table 3 about here]

We will 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. This can help us create a benchmark to statistically distinguish real deregulation effects from what can be obtained by data-snooping. In constructing a placebo deregulation, we can randomly draw a county-pair from these borders, choose the year for the placebo deregulation, and apply it earlier to one side of the border than the other. And then we will calculate "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 1 of the 11 years (1979-1989)[13]; and (c) either side of the border (for the deregulation to take place earlier). Therefore the total number of all possible combinations is 5,852 (i.e., 266 × 11 × 2).

[13] The "pre-" period is ten 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 at 1994, the last year possible for a placebo deregulation with a five-year "post-" period has to be 1989.

The schedules of placebo deregulations are standardized so that the "post-" 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-" 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 income gap and growth opportunity gap

As discussed in Section 3.2, income gap and growth opportunity gap between treatment and control group, if not controlled for, would bias the point estimate of treatment effects. Thus, after each simulation, we calculate not only the raw treatment effects, but also the changes in income gap and growth opportunity gap between the "pre-" and "post-" periods. Then we pool together the information of all of the 5,852 simulations, and estimate an OLS regression of the *raw* treatment effects against changes in income gap and growth opportunity gap, as specified in equation (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.[14]

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

Note that the standard errors of these OLS coefficients are clearly under-estimated, because a county is used in separate scenarios for many times, and thus, included for 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, will be used in this paper.

[14] By construction of the simulations, i.e., a county is used as 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 two control variables, change in income gap and change in growth opportunity gap, have zero as their means. The standard deviation of "change in income gap" is 10.8%, while that for "change in growth opportunity gap" is 0.366.

The negative coefficient on 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 happens, 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 effects.

The positive coefficient on growth opportunity gap confirms that change in either local industrial structure or regional growth pattern/trend has important impacts on future growth. If county A has a lower manufacturing share than its neighbor's, and this remains unchanged 10 years before and at the time of deregulation, but regional manufacturing grows slower than non-manufacturing in the "post-" period than in the "pre-" period, then county A will naturally tend to grow faster even in the absence of a deregulation. Similarly, if the regional growth pattern remains unchanged in the "pre-" and "post-" periods (and manufacturing grows slower than non-manufacturing), but county A's manufacturing share drops even further during the 10-year pre-period; then subsequently after deregulation, county A will naturally accelerate further even absent of 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 Figure 4, the whole 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 effects when there are actually no treatments at all. Note that by construction (that a placebo deregulation could occur earlier in both side of the border, in separate simulation scenarios), the two-tails distribution of the fictitious treatment effects obtained from the population of placebo simulations is always symmetrical with zero as the mean.

[insert Figure 4 about here]

The distribution in Figure 4 tells us that when studying a non-event border along which there is only one county-pair, there is a 10% random chance that we could find treatment effects (growth acceleration) greater than 1.82%, even in the absence of any actual occurrence of treatments 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, and the point estimate of the treatment effect is 1.81%, we still cannot establish at 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 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 p-value 0.10, 0.05, 0.01 is $K_{0.10,1}$=1.82%, $K_{0.05,1}$=2.45%, and $K_{0.01,1}$=4.20%, respectively.

[insert Table 4 about here]

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 segment of the border, 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, ... ,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 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 will 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 of 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 use the extrapolated critical values produced in the last section to *the mean* of the N treatment effects.

Spatial correlation, however, 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 will 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 extrapolate them to the mean treatment effects of N>1 cases using the formula $K_n = K_1 \big/ \sqrt{N}$, assuming that treatment effects for neighboring county-pairs along a border are independent of each other. In the new series of placebo deregulations, in each of them, instead of an individual county-pair we draw a chain of N (N>1) neighboring county-pairs along a border, and as usual choose the year of deregulation, and select which side of the border is to receive the deregulation first. The treatment effect is calculated in the same way as in the case of single county-pair. What differs is that now we will 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 chain of neighboring counties can be sampled.

As the products of the simulations, we obtain 50 empirical distributions of *mean* treatment effects, for N=1, 2, ... ,50, respectively. Based on these distributions, we can then 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 size from 1 to 50. To illustrate the changes in critical values after taking into account spatial correlations, Figure 5 plots two curves based on the 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.

[insert Table 5 and Figure 5 about here]

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, and 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 the mean treatment effects will be greater than $2.45\% / \sqrt{10} = 0.78\%$ in 5% of the time, 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 chance actually exists that a mean treatment effect greater than 1.16% will be found. 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 will mainly use Table 5 to evaluate statistical significance of the estimated treatment effects.

5. Evaluating Twenty-Three Actual Events of Deregulations

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 on each of the 23 actual events of branching deregulations 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.

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), respectively, on several variables of interest, including the means and medians of growth rates, income per capita, and manufacturing share. The averages and medians are calculated by pooling all county-pairs used in the study, from all 23 events of deregulations, and serve to help readers gain an overall picture of the range of average growth rate in the "pre-" and "post-" periods. Assessments, however, will be conducted separately for each individual deregulation event. Pooling will obscure the important idiosyncratic information of each individual event, because Wall (2004) already 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 county as unit of analysis has increased the degree of freedom in our estimations.

[insert Table 6 about here]

5.1. Obtaining point estimates of treatment effects

We first need to obtain a correct point estimate of mean treatment effect for each actual deregulation event, adjusted for biases potentially created by, as discussed in Section 3.2, change in income gap and change in growth opportunity gap between the "pre-" and "post-" periods. As discussed in Section 3.4, for the adjustment to truly reflect effects unrelated to the deregulation itself, we will 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 create upward bias for the point estimate of treatment effect. The following formula based on coefficients obtained from Eq. (5) in Section 4.2 can help us correct for the biases.

$$\textit{Adjusted Treatment Effect (TE)} = \textit{Raw TE} + 0.1293932 \times \textit{change in income gap} - 0.3815534 \times \textit{change in growth opportunity gap} \quad (6)$$

The coefficients are obtained from the non-event sample. Note that had we estimated and used the coefficients based on the in-sample, 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. This truly raises the degree of freedom in estimation and reduces standard errors of the point estimates. In Jayaratne and Strahan (1996), and other similar studies that use state as the unit of analysis, only one treated subject (state) can be evaluated for each deregulation event. To nominally raise degree of freedom and reduce estimation standard errors of OLS coefficients, they typically had to pool together all times-series information and all deregulation events. This strategy has a potential problem: Bertrand et al. (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, it is a general phenomenon, not driven by individual cases, that deregulated states grew relatively faster after deregulation as compared to control states that at first had not yet deregulated. They show that, of the 35 states that deregulated since 1972, all but 6 states performed better (but not necessarily statistically significantly) than the corresponding control states. The six exceptions were New Hampshire, Florida, Michigan, Kansas, Colorado, and New Mexico.

In Table 7, we report, for each of the 23 actual deregulation events, 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-" 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 7 out of the 23 events examined in the 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 slower after deregulation, compared to control states.

[insert Table 7 about here]

5.2. Establishing statistical significance

Furthermore, 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 distinguished from what can be obtained in fictitious treatments.

The evaluation results of statistical significance are also indicated in Table 7. Out of the 23 actual events of branching deregulations, 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 them against the two tables of critical values: one (Table 4) ignores spatial dependence within a chain of neighboring county-pairs and is biased downward, whereas the other (Table 5) adjusts for it. Using the data from the table that assumes no spatial dependence, which underestimates the standard errors, there are only seven events where we can establish statistical significance at higher than 90% level. After adjusting for downward-biased standard errors due to positive spatial correlations, only five are left that are statistically significant at 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 treatment effect, and statistical confidence level.

Based on the methodology of this study, we can establish that in these five states, growth accelerations indeed occurred in the years surrounding the deregulation events. These five growth accelerations are economically quite sizable considering that the average (unconditional) annual growth rates in the "pre-" 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 total) of the state-level branching deregulations we examine, significant economic growth accelerations are not able to be established in the years surrounding the deregulation events.

It is worth mentioning that all of the five growth accelerations took place after 1985, in the later part of our sample period. Prior to that, there was no single case of significant growth accelerations and the average treatment effect is – 0.12%. Year 1985 was the beginning of a period of dramatically increased bank failure rates, which drove small banks to drop their opposition to intra- and inter-state acquisitions to find higher purchase prices. Thus, these deregulations took place in totally different circumstances, were more unexpected, and could have been driven by different conditions than their predecessors. Another important difference of these five events from others is that the *interstate* banking deregulations in all five cases took place before or at the same year of the *intrastate* branching deregulations, and therefore, these branching deregulations may introduce stronger potential competitions than in other states, by also allowing out-of-state (e.g., from New York) large competitors to participate. To sum up, there could be a structural break in 1985 on the nature and characteristic of the branching deregulations and on the relations between deregulation events and growth accelerations.

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, and branching deregulation is but one example. Thus, we do not intend to explore very deeply to provide rigorous evidence to explain why we have found what we have found, although we will offer some plausible explanations later in Section 7.

So far we have established that, in 5 out of 23 cases, *local economic growth appeared to significantly accelerate in the years surrounding the deregulation events*, although it is a different question whether deregulations had *caused* them. In the other events, no significant *correlation* between deregulation events and growth accelerations can be statistically established. Hopefully, future research can go deeper into what we have found empirically. Before providing some of our explanations of the results, we will first spend some time in Section 6 to establish the robustness of the methodology used in this study.

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 done by Federal Reserve Banks, the local market outside metropolitan areas is usually defined as a single county.[15] 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 et al. (1997) both find that in the 1980s, when most of the branching deregulations took place, the *median*

[15] The Fed's definition of local banking market is mainly based on the commuting pattern information obtained from the "Journey to Work" Census, assuming that if people do cross borders in a 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 anti-trust analysis, but it is also helpful in supporting the empirical design of this study.

distance between banks and borrowers was 4 miles (and the *75th percentile* is 12 miles), which is well within county boundaries. Petersen and Rajan (2002) also find that 67% of the communications between banks and borrowers were done by face-to-face personal meeting. Garmaise and 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 (2004) show that distance is if anything becoming more of an 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, they could incur substantial costs in the process of going through the court system in a different state because their own in-house legal specialists could not have accumulated sufficient experiences in the neighboring state's bankruptcy and foreclosure laws.

To sum up, even if borrowers are willing to take the great hassles to travel across state borders, bankers could find it costly to lend to them, for 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 the newly deregulated states could now 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 perform the same difference-in-differences analysis. The members of the treatment group remain the same. We will now 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 Figure 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 there were spillovers of deregulation effects that affected our previous results, which use border counties as control, the use of hinterland counties as control should reduce such influence, and the same difference-in-differences tests should signal many more cases of significant growth accelerations. 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, because spillovers should mainly benefit the border counties, if it is assumed that it takes lenders more efforts to do business with more distant borrowers, and that people have more friends in immediate adjacent counties. The empirical design, thus, 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 growth acceleration marginally significant at 90% confidence level. Furthermore, the statistical significance levels of the original five growth acceleration 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. The results in general suggest that cross-border spillover of deregulation effects should not have first-order influence on our previous results.

[insert Table 8 about here]

7. Discussions

Did removal of restrictions on statewide branching create significant growth accelerations in deregulated U.S. states? Previous empirical literature has found 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 at first 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). In only 5 out of 23 of the deregulation events examined, statistically significant growth acceleration can be firmly established at a >90% confidence level.

The endogeneity problem could be one of the reasons why previous studies tend to find correlation between deregulation and growth accelerations. 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 deregulations being *induced* by an expectation of growth opportunities that are *not observed by econometricians*. State-level deregulations occurred in waves, usually clustered by region, and correlations identified in existing literature could pick up regional growth trends. The advantage of studying county-level growth is that it is unlikely that economic conditions of a county had influenced regulatory decisions at state level made by state legislatures, which have 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 compared earlier deregulated states in these regions with states in other regions, it is possible that they picked up the region-wide growth acceleration trend as evidence for the impact of banking deregulation at the state level. Our analysis at the lower 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 deregulations, nonbank financial institutions had developed gradually 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 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) have pointed out the irony that the cost of regulation is usually the lowest at the time it is removed. In the history of the U.S. 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), lending distance of nonbank finance companies was growing rapidly in the 1970s and 1980s.[16] The increased ability of finance companies to lend to distant borrowers without setting up local branches clearly had made branching regulations less effective over time in protecting the rents of local banks, which could explain why branching deregulations, at the time they took place, usually had already lost relevance to the local economy.

In the short term, it was still possible that, in the past, regulations and geographic restrictions on banks' expansions had inflicted large costs on the U.S. economy, in particular at the early stage of industrialization, as the absence of big banks posed constraints on financial needs of growing industrial corporations[17]. In the long term, such constraints have been greatly relieved because the development of capital market and unregulated nonbank financial institutions has turned the U.S. economy into one that is less bank-dependent than its European counterparts. Furthermore, market players, to meet the frustrated demand and to exploit profit opportunities, have been constantly circumventing and eroding the burdensome regulations via legal loopholes, contractual and information-processing innovations, regulatory/structural arbitrage, and interpretive changes in statute-implementing regulations that regulatory bodies actually enforce (Kane [1981, 1984, 1996] has provided 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, had taken up significant time, talents, and money. To sum up, in the past, banking regulation could have inflicted costs on the economy in the endless "arm race" in loophole-mining and re-regulation between market players and regulators. Despite its long-term irrelevance, branching restrictions

[16] The median lending distance of nonbanks 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 just dropped from 77% to 67%.

[17] 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.

in the U.S. could still be bad because it may have inflicted costs in the short term, which could mean several decades.

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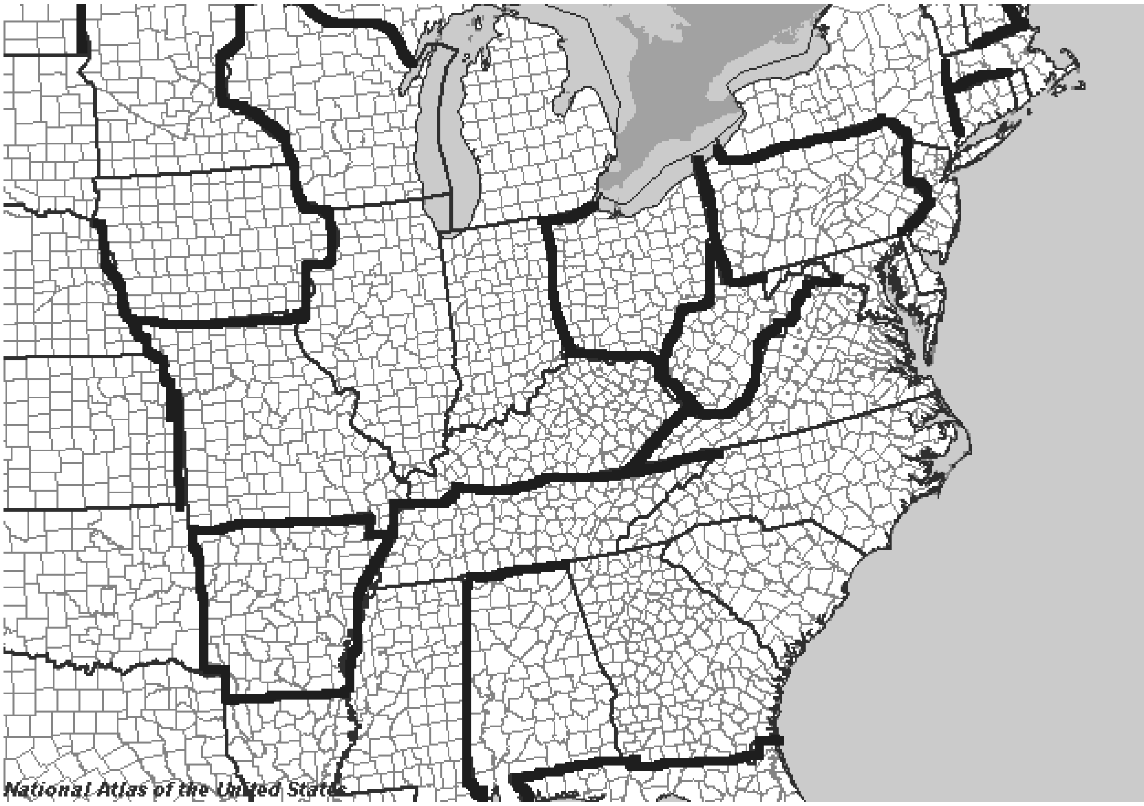
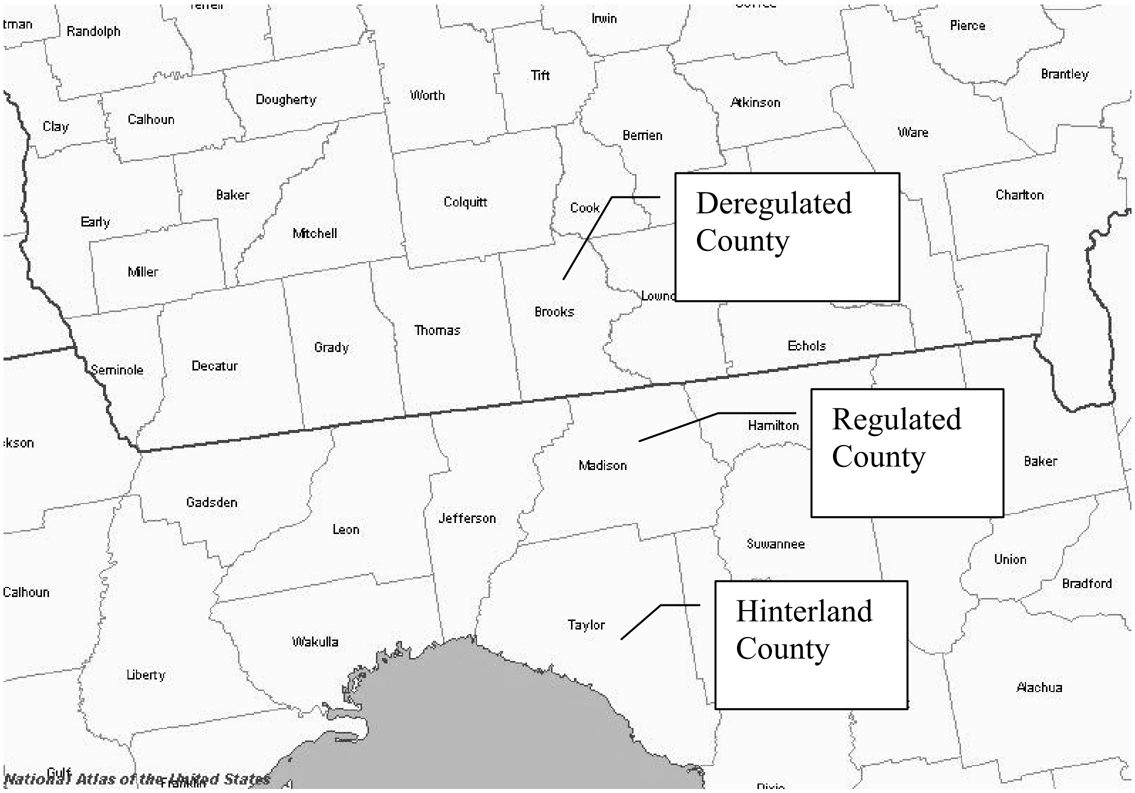
Figure 1: **“**Regulation change borders”



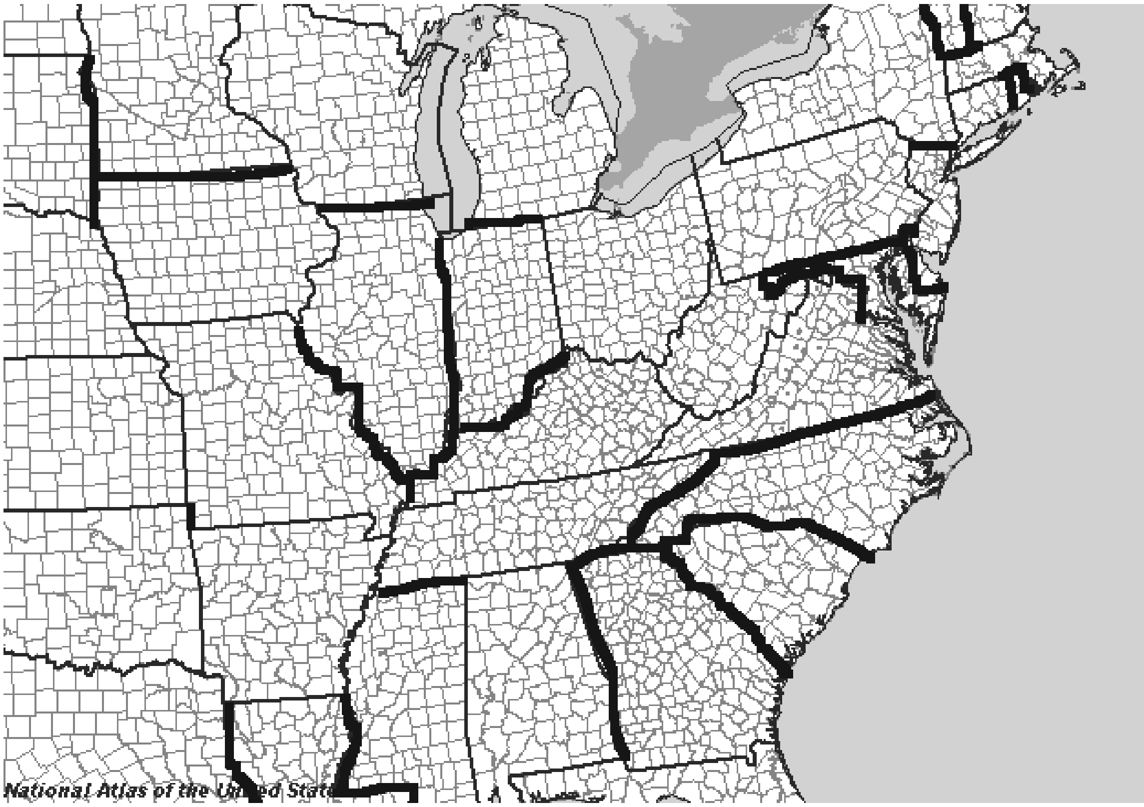
Note: This study identifies 38 segments of so-called *regulation change borders*, which are highlighted in the map. For at least three years, and on average six years, there were regulatory differences across these regulation change borders: banks on side of the borders were relieved from restriction on statewide branching; while on the other side, restrictions were eventually removed but at least three years later. See Section 2.1 for details.

Figure 2: Deregulated county, regulated county, and hinterland county



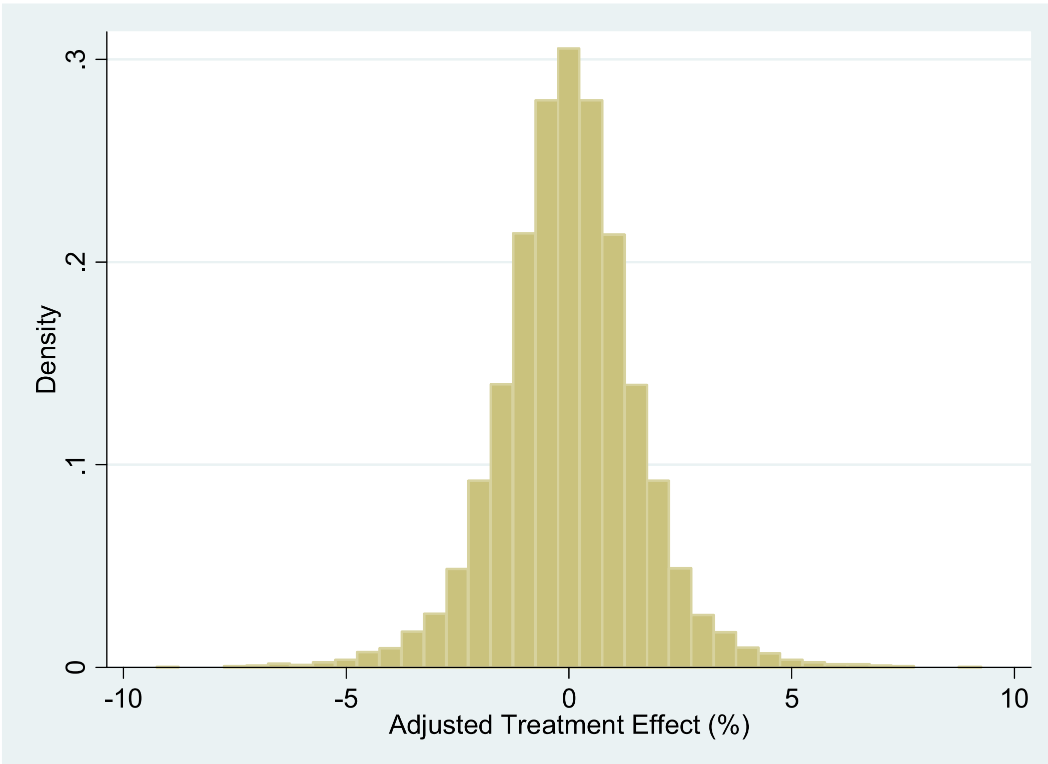
Note: This is a map of the area around the Georgia-Florida border. Georgia removed restrictions on statewide branching in 1983, whereas Florida removed them in 1988. Thus, there were regulatory differences across the Georgia-Florida border during the 1983-1988 period. An example is given in the map: 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 details.

Figure 3: **“**Non-event borders”



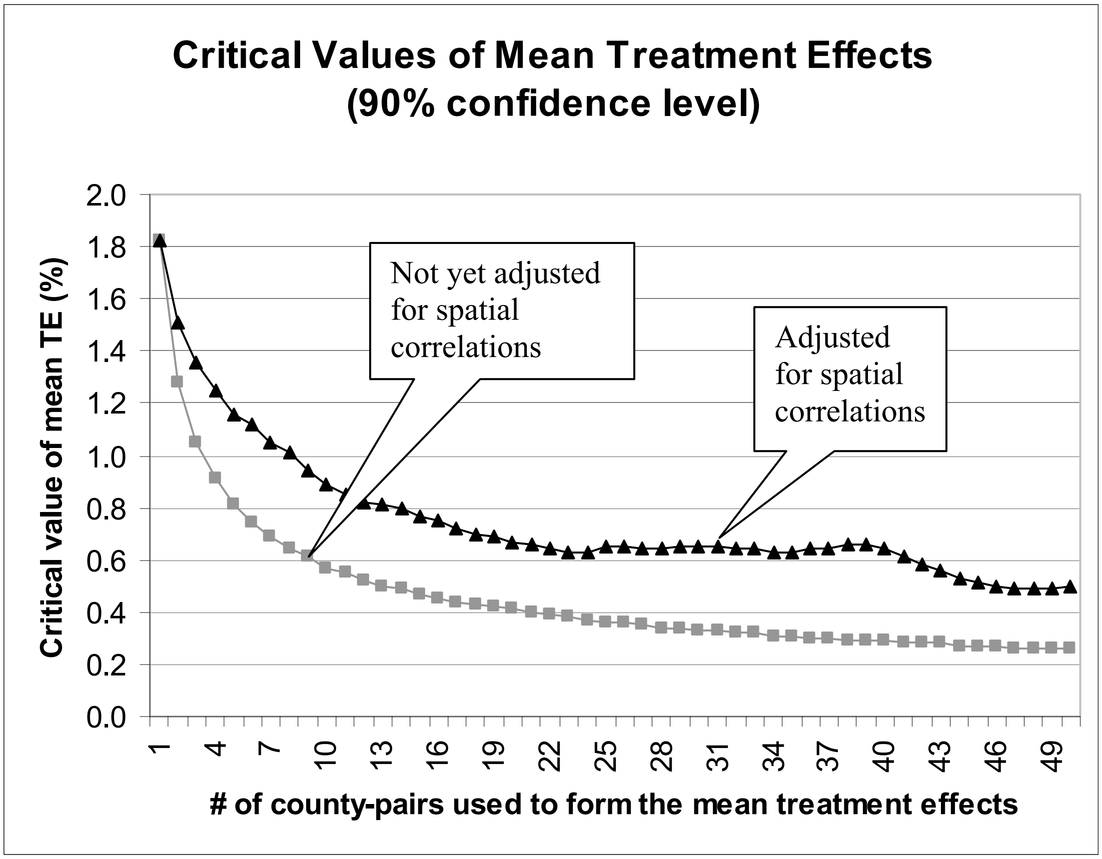
Note: This study identifies 32 segments of so-called *non-event borders*, which are highlighted in the map below. Across the so-called *regulation change borders*, for at least three years there were regulatory differences, with commercial banks on only one side of the borders free from restriction on statewide branching. Across these *non-event borders*, however, there were no such dramatic situations. 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 later 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 Section 3 and Section 4 for details.

Figure 4: Empirical distribution of fictitious treatment effects obtained from the placebo deregulation events



Note: In the study, fictitious placebo deregulations are randomized on the non-event borders, and then 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, by a certain percentage of chance, how large 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 details. The bin size of the histogram is 0.5%.

Figure 5: Empirical critical values of mean treatment effects: Before and after adjusted for spatial correlations



Note: 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, by a certain percentage of chance, how large a mean treatment effect we could obtain from data-snooping on the non-event borders where deregulations do not take place in reality; and thus, when one obtains a mean treatment effect from an actual deregulation event that actually occurs, how likely it 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, thus, is 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 had we not adjusted for positive spatial correlations. See Section 4.4 for details.

Table 1: Paired states and regulation change borders

Early Deregulator			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

Note: Pairs of states that bilaterally form the 38 segments of regulation change borders 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).

Table 2: How does the use of contiguous counties help 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**

Note: To give readers a sense of how geographic matching has improved 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 differences (in terms of income per capita and manufacturing income ratio, respectively) 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, which produce point estimate of treatment effects by comparing at certain points in time deregulated states with 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 will be the average differences between treatment group and control group counties, in terms of the two observable characteristics. In the Table, averaged by deregulation event, we present and compare the observed absolute differences between treatment and control group, achieved through the two different approaches of control-group sampling. It is clear that in almost all cases, geographic matching produces 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 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

Note: Thirty-two segments of so-called *non-event borders* 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 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, too.) 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 treatment and control groups in separate scenarios.

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

Note: Along the 32 segments of non-event borders, 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 will be scheduled to deregulate five years later. Then, the raw treatment effects will be calculated based on the difference-in-differences of average annual growth rate between "post-" and "pre-" period and between the two contiguous counties. The "adjusted treatment effect" is then obtained by taking the residuals from a regression of raw treatment effect on change in income gap and growth opportunity gap between the "post-" and "pre-" period.
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: by a certain percentage of chance how large a "treatment effect" we could obtain by randomly selecting a county-pair from borders where cross-border differential treatments did not occur in reality. In actual events of deregulations, 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: $K_N=K_1/SQRT(N)$, 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 the multiples of 5s.

Let's take an actual deregulation event as an example 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 90%, 0.82 for 95%, and 1.40 for 99% confidence level. Since the actual treatment effect 0.46 is smaller than 0.61, it is established that in the case of Illinois, significant treatment effect cannot be established statistically in the years surrounding the deregulation event. The reason is that even by data-snooping, in more than 10% of chance you can find a mean treatment effect greater than 0.61 if 9 *independent* county-pairs are drawn from borders where such differential treatments did not occur in reality. See Section 4.3 for details.

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

Note: Along the 32 segments of non-event borders, 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 will be scheduled to deregulate five years later. Raw treatment effects will be calculated based on the difference-in-differences of average annual growth rate between the "post-" and "pre-" period and between the two contiguous counties. The "adjusted treatment effect" is then obtained by taking the residuals from a regression of raw treatment effect on change in income gap and growth opportunity gap between the "post-" and "pre-" 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 done 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, by a certain percentage of chance how large a *mean* treatment effect we can obtain by 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 multiples of 5s.

Let's take an actual deregulation event as an example 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 90%, 1.23 for 95%, and 2.08 for 99% confidence level. Since the actual treatment effect 0.46 is smaller then 0.94, in the case of Illinois, significant treatment effect cannot be established statistically in the years surrounding the deregulation. The reason is that even by data-snooping, by a greater than 10% random chance, a mean treatment effect greater than 0.61 can occur if a chain of 9 neighboring county-pairs is drawn from borders where treatments did not actually occur in reality. See Section 4.4 for details.

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

	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-" period (%)	1.74	1.69	1.75	1.67	1.66	1.49
Average growth rate in "post-" period" (%)	1.40	1.34	0.99	1.00	1.07	0.92
Within "Acceleration" (%)	-0.34	-0.06	-0.76	-0.57	-0.59	-0.56
Standard deviation of these "accelerations"	(2.50)		(2.30)		(2.36)	
Income per capita (at the time of deregulation, in 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

Note: For the actual deregulations 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), respectively. 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 deregulations spanned two decades of radical changes in the banking sector and their effects were heterogeneous across events; therefore, whether a significant growth acceleration had 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 VII and VIII.

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-" 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

Note: Each of the 23 events of bank branching deregulations is assessed separately to establish the statistical significance of its mean treatment effect. A different number of county-pairs is used in each deregulation event, determined by 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 average growth rate in the "pre-" and "post-" periods between the treatment counties and the control counties. Adjusted treatment effects control for change in income gap and growth opportunity gap between the "pre-" and "post-" periods, which if not adjusted for 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.

Table 8: 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-" 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

Note: Each of the 23 events of branching deregulation is assessed separately to establish the statistical significance of its mean treatment effect. A different number of county-pairs is used in each deregulation event, determined by 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 average growth rate in the "pre-" and "post-" periods between the treatment counties and the control counties (in this case, the second control group of "hinterland counties"). Adjusted treatment effects control for change in income gap and growth opportunity gap between "pre-" and "post-" periods, which if not adjusted for 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.

Appendix: Contiguous Counties across regulation change borders

Note: The table lists the names of treatment states/counties that deregulated bank branching earlier than their neighbors, and their paired control states/counties, which remained regulated for a longer period of time. The first control group includes border contiguous counties in the regulated states, whereas the second control group includes "hinterland counties" farther away from the state borders. The treatment effects estimated based on the difference-in-differences growth rates between the treatment counties and their paired contiguous counties are reported. The adjusted treatment effects correct the bias created by income gap and growth opportunity gap between the treatment and control groups, using the formula specified in Eq.(6) .

Treatment state	Treatment county	Control state	Contiguous county	Hinterland county	Raw treatment effect (%)	Adjusted treatment effect (%)
Maine	Oxford	New Hampshire	Coos		-0.3	-0.7
Maine	Oxford	New Hampshire	Carroll	Belknap	-3.7	-3.1
Maine	York	New Hampshire	Carroll	Belknap	-1.2	-1.2
Maine	York	New Hampshire	Strafford	Merimack	-2.1	-0.4
New York	Dutchess	Connecticut	Litchfield	Hartford	-1.0	-0.7
New York	Putnam	Connecticut	Fairfield	New Haven	0.7	0.9
New York	Westchester	Connecticut	Fairfield	New Haven	-0.7	-0.7
New York	Rensselaer	Massachusetts	Berkshire	Hampshire	0.1	0.1
New York	Columbia	Massachusetts	Berkshire	Hampshire	-1.3	-0.3
New York	Chautauqua	Pennsylvania	Erie	Crawford	1.5	0.7
New York	Chautauqua	Pennsylvania	Warren	Forest	1.9	1.0
New York	Cattaraugus	Pennsylvania	McKean	Elk	0.4	0.2
New York	Allegany	Pennsylvania	Potter	Clinton	0.0	-0.3
New York	Steuben	Pennsylvania	Tioga	Lycoming	1.2	0.0
New York	Chemung	Pennsylvania	Bradford	Sullivan	-0.5	-0.6
New York	Tioga	Pennsylvania	Bradford	Wyoming	1.0	0.5
New York	Broome	Pennsylvania	Susquehanna	Wyoming	-0.5	-0.3
New York	Delaware	Pennsylvania	Wayne	Lackawanna	1.9	-0.1
New York	Sullivan	Pennsylvania	Pike		1.1	-0.2
New Jersey	Sussex	Pennsylvania	Pike		1.2	0.4
New Jersey	Warren	Pennsylvania	Monroe	Lackawanna	0.3	1.1
New Jersey	Warren	Pennsylvania	Northampton	Lehigh	0.6	0.6
New Jersey	Hunterdon	Pennsylvania	Bucks	Montgomery	1.8	1.5
New Jersey	Mercer	Pennsylvania	Bucks	Montgomery	0.4	0.5
New Jersey	Burlington	Pennsylvania	Bucks	Lehigh	1.9	1.2
New Jersey	Camden	Pennsylvania	Philadelphia	Montgomery	1.1	0.8
New Jersey	Gloucester	Pennsylvania	Delaware		-2.1	-1.4
Virginia	Buchanan	Kentucky	Pike	Martin	-0.7	-0.3
Virginia	Dickinson	Kentucky	Pike	Floyd	-4.5	-2.4
Virginia	Wise	Kentucky	Letcher	Knott	-4.2	-0.6
Virginia	Lee	Kentucky	Harlan	Leslie	0.0	1.4
Virginia	Lee	Kentucky	Bell	Knox	-1.6	0.7
Virginia	Lee	Tennessee	Claiborne	Union	0.4	1.1
Virginia	Lee	Tennessee	Hancock	Grainger	-4.2	0.1
Virginia	Scott	Tennessee	Hawkins		0.5	0.5
Virginia	Scott	Tennessee	Sullivan	Washington	-0.5	0.2

Virginia	Washington	Tennessee	Sullivan		-0.7	0.4
Virginia	Washington	Tennessee	Johnson		2.0	1.3
Virginia	Loudoun	West Virginia	Jefferson		0.7	0.7
Virginia	Clarke	West Virginia	Jefferson		-3.6	-1.3
Virginia	Frederick	West Virginia	Berkeley		1.5	1.3
Virginia	Frederick	West Virginia	Morgan		3.6	1.3
Virginia	Frederick	West Virginia	Hampshire		2.7	0.8
Virginia	Shenandoah	West Virginia	Hardy		0.6	0.3
Virginia	Rockingham	West Virginia	Pendleton	Randolph	2.8	0.5
Virginia	Highland	West Virginia	Pocahontas	Randolph	0.1	0.7
Virginia	Bath	West Virginia	Pocahontas	Webster	-0.7	0.4
Virginia	Alleghany	West Virginia	Greenbrier	Nicholas	3.7	1.7
Virginia	Craig	West Virginia	Monroe		-0.3	0.3
Virginia	Giles	West Virginia	Monroe		0.6	0.5
Virginia	Giles	West Virginia	Mercer	Summers	3.6	1.6
Virginia	Bland	West Virginia	Mercer	Raleigh	2.3	1.9
Virginia	Tazewell	West Virginia	McDowell	Wyoming	2.1	2.3
Virginia	Buchanan	West Virginia	McDowell	Mingo	-1.9	1.6
Ohio	Williams	Indiana	Steuben	LaGrange	-0.7	-1.0
Ohio	Defiance	Indiana	De Kalb	Noble	0.7	0.3
Ohio	Paulding	Indiana	Allen	Whitley	-1.4	-0.9
Ohio	Van Wert	Indiana	Adams	Wells	-1.7	-0.8
Ohio	Mercer	Indiana	Jay	Blackford	-0.6	0.6
Ohio	Darke	Indiana	Randolph	Delaware	-0.7	0.0
Ohio	Preble	Indiana	Wayne	Henry	-1.5	-0.5
Ohio	Preble	Indiana	Union	Fayette	-0.9	1.1
Ohio	Butler	Indiana	Franklin	Decatur	0.4	0.2
Ohio	Hamilton	Indiana	Dearborn	Ripley	0.9	0.5
Ohio	Hamilton	Kentucky	Boone	Gallatin	1.5	0.6
Ohio	Hamilton	Kentucky	Kenton	Grant	0.7	0.4
Ohio	Hamilton	Kentucky	Campbell	Pendleton	1.2	0.7
Ohio	Clermont	Kentucky	Campbell	Pendleton	-0.2	0.0
Ohio	Clermont	Kentucky	Bracken	Robertson	-1.3	-0.7
Ohio	Brown	Kentucky	Mason	Fleming	-0.8	0.0
Ohio	Adams	Kentucky	Lewis	Rowan	2.4	1.2
Ohio	Scioto	Kentucky	Greenup	Carter	3.0	-1.1
Ohio	Lawrence	Kentucky	Boyd	Lawrence	1.4	-0.1
Ohio	Williams	Michigan	Hillsdale	Jackson	0.8	0.4
Ohio	Fulton	Michigan	Lenawee	Washtenaw	0.8	0.2
Ohio	Lucas	Michigan	Monroe	Wayne	-0.2	-0.1
Ohio	Ashtabula	Pennsylvania	Erie		-0.4	-0.6
Ohio	Ashtabula	Pennsylvania	Crawford		0.6	1.0
Ohio	Trumbull	Pennsylvania	Mercer	Venango	-1.3	-1.2
Ohio	Mahoning	Pennsylvania	Lawrence	Butler	-0.3	-0.1
Ohio	Columbiana	Pennsylvania	Beaver	Allegheny	0.4	-1.5
Ohio	Lawrence	West Virginia	Wayne	Mingo	0.5	-0.2
Ohio	Lawrence	West Virginia	Cabell	Lincoln	-0.3	-0.5
Ohio	Gallia	West Virginia	Mason	Putnam	0.5	1.2
Ohio	Meigs	West Virginia	Mason	Putnam	0.9	0.8
Ohio	Meigs	West Virginia	Jackson	Roane	2.1	0.7

Ohio	Athens	West Virginia	Wood	Wirt	0.7	-0.5
Ohio	Washington	West Virginia	Wood	Wirt	0.3	-0.1
Ohio	Washington	West Virginia	Pleasants	Ritchie	2.3	0.3
Ohio	Monroe	West Virginia	Tyler	Doddridge	-0.3	0.4
Ohio	Monroe	West Virginia	Wetzel	Harrison	-2.2	-0.5
Ohio	Belmont	West Virginia	Marshall		1.0	0.3
Ohio	Belmont	West Virginia	Ohio		-1.8	-0.9
Ohio	Jefferson	West Virginia	Brooke		0.0	0.1
Ohio	Jefferson	West Virginia	Hancock		1.6	0.6
Connecticut	Litchfield	Massachusetts	Berkshire		-1.9	-0.7
Connecticut	Hartford	Massachusetts	Hampden	Hampshire	-0.1	0.0
Connecticut	Tolland	Massachusetts	Hampden	Hampshire	-1.0	-0.5
Connecticut	Windham	Massachusetts	Worcester		1.1	0.2
Alabama	Baldwin	Florida	Escambia		-1.3	0.4
Alabama	Escambia	Florida	Escambia		-1.2	0.0
Alabama	Escambia	Florida	Santa Rosa		-1.7	-0.5
Alabama	Covington	Florida	Okaloosa		-2.1	-1.4
Alabama	Covington	Florida	Walton		-0.7	-0.4
Alabama	Geneva	Florida	Holmes	Washington	-1.3	-1.1
Alabama	Houston	Florida	Jackson	Calhoun	-1.4	-0.4
Alabama	Lauderdate	Mississippi	Tishomingo	Alcorn	5.4	4.0
Alabama	Colbert	Mississippi	Tishomingo	Prentiss	4.2	3.3
Alabama	Franklin	Mississippi	Itawamba	Lee	-7.2	-3.1
Alabama	Marion	Mississippi	Itawamba	Lee	-1.7	-0.8
Alabama	Lamar	Mississippi	Monroe	Chicksaw	0.4	2.3
Alabama	Pickens	Mississippi	Lowndes	Oktibbeha	2.4	0.9
Alabama	Pickens	Mississippi	Noxubee	Winston	2.1	1.2
Alabama	Sumter	Mississippi	Kemper	Neshoba	-0.3	-2.0
Alabama	Sumter	Mississippi	Lauderdale	Newton	-0.8	-0.3
Alabama	Choctaw	Mississippi	Clarke	Jasper	4.9	4.5
Alabama	Washington	Mississippi	Wayne	Jones	2.4	1.8
Alabama	Washington	Mississippi	Greene	Perry	1.8	3.4
Alabama	Mobile	Mississippi	George	Stone	0.0	1.0
Alabama	Mobile	Mississippi	Jackson	Harrison	-0.8	-0.3
Alabama	Lauderdale	Tennessee	Wayne	Perry	-0.1	-0.7
Alabama	Lauderdale	Tennessee	Lawrence	Lewis	-0.2	-1.4
Alabama	Limstone	Tennessee	Giles	Maury	2.2	1.9
Alabama	Madison	Tennessee	Lincoln	Marshall	2.8	1.6
Alabama	Jackson	Tennessee	Franklin	Coffee	-2.4	-2.0
Alabama	Jackson	Tennessee	Marion	Grundy	0.2	-1.5
Pennsylvania	Beaver	West Virginia	Hancock		-3.2	-1.0
Pennsylvania	Washington	West Virginia	Brooke		-0.9	0.7
Pennsylvania	Washington	West Virginia	Ohio		-1.2	-0.3
Pennsylvania	Greene	West Virginia	Marshall		-0.7	0.0
Pennsylvania	Greene	West Virginia	Monongalia	Marion	-3.2	-1.8
Pennsylvania	Fayette	West Virginia	Preston	Barbour	-2.3	-1.7
Georgia	Seminole	Florida	Jackson	Washington	-1.8	-1.6
Georgia	Decatur	Florida	Gadsden	Liberty	-0.4	-0.1
Georgia	Grady	Florida	Leon	Wakulla	0.6	-0.3
Georgia	Thomas	Florida	Jeferson	Taylor	0.0	0.1

Georgia	Brooks	Florida	Madison	Taylor	4.7	1.9
Georgia	Lowndes	Florida	Madison	Lafayette	-1.0	0.7
Georgia	Echols	Florida	Hamilton	Suwannee	0.3	-1.5
Georgia	Clinch	Florida	Columbia	Gilchrist	-2.1	-0.9
Georgia	Ware	Florida	Baker	Union	-3.5	-2.9
Georgia	Charlton	Florida	Baker	Bradford	-5.1	-2.5
Georgia	Charlton	Florida	Nassau	Duval	-3.4	-2.6
Georgia	Camden	Florida	Nassau	Duval	-1.6	-0.5
Massachusetts	Worcester	New Hampshire	Cheshire	Sullivan	-0.3	0.8
Massachusetts	Middlesex	New Hampshire	Hillsborough	Merrimack	1.6	0.4
Massachusetts	Essex	New Hampshire	Rockingham	Strafford	0.3	-0.3
Nebraska	Dakota	Iowa	Woodbury	Ida	0.7	0.0
Nebraska	Thurston	Iowa	Monona	Crawford	1.2	0.6
Nebraska	Burt	Iowa	Monona	Crawford	-1.7	-1.0
Nebraska	Burt	Iowa	Harrison	Shelby	0.0	-1.0
Nebraska	Washington	Iowa	Harrison	Shelby	-1.2	-0.1
Nebraska	Douglas	Iowa	Pottawatamie	Cass	0.3	0.9
Nebraska	Sarpy	Iowa	Mills	Montgomery	-1.1	0.5
Nebraska	Cass	Iowa	Mills	Montgomery	-1.1	-0.2
Nebraska	Otoe	Iowa	Fremont	Page	1.6	0.6
Nebraska	Nernaha	Missouri	Atchison	Nodaway	1.8	1.4
Nebraska	Richardson	Missouri	Holt	Nodaway	3.6	0.8
Tennessee	Lauderdate	Arkansas	Mississippi	Craighead	-2.3	-0.2
Tennessee	Tipton	Arkansas	Mississippi	Poinsett	-0.7	0.7
Tennessee	Shelby	Arkansas	Crittenden	Cross	0.4	0.6
Tennessee	Lake	Kentucky	Fulton	Hickman	7.5	3.7
Tennessee	Obion	Kentucky	Fulton	Hickman	3.0	3.5
Tennessee	Weakley	Kentucky	Graves	McCracken	1.6	1.3
Tennessee	Henry	Kentucky	Calloway	Marshall	0.6	-0.4
Tennessee	Stwewart	Kentucky	Trigg	Lyon	1.6	1.4
Tennessee	Montgomery	Kentucky	Christian	Hopkins	1.8	1.2
Tennessee	Montgomery	Kentucky	Todd	Muhlenberg	-1.1	-1.7
Tennessee	Robertson	Kentucky	Logan	Butler	0.8	0.9
Tennessee	Robertson	Kentucky	Simpson	Warren	0.0	0.7
Tennessee	Summer	Kentucky	Simpson	Warren	-0.6	0.6
Tennessee	Summer	Kentucky	Allen	Warren	2.6	3.2
Tennessee	Macon	Kentucky	Allen	Barren	6.7	4.0
Tennessee	Macon	Kentucky	Monroe	Barren	2.9	-0.4
Tennessee	Clay	Kentucky	Monroe	Metcalfe	3.4	1.9
Tennessee	Clay	Kentucky	Cumberland	Adair	2.3	3.0
Tennessee	Pickett	Kentucky	Clinton	Russell	1.4	2.3
Tennessee	Pickett	Kentucky	Wayne	Pulaski	0.3	1.5
Tennessee	Scott	Kentucky	McCreary	Pulaski	-1.4	0.6
Tennessee	Campbell	Kentucky	Whitley	Laurel	0.7	0.8
Tennessee	Claibome	Kentucky	Bell	Clay	0.7	0.4
Tennessee	Lake	Missouri	New Madrid	Stoddard	5.4	1.4
Tennessee	Dyer	Missouri	Pemiscot	Dunklin	0.4	1.6
Mississippi	DeSoto	Arkansas	Crittenden	Saint Francis	-2.6	0.3
Mississippi	Tunica	Arkansas	Lee	Monroe	4.5	1.8
Mississippi	Coahoma	Arkansas	Philips	Arkansas	1.2	1.6

Mississippi	Bolivar	Arkansas	Desha	Lincoln	-1.7	0.4
Mississippi	Washington	Arkansas	Chicot	Ashley	-1.6	-1.1
Kansas	Doniphan	Missouri	Holt	Nordaway	-2.1	-0.3
Kansas	Doniphan	Missouri	Andrew	Gentry	-2.9	-0.7
Kansas	Doniphan	Missouri	Buchanan	DeKalb	-1.5	-0.3
Kansas	Atchison	Missouri	Buchanan	Clinton	0.8	-0.5
Kansas	Leavenworth	Missouri	Platte	Clinton	4.7	2.2
Kansas	Johnson	Missouri	Jackson	Lafayette	0.1	1.1
Kansas	Miami	Missouri	Cass	Johnson	-1.4	-1.3
Kansas	Linn	Missouri	Bates	Henry	0.1	-0.4
Kansas	Bourbon	Missouri	Vemon	Cedar	-3.3	-4.4
Kansas	Crawford	Missouri	Barton	Dade	4.6	3.5
Kansas	Cherokee	Missouri	Jasper	Lawrence	1.9	0.1
Michigan	Gogebic	Wisconsin	Iron	Ashland	2.5	1.2
Michigan	Gogebic	Wisconsin	Vilas	Oneida	3.0	2.0
Michigan	Iron	Wisconsin	Forest	Langlade	0.9	0.5
Michigan	Dickinson	Wisconsin	Marinette	Oconto	4.7	3.5
Michigan	Menominee	Wisconsin	Marinette	Oconto	2.4	2.5
North Dakota	Pembina	Minnesota	Kittson	Roseau	0.5	2.2
North Dakota	Walsh	Minnesota	Marshall	Beltrarni	-3.5	-1.4
North Dakota	Grand Forks	Minnesota	Polk	Clearwater	2.6	1.9
North Dakota	Traill	Minnesota	Norman	Mahnomen	-0.1	-1.1
North Dakota	Cass	Minnesota	Clay	Becker	1.2	1.4
North Dakota	Richland	Minnesota	Wilkin	Otter Tail	1.4	0.7
West Virginia	Wayne	Kentucky	Boyd	Carter	0.3	0.6
West Virginia	Wayne	Kentucky	Lawrence	Elliott	1.9	1.0
West Virginia	Mingo	Kentucky	Martin	Johnson	1.0	1.1
West Virginia	MIngo	Kentucky	Pike	Floyd	0.3	0.4
Illinois	Jo Daviess	Iowa	Dubuque	Delaware	-0.8	0.6
Illinois	Jo Daviess	Iowa	Jackson	Jones	-1.3	0.8
Illinois	Carroll	Iowa	Jackson	Jones	-1.7	0.9
Illinois	Whiteside	Iowa	Clinton	Cedar	0.1	-0.7
Illinois	Rock Island	Iowa	Scott	Cedar	-0.4	-0.3
Illinois	Rock Island	Iowa	Muscatine	Johnson	-0.3	-0.4
Illinois	Mercer	Iowa	Louisa	Washington	1.3	0.4
Illinois	Henderson	Iowa	Des Moines	Henry	2.4	1.5
Illinois	Hancock	Iowa	Lee	Henry	1.4	1.5
Louisiana	Caddo	Arkansas	Miller	Hempstead	0.3	0.9
Louisiana	Bossier	Arkansas	Lafayette	Hempstead	-1.6	1.9
Louisiana	Webster	Arkansas	Columbia	Neveda	0.3	-0.2
Louisiana	Claiborne	Arkansas	Columbia	Ouachita	1.0	0.8
Louisiana	Union	Arkansas	Union	Calhoun	2.7	1.8
Louisiana	Morehouse	Arkansas	Ashley	Drew	-0.7	0.9
Louisiana	West Carroll	Arkansas	Chicot	Drew	1.3	1.9
Louisiana	East Carroll	Arkansas	Chicot	Drew	2.3	1.2
Oklahoma	Delaware	Arkansas	Benton	Madison	2.0	1.8
Oklahoma	Adair	Arkansas	Washington	Madison	2.1	2.1
Oklahoma	Sequoyah	Arkansas	Crawford	Franklin	-0.3	0.3
Oklahoma	Le Flore	Arkansas	Sebastian	Logan	2.5	2.0
Oklahoma	Le Flore	Arkansas	Scott	Yell	0.9	0.3

Oklahoma	McCurtain	Arkansas	Polk	Montgomery	2.9	2.4
Oklahoma	McCurtain	Arkansas	Sevier	Howard	0.7	2.5
Oklahoma	McCurtain	Arkansas	Littler River	Hempstead	1.3	1.8
Texas	Bowie	Arkansas	Littler River	Howard	-0.6	-1.6
Texas	Cass	Arkansas	Miller	Hempstead	-1.2	-0.5
Missouri	McDonald	Arkansas	Benton	Madison	3.0	1.1
Missouri	Barry	Arkansas	Carroll	Madison	2.0	-0.4
Missouri	Stone	Arkansas	Carroll	Newton	1.2	0.6
Missouri	Taney	Arkansas	Boone	Newton	4.4	3.9
Missouri	Ozark	Arkansas	Marion	Searcy	0.7	-0.7
Missouri	Ozark	Arkansas	Baxter	Stone	0.9	-0.2
Missouri	Howell	Arkansas	Fulton	Izard	-0.1	0.1
Missouri	Oregon	Arkansas	Sharp	Independence	0.8	0.1
Missouri	Oregon	Arkansas	Randolph	Lawrence	0.0	-0.7
Missouri	Ripley	Arkansas	Randolph	Lawrence	1.3	1.3
Missouri	Ripley	Arkansas	Clay		0.6	0.3
Missouri	Butler	Arkansas	Clay		1.1	0.6
Missouri	Dunklin	Arkansas	Clay		1.1	1.0
Missouri	Dunklin	Arkansas	Greene	Lawrence	0.7	1.4
Missouri	Dunklin	Arkansas	Mississippi	Poinsett	2.8	2.1
Missouri	Pemiscot	Arkansas	Mississippi	Poinsett	2.5	0.9
Missouri	Atchison	Iowa	Fremont		-3.2	3.4
Missouri	Nodaway	Iowa	Page	Montgomery	-0.9	-0.4
Missouri	Worth	Iowa	Taylor	Adams	-4.8	0.0
Missouri	Worth	Iowa	Ringgold	Union	-4.1	0.2
Missouri	Harrison	Iowa	Ringgold	Union	-1.2	0.0
Missouri	Harrison	Iowa	Decatur	Clarke	1.4	0.9
Missouri	Mercer	Iowa	Wayne	Lucas	9.5	10.1
Missouri	Putnam	Iowa	Wayne	Lucas	-1.1	0.2
Missouri	Putnam	Iowa	Appanoose	Monroe	-2.2	-0.3
Missouri	Schuyler	Iowa	Davis	Wapello	1.1	-0.2
Missouri	Scotland	Iowa	Van Buren	Jefferson	1.8	1.9
Missouri	Clark	Iowa	Lee	Henry	3.6	3.0
Wisconsin	Vernon	Iowa	Allamakee	Winneshiek	2.4	-0.8
Wisconsin	Crawford	Iowa	Allamakee	Winneshiek	2.7	0.7
Wisconsin	Grant	Iowa	Clayton	Fayette	4.2	0.2
Wisconsin	Grant	Iowa	Dubuque	Delaware	0.2	-1.0
Wisconsin	Douglas	Minnesota	Carlton	Aitkin	0.1	-0.8
Wisconsin	Burnett	Minnesota	Pine	Kanabec	0.3	-1.2
Wisconsin	Polk	Minnesota	Chisago	Isanti	0.6	0.1
Wisconsin	St. Croix	Minnesota	Washington	Ramsey	0.4	-0.5
Wisconsin	Pierce	Minnesota	Goodhue	Rice	1.1	-0.5
Wisconsin	Pepine	Minnesota	Wabasha	Olmsted	2.3	0.9
Wisconsin	Buffalo	Minnesota	Wabasha	Olmsted	2.4	1.2
Wisconsin	Buffalo	Minnesota	Winona	Olmsted	2.0	1.0
Wisconsin	Trempealeau	Minnesota	Winona	Olmsted	0.6	-0.2
Wisconsin	La Crosse	Minnesota	Winnoa	Olmsted	0.5	-0.2
Wisconsin	La Crosse	Minnesota	Houston	Filmore	1.5	0.2
Wisconsin	Vernon	Minnesota	Houston	Filmore	1.5	-1.3