# Banking, Geographic Restrictions and Consumer Bankruptcy: A Closer Examination[1]

Chintal A. Desai*
Department of Finance, Insurance and Real Estate
Virginia Commonwealth University

David H. Downs
The Kornblau Institute
Virginia Commonwealth University

December 21, 2015

*JEL* Codes: G28; G21; D12.
Keywords: Intrastate bank branching; Consumer (Personal) Bankruptcy; Difference-in-difference.

---

[1] The authors are thankful for comments from Sumit Agarwal, Etti Baranoff, Douglas Davis, Pedro Gete, Manu Gupta, Oleg Korenok, Ed Milner, Mayoor Mohan, Gabriel Ramirez (Discussant), Herman Saheruddin, Dan Salandro, Mira Straska, Ko Wang, and seminar participants at the Virginia Commonwealth University, American Real Estate and Urban Economics Association's international meetings 2015, and Financial Management Association meetings 2015. The authors are grateful to Carl Gourdes and Kevin Scott of the Administrative Office of the U.S. Courts and Susan Campolongo of Bureau of Labor Statistics for kindly providing county-level bankruptcy filings and unemployment data, respectively. Bhargav Vattam provided research assistance. Desai acknowledges the financial support of the VCU Kornblau Institute and the VCU School of Business summer research grant program. The usual disclaimers apply.

* Corresponding Author: Snead Hall, 301 W. Main Street, Richmond, VA-23284; Tel.: +1 804 828 7194; fax: +1 804 828 3972; Email: cdesai2@vcu.edu.

# Banking, Geographic Restrictions and Consumer Bankruptcy: A Closer Examination

## Abstract

This paper analyzes the effect on consumer bankruptcy filings as a result of removing geographic restrictions on intrastate banking. The empirical strategy is to examine this issue more closely by considering three distinct methodologies applied to U.S. county-level data. The analysis based on (i) panel data for all counties, and (ii) panel data of contiguous counties along state borders reveals an insignificant effect. When we use a quasi-natural experiment based on 186 (treatment) events and 4,870 non-event (i.e., placebo) pairs of contiguous counties, we find that bankruptcy filings increase (decrease) in only five (one) of seventeen states with treatment counties.

## 1. Introduction

The consumer bankruptcy filing rate in the United States has increased over the last three decades. The number of bankruptcy filings per population of 1,000 was 0.892 in 1980. That number went up to 2.168 in 1994 and 3.885 in 2010 (Figure 1). Using state-level data on Chapter 7 bankruptcy filings, Dick and Lehnert (2010) attribute this increase, mainly, to the removal of interstate banking restriction and, to some extent, to the removal of intrastate bank branching restriction.[1] On one hand, the geographic expansion of banking activities helps increase local economic growth, output, and new business formation by improving lending quality (Jayaratne & Strahan 1996; Strahan 2003). On the other hand, it can increase credit availability with a possibility of lowering credit standards. These factors can influence bankruptcy filings. In this paper, we analyze the effect of removal of geographic restrictions on intrastate bank branching by a state on its bankruptcy filings. Our contribution relies on the fact that we more closely examine the causal link between geographic restrictions on banking and consumer bankruptcy by (1) using less-aggregated data (i.e., county-level data), (2) analyzing Total bankruptcy filings as well as Chapter 7 and Chapter 13 filings, and (3) using three distinct methodologies found in the literature.

The first approach is the traditional difference-in-difference (*DiD*) method.[2] We use cross-sectional time-series data for all U.S. counties for the period 1980 to 1994. The results show that the effect of removing of intrastate bank branching restrictions on bankruptcy filings is statistically insignificant.

[1] The other explanations for the rise in bankruptcy are lower transaction costs of lending due to technological advances in consumer credit industry (Livshits, MacGee & Tertilt 2010), lower social opprobrium toward loan default and filing for bankruptcy to relieve debt obligations (Fay, Hurst & White 2002; Gross & Souleles 2002), increase in healthcare costs (Himmelstein *et al.* 2005), disruption of family structure due to increase in number of divorces and percentage of population remaining singles (Domowitz & Sartain 1999; Sullivan, Warren & Westbrook 2000), among others.
[2] Dick and Lehnert (2010) use this approach.

In the second approach, we perform a quasi-natural experiment following the methodology applied by Huang (2008). The focus is on the bankruptcy filings in border counties of seventeen 'treatment' states that removed intrastate bank branching restrictions during 1981 to 1994. The first task is to form a sample of 186 contiguous county pairs such that in a pair, one county is of a treatment state and the other county is of the neighboring (control) state. The main difference between a treatment and its control state is that in the period following the removal of branch restrictions in the treatment state, the control state still imposes such restrictions. The next task is to construct the placebo sample of counterfactuals. Based on four different possibilities and by avoiding the overlap with the actual event window, we identify 4,870 combinations of fictitious event year and contiguous county-pairs for the placebo sample.[3] The final task is to use the placebo sample (1) to adjust the 'raw' treatment effect and (2) to assess the statistical significance of the adjusted treatment effect on a state-by-state basis. Our results show that removing restrictions on bank branching within a state resulted in Total bankruptcy increasing in five (Alabama, Georgia, Tennessee, Mississippi, and Louisiana) and decreasing in one (Wisconsin) of seventeen treatment states. In the case of Chapter 13 filings, the increase effect is found in four (Georgia, Tennessee, Louisiana, and Illinois) and the decrease effect is in three (Alabama, Oklahoma, and Missouri) of seventeen treatment states.

The third approach is based on Dube, Lester and Reich (2010). As per the authors, "the estimation based on traditional fixed-effects specifications exhibit a strong downward bias due to unobserved heterogeneity in the outcome variable. In addition, this unobserved heterogeneity is spatial in nature. Therefore, the precision of individual case-study approach [our second method]

[3] Although our second approach, for the most part, is similar to that of Huang (2008), our contribution to this methodology, is in carefully identifying the sample of counterfactuals. In section 4, we explain our quasi-natural experiment in detail.

is overstated. By pooling all such local comparisons, we can address the dual problem of omitted variable bias and bias in the estimated standard errors." In this approach, we use a pooled sample of 571 contiguous county-pairs involving border counties of seventeen treatment states and their matching counties from neighboring states for the entire period of 1980 to 1994. A border county can be part of multiple county-pairs along a border segment, which can induce error correlation across county-pairs and potentially across the border segment.[4] Therefore, in this analysis, the robust standard errors are clustered on two dimensions -- at the state level and at the border segment level (Cameron, Gelbach & Miller 2011). The results are similar to those obtained using the traditional *DiD* with panel data. The effect of deregulation by permitting intrastate bank branching on bankruptcy filings is statistically insignificant.

Finally, we apply the empirical specification of Dick and Lehnert (2010) on state-level and county-level bankruptcy data. Using the state-level data, the effect of removal of interstate banking restriction on consumer bankruptcy filing is positive. However, this effect is insignificant, when we use county-level data.

The remainder of the paper is organized as follows. The next section describes data, followed by three sections on empirical methodologies and their results. The penultimate section reports findings based on Dick and Lehnert's (2010) specification on county-level data. The concluding remarks are in the final section.

## 2. Data - Bankruptcy filings and Intrastate bank branching

[4] The border segment is defined as the border of a state with one particular neighboring state. For example, Florida shares a border with Alabama and Georgia, resulting in two border segments -- Florida and Georgia, and Florida and Alabama.

The yearly county-level data on personal (non-business/consumer) bankruptcy filings are from the Report F-5A of the Administrative Office of the U.S. Courts. The Total bankruptcy rate, Chapter 7 bankruptcy rate, and Chapter 13 bankruptcy rate are the dependent variables of this study. The variable *TotBKR* is defined as the number of bankruptcy filings (under both Chapter 7 and Chapter 13 procedures) in a given year per 1,000 persons. The variables *Ch7BKR* and *Ch13BKR* are defined as the number of bankruptcy filings under the Chapter 7 and Chapter 13 procedures in a given year per 1,000 population, respectively.[5] The yearly county-level population data are from the U.S. Census.

Figure 2 shows the geographic distribution of consumer bankruptcy rates (number of bankruptcy filings per 1,000 persons) at the county-level. Most of the west coast counties and parts of the mid-west region (Ohio, Indiana, and Illinois) fall into the quintile of highest bankruptcy rates in 1980. Similarly, most counties in the northeast, South Carolina, Florida, and Texas are in the quintile of lowest bankruptcy rates in 1980. By 1994, most U.S. counties are in the highest category of bankruptcy rate as shown in Figure 3, which uses the same 1980 quintile range.

In the United States, state-level removal of intrastate bank branching restrictions was completed between 1975 and 1994.[6] We define the dummy variable *IntraState* which takes a value of one in a given year if that county belongs to a state that allows intrastate bank branching in that year, and zero otherwise. The sample period of our study is 1980 to 1994. During this period, there were no major bankruptcy reforms at the federal level and no significant changes in state-level bankruptcy exemptions. As shown in Figure 4, the average bankruptcy filing rate for all states

[5] An individual can file for bankruptcy protection under Chapter 7 or Chapter 13 procedure. In the case of bankruptcy under Chapter 7, the borrower's personal assets are liquidated to repay the unsecured creditor. In the case of Chapter 13 filing, the borrower retains all personal assets and agrees to repay the unsecured debt from future income over the next 3 to 5 years. In both procedures, the maximum amount that the lender receives is equal to the total unpaid debt minus the permissible bankruptcy exemptions (e.g., homestead exemptions). On average, 70% of consumer bankruptcy filings are under Chapter 7 procedure.

[6] See Tables 1 of Dick and Lehnert (2010), Huang (2008), and Jayaratne and Strahan (1997).

allowing intrastate bank branching in 1980 was one person per 1,000 population. That statistic was 0.86 for a state restricting intrastate bank branching. The average bankruptcy filing rate in states allowing intrastate bank branching was higher than for states restricting intrastate branching for every year from 1980 to 1993. This evidence suggests that removing bank branching restrictions within a state may increase its bankruptcy filing rate.

**3. Analysis based on the panel data of all U.S. counties**

The first empirical methodology performs the Ordinary Least Squares (OLS) analysis using the following specification:

$$Y_{cst} = \alpha + cX_{cst} + \beta I_{st} + A_c + B_t + \varepsilon_{cst}\,, \qquad (1)$$

where $Y_{cst}$ is the outcome of interest -- *TotBKR*, *Ch7BKR* or *Ch13BKR* -- for a given county $c$ of state $s$ in year $t$. The dummy variables $A_c$ and $B_t$ control for the county- and year-fixed effects, respectively. The variables $X_{cst}$ are the county specific controls and the error term is $\varepsilon_{cst}$. The explanatory variable of interest is $I_{st}$, our dummy variable *IntraState.* The estimated impact of removing of intrastate bank branching restriction is the OLS estimate $\hat{\beta}$. For this analysis, we use a dataset of 3,106 counties across the 50 states and District of Columbia from 1980 to 1994. The policy variable $I_{st}$ and outcome variable $Y_{cst}$ are serially correlated within a county. The standard error of the estimate $\hat{\beta}$ of an OLS estimation is biased downwards, which can mislead by generating a narrow confidence interval, large *t*-statistics, and low *p*-values (Bertrand, Duflo & Mullainathan 2004; Cameron & Miller 2015). We use robust standard errors clustered at the state-level to address this issue.

The county-level controls ($X_{cst}$) take into account other factors influencing bankruptcy rate such as local economic conditions and demographics (Desai, Elliehausen & Lawrence 2014).

Specifically, we use county-level data on the unemployment rate, per capita real income, growth in per capita real income, and percentage of the population in the 25 to 44 age group. The unemployment rate of a county is the ratio of people seeking jobs to the total number of people in the labor force. Using the consumer price index, the nominal per capita income (in $1,000s) is deflated at the 1980 level to control for inflation. A county's growth rate in real income serves as a proxy for its economic growth (Huang, 2008). The income growth is computed as the percentage growth in real per capita income over the previous year. The income data are from the Bureau of Economic Analysis (BEA). The consumer price index and unemployment data are from the Bureau of Labor Statistics (BLS). The percentage of young population is defined as the ratio of population in the age group of 25 to 44 years to the total population. The young population tends to have higher level of debt, which in turn, increases their probability to file for bankruptcy. Appendix A, Panel 1 reports summary statistics for these variables.

Table 1 reports the results of this approach. As a county's unemployment rate increases, its bankruptcy filing rate under Chapter 7 also increases. This result is intuitive. An individual who experiences an income shock due to a job-loss is uncertain about future income. The bankruptcy filing under Chapter 7 protects the borrower's future income and provides a "fresh-start," unlike filing under Chapter 13 which requires a debt repayment within three to five years post-bankruptcy. In addition, an increase in a county's share in the 25 to 44 demographic increases its level of Chapter 13 filings. This result suggests that a growing household is more concerned about their personal assets including their homes. By filing bankruptcy under Chapter 13, they can retain their personal assets and neighborhood ties for their children (White 2006; White & Zhu 2010).

Most importantly, the effect of *IntraState* is statistically insignificant in all specifications. Its direction is positive for Chapter 7 filing rate but negative for Chapter 13 filing rate. The

coefficient of *IntraState* on *Ch7BKR* as per the specification (6) of Table 1 is 0.021. Using the sample median of 0.985 Chapter 7 filings per 1,000 persons, we can trace only 2.1% increase in Chapter 7 bankruptcy rate directly to the removal of intrastate bank branching restriction. This suggests that the effect has economic insignificance on the Chapter 7 filings.[7]

## 4. Analysis based on a quasi-natural experiment

Broadly speaking, in this approach, there are four main steps. The first step is to identify treatment sample of contiguous county-pairs. The second step is to obtain placebo sample of contiguous county pairs. In the third step, we use the placebo sample to obtain coefficients of factors that influence the bankruptcy filings and to obtain the statistical table of critical values. In the final step, we adjust the point estimate obtained from the treatment sample and evaluate the adjusted treatment effect against the critical values.

### *4.1. Construction of treatment sample and initial findings*

Between 1981 and 1990, seventeen U.S. states removed geographic restrictions on intrastate bank branching. The event year is the year in which the state removes the restrictions on intrastate bank branching. The names and the event year for these states are: Alabama (1981), Pennsylvania (1982), Georgia (1983), Massachusetts (1984), Nebraska (1985), Tennessee (1985), Mississippi (1986), Kansas (1987), Michigan (1987), North Dakota (1987), West Virginia (1987), Illinois (1988), Louisiana (1988), Oklahoma (1988), Texas (1988), Missouri (1990), and Wisconsin (1990).

[7] Following Dube et al. (2010), we also use dummy interactions of the census division and year, and state-specific trend variables. The results remain similar.

Following Huang (2008), we examine the eastern U.S. states, which are located east of Montana, Wyoming, Colorado, and New Mexico. The main reason for this criterion is that the counties in western states are, generally, very large-size (geographic not in population) and few in number. The empirical methodology based on the contiguous-county pairs is more appropriate for small size counties as found in the eastern U.S. We first identify event borders of the treatment states where the entry barriers for bank branching were removed at a time earlier than their neighboring control states, during 1981 to 1990. We ensure that the event year in the treatment state is at least two years prior to deregulation event in the control state. Next, the contiguous county-pairs are identified such that both counties of a pair are border counties of their respective state and the two counties share a border such that one is in the treatment state and the other is a control state. The "treatment border county" is in the treatment state that removed the restrictions on bank branching in the event year. The "contiguous control county" is in the neighboring control state that has still not permitted intrastate bank branching in the treatment event year. The main reason for identifying contiguous pairs based on geographic proximity is that the economic, demographic, cultural and weather conditions are most likely similar in treatment and control (comparison) counties. If we observe any change in the bankruptcy filing rates during the event period, then it will be attributed to the policy changes on one side of the border such as banking sector reforms.

Table 2 reports the event-borders, number of contiguous county-pairs, and intrastate bank branching deregulation years in treatment and control states. The border between Georgia and Florida, for example, is one of the event-borders. Georgia is a treatment state which removed barriers on intrastate bank branching in 1983. Five years after that, Florida also removed restrictions thereby making it a control state. The event year is 1983 when the treatment state

undertook the banking sector reforms. As shown in Appendix B, Figure B1, the county of Grady, Georgia and Leon county of Florida are one of the contiguous county-pairs. Grady County is the treatment county and Leon County is the contiguous, control county. As shown in Table 2, among the seventeen states, Alabama, Tennessee, and Missouri have the highest number (27, 25, and 28, respectively) of contiguous county-pairs from the total of 186 county-pairs. The lowest numbers of county-pairs occur for Texas, Massachusetts, and West Virginia (2, 3, and 4, respectively). Appendix B, Figure B2 shows the contiguous-county pairs on the U.S. map.[8] The treatment counties are in green and comparison (control) counties are in red.

We define the pre-event period as the three years prior to deregulation; however due to data limitation this period may be shorter, though we require at least one year for the pre-event period. For example, in the case of Alabama, the prior period is only one year, 1980, since the county-level bankruptcy data are available only after 1980. For Pennsylvania, it is two years, 1980 and 1981. The post-event period window is defined as three years after the event (treatment) year. In some cases, it is defined as two years after the event (treatment) year due to the sample period or a subsequent removal of intrastate bank branching restriction in the comparison (i.e., control) state. For example, the deregulation (treatment) year for Massachusetts is 1984 and New Hampshire is 1987. The post-event period for the three contiguous county pairs of this event-border is two years, 1985 and 1986. This is also the case for the event-borders of Kansas-Missouri, Michigan-Wisconsin, West Virginia-Kentucky, and Wisconsin-Minnesota. By requiring a minimum of two year for the post-period, we ensure a reasonable amount of time is given for lender-borrower behavior to adjust to the deregulation environment and for assessing the effects

[8] We thank Rocco Huang for providing the list of county-pairs of his treatment sample on his website.

of deregulation on the number of bankruptcy filings. In addition, it helps avoid combining the deregulation event year of the control state in the post-event period.

Based on the pre-event period and post-event period the average bankruptcy filing rates are calculated for all treatment and control counties. Then the difference-in-difference approach is used to assess the 'raw' treatment effect. It is given by the following:

$$RTE = \left(Y_{tc,post} - Y_{tc,pre}\right) - \left(Y_{cc,post} - Y_{cc,pre}\right), \quad (2)$$

where, $RTE$ is the raw treatment effect and $Y$ is the dependent variable of the study (i.e., Total (*TotBKR*), Chapter 7 (*Ch7BKR*) or Chapter 13 (*Ch13BKR*) bankruptcy filing rate). The variable subscripts $tc$ and $cc$ are for treatment county and control county, respectively, and subscripts $pre$ and $post$ are for the pre-event period and post-event period.

Table 3 reports the results using a difference-in-difference approach on the total sample of 186 contiguous county-pairs. The average total bankruptcy rate during the pre-event period is 1.110 and 1.004 for treatment and control counties, respectively. After the geographic restrictions on intrastate bank branching are removed in the treatment states, the average total bankruptcy rate rises to 1.754 suggesting a bankruptcy acceleration (time-difference) of 0.644 in the post-event period for treatment counties. This bankruptcy acceleration for the control counties, where intrastate bank branching restriction is still in-place, is 0.431. Therefore, the bankruptcy acceleration gap, which is also the difference-in-difference of bankruptcy rates or the raw treatment effects, is 0.213 (not reported in table). In other words, the 'net' increase in bankruptcy rate is 0.213 in the treatment counties after the removal of restrictions on intrastate bank branching. The median value of the *change* in Total bankruptcy rate in the post-event period for the sample

of treatment counties is 0.430 and 0.377 for the control counties. Results are similar for the Chapter 7 and Chapter 13 bankruptcy filing rates.[9]

The difference-in-difference results, aggregated at the national-level, shows that the bankruptcy filings increased after states removed intrastate bank branching restrictions. The major issues to be addressed are that the standard errors are underestimated. In addition, the magnitude of the point estimates, for example 0.213 in the case of total bankruptcy rate, also needs to be adjusted using factors likely to influence the bankruptcy rate. Therefore, we undertake randomization or permutation testing which is popular in clinical trial studies to establish meaningful conclusions regarding the *real* effects of a treatment.

*4.2. Construction of placebo sample based on counterfactuals*

The observed raw treatment effect, which is the effect of removing intrastate bank branching restrictions on personal bankruptcy filings, may be due to chance. The event of removal of restrictions on intrastate bank branching is not randomly selected. Several researchers have used common datasets on personal bankruptcy filing for period 1980 to 1994. In addition, as we saw in Figure 1, there is an upward trend in bankruptcy filings between 1980 and 1994. Therefore, our observed results may be due to a host of data snooping and data mining biases. One approach to overcome this limitation, as suggested in Huang (2008), is to conduct a similar experiment on a set of fictitious or placebo contiguous-county-pairs. This set of counterfactual treatments allows us to (1) identify point estimates to adjust the raw treatment effect, and (2) construct a distribution

[9] In an untabulated results, using the standard paired-*t* test, we find that the treatment effect is statistically significant for Total (at the 1% level), Chapter 7 bankruptcy rate (at the 5% level), and Chapter 13 bankruptcy rate (at the 1% level). The comparison of median values of the change in bankruptcy rate using the Two-sample Wilcoxon rank-sum (Mann-Whitney) test yields similar results.

of *adjusted* treatment effects. Using the critical values of 1 percentile, 5 percentile, and 10 percentile of the distribution, we can assess the effect of the actual treatment experiment.

Table 4 reports the non-event borders and number of county-pairs that we use to generate placebo sample of counterfactuals. A border segment is qualified as a non-event border if it is not part of either an event-border group or Montana, Wyoming, Colorado, and New Mexico. As an example, Alabama is one of the seventeen treatment states and its event year is 1981. In the event-border sample (see Table 2), the borders of Alabama with Florida, Mississippi, and Tennessee are already considered. However, its border with Georgia is excluded because Georgia removed intrastate bank branching restrictions in 1983, which was less than two years from the event year of Alabama. The border segment Alabama-Georgia is in the out-of-sample non-event border group. In the case of the non-event border between Pennsylvania and Maryland, the event year for Pennsylvania is 1982. However, Maryland removed restrictions on bank branching before 1975. All twelve counties on this border segment are eligible for the placebo test. Altogether, we identify 385 county-pairs for the placebo test in the non-event border (out-of-sample) group. Appendix C shows a U.S. map with the non-event border segments and their contiguous county-pairs used to generate the placebo sample and fictitious treatment effects.

The actual treatment test requires the treatment counties have restrictions on intrastate bank branching during the prior period, as do the control counties of the neighboring state. After the event (treatment) year, the entry barriers for geographic expansion were removed in the treatment counties during the post-event period. However, the contiguous control counties in neighboring states continued to have bank branching restrictions in place during the post-period. The main hypothesis states that a net increase in bankruptcy filings for the treatment counties is attributable to the removal of intrastate bank branching restrictions, holding all else constant.

We consider all the possible combinations for the placebo test other than the one used in the main treatment event. We first choose a potentially fictitious event year between 1981 through 1992. The main reasons for selecting this period are (1) our sample period is between 1980 and 1994, and (2) we require three years for both the pre-event and post-event periods. When data are limited, we require a pre-event period of at least one year and a post-event period of at least two years. To qualify as a fictitious event year, we ensure that the fictitious event window – the period comprising pre-event period, fictitious event year, and post-event period – does not overlap with the actual event windows of the treatment and control states. This requirement is applied to a border segment of both the event-border group (Table 2) and to that of the non-event border group (Table 4). For example, consider the non-event border of Alabama and Georgia, the years 1981 and 1983 are ineligible as fictitious event years because both are the actual treatment years for Alabama and Georgia, respectively. The year 1985 is not qualified because it is part of the actual post-event period of Georgia. The year 1986 is disqualified because its prior period (1985) is part of the post-event period for Georgia. The years 1987 to 1992 are qualified for the fictitious event years, with only differences in the length of the pre- and post- 'fictitious' event periods. For 1987, the pre- and post- period lengths are one and three years, respectively. For 1988 and 1989, both post-periods are three years in length, however, the pre-period are two and three years, respectively. For the fictitious event year 1992, the pre-period length is three years, but the post period length is two years due to the analysis period ending in 1994. In this manner, a total of 2,435 placebo treatments (county-pair and fictitious treatment year combinations) are identified for the counterfactual tests.

As reported in Table 5, out of 2,435 placebo treatments, 1,807 combinations are from the non-event border group (out-of-sample) and 628 combinations are from the event border group

(in-sample). The counterfactuals are further identified by four different categories depending on the regulation and deregulation of intrastate bank branching restrictions in the treatment and control states during the fictitious event windows. Category I is comprised of those placebo treatments in which the treatment and control states have restrictions on bank branching during both the pre- and post-periods of the fictitious event year. As an example, consider the non-event border segment of Nebraska and Kansas and the fictitious event year is 1981. During 1980 to 1983 (fictitious event window), both states have entry barriers on intrastate bank branching. Category II consists of county-pair-fictitious treatment year combinations for which the treatment and control states have already removed the intrastate bank branching restrictions during the fictitious event window. The fictitious event year of 1992 for the event border segment Alabama and Florida is an example of this category. During the event window of 1991 to 1994, both the states have already deregulated geographic bank branching expansion.

Category III includes instances in which the treatment state still restricts the bank branching during the fictitious event window, while the control state has already removed such restrictions. Consider the fictitious event year 1983, for the non-event border segment of West Virginia and Virginia, the treatment state (West Virginia) removed restrictions in 1987. During the event window of 1981 to 1985 surrounding fictitious event year 1983, West Virginia has regulations in-place, whereas Virginia had removed the restrictions. Finally, Category IV is defined as the combinations in which the treatment state had lifted bank branching restrictions and the control state still had such restrictions in-place during the fictitious event window. For example, consider the event border segment of Nebraska and Iowa and the fictitious event year is 1989. The pre-period is 1988 and the post-period is from 1990 to 1992. Nebraska's actual treatment event year is

1985 and Iowa's treatment event is considered as 1994. During the fictitious event window, Nebraska removed bank branching restrictions and Iowa still had the restrictions in-place.

As reported in Table 5, the largest number of counterfactuals is from Category II, followed by Category I. For the placebo test, we use treatment and control states interchangeably as an actual event does not occurred. Therefore, we have 2 x 2,435 = 4,870 maximum possible placebo treatments for the counterfactual tests.[10]

*4.3. Adjustment of raw treatment effects using the placebo treatments*

We use our sample of 4,870 placebo county-pair and fictitious event year combinations (1) for adjusting the magnitude of the raw treatment effect for the sample of 186 actual treatment events, thereby obtaining the *adjusted* treatment effect, and (2) to obtain the empirical distribution and critical values against which we can ascertain statistical significance of the adjusted treatment effect.

Recall from Table 3, the raw treatment effect or the net effect of removing bank branching restrictions on total bankruptcy rate is 0.213. In the case of Chapter 7 bankruptcy, the rate is 0.094 and 0.120 in the case of Chapter 13 bankruptcy filing rate. The bankruptcy rate of a county, in general, depends on its local economic conditions and demographics. The following empirical specification is used on the sample of 4,870 placebo treatments:

$$BAG_{pt} = \beta_1 \times UAG_{pt} + \beta_2 \times IAG_{pt} + \beta_3 \times GAG_{pt} + \beta_4 \times YAG_{pt} + \varepsilon_{pt}\,, \qquad (3)$$

[10] While our second empirical methodology is largely based on Huang (2008), the approach differs in our identification of a combination of fictitious event year and a county-pair for the counterfactual sample. For his study, Huang (2008) constructs placebo combinations based on the 266 county-pairs for non-event borders. The fictitious event year is chosen as one of the 11 years between 1979 and 1989. This results in 266 x 11 x 2 = 5,852 possible combinations (page 691). In our approach, we consider the possible combinations using both the event-border sample and the non-event border sample, and make sure that there is no overlap between the fictitious event window and the actual event windows of either the treatment state or the neighboring control state.

where $BAG_{pt}$ is the bankruptcy acceleration gap for a county-pair $p$ in the fictitious event year $t$. This is also the difference-in-difference of the bankruptcy rate for the 'fictitious' treatment and 'fictitious' control counties, similar to the one shown in equation (2). Similarly, $UAG_{pt}$ is the unemployment acceleration gap and it is defined as the difference-in-difference of the unemployment rate between the 'fictitious' treatment and control counties. The variables $IAG_{pt}$, $GAG_{pt}$, and $YAG_{pt}$ are the per capita real income (in $1,000s) acceleration gap, income growth acceleration gap, and the acceleration gap for the percentage of young (age group 25 to 44 years) population. The intercept is zero because we have interchanged the fictitious treatment and control counties, making the placebo sample symmetrical. The OLS methodology is performed using the above specification on the total bankruptcy rate, Chapter 7 bankruptcy rate and Chapter 13 bankruptcy rate. We consider the full sample of placebo treatments, we restrict the sample to only Category I placebo treatments and, then, only Category II placebo combinations. The adjusted treatment effect is the residuals of equation (3).

In Table 6, we report the coefficients for each of the above specifications. As shown in the first column, which explains the cross-sectional variation in the bankruptcy acceleration gap for the total bankruptcy filing rate, the coefficients on the unemployment acceleration gap and income acceleration gap are significantly negative and positive, respectively.[11] The coefficient signs are, for the most part, intuitive. As the county's unemployment rate increases, its bankruptcy rate also increases due to job losses and the inability of households to repay loans. The increase in a county's income level suggests an increase in ability of households to repay loans, resulting in a decline in its bankruptcy filing rate. An increase in a young population suggests increased bankruptcy, which

[11] For this analysis, we pool the data of fictitious county-pairs. A county can be a part of multiple county-pairs. In addition, we interchange the fictitious treatment and control counties and generate symmetrical distribution. Therefore, the resulting standard error will be biased downwards, making the *t*-statistics inflated.

is intuitive. Young households, generally, have higher demand for credit to satisfy the needs of their growing family. The increased credit usage increases vulnerability to financial distress. The explanatory power of the independent variables increases when the regression is restricted to Category I placebo treatments for the total bankruptcy rate and Chapter 7 bankruptcy rate.

For simplicity, we report results using the coefficients obtained from the full sample of counterfactuals. Figure 4 shows the frequency distribution of adjusted fictitious treatment effects for the total bankruptcy rate. The distribution is symmetrical because we interchanged the treatment and control counties for the placebo test. The test statistics for the 90$^{th}$, 95$^{th}$ and 99$^{th}$ percentile values of the adjusted treatment effect are 0.781, 1.121, and 2.313, respectively, for the Total bankruptcy rate on our sample of 4,786 county-pair fictitious event year combinations (placebo treatments). These numbers imply that a real treatment effect will be statistically significant at the 5% level if the treatment sample that involves only one contiguous county-pair has an adjusted treatment effect above 1.121. These critical value numbers are also reported in the first row of Table 7. If the analysis involves more than one county-pair, then critical values decrease with increasing *N*. Specifically, the critical value changes to $1.121/\sqrt{N}$, for the 95 percentile or the 5% significance level. For example, if we analyze the sample of 186 county-pairs and our objective is to find significance at the 1% level, then the adjusted treatment effect on the actual treatment sample of 186 county pairs needs to be at least 0.170.

After obtaining the coefficients on $UAG$, $IAG$, $GAG$, and $YAG$, we use these coefficients to find the adjusted treatment effect for the treatment sample of 186 county-pairs. As an example, the adjusted treatment effect ($ATE$) for the total bankruptcy rate is obtained using the following equation:

$$ATE = BAG_{i,t} - \left[0.023 \times UAG_{i,t} + (-0.085) \times IAG_{i,t} + 0.624 \times GAG_{i,t} + 0.043 \times YAG_{i,t}\right], \quad (4)$$

where the raw treatment effect is the $BAG$ (bankruptcy acceleration gap obtained using equation (2)) for the total bankruptcy filing rate (*TotBKR*) of the 186 actual treatment county-pairs. The $UAG$, $IAG$, $GAG$, and $YAG$ are the actual values of the unemployment acceleration gap, income acceleration gap, growth acceleration gap, and acceleration gap of young population for the treatment county-pairs. A similar adjustment is performed on the raw treatment effects of Chapter 7 bankruptcy rate (*Ch7BKR*) and Chapter 13 bankruptcy rate (*Ch13BKR*), using their respective coefficient values.

*4.4. Results of a quasi-natural experiment on contiguous county-pairs*

Table 8 reports the results of a quasi-natural experiment at the individual state level and at the national level. As reported in the last row of Table 8, the raw treatment effect is 0.213 for the treatment sample of 186 counties. The adjusted treatment effect, after adjusting for economic and demographic factors, is 0.220 for the sample of 186 treatment county-pairs. Using the critical values table (Table 6) for $N$ = 186 the cut-off value of the adjusted treatment effect at the 99$^{th}$ percentile level is 0.170. Therefore, when evaluated using the critical values obtained from the non-parametric randomization test, the Total bankruptcy filing rate increases following intrastate bank branching. This change is statistically significant at the 1% level.

When we take a closer look at the individual state-level results as shown in Table 8, we find that the effect is statistically significant with critical values above the 10% level in only five of seventeen evaluated states. These states are Alabama, Georgia, Tennessee, Mississippi, and Louisiana. Among these five states, the largest effect is recorded for Tennessee. On the other hand, for Wisconsin, the effect is opposite. The net bankruptcy rate declines after removing intrastate bank branching restrictions.

Table 9 reports the results of state-level analysis for Chapter 7 and Chapter 13 filing rates. As reported in Panel A of Table 9, using the non-parametric test on the entire sample of treatment counties ($N$=186), the overall effect of bank branching deregulation on Chapter 7 filings is positive and significant at the 1% level.[12] At the state level, however, the increasing effect is limited to five states -- Alabama, Pennsylvania, Georgia, Mississippi, and North Dakota. The effects are not significant for Tennessee and Louisiana. Interestingly, the decreasing effect is observed for three states -- Nebraska, Illinois, and Wisconsin. Table 9, Panel B shows the results for the Chapter 13 filing rate. With randomization test, the overall effect is significant at the 5% level. At the state-level, the effect is limited to only four states -- Georgia, Tennessee, Illinois, and Louisiana. In the case of Alabama, Oklahoma, and Missouri, the removal of intrastate bank branching decreases the Chapter 13 bankruptcy filings. In addition, the results suggest that the significant effects observed for the total bankruptcy rate are mainly driven by the increases in the Chapter 13 filing rates in Tennessee and Louisiana.

Overall, the results provide additional perspective on the effect of removing banking geographic restrictions on bankruptcy filings. If analyzed at the aggregate level, the removal of entry barriers on bank branching increased the bankruptcy filing rate. However, by more closely looking at the state level, the increase is limited to few states. The results suggest that the removal of intrastate bank branching restrictions did not have a consistent effect across states on bankruptcy filing rates.

## 5. Analysis based on the panel data of contiguous county-pairs of treatment states

[12] The critical values table is reported in Appendix D.

In the third approach, we implement the methodology suggested in Dube, Lester and Reich (2010). We focus on the panel data of contiguous county pairs by combining 186 contiguous county-pairs of the event border group (Table 2) and an additional 385 contiguous county-pairs for the non-event border group (Table 4). As a result, we form 571 contiguous county-pairs spanning 58 border segments of 17 treatment states. The sample period for this analysis is 1980 to 1994. The maximum number of observations is 17,130 [571 x 2 x 15]. Under this methodology, we estimate the following pooled-OLS specification, where the subscript $p$ denotes the contiguous county pair (from 1 to 571):

$$Y_{pcst} = \alpha + A_c + B_t + cX_{cst} + \beta I_{st} + \varepsilon_{pcst}\,, \tag{5}$$

where the remaining notations are as described in equation (1). In each specification, we also control for county and year fixed effects. Appendix A, Panel 2 provides summary statistics for variables used in this approach.

In this analysis, a county can be a part of multiple county-pairs. In addition, the robust standard errors are clustered at the state level. This can induce a mechanical correlation across county-pairs and potentially along the entire border segment (Dube et al. 2010). In other words, the error term is correlated if the counties are from the same state or the two pairs are within the same border segment. Formally, $E\left(\varepsilon_{pcst}, \varepsilon_{p'c'st'}\right) \neq 0$ if $c, c' \in s$ or $p, p' \in b$, where notation $b$ is for the border segment. Therefore, in this type of situation, the heteroskedastic-robust standard error of the point estimate $\beta$ needs to be clustered on two dimensions -- at the state level and at the border segment level (Cameron, Gelbach & Miller 2011).[13]

[13] Specifically, for this analysis, we run the STATA® user-written command --cgmreg. We thank Doug Miller for making this program available on his website. The number of clusters for the state and border segment dimensions are 36 and 58, respectively, which is large enough for a reliable inference using clustered standard errors.

Table 10 reports the results of our third approach. The unemployment rate of a county has a direct relation with its Chapter 7 bankruptcy rate. An increase in a county's population in the age group of 25 to 44 increases its Chapter 13 filing rate. These results are similar to the ones obtained using the entire panel data sample (Table 1). Importantly, the effect of removing bank branching restrictions within a state has statistically insignificant impact on the state's Total, Chapter 7 and Chapter 13 bankruptcy filing rate. While insignificant, the direction of the effect is positive in the case of Chapter 7 filings and negative for Chapter 13 filings.

**6. Comparison with previous findings of Dick and Lehnert (2010)**

Our next task is to reproduce the results of Dick and Lehnert (2010) and then use their empirical specification on county-level data. Specifically, we replicate their results using their specification (i) of Table III (page 668). For this analysis, we use an additional dummy variable to measure the effect of removing of restrictions on interstate banking activities. The dummy variable *InterState* takes on a value of one for a given year if the state allows interstate banking in that year, and zero otherwise. These data are from Table 1 of Dick and Lehnert (2010). The following empirical specification is used on the state-level data:

$$Y_{st} = \alpha + A_s + B_t + cX_{st} + \beta_1 I_{st}^1 + \beta_2 I_{st}^2 + \varepsilon_{st}\,, \qquad (6)$$

where, $I_{st}^1$ and $I_{st}^2$ are dummy variables for *IntraState* and *InterState*, respectively. We aggregate the county-level bankruptcy count for Total, Chapter7 and Chapter 13, as well as population data, at the state-level to obtain state-level measures of *TotBKR*, *Ch7BKR*, and *Ch13BKR*. Regarding the state controls $X_{st}$, we use the unemployment rate lagged one year and the current growth in nominal per capita income as well as its one-year lagged value. The yearly income and unemployment data at the state-level are from BEA and BLS, respectively. Following Dick and

Lehnert (2010), we exclude Delaware and South Dakota as these states had tax incentives for credit card banks during our sample period.[14] In all specifications, we use heteroskedastic-robust standard errors clustered at the state level. In Appendix A, Panel 3 we report summary statistics. Comparing our state-level data with their data (Table II on page 665), the mean and standard deviation values of the dependent variable (*Ch7BKR*) and independent variables are similar.

Table 11 reports the results. As seen in specification (1) of our Table 11, the coefficients and standard errors are very close to those shown in Table III, specification (i) of Dick and Lehnert (2010). A county's Chapter 7 bankruptcy rate for the current year is directly related to its unemployment rate of the previous year. In addition, both the current and previous year's nominal income growth is inversely related to the current Chapter 7 bankruptcy rate. More importantly, the coefficient on *InterState* is positive and statistically significant. The effect of *IntraState* is positive but statistically insignificant. As seen in our specifications (2) and (3) of Table 11, the coefficient on *InterState* is positive and statistically significant. The removal of interstate banking restrictions by a state increases its Chapter 13 bankruptcy filing rate and the Total bankruptcy rate. In summary, based on the state-level data, the effect of banking deregulation is to increase the level of bankruptcy filings.[15]

Next, we use the same empirical specification and on county-level data. Here, too, we exclude South Dakota, Delaware, and District of Columbia. We run the following specification:

$$Y_{cst} = \alpha + A_c + B_t + cX_{cst} + \beta_1 I_{st}^1 + \beta_2 I_{st}^2 + \varepsilon_{cst}. \quad (7)$$

[14] Following them, we also exclude District of Columbia as the analysis is at the state-level.

[15] In this paper, we mainly focus on the effect of intrastate bank branching on personal bankruptcy rate, and not on the effect of interstate banking. Our empirical approaches (methods 2 and 3) based on contiguous counties require time duration between the policy change events in the treatment and control states. In the U.S., 35 states enacted legislation allowing interstate banking in a short time-window from 1985 to 1988 (see Table 1 of Dick and Lehnert, 2010). Therefore, we could have not constructed the treatment sample based on contiguous counties to assess the *real* effect of interstate banking on personal bankruptcy rate.

Specification (4) of Table 11 shows the results using the county-level Chapter 7 bankruptcy filing rate. The effects of previous year unemployment and current plus last year's nominal income growth remain intact, i.e., higher unemployment leads to higher Chapter 7 filing, and higher income growth leads to lower Chapter 7 filings. These results are similar to the state-level results reported in specification (1) of this Table 11.

The same results do not hold for the deregulation variables. Removing interstate banking restrictions seems to decrease the Chapter 7 bankruptcy filing rate, although the effect is not statistically significant. The lack of significance is noteworthy as it stands in sharp contrast to the results based on the state-level data (our specification (1)). The insignificance is also evident for the Chapter 13 and Total bankruptcy filing rates. One possible explanation is that county-level data provide larger cross-sectional variation of the dependent variable. The large sample size in the case of county-level data helps increase the precision of the statistical estimates.

Overall, the analyses in this section combined with the previous sections suggest it is a stretch to conclude that geographic banking deregulation spurs consumer bankruptcy filings.

## 7. Concluding remarks

This paper offers a closer examination of consumer bankruptcy over a period when various U.S. states removed intrastate branching restrictions. The research objective is to evaluate the notion that the deregulation in banking sector has increased the financial distress. The outcome variables are the county-level personal bankruptcy filings -- both under Chapter 7 and Chapter 13 procedures. Three available empirical methodologies are applied to answer the research question.

Our results, based on the non-parametric approach using the contiguous county-pairs, show that in aggregate there is a statistically significant increase in consumer bankruptcy due to the

removal of intrastate branching restriction. The use of a difference-in-difference framework, along with a focus on contiguous counties separated by state borders, is the basis for our causal argument under this approach. When we examine state-by-state the effect of intrastate bank branching deregulation on bankruptcy filings, we find that the increase in bankruptcy filings differs across U.S. states. Overall, we find that only five of seventeen 'treatment' states show an increase in bankruptcy rate. Moreover, the same results hold for Chapter 7 and Chapter 13 filings. When we apply the methodologies incorporating panel data of all U.S. counties and those of contiguous county pairs of treatment states, our results show the effect of removing intrastate bank branching restriction on financial distress is insignificant.

Geographic-based restrictions for the banking sector were resolved by U.S. federal law (Riegle-Neal Interstate Banking and Branching Efficiency Act) during the time following our analysis period. In essence, this created a level-playing field across all U.S. states. However, the presence of banking regulations based on geographic restrictions remains in many developed and emerging markets across the global economy. It is our hope that the conclusions reached in our work will facilitate policy discussions concerning currently regulated markets. In addition, the role of regulators in the U.S. banking sector has increased following the global financial crisis. The enactment of the Credit CARD Act of 2009 and Dodd-Frank Act of 2010 are examples. The results of our study should inform ongoing debate concerning the need for additional regulations in the banking sector.

**References:**


Bertrand, M., Duflo, E., Mullainathan, S., 2004. How Much Should We Trust Differences-in-Differences Estimates? Quarterly Journal of Economics 119, 249-275

Cameron, A.C., Gelbach, J.B., Miller, D.L., 2011. Robust Inference with Multiway Clustering. Journal of Business & Economic Statistics 29, 238-249

Cameron, A.C., Miller, D.L., 2015. A Practitioner's Guide to Cluster-Robust Inference. Journal of Human Resources 50, 317-372

Desai, C., Elliehausen, G., Lawrence, E., 2014. On the County-Level Credit Outcome Beta. Journal of Financial Services Research 45, 201-218

Dick, A.A., Lehnert, A., 2010. Personal Bankruptcy and Credit Market Competition. The Journal of Finance 65, 655-686

Domowitz, I., Sartain, R.L., 1999. Determinants of the consumer bankruptcy decision. The Journal of Finance 54, 403-420

Dube, A., Lester, W.T., Reich, M., 2010. Minimum wage effects across state borders: Estimates using contiguous counties. Review of Economics & Statistics 92, 945-964

Fay, S., Hurst, E., White, M.J., 2002. The Household Bankruptcy Decision. The American Economic Review 92, 706-718

Gross, D.B., Souleles, N.S., 2002. An Empirical Analysis of Personal Bankruptcy and Delinquency. The Review of Financial Studies 15, 319-347

Himmelstein, D.U., Warren, E., Thorne, D., Woolhandler, S., 2005. Illness and injury as contributors to bankruptcy. Health Affairs 24, 63-73

Huang, R.R., 2008. Evaluating the real effect of bank branching deregulation: Comparing contiguous counties across US state borders. Journal of Financial Economics 87, 678-705

Jayaratne, J., Strahan, P.E., 1996. The finance-growth nexus: Evidence from bank branch deregulation. The Quarterly Journal of Economics 111, 639-670

Jayaratne, J., Strahan, P.E., 1997. The benefits of branching deregulation. Economic Policy Review 3

Livshits, I., MacGee, J., Tertilt, M., 2010. Accounting for the Rise in Consumer Bankruptcies. American Economic Journal: Macroeconomics 2, 165-193

Strahan, P.E., 2003. The real effects of US banking deregulation. Federal Reserve Bank of St. Louis Review 85

Sullivan, T.A., Warren, E., Westbrook, J.L., 2000. The fragile middle class: Americans in debt. Yale University Press.

White, M.J., 2006. Bankruptcy and consumer behavior: Theory and U.S. evidence. In: Bertola G, Disney R & Grant C (eds.) The economics of consumer credit. MIT Press, Cambridge, Massachusetts, pp. 239-274.

White, M.J., Zhu, N., 2010. Saving Your Home in Chapter 13 Bankruptcy. The Journal of Legal Studies 39, 33-61

**Table 1: Results based on 'Traditional' panel data approach**

| Dep. Var. | TotBKR | | | Ch7BKR | | | Ch13BKR | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| IntraState | -0.040 | -0.025 | -0.027 | 0.015 | 0.021 | 0.021 | -0.055 | -0.046 | -0.047 |
| | (0.100) | (0.101) | (0.100) | (0.070) | (0.069) | (0.069) | (0.069) | (0.069) | (0.069) |
| Unemp | | 0.029** | 0.030** | | 0.024*** | 0.025*** | | 0.005 | 0.006 |
| | | (0.011) | (0.011) | | (0.007) | (0.007) | | (0.009) | (0.009) |
| Income | | 0.037 | 0.031 | | -0.016 | -0.016 | | 0.053* | 0.048 |
| | | (0.041) | (0.041) | | (0.025) | (0.025) | | (0.031) | (0.029) |
| RealIncGrwth | | -0.269 | -0.227 | | -0.130 | -0.128 | | -0.139 | -0.099 |
| | | (0.167) | (0.164) | | (0.095) | (0.095) | | (0.114) | (0.107) |
| Pop25to44 | | | 0.076** | | | 0.003 | | | 0.073** |
| | | | (0.031) | | | (0.013) | | | (0.029) |
| Intercept | 0.876*** | 0.359 | -1.544 | 0.679*** | 0.600*** | 0.524 | 0.197*** | -0.241 | -2.068** |
| | (0.085) | (0.367) | (1.044) | (0.037) | (0.173) | (0.373) | (0.070) | (0.312) | (1.001) |
| $R^2$(within) | 0.46 | 0.47 | 0.47 | 0.44 | 0.45 | 0.45 | 0.19 | 0.19 | 0.20 |
| $R^2$ (overall) | 0.21 | 0.22 | 0.27 | 0.21 | 0.20 | 0.20 | 0.06 | 0.04 | 0.05 |
| # of obs. | 46230 | 45749 | 45749 | 46230 | 45749 | 45749 | 46230 | 45749 | 45749 |
| # of counties | 3136 | 3106 | 3106 | 3136 | 3106 | 3106 | 3136 | 3106 | 3106 |
| Fixed effects | | | | | | | | | |
| County | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: The empirical specification is $Y_{cst} = \alpha + A_c + B_t + cX_{cst} + \beta I_{st} + \varepsilon_{cst}$. The dependent variables in specifications 1-3, 4-6, and 7-9 are TotBKR, Ch7BKR, and Ch13BKR, respectively. The robust standard errors are clustered at the state level. The symbols *,**,*** denote statistical significance at the 10%,5%, and 1% levels, respectively.

**Table 2: Sample description for non-parametric test**

| Event border (Treatment State - Neighboring Control State) | Deregulation Year in Treatment State | Deregulation Year in Neighboring (control) state | Number of contiguous county pairs | Total number of county pairs for treatment state | % of sample |
|---|---|---|---|---|---|
| Alabama – Tennessee | 1981 | 1985 | 6 | | |
| Alabama – Mississippi | 1981 | 1986 | 14 | | |
| Alabama – Florida | 1981 | 1988 | 7 | 27 | 14.5% |
| Pennsylvania - West Virginia | 1982 | 1987 | 6 | 6 | 3.2% |
| Georgia – Florida | 1983 | 1988 | 12 | 12 | 6.5% |
| Massachusetts - New Hampshire | 1984 | 1987 | 3 | 3 | 1.6% |
| Nebraska – Missouri | 1985 | 1990 | 2 | | |
| Nebraska – Iowa | 1985 | 1994 | 9 | 11 | 5.9% |
| Tennessee - Kentucky | 1985 | 1990 | 20 | | |
| Tennessee – Missouri | 1985 | 1990 | 2 | | |
| Tennessee - Arkansas | 1985 | 1994 | 3 | 25 | 13.4% |
| Mississippi - Arkansas | 1986 | 1994 | 5 | 5 | 2.7% |
| Kansas – Missouri | 1987 | 1990 | 11 | 11 | 5.9% |
| Michigan – Wisconsin | 1987 | 1990 | 5 | 5 | 2.7% |
| North Dakota - Minnesota | 1987 | 1993 | 6 | 6 | 3.2% |
| West Virginia - Kentucky | 1987 | 1990 | 4 | 4 | 2.2% |
| Illinois – Iowa | 1988 | 1994 | 9 | 9 | 4.8% |
| Louisiana – Arkansas | 1988 | 1994 | 8 | 8 | 4.3% |
| Oklahoma - Arkansas | 1988 | 1994 | 8 | 8 | 4.3% |
| Texas – Arkansas | 1988 | 1994 | 2 | 2 | 1.1% |
| Missouri – Arkansas | 1990 | 1994 | 16 | | |
| Missouri – Iowa | 1990 | 1994 | 12 | 28 | 15.1% |
| Wisconsin - Minnesota | 1990 | 1993 | 12 | | |
| Wisconsin – Iowa | 1990 | 1994 | 4 | 16 | 8.6% |
| | | Total | 186 | 186 | 100.0% |

**Table 3: Preliminary comparison of treatment and contiguous control counties**

| | Treatment Group | | | Control group using contiguous counties | | |
|---|---|---|---|---|---|---|
| | *N* | Mean | Median | *N* | Mean | Median |
| Total bankruptcy rate (TotBKR) | | | | | | |
| Pre-period | 186 | 1.110 | 0.977 | 186 | 1.004 | 0.851 |
| Post-period | 186 | 1.754 | 1.434 | 186 | 1.435 | 1.265 |
| Time-series difference | 186 | 0.644 | 0.430 | 186 | 0.431 | 0.377 |
| Std. dev. of difference | | 0.710 | | | 0.663 | |
| Chapter 7 bankruptcy rate (Ch7BKR) | | | | | | |
| Pre-period | 186 | 0.840 | 0.814 | 186 | 0.853 | 0.741 |
| Post-period | 186 | 1.253 | 1.159 | 186 | 1.172 | 1.075 |
| Time-series difference | 186 | 0.412 | 0.364 | 186 | 0.319 | 0.295 |
| Std. dev. of difference | | 0.413 | | | 0.556 | |
| Chapter 13 bankruptcy rate (Ch13BKR) | | | | | | |
| Pre-period | 186 | 0.270 | 0.106 | 186 | 0.270 | 0.106 |
| Post-period | 186 | 0.502 | 0.207 | 186 | 0.263 | 0.115 |
| Time-series difference | 186 | 0.232 | 0.067 | 186 | 0.112 | 0.038 |
| Std. dev. of difference | | 0.515 | | | 0.282 | |

**Table 4: Sample description of fictitious event (out-of-sample) group**

| Non - Event border (Treatment State - Neighboring Control State) | Deregulation Year in Neighboring (control) state | Number of county pairs |
|---|---|---|
| Alabama - Georgia | 1983 | 18 |
| Pennsylvania - Ohio | 1979 | 5 |
| Pennsylvania - New York | 1976 | 16 |
| Pennsylvania - New Jersey | 1977 | 10 |
| Pennsylvania - Maryland | Before 1975 | 12 |
| Pennsylvania - Delaware | Before 1975 | 2 |
| Georgia - North Carolina | Before 1975 | 4 |
| Georgia - South Carolina | Before 1975 | 21 |
| Georgia - Tennessee | 1985 | 7 |
| Massachusetts - Vermont | Before 1975 | 2 |
| Massachusetts - New York | Before 1975 | 2 |
| Massachusetts - Connecticut | 1980 | 4 |
| Massachusetts - Rhode Island | Before 1975 | 5 |
| Nebraska - South Dakota | Before 1975 | 18 |
| Nebraska - Kansas | 1987 | 20 |
| Tennessee - Virginia | 1978 | 6 |
| Tennessee - North Carolina | Before 1975 | 15 |
| Tennessee - Mississippi | 1986 | 8 |
| Mississippi - Louisiana | 1988 | 19 |
| Kansas - Oklahoma | 1988 | 23 |
| Michigan - Ohio | 1979 | 4 |
| Michigan - Indiana | 1989 | 8 |
| North Dakota - South Dakota | Before 1975 | 10 |
| West Virginia - Ohio | 1979 | 18 |
| West Virginia - Maryland | Before 1975 | 9 |
| West Virginia - Virginia | 1978 | 21 |
| Illinois - Indiana | 1989 | 18 |
| Illinois - Kentucky | 1990 | 5 |
| Illinois - Wisconsin | 1990 | 7 |
| Illinois - Missouri | 1990 | 19 |
| Louisiana - Texas | 1988 | 13 |
| Oklahoma - Texas | 1988 | 29 |
| Oklahoma - Missouri | 1990 | 3 |
| Missouri - Kentucky | 1990 | 4 |
| | Total | 385 |

Notes: The actual deregulation year of the treatment state is provided in Table 2.

**Table 5: Description of placebo county-pair year combinations**

| | Category I | Category II | Category III | Category IV | Total |
|---|---|---|---|---|---|
| From event border group (in - sample) | 496 | 103 | -- | 29 | 628 |
| From Non-event border group (out of sample) | 559 | 1,094 | 154 | -- | 1,807 |
| Total | 1,055 | 1,197 | 154 | 29 | 2,435 |

Notes: The detailed explanation of Categories I, II, III, and IV with examples is in Section 4.2 of the manuscript.

**Table 6: Cross-sectional variations in raw treatment effect – bankruptcy acceleration gap (*BAG*)**

| | *BAG* - TotBKR | | | *BAG* - Ch7BKR | | | *BAG* - Ch13BKR | | |
|---|---|---|---|---|---|---|---|---|---|
| | Full Sample | Only Category I | Only Category II | Full Sample | Only Category I | Only Category II | Full Sample | Only Category I | Only Category II |
| *UAG* | 0.023*** | 0.022*** | 0.013 | 0.006** | 0.017*** | -0.012** | 0.016*** | 0.006*** | 0.025*** |
| | [5.22] | [5.90] | [1.50] | [2.17] | [5.12] | [-2.13] | [5.27] | [2.96] | [3.79] |
| *IAG* | -0.085*** | -0.070*** | -0.086** | -0.105*** | -0.089*** | -0.114*** | 0.020 | 0.019** | 0.028 |
| | [-4.45] | [-4.30] | [-2.31] | [-8.12] | [-6.35] | [-4.89] | [1.44] | [2.31] | [1.02] |
| *GAG* | 0.624*** | 1.030*** | -0.622 | 0.843*** | 1.078*** | 0.324 | -0.219 | -0.048 | -0.947** |
| | [3.24] | [7.27] | [-1.20] | [6.49] | [8.84] | [1.00] | [-1.60] | [-0.68] | [-2.45] |
| *YAG* | 0.043** | -0.005 | 0.063* | -0.014 | 0.007 | -0.035* | 0.058*** | -0.012 | 0.098*** |
| | [2.15] | [-0.26] | [1.86] | [-1.04] | [0.40] | [-1.66] | [4.00] | [-1.20] | [3.88] |
| *N* | 4786 | 2074 | 2376 | 4786 | 2074 | 2376 | 4786 | 2074 | 2376 |
| R-square | 0.01 | 0.05 | 0.01 | 0.02 | 0.06 | 0.01 | 0.01 | 0.01 | 0.01 |

Notes: The symbols *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

**Table 7: Critical values for the Total Bankruptcy Rate (TotBKR) adjusted treatment effect (ATE)**

| Number of county pairs used to form the mean | Statistical confidence level | | |
|---|---|---|---|
| | 90% | 95% | 99% |
| 1 | 0.781 | 1.121 | 2.313 |
| 2 | 0.552 | 0.793 | 1.636 |
| 3 | 0.451 | 0.647 | 1.336 |
| 4 | 0.391 | 0.561 | 1.157 |
| 5 | 0.349 | 0.502 | 1.035 |
| 6 | 0.319 | 0.458 | 0.944 |
| 7 | 0.295 | 0.424 | 0.874 |
| 8 | 0.276 | 0.396 | 0.818 |
| 9 | 0.260 | 0.374 | 0.771 |
| 10 | 0.247 | 0.355 | 0.732 |
| 11 | 0.236 | 0.338 | 0.698 |
| 12 | 0.225 | 0.324 | 0.668 |
| 13 | 0.217 | 0.311 | 0.642 |
| 14 | 0.209 | 0.300 | 0.618 |
| 15 | 0.202 | 0.290 | 0.597 |
| 16 | 0.195 | 0.280 | 0.578 |
| 17 | 0.189 | 0.272 | 0.561 |
| 18 | 0.184 | 0.264 | 0.545 |
| 19 | 0.179 | 0.257 | 0.531 |
| 20 | 0.175 | 0.251 | 0.517 |
| 21 | 0.170 | 0.245 | 0.505 |
| 22 | 0.167 | 0.239 | 0.493 |
| 23 | 0.163 | 0.234 | 0.482 |
| 24 | 0.159 | 0.229 | 0.472 |
| 25 | 0.156 | 0.224 | 0.463 |
| 26 | 0.153 | 0.220 | 0.454 |
| 27 | 0.150 | 0.216 | 0.445 |
| 28 | 0.148 | 0.212 | 0.437 |
| 29 | 0.145 | 0.208 | 0.430 |
| 30 | 0.143 | 0.205 | 0.422 |
| 50 | 0.110 | 0.159 | 0.327 |
| 70 | 0.093 | 0.134 | 0.277 |
| 100 | 0.078 | 0.112 | 0.231 |
| 186 | 0.057 | 0.082 | 0.170 |
| 200 | 0.055 | 0.079 | 0.164 |

**Table 8: State level effects of deregulation on Total Bankruptcy Rate**

| State | Deregulation year | # of county-pairs | Avg. for the pre-period | Raw Treatment effect (Diff-in-Diff) | Adjusted Treatment effect | Statistical Significance |
|---|---|---|---|---|---|---|
| Alabama | 1981 | 27 | 0.777 | 0.285 | 0.263 | 5% |
| Pennsylvania | 1982 | 6 | 0.731 | 0.357 | 0.302 | Insignificant |
| Georgia | 1983 | 12 | 0.794 | 0.564 | 0.550 | 5% |
| Massachusetts | 1984 | 3 | 0.399 | 0.023 | 0.059 | Insignificant |
| Nebraska | 1985 | 11 | 1.082 | -0.201 | -0.191 | Insignificant |
| Tennessee | 1985 | 25 | 1.707 | 0.782 | 0.882 | 1% |
| Mississippi | 1986 | 5 | 1.519 | 0.706 | 0.695 | 5% |
| Kansas | 1987 | 11 | 1.549 | 0.184 | 0.204 | Insignificant |
| Michigan | 1987 | 5 | 0.490 | -0.052 | -0.027 | Insignificant |
| North Dakota | 1987 | 6 | 0.726 | 0.320 | 0.291 | Insignificant |
| West Virginia | 1987 | 4 | 0.631 | 0.075 | 0.115 | Insignificant |
| Illinois | 1988 | 9 | 1.847 | -0.068 | -0.060 | Insignificant |
| Louisiana | 1988 | 8 | 1.779 | 0.662 | 0.705 | 5% |
| Oklahoma | 1988 | 8 | 0.740 | -0.191 | -0.210 | Insignificant |
| Texas | 1988 | 2 | 0.570 | 0.075 | 0.044 | Insignificant |
| Missouri | 1990 | 28 | 1.004 | -0.061 | -0.082 | Insignificant |
| Wisconsin | 1990 | 16 | 0.993 | -0.217 | -0.228 | 10% |
| Overall | | 186 | 1.110 | 0.213 | 0.220 | 1% |

**Table 9: State level effects of deregulation by type of bankruptcy procedure**

Panel A: For Chapter 7 Bankruptcy Rate

| State | Deregulation year | # of county-pairs | Avg. for the pre-period | Raw Treatment effect | Adjusted Treatment effect | Statistical Significance |
|---|---|---|---|---|---|---|
| Alabama | 1981 | 27 | 0.412 | 0.388 | 0.383 | 1% |
| Pennsylvania | 1982 | 6 | 0.697 | 0.305 | 0.305 | 10% |
| Georgia | 1983 | 12 | 0.541 | 0.307 | 0.290 | 5% |
| Massachusetts | 1984 | 3 | 0.329 | 0.050 | 0.082 | Insignificant |
| Nebraska | 1985 | 11 | 0.876 | -0.289 | -0.299 | 5% |
| Tennessee | 1985 | 25 | 0.942 | 0.030 | 0.090 | Insignificant |
| Mississippi | 1986 | 5 | 1.094 | 0.605 | 0.629 | 1% |
| Kansas | 1987 | 11 | 1.268 | 0.163 | 0.158 | Insignificant |
| Michigan | 1987 | 5 | 0.477 | -0.059 | -0.042 | Insignificant |
| North Dakota | 1987 | 6 | 0.696 | 0.334 | 0.329 | 5% |
| West Virginia | 1987 | 4 | 0.536 | 0.120 | 0.134 | Insignificant |
| Illinois | 1988 | 9 | 1.574 | -0.222 | -0.243 | 10% |
| Louisiana | 1988 | 8 | 1.251 | 0.021 | 0.013 | Insignificant |
| Oklahoma | 1988 | 8 | 0.701 | -0.004 | -0.028 | Insignificant |
| Texas | 1988 | 2 | 0.488 | -0.145 | -0.194 | Insignificant |
| Missouri | 1990 | 28 | 0.949 | 0.051 | 0.063 | Insignificant |
| Wisconsin | 1990 | 16 | 0.930 | -0.164 | -0.164 | 10% |
| Overall | | 186 | 0.840 | 0.094 | 0.099 | 1% |

Panel B: For Chapter 13 Bankruptcy Rate

| State | Deregulation year | # of county-pairs | Avg. for the pre-period | Raw Treatment effect | Adjusted Treatment effect | Statistical Significance |
|---|---|---|---|---|---|---|
| Alabama | 1981 | 27 | 0.365 | -0.102 | -0.120 | 10% |
| Pennsylvania | 1982 | 6 | 0.034 | 0.052 | -0.003 | Insignificant |
| Georgia | 1983 | 12 | 0.253 | 0.256 | 0.260 | 5% |
| Massachusetts | 1984 | 3 | 0.070 | -0.027 | -0.023 | Insignificant |
| Nebraska | 1985 | 11 | 0.207 | 0.088 | 0.108 | Insignificant |
| Tennessee | 1985 | 25 | 0.765 | 0.752 | 0.792 | 1% |
| Mississippi | 1986 | 5 | 0.424 | 0.101 | 0.066 | Insignificant |
| Kansas | 1987 | 11 | 0.280 | 0.020 | 0.045 | Insignificant |
| Michigan | 1987 | 5 | 0.013 | 0.007 | 0.015 | Insignificant |
| North Dakota | 1987 | 6 | 0.031 | -0.014 | -0.038 | Insignificant |
| West Virginia | 1987 | 4 | 0.096 | -0.045 | -0.018 | Insignificant |
| Illinois | 1988 | 9 | 0.273 | 0.154 | 0.183 | 10% |
| Louisiana | 1988 | 8 | 0.528 | 0.641 | 0.692 | 1% |
| Oklahoma | 1988 | 8 | 0.040 | -0.188 | -0.182 | 10% |
| Texas | 1988 | 2 | 0.082 | 0.219 | 0.239 | Insignificant |
| Missouri | 1990 | 28 | 0.055 | -0.112 | -0.146 | 10% |
| Wisconsin | 1990 | 16 | 0.063 | -0.053 | -0.064 | Insignificant |
| Overall | | 186 | 0.270 | 0.120 | 0.099 | 5% |

**Table 10: Results based on cross-sectional time-series data of contiguous county-pairs**

| Dep. Var. | TotBKR | | | Ch7BKR | | | Ch13BKR | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| IntraState | -0.018 | -0.007 | -0.015 | 0.028 | 0.023 | 0.022 | -0.046 | -0.030 | -0.037 |
| | (0.115) | (0.115) | (0.114) | (0.043) | (0.043) | (0.043) | (0.097) | (0.098) | (0.096) |
| Unemp | 0.021 | 0.022* | 0.023* | 0.020** | 0.018** | 0.018** | 0.001 | 0.004 | 0.005 |
| | (0.013) | (0.012) | (0.012) | (0.008) | (0.007) | (0.007) | (0.010) | (0.010) | (0.009) |
| Income | | 0.049 | 0.042 | | -0.036* | -0.036* | | 0.085 | 0.079 |
| | | (0.074) | (0.076) | | (0.020) | (0.020) | | (0.062) | (0.063) |
| IncGrwth | | -0.430 | -0.387 | | -0.133 | -0.130 | | -0.297 | -0.257 |
| | | (0.314) | (0.313) | | (0.104) | (0.104) | | (0.249) | (0.248) |
| Pop25to44 | | | 0.095** | | | 0.007 | | | 0.088** |
| | | | (0.038) | | | (0.020) | | | (0.039) |
| Intercept | 0.991*** | 0.602 | -1.779 | 0.839*** | 1.115*** | 0.943* | 0.152 | -0.513 | -2.722** |
| | (0.119) | (0.607) | (1.292) | (0.062) | (0.142) | (0.545) | (0.110) | (0.534) | (1.346) |
| N_obs | 16987 | 16987 | 16987 | 16987 | 16987 | 16987 | 16987 | 16987 | 16987 |
| R-square | 0.79 | 0.79 | 0.79 | 0.75 | 0.75 | 0.75 | 0.75 | 0.75 | 0.75 |
| Fixed effects: | | | | | | | | | |
| County | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: This table is based on the approach as suggested in Dube, Lester and Reich (2010). The sample includes 571 contiguous county-pairs of 17 treatment states that removed the restrictions on intrastate bank branching during 1980 to 1994. The heteroskedastic robust standard errors are clustered at the state level and at the border segment level, and are reported in parentheses. The symbols *,**, and *** denote statistical significance at the 10%, 5%, and 1%, respectively.

**Table 11: Comparison with the earlier findings of Dick and Lehnert (2010)**

| | State-level data | | | County-level data | | |
|---|---|---|---|---|---|---|
| Dep. Var. | Ch7BKR | Ch13BKR | TotBKR | Ch7BKR | Ch13BKR | TotBKR |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| InterState | 0.157** | 0.099** | 0.256*** | -0.024 | -0.003 | -0.027 |
| | (0.060) | (0.048) | (0.094) | (0.042) | (0.041) | (0.063) |
| IntraState | 0.027 | -0.020 | 0.007 | 0.011 | -0.075 | -0.064 |
| | (0.076) | (0.077) | (0.119) | (0.071) | (0.072) | (0.102) |
| L1.Unemp | 0.139*** | -0.000 | 0.139*** | 0.020*** | 0.001 | 0.021* |
| | (0.026) | (0.023) | (0.036) | (0.008) | (0.009) | (0.013) |
| NomIncGrwth | -1.822*** | 0.023 | -1.799** | -0.342*** | 0.003 | -0.339*** |
| | (0.560) | (0.468) | (0.787) | (0.100) | (0.056) | (0.126) |
| L1.NomIncGrwth | -1.495** | -0.277 | -1.772** | -0.237** | -0.058 | -0.295** |
| | (0.579) | (0.435) | (0.766) | (0.098) | (0.057) | (0.125) |
| Intercept | 0.464** | 0.326** | 0.790*** | 0.614*** | 0.203** | 0.817*** |
| | (0.207) | (0.153) | (0.265) | (0.057) | (0.090) | (0.115) |
| $R^2$ (within) | 0.84 | 0.40 | 0.79 | 0.45 | 0.19 | 0.47 |
| $R^2$ (overall) | 0.45 | 0.11 | 0.41 | 0.21 | 0.06 | 0.21 |
| # of obs. | 720 | 720 | 720 | 44749 | 44749 | 44749 |
| # of groups | 48 | 48 | 48 | 3036 | 3036 | 3036 |
| Fixed effects: | | | | | | |
| State | Yes | Yes | Yes | | | |
| County | | | | Yes | Yes | Yes |
| Year | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: The specifications (1) to (3) use data aggregated at the state-level, while the specifications (4) to (6) use county-level data. The sample excludes South Dakota, Delaware, and District of Columbia. The IntraState (InterState) is a dummy variable that takes on a value of one if a state in a given year allows the intrastate branching (interstate banking), and zero otherwise. The robust standard errors clustered at the state level are reported in the parentheses. The variable *NomIncGrwth* is the current growth in nominal per capita income. The L1 indicates one-year lag value. The symbols *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

**Figure 1: Personal Bankruptcy filings in the U.S. (1980 - 2012)**

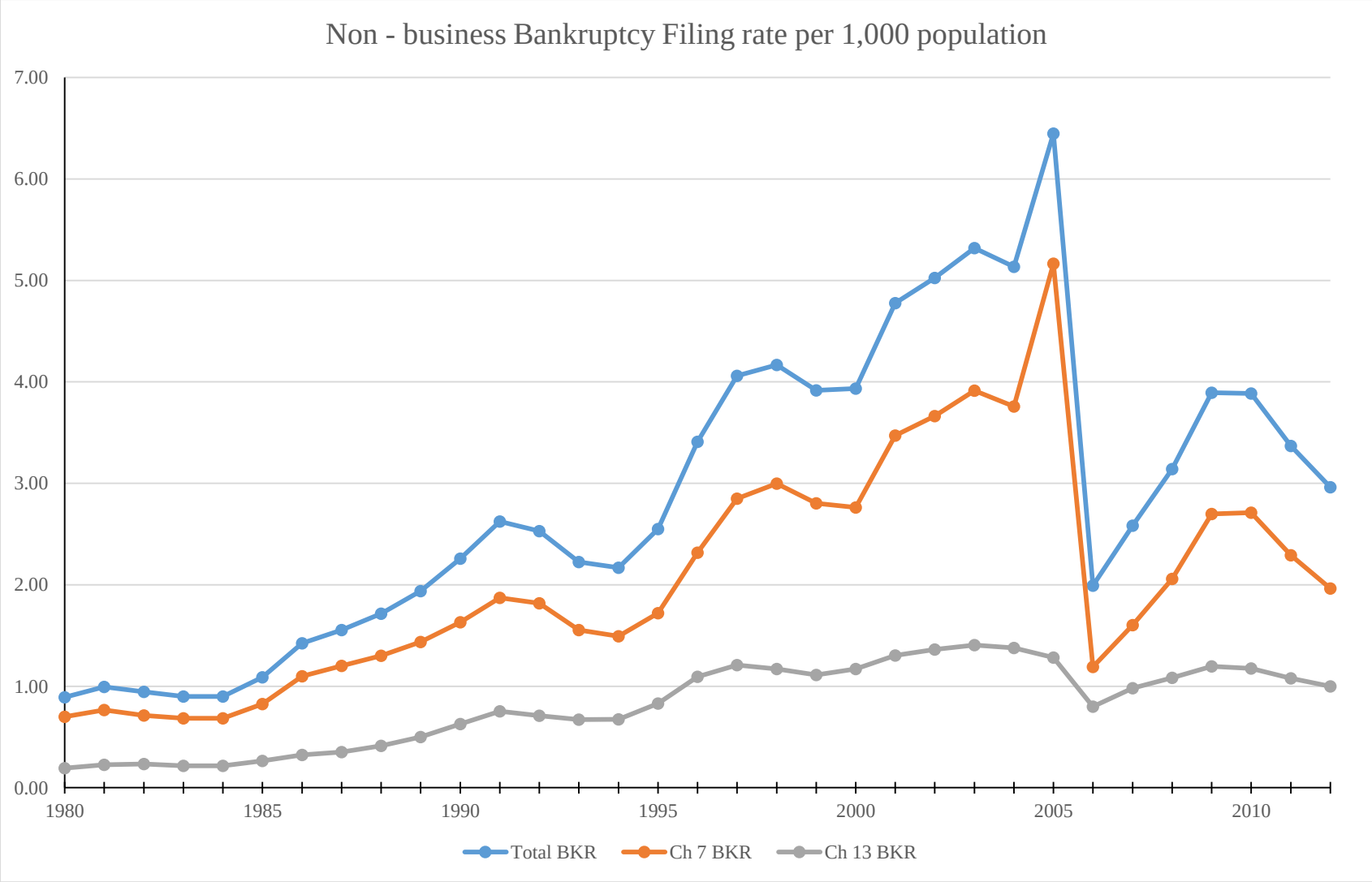
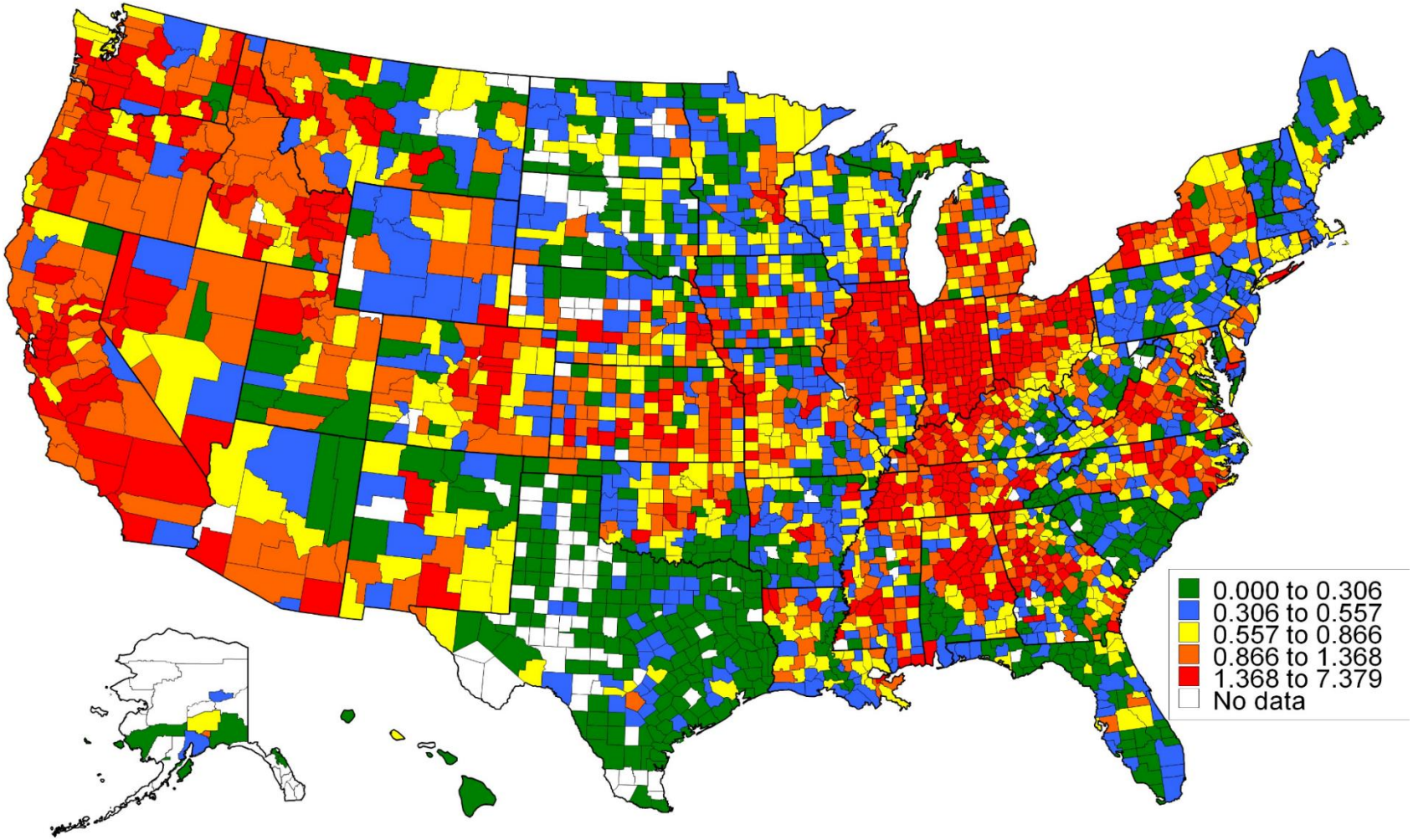


**Figure 2: Geographic distribution of total bankruptcy rates (TotBKR) in 1980**




*Notes*: The bankruptcy rate is the number of total bankruptcy filings (sum of Chapter 7 and Chapter 13) per 1,000 population. The county-level data on personal bankruptcy filings are from the Administrative Office of the U.S. Courts, and the population data are from the U.S. Census.

**Figure 3: Geographic distribution of total bankruptcy rates (TotBKR) in 1994**

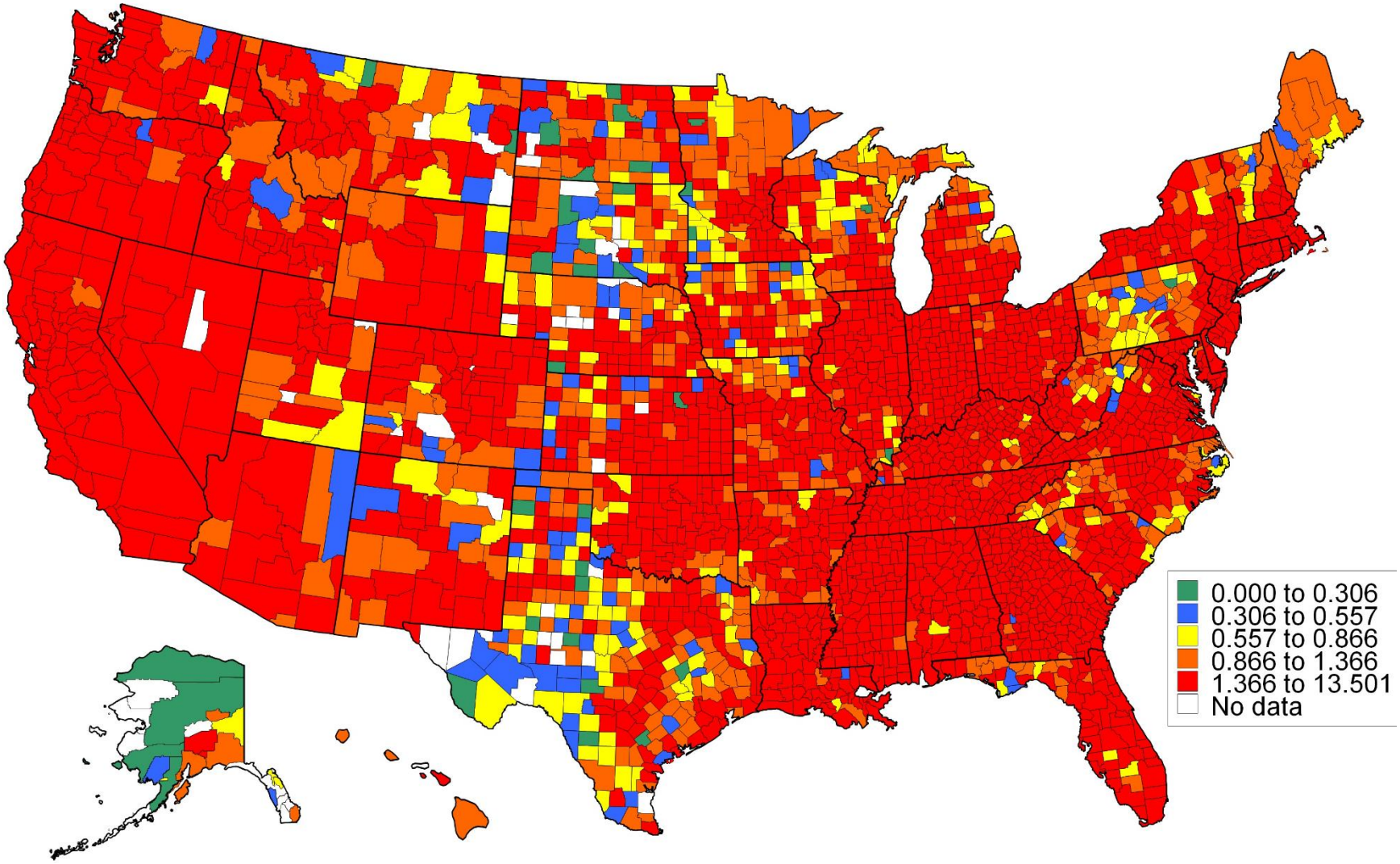
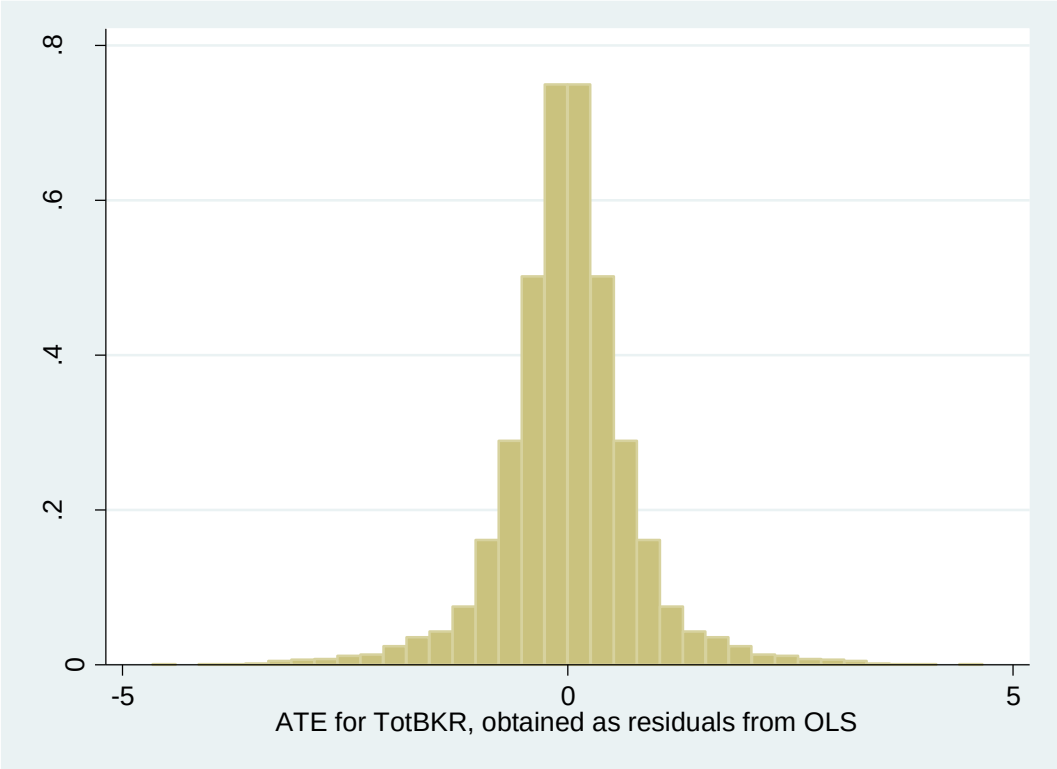


**Figure 4: Total Bankruptcy rate in states restricting versus allowing intrastate bank branching**


*Notes*: The bankruptcy rate is the number of total bankruptcy filings (sum of Chapter 7 and Chapter 13) per 1,000 population. The data on personal bankruptcy filings and population are from the Administrative Office of the U.S. Courts and the U.S. Census, respectively. The data on states restricting/allowing intrastate bank branching are from Huang (2008), Dick and Lehnert (2010), and Jayaratne and Strahan (1996).

**Figure 5: Distribution of adjusted treatment effects on placebo sample pairs for total bankruptcy rate**




Note: ATE stands for adjusted treatment effect.

## Appendix A: Summary Statistics

Panel 1: Variables used in the panel data analysis involving all counties [Table 1]

| Variables | Mean | Std. dev. | 5th Percentile | Median | 95th Percentile | N |
|---|---|---|---|---|---|---|
| TotBKR | 1.615 | 1.347 | 0.222 | 1.273 | 4.121 | 46,230 |
| Ch7BKR | 1.189 | 0.917 | 0.136 | 0.985 | 2.910 | 46,230 |
| Ch13BKR | 0.426 | 0.788 | 0.000 | 0.162 | 1.730 | 46,230 |
| Unemp (%) | 7.76 | 3.91 | 2.90 | 7.00 | 15.10 | 46,974 |
| Income (in $1,000) | 8.517 | 2.019 | 5.723 | 8.317 | 12.027 | 46,547 |
| RealIncGrwth | 0.0160 | 0.0706 | -0.0714 | 0.0156 | 0.1003 | 46,542 |
| Dummy variable | Value = 1 | | | | | N |
| IntraState | 28,536 | | | | | 47,055 |

Panel 2: Variables used in the panel data analysis involving contiguous county-pairs [Table 10]

| Variables | Mean | Std. dev. | 5th Percentile | Median | 95th Percentile | N |
|---|---|---|---|---|---|---|
| TotBKR | 1.558 | 1.376 | 0.224 | 1.206 | 4.006 | 16,995 |
| Ch7BKR | 1.116 | 0.790 | 0.145 | 0.942 | 2.676 | 16,995 |
| Ch13BKR | 0.442 | 0.917 | 0.000 | 0.136 | 1.973 | 16,995 |
| Unemp (%) | 8.02 | 3.83 | 2.90 | 7.40 | 15.20 | 17,122 |
| Income (in $1,000) | 8.219 | 1.803 | 5.612 | 8.070 | 11.221 | 17,130 |
| RealIncGrwth | 0.0163 | 0.0618 | -0.0704 | 0.0164 | 0.1023 | 17,130 |
| Pop25to44 (%) | 27.58 | 3.05 | 22.64 | 27.61 | 32.74 | 17,130 |
| Dummy variable | Value = 1 | | | | | N |
| IntraState | 10,217 | | | | | 17,130 |

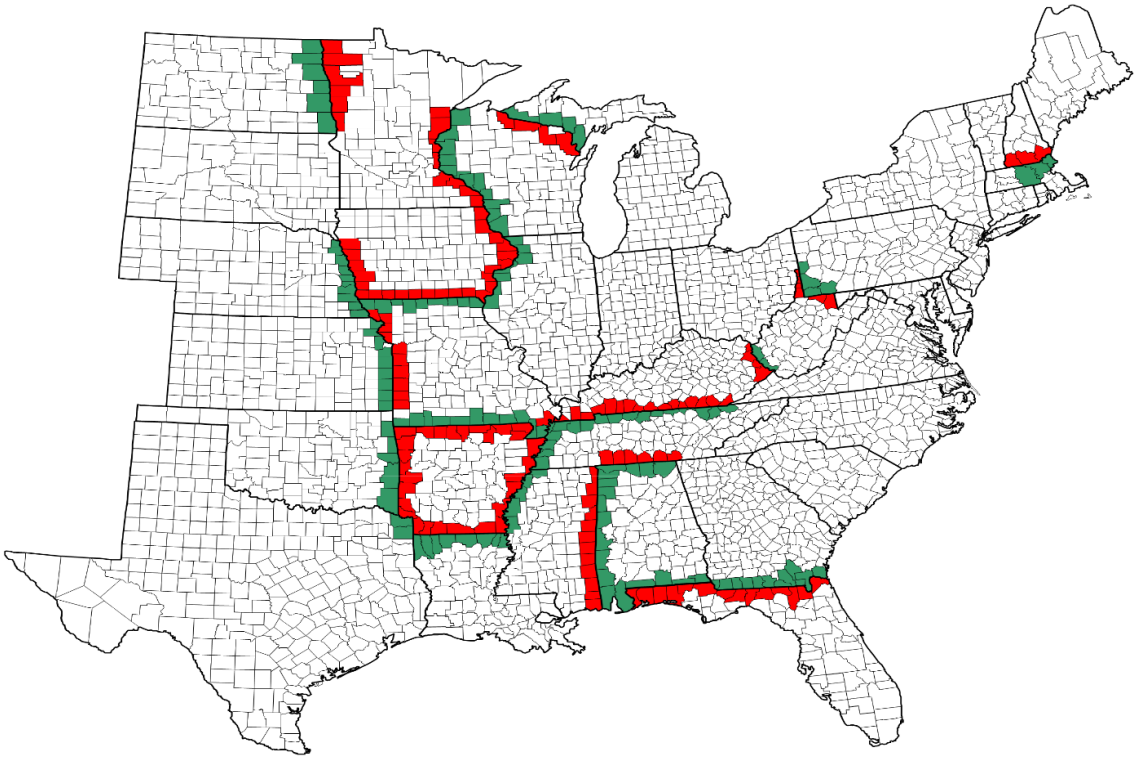
Panel 3: Variables used in the analysis replicating results of Dick and Lehnert (2010) [Table 11]

| | State level data | | | County level data | | |
|---|---|---|---|---|---|---|
| Variables | Mean | Std. dev. | N | Mean | Std. dev. | N |
| TotBKR | 2.074 | 1.304 | 720 | 1.633 | 1.352 | 45,241 |
| Ch7BKR | 1.508 | 0.872 | 720 | 1.199 | 0.920 | 45,241 |
| Ch13BKR | 0.566 | 0.682 | 720 | 0.434 | 0.794 | 45,241 |
| L1.Unemp (%) | 6.80 | 2.17 | 720 | 7.80 | 3.91 | 45,913 |
| NomIncGrwth | 0.0616 | 0.0308 | 720 | 0.0623 | 0.0750 | 45,492 |
| L1.NomIncGrwth | 0.0655 | 0.0321 | 720 | 0.0667 | 0.0789 | 45,476 |
| Dummy variable | | | | | | |
| | Value = 1 | | | Value = 1 | | |
| InterState | 409 | | 720 | 25,535 | | 46,005 |
| IntraState | 482 | | 720 | 22,562 | | 46005 |

## Appendix B: Map of 'treatment' contiguous county-pairs

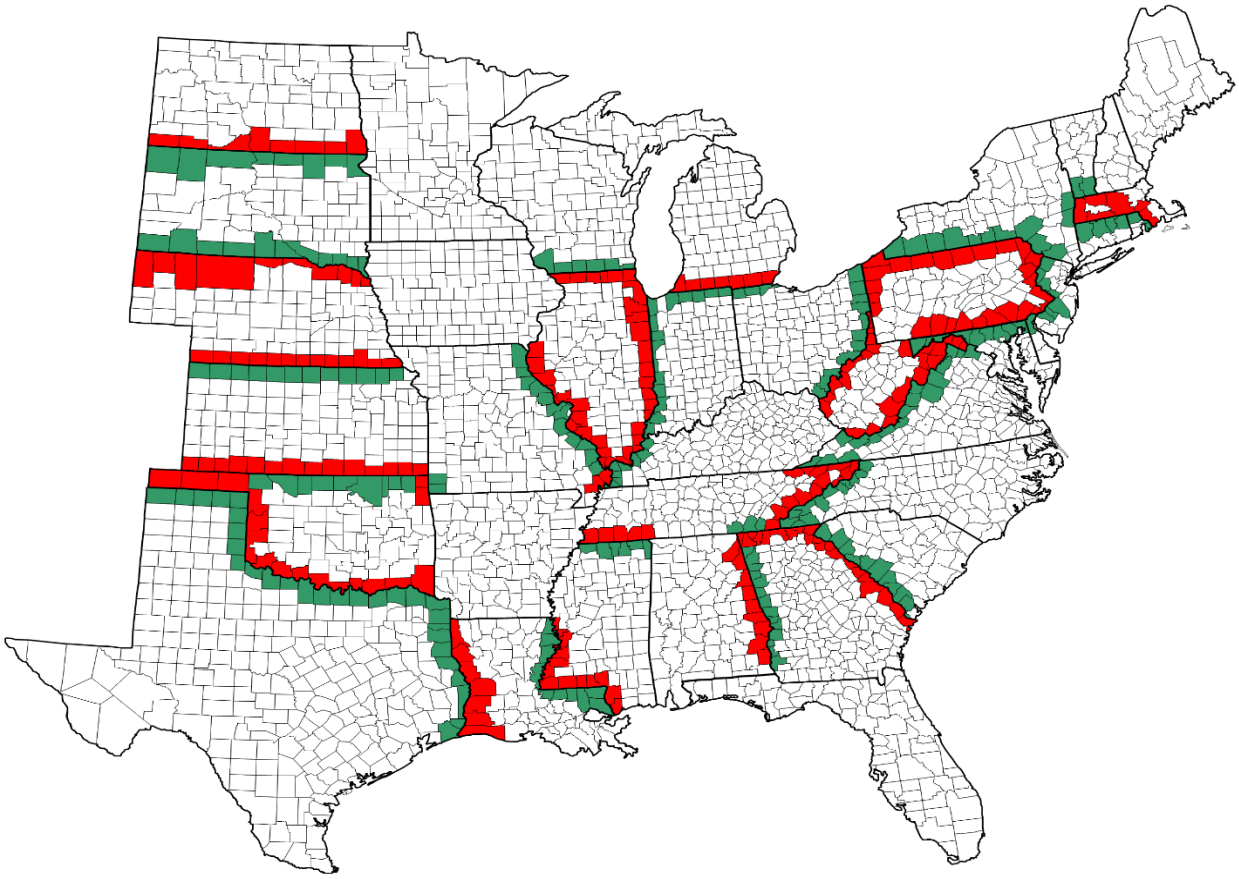
Figure B1: Example of a treatment county and its contiguous control county


Figure B2: Map of Event border and 186 treatment contiguous county-pairs



Notes: Green - Treatment counties and Red - Control counties

## Appendix C: Map of 385 placebo contiguous county-pairs



Notes: Green - Fictitious treatment counties and Red - Fictitious control counties. Both are interchanged to construct the placebo sample, making the distribution of 'fictitious' treatment effect symmetrical.

**Appendix D: Critical values table for Chapter 7 (*Ch7BKR*) and Chapter 13 (*Ch13BKR*)**

| Number of county pairs used to form the mean | For *Ch7BKR* | | | For *Ch13BKR* | | |
|---|---|---|---|---|---|---|
| | Statistical confidence level | | | Statistical confidence level | | |
| | 90% | 95% | 99% | 90% | 95% | 99% |
| 1 | 0.579 | 0.786 | 1.318 | 0.396 | 0.784 | 1.765 |
| 2 | 0.410 | 0.556 | 0.932 | 0.280 | 0.555 | 1.248 |
| 3 | 0.334 | 0.454 | 0.761 | 0.228 | 0.453 | 1.019 |
| 4 | 0.290 | 0.393 | 0.659 | 0.198 | 0.392 | 0.882 |
| 5 | 0.259 | 0.351 | 0.590 | 0.177 | 0.351 | 0.789 |
| 6 | 0.236 | 0.321 | 0.538 | 0.162 | 0.320 | 0.721 |
| 7 | 0.219 | 0.297 | 0.498 | 0.150 | 0.297 | 0.667 |
| 8 | 0.205 | 0.278 | 0.466 | 0.140 | 0.277 | 0.624 |
| 9 | 0.193 | 0.262 | 0.439 | 0.132 | 0.261 | 0.588 |
| 10 | 0.183 | 0.248 | 0.417 | 0.125 | 0.248 | 0.558 |
| 11 | 0.175 | 0.237 | 0.398 | 0.119 | 0.237 | 0.532 |
| 12 | 0.167 | 0.227 | 0.381 | 0.114 | 0.226 | 0.509 |
| 13 | 0.161 | 0.218 | 0.366 | 0.110 | 0.218 | 0.490 |
| 14 | 0.155 | 0.210 | 0.352 | 0.106 | 0.210 | 0.472 |
| 15 | 0.150 | 0.203 | 0.340 | 0.102 | 0.203 | 0.456 |
| 16 | 0.145 | 0.196 | 0.330 | 0.099 | 0.196 | 0.441 |
| 17 | 0.140 | 0.191 | 0.320 | 0.096 | 0.190 | 0.428 |
| 18 | 0.137 | 0.185 | 0.311 | 0.093 | 0.185 | 0.416 |
| 19 | 0.133 | 0.180 | 0.302 | 0.091 | 0.180 | 0.405 |
| 20 | 0.130 | 0.176 | 0.295 | 0.088 | 0.175 | 0.395 |
| 21 | 0.126 | 0.171 | 0.288 | 0.086 | 0.171 | 0.385 |
| 22 | 0.123 | 0.168 | 0.281 | 0.084 | 0.167 | 0.376 |
| 23 | 0.121 | 0.164 | 0.275 | 0.083 | 0.164 | 0.368 |
| 24 | 0.118 | 0.160 | 0.269 | 0.081 | 0.160 | 0.360 |
| 25 | 0.116 | 0.157 | 0.264 | 0.079 | 0.157 | 0.353 |
| 26 | 0.114 | 0.154 | 0.259 | 0.078 | 0.154 | 0.346 |
| 27 | 0.111 | 0.151 | 0.254 | 0.076 | 0.151 | 0.340 |
| 28 | 0.109 | 0.148 | 0.249 | 0.075 | 0.148 | 0.334 |
| 29 | 0.108 | 0.146 | 0.245 | 0.073 | 0.146 | 0.328 |
| 30 | 0.106 | 0.143 | 0.241 | 0.072 | 0.143 | 0.322 |
| 50 | 0.082 | 0.111 | 0.186 | 0.056 | 0.111 | 0.250 |
| 70 | 0.069 | 0.094 | 0.158 | 0.047 | 0.094 | 0.211 |
| 100 | 0.058 | 0.079 | 0.132 | 0.040 | 0.078 | 0.176 |
| 186 | 0.042 | 0.058 | 0.097 | 0.029 | 0.058 | 0.129 |
| 200 | 0.041 | 0.056 | 0.093 | 0.028 | 0.055 | 0.125 |