Internet Appendix of
Banking regulatory constraints and personal bankruptcy filings

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Internet Appendix A: Results of replication exercise

In Internet Appendix Table A1, we report the results of our replication exercise for Table III on page 668 of Dick and Lehnert (2010).

Internet Appendix B: Non-parametric randomization test based on counterfactuals

A. Identifying the non-event border-segments

Internet Appendix Table B1 reports the non-event border segments and their number of contiguous county-pairs. A non-event border-segment is not part of treatment sample and is not a border of Montana, Wyoming, Colorado, and New Mexico. As an example, Alabama is one of the seventeen treatment states and its branch deregulation year is 1981. In the treatment sample (see Table 6 of the paper), the borders of Alabama with Florida, Mississippi, and Tennessee are already considered. However, Alabama's border with Georgia was excluded because Georgia removed intrastate bank branching restrictions in 1983, which was less than two years from the event year of Alabama. In the case of the non-event border segment Pennsylvania-Maryland, the branch deregulation year for Pennsylvania is 1982. However, Maryland removed restrictions on bank branching before 1975. All twelve contiguous county-pairs on this border segment are eligible for the placebo test. Altogether, 385 contiguous county-pairs are identified from 34 non-event border segments. Internet Appendix Figure B1 shows the non-event border segments and their contiguous county-pairs on a US map excluding the West.

B. Construction of placebo sample of counterfactuals

The potential fictitious event years are for the period 1981-1992. The main reasons for selecting this period are (1) the branch deregulation period is 1980-1994, 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 the pre-event period, fictitious event year, and post-event period, does not overlap with the actual event windows of the treatment and control states. For example, considering the non-event border segment Alabama-Georgia, the years 1981 and 1983 are ineligible as fictitious event years because both are actual treatment years for Alabama and Georgia, respectively. The year 1985 is disqualified because it is part of the actual post-event period of Georgia. The year 1986 is disqualified because its prior period (i.e., 1985) is part of the post-event period for Georgia. The years 1987-1992 are qualified for the fictitious event years, with only differences in the length of the pre- and post- "fictitious" event periods. For the fictitious event year 1987, the pre- and post- period lengths are one and three years, respectively. For the fictitious event years 1988 and 1989, both post-periods are three years in length, however, the pre-periods 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 sample period ending in 1994.

We identify 1,807 fictitious event year and contiguous county-pair combinations. They are part of one of the three distinct categories, depending on intrastate bank branching restrictions during the fictitious event window. In the first category, both the counties of a contiguous county-pair have restrictions on bank branching during the pre- and post-periods of the fictitious event year. As an example, consider the non-event border segment of Nebraska-Kansas and the fictitious event year is 1981. During 1980-1983 (fictitious event window), both states have entry barriers on intrastate bank branching. In the first category, there are 559 counterfactuals. In the second category, both counties of a contiguous county-pair allow intrastate bank branching during the fictitious event window. The fictitious event year of 1992 for the Alabama-Florida border segment

is an example of this category. During the fictitious event window of 1991-1994, both the states permit opening of a bank branch within a state. In this category, 1,094 counterfactuals are identified. Finally, in the third category, one county of a pair restricts while the other permits the bank branching during the fictitious event window. A border segment of West Virginia-Virginia for the fictitious event year 1983 is an example of this category. West Virginia removed restrictions in 1987 and Virginia removed them in 1978. During the fictitious event window 1981-1985 surrounding fictitious event year 1983, West Virginia has regulations in place, whereas Virginia allows intrastate bank branching. From the third category, 154 counterfactuals are obtained.[1,2]

C. Parallel trend assumption for the placebo sample

For each of the 1,807 fictitious event year - contiguous county-pair combinations, pre-period average bankruptcy rates for Chapter 13 are computed by taking the average of the bankruptcy rate in the one, two, and three years prior to the fictitious event year. Similarly, the average values for the post-period is computed using bankruptcy rates for the one, two, and three years after the fictitious event year. This is done for both counties of a contiguous-county pair. Out of 1,807 pairs, data are missing in 42 pairs. Therefore, the placebo sample is of 1,765 counterfactuals. Internet Appendix Figure B2 plots the trends in Chapter 13 bankruptcy filings relative to the fictitious event year for the placebo sample. There is a time trend in bankruptcy filings. However, there are no notable change Chapter 13 filings after the fictitious event-year. The

[1] While the empirical methodology of the non-parametric approach is largely based on Huang (2008), the approach differs in the identification of a combination of fictitious event-year and county-pair for the counterfactual sample. Huang (2008) constructs placebo combinations based on the 266 contiguous 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 this paper, we 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.

[2] In untabulated results, 628 counterfactuals are also selected from the treatment sample. The results are similar.

trends for Chapter 13 filings are parallel during the fictitious event window [-3, +3] for placebo treatment and placebo control samples.

D. Placebo [Fictitious] treatment effect and its distribution

For each of 1,765 counterfactuals, the fictitious treatment effect is the difference-in-differences of the bankruptcy rate. It is the difference of the change in bankruptcy rate of a county and that of its contiguous county. The change is the difference in the bankruptcy rate during the post-period. The fictitious event year, here also, is not part of a pre- or post-period. Both counties of a contiguous county-pair are of the placebo sample. Therefore, they both can be interchanged for computing the fictitious treatment effect. This results in a placebo sample of 1765 x 2 = 3,530 fictitious treatment effects.

Internet Appendix Figure B3 plots the frequency distribution of the [fictitious] treatment effect of the counterfactuals. By construction, it is symmetrical with a mean of zero. The distribution of the placebo treatment effect of Chapter 13 filings is centered to zero. This was expected based on the parallel trend in Chapter 13 filings for the placebo sample. The 90^{th} percentile, 95^{th} percentile, and 99^{th} percentile of Chapter 13 bankruptcy rate are 0.451, 0.912, and 1.878, respectively. The number 0.912, for example, informs that out of 100 random experiments using Chapter 13 bankruptcy data of a typical contiguous county-pair, in five cases we may observe the effect as large as 0.912. It also indicates that for a sample size of one contiguous county-pair, the actual treatment effect for Chapter 13 bankruptcy needs to be larger than 0.912, then only we can say that the effect is significant at the 5 percent level. While constructing the treatment effect distribution of counterfactuals, the assumption is that the contiguous county-pairs are independent of each other. Therefore, for a sample size of N contiguous county-pairs, we need to reduce the

90^{th}, 95^{th}, and 99^{th} percentile cut-offs by dividing them by $\sqrt{N}$. As an example, in the case of Texas, we have two contiguous county-pairs; therefore, the actual treatment effect for the Chapter 13 bankruptcy rate needs to be above $0.645 = \frac{0.912}{\sqrt{2}}$, to be considered significant at the 5 percent level and free from data mining and data snooping biases.

References:

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

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

Internet Appendix Table A1: Results of replication exercise of Dick and Lehnert (2010)'s Table III

	Dependent variable: Chapter 7 bankruptcy filings per 1,000 persons							
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)
Interstate banking indicator	0.160	0.162	0.141	0.185	0.065	0.072	0.046	0.096
	(0.058)***	(0.057)***	(0.055)**	(0.061)***	(0.062)	(0.062)	(0.058)	(0.062)
Intrastate branching indicator	0.029	0.053	0.038	0.095	0.041	0.069	0.048	0.117
	(0.073)	(0.069)	(0.070)	(0.076)	(0.077)	(0.075)	(0.074)	(0.079)
Personal income growth	-1.766	-1.608	-1.525	-1.449	-2.511	-2.431	-2.319	-2.627
	(0.527)***	(0.534)***	(0.492)***	(0.528)***	(0.611)***	(0.628)***	(0.598)***	(0.724)***
Personal income growth (t-1)	-1.487	-1.422	-1.433	-1.061	-4.096	-4.031	-4.019	-3.652
	(0.573)**	(0.574)**	(0.586)**	(0.527)*	(1.231)***	(1.239)***	(1.263)***	(1.116)***
Unemployment rate (%)	0.141	0.140	0.139	0.145				
	(0.026)***	(0.025)***	(0.026)***	(0.027)***				
Number of top state banks		0.003				0.004		
		(0.002)				(0.002)*		
HHI			-1.717				-2.184	
			(0.820)**				(0.803)***	
Deposit share of small banks				1.054				1.333
				(0.482)**				(0.569)**
Intercept	0.453	0.404	0.583	0.672	1.558	1.481	1.703	1.751
	(0.201)**	(0.203)*	(0.219)**	(0.245)***	(0.177)***	(0.192)***	(0.191)***	(0.161)***
Observations	720	706	706	672	720	706	706	672
R-squared (within)	0.84	0.84	0.85	0.85	0.80	0.80	0.80	0.80
R-squared (overall)	0.45	0.46	0.41	0.45	0.44	0.44	0.38	0.40

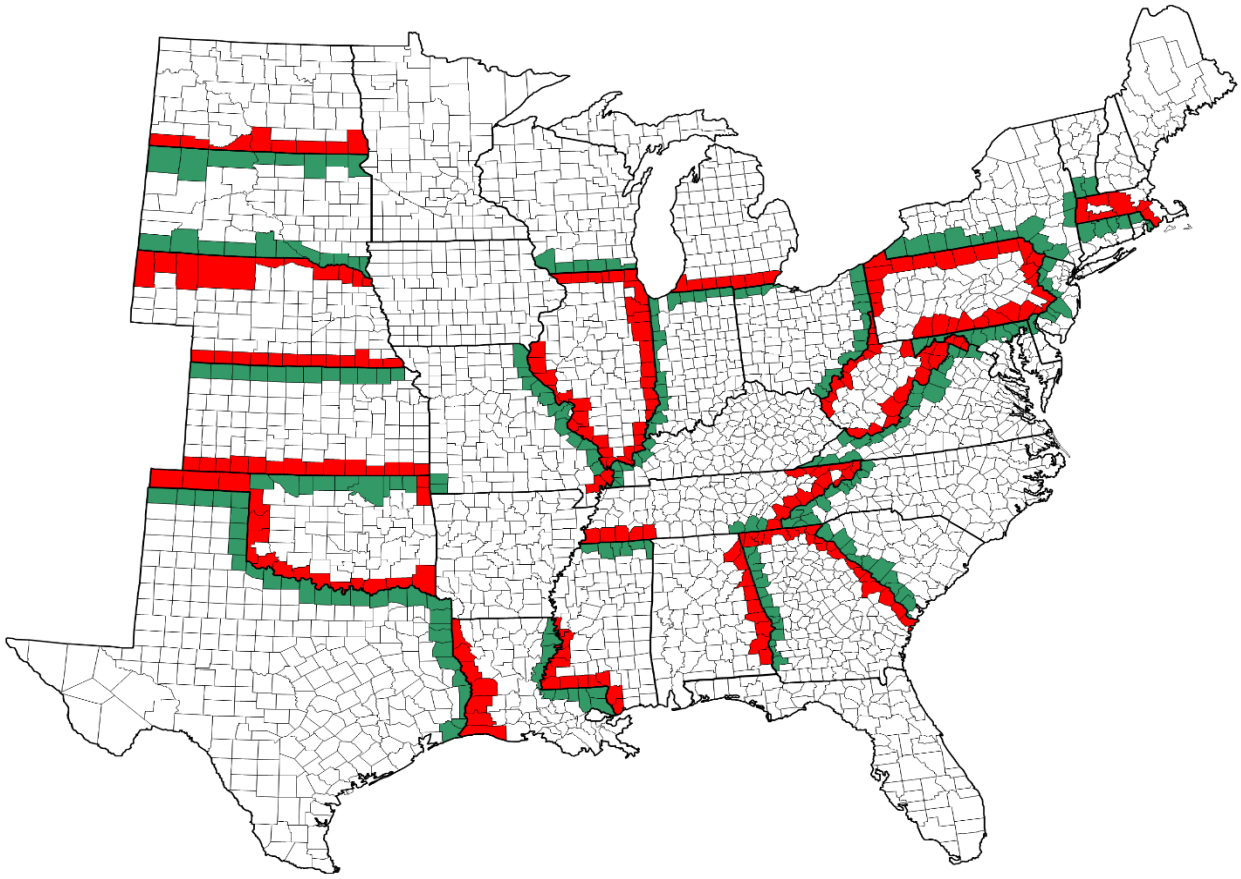
Notes: This table reports the results of our replication exercise to verify the effect of interstate banking on Chapter 7 filing rate, as reported in Dick and Lehnert (2010). The specifications (i), (ii), (iii), and (iv) of this table correspond with specifications (i), (ii), (iv), and (vi) of Table III of Dick and Lehnert (2010). The specifications (v) to (viii) of this table are similar to (i) to (iv), except that we exclude unemployment rate. HHI is the abbreviation for Herfindahl-Hirschman Index. Each regression controls for time effects and state-specific fixed effects. The robust standard errors clustered at the state level are in parentheses below the coefficients. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Internet Appendix Table B1: Sample description of fictitious event (out-of-sample) group

Non - Event border (Treatment State - Neighboring Control State)	Branching 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

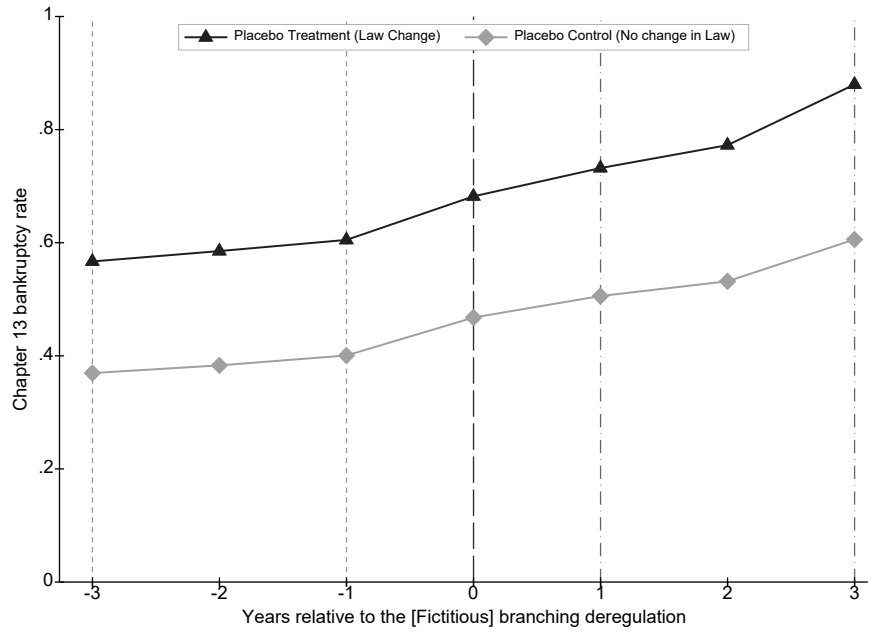
Note: The actual branching deregulation year of the treatment state is provided in Table 6 of the paper.

Internet Appendix Figure B1: Map of 385 contiguous county-pairs of the placebo sample



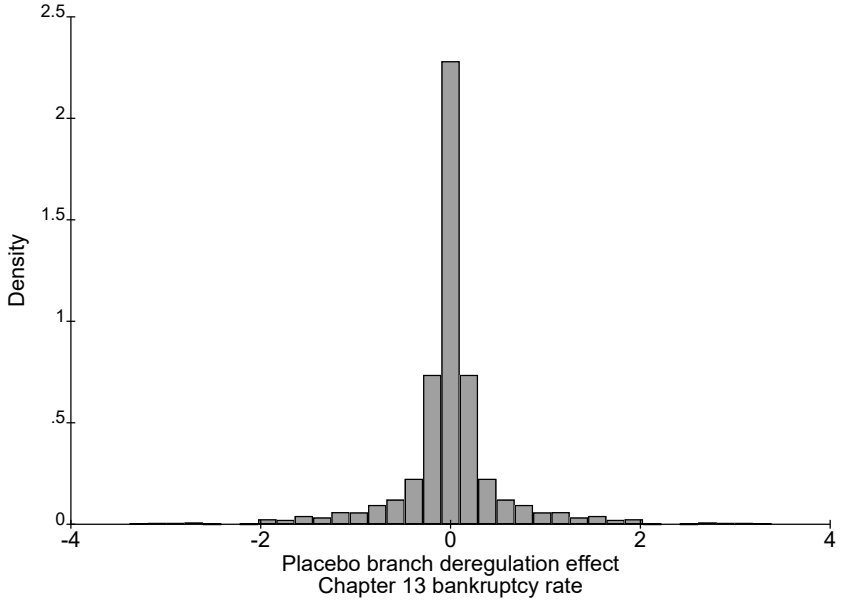
Notes: The figure is based on 1,807 county-year combinations of 385 contiguous county-pairs of the placebo sample. Year 0 represents the fictitious event year.

Internet Appendix Figure B2: Trend in Chapter 13 rate for the placebo sample of contiguous county-pairs



Notes: The figure is based on 1,807 county-year combinations of 385 contiguous county-pairs of the placebo sample. Year 0 represents the fictitious event year. The short-dash lines are for the pre-period, the long-dash line is for the event (branch deregulation) year, and the dash-dot lines are for the post-period.

Internet Appendix Figure B3: Empirical distribution of fictitious treatment effects



Note: The empirical distributions are based on 1,765 x 2 = 3,530 counterfactuals (fictitious event year - contiguous county pair) of the placebo sample.