



Can bank branching deregulation affect innovation? ☆

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A B S T R A C T

We exploit the deregulation of interstate bank branching laws to test whether banking competition affects innovation. Using robust evidence from banking competition, we find that deregulation reduces state-level innovation by public corporations headquartered within deregulated states. Innovation increases among private firms that are dependent on external financing and that have limited access to credit from local banks. We argue that banking competition enables small innovative firms to secure financing instead of being crowded out by public corporations. Therefore, banking competition reduces the supply of public targets, which reduces the portion of state-level innovation attributable to public corporations. Overall, these results shed light on the relationship between banking competition and the determinants of innovation.

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Can bank branching deregulation affect innovation? Underlying the determination of innovation is important to the innovation process.

establish companies and are important for economic growth (Porter, 1992; Solow, 1957). A growing body of literature has examined the relationship between innovation and various factors. However, this literature has not empirically examined the relationship between banking competition and innovation. To the best of our knowledge, the literature by Porter (1992) and Solow (1957) is the only one that examines the relationship between banking competition and innovation.

A major contribution of the literature on innovation is the finding that banking competition is negatively related to innovation. However, little empirical evidence has been provided to support this claim. The causal relationship between banking competition and innovation is still unclear. The deregulation of interstate bank branching laws provides a natural setting to examine the relationship between banking competition and innovation.

United States. The U.S. Congress passed the Interstate Banking and Branching Efficiency Act (IBBEA) in 1994. In addition to allowing unrestricted interstate banking, the IBBEA legalized interstate branching across the U.S. starting in 1997. As [Rice and Strahan \(2010, p. 861\)](#) explain, “Allowing interstate branching was the watershed event of IBBEA.” Rice and Strahan show that, when states relax bank branching restrictions, more bank branches open and compete with one another. This increase in competition expands the availability of credit within a state and lowers the cost of capital therein.

We construct tests using these deregulatory events as plausibly exogenous increases in the supply of state-level finance. Given the economic effects documented by [Rice and Strahan \(2010\)](#), we expect state-level innovation to increase following deregulation because companies headquartered within deregulating states could take advantage of the greater supply of finance to increase innovation output. Surprisingly, however, we find robust evidence that increases in banking competition cause states' innovation outputs to decline. We find that states that are completely open to interstate branching generate a total of 30.8% fewer patents (i.e., 920 fewer patents) three years after branching deregulation than states with the most restrictions on interstate branching. We find a similar result when we use patent citations as a proxy for innovation: States that are completely open to interstate branching generate patents that receive a total of 23.2% fewer citations (i.e., 9,068 fewer citations) three years after branching deregulation than states with the most restrictions on interstate branching. These results are robust to controlling for state-level and state-industry-level labor force concentration, banking deregulatory events that precede IBBEA, state fixed effects, and year fixed effects.

To gain a clearer understanding of this result, we decompose state-level patents into patents produced by public corporations and private firms.¹ Private firms could be more sensitive to local banking conditions than public corporations, so the effects of state-level banking competition could be different for these two groups. Indeed, we find the overall negative effect of deregulatory events on state-level innovation is driven by corporations headquartered within deregulating states. In contrast, relative to corporations, private firms experience increases in innovation output following deregulatory events. (We find no direct effect of deregulation on private firms' innovation outputs.) These findings support the notion that small, private firms take advantage of the improved credit conditions to finance innovative projects.

Although the staggered deregulation of interstate bank branching laws provide exogenous changes to banking competition, [Kroszner and Strahan \(1999\)](#) argue that state-level factors that manifest differently across states could have affected the timing of deregulation in different states. Therefore, it is possible that our results are driven

by reverse causality, whereby differences in innovation intensities across states triggered deregulation. We employ the methodology of [Bertrand and Mullainathan \(2003\)](#) to address this concern. We examine the dynamics of innovation surrounding the deregulatory events and we find no prior trend in innovation output. This finding indicates reverse causality does not explain our main results.

Another potential explanation for our results is that an omitted variable coinciding with branching deregulation could be the true underlying cause of changes in innovation. If this is the case, then the changes in innovation we attribute to branching deregulation reflect mere associations rather than a causal effect. Our baseline identification strategy employs shocks that affect different states at different times. It is unlikely that an omitted variable unrelated to branching deregulation would fluctuate every time (or even most of the time) a deregulatory event occurs. Therefore, our strategy of using multiple shocks due to staggered banking deregulation across states mitigates the omitted variables concern.

Still, we address this possibility by conducting placebo tests. We begin by obtaining an empirical distribution of years when states deregulated from [Rice and Strahan \(2010\)](#). Next, we randomly assign states into each of these deregulation years (without replacement) following the empirical distribution. This approach maintains the distribution of deregulatory years from our baseline specification, but it disrupts the proper assignment of deregulation years to states. Therefore, if an unobservable shock occurs at approximately the same time as the deregulation events in the mid-1990s, it should still reside in the testing framework, and thus have an opportunity to drive the results. However, if no such shock exists, then our incorrect assignments of deregulatory years to states should weaken our results when we re-estimate the baseline tests. Indeed, we find these falsely assumed deregulatory events have no effect on innovation. These non-results corroborate the notion that the paper's main results are not driven by an omitted variable.

After demonstrating that there is an aggregate decrease in patents and patent citations following increased banking competition from the IBBEA, we examine three possible channels to explain this result. First, we test whether companies' external finance dependence affects the way their innovation outputs respond to changes in state-level banking competition. We expect that banking competition relaxes financing constraints for private firms, mainly in external-finance-dependent industries. Therefore, these private firms should experience increases in innovation output. This is precisely what we find. We use the measure of external finance dependence developed by [Duchin, Ozbas, and Sensoy \(2010\)](#) and find external-finance-dependent private firms located in states that are completely open to interstate branching generate a total of 7.6% more patents and 6.4% more citations three years after branching deregulation than firms in states with the most restrictions on interstate branching. This result is robust to a variety of alternative proxies for external finance dependence. We partition the data by company size, age, and bank dependence following [Acharya, Imbs,](#)

¹ To aid in distinction, we use the word “corporation” throughout the remainder of the paper to designate Compustat-reported entities. We reserve the word “firm” for private firms whose stocks are not listed on stock exchanges. We use the word “company” as a general term for either public corporations or private firms.

and Sturgess (2011) and by the size-age (SA) index following Hadlock and Pierce (2010), and we observe qualitatively similar results. In contrast, we observe no effect or a negative effect of banking deregulation on private firms with below-median dependence on external finance.

Second, companies' banking relationships prior to deregulatory events provide another way to test how the need for external finance interacts with changes in banking competition. As banking competition increases, we expect the innovation output of companies with existing loans from in-state banks to react differently compared to companies with loans from out-of-state banks. We hypothesize that in-state banking relationships are evidence that companies are able to satisfy their demand for external finance from nearby banks. However, if companies borrow from out-of-state banks, it indicates that they are unable to satisfy their demand for finance from nearby banks. If banking competition expands access to finance, and if companies use it to finance innovative projects, then the innovation output of the latter group of companies should increase following increases in banking competition. Indeed, we observe an increase in the innovation output of private firms with high pre-existing out-of-state bank loans after deregulatory events and no change in the innovation output of public corporations and private firms with prior in-state banking relationships. Like the external finance dependence results, these results provide evidence that companies that are most likely to benefit from expanded access to bank finance take advantage of state-level banking competition to improve their innovative output.

Finally, we test a mergers and acquisitions-based explanation for the negative effect of branching deregulation on corporate innovation. Because proximal banking conditions matter for small firms (Petersen and Rajan, 2002), changes in state-level banking competition could alter the market for target firms in corporations' headquarter states.² Given founder-managers' private benefits of control, the owners of small firms prefer to secure financing while giving away as little control as possible to the financiers (Bolton and Von Thadden, 1998). On the other hand, Erel, Jang, and Weisbach (forthcoming) find that M&As significantly ease financial constraints of target firms and Sevilir and Tian (2012) show that a significant portion of corporations' innovation output derives from acquisitions of innovative targets. Together, these papers suggest that increases in state-level banking competition could allow small, innovative firms to secure bank financing to fund innovative projects and remain independent instead of being acquired by corporations. This effect could lead to a dearth of willing targets which would generate a reduction in corporate innovation.

Consistent with our conjecture that corporations' abilities to acquire small, innovative target firms are impaired after bank branching deregulation, we find the overall negative effects of banking competition on innovation are

particularly strong among corporations that are frequent acquirers and have high M&A expenditures before deregulatory events. Further, for a given corporation, we find the average innovativeness of the targets it acquires declines after banking competition increases in its headquarter state. This result indicates that the pool of potential targets within a state contains less innovative firms after deregulation. Finally, we find the ratio of target firms that produce at least one patent in a year to total private firms located within a state declines after banking competition increases. All of these results suggest that a reduction in the supply of innovative targets is a possible mechanism that helps explain the overall negative relation between state-level banking competition and corporate innovation. Further, the finding that target firms seek bank financing after deregulation supports the view that banking was repressed in the United States until branching deregulation was enacted.

The rest of the paper proceeds as follows. Section 2 describes how this paper relates to existing literature. Section 3 describes the data and variable construction. Section 4 presents the baseline results and endogeneity tests. Section 5 discusses the underlying mechanisms of our baseline results and Section 6 concludes.

2. Relation to existing literature

Our paper contributes to two strands of literature. First, our paper is related to the literature that examines the real effects of banking deregulation. This literature begins with Jayaratne and Strahan (1996), who show intrastate branching deregulation significantly increases the rates of real per capita growth in income and output. Following this study, a large literature has examined various consequences of the *intrastate branching* and *interstate banking* deregulation events that occurred in the U.S. in the 1970s and 1980s. These studies find that deregulation spurs entrepreneurship (Black and Strahan, 2002), makes state business cycles smaller and more alike (Morgan, Rime, and Strahan, 2004), allows firm entry and access to bank credit (Cetorelli and Strahan, 2006), promotes creative destruction (Kerr and Nanda, 2009), and increases personal bankruptcy rates (Dick and Lehnert, 2010). Rice and Strahan (2010) show that the *interstate branching* deregulation that occurred in the U.S. in the mid-1990s expands credit supply by reducing the cost of credit but has no effect on the amount borrowed by small firms. We examine the effects of the same deregulatory event as Rice and Strahan (2010). We advance this line of inquiry by showing that the reduced cost of credit allows private, external-finance-dependent firms to secure bank financing to fund innovative projects.

corporate governance (Meulbroek, Litchell, Mulherin, Netter, and Poulsen, 1990), stock liquidity (Fang, Tian, and Tice, 2014), firm boundaries (Seru, 2014), analyst coverage (He and Tian, 2013), and institutional ownership (Aghion, Van Reenen, and Zingales, 2013). Few empirical studies examine the link between capital market development and firm innovation. Benfratello, Schiantarelli, and Sembenelli (2008) show that local banking development increases the “process innovation” (but not necessarily the “product innovation”) of Italian manufacturing firms. Hsu, Tian, and Xu (2014) find that industries that are more dependent on external finance and that are more high-tech intensive exhibit a higher innovation level in countries with better developed equity markets, but the development of credit markets appears to discourage innovation in industries with these characteristics, using a sample of 32 developed and emerging countries.

Our study is related to three contemporaneous papers. Amore, Schneider, and Zaldokas (2013) find that interstate banking deregulation in the 1980s has a positive effect on the innovative performance of public corporations. Chava, Oettl, Subramanian, and Subramanian (2013) show contrasting effects of intrastate branching and interstate banking deregulation on innovation by private firms. These authors find that interstate banking deregulation increases innovation by young, private firms but intrastate branching deregulation decreases innovation by these firms. Hombert and Matray (2013) examine the same deregulatory events as the above two papers and find the number of innovators decreases after these two deregulatory events. Unlike these three studies that examine the effects of deregulatory events that occurred in the 1970s and 1980s, we focus on the effects of interstate branching deregulation which occurred in the mid-1990s. We find that interstate branching deregulation caused a decrease in the innovation output of corporations, but an increase in the innovation output of external-finance-dependent private firms. We also show that declined acquisition of small, innovative firms by public corporations is an underlying mechanism that drives the reduction in corporate innovation post-deregulation.

3. Sample selection and summary statistics

3.1. Sample selection

The sample includes U.S.-listed corporations and private firms during the period of 1976 to 2006. We compile our data set from several databases. We collect annual patent and citation information from the latest version of the National Bureau of Economic Research (NBER) Patent Citation database. We obtain merger and acquisition (M&A) information from the Securities Data Company (SDC) M&A database, bank loan data from the Thomson Reuters DealScan database, and the number of private non-farm businesses from the U.S. Census Bureau. To construct corporation-level control variables, we collect financial statement items from Compustat Industrial Annual Files.

3.2. Variable measurement

3.2.1. Measuring innovation

We construct the innovation variables from the latest version of the NBER Patent Citation database initially created by Hall, Jaffe, and Trajtenberg (2001). The database provides detailed information on more than three million patents granted by the United States patent and trademark office (USPTO) from 1976 to 2006. The patent database provides annual information on patent assignee (owner)

To gauge public corporations' innovation productivity, we merge the NBER patent data with the Compustat firm sample using a bridge file provided by the NBER database in which GVKEY is the common identifier. For cases in which the corporate headquarters is different from the assignee state, we use the headquarter state of the corporation. To measure private firms' innovation output, we classify a firm as private if it does not have a GVKEY and therefore cannot be matched with the Compustat database. We cross check this information with the United States Patent and Trademark Office (USPTO) (available at <http://www.uspto.gov/>) and exclude assignees that are either governments, universities, or individuals. Following the innovation literature, we set the number of patents to zero for companies that have no patent information available from the NBER database.

In the baseline analysis, we aggregate patents generated by corporations and firms to the state level and our tests are based on state-year observations. In later tests in which we examine the economic mechanisms underlying the baseline analysis, we conduct tests at the company level. The distribution of patent grants in the sample is right-skewed. Therefore, we use the natural logarithm of the total number of weight-factor adjusted patent counts and the natural logarithm of the citation-lag adjusted citations for patents generated in the subsequent three years, *LnPat* and *LnCite*, as the main innovation measures in our analysis. To avoid losing observations with zero patents or zero citations, we add one to the actual values when calculating the natural logarithm.

3.2.2. Measuring banking competition and control variables

We follow Rice and Strahan (2010) to construct an index of interstate branching restrictions, *RSindex*. As described in Rice and Strahan (2010), the Interstate Banking and Branching Efficiency Act (IBBEA) allowed states to employ a variety of means to erect out-of-state entry barriers from the time of enactment in 1994 until the branching trigger date of June 1, 1997. Specifically, states could set regulations on interstate branching with regard to four provisions: (i), the minimum age of the target institution; (ii), *de novo* interstate branching; (iii), the acquisition of individual branches; and (iv) a statewide deposit cap. When a state adds any of the above four barriers, we add one to the *RSindex*. Therefore, the *RSindex* ranges from zero to four with zero indicating the most open stance toward interstate entry and four indicating the most restrictive stance toward interstate entry. See Rice and Strahan (2010) for a detailed discussion on the institutional background and construction of the index.

In our baseline state-level analysis, we follow Morgan, Rime, and Strahan (2004) and compute the state-level labor force composition for eight different industry segments: Mining, Construction, Manufacturing, Transportation, Trade, Finance, Service, and Government. We include these measures in the baseline analysis. We also control for state-level labor force concentration and nominal gross product. To control for the effects of other banking deregulatory events which occurred early in our sample period (i.e., the 1970s and 1980s), we construct two dummy variables: *Intra* that equals zero the years before the focal

state pursued intrastate branching deregulation and one otherwise (Jayaratne and Strahan, 1996), and *Inter* that equals zero the years before the focal state pursued interstate banking deregulation and one otherwise (Black and Strahan, 2002).

We conduct tests with corporation-year level data later in the paper to better understand the mechanisms underlying our baseline findings. In this analysis, we control for a vector of corporation and industry characteristics that may affect a corporation's future innovation output. Following the innovation literature, we compute all variables for corporation *i* over its fiscal year *t*. Control variables include investments in intangible assets (research and development expenditures divided by total assets), profitability (return on assets), asset tangibility (net property plant and equipment divided by total assets), leverage, capital expenditures (capital expenditures divided by total assets), industry concentration (Herfindahl index based on sales), growth opportunities (Tobin's *Q*), corporation size (the natural logarithm of book value assets), and corporation age. To mitigate nonlinear effects of product market competition on innovation output (AghionBloom, Blundell, Griffith, and Howitt, 2005), we include the squared Herfindahl index in our baseline regressions. To minimize the effect of outliers, we winsorize all variables that have the potential to be unbounded at the 1st and 99th percentiles of their empirical distributions.

3.3. Summary statistics

Table 1 provides summary statistics of the variables used in this study. Panel A provides information in the aggregate state-year level. On average, a state in our sample has 2,988 granted patents in the subsequent three years and these patents receive a total of 39,085 citations. Among them, 1,962 patents are generated by public corporations and 1,026 patents are produced by private firms. The average value of *RSindex* is 3.5 in the pooled sample. In Panels B and C, we separately report descriptive statistics for public corporations and private firms, respectively. As reported in Panel B, at the corporation-year level, an average corporation generates five patents and 58 citations over three years, has an external finance dependence (*EFD*) value of -0.27 , has an R&D-to-assets ratio of 4%, ROA of 2%, PPE-to-assets ratio of 32%, leverage of 31%, capital expenditure-to-assets ratio of 7%, Tobin's *Q* of 2.1, book value assets of \$1.02 billion, and is 20 years old since its founding date. Panel C reports summary statistics for private firms. At the firm-year level, an average firm generates 0.7 patent and seven citations per year, has an *EFD* value of -0.31 , borrows \$0.04 million from in-state lenders, and borrows \$0.36 million from out-of-state lenders.

Fig. 1 displays the time series of the total number of patents in the aggregate state-level produced by all companies across all states, by public corporations only, and by private firms only. We show that the aggregate level of patents has steadily increased since 1976. Although public corporations always generate more patents than private firms, the fraction of patents produced by public corporations appears to decline relative to those produced by

Table 1

Summary statistics.

This table reports summary statistics for the state-year, public corporation-year, and private firm-year observations in this paper's sample, including dependent, independent, and control variables. Corporation and patent data come from Compustat and the NBER patent database from 1976–2006. We obtain data from DealScan to construct banking relationship measures from 1987–2006 and data from SDC to construct acquisition measures from 1976–2006. Definitions of the variables are in [Appendix A](#).

<i>Panel A: State-year observ</i>						
Variable	p25	Median	Mean	p75	S.D.	N
<i>RSindex</i>	4	4	3.5	4	1.11	1,426
<i>Pat (state)</i>	157	798	2,988	3,166	6,199	1,426
<i>Pat (corporation)</i>	32	305	1,962	1,643	4,483	1,426
<i>Pat (firm)</i>	106	388	1,026	1,209	1,852	1,426
<i>Cite (state)</i>	1,379	7,521	39,085	36,277	95,369	1,426
<i>Cite (corporation)</i>	250	2,531	26,778	18,767	69,552	1,426
<i>Cite (firm)</i>	913	3,848	12,307	13,623	27,510	1,426
<i>Mining (%)</i>	0.16	0.56	3.42	2.56	7.16	1,426
<i>Construction (%)</i>	3.90	4.44	4.62	5.13	1.40	1,426
<i>Manufacturing (%)</i>	12.0	17.2	17.2	23.1	8.0	1,426
<i>Transportation (%)</i>	2.75	3.43	3.64	4.11	1.83	1,426
<i>Trade (%)</i>	13.8	15.3	14.8	16.4	2.6	1,426
<i>Finance (%)</i>	4.28	5.11	5.96	6.38	3.41	1,426
<i>Services (%)</i>	13.4	16.6	17.6	21.1	5.8	1,426
<i>Government (%)</i>	11.2	13.3	14.3	15.8	5.0	1,426
<i>Concentration (%)</i>	11.5	12.5	13.1	13.8	2.9	1,426
<i>Gross product (billion)</i>	25.4	58.0	113.9	136.8	158.4	1,426
<i>Panel B: Public corporation-year observ</i>						
Variable	p25	Median	Mean	p75	S.D.	N
<i>Pat</i>	0	0	4.97	2	15.38	114,937
<i>Cite</i>	0	0	57.76	22.53	178.73	114,937
<i>EFD</i>	−0.63	−0.38	−0.27	−0.11	0.78	114,728
<i>In-state loan (millions)</i>	0	0	7	0	150	79,101
<i>Out-of-state (millions)</i>	0	0	33	0	288	79,101
<i>Assets (millions)</i>	20	99	1,018	625	2,157	84,902
<i>Age</i>	8	17	20.10	31	14.30	88,986
<i>SA index</i>	−15.27	−9.36	−10.58	−5.49	6.08	84,902
<i>RE&DAssets</i>	0	0	0.04	0.03	0.10	133,643
<i>ROA</i>	0.02	0.11	0.02	0.17	0.40	133,643
<i>PPEAssets</i>	0.12	0.26	0.32	0.48	0.25	133,643
<i>Leverage</i>	0.11	0.26	0.31	0.41	0.3	133,643
<i>CapexAssets</i>	0.02	0.05	0.07	0.09	0.08	133,643
<i>Hindex</i>	0.11	0.18	0.23	0.30	0.18	133,643
<i>TobinQ</i>	0.99	1.3	2.1	2.0	3	133,643
<i>Dollar volume (millions)</i>	0	0	4.1	0	36.2	119,843
<i>Number of deals</i>	5.49					
			Cite			

private firms towards the end of the sample when the interstate branching deregulation took place.

4. Empirical results

4.1. Baseline specification and results

As we discussed in the introduction, a major challenge of our study is the identification of the causal effects of

banking competition on innovation, due to both omitted variables and reverse causality concerns. First, unobservable industry or state characteristics related to both local banking competition and innovation could remain in the residual term of regressions. These unobservable characteristics make it difficult to draw correct statistical inferences from standard Ordinary least squares (OLS) regressions. Second, there is an old debate on the direction of causality between finance and economic growth. [See

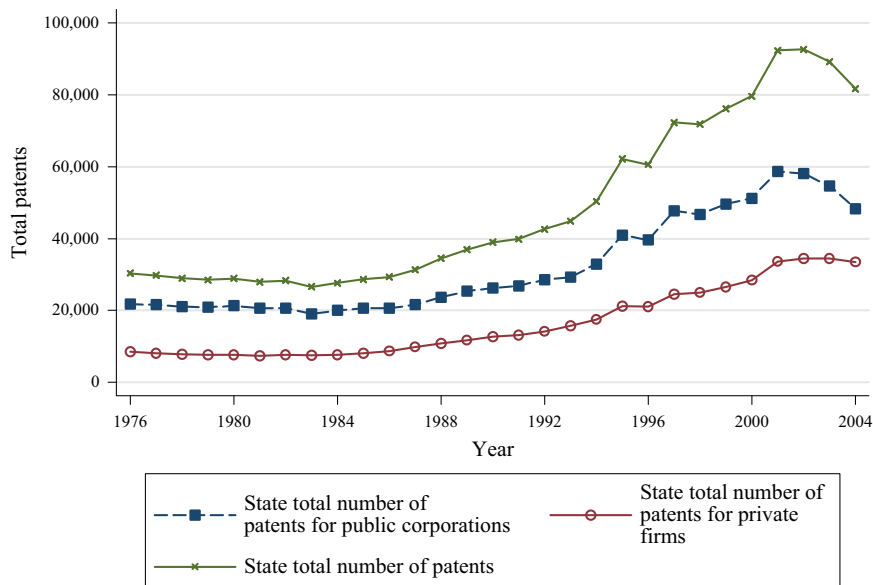


Fig. 1. Patent production through time for corporations and private firms. This figure shows, for each year from 1976–2004, the total number of patents produced by U.S. corporations, the total number of patents produced by private firms in the U.S., and the sum of the two. We identify a patent as being produced by a corporation if it matches to a firm in the Compustat database. We assume a private firm produces the patent if it does not match to a firm in Compustat. We remove from the sample patents produced by universities, governments, and foreign companies.

Butler and Cornaggia (2011) for a recent review of this literature.] A large literature starting from Schumpeter (1911) argues that finance leads to economic growth [innovation is an important driver of economic growth (Solow, 1957)], while another large literature follows Robinson (1952, p. 86) who famously argues that “where enterprise leads finance follows.” Our identification strategy is to exploit the staggered deregulation of interstate bank branching laws in the U.S., which generate plausibly exogenous variation in states’ banking competition environments, following Rice and Strahan (2010). One key advantage of this identification strategy is that there are multiple shocks that affect different states at different times, which allows us to overcome a common difficulty facing studies with a single shock: the potential existence of omitted variables coinciding with the shock that affect economic outcomes (innovation, in our case).

Specifically, to assess how banking competition affects innovation, we estimate the following model:

$$\ln Pat_{i,t+1 \text{ to } t+3} = \alpha + \beta RSindex_{i,t} + \gamma Z_{i,t} + Year_t + State_i + \varepsilon_{i,t}, \quad (1a)$$

where i indexes state and t indexes time. The dependent variable in Eq. (1a) is the natural logarithm of the total number of patents generated in a state in the following three years. We measure the banking competition variable, $RSindex$, for state i in year t . Z is a vector of controls that includes state-level labor force composition for eight different industry segments, labor force concentration, gross product, and dummies for other early banking deregulatory events. $Year_t$ and $State_i$ capture year and state fixed effects, respectively. We cluster standard errors by year in our baseline tests.

We include state fixed effects in the baseline regression. This approach addresses the concern that unobservable

variables omitted from Eq. (1a) that generate variation in a state’s stance toward openness to interstate branching might be correlated with innovativeness, rendering our findings spurious. For example, if states with vibrant economic activities and strong growth opportunities are more likely to deregulate, then the state-level economic activities and growth opportunities are unobservables that correlate with both innovativeness and $RSindex$, which could bias our coefficient estimate of $RSindex$ downward. Including state fixed effects will strip out any persistent differences across states. We report the OLS regression results estimating Eq. (1a) in column 1 of Table 2 Panel A.

The coefficient estimates of $RSindex$ are positive and significant at the 1% level. This finding suggests that an increase in banking competition due to deregulation (i.e., a decrease in $RSindex$) leads to a decrease in the number of patents in the first three subsequent years. To be more concrete, based on the coefficient estimate of $RSindex$ in column 1, states that are completely open to interstate branching generated a total of 30.8% ($= 4 \times 0.077$) fewer patents in the subsequent three years post-branching deregulation than states with the most restrictions on interstate branching three years after deregulation.

The effect of banking competition on patent quantity is large: The above reduction translates to a drop of 920 patents over the three years after a deregulatory event. (Table 1 Panel A shows the average number of patents produced in a state over the three years after a deregulatory event is 2,988. The product of 2,988 and 30.8% is 920.) This reduction in patent quantity is also large relative to the variability of patent production over time. We make this comparison by taking the average of state-level patent production in each of the 31 years of our sample. The standard deviation of these 31 observations is 449 patents. Therefore, because a reduction of 920 patents per state over

Table 2

Baseline regressions.

This table reports OLS regression estimates for baseline regressions. Panel A reports the OLS regression estimates of Eqs. (1a) and (1b). The dependent variable in columns 1–3 is the natural logarithm of one plus the sum of the patents generated in a state in the next three years. The dependent variable in columns 4–6 is the natural logarithm of one plus the number of citations for patents generated in a state in the next three years. Panel B reports the OLS regression estimates of Eqs. (1a) and (1b). The dependent variable in columns 1–3 is the patent generality score for patents generated in the next three years. The dependent variable in columns 4–6 is the patent originality score generated in the next three years. Definitions of control variables are in Appendix A. Robust standard errors clustered by year are reported in parentheses. Significance at the 1%, 5%, and 10% level is indicated by ***, **, and *, respectively. Observations are at the state-year level from 1976–2003.

Panel A: Patents and patent citations						
	LnPat			LnCite		
	Total (1)	Public (2)	Private (3)	Total (4)	Public (5)	Private (6)
<i>RSindex</i>	0.077*** (0.016)	0.102*** (0.021)	0.030 (0.018)	0.058*** (0.019)	0.066** (0.028)	0.041 (0.024)
<i>Mining</i>	0.013 (0.008)	0.031** (0.015)	0.001 (0.006)	0.013 (0.013)	0.098*** (0.028)	0.000 (0.010)
<i>Construction</i>	−0.012 (0.008)	0.055* (0.029)	−0.028** (0.010)	−0.004 (0.010)	0.115** (0.050)	−0.023 (0.014)
<i>Manufacturing</i>	0.016 (0.010)	0.023 (0.020)	−0.001 (0.007)	0.014 (0.014)	0.077*** (0.023)	−0.000 (0.011)
<i>Transportation</i>	−0.063** (0.029)	−0.139* (0.077)	−0.034* (0.019)	−0.082** (0.031)	−0.190 (0.113)	−0.042* (0.022)
<i>Trade</i>	0.122*** (0.015)	0.116*** (0.035)	0.090*** (0.014)	0.125*** (0.024)	0.172*** (0.058)	0.077*** (0.022)
<i>Finance</i>	−0.005 (0.009)	−0.027 (0.016)	0.048*** (0.007)	−0.017 (0.011)	−0.010 (0.022)	0.037*** (0.009)
<i>Service</i>	−0.090*** (0.010)	−0.103*** (0.023)	−0.083*** (0.008)	−0.104*** (0.016)	−0.060** (0.025)	−0.106*** (0.014)
<i>Government</i>	−0.045*** (0.010)	−0.069*** (0.022)	−0.058*** (0.010)	−0.045** (0.017)	−0.039 (0.037)	−0.058*** (0.018)
<i>Concentration</i>	0.002 (0.017)	0.075*** (0.020)	−0.028* (0.015)	0.009 (0.020)	0.060** (0.026)	−0.043** (0.019)
<i>Gross product</i>	−0.000 (0.000)	−0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	−0.000 (0.000)	0.000 (0.000)
<i>Intra</i>	−0.226*** (0.040)	−0.402*** (0.060)	−0.058*** (0.020)	−0.253*** (0.038)	−0.468*** (0.067)	−0.098** (0.039)
<i>Inter</i>	−0.095** (0.035)	−0.084 (0.055)	−0.045 (0.038)	−0.166*** (0.044)	−0.216** (0.087)	−0.091* (0.046)
Constant	2.548*** (0.455)	−0.003 (1.232)	3.512*** (0.471)	4.764*** (0.683)	−1.876 (1.572)	6.068*** (0.682)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,426	1,426	1,426	1,426	1,426	1,426
Adjusted R ²	0.966	0.927	0.971	0.954	0.901	0.954
Panel B: Generality and originality						
	Generality			Originality		
	Total (1)	Public (2)	Private (3)	Total (4)	Public (5)	Private (6)
<i>RSindex</i>	−0.002 (0.002)	−0.005 (0.005)	−0.002 (0.003)	−0.002 (0.003)	−0.003 (0.005)	−0.002 (0.003)
<i>Mining</i>	−0.001 (0.002)	0.001 (0.003)	−0.001 (0.002)	−0.000 (0.002)	−0.001 (0.005)	−0.000 (0.002)
<i>Construction</i>	−0.001 (0.002)	−0.000 (0.006)	−0.002 (0.002)	−0.001 (0.004)	−0.003 (0.005)	−0.002 (0.004)
<i>Manufacturing</i>	−0.000 (0.001)	0.002 (0.003)	−0.001 (0.002)	−0.001 (0.002)	−0.004 (0.004)	−0.002 (0.002)
<i>Transportation</i>	−0.001 (0.003)	−0.004 (0.009)	0.004 (0.005)	−0.000 (0.007)	0.005 (0.012)	0.001 (0.006)
<i>Trade</i>	−0.000 (0.003)	0.006 (0.007)	0.001 (0.004)	−0.001 (0.004)	−0.007 (0.008)	−0.000 (0.004)
<i>Finance</i>	−0.004*** (0.001)	0.000 (0.003)	−0.006*** (0.002)	−0.003 (0.002)	−0.005 (0.004)	−0.004** (0.002)
<i>Service</i>	−0.001 (0.003)	0.009 (0.006)	−0.002 (0.003)	−0.000 (0.002)	0.001 (0.006)	−0.002 (0.002)

Table 2 (continued)

Panel A: Patents and patent citations						
	LnPat			LnCite		
	Total (1)	Public (2)	Private (3)	Total (4)	Public (5)	Private (6)
Government	0.001 (0.002)	0.003 (0.004)	0.001 (0.002)	−0.004 (0.003)	−0.005 (0.004)	−0.003 (0.003)
Concentration	0.008*** (0.002)	0.003 (0.005)	0.005** (0.002)	0.005** (0.003)	0.006 (0.006)	0.004* (0.002)
Gross product	−0.000** (0.000)	−0.000 (0.000)	−0.000** (0.000)	−0.000 (0.000)	−0.000 (0.000)	−0.000 (0.000)
Intra	−0.006 (0.006)	0.002 (0.013)	−0.008 (0.006)	0.004 (0.007)	0.019 (0.013)	0.004 (0.006)
Inter	−0.001 (0.008)	−0.010 (0.015)	−0.005 (0.008)	−0.006 (0.007)	0.000 (0.014)	−0.014* (0.008)
Constant	0.313*** (0.097)	0.086 (0.184)	0.278** (0.122)	0.192 (0.145)	0.235 (0.234)	0.182 (0.138)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,417	1,312	1,414	1,418	1,324	1,417
Adjusted R ²	0.913	0.802	0.893	0.895	0.725	0.901

three years implies a reduction of 307 patents per state-year, this comparison shows that the reduction in patent quantity as a result of branching deregulation is equal to 68.4% of the variability of patent production over time. (The quotient of 307 patents and 449 patents is 68.4%).⁵

Although our evidence in column 1 suggests that branching deregulation adversely affects innovation by companies headquartered in the state, the negative effect could be different for public corporations and private firms. In particular, private firms could be more sensitive to local banking conditions than public corporations. In the next two columns, we separate patents generated by public corporations and those generated by private firms to examine whether banking competition affects innovation by these two types of companies differently. We estimate Eq. (1a) separately for public corporations and private firms and report regression results in columns 2 and 3, respectively. The coefficient estimate of *RSindex* in the sample of patents generated by public corporations is positive and significant at the 1% level, while that in the sample of patents produced by private firms is not statistically significant. The evidence suggests that the negative effect of banking competition due to deregulation on patent counts is driven by public corporations. This test uncovers no effect of deregulation on patent quantity of private firms.⁶

⁵ In an untabulated analysis, we examine the effect of banking deregulation on the number of patents generated in individual years (i.e., one, two, and three years) after deregulation. Consistent with our results in Table 2 Panel A, we find that an increase in banking competition due to deregulation leads to a decrease in the number of patents in each of the first three subsequent years.

⁶ In an untabulated analysis, we further explore the different effects of deregulation on innovation by public corporations and private firms by replacing the dependent variable with the raw difference between the number of patents produced by these two types of companies as well as the ratio of patents produced by these two types of companies. We find

Next, we examine the effect of banking competition on patent quality by replacing the dependent variable with our innovation quality proxy, the natural logarithm of patent citations, and estimate the following model:

$$\text{LnCite}_{i,t+1 \text{ to } t+3} = \alpha + \beta \text{RSindex}_{i,t} + \gamma Z_{i,t} + \text{Year}_t + \text{State}_i + \varepsilon_{i,t}. \quad (1b)$$

We report the results in columns 4–6. These tests parallel the first three columns in Panel A. In column 4, we find that the coefficient estimate of *RSindex* is positive and significant at the 1% level, suggesting that an increase in banking competition due to deregulation results in a decrease in the quality of patents generated by the companies headquartered in the state in the subsequent three years. To gauge the economic significance, based on the coefficient estimate of *RSindex* in column 4, states that are completely open to interstate branching generated a total of 23.2% ($=4 \times 0.058$) fewer citations for patents generated in the subsequent three years post-branching deregulation than states with the most restrictions on interstate branching three years after deregulation.

The effect of banking competition on patent quality is large: The above reduction translates to a drop of 9,068 citations per state for patents generated over the three years after a deregulatory event. (Table 1 Panel A shows the average number of citations on patents produced in a state over the three years after a deregulatory event is 39,085. The product of 39,085 and 23.2% is 9,068.) This reduction in patent quality is also large relative to the variability of patent citations over time. We make this comparison by taking the average of citations on state-level patents in each of the 31 years of our sample. The standard deviation of these 31 observations is 8,486 citations. Therefore, because

(footnote continued)

that the gap in patent output between public corporations and private firms decreases in the three years following deregulation.

a reduction of 9,086 patent citations over three years implies a reduction of 3,022 citations per state-year, this comparison shows that the reduction in patent quality as a result of branching deregulation is equal to 35.6% of the variability of patent citations over time. (The quotient of 3,022 citations and 8,486 citations is 35.6%).

In the next two columns, we once again separate patents generated by public corporations and those generated by private firms and estimate Eq. (1b) to evaluate whether banking competition affects patent quality by these two types of firms differently. We continue to observe a positive and significant coefficient estimate of *RSindex* in the sample of patents generated by public corporations but an insignificant estimate of *RSindex* in the sample of patents generated by private firms. Consistent with our findings from patent counts, the evidence suggests that the negative effect of banking competition on patent quality is driven by public corporations.

Although a larger number of patent citations is typically interpreted as the patent having greater impact, the distribution of citations is also important. Hence, we construct two more patent-based measures, patent originality and generality, following Hall, Jaffe, and Trajtenberg (2005). We then examine the effect of banking competition on these two measures that capture the underlying nature of innovation being patented. Patents that cite a wider array of technology classes of patents are viewed as having greater originality. We define a patent's originality score as one minus the Herfindahl index of the three-digit technology class distribution of all the patents it cites. Therefore, the higher a patent's originality score, the more the patent draws upon a diverse array of existing knowledge. In a similar spirit, patents being cited by a wider array of technology classes of patents are considered as having greater generality. We define a patent's generality score as one minus the Herfindahl index of the three-digit technology class distribution of all the patents that cite it. The higher a patent's generality score, the more that the patent is being drawn upon by a more diverse array of subsequent patents. We then average the individual patents' originality and generality scores at the state level. For states that generate no patents in a year, their patents generality and originality scores are undefined and therefore treated as missing.

We replace the dependent variable with patent originality and generality in Eq. (1a) and report the regression results in Panel B. Like before, we evaluate the effect of banking competition on patent originality and generality generated by both types of companies, by public corporations only, and by private firms only in columns 1–3 and columns 4–6, respectively. The coefficient estimates of *RSindex* are statistically insignificant in all columns, suggesting that while banking deregulation negatively affects patent quantity and quality as we documented before, it does not directly affect the underlying nature of innovation being patented.⁷ Therefore, in the rest of the paper,

we focus on the analyses that examine the effect of banking competition on innovation quantity and quality measured by patent counts and citations.

We undertake a rich set of robustness tests for our baseline results and discuss the details of these tests in the Internet Appendix. We find that our baseline results are robust to alternative ways of clustering standard errors, alternative econometric models that address the certain features of innovation variables (i.e., right-skewed, non-negative discrete variable), alternative subsamples, and several specifications designed to rule out reverse causality concerns arising from companies' decisions on where to establish or relocate their headquarters. Overall, our baseline results suggest that an exogenous increase in banking competition due to staggered interstate branching deregulation results in a lower state-level innovation output in subsequent years. This reduction in innovation output is driven by public corporations.

4.2. Additional endogeneity tests

As discussed above, although the staggered deregulation of interstate branching laws in the U.S. represents an exogenous shock to banking competition, Kroszner and Strahan (1999) argue that state-level factors that manifest differently across states could have affected the timing of deregulation in different states. A reverse causality concern may arise if the states also differ in their innovation intensities and such differences triggered the deregulation. Our robustness tests reported in the Internet Appendix mitigate such concerns. Specifically, we restrict our state-level analysis to patents produced by corporations that do not relocate their headquarters any time during the sample period or corporations that are headquartered within a state at least three years before any changes in bank branching laws and find robust results. To further explore the possibility of reverse causality, we follow Bertrand and Mullainathan (2003) to examine the dynamics of innovation surrounding deregulatory events. If reverse causality is indeed present, we should observe changes in innovation prior to deregulatory events.

To check the pre-existing trends in innovation, we restrict our sample to a 21-year window surrounding state-deregulation years (ten years before and ten years after). We decompose each of the four components of the *RSindex* into four dummy variables associated with four periods around the deregulation year: all years up to and including two years prior to deregulation, one year prior to deregulation, one year post-deregulation, and two years or more post-deregulation. We then sum over the four components of the *RSindex* to obtain four new variables, *Before*²⁺, *Before*¹, *After*¹, and *After*²⁺, corresponding to the four time periods around each deregulation. The deregulation year is the reference year in this setting. The coefficient estimates of *Before*²⁺ and *Before*¹ are especially important because their significance and magnitude would indicate whether there is any relation between innovation and deregulatory events before interstate branching laws were changed. We

⁷ Because both patent originality and generality scores are bounded between zero and one, we use a Tobit model and find qualitatively similar results.

Table 3

Endogeneity tests.

This table reports OLS regression estimates of Eq. (2). We truncate the state-year panel to a \pm ten-year window around deregulatory events. Definitions of variables are in Appendix A. Robust standard errors clustered by year are reported in parentheses. Significance at the 1%, 5%, and 10% level is indicated by ***, **, and *, respectively.

	LnPat			LnCite		
	Total (1)	Public (2)	Private (3)	Total (4)	Public (5)	Private (6)
<i>Before</i> ²⁺	0.033 (0.058)	0.060 (0.037)	−0.006 (0.052)	0.014 (0.035)	−0.026 (0.073)	0.066 (0.047)
<i>Before</i> ¹	−0.103 (0.095)	−0.031 (0.032)	−0.111 (0.089)	−0.067 (0.057)	−0.094 (0.131)	−0.019 (0.073)
<i>After</i> ¹	−0.086* (0.050)	−0.081** (0.030)	−0.063 (0.049)	−0.028 (0.049)	−0.056 (0.124)	0.001 (0.036)
<i>After</i> ²⁺	−0.130* (0.070)	−0.091*** (0.029)	−0.100 (0.065)	−0.075** (0.031)	−0.169* (0.083)	−0.045 (0.040)
Constant	0.627*** (0.117)	0.573*** (0.075)	0.012 (0.105)	2.351*** (0.070)	2.432*** (0.146)	−0.132 (0.095)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	910	910	910	910	910	910
Adjusted R ²	0.936	0.925	0.924	0.940	0.893	0.937

estimate the following model:

$$\begin{aligned} \text{LnPat}_{i,t}(\text{LnCite}_{i,t}) = & \alpha + \beta_1 \text{Before}_{i,t}^{2+} + \beta_2 \text{Before}_{i,t}^1 \\ & + \beta_3 \text{After}_{i,t}^1 + \beta_4 \text{After}_{i,t}^{2+} + \text{Year}_t + \text{State}_i + \varepsilon_{i,t}, \end{aligned} \quad (2)$$

where i indexes state and t indexes time.

In Table 3 we report the regression results estimating Eq. (2). In column 1, we report the results for patents generated by both public corporations and private firms. The coefficient estimates of *Before*²⁺ and *Before*¹ are not significant, suggesting that state-level innovation shows no significant change prior to interstate branching deregulation. The coefficient estimates of *After*¹ and *After*²⁺ are negative and significant, consistent with our baseline findings. In column 2, we focus on patents generated by public corporations and find similar results: The coefficient estimates of *Before*²⁺ and *Before*¹ are not significant while those of *After*¹ and *After*²⁺ are negative and significant. We report results for patents generated by private firms in column 3. None of *Before*²⁺, *Before*¹, *After*¹, and *After*²⁺ is statistically significant. This non-result indicates trends in innovation by private firms do not reverse-cause branching deregulation. In columns 4–6, we repeat these tests with patent citations as the dependent variable and we observe a similar pattern around the deregulation year. The evidence for patent citations is slightly weaker than what we find for patent production, but the results show that patent citations decline only after branching deregulation. Overall, the results presented in Table 3 suggest that, whether we consider all companies, or public corporations and private firms separately, there is not a pre-existing trend in innovation before interstate branching deregulation. These results mitigate concerns about reverse causality.

Another concern that prevents us drawing a causal interpretation of banking competition on innovation from our baseline regressions is an omitted variables problem: Unobservable shocks or variables that are omitted from

our analysis but coincide with state-level deregulatory events could drive our results. To address this concern, we conduct placebo tests to check whether our results disappear when we artificially (i.e., incorrectly) assume the deregulation occurs in years other than the actual deregulation year. We do this by first obtaining an empirical distribution of years when states deregulated from Rice and Strahan (2010). We then randomly assign states into each of these deregulation years (without replacement) following the empirical distribution. This approach maintains the distribution of deregulatory years from our baseline specification, but it disrupts the proper assignment of deregulation years to states. Therefore, if an unobservable shock occurs at approximately the same time as the deregulation events in the mid-1990s, it should still reside in the testing framework, and thus have an opportunity to drive the results. However, if no such shock exists, then our incorrect assignments of deregulatory years to states should weaken our results when we re-estimate our baseline regressions in Eqs. (1a) and (1b). We report the results in Table 4. The coefficient estimates of *RSindex* are statistically insignificant and not different from zero in all columns.

Overall, the various tests for reverse causality and omitted variables reported in this subsection support the notion that banking competition due to deregulation appears to have a causal, negative effect on state-level innovation output, which is mainly driven by public corporations.

5. Mechanisms

Our evidence so far shows a robust, negative effect of state-level banking competition on innovation by public corporations, which does not appear to be driven by a pre-existing trend in innovation output prior to deregulation nor omitted shocks coinciding with the deregulation. However, our tests reveal no effect of banking competition

Table 4

Randomization of deregulations.

This table reports OLS regression estimates of Eqs. (1a) and (1b) with randomized state deregulations. The dependent variable in columns 1–3 is the natural logarithm of one plus the sum of the patents generated in the next three years. The dependent variable in columns 4–6 is the natural logarithm of one plus the number of citations for patents generated in the next three years. Definitions of control variables are in Appendix A. Robust standard errors clustered by year are reported in parentheses. Significance at the 1%, 5%, and 10% level is indicated by ***, **, and *, respectively. Observations are at the state-year level from 1976–2003.

	LnPat			LnCite		
	Total (1)	Public (2)	Private (3)	Total (4)	Public (5)	Private (6)
<i>RSindex</i>	0.002 (0.038)	−0.028 (0.070)	0.020 (0.027)	−0.007 (0.041)	−0.091 (0.095)	0.019 (0.033)
<i>Mining</i>	0.011 (0.032)	0.030 (0.048)	0.001 (0.027)	0.012 (0.033)	0.098* (0.053)	−0.000 (0.028)
<i>Construction</i>	−0.009 (0.037)	0.059 (0.068)	−0.027 (0.023)	−0.002 (0.043)	0.118 (0.080)	−0.022 (0.033)
<i>Manufacturing</i>	0.017 (0.026)	0.025 (0.044)	0.000 (0.023)	0.015 (0.028)	0.076 (0.054)	0.001 (0.023)
<i>Transportation</i>	−0.069 (0.068)	−0.144 (0.151)	−0.037 (0.038)	−0.086 (0.072)	−0.190 (0.214)	−0.046 (0.041)
<i>Trade</i>	0.130** (0.061)	0.126 (0.109)	0.093** (0.041)	0.131* (0.072)	0.177 (0.137)	0.082 (0.054)
<i>Finance</i>	−0.004 (0.038)	−0.025 (0.032)	0.047* (0.033)	−0.016 (0.047)	−0.004 (0.035)	0.037 (0.031)

on innovation by private firms. In this section, we explore possible underlying mechanisms through which banking competition may affect companies' innovation output. Specifically, we examine whether external finance dependence, prior banking relationships, and acquisitions of private, innovative firms by public corporations are possible underlying economic mechanisms through which banking competition affects innovation. To facilitate the exploration of mechanisms, we expand the state-year sample used in the baseline tests in Section 4 to corporation-year and private firm-year samples in this section. To save space, we suppress the coefficient estimates of all controls in the tables reported in this section.

5.1. External finance dependence

Although we show banking competition has a negative effect on public corporations' innovation and has no effect on private firms' innovation, the reduced cost of capital within states due to bank branching deregulation (Rice and Strahan, 2010) may still relax financing constraints for these

firms. Therefore, it is possible that public corporations as well as private firms that are in external-finance-dependent industries could take advantage of state-level banking competition to improve their innovative output. In this section, we examine whether companies' dependence on external finance is an underlying mechanism.

If banking competition directly affects the access to credit of corporations and firms, which increases their innovativeness as suggested by the literature showing finance creates growth, we expect that corporations and firms that are external-finance-dependent should have an increase, instead of a decrease, in innovation output after banking competition increases. Using the measure of external finance dependence developed by Duchin, Ozbas, and Sensoy (2010), we construct a dummy variable, *Dependence_{it}*, which equals one for corporation-years below the industry median external finance dependence (*EFD*) value (i.e., less external-finance-dependent) and zero for corporation-years above the industry median external finance dependence value (i.e., more external-finance-dependent). To capture private firms' external finance dependence, we first define a private firm's industry

Table 5

External financial dependence.

This table reports OLS regression estimates of Eq. (3)

membership based on the technology classifications of patents which it files. We use a mapping file provided by Brian Silverman (available at http://www-2.rotman.utoronto.ca/~silverman/ipcsic/documentation_IPC-SIC_concordance.htm) which links the International Patent Code (IPC) assigned to each patent by USPTO to a distribution of three-digit standard industrial classification (SIC) codes. We then impute EFD numbers from public corporations in industry j in year t to private firms in the same industry and year. For each private firm-year, we then compute the average EFD and carry this value forward for private firm-years in which no patents are generated. Similarly, we construct a dummy variable, $Dependence_{it}$, for private firms, which equals one for firm-years below the industry median EFD value and zero for firm-years above the industry median external finance dependence value.

To examine how a company's external finance dependence alters the marginal impact of banking competition on its innovation output, we estimate the following model:

$$\begin{aligned} \ln Pat_{i,t+1 \text{ to } t+3} & (\ln Cite_{i,t+1 \text{ to } t+3}) \\ &= \alpha + \beta_1 RSindex_{k,t} + \beta_2 Dependence_{i,t} + \beta_3 RSindex_{k,t} \\ & \quad \times Dependence_{i,t} + \gamma Z_{i,k,t} + Industry_j + Year_t + State_k + \varepsilon_{i,t}, \end{aligned} \quad (3)$$

where i indexes company, t indexes time, j indexes industry, and k indexes state. The dependent variable captures company innovation outcomes and is either patent counts or patent citations. We add the new variable, $Dependence_{it}$, and the interaction term between $RSindex$ and $Dependence_{it}$. The coefficient estimate of $RSindex$ captures the effect of banking competition on innovation for companies that are more external-finance-dependent. The coefficient on the interaction term reflects the different effects of banking competition on innovation for companies that are less external-finance-dependent. If our conjecture is correct, i.e., if more external-finance-dependent companies take advantage of banking competition and

improve their innovation output, then we expect β_1 to be negative and significant.

We report the results estimating Eq. (3) in Table 5. In columns 1 and 2, we present the regressions for private firms. The coefficient estimates of $RSindex$ are negative in both specifications and significant at the 1% or 5% level, which suggests that banking competition has a positive effect on innovation output by private firms that are more dependent on external finance. Based on the coefficient estimate reported in column 1, more external-finance-dependent firms located in states that are completely open to interstate branching generate a total of 7.6% ($= 4 \times 0.019$) more patents in the first three years post-branching deregulation than firms in states with the most restrictions on interstate branching. These findings suggest that banking competition enhances innovation by more external-finance-dependent private firms.

Columns 3 and 4 present the results for public corporations. The coefficient estimates of $RSindex$ are negative but insignificant, suggesting that an increase in banking competition (i.e., a decrease in $RSindex$) does not appear to affect innovation output for corporations that are more dependent on external finance. Based on the coefficient estimate of the interaction term reported in column 3, the marginal effect of $RSindex$ for corporations that are less external-finance-dependent is 0.136.⁸ The evidence suggests that although deregulation adversely affects public corporations' innovation on average, this effect mainly comes from the group of corporations that are less external-finance-dependent. For external-finance-dependent corporations, deregulation does not adversely affect their innovation output.

We examine the robustness of this finding by splitting our sample according to a variety of alternative partition variables that are proxies for external finance dependence, following Acharya, Imbs, and Sturgess (2011) and Hadlock and Pierce (2010). Specifically, we partition our sample based on company size, age, bank-dependence, and the SA index. We follow the same procedure described above for the EFD measure to impute these alternative partition variables from public corporations in industry j in year t to private firms in the same industry and year. We also define $Dependence_t$ for these alternative partition variables the same way as before, i.e., it equals one for the firm-years (corporation-years) that are considered to be less external-finance-dependent and zero for firm-years (corporation-years) that are considered to be more external-finance-dependent. For brevity, we focus our attention on private firms in this robustness analysis and report the results in Table 6. Untabulated results from public corporations are qualitatively similar to those reported in Table 5.

Panel A presents the results when we use assets as the partition variable. Panel B reports the findings if we use age to slice the sample. Panel C presents the results based on bank dependence. We define a sector as bank-dependent if the cumulative dollar value of loans borrowed by companies in the sector up to a given year is greater than the median in the year. Finally, in Panel D, we report the results when the sample is partitioned by the SA index. We observe the coefficient estimates of $RSindex$ are all negative and significant except for columns 4 and 8, suggesting that banking competition has a positive effect on both patent quantity and quality by private firms that are more dependent on external finance. The evidence is consistent with the findings reported in Table 5.

Overall, the evidence reported in this section indicates that banking competition relaxes financial constraints for private firms that are more dependent on external finance. As a consequence, these firms generate more patents and their patents receive more citations after deregulation. Therefore, dependence on external finance appears to be one underlying mechanism through which banking competition affects innovation.

5.2. Prior banking relationships

Next, we examine whether companies' prior banking relationships interact with changes in banking competition to affect their innovation output. We hypothesize that in-state banking relationships prior to deregulatory events are evidence that companies are able to satisfy their demand for capital from nearby banks. However, if companies borrow from out-of-state banks prior to deregulatory events, it indicates that such companies are unable to satisfy their demand for capital from nearby banks. Therefore, if banking competition due to interstate branching

In the case that multiple lenders are identified as the lead arranger, we use the lead arranger with the largest percentage share in the loan.⁹ Using the data on bank location, we then calculate the cumulative dollar amount of loans a company has borrowed up to year t from in-state and out-of-state banks, separately. We set cumulative loans to zero if the company does not have any bank loans prior to the deregulatory event. We then construct an indicator variable, *BankLoans*, which equals one if the cumulative dollar value of loans borrowed by a company up to a given year is smaller than the median in that year and zero otherwise. We then estimate the following model:

$$\begin{aligned} \text{LnPat}_{i,t+1 \text{ to } t+3} &= \alpha + \beta_1 \text{RSindex}_{k,t} + \beta_2 \text{BankLoans}_{i,t} + \beta_3 \text{RSindex}_{k,t} \\ &\quad \times \text{BankLoans}_{i,t} + \gamma \text{Z}_{i,k,t} + \text{Industry}_j + \text{Year}_t + \text{State}_k + \varepsilon_{i,t}, \end{aligned} \quad (4)$$

where i indexes company, t indexes time, j indexes industry, and k indexes state. The interaction term in Eq. (4) is the key variable of interest in this analysis. If the data support our conjecture, then the coefficient estimate of *RSindex*, β_1 , will be negative and significant for out-of-state banking relationships. Furthermore, compared to public corporations, private firms are more likely to rely on financing from local banks. Therefore, if the data support our conjecture, the distinction between how prior in-state and out-of-state banking relationships interact with banking competition to affect innovation should be more pronounced for private firms than for public corporations.

We report the regression results estimating Eq. (4) in Table 7. In Panel A, we focus on the sample of private firms. None of the coefficient estimates of *RSindex* is significant in columns 1 and 2, in which in-state bank loans are used as a proxy for prior bank relationships. In contrast, in columns 3 and 4, in which we consider out-of-state loans, we observe negative and significant coefficient estimates of *RSindex*. In addition, these coefficients are larger in economic magnitude compared to their counterparts in columns 1 and 2. Consistent with our conjecture, this result suggests that in-state banking relationships indicate that a firm is able to successfully raise capital from nearby banks prior to any deregulatory events. These firms are insensitive to changes in the in-state banking environment because they have pre-existing banking relationships. However, firms relying more on out-of-state bank credit before deregulation benefit most from the deregulation because it allows them to access credit from in-state banks. These firms appear to take advantage of expanded access to in-state bank finance in the sense that they generate a larger number of patents after deregulation, consistent with our conjecture.

In Panel B, we turn our attention to the sample of public corporations. The coefficient estimates of *RSindex* are insignificant irrespective of whether in-state bank loans or out-of-state loans are used as a proxy for prior

bank relationships. These results suggest that the mechanism of prior banking relationships plays a role only for private firms that are more likely to rely on financing from local banks.

Overall, the evidence presented in this section suggests that an increase in access to credit due to banking deregulation positively affects firms' innovation output if they mainly obtain loans from out-of-state banks before the deregulatory event.

5.3. Acquisitions of private firms by public corporations

Our evidence so far shows that state-level banking competition has a positive effect on innovation by external-finance-dependent private firms and a negative effect on innovation by public corporations. In this section, we explore a possible mechanism—the acquisitions of small, innovative firms—that might explain why banking competition negatively affects corporations' innovation output. Sevilir and Tian (2012) show that U.S. publicly traded corporations enhance their innovativeness through acquisitions of small, innovative firms (that are usually privately held) and that acquisitions play at least as important a role as R&D expenditures in promoting innovation. If interstate branching deregulation expands the availability of credit to small, innovative firms at a lower cost, these firms could be more likely to take advantage of bank lending to finance themselves and remain independent instead of agreeing to an acquisition to ease their financial constraints.¹⁰ Put differently, banking competition may affect corporations' innovation output as these corporations become less able to acquire innovative target firms after interstate branching deregulation. We test this hypothesis from two perspectives.

We first examine this hypothesis from the demand (acquirer) side by studying whether corporations are less likely to acquire innovative target firms after deregulation. It is natural to believe that, based on the findings in Sevilir and Tian (2012), corporations that make many acquisitions (frequent acquirers) have greater willingness or ability to enhance innovation through acquisitions compared to corporations that make acquisitions less frequently or corporations that never make acquisitions. If banking competition affects corporate innovation output through impaired abilities of corporations to acquire small, innovative target firms, we should expect that the negative effect of banking competition on corporate innovation output is more pronounced for frequent acquirers.

To test this conjecture, we conduct our analyses at the corporation level for a sample of publicly traded corporations. We assign the value of *RSindex* based on the corporation's headquarter state. We estimate the following model:

$$\begin{aligned} \text{LnPat}_{i,t+1 \text{ to } t+3} &= \alpha + \beta_1 \text{RSindex}_{k,t} + \beta_2 \text{Acquisition}_{i,t} \\ &\quad + \beta_3 \text{RSindex}_{k,t} \times \text{Acquisition}_{i,t} \\ &\quad + \gamma \text{Z}_{i,k,t} + \text{Industry}_j + \text{Year}_t + \text{State}_k + \varepsilon_{i,t}, \end{aligned} \quad (5)$$

⁹ In rare cases in which multiple lead arrangers share the largest percentage of the facility, we retain those observations where all the lead arrangers reside in the same state and discard the remaining observations.

¹⁰ Erel, Jang, and Weisbach (forthcoming) find that mergers and acquisitions significantly ease financial constraints of target firms.

Table 7

Banking relationships.

This table reports OLS regression estimates of Eq. (4). Panel A shows the results for private firms and Panel B shows the results for public corporations. *BankLoans* is an indicator taking a value of one if the cumulative dollar value of loans borrowed by company *i* up to year *t* is greater than the median in that year. We set cumulative loans to zero for companies that do not have any bank loans prior to the passage of any legislation enforcing IBBEA changes in the companies' headquarters states. In columns 1–2 we consider only the loans borrowed by company *i* by banks located in the same state as company *i*'s headquarters. In columns 3–4 we consider only the loans borrowed by company *i* from banks outside company *i*'s headquarter state. Definitions of control variables are in Appendix A. Robust standard errors clustered by year are reported in parentheses. Significance at the 1%, 5%, and 10% level is indicated by ***, **, and *, respectively. Observations are at the firm-year level from 1976–2003.

Panel A: Private firms				
	In-state		Out-of-state	
	<i>LnPat</i> (1)	<i>LnCite</i> (2)	<i>LnPat</i> (3)	<i>LnCite</i> (4)
<i>RSindex</i>	−0.003 (0.002)	0.002 (0.007)	−0.021*** (0.003)	−0.040*** (0.008)
<i>BankLoans</i>	−0.381*** (0.020)	−0.652*** (0.056)	−0.561*** (0.031)	−1.050*** (0.061)
<i>RSindex</i> × <i>BankLoans</i>	0.036*** (0.011)	0.038 (0.025)	0.079*** (0.016)	0.148*** (0.033)
Constant	1.377*** (0.145)	3.038*** (0.386)	1.465*** (0.147)	3.179*** (0.394)
Controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	223,655	223,655	223,655	223,655
Adjusted R ²	0.207	0.092	0.211	0.094
Panel B: Public corporations				
	In-state		Out-of-state	
	<i>LnPat</i> (1)	<i>LnCite</i> (2)	<i>LnPat</i> (3)	<i>LnCite</i> (4)
<i>RSindex</i>	0.020 (0.017)	0.009 (0.032)	0.009 (0.015)	−0.020 (0.021)
<i>BankLoans</i>	−0.130* (0.073)	−0.184* (0.104)	−0.745*** (0.040)	−1.077*** (0.087)
<i>RSindex</i> × <i>BankLoans</i>	0.028 (0.021)	0.036 (0.032)	0.050*** (0.011)	0.085*** (0.022)
Constant	1.019*** (0.167)	2.428*** (0.282)	1.436*** (0.162)	3.022*** (0.269)
Controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	44,702	44,702	44,702	44,702
Adjusted R ²	0.130	0.162	0.148	0.173

where *i* indexes corporation, *t* indexes time, *j* indexes industry, and *k* indexes state. We de-mean both variables of the interaction term for ease of the interpretation of β_3 . To create an acquisition measure, we use the transaction value in SDC that is reported for each deal. In the case that a transaction value is not reported we replace the transaction value with zero. We construct three proxies for a corporation's acquisition volume: *Dollar volume*, which is the corporation's total dollar amount spent on mergers and acquisitions (M&As) divided by corporation assets; *Number of deals*, which is the total number of M&As a corporation undertakes in a year; and *Dollar per deal*, which is the total dollar amount of M&A spending divided by corporation assets and the number of M&A deals. *Z* is a vector of corporation and industry characteristics that

includes investments in intangible assets (R&D/total assets), profitability (ROA), asset tangibility (net PPE/total assets), leverage, capital expenditures (capital expenditures/total assets), industry concentration (Herfindahl index based on sales), growth opportunities (Tobin's *Q*), corporation size (the natural logarithm of book value assets), and corporation age. The key variable of interest is β_3 , the coefficient estimate of the interaction term. If our conjecture is correct, we expect to observe a positive and significant coefficient estimate of β_3 .

Table 8 reports the regression results from estimating Eq. (5). The coefficient estimates of the interaction terms are positive and statistically significant at the 5% level in columns 1 and 3, suggesting that the negative effects of banking competition on corporation innovation output are

Table 8

Dollar volume of acquisitions.

This table reports OLS regression estimates from Eq. (5) using the natural logarithm of one plus the sum of the patents generated in the next three years as the dependent variable. The acquisition measure, *Acquisition*, for column 1 is the dollar volume spent on acquisitions for corporation *i* in year *t*, divided by the corporation's total assets in year *t*. In column 2 *Acquisition* is the number of acquisitions made by corporation *i* in year *t*. In column 3 *Acquisition* is the dollar volume spent on acquisitions for corporation *i* in year *t* divided by total assets of corporation *i* in year *t* and the number of acquisitions made by corporation *i* in year *t*. We de-mean both *RSindex* and the *Acquisition* measures for ease of interpretation. Definitions of control variables are in Appendix A. Robust standard errors clustered by year are reported in parentheses. Significance at the 1%, 5%, and 10% level is indicated by ***, **, and *, respectively. The sample includes Compustat firms from 1976–2003 and the observations are at the firm-year level.

	Dollar volume (1)	Number of deals (2)	Dollar per deal (3)
<i>RSindex</i>	0.008* (0.004)	0.007 (0.004)	0.008* (0.004)
<i>Acquisition</i>	0.008 (0.014)	0.115*** (0.020)	–0.000 (0.016)
<i>Acquisition</i> × <i>RSindex</i>	0.040*** (0.018)	0.015 (0.010)	0.044** (0.021)
Constant	–2.259*** (0.071)	–2.263*** (0.072)	–2.258*** (0.071)
Controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Observations	72,093	72,093	72,093
Adjusted R ²	0.420	0.421	0.420

more pronounced for corporations that are frequent acquirers. For example, based on the coefficient estimate reported in column 1, the marginal effect of banking competition on corporations' innovativeness is 0.008 (*p*-value=0.077) if a corporation's acquisition amount is at the sample mean. If a corporation's acquisition amount is above the sample mean by one standard deviation (0.548), then the marginal effect of banking competition on innovation output in the next three years increases by 2.3% (0.008+0.040×0.548=0.023).¹¹ These results indicate that frequent acquirers experience a decrease in their post-deregulation innovation output, which is consistent with our conjecture.

Next, we test whether small, innovative firms' improved access to credit allows them to stay independent, which indirectly hurts public corporations' innovation output. We construct two measures that capture the innovativeness of the target firms acquired by corporations in our sample. (We refer to these two measures as *TargetInnov* in Eq. (6) below.) The first measure is the total number of patents generated during the entire sample period by all target firms acquired in year *t* (*TargetCount*). The second measure is the total number of patents generated during the entire

sample period by all target firms acquired in year *t* normalized by the number of target firms acquired in a year (*TargetCountPerAcq*).¹² To construct the two measures, we begin by matching patent filings from the NBER database to each target firm in SDC and sum the number of patents each target generates over the course of the sample period. Next we merge the M&A deal data in SDC to acquiring corporations in Compustat.¹³ For states with multiple regulation changes, we repeat the observations from that state for every regulation change reported in Rice and Strahan (2010). Because both of these measures are highly right-skewed, we use a zero-inflated negative binomial model to estimate the following equation:

$$\text{TargetInnov}_{i,t} = \alpha + \beta \text{RegChg}_{i,t} + \gamma Z_{i,t} + \text{Firm}_i + \text{Year}_t + \varepsilon_{i,t}, \quad (6)$$

where *i* indexes corporation and *t* indexes time.¹⁴ *RegChg* is a dummy that equals one if a deregulation occurs in the state in which the corporation is headquartered and zero otherwise. The above specification is essentially the difference-in-differences approach used by Bertrand and Mullainathan (2003). The year fixed effects control for aggregate fluctuations in target innovativeness, and by demeaning the independent variables we control for any time-invariant differences between treatment corporations (corporations located in states where deregulation occurs) and control corporations (corporations located in states where deregulation does not occur). The key variable of interest is *RegChg*, and β is the differences-in-differences estimator.

We present the regression results in Table 9. We report the marginal effects of independent variables because the coefficients of the negative binomial model are difficult to interpret. The coefficient estimates of *RegChg* are negative and significant in both columns, suggesting the innovativeness of target firms acquired by corporations is lower after deregulation. The marginal effect at the mean for column 1 of Table 9 is –0.07. Considering the mean value of the sample in Table 9 is 0.33 patents per corporation-year, the magnitude of *RegChg* suggests that the targets

¹² We round this variable to the nearest integer to facilitate our estimation of Eq. (3) in the zero-inflated negative binomial model. We discuss these results below.

¹³ When we begin matching the NBER patent data to the list of SDC acquisition deals, there are 10,955 target-acquirer pairs in SDC. Of these acquisition deals, approximately 21% (2,359) involve a target firm that generates at least one patent during the sample period. Because an acquiring corporation can transact multiple deals in a single year, we aggregate the deals to the acquiring corporation-year level and merge the data with the corporation data in Compustat. There are 15,700 corporations in the main sample. Nine percent (1,454) of the corporations acquire a target that produces patents. However, because acquiring corporations tend to have a shorter life span than non-acquiring corporations in our sample period, the overall number of corporation-year observations with non-zero patents from acquisitions is very small. In fact, of the 133,792 corporation-year observations, only 1.63% (2,185) have non-zero patents from acquisitions.

¹⁴ We conduct the overdispersion test and Vuong test for zero inflation and both are statistically significant at the 1% level, which suggests that the zero-inflated negative binomial model is best suited for our data compared to the Poisson model and the standard negative binomial model.

¹¹ The standard deviation of dollars used in acquisitions each year is \$36.2 million. However, in Eq. (5), we standardize the independent variable *Acquisition_{i,t}* by a corporation's total assets in year *t*. The standard deviation of this variable is 0.548.

Table 9

Patents from acquisition targets.

This table reports zero-inflated negative binomial marginal effect estimates from Eq. (6). The dependent variable in column 1 is *TargetCount*, which we calculate in two steps. First, we calculate the sum of all patents generated by a target during the 1976–2006 sample. Then we sum the number of patents acquired by all the targets of corporation *i* in the year that the targets are acquired. In column 2 the dependent variable is *TargetCountPerAcq*, which is *TargetCount* divided by the number of targets corporation *i* acquires in year *t*. *RegChg* is a dummy variable set to one in the year of (and all years following) a regulation change. We repeat the observations of states with multiple regulation changes for each regulation change. We de-mean the independent variables and include year fixed effects. Significance at the 1%, 5%, and 10% level is indicated by ***, **, and *, respectively.

	<i>TargetCount</i> (1)	<i>TargetCountPerAcq</i> (2)
<i>RegChg</i>	−0.070* (0.036)	−0.056*** (0.028)
Controls	Yes	Yes
Year FE	Yes	Yes
Firm FE	Yes	Yes
Observations	151,883	151,883

acquired after deregulation produce 21% fewer patents over their lifetimes than targets acquired before deregulation.

Overall, our evidence from the demand (acquirer) side suggests that the decrease in innovation output of public corporations after deregulation is at least partly due to the fact that they are less able to acquire innovative target firms. After deregulation, small and innovative firms have better access to bank financing, and therefore can remain independent instead of being acquired.

Next, we explore this conjecture from the supply (target) side by examining whether small and innovative firms are indeed less likely to be acquired (i.e., more likely to remain independent) after deregulation. Specifically, we examine the change in the proportion of innovative target firms surrounding interstate bank branching deregulation. We define a variable, *Innovative target ratio (ITR)*, the number of target firms that produce at least one patent in year *t* headquartered in a state, scaled by the number of private non-farm businesses in that state-year. (We collect data on the number of private non-farm businesses from the U.S. Census Bureau (<http://www.census.gov/>)). We plot *ITR* in event time in Fig. 2. The event year is the year when deregulation occurred in a particular state. This measure captures the innovative intensity of firms that are acquired within a state-year. We calculate annual averages of this measure from five years before to five years after the deregulatory event years. Fig. 2 shows a monotonic drop of *ITR* after deregulation, which is consistent with our argument that small, innovative firms are more likely to remain independent after deregulation because they gain better access to bank credit. This decrease in supply of innovative targets contributes to the post-deregulation drop of innovativeness of public corporations.

In summary, in this section, we discuss a third possible mechanism through which banking deregulation causes a reduction in innovation by public corporations. Deregulation allows small, innovative firms to access bank financing, which allows them to finance themselves and remain

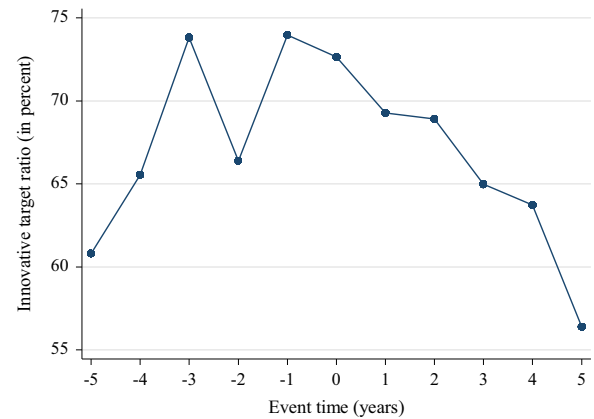


Fig. 2. Innovative target ratio of acquisition targets across state regulation changes. This figure plots states' average Innovative target ratios (*ITR*) from five years before to five years after a state-level legislative change. We calculate *ITR_t* by dividing the number of targets headquartered within a state with at least one patent filing in year *t* by the total number of private firms in that state-year. We transform the state-year observations to event time, with a state legislative change following IBBEA being an event. States with multiple legislative changes appear in the sample multiple times, once for each event.

independent instead of being acquired. This mechanism helps explain the robust, negative effect of bank deregulation on corporate innovation output documented in our baseline analysis.

6. Extensions and conclusion

We extend our work by examining the impact on innovation of the changes in bank entry and failure rates that resulted from the deregulation events we study in this paper. Subramanian and Yadav (2012) show that deregulation of bank entry enhances bank stability by lowering instances of bank failures. It is possible that our primary independent variable, *RSindex*, merely proxies for bank entry or failure rates. Therefore, we test whether our results are sensitive to controlling for bank entry and bank failures. In results tabulated in the Internet Appendix, we show that our results are robust to including these controls. We also extend our work by examining whether banking competition affects the way banks lend to external-finance-dependent companies. Using bank lending data from DealScan and the proxies for external finance dependence used in Section 5.1, we find suggestive evidence that banking competition leads to more loans and larger loans to external-finance-dependent companies. We report these results in the Internet Appendix, as well.

In conclusion, this paper examines the effect of banking competition on innovation. We exploit the staggered deregulation of state-level branching laws to identify changes in banking competition. We find that these events cause overall reductions in state-level innovation. This result is driven by corporations headquartered within states that deregulate. In contrast, we find that innovation increases among external-finance-dependent private firms. Branching deregulation expands access to credit for these firms, which relaxes their financial constraints and

that banking was repressed in the United States until branching deregulation was enacted.

Appendix A

See [Table A1](#).

Table A1
Variable definitions.

Variable	Definition
<i>Measures of innovation</i>	
<i>LnPat (state)</i>	Natural logarithm of one plus state j 's total number of patents filed (and eventually granted) in years $t+1$ through $t+3$.
<i>LnCite (state)</i>	Natural logarithm of one plus state j 's total number of citations received on the firm's patents filed (and eventually granted) in years $t+1$ through $t+3$.
<i>LnPat (corporation)</i>	Natural logarithm of one plus corporation i 's total number of patents filed (and eventually granted) in years $t+1$ through $t+3$.
<i>LnCite (corporation)</i>	Natural logarithm of one plus corporation i 's total number of citations received on the firm's patents filed (and eventually granted) in years $t+1$ through $t+3$.
<i>LnPat (firm)</i>	Natural logarithm of one plus firm i 's total number of patents filed (and eventually granted) in years $t+1$ through $t+3$.
<i>LnCite (firm)</i>	Natural logarithm of one plus firm i 's total number of citations received on the firm's patents filed (and eventually granted) in years $t+1$ through $t+3$.
<i>Generality (state)</i>	One minus the Herfindahl index of the three-digit technology class distribution of all the patents that cite a given patent. We then take the average for all patents generated by the state.
<i>Originality (state)</i>	One minus the Herfindahl index of the three-digit technology class distribution of all the patents that a given patent cites. We then take the average for all patents generated by the state.
<i>TargetCount</i>	<i>TargetCount</i> is a measure of the patents corporation i receives from acquiring targets and is calculated in two steps. First, we calculate the sum of all patents generated by a target during the 1976–2006 sample. Then we sum the number of patents generated by all the targets of corporation i in year t , which is the year that the targets are acquired.
<i>TargetCountPerAcq_{i}</i>	<i>TargetCountPerAcq</i> is a measure of the patents corporation i receives per target and is calculated in three steps. First, we calculate the sum of all patents generated by a target during the 1976–2006 sample. Then we sum the number of patents generated by all the targets of corporation i in year t , which is the year that the targets are acquired. Finally, we divide by the number of targets acquired by corporation i in year t .
<i>Measure of deregulation and control variables used in baseline specifications</i>	
<i>RSindex_{i}</i>	Rice-Strahan index of interstate banking deregulation based on Rice and Strahan (2010) . It ranges from zero (deregulated) to four (highly regulated) based on regulation changes in a state.
<i>Intra</i>	An indicator variable that takes the value of zero prior to intrastate deregulation during the 1970s, 1980s, and 1990s as described in Jayaaratne and Strahan (1996) . The indicator variable takes the value of zero prior to interstate deregulation during the 1970s, 1980s, and 1990s as described in Jayaaratne and Strahan (1996) . The indicator variable takes the value of zero prior to interstate deregulation during the 1970s, 1980s, and 1990s as described in Jayaaratne and Strahan (1996) .

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