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Credit supply and corporate innovation ☆

Mario Daniele Amore [a], Cédric Schneider [b], Alminas Žaldokas [c,*]

[a] Bocconi University, Italy
[b] Copenhagen Business School, Denmark
[c] Hong Kong University of Science and Technology, Hong Kong

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ABSTRACT

We present evidence that banking development plays a key role in technological progress. We focus on manufacturing firms' innovative performance, measured by patent-based metrics, and employ exogenous variations in banking development arising from the staggered deregulation of banking activities across US states during the 1980s and 1990s. We find that interstate banking deregulation had significant beneficial effects on the quantity and quality of innovation activities, especially for firms highly dependent on external capital and located closer to entering banks. Furthermore, we find that these results are strongly driven by a greater ability of deregulated banks to geographically diversify credit risk.

> The banker […] has either replaced private capitalists or become their agent; he has himself become the capitalist par excellence. He stands between those who wish to form new combinations and the possessors of productive means. He is essentially a phenomenon of development, though only when no central authority directs the social process. Schumpeter (1911).

1. Introduction

It has long been argued that well-functioning financial systems are essential for promoting economic and technological progress [see Schumpeter (1911), or a more recent discussion in King and Levine (1993a)]. For instance, financial intermediaries channel savings to investment (Bencivenga and Smith, 1991) and increase the productivity of that investment by allocating funds to the most qualified firms (Greenwood and Jovanovic, 1990; King and Levine, 1993b). However, less is known about how this effect differs across financial intermediaries. While previous literature has clearly shown that venture capital and private equity firms foster innovation (e.g., Kortum and Lerner, 2000; Lerner, Sørensen, and Strömberg, 2011), the effect of banks on technological progress remains a matter

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* Corresponding author.
E-mail address: alminas@ust.hk (A. Žaldokas).

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of debate, due to theoretically ambiguous predictions as well as endogeneity concerns. Arguably, general economic conditions, industry characteristics, and unobserved factors could influence both firms' innovation and credit availability, thus biasing the effect of finance on technological progress. Alternatively, the effect could even be reversed if firms with higher value-added projects create demand for more efficient financial institutions that then arise endogenously (Laeven, Levine, and Michalopoulos, 2012). In general, the literature is skeptical that innovative firms, especially listed ones, could benefit from bank credit to finance their operations (Atanassov, Nanda, and Seru, 2007).

Contrary to this view, we find strong evidence that banking development influences innovation by publicly traded manufacturing firms. In our empirical design, we employ the staggered passage of interstate banking deregulation in the US banking industry during the 1980s and 1990s as a source of exogenous variation in the geographic spread of US banking institutions. By allowing bank holding companies to expand across states, this state-level deregulation increased the credit supply, was associated with the adoption of better screening and monitoring technologies, and facilitated banks' geographic diversification of credit risk (Demyanyk, Østergaard, and Sørensen, 2007; Goetz, Laeven, and Levine, 2012). After controlling for a host of firm characteristics, firm fixed effects, and other confounding factors, we find that interstate banking deregulation caused a 12.6% rise in the number of patents granted to firms. Furthermore, we find a 10.1% increase in the *importance* of patents, measured by citations received from future patent applications, as well as an increase in citations' dispersion, and in the *originality* and *generality* of patents. Taken together, these findings suggest that firms exposed to deregulation adopted a bolder innovation policy.

Focusing on the supply side, we claim that the main channel behind this change in innovation policy relates to a greater willingness of deregulated banks to take risks once they become better diversified geographically. Out-of-state banks may be willing to lend on more favorable terms and all the more so if credit risk in the deregulating state is less correlated with their existing exposure. We find that the positive effect of deregulation on innovation occurred primarily among firms located in states whose economies exhibited the least comovement with the overall US economy, and in states least comoving with the entering banks' home states. Moreover, the effect was present primarily among firms in states that after deregulation experienced the largest change in the average geographic diversification of banks. Finally, the effect was larger for firms located closer to the entering out-of-state banks, i.e., for firms subject to a larger credit expansion.

We find that the positive effect of banking deregulation on corporate innovation was highly heterogeneous. In particular, the effect was larger for firms operating in industries that were highly dependent upon external capital, and firms that tend to rely more on bank debt. Finally, the effect was larger for firms with a high level of research and development (R&D) expenditures in the post-deregulation period. Taken together, these results suggest that the effect of deregulation on innovation was driven by relaxation of financial constraints for bank-dependent firms.

The existing body of research does not suggest an unambiguous effect of banking on innovation. On the one hand, a debt contract might be ill suited to finance an activity such as innovation that has uncertain returns (Atanassov, Nanda, and Seru, 2007; Stiglitz, 1985). On the other hand, public firms can use private debt to fund innovation when they incur costs to raise capital in public markets. Indeed, funding innovation with public capital can provide sensitive information to competitors (Bhattacharya and Ritter, 1983; Maksimovic and Pichler, 2001), or it can be costly to the manager because of low tolerance for failure in the public markets (Ferreira, Manso, and Silva, in press). Mixed empirical findings mirror these theoretical ambiguities. For instance, Benfratello, Schiantarelli, and Sembenelli (2008), Ayyagari, Demirgüç-Kunt, and Maksimovic (2011), Smith (2011), and Nanda and Nicholas (2011) find a positive effect, while Atanassov, Nanda, and Seru (2007) argue against the importance of bank finance for firms' innovative activity. Our evidence indicates that interstate banking deregulation fostered corporate innovation; yet, it does not necessarily imply that, following deregulation, firms financed innovation projects directly with bank loans. For instance, by employing deregulated banks' debt to finance traditional investment, firms could have diverted more internal resources to innovation expenses or deregulated banks could have fostered the development of non-bank financial institutions, which in turn provided funding for innovative activities.[1] Finally, individuals could have increased borrowing capacity, and due to home bias, channeled their portfolio investments to the nearby firms' equity offerings.

Laeven, Levine, and Michalopoulos (2012) present a theoretical model in which technological innovation can only happen with improvements of the financial systems. Indeed, if there were no constraints to the growth of financial systems, financial development would arise endogenously based on the expected technological progress. Thus, to test whether financial systems drive innovation, one must employ a setting in which the external constraints to financial development are removed. Our empirical setting fully satisfies this condition, since our identification is based on the exogenous boost of financial innovation in the banking sector following deregulation events. Banking deregulation can be seen as the financial innovation in the sense of Laeven, Levine, and Michalopoulos (2012) as it reduces the costs of monitoring or screening potential inventors. Our causal evidence thus contributes to the earlier literature on the positive association between banking development and economic growth (King and Levine, 1993a; Jayaratne and Strahan, 1996; Demirgüç-Kunt and Maksimovic, 1998; Levine and Zervos, 1998), as well as to the broad research on the relationship between financial development and growth

[1] Indeed, in unreported analyses, we find that the amount of new funds raised by venture capital and private equity firms was positively correlated with interstate banking deregulation.

(King and Levine, 1993b; Guiso, Sapienza, and Zingales, 2004).[2]

Existing studies have investigated the effect of banking deregulation on entrepreneurship and Schumpeterian creative destruction (Bertrand, Schoar, and Thesmar, 2007; Black and Strahan, 2002; Cetorelli and Strahan, 2006; Kerr and Nanda, 2009, 2010).[3] There is also a contemporaneous research on banking and innovation, which has found mixed effects depending on the type of deregulation enacted (Chava, Oettl, Subramanian, and Subramanian, 2012; Cornaggia, Tian, and Wolfe, 2012; Hombert and Matray, 2012). We add to this evidence by not only showing that interstate banking deregulation had positive effects on listed manufacturing firms' innovative activity, but also by highlighting a new channel behind our findings, i.e., the higher willingness of deregulated banks to take risk due to geographic diversification.

Finally, our study complements a recent literature that, motivated by the recent financial crisis and dry-up of credit, investigates how variations in access to external finance affect corporate policies (Campello, Graham, and Harvey, 2010; Duchin, Ozbas, and Sensoy, 2010; Leary, 2009; Lemmon and Roberts, 2010). Although we do not establish whether the increase in innovation stems directly from bank lending to innovative firms, our results reinforce the notion that changes in the supply of credit have strong effects on corporate policies.

Section 2 describes the policy changes that transpired in the US banking industry. Section 3 presents the data. Section 4 outlines our empirical methodology. Section 5 presents our main finding. Section 6 shows the effect of deregulation on patenting quality, technological nature, and risk. Section 7 relates our results to the improved ability of banks to diversify credit risk, and also shows variations depending on firms' location and reliance on external finance. Section 8 discusses the association between innovation and industry growth. Section 9 concludes.

2. Deregulation in the US banking industry

Our identification strategy exploits the staggered passage of interstate banking deregulation, which represented a positive shock to the geographic spread of US banking institutions.

The geographic expansion of banking activities in the US has been historically restricted by laws such as the McFadden Act of 1927 and the Douglas Amendment to the Bank Holding Company Act of 1956. However, during the period of 1970–1990s, US states largely terminated the restrictions on the expansion of banks across and within states. The first state to pass an interstate banking deregulation was Maine in 1978, followed by Alaska and New York in 1982. The wave of deregulation continued until the mid-1990s, when piecemeal changes in legislation, outside events, and competition among regulators led states to permit some type of interstate banking on a reciprocal or nonreciprocal basis (Johnson and Rice, 2008). Table 1, Panel A, illustrates the timeline of interstate banking deregulation by state and year.

After interstate banking deregulation, out-of-state bank holding companies were allowed to acquire banks chartered in the deregulating states. Banks took advantage of these deregulation laws and indeed expanded across state borders. In the average state, the fraction of assets held by out-of-state bank holding companies rose from 0% in mid-1970 to 23% in mid-1990. The total number of banks in the US fell but this reduction was mostly driven by a drop in the number of small local banks (Kerr and Nanda, 2009) due to an intense merger and acquisition (M&A) activity that spurred banks' efficiency (Jayaratne and Strahan, 1998). The expansion of banks across state lines also led to an increase in credit supply. Using state-level data on commercial bank loans provided by the Federal Deposit Insurance Corporation (FDIC) for the period of 1976–1995, we find that, after controlling for year and state fixed effects, interstate banking deregulation was associated with an 8% increase in total net loan supply. Moreover, expanding out-of-state banks used more sophisticated monitoring and screening technologies than local banks (Dick and Lehnert, 2010). Finally, fewer restrictions on banking across states improved the scope for geographic diversification (Goetz, Laeven, and Levine, 2012), allowing banks to finance risky projects, such as innovation activities, more freely and without increasing the banks' overall risk.

The period of 1970–1990s was associated with other policy changes in the banking industry that reduced the obstacles for existing banks to open new branches within and between US states. As shown in Table 1, Panel B, US states lessened restrictions on intrastate banking during the mid-1970s and 1980s. Furthermore, the Riegle-Neal Interstate Banking and Branching Efficiency Act (IBBEA) enacted a nationwide deregulation of the banking sector. As of 1995, the IBBEA removed any remaining federal restrictions on interstate banking. Moreover, the IBBEA permitted national or state banks to engage in interstate branching (see Table 1, Panel C), although it also allowed states considerable leeway in deciding the rules governing entry by out-of-state branches (Rice and Strahan, 2010; Johnson and Rice, 2008). In the empirical analysis, we control for these confounding policies.

One concern with our identification strategy is that deregulation could have been correlated with pre-existing trends in financial and economic development in the legislating states. In other words, what matters for our specification is that states deregulating at different points in time did not have diverging pre-deregulation trends in innovation, though they could have had different levels of financing conditions, as suggested by Kroszner and Strahan (1999) who find that legislators were more in favor of interstate deregulation when the fraction of small banks in their state was low. In the case of diverging innovation trends, our empirical approach would reflect pre-deregulation trends rather than an

[2] See Levine (2005) for a comprehensive review of this literature.

[3] Furthermore, existing works have shown an effect of banking deregulation on income distribution (Beck, Levine, and Levkov, 2010), industry reallocation (Acharya, Imbs, and Sturgess, 2011; Bertrand, Schoar, and Thesmar, 2007), trade flows (Michalski and Ors, 2012), and output volatility (Correa and Suarez, 2009).

Table 1
Major deregulation in the US banking industry.

Panel A: Interstate banking deregulation	
Year	State
1978	Maine
1982	New York, Alaska
1983	Connecticut, Massachusetts
1984	Rhode Island, Utah, Kentucky
1985	North Carolina, Ohio, Virginia, District of Columbia, Nevada, Maryland, Idaho, Georgia, Tennessee, Florida
1986	Arizona, New Jersey, South Carolina, Pennsylvania, Oregon, Michigan, Illinois, Indiana, Missouri, Minnesota
1987	California, Alabama, Washington, New Hampshire, Texas, Oklahoma, Louisiana, Wyoming, Wisconsin
1988	Delaware, Vermont, South Dakota, Mississippi, West Virginia, Colorado
1989	New Mexico, Arkansas
1990	Nebraska
1991	North Dakota, Iowa
1992	Kansas
1993	Montana
After 1993	Hawaii
Panel B: Intrastate branching deregulation	
Year	State
Before 1981	Maine, Alaska, Rhode Island, North Carolina, Virginia, District of Columbia, Nevada, Maryland, Idaho, Arizona, South Carolina, Delaware, California, Vermont, South Dakota, New York, New Jersey, Ohio, Connecticut
Between 1981 and 1985	Utah, Alabama, Pennsylvania, Georgia, Massachusetts, Tennessee, Oregon, Washington, Nebraska
Between 1986 and 1990	Mississippi, Hawaii, Michigan, New Hampshire, West Virginia, North Dakota, Kansas, Florida, Illinois, Texas, Oklahoma, Louisiana, Wyoming, Indiana, Kentucky, Missouri, Wisconsin, Montana
After 1990	Colorado, New Mexico, Minnesota, Arkansas, Iowa
Panel C: Interstate Banking and Branching Efficiency Act (IBBEA)	
Provision	Implementation
Interstate banking	Took effect in September 1995; permitted the Board of Governors of the Federal Reserve System to approve interstate bank acquisitions, regardless of whether such acquisitions would have been permitted under "the law of any State." States were not permitted to opt out of the interstate banking rules
Interstate branching	Took effect in June 1997; permitted national or state bank to engage in interstate branching. States could opt out of interstate branching or impose restrictions such as the minimum age of the target institution to be acquired or statewide deposit cap

increase in innovation due to the exogenous changes in credit markets.

We rule out this concern in several ways. First, we draw on the political economy of deregulation. Interstate banking deregulation was partly driven by the savings and loans crisis in the early 1980s, after which federal legislators enacted the Garn-St. Germain Depository Institutions Act. One provision of this act authorized federal banking agencies to arrange interstate acquisitions for failed banks with total assets of over $500 million, even when such acquisitions were not in accordance with state law. These changes paved the way for bilateral and regional agreements between states to allow interstate banking and thus the creation of larger and more diversified banks that were less susceptible to failure (Kerr and Nanda, 2009). Importantly, the adoption of these agreements was not characterized by clear patterns (Amel, 1993). For instance, Goetz, Laeven, and Levine (2012) find no evidence that states were more likely to sign agreements with neighbor states than with distant states. Overall, it is unlikely that the adoption of agreements, and the resulting sequence of deregulation events, mirrored differences in innovation potential across states. Empirically, we show that deregulation did not have a significant effect on innovation prior to the actual passage of the deregulation laws (see Table 4, Panel A), and thus that our estimates do not merely reflect pre-deregulation innovation trends. Moreover, we check that the average number of all pre-deregulation patents in a given state does not significantly influence the timing of deregulation (p-value$=$0.30).

3. Data and summary statistics

We measure innovation by successful patent applications, a widely used approach to quantify innovative performance (Griliches, 1990). We start with the patent data set assembled at the National Bureau of Economic Research (NBER), which contains information on all the patents awarded by the US Patent and Trademark Office (USPTO) as well as the citations made to these patents (Hall, Jaffe, and Trajtenberg, 2001). We focus the analysis on granted patents applied for in the period 1976–1995, which covers all years when interstate banking rules were deregulated but also includes a few years before the passage of the first interstate banking deregulation, in 1978. We do not extend our sample after 1995 as this was the year when the interstate *banking* provisions of the IBBEA went into effect. Ending our sample in the mid-1990s also ensures that our findings are not contaminated by the passage of state laws surrounding the IBBEA implementation that affected the evolution of interstate *branching* deregulation from 1994 to 2005 (Johnson and Rice, 2008; Rice and Strahan, 2010).[4]

Following the literature on US banking deregulation, we exclude Delaware and South Dakota because these states were subject to special tax incentives for credit card banks (Black and Strahan, 2002; Dick and Lehnert, 2010).

Conducting the analysis at the firm level, rather than at the state level, is particularly important for two reasons. First, firm-level data allow controlling for unobserved time-invariant firm effects, and thus better mitigate the concern of omitted factor bias. Second, they permit to look into the heterogeneous response to deregulation within a given state. To this purpose, we match the NBER patent data set with Compustat data following the procedures developed in Hall, Jaffe, and Trajtenberg (2001) and Bessen (2009). We exclude firms with negative or zero book value of assets and sales, and firms headquartered outside the US.[5] As shown in Scherer (1983) and more recently in Balasubramanian and Sivadasan (2011), the bulk of patenting activity occurs within the manufacturing sector. Thus, following, e.g., Hall, Jaffe, and Trajtenberg (2005), we only consider firms in the industries with standard industrial classification (SIC) codes up to 4000 (mostly manufacturing firms). In this way, we exclude industries such as financial services or utilities, which typically operate under specific regulations, as well as the software industry. Since the latter is primarily dependent upon non-debt finance such as equity and venture capital, we believe that our identification strategy is in any case less applicable for identifying the banking-innovation nexus in the software industry.

Table 2 reports summary statistics for the sample obtained after dropping observations with missing values in the explanatory variables described in the next section. As shown in previous work on the Compustat-NBER patent data set, citation statistics are very skewed. In our sample, the average number of patents is approximately ten but the median is zero. The detailed construction of all control variables is described in Appendix A.

4. Methodology

We use a difference-in-differences model to explore the causal relationship between firm innovation and interstate banking deregulation. The important advantage of this approach is that we can control for omitted variables and absorb nationwide shocks or common trends that might affect the outcome of interests.

We conduct our analysis using firm-level patent data and exploiting the information on the location of the firm's headquarter. Our key variable of interest is *Interstate deregulation*$_{jt}$ which is equal to one if a firm is headquartered in a state j that has passed an interstate banking deregulation by time t, and zero otherwise.[6] Hence, *Interstate deregulation*$_{jt}$ captures the effect of interstate banking deregulation on patenting across states by comparing firm outcomes before and after each deregulation year vis-à-vis deregulation passed later. To deal appropriately with the nature of our innovation measures, we employ count data models that are widely used in the econometric analysis of patents. Following Hausman, Hall, and Griliches (1984), we hypothesize that the expected number of patents is an exponential function of the interstate deregulation treatment, *Interstate deregulation*$_{jt}$, and other explanatory variables $X_{ij\tau-1}$. More specifically, we estimate Poisson models with conditional mean:

$$E[Y_{ijt}|\text{Interstate deregulations}_{jt}] = \exp(\alpha + \beta\ \text{Interstate deregulations}_{ijt} + \eta_i + \tau_t) \quad (1)$$

The model is estimated by the method of Quasi-Maximum-Likelihood (QMLE), which provides consistent estimates as long as the conditional mean is correctly specified even if the true underlying distribution is not Poisson (Wooldridge, 1999). Since our deregulation treatment is defined at the state level, we cluster standard errors by state. Given that US patenting activity increased substantially starting in the mid-1980s (see, e.g., Hall, 2004), we control for aggregate trends by including a full set of year dummies, denoted by τ_t. We also control for industry linear trends by estimating annual three-digit SIC industry averages of the dependent variables, computed excluding the firm in question.[7] Furthermore, we include firm fixed effects, denoted by η_i, and a vector $X_{ij\tau-1}$ of time-varying controls, all lagged by one year to reduce simultaneity concerns. Specifically, we control for the logarithm of firm sales and capital-labor ratio, following the literature on the production function of patents (see, e.g., Galasso and Simcoe, 2011; Aghion, Van Reenen, and Zingales, 2013). In additional analyses, we include other lagged controls such as firm age and asset tangibility, to control for existing dependence and access to bank credit; the stock of R&D[8] to establish the effect on firms' innovative

[4] Another advantage of ending the sample in 1995 is that our analysis is not affected by the dramatic increase in cash flow and equity financing of R&D activities experienced by young US firms during the second half of the 1990s (Brown, Fazzari, and Petersen, 2009).

[5] A concern arises from the fact that Compustat only reports the last state of operations, and we may be unable to observe changes of headquarters from one state to another that are potentially endogenous to the deregulation. However, using data on headquarter relocations from the Compact Disclosure database, Pirinsky and Wang (2006) argue that most of the headquarter changes are driven by mergers and acquisitions. After excluding these and other major restructuring events, they find 118 relocations from a sample of more than 4,000 firms in the period 1992–1997. Our results are robust to excluding firm-year observations with asset or sales growth exceeding 100%, which are typically associated with mergers, restructuring, and other major corporate events (Almeida, Campello, and Weisbach, 2004). Furthermore, in a robustness check reported in Section 5.3, we alternatively consider the state of the first inventor as recorded in the NBER patent data set.

[6] As shown by, e.g., Bharath, Dahiya, Saunders, and Srinivasan (2011) and Dass and Massa (2011), because information gathering and processing is easier with smaller physical distance between a lender and a borrower, even public firms have a strong propensity to borrow from local lenders. We assume that firms should be primarily affected by the banking deregulation in the state of their headquarters.

[7] Our results hold if we estimate the model with OLS and control for industry effects by including the interactions between industry and year dummies, as suggested by Gormley and Matsa (2012).

[8] As stressed by Aghion, Van Reenen, and Zingales (2013), not controlling for the R&D stock implies that the coefficient of the variable of interest on the right-hand side will reflect both the increase in R&D expenditures and the productivity of R&D. By contrast, when the R&D

Table 2
Firm characteristics.
This table illustrates summary statistics. Ln (patents) is the logarithm of a firm's number of patents. Patent counts represent a firm's number of patents. Cite-weighted patent counts represent a firm's patents weighted by the number of future citations and adjusted for truncation. R&D to total investment is the ratio of R&D expenditures to the sum of R&D and capital expenditures. Ln (sales) is the logarithm of a firm's sales. Ln (K/L) is the logarithm of capital-to-labor ratio. Ln (age) is the logarithm of 1 plus the number of years a firm has been in Compustat. Asset tangibility is non-current and non-intangible assets divided by total assets. ROA is return on assets, measured as the ratio of earnings before interest and depreciation (EBITDA) divided by the book value of assets. Cash holdings is the ratio of cash and marketable securities to total assets. HHI is the Herfindahl-Hirschman Index, computed as the sum of squared market shares based on sales in a given three-digit SIC industry in each year. See Appendix A for a full description of each variable.

	Number of observations	Mean	Standard deviation	Median
Panel A: Innovation measures				
Ln (patents)	11,276	1.626	1.514	1.386
Patent counts	22,894	10.183	40.252	0
Cite-weighted patent counts	22,894	155.420	767.134	0
Ln (R&D)	21,361	0.876	2.211	0.798
Panel B: Firm characteristics				
Ln (sales)	22,828	4.273	2.414	4.195
Ln (K/L)	22,617	2.841	1.013	2.805
Ln (age)	22,894	2.499	0.800	2.565
Asset tangibility	21,942	0.336	0.171	0.316
ROA	22,625	0.091	0.194	0.134
Cash holdings	22,833	0.139	0.173	0.070
HHI	22,854	0.187	0.128	0.149

productivity; return on assets (ROA), and cash holdings to control for the role of internal resources in financing innovation (Himmelberg and Petersen, 1994); and the Herfindahl-Hirschman Index (HHI), based on the distribution of revenues of the firms in a particular three-digit SIC industry, to control for the impact of industry concentration on innovation.

5. Innovation activity

5.1. Innovation outputs

Table 3, column 1, shows that allowing out-of-state banks to enter a given state increased the expected patent counts by 13.8%. While in column 1 we only control for firm and year fixed effects, in column 2 we also control for the logarithm of sales and capital-to-labor ratio, and in column 3 we further control for the stock of R&D. As expected, the stock of R&D has a positive and significant effect on patenting; however, the deregulation coefficient remains significant at 1%. In column 4, we confirm this finding by including a host of industry-level and firm-level factors that could potentially affect innovation, such as HHI, firm age, ROA, tangibility, and cash holdings (coefficients are unreported to save space). As shown, the deregulation coefficient remains both statistically and economically relevant, indicating a 12.6% increase in patenting.

Table 3
Innovation outcomes.
This table reports Poisson regression results. We use patent counts as dependent variable. Column 4 includes an additional set of firm and industry controls. Specifically, they include one-year lagged Ln (age), HHI, ROA, tangibility, and cash holdings. Coefficients, unreported to save space, are available upon request. The construction of control variables is described in Appendix A. Standard errors clustered by state of operation are reported in parentheses. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Dependent variable: Patent counts				
	(1)	(2)	(3)	(4)
Interstate deregulation	0.1293**	0.1168**	0.1169***	0.1188***
	(0.0639)	(0.0472)	(0.0450)	(0.0397)
Ln (sales)		0.7096***	0.4980***	0.5360***
		(0.0626)	(0.0782)	(0.0901)
Ln (K/L)		0.2566***	0.2443***	0.1969**
		(0.0632)	(0.0764)	(0.0789)
Ln (R&D stock)			0.3629***	0.3264***
			(0.1194)	(0.1196)
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Industry trends	Yes	Yes	Yes	Yes
Additional controls	No	No	No	Yes
Number of obs.	18,066	18,066	18,066	18,066

5.2. Dynamic effects

The real consequences of interstate banking deregulation on credit markets caused by the actual entry of banks in the new states could manifest over several years after the deregulation passage. Also, filing a patent is the outcome of a process that might take some time. In this section, we look at how the patenting activity evolved after the deregulation was enacted.

We test for dynamic effects by drawing on specifications similar to Kerr and Nanda (2009). First, we construct a dynamic difference-in-differences model employing a set of dummies that measure the distance in years from each deregulation passage, using as the reference group the period of three years or earlier before deregulation. Results, reported in Table 4, Panel A, show that the coefficient prior to deregulation is small and statistically insignificant, thus indicating that our results are not driven by diverging pre-deregulation trends. By contrast, the post-deregulation coefficients are all positive and significant at conventional levels. Importantly, they become larger as we move forward from the reform year, with the largest effect corresponding to six and seven years after interstate banking deregulation.

Second, we allow the effect of deregulation on innovation to grow linearly over time, using a variable equal to zero up to the deregulation year and then equal to the

(*footnote continued*)
stock is included in the specification, the effect of the variables of interest can be interpreted as an effect on the innovative productivity of firms. The R&D stock is computed following the conventional 15% depreciation rate used in the related literature (see, e.g., Hall, Jaffe, and Trajtenberg, 2005). Also, we use linear interpolations to replace missing values of R&D; however, our results are robust to leaving those observations missing or treating them as zeros.

Table 4
Dynamic effects.

This table reports Poisson regression results using a dynamic specification. We use patent counts as dependent variable. In Panel A, the response to interstate deregulation is modeled by leads and lags consolidated into two-year increments extending from two years before to eight years or more after the deregulation. Coefficients are relative to the period three years or earlier before deregulation. In Panel B, Years since interstate deregulation is a variable equal to the number of years after the deregulation passages, with a long-term effect at eight years, and equal to zero before the deregulation. The construction of control variables is described in Appendix A. Standard errors clustered by state of operation are reported in parentheses. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Panel A: Time dummies

Dependent variable: Patent counts	(1)	(2)
Years 1–2 before deregulation	0.0358	0.0424
	(0.0322)	(0.0263)
Years 0–1 after deregulation	0.1710***	0.1759***
	(0.0567)	(0.0430)
Years 2–3 after deregulation	0.1969**	0.2181***
	(0.0924)	(0.0816)
Years 4–5 after deregulation	0.2453**	0.2705***
	(0.1110)	(0.1033)
Years 6–7 after deregulation	0.3904***	0.4129***
	(0.1335)	(0.1285)
Years 8+ after deregulation	0.4121***	0.4355***
	(0.1123)	(0.1084)
Ln (sales)	0.4942***	0.5321***
	(0.0790)	(0.0898)
Ln (K/L)	0.2445***	0.1988***
	(0.0762)	(0.0770)
Ln (R&D stock)	0.3663***	0.3292***
	(0.1184)	(0.1186)
Firm fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Industry trends	Yes	Yes
Additional controls	No	Yes
Number of obs.	18,066	18,066

Panel B: Linear treatment effects

Dependent variable: Patent counts	(1)	(2)
Years since deregulation	0.0324*	0.0328*
	(0.0189)	(0.0179)
Ln (sales)	0.4970***	0.5357***
	(0.0797)	(0.0905)
Ln (K/L)	0.2269***	0.1889**
	(0.0679)	(0.0745)
Ln (R&D stock)	0.3690***	0.3328***
	(0.1232)	(0.1232)
Firm fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Industry trends	Yes	Yes
Additional controls	No	Yes
Number of obs.	18,066	18,066

number of years since a deregulation was passed, capping the treatment effect at eight years. Results, reported in Table 4, Panel B, confirm that interstate deregulation had a growing impact on firms' patenting activity.

5.3. Robustness

In Table 5, we present a number of robustness checks. To save space, regression results are reported in rows rather than columns. We start by excluding firms headquartered in California and Massachusetts, which account for 24% of observations in our sample. Row 1 shows that excluding these states does not materially affect our main result.

As Kerr and Nanda (2009) show, interstate banking deregulation fostered the creation and closure of firms. Hence, one concern is that our results are driven by firm entry. We assess this issue by restricting the analysis to firms that are present in the sample from 1976 to the last year, 1995. As shown in row 2, adding this restriction does not significantly alter our estimates despite the large drop in sample size.

In our main analyses, we infer firms' location based on the location of the headquarters as reported in Compustat. However, R&D centers of publicly traded firms could be located across multiple states. In row 3, we infer these multiple locations from the states of the first inventor in the NBER patent database and apply the deregulation treatment according to inventors' states. The effect of deregulation on innovation is equally strong.

In row 4, we provide a more restrictive specification which controls for state-level macroeconomic variables that are potentially correlated with both credit availability and innovation activities. Specifically, in addition to the usual controls used in Table 3, column 4, we control for the GDP growth and logarithm of population, obtained from the US Bureau of Economic Analysis (BEA). Our results are only marginally affected by the inclusion of these controls.

Next, we control for geographic trends. As shown in row 5, our findings are unchanged if we augment our specification with regional trends, computed as year averages of the dependent variables by region excluding the firm in question.[9]

Furthermore, we show that our results are not affected by the time period considered. As discussed in Section 2, the interstate banking and branching provisions of the

[9] Regions are defined according to the four-grouping classification provided by the US Census: west, midwest, northeast, and south (http://www.census.gov/geo/www/us_regdiv.pdf). Re-examining the findings in Black and Strahan (2002), Wall (2004) shows that the effect of deregulation on entrepreneurship was positive in some US regions but significantly negative in others. If we estimate region-specific deregulation effects, we find that the interstate deregulation coefficients are all positive, though their statistical and economic significance is not homogeneous across US regions.

Table 5
Robustness checks.

This table reports Poisson regression results using patent counts as the dependent variable and the binary interstate banking deregulation treatment as main explanatory variable, unless differently specified. Each regression includes firm and time fixed effects, industry trends, as well as an additional set of firm and industry controls. Specifically, it includes one-year lagged Ln (age), HHI, ROA, tangibility, and cash holdings. Coefficients, unreported to save space, are available upon request. The construction of control variables is described in Appendix A. Row 1 excludes firms headquartered in California and Massachusetts. Row 2 restricts the analysis to firms that remain in the sample from 1976, i.e., the first year of our analysis until 1995, i.e., the last year of our sample. In row 3, we apply the deregulation treatment according to first inventors' states, as recorded in the NBER patent database. Row 4 includes state-level macroeconomic controls, such as the GDP growth and logarithm of population, both obtained from the US Bureau of Economic Analysis. Row 5 includes regional trends, computed as annual average of the dependent variable by US region (as defined by the US Census), excluding the firm in question. Row 6 adopts a sample extended up to 1997, whereas row 7 restricts the sample up to 1994. In row 8, the sample starts in 1978. Row 9 replaces our main deregulation dummy with a continuous treatment equal to the logarithm of the number of states from which the entry of banks has been permitted by the particular year. To avoid dropping zero values, we add 0.1 to the number of states before taking the logarithm. Row 10 uses the continuous treatment of row 9 weighted by the population in each state. Row 11 includes a dummy that captures the effect of interstate branching deregulation. Row 12 extends our sample up to 2006 and further controls for the Rice and Strahan (RS) (2010) index of restrictions to interstate branching. Row 13 controls for anti-takeover legislations by including a dummy for the passage of BC laws. Row 14 excludes "Semiconductors and Related Devices" technological field (SIC 3674). Row 15 excludes 1% of outliers, whereas row 16 excludes 2.5% of outliers. Row 17 additionally controls for the square of firm sales and the squared HHI. Row 18 additionally controls for the logarithm of amount of venture capital and private equity funding in a given state and year. Standard errors clustered by state of operation are reported in parentheses. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Dependent variable: Patent counts			
	Interstate deregulation coefficient	Standard error	Number of obs.
(1) Excluding California and Massachusetts	0.1044***	(0.0347)	13,894
(2) Constant sample	0.1144***	(0.0418)	12,333
(3) State as in inventor's location	0.0847**	(0.0347)	13,235
(4) Including state-level controls	0.1239***	(0.0414)	18,063
(5) Including regional trends	0.1315***	(0.0366)	18,066
(6) Time window up to 1997	0.1339*	(0.0702)	20,571
(7) Time window up to 1994	0.1051***	(0.0365)	16,969
(8) Time window starting in 1978	0.1155***	(0.0372)	15,801
(9) Continuous deregulation treatment	0.0260*	(0.0137)	18,066
(10) Population-weighted continuous deregulation treatment	0.0064***	(0.0024)	18,066
(11) Controlling for intrastate deregulation	0.1386***	(0.0395)	18,066
(12) Controlling for RS index, time window up to 2006	0.1634*	(0.0886)	31,994
(13) Controlling for BC laws	0.1130***	(0.0433)	16,070
(14) Excluding "Semiconductors and Related Devices"	0.0945***	(0.0335)	17,445
(15) Excluding 1% observations on the right tail	0.1111***	(0.0416)	17,837
(16) Excluding 2.5% observations on the right tail	0.1096***	(0.0394)	17,477
(17) Controlling for firm size squared and HHI squared	0.1068***	(0.0359)	18,066
(18) Controlling for VC funding	0.1169***	(0.0385)	18,066

IBBEA enacted a nation-wide deregulation of banking activities. In our main analysis we consider 1995 as the last year of the sample, i.e., the year when the interstate banking provisions of the IBBEA were implemented. Yet, in rows 6 and 7 we show that our results do not change if we end our sample in 1994 (one year before the nation-wide interstate banking deregulation enacted by the IBBEA) or if we extend the sample up to 1997 (the year the nationwide interstate branching deregulation was enacted by the IBBEA). In row 8, we also check that our results hold if we start the analysis in 1978, the first interstate banking deregulation year.

A concern with our identification is that, while our main treatment dummy is based on the year when the state enacted its first agreement allowing out-of-state banks to establish operations (most often through M&A), in almost all cases those first agreements were effective on a regional reciprocal level.[10] Our dummy variable is unable to capture this greater variation across states and time. We accommodate this issue by building on the data from Amel (1993). Specifically, we construct a finer variable equal to the logarithm of the number of states from which the entry of banks has been permitted by the particular year. Thus, we are able to exploit not just whether a state deregulated of not, but also the "depth" of the deregulation process. Row 9, in which we report the result obtained using this variable instead of our main deregulation dummy, confirms our main insight that interstate deregulation had a significant and positive impact on firm innovation. One might be further concerned that states differed in terms of importance. We reconfirm this result

[10] For instance, on July 1, 1986, Minnesota enacted a law on reciprocal terms allowing out-of-state bank entry from Iowa, North Dakota, South Dakota, and Wisconsin. On August 1, 1988, the region was expanded to include Colorado, Idaho, Illinois, Kansas, Missouri, Montana, Nevada, Washington, and Wyoming. On 1990, the region was further expanded to include Indiana, while in 1992 to include Michigan

(*footnote continued*)
and Ohio. On April 22, 1994, Minnesota allowed an entry from any state banks based on reciprocal terms while on September 25, 1995, the legislatures extended it to hold nationally on nonreciprocal terms. However, to identify the actual states from where entry was permitted, we have to take into account reciprocity. For instance, none of the states from which Minnesota allowed entry in 1986 reciprocated immediately and in 1987, only Wisconsin did so. In 1988, South Dakota, Idaho, Washington, and Wyoming reciprocated. Thus, in 1987, Minnesota allowed entry from one state, while in 1988, five states. Further analysis shows that the number of states was extended to eight in 1990, 11 in 1991, 13 in 1992, 14 in 1993, 41 in 1994, and all US states in 1995.

in row 10 using the logarithm of the population from which the entry of banks has been permitted by the particular year (similarly to Goetz, Laeven, and Levine, 2012).

Next, we deal with other policies potentially affecting innovation that were adopted around the same period as the interstate banking deregulation. As discussed in Table 1, Panel B, US states also deregulated intrastate branching activities during the period considered in our analysis. A concern for our analysis is that US states might have deregulated intrastate branching and interstate banking at the same time, or within a few years, and this overlap of different deregulation events could bias our identification. In row 11, we show that our results are unchanged if we augment our specification with an indicator controlling for the effect of intrastate branching deregulation on patents.

As discussed in Section 2, although IBBEA permitted interstate branching, states were granted the right to erect some barriers for the branches of out-of-state banks (Johnson and Rice, 2008; Rice and Strahan, 2010). In row 12, we extend our sample to 2006 and include the state-level interstate branching restriction index developed by Rice and Strahan (2010) as a further control. As shown, our coefficient of interest remains economically relevant and its statistical significance is confirmed at the 10% level.

In the late 1980s, 30 US states passed a set of business combination (BC) laws that reduced the threat of hostile takeovers, thus weakening the governance role of the market for corporate control (Bertrand and Mullainathan, 2003). These laws might have influenced our results if corporate governance affected the managerial incentives to innovate,[11] and that effect would not be captured by our specification since BC laws affected firms at their state of incorporation. To mitigate this concern, we include as control a dummy equal to one if firms were incorporated in the states that passed a BC law, from the year of the passage onwards, and zero otherwise (row 13). Furthermore, in untabulated regressions we interact BC laws dummy with an interstate banking deregulation dummy, to allow for heterogeneous effects of deregulation on innovation depending on whether the firm was subject to BC laws. Our estimates indicate that the positive effect of banking deregulation on firm innovation is not affected by the changes in corporate governance brought about by BC laws.

Another potential confounding event took place in 1982, when the US Congress created the Court of Appeals for the Federal Circuit (CAFC) that became the sole US appeals court in patent cases. As described in Henry and Turner (2006), CAFC unified the patent law and effectively ended forum shopping between geographical circuit courts of appeal. By making patent invalidity defenses less viable, CAFC has earned a reputation as a pro-patent court. However, Henry and Turner (2006) claim that, even if CAFC provisions affected patent scope, “it is not clear whether they have, on balance, favored patentees.” In any event, the creation of CAFC should have led to a secular increase in patenting by US firms and we control for it with time fixed effects and average annual patenting in the industry. Also, our results hold by excluding the “Semiconductors and Related Devices” industry (row 14), which Hall and Ziedonis (2001) claim to have been most affected by CAFC creation. Finally, in Section 7 we provide evidence that the impact of deregulation on innovation is higher when the credit supply shock was higher, which should not be the case if our results are merely driven by trends.

In rows 15 and 16, we check that our results are not driven by influential patent filers. Excluding 1% or 2.5% of observations on the right tail of the firm-patent distribution does not change the economic and statistical magnitude of the result. In row 17, we check that our results hold even after including the squared HHI to capture potential nonlinear effects of competition on innovation (Aghion, Bloom, Blundell, Griffith, and Van Reenen, 2005), as well as firm size squared. Further, we try to separate out our effect from other channels such as equity-like funding by controlling for the new funds raised by venture capital and private equity firms (hereafter, VC funding) in the state. Data come from SDC VentureXpert database. In row 18, we use as additional control of our baseline regression the amount of VC capital in a given state and year. As shown, our main finding is robust to the inclusion of this control.

Finally, Appendix B provides evidence from an opposite experiment, i.e., a negative banking shock, which adds to the external validity of our findings.

6. Nature of innovation

So far our results indicate that firms subject to interstate banking deregulation patented more. In this section, we test whether the average quality of patents has risen as well, i.e., whether the effect did not come purely from a larger credit supply and thus less rationing of projects being financed. We posit that the average increase in the quality of innovation stems from a rise in the risk of innovative projects being financed and greater willingness of banks to take credit risk. We then test whether the increase in patents corresponded to a more ambitious and risky innovation policy.

6.1. Innovation quality

A possible interpretation of the increase in patents could relate to the fact that, by substituting local banks for large out-of-state institutions, deregulation weakened relationship-based lending practices. Thus, firms might have used patents as hard evidence when seeking credit from deregulated banks.[12] We explore this interpretation

[11] The effect of corporate governance on innovation is ambiguous. Atanassov (in press) finds that worse governance reduces the incentives to innovate. Chemmanur and Tian (2011) argue that by isolating CEOs from short-term pressures, managerial entrenchment can be beneficial for innovation. Sapra, Subramanian, and Subramanian (forthcoming) show that the effect of corporate governance on innovation is U-shaped.

[12] Intangible assets are typically thought to represent poor collateral, due to uncertainty and information asymmetry. However, recent

Table 6
Innovation quality.

This table reports Poisson regression results using cite-weighted and truncation-adjusted patent counts as the dependent variable. Column 4 includes an additional set of firm and industry controls. Specifically, it includes one-year lagged Ln (age), HHI, ROA, tangibility, and cash holdings. Coefficients, unreported to save space, are available upon request. The construction of control variables is described in Appendix A. Standard errors clustered by state of operation are reported in parentheses. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Dependent variable: Cite-weighted patent counts				
	(1)	(2)	(3)	(4)
Interstate deregulation	0.1338**	0.1008**	0.0947**	0.0964***
	(0.0665)	(0.0447)	(0.0377)	(0.0371)
Ln (sales)		0.6855***	0.3856***	0.4297***
		(0.0580)	(0.0765)	(0.1015)
Ln (K/L)		0.2474***	0.2227***	0.1978***
		(0.0640)	(0.0801)	(0.0756)
Ln (R&D stock)			0.5318***	0.5180***
			(0.0984)	(0.1118)
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Industry trends	Yes	Yes	Yes	Yes
Additional controls	No	No	No	Yes
Number of obs.	17,922	17,922	17,922	17,922

by testing whether the mere number of patents increased, or whether their economic relevance changed as well.

Existing research has demonstrated that patents differ greatly in "value" and that simple patent counts (i.e., the measure adopted in the previous sections) do not necessarily capture the relative importance of the underlying inventions (Harhoff, Narin, Scherer, and Vopel, 1999; Hall, Jaffe, and Trajtenberg, 2005). In this section, we measure innovation quality by weighting each patent using the number of future citations received from subsequent patents (Trajtenberg, 1990). Forward citations reflect the technological importance of patents as perceived by the inventors themselves (Jaffe, Fogarty, and Trajtenberg, 2000) and knowledgeable peers in the technological field (Albert, Avery, Narin, and McAllister, 1991). Furthermore, citations reflect the economic importance of patents, as shown by Hall, Jaffe, and Trajtenberg (2005). Because forward citations suffer from truncation problems, we weight patent counts by truncation-adjusted cite counts from the NBER data set (see, e.g., Hall, Jaffe, and Trajtenberg, 2001, 2005).[13]

Results reported in Table 6 indicate that interstate banking deregulation led to a 10.1% increase in the expected number of cite-weighted patent counts. Results remain statistically and economically relevant irrespective of whether no control variables apart from firm- and time-fixed effects are used (column 1); whether the logarithm of sales and capital-to-labor ratio are included as the controls (column 2); whether, additionally, R&D stock is included as the control (column 3); and whether other confounding industry- and firm-level variables, such as HHI, firm age, ROA, tangibility, and cash holdings are also controlled for (column 4). The deregulation coefficient remains both statistically and economically relevant, indicating a 10.1% increase in expected forward citation counts. Given that this effect could be partly driven by the increase in patent counts shown in Section 5, we include patent counts as a control variable; our result confirms that firms subject to deregulation increased citations as compared to firms not subject to deregulation but having the same number of patents. Notice that the increase in patent citations remains significant if we use the deregulation "depth" measure introduced in Table 5, rows 9–10 instead of our deregulation dummy.

6.2. Technological fields

Previous results have indicated that innovation quality rose following deregulation. We now investigate changes in the technological nature of corporate patenting. First, we combine citations with information on patents' technological fields. Second, we check if there is a simultaneous increase in both high-quality and low-quality patents. Finally, we analyze the volatility of successful patenting.

Primary technological fields contained in the NBER patent data set consist of about 400 main (three-digit) patent classes, as defined by the USPTO. We use the *generality* and *originality* indexes, developed by Trajtenberg, Jaffe, and Henderson (1997) and computed by Hall, Jaffe, and Trajtenberg (2001), to capture the fundamental nature of the research being patented. The generality index is equal to $1-\sum_j^{n_i} s_{ij}^2$, where s_{ij}^2 denotes the percentage of citations received by a patent i that belong to the patent technology class j out of n_i patent classes. The index will take high values (high generality) if a patent receives citations from subsequent patents that belong to many different technological fields. The originality index is constructed in a similar way, but its computation relies on the citations made rather than citations received; hence, it will take a high value if a patent cites other patents that belong to many different fields (high originality).

We use these two indexes as the dependent variables in separate specifications similar to the ones used in Table 3. As reported in Table 7, interstate banking deregulation had a positive and significant effect on the originality of patents: firms subject to deregulation exhibited a higher propensity to patent within broader technological fields (columns 1 and 2). Moreover, firms increased the generality of patents (columns 3 and 4). Taken together with our previous finding on citations, these results reinforce the notion that deregulation induced a change in the type of firms' innovative

(*footnote continued*)
contributions suggest that collateralization of intangibles has increased in the recent decade (Loumioti, 2012).

[13] The truncation problem arises since "citations to a given patent typically keep coming over long periods of time, but we only observe them until the last date of the available data" (Hall, Jaffe, and Trajtenberg, 2005). Besides the use of truncation-adjusted citation counts, the problem is mitigated by the inclusion of year fixed effects. In fact, our results are robust to the adoption of unadjusted citation counts.

Table 7
Patents and technological fields.

This table reports Poisson regression results using as dependent variable the originality index (columns 1–2) and generality index (columns 3–4). The construction of these indexes and control variables is described in Appendix A. Columns 2 and 4 include an additional set of firm and industry controls. Specifically, it includes one-year lagged Ln (age), HHI, ROA, tangibility, and cash holdings. Coefficients, unreported to save space, are available upon request. Standard errors clustered by state of operation are reported in parentheses. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Dependent variable:	Originality index		Generality index	
	(1)	(2)	(3)	(4)
Interstate deregulation	0.1010**	0.1012**	0.1324***	0.1335***
	(0.0505)	(0.0446)	(0.0488)	(0.0450)
Ln (sales)	0.5024***	0.5344***	0.5213***	0.5585***
	(0.0727)	(0.0829)	(0.0734)	(0.0903)
Ln (K/L)	0.2510***	0.2225**	0.2329***	0.1670**
	(0.0837)	(0.0874)	(0.0644)	(0.0754)
Ln (R&D stock)	0.3390***	0.3046***	0.4126***	0.3793***
	(0.1170)	(0.1179)	(0.1257)	(0.1309)
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Industry trends	Yes	Yes	Yes	Yes
Additional controls	No	Yes	No	Yes
Number of obs.	17,347	17,347	16,722	16,722

Table 8
Volatility of patent citations.

This table reports OLS regression results using the standard deviation of Ln(cite-weighted patent counts) in the pre- and post-interstate banking deregulation periods as the dependent variable. Controls are constructed estimating the average measures in the pre- and post-deregulation periods. Column 2 includes an additional set of firm and industry controls. Specifically, it includes one-year lagged Ln (age), HHI, ROA, tangibility, and cash holdings. Coefficients, unreported to save space, are available upon request. The construction of control variables is described in Appendix A. Robust standard errors are reported in parentheses. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Dependent variable: s.d. (Cite-weighted patent counts)		
	(1)	(2)
Interstate deregulation	0.1479**	0.1446**
	(0.0636)	(0.0681)
Ln (sales)	0.0077	0.0986
	(0.0650)	(0.0756)
Ln (K/L)	−0.0345	−0.0502
	(0.0642)	(0.0834)
Ln (R&D stock)	−0.1109*	−0.1879***
	(0.0652)	(0.0650)
Firm fixed effects	Yes	Yes
Industry trends	Yes	Yes
Additional controls	No	Yes
Number of obs.	866	866

activities. More general and original patents require a bolder innovation policy. The results also further suggest that firms did not simply patent existing innovation to provide hard information to out-of-state banks. Same as for patent citations, the results of this section are largely robust to the use of the alternative deregulation measures used in Table 5, rows 9–10.

6.3. Patenting risk

A more ambitious innovation policy could entail more potential failures. This section provides evidence in line with the notion that firms' successful patenting indeed became riskier. First, we analyze patenting risk by testing how deregulation affected the volatility of patent citations. To start, we analyze the distribution of citations and we find that, relative to the pre-deregulation period, both standard deviation and third moment of citations increased. We validate the notion of increased citation dispersion using as dependent variable the standard deviation of the logarithm of cite-weighted patent counts computed in the pre- and post-interstate deregulation periods, restricting the analysis to firms that are present at least two years in each period. We estimate a regression including the interstate deregulation dummy, the usual controls averaged over the pre- and post-deregulation periods, and the firm fixed effects. Results reported in Table 8 indicate that interstate banking deregulation increased the dispersion of patent citations.

We then use as the dependent variable the number of patents that receive zero citations, i.e., low-quality patents. Consistent with the notion of increased innovation failures following deregulation, columns 1 and 2 in Table 9 show that the number of patents that eventually received zero citations increased. This result also holds after controlling for the overall change in patenting activity (columns 3 and 4).[14]

7. Channels

7.1. Banks' geographic diversification

One of the ways to explain higher and riskier corporate patenting after the entrance of new banks is that out-of-state banks were better able to finance riskier projects due to lower exposure to the background risks of the state's economy. At the same time, credit in this state provides out-of-state banks an opportunity to diversify their loan portfolio, for instance, due to a different industry composition of the state. In fact, better geographic diversification was often mentioned as a potential consequence of interstate banking deregulation.[15] We use three empirical tests that provide empirical support to this argument.

In our first test, we separate the states according to how their economic activity comoves with the rest of the

[14] The result that firms experienced an increase in low-quality patents is not in contrast with our previous argument that, on average, the patenting quality rose following deregulation; rather, it suggests that the increase in high-quality patents was large enough to offset the increase in low-quality patents.

[15] For instance, Federal Reserve Board Governor Martha Seger noted in her address before the California League of Savings Institutions in San Diego in April 1986, that "interstate expansion allows for the diversification of sources and uses of funds. Dependence on economic conditions in a very limited number of local markets can be reduced by a wider range of operations" and that "if lenders had been able to spread their loan portfolios over larger geographic areas, fewer institutions would have had such high concentrations of agricultural and energy loans."

Table 9
Zero-citation patents.

This table reports Poisson regression results using zero-citation patent counts as the dependent variable. Columns 2 and 4 include an additional set of firm and industry controls. Specifically, they include one-year lagged Ln (age), HHI, ROA, tangibility, and cash holdings. Coefficients, unreported to save space, are available upon request. Furthermore, columns 3 and 4 control for cite-weighted patent counts. The construction of control variables is described in Appendix A. Standard errors clustered by state of operation are reported in parentheses. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Dependent variable: Zero-citation patent counts				
	(1)	(2)	(3)	(4)
Interstate deregulation	0.2163***	0.2293***	0.1966***	0.2094***
	(0.0542)	(0.0639)	(0.0503)	(0.0614)
Ln (sales)	0.4755*	0.5536**	0.3640	0.4126*
	(0.2474)	(0.2579)	(0.2287)	(0.2358)
Ln (K/L)	0.4218***	0.2286	0.3591***	0.1467
	(0.1231)	(0.1841)	(0.1143)	(0.1784)
Ln (R&D stock)	0.2308	0.1775	0.2192	0.1661
	(0.2618)	(0.2416)	(0.2449)	(0.2224)
Cite-weighted patent counts			0.0001	0.0001
			(0.0001)	(0.0001)
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Industry trends	Yes	Yes	Yes	Yes
Additional controls	No	Yes	No	Yes
Number of obs.	8,451	8,451	8,451	8,451

US economy. Here we expect that states that are least correlated with the activity of other states would provide the highest diversification benefits for entering banks and thus would experience the highest increase in the number of patents. In particular, we extract a coincident index that summarizes state-level economic indicators from the Federal Reserve Bank of Philadelphia. The coincident index combines data on nonfarm payroll employment, average hours worked in manufacturing, unemployment rate, and wage and salary disbursements deflated by the consumer price index (US city average). The trend for each state's index is set to the trend of its gross domestic product (GDP) so that long-term growth in the state's index matches long-term growth in its GDP (Crone and Clayton-Matthews, 2005). We estimate the correlation between a state's economy and the rest of the US using monthly values of the coincident indexes over 1979–1984, i.e., before interstate banking deregulation started to come into effect. We call this variable *US/state correlation*. In Table 10, columns 1 and 2, we show that the increase in patenting was higher in the states with a recent history of least covariation with the rest of the US.[16]

Our second test relies on the location of banking institutions that enter a given state. We investigate whether the effect on innovation was highest in those states where new out-of-state banks were entering from the states least comoving with the state in question. In particular, for each pair of states we estimate the correlation of their monthly values of coincident indexes over 1979–1984. We then calculate the weighted average of these comovement measures across all out-of-state banking institutions operating in the state, based on the location of their bank holding companies. As a weight for each institution, we use the assets it has in the state as a fraction of the total assets in the state held by out-of-state banking institutions. We estimate such a measure, decreasing in the actual diversification, for each state and year. We call this variable *Entering banks/state correlation*. Our data on the banking institutions come from the Reports of Condition and Income (Call Reports) that provide information on the financial activities and ownership structures of each banking institution. All banking institutions regulated by the FDIC, the Federal Reserve, or the Office of the Comptroller of the Currency are required to file Call Reports. In Table 10, columns 3 and 4, we report that the increase in patenting was higher in the states that experienced the entry of the banks from states with the least comoving economic indicators.

Our third test explores the differences in the diversification of banks in each state. If our argument is correct, those states that experienced the largest change in the average diversification of banks operating in their state should have experienced higher risk-taking by banks and thus a subsequent larger increase in innovative activities. We use the data from the Call Reports to estimate the diversification of each bank. Our procedure follows three steps. First, for each bank we identify whether it is a subsidiary to some other financial institution, i.e., whether it is controlled by a bank holding company. Second, for each bank holding company we estimate the distribution of total assets across states, based on where its subsidiaries are located. In particular, we compute Herfindahl-Hirschman Index (HHI) as our diversification measure (Goetz, Laeven, and Levine, 2012). Third, for each state we calculate the weighted average of these diversification measures across all banking institutions operating in the state. As a weight for each institution, we use the assets it has in the state as a fraction of the total banking assets in the state of all institutions. In summary, our measure, decreasing in the actual diversification, *Geographic diversification,* is estimated as:

$$\sum_{i}^{N_i}\left(\frac{A_{ij}}{A_j}\sum_{j}^{K_i}\left(\frac{A_{ij}}{A_i}\right)^2\right),$$

[16] Due to the type of nonlinear models used in the analysis, we are not able to evaluate how the coefficients across our sample splits compare in the statistical sense. However, we do observe clear differences in terms of economic magnitude.

Table 10
Diversification benefits.

This table reports Poisson regression results using number of patents as the dependent variable. US/state correlation refers to state economy's comovement with the rest of the US, measured as the correlation of state's coincident index to the US coincident index. We estimate it from the monthly values of the indexes over 1979–1984. The coincident index combines data on nonfarm payroll employment, average hours worked in manufacturing, the unemployment rate, and wage and salary disbursements deflated by the consumer price index (US city average). Entering banks/state correlation refers to the weighted average of the comovements between the state and the states where the bank holding companies of its out-of-state banks are located. We estimate the pairwise correlations between all states from the monthly values of the coincident indexes over 1979–1984. We then calculate the weighted average of these comovement measures across all out-of-state banking institutions operating in the state, based on the location of the bank holding company. As a weight for each institution, we use the assets it has in the state as a fraction of the total assets in the state held by out-of-state banking institutions. Geographic diversification refers to the weighted average of diversification of all banking institutions operating in the state. As a weight for each institution, we use the assets it has in the state as a fraction of the total banking assets in the state. As a measure of each banking institutions' diversification, we estimate the Herfindahl-Hirschman Index based on the distribution of the assets of its subsidiaries across states. Additional controls include one-year lagged Ln (age), HHI, ROA, tangibility, and cash holdings. The construction of the control variables is described in Appendix A. Standard errors clustered by state of operation are reported in parentheses. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Dependent variable: Patent counts						
	High US/state correlation	Low US/state correlation	High entering banks/state correlation	Low entering banks/state correlation	Low geographic diversification	High geographic diversification
	(1)	(2)	(3)	(4)	(5)	(6)
Interstate deregulation	0.0328	0.1673**	0.0568	0.1861***	0.0760	0.1902***
	(0.0343)	(0.0817)	(0.0415)	(0.0536)	(0.0494)	(0.0505)
Ln (sales)	0.4267***	0.4540***	0.5669***	0.3079***	0.5364***	0.3420***
	(0.1149)	(0.1109)	(0.1090)	(0.0957)	(0.1179)	(0.0924)
Ln (K/L)	0.1943**	0.1420	0.0591	0.3162***	0.0359	0.2994***
	(0.0874)	(0.1116)	(0.1372)	(0.0788)	(0.1518)	(0.0717)
Ln (R&D stock)	0.2045	0.5244***	0.3251	0.5143***	0.3977*	0.4459***
	(0.1754)	(0.1760)	(0.2220)	(0.1268)	(0.2235)	(0.1250)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry trends	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	8,172	9,680	7,509	10,377	7,444	10,442

where A_{ij} denotes the total assets of all subsidiaries of bank i in state j; A_i denotes the total assets of all subsidiaries of bank i across all K_i states it operates in; A_j denotes the total assets of all banks in state j. A large increase in diversification might mean that new out-of-state banks were well diversified and also that in-state banks better diversified across other states. In Table 10, columns 5 and 6, we show that patenting mainly increased in the states with the largest diversification of banking activities.

7.2. Geographic proximity

We have shown that the deregulation effect on firms' innovation was highest in the states where out-of-state banks could achieve the best geographic diversification. However, even within the state, the exposure to deregulation differed across the firms based on their location; some firms experienced more new banks entering their area than others. We thus expect the increase in patenting following deregulation to be stronger for the firms located in areas where the presence of out-of-state banks was particularly high after the interstate deregulation.

Based on firms' zip codes, we estimate the spherical distance between each firm's headquarters[17] and all banking institutions that filed the Call Reports in 1995. For each firm, we then identify the number of all banks within a ten-mile radius, the number of banks owned by out-of-state bank holding companies within a ten-mile radius, as well as the closest bank owned by an out-of-state bank holding company. We find that the median firm had seven banks within a ten-mile radius, out of which, on average, zero were owned by out-of-state institutions. The median distance from the firm to an out-of-state bank was 11 miles. Within 50 miles, the median firm had access to three out-of-state banks in 1995.

Table 11, columns 1 and 2 show that firms that had any out-of-state banks within ten miles experienced a larger increase in patenting; however, our main results do not hold for firms without proximate out-of-state banks. Moreover, columns 3 and 4 report that if we further split the subsample of firms with proximate out-of-state banks into those that had above and below the median number of

[17] The sample used in this section is smaller because not all firms report their zip codes in Compustat.

Table 11
Proximity to entering banks.

This table reports Poisson regression results using patent counts as the dependent variable. Columns 1 and 2 report results separately for firms with and without proximate out-of-state banking institutions in 1995. Proximity is defined as the spherical distance of less than ten miles from the firm's headquarters. Columns 3 and 4 report results for firms with proximate out-of-state banking institutions in 1995, separately for firms with many and few proximate banking institutions overall. The split is based on the subsample median of 14 banking institutions. Proximity is defined as the spherical distance of less than ten miles from the firm's headquarters. Columns 5 and 6 report results separately for firms with and without proximate out-of-state banking institutions in 1995. Proximity is defined as the spherical distance of less than 50 miles from the firm's headquarters. Coefficients, unreported to save space, are available upon request. Additional controls include one-year lagged Ln (age), HHI, ROA, tangibility, and cash holdings. The construction of control variables is described in Appendix A. Standard errors clustered by state of operation are reported in parentheses. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Dependent variable: Patent counts						
	Out-of-state banks within 10 miles	No out-of-state banks within 10 miles	Out-of-state banks within 10 miles and few local banks	Out-of-state banks within 10 miles and many local banks	Many out-of-state banks within 50 miles	Few out-of-state banks within 50 miles
	(1)	(2)	(3)	(4)	(5)	(6)
Interstate deregulation	0.2577*	0.1180	0.3318**	0.0555	0.3045***	0.1196
	(0.1534)	(0.1206)	(0.1486)	(0.1261)	(0.1068)	(0.1209)
Ln (sales)	0.3049***	0.4455**	0.4201***	0.0756	0.3535**	0.6038***
	(0.0873)	(0.1752)	(0.1284)	(0.0930)	(0.1400)	(0.1710)
Ln (K/L)	0.2303	0.0902	0.2305	0.0847	0.2876**	-0.2074
	(0.1976)	(0.1252)	(0.2292)	(0.0849)	(0.1449)	(0.1389)
Ln (R&D stock)	0.5103**	0.3185	0.2932	0.6202***	0.5596***	0.0714
	(0.2087)	(0.2116)	(0.1836)	(0.1297)	(0.1889)	(0.1959)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry trends	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	4,189	4,805	2,187	2,002	6,065	2,929

banks in general within ten miles, we find that the result holds in the subsample that had lower concentration of local banks, i.e., where out-of-state banks might have had a bigger impact. In an alternative specification (columns 5 and 6), we check the robustness of our finding using a 50-mile radius.

Taken together, these results indicate that the increase in corporate innovation during the post-deregulation period was strongly influenced by how deregulation affected the geography of credit markets.

7.3. Financial dependence and innovation inputs

If easier access to credit was a channel through which banking deregulation affected corporate innovation, we expect the effect to be more prevalent among firms that operate in industries requiring high external finance. We shed light on this notion by testing how our main finding varies depending on the industry-level reliance on external capital. To this end, we classify firms based on whether the industry in which they operate was above or below the across-industry median of external financial capital raised at the time of the interstate banking deregulation. Our measure is thus similar to Rajan and Zingales's (1998) proxy for an industry's financial constraints. We estimate it in two ways. First, we use balance sheet measures. In particular, we take the average across the industry of the combined net change in equity and debt normalized by the book value of assets. Second, we use the data in the SDC New Issues database and estimate financial dependence as the total proceeds from issuance of securities over the year divided by the book value of assets. Results reported in Table 12 indicate that the positive effect of interstate banking deregulation on firm innovation is more pronounced for firms operating in industries that are highly dependent upon external finance.

Turning our attention to firm-level characteristics, we posit that our finding should be stronger for firms that were more dependent upon bank credit prior to deregulation. We investigate this notion by first considering firm age. Because old and well-established firms can typically access the public debt market or easily raise equity, they should not be influenced by changes in bank credit supply. By contrast, young firms, which are typically more financially constrained due to asymmetric information problems, are expected to respond to changes in bank credit. We focus on the subsample of firms that are present for fewer than ten years in Compustat (Rajan and Zingales, 1998; Cetorelli and Strahan, 2006) at the time of the interstate banking deregulation, and firms that entered the sample after the deregulation. As shown in Table 13, columns 1 and 2, the effect of deregulation on firm

Table 12
Industry-level financial dependence.

This table reports Poisson regression results using number of patents as the dependent variable. In columns 1 and 2 financial dependence is estimated based on the balance sheet measures. We take the average across industry of the combined net change in equity and debt normalized by the firm's book value of assets. We then sort industries into high and low financial dependence based on the median financial dependence. In columns 3 and 4, financial dependence is estimated based on the data in the SDC New Issues database. In particular, we take the average across industry of the total proceeds from issuance of securities divided by the book value of assets. We then sort industries into high and low financial dependence based on the median financial dependence. All regressions include an additional set of firm and industry controls. Specifically, they include one-year lagged Ln (age), HHI, ROA, tangibility, and cash holdings. Coefficients, unreported to save space, are available upon request. The construction of control variables is described in Appendix A. Standard errors clustered by state of operation are reported in parentheses. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Dependent variable: Patent counts				
	High financial dependence	Low financial dependence	High financial dependence	Low financial dependence
	(from balance sheets)		*(from new issuances)*	
	(1)	(2)	(3)	(4)
Interstate deregulation	0.1758***	0.0611	0.1611***	0.0752*
	(0.0581)	(0.0662)	(0.0595)	(0.0421)
Ln (sales)	0.3035***	0.6344***	0.4622***	0.4903***
	(0.1144)	(0.0918)	(0.1366)	(0.0984)
Ln (K/L)	0.3459***	−0.0949	0.2467**	0.0711
	(0.0799)	(0.1015)	(0.1060)	(0.0862)
Ln (R&D stock)	0.4936***	0.0702	0.3494**	0.2391
	(0.1864)	(0.1165)	(0.1775)	(0.1591)
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Industry trends	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes
Number of obs.	9,801	8,265	10,140	7,926

Table 13
Bank dependence and innovation inputs.

This table reports Poisson regression results using patent counts as the dependent variable. Columns 1 and 2 report results separately for young firms and mature firms. Young firms are defined as firms that were present for less than ten years in the Compustat data set at the time of the interstate banking deregulation, or that entered the data set after deregulation. Mature firms are all other firms. Columns 3 and 4 report results separately for firms that had or did not have a bond rating in any year in the period 1985–1995. Columns 5 and 6 report results separately for firms with a Kaplan-Zingales (KZ) (1997) score above or below the median value. Columns 7 and 8 report results separately for firms with R&D expenditures above the industry median in the post-deregulation years, and for all other firms. All regressions include an additional set of firm and industry controls. Specifically, they include one-year lagged Ln (age), HHI, ROA, tangibility, and cash holdings. Coefficients, unreported to save space, are available upon request. The construction of control variables is described in Appendix A. Standard errors clustered by state of operation are reported in parentheses. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Dependent variable: Patent counts								
	Young firms	Mature firms	Firms without a bond rating	Firms with a bond rating	Constrained (high KZ index)	Unconstrained (low KZ index)	Innovative firms	Other firms
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Interstate deregulation	0.2075*	0.1172***	0.2654***	0.0819*	0.1895*	0.0661*	0.1177***	-0.0109
	(0.1129)	(0.0389)	(0.0833)	(0.0456)	(0.1142)	(0.0360)	(0.0405)	(0.1240)
Ln (sales)	0.1039*	0.6288***	0.2875***	0.6271***	0.3414***	0.4769***	0.5553***	0.4056***
	(0.0531)	(0.0916)	(0.0786)	(0.1008)	(0.1175)	(0.1019)	(0.1032)	(0.0697)
Ln (K/L)	-0.0391	0.2188***	0.0389	0.2306***	0.1484	0.2085*	0.2136**	0.1145
	(0.1169)	(0.0809)	(0.0736)	(0.0859)	(0.1349)	(0.1073)	(0.0882)	(0.1061)
Ln (R&D stock)	0.6762***	0.2426**	0.6126***	0.2229*	0.6255***	0.1549	0.2815**	0.4285***
	(0.1266)	(0.1195)	(0.0822)	(0.1304)	(0.1020)	(0.1335)	(0.1361)	(0.1531)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	5,329	12,737	12,884	5,182	7,002	7,685	8,910	9,156

Table 14
Neighboring state.
This table reports Poisson regression results using patent counts as the dependent variable. Each column includes an additional set of firm and industry controls. Specifically, it includes one-year lagged Ln (age), HHI, ROA, tangibility, and cash holdings. Coefficients, unreported to save space, are available upon request. The construction of control variables is described in Appendix A. Neighboring state's interstate deregulation treatment is based on the state that has the border closest in terms of the distance from the centroid of firm's county. Standard errors clustered by state of operation are reported in parentheses. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Dependent variable: Patent counts		
	(1)	(2)
Neighboring state's interstate deregulation	0.0332	0.0336
	(0.0522)	(0.0609)
Firm state's interstate deregulation		0.1270*
		(0.0700)
Firm fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Industry trends	Yes	Yes
Additional controls	Yes	Yes
Number of obs.	11,310	11,310

patents was positive and statistically significant both for young and mature firms. However, the economic magnitude is much larger among young firms: while young firms subject to interstate banking deregulation experienced a 22.7% increase in patents, the effect is 11.7% for mature firms.

Next, we sort firms according to whether they were assigned a long-term bond rating by Standard & Poors (S&P). By allowing firms to access public debt markets, a bond rating is related to lower credit constraints (Kashyap, Lamont, and Stein, 1994; Almeida, Campello, and Weisbach, 2004; Faulkender and Petersen, 2006; Denis and Sibilkov, 2010) and, consequently, lower responsiveness to changes in bank finance (Leary, 2009). We construct two subsamples depending on whether a firm reported a bond rating or not in any of the years from 1985 (i.e., when the coverage of S&P ratings in Compustat started) to 1995. Columns 3 and 4 show that the effect of deregulation on innovation is significantly larger for firms experiencing tougher access to the public bond market. In fact, using the actual public bond issuance data from SDC New Issuances we confirm this result. Firms that did not have public bonds outstanding over the period 1985–1995 experienced an increase in innovative activities after deregulation while there was no statistically significant effect for firms that were active in the public bond market.

Overall, these results suggest that the effect of interstate banking deregulation on corporate innovation was shaped by bank dependence: the effect was economically larger among firms that were younger and that had worse access to other segments of the credit market. This evidence is consistent with previous findings that bank credit is most relevant for less-established and informationally opaque firms (Hadlock and James, 2002).

We further study the importance of financial constraints by constructing the Kaplan and Zingales (KZ) index and estimating our model separately for constrained (above-median KZ index) and unconstrained (below-median KZ index) firms. To compute the KZ index, we follow Lamont, Polk, and Saa-Requejo (2001) who use the original coefficient estimates of Kaplan and Zingales (1997). Results, reported in columns 5 and 6, show that the effect of interstate banking deregulation was economically more relevant among constrained firms, though the statistical significance is present in both subsamples.

Finally, if firms innovated more due to the relaxation of financial constraints following deregulation, we should expect our result to be increasing in the level of post-deregulation innovation expenditures. We explore this aspect by classifying firms depending on whether in the post-deregulation period they invested more than their industry peers in R&D expenditures. Results are reported in columns 7 and 8. As expected, the positive effect of deregulation on firm patents is only present among firms that invested heavily in innovation in the post-deregulation period.

7.4. Financing vs. spillovers

Our results so far indicate that, on average, listed manufacturing firms in the US patented more following deregulation, and that this increase is particularly large for firms that are more dependent on bank credit. Therefore, our results are consistent with the notion that deregulation relaxed the financial constraints of bank-dependent firms. However, our evidence could also be consistent with the increase in innovation being driven by knowledge spillovers from entrepreneurial firms. Since deregulation encouraged entrepreneurial activity as well as firm exit (Kerr and Nanda, 2009), it is possible that our results are driven by churning in the entrepreneurial sector and that innovation in public firms rose only because of spillovers rather than relaxed financial constraints.

In this section, we present evidence that attempts to disentangle the financing from the spillovers explanations. First, we analyze the effect of deregulation on the innovation of software firms. Since firms in this industry use primarily equity to finance their operations, the effect of deregulation via less binding financial constraints should be negligible. If instead there were a spillover effect at play, then software firms could have experienced an increase in innovation that is, however, not related to financial constraints. Estimating our specification on the software industry only (SIC 73), we find that the deregulation effect was negative and statistically insignificant (p-value$=$0.21). Hence, this result does not provide support to the spillover interpretation.

Second, we look at whether the innovation of firms located close to the state borders was affected by banking deregulation in the neighboring states. Presumably, spillover effects prevail across state borders and thus, if our previous finding of increased innovation by public firms came only from spillovers, there should also be a positive effect when the neighboring state deregulates. However, in Table 14, we do not find support for this argument. In particular, we limit our sample to the firms that have their counties reported in Compustat and match county information to the geographical border data used in Holmes (1998). For each firm we pick the state that has the border closest in terms of the distance from the centroid of the firm's county. We find that only the deregulation of a firm's state of headquarter is relevant for innovation but not that of the closest other state.

Third, in unreported analyses we control for linear patenting trends by technological class and state. Since knowledge externalities tend to be localized (Jaffe, Trajtenberg, and Henderson, 1993), this regression allows us to control for spillovers in given geographical and technological areas. Specifically, we take the most represented technological class in terms of patent citations by state and year, and then we include the number of citations of this class as an explanatory variable in our main specification. Estimating a model as in Table 3, column 4, we find that the increase in the expected number of patents remains economically relevant and statistically significant at the 1% level. Similar results are obtained if we control for patent trends computed by state and 3-digit SIC industry.

8. Discussion

While the relationship between economic prosperity and banking development is widely debated, establishing the direction of causality has proven to be a challenging task. We focus on manufacturing firms' innovative performance and exploit the passage of interstate banking deregulation during the 1980s and 1990s in the US to generate exogenous variations in the geographic spread of US banking institutions. Interstate banking deregulation allowed banks to expand geographically, increased the availability and quality of credit, and was associated with the adoption of new screening and monitoring technologies.

Our main result indicates that interstate banking deregulation spurred corporate innovation, as measured by patent-based metrics. Furthermore, we find that the effect was not imminent, and that it was driven mainly by firms that were located closer to the entering banks, as well as firms that operated in industries requiring high external capital. Finally, we provide evidence that the increase in firms' innovation activities is associated with a better ability of out-of-state banks to diversify credit risk geographically and thereby lend to riskier borrowers.

Since innovation is a key driver of economic progress, the importance of our results goes beyond the effect on corporate patenting. To shed light on the aggregate economic implications of our findings, we estimate separate regressions as in Table 3, column 1, for each SIC two-digit industry and then rank the industries by how much the patenting activity in a specific industry was affected by interstate banking deregulation (i.e., by the size of the deregulation coefficient).[18] Correlating these industry-level effects with future industry growth (measured as growth in the value of shipments over 1995–2000), we find a positive and 7% significant association. Industries where deregulation had a higher impact on patenting experienced a subsequent increase in output growth. For instance, the five industries with the largest deregulation estimates grew, on average, by 4.9% annually over 1995–2000. By contrast, the five industries with the smallest deregulation estimates grew, on average, by 0.2%.

In summary, our research suggests that bank geographic diversification is an important determinant of banks' willingness to take risk and thus contributes to technological progress and growth.

Appendix A. List of main variables

Name	Description	Source
Interstate banking deregulation variables		
Interstate deregulation	Dummy, set equal to one from the year of interstate banking deregulation in a firm's headquarter state, and set to zero for the years prior to deregulation	
Continuous deregulation treatment	Logarithm of the number of states from which the entry of banks has been permitted by the particular year in firm's headquarter state	
Population-weighted continuous deregulation treatment	Logarithm of the population of states from which the entry of banks has been permitted by the particular year in firm's headquarter state	
Neighboring state's interstate deregulation	Dummy, set equal to one from the year of interstate banking deregulation in the state that has border closest in terms of the distance from the centroid of firm's county, and set to zero for the years prior to deregulation	
Innovation variables		
Patent counts	Count of a firm's number of patents	NBER
Ln (patents)	Logarithm of a firm's number of patents	NBER
Cite-weighted patent counts	Count of a firm's number of patents weighted by future citations received and adjusted for truncation (as described in Hall, Jaffe, and Trajtenberg (2001, 2005))	NBER
Zero-citation patent counts	Count of a firm's number of patents with zero forward citations	NBER
s.d. (Cite-weighted patent counts)	Standard deviation of the logarithm of a firm's count of number of patents for the period 1976–1995 weighted by future citations received and adjusted for truncation. Standard deviations are computed in the pre-and post-deregulation periods, keeping in the computation firms that remain in each period at least two years	NBER
Originality index	Equal to $1-\sum_j^{n_i} s_{ij}^2$, where s_{ij}^2 denotes the percentage of citations made by a patent i that belong to the patent technology class j out of n_i	NBER

[18] The effect of deregulation laws on firm patenting was largest in primary metal (SIC 33), furniture and fixtures (SIC 25), and petroleum refining and related industries (SIC 29). We find no difference in growth rates between these groups of industries prior to deregulation (in 1980–1985).

	patent classes. Technology classes are defined by the USPTO and consist of about 400 main patent classes (three-digit level). The index will take high values (high originality) if a patent cites other patents that belong to many different technological fields	
Generality index	Equal to $1-\sum_j^{n_i} s_{ij}^2$, where s_{ij}^2 denotes the percentage of citations received by a patent i that belong to the patent technology class j out of n_i patent classes. The index will take high values (high generality) if a patent receives citations from subsequent patents that belong to many different technological fields	NBER
Ln (R&D stock)	Logarithm of (1+cumulative R&D expenditures), computed assuming a 15% annual depreciation rate and using linear interpolation to replace missing values of R&D	Compustat
Firm characteristics		
Ln (sales)	Logarithm of a firm's sales	Compustat
Ln (K/L)	Logarithm of capital-to-labor ratio, where capital is represented by property, plants, and equipment (PPE), and labor is the number of employees	Compustat
Ln (age)	Logarithm of (1+age), where age is the number of years that the firm has been in Compustat	Compustat
ROA	Earnings before interest, taxes, depreciation and amortization (EBITDA) to total assets, dropping 1% of observations at each tail of the distribution to mitigate the effect of outliers	Compustat
Cash holdings	Cash and marketable securities to total assets	Compustat
Tangibility	1- ((current assets +intangible assets) to total assets)	Compustat
HHI	Herfindahl-Hirschman Index, computed as the sum of squared market shares of all firms, based on sales, in a given three-digit SIC industry in each year. We drop 2.5% of observations at the right tail of the distribution to mitigate potential misclassifications	Compustat
Young (resp. mature) firms	Dummy, set equal to one if a firm was present for less (resp. more) than ten years in Compustat at the time of the interstate banking deregulation or if it entered the sample after the deregulation year, and set to zero otherwise	Compustat
Firms with (resp. without) a bond rating	Dummy, set equal to one if a firm reports (resp. does not report) an S&P bond rating in any year of the period 1985–1995, and set to zero if it never reports an S&P bond rating over that period	Compustat
Constrained (resp. unconstrained)	Dummy, set equal to one if a firm's KZ index (estimated as in Kaplan and Zingales, 1997) is above (resp. below) a median index across all firms, and set to zero otherwise	Compustat
Innovative (resp. other firms)	Dummy, set equal to one if a firm's R&D expenditures were above (resp. below) median R&D expenditures in its industry in the post-deregulation years, and set to zero otherwise	Compustat
Industry characteristics		
Industry trends	Average of the dependent variable across all firms in the same three-digit SIC industry of a given firm, where averages are computed excluding the firm in question	Compustat
High (resp. low) financial dependence (from balance sheet)	Dummy variable, set equal to one if the four-digit SIC industry's net change in capital is greater (resp. lower) than the median net change in capital across all industries in 1984 and set to zero otherwise. For every firm, net change in capital is estimated as the change in the amount of financial debt plus the amount of newly issued common and preferred stock minus the amount of repurchased common and preferred stock, scaled by the size of assets	Compustat
High (resp. low) financial dependence (from new issuances)	Dummy variable, set equal to one if the four-digit SIC industry's net change in capital is greater (resp. lower) than the median net change in capital across all industries in 1984 and set to zero otherwise. For every firm net change in capital is estimated as the proceeds from public bond issues plus the proceeds from secondary equity offerings, scaled by the size of assets	SDC
State-level diversification variables		
US/state correlation	State economy's comovement with the rest of the US, measured as the correlation of the state's coincident index to the US coincident index. We estimate it from the monthly values of the indexes over 1979–1984.	Federal Reserve Bank of Philadelphia

	The coincident index combines data on nonfarm payroll employment, average hours worked in manufacturing, the unemployment rate, and wage and salary disbursements deflated by the consumer price index (US city average)	
Entering banks/state correlation	Weighted average of the comovement between the state and the states where the bank holding companies of its out-of-state banks are located. We estimate the pairwise correlations between all states from the monthly values of the coincident indexes over 1979–1984. We then calculate the weighted average of these comovement measures across all out-of-state banking institutions operating in the state, based on the location of bank holding company. As a weight for each institution, we use the assets it has in the state as a fraction of the total assets in the state held by out-of-state banking institutions.	Federal Reserve Banks of Philadelphia and Chicago
Geographic diversification	Weighted average of diversification of all banking institutions operating in the state. As a weight for each institution, we use the assets it has in the state as a fraction of the total banking assets in the state. As a measure of each banking institutions' diversification, we estimate the Herfindahl-Hirschman Index based on the distribution of the assets of its subsidiaries across states.	Federal Reserve Bank of Chicago
Firm-bank proximity variables		
Out-of-state banks within ten (resp. 50) miles	Number of banking institutions in 1995 that operate in the firm's state, are located within ten (resp. 50) miles from the firm's headquarter, and are owned by an out-of-state bank holding company	Compustat, Federal Reserve Bank of Chicago
All banks within ten miles	Number of banking institutions in 1995 that operate in the firm's state and are located within ten miles from the firm's headquarters	Compustat, Federal Reserve Bank of Chicago
Most proximate out-of-state bank	Miles between the firm and the most proximate banking institutions in 1995 that operate in the firm's state and are owned by an out-of-state bank holding company	Compustat, Federal Reserve Bank of Chicago

Appendix B. Evidence from an opposite experiment

We provide support to the external validity of our findings by looking at the opposite experiment, i.e., whether an exogenous dry-up of bank credit reduces corporate innovation. Following Ashcraft (2005), we look into the case of the FDIC-induced failure of 18 healthy subsidiaries of First City Bancorporation in 1992. As described in Ashcraft (2005), the cross-guarantee provision of the Financial Institutions Reform, Recovery, and Enforcement Act of 1989 permitted FDIC to charge off any expected losses related to the failure of one subsidiary bank of a bank holding company to the capital of the other subsidiary bank. In October 1992, the FDIC exercised this authority when the Dallas and Houston banks of First City Bancorporation were declared insolvent and, as the result of FDIC action, 18 other subsidiaries also failed. Importantly, these bank failures occurred for reasons that had little to do with local area economic activity of the 18 subsidiary banks as losses came from the loans made to Iraqi banks before the Gulf War, as well as from the weakness in the Dallas and Houston commercial real estate markets. As argued in Ashcraft (2005), the only mechanism through which these failures could have plausibly affected real economic activity is the destruction of credit relationships with local borrowers.

We test whether the closure of these banks affected the patenting of firms headquartered in those counties. We limit our analysis to Texas-headquartered and thus only consider 116 firms. Our continuous treatment takes the value of zero before 1992 and the value equal to the ratio of failed healthy bank deposits to county income on and after 1992. We posit that the larger the failed healthy subsidiaries of the First City Bancorporation, the larger the reduction in innovation. We find that innovation decreased more for the firms that were located in the counties with the failed healthy banks. This result is consistent with our main result; yet, it should be interpreted with caution given the very small sample size.

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