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Banking deregulation and innovation ☆

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ABSTRACT

We document empirical support for a key micro-level channel—innovation by young, private firms—through which financial sector deregulation affects economic growth. We find that intrastate banking deregulation, which increased the local market power of banks, decreased the level and risk of innovation by young, private firms. In contrast, interstate banking deregulation, which decreased the local market power of banks, increased the level and risk of innovation by young, private firms. These contrasting effects on innovation also translated into contrasting effects on economic growth. Our study suggests that the nature of financial sector deregulation crucially affects its potential benefits to the real economy.

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1. Introduction

Although the financial economics literature provides robust evidence supporting the Schumpeterian view that financial development fosters economic growth,[1] evidence on the micro-level channels through which this relation manifests remains relatively sparse. In this paper, we build on the premise of endogenous growth theory (e.g., Aghion and Howitt, 1992) to show that financial sector deregulation impacts firm-level innovation and, thereby, economic growth. Interestingly, however, we find that the effects of financial deregulation crucially depend on the *nature* of deregulation and how it affects competition in credit markets. We focus on the impact of intrastate and interstate banking deregulations, and show that they had contrasting effects on innovation by young, private firms

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[1] King and Levine (1993a,b), Demirgüç-Kunt and Maksimovic (1998), Rajan and Zingales (1998), and Beck and Levine (2004) provide cross-country evidence, while Jayaratne and Strahan (1996, 1998), Cetorelli and Strahan (2006), and Beck, Levine, and Levkov (2010) provide evidence within the U.S.

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that led to corresponding effects on economic growth. Our results highlight that innovation by young, private firms is a key channel through which finance affects economic growth.

We focus on the impact of banking deregulation on the innovative activity of young, private firms for two important reasons. First, banks play an important role in financing young, private firms (e.g., Berger, 2010; Nanda and Nicholas, 2012). Even with the growth of venture capital in recent years, empirical and anecdotal evidence suggests that banks continue to play an important role in the financing of innovation. Robb and Robinson (forthcoming) find that firms in the Kauffman Firm Survey—primarily young, private firms—rely extensively on bank financing. Berger and Udell (1998) find that commercial bank loans provide 19% of all financing for small businesses in the National Survey of Small Business Finances sample.[2] Cetorelli and Strahan (2006) document that 70% of the surveyed firms in the 1998 Survey of Small Business Finance (SSBF) use banks for credit.[3] Second, innovation is a fountainhead of economic growth and a significant body of empirical evidence shows that young, private firms are key drivers of path-breaking innovation (e.g., Akcigit and Kerr, 2011; Acs and Audretsch, 1987, 1988, 1993; Zucker, Darby and Brewer, 1998; Kortum and Lerner, 2000; Samila and Sorenson, 2010; Darby and Zucker, 2003).

We disentangle the effects of two disparate forms of banking deregulation in the U.S.: *intrastate* deregulation that allowed banks to expand within states, and *interstate* deregulation that allowed banks to expand beyond state boundaries. Using the results of previous studies, we hypothesize that intrastate and interstate deregulations had contrasting effects on the local market power of banks and, thereby, innovation by young, private firms. Consistent with our hypotheses, we show that while intrastate deregulation *decreased* the level and risk of innovation by young, private firms, interstate deregulation *increased* both. Further, the contrasting effects of intrastate and interstate deregulation on innovation resulted in corresponding effects on economic growth. Our findings, therefore, highlight that the nature of financial deregulation crucially impacts its benefits to the real economy.

Existing evidence suggests that intrastate deregulation increased banks' bargaining power with young, private firms, while interstate deregulation decreased the same. Though both intra- and interstate banking deregulation increased consolidation in the banking industry, they had contrasting effects on the bargaining power of young, private firms vis-à-vis their lenders. Mergers following intrastate deregulation often involved some overlap in local banking markets and served to increase concentration in local markets. Further, after intrastate deregulation, efficient banks acquired inefficient banks within the same state. For example, Jayaratne and Strahan (1998) document that, after intrastate deregulation, banks' profitabilities improved as reflected in decreased non-interest costs and, crucially, reduced loan losses. Moreover, the market share of small banks within a state decreased sharply (e.g., Strahan, 2002). Compared with small banks, large banks lend disproportionately less to small firms (e.g., Berger, Kashyap, and Scalise, 1995; Cole, Goldberg, and White, 2004; Berger, Miller, Petersen, Rajan, and Stein, 2005). Therefore, we argue that intrastate deregulation increased the bargaining power of banks vis-à-vis young, private firms by creating more efficient banks and decreasing the proportion of small banks within a state.

In contrast, interstate deregulation led to an active market for corporate control among banks (e.g., Berger, Kashyap, and Scalise, 1995; Hubbard and Palia, 1995; Berger, 2010). Annual acquisition rates across states increased significantly (e.g., Strahan, 2002; Stiroh and Strahan, 2006). The lower entry barriers following interstate deregulation increased competition from strong, out-of-state banks (in contrast to intrastate deregulation, where the competition from in-state banks reduced). This expanded the set of banks from which firms could borrow and thereby reduced banks' bargaining power.

Prior literature generates ambiguous predictions for the impact of banks' market power on the availability of credit to young, private firms (e.g., Berger, 2010) and, therefore, innovation by these firms. Under the traditional structure-conduct-performance hypothesis (e.g., Berger, Hasan, and Klapper, 2004), a decrease in competition restricts the supply of credit and, thereby, decreases innovation. In addition, because innovation is a risky activity, the number of "very good" and "very bad" projects with payoffs in the "right" and "left" tails decrease. The incomplete contracting literature (e.g., Grossman and Hart, 1986; Hart and Moore, 1990; Hart, 1995) delivers similar predictions. Intuitively, an increase in the bargaining/holdup power of banks lowers entrepreneurs' ex post rents from innovation and thereby dampens potential entrepreneurs' incentives to invest in innovation ex ante. Therefore, an increase in banks' bargaining power lowers the overall amount of innovation.[4]

In contrast, studies in the relationship banking literature (e.g., Rajan 1992; Petersen and Rajan, 1994, 1995) predict that an increase in the bargaining power of banks could lead to *more* innovation by young, private firms. An increase in banks' bargaining power increases the

[2] Other outside funding sources are trade credit at 16%, finance company loans at 5%, venture capital investments at 2%.

[3] While the venture capital market has grown considerably since the late 1990s, it has largely focused on the high-tech and biotech sectors. Innovative firms, especially young, private ones, in other industries continue to depend on banks as primary sources of credit. For example, *The Austin Business Journal* (May 25, 1997) reports that banks such as Imperial Bank, Silicon Valley Bank, Texas Commerce Bank, and Bank One have created a niche somewhere between conventional lending and venture capital by lending to high-tech companies.

[4] These arguments can be formalized in a simple framework where risk-averse entrepreneurs obtain financing from banks for their innovative projects (see Chava, Oettl, Subramanian, and Subramanian, 2012 for details). In this framework, an entrepreneur's expected utility declines with project risk. Therefore, she chooses to invest in innovation if and only if the risk is below a threshold. An increase in banks' bargaining power decreases the entrepreneurs ex post rents and, thereby, *lowers* the threshold level of risk below which projects are undertaken ex ante. Consequently, the amount of innovation as well as the number of projects with payoffs in the left and right tails decrease with an increase in banks' bargaining power.

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availability of credit to relationship borrowers by encouraging banks to invest in relationships that provide future benefits. Reduced competition helps banks to enforce implicit contracts in which borrowers receive cheaper loans in the short-term, but expect to pay higher rates in the long-term. A decrease in bank competition, therefore, encourages innovation by enhancing credit availability to young, private firms.

The contrasting predictions for the effects of changes in banks' market power on innovation motivate our two refutable hypotheses. First, an increase in banks' bargaining/market power leads to a decrease in the level of innovation by young, private firms. Second, an increase in banks' bargaining/market power leads to a decrease in the number of innovative projects undertaken by young, private firms that have payoffs in the "right" and "left" tails. Since intrastate deregulation increased the local market power of banks, we hypothesize that intrastate deregulation decreased the overall level and risk of innovation by young, private firms. In contrast, since interstate deregulation decreased banks' bargaining power, we hypothesize that interstate deregulation increased the overall level and risk of innovation by young, private firms.

As proxies for the level of innovation, we use the number of patents filed by firms as well as the cumulative number of citations to these patents, which provide a measure of the quality of innovation (e.g., Griliches, 1990; Hall, Jaffe, and Trajtenberg, 2001).[5] As a proxy for the risk of innovation, we use the number of patents with "high" citations and those with "low" citations. In our main specifications, we include state and year fixed effects to control for time-invariant determinants of our dependent variables at the state level as well as time-dependent determinants that are constant across states. In additional specifications, we separately control for state-specific time trends and industry-specific time trends. Our results are both quantitatively and qualitatively unchanged by their inclusion. These time trends allow us to more precisely distinguish the effects of banking deregulation on innovation using deviations from state- and industry-level mean trends due to confounding factors.[6]

We find results consistent with our hypotheses. Intrastate deregulation reduced the number of patents filed by young, private firms by 23%, while interstate banking deregulation increased the same by 17%. In addition, intrastate deregulation led to a decrease in the variance of the quality of patents while interstate deregulation led to an increase in the same. Specifically, intrastate deregulation decreased the number of good and bad patents (patents with citations in the top and bottom quartiles of the citation distribution) by 33% and 19%, respectively. In contrast, interstate deregulation increased the number of good and bad patents by 21% and 22%, respectively.

To address potential concerns about reverse causality, we examine the *dynamic* effects of intrastate and interstate deregulation on the level and risk of innovation. We find that there were no effects prior to deregulation. While the effects manifested within a year, they were much stronger three years after deregulation. Apart from mitigating the possibility of reverse causality, the time lag in the effects of banking deregulation is consistent with the fact that innovative projects involve long gestation periods.

In unreported results, we undertake the tests separately for mature, private firms as well as public firms. Such firms have alternate financing avenues, and are likely to be less significantly affected by changes in the nature of bank competition. Consistent with these arguments, we do not find a significant effect of intrastate or interstate deregulation on innovation by such firms. These falsification tests provide further support for our hypotheses.

We also conduct tests that distinguish between explorative and exploitative innovation (e.g., Sørensen and Stuart, 2000) as well as product and process innovation. Explorative innovation is likely to be more path-breaking and riskier than exploitative innovation, which builds on prior innovation by the firm (e.g., March, 1991). We find that the changes in the level and risk of innovative activity following deregulation were contributed largely by explorative rather than exploitative innovation. Apart from introducing these innovation measures into the finance literature, a key contribution of our study is to show that finance possibly contributes to economic growth by fostering mold-breaking, explorative innovation by young, private firms. Product innovation, which involves the creation of new products, is likely to be riskier than process innovation. Consistent with our hypotheses, we find that banking deregulation impacted product innovation, but not process innovation.

Next, we exploit interstate differences in the proportion of large firms in a state before deregulation to further probe the channels through which banking deregulation affected innovation by young, private firms. Because small firms are likely to have lower bargaining power vis-à-vis banks, a change in bank bargaining power would disproportionately affect innovation by small firms compared to large firms. Consequently, we would expect that the impact of deregulation was relatively muted in states with relatively large firms when compared to states with relatively small firms. Consistent with these arguments, we find that the impact of banking deregulation on innovation was, indeed, relatively muted in states where firms were relatively large.

Motivated by the literature on endogenous growth and evidence in Jayaratne and Strahan (1996), we finally examine whether the differential impacts of intrastate and interstate banking deregulations on innovation led

[5] Chava, Chong, and Nanda (2012) provide evidence that lenders value both the level and quality of patents of borrowers. They document that ex ante firms with significant patent activity receive cheaper bank loans as compared to firms with low patent activity. Lenders, especially experienced banks, provide cheaper loans when patents are of high quality, i.e., well cited and more general patents. Ex post, when covenants are violated and control rights pass to lenders, experienced lenders cut research and development (R&D) significantly, specifically when the violating firm has a lower R&D efficiency. Consistent with these results, stock markets react less negatively to technical covenant violations of innovative firms when the bank is experienced.

[6] The inclusion of a time trend for each state ensures that any differential pre-trends in innovation across states are controlled for (e.g., Angrist and Pischke, 2008). Although industry-specific trends in innovation are unlikely to be correlated with the timing of banking deregulation in a state, the tests employing industry-specific time trends nevertheless control for this possibility.

to similar contrasting effects on economic growth.[7] For this purpose, we construct an industry-level measure of the propensity of young, private firms in an industry to innovate. We classify an industry as "innovative" if the value of the "innovation" proxy for young, private firms in that industry is greater than the median value of the proxy for young, private firms across all industries. By interacting this proxy for innovative industries with the dummies for intrastate and interstate deregulations, we find that the positive effect of interstate deregulation on innovation led to a positive effect on economic growth. The effect lasted at least eight years after interstate deregulation and was economically large. Growth due to interstate deregulation in the innovative industries was approximately 0.40% greater per annum than other industries. The coefficients representing the effects of intrastate deregulation on economic growth through innovation by young, private firms are not statistically significant at conventional levels, but are all uniformly negative. This result is also consistent with intrastate deregulation having a negative effect on economic growth because of its negative effect on innovation by young, private firms.

Our results suggest that policies that are aimed at developing financial markets can have a positive externality on the economy by boosting innovative activity and, thereby, long-term economic growth. The negative effects of intrastate deregulation and the positive effects of interstate deregulation on innovation, however, suggest that the manner in which financial sector reform is carried out is important to realize its potential benefits to the real economy. To the best of our knowledge, ours is the first study to: (i) investigate innovation by young, private firms as a channel through which the financial sector affects growth; (ii) show the contrasting effects of intrastate and interstate deregulations on real outcomes; and (iii) distinguish between explorative and exploitative innovation and show that financial deregulation primarily impacts mold-breaking, explorative innovation.

The rest of the paper proceeds as follows. We briefly review related literature in Section 2. Section 3 describes the hypotheses we test in the paper and explains the sources and construction of the data used in the empirical analysis. Our empirical results are presented in Section 4. We provide concluding thoughts in Section 5.

2. Related literature

Our study relates to the literature that examines the real effects of financial development (e.g., Jayaratne and Strahan, 1996; Black and Strahan, 2002; Kerr and Nanda, 2009; Beck, Demirgüç-Kunt, Laeven, and Levine, 2008; Beck, Demirgüç-Kunt, and Maksimovic, 2004). Our study contributes to this literature by focusing on an important micro-level channel, innovation, through which the financial sector could affect economic growth. Our results suggest that financial development influences both the level and risk of innovative activities and, thereby, economic growth.

Rice and Strahan (2010) use the differences in state openness to interstate branching as an instrument for variation in credit competition. They find that in states that were more open to branching, small firms borrowed at significantly lower interest rates than firms operating in states that were less open. However, while more firms used bank debt in states open to interstate branching, this increase in bank debt did not translate into more total borrowing, higher rates of credit approval, or changes in debt maturity. Their results suggest that competition may *increase* credit rationing even as the price of credit falls. Our results complement Rice and Strahan (2010) by highlighting the differential effects of intrastate and interstate banking deregulation and the consequent change in local market power of lenders on the innovative performance of small, young, private firms.

Our paper also contributes to the literature that studies the impact of banking consolidation on measures of credit availability and economic performance. As Berger (2010) highlights, the empirical evidence on this issue is mixed so far. Some studies find favorable effects of concentration and other restrictions on competitiveness on measures of credit availability (e.g., Petersen and Rajan, 1995; Cetorelli and Gambera, 2001; Bonaccorsi di Patti and Dell'Ariccia, 2004; Cetorelli, 2004). Some other studies, however, document unfavorable effects (e.g., Black and Strahan, 2002; Berger, Hasan, and Klapper, 2004; Karceski, Ongena, and Smith, 2005; Cetorelli and Strahan, 2006). Black and Strahan (2002) also find contrasting effects of intrastate and interstate deregulation on economic outcomes.[8] Our results complement the findings in the aforementioned studies by showing that intrastate and interstate deregulations had contrasting effects on innovation and economic growth.

A number of studies find that banks became larger after deregulation. Cole, Goldberg, and White (2004) and Berger, Miller, Petersen, Rajan, and Stein, (2005) find that large banks are likely to lend less often to young, private firms. On the other hand, Berger, Saunders, Scalise, and Udell (1998) find that other banks in the local market—particularly small banks that have a comparative advantage in lending to young, private firms—substitute for lower lending to young, private firms by large banks. Therefore, the effects of the change in bank size due to deregulation on innovation is not clear. As our tests that include state-specific trends control for the effects due to

[7] We replicate the results in Jayaratne and Strahan (1996) and find that banking deregulation increased economic growth by 1.354%, which is very close to the 1.4% that Jayaratne and Strahan (1996) estimate.

[8] In Table 5 (columns 3 and 4) of their study, Black and Strahan (2002) find that interstate deregulation had a positive effect on business incorporations, while intrastate deregulation had a negative, statistically significant effect on business incorporations. Cetorelli and Strahan (2006) find that firm size decreased and firm entry increased after interstate deregulation. However, in Table 5 (columns 1, 3, and 5) they find that the effects of interstate and intrastate deregulations on the number of establishments were opposite in sign to each other even though the coefficients for the interaction of financial dependence with intrastate deregulation are insignificant. Similarly, in Table 8 as well (columns 1, 2, 3, and 5) they find opposing effects of intra- and interstate deregulations.

changes in bank size, our results suggest that the effects of changes in banks' market power dominated those of any changes in bank size.

Three contemporaneous papers also examine the impact of banking deregulation on innovation. Two studies examine the effects of *interstate* deregulation on innovation by public firms (e.g., Amore, Schneider, and Zaldokas, 2012; Cornaggia, Tian, and Wolfe, 2012); whereas one examines the effects of *intrastate* deregulation on innovation by all firms (e.g., Hombert and Matray, 2012). Amore, Schneider, and Zaldokas (2012) provide evidence that interstate banking deregulation had a beneficial impact on innovation by public firms, whereas Cornaggia, Tian, and Wolfe (2012) find the opposite. Hombert and Matray (2012) find that intrastate deregulation decreased innovation by all firms. We differ from these studies in five important ways. First, we examine the effects of both intrastate and interstate banking deregulation on innovation and find contrasting effects between them. Our results show that it is important to disentangle the effects of intrastate and interstate deregulation on innovation. Second, we focus on the effects of banking deregulation on innovation by *young, private firms*. As we discussed earlier, young, private firms are more likely to depend on bank debt to finance innovation. Further, private firms are the dominant source of path-breaking innovation. Mature, private, and public firms can access financing from sources other than bank financing, and innovation by such firms is often due to spillover effects of innovation by young, private firms. Third, we examine the effects of banking deregulation not only on the level of innovation, but also on the risk of innovation. Fourth, we show that the effects of deregulation on innovation by young, private firms were associated with corresponding effects on economic growth as well. Finally, we distinguish between explorative and exploitative innovations and show that changes in innovation due to deregulation stemmed primarily from relatively more path-breaking and riskier, explorative innovation. As the concept of "creative destruction" as enunciated by Schumpeter (1942) suggests that explorative/path-breaking product innovation is the main driver of economic growth, our study contributes to the literature by studying such nuanced differences among different types of innovation.

Our work also relates to the emerging literature examining the effects of laws and regulations on innovation. Acharya and Subramanian (2009) find that debtor-friendly bankruptcy laws foster innovation and economic growth, while Acharya, Baghai, and Subramanian (2012) argue theoretically and provide empirical evidence that laws that impose restrictions on dismissal of employees encourage innovation and entrepreneurship. Chemmanur and Tian (2012) find that firm-level anti-takeover provisions encourage innovation. Sapra, Subramanian, and Subramanian (2013) develop a theory of the effects of external and internal corporate governance mechanisms on innovation. They show empirical support for the testable prediction of their theory that the degree of innovation varies in a U-shaped manner with the severity of external takeover pressure as measured by the stringency of state-level anti-takeover laws.

3. Hypotheses and data

3.1. Testable hypotheses

Our discussion in Section 1 leads to the following refutable hypotheses:

Hypothesis 1. Intrastate deregulation led to a decrease in the level of innovation by young, private firms.

Hypothesis 2. Intrastate deregulation led to a decrease in the number of innovative projects undertaken by young, private firms that have payoffs in the "high" and "low" tails.

Hypothesis 3. Interstate deregulation led to an increase in the level of innovation by young, private firms.

Hypothesis 4. Interstate deregulation led to an increase in the number of innovative projects undertaken by young, private firms that have payoffs in the "high" and "low" tails.

3.2. USPTO data on patents and citations

To construct our innovation measures, we use patents filed by U.S. firms with the United States Patent and Trademark Office (USPTO) and the citations to these patents, compiled in the National Bureau of Economic Research (NBER) Patents File (e.g., Hall, Jaffe, and Trajtenberg, 2001).[9] We date our patents according to the year in which they were applied for to avoid anomalies that may be created due to the lag between the date of application and the date of granting of the patent (e.g., Hall, Jaffe, and Trajtenberg, 2001). Note that although we use the application year as the relevant year for our analysis, the patents appear in the database only after they are granted. Hence, we use the patents actually granted (rather than the patent applications) for our analysis.

The unit of analysis is the state-year. We include all 51 states (including the District of Columbia) and the years from 1975 to 2005 following Beck, Levine, and Levkov, (2010), which leads to a final sample of 1,581 (51×31) observations. In some specifications, we measure innovation at the level of the one-digit NBER technology class (e.g., Hall, Jaffe, and Trajtenberg, 2001), which results in 9,486 observations (51 states $\times$ 31 years $\times$ 6 technology classes). We drop firms with fewer than two lifetime (between 1975 and 2011) patents in a given state.

Patents have long been used as indicators of innovative activity in both micro- and macro-economic studies (e.g., Pakes and Griliches, 1980; Griliches, 1990). Although patents provide an imperfect measure of innovation, there is no other widely accepted method that can be applied to capture technological advances. An an alternative to patents, R&D spending at the firm/industry level could

[9] The NBER patent data set provides (among other items) annual information on patent assignee names, the number of patents, the number of citations received by each patent, the technology class of the patent, and the year that the patent application is filed. The USPTO defines "assignee" as the entity to which a patent is assigned. To link the patent data with Compustat, we exploit the fact that each assignee in the NBER patent data set is given a unique and time-invariant identifier.

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be a potential proxy for innovation. However, this presents several challenges. First, accounting norms—particularly whether R&D is capitalized or is expensed—would have a mechanical effect on R&D spending. Because such practices may vary across firm/industries, these mechanical effects may influence the measures of R&D spending.[10] Moreover, R&D spending represents the input to innovation while patents and citations capture the output of innovation. In any case, Griliches (1990) emphasizes that there is a strong relationship in the U.S. between R&D and the number of patents received at the cross-sectional level across firms and industries. The median R^2 is of the order of 0.9.

3.3. *Proxies for the level and risk of innovation: dependent variables*

We measure the level of innovation using the following:

- *Number of patents*: This variable captures a simple count of the number of distinct patents applied for (and subsequently granted) by assignees in state s in year t.
- *Number of citations*: This variable captures the number of citations to patents applied for (and subsequently granted) by assignees in state s in year t. Citations capture the importance of a patent (e.g., Pakes and Griliches, 1980).[11] While we only examine patenting activity that takes place between 1975 and 2005, we use patent-citation data up through and including 2011 that we obtain directly from the USPTO.

We measure the risk of innovation using the following:

- *Number of patents with "high" citations*: the number of patents applied for (and subsequently granted) by assignees in state i in year t whose citations are above the 75th percentile (4th quartile) of year t's citation distribution.
- *Number of patents with "low" citations*: the number of patents applied for (and subsequently granted) by assignees in state i in year t whose citations are below the 25th percentile (1st quartile) of year t's citation distribution.

We identify a patent assignee as a private firm if there is no GVKEY match for the assignee in the NBER patent database. In our main tests, we classify a private firm as young if it has three or fewer years of patenting experience in the focal state. In robustness tests in Table 7, we also consider break points of five and ten years to define young firms among the class of private firms.

3.4. *Banking deregulation: independent variables*

Given our hypotheses, the two independent variables of interest are the following:

- *Intra* is a dummy variable that switches to one the year after the focal state implemented intrastate banking deregulation. Since intrastate banking deregulation consisted of both de novo and mergers and acquisitions (M&A) deregulation, we follow previous literature (e.g., Jayaratne and Strahan, 1996, 1998) by classifying a state as "intrastate deregulated" the year after either de novo or M&A deregulation.
- *Inter* is a dummy variable that switches to one the year after the focal state implemented interstate banking deregulation (e.g., Kerr and Nanda, 2009; Jayaratne and Strahan, 1996, 1998).

3.5. *Descriptives*

Table 1 presents summary statistics of our key variables. The mean state applies for just under 850 patents in an average year and receives 12,539 citations to these patents. Younger firms (three years or less experience) in a state account for fewer patents, but their average patent quality as measured by citations is higher (16.5 citations per patent) than older firms (14.3 citations per patent).

3.6. *Graphical evidence*

Fig. 1 presents plots from spline regressions of the relationship between intrastate and interstate deregulation on young, private firms. The graphs represent coefficients plots (and their associated 95% confidence intervals as represented by the vertical bars) from a Poisson regression of the number of patents on a series of dummy variables corresponding to pre-treatment leads (years up to and including t_{-3}, and t_{-2}) and post-treatment lags (t_0, …,t_5, and years t_6 and all subsequent years). The omitted variable in these splines is the year before intrastate (interstate) deregulation, which implies that the coefficients in these plots capture the rate of change in patenting in any year vis-à-vis the year before intrastate (interstate) deregulation.

In Fig. 1, we present the results from the spline regressions for young, private firms after intrastate deregulation and interstate deregulation, respectively. In these graphs, we first notice that the rate of change in patenting before the deregulation is insignificant. If bank deregulation caused a change in innovation but not vice versa, then the rate of patenting in the year before deregulation should be statistically indistinguishable from all other years prior to deregulation. This is indeed what we observe, which suggests that reverse-causal effects of changes in innovation in a state on the deregulation are not very plausible in our setting. Second, when compared

[10] It is also possible that financing constraints at the firm/industry level influence the decision to expense/capitalize R&D; if that were the case, the impact on financing constraints due to banking deregulation would introduce a source of endogeneity that would mar identification.

[11] Pakes and Schankerman (1984) show that the distribution of the importance of patents is extremely skewed; i.e., most of the value is concentrated in a small number of patents. Hall, Jaffe, and Trajtenberg (2001) (among others) demonstrate that patent citations are a good measure of the value of innovations.

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Table 1
Descriptive statistics.

The sample consists of 1,581 observations during 1970–2005 with the unit of analysis as state–year. Panel A reports the descriptive statistics for all firms while Panel B and Panel C report descriptive statistics for subsamples that consist of private and public firms, respectively. "Intra dereg" (Inter dereg) is a dummy variable that turns to one the year after the focal state implemented intrastate (interstate) banking deregulation. "Patents" is the number of distinct patents applied for (and subsequently granted) by assignees in state *s* in year *t*. "Citations" is the number of citation-weighted patents applied for (and subsequently granted) by assignees in state *s* in year *t*. "1st Quartile cites" (4th Quartile cites) is the number of patents applied for (and subsequently granted) by assignees in state *s* in year *t* that are in the 1st (4th) quartile of year *t*'s citation distribution.

	Panel A: all firms				*Panel B: private firms*				*Panel C: public firms*			
Variables	Mean	Std. dev.	Min	Max	Mean	Std. dev.	Min	Max	Mean	Std. dev.	Min	Max
Patents	849.15	1,718.73	0	19,535	296.57	522.2	0	6,485	559.15	1,266.32	0	13,541
Patents≤3 yrs	184.77	383.77	0	5,332	125.15	243.96	0	3,332	61.71	149.5	0	2,072
Patents > 3 yrs	664.38	1,379.32	0	14,329	171.42	304.95	0	3,153	497.44	1,144.56	0	11,498
Citations	12,539.45	29,322.47	0	378,168	4,283.3	9,157.76	0	116,817	8,349.62	21,056.84	0	263,879
Citations≤3 yrs	3,048.53	8,141.2	0	118,574	2,011.74	4,953.52	0	70,637	1,068.78	3,362.21	0	52,033
Citations > 3 yrs	9,490.92	22,000.61	0	259,594	2,271.55	4,501.08	0	53,217	7,280.83	18,272.08	0	211,846
1st Quartile cites	243.17	473.64	0	4,945	86.19	147.67	0	1,569	159.2	353.34	0	3,450
1st Quartile cites ≤3 yrs	47.81	93.36	0	1,320	32.08	57.88	0	820	16.38	38.99	0	534
1st Quartile cites > 3 yrs	195.36	393.55	0	3,736	54.11	99.57	0	890	142.82	323.11	0	2,937
4th Quartile cites	204.45	477.38	0	5,844	69.75	149.35	0	1,987	136.22	342.18	0	3,979
4th Quartile cites ≤3 yrs	50.4	131.49	0	1,757	33.45	82.74	0	1,124	17.46	51.39	0	678
4th Quartile cites > 3 yrs	154.05	357.62	0	4,087	36.29	71.17	0	883	118.76	298.31	0	3,310
Intra dereg.	0.71	0.45	0	1	0.71	0.45	0	1	0.71	0.45	0	1
Inter dereg.	0.59	0.49	0	1	0.59	0.49	0	1	0.59	0.49	0	1

to the year before intrastate deregulation, we notice a statistically significant decrease in patenting in the year of intrastate deregulation. Third, when compared to the year before interstate deregulation, we notice a statistically significant increase in patenting one year after interstate deregulation. Thus, while the effects of intrastate deregulation manifested in the year of deregulation itself, the effects of interstate deregulation manifested one year later. Fourth, we find that the decrease (increase) in patenting following intrastate (interstate) deregulation continues to remain statistically significant for at least six years after deregulation occurs.

4. Results

4.1. Empirical strategy

Our main econometric model focuses on the relationship between indicator variables for intrastate and interstate banking deregulation and our proxies for the level and risk of innovative activity. The empirical specification we estimate is as follows:

$$E[Y_{it}] = \exp(\beta * Intra_{it} + \gamma * Inter_{it} + \delta_t + \phi_i), \qquad (1)$$

where Y_{it} is one of our four dependent variables measured in state i in year t, δ_t represents a set of year dummies, and ϕ_i a set of state dummies. The inclusion of state dummies results in the identification of β and γ solely from within-state variation across time. All time-invariant characteristics of the state that may influence its innovative output are controlled for with state dummies. For example, in a specification without state fixed effects, a state's (time-invariant) comparative advantage/disadvantage in innovative activity may influence the estimated effect of banking deregulation on innovation if deregulation occurs earlier/later in a systematically correlated manner with such state-level, unobserved factors. All state-invariant time trends are controlled for with the time dummies. Thus, any secular (i.e., across the U.S.) shocks in innovation coinciding with the timing of banking deregulation (which varies from state to state) are controlled for using the time fixed effects.

Due to the count-based nature of our dependent variables, we employ a fixed-effects Poisson estimator (e.g., Hausman, Hall, and Griliches, 1984). This estimator is computationally straightforward and also has strong robustness features when estimated by quasi-maximum likelihood (QML). We report "Wooldridge" robust standard errors (e.g., Wooldridge, 1999) that are robust to overdispersion, are valid under any variance assumption, and allow for arbitrary serial correlation that is often found in difference-in-differences settings (e.g., Bertrand, Duflo, and Mullainathan, 2003).

Since deregulation occurred gradually and states deregulated at different times, we can use states that had not deregulated at a point in time to control for potentially confounding effects and thereby estimate a difference-in-differences: the difference in the level of innovation in a state before and after the deregulation compared to this difference for states that did not undergo a deregulation during the same period. Furthermore, because interstate deregulation followed intrastate deregulation after a gap of a few years, and the gap between intrastate and interstate deregulation varied across states, we are able to disentangle the effects of these two forms of deregulation.

4.2. Effects on innovation by young, private firms

We focus our empirical analysis on patenting activity by young, private firms. As discussed earlier, since young, private firms depend primarily on bank debt for external

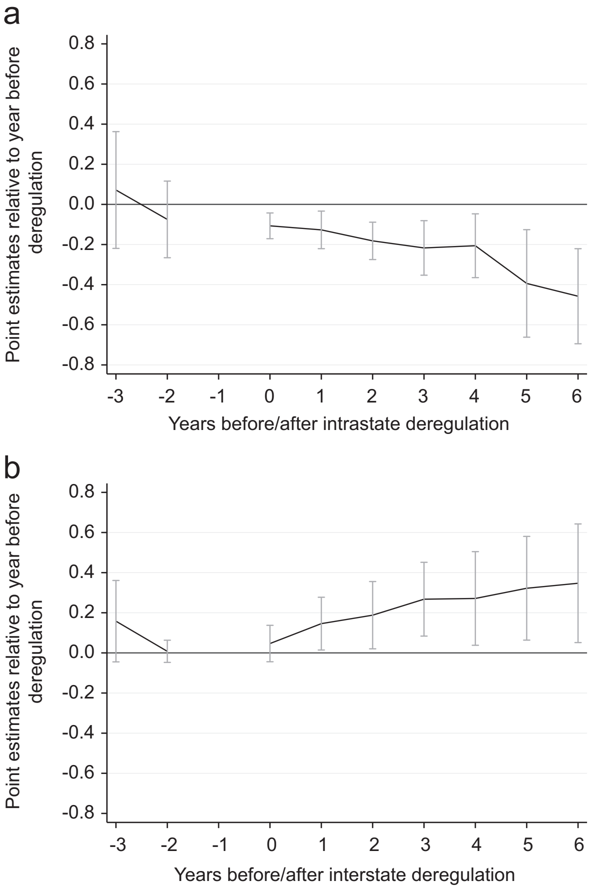

Fig. 1. This figure presents patenting rates for private firms$\leq$3 years of age before and after intra- (Panel a) and interstate banking deregulation (Panel b). This figure plots point estimates for leading and lagging indicators of banking deregulation of the following specification: $Patents_{st} = \exp(\alpha_{-3}\ Intra_{st-3} + \alpha_{-2}\ Intra_{st-2} + \alpha_0 Intra_{st} + \cdots + \alpha_6 Intra_{st+6} + \beta_{-3}\ Inter_{st-3} + \beta_{-2}\ Inter_{st-2} + \beta_0 Inter_{st} + \cdots + \beta_6 Inter_{st+6} + \mu_s + \delta_t)$. "*Patents*" is the number of distinct patents applied for (and subsequently granted) by assignees in state *s* in year *t*. "$Intra_{st}$" ($Inter_{st}$) is a dummy variable set to one if state *s* deregulated in year *t* and zero otherwise. $Intra_{st-3}/Inter_{st-3}$ is set to one for years up to and including three years prior to intrastate/interstate banking deregulation and zero otherwise. $Intra_{st+6}/Inter_{st+6}$ is set to one for all years six years after intrastate/interstate banking deregulation and zero otherwise. μ_s and δ_t are state and year fixed effects, respectively. The vertical bars correspond to 95% confidence intervals with state-clustered standard errors.

financing, we expect the effects of banking deregulation to manifest primarily for these firms.

Table 2 shows the effects of banking deregulation on innovation by young, private firms (firms that do not have a Compustat GVKEY assigned in the NBER patent data set and that have three or fewer years of patenting experience in state *i* in year *t*). In specifications 1–4, we estimate Eq. (1) where we include state and year fixed effects. In specifications 5–8, we control for the presence of time trends that could differ across states by employing the following specification:

$$E[Y_{it}] = \exp(\beta * Intra_{it} + \gamma * Inter_{it} + \delta_t + \phi_i + t \cdot \phi_i), \qquad (2)$$

where Y_{it} is one of our four dependent variables measured in state *i* in year *t*. Doing so enables to not only allow states to have different intercepts (ϕ_i), but also different slopes across time ($t \cdot \phi_i$).

In specifications 9–12, we include fixed effects corresponding to the technology-class as well as time trends that could vary by technology-class by employing the following specification:

$$E[Y_{ikt}] = \exp(\beta * Intra_{it} + \gamma * Inter_{it} + \delta_t + \phi_i + \eta_k + \eta_k \cdot t), \qquad (3)$$

where the difference compared to Eq. (1) pertains to the inclusion of technology-class effects (η_k) and trends ($t \cdot \eta_k$) in addition to the state and year fixed effects. By accounting for state-specific and technology-specific time trends in specifications 5–8 and 9–12, respectively, we identify the hypothesized effect using deviations (at the technology-class level) from the average time trend for each state and that for each technology class. Since other state- or technology-level factors accompanying banking deregulation could lead to state-specific as well as technology-specific time trends, these controls enable us to isolate precisely the pure effect of deregulation on innovation.

Specifications that control for state-specific time trends are particularly important because we attempt to estimate causal effects of banking deregulation on innovation through a difference-in-differences framework. As Angrist and Pischke (2008) describe "(difference-in-differences) strategies punt on comparisons in levels, while requiring the counterfactual trend behavior of treatment and control groups to be the same." Inclusion of a time trend for each state ensures that any differential pre-trends in innovation in the treatment and control groups are controlled for.

Across the three different sets of specifications described above, we find the coefficient of intrastate deregulation to be negative and the coefficient of interstate deregulation to be positive. The coefficients are statistically significant at the 5% level or lower in all the specifications. The results presented in Table 2 are, therefore, consistent with our four hypotheses. In particular, the coefficients of intrastate and interstate deregulation do not change significantly across columns 1, 5, and 9. This pattern is repeated for each of the four dependent variables that we employ in Table 2. If differential time trends in innovation across states were correlated with banking deregulation, then the resulting omitted-variable bias would alter the coefficient of intrastate and interstate deregulation when we include the state-specific time trends. However, the fact that the coefficient remains unchanged across the three different specifications for each of the four dependent variables suggests that the estimates are unlikely to be affected by state-specific nor technology-specific trends. Our preferred specifications, therefore, exclude the more computationally intensive year trends and base statistical inference on the inclusion of state and year fixed effects as in columns 1–4.

Quantitatively, intrastate banking deregulation resulted in a 23% ($e^{-0.266}-1 = -0.23$) decrease in patents and a 32% decrease in citation-weighted patents being filed by young, private firms. Moreover, the mass of patents in the left and right tails of the citation distribution decreased by 19% and 33%, respectively, reflecting a

Table 2
Effect of bank deregulation on innovation by young, private firms.

The following table reports regression results for young, private firms. "*Intra*" (*Inter*) is a dummy variable that turns to one the year after the focal state implemented intrastate (interstate) banking deregulation. "Patents" is the number of distinct patents applied for (and subsequently granted) by assignees in state *s* in year *t*. "Citations" is the number of citation-weighted patents applied for (and subsequently granted) by assignees in state *s* in year *t*. "1Q Cites" (4Q Cites) is the number of patents applied for (and subsequently granted) by assignees in state *s* in year *t* that are in the first (fourth) quartile of year *t*'s citation distribution. Results in columns 1–4 include year fixed effects and state fixed effects. Results in columns 5–8 include state *x* year trends in addition to the year and state fixed effects. Columns 9–12 include technology class *x* year trends in addition to the year and state fixed effects. State-clustered robust standard errors are reported in parentheses. *, **, and *** indicate significance better than 10%, 5%, and 1%, respectively.

Dependent variable	(1) Patents	(2) Citations	(3) 1Q Cites	(4) 4Q Cites	(5) Patents	(6) Citations	(7) 1Q Cites	(8) 4Q Cites	(9) Patents	(10) Citations	(11) 1Q Cites	(12) 4Q Cites
Intra	−0.266**	−0.381***	−0.213**	−0.397***	−0.260**	−0.374***	−0.209**	−0.388***	−0.267**	−0.391***	−0.206**	−0.405**
	(0.115)	(0.137)	(0.099)	(0.154)	(0.113)	(0.134)	(0.098)	(0.151)	(0.117)	(0.139)	(0.101)	(0.158)
Inter	0.160***	0.150**	0.198***	0.192***	0.158***	0.148**	0.197***	0.189***	0.168***	0.157**	0.205***	0.206***
	(0.058)	(0.070)	(0.069)	(0.072)	(0.057)	(0.069)	(0.070)	(0.071)	(0.062)	(0.074)	(0.073)	(0.075)
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
State time trend					✓	✓	✓	✓				
Tech. class FE									✓	✓	✓	✓
Tech. time trend									✓	✓	✓	✓
Observations	1,581	1,581	1,581	1,581	1,581	1,581	1,581	1,581	9,486	9,486	9,486	9,486
Num. states	51	51	51	51	51	51	51	51	51	51	51	51

reduction in the risk of innovative activity undertaken by young, private firms. In contrast, the level of innovative activity of young, private firms increased significantly after interstate banking deregulation. Patenting increased 17% after interstate banking deregulation while citation-weighted patenting increased by 16%. The mass of patents in the left and right tails of the citation distribution also increased 22% and 21%, respectively.

Overall, the results in Table 2 support our hypotheses. We find that banking deregulation significantly affected the level and risk of innovative activity by young, private firms. Further, intrastate and interstate deregulations had contrasting effects on the level and risk of innovation by young, private firms.

4.3. *Falsification tests*

As we discussed earlier, we would not expect to find a significant effect of banking deregulation on the level and risk of innovation by mature, private, or public firms. To investigate this, we estimate Eq. (1) on the innovative activity of three different samples of firms: mature private firms, public firms, and the aggregate sample of all patents in the NBER patent data set. We do not find a significant effect of intrastate or interstate deregulation on either the level or the risk of innovative activity undertaken by either set of these firms that are less dependent on bank financing. We omit these results in the interest of brevity; they are available from the authors on request.

Our investigation of the effects of banking deregulation on innovation by mature, private, or public firms serves as a useful "placebo" test of our hypotheses. The presence of a significant impact of banking deregulation exactly where it is predicted to have one—innovation by young, private firms—and not for other firms provides support for the channels through which intrastate and interstate banking deregulations impacted innovation.

4.4. *Temporal dynamics of innovation by young, private firms*

Kroszner and Strahan (1999) suggest that state-level factors that manifested differently across states could have affected the timing of deregulation in different states. If the states also differed in their innovation intensities, it could be the case that such differences triggered the deregulation, thereby suggesting the presence of a reverse-causal relationship between deregulation and innovation. Our earlier tests, in which we controlled for *state-specific time trends* (that is, time trends in innovation that differ across states) significantly mitigate such concerns. To further explore the possibility of reverse causality, we examine the *dynamics* of innovation by young firms. If reverse causality is indeed present, we should see changes in innovation prior to the deregulation events.

In Table 3, we examine the dynamics of innovation by young, private firms following banking deregulation. To investigate the temporal dynamics, we introduce a series of timing dummies. $Intra(\leq -2)/Inter(\leq -2)$ is a dummy variable set to one for all years up to and including two years prior to intrastate/interstate banking deregulation. $Intra(0)/Inter(0)$ is set to one the year intrastate/interstate deregulation occurs. $Intra(1{,}3)/Inter(1{,}3)$ is set to one for years 1, 2, and 3 after intrastate/interstate deregulation. $Intra(>3)/Inter(>3)$ is set to one for years that are more than three years after intrastate/interstate deregulation. The omitted category is the year before bank deregulation.

The coefficients of $Intra(\leq -2)/Inter(\leq -2)$ are all insignificant, indicating that the level and risk of innovative activity of young, private firms were not significantly affected prior to intrastate or interstate deregulation. The results of our earlier tests that include state-specific time trends *in addition* to the finding that there were no effects on innovation by young, private firms prior to deregulation substantially alleviate concerns about reverse causality.

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Table 3
Dynamic effects of bank deregulation and innovation.

The following table explores the temporal dynamics of innovation by young, private firms. "Intra(≤−2)" (Inter(≤−2) is a dummy variable set to one for all years up to and including two years prior to intrastate (interstate) banking deregulation. "Intra(0)" (Inter(0)) is set to one the year intrastate (interstate) deregulation occurs. "Intra(1,3)" (Inter(1,3)) is set to one for years 1, 2, and 3 after intrastate (interstate) deregulation. "Intra(> 3)" (Inter(> 3)) is set to one for all years three years after intrastate (interstate) deregulation. "Patents" is the number of distinct patents applied for (and subsequently granted) by assignees in state *s* in year *t*. "Citations" is the number of citation-weighted patents applied for (and subsequently granted) by assignees in state *s* in year *t*. "1Q Cites" (4Q Cites) is the number of patents applied for (and subsequently granted) by assignees in state *s* in year *t* that are in the first (fourth) quartile of year *t*'s citation distribution. State-clustered robust standard errors are reported in parentheses. *, **, and *** indicate significance better than 10%, 5%, and 1%, respectively.

Dependent variable	(1) Patents	(2) Citations	(3) 1Q Cites	(4) 4Q Cites
Intra(≤−2)	−0.160	−0.025	−0.246	−0.108
	(0.180)	(0.139)	(0.226)	(0.168)
Intra(0)	−0.175***	−0.097**	−0.239***	−0.130**
	(0.054)	(0.045)	(0.072)	(0.058)
Intra(1,3)	−0.238***	−0.238***	−0.292***	−0.289***
	(0.090)	(0.073)	(0.112)	(0.091)
Intra(> 3)	−0.484***	−0.512***	−0.484***	−0.585***
	(0.151)	(0.138)	(0.169)	(0.162)
Inter(≤−2)	−0.105	−0.139*	−0.129	−0.139
	(0.081)	(0.072)	(0.111)	(0.098)
Inter(0)	0.000	0.026	−0.038	0.097*
	(0.048)	(0.062)	(0.070)	(0.057)
Inter(1,3)	0.127**	0.121*	0.135*	0.186**
	(0.061)	(0.074)	(0.082)	(0.079)
Inter(> 3)	0.212**	0.186*	0.264***	0.323***
	(0.087)	(0.110)	(0.093)	(0.109)
Year FE	✓	✓	✓	✓
State FE	✓	✓	✓	✓
Observations	1,581	1,581	1,581	1,581
Num. states	51	51	51	51

Across all the specifications we see the effects of bank deregulation on the level and risk of innovative activity of young, private firms increase as the time since deregulation increases. The results show that the effects of intrastate deregulation manifested in the first year of deregulation, while the effects of interstate deregulation take more than one year to commence.

4.5. Evidence on the channels

4.5.1. Explorative and exploitative innovation

We now conduct further tests to probe the channels through which banking deregulation affected innovation by young, private firms. *Explorative* innovation is likely to be riskier and characterized by a greater degree of asymmetric information than *exploitative* innovation, which builds on prior innovation by the firm. The strategy literature contends that exploitative innovation involves the refinement and extension of existing technologies and leads to incremental innovation as it is based on "localized learning." In contrast, explorative innovation involves experimentation with new alternatives to the firm and contributes to radical innovation since it is based on "learning-by-experimentation" (e.g., Henderson, 1993; Levinthal and March, 1993). Moreover, exploitative innovation increases the efficiency of existing technologies while explorative innovation is more likely to lead to the discovery of new high-quality and high-impact technologies (e.g., Ahuja, 2000).

Following Sørensen and Stuart (2000), we define *exploiting patents* as those that include at least one *self-citation* by the assignee. In other words, if the assignee of a focal patent includes a citation to a patent previously issued to the same assignee, we call this an exploiting patent. Intuitively, a patent that builds upon a firm's prior innovations exploits previous innovations by the firm. Conversely, an *exploring patent* is one that does not include any self-citations. Consistent with anecdotal evidence, and the evidence discussed above that young, private firms are more likely to indulge in path-breaking innovation, approximately 83% of patents filed by young, private firms are exploring, while 63% of those filed by older, private firms are exploring.

In Table 4, we present results from our classification of patents into *exploring* and *exploiting* patents. Consistent with the intuition that exploratory innovation is likely to be riskier and more path-breaking than exploitative

Table 4
Effect of bank deregulation on exploring/exploiting patents and product/process patents.

The following table reports regression results of exploration/exploitation patents, and product/process patents of young, private firms. "*Intra*" (*Inter*) is a dummy variable that turns to one the year after the focal state implemented intrastate (interstate) banking deregulation. "Exploring patents" is the number of distinct process patents applied for (and subsequently granted) by assignees in state *s* in year *t* that do not include any citations to the assignee's prior patents. These patents can thus be seen as *exploring* new technological domains. "Exploiting patents" is the number of distinct patents applied for (and subsequently granted) by assignees in state *s* in year *t* that include one or more citations to the assignee's prior patents. These patents thus build upon the firm's prior innovative activities. "Product patents" is the number of distinct product patents applied for (and subsequently granted) by assignees in state *s* in year *t*. "Process patents" is the number of distinct process patents applied for (and subsequently granted) by assignees in state *s* in year *t*. State-clustered robust standard errors are reported in parentheses. *, **, and *** indicate significance better than 10%, 5%, and 1%, respectively.

Dependent variable	(1) Exploring patents	(2) Exploiting Patents	(3) Product Patents	(4) Process Patents
Intra	−0.273**	−0.164	−0.270**	−0.112
	(0.111)	(0.146)	(0.116)	(0.120)
Inter	0.151***	0.165	0.170***	−0.135
	(0.056)	(0.104)	(0.058)	(0.160)
Year FE	✓	✓	✓	✓
State FE	✓	✓	✓	✓
Observations	1581	1581	1581	1581
Num. states	51	51	51	51

innovation, we observe a 24% decrease in exploring patents by young, private firms after intrastate deregulation, and an increase of 16% after interstate deregulation in specification 1. In contrast, in specification 2, both intrastate and interstate deregulation do not have a significant impact on exploiting innovative activity. In unreported results, we find that banking deregulation did not affect explorative or exploitative innovation by mature and public firms. The results of Table 4 provide further support for our central premise that banking deregulation affected both the level and quality of innovations produced by young, private firms. Our findings show that banking deregulation largely did so by affecting riskier, path-breaking, exploratory innovation by young, private firms.

4.5.2. Product and process innovation

Product innovation, which leads to the creation of new products, is likely to involve a higher level of innovation and more risk than process innovation, which typically involves improving the efficiency of existing production processes. As a result, we should see the effects of deregulation manifest more strongly for product innovation rather than process innovation.[12]

In columns 3 and 4 of Table 4, we show the results of tests in which we classify patents according to whether they are *product* patents or *process* patents. To construct our product and process patent measures, we focus on the International Patent Classification (IPC) category that largely focuses on Physical or Chemical Processes, IPC category B01. We construct a count of all patents that fall into IPC category B01 as *process patents*, and all others as *product patents*. Using this proxy, we find that approximately 3% of the patents filed correspond to process patents.

Consistent with the intuition discussed above, we see that intrastate (interstate) deregulation had a significant negative (positive) effect on product innovation by young, private firms, but no effect on process innovation. In unreported results, we also find that banking deregulation did not have a significant effect on either product or process innovation by mature, private firms or public firms.

While the above results provide further support for our hypotheses, we should mention that our classification of patents into product and process patents is not entirely clean: patents in other IPC categories may also include process patents. These results, therefore, should be viewed as primarily supportive in nature.

4.5.3. Differential impact based on percentage of small firms in the state

We exploit interstate differences in the proportion of large firms in a state before deregulation to further probe the channels through which banking deregulation affected innovation by young, private firms. Because small firms are likely to have lower bargaining power vis-à-vis banks, a change in bank bargaining power would disproportionately affect innovation by small firms relative to large firms. Therefore, if the changes in banks' bargaining power due to intrastate and interstate deregulations indeed explains the above results, then the impact of the deregulations would be relatively muted in states where firms were relatively large before the deregulations when compared to states where firms were relatively small.

In Table 5, we consider the impact of banking deregulation on innovation in states that are sorted based on the share of small firms in the state before deregulation. We employ the following specification:

$$E[Y_{ikt}] = \exp(\beta \cdot Intra_{it} + \gamma \cdot Inter_{it} + [\eta \cdot Intra_{it} + \rho \cdot Inter_{it}] * Few\ Small\ Firms_i + \delta_t + \phi_i). \quad (4)$$

In the above, “*Few small firms*” is a dummy set to one if the share of small firms in the focal state was below the mean of the year's small firm share the year prior to deregulation, and zero otherwise. This variable is time-invariant within states. We find that intrastate banking deregulation has a negative effect, and interstate banking deregulation has a positive effect on the level and riskiness of innovation of young, private firms. Crucially, we notice that the coefficient of the interaction of interstate deregulation with the dummy for states where the share of small firms was low is consistently negative and statistically significant. We can, therefore, infer that the impact of interstate deregulation on innovation by young, private firms is much lower in states where the share of small firms is lower as compared to the states where the share of small firms is higher. Similarly, we notice that the coefficient of the interaction of intrastate deregulation with the dummy for states where the share of small firms was low is consistently positive and statistically significant in two of the four specifications. We can, therefore, infer that the impact of intrastate deregulation on innovation by young, private firms is much lower in states where the share of small firms is lower as compared to the states where the share of small firms is higher. These results provide additional support for our argument that banking deregulation affected innovation by altering banks' bargaining power vis-à-vis young, private firms.

4.6. Effects on economic growth through innovation by young, private firms

As the literature on endogenous growth (e.g., Romer, 1990; Grossman and Helpman, 1991; Aghion and Howitt, 1992) posits that firm-level innovation is an essential ingredient for economic growth, we now investigate whether the differential impacts of intrastate and interstate banking deregulations on innovation led to corresponding contrasting effects on economic growth. This investigation assumes importance given the evidence in Jayaratne and Strahan (1996) that banking deregulation fostered economic growth as measured by the Gross State Product (GSP). To begin with, we replicate the results in Jayaratne and Strahan (1996). For this purpose, we

[12] In a recent paper, Benfratello, Schiantarelli, and Sembenelli (2008) investigate the effect of local banking development on firms' innovative activities using a rich data set on innovation for a large number of Italian firms. They present evidence that banking development affects the probability of process innovation, particularly for firms that are small.

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Table 5
Effect of deregulation on innovation in states with fewer small firms.

The following table reports regression results of interactions between bank deregulation and states that had fewer small firms prior to deregulation on younger, private firm innovation. "*Intra*" (*Inter*) is a dummy variable that turns to one the year after the focal state implemented intrastate (interstate) banking deregulation. "*Few small firms*" is a dummy set to one if the share of small firms in the focal state was below the mean of the year's small firm share the year prior to deregulation, and zero otherwise. This variable is time-invariant within states. "Patents" is the number of distinct patents applied for (and subsequently granted) by assignees in state *s* in year *t*. "Citations" is the number of citation-weighted patents applied for (and subsequently granted) by assignees in state *s* in year *t*. "1Q Cites" (4Q Cites) is the number of patents applied for (and subsequently granted) by assignees in state *s* in year *t* that are in the first (fourth) quartile of year *t*'s citation distribution. State-clustered robust standard errors are reported in parentheses. *, **, and *** indicate significance better than 10%, 5%, and 1%, respectively.

Dependent variable	(1) Patents	(2) Citations	(3) 1Q Cites	(4) 4Q Cites
Intra	−0.284**	−0.355***	−0.278*	−0.374**
	(0.141)	(0.135)	(0.144)	(0.161)
Intra * *Few small firms*	0.264*	0.233*	0.275	0.285
	(0.153)	(0.142)	(0.168)	(0.174)
Inter	0.315***	0.325***	0.330***	0.377***
	(0.072)	(0.069)	(0.087)	(0.078)
Inter * *Few small firms*	−0.441***	−0.518***	−0.360**	−0.556***
	(0.124)	(0.123)	(0.142)	(0.133)
Year FE	✓	✓	✓	✓
State FE	✓	✓	✓	✓
Observations	1,581	1,581	1,581	1,581
Num. states	51	51	51	51

undertake the following regression:

$$y_{it} = \delta_t + \phi_i + \beta * Deregulation_{it} + \varepsilon_{it}, \qquad (5)$$

where $y_{it} = gsp_{it}/gsp_{i,t-1} - 1$ equals the growth rates in GSP in state i in year t, Deregulation$_{it}$ equals one for states permitting M&A and zero otherwise as in Jayaratne and Strahan (1996). Column 1 in Table 6 shows that deregulation increased economic growth by 1.354%, which is very close to the 1.4% that Jayaratne and Strahan (1996) estimate.

To examine the effect of banking deregulation on economic growth through the innovative activity of young, private firms, we construct an industry-level measure of the propensity of young, private firms in an industry to innovate. For this purpose, we match the industry classification employed by the Bureau of Economic Analysis (BEA) to the technology classes employed in the USPTO data. We perform this match by first matching the BEA industry classification to the two-digit Standard Industrial Classification (SIC) and then using the matching of technology classes in the USPTO data to the two-digit SIC match. We classify a BEA industry as "innovative" if the value of the innovative proxy for young, private firms in that industry is greater than the median value of the innovative proxy for young, private firms across all BEA industries over the entire sample period. For example, using the number of patents filed by young, private firms as the proxy for innovation, we define the "Innovative industry dummy" for BEA industry j as follows:

$$\text{Innovative industry dummy}_j = \begin{cases} 1 & \text{if } \sum_t Patents_{jt} > \underset{j}{Median}\left(\sum_t Patents_{jt}\right) \\ 0 & \text{if } \sum_t Patents_{jt} \le \underset{j}{Median}\left(\sum_t Patents_{jt}\right). \end{cases} \qquad (6)$$

Defining innovative industries in this *time-invariant* manner avoids any endogeneity in the classification of industries due to the effect of the regulation.

Since the effect of banking deregulation on economic growth through the innovation channel would manifest over time, we employ the following specification that accounts for dynamic effects:

$$\begin{aligned} y_{ijt} = {} & \phi_i + \mu_j + \delta_t[\beta_{-1} \cdot Intra(-4,-1)_{it} + \beta_0 \cdot Intra(0,3)_{it} \\ & + \beta_1 \cdot Intra(\ge 4)_{it}] * \text{Innovative industry dummy}_j \\ & + [\gamma_{-1} \cdot Inter(-4,-1)_{it} + \gamma_0 \cdot Inter(0,3)_{it} \\ & + \gamma_1 \cdot Inter(\ge 4)_{it}] * \text{Innovative industry dummy}_j \\ & + [\beta'_{-1} \cdot Intra(-4,-1)_{it} + \beta_0' \cdot Intra(0,3)_{it} \\ & + \beta'_1 \cdot Intra(\ge 4)_{it}] + [\gamma'_{-1} \cdot Inter(-4,-1)_{it} \\ & + \gamma'_0 \cdot Inter(0,3)_{it} + \gamma'_1 \cdot Inter(\ge 4)_{it}] + \varepsilon_{it}. \end{aligned} \qquad (7)$$

In the above, $\beta_{-1}, \beta_0, \beta_1$ $(\gamma_{-1}, \gamma_0, \gamma_1)$ together capture the dynamic effects of intrastate (interstate) banking deregulation on economic growth through innovation by young, private firms. $\beta_{-1}(\gamma_{-1})$ captures the effect of intrastate (interstate) four years prior to one year prior to deregulation, $\beta_0(\gamma_0)$ captures the effect from the year of intrastate (interstate) deregulation to three years after, while $\beta_1(\gamma_1)$ captures the effect of intrastate (interstate) deregulation four years after and beyond. The omitted category is more than four years prior to intrastate (interstate) deregulation. ϕ_i, μ_j, and δ_t capture state, industry, and time fixed effects, respectively.

Columns 2–5 in Table 6 show the results from estimating Eq. (7) using each of the four different proxies for innovation by young, private firms (Number of patents, Number of citations, Number of patents with "high" citations, and Number of patents with "low" citations) to generate the "Innovative industry dummy." Across all four specifications, we notice that the coefficients γ_0 and γ_1,

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Table 6
Growth effects of banking deregulation through innovation by young, private firms.

The following table explores the growth effects of banking deregulation through innovation by young, private firms. In column 1, we replicate the results in Jayaratne and Strahan (1996) by estimating the following specification using Ordinary Least Squares (OLS): $y_{it}=\phi_i+\delta_t+\beta' IntrastateDeregulation_{it}+\varepsilon_{it}$, where $y_{it}=gsp_{it}/gsp_{i,t-1}-1$ equals the growth rates in GSP in state i in year t, Deregulation$_{it}$ equals one for states permitting M&A and zero otherwise as in Jayaratne and Strahan (1996). In columns 2–5, we estimate the following specification using OLS: $y_{ijt}=\phi_i+\mu_j+\delta_t[\beta_{-1}\cdot Intra(-4,-1)_{it}+\beta_0\cdot Intra(0,3)_{it}+\beta_1\cdot Intra(\geq 4)_{it}]$*Innovative industry dummy$_j+[\gamma_{-1}\cdot Inter(-4,-1)_{it}+\gamma_0\cdot Inter(0,3)_{it}+\gamma_1\cdot Inter(\geq 4)_{it}]$*Innovative industry dummy$_j+[\beta'_{-1}\cdot Intra(-4,-1)_{it}+\beta'_0\cdot Intra(0,3)_{it}+\beta'_1\cdot Intra(\geq 4)_{it}]+[\gamma'_{-1}\cdot Inter(-4,-1)_{it}+\gamma'_0\cdot Inter(0,3)_{it}+\gamma'_1\cdot Inter(\geq 4)_{it}]+\varepsilon_{it}$ where $\beta_{-1},\beta_0,\beta_1$ $(\gamma_{-1},\gamma_0,\gamma_1)$ together capture the dynamic effects of intrastate (interstate) banking deregulation on economic growth through innovation by young, private firms. $\beta_{-1}(\gamma_{-1})$ captures the effect of intrastate (interstate) four years prior to one year prior to deregulation, $\beta_0(\gamma_0)$ captures the effect from the year of intrastate (interstate) deregulation to three years after, while $\beta_1(\gamma_1)$ captures the effect of intrastate (interstate) deregulation four years after and beyond. The omitted category is more than four years prior to intrastate (interstate) deregulation. ϕ_i, μ_j, and δ_t capture state, industry, and time fixed effects, respectively. The Innovative industry dummies in columns 2–5 are set to one for industries that are above the mean patent, citations, 1st quartile citations, and 4th quartile citations, respectively. State-clustered robust standard errors are reported in parentheses. *, **, and *** indicate significance better than 10%, 5%, and 1%, respectively.

Innovative industry dummy is based on:	(1)	(2) Patents	(3) Citations	(4) 1Q Cites	(5) 4Q Cites
Intrastate deregulation	1.354**				
	(0.342)				
Intra(−4,−1) * Innovative industry dummy		−0.234	−0.123	−0.118	−0.102
		(0.220)	(0.234)	(0.229)	(0.208)
Intra(0,3) * Innovative industry dummy		−0.338	−0.315	−0.372	−0.331
		(0.226)	(0.245)	(0.233)	(0.210)
Intra(≥4) * Innovative industry dummy		−0.173	−0.225	−0.202	−0.136
		(0.183)	(0.190)	(0.185)	(0.175)
Inter(−4,−1) * Innovative industry dummy		0.127	0.281	0.256	0.269
		(0.226)	(0.247)	(0.237)	(0.214)
Inter(0,3) * Innovative industry dummy		0.394*	0.419*	0.400*	0.353*
		(0.226)	(0.249)	(0.230)	(0.205)
Inter(≥4) * Innovative industry dummy		0.334*	0.461**	0.409**	0.229
		(0.195)	(0.205)	(0.194)	(0.183)
Intra(−4,−1)		−0.269	−0.359	−0.363	−0.377*
		(0.219)	(0.234)	(0.229)	(0.205)
Intra(0,3)		0.800***	0.784***	0.833***	0.791***
		(0.227)	(0.246)	(0.234)	(0.210)
Intra(≥4)		1.011***	1.057***	1.036***	0.981***
		(0.198)	(0.205)	(0.201)	(0.190)
Inter(−4,−1)		2.638***	2.505***	2.527***	2.519**
		(0.249)	(0.270)	(0.260)	(0.238)
Inter(0,3)		3.041***	3.009***	3.029***	3.068***
		(0.242)	(0.261)	(0.245)	(0.226)
Inter(≥4)		2.812***	2.694***	2.744***	2.894***
		(0.231)	(0.238)	(0.231)	(0.223)
State FE	✓	✓	✓	✓	✓
Industry FE		✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Observations	1,173	20,456	20,456	20,456	20,456
R^2	0.469	0.451	0.451	0.451	0.451

which capture the effects after interstate deregulation on economic growth through innovation by young, private firms, are positive and statistically significant. This result is consistent with interstate deregulation having a positive effect on innovation by young, private firms and, thereby, fostering economic growth over at least the next eight years. Further, even though the coefficients of the interaction of intrastate deregulation with the Innovative industry dummy are not significant, they are all uniformly negative, which is also consistent with intrastate deregulation having a negative effect on economic growth because of its negative effect on innovation by young, private firms. The effects are not statistically significant at conventional levels possibly because of the fact that interstate deregulations were passed about two years after intrastate deregulations, which was perhaps not long enough for the effects on innovation to translate into economic growth. The growth effects through innovation by young, private firms is economically large. Growth due to interstate deregulation in the innovative industries is approximately 0.40% greater per annum than other industries. We also notice that the direct effects of intrastate and

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Table 7
Robustness tests: different age cutoffs to classify private firms into young and mature.

The following table reports regression results exploring the robustness of age cutoffs in classifying firms as young and mature. "*Intra*" ("*Inter*") is a dummy variable that turns to one the year after the focal state implemented intrastate (interstate) banking deregulation. "Patents" is the number of distinct patents applied for (and subsequently granted) by assignees in state *s* in year *t*. State-clustered robust standard errors are reported in parentheses. *, **, and *** indicate significance better than 10%, 5%, and 1%, respectively.

	Dependent variable: patents			
	(1) Firm age ≤5 yrs	(2) Firm age > 5 yrs	(3) Firm age ≤10 yrs	(4) Firm age > 10 yrs
Intra	−0.277**	0.452	−0.220*	0.856
	(0.116)	(0.377)	(0.125)	(0.572)
Inter	0.177***	0.065	0.184**	−0.020
	(0.061)	(0.124)	(0.072)	(0.166)
Year FE	✓	✓	✓	✓
State FE	✓	✓	✓	✓
Observations	1,581	1,581	1,581	1,581
Num. states	51	51	51	51

interstate deregulation on economic growth are positive and extend through time. Note that the positive estimate for γ'_{-1}, which captures growth effects four years prior to interstate deregulation, could be due to the effect of intrastate deregulation as it preceded interstate deregulation.

The above tests enable us to conclude that the contrasting effects of intrastate and interstate banking deregulation on innovation by young, private firms led to similar contrasting effects on economic growth.

4.7. *Robustness tests and alternative interpretations*

4.7.1. *Robustness to definition of young, private firms*

Banking deregulation primarily impacted innovation by young, private firms. So far, we have classified a private firm as young if it has three or fewer years of patenting experience in the focal state. In Table 7, we check for the robustness of our results where we use different cutoffs to define young and mature firms. We see that the results continue to hold when the cutoff is set at five years. Not surprisingly, the results are less statistically significant when the cutoff is set to ten years.

4.7.2. *Alternative interpretations*

Having documented a series of strong and robust results that highlight the relationship between banking deregulation innovative activity by young, private firms, we now address possible alternative explanations for our findings.

Mean reversion: We find that intrastate deregulation had a negative effect, while interstate deregulation had a positive effect on innovation by young, private firms. Since interstate deregulation typically followed intrastate deregulation with the lag of a few years, a possible alternative interpretation may be that these results are due to a combination of: (i) a decline in innovation coinciding with the passage of interstate deregulation due to other extraneous factors and; and (ii) subsequent mean reversion in innovation with a lag equal to the time lag between intrastate and interstate deregulations.

First, we should note that "mean reversion" cannot strictly be assumed to exist; there must be an underlying economic explanation for the phenomenon. Suppose that we were to assume that mean reversion mechanically exists. If the mean reversion were to manifest secularly across all states, the year dummies would control for such secular patterns in innovation. In fact, in order for mean reversion to be a plausible alternative explanation, we would have to observe differing reversion rates between the treatment and control groups of states in order to explain the results from our difference-in-differences results. Further, mean reversion of this type must satisfy three additional conditions to account for our results in their entirety. First, the mean reversion should be present in young, private firms but not in mature, private firms, or public firms. Second, the differential rates of mean reversion in the treatment and control groups of states must not be captured by the state-specific time trends that we included in our other specifications. Third, given the fact that we did not observe any effects on innovation prior to interstate deregulation, the time period within which mean reversion occurs must equal the average time lag between intrastate and interstate deregulation. It is quite implausible that these requirements hold together, which suggests that our results are robust to this alternative interpretation.

Creation of the court of appeals of the federal circuit: The U.S. Court of Appeals of the Federal Circuit (CAFC) was created by Congress in 1982 with a jurisdiction over appeals made pertaining to U.S. patent law. Following the establishment of the court, there was a large surge in patenting in the U.S. that was commonly ascribed to the creation of the court although Kortum and Lerner (1999) attribute the same to other factors such as changes in the management of research. The spur in patenting activity also overlaps with the period when banking deregulation occurred. Our results are, however, unlikely to be driven by this phenomenon for the following reasons. First, the creation of CAFC possibly led to a secularly increasing trend in patenting by U.S. firms. As we find a negative effect on innovation due to intrastate deregulation and a

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positive effect due to interstate deregulation, a uniformly increasing trend in patenting by U.S. firms could not be the omitted variable that leads to such disparate effects. Second, if the creation of CAFC accounted for our results, we should have found statistically significant effects for innovation by mature, private firms, and public firms, which is not the case. Therefore, it is unlikely that our results are driven by the creation of CAFC in 1982.

5. Conclusions

We show that financial sector reform can affect long-term economic growth by influencing innovation by young, private firms. Motivated by prior studies in the structure-conduct-performance, incomplete contracting, and relationship banking literatures, we develop refutable hypotheses for the effects of banks' bargaining/market power on innovation by young, private firms.

We find that the increase in local market power of banks after intrastate deregulation had a negative effect on the innovative activity of young, private firms. Both the level of innovation (as measured by the number of patents and the number of citation-weighted patents) and the risk of innovation (as measured by the number of patents in the tails of the citation distribution) decreased significantly after intrastate banking deregulation. On the other hand, the decrease in local market power of banks after interstate deregulation had a positive effect on innovation by young, private firms. Both the level and risk of innovation increased after interstate banking deregulation.

While intrastate and interstate banking deregulations affected innovation by young, private firms, neither had any effect on innovation by mature, private firms, or public firms. To provide further evidence consistent with the channels through which deregulation affects innovation, we show that the deregulations impacted explorative and product innovation, but not exploitative and process innovation. We document additional support for the hypothesized channels through tests that exploit interstate differences in initial conditions before deregulation.

Our findings suggest that financial development can benefit economic growth by relaxing financial constraints and boosting innovation. More importantly, the evidence suggests that financial development benefits young firms that are more likely to transform industries with their technological breakthroughs than large firms that typically undertake incremental and path-dependent innovation. The contrasting effects of intrastate and interstate deregulations on innovation, however, suggest that the nature of financial sector reform is crucial in unlocking its potential benefits to the real economy.

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