Bank Deregulation and Stock Price Crash Risk

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Abstract

This paper examines two opposing views on the relation between bank branch deregulation restrictions and stock price crash risk: efficiency and relationship. We find robust evidence that the intrastate branching deregulation leads to lower future stock price crash risk, consistent with the efficiency view that branch reform improved bank monitoring efficiency and allowed banks to better constrain borrowers' bad-news-hoarding behavior. This mitigating effect is more pronounced for firms that are more dependent on external finance and lending relationships. Our findings suggest that, as a law aimed at removing restrictions on bank branch expansion, bank deregulation also helps protect shareholders' wealth.

JEL Classification: G3, G20, G14

Keywords: Bank deregulation; stock price crash risk; monitoring; external financial dependence; lending relationships.

I. Introduction

During the last quarter of the twentieth century, most U.S. states removed branching restrictions in the banking sector by allowing banks to open branches within and across state borders. A large body of research has documented that bank deregulation has significantly changed regional banking market structures and led to economic growth (e.g., Jayaratne and Strahan (1996, 1998), Berger, Demsetz, and Strahan (1999), Kroszner and Strahan (1999), and Black and Strahan (2002)).[1] Meanwhile, a growing literature examines the impact of bank deregulation on corporate behavior, such as corporate financing and investment (Zarutskie (2006), Rice and Strahan (2010)), entrepreneurship (Black and Strahan (2002), Ceterolli and Strahan (2006)), and innovation (Chava, Oettl, Subramanian, and Subramanian (2013), Cornaggia, Mao, Tian, and Wolfe (2015), and Hombert and Matray (2016)). However, relatively little is known about whether and how such deregulation affects firm-specific downside risk in the equity market. Thus, this study attempts to fill this literature void by investigating the impact of bank branch deregulation on firms' stock price crash risk.

Previous literature suggests that managers who have privileged access to the firm's private information may have incentives to withhold unfavorable information within the firm or opportunistically manage the timing of disclosing such information (e.g., Jin and Myers (2006), Kim, Li, and Zhang (2011a), (2011b), and Hong, Kim, and Welker (2017)). Although managers can accumulate bad news for an extended period, they will likely reach a tipping point, beyond

[1] For instance, the banking system becomes more integrated after bank deregulation, which stabilizes economic growth (Morgan, Rime, and Strahan (2004)). Moreover, bank branch reform mitigates income inequality by boosting incomes in the lower part of the income distribution (Beck, Levine, and Levkov (2010)).

which the cost of hoarding bad news exceeds the benefit of doing so. It is at this point that the concealed negative information will be made public, leading to a sudden collapse in stock price, namely a stock price crash (Kim et al. (2011a), (2011b)).

Bank deregulation can affect borrowers' bad-new-hoarding behavior in two opposite ways. On the one hand, the efficiency view suggests that branch deregulation reduces stock price crash risk as borrowers are more efficiently constrained from withholding bad news. As a result of bank deregulation, many small banks were acquired and incorporated as branches into large banks, providing an important selection mechanism to remove less efficient banks (Jayaratne and Strahan (1996), (1998), Strahan (2003)). After deregulation, banks that can more efficiently monitor their borrowers are able to maintain their business at the expense of inefficient banks. Jayaratne and Strahan (1996, p.641) conclude that following branch reforms "banks do not necessarily lend more, but they appear to lend better". Further, bank deregulation may lead to greater bank efficiency and better monitoring through the use of borrowers' hard information by large banks. Unlike their smaller counterparts, large banks enjoy a comparative advantage in collecting and processing hard information at lower transaction costs (Liberti and Petersen (2018)), rather than frequently sending loan officers to contact in a personal way (Petersen and Rajan (2002)). Given the above, after branch reforms, banks should monitor their borrowers in a more efficient manner and prevent them from hiding unfavorable information about their financial performance. Thus, the efficiency view predicts that the passage of intrastate branching deregulation mitigates firms' future stock price crash risk.

On the other hand, however, the competing relationship view predicts that branch deregulation may increase borrowers' crash risk. Bank branch deregulation encouraged a shift in bank monitoring nature from relationship-based to arm's-length. In a relationship-based system, banks can effectively collect soft private information about their borrowers through frequent personal interactions and observations, which subsequently mitigate informational frictions between lenders and borrowers (Rajan, (2002), Agarwal and Hauswald (2010), Li, Lu, and Srinivasan (2019)). Compared to large banks with diversified loan portfolio, small local banks possess a relative advantage in collecting and verifying soft information because they have more concentrated exposure to a sector or a region(Berger, Bouwman, and Kim (2017), Berger, Minnis, and Sutherland (2017)). However, such informational advantage of small banks tends to be reversed after branch reforms because large hierarchical banking organizations implement arm's-length monitoring (Chen and Vashishtha (2017)). Therefore, to the extent that branch deregulation impaired banks' ability in acquiring borrowers' soft information, the relationship view would expect an increase in stock price crash risk following bank deregulation.

To test the above competing views, we examine the impact of bank branch deregulation on firms' future stock price crash risk using a difference-in-differences (DID) specification. Consistent with the efficiency view, we find a significant and negative association between deregulation and firm-specific stock price crash risk. The economic impact of bank deregulation is sizable. Bank deregulation reduced stock price crash risk, as proxied by conditional negative skewness (*NCSKEW*) and down-to-up volatility (*DUVOL*) of firm-specific weekly returns, by 14% and 12.7% of their mean values for the whole sample, respectively.

The staggered nature of bank branch deregulation acts as a plausibly exogenous shock to bank lending activities at different points in time, and thus helps to alleviate endogeneity concerns. However, one may still be reasonably concerned that some unobserved factors (e.g., lobbying) that varied across states might have affected the timings of the deregulation events. If this were the case, our results would be spurious and affected by reverse causality. To rule out this potential concern and ensure that the parallel trends assumption is valid, we follow Bertrand and Mullainathan (2003) and conduct a pre-reform trend analysis, that is, we examine the dynamics of stock price crash risk surrounding the deregulatory years. We find no effects prior to the bank deregulation, suggesting that the parallel trends assumption is satisfied.

Another potential endogeneity concern with our analysis is the presence of omitted variables or unobserved shocks that might have coincided with bank branch deregulation and, meanwhile, could determine the changes in firm-level stock price crash risk. This omitted-variable problem may invalidate our interpretation of the causal effect of bank deregulation on stock price crash risk. To circumvent this concern, we follow Cornaggia et al. (2015) and conduct placebo tests by randomly assigning states into each of these deregulation years (without replacement) while maintaining the empirical distribution of those years. If unobservable shocks related to firm-specific stock price crash risk occurred simultaneously with the deregulation, then, despite the incorrect assignments of deregulatory years to states, we would still observe a significant and negative relationship between bank deregulation and crash risk. However, the results of the falsification tests indicate that these counterfactual bank deregulatory events have no effects on stock price crash risk, suggesting that the omitted-variable bias is unlikely to be a concern in our analysis.

Moreover, in further attempts to circumvent endogeneity concerns, we control for firm fixed effects and use propensity score matching to balance observed covariates between the treated and control firms. We also control for a set of additional variables at the firm and state levels, including firm riskiness, innovation, GDP growth rate, GDP per capita, and political balance. Our results are robust to those alternative identifications.

Further, we examine the identifying assumption that the timing of deregulation is exogenous to firms' stock price crash risk. Kroszner and Strahan (1999) document a set of interest group factors that are related to bank deregulation, showing that deregulation is not random. We estimate a hazards model to investigate whether the timing of bank deregulation can be explained by stock price crash risk. Overall, these analyses provide support for the validity of our identification strategy and a causal interpretation of a negative effect of bank branch deregulation on firms' stock price crash risk. Consistent with Kroszner and Strahan (1999), our results suggest that the timing of deregulation is associated with interest group factors such as unit banking, the prevalence of small banks and small firms etc., but is unrelated to stock price crash risk.

As additional robustness checks, we control for another form of bank deregulation, namely interstate bank deregulation. The passage of interstate deregulation laws allowed bank holding companies to freely enter other states and to operate branches across state lines. The results show a significantly negative relation between intrastate branching deregulation and stock price crash risk but an insignificant relation between interstate branching deregulation and crash risk, consistent with the former type of reform playing a more profound role in improving bank intermediation efficiency than the latter (Calem (1994), Jayaratne and Strahan (1996)). When we restrict the sample to firms existing both before and after the deregulatory events, or different sample period, the results are qualitatively similar. Further, we employ alternative measures of stock price crash risk and bank deregulation. Following prior research, such as Hutton, Marcus, and Tehranian (2009) and Kim et al. (2011b), we measure stock price crash risk as the likelihood that a firm experiences more than one price crash week in a fiscal year (*CRASH*). Following Black and Strahan (2002) and Hombert and Matray (2016), we use a deregulation index (*DERINDEX*) to proxy for intrastate branching deregulation. We find that the results based on those alternative measures are in line with the main findings.

We then conduct cross-sectional analyses by conditioning on external financial dependence and lending relationships. Our analysis is motivated by the extant literature deeming bank deregulation as an exogenous shock to credit supply (Black and Strahan (2002), Amore, Schneider, and Zaldokas (2013)) and lending relationship (Hombert and Matray (2016)). If bank regulation indeed affects stock price crash risk through the monitoring channel, this effect should be more noticeable among firms with higher dependence on external financing, which are subject to more intensive bank monitoring. To test this conjecture, we measure external financial dependence as external financial dependence ratio, net change in capital, and bank loan ratio. Consistent with expectation, our results show that the mitigating effect of bank deregulation on stock price crash risk is more pronounced for firms with higher external financial dependence.

Moreover, we argue that bank deregulation tends to improve bank monitoring via increased use of borrowers' hard information. Following this line of reasoning, the effect of bank deregulation on stock price crash risk should be more conspicuous for firms with higher dependence on lending relationships, which used to obtain loans mainly through private communication with banks. Following Hombert and Matray (2016), we construct three variables based on the National Survey of Small Business Finances to classify industries by the strength of their lending relationships. As expected, the mitigating effect of intrastate deregulation on crash risk is more pronounced for firms with higher dependence on lending relationships.

This paper contributes to the literature in several ways. First, it adds to the literature on the economic consequences of bank deregulation, in particular, the real effects of branch reform on stock return distributions at the firm level. Prior studies examine how bank deregulation affects borrowing firms from different aspects (e.g., Black and Strahan (2002), Ceterolli and Strahan (2006), Zarutskie (2006), Rice and Strahan (2010), Chava et al. (2013), Cornaggia et al. (2015), Bai, Carvalho, and Phillips (2018), and Hombert and Matray (2016)). The existing research on bank deregulation largely exploits the bank branch reform as a regulatory shock to bank competition and credit supply. However, although Jayaratne and Strahan (1996), (1997) argue that intrastate branching deregulation exogenously changed bank monitoring mechanisms, few studies have to date provided empirical evidence on that effect. Our study adds to this literature by examining whether bank deregulation reduces corporate stock price crash risk via its monitoring function.

As a contemporaneous related paper, Jiang, Levine, Lin, and Wei (2020) examine the impact of bank deregulation on corporate risk. They find that *interstate* bank deregulation which occurred in the mid-1990s reduced borrowers' operational risk. We differ from this study in three important ways. First, we examine the impact of *intrastate* bank deregulation which occurred earlier since 1970s. Our results are robust to controlling for interstate bank deregulation as an additional variable. Second, we focus on firm-level stock price crash risk whereas Jiang et al. (2020) measure firm risk using ROA volatility and idiosyncratic risk. Unlike those common firm risk measures, stock price crash risk is associated with managerial bad-news-hoarding behavior (Jin and Myers, (2006), Hutton et al. (2009)). When managers mask firm risk levels by hiding information about the volatility of underlying earnings from outside investors, corporate earnings volatility may be reduced but meanwhile stock price crash risk is increased. Third, Jiang et al. (2020) suggest that interstate bank deregulation reduced firm risk through the channels of intensified competition among banks and the relaxation of financing constraints. Increased credit supply from geographically diversified banks eases firms' adverse shocks. However, we argue that bank deregulation can affect firm stock price crash risk due to the improved bank monitoring efficiency following intrastate branch reform (Jayaratne and Strahan (1996)).

Second, our study adds to the literature on stock price crash risk. Recent research has documented a number of firm-specific determinants of crash risk, such as financial reporting quality (Hutton et al. (2009), Kim, Li, Lu, and Yu (2016), Ertugrul, Lei, Qiu, and Wan (2017), and Kim and Zhang (2016)), equity-based executive compensation (Kim et al. (2011a), Xu, Li, Yuang, and Chan (2014)), tax avoidance (Kim et al. (2011b)), and dividend policy (Kim, Luo, Xie (2018)). This literature has also shown some other factors that are related to crash risk, such as religiosity (Callen and Fang (2015)), stock liquidity (Chang, Chen, and Zolotoy (2017)), CEO age (Andreou, Louca, and Petrou (2016)), among others. However, one major challenge of this stream of research is that the determinants of stock price crash risk may be endogenously linked with unobserved firm and managerial characteristics, making inference difficult. Despite the above, there is relatively little understanding of how market structure of the financial industry affects firms' disclosure incentives. The staggered passage of bank branch deregulation allows us to establish a causal effect of the market structure change in the banking industry on corporate crash risk.[2]

[2] Our study is also related to a few recent studies of crash risk that have employed quasi-natural experiment settings for identification purposes. For instance, Ali, Li, and Zhang (2018) find that firms' stock price crash risk is greater in states that have adopted the Inevitable Disclosure Doctrine (IDD) than in non-adopting states. Balachandran, Duong, Luong, and Nguyen (2019) document that the staggered passage of mergers and acquisitions (M&A) laws in 32 countries increases the threat of takeover that disciplines managerial misbehavior and leads to reduced stock price crash risk. Our study adds to this strand of research by utilizing a novel quasi-natural experiment, namely, bank branch reform in the banking industry.

The rest of the paper proceeds as follows. Section II briefly reviews related literature and develops the hypotheses. Section III discusses data and research design. Section IV presents the empirical results of the main analysis. We provide results of additional analyses in Section V, and Section VI concludes.

II. Literature Review and Hypothesis Development

A. Bank Branch Deregulation and Relevant Literature

Traditionally, U.S. banks were subject to extensive regulations on geographical expansion due to the unique features of the U.S. federalism and the political pressure of minority groups (Calomiris (2006)). The 1927 McFadden Act clarified the authority of the states over the regulation of national banks' branching activities within their borders. Consequently, the number and size distribution of banking organizations vary dramatically across states. In most regulated states bank holding companies separately owned capitalized and licensed banks within state borders and some banks were typically allowed to run unit offices. For example, prior to 1987 the regulated Texas had a substantial number (hundreds) of banks and sparse branches, while the unregulated California had a handful of banks but considerable branches.

Up to the 1970s, only 12 states allowed unrestricted statewide branching. The other 38 states progressively relaxed their branching restrictions between the 1970s and the passage of the Interstate Banking and Branching Efficiency Act (IBBEA) in 1994. Two classes of branching restrictions were lifted in the 1970s through 1990s. First, states permitted multibank holding companies (MBHCs) to convert subsidiary banks (existing or acquired) into branches. MBHCs could then expand geographically by acquiring banks and converting them into branches. Second, states permitted *de novo* branching, whereby banks could open new branches anywhere within state borders. Table 1 depicts the years each state relaxed the restrictions on bank branching.

[Insert Table 1 about here]

Our study is related to the literature that examines the economic consequences of deregulating bank branch restrictions. In an early study, Jayaratne and Strahan (1996) suggest that intrastate branching deregulation significantly increases the rates of real per capita growth in income and output. Following this study, a few papers have documented additional evidence that intrastate deregulation is beneficial to the economy. For instance, Black and Strahan (2002) show that following the deregulation the rate of new corporations increases. Ceterolli and Strahan (2006) find that concentrated banking after branching reform restrains potential firm entrants from gaining access to credit. Kerr and Nanda (2009) document that branch banking deregulation brings about exceptional growth in both entrepreneurship and business closures. Beck, Levine, and Levkov (2010) contend that bank branch reform leads to the reduction in total income inequality by boosting the relative demand for low-skilled workers. However, a recent study of Hombert and Matray (2016) suggests that intrastate deregulation may result in unintended negative consequences. Specifically, they find that the number of innovators decreases after bank deregulation because the increase in competition for lending reduces financial constraints for firms in more tangible sectors, but tightens financial constraints for small innovative firms. In a similar vein, Chava et al. (2013) show that intrastate deregulation leads to less supply of credit and less innovation for young and private firms.

In addition to intrastate deregulation, there was another form of bank branch deregulation in the U.S., namely, *interstate* branching deregulation, which allowed banks to expand across state borders. Under this reform, states gradually lifted branching restrictions for bank holding companies to expand beyond state boundaries. Both intrastate and interstate branching deregulation were completed following the passage of the IBBEA of 1994. The literature suggests that interstate deregulation affects state business cycles (Morgan et al. (2004)), bank competition and credit supply (Zarutskie (2006), Rice and Strahan (2010)), corporate innovation (Amore et al. (2013), Cornaggia et al. (2015)), as well as bidder returns (Becher, 2009). However, in this paper we focus on *intrastate* deregulation, rather than *interstate* deregulation. This is primarily because prior studies have suggested that the latter type of branching reform has a limited impact on the structure of the banking sector (e.g., Amel and Liang (1992), Calem (1994), MacLaughlin (1995), and Strahan (2003)), which, as argued above, is important to bank monitoring and stock price crash risk. For instance, Jayaratne and Strahan (1996) show that the deregulation of restrictions on geographic expansion beyond state boundaries has little effect on the costs of intermediation. Moreover, we seek to isolate the effect of an exogenous shock to bank monitoring without any systematic change in banks' ability to diversify geographically. Given these arguments, our main analysis focuses on the intrastate branching deregulation. Nevertheless, in a robustness check we also control for the effect of the interstate branching deregulation.

B. Literature on Stock Price Crash Risk

Our study is also related to the literature investigating the determinants of firm-specific stock price crash risk. In an early study, Chen, Hong, and Stein (2001) find that the recent average monthly turnover and past returns can forecast future stock price crashes. Jin and Myers (2006) then introduce an analytical model, in which stock price crashes occur when managers' accumulated bad news is revealed to the public at once. A key takeaway of their study is that opaque information environment is more likely to stock price crashes.

In a number of empirical studies, factors associated with the accumulation of bad news are shown to lead to future stock price crashes. Some studies investigate the relationship between financial statement factors and crash risk. For example, Hutton et al. (2009) show that financial statement opacity leads to less information revelation and more managerial bad-news-hoarding activities, hence higher crash risk. Kim, Wang, and Zhang (2019) find that less readable 10-K reports allow managers to withhold adverse information and hence lead to higher stock price crash risk. Kim et al. (2011a), (2011b) document that equity incentives and corporate tax avoidance instruments can incentivize managers to purposely withhold negative information, leading to a higher likelihood of future stock price crashes. Some other studies also document factors that affect managers' incentive to withhold bad news and stock price crash risk. Callen and Fang (2015) find that strong religion acting as a social norm can inhibit managers from hoarding bad news and render lower stock price crash risk. Andreou et al. (2016) show that firms managed by younger CEOs are more likely to crash because younger CEOs care more about securing increased permanent compensation early in their career and have more incentives to withhold bad news. Chang et al. (2017) suggest that liquid stocks are prone to crash as they tend to attract transient short term institutional investors who impose pressures on managers to accumulate bad news. Jia (2018) show that corporate innovation strategy may affect stock price crash risk and exploration-oriented firms have higher failure-to-success ratio and are reluctant to disclose negative information about innovation and, hence, more likely to crash. Li and Zhan (2018) find that firms facing more product market threats have higher crash risk as competitive pressure from the product market aggravates managers' incentive to conceal bad news. Our study adds to this strand of literature by examining whether firms' stock price crash risk is influenced by bank deregulation, which is a plausibly exogenous regulatory shock in the banking industry. Importantly, our results reveal that the structural change in the banking industry, as a consequence of bank branch reform, not only affects debtholders' interest but also protects shareholders' wealth.

C. Hypotheses Development

We argue that lifting branching restrictions may affect borrowers' stock price crash risk through the change in bank monitoring. There are two offsetting mechanisms for this effect, namely efficiency and relationship views. Efficiency view implies that branch deregulation improves bank monitoring efficiency, allowing banks to curb borrowers' bad-news-hoarding behavior and, hence, reduces stock price crash risk. Economides, Hubbard, and Palia (1996) show that states with many small, poorly capitalized banks supported the 1927 McFadden Act, which gave states the primary authority over national banks' ability to branch. Jayaratne and Strahan (1997) suggest that branching restrictions retarded the "natural" evolution of the banking industry by preventing better-run banks from establishing branches. Once those branching restrictions were removed, banks were able to acquire their peers and convert them into branches, or were permitted entry via *de novo* branching within state borders (McLaughlin (1995), Rice and Strahan (2010), and Chava et al. (2013)). Indeed, Calem (1994) and Strahan (2003) show that the market share of small banks significantly declined following the branching reform. These entry and consolidation activities play an important role in removing less efficient banks and sharply reduce loan losses (Jayaratne and Strahan (1996), Kroszner and Strahan (1997), (1999), and Dick and Lehnert (2010)). Further, some literature shows that large banks can better monitor their borrowers than small counterparts. Diamond (1984) suggests that large better-diversified banks have greater incentives to monitor borrowers. Unlike small banks, large banks are better equipped to collect and process borrowers' hard information, which is more standardized and verifiable (Stein (2002), Berger, Miller, Petersen, Rajan, and Stein (2005), and Liberti and Petersen (2018)). As such, large banks can make decisions based more on borrowers' hard information and monitor them at lower transaction costs than small banks. For example, Petersen and Rajan (2002) find that loan officers do not have to make regular visits to the borrowing firms but process their financial histories, credit reports, and scoring methods. As a result, banks can better distinguish promising projects from bad ones and effectively monitor borrowers after branch reform. By efficiently acquiring borrowers' information about economic fundamentals and operation status, post-deregulation banks can constrain managers' ability to pile up bad news for an extended period, leading to the following hypothesis:

Hypothesis H1a. The passage of intrastate branching deregulation reduces future firm-specific stock price crash risk.

In contrast, relationship views predicts that branch deregulation exacerbates borrowers' bad-news-hoarding behavior and crash risk by weakening banks' ability in accessing firm private information. Prior to the bank deregulation, bank industry was primarily relationship-based, featuring interpersonal linkages between small banks and borrowers. Such lending relationship allows banks to have an informational advantage (Rajan, (2002), Li et al. (2019)). Berger et al. (2005) argue that small banks are better able to collect and act on soft information than large banks through frequent personal contact with borrowers. Unlike hard information, soft information is difficult to verify and can barely be communicated in numbers (Petersen and Rajan (1994)). Small local banks usually have more concentrated portfolio in a sector or a region so that they are able to access more private soft information about borrowers (Berger, Bouwman, and Kim (2017), Berger, Minnis, and Sutherland (2017)). In a similar vein, Liberti and Petersen (2018) suggest that lending relationships play a useful role in eliciting private information, given that a loan officer can use their discretion to more accurately evaluate a long-term borrower's creditworthiness. However, bank deregulation encouraged banking competition among numerous small local banks and a handful of large diversified banks (Black and Strahan (2002), Stiroh and Strahan (2003)). Consequently, lending relationships were damaged and the banking system switched from relationship-based to arm's-length (Hombert and Matray (2016)). To the extent that arm's-length banking system cripples banks' ability in collecting, processing, and transmitting borrowers' private soft information (Skrastins and Vig (2019)), branch reform induces borrowers to engage in accumulating negative information without being efficiently revealed by large complex banking organizations. Thus, we formulate the competing hypothesis below.

Hypothesis H1b. The passage of intrastate branching deregulation increases future firm-specific stock price crash risk.

III. Data and Methodology

A. Sample Selection

We draw the financial data of U.S. public firms from the COMPUSTAT annual files and stock return data from the Centre for Research in Security Prices (CRSP) database for the period 1962–2001. Our sample starts from the first year in COMPUSTAT and ends two years after the completion of the bank deregulation. Following prior studies (e.g., Hutton et al. (2009), Kim et al. (2011a), (2011b), Kim et al. (2016), and Chang et al. (2017)), we exclude financial firms, firms with year-end share prices below $1, firms with fewer than 26 weeks of stock return data in fiscal years, firm-year observations with negative total assets and book values of equity, and firm-year observations with insufficient financial data to calculate relevant variables. After applying these selection criteria, our final sample comprises 79,231 firm-year observations (8,512 unique firms) from 1962 to 2001.

B. Measuring Bank Branch Deregulation

Consistent with Jayaratne and Strahan (1996), we choose the date of branch deregulation as one on which a state permitted branching via M&A through the holding company structure or *de novo* branching. Our main test variable, the bank branch deregulation indicator (*BRANCH*), is a dummy variable that equals one if a state has implemented intrastate branching deregulation and zero otherwise. As mentioned, Table 1 shows the years of bank branch deregulation on a state-by-state basis.[3]

C. Measuring Stock Price Crash Risk

We follow Hutton et al. (2009) and calculate firm-specific weekly returns by estimating the following equation:

$$r_{j,\tau} = \alpha_j + \beta_{1,j} r_{m,\tau-1} + \beta_{2,j} r_{i,\tau-1} + \beta_{3,j} r_{m,\tau} + \beta_{4,j} r_{i,\tau} + \beta_{5,j} r_{m,\tau+1} + \beta_{6,j} r_{i,\tau+1} + \varepsilon_{j,\tau} \quad (1)$$

where $r_{j,\tau}$ is the weekly return on stock j in week τ, $r_{m,\tau}$ is the return on CRSP value-weighted market index, and $r_{i,\tau}$ is the Fama and French value-weighted industry index in week τ. The lead and lag terms of the market and industry returns are included to account for nonsynchronous trading (Dimson (1979)). We use weekly returns to avoid the concern caused by thinly traded stocks and estimate weekly returns from Wednesday to Wednesday to avoid any contaminating

[3] Following Jayaratne and Strahan (1996) and Beck et al. (2010), we confirm the robustness of the empirical results by dropping Delaware and South Dakota as their banking systems were heavily affected by laws that provided a tax incentive for credit card banks to operate. For example, during the mid-1980s the banking industry in those states expanded quickly and contributed significantly more to economic growth than the banking system in other states.

effects from weekends and Mondays (Wang, Li, and Erickson (1997), Bartholdy and Peare (2005)). The firm-specific weekly return ($W_{j,\tau}$) is calculated as the log value of one plus the residual return from Eq. (1).

We then follow Chen et al. (2001) and Kim et al. (2011a), (2011b) and calculate our primary measure of stock price crash risk, negative conditional skewness (*NCSKEW*), as negative of the third moment of each stock's firm-specific weekly returns divided by the standard deviation raised to the third power. For firm *j* in fiscal year *t*, this measure is defined as

$$NCSKEW_{j,t} = -[n(n-1)^{3/2} \sum W_{j,\tau}^3] / \left[(n-1)(n-2)(\sum W_{j,\tau}^2)^{3/2}\right] \quad (2)$$

where *n* is the number of observations of weekly returns in fiscal year *t*. Firms with high *NCSKEW* are more likely to experience a stock price crash.

Our second measure of firm-specific crash risk is "down-to-up volatility" (*DUVOL*), which is calculated as follows:

$$DUVOL_{j,t} = log\left\{(n_u - 1) \sum_{Down} W_{j,\tau}^2 / (n_d - 1) \sum_{Up} W_{j,\tau}^2\right\} \quad (3)$$

where n_u and n_d are the number of up and down weeks over the fiscal year *t*, respectively. For each stock *j* over fiscal year *t*, we partition all firm-specific weekly returns into down (up) weeks when the weekly returns are below (above) the annual mean. We then calculate the standard deviation of firm-specific weekly returns for each group separately. *DUVOL* is the log ratio of the standard deviation in the down weeks to the standard deviation in the up weeks. A stock with a higher value of *DUVOL* is likely to be more crash prone. This alternative measure of crash risk may be less likely to be excessively influenced by a handful of extreme returns as it does not involve the third moments (Chen et al. (2001)).

D. Control Variables

Following prior literature (e.g., Chen et al. (2001), Jin and Myers (2006), and Hutton et al. (2009)), we include a set of control variables that have been identified to potentially determine stock price crash risk. Detrended stock trading volume ($DTURN_t$) is a proxy for the heterogeneity of investor opinions, calculated as the difference between the average monthly share turnover over fiscal year t and t–1. Stock return volatility ($SIGMA_t$) is calculated as the standard deviation of firm-specific weekly returns over fiscal year t. Past stock returns (RET_t) is calculated as the average firm-specific weekly returns over fiscal year t. Chen et al. (2001) find that firms with a higher intensity of the differences of investor opinions, past stock return mean and volatility are more inclined to crash in the future. Firm size ($SIZE_t$) is calculated as the log of market value of equity at the end of fiscal year t. Market-to-book ratio (MB_t) is calculated as the market value of equity divided by the book value of equity at the end of fiscal year t. Financial leverage (LEV_t) is calculated as the book value of total debt scaled by total assets at the end of fiscal year t. Return on assets (ROA_t) is calculated as income before extraordinary items divided by total assets at the end of fiscal year t. Past stock price crash risk ($NCSKEW_t$) is calculated as the negative conditional skewness for firm-specific weekly returns in fiscal year t. Opacity ($ACCM_t$), calculated as the absolute value of discretionary accruals, where discretionary accruals are the residuals estimated from the modified Jones model (Dechow, Sloan, and Sweeney (1995)). Hutton et al. (2009) find that financial reporting opacity is positively associated with future stock price crash risk. Appendix A provides the definitions of all variables used in this study. To eliminate the effect of outliers, we winsorize all continuous variables at the 1% and 99% levels.

IV. Empirical Results

A. Descriptive Statistics

Table 2 presents the summary statistics of all variables used in our regressions. For the two price crash risk measures, $NCSKEW_{t+1}$ and $DUVOL_{t+1}$, their mean values are –0.2 and –0.118, respectively. We note that the average value of *NCSKEW* is very close to that reported by Kim and Zhang (2015), who also use a similar sample period from 1962 to 2007.The mean of the bank branch deregulation indicator, *BRANCH*, is 0.679, similar to that reported by Cetorelli and Strahan (2006). The summary statistics of the control variables are largely in line with those reported in prior studies (e.g., Kim et al. (2011a), (2011b), Callen and Fang (2015), and Chang et al. (2017)), and thus are not discussed herein to preserve space.

[Insert Table 2 about here]

B. Baseline Specification and Results

Our baseline regression model focuses on the relationship between bank branch deregulation and firm-specific stock price crash risk. The equation we estimate is as follows:

$$Crash\ Risk_{j,t+1} = \beta_0 + \beta_1 BRANCH_{j,t} + \beta_2 DTURN_{j,t} + \beta_3 SIGMA_{j,t} + \beta_4 RET_{j,t} + \beta_5 SIZE_{j,t} + \beta_6 MB_{j,t} + \beta_7 LEV_{j,t} + \beta_8 ROA_{j,t} + \beta_9 NCSKEW_{j,t} + \beta_{10} ACCM_{j,t} + Year_t + State_i + \varepsilon_{j,t}, \quad (4)$$

where the dependent variable $Crash\ Risk_{t+1}$ is measured by *NCSKEW* or *DUVOL* in year t+1 and all right-hand-side variables are measured in year t. The independent variable of interest is the bank branch deregulation indicator ($BRANCH_t$). We control for year and state fixed effects and cluster standard errors by state in our baseline regressions. Including state fixed effects helps address the concern that (unobservable) time-invariant omitted variables that generate variation in a state's stance toward openness to bank branching might be simultaneously correlated with the stock price crash risk of firms in the state.

Since our paper exploits the staggered introduction of bank branch deregulation across states, the specification we use is a generalized difference-in-differences model. The effect of bank branch deregulation is estimated as the difference between the change in stock price crash risk before and after deregulation. Under this specification, a firm in the pre-deregulation periods serves as its own control group for the same firm in the post-reform periods.

Table 3 presents the baseline results. In the first two columns, we regress crash risk, $NCSKEW_{t+1}$ or $DUVOL_{t+1}$, on the bank branch deregulation dummy variable, $BRANCH_t$, without any firm-level control variables but with year and state fixed effects. The results show that the coefficients on $BRANCH_t$ are significantly negative (*t-stat* = –3.05 in Column (1) and –3.24 in Column (2)). We further control for a set of crash risk determinants in the remaining columns. The results show that the coefficients on $BRANCH_t$ are still significantly negative for both crash risk measures (*t-stat* = –3.05 in Column (3) and in –3.34 Column (4)). This suggests that the intrastate branching deregulation reduces firms' future stock price crash risk, consistent with the efficiency hypothesis that the improved bank monitoring after bank deregulation allows banks to better restrict borrowers from hiding bad news.

We further evaluate the economic significance of the effect of bank branch deregulation on firms' future crash risk. The coefficients on $BRANCH_t$ in Columns (3) and (4) of Table 3 are –0.028 and –0.015, respectively, indicating that, holding other factors unchanged, $NCSKEW_{t+1}$ ($DUVOL_{t+1}$) decreases by about 0.028 (0.015). Such effect is economically large given that the mean values of $NCSKEW_{t+1}$ and $DUVOL_{t+1}$ are –0.200 and –0.118, respectively. Thus, the results indicate that branch deregulation led to 14% (12.7%) reduction in stock price crash risk, on average.

These results suggest that the negative association between intrastate deregulation and crash risk is not only statistically significant but also economically important.

Turning to the control variables, we find that the coefficients on stock turnover ($DTURN_t$) are significant and positive, consistent with Chen et al. (2001), suggesting that stocks with higher turnover are more likely to exhibit higher crash risk. Also, consistent with Hutton et al. (2009), $ACCM_t$ is significantly positively associated with stock price crash risk, suggesting that opaque firms are more prone to stock price crashes. Moreover, the coefficients on the remaining control variables such as *SIZE*, *LEV*, *MB*, and *NCSKEW* are also in line with prior studies (e.g., Hutton et al. (2009) and Callen and Fang (2015)).

[Insert Table 3 about here]

C. Endogeneity Tests

1. Pre-treatment Trend Analysis

Our identification is based on the idea that the staggered deregulation of bank branching laws can represent an exogenous shock to bank monitoring effectiveness, thus affecting firms' stock price crash risk. However, one concern with our setting is that, although we have controlled for state fixed effects in the main specification, there may remain omitted state-level factors that could potentially trigger the deregulation in different states. Under this scenario, there might be a reverse causality problem if the states differ in their firms' stock price crash risk and such variation further affects the timing of bank branch deregulation in each state. Following Bertrand and Mullainathan (2003) and Cornaggia et al. (2015), we address the possible reverse causality concern by investigating the dynamic trends of stock price crash risk surrounding the deregulatory events. If reverse causality indeed exists, we should also observe significant changes in stock price crash risk prior to the deregulatory events.

We employ two pre-treatment trend estimation approaches to test whether firms headquartered in states that passed bank deregulation laws (treated) and those that have not yet passed (control) follow a parallel pre-existing trend. First, following Cornaggia et al. (2015), we construct four dummy variables indicating four periods around the deregulation: $Before^{2+}$, $Before^{1}$, $After^{1}$, and $After^{2+}$. $Before^{2+}$ takes one for observations up to, and including, two years prior to deregulation; $Before^{1}$ takes one for one year prior to deregulation; $After^{1}$ takes one for one year post-deregulation; and $After^{2+}$ takes one for two years or more post-deregulation. Specifically, we estimate the following model:

$$Crash\ Risk_{j,t+1} = \beta_0 + \beta_1 Before_{i,t}^{2+} + \beta_2 Before_{i,t}^{1} + \beta_3 After_{i,t}^{1} + \beta_4 After_{i,t}^{2+} + Controls_{j,t} + Year_t + State_i + \varepsilon_{j,t}. \quad (5)$$

Columns (1) and (2) of Table 4 present the estimation results. We regress both measures of crash risk on the four period indicators along with the control variables, year and state fixed effects. The results show that the coefficients on $Before^{2+}$ and $Before^{1}$ are statistically insignificant, suggesting that stock price crash risk experiences no significant change prior to bank branch deregulation. The coefficients on $After^{1}$ and $After^{2+}$ are significantly negative, which is consistent with the baseline findings.

In a similar vein, we follow Hombert and Matray (2016) and define four alternative indicator variables: $Before^{5+}$, $Before^{1,4}$, $After^{1,4}$, and $After^{5+}$. $Before^{5+}$ takes one for all years up to and including five years prior to deregulation. $Before^{1,4}$ takes one for the four years preceding deregulation. $After^{1,4}$ takes one for the four years following deregulation. $After^{5+}$ takes one for all years five years after deregulation. The estimation results in Columns (3) and (4) of Table 4 suggest a similar pattern. Again, the coefficients on pre-deregulation indicators, $Before^{5+}$ and $Before^{1,4}$, are insignificant whereas those on post-deregulation indicators, *After*^{1,4} and *After*$^{5+}$, are significantly negative.

[Insert Table 4 about here]

Following Beck et al. (2010), we examine the dynamics impact of branch deregulation on stock price crash risk and present the graphical evidence in Figure 1. We replace the branch dummy variable in Eq. (4) with a series of dummy variables corresponding to pre-treatment leads (up to 4 years) and post-treatment lags (up to 8 years) to trace out the year-by-year effects of intrastate deregulation on crash risk. In Figure 1, we plot the estimated coefficients and the 95% confidence intervals, adjusted for state-level clustering. As shown, the coefficients on the deregulation dummy variables are insignificant for all years before deregulation, suggesting no pre-treatment trends in crash risk. However, the mitigating impact of deregulation on crash risk emerges following the deregulation, evidenced by the declining pattern of the coefficients on the post-deregulation dummy variables. Overall, the results reported in Table 4 and Figure 1 suggest that there is little evidence of pre-treatment trends in firms' stock price crash risk. These results help validate the important assumption about parallel trends and mitigate the concern about reverse causality.

[Insert Figure 1 about here]

2. Placebo Analysis

Another source of endogeneity that might affect our identification strategy is the existence of potential omitted unobservable shocks that occur at approximately the same time as state-level bank branch deregulation events. To address this concern, we follow Cornaggia et al. (2015) and conduct placebo tests by requiring the deregulation events to happen in years other than the actual deregulatory years. Specifically, we randomly assign each state into a different deregulation year following the empirical distribution of years (see Table 1) while allowing the distribution of deregulatory years to be consistent with our baseline specification but disrupting the proper assignment of deregulation years to states. If unobservable shocks related to firms' crash risk exist and coincide with the deregulation events, they should potentially drive the baseline findings. Otherwise, the estimation results should be weakened by the random assignments of deregulatory years to states. Table 5 reports the results of our placebo tests. The coefficients on *BRANCH* are statistically insignificant across all columns, suggesting that there are no unobservable shocks coinciding with bank branch deregulation.

[Insert Table 5 about here]

Further, we follow Bradley, Kim, and Tian (2016) and iterate the regression in 1,000 times. In each iteration, we randomly assign states to deregulation years and then estimate the regression model Eq. (4) using $NCSKEW_{t+1}$ as crash risk measure. Figure 2 presents the histogram of the *t*-statistics obtained from those placebo regressions. To facilitate comparisons, we also include a vertical line that represents the actual *t*-statistics estimated in Column (3) of Table 3. The results indicate that when using randomly assigned (incorrect) state data, the treatment effect of bank branch deregulation on stock price crash risk is mostly insignificant. This finding strengthens the inference from our placebo test analysis, suggesting that the significantly negative coefficients on branch deregulation estimated in our baseline regressions are unlikely to be driven by some confounding events.

[Insert Figure 2 about here]

3. Firm Fixed Effects and Propensity Score Matching

Furthermore, we control for firm fixed effects to account for time-invariant unobserved firm-level heterogeneities and re-examine the baseline regression model. The results are reported in Table 6. The coefficients on $BRANCH_t$ continue to be significantly negative, which is consistent with our baseline findings.

[Insert Table 6 about here]

Next, to balance the observed covariate differences between the treatment group and the control group, we repeat our difference-in-differences estimation using a propensity-score-matched sample (DeFond, Hung, Li, and Li (2014)). As the majority of firm-year observations in our sample are post-deregulation (67.9%), we select pre-deregulation firms as the treatment group and post-deregulation firms as the control group (Dehejia and Wahba (2002)). Then, we perform an one-to-one matching, without replacement, to the nearest neighborhood, based on state, year, and all control variables specified in our baseline model (Eq. (4)). [4] We identify 6,533 pairs of pre- and post- deregulation firm-years and compare observable firm characteristics between the treatment and control groups. Panel A of Table 7 suggests that all the univariate difference test statistics are statistically insignificant, suggesting that the difference in crash risk between the treatment and control groups is only due to bank deregulation rather than other observable firm characteristics. Panel B reports the average treatment effect on the treated firms (ATT). The mean values of both crash risk measures, $NCSKEW_{t+1}$ and $DUVOL_{t+1}$, for treated group firms are significantly higher than those for control firms. In Panel C of Table 7, we re-estimate the specification model Eq. (4) using the propensity-score-matched sample. The results are in line with

[4] Alternatively, we match the treatment and control groups on some additional firm characteristics such as firm age, cash holdings, Altman's Z-score etc. The untabulated results are consistent with Table 7.

the above baseline findings that bank branch deregulation exerts a mitigating effect on stock price crash risk. Overall, the results of firm fixed effects and propensity score matching analysis lend further support to our main inference.

[Insert Table 7 about here]

4. Additional Control Variables at Firm and State Levels

To further alleviate the omitted variable bias, we control for a set of firm-level and state-level variables that may potentially affect future crash risk and meanwhile are related to bank deregulation. Specifically, we control for earnings volatility (*EARNVOL*) and capital expenditure (*CAPEX*) as proxies for firm riskiness. Managers in risky firms tend to withhold information about volatility to avoid being challenged by outside investors, thereby leading to high crash risk (Kim et al. 2011b). We also control for firm innovation due to some recent studies on the association between bank deregulation and innovation (Amore et al. (2013), Chava et al. (2013), Cornaggia et al. (2015), and Hombert and Matray (2016)). Following those papers, our measures of firm innovation are number of patents (LNPAT) and citation-weighted patent counts (*TCW*). The data are from Kogan, Papanikolaou, Seru, and Stoffman (2017).[5] Kogan et al. (2017) extract information on patents and citations from both National Bureau of Economic Research (NBER) and Google Patents database. The data is more comprehensive and has been increasingly explored by recent studies (e.g., Lyandres and Palazzo (2016), Mao and Zhang (2018), Chemmanur, Kong, Krishnan, and Yu (2019), and Gao and Zhang (2019)). The results are reported in Panel A of Table

[5] These data are available on Noah Stoffman's website: https://iu.app.box.com/v/patents. We thank the authors for sharing the data.

8. We find that the coefficients on $BRANCH_t$ remain significantly negative regardless of whether we add each single variable or all four variables to the main model.

Further, some studies have considered the implications of both intrastate and interstate bank branching deregulation; the latter allows banks to establish branches across states (Rice and Strahan (2010), Amore et al. (2013), and Chava et al. (2013)). We thus include an interstate branching deregulation indicator (*INTER*), which equals one after a state implemented interstate deregulation and otherwise zero. In Panel B of Table 8, Columns (1) and (2) show that our main results regarding the impact of intrastate deregulation continue to hold after controlling for interstate deregulation. However, the impact of interstate deregulation is insignificant, consistent with the aforementioned argument that this form of bank deregulation has little impact on bank efficiency (Calem (1994), Jayaratne and Strahan (1996)) and, hence, is less relevant for our analysis of bank monitoring and firms' stock price crash risk. To account for unobserved time-varying differences at state levels, we control for several local economic and political variables that might be correlated with the bank deregulation laws. Specifically, we incorporate the three state-level variables to Eq. (4), namely real GDP per capita, real GDP growth rate, and political balance which is defined as the ratio of the Democrat to Republican state representatives in the House of Representatives (e.g., Acharya, Baghai, and Subramanian (2014)). The results in Columns (3) to (8) show that the reducing effects of bank deregulation on crash risk are insensitive to the incorporation of these state-level variables. We further control for all those additional state-level variables in Columns (9) and (10) and find that the negative association between crash risk and bank deregulation still holds. Overall, our main findings are robust to including additional controls at firm and state levels to mitigate the concern on omitted correlated variables.[6]

[Insert Table 8 about here]

D. The Timing of State-level Branching Deregulation: Hazard Model

In this subsection, we investigate whether the timing of branching deregulation is exogenous to stock price crash risk. Kroszner and Strahan (1999) document a number of interest group factors that drive bank deregulation, demonstrating that the staggered state-level deregulation laws are not random cases. Our pre-treatment trend and falsification tests can plausibly circumvent the concern that the timing of deregulation is associated with changes in crash risk other than via the deregulation channel. Nevertheless, to further alleviate such endogeneity concern, we attempt to provide additional evidence that stock price crash risk is not related to the timing of deregulation.

Following Kroszner and Strahan (1999), we use a Weibull proportional hazards model to estimate the "duration of regulation" or the "time until deregulation". The hazard rate function takes the form:

$$h(t, X_t, \beta) = h_0(t)\exp[X_t^{\prime}\beta], \tag{6}$$

where X_t is a vector of time-varying covariates; β is a vector of unknown parameters to be estimated; and the baseline hazard rate, $h_0(t)$, is pt^{p-1} with shape parameter p that will be estimated from the data. Following Kroszner and Strahan (1999) and Beck et al. (2010), we exclude states that deregulated before 1970 and observe each state in each year up to and including the year of

[6] Following Hombert and Matray (2016), we also incorporate state-level innovation measures as additional variables. The untabulated results still support our main findings.

deregulation, yielding a total of 604 observations. Table 9 reports the coefficients β^* scaled by Weibull shape parameter. The β^* coefficients indicate the percentage change in the time to deregulation for a one-unit change in the covariates.

In Column (1) of Table 9, we follow Kroszner and Strahan (1999) and incorporate the main explanatory variables of deregulation. Consistent with their estimation, states deregulate later when small banks have greater share or relatively high capital-to-assets ratio, whereas a larger small firm share leads to earlier adoption of deregulation. In Column (2), we include two state-level factors used in Table 7: state GDP and its growth. The insignificant coefficients on both variables suggest that state economic status may not affect deregulation. In the last four columns, we test whether the timing of deregulation is related to stock price crash risk. We define state-level crash risk as the average values of the two crash risk measures, respectively. Columns (3) and (4) include only explanatory variable while Columns (5) and (6) include both state-level macro variables and main variables used by Kroszner and Strahan (1999). The coefficients on both crash risk variables are insignificant across the last four columns, whereas Kroszner-Strahan variables still have significant predictive power. Thus, the results of our duration model suggest that the timing of deregulation does not vary with stock price crash risk.

[Insert Table 9 about here]

E. Robustness Tests

1. Alternative Sample Periods

Next, we examine whether our baseline findings remain robust to alternative samples. To deal with survivorship bias, we repeat the estimation of Eq. (4) for firms that exist both before and after the deregulatory event years.. The results for such a restricted sample, as reported in Columns

(1) and (2) of Table 10, show significantly negative associations between bank deregulation and stock price crash risk, consistent with the baseline findings. Next, we use two alternative sample periods. First, we end the sample in 1994 when the deregulation of branching restrictions was completed with the passage of the IBBEA. Cetorelli and Strahan (2006) argue that it becomes less plausible to view markets in banking as local after 1994, because of the completion of deregulation as well as the fact that new technologies have allowed banks to lend to borrowers not physically close to their banks. Second, following Hombert and Matray (2016), who also focus on intrastate branching deregulation, we restrict the sample to the period 1968 to 1998. The results for the alternative samples are presented in Columns (3) to (6) of Table 10. Our main finding is robust to those alternative samples.

[Insert Table 10 about here]

2. Robustness Tests: Alternative Measures

Following Hutton et al. (2009), Kim et al. (2011b), and Chang et al. (2017), we further measure future stock price crash risk as a dummy variable that equals one if a firm experiences more than one price crash week in a fiscal year, and otherwise zero (*CRASH*). Specifically, we define crash weeks in a given fiscal year as those during which a firm experiences firm-specific weekly returns 3.09 standard deviations below the mean weekly returns over the whole fiscal year, with 3.09 chosen to generate a frequency of 0.1% in the normal distribution. We present the logistic regression results in Columns (1) of Panel A Table 11. In line with our main findings, *BRANCH* is significantly and negatively related to *CRASH* variable.

We have thus far defined bank branch deregulation as a dummy variable. In this test, we follow Black and Strahan (2002) and Hombert and Matray (2016) and employ a continuous measure, an intrastate bank deregulation index (*DERINDEX*). As mentioned, starting in 1970, all states progressively lifted restrictions on branching within their borders. They generally relaxed restrictions on within-state bank expansion in three steps: first, permitting the formation of multibank holding companies; then, permitting branching by means of mergers and acquisitions (M&As) only; and finally, permitting unrestricted (*de novo*) branching, thereby allowing banks to enter markets by opening new branches. We define the deregulation index to be zero if a state did not permit branching via any of the three approaches; otherwise, the index equals the sum of the number of ways that banks may expand within a state. Hence, the value of the deregulation index (*DERINDEX*) ranges from zero (full regulation) to three (full deregulation). We regress each of three different crash risk measures, namely $CRASH_{t+1}$, $NCSKEW_{t+1}$, and $DUVOL_{t+1}$, on deregulation index and a set of control variables, and present the estimation results in Columns (2) to (4) of Panel A Table 11. The coefficients on deregulation index ($DERINDEX_t$) are significantly negative for all crash risk measures. Overall, we conclude that our main findings are robust to alternative measures of future stock price crash risk and intrastate branching deregulation.

The main analysis focuses on the impact of bank deregulation on one-year-ahead stock price crash risk. As a robustness test, we examine whether bank regulation has longer-term effect on borrowers' crash risk. To this end, we regress stock price crash measures in year t+2 and t+3 on bank regulation in year t. The results reported in Panel B Table 11 suggest that the mitigating effect of bank regulation on crash risk persist over a long time horizon.

[Insert Table 11 about here]

V. Additional Analyses

Thus far we have shown a robust negative effect of bank branch deregulation on stock price crash risk. In this section, we explore how the association between branch banking reform and stock price crash risk varies with external financial dependence and lending relationship dependence.

A. External Financial Dependence

To the extent that lifting intrastate branching restrictions significantly changed the structure of banking industry and bank monitoring efficiency (Jayaratne and Strahan (1996), (1998)), firms that are more dependent on external finance should experience more intensive monitoring as a result of the bank deregulation. Thus, we partition the whole sample into two groups based on industry-level external financial dependence and expect to observe a more pronounced impact of branch deregulation on crash risk for firms in dependent industries.

We employ three proxies to measure the degree of external financial dependence: the external finance dependence ratio (*EXDEP*), net change in capital (*NCC*), and bank loan ratio (*BANKLOAN*). Following Rajan and Zingales (1998), we define a firm's external finance dependence as the amount of desired investment that cannot be financed through internal sources. Accordingly, the external finance dependence ratio is calculated as investment plus R&D expenses and acquisitions minus operating income before depreciation, divided by investment. Following Amore et al. (2013) and Frank and Goyal (2003), we compute net change in capital as net change in equity and debt normalized by total assets. Bank loan ratio is the amount of cumulative bank loan scaled by the total assets. The syndicated bank loan data is from the Loan Pricing Corporation Dealscan database.[7] All the three measures reflect firms' demand for external finance and sensitivity to credit supply shock. Then, we define industry-level external finance dependence as

[7] Loan Pricing Corporation Dealscan database contains comprehensive historical information on loan pricing and contracts details. However, the data is not comprehensively available before 1988. As such the number of observations in Columns (5) and (6) is much smaller than the whole sample.

the average value of each proxy at the three-digit SIC level and construct three indicator variables to proxy for highly dependent industries, namely those with above-median industry-level external finance dependence ratio, net change in capital, and bank loan ratio.

Table 12 shows that the interaction terms of bank deregulation and external financial dependence variables are significantly negative. These results suggest that the negative effect of bank deregulation on crash risk is stronger for firms in industries with greater dependence on external finance. In other words, external financial dependence appears to be an important mechanism through which branch deregulation affects firms' stock price crash risk.

[Insert Table 12 about here]

B. Lending Relationship Dependence

To further test the efficiency view versus relationship view, we investigate how borrowers' lending relationship dependence could affect the association between bank deregulation and crash risk. Bank branch deregulation did not only give rise to the consolidation of banking industry, but also implicitly changed the banking organization type from relationship-oriented to arm's length-oriented. The borrower-bank tie is valuable for both parties as it increases availability of credit for borrowers and the precision of the lender's private soft information about the borrower (Petersen and Rajan (1994)). If the efficiency view dominates relationship view, bank monitoring after deregulation should be more efficient than that before deregulation. Therefore, we expect a more pronounced effect of branch deregulation on crash risk for firms that depend mainly on lending relationship.

Following Hombert and Matray (2016), we obtain data from the National Survey of Small Business Finances (1987 and 1998) and employ three industry-level proxies of lending relationship dependence, namely the average distance between firms and their main lenders in 1987 at the two-digit SIC level, the average increase in distance between banks and borrowers between 1987 and 1998, and the average length of the relationship between banks and borrowers in 1987. A greater (increase in) distance between the banks and the borrowers indicates that their interaction is becoming more impersonal and dominated more by hard information (Petersen and Rajan (2002)). We classify an industry as being more dependent on lending relationships if the average (increase in) distance between firms and their main lenders is below the sample median or if the average length of the relationship is above the sample median.

In Table 13, we regress each crash risk measure on bank branch deregulation variable (*BRANCH*) and its interaction with each of the three indicator measures of lending relationship dependence (*AVDIS*, *GROWDIS*, and *AVLENGTH*), while controlling for a set of controls and fixed effects. The results show that the coefficients on the interaction terms are significantly negative, suggesting a stronger effect of deregulation in more relationship-dependent industries. Consistent with our expectation, firms depending more on lending relationships are more sensitive to the enhanced bank monitoring following intrastate bank deregulation. As a consequence, post-deregulation banks can better constrain those borrowing firms from withholding negative information, lowering their stock price crash risk.

[Insert Table 13 about here]

VI. Conclusion

In this study, we use the staggered passage of state-level intrastate branching deregulation laws as a quasi-natural experiment to investigate the impact of bank deregulation on stock price crash risk. We test two competing views, namely efficiency versus relationship. The efficiency view argues that intrastate bank deregulation encouraged the consolidation activities which eliminated inefficient small banks and improved the overall monitoring efficiency in the banking industry. Bank deregulation allowed banks to monitor borrowers at lower costs, leading to less bad-news-hoarding behavior and lower future stock price crash risk. The relationship view suggests that the arm's length lending after branch reforms damaged lending relationships and impeded banks' ability to process private soft information about borrowing firms. Thus, managers in borrowing firms are likely to hide negative information without being revealed by banks, leading to higher crash risk after deregulation.

Our empirical evidence shows that lifting the restrictions on intrastate bank branching leads to a lower level of firm-specific stock price crash risk. This finding remains robust after addressing potential endogeneity concerns about reverse causality and omitted variable bias. Our empirical results are also robust to the use of alternative samples and various measures of key variables. Overall, those results support the argument that after branching reform banks are able to monitor their borrowers more effectively and prevent them from withholding bad news. We further analyze the role of external financial dependence and lending relationships in the relation between bank deregulation and crash risk. The results show that the negative relation between branching reform and crash risk is more pronounced for firms that are more reliant on external finance and lending relationships.

Overall, our study contributes to research on bank deregulation and stock price crash risk. The financial economics literature provides robust evidence that the liberalization of the banking industry is beneficial to economic growth (Levine, 2005). Our paper complements the literature by documenting new evidence that intrastate bank deregulation reduces firms' stock price crash risk. Our finding extends the view of Jayaratne and Strahan (1996) that the key to the beneficial growth effects of bank branch reform is the improvement in lending quality. We show that firms can benefit from deregulatory policies in the banking industry through the improved protection of shareholders' wealth.

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Figure 1. The Impact of Banking Deregulation on Stock Price Crash Risk

The figure shows the dynamic impact of branch deregulation on stock price crash risk. Crash risk is measured as negative conditional skewness (*NCSKEW*). We estimate the following specification including leading and lagged indicators of banking deregulation:

$$Crash\ Risk_{j,t+1} = \alpha + \beta_1 D_{i,t-4} + \beta_2 D_{i,t-3} + \cdots + \beta_{12} D_{i,t+8} + Controls + Year_t + State_i + \varepsilon_{j,t},$$

where $D_{i,t}$ is a dummy variable set to one if state is deregulated in year t and zero otherwise. $D_{i,t-4}$ is set to one for years up to four years prior to bank deregulation and zero otherwise. $D_{i,t+8}$ is set to one for all years eight years after bank deregulation and zero otherwise. The connected points indicate the estimated coefficients. The dashed lines represent 95% confidence intervals, adjusted for state-level clustering.

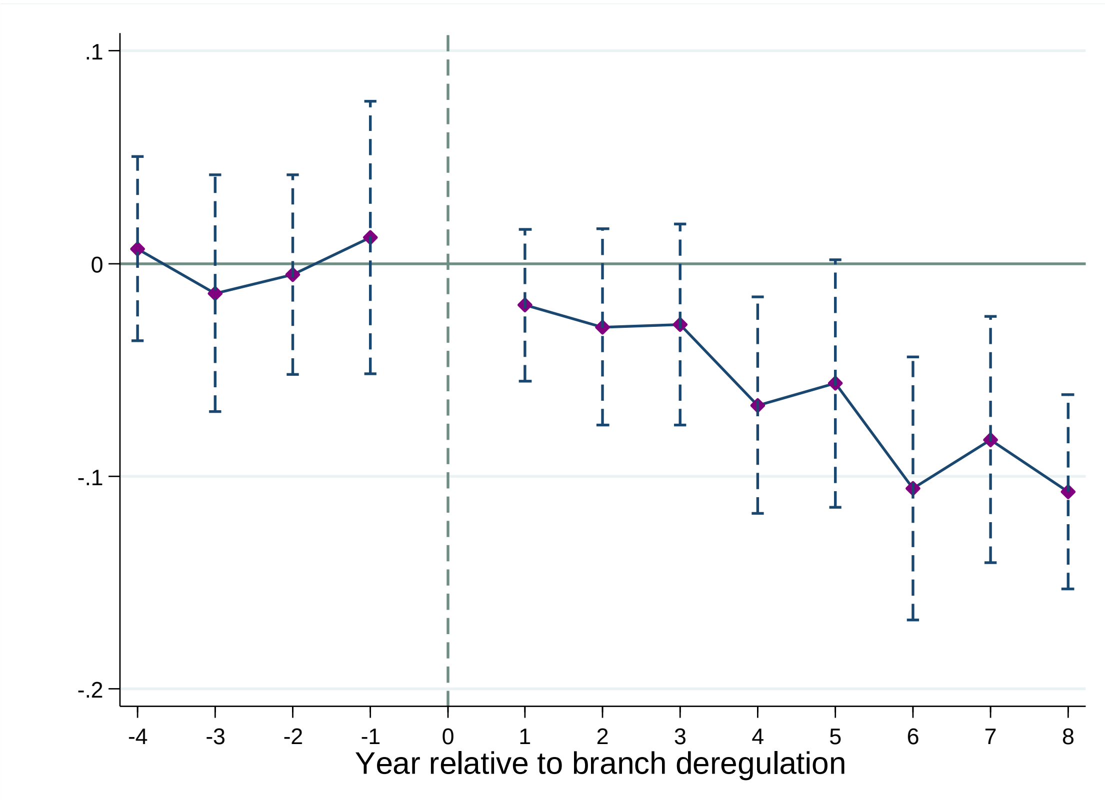

Figure 2. Histogram of T-statistics in Placebo Tests

This figure plots the histogram of the distribution of the *t*-statistics of the coefficient on branch deregulation variable (*BRANCH*) from 1,000 placebo tests. In each iteration we randomly assign states into deregulation years in Table 1 without replacement, while keeping the distribution of deregulatory years consistent. The regression model is based on Eq. (4) while the dependent variable crash risk is measured as *NCSKEW*. The dashed vertical line represents the true *t*-statistic from regressing crash risk on the branch deregulation variable and the controls.

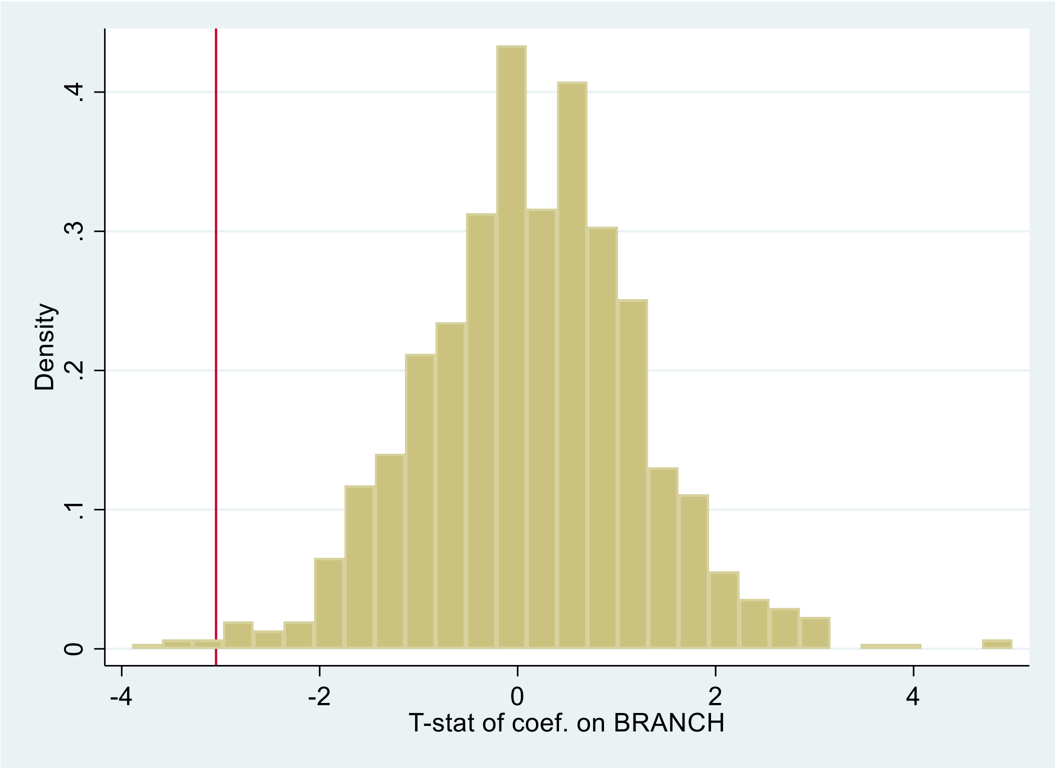

Table 1. Year of State-level Branch Deregulation

This table reports the year of bank branch deregulation in each state. Source: Strahan (2003) and Beck et al. (2010).

State	Year of deregulation	State	Year of deregulation
Alabama	1981	Montana	1990
Alaska	1960	Nebraska	1985
Arizona	1960	Nevada	1960
Arkansas	1994	New Hampshire	1987
California	1960	New Jersey	1977
Colorado	1991	New Mexico	1991
Connecticut	1980	New York	1976
Delaware	1960	North Carolina	1960
District of Columbia	1960	North Dakota	1987
Florida	1988	Ohio	1979
Georgia	1983	Oklahoma	1988
Hawaii	1986	Oregon	1985
Idaho	1960	Pennsylvania	1982
Illinois	1988	Rhode Island	1960
Indiana	1989	South Carolina	1960
Iowa	1999	South Dakota	1960
Kansas	1987	Tennessee	1985
Kentucky	1990	Texas	1988
Louisiana	1988	Utah	1981
Maine	1975	Vermont	1970
Maryland	1960	Virginia	1978
Massachusetts	1984	Washington	1985
Michigan	1987	West Virginia	1987
Minnesota	1993	Wisconsin	1990
Mississippi	1986	Wyoming	1988
Missouri	1990		

Table 2. Descriptive Statistics

This table reports the descriptive statistics for variables used in the baseline empirical analyses. The sample consists of 79,231 firm-years observations for 8,512 public U.S. firms over the period 1962-2001. All variables are winsorized at the 1% and 99% levels. Variable definitions are listed in Appendix A.

Variable	N	Mean	Std. Dev.	25^{th}	Median	75^{th}
Main dependent variables						
$NCSKEW_{t+1}$	79,231	-0.200	0.730	-0.583	-0.197	0.170
$DUVOL_{t+1}$	79,231	-0.118	0.356	-0.348	-0.124	0.101
Alternative dependent variable						
$CRASH_{t+1}$	79,231	0.112	0.315	0.000	0.000	0.000
Main independent variable						
$BRANCH_{t}$	79,231	0.679	0.467	0.000	1.000	1.000
Alternative dependent variable						
Deregulation $index_{t}$	79,231	2.088	1.126	1.000	3.000	3.000
Control variables						
$DTURN_{t}$	79,231	0.012	0.765	-0.113	0.000	0.110
$SIGMA_{t}$	79,231	0.072	0.041	0.042	0.063	0.091
RET_{t}	79,231	-0.339	0.505	-0.410	-0.197	-0.087
$SIZE_{t}$	79,231	4.779	1.949	3.319	4.642	6.137
MB_{t}	79,231	2.293	2.753	0.915	1.504	2.595
LEV_{t}	79,231	0.246	0.187	0.092	0.234	0.364
ROA_{t}	79,231	0.020	0.135	0.011	0.045	0.078
$NCSKEW_{t}$	79,231	-0.207	0.711	-0.588	-0.207	0.158
$ACCM_{t}$	79,231	0.068	0.079	0.018	0.043	0.087

Table 3. Impact of Bank Deregulation on Stock Price Crash Risk

This table presents the regression results of the effect of bank branch deregulation on firm-level stock price crash risk. The dependent variable crash risk is proxied by negative conditional skewness (*NCSKEW*) and down-to-up volatility (*DUVOL*) in year *t*+1. Bank branch deregulation (*BRANCH*) is measured as an indicator variable that equals one after a state implemented intrastate branching deregulation and zero otherwise. The years each state relaxed the restrictions on intrastate branching are shown in Table 1. See Appendix A for other variable definitions. All models include state and year fixed effects. The numbers reported in parentheses are *t*-statistics based on standard errors clustered at the state level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	$NCSKEW_{t+1}$	$DUVOL_{t+1}$	$NCSKEW_{t+1}$	$DUVOL_{t+1}$
$BRANCH_t$	-0.031***	-0.016***	-0.028***	-0.015***
	(-3.05)	(-3.24)	(-3.05)	(-3.34)
$DTURN_t$			0.011***	0.006***
			(5.54)	(5.87)
$SIGMA_t$			0.483*	-0.028
			(1.99)	(-0.25)
RET_t			0.030	0.003
			(1.55)	(0.39)
$SIZE_t$			0.071***	0.033***
			(28.37)	(26.77)
MB_t			0.008***	0.004***
			(6.72)	(6.45)
LEV_t			-0.040**	-0.024***
			(-2.31)	(-3.04)
ROA_t			0.308***	0.164***
			(17.54)	(23.84)
$NCSKEW_t$			0.039***	0.019***
			(9.22)	(9.82)
$ACCM_t$			0.136***	0.060***
			(3.24)	(3.08)
Constant	0.049	0.001	-0.367***	-0.189***
	(0.48)	(0.02)	(-3.21)	(-2.94)
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
No. of obs.	79,231	79,231	79,231	79,231
Adj. R^2	0.027	0.033	0.073	0.080

Table 4. Endogeneity Tests: Pre-treatment Trend Analysis

This table presents the estimation results of the pre-treatment trend analysis using a dynamic specification. We replace bank deregulation indicator by a set of time indicators in our baseline model (Eq. (4)). In Columns (1) and (2), $Before^{2+}$ is an indicator variable that takes one for observations with two years or more prior to deregulation and zero otherwise. $Before^{1}$ is an indicator variable that takes one for observations with one year prior to deregulation and zero otherwise. $After^{1}$ is an indicator variable that takes one for observations with one year post-deregulation and zero otherwise. $After^{2+}$ is an indicator variable that takes one for observations with two years or more post-deregulation and zero otherwise. In Columns (3) and (4), $Before^{5+}$ is an indicator variable that takes one for all years up to and including five years prior to deregulation. $Before^{1,4}$ is an indicator variable that takes one for the four years preceding deregulation. $After^{1,4}$ is an indicator variable that takes one for the four years following deregulation. $After^{5+}$ is an indicator variable that takes one for all years five years after deregulation. See Appendix A for variable definitions. All models include state and year fixed effects. The numbers reported in parentheses are *t*-statistics based on standard errors clustered at the state level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	$NCSKEW_{t+1}$	$DUVOL_{t+1}$	$NCSKEW_{t+1}$	$DUVOL_{t+1}$
$Before^{2+}$	-0.017	-0.006		
	(-0.80)	(-0.63)		
$Before^{1}$	-0.017	-0.006		
	(-0.63)	(-0.46)		
$After^{1}$	-0.037**	-0.017**		
	(-2.37)	(-2.09)		
$After^{2+}$	-0.047**	-0.025**		
	(-2.09)	(-2.16)		
$Before^{5+}$			-0.020	-0.008
			(-0.91)	(-0.75)
$Before^{1,4}$			-0.002	0.000
			(-0.06)	(0.01)
$After^{1,4}$			-0.038**	-0.017*
			(-2.03)	(-1.74)
$After^{5+}$			-0.043**	-0.022**
			(-2.21)	(-2.24)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
No. of obs.	79,231	79,231	79,231	79,231
Adj. R^2	0.138	0.171	0.138	0.171

Table 5. Falsification Test: Randomization of Bank Deregulation

This table presents the falsification test results of Eq. (4) with randomized state deregulations. We assume the deregulatory events do not occur in the actual deregulation years shown in Table 1 and define a new deregulation variable (*BRANCH*) as an indicator that equals one after a state implemented randomly assigned intrastate branching deregulation and zero otherwise. See Appendix A for variable definitions. All models include state and year fixed effects. The numbers reported in parentheses are *t*-statistics based on standard errors clustered at the state level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)
	$NCSKEW_{t+1}$	$DUVOL_{t+1}$
$BRANCH_t$	-0.001	0.001
	(-0.05)	(0.14)
$DTURN_t$	0.011***	0.006***
	(5.63)	(5.96)
$SIGMA_t$	0.491*	-0.024
	(2.00)	(-0.21)
RET_t	0.030	0.003
	(1.55)	(0.40)
$SIZE_t$	0.071***	0.033***
	(28.34)	(26.65)
MB_t	0.008***	0.004***
	(6.72)	(6.45)
LEV_t	-0.042**	-0.025***
	(-2.32)	(-3.04)
ROA_t	0.307***	0.163***
	(17.39)	(23.75)
$NCSKEW_t$	0.040***	0.020***
	(9.30)	(9.90)
$ACCM_t$	0.135***	0.060***
	(3.24)	(3.08)
Constant	-0.373***	-0.192***
	(-3.22)	(-2.96)
Year FE	Yes	Yes
State FE	Yes	Yes
No. of obs.	79,231	79,231
Adj. R^2	0.072	0.080

Table 6. Impact of Bank Deregulation on Stock Price Crash Risk: Firm Fixed Effects

This table presents the firm fixed-effect regression results of the impact of bank branch deregulation on stock price crash risk. See Appendix A for variable definitions. All models include firm and year fixed effects. The numbers reported in parentheses are *t*-statistics based on standard errors clustered at the state level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) $NCSKEW_{t+1}$	(2) $DUVOL_{t+1}$	(3) $NCSKEW_{t+1}$	(4) $DUVOL_{t+1}$
$BRANCH_t$	-0.031***	-0.016***	-0.027**	-0.014**
	(-2.82)	(-2.95)	(-2.25)	(-2.36)
$DTURN_t$			0.009***	0.004***
			(3.92)	(3.84)
$SIGMA_t$			-0.412*	-0.299***
			(-1.97)	(-3.09)
RET_t			-0.010	-0.009
			(-0.79)	(-1.57)
$SIZE_t$			0.174***	0.087***
			(29.13)	(30.90)
MB_t			0.006***	0.003***
			(4.27)	(4.73)
LEV_t			0.076**	0.029**
			(2.65)	(2.11)
ROA_t			0.189***	0.093***
			(7.79)	(7.63)
$NCSKEW_t$			-0.077***	-0.034***
			(-15.23)	(-13.52)
$ACCM_t$			0.004	0.002
			(0.11)	(0.09)
Constant	-0.091	-0.069	-0.679***	-0.358***
	(-0.81)	(-1.10)	(-5.79)	(-5.54)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
No. of obs.	79,231	79,231	79,231	79,231
Adj. R^2	0.018	0.024	0.053	0.060

Table 7. Propensity Score Matching Analysis

This table reports the results of the one-to-one propensity score matching (PSM). We match control and treatment firms on all control variables in our baseline model, industry, state, and year, using a caliper of 0.5% and without replacement. The treatment group consists of firms headquartered in states that were subject to branching restrictions. The control group consists of firms headquartered in states that implemented branching deregulation. Panel A reports presents diagnostic statistics for the difference in firm characteristics between treatment and control groups. Panel B reports the average treatment effects. Panel C reports the regression results based on the propensity-score-matched sample. See Appendix A for other variable definitions. All models in Panel C include state and year fixed effects. The numbers reported in parentheses are *t*-statistics based on standard errors clustered at the state level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Diagnostics stat-difference in means of variables

	Treatment group		Control group		
Variables	N	Mean	N	Mean	T-STAT
DTURN	6,533	0.0225	6,533	0.0109	1.21
SIGMA	6,533	0.0719	6,533	0.0708	1.39
RET	6,533	-0.3459	6,533	-0.3427	-0.36
SIZE	6,533	4.8588	6,533	4.8398	0.55
MB	6,533	1.7395	6,533	1.7331	0.19
LEV	6,533	0.2514	6,533	0.2496	0.63
ROA	6,533	0.0418	6,533	0.0408	0.66
NCSKEW	6,533	-0.2525	6,533	-0.2345	-1.53
ACCM	6,533	0.0585	6,533	0.0594	-0.74

Panel B. Average treatment effects

	Treatment group (Pre-deregulation)	Control group (Post-deregulation)	Difference	T-STAT
$NCSKEW_{t+1}$	-0.236	-0.272	0.036***	2.92
$DUVOL_{t+1}$	-0.135	-0.154	0.019***	3.15

Panel C. Regression with the propensity-score-matched samples

	(1)	(2)
	$NCSKEW_{t+1}$	$DUVOL_{t+1}$
$BRANCH_t$	-0.034*	-0.021**
	(-1.85)	(-2.23)
Controls	Yes	Yes
Year FE	Yes	Yes
State FE	Yes	Yes
No. of obs.	13,066	13,066
Adj. R^2	0.081	0.086

Table 8. Regression Analysis with Additional Controls.

This table presents the regression results with additional controls at firm and state levels. In Panel A, we include earnings volatility (*EARNVOL*), capital expenditure (*CAPEX*), number of patents (*LNPAT*), and citation-weighted patent counts (*TCW*). In Panel B, we include interstate deregulation variable (*INTER*), U.S. GDP growth, GDP per capita, and political balance as additional control variables. See Appendix A for variable definitions. To economize on space, all the control variables (see Table 3) are suppressed. All models include state and year fixed effects. The numbers reported in parentheses are *t*-statistics based on standard errors clustered at the state level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Additional firm-level controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	$NCSKEW_{t+1}$	$DUVOL_{t+1}$	$NCSKEW_{t+1}$	$DUVOL_{t+1}$	$NCSKEW_{t+1}$	$DUVOL_{t+1}$	$NCSKEW_{t+1}$	$DUVOL_{t+1}$	$NCSKEW_{t+1}$	$DUVOL_{t+1}$
$BRANCH_t$	-0.029***	-0.016***	-0.027***	-0.015***	-0.025*	-0.013**	-0.024*	-0.013*	-0.026*	-0.014**
	(-3.03)	(-3.22)	(-2.95)	(-3.30)	(-1.93)	(-2.06)	(-1.84)	(-1.99)	(-1.94)	(-2.10)
EARNVOL	0.000**	0.000**							-0.001*	-0.000*
	(2.34)	(2.57)							(-1.98)	(-1.86)
CAPEX			0.169***	0.099***					0.130	0.090**
			(4.31)	(4.99)					(1.52)	(2.16)
LNPAT					-0.017***	-0.006**			-0.017***	-0.007**
					(-3.63)	(-2.62)			(-3.32)	(-2.66)
TCW							-0.007	0.001	-0.001	0.004
							(-0.71)	(0.33)	(-0.17)	(1.14)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	76,730	76,730	78,314	78,314	25,879	25,879	25,827	25,827	24,763	24,763
Adj. R^2	0.071	0.078	0.072	0.080	0.082	0.092	0.082	0.092	0.079	0.089

Panel B. Additional state-level controls										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	$NCSKEW_{t+1}$	$DUVOL_{t+1}$	$NCSKEW_{t+1}$	$DUVOL_{t+1}$	$NCSKEW_{t+1}$	$DUVOL_{t+1}$	$NCSKEW_{t+1}$	$DUVOL_{t+1}$	$NCSKEW_{t+1}$	$DUVOL_{t+1}$
$BRANCH_t$	-0.030***	-0.016***	-0.028***	-0.015***	-0.040*	-0.020*	-0.027***	-0.014***	-0.040**	-0.020*
	(-3.18)	(-3.40)	(-2.85)	(-3.09)	(-1.93)	(-1.83)	(-3.05)	(-3.12)	(-2.10)	(-1.95)
$INTER_t$	0.019	0.010							-0.015	-0.010
	(1.20)	(1.11)							(-0.55)	(-0.74)
$GDPGROWTH_t$			0.001	0.002					0.309	0.174*
			(0.01)	(0.04)					(1.67)	(1.92)
$GDPPERCAP_t$					-0.000	-0.000			-0.000	-0.000
					(-0.82)	(-0.64)			(-0.46)	(-0.40)
$POLBALANCE_t$							-0.066***	-0.030***	-0.098***	-0.046***
							(-3.67)	(-3.75)	(-2.74)	(-2.97)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	79,231	79,231	79,044	79,044	45,331	45,331	74,199	74,199	44,850	44,850
Adj. R^2	0.073	0.080	0.073	0.080	0.056	0.061	0.071	0.078	0.057	0.061

Table 9. Timing of Bank Deregulation and Stock Price Crash Risk: The Duration Model

This table reports results from a Weibull proportional hazard model where the "failure event" is the adoption of intrastate branching deregulation in a given U.S. state. The dependent variable is the log of expected time to intrastate branching deregulation. The sample period is 1970 to 1994 and the sample comprises 39 states that deregulated after 1970. All the explanatory variables are included in state levels. States are dropped from the sample once they deregulate. In Column (1) we include the following determinants from Kroszner and Strahan (1999): (1) small bank share of all banking assets, (2) capital ratio of small banks relative to large, (3) relative size of insurance, (4) an indicator that takes a value of one if banks may sell insurance, (5) small firm share, (6) share of state government controlled by Democrats, (7) an indicator that takes a value of one if the state is controlled by one party, (8) an indicator that takes a value of one if the state has unit banking laws, and (9) an indicator that takes a value of one if the state changes bank insurance powers. In Column (2) we include state GDP and its growth as potential state-level determinants of crash risk. The last four columns include state-level NCSKEW and DUVOL as predictors. See Appendix A for variable definitions. The numbers reported in parentheses are *t*-statistics based on standard errors clustered at the state level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
NCSKEW			-0.037		-0.087	
			(-0.24)		(-0.90)	
DUVOL				-0.158		-0.215
				(-0.49)		(-0.91)
GDP		-0.509			0.097	0.110
		(-0.56)			(0.15)	(0.17)
GDP Growth		-0.037			0.029	0.028
		(-0.77)			(0.58)	(0.56)
Small bank asset share	4.465***				4.536***	4.545***
	(3.48)				(5.76)	(5.78)
Capital ratio of small banks relative to large	10.016**				7.333*	7.331*
	(2.03)				(1.80)	(1.78)
Relative size of insurance	-0.682				-0.111	-0.129
	(-1.35)				(-0.26)	(-0.30)
Banks selling insurance indicator	0.187*				-0.044	-0.043
	(1.79)				(-0.42)	(-0.41)
Small firm share	-9.653**				-14.733***	-14.659***
	(-2.46)				(-4.78)	(-4.78)
Share of Democrats	0.215*				0.105	0.109
	(1.68)				(0.82)	(0.85)
state controlled by one party indicator	-0.036				0.165**	0.164**
	(-0.46)				(2.11)	(2.11)
Unit banking indicator	0.219**				0.327***	0.329***
	(2.04)				(3.71)	(3.82)
Change in bank insurance power indicator	-0.090				-0.263*	-0.263*
	(-0.65)				(-1.82)	(-1.83)
Regional indicators	No	No	No	No	Yes	Yes
No. of obs.	604	604	604	604	604	604
Adj. R^2	0.000	0.623	0.808	0.621	0.000	0.000

Table 10. Robustness Checks: Alternative Samples

This table presents the regression results of robustness tests. In Columns (1) and (2), we restrict the sample to firms that experienced intrastate bank deregulation events during their lifetime. We test alternative sample periods from 1963 to 1994 in Columns (3) and (4), and from 1968 to 1998 in Columns (5) and (6). To economize on space, all the control variables (see Table 3) are suppressed. See Appendix A for other variable definitions. All models include state and year fixed effects. The numbers reported in parentheses are *t*-statistics based on standard errors clustered at the state level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	Firms that experienced intrastate bank deregulation during lifetime		Alternative sample period			
			1963-1994		1968-1998	
	(1)	(2)	(3)	(4)	(5)	(6)
	$NCSKEW_{t+1}$	$DUVOL_{t+1}$	$NCSKEW_{t+1}$	$DUVOL_{t+1}$	$NCSKEW_{t+1}$	$DUVOL_{t+1}$
$BRANCH_t$	-0.043***	-0.021***	-0.033***	-0.017***	-0.028***	-0.014***
	(-3.59)	(-3.37)	(-3.18)	(-3.52)	(-2.92)	(-3.05)
$DTURN_t$	0.007**	0.004**	0.015***	0.007***	0.013***	0.007***
	(2.19)	(2.47)	(3.09)	(2.97)	(4.86)	(5.28)
$SIGMA_t$	0.335	-0.109	0.012	-0.305**	0.288	-0.171
	(1.17)	(-0.75)	(0.04)	(-2.09)	(1.02)	(-1.38)
RET_t	0.023	-0.000	0.002	-0.013	0.019	-0.004
	(1.02)	(-0.04)	(0.08)	(-1.45)	(0.91)	(-0.44)
$SIZE_t$	0.069***	0.033***	0.068***	0.032***	0.068***	0.032***
	(24.28)	(22.35)	(22.32)	(20.51)	(21.82)	(20.12)
MB_t	0.010***	0.004***	0.009***	0.004***	0.008***	0.004***
	(7.25)	(6.22)	(6.18)	(6.74)	(6.25)	(5.71)
LEV_t	0.030	0.003	-0.002	-0.009	-0.031*	-0.020**
	(0.97)	(0.18)	(-0.10)	(-0.86)	(-1.89)	(-2.54)
ROA_t	0.451***	0.219***	0.351***	0.181***	0.342***	0.175***
	(7.60)	(7.90)	(11.94)	(14.84)	(17.05)	(20.71)
$NCSKEW_t$	0.041***	0.021***	0.046***	0.023***	0.043***	0.021***
	(6.32)	(7.08)	(6.64)	(7.19)	(8.94)	(9.16)
$ACCM_t$	0.175***	0.092***	0.118**	0.051**	0.146***	0.063***
	(2.78)	(2.92)	(2.61)	(2.26)	(3.50)	(3.17)
Constant	-0.346**	-0.186*	-0.330***	-0.173**	-0.565***	-0.271***
	(-2.17)	(-1.93)	(-2.77)	(-2.62)	(-15.70)	(-14.67)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	36,530	36,530	56,051	56,051	67,966	67,966
Adj. R^2	0.080	0.087	0.067	0.074	0.062	0.067

Table 11. Robustness Tests: Alternative Measures and Longer Forecast Windows

This table presents the regression results of robustness tests using alterative measures of crash risk and bank branch deregulation. Columns (1) and (2) of Panel A presents the logit regression results in which crash risk is proxied by an indicator variable (*CRASH*) that takes one if a firm experiences more than one price crash week in a fiscal year. In Columns (2) to (4) of Panel A, we follow Hombert and Matray (2016) and compute a bank branch deregulation index (*DERINDEX*) which equals zero if a state does not permit branching via M&As, *de novo* branching, or the formation of multibank holding companies; otherwise, the index equals the sum of the number of ways that banks may expand within a state. Panel B presents the results of the impact of deregulation on stock price crash risk over two- and three-year horizon. See Appendix A for other variable definitions. All models include state and year fixed effects. The numbers reported in parentheses are t-statistics (z-statistics) based on standard errors clustered at the state level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Alternative measures of crash risk and branch deregulation

	(1)	(2)	(3)	(4)
	$CRASH_{t+1}$	$CRASH_{t+1}$	$NCSKEW_{t+1}$	$DUVOL_{t+1}$
$BRANCH_t$	-0.114**			
	(-2.23)			
$DERINDEX_t$		-0.041**	-0.012***	-0.006**
		(-2.16)	(-2.70)	(-2.66)
$DTURN_t$	0.032***	0.032***	0.011***	0.006***
	(2.68)	(2.70)	(5.61)	(5.92)
$SIGMA_t$	-3.438***	-3.395***	0.492**	-0.024
	(-3.44)	(-3.42)	(2.04)	(-0.21)
RET_t	-0.100	-0.098	0.030	0.003
	(-1.28)	(-1.25)	(1.57)	(0.41)
$SIZE_t$	0.045***	0.045***	0.071***	0.033***
	(3.84)	(3.85)	(28.39)	(26.81)
MB_t	0.020***	0.020***	0.008***	0.004***
	(5.85)	(5.88)	(6.71)	(6.42)
LEV_t	-0.108**	-0.105**	-0.040**	-0.024***
	(-2.41)	(-2.35)	(-2.26)	(-2.98)
ROA_t	0.729***	0.727***	0.308***	0.163***
	(8.85)	(8.84)	(17.28)	(23.68)
$NCSKEW_t$	0.085***	0.084***	0.039***	0.019***
	(4.90)	(4.91)	(9.27)	(9.87)
$ACCM_t$	0.946***	0.941***	0.134***	0.060***
	(7.07)	(7.11)	(3.22)	(3.07)
Constant	-2.060***	-2.085***	-0.480***	-0.271***
	(-2.60)	(-2.63)	(-18.99)	(-21.91)
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
No. of obs.	79,228	79,153	79,156	79,156
Adj./Pseudo R^2	0.029	0.029	0.073	0.080

Panel B. Impact of deregulation during two-year and three-year windows				
	(1)	(2)	(3)	(4)
	$NCSKEW_{t+2}$	$DUVOL_{t+2}$	$NCSKEW_{t+3}$	$DUVOL_{t+3}$
$BRANCH_t$	-0.026**	-0.014***	-0.023**	-0.014***
	(-2.29)	(-2.70)	(-2.15)	(-2.77)
$DTURN_t$	-0.000	0.000	0.003	0.001
	(-0.09)	(0.14)	(1.01)	(0.83)
$SIGMA_t$	-0.045	-0.257**	0.202	-0.109
	(-0.20)	(-2.51)	(1.18)	(-1.19)
RET_t	0.005	-0.005	0.004	-0.004
	(0.35)	(-0.84)	(0.29)	(-0.61)
$SIZE_t$	0.066***	0.031***	0.064***	0.030***
	(24.79)	(24.46)	(27.03)	(25.79)
MB_t	0.004***	0.002***	0.002	0.001
	(3.72)	(3.70)	(1.67)	(1.30)
LEV_t	-0.059**	-0.034***	-0.055**	-0.026**
	(-2.55)	(-3.11)	(-2.30)	(-2.44)
ROA_t	0.241***	0.130***	0.185***	0.107***
	(5.91)	(7.46)	(4.89)	(7.48)
$NCSKEW_t$	0.026***	0.014***	0.019***	0.009***
	(6.09)	(6.91)	(3.87)	(3.87)
$ACCM_t$	0.174***	0.068***	0.030	0.013
	(3.95)	(3.09)	(0.76)	(0.66)
Constant	-0.474***	-0.262***	-0.668***	-0.343***
	(-3.61)	(-3.45)	(-4.39)	(-5.28)
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
No. of obs.	69,945	69,945	62,110	62,110
Adj. R^2	0.064	0.071	0.058	0.065

Table 12. The Role of External Financial Dependence

This table presents the results conditional on external financial dependence. Industry-level external financial dependence is proxied by three measurements, namely external finance dependence ratio, financial leverage, and net change in capital. In Columns (1) and (2), we follow Rajan and Zingales (1998) and compute the firm-level external finance dependence ratio as investment plus R&D expenses and acquisitions minus operating income before depreciation, divided by investment, and we take the average ratio at the three-digit SIC level. Then we set a dummy variable (*EXDEP*) to one for industries with above-median industry-level external finance dependence ratio and zero otherwise. In Columns (3) and (4), we follow Frank and Goyal (2003) and defined firm-level net change in capital as long-term debt issuance minus long-term debt reduction, scaled by total assets, and we take the average net change in capital at the three-digit SIC level. Then, we set a dummy variable (*NCC*) for industries with above-median net change in capital and zero otherwise. In Columns (5) and (6), we calculate the average loan ratio by three-digit SIC industry and set a dummy variable (*BANKLOAN*) to one for industries with above-median loan ratio and zero otherwise. Bank loan ratio is calculated as the amount of cumulative bank loan as reported in DealScan scaled by the total assets in year t. See Appendix A for other variable definitions. All models include state and year fixed effects. The numbers reported in parentheses are *t*-statistics based on standard errors clustered at the state level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	$NCSKEW_{t+1}$	$DUVOL_{t+1}$	$NCSKEW_{t+1}$	$DUVOL_{t+1}$	$NCSKEW_{t+1}$	$DUVOL_{t+1}$
$BRANCH_t$	-0.017*	-0.009*	-0.018*	-0.010*	-0.011	-0.012
	(-1.72)	(-1.77)	(-1.73)	(-1.99)	(-0.28)	(-0.61)
$EXDEP_t$	0.033***	0.019***				
	(3.33)	(3.77)				
$BRANCH_t \times EXDEP_t$	-0.022*	-0.014**				
	(-1.87)	(-2.49)				
NCC_t			0.042***	0.021***		
			(4.83)	(5.03)		
$BRANCH_t \times NCC_t$			-0.023**	-0.012**		
			(-2.20)	(-2.53)		
$BANKLOAN_t$					0.067*	0.026
					(1.92)	(1.55)
$BRANCH_t \times BANKLOAN_t$					-0.068**	-0.027*
					(-2.30)	(-1.86)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	79,231	79,231	79,231	79,231	24,928	24,928
Adj. R^2	0.073	0.080	0.073	0.081	0.068	0.076

Table 13. The Role of Lending Relationship Dependence

This table presents the results regarding the impact of bank branch deregulation on future stock price crash risk conditional on lending relationship dependence. We use the National Survey of Small Business Finances (1987 and 1998) and employ three industry-level proxies of lending relationship dependence, namely (1) the average distance between firms and their main lenders in 1987 at the two-digit SIC level, (2) the average increase in distance between banks and borrowers between 1987 and 1998, and (3) the average length of the relationship between banks and borrowers in 1987. In Columns (1) and (2), we set a dummy variable (*AVDIS*) to one for industries with below-median distance and zero otherwise. In Columns (3) and (4), we set a dummy variable (*GROWDIS*) to one for industries with below-median increase in distance and zero otherwise. In Columns (5) and (6), we set a dummy variable (*AVLENGTH*) to one for industries with above-median relationship length and zero otherwise. To economize on space, all the control variables (see Table 3) are suppressed. See Appendix A for other variable definitions. All models include state and year fixed effects. The numbers reported in parentheses are *t*-statistics based on standard errors clustered at the state level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	$NCSKEW_{t+1}$	$DUVOL_{t+1}$	$NCSKEW_{t+1}$	$DUVOL_{t+1}$	$NCSKEW_{t+1}$	$DUVOL_{t+1}$
$BRANCH_t$	-0.013	-0.005	-0.007	-0.005	-0.017	-0.008
	(-1.22)	(-0.96)	(-0.66)	(-0.89)	(-1.53)	(-1.38)
$AVDIS_t$	0.009	0.008				
	(0.90)	(1.66)				
$BRANCH_t \times AVDIS_t$	-0.032***	-0.021***				
	(-3.02)	(-4.10)				
$GROWDIS_t$			0.022*	0.012**		
			(1.99)	(2.17)		
$BRANCH_t \times GROWDIS_t$			-0.041***	-0.021***		
			(-3.61)	(-3.57)		
$AVLENGTH_t$					0.025**	0.013**
					(2.12)	(2.05)
$BRANCH_t \times AVLENGTH_t$					-0.022*	-0.014**
					(-1.79)	(-2.02)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	72,701	72,701	72,701	72,701	72,701	72,701
Adj. R^2	0.073	0.081	0.073	0.081	0.073	0.081

Appendix A. Variable definition

Crash risk variables

NCSKEW is the negative skewness of firm-specific weekly returns over the fiscal year.

DUVOL is the log of the ratio of the standard deviations of down-week to up-week firm-specific weekly returns.

For both crash risk variables, the firm-specific weekly return (*W*) is equal to ln (1 + residual), where the residual is from the following expanded market model regression:

$$r_{j,\tau} = \alpha_j + \beta_{1,j} r_{m,\tau-1} + \beta_{2,j} r_{i,\tau-1} + \beta_{3,j} r_{m,\tau} + \beta_{4,j} r_{i,\tau} + \beta_{5,j} r_{m,\tau+1} + \beta_{6,j} r_{i,\tau+1} + \varepsilon_{j,\tau},$$

where $r_{j,\tau}$ is the return on stock j in week τ, $r_{m,\tau}$ is the return on CRSP value-weighted market index, and $r_{i,\tau}$ is the Fama and French value-weighted industry index in week τ.

Bank branch deregulation variables

BRANCH is a dummy variable that equals one after a state implemented intrastate branching deregulation and zero otherwise. The years each state relaxed the restrictions on intrastate branching are shown in Table 1.

DERINDEX is a bank branch deregulation index. Following Hombert and Matray (2016), it equals zero if a state does not permit branching via M&As, de novo branching, or the formation of multibank holding companies; otherwise, the index equals the sum of the number of ways that banks may expand within a state.

Control variables

DTURN is the average monthly share turnover over the current fiscal year minus the average monthly share turnover over the previous fiscal year, where monthly share turnover is calculated as the monthly trading volume divided by the total number of shares outstanding during the month.

SIGMA is the standard deviation of firm-specific weekly returns over the fiscal year.

RET is the mean of firm-specific weekly returns over the fiscal year, times 100.

MB is the market value of equity (csho $\times$prcc_f) divided by the book value of equity (market-to-book).

SIZE is the natural logarithm of market capitalization (csho $\times$prcc_f) at the end of the fiscal year.

LEV is total debt (dltt+dlc) divided by total assets (at).

ROA is income before extraordinary items (ib) divided by total assets (at).

ACCM is the absolute value of discretionary accruals, where discretionary accruals are estimated by the modified Jones model.

Other variables

$Before^{2+}$ is an indicator variable that takes one for observations with two years or more prior to deregulation and zero otherwise.

$Before^{1}$ is an indicator variable that takes one for observations with one year prior to deregulation and zero otherwise.

$After^{1}$ is an indicator variable that takes one for observations with one year post-deregulation and zero otherwise.

After^{2+} is an indicator variable that takes one for observations with two years or more post-deregulation and zero otherwise.

Before^{5+} is an indicator variable that takes one for all years up to and including five years prior to deregulation.

Before^{1,4} is an indicator variable that takes one for the four years preceding deregulation.

After^{1,4} is an indicator variable that takes one for the four years following deregulation.

After^{5+} is an indicator variable that takes one for all years five years after deregulation.

EARNVOL is earning volatility measured as the standard deviation of the ratio of earnings, excluding extraordinary items and discontinued operations, to lagged total equity during the past three years.

CAPEX is capital expenditures (capx) scaled by total assets (at).

LNPAT is the natural logarithm of one plus total number of patents filed. Data source: Kogan et al. (2017), available at https://iu.app.box.com/patents.

TCW is citation-weighted patent counts. The weight is computed as one plus the number of citations scaled by the average number of cites to patents issued in the year *t*. Data source: Kogan et al. (2017), available at https://iu.app.box.com/patents.

GDPGROWTH is GDP growth measured as state-level GDP percent change (source: Bureau of Economic Analysis).

GDPPERCAP is GDP per capita measured as state-level GDP over state-level population (source: Bureau of Economic Analysis).

POLBALANCE is political balance measured as state-level fraction of the members of the House of Representatives from the Democratic Party in the current year.

EXDEP is a dummy variable set to one for industries with above-median industry average external finance dependence and zero otherwise. As reported in Rajan and Zingales (1998), external financial dependence ratio is defined as investment (capital expenditure (capx) + R&D expenses (xrd) + acquisitions using cash (aqc)) minus operating income before depreciation (oibdp), divided by investment.

NCC is a dummy variable set to one for industries with above-median net change in capital and zero otherwise. Net change in capital is defined as net change in equity and debt (long-term debt issuance (dltis) minus long-term debt reduction (dltr) plus sale of common stock (sstk) minus stock repurchases (prstkc)), scaled by total assets (at), as reported in Frank and Goyal (2003).

BANKLOAN is the amount of cumulative bank loan scaled by the total assets (at) in year *t* (source: DealScan).

AVDIS is a dummy variable set to one for industries with below-median distance from the main lender and zero otherwise. The data on average distance from the main lender by two-digit SIC industry is obtained in the 1987 survey (variable r6481).

GROWDIS is a dummy variable set to one for industries with below-median growth rate of the average distance between banks and borrowers between 1987 (variable r6481 in the 1987 survey) and 1998 (variable idist1 in the 1998 survey).

AVLENGTH is a dummy variable set to one for industries with above-median age-adjusted relationship length and zero otherwise. Following Hombert and Matray (2016), we regress log of length of relationship (variable r3311 in the 1987 survey) on log of firm age (1987 minus the foundation year, variable r1008) at the firm-level: $\log(Length_i) = \mathrm{a} + \mathrm{b} \cdot \log(Age_i) + \varepsilon_i$, and then we calculate the age-adjusted length of relationship as $\log\left(Length_i^{Adj}\right) = \log(Length_i) - \hat{b} \cdot (\log(Age_i) - \overline{\log(Age)})$, where $\overline{\log(Age)}$ is the average log of firm age in the sample.